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Prepared by Alpha-Gamma Technologies, Inc.
4700 Falls of Neuse Road, Suite 350, Raleigh, NC 27609

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Information Disclosure and Risk Reduction: The Sources of Varying State Performance in Control of Toxic Chemical Emissions

Michael E. Kraft
kraftm@uwgb.edu

Troy D. Abel
abelt@uwgb.edu

University of Wisconsin-Green Bay
Green Bay, WI 54311

and

Mark Stephan
stephanm@vancouver.wsu.edu
Washington State University, Vancouver
Vancouver, WA 98686

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Abstract

This paper reports on initial findings of a research project that examines the effects of information disclosure policies on environmental decisionmaking, specifically, actions related to control of toxic chemical emissions in the United States. The project seeks to determine why some companies do more to reduce toxic chemical pollution than others and why some communities encourage such pollution reduction more than others. Ultimately, we will try to identity the variables that most directly affect pollution reduction and by implication improvements in public health.

Theory: We examine state trends in reduction of toxic chemical emissions through two theoretical frameworks: the lens of comparative state environmental policy and a perspective derived from the politics of information disclosure. We hypothesize that state environmental release and waste reductions are a function of: (1) population size and economic prosperity; (2) state policy resources; (3) the structure of environmental and industrial interests; and (4) state political liberalism. Method: Ordinary Least Squares regression is used on data representing trends in reported releases and production-related waste of 11,353 facilities between 1991 and 1997. Results: Consistent with theories of information disclosure politics, the level of conservation group membership is the most influential factor influencing a state's ratio of firms reducing toxic releases to firms increasing them. States with less ideologically polarized politics also tend to host more release reducers than increasers. However, multiple regression models could only weakly account for trends in production-related waste. These findings reinforce our longer-term goal of incorporating sub-state level analysis (quantitative and qualitative) in an effort to explain the patterns of toxic chemical releases and the effect of information disclosure policies.

Since the 1980s, information disclosure policies have emerged as one of the more promising alternatives in the public policy repertoire. Considerable confidence has been expressed in their utility and potential impact in achieving diverse societal goals: corporate financial responsibility, food safety and nutrition awareness, drug safety, auto safety and fuel economy, accountable campaign financing, and environmental protection, among others (Graham 2002; Weiss and Tschirhart 1994). As a policy approach, information disclosure represents what Schneider and Ingram (1990, 1997) refer to as capacity building tools, that is, policies or programs that aim to inform or enlighten and thus to empower people to act on their concerns. These tools are attractive in part because they may complement or replace government regulation, thereby reducing the costs and burdens often imposed by enforcement of regulatory standards. They also are consistent with widely held values promoting citizen access to information in a democracy, captured in the phrase "right-to-know" (Hadden 1989). In addition, they reflect conviction on the part of many analysts that new policy strategies may improve both public and private sector performance, particularly in areas such as environmental protection, where emphasis is placed increasingly on community-level action to promote public and environmental health (Mazmanian and Kraft 1999; National Academy of Public Administration 2000; Portney 2003; Sexton et al. 1999; Tietenberg and Wheeler 1998).

Despite their widespread use, there has been little systematic inquiry into how information disclosure policies actually affect corporate or community decisionmaking, and how they might be designed to maximize their effectiveness—for example, in the way information is communicated to the public and actions that could improve the public's capacity to understand and use the information. The development of training programs in use of such data is one example.[1] As one prominent illustration, the federal Toxics Release Inventory (TRI) program authorized by a provision of the Superfund Amendments and Reauthorization Act (SARA) of 1986 often has been cited as a success story in dissemination of information about releases of toxic chemicals by industrial facilities. Title III of SARA created the Emergency Planning and Community Right-to-Know Act (EPCRA), section 313 of which mandates that manufacturing facilities report their annual releases of listed toxic chemicals to the EPA; the agency in turn makes the information public.[2] The information is available in an online database that can be accessed by the public and stakeholders, and summary statistics are provided in a TRI Public Data Release report each year. In addition, some environmental groups, most notably Environmental Defense, make the data available online in a variety of graphic formats that allow community residents to assess what each industrial facility in their communities is emitting (www.scorecard.org).

The TRI program has been evaluated positively because of its appearance of effectiveness. Indeed, the EPA itself calls it a "tremendously successful program," the results of which "speak loudly for themselves" (U.S. EPA 2002a).[3] The agency comments refer to the dramatic reduction since the late 1980s in the volume of toxic chemicals released by manufacturing facilities reporting under the program. Positive conclusions about the program's utility are reached even when observers acknowledge that most of the reductions in chemical releases occurred during the first five years of the program and that current volumes of releases remain quite large and continue to pose a significant risk to public and environmental health (Press and Mazmanian 2003). Still, it is easier to document reductions in the release of such chemicals than it is to explain the mechanism by which these reductions have occurred since the late 1980s, or to anticipate how future reductions in emissions might occur if such disclosure policies remain a major component of federal and state environmental protection efforts (Stephan 2002).

As Graham and Miller (2001), among others, argue, the overall reductions in release of toxic chemicals reported in the TRI require careful interpretation in light of the complexity of the reporting system, major changes made to it over time, and the multiplicity of variables that can affect corporate environmental decisions. They note, for example, that reported decreases in chemical releases “mask widely varying trends in major manufacturing industries” (15). It is apparent that economic factors affecting particular industries, new regulations or enforcement actions by federal and state regulators, and decisions made by managers of particular facilities with large releases can significantly affect the national trends on which analysts usually focus (see also Natan and Miller 1998).[4] The EPA itself regularly includes comparable warnings in its annual TRI report on the “limitations that must be considered when using the data”; these include the widespread use of estimated rather than actual data on chemical releases and significant variation among companies in the way they estimate such releases (U.S. EPA 2002a, ES-13).

One conclusion is that to understand the way such information disclosure works, why it is successful (or not), and its potential for the future requires analysis that is directed at state-level trends and especially at decisions made in specific communities that are located near facilities producing large quantities of toxic emissions or emissions with high risk levels. Even here, however, interpretations of the data differ. The EPA has been quite optimistic about the way the TRI data are likely to be used by governments and citizens at these levels, as indicated in the following statement from the press overview that accompanied its 2002 TRI report:

> Governments—federal, state, and local—have used the TRI to set priorities, measure progress, and target areas of special and immediate concern. The public, our most important customer, has used the TRI data to understand their local environment, to participate in local and national debates about the choices being made that may affect their health and the health of their children and, ultimately, to exert their influence on the outcomes of these debates (U.S. EPA 2002a, 1).

Many analysts seem to agree with the EPA’s assessments. They argue that use of the TRI information has “contributed significantly to community organizing efforts to change facility emission behavior” (Bouwes, Hassur, and Shapiro 2001, 2). Similarly, a survey of corporate leaders found that over half acknowledged that “pressure from community activists” has affected their companies’ behavior, sometimes leading to a reduction in chemical pollution (cited in Bouwes, Hassur, and Shapiro 2001).

A common argument, captured in a recent report by EPA analysts, is that public access to information “can drive change more effectively than regulations alone,” and that the release of such information “can help to empower community residents, heighten industry accountability to the citizenry, and support efforts to ensure environmental justice” (Bouwes, Hassur, and Shapiro 2001, 1). The authors do state clearly that availability of data is a necessary but not sufficient condition to achieve such goals. Beyond use by such activists, state and local agencies use TRI data in developing emergency planning procedures, formulating legislation, and monitoring toxic waste. Many states also have supplemented EPCRA with their own right-to-know legislation and regulations, and some mandate reduction in a facility’s toxic emissions.

Even with such qualifications, we wonder whether the confidence in the use of TRI data expressed by the EPA and others is fully warranted, particularly as such use applies to citizens and stakeholder groups at the community level. For most of the life of the TRI program, these citizens and groups worked with data that reflected only the total pounds of chemical releases, not a more meaningful indicator of public or environmental health risks (that is, information about exposure and toxicity). Some scholars have expressed somewhat similar reservations about the effectiveness of voluntary pollution reduction measures adopted as an alternative to conventional regulation, where companies may lack the incentives to do as much as they would under a regulatory regime with consistent enforcement of the law (e.g., Harrison and Antweiler 2003). Only empirical research can adequately address these concerns.

These observations lead us to some basic questions that need to be asked about information disclosure policies of this kind. We focus on environmental policies, but the questions are equally applicable to other forms of information disclosure. How does the collection and dissemination of such information bring about a change in corporate behavior that leads to a reduction in the amount of toxic chemicals released to the local environment? What specific changes in incentives occur as a result of such policies that might lead corporate officials to alter manufacturing processes or take other actions to reduce releases of toxic chemicals?[5] What are the effects of information disclosure on communities themselves, specifically on the level of attention paid to and concern over chemical risks in a local area? What efforts do public officials, community leaders, environmental and health groups, and others make to try to reduce emissions by local industry, and how effective are those efforts?

Our larger research agenda includes the building of a framework for understanding the effects of environmental information disclosure on corporations and communities, using the case of the federal TRI program. The research seeks to answer two major questions: What effect does the release of information about pollution output have on decisionmaking by corporate officials, community leaders, and representatives of local and regional nongovernmental organizations, and on environmental quality outcomes? What factors mediate the use of such information and thus condition behavioral changes and the environmental outcomes they produce?[6] If those questions can be answered, we hope to be able to determine which components of the TRI are most likely to affect behavior, in what ways, and why. That knowledge should add to our understanding of how the processes of environmental information disclosure work within both a corporate and community setting, and how they result in improved environmental outcomes. There are important implications for the design and implementation of information disclosure policies, and for the TRI program in particular.[7]

The Analytic Framework

A rich body of work exists on the emergence of non-regulatory measures for businesses and communities that are aimed at increasing public involvement as well as the dissemination of information (Dietz and Stern 2003). However, these studies give little attention to factors normally considered from a political science perspective that can help to explain the relationship between environmental performance in industrial facilities and decisionmaking within surrounding communities (Stephan 2002). We hope the project will help to fill that void by integrating a number of different theoretical perspectives that bear on non-regulatory environmental decisionmaking into a useful analytic framework.

These perspectives are drawn from studies of (1) risk perception and communication (where variables such as trust in the source of information, a capacity to understand it, and surprise or shock upon release are important); (2) organizational behavior (for example, corporate embarrassment or shame when emissions data are released, and internal firm characteristics that affect action on chemical risks); (3) transaction costs in acquiring and using information (disclosure policies may reduce the cost of information for citizens and thereby encourage a higher level of participation); (4) environmental justice (inequities in the distribution of chemical risks and economic benefits, the likelihood of variable levels of citizen action in communities with different racial and economic profiles); (5) political agenda setting (new information may alter the way problems and potential solutions are viewed as well as their saliency); and (6) policymaking processes at state and local levels (such as different capacities among states and communities to take action on environmental risks).

For the present paper we outline the framework only briefly because we focus below on one component of firm environmental performance in the fifty states. One dependent variable is a state's ratio of TRI facilities that reduce production related waste to the number of TRI facilities that expand production related waste. Another is the ratio of TRI facilities that reduce their total releases to the number of TRI facilities that expand their total releases.[8] A third is the ratio of facilities that decrease both releases and production related waste to those that increase both. We seek to explain variation among the fifty states in these measures of environmental performance. We do so partly because theory suggests that state variations do matter and reflect relevant differences across states (see the literature review below). In later papers, we will examine the related dependent variable of actual reduction in health risk to exposed populations using the U.S. EPA's RSEI model discussed in note 1.

The key assumption in our working model is that TRI information disclosures affect community and corporate decisionmaking through an alteration in risk perception and a change in environmental beliefs and values. That is, the release of such information is likely to affect the way corporate officials and community residents think about environmental and public health and the value they attach to them. It may be that the information enhances knowledge of previously uncertain health risks, that it conflicts with established expectations of industry behavior or community health, or that it raises the saliency of these issues. The new knowledge and attention given to it within both the corporation and community may also raise levels of concern, propel the issues to a higher status on corporate and community agendas, mobilize community leaders and activists, and as a result spur action to reduce emissions. How the information releases, the environmental risks themselves, and the associated corporate and community actions are covered by local media should be a significant variable as well.

Decisionmaking within a corporate setting and within a community obviously is complex and is affected by many factors (including, as reported here, state differences). For example, pollution reduction by companies may come about because of the actions of green investors and consumers who cajole them to change their ways, pressures from the local community and interest groups, competitive business practices (including perception of liability), shared learning within the industrial sector, the development of new technologies, efforts to forestall anticipated regulation through voluntary action, or calculations that the benefits of reducing releases of toxic chemicals exceed the costs (Harrison and Antweiler 2003; Press and Mazmanian 2003). Some companies may engage in what appears to be voluntary reduction in release of toxic chemicals but which in reality is part of the firm's compliance with what it believes to be (or soon will be) required by federal or state law.[9]

We will explore many of these possibilities during the field work stage of the project when interviews will be conducted with corporate officials and community leaders and activists. For this paper, as noted, we focus on variation in facility performance across the fifty states. We distinguish facilities that decrease production-related waste and toxic chemical emissions (and thus risk) from those that show an increase in either and we look for patterns across the states to help explain these variations.

As indicated in Table 1, this dimension is one of two that can be used to distinguish the states by placing them into four categories: greening, browning, safer but still dirty, and cleaner but riskier. The x-axis represents a continuum of production-related waste where facilities on the left-hand side increase waste. If they decrease waste, they progress to the right side of the axis, towards cleaner production. A continuum of risk runs along the y-axis, with facilities that increase pollution risks on the bottom. If they reduce risk, they progress upwards towards safer production. When facilities reduce both production-related waste and pollution risk, they move from the lower-left to the upper-right, reflecting an ideal case of cleaner and safer production. As discussed above, we will fill out the typology in a related paper that will include estimates of actual risk reduction using EPA's RSEI model.

Industrial Pollution and Environmental Decisionmaking Within and Across States

That states vary in pollution production, management, and reduction, is not really in doubt. However, scholars have tried to explain why some states do more than others to manage or reduce their toxic pollution levels (including hazardous waste production), and why some states see marked improvement in pollution control, while other states do not exhibit the same success (Bacot and Dawes 1997; Grant 1997; Lester 1983; Lester and Lombard 1990; Potoski and Woods 2002; Ringquist 1993; Williams and Matheny 1984; Yu et al. 1998). Some common themes have emerged over decades of work.

Research that seeks to improve understanding of *state-level policy choices or priorities* (as defined by regulatory decisions or budget expenditures) has focused mainly on whether policy has more to do with political factors (such as interest group strength), administrative capacities (both in terms of fiscal health and organizational capacities), or the severity of the pollution problem. Results have varied, suggesting at the least that multiple factors help to shape the choices made by states. For example, some studies have found that pollution severity is itself a significant influence on state choices (Bacot and Dawes 1997; Lester et al. 1983; Ringquist 1994), while others have had mixed results (Potoski and Woods 2002) or found no such evidence (Lombard 1993; Williams and Matheny 1984). A variety of studies have suggested that administrative or state capacity is critical, whether the factor is measured by a consolidated environmental bureaucracy (Lester et al. 1983), state size (Potoski and Woods 2002), or the level of professionalism of state legislatures (Lester et al. 1983; Ringquist 1994). In contrast, Lombard (1993) suggests that federal actions may be more critical to state decisionmaking than state capacity factors. In particular, state enforcement activity may follow closely in the wake of federal enforcement action, regardless of other state-level factors.

The most contradictory results relate to political factors, with some research suggesting that the strength of interest groups or political parties is critical (Bacot and Dawes 1997; Ringquist 1994; Sigman 2003; Williams and Matheny 1984), while others present findings that undermine this thesis (Davis and Feiock 1992; Lester et al. 1983; Lombard 1993). In one of the most interesting studies to date, Potoski and Wood (2002) find evidence that suggests political factors influence some state policy choices, but not all, and that different types of environmental programs will have different sets of drivers. They conclude that programs "that allocate resources may be influenced primarily by the interplay of affected interests within the larger political environment"(211). Because different environmental arenas tend to draw different interests, we should not be surprised that the significant factors vary. Another potential political variable is state opinion liberalism which may have an indirect affect on state choices by influencing the strength of interest group and party behavior (Erikson, Wright, and McIver 1989; Ringquist 1994; Wright, Erickson, and McIver 1987).

Research focused on understandings *changes in industrial pollution levels over time* has tended to focus on factors such as regulatory pressures, non-regulatory pressures (e.g., information disclosure programs), and industrial practices. Ringquist (1993) finds that the stringency of regulation has a significant and negative relationship to sulfur oxide (SO_2) and nitrogen oxide (NO_x) emissions. In a related vein, Yu et al. (1998) find that as the direct regulation of pollution emissions increases, the levels of emissions fall over time. Research also suggests that variations in state-level information disclosure programs are related to changes in emissions. States that fund information disclosure programs (Grant 1997) and extend their outreach to affected communities (Yu et al.1998) are able to reduce toxic emissions, even when controlling for factors such as industry production levels, state wealth, and enforcement activities.

Regulatory and non-regulatory actions can also move in tandem and interact in important ways (Yu et al. 1998). To use Schneider and Ingram's (1997) "policy tool" terminology, it may not be a question of whether "informational tools" are better or worse than "authoritative tools," but rather the extent to which both may together have an impact on pollution levels. Though shifts in industrial practices attributable to technological updates or economic changes undoubtedly have an affect on pollution levels (Stephan 2003; Yu et al. 1998), the extent to which these occur as exogenous pressures remains unclear. Arguably improvements in pollution levels may in turn help to spur the spread of technological practices and may help revive states dealing with economic downturns.

In sum, previous research suggests a multi-faceted examination of the policy relevant factors which may influence changes in industrial pollution over time. Key categories of variables include both political and administrative factors. Regulatory and non-regulatory variations across states are potentially critical and cannot be ignored. Finally, control variables--such as the severity of the problem--must be included in order to better assess whether policy choices are proactive or reactive.

Data and Methods

EPA's online TRI Explorer (www.epa.gov/triexplorer) provided facility-level data for this study's dependent measures. To characterize state-level TRI trends, the study analyzed a sample of facilities (11,353) reporting in both 1991 and 1997 and their changes in reported releases of toxic chemical pollutants as well as production-related-waste (PRW). The PRW is the sum of all toxic wastes generated across a firm's production processes that a facility reports as recycled, recovered for energy, treated on and off-site, or released on and off-site. The year 1991 was selected because it was the first year in which PRW was reported in response to the 1990 Pollution Prevention Act. We end with 1997 because it was the last year for which the EPA's first version of the RSEI model included facility-level data. We will extend the period covered in later work. The sample included only the 1991 core chemicals to assure consistent comparisons of facility-level toxic chemical management across the two years.[10] These facility characteristics were then aggregated to explore state-level factors related to environmental waste management changes. However, ten states were excluded from the analysis because their relative smaller concentrations of TRI facilities distorted statistical comparisons.[11]

Candidate independent variables encompassed state measures of *policy*, *politics*, and *resources* (see Table 4). No single variable was critical in our initial data gathering. Rather, we sought to identify a set of interrelated clusters of variables that captured our understanding of what the literature suggests are the compelling factors in driving state differences.

A set of state *policy* measures were obtained from Bob Hall and Mary Lee Kerr's *1991-1992 Green Index*. Their index of "state policy initiatives" encompassed 73 different policies, and we included this broad measure while also breaking out some of its subcomponents that addressed toxic waste. Specific policy measures included the presence or absence of a state program promoting access to information about toxic chemical usage and releases, the imposition of fees that support community right-to-know (RTK), and laws requiring toxics reduction plans and reporting. Another variable was included: a measure for state spending on air pollution per capita. The policy measures were intended to capture the extent to which states have policies, programs, and budgeted funds in place to deal with pollution.

Political variables included several measures from Erikson, Wright, and McIver's (1993) work. Their composite index of policy liberalism was included, as were measures for state partisan identification (whether citizens lean Democratic or Republican) and ideological identification (whether citizens lean conservative or liberal). Further variables included a measure of mass ideological polarization (the extent to which party members in a state contrast ideologically with the members from the opposing party) and measures for Democratic/Republican elite ideology (the average ideological stance for party leaders in the state). Beyond these "statehouse democracy" variables, separate measures were used for the number of members of conservation groups, community improvement or capacity-building groups, and philanthropic groups (all per 1,000 population) to further understand the role of interest groups, both state-level and local. In sum, the variables were meant to capture the extent to which political forces both within and outside of state governments might either directly or indirectly influence pollution reduction.

Our *resource* variables were meant to serve as controls. Our intent was to avoid attributing to policy or politics what may have more to do with demographic or economic differences across states (although the three categories of variables are intertwined to some degree). Population, poverty, unemployment, and income measures all came from the U.S. Census**.** Following Potoski and Woods (2002) and Ringquist (1993, 1994), we included measures of industry group strength, such as the value added by manufacturing (as a percentage of the state's gross product) associated with state firms most responsible for air pollution.

Results

Forty-one percent of facilities (4,655) reported reductions in chemical releases and reductions in production related waste, while thirty-four percent of sampled facilities increased both (see Table 1). On average, 41% of a state's facilities achieved both pollution and waste reductions, with the top percentile occupied by North Carolina, Maine, and Connecticut, which had more than 48% of their TRIs moving towards safer and cleaner production.[12] A second group of state performers included New Hampshire, Delaware, New York, Virginia, New Jersey, Massachusetts and Rhode Island, which saw more greening TRIs than 75% of their peers. Lagging states, where less than 37% of TRI facilities achieved pollution and waste reductions, included Kentucky, Georgia, Utah, Michigan, Louisiana, and Maryland. Arizona and Tennessee hosted the fewest TRIs moving towards greener production, with less than 32% reducing waste and releases (See Table 2 and Figure 1). Table 3 presents state TRI aggregations for total facilities, the percentage reducing both waste and pollution releases, and the ratio of waste and release reducers to increasers.

