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How Firms Respond to Mandatory Information Disclosure

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HOW FIRMS RESPOND TO MANDATORY INFORMATION DISCLOSURE

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Mandatory information disclosure regulations seek to create institutional pressure to spur performance improvement. By examining how organizational characteristics moderate establishments' responses to a prominent environmental information disclosure program, we provide among the first empirical evidence characterizing heterogeneous responses by those mandated to disclose information. We find particularly rapid improvement among establishments located close to their headquarters and among establishments with proximate siblings, especially when the proximate siblings are in the same industry. Large establishments improve more slowly than small establishments in sparse regions, but both groups improve similarly in dense regions, suggesting that density mitigates the power of large establishments to resist institutional pressures. Finally, privately held firms' establishments outperform those owned by public firms. We highlight implications for institutional theory, managers, and policymakers.

Keywords: information disclosure, institutional theory, environmental strategy, mandatory disclosure, environmental performance.

INTRODUCTION

Organizations respond to pressures from a variety of constituencies and stakeholders. Community members, consumers, investors, and activist groups can exert pressure on firms to curb undesired behaviors. In some cases, mandatory disclosure programs, a market-based form of regulation, provide information that fuels such pressure. Recent years have seen a significant increase in the use of information disclosure as a regulatory mechanism; for example, forcing manufacturers to reveal details of the pollution they generate, food producers to include nutritional information on product labels, and restaurants to post kitchen hygiene ratings.

Most research examining the effectiveness of information disclosure has focused on aspects of disclosure programs that influence their success (Weil *et al*., 2006; Fung *et al*., 2007), and either average effects on how effects vary based on firms' external environment (Delmas and Toffel, 2012). We theorize that characteristics of establishments and their broader organizations, as well as how these interact with their institutional environment, influence how establishments react to information disclosure requirements.[1] In doing so, we extend both the literature that evaluates the effectiveness of information disclosure programs (e.g., Fung, Graham, and Weil, 2007; Jin and Leslie, 2009) and institutional theory research that examines heterogeneous responses to institutional pressures (Oliver, 1991; Delmas and Toffel, 2008).

We hypothesize that greater improvement will be seen in establishments subject to greater internal and external pressure to improve and in those with greater access to the necessary capabilities. We test our hypotheses using data from one of the most famous instances of information disclosure regulation, the Toxics Release Inventory (TRI), often credited with eliciting significant improvements in environmental performance (Hart, 2010). We examine how thousands of establishments have responded to this regulatory requirement to publicly disclose emissions of hundreds of toxic chemicals and we exploit an exogenous shock that occurred when the list of reportable chemicals was expanded. Our research is especially important given the prominence of the TRI program, the largely untested faith in the power of disclosure and transparency to alter firm behavior, and the societal benefits associated with improved environmental performance.

Our examination of the differential environmental performance improvement of establishments is based on five organizational moderators: size; proximity to headquarters; proximity to corporate siblings; industry overlap among proximate siblings; and ownership structure (i.e., whether the parent firm is publicly traded or privately held). Using emissions reductions as our performance indicator, we find that

[1] Information disclosure programs often target single units of a firm, such as individual restaurants or production facilities. In this paper, we use the terms 'establishment' and 'facility' to refer to these units and use 'firm' or 'organization' to refer to the larger entity to which such establishments belong.

establishments close to their headquarters outperform those with headquarters farther away. We also find that establishments with proximate siblings outperform those with siblings that are not proximate and that those with proximate siblings in the same industry are especially likely to improve. Large establishments improve more slowly than small establishments in sparse regions, but both groups improve similarly in dense regions, suggesting that density mitigates the power of large establishments to resist institutional pressures. Finally, establishments owned by privately held firms outperform those owned by publicly traded firms.

RELATED LITERATURE

Our research relates to two streams of literature: studies that examine how organizations and their stakeholders respond to mandatory information disclosure policies, and neo-institutional research that explores how newly disclosed information becomes a form of institutional pressure to which organizations are expected to respond.

Responses to information disclosure

Mandatory information disclosure policies are premised on the notion that requiring an organization to reveal information will induce stakeholder pressure that will prompt the organization to improve along the disclosed dimension (Greenstone, Oyer, and Vissing-Jorgensen, 2006; Weil *et al.,* 2006). Much of the literature examining the effects of information disclosure has focused on how stakeholders such as journalists, investors, customers, and regulators respond to information disclosed about an organization. For example, information disclosed about organizations' pollution levels has stimulated media coverage and depressed market valuations both of the organizations (Hamilton, 1995) and of neighboring homes (Oberholzer-Gee and Mitsunari, 2006). Mandatory disclosure of nutrition information has led customers to reduce caloric consumption, especially in areas populated by wealthier, more highly educated consumers (Bollinger, Leslie, and Sorensen, 2011).

More closely related to our research are studies that investigated how organizations respond to information disclosed about them. Several studies have concluded that government programs requiring information disclosure have spurred companies to improve their environmental performance (Blackman, Afsah, and Ratunanda, 2004; Konar and Cohen, 1997; Scorse, 2010), food and water safety (Bennear and Olmstead, 2008; Jin and Leslie, 2003), and surgical outcomes (Cutler, Huckman, and Landrum, 2004; Hannan *et al.*, 1994; Peterson *et al.*, 1998). Some have found that performance improvement was especially pronounced among organizations whose initial disclosure performance was below average in environmental performance (Blackman *et al.*, 2004; Chatterji and Toffel, 2010; Scorse, 2010), restaurant hygiene (Jin and Leslie, 2009), and graduate school ranking (Elsbach and Kramer, 1996; Espeland and Sauder, 2007).

Institutional theory

Institutional theory provides a basis for studying how external pressures affect organizational behavior. In this context, mandatory information disclosure programs provide material with which stakeholders can pressure firms to improve along the metrics of the information disclosed. Our work relates to studies of firms' reactions to institutional pressures exerted by regulators (Henriques and Sadorsky, 1996; Khanna and Anton, 2002; Reid and Toffel, 2009), local communities (Florida and Davison, 2001; Henriques and Sadorsky, 1996), customers (Delmas and Montiel, 2008), competitors (Darnall, 2009), and shareholders (Reid and Toffel, 2009). But whereas these studies tend to focus on average organizational responses, our work examines heterogeneous responses and seeks to respond to a 'lack of understanding of the conditions under which institutional pressures and organizational characteristics explain the adoption of beyond compliance strategies' (Delmas and Toffel, 2012: 231). Recent studies have found evidence that an organization's responses to institutional pressures are moderated by its structure (Delmas and Toffel, 2008; Okhmatovskiy and David, 2011), location (Lounsbury, 2007), and the marginal cost and perceived benefits of responding (Chatterji and Toffel, 2010). By theorizing and empirically testing hypotheses that certain organizational characteristics moderate how organizations respond to institutional pressure, we contribute to the nascent literature that examines heterogeneous responses to institutional pressures.

THEORY AND HYPOTHESES

Information disclosure, pressure, and performance

For firms, as for individuals, the effectiveness of information disclosure in changing their behavior hinges on the perceived costs and benefits of the changes (Chatterji and Toffel, 2010; Fung *et al.*, 2007; Jin and Leslie, 2009). Chatterji and Toffel (2010) find that, among firms receiving poor environmental ratings, those in environmentally sensitive industries are especially likely to improve performance, given their heightened scrutiny and potential to be inspected. This scrutiny is the lever that disclosure programs utilize in order to 'reduce specific risks or performance problems' (Fung *et al.*, 2007: 5). That is, information disclosure, whether mandated by government or promulgated by private parties such as ratings agencies, is intended to shine a light on previously hidden dimensions of performance with the intention of spurring improvement.

We suggest that information disclosure is more likely to lead to improved performance among establishments that attract particularly salient pressures from internal or external stakeholders and that have preferential access to intra-organizational expertise. That is, we argue that particular characteristics of an establishment and its relationship with the rest of its firm affect its performance following information disclosure.

The role of internal pressure and ease of capability transfer in performance improvement

Information that reveals poor performance can harm an establishment's reputation, as has been shown for firms (Chatterji and Toffel, 2010), restaurants (Jin and Leslie, 2009), educational institutions (Elsbach and Kramer, 1996; Espeland and Sauder, 2007), and factories (King and Lenox, 2002). Such information can also impugn the establishment's parent organization and sibling establishments. Poor performance revealed by information disclosure requirements can harm organizations' reputations and stock prices

(Hamilton, 1995; Konar and Cohen, 1997) and can therefore prompt investment in improved procedures (including staff training and internal monitoring) and capital equipment aimed at improving performance (Chatterji and Toffel, 2010). We consider how disclosure of an establishment's performance is especially likely to spur improvement when the establishment is more firmly embedded in its community due to the nearness of its firm's headquarters or of a sibling establishment.

Local embeddedness

Because firms tend to be particularly embedded in the communities in which they are headquartered (Marquis and Battilana, 2009), a firm's headquarters and its nearby affiliated establishments are especially motivated to preserve their relationships with their community. Accordingly, research has found that establishments near headquarters are less likely to lay off workers (Greenwood *et al.*, 2010) and more likely to source from local firms (Audia and Rider, 2010). We argue, therefore, that establishments located in their headquarters' communities will be particularly responsive to stakeholder pressures. Proximity to headquarters also magnifies internal pressure on establishments revealed to be performing poorly; the disclosed information is particularly visible and salient to top management, which has both the incentive and the authority to press for improved performance. Proximity also helps headquarters to both monitor and assist such establishments. To summarize, proximity to headquarters is likely to magnify pressure from external and internal stakeholders following information disclosure, which in turn increases the intensity of the establishment's response, yielding a superior performance trend on the disclosed metric.

> **H1: Following mandatory information disclosure, establishments proximate to their headquarters will improve relative to those not near their headquarters.**

Proximity to sibling establishments

Proximity to other establishments owned by the same parent can also lead poorly performing establishments to improve more quickly, due to embeddedness, reputational spillovers, and capability transfers. We elaborate on these mechanisms below.

Establishments with proximate siblings, like those with proximate headquarters, are more embedded in their community than a single establishment would be. Collectively, the siblings employ more people and more community members live near their plants and thus have a personal interest in the establishments' performance. Information about one establishment, whether positive or negative, can affect the reputation of its siblings (Carney and Gedajlovic, 1991), especially those in the same area (Jin and Leslie, 2009). Managers of establishments with proximate siblings will therefore be particularly responsive to stakeholder pressures and poorly performing establishments with proximate siblings can experience strong internal pressure to improve.

Proximity to a sibling can also facilitate the transfer of capabilities needed to improve performance. While proximity does not guarantee such a transfer, it does create a greater opportunity when the ability and willingness to transfer knowledge are there (Argote, McEvily, and Reagans, 2003). Geographical proximity can enhance social relationships (Boschma, 2005), which promotes knowledge sharing (Szulanski, 1996). Conversely, the face-to-face communication that facilitates knowledge transfer (Darr, Argote, and Epple, 1995) becomes more costly and difficult with distance (Lafontaine and Slade, 2007; Berchicci, Dowell, and King, 2011).

H2: Following mandatory information disclosure, establishments proximate to corporate siblings will improve relative to those not near corporate siblings.

Capability transfer

Proximity enhances the potential for capability transfer, but does not guarantee it. While prior studies have demonstrated that knowledge can be transferred more easily within a firm than across firms (Darr, Argote, and Epple, 1995), there remain significant barriers even to transfers within a firm (Szulanski, 1996). In particular, transferring capabilities and knowledge between establishments is more difficult when the establishments have different operating procedures (Maritan and Brush, 2003; Szulanski, 1996). We therefore suggest that capability transfers among proximate siblings are particularly likely to lead to improvement in disclosed performance when those establishments are in the same industry and thus more likely to have similar production processes and to have common suppliers and customer demands

(Capron, Mitchell, and Swaminathan, 2001). This increases both the relevance of the knowledge one sibling establishment has to offer and the ability of the other sibling to absorb that knowledge.

Information disclosure can create the impetus for capability transfer. Even when there is a proximate sibling in the same industry, significant barriers to transferring capabilities often result in persistent performance differences between the establishments (Chew, Bresnahan, and Clark, 1990; O'Dell and Grayson, 1998; Szulanski, 1996). In normal operating conditions, these barriers can impede the transfer of capabilities to where they are needed most (Berchicci *et al.*, 2011). Disclosure of an establishment's poor performance, however, may increase the incentives to overcome the barriers and transfer the capabilities. Thus, to the extent that intra-organizational capability transfers are activated following disclosure, greater performance improvement will be observed among establishments that have a proximate sibling in the same industry.

