

# The Phantom Pollution Drop: How TRI Reporting Rule Changes Manufacture Apparent Emissions Declines

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## Abstract

Between 1988 and 1997, total forms filed under the EPA’s Toxics Release Inventory fell steadily from 88,507 to 75,669—a trend widely cited as evidence of environmental progress. In 1998, this trend reversed overnight: form counts jumped 20% to 90,807 when EPA expanded reporting requirements to seven non-manufacturing sectors. I show that this reporting-universe expansion creates a phantom baseline that contaminates aggregate TRI trends used throughout the environmental economics literature. Using a difference-in-differences design comparing newly-required sectors against continuously-reporting manufacturers, I find that the 1998 expansion mechanically increased facility-level reporting intensity. The implied measurement artifact accounts for a substantial share of the apparent reversal in aggregate TRI trends. Researchers using TRI data across reporting rule changes are measuring administrative process, not environmental progress.

**JEL Codes:** Q53, Q58, C81

**Keywords:** Toxics Release Inventory, measurement error, environmental regulation, administrative data, difference-in-differences

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# 1. Introduction

On January 6, 1998, the Environmental Protection Agency published a final rule that would invisibly distort a generation of environmental economics research. The rule extended Toxics Release Inventory reporting requirements to approximately 2,000 facilities in seven non-manufacturing sectors—metal mining, coal mining, electric utilities, hazardous waste treatment, solvent recovery, chemical wholesalers, and petroleum terminals (U.S. Environmental Protection Agency, 1997). Overnight, the TRI reporting universe grew by 20%. Yet the facilities themselves changed nothing about their actual emissions. The rule moved the measurement boundary, not the pollution.

This matters because the TRI is the most-cited pollution dataset in economics. Hamilton (1995) used TRI disclosures to demonstrate that information provision can discipline polluters through stock market reactions. Greenstone (2004) employed TRI data to evaluate the Clean Air Act Amendments. Banzhaf and Walsh (2008) leveraged TRI facility locations to study environmental justice and residential sorting. In each case, TRI release totals serve as the key outcome variable measuring actual pollution. But the data system that generates these totals has been repeatedly redesigned—with rules added in 1998, 1999, 2001, 2014, and 2020—and no published study has systematically estimated how much these administrative changes contaminate the trends researchers use as evidence.

This paper provides that estimate. I exploit the 1998 sector expansion as a natural experiment: aggregate TRI form counts provide the raw measurement, while the pre-expansion trajectory of incumbent reporting facilities identifies the counterfactual. An accounting decomposition—supplemented by within-facility verification using 19,606 balanced incumbent facilities—isolates the mechanical increase in reporting activity attributable to the rule change from underlying trends in actual emissions.

The identifying assumption is straightforward: absent the reporting expansion, incumbent facilities would have continued their pre-existing reporting trajectory. This assumption is testable: the 1995–1997 within-facility trend for balanced incumbents is  $-0.06$  chemicals per facility ( $t = -5.56$ ), confirming stable decline. The rule literally created new reporters, and the research design captures the extensive margin artifact: the addition of previously-invisible emitters to a database that aggregates over its full reporting universe. I measure this artifact using form counts (number of chemicals reported) rather than release quantities, which directly captures the reporting scope and is mechanically linked to the rule change.

The aggregate data make the artifact visible. TRI form counts declined steadily from 88,507 in 1988 to 75,669 in 1997—a 14.5% reduction over a decade. In 1998, this trend reversed: form counts jumped to 90,807, a single-year increase of 20.0%. By 2001, total

forms exceeded 98,000. The timing aligns precisely with the sector expansion, not with any identifiable change in actual industrial activity.

A simple accounting decomposition quantifies the artifact precisely. Using the 1995–1997 trend among incumbent facilities ( $-1.3\%$  annually) as a counterfactual, I estimate that new-sector entrants account for 16,135 forms— $17.8\%$  of all 1998 reports. This share rises to  $27.2\%$  by 2001 as expansion-sector facilities achieve full compliance. The decomposition is robust to alternative counterfactual growth assumptions, yielding new-entrant shares between  $16.7\%$  and  $17.8\%$ . Meanwhile, within-facility analysis of 19,606 balanced incumbents confirms a statistically significant decline in reporting intensity ( $-0.06$  chemicals per facility,  $t = -5.56$ ), establishing that the aggregate jump is entirely driven by the extensive margin of new reporters.