While states, on average, saw more firms reducing releases than increasing them (134 to 99, respectively, or a ratio of 1.41), the opposite was true for production-relate waste trends. An average of 126 firms per state were managing more hazardous waste in 1997 than in 1991, while 119 saw, on average, less PRW (see Table 4). These figures suggest that the TRI program is perhaps not as successful as many have assumed it to be. Much depends on which indicator one selects for analysis. As we show here, substantial decreases in overall emissions can occur at the same time that many firms are increasing production-related waste. More recycling or energy recovery may be occurring but firms can move closer to clean production with source reduction.

Bivariate correlations for a variety of socioeconomic, policy, and political measures (listed in Table 4) on two sets of dependent variables (state concentrations of release and waste reducer facilities) yielded a diversity of expected and unexpected patterns (see Table 5). As previous research would suggest (Yu et al., 1998), informational tools in the form of state Right-to-Know (RTK) initiatives produced a moderate correlation with the ratio of firms in a state reducing production-related waste (PRW). However, an unexpected negative correlation appeared between PRW reduction ratios and an index of state environmental policy initiatives. The percentage of a state's firms reducing PRW also produced a negative correlation with unemployment trends. However, because the variable was operationalized as the difference in unemployment percentages from 1990 to 1996, higher positive values meant more job losses and the negative correlation meant reductions were associated with unemployment decreases.

Similarly, a state's concentration of firms reducing toxic chemical releases also yielded a negative correlation with unemployment increases and the index of state environmental policy effort. Additional positive correlations between release reducer concentrations appeared with environmental conservation membership levels, state ideological identification, a composite index of policy liberalism, and, unexpectedly, with Republican elite ideology. These patterns are somewhat consistent with findings in the comparative state policy literature. A slight variation appeared when the release variable was operationalized in percentage terms instead of a ratio. State partisan identification emerged with a significant correlation while no significant relationships emerged for policy liberalism or Republican elite ideology.

When our comparison measure was the sheer amount of toxic releases, bivariate correlation patterns mirrored relationships indicative of the states' size, and therefore their pollution levels (see Table 5). The strongest correlations appeared between 1991 release levels and population, Manufacturing Gross State Product, and Air Polluters Gross State Product, while moderate relationships appeared with state poverty levels and the number of community and foundation groups. Conservation group membership density, on the other hand, produced a negative correlation with total release levels. On a measure of release trends, the correlations again produced a pattern reflecting the influence of state size, with positive relationships appearing with population, manufacturing GSP, and air polluters GSP. Finally, when the percent change relative to a state's 1991 release amount base was examined, only the index of state pollution prevention effort and conservation membership density produced significant negative correlations.

In the next phase of the study, the correlation analysis and our theoretical framework guided the exploration of multiple regression models of environmental waste and release reductions using ordinary least squares (OLS). Since our key dependent variables are interval measurements, OLS is an appropriate statistical estimation technique. An initial analysis was performed with all of our candidate predictors, although it was clear that issues of multicollinearity made this model inappropriate.[13] Candidate predictors were eliminated if they presented multicollinearity problems or they were dropped through a series of likelihood ratio tests for variables that had a consistently low impact on the model. Combinations of independent variables from each theoretical grouping (policy, politics, and resources) were followed with the calculation of a Variance Inflation Factor (VIF).When predictors are highly correlated, standard errors of fitted coefficients are inflated and commonly diagnosed with the VIF procedure in statistical software packages. Independent variable combinations were excluded if items exceeded a VIF of three. Finally, adjusted R-squared were compared across models in order to better understand what variables seemed to consistently hold up under multiple specifications.[14]

Although concerns about endogeneity were considered, we were unable to perform comprehensive tests to cover this contingent. Our main concern was whether our policy factors might be explained partly by our political or resource variables. Initial analyses suggest that there is little or no direct relationship between variables such as "citizen right-to-know laws" and our key political and resource measures.[15] Part of the problem is that our measure for pollution releases (TRI data) is truncated. We do not have good information before 1989, and therefore we cannot check to see whether changes in pollution output have driven the creation of policies, the nature of politics, or the quality of resources before the late 1980s. Future research will address these concerns in more depth.

These iterations left just six independent variables in our models of toxic chemical trends among facilities aggregated across the states. In the first model of the percentage of firms reducing releases, conservation group membership produced the largest standardized coefficient estimate. Ideological polarization was the second largest coefficient, but in a negative direction, while state initiatives on citizen right-to-know produced a third significant and positive coefficient. Insignificant factors included population, a pollution prevention index, and an index of policy liberalism. The adjusted R^2 indicated that over 30% of the variance in the dependent variable could be explained by the model's combination of independent variables (see Table 6). Model 2 displayed a higher R^2 (45.6%) and showed that both conservation group membership and ideological polarization relate to a state's ratio of release reducer facilities to increasers. In both models, the statistically significant F-test demonstrated that rejection of the null hypothesis that each independent variable except the constant are equal to zero could be rejected with 99% confidence.

The same multiple regression model faired much worse when the dependent variable encompassed Production-Related Waste (PRW) trends in percentage or ratio form. In model 3, a state's percentage of facilities reducing PRW did not produce a commonly accepted level of statistical significance on the F-ratio (see Table 7). However, in bivariate regressions, the citizen right-to-know measure did produce a statistically significant model with an adjusted R^2 of 0.232 and 0.117 (see Table 8).

In three final models, we performed multiple regressions on three dependent variables that combined facility-level waste and release performance: (1) a ratio measure of facilities reducing both toxic waste and releases to facilities increasing them; (2) the percentage of toxic waste and release reducers; and (3) the percentage of toxic waste and release increasers. The model of our sample's ratio of toxic reducers displayed an adjusted R^2 indicating that the combination of independent variables accounted for nearly half (48.8%) of the variance in the dependent variable (see Table 9). Conservation group membership again achieved the largest standardized and significant coefficient, followed by ideological polarization in the negative direction and value added by air polluters with a positive correlation.

When the percentage of toxic reducers (both waste and releases) became the dependent variable, the regression's performance dropped substantially and achieved an adjusted R^2 accounting for less that 18% of the variance. Conversely, our explanatory model of toxic increasers accounted for almost half of the dependent variable's variance (the adjusted R^2 = 0.494) with significant but negative coefficients on conservation group membership and value added by air polluters and a positive coefficient for ideological polarization.

Discussion

The results, taken in their entirety, are suggestive rather than conclusive. There are at least three conclusions we take away from them.

The first is that policy factors and political factors may both play a role in driving state differences. For example, policy liberalism correlates positively with the number of facilities making release reductions between 1991 and 1997 (see Table 5) while ideological polarization seems to have a negative influence on a number of the dependent variables, including the percentage of firms reducing releases and the ratio of toxic reducers to increasers (see Table 6). Interestingly enough, the same cannot be said of reductions in PRW when observed in isolation (see Table 7). This actually makes sense given the nature of the TRI as a form of information disclosure. Pollution releases have a much greater salience in the media and in communities than does PRW. Moreover, the results relating to ideological polarization may suggest that politically liberal groups are having more of an impact in less polarized states, or facilities may be more sensitive to this threat and more likely to be reducing release levels. Though PRW data are publicly available, it is the subset of pollution releases that gets greater attention from interest groups and average citizens. Other PRW besides releases are "off the radar" for all but the closest of examiners of TRI information.

The consistency of statistical significance in our measure of environmental group membership levels further reinforces the influence of a state's political environment (see Tables 6 through 9). Two interpretations are possible. First, facility pollution reductions in states with more conservation group participation may be evidence that companies are facing more pressure from organized environmentalists in some states and not others. Or, companies may be anticipating pressure from a more vigorous environmentalism and be making efforts to forestall unwanted attention by reducing emissions. A definitive conclusion one way or another must await more focused research.

There is some evidence to suggest that the presence of state-level citizens right-to-know programs helps to increase the percentage of TRI facility reducers in a given state across both PRW and total releases (see Tables 6 to 8). This is not surprising, given that others have found related results (Grant 1997; Yu et al. 1998). At the same time, we can only speculate why this is true. One possibility is that state level right-to-know programs may actually enhance the abilities of facilities to measure their own performance, which in turn may be driving them to reduce their overall waste production. Facilities in the 1990s were measuring aspects of their production processes that they were not measuring ten years previously. Alternatively, state level programs may increase the salience of both total releases and PRW for citizens outside of these firms, which in turn motivates them to put pressure on the facilities for change. These results raise further questions about the extent to which information disclosure programs are driving firm behavior from within rather than firms responding to external demands for change.

The second major conclusion is that results in the correlation matrix suggest the possibility of an interesting dynamic that defies straightforward explanation (see Table 5). When comparing results for the ratio or percentage of reducers in total releases to the results for changes to total pounds of pollution reduced in each state, the findings suggest that two separate processes may be going on. It is conceivable that key political factors, such as policy liberalism within a state, improve the likelihood that facilities will move towards pollution reductions rather than expansions, while at the same time macro-level variables such as population and manufacturing gross state product are driving the amount of change in pollution output overall. The former suggests that political factors can influence the *direction* of change, while non-political factors may influence the *intensity* of change.

In the context of theory about the function of information disclosure policies, it may be that such programs can help to motivate companies to improve their environmental practices, but the robustness of those improvements may be constrained by factors that no level of information transfer can influence. Relatedly, the dynamic here may be similar to the findings of Potoski and Woods (2002), in which different environmental issues draw different sets of interests to them.

The third major finding, the lack of significant results across most of our independent variables, is itself telling. Arguably, our broad argument about the influence of information disclosure programs remains viable: decision-making is most critically influenced closer to the source—either through interactions with the community or within facilities (and companies) themselves. State differences can mediate some of what happens at the local level, but only partially.

Future Directions

Measuring and modeling the factors that influence innovative environmental decisions and outcomes will significantly advance our understanding of their relationship to information disclosure policies. However, we also intend to augment this modeling with qualitative analysis through the use of questionnaires, interviews, and case studies. Such a precedent was outlined by Meier and Kaiser (1996) when they leveled a provocative criticism of traditional regression techniques. They point out that these focus on average cases when more interest may lie in unusual cases. For the research under way, this would be communities with high concentrations of facilities that have undertaken source reduction or have decreased pollution levels beyond what would have been expected (performers). If the most ideal presumptions about information disclosure are right, we would expect to find performing firms to indicate that their environmental management choices were partially or even fully influenced by community factors. On the other hand, much can be learned from communities hosting facilities that struggle to change (or regress on) their environmental management and/or their pollution levels (strugglers).

We can offer a few comments about the direction in which our research efforts are heading. As noted, these kinds of initial results will guide our sampling of leading and lagging facilities and the communities in which they are located**.** We will use a survey questionnaire to be sent to corporate officials, administrative agency officials, environmental group leaders, and community leaders in 30 communities. The questionnaire will focus on how environmental decisions have been affected by TRI information disclosures, with attention to the range of variables identified earlier in the paper. Special consideration will be given to opportunities for communication between communities and industrial facilities. As noted above, we will also gather information on corporate environmental commitment, environmental expertise, and management structure**.**

Following analysis of the data from the questionnaires, we will select ten communities for in-depth field research. This effort will involve interviews with corporate officials as well as environmental agency administrators, environmental group leaders, and community leaders, and will focus on the effects of information disclosure on decisionmaking. We will match five communities with leading industrial facilities and five with laggard facilities in terms of TRI reductions and toxics management. The flexibility inherent in interviews will allow us to probe more effectively on issues raised by our respondents that expand our understanding of information disclosure programs and give us some indication of possible reforms or policy changes.

Our research strategy is to compare leading and lagging firms and the communities in which those firms are located. Selecting firms from the greening and browning categories should facilitate this comparative case study approach, and the qualitative phase of our research should enable us to learn much about why these firms make the kinds of decisions they do about toxic chemical pollution.

As suggested early in the paper, there are policy implications to work of this kind. Until we know more about the effect of information disclosure programs we cannot speak with confidence about either their previous success or what changes in policy design or implementation might make them more effective in the future. In later reports we hope to be able to address how the TRI program might be redesigned to provide greater incentives to industrial facilities to reduce both production-related waste and the volume of high-risk chemicals released to the environment. We also expect to say more about how communities can use TRI data (including new data coming from the RSEI model) to become better informed about health and environmental risks and help to influence corporate environmental decisions that can have a substantial effect on those risks.

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Table 1. Firm Environmental Performance Associated with Increasing or Decreasing Waste Production and Risk

Risk	Production-Related Waste	
	Increasing (Dirtier)	**Decreasing (Cleaner)**
Decreasing (Safer)	**Safer, but Still Dirty** 1,898 (16.7%) Example: a firm could substitute a more benign chemical for one of its most toxic air releases but still generate and even release large quantities of less toxic pollutants.	**Greening Firms** 4,655 (41.0%) Example: a firm installs new pollution control equipment that decreases the volume of its more toxic air releases and initiates source reduction activity that reduces its production-related waste.
Increasing (Riskier)	**Browning Firms** 3,911 (34.4%) Example: a firm increases production but takes no steps to control the higher volume of toxic air releases and production-related waste.	**Cleaner, but Riskier** 889 (7.8%) Examples: a firm targets its biggest waste streams for reductions while maintaining or even increasing a low volume, but highly toxic air release.

Note: Production-related waste is the sum of all toxic wastes generated across a firm's production processes that a facility reports as recycled, recovered for energy, treated on and off –site, or released on and off-site. For this paper we use TRI total releases as a surrogate for risk. In future papers we will apply the U.S. EPA's RSEI model to toxic air emissions to gain a more useful measure of actual risk to exposed populations.

Table 2. State Performance in Hosting TRI Facilities That Reduce Both Production-Related Waste and Pollution Releases

Brownest Less Than 37%	Browns 37% to 40%	Yellows 40% to 43%	Greens 43% to 46%	Greenest More Than 46%
Tennessee	Nebraska	Iowa	New Hampshire	North Carolina
Arizona	West Virginia	South Carolina	Delaware	Maine
Maryland	Oregon	Washington	New York	Connecticut
Louisiana	Pennsylvania	California	Virginia	
Michigan	Missouri	Illinois	New Jersey	
Utah		Kansas	Massachusetts	
Georgia		Oklahoma	Rhode Island	
Kentucky		Ohio		
		Mississippi		
		Minnesota		
		Indiana		
		Wisconsin		
		Alabama		
		Texas		
		Florida		
		Colorado		
		Arkansas		

Note: Ten states were dropped from the analysis because they had too few facilities to permit comparative statistical analysis without distorting the results: Alaska, Hawaii, Idaho, Montana, Nevada, New Mexico, North Dakota, South Dakota, Vermont, and Wyoming.

Table 3. Trends in Toxic Chemical Waste and Release Reductions for the Fifty States

State	Reporting TRIs 1991, 97	% Waste & Release Reducers	Ratio of Reducers to Increasers	State	Reporting TRIs 1991, 97	% Waste & Release Reducers	Ratio of Reducers to Increasers
Alabama	270	40 %	0.99	Montana	15	33 %	1.00
Alaska	4	25 %	.50	Nebraska	72	38 %	0.96
Arizona	89	31 %	0.67	Nevada	17	38 %	1.29
Arkansas	219	40 %	1.05	New Hampshire	61	46 %	1.40
California	662	43 %	1.57	New Jersey	335	44 %	1.63
Colorado	78	40 %	1.24	New Mexico	22	41 %	1.29
Connecticut	201	48 %	1.81	New York	371	45 %	1.62
Delaware	42	45 %	2.38	North Carolina	471	51 %	1.71
Florida	226	40 %	0.85	North Dakota	10	20 %	0.33
Georgia	347	37 %	0.90	Ohio	931	42 %	1.27
Hawaii[a]	5	----	------	Oklahoma	138	42 %	1.21
Idaho	18	39 %	1.40	Oregon	135	39 %	0.95
Illinois	702	43 %	1.30	Pennsylvania	681	39 %	1.11
Indiana	565	40 %	1.02	Rhode Island	71	44 %	1.41
Iowa	201	43 %	1.16	South Carolina	293	43 %	1.25
Kansas	139	42 %	1.05	South Dakota	29	41 %	1.09
Kentucky	240	37 %	1.14	Tennessee	306	31 %	0.69
Louisiana	177	36 %	1.25	Texas	657	40 %	1.22
Maine	52	48 %	2.08	Utah	63	37 %	0.85
Maryland	88	35 %	1.03	Vermont	21	48 %	2.00
Massachusetts	279	44 %	1.63	Virginia	262	44 %	1.19
Michigan	497	36 %	0.96	Washington	153	43 %	1.38
Minnesota	252	40 %	1.10	West Virginia	90	38 %	1.26
Mississippi	170	41 %	1.13	Wisconsin	478	40 %	1.12
Missouri	289	39 %	0.96	Wyoming	10	30 %	0.60

[a] No percentage is entered for Hawaii because the state has too few cases to permit comparison with the other states.

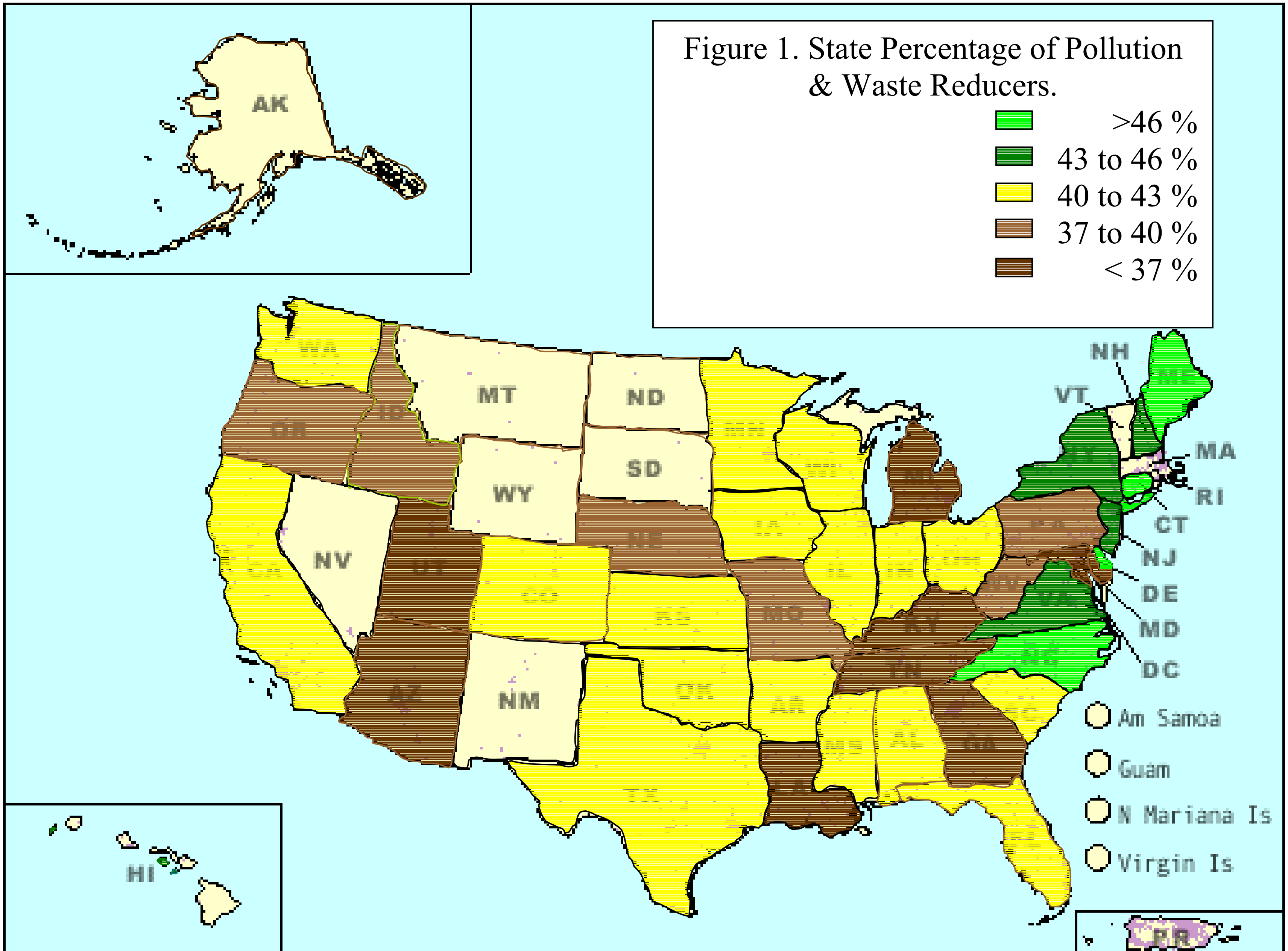

Figure 1. State Percentage of Pollution & Waste Reducers.