> **H3: Following mandatory information disclosure, establishments proximate to same-industry corporate siblings will improve relative to establishments proximate to different-industry corporate siblings.**

The role of external pressure in performance improvement

Organizational characteristics can also affect the salience of external pressures. We hypothesize that size and ownership structure will moderate the responses of establishments that face common pressures stemming from information disclosure. Below, we theorize that an establishment's relative size within its region affects the pressure put on it and thus its response. We further propose that establishments owned by publicly traded firms face different pressures than establishments owned by private firms face, which lead to different responses. For each of these organizational characteristics, we offer competing theories that predict opposing moderating effects.

Establishment size and regional density

Although theory suggests that size is likely to moderate an establishment's sensitivity to external pressures, whether it promotes or inhibits such sensitivity remains ambiguous. Some institutional theorists, for example, have argued that larger establishments' greater visibility makes them especially anxious to maintain legitimacy (Goodstein, 1994; Ingram and Simons, 1995). Their greater visibility in their communities also makes them more likely to attract media attention (Ingram and Simons, 1995) and to be held to higher standards than smaller establishments (Goodstein, 1994). This suggests that larger establishments would be particularly sensitive to external pressures occasioned by disclosure.

Larger establishments may, conversely, be *less* sensitive to local pressure generated by disclosure. Larger establishments can accrue power through superior political access and can more easily afford to lobby or donate to politicians and to sue regulatory agencies (Drope and Hansen, 2006; Hansen, Mitchell, and Drope, 2005; Schuler, 1996). Thus, though they cannot avoid the information disclosure requirements, larger establishments may be able to insulate themselves from the resulting pressures. Greater political and social capital may also accrue to larger establishments because they provide greater employment opportunities. In addition, larger firms may make greater investments in resources that insulate them from competition and therefore be less likely to invest in performance improvements (Madsen and Walker, 2007). To the extent that larger establishments are more powerful and thus less sensitive to local pressure groups, they should be expected to be better able to resist external pressures and to show less improvement in the wake of disclosure (Grant, Bergesen, and Jones, 2002).

We propose that these competing predictions can be reconciled by considering the influence of an establishment's economic, social, and political power in the region from which the pressure emanates. A large number of establishments in a given area dilutes any given establishment's potential power there, rendering it more vulnerable to pressure exerted by local regulators and concerned groups. For example, a 1,000-employee establishment that accounts for only a tiny fraction of a region's economic activity has less power than an establishment of similar size that accounts for a large proportion of the region's economic activity. A large establishment in a sparse region will likely be far better able to leverage its contributions to community tax revenues and employment (e.g., Boal and Ransom, 1997), its membership in the local elite, and its influence with local regulators (Marquis and Battalina, 2009).

These arguments suggest the following mechanism by which the number of other establishments in a region moderates the effect of establishment size on performance improvement following information disclosure. As implied above, we refer to regions with many establishments as 'dense' and regions with few establishments as sparse.[2] We expect the relative improvement of large establishments over small establishments to be greater in dense regions (where their power to resist community pressures is attenuated) than in sparse regions (where their political power is stronger).[3] Put another way: Size matters, but even more so the sparser the region.

> **H4: Following mandatory information disclosure, regional density moderates the relationship between organizational size and improvement. Whereas larger establishments will demonstrate less improvement than smaller establishments in sparse regions, this gap will be attenuated in dense regions.**

Public ownership

Following disclosure, there are several reasons why performance improvement can be expected to be greater among establishments owned by publicly traded firms than among establishments owned by privately held firms. First, publicly traded firms are accustomed to reporting a wide range of information about their operations and are accountable to a greater number of audiences (Fischer and Pollock, 2004; Mascarenhas, 1989). This increased degree of transparency and accountability may make the public firms particularly sensitive to the effects of additional information disclosure. Second, publicly traded firms are vulnerable to investors who seek to influence management decisions through publicity-generating shareholder resolutions and other mechanisms, whereas private firms seldom encounter activist investors (David, Bloom, and Hillman, 2007; Gillan and Starks, 2000; Reid and Toffel, 2009). Third, disclosed information can affect the stock price of publicly traded firms (Konar and Cohen, 1997), which can lead corporate managers to increase pressure on subsidiary establishments to improve performance.

> **H5a: Following mandatory information disclosure, establishments owned by publicly traded firms will improve relative to those owned by privately held firms.**

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[2] In our empirical analysis, we measure dense and sparse regions based on establishment count per city. We also conduct robustness tests in which we broaden the geographic domain to the county level and others in which we define dense and sparse in terms of employment rather than establishment count.

[3] Foreshadowing our empirical approach, we analyze each of these two difference-in-differences, which compare performance trends of large establishments to small establishments in sparse regions and separately in dense regions, then compare the trend differences estimated by the two regressions.

Conversely, since it is often the case that shareholders in public firms are principally concerned with share price, those firms may be less sensitive than private firms to mandatory disclosure of performance information. Operational improvements (e.g., capital equipment, development of new capabilities) often require significant investments that have long payback periods and may not be profitable even in the long run (Christmann, 2000; King and Lenox, 2002). We expect publicly traded firms under pressure to maintain short-term profits and stock prices to be less likely to make investments in operational performance improvements when the financial returns are unclear or occur only over long time horizons (Fischer and Pollock, 2004). Private owners, on the other hand, can emphasize nonfinancial objectives. To the extent that these owners identify strongly with their firms, they may be more sensitive to institutional pressures such as those that follow from information disclosure (Berrone *et al.*, 2010). Private firm owners are likely to have significant portions of their own wealth concentrated in their firms (Moskowitz and Vissing-Jørgensen, 2002), which also increases the likelihood that they will make long-term investments to secure the firms' survival (Schulze *et al.*, 2001).

H5b: Following mandatory information disclosure, establishments owned by privately held firms will improve relative to those owned by publicly traded firms.

DATA AND MEASURES

Empirical context and sample

We empirically test our hypotheses by taking advantage of a policy change that occurred when the U.S. Environmental Protection Agency (EPA) expanded the scope of the Toxics Release Inventory (TRI). The U.S. Emergency Planning and Community Right-to-Know Act of 1986 created the TRI, which requires establishments to report—publicly and annually—waste, transfers, and releases of certain toxic chemicals. An establishment is required to report if it (1) operates within particular industry sectors, including manufacturing, mining, electric utilities, hazardous waste treatment, and chemical distribution, (2) employs ten or more people, and (3) manufactures, imports, processes, or otherwise uses any of the listed toxic chemicals in amounts that exceed reporting thresholds (U.S. Environmental Protection Agency, 2004). TRI-reporting establishments must provide their location, industry classification, and parent company as well as data about each qualifying chemical, including the pounds of each that were emitted to air, water, land, and underground injection; processed through on-site waste treatment; and transferred offsite for treatment or recycling (U.S. Environmental Protection Agency, 2011). This information is available to the public on the EPA's website (www.epa.gov/tri).

Since the TRI became operational in 1987, the EPA has periodically expanded the list of chemicals to be reported. We leverage this fact in our identification strategy, as described below. As of 2011, the EPA required disclosure of 593 individual chemicals in 30 chemical categories (U.S. Environmental Protection Agency, 2011). To construct our database, we supplement establishments' annual TRI reports with Dun and Bradstreet data obtained from the National Establishment Time-Series (NETS) database, as described below. Our resulting panel dataset consists of 38,175 establishments over the years 1995 to 2000 (217,575 establishment-years), the six-year period that followed the EPA's largest expansion of the TRI chemical list.

Dependent variable

We measure environmental performance based on toxic chemical emissions data from the TRI database, a widely used approach (e.g., Berrone *et al.*, 2010; Bui and Kapon, 2012; Chatterji and Toffel, 2010; Gamper-Rabindran, 2006; King and Lenox, 2000; King and Shaver, 2001; Toffel and Marshall, 2004). In November 1994, the EPA added 243 toxic chemicals to the 363 already required to be reported, effective in 1995 (U.S. Environmental Protection Agency, 1994). Our outcome measure is *log releases* of these 243 chemicals; it includes the total pounds each firm reported to the TRI as production waste, transfers offsite, and emissions. We obtained TRI data from the EPA's Risk-Screening Environmental Indicators (RSEI) Model (versions 2.1.2 and 2.1.3) (U.S. Environmental Protection Agency, 2010). Our models use the log of these annual values after adding one. Whereas some studies apply various weights to these chemicals to account for differences in toxicity, simply summing the pounds of emissions was a commonly used approach by the media and prominent nonprofit organizations and in government publications during the sample period (Toffel and Marshall, 2004) as well as by academic studies examining institutional pressure and responses to TRI releases (e.g., Chatterji and Toffel, 2010; Dooley and Fryxell, 1999; Feldman, Soyka, and Ameer, 1997; Konar and Cohen, 2001).

Moderators

Headquarters proximity

We measure proximity to headquarters as a dichotomous variable, *proximate headquarters*, coded '1' for establishments located in the same city as their headquarters and '0' otherwise. We obtained establishment addresses from the TRI database and headquarters addresses from the NETS database. To cleanly identify the effect of the 1995 policy change on firm behavior, we pursue the customary practice of measuring the hypothesized establishment-level characteristics fixed at their value in 1994, just prior to the policy change. In the absence of a 1994 value, we use an establishment's 1993 value.[4] We use this practice for all hypothesized moderators described below, but all our results are very similar when we use time-variant moderators, as described in the robustness tests section below.

Sibling proximity

Our measure of the extent to which an establishment's poor performance might impugn the reputation of other establishments in its corporate family is based on whether there are any TRI-reporting sibling establishments in the same city.[5] We created *proximate sibling* as a dichotomous, establishment-level, time-invariant variable coded '1' for establishments with at least one sibling in the same city in 1994 and '0' otherwise. We obtained the identities and addresses of each establishment's siblings from the NETS database.

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[4] Our results were substantively similar when headquarters proximity is measured as sharing the same three-digit ZIP code or the same state (see Columns 1–2 of Table B1 in Appendix B in the online Appendices).

[5] Our results were substantively similar when sibling proximity is measured as sharing the same three-digit ZIP code (Column 3 of Table B1 in Appendix B). Relying on a broader geographic definition of proximity (same county) also yields a negative coefficient, but one that is half the magnitude and not statistically significant (Column 4 of Table B1 in Appendix B: $\beta = -0.008$, $p < 0.29$). These results begin to sketch the boundaries that limit the geographic scope of reputation spillovers.

Sibling proximity and industry similarity

We created two dichotomous variables to indicate whether any of the focal establishment's proximate siblings were in the same industry. We coded *proximate same-industry sibling* as '1' if at least one proximate (same-city) sibling operated in the same industry (two-digit Standard Industrial Classification [SIC] code) and '0' otherwise. Similarly, we coded *proximate different-industry sibling* as '1' if at least one proximate (same-city) sibling operated in a different industry (two-digit SIC code) and '0' otherwise. [6]

Large establishments and regional density

In H4, we predict that the power of large establishments is exacerbated in sparse regions, where such organizations are more salient, and attenuated in dense regions. As such, we measure organizational power via establishment size and salience via regional density. To identify the organizations most likely to possess the power to influence state regulatory agencies, we created *large establishment* as a dichotomous, time-invariant, establishment-level variable coded '1' if an establishment's employment in 1994 exceeded the median employment of all TRI-reporting establishments in the same state that year and '0' otherwise. We obtained employment levels from NETS. Though our threshold differs by state, national averages for large and small establishments are 406 and 36 employees, respectively. We distinguish the salience of large employers by considering regional density. We distinguish sparse from dense regions based on the density of TRI-reporting establishments in a city. Specifically, we define a *sparse city* as a city with no more TRI-reporting establishments in 1994 than the median number of 10.5 for all U.S. cities that year; a *dense city* is defined as a city with more TRI-reporting establishments in 1994 than the median city.[7]

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[6] Our results are similar when we adopt a narrower definition of same-industry—sharing a three-digit SIC code (Column 1 of Table B2 in Appendix B)—and when we adopt a broader definition of geographic proximity—sharing a three-digit ZIP code or being in the same county (Columns 2–3 of Table B2).

[7] As described below, our results are robust to alternative definitions of regional density, including a dummy variable based on the number of establishments per county and a continuous measure based on the number of establishments per city.

Public ownership

We created a dichotomous, time-invariant, establishment-level variable, *public ownership,* coded '1' if, in 1994, an establishment was owned by a publicly traded firm and '0' if owned by a privately owned firm, based on data from the NETS database.