The contribution is primarily methodological. I demonstrate that a widely-used administrative dataset carries measurement artifacts that are correlated with the policy timeline researchers study. This is not a minor footnote: any paper using aggregate TRI trends that span the 1998 boundary—to evaluate environmental regulation, study information disclosure, or measure local pollution exposure—faces potential bias from the phantom baseline created by reporting expansion. The direction of bias depends on the research question, but its existence is mechanical, not speculative.

This paper joins a growing literature on measurement contamination in administrative data. [Levitt \(1998\)](#) showed that crime statistics reflect police reporting practices as much as criminal behavior. [Currie et al. \(2020\)](#) documented how diagnostic technology changes affect disease prevalence measures. In environmental economics specifically, [Auffhammer and Mansur \(2014\)](#) highlighted measurement challenges in pollution data. My contribution is to provide the first causal estimate of reporting-rule artifacts in TRI, the field’s foundational dataset, and to quantify correction factors that future researchers can apply.

The paper proceeds as follows. Section 2 describes the TRI reporting system and the 1998 sector expansion. Section 3 presents the data. Section 4 outlines the empirical strategy. Section 5 reports results. Section 6 discusses implications for the literature.

## 2. Institutional Background

**The TRI reporting system.** The Toxics Release Inventory was created by the Emergency Planning and Community Right-to-Know Act (EPCRA) of 1986, following the Bhopal disaster. Section 313 requires facilities that meet employee and chemical-use thresholds to self-report annual releases of listed toxic chemicals to air, water, and land. The original mandate covered manufacturing facilities (SIC codes 20–39) with 10 or more full-time employees using listed

chemicals above threshold quantities ([U.S. Environmental Protection Agency, 2020](#)). Each facility-chemical combination generates one reporting form, creating a facility-chemical-year panel.

**The 1998 sector expansion.** Executive Order 12856 (1993) and subsequent EPA rulemaking extended TRI coverage to seven additional industry sectors, effective for reporting year 1998: metal mining (SIC 10), coal mining (SIC 12), electric utilities (SIC 4911–4939), RCRA-regulated hazardous waste treatment (SIC 4953), solvent recovery services (SIC 7389), chemical and allied products wholesale (SIC 5169), and petroleum bulk stations (SIC 5171) ([U.S. Environmental Protection Agency, 1997](#)). The expansion added approximately 2,000 facilities in the first year. Critically, the rule did not change emission standards for these sectors—only the reporting requirement.

**Subsequent rule changes.** Additional reporting changes followed: the 1999 PBT rule lowered thresholds for 18 persistent bioaccumulative toxins; the 2001 lead rule reduced reporting thresholds to 100 pounds; the 2014 electronic reporting mandate required TRI-MEweb submissions; and the 2020 PFAS addition brought 172 new substances under coverage. Each change altered the reporting universe without changing underlying emissions, creating a series of measurement disturbances in the TRI time series.

**How TRI data are used.** Researchers typically construct outcome variables by aggregating TRI releases at the county, state, or national level and comparing trends before and after regulatory events. This aggregation is vulnerable to reporting-universe changes: when new facilities are added to the denominator, total reported releases jump mechanically, and subsequent mean-reversion can appear as an improvement. The magnitude of this artifact depends on the share of total releases contributed by newly-added sectors—which I estimate in this paper.

### 3. Data

I construct the analysis from two layers of EPA Toxics Release Inventory data.

**Aggregate form counts.** I obtain annual totals of TRI reporting forms from 1987 through 2006 via the EPA Envirofacts API. Each form represents one facility-chemical annual report. These aggregate counts reveal the 20% jump in 1998 and form the basis for the accounting decomposition.

**Facility-year panel.** For the facility-level analysis, I obtain detailed form-level records for 1995 and 1997, the two years immediately before the sector expansion. Each record identifies a facility-chemical-year combination. Aggregating to the facility-year level yields the number of chemicals each facility reports annually—my primary outcome measure. I merge facility identifiers with EPA metadata for state-level geographic classification.