Table 4. Descriptive Statistics for Variables

Independent Variables	N	Mean	Std. Deviation	Minimum	Maximum	Skewness Statistic	Skewness Std. Error	Kurtosis Statistic	Kurtosis Std. Error
State census population (90)[a]	50	4963.14	5462.411	454	29786	2.55	0.337	8.301	0.662
Conservation Members[b]	50	8.544	3.58467	2.5	20.2	0.64	0.337	0.831	0.662
Manufacturing GSP[c]	50	20324.38	22457.41	486	113253	1.98	0.337	5.004	0.662
Air Polluters' GSP[c]	50	6791.14	7470.428	120	33794	1.80	0.337	3.515	0.662
Value Added by Air Polluters[c]	50	0.33584	0.140083	0.105	0.751	1.01	0.337	1.139	0.662
State Spending on Air Pollution (per capita)	50	0.629	0.498317	0.14	3.26	3.24	0.337	15.256	0.662
Comm. Improvement Groups[d]	50	159.3	165.6466	18	770	1.94	0.337	3.916	0.662
Foundations (1988)[d]	50	789.58	1096.155	30	6417	3.33	0.337	14.133	0.662
Median household income[c]	50	29102.82	5560.394	20136	41721	0.64	0.337	-0.151	0.662
Per capita income in 1989	50	13658.58	2347.937	9648	20189	0.62	0.337	0.057	0.662
State Pollution Prevention[e]	50	1.80006	1.529701	0	6	1.06	0.337	0.768	0.662
State Policy Initiatives[e]	50	25.5	14.57738	1	50	0.00	0.337	-1.200	0.662
Green Policies rank[e]	50	2200.64	670.2726	764	3230	-0.31	0.337	-1.101	0.662
Citizen's Right to Know[f]	50	0.4	0.494872	0	1	0.42	0.337	-1.900	0.662
Aid for Right to Know[f]	50	0.42	0.498569	0	1	0.33	0.337	-1.969	0.662
Toxic Cuts Law[f]	50	0.32	0.471212	0	1	0.80	0.337	-1.425	0.662
Plan and Report Cuts[f]	50	0.26	0.443087	0	1	1.13	0.337	-0.759	0.662
Focus: Reduce Toxics[f]	50	0.06	0.239898	0	1	3.82	0.337	13.124	0.662
Partisan identification[h]	48	7.110417	11.38368	-17.4	35.3	0.16	0.343	-0.125	0.674
Ideological identification[h]	48	-14.3	7.514525	-28	-0.2	-0.05	0.343	-0.748	0.674
Ideological polarization[i]	48	35.46458	9.118359	18.6	54	-0.08	0.343	-0.684	0.674
Policy liberalism[j]	48	-0.006875	0.986043	-1.54	2.12	0.19	0.343	-1.093	0.674
Dem. elite ideology[h]	46	3.176957	2.045752	-0.71	7.47	-0.19	0.350	-0.806	0.688
Rep. elite ideology[h]	46	-3.176087	1.754952	-6.05	-0.4	0.13	0.350	-1.251	0.688
Total TRIs reporting in 1991 and 1997[g]	50	284.12	273.4119	5	1117	1.23	0.337	0.956	0.662
Dependent Variables									
PRW Increasers[g]	50	125.92	120.7923	0	492	1.22	0.337	0.881	0.662
PRW Decreasers[g]	50	118.84	116.9886	1	483	1.30	0.337	1.188	0.662
PRW Unchanged[g]	50	2.18	2.869189	0	13	2.00	0.337	4.351	0.662
PRW Unreported[g]	50	37.18	35.62084	1	131	1.11	0.337	0.476	0.662
Percent of State TRIs reducing PRW[g]	50	0.416179	0.083539	0.2	0.83	2.19	0.337	12.598	0.662
PRW Ratio[g]	49	0.928571	0.207385	0.33	1.63	0.54	0.340	3.100	0.668
REL Increasers[g]	50	99.98	95.26536	1	388	1.17	0.337	0.769	0.662
REL Decreasers[g]	50	133.68	131.0151	2	551	1.30	0.337	1.246	0.662
REL Unchanged[g]	50	14.34	17.78627	0	96	2.52	0.337	8.467	0.662
REL Unreported[g]	50	36.04	34.63274	0	131	1.12	0.337	0.555	0.662
REL Ratio[g]	50	1.408107	0.589504	0.22	4	2.07	0.337	7.268	0.662

a. 1990 census, in thousands.
b. Membership in three environmental groups (Sierra Club, Greenpeace, and the National Wildlife Federation) per 1,000 in population.
c. As of 1989. Value added by air polluters refers to the percentage of a state's gross product added by manufacturing industries most responsible for air pollution. The Manufacturing GSP refers to the manufacturing share of the Gross State Product, expressed in millions of dollars, as of 1989. Following Ringquist (1993), the Air Polluters GSP refers to the sum of contributions to the Gross State Product contributed by seven key manufacturing sectors: paper and allied products; chemicals and allied products; petroleum and coal products; rubber and miscellaneous plastics products; stone, clay and glass products; primary metal industries; and other transportation equipment (not including vehicles). The data are taken from a Commerce Department Web site: www.bea.doc.gov/bea/regional/gsp.
d. As of 1988. The measure of community improvement groups is the number of community improvement or capacity building groups per 1,000 population. The foundations variable refers to the number of philanthropic groups per 1,000 population.
e. Indices are taken from the 1991-1992 Green Index (Hall and Kerr 1991).
f. Dichotomous (1=yes, 2=no) measure taken from 1991-1992 Green Index (Hall and Kerr 1991). The toxic cuts law variable refers to state policies intended to reduce toxic chemical emissions. The citizen right-to-know variable refers to whether a state promotes public access to information about toxic chemical usage and emissions. Aid for right-to-know refers to whether a state provides funding for right-to-know programs. The plan and report cuts variable refers to state policies that require facilities to plan and report on toxic chemical use and emissions. The focus: reduce toxics variable refers to state policies that go beyond the planning and reporting requirements to require source reduction.
g. State aggregations from EPA's TRI Explorer. The PRW measures used in the paper are not normalized for production because we did not have access to production data. However, we did control in multiple regressions models for total state releases in 1991, which is a surrogate of a facility's production level.
h. These measures all use a mean score for the period 1976 to 1988, and are taken from Erickson, Wright, McIver (1993). Partisan identification and ideological identification are aggregate measures based on *New York Times/CBS* surveys of registered voters in each state over this time period. Democratic elite ideology and Republican elite ideology are measures built with factor analysis from surveys of county party chairpersons, state legislators, convention delegates, and congressional candidates by different researchers, most of which are from the same time period. They also are drawn from Erickson, Wright, and McIver (1993, pp. 98-99)).
i. Ideological polarization refers to the differences in mean ideology scores between registered voters within the state who identified themselves as Democrats or Republicans (but not independents) in surveys conducted during the 1976 to 1988 period (from Erickson, Wright, McIver 1993).
j. Standardized composite index of state policy measures on eight issues, including education funding, Medicaid coverage, welfare eligibility, consumer protection enactments, criminal justice liberalism (victim compensation, absence of death penalty, etc.), legalized gambling, women's rights, and tax progressivity, taken from Erickson, Wright, McIver (1993).

Table 5. Selected Pearson Correlations

	State Facility PRW Reducer Ratio	State Facility Release Reducer Ratio	State Facility PRW Reducers (%)	State Facility Release Reducers (%)	Total State Releases (91)	Total State Releases (97)	State Release Difference (91-97)	Release Change (% of 91 base)
State population	0.113	-0.0032	0.0041	0.0442	**0.5526*** **	**0.3943** **	**0.3476** **	0.0022
ST Poverty Percent	-0.167	-0.2184	-0.0700	-0.0473	**0.3192***	**0.2814***	0.1154	0.0138
ST Unemp Change	**-0.337***	**-0.4763*** **	**-0.4899*** **	**-0.3377** **	0.2011	**0.3045***	-0.1298	-0.2004
Median House Inc	0.287	0.4142	0.2031	0.2676	-0.1865	-0.1445	-0.0991	-0.0921
Per capita income	0.0945	0.3543	0.1402	0.2629	-0.1764	-0.1430	-0.0838	-0.1112
Manufacturing GSP	0.1849	0.0184	0.0542	0.0960	**0.6211*** **	**0.4774*** **	**0.3362***	-0.0298
Air Pol Val Add	0.0440	0.0412	0.0688	0.2270	0.2396	0.1910	0.1189	0.0850
ST Air Pol Spend	-0.0356	0.1953	0.0592	0.1305	0.0203	-0.1461	0.2685	0.1732
ST Pollute Prevent	0.1343	0.2127	0.0242	0.1613	0.1369	0.2167	-0.1033	**-0.3153***
ST Policy Initiatives	**-0.2802***	**-0.3661** **	-0.2261	**-0.2917***	0.0462	0.0491	0.0035	0.0808
ST Green Plcy Rank	**-0.2786***	**-0.3590** **	-0.2042	**-0.2734***	0.0714	0.0768	0.0038	0.0877
Citizen's RTK	**0.4346*** **	0.2046	0.2613	0.2634	-0.0586	-0.0188	-0.0736	-0.1451
Aid for RTK	-0.0402	0.1263	-0.0106	0.2465	0.1912	0.0816	0.2076	0.1510
Toxic Cuts Law	0.2041	0.1317	0.0803	0.1390	0.2223	0.2610	-0.0230	-0.1768
Plan/Report Cuts	0.2361	0.1839	0.0963	0.1604	0.0478	0.1035	-0.0805	-0.1990
Reduce Toxics	0.0267	0.0367	0.0150	0.0654	-0.1360	0.1862	**-0.5364*** **	**-0.5900*** **
Cons. Members	0.1286	**0.4560*** **	0.1187	**0.2893***	**-0.4102** **	**-0.357** **	-0.1544	-0.0378
Air Polluters GSP	0.1661	0.0130	0.0578	0.1281	**0.7330*** **	**0.5670*** **	**0.3911** **	0.0250
Comm. Groups	0.1357	0.0401	0.0278	0.0934	**0.5327*** **	**0.4393*** **	0.2410	-0.0788
Foundations	0.1314	0.0983	0.0343	0.1040	**0.362** **	**0.3029***	0.1583	-0.1095
Partisan ID	-0.0002	0.1005	0.1369	**0.3043***	0.2140	0.2754	-0.0594	-0.1764
Ideological ID	0.0975	**0.4253*** **	0.1946	**0.3596** **	-0.2152	-0.1228	-0.1811	-0.2006
Ideol Polarization	-0.0352	0.0522	-0.0226	-0.0284	-0.0928	-0.1178	0.0232	0.0175
Policy liberalism	0.1167	**0.4125** **	0.1349	0.2292	-0.1833	-0.1275	-0.1185	-0.1544
Dem. elite ideology	-0.0601	0.1598	-0.1105	-0.0466	-0.1828	-0.1804	-0.0345	-0.0631
Rep. elite ideology	0.0300	**0.4247*** **	0.1223	0.2264	-0.2795	-0.1685	-0.2179	-0.1490

Correlation is significant at the 0.001 level (2-tailed) ***; at the 0.01 level (2-tailed)**; or at the 0.05 level*.

The “reducer” ratios above are simple ratios of reducing facilities to increasing facilities, where any value over 1.0 means a state has more reducers than increasers, and vice versa for all values below 1.0. The “percent reducers” is a measure of the number of reducing facilities divided by total number of facilities.

Table 6. Ordinary Least Squares Regression on Toxics Release Inventory (TRI) Trends

Independent Variables	State Percentage of Release Reducers Estimate[a]	(Standard Error)	State Ratio of Release Reducers Estimate	(Standard Error)
Socioeconomic Factors				
Population	-0.019	(0.000)	-0.021	(0.000)
State Policy Factors				
Pollution Prevention	0.068	(0.007)	0.170	(0.040)
Citizen Right To Know	0.374**	(0.020)	0.164	(0.110)
State Political Factors				
Conservation Group Membership	0.717**	(0.004)	0.884***	(0.240)
Ideological Polarization	-0.576**	(0.001)	-0.628***	(0.007)
Index of Policy Liberalism	-0.213	(0.014)	-0.086	(0.078)
Adjusted R^2	0.3118		0.456	
Standard Error	0.0532		0.2935	
F	4.0207		6.588	
Significance of *F*	0.004		0.000	
(*N*)	40		40	

[a] All of the regression coefficients reported here are standardized coefficients.

Table 7. Ordinary Least Squares Regression on Production-Related Waste (PRW) Trends

Independent Variables	State Percentage of PRW Reducers Estimate[a]	(Standard Error)	State Ratio of PRW Reducers Estimate	(Standard Error)
Socioeconomic Factors				
Population	0.075	(0.000)	0.138	(0.000)
State Policy Factors				
Pollution Prevention	-0.023	(0.005)	0.013	(0.020)
Citizen Right To Know	0.547**	(0.015)	0.349	(0.056)
State Political Factors				
Conservation Group Membership	0.184	(0.003)	0.201	(0.012)
Ideological Polarization	-0.239	(0.001)	-0.267	(0.004)
Index of Policy Liberalism	-0.071	(0.011)	0.024	(0.040)
Adjusted R^2	0.165		0.046	
Standard Error	0.0039		0.1494	
F	2.314		1.320	
Significance of *F*	0.056		0.275	
(*N*)	40		40	

[a] All of the regression coefficients reported here are standardized coefficients.

Table 8. Bivariate Regression on Production-Related Waste (PRW) Trends

Independent Variable	State Percentage of PRW Reducers Estimate[a] (Standard Error)		State Ratio of PRW Reducers Estimate (Standard Error)	
State Policy Factors				
Citizen Right to Know	0.501**	(0.012)	0.373*	(0.047)
Adjusted R^2		0.232		0.117
Standard Error		0.0039		0.1455
F		12.768		6.155
Significance of F		0.001		0.018
(N)		40		40

[a] All of the regression coefficients reported here are standardized coefficients.

Table 9. Ordinary Least Squares Regression on Toxic Reducers and Increasers

Independent Variables	Ratio of Toxic Reducers to Increasers Estimate[a] (Standard Error)		Percentage of Toxic Reducers Estimate (Standard Error)		Percentage of Toxic Increasers Estimate (Standard Error)	
Socioeconomic Factors						
Manufacturing GSP	0.151	(0.240)	0.111	(0.000)	-0.232	(0.000)
State Policy Factors						
Value added by air polluters	0.339**	(0.337)	0.024	(0.050)	-0.388**	(0.059)
Citizen Right to Know	0.155	(0.015)	0.161	(0.014)	-0.161	(0.016)
State Political Factors						
Conservation Group Membership	0.852***	(0.017)	0.599**	(0.003)	-0.798***	(0.003)
Ideological Polarization	-0.473**	(0.001)	-0.344	(0.000)	0.475**	(0.001)
Adjusted R^2		0.488		0.175		0.494
Standard Error		0.253		0.038		0.045
F		8.42		2.66		8.62
Significance of F		0.000		0.039		0.000
(N)		40		40		40

[a] All of the regression coefficients reported here are standardized coefficients.

Notes

[1] In 2003, the U.S. EPA's Office of Pollution Prevention and Toxics released for public comment a draft version of a *Community Air Screening How to Manual* that carried the subtitle "A Step-by-Step Guide to Using a Risk-Based Approach to Identify Priorities for Improving Outdoor Air Quality." The manual illustrates well how government agencies might take action to improve the

public's capacity to understand and use data, in this case Toxics Release Inventory data. The manual strongly endorses the establishment of partnerships between communities and local industry, with broad stakeholder involvement, as the best way to establish local priorities and promote their achievement.

The agency's educational effort focuses on the Risk-Screening Environmental Indicators (RSEI) model that we used in a paper presented at the annual meeting of the American Political Science Association in August 2003 that covered only EPA Region V. We will use the model again but for the entire nation in a paper to be presented at the 2004 annual meeting of the American Political Science Association. The history of the model's development and its use to date is described in Schmidt (2003) and in Bouwes, Hassur, and Shapiro (2001). Further information is available at EPA's RSEI Web site: www.epa.gov/opptintr/rsei.

[2] TRI facilities include all industrial firms that are required by the EPA to self-report the release of any toxic chemical into the environment. The federal guidelines stipulate that a facility must file a report for the TRI program if it conducts manufacturing operations within Standard Industrial Classification codes 20 through 39 (with a broader set of categories applicable after 1998, such as metal mining, coal mining, and electric utilities that burn coal); has ten or more full-time employees; and manufactures or processes more than 25,000 pounds or otherwise uses more than 10,000 pounds of any listed chemical during the year. For 2000, the TRI was expanded to include new persistent bioaccumulative toxic (PBT) chemicals, with lower reporting thresholds. The full TRI list now includes over 650 chemicals.

[3] The quote comes from the "overview" section of "The Toxics Release Inventory (TRI) and Factors to Consider When Using TRI Data": www.epa.gov/tri/tridata/tri00/press/overview.pdf.

[4] One striking figure drives home the importance of large manufacturing facilities. In 1999, just 50 facilities out of the 21,000 reporting that year accounted for 31 percent of all the TRI releases nationwide (cited in Graham and Miller 2001). It also is apparent that larger facilities have been more successful on the whole in reducing toxic releases than have smaller facilities.

[5] One interesting finding is that subsidiaries of major corporations apparently have "significantly higher emissions rates"; that is, they release a greater percentage of on-site TRI chemicals to the environment. The study reporting this finding used data for 1990, and many changes in the TRI program have been made since then. However, if the findings hold up over time, they suggest that firms with less visibility have fewer incentives to reduce toxic chemical pollution. See Grant and Jones (2003).

Given the design of the TRI program, a long-time concern is that facility managers have an incentive to reduce the emission of listed chemicals and thereby to avoid or limit undesired media attention and public pressures to alter business practices. But they might choose to do so in a way that merely shifts use of unlisted chemicals for those on the list, or they might seek a cheap and quick way to reduce the volume of emissions that does not reduce the risks to public health as much as might be achieved through a more thorough alteration in production processes.

[6] In a paper delivered at the 2003 annual meeting of the American Political Science Association, we identified five mediating factors as likely to be most important in shaping the way in which information disclosure affects community and corporate risk perceptions and environmental beliefs and values: community social capital, local and state political system characteristics (including regulatory stringency), local social and economic conditions, firm or facility characteristics (such as size, age of facility, and profitability), and the nature of media coverage of the information released as well as coverage of corporate actions.

[7] The research reported in this paper is supported by the National Science Foundation under Grant No. 0306492, Information Disclosure and Environmental Decision Making. Michael Kraft and Troy Abel are co-principal investigators, and part of the research is being conducted by Mark Stephan. Any opinions, findings, and conclusions or recommendations expressed in the paper are those of the authors and do not necessarily reflect the views of the National Science Foundation.

[8] As reported below, "total releases" can be understood as a subset of "production-related waste." Total releases are simply the releases to land, water, and air, while production related wastes also include produces that are recycled, recovered for energy, and treated either on or off-site.

[9] In a survey conducted in the early 1990s, Santos, Covello, and McCallum (1996) found that regulatory compliance was one of the two reasons that facilities cited most frequently for reduction of their TRI releases and transfers. The other was employee health. That is, where many observers assume that TRI reductions are made voluntarily because the program is non-regulatory in nature, this kind of evidence suggests a more realistic explanation would acknowledge the incentives created by the larger regulatory environment, including company concern over civil liability and state regulatory action. Without the requirements imposed by such federal and state environmental regulation, information disclosure programs might be considerably less effective.

[10] EPA doubled the reportable chemical list in 1996 that potentially distorts longitudinal analyses.

[11] A total of 151 TRI facilities were excluded.

[12] We use the term "TRIs" here as a shorthand for "TRI reporting facilities."

[13] Results are available from the authors upon request.

[14] Full results are available upon request.

[15] There was some slight evidence that policy liberalism within a given state might influence the likelihood of citizen right-to-know laws, but the results were not stable across multiple specifications. Results are available upon request.

The Effect of Reporting Thresholds on the Validity of TRI Data as Measures of Environmental Performance: Evidence from Massachusetts

by:

Lori D. Snyder
John F. Kennedy School of Government
Harvard University
lori_snyder@ksg.harvard.edu

1 Introduction

The old adage, "You can't manage what you don't measure," is the primary rationale for conducting systematic evaluations of the effectiveness of various environmental policy initiatives. Only if governments, non-profits, industries, and communities have good measures of environmental outcomes (changes in pollution levels, risk levels, etc.) can they evaluate what policies work, how well they work, and how to improve their effectiveness. If the metrics are not valid, then neither are the policy inferences drawn from these metrics. This paper examines one aspect of the validity of a frequently used measure of environmental performance—pollution releases reported under the federal Toxics Release Inventory (TRI) program.

The TRI data are the most comprehensive data available on facility-level releases of toxic chemicals. Facilities are required to disclose publicly their releases to all media—air, water, land, and underground injection (on-site releases) —as well as their transfers of chemicals off-site for recycling or disposal (off-site releases), for over 600 toxic chemicals. Because the TRI data capture releases to all media and include measures of the environmental impact of a facility's activities beyond the facility's fence (off-site releases), many have argued that the TRI data provide a more complete picture of facility-level environmental performance than other available metrics (Karkkainen, 2001).

In fact, the TRI data are used frequently for the purposes of comparing environmental performance across geographic areas and over time by government agencies, non-profits, and academic researchers. Environmental Defense uses the TRI data as one of the indicators of environmental performance in its Scorecard, an online database that allows the public to compare counties on a number of environmental metrics.[1] The Public Interest Research Group (PIRG) and its state affiliates frequently compile lists of the worst polluters in a state or region based on releases reported to the Toxics Release Inventory.[2] Similarly, EPA ranks states and industries by their total releases as reported in the TRI (U.S. EPA, 2002a). State environmental agencies also publish annual progress reports that measure changes in environmental performance based on changes in releases to the TRI, and label particular industries and facilities as the top polluters based on these releases.[3] Academic researchers have used the TRI data as outcome variables in evaluations designed to determine what factors affect environmental performance (Arora and Cason 1998; Grant and Jones 2003; Helland and Whitford 2003; Khanna and Anton 2002; King and Lenox 2000). Researchers have also used TRI data to evaluate whether requiring facilities to publicly disclose pollution leads facilities to decrease pollution. Fung and O'Rourke (2000) and Wolf (1996)

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[1] The Environmental Defense scorecard can be accessed at www.scorecard.org.