Controls

We obtained control variables to account for the possibility that establishments' emissions are influenced by its historic performance, changes in its production levels, its industry, and its local community.

Historical performance trends

Our analysis examines the environmental performance trends of toxic chemical emissions in the first few years after public disclosure was required. One concern is that the performance trends that became publicly observable with the new disclosure requirements had already been occurring, but were known only to the establishments. While we cannot observe earlier trends for the 243 chemicals added to the TRI in 1995, we can observe them for the original 363 TRI chemicals whose reporting had been required before the 1995 policy change. We calculated each establishment's *historical releases trend* based on the other toxic chemicals that were required to be reported from 1991 to 1994, the years immediately preceding the 1995 expansion. Specifically, for each establishment *i*, we calculated a percent change metric that is robust to outliers, using the following equation that takes the difference between emissions averaged over 1993 and 1994 (denoted $_{1993–1994}$) and the emissions averaged over 1991 and 1992 (denoted $_{1991–1992}$):

$$\textit{historical releases trend}_i = (\textit{average log releases}_{i,1993–1994} - \textit{average log releases}_{i,1991–1992}) \,/\, (0.5\times \textit{average log releases}_{i,1993–1994} + 0.5\times \textit{average log releases}_{i,1991–1992})$$

By construction, this metric allays concerns arising from outliers by limiting the range from –2 to +2.

To control for the influence of historical performance trends (1991–1994) on establishments' subsequent performance trends (1995 and after), we add to our model *historical releases trend* as well as the interaction between *historical releases trend* and an *annual counter* (defined below).[8]

Establishment size

We control for changes in establishment size in two ways. We obtained establishments' annual employment (Aravind and Christmann, 2011; Chatterji and Toffel, 2010; Diestre and Rajagopalan, 2011; King and Lenox, 2000; King, Lenox, and Terlaak, 2005; Potoski and Prakash, 2005; Russo and Harrison, 2005) from NETS. Mean employment of the establishments in our sample is 227 (SD = 589). To reduce skew, we used *log employment* in our models.

We also controlled for changes in production volume by obtaining annual production ratios (i.e., the ratio of an establishment's production level in a given year to its production level the prior year) from the TRI database (Berrone and Gomez-Mejia, 2009; Lenox and King, 2004; Scorse, 2010; Terlaak and King, 2006). Establishments are required to provide an annual production ratio for each chemical reported to the TRI database. For establishments that reported multiple production ratios in a given year (e.g., for different production lines in a plant), we used the median value for the establishment-year. Across the distribution of median annual establishment production ratios, we winsorized at the 5th and 95th percentiles to avoid undue influence from outliers. We then linearly interpolated missing interior production ratio values to arrive at $production\ ratio_{i,t}$ for establishment *i* in year *t*. We normalized relative production $ratio_{i,1994}$ to '1' and calculated $relative\ production\ level_{i,t}$ for each subsequent year for establishment *i* in year *t* as follows:

$$relative\ production\ level_{i,t} = \prod_{y=1995}^{t} (production\ ratio_{i,y})(production\ ratio_{i,y})$$

[8] Because *historical releases trend* was sometimes undefined, owing to an establishment's total releases throughout 1991–1994 being below the reporting threshold or actually zero, we recoded missing values to zero and created a dummy variable to indicate these instances of recoding. We included in the regression both this dummy variable and its interaction with the *annual counter*.

In our regressions, we include the log of *relative production level* to match our log dependent variable and a dummy variable coded '1' if an observation's value was based on an interpolated production ratio and '0' otherwise.[9]

Local environmental preferences

Some prior studies have highlighted the influence of community environmental pressures on establishments' environmental management practices (e.g., Delmas and Toffel, 2008; Hamilton, 1999). Like others, we measure community environmental pressures using the League of Conservation Voters (LCV) National Environmental Scorecard, which calculates the proportion of environmental bills favored by each member of the U.S. Congress and ranges from 0 to 100 percent. We use the *Congressional district LCV score*, which captures the voting record of the U.S. House of Representative member from the establishment's Congressional district.

Industry

Toxic chemical emissions being, in part, a function of industry activities (Berrone *et al.*, 2010; Diestre and Rajagopalan, 2011; King and Lenox, 2000; Potoski and Prakash, 2005), we control for differences between industries by including a full set of industry dummies based on two-digit SIC codes, using NETS data. To facilitate model convergence, we collapsed relatively rare SIC codes—those with fewer than 100 establishment-year observations in each of our samples—into a single 'other' category.

Summary statistics and correlations are reported in Table 1. The distribution of industries in our sample is reported in Table A1 in Appendix A the online Appendices.

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Insert Table 1 here

[9] Including *relative production level* in our models provides an additional, albeit incomplete, way to control for changes in establishment size, augmenting annual employment used by other scholars (e.g., Grant and Jones, 2003; Klassen and Whybark, 1999; King and Lenox, 2000; Russo, 2009). However, after interpolating 2.8 percent of production ratio values, 62 percent of the *relative production level* values remained missing due to widespread missing values of production ratios. While we followed standard practice of recoding those missing values to '0' and adding to our models a dummy variable indicating such recoding, the high number of recoded missing values nonetheless led us to also estimate alternative models that omitted relative production values. The results of these models that relied entirely on log employment to control for changes in establishment size (not shown) are very similar to and corroborate our primary findings.

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EMPIRICAL APPROACH

Model specification

Establishments are required to report to the EPA only those toxic chemical emissions that exceed specific TRI reporting thresholds. As a result, our dependent variable, *log releases,* is missing (left-censored) in years during which an establishment's emissions fall below the thresholds. Prior research indicates that establishments for which emissions dip below reporting thresholds generally continue to use these chemicals, which suggests that there are unreported emissions and that treating such missing values as zero would likely result in biased estimates (Bennear, 2008). We therefore estimate an interval regression whereby left-censored observations of the latent dependent variable, $Y^*_{i,t}$, are specified to range between 0 and the most recent level of *log releases* reported by establishment *i* prior to year *t*. If the establishment has no prior reported level, we set the top end of the range to the minimum positive value of *log releases* reported by other establishments that year.[10] We estimate the following model for establishment *i* in year *t*:

$$Y^*_{i,t} = \beta_1(M_i \times \gamma_t) + \beta_2 M_i + \beta_3\gamma_t + \beta_4(H_i \times \gamma_t) + \beta_5 H_i + \beta_6 X_{i,t} + \varepsilon_{i,t}$$

M_i represents the time-invariant moderator described above, γ_t is an *annual counter* (0 in 1995, 1 in 1996, and so on) that captures the secular trend, H_i represents the *historical release trend*, and $X_{i,t}$ represents the remaining control variables (*log employment*, *log relative production level*, *production ratio interpolated, Congressional district LCV score,* and industry dummies). This model estimates comparative trends because it includes (1) an annual counter, γ_t, that estimates the secular trend of the

[10] Employing interval regression enables us to leverage the insights of Bennear (2008), who found that, among instances in which establishments stop reporting a chemical to TRI, nearly two-thirds of the time they continued to use the chemical, but in quantities below the reporting threshold. As a robustness test, we also used an alternative coding of our dependent variable: For all missing values, we coded the top end of the range to the minimum positive value of *log releases* reported by other establishments that year. The results were nearly identical. As an additional robustness test described below, our OLS estimates with establishment-level fixed effects replicates Bennear's (2008) approach to generating lower bound estimates by assuming that when an establishment ceases reporting, the true value of its releases is indeed zero.

comparison group and (2) an interaction term between the counter and the moderator, which estimates the incremental secular trend of the moderator group (Campbell and Stanley, 1963: 11; Lewis-Beck and Alford, 1980). We rely on the magnitude and statistical significance of the coefficient on this interaction term, β_1, to identify whether the moderator group's trend differs significantly from the comparison group's trend, which is estimated via the coefficient on the main counter variable, β_3.

Identification

Our identification strategy relies on the exogenous policy shock that occurred in 1995 when the EPA increased the number of chemicals required to be reported to the TRI from 363 to 606. Our analysis compares how various types of establishment responded to this. Specifically, we compare performance trends during a six-year period from in 1995, the year the new chemicals were added, to 2000.

Hypotheses 2 and 3 predict behaviors based on the relationship between a focal establishment and its siblings. To sharpen the identification, our empirical analysis of these hypotheses is based on a sample restricted to establishments with at least one TRI-reporting sibling. This enables us, when testing H2, to compare the behavior of establishments with proximate siblings to that of establishments with non-proximate siblings and, when testing H3, to compare performance trends between (a) establishments with intra-firm access to knowledge and capabilities from proximate siblings in the same industry and (b) establishments with proximate siblings in different industries.

To estimate how density moderates the behavior of large organizations (Hypothesis 4), we first examine whether in sparse cities the performance trend of large establishments lags that of small establishments. We then compare the performance trends of large to small establishments in dense cities. Finally, we use seemingly unrelated regression to test whether the gap in performance trends between large and small establishments in the sparse cities exceeds the corresponding gap in dense cities. Comparing these two difference-in-differences estimates is akin to a triple-difference approach (i.e., differences-in-differences-in-differences), which has been used in many domains to facilitate comparisons across two groups in two different contexts (e.g., Basker and Noel, 2009; Costa and Kahn, 2000; Currie *et al.*, 2009; Gruber, 1994).

RESULTS

We estimate our models using interval regression and include each interaction term in a separate regression model. In all cases, we report standard errors clustered by establishment and include dummy variables coded '1' if *relative production level, log employment*, or *Congressional district LCV score* was recoded from missing to zero and coded '0' otherwise (Greene, 2008: 62; Maddala, 1977: 202). This approach, common in econometric analysis, is algebraically equivalent to recoding missing values with the variable's mean (Greene, 2008: 62). Our primary results are reported in Table 2, of which Column 1 presents the results of a regression including only the control variables as a baseline reference.[11]

****
Insert Table 2 here
****

Headquarters proximity

The model displayed in Column 2 includes the interaction between the secular trend and *proximate headquarters*. The results indicate that establishments in the same city as their headquarters exhibited a superior environmental performance trend compared to establishments with more distant headquarters ($\beta = -0.051$, $p < 0.01$), which supports Hypothesis 1. This interaction-term coefficient represents the average annual difference in emissions trends between these two groups of establishments. To interpret the magnitude of this effect, we note the positive annual trend among establishments with headquarters in a different city (the baseline group) ($\beta = 0.087$, $p < 0.01$), which equates to an average increase of 0.52 log points over our six-year sample period (1995–2000) (calculated as 0.087*6) and an increase of 59 percent beyond the *log releases* sample mean of 0.88 log points. In contrast, the average headquarters-proximate establishment increased *log releases* by just 0.22 log points (calculated as [0.087–0.051]*6), 25 percent of

[11] Results are also presented graphically in Figures A1 to A4 in the online Appendix A.

the sample mean. Given that the latter constitutes less than half the growth rate of the former, we conclude that this statistically significant difference is also a substantial one.[12]

Reputation spillover

To analyze the effects of a proximate sibling, we restrict the sample to establishments with at least one TRI-reporting corporate sibling and control for the number of siblings (fixed at their 1994 values, then logged). The results in Column 3 reveal that establishments with proximate siblings exhibit a superior performance trend compared to establishments with non-proximate siblings ($\beta = -0.020$, $p < 0.02$), which supports Hypothesis 2. The average annual trend among establishments with non-proximate siblings ($\beta = 0.091$, $p < 0.01$) implies a total 0.55-log-point increase over the six-year sample period. In comparison, establishments with proximate siblings increased *log releases* by 0.43 log points, a 22 percent lower growth rate.

Capabilities transfer

To test Hypothesis 3, we restrict the sample to establishments with at least one TRI-reporting corporate sibling and control for the (log) number of siblings in 1994 (as we did earlier to estimate the overall proximity effect). The results reported in Column 4 indicate that the performance improvement among establishments with proximate same-industry siblings ($\beta = -0.036$, $p < 0.01$) significantly outpaces that of establishments with proximate different-industry siblings ($\beta = 0.023$, $p < 0.17$; Wald χ^2 comparing coefficients = 9.12, $p < 0.01$), which supports Hypothesis 3. Establishments with proximate same-industry siblings increased total releases by 0.33 log points over the six-year sample period. By comparison, establishments with proximate different-industry siblings increased total releases by 0.68 log points.

Large establishments and regional density

Among establishments in sparse regions (cities with fewer TRI-reporters than the sample median), we find a greater increase in *log releases* among larger establishments than among smaller ones (Column 5: β

[12] We comment on and graph the increasing secular trends in Appendix A in the online Appendices.