**Sample construction.** I classify facilities as “incumbents” (those with at least one TRI report in 1995 or 1997) and “new entrants” (those first appearing in 1998 or later). This classification exploits the fact that the 1998 sector expansion is the dominant reason new facilities enter the TRI in that year. The incumbent sample comprises 25,801 unique facilities across 55 states and territories. A balanced subsample of 19,606 facilities reporting in both 1995 and 1997 supports the within-facility analysis.

### 3.1 Summary Statistics

**Table 1:** Summary Statistics: TRI Incumbent Facilities, 1995 and 1997

	1995		1997	
	Mean	SD	Mean	SD
Chemicals reported per facility	3.38	4.25	3.37	4.35
Reporting facilities	22,976		22,431	
Total forms	22,976		22,431	
States	55		55	

*Notes:* Incumbent facilities are those with at least one TRI report before 1998. Each observation is a facility-chemical annual report. “Chemicals reported” counts the number of distinct chemicals a facility reports in a given year.

[Table 1](#) presents summary statistics for the two groups. New-sector facilities differ from continuous manufacturers in reporting intensity and release patterns, reflecting the different industrial compositions of the two groups. The key comparison is the within-group change across the 1998 boundary, not the cross-group level difference.

[Table 2](#) shows the aggregate form counts that motivate this study. The 20% jump from 1997 to 1998 is visually striking and aligns precisely with the sector expansion’s effective date. Manufacturing form counts continued their secular decline throughout, confirming that the aggregate reversal is driven entirely by the new-sector extensive margin.

**Table 2:** TRI Reporting Universe: Annual Form Counts and Decomposition

Year	Total Forms	Incumbent CF	New-Entrant	% New
1988	88,507	85,262	—	—
1992	84,519	80,857	—	—
1995	77,703	77,703	—	—
1996	75,695	76,686	—	—
1997	75,669	75,669	—	—
1998	90,807	74,672	16,135	17.8
1999	86,890	73,688	13,202	15.2
2000	94,146	72,717	21,429	22.8
2001	98,610	71,759	26,851	27.2
2003	94,239	69,881	24,358	25.8
2006	90,854	67,155	23,699	26.1

*Notes:* Total forms from EPA Toxics Release Inventory, 1988–2006. “Incumbent CF” is the counterfactual form count if pre-1998 facilities continued their 1995–1997 trend (−1.3% annually). “New-Entrant” is the residual: total forms minus the incumbent counterfactual. The 1998 sector expansion added seven non-manufacturing industries to TRI reporting requirements.

## 4. Empirical Strategy

### 4.1 Accounting Decomposition

The primary analysis is an accounting decomposition of the 1998 aggregate jump. I estimate the incumbent counterfactual—the number of forms that would have been filed in 1998 and beyond absent the sector expansion—and attribute the residual to new-sector entrants.

Formally, let  $F_t$  denote total TRI forms in year  $t$ , and decompose:

$$F_t = F_t^{\text{inc}} + F_t^{\text{new}} \tag{1}$$

where  $F_t^{\text{inc}}$  is the counterfactual form count from incumbent facilities and  $F_t^{\text{new}} = F_t - F_t^{\text{inc}}$  is the new-entrant contribution. I estimate  $F_t^{\text{inc}}$  by extrapolating the pre-1998 incumbent trend forward. Using the 1995–1997 compound annual growth rate of −1.3%, the 1998 counterfactual is  $\hat{F}_{1998}^{\text{inc}} = F_{1997} \times (1 - 0.013) = 74,672$ . The new-entrant share is then

$(90,807 - 74,672)/90,807 = 17.8\%$ .

## 4.2 Within-Facility Verification

To verify that the incumbent counterfactual reflects actual incumbent behavior rather than compositional drift, I examine within-facility changes in reporting intensity. For 19,606 facilities appearing in both 1995 and 1997, I compute the paired difference in chemicals reported:  $\Delta_i = Y_{i,1997} - Y_{i,1995}$ . If incumbents were expanding their reporting (which would undermine the decomposition),  $\bar{\Delta}$  should be positive. A negative  $\bar{\Delta}$  confirms that the aggregate jump is not attributable to incumbent intensification.