[2] See, for example, U.S. Public Interest Research Group 1998.

[3] A full list of state TRI programs with links to the state annual reports can be found at www.epa.gov/tri/programs/state_programs.htm.

argue that the observed 45 percent decline in overall TRI releases from 1988 to 1995 indicates that information disclosure is a valuable regulatory tool for reducing pollution.[4]

Despite the frequent use of TRI data for policy analyses, there are several known concerns about the validity of these data as measures of environmental performance. This paper defines the characteristics of a valid measure of environmental performance and outlines several known threats to the validity of the TRI data. The paper then focuses on estimating the magnitude of the measurement error created by the existence of arbitrary reporting thresholds. The potential for measurement error exists because facilities are only required to report *releases* to TRI if their *use* of a chemical exceeds some threshold. This creates an incentive for facilities to reduce their use of a listed chemical to a level just below the reporting threshold. However, this does not necessarily represent a real improvement in environmental performance, as the facility's release level may remain largely unchanged (or potentially could even increase). As a result, observed decreases in reported releases might overstate the true change in environmental performance. This paper asks the question: How much of any observed decline in reported releases could be artificially created by the existence of the reporting thresholds?

The TRI data are also used to rank facilities based on their pollution levels. Truncation at the reporting threshold may also have an effect of the validity of these cross-sectional rankings. This paper also asks the question: How much would our rankings of facilities change if we could account for releases by facilities that are below the reporting threshold?

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[4] See U.S. EPA 2003 for a list of detailed discussion of how TRI data have been used by government, business, academics, and citizen groups.

While the potential for bias introduced by reporting thresholds is well known, there has previously been little that users of the data could do to ascertain the magnitude of this bias. The TRI data provide scant information that would allow a user of the data to ascertain whether a facility that ceases reporting did so because it went below the reporting threshold. However, the State of Massachusetts has expanded the disclosure requirements under TRI in ways that better allow for assessment of the reasons facilities cease reporting. This paper utilizes data from the Massachusetts Toxics Use Reduction Act (TURA) to estimate, for facilities in Massachusetts, bounds on the degree of bias introduced by the reporting thresholds in both time series and cross-sectional analyses.

The TURA data are similar to the TRI data. Indeed for Massachusetts facilities, data reported to TRI is a subset of the data reported to TURA. However, the TURA data include two additional features missing from the national data that make possible estimation of bounds on the truncation bias. First, the TURA reporting forms contain an optional question on why facilities are no longer reporting a chemical they had previously reported. Second, the TURA program requires facilities to report their chemical *use* in addition to their chemical *releases.* These two features of the data for Massachusetts allow estimation of the number of facilities that cease to report because they reduced use below the reporting threshold, but still use the chemical in positive quantities. These data are also used to estimate bounds on the amount of "missing" releases that result. Because the TURA data are in other ways identical to the TRI data, analysis of the TURA data provides some preliminary evidence on whether truncation bias is likely to be a large or small problem for the national TRI data.

The paper begins in Section 2.2, by describing the TRI and TURA data. Section 2.3 articulates a specific definition of "validity" of an environmental performance metric and highlights several threats to the validity of the TRI/TURA data as measures of environmental performance under this definition. This section defines truncation bias at the reporting thresholds and details how this bias may invalidate both time-series and cross-sectional comparisons. Section 2.4 estimates the magnitude of the truncation bias created by the existence of the reporting thresholds using the TURA data. The results suggest that truncation bias is indeed a serious threat to the validity of these data as measures of environmental performance, particularly in cross-sectional comparisons. Time-series estimates are off by roughly 40 percent in Massachusetts in the worst-case scenario. That is, 40 percent of the observed decrease in releases in Massachusetts may be artificial declines created by the reporting thresholds. For cross-sectional comparisons the results for Massachusetts indicate that quartile rankings of facilities may be wrong as often as 45 percent of the time when truncation bias is not accounted for. Section 2.4 ends with a discussion of the implications of the Massachusetts findings for the use of nationwide TRI data. Given the potential importance of these effects for policy analysis, Section 2.5 presents suggestions for adjusting policy analysis to account for truncation bias.

2 The U.S. Toxics Release Inventory and the Massachusetts Toxics Use Reduction Act

In December 1984, a Union Carbide pesticide plant in Bhopal, India accidentally released 40 metric tons of methyl isocyanate, killing an estimated 2,000 people and injuring 100,000 others. Shortly thereafter, a pesticide release in West Virginia hospitalized 150 people. Partly in response to concerns raised by these high-profile accidental releases, Congress passed the Emergency Planning and Community Right to Know Act (EPCRA) in

1986. EPCRA requires manufacturing firms to report releases of specific toxic chemicals on an annual basis, and to make these reports available to the public. The U.S. Environmental Protection Agency's (EPA) implementation of EPCRA resulted in the creation of the Toxic Release Inventory (TRI), which requires large manufacturing facilities publicly disclose total releases of listed toxic chemicals to all media on an annual basis.

There are three factors that determine whether a facility is subject to the disclosure requirements. The first is industrial sector. Originally, only manufacturing facilities were subject to the TRI reporting requirements. Subsequently, several other industrial sectors were added including: federal facilities, metal and coal mining, electrical generating facilities that combust coal or oil, chemical wholesale distributors, petroleum terminals and bulk storage facilities, and hazardous waste treatment storage and disposal facilities. The final two determinants of regulatory eligibility concern facility size. Only facilities with 10 or more full-time equivalent employees are required to report pollution levels. In addition, facilities are only required to disclose releases of listed chemicals for which they either: (1) manufacture or process more than 25,000 pounds or (2) otherwise use more than 10,000 pounds. Figure 2-1 provides a flow chart of TRI reporting requirements.

Several states have subsequently required additional public disclosure for plants in their jurisdictions. In 1989, Massachusetts passed the Toxics Use Reduction Act which expanded the reporting requirements for facilities in that state (Massachusetts Department of Environmental Protection 2003). There are three main differences between the TURA and the TRI requirements. First, the list of chemicals for which facilities are required to report is larger in Massachusetts. Massachusetts facilities must report releases of all chemicals required by TRI and also any chemical listed under the Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA), commonly known as Superfund.

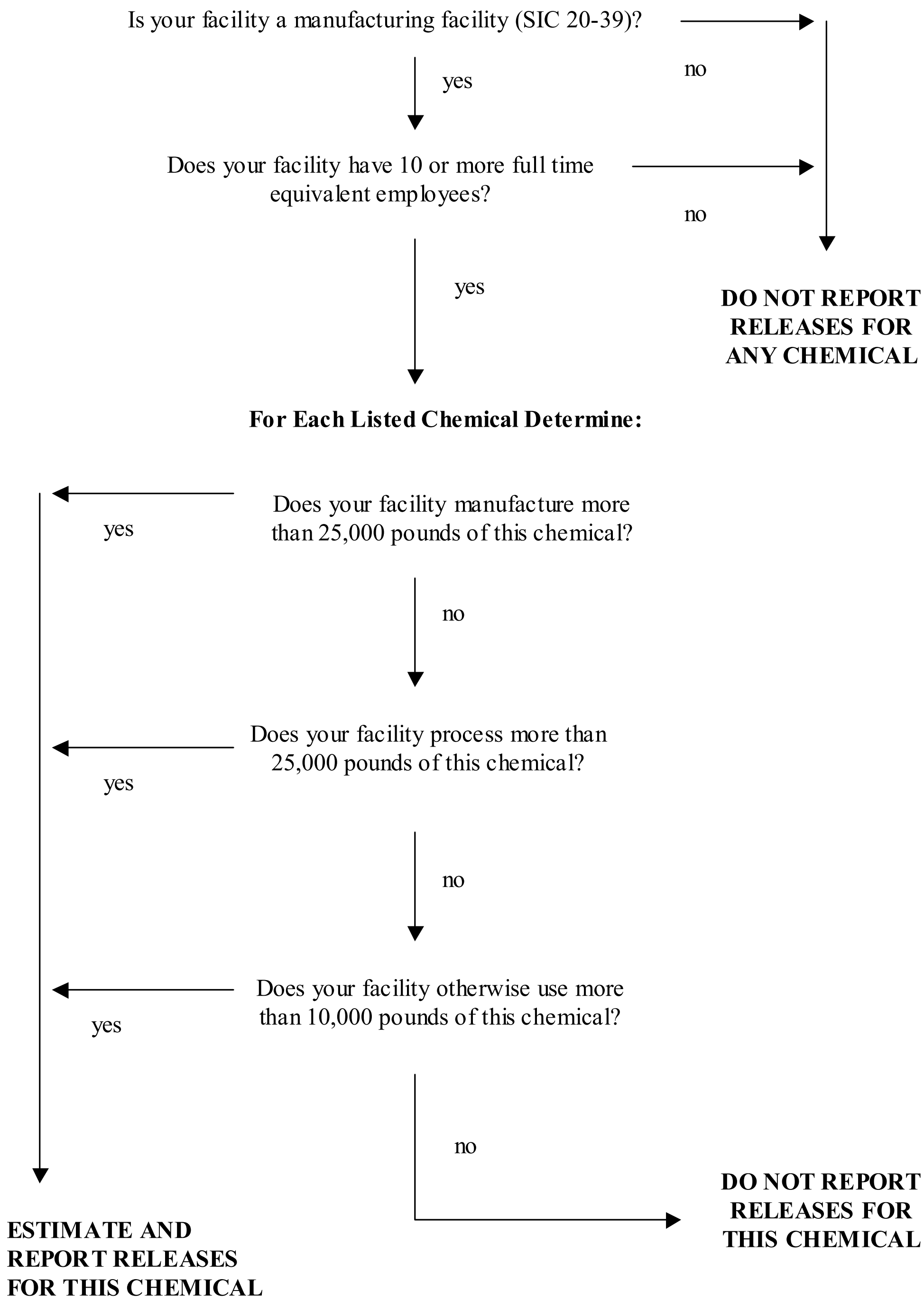

Figure 2-1: Flow Chart of TRI Reporting Requirements

The second difference is that the reporting thresholds are weakly lower in Massachusetts. If a facility triggers the federal reporting threshold for at least one listed chemical (i.e., manufacturers or processes more than 25,000 pounds or otherwise uses more than 10,000 pounds), the facility must report for all listed chemicals for which total manufacture, process, and use is greater than 10,000 pounds. Therefore, for most facilities in Massachusetts there are not three separate reporting thresholds, but one binding threshold at 10,000 pounds of total use for each listed chemical.

The final difference is that facilities in Massachusetts are required to report chemical *use* in addition to chemical *release*. The reporting thresholds are based on chemical use, but the federal program does not require public disclosure of use levels. In Massachusetts facilities must report both total use of the chemical and total releases of the chemical. Figure 2-2 provides a flow chart for the TURA reporting requirements.

This paper is concerned about the effects of the reporting thresholds present in both the TRI and the TURA designs, on the valid uses of these data for policy analysis. Before specifically examining the TRI and TURA data and determining whether or not these data are valid measures of environmental performance, it is worthwhile to articulate a definition of valid measurement of environmental performance. That is the subject of the next section.

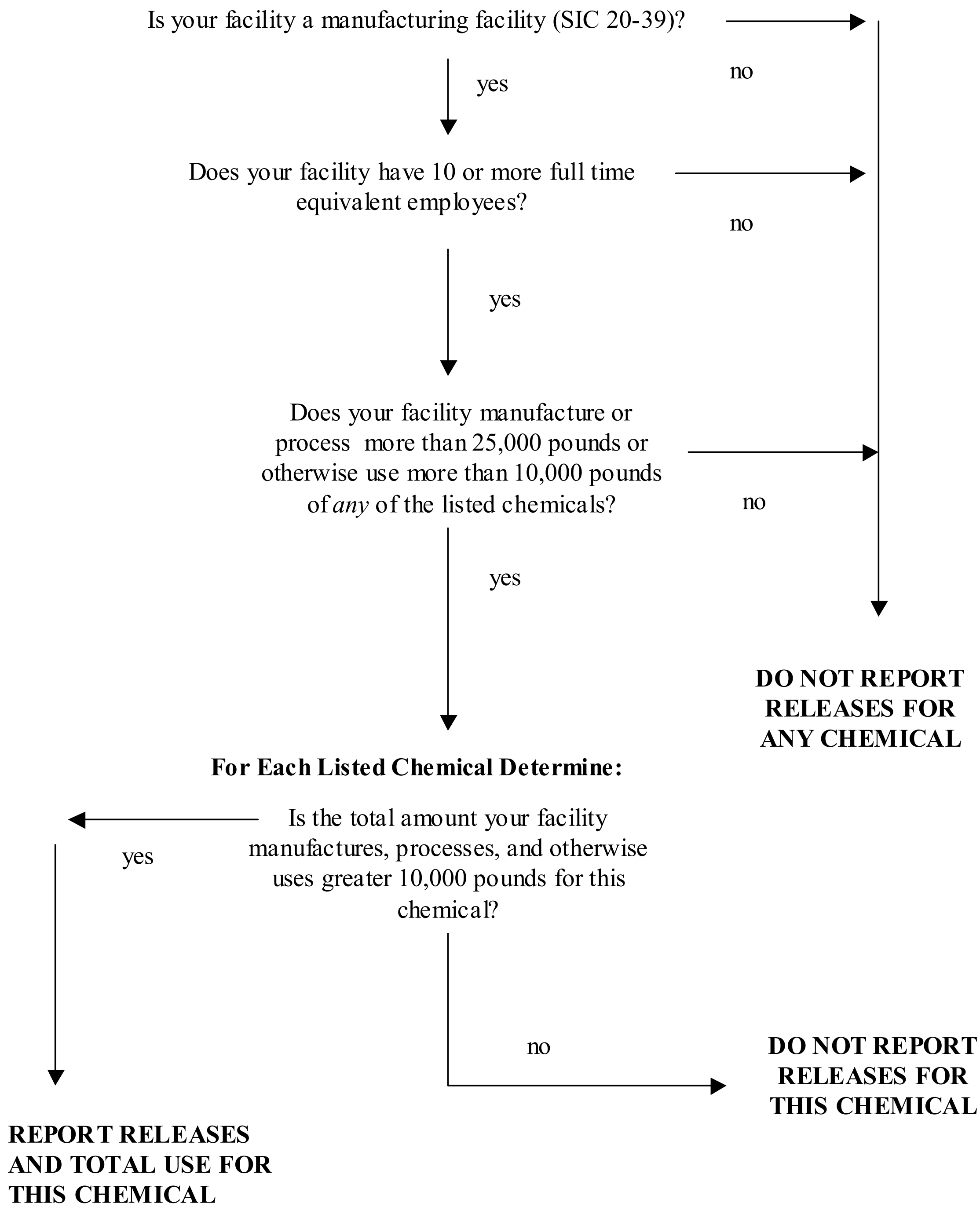

Figure 2-2: Flow Chart of TURA Reporting Requirements

3 The TRI/TURA Data as Measures of Environmental Performance

3.1 Defining a valid measure of environmental performance

There are a number of potential characteristics of a valid measure of environmental performance (National Academy of Engineering, 1999; White and Zinkl, 1997; U.S. EPA, 1992). The most frequently discussed characteristics of a valid performance metric are:

1. Significance: The measure has a clear relationship to environmental performance.
2. Precision: The stochastic component of the measure is small relative to the deterministic component.
3. Verifiability: The measure is publicly available and can be validated by a third party.
4. Comparability: The measure allows for comparisons across facilities, industries, countries, and across time.

Focusing on the last of these, comparability, there are potentially two standards of comparability that could be required--cardinal comparability or ordinal comparability. Cardinal comparability implies that the measure can be compared in magnitude across different entities. Define releases by facility *i* to be X_i. If X is cardinally comparable, $X_1 > X_2$ implies that facility 1 is doing better than facility 2 and is, in fact, doing precisely $X_1 - X_2$ better.[5]

[5] Cardinal comparability is often present in outcome measures used to evaluate program effectiveness by many other federal and state agencies such as the Department of Health and Human Services and the Labor Department. If participants in a pilot job training program earn an average of

A weaker comparability standard would be ordinal comparability. Ordinal comparability implies that if $X_1 > X_2$ then facility 1 is doing better than facility 2. However, it is not the case that the magnitude of the difference in performance is captured by the magnitude of the difference in the performance metric, X. Ordinal comparability is the minimum comparability standard for a valid performance metric. The whole point of program or policy evaluation is to ascertain whether a program or policy made society better off. If the metric used cannot be relied upon to order outcomes from better to worse, then the metric cannot be relied upon to measure whether a program resulted in a better outcome. Cardinal comparability is not necessary if the goal of the evaluation is to determine whether a specific initiative improved outcomes. However, cardinal comparability is necessary if the goal of the evaluation is to determine how much of an improvement was obtained by the firm, industry, state, or policy initiative. For example, cardinal comparability is necessary for cost-effectiveness analyses that compare policies on the basis of the amount of risk reduction per dollar spent.

The remainder of this paper evaluates the Toxics Release Inventory data as measures of plant-level environmental performance in relationship to these four characteristics of validity. In particular the paper highlights the potential threat to validity presented by the existence of minimum reporting thresholds and attempts to quantify the degree of the bias created by these thresholds.

3.2 Validity of TRI Data as a Measure of Environmental Performance

There are several known threats to the validity of the TRI data. The first is that data are only collected for certain industries and chemicals. Originally data were collected for

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$25,000 per year and non-participants earns an average of $20,000 per year, then trainees are better off than non-trainees by exactly $5,000 per year, on average.

manufacturing facilities and for approximately 300 chemicals. The fact that the TRI data are not comprehensive may reduce the *significance* of these data as a measure of environmental performance. For the measure to be significant in needs to have a clear relationship to environmental performance. If the TRI data only capture releases of a small subset of total chemicals, the significance of these data may be compromised.

This concern was recognized early in the program (U.S. Government Accounting Office 1991) and the program has since been expanded to more than double the number of chemicals and to include federal facilities, metal and coal mining facilities, electric generating facilities that combust coal or oil, chemical wholesale distributors, petroleum terminals and storage facilities, and hazardous waste treatment storage and disposal facilities. Obviously the TRI data cannot be used as a valid measure of environmental performance for plants that are not obligated to report these data. Thus, these data will necessarily by limited to use only in evaluations that affect these industrial sectors.

A second threat to the validity of the TRI data as a measure of environmental performance stems from the fact that TRI data are reported in total pounds of a chemical released, but it is widely acknowledged that pounds of different chemicals present widely different levels of risk. Similarly, a pound of a chemical released in rural Oklahoma may have a different impact than a pound of the same chemical released in downtown Manhattan. Some of the factors that determine the overall environmental risk presented by a facility's activities include the total pounds of releases, the toxicity of these releases, the exposure level in the population, the duration of exposure, and the sensitivity of the population. Simply adding up total pounds of different chemicals released and comparing across facilities may provide an incorrect ordering of facilities according to relative risk. For example, a facility that releases a few pounds of a very toxic, persistent, and bio-accumulating toxin may appear to have much better "environmental performance" than a facility that releases more pounds of a less hazardous and more degradable substance.

Even if the point estimate of environmental performance provided by TRI is biased by lack of information on toxicity, one might hope that *changes* in TRI releases still provide a measure of *changes* in environmental performance. That is, perhaps time series comparability is maintained even if cross-sectional comparability is compromised. For example, even if pounds of different chemicals are not equal in their risk profile, one might argue that fewer pounds is better than more pounds, holding all else constant. But the key is that all else must be held constant. The assumption that the trend in TRI releases is a valid indicator of the trend in environmental performance is likely to hold if, among other things, the facility consistently uses and reports on the same chemicals in every year. One change in reporting that may threaten the validity of changes in TRI releases as measures of changes in environmental performance is if a facility substitutes from a less toxic to a more toxic chemical (or vice versa). Even if the facility reduces releases by a substantial amount, the total risk presented by the facility may remain the same, decrease by less, or even increase. Thus, not adjusting for toxicity compromises the *significance* and the *comparability* of the TRI data both across facilities in a given year and within a facility over time.

EPA and academic researchers have been working to develop indicators that take the TRI data as an input, combine it with toxicity, exposure, and degradability information, and then calculate a risk-based metric. EPA has recently released its Risk Screening Environmental Indicators (RSEI) model, which combines TRI data and risk information to develop a risk-adjusted pollution measure for peer review (U.S. EPA, 2004). These changes will doubtless improve the quality of estimates of environmental performance using TRI data.

A third threat to the validity of the TRI data stems from the fact that these data are self-reported by facilities. This affects both the *precision* and the *verifiability* of the data as measures of environmental performance. Changes in how a facility estimates releases or even how it classifies releases may lead to changes in reported releases that are not reflective of real changes in environmental performance (Graham and Miller 2001). In the initial years following the establishment of TRI, the accuracy of the self-reported releases was thought to be poor (U.S. Government Accounting Office 1991); however, most observers believe it has improved over time. EPA has issued updated guidance on estimating releases both for different industries and chemicals,[6] and it audits a small number of facilities' TRI reports each year. Despite these efforts, concerns over the precision and verifiability of the self-reported data remain.

The final threat to the validity of the TRI data is due to truncation in reported releases caused by the existence of reporting thresholds. It is this threat that this paper addresses. Reporting thresholds set a minimum level of chemical use at a facility, below which the facility is not required to report releases. For the TRI data, the reporting thresholds are 25,000 pounds for manufacture and processing of a listed chemical and 10,000 pounds for any other use of the chemical. Similarly, the TURA data require reporting only if total use (manufacturing plus processing plus otherwise use) is greater than 10,000 pounds. These reporting thresholds create incidental truncation in the TRI data. This truncation threatens the *significance* and *comparability* of the TRI data for both ordinal and cardinal analyses. To illustrate the potential bias, define:

y_{ict} = actual releases of chemical c, by facility i, in time t

[6] For example, U.S. EPA (2000) provides detailed guidance for the Textile Processing Industry. The set of all industry and chemical guidance can be obtained online at www.epa.gov/tri/guided_docs.