= 0.037, $p < 0.01$). In dense regions (the opposite subsample: cities with more TRI-reporters than the sample median), we find virtually no difference in the performance trends between small and large establishments (Column 6: $\beta = -0.007$, $p < 0.41$). To test whether the performance gap observed in sparse regions was significantly attenuated in dense regions, we estimated a seemingly unrelated regression that simultaneously estimated these two models. The results revealed a statistically significant difference between these two interaction-term coefficients of opposing signs (Wald $\chi^2 = 15.20$, $p < 0.01$). These results support the contention in Hypothesis 4 that density mitigates the power of large establishments to resist institutional pressures.

What is the magnitude of these differences? In sparse regions, the positive (worsening) performance trend of smaller establishments (the baseline group) ($\beta = 0.043$, $p < 0.01$) amounts to a 0.26-log-point increase in *log releases* over the six-year sample period. In contrast, larger establishments increased *log releases* by 0.48 log points, nearly twice (1.8 times) the increase of the smaller establishments. In dense regions, annual growth among smaller establishments ($\beta = 0.065$, $p < 0.01$), the baseline in the regression, amounts to 0.39 log points of *log releases* over six years. In contrast, larger establishments experienced an average total increase of 0.35 log points, 90 percent of that of the smaller establishments.

We also considered a continuous measure of density (*Log establishments in city)*, defined as the log of the number of TRI-reporting establishments in the focal establishment's city in 1994. We interacted this continuous variable with both the *annual counter* and *large establishment*, estimated the model on the full sample (all regions), and report results in Column 7. The statistically significant negative coefficient on the triple interaction term ($\beta = -0.019$; $p<0.01$) indicates that the faster pace of increases in *total releases* among larger establishments (compared to smaller ones) ($\beta = 0.063$; $p<0.01$) is attenuated in denser regions where such organizations have less political power. This additional evidence indicates that our empirical support for Hypothesis 4 is not sensitive to measuring regional density dichotomously.

Public ownership

As reported in Column 8, average environmental performance trends were better for privately held establishments than for publicly owned establishments ($\beta = 0.073$, $p < 0.01$), which supports Hypothesis 5b, but not Hypothesis 5a. The average annual trend of publicly owned establishments (0.041 + 0.073 = 0.114) was nearly triple (2.8 times) the average annual trend of privately owned establishments ($\beta = 0.041$, $p < 0.01$). Over the six-year sample period, these average trends amount to total emissions increasing by 0.068 log points (77% of the sample mean) for establishments with publicly owned parent firms, compared to 0.25 log points (28% of the sample mean) for establishments with privately held parent firms.

Robustness tests

We conducted a number of robustness tests to assess the sensitivity of our results with respect to model specification, moderator measurement, and subsampling strategy.

Alternative specifications

As an alternative to interval regression, we reestimated our models using ordinary least squares (OLS) with establishment-level fixed effects, an approach used by others to estimate toxic chemical emissions from the TRI database (e.g., Hanna and Oliva, 2010; King and Lenox, 2000; Russo, 2009). We continue to cluster standard errors by establishment. The OLS fixed-effects results, reported in Table 3, support our hypotheses, just as our primary interval regression models do. Estimates using a first-differences approach (Wooldridge, 2000: 429), described in the online Appendix B and reported in Table B3, yield inferences very similar to those generated by our models estimated with interval and fixed-effects regression.

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Insert Table 3 here
****

Time-varying moderators

In our primary analysis, we fixed the moderators at their values in 1994, the year before the policy change (or at their 1993 values if 1994 values were missing). In our analysis period of 1995 to 2000, we found no annual variation in proximate headquarters status. Less than 1 percent of establishments experienced a change in their proximate sibling value or in their proximate same-industry sibling value. In contrast, 10 percent of the establishments experienced a change in their annual 'large establishment' status and 13 percent experienced a change in their annual public ownership status. We therefore examined whether our primary results were sensitive to fixing their values based on their status in 1994 (or 1993). In particular, we estimated models in which we reassigned moderator values based on each year of the sample.[13] Overall, these results, reported in Table B4 in the online Appendix B, are substantively similar to our primary results, with the exception of Hypothesis 4, where the difference between large and small establishments in sparse versus dense regions was no longer statistically significant (Wald $\chi^2 = 2.04$, $p < 0.16$). Overall, these results indicate that our analysis is largely robust to whether we code moderator values based on their 1993–1994 values or set their values annually throughout the sample period.

Alternative measurement of establishment size and regional density

In our primary test of Hypothesis 4, concerning the moderating effect of local density on the relative improvement of large and small establishments, we measured density at the city level and considered establishments to be large if they employed more people than the median establishment in their state. As robustness tests, we considered several alternative measures of regional density and establishment size.

Categorizing sparse versus dense communities based on the number of TRI establishments in the same county being less or greater than the nationwide county median of 39 yields additional support for Hypothesis 4 (Columns 1–2 of Table B5 in the online Appendix B). Next, as an alternative measure of density, we distinguished between cities with low versus high employment. Specifically, we identify low-

[13] Because our moderators are unlikely to be affected by our outcome measure (*log releases*), these annual assignments are unlikely to risk endogeneity concerns that might bias our estimates.

employment cities as those with no more than the median city TRI employment of 1,500 and high-employment cities as those with greater than the median of 1,500 TRI employees in the city. Comparing the relative improvement of large and small establishments across both types of cities (Columns 3 and 4 of Table B5) yields results consistent with our primary specification.

We also assessed the sensitivity of our results to our decision to measure large establishments as those whose employment exceeded the median establishment employment in their state. As alternative thresholds to categorize an establishment as large, we considered (a) whether its employment exceeds the top quartile of establishments in the state and (b) whether its employment exceeds the state average. These yielded results very similar to our primary results (Columns 5–8 of Table B5).[14] As a third alternative metric, we considered a continuous measure of *establishment size*. Because *establishment size* is meant to represent the establishment's potential power to resist its state's regulatory agency, we normalized employment of the focal establishment by deducting from it the average establishment employment for its state and dividing the result by the state standard deviation of employment in 1994. This yields a size metric that accommodates differences in levels and variation across states. The results are reported in Columns 9 and 10 in Table B5. A Wald test indicates that the coefficient on this interaction term in dense cities is statistically significantly smaller than in sparse cities (Wald χ^2=10.34, p<0.01), which lends support to Hypothesis 4.

As a final robustness test of Hypothesis 4, we include the interaction between our continuous size measure (*establishment size*), our continuous density measure (*Log establishments in city*), and the annual counter. The results of this model, reported in Table B6, yield a negative coefficient on this triple-interaction term. Interpreted in light of the other interaction terms, this indicates that the faster pace of

[14] In sparse cities, we continue to observe large establishments exhibiting significantly worse performance trends than smaller firms, whether we defined large as top quartile or above average (Columns 5 and 7, respectively, of Table B5: β=0.059, p<0.01 in both instances). In dense cities, we find little difference in performance between large and small establishments using either alternative measure (β=0.008 and β=0.016 in Columns 6 and 8, respectively; neither is statistically significant). Seemingly unrelated regression analysis continues to indicate that the difference between these trends in sparse versus dense cities is statistically significant, whether large is defined as employment exceeding the state's top quartile (Wald χ^2=13.22, p<0.01, Columns 5 vs. 6) or the state average (Wald χ^2=9.46, p<0.01, Columns 7 vs. 8).

increase in total releases among larger establishments (compared to smaller ones) is attenuated in denser regions p<0.01), where such organizations have less political power, which provides additional support for Hypothesis 4.

Collectively, these results (from Tables B5 and B6) indicate that support for Hypothesis 4 is not sensitive to the particular metric used to measure establishment size, the particular definition of sparseness, or whether the moderator is dichotomous or continuous.

Controlling for parent company size

Though we do not have comprehensive data on parent company size, we developed some proxies based on the TRI database and corresponding NETS data. For each establishment-year, we calculated the annual *log number of TRI-reporting siblings* and created a dummy indicating whether or not each establishment *has any TRI-reporting siblings* that year. We also calculated *company-wide log employment (TRI-reporters)* by taking the log of total employment of these sibling establishments and the focal establishment using NETS data (King and Lenox, 2000). We estimated interval regression models that included these three parent-level controls, clustering standard errors at the firm level. The results of these models (not shown) are nearly identical to those of our primary models, which suggests that our results are not sensitive to the inclusion or exclusion of parent-level size proxies in our models.

DISCUSSION

Our analysis reveals that organizational characteristics influence the degree to which establishments improve their environmental performance in response to mandatory information disclosure. Below, we describe how our study contributes to the literatures on information disclosure and institutional theory.

Contributions to information disclosure research

Mandating information disclosure as a means of regulating organizational behavior has become more prevalent in recent years, yet the circumstances under which these programs change organizations' actions are only beginning to be understood (Fung *et al.*, 2007; Toffel and Short, 2011). The need for evaluation is especially great in the field of environmental policy, in which information disclosure is especially prevalent and has been referred to as the 'third wave' of policy instruments, following earlier eras of command-and-control (e.g., technology mandates) and market-based mechanisms such as tradable permits (Delmas, Montes-Sancho, and Shimshack, 2010; Tietenberg, 1998). In this paper, we identify several key organizational attributes that moderate the effect of information disclosure mandates on organizational performance. We obtain five significant results. First, establishments near their firm's headquarters improve more rapidly than other establishments. Second, establishments proximate to corporate siblings improve more rapidly. Third, establishments with proximate siblings in the same industry improve more rapidly than those with proximate siblings in different industries. Fourth, density mitigates the power of large establishments to resist institutional pressures: large establishments improve more slowly than small establishments in sparse regions, but both groups improve similarly in dense regions. Finally, establishments that belong to publicly traded firms improve more slowly than those owned by privately held firms.

The findings related to proximity to headquarters and to corporate siblings suggest that firms are particularly careful to protect their images close to their headquarters and clustered operations. Prior research has argued that firms are embedded in their headquarters' communities and hence are more concerned with their reputations within those communities (Audia and Ryder, 2010; Marquis and Battilana, 2009). Our results are consistent with this and further suggest that embeddedness can occur with respect not only to headquarters but also to clusters of establishments within a region.

We note that our result regarding headquarters proximity contradicts recent research in sociology. Grant, Jones, and Trautner (2004) find no difference in pollution rates between establishments that are in the same state as their headquarters and those that are not. Their analysis, however, considered absolute levels of emissions rather than improvement over time and their use of a broader geographic region might mute the effects of both community embeddedness and transfer of capabilities.

One explanation for our proximity results is that proximity enhances the transfer of capabilities (Berchicci *et al.*, 2011), including the capabilities needed to improve a disclosed poor performance. Our result that establishments with proximate siblings in the same industry demonstrate more rapid improvement suggests that capability transfer is a significant mechanism by which establishments improve following disclosure. It is, of course, possible that both pressure from siblings and capability transfer facilitate improvement. Future research should attempt to isolate these effects further and ascertain when one mechanism dominates the other.

Our results with respect to establishment size suggest that the density of local manufacturing establishments moderates the effect of size on improvement. In relatively dense areas, the visibility of large establishments appears to outweigh the power they possess, whereas in sparse areas, they seem more able to exercise their power. Our results extend findings in sociology regarding the size-environmental performance relationship that suggest that large establishments can abuse their powerful positions within society; for example, by creating disproportionate amounts of pollution (Freudenberg, 2005; Grant *et al.*, 2002).

Contributions to institutional theory

Institutional theory is particularly well suited to illuminating how external pressures affect organizations. Of particular importance to strategic management scholarship is the need to refine institutional theory to further our understanding of why firms respond differently to common institutional pressures. For example, why, among firms in similar institutional environments, do some implement environmental management strategies that go well beyond regulatory compliance requirements while others pursue more laggard approaches (Delmas and Toffel, 2012; Short and Toffel, 2010)? In particular, what organizational attributes moderate the effects of institutional pressures?

Our findings suggest that institutional pressures are especially influential on organizations with particular characteristics and capabilities. We show that organizational features moderate the effect of community pressures, a phenomenon worthy of greater attention (Marquis and Battalina, 2009). Our work extends prior research showing that firms' embeddedness in their headquarters cities influences their actions (Greenwood *et al.*, 2010; Lounsbury, 2007). Our findings go beyond prior studies by explicitly contrasting responses to institutional pressures by establishments near their headquarters with those of establishments farther from their headquarters. Our results suggest that institutional pressures are intensified by internal pressure to improve performance, especially when the headquarters has a direct interest in avoiding damaging its (and the establishment's) relationship with its home community. Our data do not allow us to distinguish the effects of increased external pressure due to being proximate to headquarters from the effects of internal pressure from the headquarters itself. Disentangling these effects is a fruitful avenue for future research.