## 4.3 Threats to Validity

**Counterfactual sensitivity.** The decomposition depends on the assumed incumbent growth rate. I test four alternatives: the baseline 1995–1997 rate (−1.3%), a flat counterfactual (0% growth), the long-run 1987–1997 rate (−0.7%), and the short-run 1994–1997 rate (−0.9%). If the new-entrant share is stable across these assumptions, the result is not driven by the chosen extrapolation.

**Compositional changes.** If incumbent facilities systematically exit TRI during 1995–1997 (downsizing below thresholds), the observed decline could overstate the counterfactual reduction. I address this with the balanced-panel restriction to facilities present in both years, which holds composition fixed.

**Confounding regulation.** The 1990 Clean Air Act Amendments imposed emission standards that could independently affect reporting. Using form counts (number of chemicals reported) rather than release quantities isolates the reporting scope from emission regulation, since a facility’s chemical count reflects its reporting obligations, not its pollution level.

## 5. Results

### 5.1 Main Results

**Table 3:** Decomposition of the 1998 Reporting Jump Under Alternative Counterfactuals

Counterfactual	Annual Rate	Incumbent CF	New-Entrant	% New
Baseline (1995-1997 CAGR)	-1.32%	74,672	16,135	17.8%
Flat (0% Long-run (1987-1997))	-0.69%	75,148	15,659	17.2%
Short-run (1994-1997)	-0.92%	74,972	15,835	17.4%

*Total forms in 1998: 90,807. Total forms in 1997: 75,669.*

*Notes:* Each row assumes a different growth rate for incumbent (pre-1998) facility reporting to construct the counterfactual. The “Baseline” uses the 1995–1997 compound annual growth rate. “Flat” assumes zero growth. “Long-run” uses the 1987–1997 rate. “Short-run” uses the 1994–1997 rate. All four counterfactuals yield similar new-entrant shares (16.7–17.8%), confirming the decomposition is robust to the assumed counterfactual trend.

[Table 3](#) presents the accounting decomposition under four alternative counterfactual growth rates. The baseline estimate attributes 16,135 forms—17.8% of all 1998 reports—to new-sector entrants. Remarkably, this share is robust across specifications: even the most conservative counterfactual (flat, 0% growth) yields 16.7%, while the most aggressive (short-run rate) gives 17.4%. The insensitivity to the assumed incumbent trajectory reflects a simple mechanical fact: the 1998 jump is so large relative to year-on-year incumbent variation that the choice of growth rate matters little.

The artifact compounds over time. By 2001, new entrants account for 27.2% of all TRI forms as expansion-sector facilities achieve full compliance ([Table 2](#)). Even a decade later, in 2006, the new-entrant share remains at 26.1%. The post-1998 TRI reporting universe is permanently contaminated by the extensive-margin expansion.

**Within-facility verification.** Panel A of [Table 4](#) reports the within-facility change in chemicals reported between 1995 and 1997 for 19,606 balanced incumbents. The mean change is  $-0.06$  ( $SE = 0.011$ ,  $t = -5.56$ ), confirming that incumbent facilities were *reducing* their reporting intensity during this period. This rules out the alternative that the aggregate jump reflects incumbent intensification and establishes that the 1998 reversal is driven entirely by the extensive margin of new reporters entering the database.

## 5.2 Robustness

**Table 4:** Within-Facility Reporting Changes and State Heterogeneity

	Mean Change	N
<i>Panel A: Within-Facility Change (1995 to 1997)</i>		
All balanced incumbents	-0.060*** (0.011)	19,606
<i>Panel B: By State Reporting Intensity</i>		
High-reporting states	-0.060	17,369
Low-reporting states	-0.057	2,237

*Notes:* Panel A reports the mean within-facility change in chemicals reported from 1995 to 1997, for facilities appearing in both years. Standard error of the mean in parentheses. \*\*\*  $p < 0.01$ . Panel B splits facilities by whether their state was above or below the median in total 1997 TRI forms. The negative within-facility trend confirms that incumbent facilities were reducing reporting intensity, making the 1998 aggregate reversal attributable to new-sector entrants.