M_{ict} = amount of chemical c manufactured by facility i in time t

P_{ict} = amount of chemical c processed by facility i in time t

U_{ict} = amount of chemical c otherwise used by facility i in time t

ς = set of all chemicals, *c*, that are reportable to TRI

For each facility there exists a true aggregate measure of releases, Y, such that

$$Y_{it} = \sum_{c \in \varsigma} y_{ict} .$$

However, we do not observe y_{ict} or Y_{it}. Instead, we observe:

$$\widetilde{Y}_{it} = \sum_{c \in \varsigma} y_{ict} r_{ict} ,$$

where

$$\begin{aligned} r_{ict} &= 1 \text{ if } (M_{ict} > 25{,}000) \text{ or } (P_{ict} > 25{,}000) \text{ or } (U_{ict} > 10{,}000) \\ &= 0 \text{ otherwise} \end{aligned} .$$

Further define:

r_{it} = vector of r_{ict} (r_{i1t}, r_{i2t}, …, r_{iCt}) for all c in ς ,

$$\begin{aligned} u_{ict} &= 1 \text{ if } (M_{ict} > 0) \text{ or } (P_{ict} > 0) \text{ or } (U_{ict} > 0) \\ &= 0 \text{ otherwise} \end{aligned} \text{, and}$$

u_{it} = vector of u_{ict} (u_{i1t}, u_{i2t}, …, u_{iCt}) for all c in ς .

Under what circumstances can we use observed data on reported releases, $\widetilde{Y}_{it}$, to make valid cross-section or time series comparisons? Begin with a cross-sectional comparison. If we observe: $\widetilde{Y}_{1t} > \widetilde{Y}_{2t}$, under what circumstances can we be confident that

$Y_{1t} > Y_{2t}$? This inference will be valid if both facilities report for all chemicals for which they use any positive quantity.[7] That is if:

$$r_{1t} = u_{1t} \text{ and } r_{2t} = u_{2t}.$$

The key point is that if there are some chemicals which a facility uses in positive quantities, that is $u_{ict}>0$, but for which the facility is not required to report because u_{ict} is below the reporting threshold, then $\widetilde{Y}_{it}$ does not necessarily equal Y_{it} and $\widetilde{Y}_{it}$ is not necessarily ordinally comparable across facilities.[8]

Turning to time-series evaluation, under what circumstances do the trends in reported releases provide valid information on the trends in actual releases? That is, under what circumstances does $\widetilde{Y}_{it} - \widetilde{Y}_{it-1} = Y_{it} - Y_{it-1}$? As with the cross-sectional comparison, time series comparisons based on reported releases will only provide valid inferences on true releases if the facility reports for every chemical that it uses in any positive quantity.[9] That is, if:

$$r_{1t} = u_{1t} \text{ and } r_{1t-1} = u_{1t-1}$$

Notice that the facility does not need to report for the same set of chemicals in both years. It just must report for all chemicals it uses in each year. If a facility stops using a chemical and, hence, stops releasing this chemical, that is a real change in environmental

7 This is the only condition under which this inference is generally valid. However, this inference may be valid in certain special cases. For example, if every facility does not report for a chemical, c, but releases of that chemical are the same for all facilities. There is no reason to think that this will hold.

8 If ordinal comparability is not preserved, then cardinal comparability is also not preserved.

9 This condition allows for valid time series inference in all cases. There may be special cases in which require less restrictive conditions. For example, if the amount of non-reported releases is constant across time, then the absence of these releases does not bias the time series comparison. However, there is no *a priori* reason for assuming non-reported releases are constant across time.

performance. But if a facility simply uses less of the chemical and is no longer required to report releases, this is not necessarily a real change in environmental performance.

The bias that results from truncation at the reporting threshold is referred to as truncation bias. The term truncation bias here has a slightly different meaning than the classic econometric definition. The truncation bias in this paper most closely resembles incidental truncation bias, which arises when facilities or individuals are observed on the basis of the outcome of another decision (Wooldridge, 2002: 552). For example, imagine one only observes wages for the employed, so observing wages is the result of another decision, in this case the labor market participation decision. The truncation bias in the TRI data is incidental truncation bias at the unit for which the data are collected, which is the facility-chemical-year level. One only observes a facility-chemical-year record if the facility triggers the reporting threshold for that chemical in that year. So observing data is the result of the chemical use decision. This incidental truncation bias is further aggravated by aggregation to other levels of analysis, such as the facility, firm, industry, or state level. The aggregation essentially treats all unobserved data as zero, which further invalidates comparisons. Perhaps a more specific name for this bias is "truncation and aggregation bias," but it will be referred to here as truncation bias for short.

While the first three threats to the validity of TRI data are well recognized and measures have been taken to address the threats, little has been done to assess or reduce the problems associated with truncation bias. This lack of attention is not the result of a lack of understanding of the problem, rather it is largely due to the fact that data have not been available that would allow analysts to estimate the extent of the problem and correct it. In contrast, the bias introduced by not weighting TRI pounds by some measure of toxicity is conceptually easier to address because risk factors can be determined with available data and the TRI data can be corrected using these risk factors. However, since facilities are generally not required to report chemical use levels, one cannot observe whether a facility is reporting for all chemicals it uses in positive quantities. Thus, the truncation bias cannot be corrected for in a systematic way using available data.

While systematic correction of the truncation bias cannot be obtained, this paper focuses on estimating bounds on the degree of bias presented by truncation of the data at the reporting thresholds. A reasonable question might be: How much of the observed decrease in TRI releases is potentially due to truncation bias? Similarly, for a cross-section one can ask how much the relative rankings of facilities could change if truncation bias were to be accounted for. A better understanding of the approximate magnitude of the bias introduced by the reporting thresholds can help ascertain whether this bias is a practical, rather than purely theoretical, threat to the validity of the TRI data as a measure of environmental performance.

4 Estimating Bounds on the Truncation Bias

There are several limitations in the TRI data that inhibit systematic estimation of the degree of potential truncation bias. First, the researcher only observes whether a facility reports releases of a chemical in a given year. If a facility does not report for a chemical in year t for which it reported in year t-1, the researcher cannot determine if the facility eliminated use of the chemical, substituted to a different chemical, or was below the reporting threshold.[10]

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[10] Facilities that report in one year and then cease reporting in future years represent only a fraction of the truncated observations. There may also be facilities that use a chemical in every year below the reporting threshold, have releases of these chemicals, but are never legally required to report. This non-reporting is equally problematic for policy analysis, but little can be done to identify facilities for which this may be true.

In contrast, the TURA data provide two sources of variation that better allow for the identification of bounds on the truncation bias. First, if a facility does not report for a chemical in year *t* for which it had reported in year *t-1*, the TURA reporting forms include an optional question that asks the facility to explain the change. Approximately, one-third of facilities that cease reporting for a chemical answer the optional question. I check to see if the facilities that respond to this optional question are representative of the set of facilities that cease reporting and find evidence that responders are not systematically different from non-responders. Thus, the responses to the optional question are used to gauge the degree to which facilities are not reporting for chemicals they use in positive quantities.

The second source of variation in the TURA data that can be used to help estimate bounds on the truncation bias is that facilities are required to report how much chemical they *use* in addition to how much they *release*. The federal TRI has reporting thresholds based on use, but only requires reporting on releases. The use data combined with the responses to the optional question about why facilities ceased reporting reveal that the distance from the reporting threshold is a good predictor of whether a facility ceases reporting because it went below the reporting threshold, but still used the chemical in positive quantities. I use this relationship to predict for non-responders the reason why they ceased reporting.

For facilities that directly reveal that they ceased reporting because they went below the reporting threshold, and for facilities that are predicted to have ceased reporting because they went below the reporting threshold, I then estimate the effect of these missing releases on trends in releases over time and on cross-sectional comparisons of facilities within a given year. Missing releases are estimated using three different procedures. The first is a lower bound estimate on total releases at the facility. The lower bound estimate assumes that when a facility ceases reporting its true releases are zero. This is the implicit assumption currently made by government agencies, non-profits, and academic researchers when aggregating releases data to the facility level (or higher levels). The second estimate of missing releases can be considered an upper bound estimate. In the upper bound scenario, if a facility ceases reporting a chemical because it went below the reporting threshold, releases are set equal to the most recent level of releases reported for that chemical at that facility. Moreover, the facility is assumed to continue releasing the chemical at the same level in perpetuity. The final estimate of missing releases is one that extrapolates the value of non-reported releases based upon linear trends in reported releases. These three scenarios present an upper and lower bound and an intermediate estimate of the degree of bias introduced by the reporting thresholds.

Section 4.1 examines why facilities claim they ceased reporting in Massachusetts using the optional question from the TURA form. This section also discusses the estimation used to predict reasons for non-reporting for facilities that did not answer the optional question. Section 4.2 discusses the estimation of the magnitude of missing releases in Massachusetts based on three estimation methods. Section 4.3 discusses the implications of the Massachusetts results for national analyses.

4.1 Why do facilities cease reporting?

Facilities in Massachusetts were required to disclose pollution and chemical use data to the TURA program beginning in 1990. From 1990 to 1999 there were a total of 23,200 chemical reports filed by 1,092 facilities. During this same time period there were 3,758 cases where a facility reported for a chemical in one year, but did not report the chemical in the following year.[11] For these facilities, the TURA form provides an optional question where the facility can explain why it is no longer reporting for this chemical. The question is multiple-choice with the following six possible responses: (1) chemical use is below the reporting threshold but greater that zero, (2) chemical was not used this year, (3) substituted a different chemical, (4) chemical use eliminated without substitution, (5) decline in business, and (6) other. If the facility answers other, they are given the option to fill in a reason. This question is answered for a total of 1,271 (or 33.8 percent) of the cases where reporting ceases. Table 2-1 provides the distribution of responses.

Table 2-1: Explanation for Non-Reporting Among Respondents to the Option Question in Massachusetts

Reason for No Longer Reporting	*Number of Respondents*	*Percentage of All Non-missing Responses*
Chemical use is below the reporting threshold but greater than zero	844	66.40
Chemical was not used this year	96	7.55
Substituted a different chemical	87	6.85
Chemical use eliminated without substitution	60	4.72
Decline in business	31	2.44
Other	153	12.04

Approximately two-thirds of all respondents to the optional question answered that they were no longer reporting because their chemical use was below the reporting threshold, but greater than zero. Of the respondents that answered "other" the most

[11] It is possible that the facility reported that chemical again in future years. This occurs 379 times.

frequent explanation was that the chemical in question had been delisted by the state and reporting was no longer legally required.

The high percentage of respondents that state they ceased reporting because their chemical use was positive, but lower than the reporting threshold, raises concern that the degree of truncation bias may not be trivial. However, before estimating bounds on the truncation bias, it is necessary to determine whether the facilities that responded to the optional question are systematically different from those that did not respond. If responders are systematically different from non-responders in ways that may also be correlated with their reason for not reporting, then the sample of responders cannot be used to impute explanations for non-reporting. To see whether the responders to the optional question are a representative sample, I compare the distribution of the data for the two groups for three key variables—two-digit SIC code, year reporting stopped, and total releases to the environment in year before reporting stopped. Table 2-2 presents the distribution of SIC codes, Table 2-3 presents the distribution of years, and Table 2-4 presents the mean and standard deviation for total pounds released.

Table 2-2: Distribution of Standard Industrial Codes by Optional Question Responders and Non-Responders

	Percentage of Total	
SIC code	*Respondents to Optional Question*	*Non-Respondents to Optional Question*
17	0.24	0.04
20	1.83	1.04
22	4.54	4.23
23	0.48	0.41
24	0.08	0.29
25	1.27	1.41
26	5.74	4.39
27	1.83	1.49
28	21.83	21.72
29	0.08	0.17
30	**5.10**	**7.21**
31	**0.80**	**1.66**
32	0.88	0.99
33	6.22	7.54
34	15.30	14.17
35	**2.15**	**3.69**
36	11.24	10.48
37	3.03	2.61
38	6.06	4.77
39	3.51	2.69
45	**0.00**	**0.21**
47	0.00	0.08
49	**2.79**	**4.85**
51	**4.38**	**2.90**
72	0.56	0.70
73	0.00	0.04
75	**0.00**	**0.21**
76	0.08	0.04

Bold SIC codes indicated that the difference in percentages is statistically significant at the 5% level. The t-test assumes unequal variances across groups.

Table 2-3: Distribution of Years When Reporting Ceased by Optional Question Responders and Non-Responders

	Percent of Total	
Year Reporting Ceased	Respondents to Optional Question	Non-Respondents to Optional Question
1990	10.96	11.76
1991	**11.36**	**8.62**
1992	15.06	13.05
1993	14.67	9.51
1994	**13.56**	**15.31**
1995	8.60	10.68
1996	**8.99**	**8.90**
1997	7.57	7.98
1998	**9.23**	**14.18**

Bold SIC codes indicated that the difference in percentages is statistically significant at the 5% level. The t-test assumes unequal variances across groups.

Table 2-4: Distribution of Reported Pollution Releases by Optional Question Responders and Non-Responders

	Mean	Standard Error
Respondents to Optional Question	16,749	4,333
Non-Respondents to Optional Question	18,102	1,443
Difference between Respondents and Non-Respondents	1,353	4,567

A t-test on the difference in average pollution releases between the two groups cannot reject the null hypothesis that this difference equals zero. The t-statistic is 0.30 allowing the variance between the two groups to be unequal.

The data suggest that responders to the optional question are not systematically different from non-responders at least on total pounds of chemicals released. The difference in the average release levels is 1,353 pounds with a standard error of 4,567 pounds. There are some systematic differences in industry and year reporting stopped. Of the 28 SIC codes, the two groups—responders and non-responders—are statistically different for 7 of them.[12] There are also some differences among responders and non-responders in the years for which reported ceased. Of the nine years, the distribution of responders and non-responders differs in four years.[13] In addition, one may be concerned that there are unobservable differences between the responders and the non-responders that also are correlated with whether the facility goes below the reporting threshold but still uses the chemical in positive quantities. In the absence of a valid instrument that can explain response to the optional question, but not explain going below the threshold, specific sample selection correction models cannot be employed. Rather in this section, I impute reasons for non-reporting for those that did not answer the optional question based on data from the sample of facilities that answered the optional question assuming that responders are reasonably representative on non-responders. I then conduct sensitivity analysis on these results which is presented in Section 2.4.3.

There are several ways one might impute these data. A common method is known as "hot deck" (Ford 1983; Little and Rubin 1987). Essentially, hot deck is a matching strategy—find a facility that responded to the question that looks like a facility that did not respond and assign the matching responders value to the non-responder. A variant of hot deck is to use regression to estimate a relationship between facility characteristics and the explanation for ceasing reporting for those that respond to the optional question, and then using this regression function, predict for non-responders what their explanation would have been.[14] This regression-based imputation strategy is employed here.

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[12] A Pearson's Chi-squared test rejects equality of the industry distributions across the two groups.

[13] A Pearson's Chi-squared test rejects equality of the industry distributions across the two groups.

[14] These two strategies differ in the degree that matching is "enforced" and the functional form assumption. Hot deck is a non-parametric strategy that does not impose a specific functional form on the relationship between the covariates and the response variable. Regression is loose matching, but

To construct the prediction relationship, I first create a new dummy variable that takes a value of one if the facility ceased reporting because it went below the reporting threshold and zero otherwise. I then use this variable as the dependent variable in a logit estimation on observable facility characteristics. The task is then to compile a set of observable characteristics that explain whether a facility will go below the reporting threshold.

One characteristic that may explain the propensity to go below the reporting threshold is the facility's distance from the reporting threshold. Degeorge, Patel and Zeckhauser (1999) demonstrate that the existence of performance thresholds for managers induces specific changes in their earnings management, with managers managing to the thresholds. For example, empirically there appears to be a concentration of profits just above zero for managers that are compensated based on whether their unit earns positive profits. Degeorge, Patel and Zeckhauser refer to this as threshold-regarding behavior. Threshold-regarding behavior is particularly pronounced among units that are very close to the profit threshold. In other words, if your unit is quite far from earning positive profits, you do not try to manipulate earnings much, because no amount of manipulation will cause your unit to earn positive profits. But, if your unit is very close to the positive profit threshold, manipulation of earnings is more valuable.

In the context of the TURA reporting thresholds, we might expect to see similar threshold-regarding behavior. Facilities that are very close to reporting threshold in year *t* have a greater incentive to manage their chemical use so that they fall below the reporting threshold in *t+1*. To measure the distance from the reporting threshold, I construct a

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does impose a functional form. The regression-based matching will perform well when the assumed parametric specification is a good approximation to the average response function.

variable that measures, for each chemical, how far the facility is from each of the reporting threshold. In Massachusetts, this process is simplified by the fact that once the facility triggers the reporting threshold for one chemical, the reporting threshold for all chemicals is 10,000 pounds of combined manufacture, process, and other use amounts. So in Massachusetts there are not three separate reporting thresholds, but one binding threshold at 10,000 pounds of total use. The facility's distance from the reporting threshold is then given by total use minus 10,000 pounds. The hypothesis is that the greater the distance from the reporting threshold, the less likely a facility is to cease reporting that chemical because its use of the chemical went below the reporting threshold.

Similarly, if there is a relationship between chemical use and chemical release, then total releases of the chemical in time *t* may predict whether the facility goes below the reporting threshold in year *t+1*. Thus, total releases are also used as an explanatory variable. Other potential explanatory variables include industry dummy variables, and year dummy variables that proxy for changes in industry best practices and exogenous technological change. The relationship estimated is then given by:

$$\text{below\_threshold}\begin{pmatrix}0\\1\end{pmatrix} = \alpha + \beta_1 \text{Distance from Threshold} + \beta_2 \text{releases} + \beta_{\text{sic}} SIC + \beta_t Year + \varepsilon$$

The results of this estimation are present in Table 2-5.

Table 2-5: Results for Estimation of Threshold Logit for Respondents to Optional Question

	Coefficients	Percent Increase in Baseline Probability Resulting from a 10% Decrease from Mean
Constant	1.02 (1.19)	NA
Distance from Threshold	-0.00001 *** (0.000003)	6.5%
Releases	-0.00003 *** (0.000006)	3.1%
SIC dummies	Yes +	NA
Year dummies	Yes +	NA
Number of observations	1,251	
Pseudo R-squared	0.17	
Baseline Probability	60.0 %	

Baseline Probability is the probability evaluated at the mean value of all continuous explanatory variables and at zero for all binary variables.
*** Significant at the 1% level
+ An F-test shows variables are jointly significant at the 1% level

Because the logit estimation is non-linear, the coefficients presented do not convey information about the marginal effect of a change in one of the explanatory variables on the dependent variable. To determine what the magnitude of the effect of each explanatory variable is on the probability that a facility ceases reporting because it goes below the reporting threshold, I first calculate the baseline probability—that is the probability of going below the threshold predicted by the logit equation when all of the continuous variables are set at their mean value and all of the binary variables are set at zero. The baseline probability evaluated at the mean is 60.0%. I then decrease each covariate in turn by 10% from its mean value and report the percentage point change from the baseline. Using this method, a 10 percent decrease from the average distance from the reporting threshold results in a 6.5 percentage point increase in the probability of going below the reporting threshold. Similarly a 10 percent decrease in total releases to the environment results in a 3.1 percentage point increase in the probability of going below the reporting threshold.

This probability function can then be used to predict whether facilities that ceased reporting for a chemical, but did not explain why, were likely to have ceased reporting because they went below the reporting threshold. The above equation was used to predict the probability of going below the threshold for the 2,482 observations with no answer to the optional question. Observations with a predicted probability greater than 50 percent were coded as going below the threshold. Table 2-6 provides a breakdown of the observations that ceased reporting by explanation. Of the 2,482 observations for which no explanation for non-reporting was provided, 1,786 (72.0 percent) were predicted to have stopped reporting because the facility went below the reporting threshold for that chemical, but still use the chemical in positive quantities.

Table 2-6: Distribution of Observations (Facility-Chemical-Year) that Cease Reporting by Explanation

	Explanation Provided by Facility	Explanation Predicted	Total
Below the Reporting Threshold	844	1,786	2,630
All Other Reasons	424	696	1,120
Total	1,268	2,482	3,750

In summary, analysis of the Massachusetts TURA data indicates that a substantial percentage of facilities that cease reporting a chemical do so because they go below the reporting threshold for that chemical, but still use it and may still have positive releases of these chemicals. While this frequency of threshold-regarding behavior seems to present some concern about the validity of the TRI data for making comparisons among facilities or across time, the level of concern may still be low if the total amount of releases that disappear from the registry is small. That is, perhaps the percentage of observations affected by the reporting thresholds is large, but the total share of releases represented by these observations is small. The next section addresses the question of the likely magnitude of the truncation bias.

4.2 What is the Magnitude of Missing Releases in Massachusetts?

The difficulty in assessing the effect of truncation at the reporting thresholds on the validity of the TRI data is that it is impossible to know how large a problem non-reportable releases are, precisely because these releases are no longer reported. The best we can do is assess how large this problem might reasonably be, given observable information. To that end, this section focuses on estimating the magnitude of "missing" releases by estimating bounds on the possible size of these releases—a lower bound estimate, an upper bound estimate, and a linear projection estimate.