Our results that privately held establishments improve more rapidly than publicly traded establishments are somewhat surprising, given that prior research has shown that publicly traded firms have significantly lower environmental management system (EMS) implementation costs than privately held firms (Darnall and Edwards, 2006). Given this, one might expect the cost of acquiescing to pressure to improve environmental performance also to be lower for such firms, but, in fact, we find that it is the establishments owned by privately held firms that are more likely to improve under pressure. Our findings suggest that, at least in our context, privately held firms' resource constraints might be outweighed by their susceptibility to pressures that accompany information disclosure and their willingness to make investments without regard to short-term financial market reactions. Future research is required to distinguish the mechanisms underlying these results. For establishments owned by publicly traded companies, greater pressure to achieve growth (Mascarenhas, 1989) might deter investments in environmental improvement projects with less certain returns or longer payback periods. In contrast, establishments owned by privately held firms, which generally have more concentrated ownership, might be more willing and able to undertake projects that create other (nonfinancial) forms of utility valued by their owners, which Berrone *et al.* (2010) refer to as 'socioemotional wealth.'

Our finding with regard to establishment size helps resolve the seemingly contradictory arguments posited in prior research about whether larger or smaller organizations are more responsive to institutional pressure. Our results demonstrate that understanding the effect of an organization's size requires understanding its context. Our finding that larger establishments tend to perform worse over time than smaller ones in sparse regions suggests that, in this context, a larger establishment's power to resist local pressure trumps its need for legitimacy.

Implications for policymakers and managers

For policymakers designing information disclosure programs, our results suggest that a program's effectiveness depends in part on the industrial organization of its target population. For example, industries largely populated by establishments owned by privately held firms may be more responsive to information disclosure programs than industries populated by establishments owned by publicly traded firms. Understanding and anticipating such differences can help regulators improve the efficiency and effectiveness of targeting schemes that supplement information disclosure so as to exert more pressure on laggards.

For corporate managers, our results suggest the need to be aware of how organizational features are likely to affect subsidiary establishments' responsiveness to information disclosure programs, which might in turn affect the firm's actions or its reputation. For example, our finding that information disclosure prompts greater performance improvement among establishments closer to headquarters might lead some corporate managers to provide extra support for (and/or exert more pressure on) more remote establishments.

CONCLUSION

Organizations exhibit heterogeneous responses to institutional pressure created by information disclosure programs. Our analysis of changes in establishments' environmental performance following expansion of the EPA's TRI program suggests that organizational characteristics explain some of this heterogeneity. With information disclosure programs proliferating, our findings on factors that magnify or dampen their effectiveness become more salient. In identifying establishment-level as well as intra-organizational attributes associated with heterogeneous responses to institutional pressure, we contribute to an important literature that seeks to leverage institutional theory to explain heterogeneous organizational strategies.

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Table 1. Descriptive statistics

Panel A. Summary statistics

		Obs	Mean	SD	Min	Max
1.	Log releases (toxic chemicals added in 1995)	222,349	0.88	2.86	0.00	17.11
2.	Proximate headquarters$_{93/94}$	218,440	0.38	0.49	0.00	1.00
3.	Proximate sibling$_{93/94}$	222,349	0.11	0.31	0.00	1.00
4.	Proximate same-industry sibling$_{93/94}$	222,349	0.09	0.28	0.00	1.00
5.	Proximate different-industry sibling$_{93/94}$	222,349	0.04	0.19	0.00	1.00
6.	Large establishment$_{93/94}$	167,592	0.51	0.50	0.00	1.00
7.	Public ownership$_{93/94}$	162,412	0.28	0.45	0.00	1.00
8.	Historical releases trend	222,349	-0.01	0.84	-2.00	2.00
9.	Relative production level	222,349	0.09	0.30	-1.89	3.46
10.	Log employment	222,349	3.59	2.16	0.00	10.09
11.	Congressional district LCV score	222,349	0.42	0.35	0.00	1.00

Panel B. Pairwise correlations

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.
1. Log releases (toxic chemicals added in 1995)	1.00									
2. Proximate headquarters$_{93/94}$	-0.08	1.00								
3. Proximate sibling$_{93/94}$	0.02	0.00	1.00							
4. Proximate same-industry sibling$_{93/94}$	0.01	0.02	0.86	1.00						
5. Proximate different-industry sibling$_{93/94}$	0.02	-0.04	0.55	0.13	1.00					
6. Large establishment$_{93/94}$	0.09	-0.21	0.07	0.07	0.03	1.00				
7. Public ownership$_{93/94}$	0.10	-0.41	0.09	0.05	0.12	0.26	1.00			
8. Historical releases trend	0.01	0.02	0.00	0.00	0.00	-0.04	-0.06	1.00		
9. Relative production level	0.12	-0.02	0.01	0.01	0.01	0.05	0.02	0.06	1.00	
10. Log employment	0.07	-0.09	0.05	0.06	0.02	0.66	0.28	-0.02	0.05	1.00
11. Congressional district LCV score	0.01	0.08	-0.03	-0.04	-0.01	-0.04	-0.04	-0.04	-0.01	-0.02

Note: $_{93/94}$ denotes dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995.

Table 2. Interval regression results
Dependent variable: Log releases

Sample:	(1) Full sample	(2) Full sample	(3) Have siblings	(4) Have siblings	(5) In sparse cities	(6) In dense cities	(7) Full sample	(8) Full sample
Proximate headquarters$_{93/94}$ × Annual counter		-0.051**						
		[0.005]						
Proximate sibling$_{93/94}$ × Annual counter			-0.020*					
			[0.009]					
Proximate same-industry sibling$_{93/94}$ × Annual Counter				-0.036**				
				[0.009]				
Proximate different-industry sibling$_{93/94}$ × Annual counter				0.023				
				[0.016]				
Large establishment$_{93/94}$ × Annual counter					0.037**	-0.007	0.063**	
					[0.008]	[0.008]	[0.011]	
Large establishment$_{93/94}$ × Log establishments per city$_{93/94}$ × Annual counter							-0.019**	
							[0.004]	
Public ownership$_{93/94}$ × Annual counter								0.073**
								[0.007]
Proximate headquarters$_{93/94}$		-0.039						
		[0.025]						
Proximate sibling$_{93/94}$			0.087+					
			[0.046]					
Proximate same-industry sibling$_{93/94}$				0.121*				
				[0.052]				
Proximate different-industry sibling$_{93/94}$				-0.043				
				[0.079]				
Large establishment$_{93/94}$					-0.172**	-0.088	-0.144*	
					[0.060]	[0.065]	[0.065]	
Large establishment$_{93/94}$ × Log establishments in city$_{93/94}$							0.004	
							[0.020]	
Log establishments in city$_{93/94}$ × Annual counter							0.010**	
							[0.003]	
Log establishments in city$_{93/94}$							-0.033**	
							[0.011]	
Public ownership$_{93/94}$								-0.004
								[0.038]
Annual counter	0.077**	0.087**	0.091**	0.091**	0.043**	0.065**	0.029**	0.041**
	[0.003]	[0.004]	[0.005]	[0.005]	[0.007]	[0.007]	[0.008]	[0.005]
Historical releases trend	-0.088**	-0.110**	-0.128**	-0.128**	-0.101**	-0.077**	-0.093**	-0.096**
	[0.015]	[0.015]	[0.021]	[0.021]	[0.023]	[0.023]	[0.016]	[0.017]
Historical releases trend × Annual counter		0.008**	0.012**	0.012**	0.008*	0.008*	0.008**	0.010**
		[0.003]	[0.004]	[0.004]	[0.004]	[0.004]	[0.003]	[0.003]
Log number of siblings (in 1994)			0.043**	0.043**				
			[0.011]	[0.012]				
Relative production level	0.202**	0.201**	0.184**	0.184**	0.143+	0.180*	0.167**	0.161**
	[0.052]	[0.052]	[0.070]	[0.070]	[0.082]	[0.081]	[0.058]	[0.058]
Log employment	0.220**	0.207**	0.254**	0.254**	0.281**	0.277**	0.277**	0.247**
	[0.012]	[0.012]	[0.016]	[0.016]	[0.025]	[0.027]	[0.018]	[0.014]
Congressional district LCV score	0.025	0.053	0.017	0.017	0.086	-0.027	0.038	0.023
	[0.034]	[0.034]	[0.048]	[0.048]	[0.057]	[0.054]	[0.039]	[0.039]
Industry (2-digit SIC) dummies	Included	Included	Included	Included	Included	Included	Included	Included
Observations	222,349	218,440	138,888	138,888	86,925	80,667	167,592	162,412
Number of establishments	38,634	37,952	23,918	23,918	15,148	14,210	29,358	28,477
Wald test: Are coefficients on both interaction terms equal? (χ^2 statistic)				9.12**	15.20**			

All models estimated with interval regression (left-censored). Brackets contain standard errors clustered by establishment; ** p<0.01, * p<0.05, + p<0.10. All samples include establishment-years during 1995–2000. The dependent variable is log (plus 1) of total releases reported to TRI of the 243 toxic chemicals that were added to the TRI chemical list in 1995. These total releases include those reported as production waste, transfers offsite, and emissions to air, land, water, and underground injection. All models also include a dummy to indicate observations for which the value of *relative production level* was interpolated and observations for which *historical releases trend*, *relative production level*, *log employment*, and *Congressional district LCV score* were missing. The dummy indicating missing observations for *historical releases trend* is interacted with the *annual counter*. $_{93/94}$ denotes a dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995.

Table 3. OLS establishment-level fixed-effects regression estimates

Dependent variable: Log releases

Sample:	(1) Full sample	(2) Have siblings	(3) Have siblings	(4) In sparse cities	(5) In dense cities	(6) Full sample
Proximate headquarters$_{93/94}$ × Annual counter	-0.040**					
	[0.005]					
Proximate sibling$_{93/94}$ × Annual counter		-0.020*				
		[0.009]				
Proximate same-industry sibling$_{93/94}$ × Annual counter			-0.035**			
			[0.010]			
Proximate different-industry sibling$_{93/94}$ × Annual counter			0.019			
			[0.017]			
Large establishment$_{93/94}$ × Annual counter				0.015+	-0.030**	
				[0.008]	[0.008]	
Public ownership$_{93/94}$ × Annual counter						0.056**
						[0.007]
Annual counter	0.068**	0.066**	0.066**	0.046**	0.065**	0.034**
	[0.004]	[0.005]	[0.005]	[0.007]	[0.007]	[0.004]
Historical releases trend × Annual counter	0.003	0.006+	0.006+	0.005	0.001	0.004
	[0.003]	[0.004]	[0.004]	[0.004]	[0.004]	[0.003]
Relative production level	0.256**	0.310**	0.310**	0.174**	0.315**	0.256**
	[0.033]	[0.044]	[0.044]	[0.046]	[0.053]	[0.036]
Log employment	0.034**	0.028*	0.028*	0.032+	0.031	0.031*
	[0.010]	[0.013]	[0.013]	[0.018]	[0.019]	[0.013]
Congressional district LCV score	0.008	0.002	0.002	0.034	0.035	0.041
	[0.028]	[0.038]	[0.038]	[0.043]	[0.046]	[0.032]
Establishment fixed effects	Included	Included	Included	Included	Included	Included
Observations	218,440	138,888	138,888	86,925	80,667	162,412
Number of establishments	37,952	23,918	23,918	15,148	14,210	28,477
Wald test: Are coefficients on both interaction terms equal? (F-statistic)			7.48**	15.43**		

All models estimated with OLS regression with establishment fixed effects. Brackets contain standard errors clustered by establishment; ** p<0.01, * p<0.05, + p<0.10. All samples include establishment-years during 1995–2000. The dependent variable is log (plus 1) of total releases reported to TRI of the 243 toxic chemicals that were added to the TRI chemical list in 1995. These total releases include those reported as production waste, transfers offsite, and releases to air, land, water, and underground injection. The main effects of the hypothesized moderators (e.g., proximate headquarters) are time-invariant at the establishment level and thus are absorbed by the establishment fixed effects. All models also include a dummy to indicate observations for which the value of *relative production level* was interpolated and observations for which *historical releases trend*, *relative production level*, *log employment*, and *Congressional district LCV score* were missing. The dummy indicating missing observations for *historical releases trend* is interacted with the *annual counter*. $_{93/94}$ denotes a dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995.