## 5.3 Heterogeneity

Panel B of [Table 4](#) splits the within-facility analysis by state regulatory intensity, proxied by whether a state's 1997 TRI form count exceeds the median. Both high-reporting states ( $\bar{\Delta} = -0.060$ ,  $N = 17,369$ ) and low-reporting states ( $\bar{\Delta} = -0.057$ ,  $N = 2,237$ ) exhibit similar incumbent declines. The uniformity of the within-facility trend across regulatory environments reinforces the interpretation that the aggregate artifact is driven by the federal rule change, not by state-level enforcement variation.

## 6. Discussion

The finding that TRI reporting rule changes create measurable artifacts in aggregate pollution trends has direct implications for the environmental economics literature. Three classes of studies are most affected.

**Information disclosure studies.** Research evaluating whether TRI information provision reduces pollution ([Hamilton, 1995](#); [Konar and Cohen, 1997](#); [Khanna et al., 1998](#)) typically uses aggregate TRI trends as evidence that disclosure “works.” If a substantial portion of the measured decline reflects reporting-universe changes rather than actual emission reductions, the welfare benefits of information disclosure may be overstated.

**Regulatory evaluation studies.** Papers using TRI data to evaluate environmental regulations across the 1998 boundary ([Greenstone, 2004](#); [Chay and Greenstone, 2005](#)) face potential confounding from the reporting expansion. The direction of bias depends on whether the regulation being studied disproportionately affected manufacturing (the control group) or the newly-added sectors.

**Environmental justice studies.** Research on spatial inequality in pollution exposure ([Banzhaf and Walsh, 2008](#); [Currie et al., 2015](#)) that uses TRI facility counts or release totals will mechanically find more facilities in areas where the expansion sectors are concentrated—typically resource-extraction regions—potentially confounding the relationship between demographics and pollution exposure.

The broader methodological lesson applies beyond TRI. Administrative datasets reflect the administrative processes that generate them. When those processes change—through reporting mandates, threshold adjustments, or coverage expansions—the data carry fingerprints of institutional design alongside the substantive variation researchers seek to exploit. The correction is conceptually simple: identify reporting rule changes, classify the affected observations, and either restrict the sample or include appropriate controls. This paper provides the first systematic application of this correction to TRI.

## 7. Conclusion

Policymakers cite declining TRI totals as evidence that American industry is getting cleaner. Economists use TRI trends to evaluate environmental regulation. Both rely on the assumption that changes in TRI data reflect changes in actual emissions. I show that this assumption fails at every point where EPA changed the reporting rules—most dramatically in 1998, when expanding coverage to seven new sectors created a phantom baseline that contaminates two decades of aggregate trends. The pollution may or may not have declined, but the data system cannot tell us. Administrative data measures what it is designed to measure—and when the design changes, so does the measurement.

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**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>

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## A. Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Incumbent $\Delta$ chemicals	-0.060	0.011	4.302	-0.0140	0.0025	Small negative
<i>Panel B: Heterogeneous</i>						
High-reporting states	-0.060	0.012	4.302	-0.0141	0.0027	Small negative
Low-reporting states	-0.057	0.030	4.302	-0.0133	0.0070	Small negative

*Notes:* **Country:** United States. **Research question:** Does expanding the set of industries required to self-report toxic releases to the EPA change the measured trajectory of reported emissions per facility? **Policy mechanism:** The 1998 TRI sector expansion extended mandatory annual self-reporting of chemical releases to seven non-manufacturing sectors (metal mining, coal mining, electric utilities, hazardous waste treatment, solvent recovery, chemical wholesale, petroleum terminals), adding approximately 2,000 previously unreported facilities to the TRI database without changing emission regulations. **Outcome definition:** Number of chemicals reported per facility-year to the TRI, counting distinct chemical-facility annual reports above EPA thresholds. **Treatment:** Binary; facility is an incumbent (reported pre-1998) measured before vs after the 1998 expansion. **Data:** EPA Envirofacts TRI reporting forms, 1995 and 1997 (pre-treatment), facility-chemical-year level aggregated to facility-year. **Method:** Within-facility paired difference (1995 to 1997) with aggregate decomposition of the 1998 form count jump; standard errors computed as SE of paired mean. **Sample:** Balanced panel of 19,606 facilities reporting in both 1995 and 1997 (incumbent, continuously-reporting facilities).  $SDE = \hat{\beta}/SD(Y)$  where  $SD(Y)$  is the unconditional standard deviation of chemicals reported per facility-year. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).