To make these results meaningful, I restrict the analysis to chemicals and industries that have been subject to the TURA reporting requirements since its inception—the so-called core group. This ensures that we are examining variation in reported releases due to facility behavior around the thresholds and not changes in releases due to changes in the regulatory requirements themselves. The analysis is also done for two different measures of pollution from the TRI. The first is on-site releases. On-site releases are releases of the pollutant to air, water, land, or under-ground injection that occur at the facility's location. The second measure is total on- and off-site releases. Total releases include on-site releases, but add transfers of waste to offsite locations for disposal or recycling. In general, it is thought that total releases is over-inclusive as a measure of environmental performance because some of the transfers are not pollution, but are transfers for recycling and reuse. On-site releases, however, are under-inclusive as a measure of environmental performance because some waste that is transferred off-site is pollution attributable to the facility. Researchers have used both on-site and total releases as measures of environmental performance, so it important to see if the reporting thresholds have different effects for the two measures.

The lower bound estimate of the value of non-reported releases is that these releases are zero. This assumes that once a facility drops below the reporting threshold for a given chemical, they no longer release any of that chemical. A conservative upper bound estimate of the value of non-reported releases is that they equal the last reported value of releases for that chemical at that facility. Thus, if a facility reports releases of 500 pounds of a chemical in year t and then drops below the reporting threshold, the upper bound estimate of missing releases is that this facility releases 500 pounds a year of that chemical in perpetuity.[15]

One might argue that assuming unobserved releases are either zero or set at their most recent value in perpetuity are extreme assumptions. There is some evidence that suggests that, on average, setting releases equal to the last reported value is not as extreme an assumption as it may first appear. There are 287 observations (1 percent of the total) where the facility stops reporting for a chemical in one year because it went below the reporting threshold and then in the future the facility begins reporting for that chemical again. How do releases of the chemical in future years compare to the reported releases in the last reported year? On average, the future reported releases are 796 pounds *greater* than

[15] Occasionally a facility will report for a chemical for a few years, then stop reporting for that chemical because they are below the reporting threshold, and then begin reporting for the chemical again in later years. In this case, the upper bound estimate of releases equals observed releases in any year in which the facility actually reports releases and in non-reporting years are set equal to the releases in the most recent year in the past for which the facility reported releases for that chemical.

the last reported releases for that chemical. [16] Of course, this does not rule out the possibility that assuming releases are constant for non-reporting facilities is a conservative upper bound. Facilities that waver above and below the reporting threshold are distinct from facilities that go and remain below the reporting threshold. Thus, we might not expect the release behavior of the facilities that oscillate around the reporting threshold to be indicative of the release behavior of facilities that go below the reporting threshold and stay there forever. But this evidence does suggest that using the last reported releases as an estimate of future reported releases might be a reasonable upper bound.

Providing upper and lower bounds on the potential bias in reported releases induced by truncation at the reporting thresholds provides useful information on how large the bias may be. However, it does not provide any information on the probability distribution of the true bias within that range. While this range is useful, perhaps, we would wish to also have an estimate of bias under a more probable scenario. An alternative assumption about the behavior of releases among non-reporting facilities is to assume that facilities do not fundamentally behave in different ways with respect to their release decisions based on whether they are above or below the reporting threshold. If this is the case, then the trends in non-reported releases might be expected to increase or decrease at the same rate as the trends in observed releases. Under this assumption, one can project trends in releases for non-reportable data based on observed trends in releases among reported chemicals.

How can we predict the trend in reported releases so that it can be extrapolated to non-reporting facilities? One might hypothesize that the amount of the chemical used, the

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[16] The largest future drop in reported releases is by 63,571 pounds and the largest future increase in reported releases is 105,600 pounds.

type of chemical, the industrial sector, and other similar factors. would all be good predictors of how much chemical a facility will release in a given year. While all of these factors do explain chemical releases, taken together they only explain about seven percent of the variation in reported releases.[17] The best predictor of reported releases turns out to be a distributed lag of past releases. A simple one period distributed lag, where current releases of each chemical are regressed on releases of that chemical from the previous year, explains 80% of the variation in releases. Increasing the number of lags increase the predictive power slightly, but makes the equation less useful for prediction because a smaller number of observations have multiple lags. The results for one-, two-, and three-period distributed lag models are presented in Table 2-7. Using the one-period distributed lag model, current on- and off-site releases are, on average, 95 percent of previous years' releases. Similarly, using a one-period distribution lag model, current on-site releases are 80 percent of previous years' releases.

[17] This estimate comes from a regression of releases on total chemical use with two-digit SIC code, chemical, and year fixed effects. Including facility-chemical fixed effects increases the predictive power of the regression to 65 percent. However, the distributed lag model, which yields a higher predictive power, also is preferred because all of the data are observable. That is, it relies only on past releases, which we observe for all facilities that cease reporting.

Table 2-7: Distributed Lag Models of Reported Releases

	Total Releases (On- and Off-site)			On-site Releases		
Specification	1	2	3	1	2	3
1 year lag	0.947 ***	0.865 ***	0.872 ***	0.798 ***	0.740 ***	0.737 ***
	(0.054)	(0.131)	(0.149)	(0.036)	(0.084)	(0.115)
2 year lag		0.105	0.086		0.087	0.130
		(0.134)	(0.143)		(0.068)	(0.089)
3 year lag			0.017			-0.021
			(0.083)			(0.046)
Observations	8586	6572	4964	8927	6952	5330
Adjusted R-squared	0.779	0.795	0.804	0.782	0.807	0.808

*** indicates the coefficient is statistically significant at the one percent level.

Using the coefficients from the one-period distributed lag model, I predict releases for each facility that ceases reporting. This generates trends for facilities that cease reporting. These projected trends are downward sloping for all facilities that cease to report for a chemical, and the rate of decrease is larger for on-site releases than for total releases. Figure 2-3 diagrams the process of estimating lower, upper, and linear projected releases for a facility and Table 2-8 provides data on the magnitude of missing releases, both total releases (on and off-site) and on-site releases, using both the upper bound assumption and the linear projection.

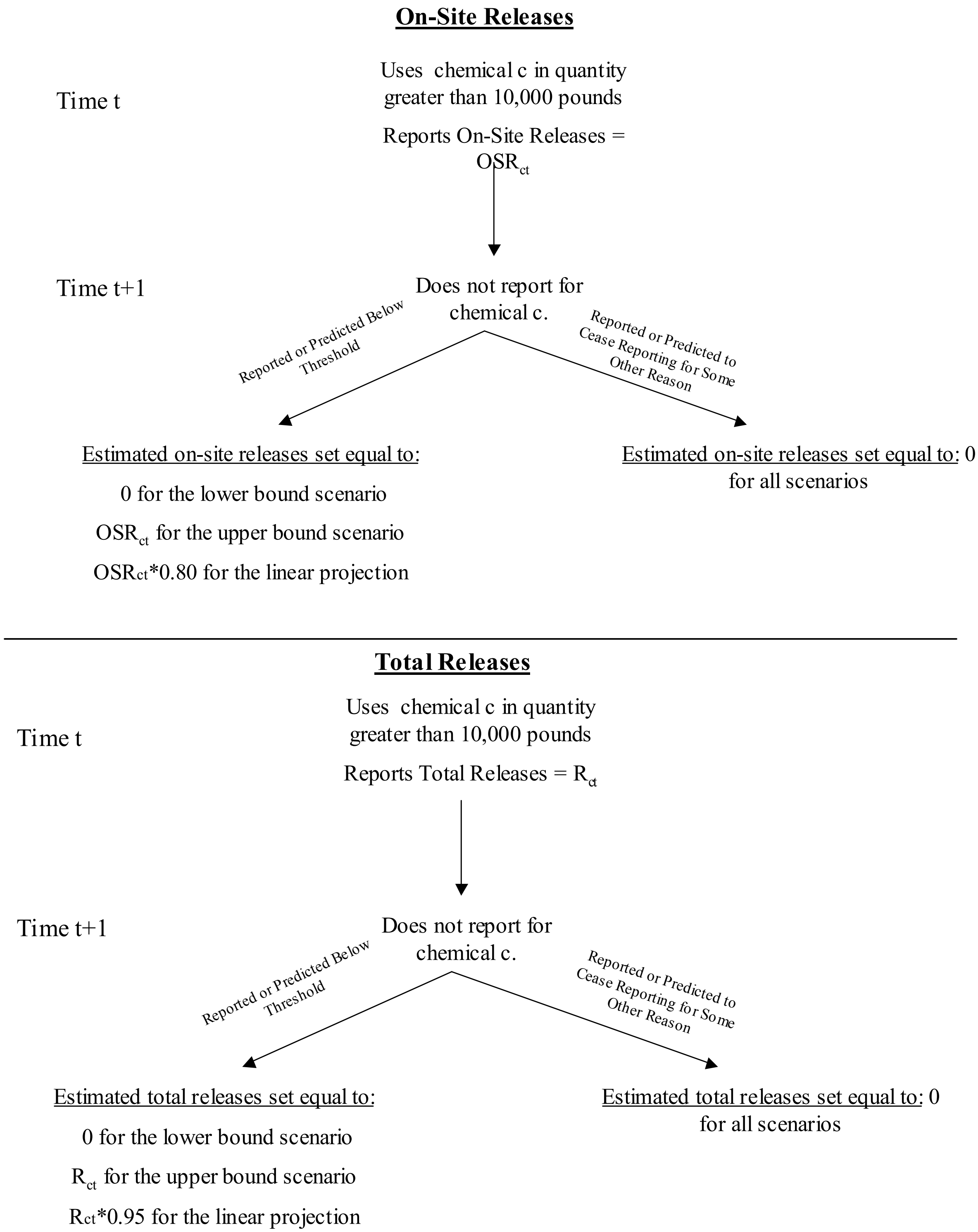

Figure 2-3: Flow Chart for Imputation of "Missing" Releases

Table 2-8: Estimates of TURA Releases in Massachusetts (in 1,000s of pounds)

	Total Reported Releases (On- and Off- Site)				On-Site Releases			
Year	Lower Bound	Linear Projection	Upper Bound	% Difference Between Lower and Upper Bounds	Lower Bound	Linear Projection	Upper Bound	Percent Difference Between Lower and Upper Bounds
1991	43,294	45,228	45,337	4.7%	17,600	18,870	19,191	9.0%
1992	44,842	48,301	48,774	8.8%	15,563	17,495	18,296	17.6%
1993	38,215	44,110	44,427	16.3%	11,857	14,640	16,060	35.4%
1994	39,052	46,131	46,345	18.7%	9,299	12,158	14,258	53.3%
1995	36,538	44,265	44,845	22.7%	8,711	11,264	13,928	59.9%
1996	36,812	44,852	45,935	24.8%	7,449	9,657	13,134	76.3%
1997	32,430	40,614	41,903	29.2%	6,357	8,337	12,318	93.8%
1998	33,026	41,623	43,495	31.7%	5,599	7,364	12,050	115.2%
1999	19,955	28,778	31,333	57.0%	5,117	6,689	11,966	113.8%

The results indicate that the degree of missing releases in the early years is relatively modest as a percentage of total releases. In 1991, missing releases are only about four percent of total (on- and off-site) releases and between seven and nine percent of reported on-site releases. However, over time, missing releases as a percentage of total reported releases rise dramatically. There are two reasons for this. First, missing releases are cumulative. In every year, about two to four percent of the previous year's releases are not reported due to facilities going below the reporting threshold. But in each year the total amount of missing releases are all those releases that are no longer being reported by all facilities, these include the releases from facilities that stopped reporting for the chemical this year, as well as the missing releases from facilities that went below the reporting threshold two years ago, three years ago, and so forth. Particularly in the upper bound scenario, when facilities that drop below the reporting threshold are assumed to continue releasing at the same level in perpetuity, the cumulative effect can be quite large.

The second explanation is that reported releases fall considerably over time, even for facilities that continue to report for all chemicals. For the upper bound case, where non-reporting facilities are assumed to continue to release at the same level forever, the relative importance of these releases increases over time—the missing releases stay the same, but the total reported releases decrease—resulting in a sharp increase in missing releases as a percentage of total reported releases. In fact, for on-site releases the difference between the upper and lower bound estimates of releases differs by 134 percent in 1999.

The data in Table 2-8 illustrate that the magnitude of missing releases generated by the existence of the reporting threshold is non-trival. But does this result in significantly biased estimates in the trend in environmental performance over time? Figures 2-4 graphs the trend in total releases over time for each of three scenarios: (1) only reported releases (lower bound estimated assumes non-reported releases equal zero), (2) reported releases plus the upper bound estimate of missing releases (sets missing releases equal to their last reported value for all years), and (3) reported releases plus linearly extrapolated estimates of missing releases.

Looking at total releases first, it is clear that the estimate of the trend in environmental performance is substantially affected by the exclusion or inclusion of the estimated releases for non-reporting facilities. Using a lower bound assumption, that all non-reported releases are zero, the change in total releases from 1990 to 1999 is 36.7 percent. However, if one instead uses the upper bound assumption, that facilities that fall below the reporting threshold continue to release the same amount forever, the change in total releases over the same ten year period was only 0.6 percent. Using the more moderate assumption that facilities that are no longer reporting continue to decrease releases over time at the same rate as the average reporting facility, the change in total releases over the ten-year period is 8.7 percent.

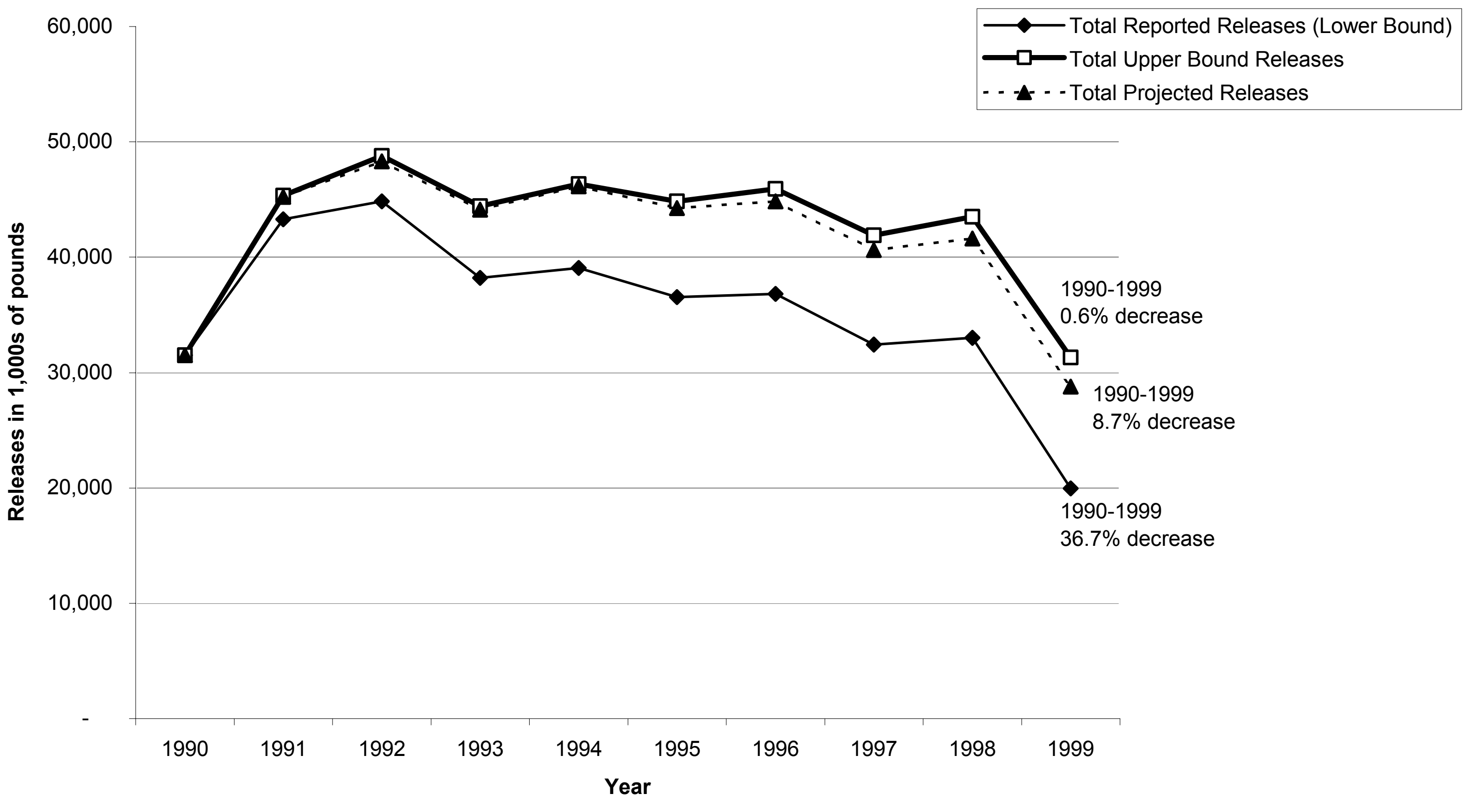

Figure 2-4: Trends in Total (On- and Off-Site) Releases in Massachusetts

The story is somewhat less stark, but still significant, if one ignores the upward trend in reported off-site releases between 1990 and 1991 and instead examines trends only for the years 1991 to 1999. For this period, the lower bound assumption leads to the conclusion that total releases have fallen by 53.9 percent, the upper bound assumption estimates the decline at 30.9 percent, and the linear projection leads to an estimate of a 36.4 percent decrease. Using the difference in trends from 1991 to 1999, it appears that in the worst case, non-reported releases by facilities that fall below the reporting threshold may account for approximately 23 percentage points of the total 54 percentage point decrease in reported releases, or roughly 40 percent.

The potential degree of bias introduced by the reporting thresholds in the trends for on-site releases is less pronounced, although still sizeable. Figure 2-5 provides the estimated trends in on-site releases. Using the lower bound assumption, the decrease in on-site releases from 1990-1999 was 75.7 percent. Using the upper bound assumption, the decrease over this period was 43.1 percent. Finally using the assumption of linearly projected decreases in non-reported releases, the change over the decade was 68.2 percent. Thus, of the observed 75 percent decline in reported releases, from 1990-1999 as much as 32.5 percentage points (or 40 percent) of this decrease may be accounted for by non-reported releases.

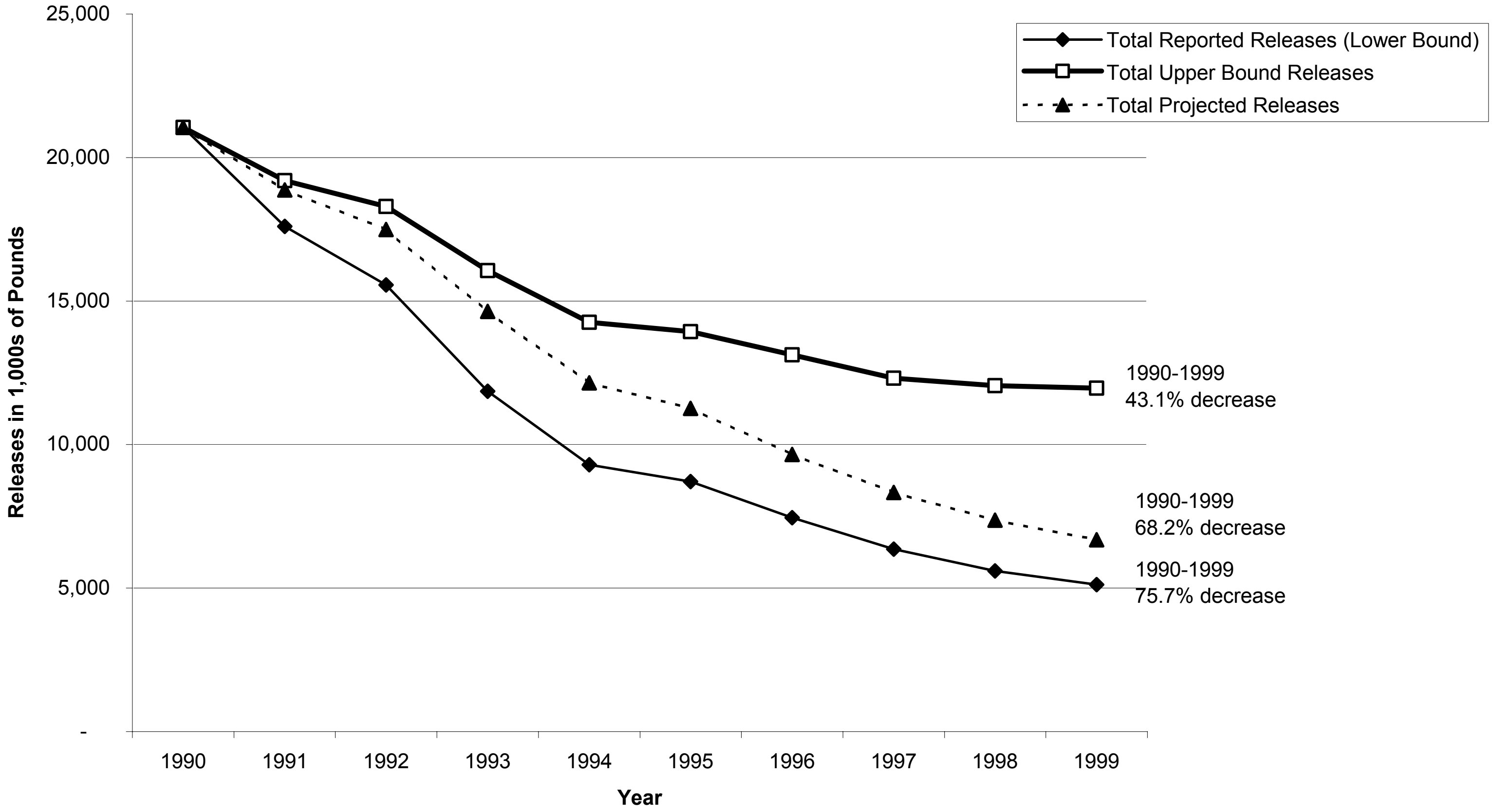

Figure 2-5: Trends in On--Site Releases in Massachusetts

As evidenced by these data, our understanding of the magnitude of the change in environmental performance is substantially affected by the truncation bias introduced by the reporting threshold. Making different assumptions about what happens to releases when facilities cease to report because they fall below the reporting threshold makes a big difference in our understanding of the level of environmental improvement. Having said that, it is somewhat reassuring that even in the upper-bound scenario, on-site releases of toxic chemicals have fallen by over 43 percent in Massachusetts. In this case, using TURA data to conduct policy analysis is very likely to result in erroneous cardinal estimates of the magnitude of improvement, but unlikely to result in erroneous ordinal estimates—things have improved by somewhere between 43 and 76 percent.