APPENDICES to Dosh, Dowell, and Toffel HOW FIRMS RESPOND TO MANDATORY INFORMATION DISCLOSURE

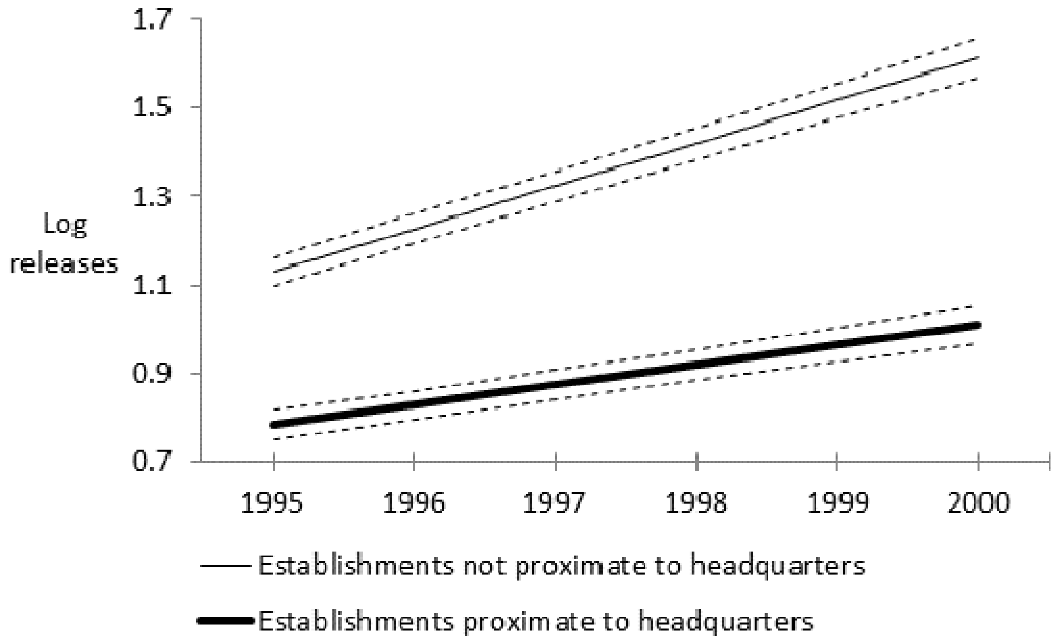
Appendix A provides more information about the primary models by reporting the distribution of industries in the sample (Table A1), illustrating the hypothesized results by graphing predicted values (Figures A1-A5), and describing the increasing secular trends of emissions of the TRI chemicals that were first required to be reported in 1995. Appendix B reports results of several robustness tests described in the paper.

APPENDIX A. ILLUSTRATING AND EXPLAINING PRIMARY RESULTS

Table A1. Industry composition of sample

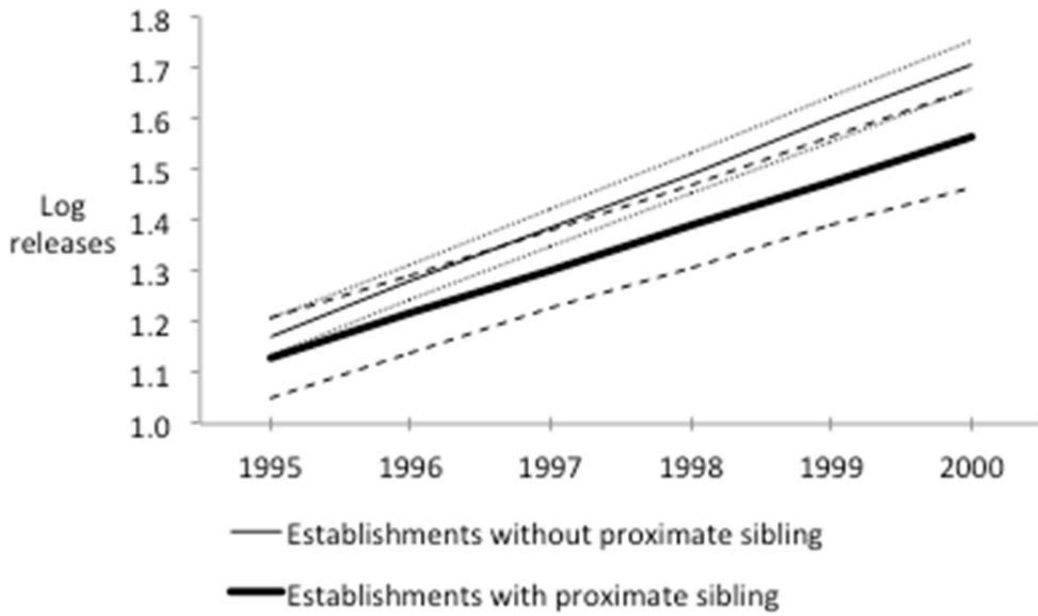
SIC	Industry	Number of Establishments	Percent
17	Construction special trade contractors	216	0.6%
20	Food and kindred products	2,809	7.4%
22	Textile mill products	694	1.8%
23	Apparel and other finished products made from fabrics and similar materials	107	0.3%
24	Lumber and wood products, except furniture	1,205	3.2%
25	Furniture and fixtures	820	2.2%
26	Paper and allied products	1,032	2.7%
27	Printing, publishing, and allied industries	553	1.5%
28	Chemicals and allied products	4,700	12.3%
29	Petroleum refining and related industries	503	1.3%
30	Rubber and miscellaneous plastics products	2,163	5.7%
31	Leather and leather products	135	0.4%
32	Stone, clay, glass, and concrete products	1,488	3.9%
33	Primary metal industries	2,648	6.9%
34	Fabricated metal products, except machinery and transportation equipment	4,359	11.4%
35	Industrial and commercial machinery and computer equipment	2,553	6.7%
36	Electronic and other electrical equipment and components, except computer equipment	2,593	6.8%
37	Transportation equipment	1,917	5.0%
38	Measuring, analyzing, and controlling instruments; photographic, medical, and optical goods; watches and clocks	875	2.3%
39	Miscellaneous manufacturing industries	609	1.6%
42	Motor freight transportation and warehousing	299	0.8%
49	Electric, gas, and sanitary services	668	1.8%
50	Wholesale trade—durable goods	973	2.6%
51	Wholesale trade—non-durable goods	1,536	4.0%
52	Building materials, hardware, garden supply, and mobile home dealers	125	0.3%
55	Automotive dealers and gasoline service stations	279	0.7%
59	Miscellaneous retail	130	0.3%
73	Business services	412	1.1%
76	Miscellaneous repair services	111	0.3%
87	Engineering, accounting, research, management, and related services	315	0.8%
	Other	1,348	3.5%
	Total	38,175	100%

Figure A1. Establishments proximate to their headquarters exhibit a superior environmental performance trend compared to establishments with more distant headquarters



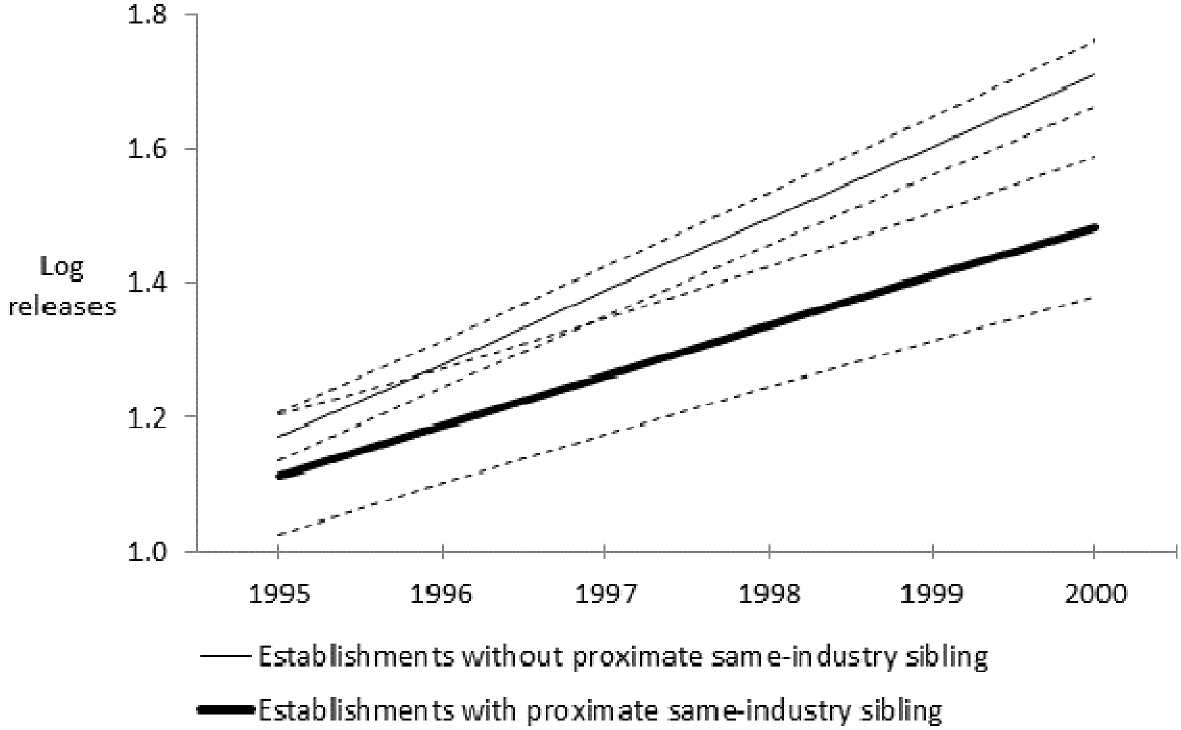
Note. This figure displays average predicted log releases based on the model reported in Column 2 of Table 2.

Figure A2. Establishments with at least one proximate sibling exhibit a superior environmental performance trend compared to other establishments with corporate siblings



Note. This figure displays average predicted log releases based on the model reported in Column 3 of Table 2.

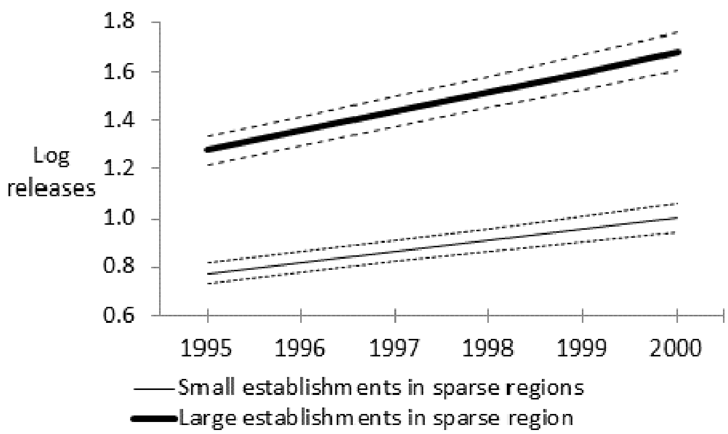
Figure A3. Establishments with at least one proximate same-industry sibling exhibit a superior environmental performance trend compared to other establishments with corporate siblings



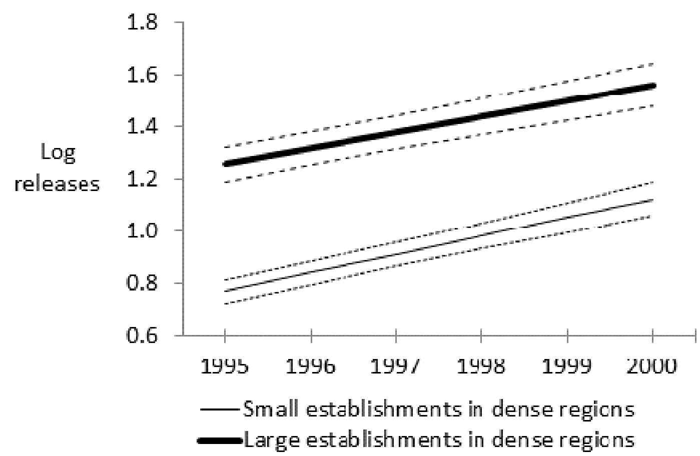
Note. This figure displays average predicted log releases based on the model reported in Column 4 of Table 2.

Figure A4. Larger establishments exhibit worse environmental performance than smaller establishments in sparse regions (Panel A), but similar performance in dense regions (Panel B). Taken together, the performance of large establishments relative to small establishments diverges in sparse and dense regions (Panel C).