A separate concern is whether the reporting thresholds induce bias in cross-sectional comparisons of facilities. Imagine that we rank facilities from lowest releases to highest releases. If the degree of non-reported releases is small relative to any given facility's total reported releases, adjusting for these non-reported releases will change our estimate of how much a facility releases, but may not change the facility's rank much. If on the other hand, the magnitude of missing releases is large relative to total reported releases for some set of facilities, then adjusting facility totals to account for this may substantially changing the rankings of facilities.

To investigate the degree of cross-sectional bias potentially introduced by the reporting thresholds, I first divide facilities into quartiles based on their reported releases. Thus, facilities whose reported releases are in the lowest 25 percent of all facilities in that year are assigned to quartile number one. Facilities whose releases are among the highest 25 percent of all facilities are assigned to quartile number four. These are the quartile rankings associated with the lower bound assumption that all non-reported releases are equal to zero. I then recalculate the facility's quartile ranking based on the upper bound assumption and the linear projection. If the relative importance of the non-reported releases is small, we would expect to see few facility quartile rankings change. In this case we would not be terribly concerned about the potential cross-sectional bias introduced by the reporting thresholds. However, if the relative importance of missing releases is large, we might expect to see more facility quartile rankings change dramatically. An alternative way to think about this is to imagine a policy maker assigning a facility a grade based on its pollution levels relative to other facilities in a given year. A facility can get a grade of excellent, good, fair, or poor. If we adjust for missing releases resulting from facilities going below the reporting threshold, do we change our grades for only a few facilities or for a substantial number of facilities? The results of the quartile distribution comparison are provided in Tables 2-9 and 2-10 for total and on-site releases, respectively.

Table 2-9: Quartile Ranking Comparisons for Total Releases

		Upper Bound Ranking			
		1	2	3	4
Lower Bound Ranking	1	**1,046**	960	794	277
	2	349	**146**	33	6
	3	256	472	**501**	105
	4	0	67	321	**1,257**

Table 2-10: Quartile Ranking Comparisons for On-Site Releases

		Upper Bound Ranking			
		1	2	3	4
Lower Bound Ranking	1	**1,279**	644	865	515
	2	232	**173**	30	5
	3	171	624	**338**	71
	4	0	177	414	**1,054**

In each table, the diagonal elements (in bold) represent facilities whose quartile ranking is unaffected by inclusion of estimates of their missing releases. All off-diagonal entries are facilities whose quartile rankings differ under the upper and lower bound assessments. For total releases, 45 percent of facilities have different quartile rankings under the upper bound assessment than under the lower bound assessment. For on-site releases 43 percent of facilities have different quartile rankings under the two different scenarios.

For the purposes of using TURA or TRI rankings for regulatory targeting purposes, perhaps one is most concerned about errors in the top and bottom quartile. That is, one is concerned most with mislabeling a facility as "poor" or "excellent." Focusing on these quartiles, we can see from the data in Table 2-9 that 24 percent of the facilities that are labeled "poor" based on their reported releases would have been labeled either "fair" or "good" once reported releases are adjusted to include an upper bound assessment of missing releases. In addition, 66 percent of facilities that were labeled "excellent" using only reported releases would have been labeled "good," "fair," or "poor" if missing releases are included. The results for on-site releases are quite similar (Table 2-10). For on-site releases 36 percent of facilities labeled "poor" using reported releases should have received a higher grade and 61 percent of facilities labeled "excellent" should have received a lower grade.

This potential variation in ordinal rankings of facilities within a given year is large enough to be of substantial concern. Indeed, the degree of variation in the cross-section rankings of facilities seems more troubling than the variation in trends over time. In the trends, we were confident in the direction of the change (there had been an improvement), but not in the magnitude of the change. With the cross-sectional rankings we are potentially assigning the wrong grade two-thirds of the time in the best quartile and a quarter of the time within the worst quartile. If regulatory or enforcement resources are targeted based on rankings of reported releases, these resources are likely to be misallocated.

4.3 Sensitivity Analysis

One of the key components of the above analysis was imputing whether or not a facility ceased reporting because it went below the reporting threshold if the facility did not answer the optional question. As discussed in Section 2.4.1, there were some observable differences between facilities that responded to the optional question and those facilities that did not respond. How sensitive are the findings to changes in the imputation method? I try two alternative imputations and provide information on the impact of these changes on the estimates of truncation bias both for trend and cross-sectional analyses.

The first sensitivity analysis is to change the probability cutoff from the logit that determines whether a facility that does not answer the optional question stopped reporting because it went below the reporting threshold. In the main analysis, facilities with a predicted probability of going below the threshold greater than 50 percent from the logit were assumed to have ceased reporting because they went below the reporting threshold. The first test is to increase this probability cutoff to 75 percent.

The second sensitivity analysis is to only examine missing releases for facilities that actually respond to the optional question and state that they cease reporting because they went below the reporting threshold, but still use the chemical in positive quantities. No imputation is made for facilities that do not answer the optional question. This is essentially the same as increasing the probability cutoff from the logit to 100 percent.

Table 2-11 presents the results of the sensitivity analysis for estimates of the effect on the trend in total and on-site releases. For comparison, the first column contains the original estimates (where the probability cutoff equals 50 percent). Under the most conservative scenario, that only used data from facilities that actually answered the optional question, up to 15 percent of the observed decrease in total releases and up to 17 percent of the observed decrease in on-site releases from 1991-1999 could be due to missing releases.

Table 2-11: Maximum Percent of Observed Decline in Releases from 1991-1999 That Could be Explained By Threshold Regarding

	50%	75%	100% (responders only)
Total Releases	42.7	23.8	14.8
On-Site Releases	46.9	27.2	16.7

Sensitivity analysis was also done on the cross-sectional rankings. Table 2-12 presents the results for the differences between the upper bound and lower bound quartile rankings for all three modifications to the probability cutoff (75%, 100%, and expected value). The first column of the table provides the main findings based on a 50 percent cutoff for comparison. In the most conservative scenario, the quartile rankings are wrong about 25 percent of the time. The rankings are only wrong about seven percent in the bottom quartile. That is only about seven percent of facilities that would have received a label of "poor" using the reported data should have received a higher grade if missing data were incorporated. In the upper quartile the percent error is still higher than average. Approximately 33 percent of facilities that would have been labeled "excellent" based on reported releases would have received a lower ranking if missing releases had been accounted for.

Table 2-12: Maximum Potential Error in Cross-Sectional Rankings Due to Threshold Regarding

	50%	75%	100% (responders only)
Total	45%	42%	25%
Bottom Quartile	24%	7%	7%
Top Quartile	66%	56%	33%

Overall the sensitivity analysis supports the general findings presented in the main analysis. First, a potentially significant share of the decrease in observed releases may be due to facilities no longer being legally obligated to report releases because their use of the chemical is below the reporting threshold. Second, the reporting thresholds may also skew the cross-sectional rankings of facilities. In particular, facilities that appear to be low releasers or good environmental performers based on TRI releases may actually not be better than other facilities with higher releases. The error in cross-sectional ranking diminishes as one moves down the distribution. The rankings are considerably less wrong about identifying the worst facilities.

4.4 Extrapolating to the National Level

So far we have examined the degree of bias potentially introduced by the reporting thresholds into both trend and cross-sectional measures of environmental performance for Massachusetts' facilities only. The reason for focusing on Massachusetts was one of data availability. Data from TURA are critical in estimating the magnitude of the truncation bias.

Can we extrapolate the findings from Massachusetts to estimate the degree of truncation bias in national trends and cross-sectional comparisons? Unfortunately, any such precise extrapolation would be shaky, at best, and outright misleading at worst. While the results for Massachusetts do give a strong reason to be concerned about truncation bias affecting the validity of national TRI data, the actual bias at the national level could be lower or higher than in Massachusetts. The set of Massachusetts' facilities is far from a representative sample of national facilities reporting to the TRI. On average, facilities in Massachusetts have reported releases that are an order of magnitude smaller than average releases for facilities in other states. The average total releases between 1990 and 1999 for Massachusetts' facilities were 22,971 pounds while the average for all other facilities were 166,368 pounds.[18] These substantial differences

[18] This difference is not only due to a smaller variance among Massachusetts facilities. If one graphs the distribution of both total and on-site releases for Massachusetts and all other states,

in the only observable variable that might be used to both predict which facilities cease reporting because they went below the reporting threshold and to estimate the magnitude of missing releases, make valid extrapolation infeasible.

Even on an intuitive level it is difficult to predict how the precise degree of truncation bias nationally will compare to the estimated degree of bias in Massachusetts. On the one hand, the reporting thresholds are lower in Massachusetts than for the federal TRI program. In Massachusetts, once a facility triggers a reporting threshold for a single chemical, the facility must report for all chemicals for which manufacturing plus processing plus otherwise use amounts are greater than 10,000 pounds. For the federal program reportability is calculated separately for each chemical based on the 25,000 pounds manufacture or process and 10,000 pounds otherwise use thresholds. This might imply that truncation bias is likely to be a larger problem for the federal TRI data than for the Massachusetts TURA data.

On the other hand, Massachusetts has an aggressive state-level pollution prevention and reporting program that provide additional incentives for facilities in that state to reduce use of their chemicals below the regulated level. Thus, we might expect to see more threshold-regarding behavior in Massachusetts than in the rest of the country. Similarly, Massachusetts' facilities do have lower average releases. This may also indicate that Massachusetts' facilities are, on average, closer to the reporting threshold and we would expect to see more threshold–regarding behavior in that state than in the nation as a whole.

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the entire distribution for Massachusetts' facilities is shifted to left. A Kolmogorov-Smirnov test rejects the null hypothesis of equality of the distribution at the 1% level.

While I cannot provide any specific estimate of the degree of truncation bias in the national trends or cross-section comparisons using TRI data, the experience in Massachusetts does suggest that concern over truncation bias is well-founded. In addition, some preliminary evidence from decreases in the federal reporting thresholds for lead provide further evidence that the reporting thresholds may affect inferences from the federal TRI data.

In 2001 the reporting threshold for lead was lowered to 100 pounds at the federal level. For the 2000 reporting year, there were 1,997 facilities reporting for lead and lead compounds and total reported releases of lead were 374 million pounds. In 2001, the first year of reporting under the lower threshold, there were 8,444 facilities reporting a total of 443 million pounds. This represents a net increase of 69 million pounds (19 percent). About half of this increase--33.5 million pounds--is attributable to facilities that did not report on lead and lead compounds in 2000.[19]

Given these results, what implications should we draw about how to use TRI data as a measure of environmental performance for policy analysis? That is the topic of the final section.

5 Policy Implications for Using TRI as a Measure of Environmental Performance

The above analysis indicates that the existence of the reporting thresholds may introduce substantial bias in both the trend and cross-sectional estimates of environmental performance using reported TRI releases. However, the TRI data are currently one of the best sources of facility-level pollution levels nationwide. What is the policy analyst to

[19] Personal communication, Cody Rice, Office of Pollution Prevention and Toxics, U.S. Environmental Protection Agency, April 26th, 2004.

do? The following recommendations are likely to enhance the validity of studies that use TRI data for policy analysis.

First, the number of chemicals reported by a facility should be treated as an additional policy outcome.[20] If EPA is investigating the effectiveness of a new regulation, it is not sufficient to examine only the effects on total TRI releases (even if these releases are adjusted for toxicity). If the policy also has an effect on the number of facilities that reduce chemical use below the reporting threshold, then estimates of the policy's effect on TRI releases are likely to be biased upward—that is, the effect of the policy will be overstated. If however, one estimates that the policy does not have an effect on the number of chemicals reported, but does have an effect on total releases, one can feel more confident that the policy has actually improved environmental performance and not simply reduced reporting.[21]

If one is only concerned about whether a policy had a positive effect on environmental performance, then a worst-case estimate of the effect may be appropriate. For this worst-case estimate one would assume that any facility that ceases reporting for a chemical did so because it went below the reporting threshold (as opposed to eliminating the chemical, going out of business, or so forth). The facility's releases could

20 It may be tempting to normalize total releases by how many chemicals the facility reports. For example, one could compare facilities both cross-sectionally and over time based on their average releases per report. However, this correction does not remove the truncation bias. For example, imagine a facility releases uses three chemicals and in the first reporting years reports releases of 200 pounds of Chemical A, 200 pounds of Chemical B, and 150 pounds of Chemical C. In that year the average releases per chemical reported is 183 pounds. In the second reporting year the facility only reports for two chemicals. It still releases 200 pounds of chemical B and 150 pounds of Chemical C. Now the average releases per reporting year are lower at 175 pounds. Releases per chemical reported declined, but the releases for that facility's reported chemicals do not change across those two years.

21 See the first chapter of this dissertation for an example of a policy analysis that estimates the effect of the policy on both releases and number of chemicals reported.

then be set to their last reported level in perpetuity. This clearly overestimates total releases, but if the policy is still found to lower releases even under this extreme assumption, then one can feel confident in the policy's effectiveness. The magnitude of the effect will be wrong, but the direction, if positive, will be correct.

One could also do sensitivity analysis on the directional effect of the policy (although again not on the magnitude of the effect) by comparing the results for the whole sample to the results for a sample only of facilities that report for the same chemicals over the relevant time frame. If one estimates positive effects of the policy in both samples, then the effects are not due only to decreases in reporting.

Such sensitivity analysis would be greatly improved by the addition of a question on the federal reporting form that asks facilities why they are not reporting for a chemical in the current year for which they reported in previous years. This question, similar to the one used on the Massachusetts TURA form, could help EPA and others assess the potential for truncation bias.

Unfortunately, the only fail-proof way to ensure that truncation bias will not affect the results, and the only way to ensure the magnitude of policy estimates is accurate, is to eliminate the reporting thresholds. EPA has the regulatory authority to change the reporting thresholds, and has done so on two occasions. In 1999, EPA reduced the reporting threshold for persistent and bio-accumulating toxins (the threshold was reduced to 10 or 100 pounds depending on the chemical). In 2000, EPA reduced the reporting threshold for lead to 100 pounds. Obviously, there are costs associated with lowering-reporting thresholds. These costs include administrative costs for promulgating a series of notice-and-comment rulemakings that are likely to be contentious. There are also substantial paperwork compliance costs for facilities that will be required to report for chemicals that were previously unreportable. These costs were estimated at 80 million dollars for the first year of reporting under the lower threshold for lead (U.S. EPA, 2001).

Despite the costs, there may be important benefits from reducing or eliminating the reporting thresholds. For example, EPA argues that responsible use of TRI data allows "Federal, state, and local governments to compare facilities or geographic areas, identify hot spots, evaluate existing environmental programs, and track pollution control and waste reduction progress" (EPA, 2002b). This statement is only correct with the caveat that analysis must also include an examination of the effect of the policy on the number of chemicals reported, or in some other way address the potentially serious issue of truncation bias. EPA has spent considerable resources developing a series of risk-based weights for the TRI data in an effort to enhance the validity of these data as a measure of environmental performance. Based on the results presented in this paper, it is worth asking whether a similar effort on reducing truncation bias is warranted.

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Transcription of Session VI Discussant Comments by John Dombrowski (U.S. EPA, Office of Environmental Information)

Thank you. I'm John Dombrowski—I'm the Associate Director of the TRI Program Division in the Office of Environmental Information. I'm really impressed by the work that Lori [Snyder] and Michael [Kraft] have done. When I read through the papers, I was actually thrilled, thinking, "Wow, people are actually using our data, and look at this!" I think you did very good work, and you should be proud of that.

Just some points of clarification that I'd like to make first, generally about the TRI Program. Then I have some thoughts, or comments, on the papers themselves and a little update to what's going on in the TRI Program, because I think some of your work is relevant to some things that we're looking at within the TRI Program.

As everyone here is aware, the EPCRA [Emergency Planning & Community Right-To-Know Act] Section 313 set the reporting thresholds; the one thing that I would encourage is [for everyone] to look at the Pollution Prevention Act [PPA], because our program is based on this act, and that's where we start encouraging facilities to go towards source reduction.

There has been a lot of reference to the risk-screening environmental indicators [tool] in [the] context of the data in risk. That's fine—we agree with that—we don't have any problem with that, except just keep in mind that the TRI Program is a *hazard*-based program. I have my hands full just *collecting* the information, getting the information out there, needless to say to worry about the *risk* of the data, but I don't think it's something that should be ignored. Again, we provide the data so that communities can make a risk determination for themselves at the community level.

There's been discussion about how the data represent just an estimate, and I assume that's meant in a broader sense, because the origin of TRI data can be based on direct monitoring. The statute does allow for direct monitoring pursuant to other laws—it doesn't *require* direct monitoring—*or* making a reasonable estimate. Then I guess if there is direct monitoring data, they can extrapolate that data over the course of a year, and that's where the estimate would come in. I just want to clarify that it *does* allow for both, and sometimes, actually, we think the engineering-estimated data are probably better than some of the direct-monitoring data that are collected out there.

There's another clarification, also, that the *total* releases—I just want people to be aware, because sometimes this is totally misunderstood—the total releases that EPA reports do *not* include transfers offsite for recycling or treatment, and the TRI Program does not include reuse in their program. "Total releases" involves either releases to environmental media onsite or transfers offsite for disposal. I just want to make sure that people are clear on the terminology that we're using for the program.

The one thing about the TRI Program, as Mike pointed out: It is a very good capacity-building tool—that's exactly the intention of the law. If you look at EPCRA Section

313h, that's what Congress intended it to be. We provide information to the public, to governments, to researchers, and yes, we think it is a *very* valuable and *very* good measurement tool. We'll stand behind a lot of those statements that are up there. However, I don't think you're going to be able to find *one* specific answer to your questions about why reductions are occurring—I think there will be *other* factors, a *lot* of factors involved in that. Yes, I think public pressure is a *big* part of that, but I think you're going to find a lot of different reasons for your answers. Don't look for just one answer is what I would suggest.

The one thing, Mike, that you pointed out in your paper (this is a *big question* for us that we always talk about, and we're starting to think about it a little bit more)—releases are going down. Why is that? Well, again, that's one of the questions that you're trying to answer, and we look forward to those answers. Part of it, also—and what I would suggest you look at, too—is the fact that we allow facilities to come in and revise their information. We don't have any set limit on that. They could also *withdraw* their information. So, as facilities gain better information about their operations and better information about how they may have estimated their releases, they may go back and revise their information using better methods. That's something that should be factored in as well, for more accurate information.

The one thing that you pointed out, Mike, in your paper is that the production-related waste is increasing. We've noticed that trend, too, but we don't really have an answer for that. Releases to the environment are going down, and that's a very good story to tell. We want to tout that story, and the media picks up on it a lot—the public picks up on it, because they understand "releases"—that's very simple to understand. But then we look at the production-related waste, and I wonder what is this increase in production-related waste attributed to? Production-related waste, again, includes releases, transfers offsite, onsite treatment, and energy recovery and recycling. Hopefully the majority of production-related waste is in recycling—that's what we're hoping for. Still, when you look at the fact that production-related waste is increasing, you wonder whether TRI is achieving the ultimate corporate behavior that you want it to achieve. It's nice that they're reducing their releases, but you want them also to start going towards source reduction. Then, you say, let's focus on PPA, because PPA is where we're starting to look at source reduction. Maybe it's partly something EPA could do more—we focused a lot on releases in the past years, in our public data releases and our public data release reports, etc. Maybe we should start putting a little bit more "context" to the data—that's been a big buzzword for us—and highlighting production-related waste and corporate behavior, if you will, and the trends that we see. Part of the difficulty of that, though, is trying to make that relevant to the public—showing what that means for a community. They understand what a *release* means, and they can take that information and use it to determine risk within their community, and that's what we would hope they do. But, what does recycling, or energy recovery, or treatment mean for that community just because the facility increased those amounts? So, it's not really easy for us to answer how to put that all in context.

When I was thinking about Lori's paper, and as I told her before the presentation, that's another big area that we always ask about internally. We look at our data, and sometimes we see facilities popping in and out of reporting through the years, and we wonder what they're doing when they're not reporting. They're obviously *below* the reporting threshold, but are they still releasing the chemical? Your analysis is very interesting to see what that impact would be. The one thing I wonder, and maybe this is irrelevant and wouldn't make a difference, but have you considered looking at where we tried to minimize the truncation bias with the PBT [Persistent, Bioaccumulative, and Toxic] chemicals? We lowered the thresholds, so that theoretically minimizes that truncation bias, and you could start looking at facilities that, for the PBT chemicals, weren't *added* as PBTs—they were just *made* PBTs—meaning they were always on the TRI list—if you look at facilities that were reporting for those chemicals prior to them being a PBT—and then maybe they dropped out before they were made a PBT, and then all of a sudden we *made* them a PBT and they came back into the universe, what are they reporting as a release there? Because now you know it's less than 25- or 10-thousand pounds, but it's greater than 100 or 10 pounds for their use or manufacturing or processing. How do those releases compare to the higher thresholds when they were reporting? Or how about facilities that reported for the first time because of the PBT chemicals? How are those releases comparing to facilities that were reporting prior to the [re-classification of the] PBT chemicals and the lowering of thresholds? That might just be something to help with the data here and verifying what you're finding, and I'd be curious to see if it does relate.

Nothing against your conclusions, *but* I thought your measure on how effective our policies are was kind of interesting. Actually, I have to give it some more thought, but the idea about having the same number of chemicals and the same number of reports based upon a policy decision, but the releases go down or maybe even the production-related waste decreases. Trying to apply that as a measure of effectiveness to our policy I find very interesting, and I'll have to think about how we might use that.

The idea about adding a question on our form to collect information about why they have no longer reported or actually reducing or eliminating the reporting thresholds—well, to be honest, that's not going to happen.

As far as the questionnaire, Mike, that you're getting ready to develop, I'm excited to see the results.

The thing I find interesting about both of these reports that kind of relates to what we're doing within TRI—we're doing a lot of modernization. One interesting note for the researchers here is we're working *really, really* hard to get the data out sooner. We're hoping that by November of this year we're going to have the 2003 data out on a facility-specific basis, so you'll be getting the data a lot earlier.

I look at the reports and I look at what we're doing now—we have a burden-reduction effort underway, and part of the burden reduction effort, for example, is to increase thresholds as one option. Then I look at Lori's paper and I go, "Oh—okay!" The one thing I find interesting, too, is that on one's really mentioning the Form A, and I'm curious to know why. Is that just because it's a lack of information now or it's useless? What's the feedback there? One of our options is to *look* at the Form A and maybe *increase* its use by increasing thresholds. Another option is "no significant change"—a facility can report what they reported the previous year just by certifying no significant change. And what is the meaning of that? Well, we have to figure that part out—that's the challenge.

I intend to read these papers again. There are some people back in my office who are working on a burden-reduction effort. I want to share these papers with them so they can consider the information presented here as they go through the rulemaking, because, again, I thought this was very good work on both of these papers.

Session V Discussant Comments
Tom Bierle, Resources for the Future

Overview

These papers deal with two of the most important questions about information disclosure:

- What does disclosed data mean? and,
- How are information disclosure programs working?

Snyder provides a very interesting addition to the body of research on just what TRI numbers mean. She adds what is to my knowledge a new and important caveat to the many qualifications—acknowledged but often unheeded—to the facile assumption that TRI is a comprehensive and relatively accurate measure of environmental performance.

Kraft, Abel, and Stephan focus in on how disclosure programs work. In a new and welcome installment to their ongoing and ambitious work on TRI, they move their analysis down to the state level on the way to examining how individual communities and facilities translate information into action.

As I turn to the individual papers, I will begin with Snyder's question of what the data mean and then move to Kraft, Abel and Stephan's analysis of how disclosure works.

Snyder

Snyder focuses on how reporting thresholds may lead to a significant under-reporting of actual TRI emissions, possibly accounting for 40% of the observed decline in emissions in Massachusetts. The argument rests on facility managers practicing "threshold-regarding behavior"—looking for relatively easy opportunities to reduce the use of chemicals below reporting thresholds and thereby avoid reporting emissions.

The analysis proceeds through three key steps:

- First was determining whether the facilities that specified in a questionnaire that they had ceased reporting because they had fallen below a use threshold were representative of other firms that had ceased reporting but had not specified why.

- Second was estimating why these other firms ceased reporting based on a model of "threshold-regarding behavior." Snyder estimated that 72% of these firms ceased reporting because they fell below thresholds, a number similar to the 66% among firms that answered the questionnaire.

- Third, was providing three estimates—upper bound, lower bound, and extrapolated—of what releases would have been if use thresholds had not existed. The upper bound used 100% of the previous year's releases, the lower bound used 0% of the previous year's releases, and the extrapolated used 79% to 93% of the previous year's releases.

Reporting thresholds are there because of the assumption that releases of chemicals used at levels below thresholds don't add much to the aggregate—they are essentially a *de minimis* exemption. Synder's analysis suggests that this assumption may be very wrong, undermining the validity of TRI as a measure of environmental performance. Not only do reporting thresholds affect the magnitude of trends but the ability to confidently rate and compare facilities using a common metric, which many analysts see as the most important aspect of TRI.

Unfortunately for this discussant, the degree to which these findings are significant rests on questions of methodology. Rather than dissect that methodology here, I will merely raise two questions:

- Barring some sort of sensitivity analysis, I suspect that one of the key variables here is the prediction that 72% of non-reporting facilities stopped reporting because they fell below a threshold. How sensitive are the results to even small changes in this number? What value would different approaches to estimation produce? What do we make of the fact that the state program explicitly encouraged companies to show a decline in use? Was there "program-regarding behavior" that encouraged firms to choose a particular response in the questionnaire?

- When looking at trends under the different scenarios, should they really begin at the same point in year 1? Wouldn't a worst case scenario involve different assumptions about what was not being reported in that first year, rather than assuming that all non-reported chemicals actually accounted for zero emissions?

Finally, I might simply acknowledge that advocates of materials accounting reporting might take issue with the contention that reducing chemical use "does not represent a real improvement in environmental performance." Indeed, advocates argued that lowering use led to lower risks from transportation, accidents at facilities, etc. As Snyder acknowledges, a specific policy goal of the Massachusetts program was reducing chemical use. Facility managers who identified a decline in chemical use in their TURA reports were doing exactly what the state program had encouraged them to do, not admitting to a crafty way of avoiding TRI-type reporting.

Kraft, Abel, Stephan

Kraft, Abel, and Stephan have undertaken a large project to take TRI data, warts and all, and understand why it affects firms' behavior. There are two aspects of this overall project that I particularly like. One is the specific focus on the agency of communities and firms and the acknowledgement that actions may be very case-specific. Second is bringing the tools of political science to this area of policy analysis, acknowledging that these programs are not operating in a political vacuum.

In this particular paper, the authors examine a variety of indicators that may explain state-level differences in the performance of firms. They find that the key variables that matter are 1) the level of conservation group membership in the state and 2) less ideologically polarized politics in the state. Political scientists must be very satisfied when political variables rise to the top.

The question arises: how should we interpret these results? Ultimately, the authors are going to combine the results of analysis at various levels of generality to come up with what I expect will be a nuanced interpretation. In the context of this paper, however, a few questions arise:

- How to interpret the results about conservation group membership? Does this mean that a more conservation-minded ethos permeates the business community; that state legislators and regulators are more likely to pursue stricter regulation; that NGOs with large memberships are able to push firms or push their members to push firms, more easily; or some other explanation? If the NGO explanation, how do we fit in the fact that most NGOs are nationally-based and don't get involved in state level policy or local decisions about facility permits, etc.?

- How to interpret the results about the polarization of politics? Do less polarized states have a more unified view about the value of environmental protection? Are they less prone to gridlock that distracts legislators, regulators, and communities? I suppose one could make the case that more polarization would have firms doing better in order to hedge against a change in administration that might punish them more. Less polarization may create the expectation of continuity and less impetus for firms to go the extra mile.

Two other questions come to mind as well:

- To what extent do these kinds of variables affect TRI differently from other environmental programs? Are we simply seeing generic differences in environmental regulation and enforcement among states or is there something different about how these variables affect community empowerment or facility managers' incentives to go beyond regulatory requirements in response to TRI.

- Finally, given that the data come from the period 1991 to 1997—a good bit of which was the era of low hanging fruit—how much does the analysis tell us about today's dynamics and opportunities for improving the program?

It will be interesting to see how these authors' future work can tease out these sorts of dynamics. I must say I am intrigued by the idea that "political factors can influence the *direction* of change, while non-political factors may influence the *intensity* of change."

Synthesis/Themes

The most challenging task for a discussant is to try to identify common themes in the papers presented. Although these two papers deal with very different questions about TRI, I do think there are some common themes.

First is the value of getting beyond analysis of aggregated national trends. At the state level, the smoothly declining slopes in national releases get far more complex. Both papers do a very good job of acknowledging this complexity, Snyder by being very cautious about applying her findings beyond Massachusetts and Kraft, Abel, and Stephan by welcoming idiosyncrasies as possible sources of interesting findings. I do wonder if state-level analysis will ultimately prove to be the most revealing level of disaggregation or merely a convenient way to divide up the world. I can see why it would be relevant in Massachusetts, where a more comprehensive disclosure program is in place. But I wonder whether Kraft, Abel, and Stephan will find that disaggregating to sub-state regions or even communities is ultimately a more revealing.

The second way these papers relate to each other is in providing insights into the motivations of corporations. Snyder discusses a dynamic of "threshold regarding behavior" by which firm managers see opportunities to "reduce" reported emissions by simply using slightly less of a particular chemical and thereby falling below reporting thresholds. This focus inside the firm on the motivations of individual managers may well be the kind of thing that Kraft, Abel and Stephan find as they move into facility-level surveys. One would not be surprised to ultimately conclude the TRI "works" because of a complex assortment of political, policy, community, and organizational motivators, of which corporate managers' keen eye on reporting thresholds is one.

Finally, Snyder's and related work does provide a bit of a thorn in the side of analysts like Kraft, Abel, and Stephan, and other people who work on disclosure. To suggest that the measuring tape we are using doesn't have the feet and inches quite right is not necessarily a welcome insight. The performers may not be the performers we thought they were (or at least in the way we thought they were) and likewise the strugglers may simply be less crafty about gaming the system. I don't think this is an insurmountable problem. The key is always keeping in mind that TRI numbers are constructs influenced by human agency, not objective measures of environmental performance. If efforts to explain why TRI numbers go up or down keep in mind the many ways that reporting facilities can make those numbers go up or down—some through real changes in the workings of physical plants and some on paper—the kind of analysis done by Snyder and the kind of analysis done by Kraft, Abel, and Stephan can be complementary rather than antagonistic. I should note that Kraft, Abel, and Stephan are taking one of these issues head on—that TRI reports on total pounds rather than an indicator or risk—in their larger work.

Ultimately, the key question for those interested in policy and EPA in particular is going to be what these and other studies tell us about how disclosed information ought to be used and how disclosure policies ought to be changed. I think both of the lines of research discussed in this panel will ultimately have that kind of pragmatic value.

Summary of the Q&A Discussion Following Session VI

Ann Wolverton (U.S. EPA, National Center for Environmental Economics)
[Note: Dr. Wolverton brought two questions for each presenter. For better cohesion and flow in this summary, the chronological order will be interrupted, with the responses from Dr. Kraft coming before the questions posed to Dr. Snyder.]

Dr. Wolverton began by addressing Dr. Kraft's "future plans of defining an analysis at the community level" and his "confidence about drilling down to the community level" because of the potential consistency between that and the state-level analysis. Mentioning that there has been a lot of recent discussion in the literature "about how results change all the time depending on the way in which you define the community," She asked Dr. Kraft how he planned to define "community" in his future research.

The second question addressed to Dr. Kraft asked whether the trend of production-related waste increasing over time "holds true when you normalize by production."

Michael Kraft (University of Wisconsin at Green Bay)
Dr. Kraft said he probably couldn't answer the second question but would pass it along to someone who could. Although he didn't believe they had normalized for that, he asserted, "I think there's still a trend there—it may be altered with normalization."

On the other hand, he said, "The first question is something that we've certainly thought about. What the question is getting at is: We're looking at state-level comparisons here, and we've said that gives us confidence that there is some kind of political factor at the state level, whether it's ideology, conservation group memberships, . . . We're only interested in how that plays out around the facility, because the whole point of the information release is that *communities*—and that's the language that's used, *communities*—take *cognizance* of this information, and, of course, some action follows. Of course, *state* groups might also take an interest in what's going on in the community, and that may be true of environmental justice groups at the *state* level or environmental groups more broadly even."

Dr. Kraft continued by saying that the community identification issue is complicated by the fact that "we're looking at *facilities*, and then we're looking at communities in which they are located." He suggested that in different circumstances "community" could mean "the city," "the county," or "the immediate two miles around a facility." Dr. Kraft went on to say, "I think we're inclined to take a broader view of surrounding community. That may get a *little* messy in a larger urban area—exactly where the boundaries ought to be," but he indicated that the risk screening model might help in that regard. Citing the successful use of this model in working with Region 5 air emissions, he explained that "it actually models the plume that would follow a factory, and you can identify exactly which neighborhoods are affected by a given facility. So, it isn't just geographic—for example, "x" blocks or miles away—you can get very specific. That's something we would certainly like to look carefully at to know what groups, what people, what citizens, what community leaders we need to talk to, so that's a *big* part of the next stage of the project."

Ann Wolverton's comments addressed to Lori Snyder:
Dr. Wolverton commented, "I was just curious about whether the same firms that are reporting in the U.S. EPA's TRI are also in the Massachusetts TRI and whether there is a way to make use of that. It may be that there's not a lot of intersection—I was just curious to know."

The second comment that Dr. Wolverton addressed to Dr. Snyder was: "You're focusing on the pounds of emissions and sort of what's going on with the threshold and how much you're *missing* in terms of *pounds.* I was curious about how much you might be missing if you attempted to translate that into a more risk-based measure?"

Lori Snyder (Harvard University)
In response to the first comment, Dr. Snyder clarified that the TRI reports are actually sort of a subset of what is reported in the other reports. She added that, consequently, one of the things she *could* do "is just look at chemicals that are reportable to TRI," although this could get "a *little* tricky" due to the fact that "sometimes, compound chemicals that are in TRI are defined differently in the State and Federal reports. Another complication would be that she has the "*use* information based on *State* thresholds." Dr. Snyder closed by saying that she did have plans to revisit this and justify her claims.

Elizabeth (Betsy) David (Stratus Consulting, Inc.)
Citing results from some "mid-to-late-90's" interviews of 100 Wisconsin firms, Dr. David said she and her colleagues found "three important reasons why people reduced their pollution," with the primary reason being "to stay in compliance." However, she added that "the *second* reason was that the *firm* perceived that its ranking on the TRI reports was *very important* to them [i.e., communities], so even though at community levels I didn't see much in the way of communities taking action on this information, *firms perceived* that the communities would, and so they changed their behavior." Dr. David commented that since this data was compiled at the same time as Dr. Kraft's data, he should also have gotten indications that "this perception was really a very important determinant of people's actions." Dr. David closed by citing the story of "a firm called Fort Hubbard Steel [in Green Bay, Wisconsin] that came out ranked very high on the TRI list" and subsequently modified its process from etching with acid to grinding (to the dismay of the Green Bay Metropolitan Sewage agency, who had been using the waste acid in their process to precipitate out phosphorous).

Dinah Koehler (University of Pennsylvania)
Dr. Koehler commented on John Dombrowski's assertion that engineering estimates sometimes provide a closer approximation of stack pollution than actual sampling

measurements. Citing her research, Dr. Koehler said “there are *huge* differences between what the National Toxics Inventory (NTI) estimates were at the 4-digit SIC level and the TRI.” She had held hopes of *potentially* using the NTI as a substitute for the TRI, “given that there’s all this noise in the TRI,” but the fact that the difference is sometimes *so* great prevents her from doing so. She wondered whether Mr. Dombrowski could comment on what might be going on there.

John Dombrowski (U.S. EPA, Office of Environmental Innovation)
In response, Mr. Dombrowski said that if one is looking at “releases for *just* stack emissions,” in that case monitoring *might* be the better of the two options. However, he stated, “In the general sense, though, I would say that when you look at fugitive emissions, for example, engineering estimates for the mass balance you’re doing might be better than just occasional monitoring of a fugitive emission, because you can account for wherever the releases may be occurring.” He clarified further, “my statement was in the sense of *all* monitoring data,” and depending on what type of release you’re talking about, one or the other of the methods might be preferable.

Mr. Dombrowski went on to say that he “just felt that people sometimes make an unfair assessment of the TRI data and discredit it because of the estimation that sometimes occurs,” whereas he feels that “you have to look at the data *specifically* as to what you’re using and then make an evaluation of *that*, but not an overall generalization of the TRI data.” He added that “NEI (National Emissions Inventory) sometimes actually supplements their data with TRI data, so we capture sources that they may not be capturing. Sometimes we have captured better air data—with mercury releases, for example, I think we were capturing much more of a complete data set than what the Air Data Set was capturing at that time. So, again, you have to be *very* specific as to what you’re looking at for the data element.”

Randy Becker (U.S. Bureau of the Census)
Dr. Becker claimed that “the best paper was actually saved for last—it sounds like required reading for anyone who wants to use the TRI.” He added that the paper “sets an *excellent* example for all of us, because what’s important is getting the questions right and the econometric specification right—you need to know where the data come from and what their limitations are.” Commenting that “we’re spending a lot of money here on studies that use data to answer questions,” Dr. Becker noted, “just because others use data blindly doesn’t mean all of us have to. Lori’s paper reminded me that we should probably also spend money on studies that evaluate the quality of the data—and probably also spend money on *improving* the quality of the data, as John said.”

Furthermore, Dr. Becker said that in looking at the TRI, “before we chalk everything up to community activism, there’s also regulation running parallel to this. In fact, some of the pollutants in TRI are actually regulated because they’re ozone precursors.” Consequently, in efforts to comply with regulations for cleaning up these precursors or other pollutants, facilities “may be cleaning up TRI and it has nothing to do with community activism whatsoever.” Dr. Becker added that “while there’s no *direct* measure of how much regulation a plant is facing, you could control for whether it is in a county that is in non-attainment of the National Ambient Air Quality Standards. Also, in terms of the conforming community, perhaps more important than TRI is something like ozone action days, which, again . . . may be driving things rather than community activism or just the information disclosure."

Michael Kraft
Dr. Kraft responded that "we do make a comment in the paper to the effect that many facilities clearly do alter their actions based on regulatory pressures or anticipated regulation—community pressure is *not* always the explanation—it's a *multi-faceted* situation. He also clarified that "there's a good bit of literature that suggests regulation is, in fact, a driving force, and maybe a lot more so than community pressure." In closing, Dr. Kraft stated, "TRI is predicated on the notion that the public has the right to know, and we're looking at what's going on in the community in part *because* of that, but that doesn't mean we're going to come to the conclusion that that's *the* most important factor."

Eric Orts (University of Pennsylvania)
Dr. Orts asked the following "quick question that also pertains to the reliability of the TRI data: . . . Have there been enforcement actions taken with respect to companies on whether they reported these data accurately or not? In another life I'm a securities law corporate academic, and certainly in the securities field reliability of information, even if there is enforcement, is sometimes suspect. But, for the most part, it's thought to be pretty good, in part because there's enforcement." Dr. Orts asked whether there are actual examples of the EPA going after companies that have reported bad data, and (because he thinks it's potentially a criminal act) whether a case has ever been referred to the Department of Justice.

John Dombrowski
In response, Mr. Dombrowski admitted that he didn't know whether there have actually been any criminal actions, but "there has been, and there continues to be, enforcement related to TRI data. He expounded by saying, "Every year our Regions do data quality audits, and they look for facilities that are under-reporting. . . . We've had some Regions even take actions on *over*-reporting, because the purpose of TRI is the public's right to know, and we want to provide the most accurate information. You also want to consider a level playing field between the facilities that *are* doing a good job with their data versus those that are doing a not-so-good job and just getting by."

Mr. Dombrowski also added, "Speaking of enforcement, there is currently an enforcement initiative ongoing right now on TRI . . . and you'll see a flyer—an enforcement alert for late reporters. We've done a lot of analysis on facilities that have reported their data late to EPA . . . and we're not talking about a trivial amount of data when you add it all up together on the late reporters. And, there is a statutory deadline for them to report and for us to get the information out to the public, so enforcement *is* quite active in the TRI program in various aspects."

Eric Orts—a follow-up question
"Have there been any cases of a civil penalty that's been assessed against a company because of infractions in this area?"

John Dombrowski
"Yes, there have been. Jon [Silberman], you might know better than I, but I have *heard*—and I don't know exact numbers—but through EPA's audit policy, for example, one of the more frequently disclosed violations by companies is TRI reporting violations."

Matt Clark (U.S. EPA, Office of Research and Development)
Dr. Clark commented, "We had one paper that we really wanted to present today (the author wasn't able to show up) that I would recommend to you—it's by Michael Vasu. He looked at the extent to which communities actually *knew* about TRI, and it turned out to be in the *low teens*. Not only that, more people reported knowing about a *fictitious* database than knew about the TRI."

Dr. Clark also added, "We are trying to get out a solicitation this year on the benefits of environmental information disclosure. It's going to cover a lot of stuff directly related to this."

Sarah Stafford (College of William and Mary)—another question
In a comment "kind of related to this idea of regulatory pressure" Dr. Stafford suggested to Dr. Kraft that "some potential regulations may be correlated with those political variables that you have in there," and she said she is "most concerned about pollution prevention regulations, both mandatory and voluntary." She cited the fact that in 1995 "at least two states had *required* pollution prevention actions" which, though hazardous waste based, "clearly had a big impact on TRI. Dr. Stafford added that some of her work suggested that there "could be some correlations as to which states adopt *voluntary* pollution prevention programs and what types of programs they have related to, specifically, the membership in environmental organizations." In closing, she said, "It's something worth looking at, just in terms of correlation."

Michael Kraft
Dr. Kraft stated, "We're not going to presume *anything* in the way that we word the questionnaire. We're certainly not going to assume that the community *knows* about the TRI . . ., and we're aware, of course, that State regulations factor into the equation."

End of Session VI Q&A