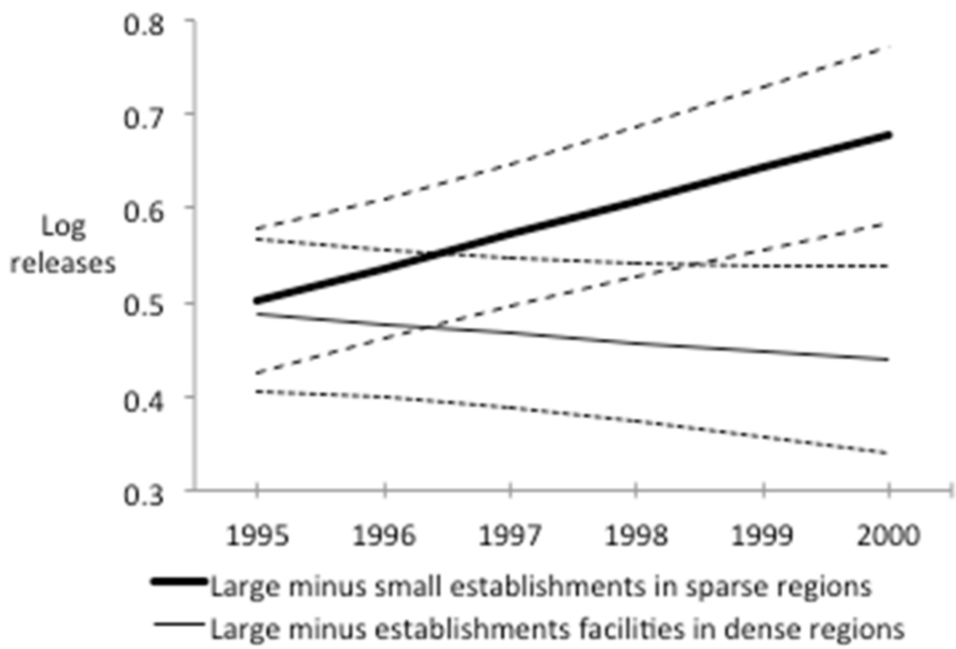
Panel A: Sparse regions



Panel B: Dense regions

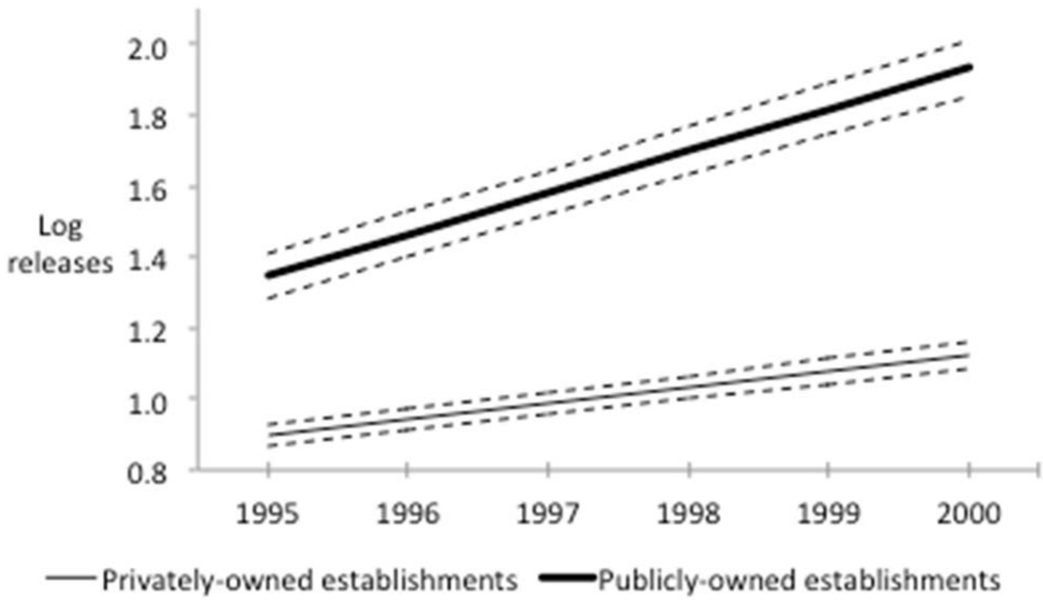


Panel C: Difference between predicted values of large versus small facilities



Note. Panels A and B display average predicted log releases based on the models reported in Columns 5 and 6, respectively, of Table 2. Panel C displays the average predicted log releases of large establishments less that of small facilities for both sparse and dense regions.

Figure A5. Privately-owned establishments exhibit a superior environmental performance trend compared to publicly-owned establishments.



Note. This figure displays average predicted log releases based on the model reported in Column 8 of Table 2.

Explanation of secular emissions trend

EPA and other commentators have often claimed that the TRI program has led to dramatic reductions in the overall amount of toxic chemical emissions, a trend often based on the “core chemicals,” those chemicals that were consistently required to be reported (and with a consistent reporting threshold) throughout the comparison period.[1] In contrast, the average emissions the chemicals EPA added to TRI in 1995 reported by establishments in our sample[2] increased from 12,439 pounds in 1995 to 17,921 pounds in 2000. The total amount of these newly-reported chemicals that these establishments reported increased from 482 million pounds in 1995 to 640 million pounds in 2000. The consistently positive, significant coefficient on *annual counter* in the models reported in Tables 2 and 3 reveals a positive secular trend during 1995 to 2000 in these emissions.

[1] For example, see Connecticut Department of Energy & Environmental Protection. 2005. Connecticut’s management of toxic air pollutants. Available at http://www.ct.gov/dep/cwp/view.asp?a=2684&q=322238&depNav_GID=1619, last updated November 2005, accessed June 2012); S. Moulton and C. Gregory. 2005. *Dismantling the public's right to know: EPA's systematic weakening of the Toxic Release Inventory*. Washington DC: OMB Watch; SM Wolf. 1996. Fear and loathing about the public right to know: the surprising success of the emergency planning and community right-to-know act. *Journal of Land Use & Environmental Law* 11(2): 217-325.

[2] For a TRI-reporting establishment to be included in our sample in a given year, it either had to report emissions of at least one TRI chemical in that or a future year, or the focal year had to precede or equal its final year of operation based on Dun & Bradstreet data. This definition enabled us to identify establishments that were alive in a given year despite their not reporting any newly added TRI chemicals.

APPENDIX B. ROBUSTNESS TESTS

First-differences OLS regression estimates

As an additional robustness test, we estimated our models using a first-differences approach, which yields year-on-year change estimates and absorbs establishment-specific time-invariant factors (Wooldridge, 2000: 429). In this model, our dependent variable is *first-differenced log releases*$_{i,t}$ (calculated as *log releases*$_{i,t}$ – *log releases*$_{i,t-1}$ for establishment *i* in year *t*). We include as controls first differences of *relative production ratio*, *log employees*, and *Congressional district LCV score* (each calculated in the same fashion). First-differencing the counter used to estimate the comparison group's secular trend yields '1' in all years, resulting in the constant term representing the estimated secular trend of the comparison group. First-differencing our interaction terms (group membership interacted with the secular trend) yields a dummy variable indicating group membership. The coefficient on this dummy variable estimates the incremental trend of the group, above and beyond the comparison group's trend represented by the constant term. We estimated this first-differences model using OLS and continued to cluster standard errors by establishment. The first-differences results, reported in Table B3, yield inferences very similar to those generated by our models estimated with interval regression, although the statistical significance level declines slightly in our test of Hypothesis 2 (to $p < 0.07$).

Table B1. Robustness tests of Hypotheses 1 and 2: Alternative definitions of proximate

Dependent variable: Log releases

	(1)	(2)	(3)	(4)
Sample:	Full sample	Full sample	Have siblings	Have siblings
Proximate measured by common:	3-digit ZIP code	State	3-digit ZIP code	County
Proximate headquarters$_{93/94}$ × Annual counter	-0.052**	-0.043**		
	[0.005]	[0.005]		
Proximate sibling$_{93/94}$ × Annual counter			-0.020**	-0.008
			[0.007]	[0.008]
Proximate headquarters$_{93/94}$	-0.040	-0.003		
	[0.025]	[0.026]		
Proximate sibling$_{93/94}$			0.119**	0.058
			[0.040]	[0.041]
Annual counter	0.089**	0.089**	0.093**	0.089**
	[0.005]	[0.005]	[0.005]	[0.005]
Historical releases trend	-0.110**	-0.110**	-0.128**	-0.128**
	[0.015]	[0.015]	[0.021]	[0.021]
Historical releases trend × Annual counter	0.008**	0.008**	0.012**	0.012**
	[0.003]	[0.003]	[0.004]	[0.004]
Log number of siblings$_{93/94}$			0.042**	0.043**
			[0.011]	[0.011]
Relative production level	0.200**	0.201**	0.184**	0.184**
	[0.052]	[0.052]	[0.070]	[0.070]
Log employment	0.207**	0.212**	0.253**	0.254**
	[0.012]	[0.012]	[0.016]	[0.016]
Congressional district LCV score	0.057+	0.051	0.016	0.014
	[0.034]	[0.034]	[0.048]	[0.048]
Industry (2-digit SIC) dummies	Included	Included	Included	Included
Observations	218,440	218,440	138,888	138,888
Number of establishments	37,952	37,952	23,918	23,918

All models estimated with interval regression (left-censored). Brackets contain standard errors clustered by establishment; ** p<0.01, * p<0.05, + p<0.10. All samples include establishment-years during 1995–2000. The dependent variable is log (plus 1) of total releases reported to TRI of the 243 toxic chemicals that were added to the TRI chemical list in 1995. These total releases include those reported as production waste, transfers offsite, and releases to air, land, water, and underground injection. All models also include a dummy to indicate observations for which the value of *relative production level* was interpolated and observations for which *historical releases trend*, *relative production level*, *log employment*, and *Congressional district LCV score* were missing. The dummy indicating missing observations for *historical releases trend* is interacted with the *annual counter*. $_{93/94}$ denotes a dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995. In the primary specification, proximity is based on city.

Table B2. Robustness tests of Hypothesis 3: Alternative definitions of proximate and same-industry

Dependent variable: Log releases

	(1)	(2)	(3)
Sample:	Have siblings	Have siblings	Have siblings
Proximate measured by common:	City	3-digit ZIP code	County
Same industry measured by common:	3-digit SIC code	2-digit SIC code	2-digit SIC code
Proximate same-industry sibling$_{93/94}$ × Annual counter	-0.036**	-0.023**	-0.025**
	[0.010]	[0.008]	[0.009]
Proximate different-industry sibling$_{93/94}$ × Annual counter	0.015	-0.000	0.028*
	[0.014]	[0.011]	[0.013]
Proximate same-industry sibling$_{93/94}$	0.096+	0.134**	0.078+
	[0.052]	[0.045]	[0.047]
Proximate different-industry sibling$_{93/94}$	0.006	0.002	-0.032
	[0.073]	[0.059]	[0.063]
Annual counter	0.090**	0.092**	0.089**
	[0.005]	[0.005]	[0.005]
Historical releases trend	-0.128**	-0.128**	-0.128**
	[0.021]	[0.021]	[0.021]
Historical releases trend × Annual counter	0.012**	0.012**	0.012**
	[0.004]	[0.004]	[0.004]
Log number of siblings (in 1994)	0.043**	0.043**	0.042**
	[0.012]	[0.012]	[0.012]
Relative production level	0.184**	0.184**	0.184**
	[0.070]	[0.070]	[0.070]
Log employment	0.254**	0.253**	0.254**
	[0.016]	[0.016]	[0.016]
Congressional district LCV score	0.017	0.017	0.015
	[0.048]	[0.048]	[0.048]
Industry (2-digit SIC) dummies	Included	Included	Included
Observations	138,888	138,888	138,888
Number of establishments	23,918	23,918	23,918
Wald test: Are coefficients on both interaction terms equal?			
χ^2 statistic	8.45	2.51	10.98
p-value	[0.00]	[0.11]	[0.00]

All models estimated with interval regression (left-censored). Brackets contain standard errors clustered by establishment; ** p<0.01, * p<0.05, + p<0.10. All samples include establishment-years during 1995–2000. The dependent variable is log (plus 1) of total releases reported to TRI of the 243 toxic chemicals that were added to the TRI chemical list in 1995. These total releases include those reported as production waste, transfers offsite, and releases to air, land, water, and underground injection. All models also include a dummy to indicate observations for which the value of *relative production level* was interpolated and observations for which *historical releases trend*, *relative production level*, *log employment*, and *Congressional district LCV score* were missing. The dummy indicating missing observations for *historical releases trend* is interacted with the *annual counter*. $_{93/94}$ denotes a dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995. In the primary specification, proximity is based on city and industry equivalence is based on two-digit SIC code.

Table B3. First-differences OLS regression estimates

Dependent variable: First-differenced log releases

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Full sample	Have siblings	Have siblings	In sparse cities	In dense cities	Full sample
Proximate headquarters$_{93/94}$	-0.040**					
	[0.005]					
Proximate sibling$_{93/94}$		-0.016+				
		[0.009]				
Proximate same-industry sibling$_{93/94}$			-0.026**			
			[0.010]			
Proximate different-industry sibling$_{93/94}$			0.015			
			[0.016]			
Large establishment$_{93/94}$				0.013+	-0.035**	
				[0.008]	[0.008]	
Public ownership$_{93/94}$						0.050**
						[0.007]
Relative production level (first difference)	0.170**	0.201**	0.202**	0.131**	0.159**	0.154**
	[0.031]	[0.043]	[0.043]	[0.047]	[0.051]	[0.036]
Log employment (first difference)	0.018*	0.024*	0.024*	0.017	0.027	0.024*
	[0.008]	[0.010]	[0.010]	[0.013]	[0.018]	[0.011]
Congressional district LCV score (first difference)	-0.015	0.005	0.005	-0.009	-0.037	-0.021
	[0.021]	[0.029]	[0.029]	[0.035]	[0.033]	[0.024]
Constant	0.081**	0.079**	0.079**	0.064**	0.087**	0.053**
	[0.006]	[0.007]	[0.007]	[0.009]	[0.009]	[0.006]
Observations	180,488	114,970	114,970	71,777	66,457	133,935
Number of establishments	37,307	23,593	23,593	14,835	13,898	27,857
Wald test: Are coefficients on both interaction terms equal? (F-statistic)			4.52*	11.08**		

All models estimated with OLS regression. Brackets contain standard errors clustered by establishment; ** p<0.01, * p<0.05, + p<0.10. All samples include establishment-years during 1995–2000. The dependent variable is the first difference of log (plus 1) of total releases reported to TRI of the 243 toxic chemicals that were added to the TRI chemical list in 1995. These total releases include those reported as production waste, transfers offsite, and releases to air, land, water, and underground injection. All models also include a dummy to indicate observations for which the value of *relative production level* was interpolated in the current and previous periods and observations for which *historical releases trend*, *relative production level*, *log employment*, and *Congressional district LCV score* were missing in the current and previous periods. $_{93/94}$ denotes a dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995.

Table B4. Interval regression estimates with time-varying moderators

Dependent variable: Log releases

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Full sample	Have siblings	Have siblings	In sparse cities	In dense cities	Full sample
Proximate headquarters × Annual counter	-0.051**					
	[0.005]					
Proximate sibling × Annual counter		-0.019*				
		[0.009]				
Proximate same-industry sibling × Annual counter			-0.035**			
			[0.009]			
Proximate different-industry sibling × Annual counter			0.022			
			[0.017]			
Large establishment × Annual counter				0.008	-0.010	
				[0.009]	[0.009]	
Public ownership × Annual counter						0.017*
						[0.008]
Proximate headquarters	-0.039					
	[0.025]					
Proximate sibling		0.085+				
		[0.046]				
Proximate same-industry sibling			0.121*			
			[0.052]			
Proximate different-industry sibling			-0.041			
			[0.079]			
Large establishment				-0.242**	-0.149*	
				[0.060]	[0.064]	
Public ownership						0.053
						[0.035]
Annual counter	0.087**	0.091**	0.091**	0.068**	0.079**	0.066**
	[0.004]	[0.005]	[0.005]	[0.007]	[0.008]	[0.005]
Historical releases trend	-0.110**	-0.128**	-0.128**	-0.097**	-0.089**	-0.096**
	[0.015]	[0.021]	[0.021]	[0.023]	[0.023]	[0.016]
Historical releases trend × Annual counter	0.008**	0.012**	0.012**	0.007+	0.005	0.006*
	[0.003]	[0.004]	[0.004]	[0.004]	[0.004]	[0.003]
Log number of siblings		0.042**	0.042**			
		[0.011]	[0.011]			
Relative production level	0.201**	0.183**	0.184**	0.130+	0.248**	0.203**
	[0.052]	[0.070]	[0.070]	[0.078]	[0.081]	[0.057]
Log employment	0.207**	0.254**	0.254**	0.299**	0.286**	0.238**
	[0.012]	[0.016]	[0.016]	[0.025]	[0.027]	[0.013]
Congressional district LCV score	0.053	0.017	0.017	0.088	-0.046	0.003
	[0.034]	[0.048]	[0.048]	[0.054]	[0.052]	[0.037]
Industry (2-digit SIC) dummies	Included	Included	Included	Included	Included	Included
Observations	218,440	138,888	138,888	94,336	86,699	175,376
Number of establishments	37,952	23,918	23,918	17,836	16,470	33,759
Wald test: Are coefficients on both interaction terms equal? (χ^2 statistic)			8.17**	2.04		

Notes: All models estimated with interval regression (left-censored). Brackets contain standard errors clustered by establishment; ** p<0.01, * p<0.05, + p<0.10. All samples include establishment-years during 1995–2000. The dependent variable is log (plus 1) of total releases reported to TRI of the 243 toxic chemicals that were added to the TRI chemical list in 1995. These total releases include those reported as production waste, transfers offsite, and emissions to air, land, water, and underground injection. All models also include a dummy to indicate observations for which the value of *relative production level* was interpolated and observations for which *historical releases trend*, *relative production level*, *log employment*, and *Congressional district LCV score* were missing. The dummy indicating missing observations for *historical releases trend* is interacted with the *annual counter*. $_{93/94}$ denotes a dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995

Table B5. Robustness tests of Hypothesis 4: Alternative definitions of large establishment and regional sparseness

Dependent variable: Log releases

Sample:	(1) In sparse counties	(2) In dense counties	(3) Low-employment cities	(4) High-employment cities	(5) In sparse cities	(6) In dense cities	(7) In sparse cities	(8) In dense cities	(9) In sparse cities	(10) In dense cities
Sparse vs. dense region based on:	Number of TRI establishments in the focal establishment's county compared to the sample median county		Employment of all TRI establishments in the focal establishment's city compared to the sample median city		Number of TRI establishments in the focal establishment's city compared to the sample mean city					
Large establishment based on:	Establishment's employment exceeds its state median		Establishment's employment exceeds its state median		Establishment's employment within top quartile of its state		Establishment's employment exceeds its state average		n/a	
Large establishment$_{93/94}$ × Annual counter	0.039**	-0.006	0.029**	0.000	0.059**	0.008	0.059**	0.016		
	[0.008]	[0.008]	[0.008]	[0.008]	[0.010]	[0.010]	[0.010]	[0.010]		
Establishment size$_{93/94}$ × Annual counter									0.026**	0.004
									[0.006]	[0.004]
Large establishment$_{93/94}$	-0.211**	-0.056	-0.011	-0.211**	0.038	0.250**	0.004	0.201**		
	[0.063]	[0.061]	[0.057]	[0.065]	[0.065]	[0.072]	[0.065]	[0.072]		
Establishment size$_{93/94}$									0.230**	0.228**
									[0.045]	[0.045]
Annual counter	0.045**	0.060**	0.042**	0.068**	0.046**	0.059**	0.045**	0.057**	0.062**	0.061**
	[0.007]	[0.007]	[0.007]	[0.007]	[0.006]	[0.007]	[0.006]	[0.006]	[0.006]	[0.006]
Historical releases trend	-0.090**	-0.085**	-0.090**	-0.087**	-0.099**	-0.073**	-0.100**	-0.073**	-0.088**	-0.066**
	[0.025]	[0.021]	[0.023]	[0.023]	[0.023]	[0.023]	[0.023]	[0.023]	[0.023]	[0.023]
Historical releases trend × Annual counter	0.011**	0.006	0.010*	0.007+	0.010*	0.009*	0.010*	0.009*	0.010**	0.010*
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Relative production level	0.065	0.244**	0.170*	0.167*	0.151+	0.183*	0.149+	0.181*	0.167*	0.194*
	[0.082]	[0.081]	[0.082]	[0.081]	[0.082]	[0.081]	[0.082]	[0.081]	[0.081]	[0.081]
Log employment	0.311**	0.242**	0.184**	0.340**	0.222**	0.197**	0.229**	0.203**	0.139**	0.150**
	[0.026]	[0.025]	[0.023]	[0.026]	[0.020]	[0.023]	[0.021]	[0.023]	[0.020]	[0.023]
Congressional district LCV score	0.098	0.026	-0.006	0.042	0.083	-0.027	0.088	-0.025	0.059	-0.045
	[0.065]	[0.052]	[0.054]	[0.056]	[0.057]	[0.054]	[0.057]	[0.054]	[0.056]	[0.054]
Industry (2-digit SIC) dummies	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Observations	83,490	84,102	82,078	85,514	86,925	80,667	86,925	80,667	86,925	80,667
Number of establishments	14,470	14,888	14,322	15,036	15,148	14,210	15,148	14,210	15,148	14,210
Wald test: Are coefficients on both interaction terms equal? χ^2 statistic	15.96**		6.88*		13.22**		9.46**		8.45**	

All models estimated with interval regression (left-censored). Brackets contain standard errors clustered by establishment; ** p<0.01, * p<0.05, + p<0.10. All samples include establishment-years during 1995–2000. The dependent variable is log (plus 1) of total releases reported to TRI of the 243 toxic chemicals that were added to the TRI chemical list in 1995. These total releases include those reported as production waste, transfers offsite, and releases to air, land, water, and underground injection. All models also include a dummy to indicate observations for which the value of *relative production level* was interpolated and observations for which *historical releases trend*, *relative production level*, *log employment*, and *Congressional district LCV score* were missing. The dummy indicating missing observations for *historical releases trend* is interacted with the *annual counter*. $_{93/94}$ denotes a dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995. Sparse counties includes establishments in counties with no more than the county median of 39 establishments; dense counties includes establishments in counties with greater than the county median of 39 establishments. Sparse cities includes establishments in cities with no more than the city median of 10.5 establishments; dense cities includes establishments in cities with greater than the city median of 10.5 establishments. Low-employment cities includes establishments in cities with no more than the city median TRI employment of 1,500; high-employment cities includes establishments in cities with greater than the city median TRI employment of 1,500.

Table B6. Additional robustness test of Hypothesis 4: Continuous measures of large establishment and regional sparseness

Dependent variable: Log releases

Sample:	Full sample
Establishment $\text{size}_{93/94}$ × Log establishments in $\text{city}_{93/94}$ × Annual counter	-0.010**
	[0.003]
Establishment $\text{size}_{93/94}$ × Log establishments in $\text{city}_{93/94}$	-0.011
	[0.019]
Establishment $\text{size}_{93/94}$ × Annual counter	0.042**
	[0.009]
Establishment $\text{size}_{93/94}$	0.262**
	[0.061]
Log establishments in $\text{city}_{93/94}$ × Annual counter	0.000
	[0.002]
Log establishments in $\text{city}_{93/94}$	-0.032**
	[0.010]
Annual counter	0.061**
	[0.006]
Historical releases trend	-0.081**
	[0.016]
Historical releases trend × Annual counter	0.010**
	[0.003]
Relative production level	0.187**
	[0.057]
Log employment	0.138**
	[0.015]
Congressional district LCV score	0.014
	[0.039]
Industry (2-digit SIC) dummies	Included
Observations	167,592
Number of establishments	29,358

All models estimated with interval regression (left-censored). Brackets contain standard errors clustered by establishment; ** p<0.01, * p<0.05, + p<0.10. All samples include establishment-years during 1995–2000. The dependent variable is log (plus 1) of total releases reported to TRI of the 243 toxic chemicals that were added to the TRI chemical list in 1995. These total releases include those reported as production waste, transfers offsite, and releases to air, land, water, and underground injection. All models also include a dummy to indicate observations for which the value of *relative production level* was interpolated and observations for which *historical releases trend*, *relative production level*, *log employment*, and *Congressional district LCV score* were missing. The dummy indicating missing observations for *historical releases trend* is interacted with the *annual counter*. $_{93/94}$ denotes a dummy variable whose value is based on establishment status in 1994 (or, if missing, in 1993), before the policy change that occurred in 1995.