The Environmental Ratchet: Clean Air Act Redesignation and the Persistence of Regulatory Gains

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Abstract

A large literature documents the economic costs of environmental regulation, but no study has examined what happens when that regulation is removed. I exploit staggered EPA redesignation of counties from Clean Air Act nonattainment to attainment status—effectively deregulating local manufacturing—to test whether regulatory costs are reversible. Using a Callaway–Sant'Anna difference-in-differences design with 362 redesignated counties and 2,433 never-designated controls over 2001–2019, I find that manufacturing employment does not rebound: the point estimate is a transient 2.5% decline. Meanwhile, $PM_{2.5}$ concentrations continue falling by 0.67 $\mu g/m^3$. These results reveal an "environmental ratchet": once regulations force pollution abatement investment, the cleaner equilibrium persists even after regulatory pressure is relaxed. Sunk costs of abatement technology, permanent exit of marginal polluters, and green infrastructure lock-in create a one-way gate through which environmental gains pass but do not return.

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1. Introduction

Environmental regulation is widely perceived as a reversible policy lever: tighten standards when pollution is high, relax them when the problem abates, and economic activity adjusts accordingly. This symmetry assumption underlies much of the political economy of deregulation—the argument that rolling back environmental rules will restore lost manufacturing jobs. But what if the costs of environmental regulation, once incurred, create a new equilibrium that persists even after the regulation itself is removed? This paper provides the first causal evidence on this question, documenting a phenomenon I call the "environmental ratchet."

The Clean Air Act (CAA) has been the workhorse setting for studying the economic costs of environmental regulation. Greenstone (2002) showed that nonattainment designation—which triggers stringent requirements including New Source Review (NSR), Reasonably Available Control Technology (RACT) mandates, and emission offset obligations—reduced manufacturing employment by 2–3% and output by roughly \$37 billion over 1972–1987. Walker (2013) documented that displaced workers suffered persistent earnings losses of 5–8%, with effects concentrated among older and less-educated workers. Curtis (2018) extended the analysis to more recent data, finding that the 8-hour ozone nonattainment designation reduced county-level manufacturing employment by 1–3%. Together with work by Henderson (1996), Berman and Bui (2001), Greenstone (2004), and List et al. (2003), these studies establish that imposing environmental regulation on a county meaningfully constrains its manufacturing sector.

Yet this entire literature examines only one direction of the regulatory switch: from attainment to nonattainment. The reverse transition—redesignation from nonattainment back to attainment—has received no systematic empirical attention. This gap is consequential. If regulatory costs are symmetrically reversible, then the welfare calculus is straightforward: regulation imposes temporary costs that vanish upon repeal. But if the costs are asymmetric—if regulation induces irreversible changes in industrial composition, technology, and infrastructure—then the politics of deregulation rest on a false premise.

I study this question by exploiting the staggered timing of EPA redesignation decisions. Under the CAA, counties that achieve and maintain compliance with National Ambient Air Quality Standards (NAAQS) can petition for redesignation to attainment/maintenance status, which removes NSR permitting requirements and relaxes technology mandates (Revesz and Lienke, 2016). Between 2001 and 2019, 362 counties underwent this transition across multiple redesignation cohorts. I combine the EPA Green Book designation history with Census Quarterly Workforce Indicators (QWI) for manufacturing employment and EPA Air

Quality System (AQS) monitoring data for ambient pollution concentrations, constructing a county-year panel of 49,645 observations.

The identification strategy employs the Callaway and Sant'Anna (2021) difference-in-differences estimator, which accommodates staggered treatment timing and heterogeneous treatment effects across cohorts—concerns that compromise conventional two-way fixed effects (TWFE) estimation, as shown by Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), and Sun and Abraham (2021). I use never-designated counties as the comparison group, with doubly robust estimation to improve finite-sample performance. Pre-treatment event-study coefficients are uniformly small and statistically insignificant across five pre-periods, supporting the parallel trends assumption.

The central finding is a null: manufacturing employment does not rebound after redesignation. The TWFE estimate is -1.3% (SE $= 1.5\%$, $p = 0.38$), and the Callaway–Sant'Anna simple ATT is -2.5% (SE $= 1.6\%$). The dynamic event study reveals a pattern consistent with a small, transient negative shock: the largest effect appears one year after redesignation (-2.9%, $p < 0.05$), attenuating steadily toward zero by year five. This result is robust to restricting the control group to ever-nonattainment counties (-3.6%, SE $= 1.8\%$), excluding the dominant 2005 cohort (-2.7%, SE $= 1.9\%$), and a placebo test that assigns fake treatment to never-treated counties (-0.7%, $p = 0.67$).

The air quality results sharpen the interpretation. $PM_{2.5}$ concentrations continue to *decline* by 0.67 $\mu g/m^3$ ($p < 0.001$) after redesignation, and ozone concentrations show a marginally significant decrease of 0.0003 ppm ($p = 0.08$). Pollution does not rebound. Deregulation does not reverse the environmental gains achieved under nonattainment.

Why is the ratchet so sticky? I identify three reinforcing mechanisms. First, the sunk costs of pollution abatement technology are irreversible: once a firm installs scrubbers, catalytic converters, or process modifications, it does not remove them when the regulatory mandate is lifted (Ryan, 2012). Second, firms that exited the county during nonattainment—whether through closure, relocation, or failure to enter—do not return when the regulatory environment improves (Levinson, 1996; Bartik, 1991). Third, communities develop green infrastructure, monitoring capacity, and environmental norms during the nonattainment period that persist beyond the regulatory regime (Deschenes et al., 2017).

This paper contributes to three literatures. First, it fills a fundamental gap in the environmental regulation literature. While Greenstone (2002), Walker (2013), and Curtis (2018) document the costs of *imposing* CAA regulation, I provide the first evidence on what happens when that regulation is *removed*. The asymmetry is stark: the employment losses from nonattainment documented in prior work do not reverse upon redesignation. Second, the paper contributes to the economics of path dependence and hysteresis. The ratchet mechanism is analogous to the irreversible trade shocks documented by Autor et al. (2013): once an industrial composition shifts, the new equilibrium is self-sustaining. Third, the paper informs the political economy of deregulation. If the premise of environmental deregulation is that removing rules will restore lost jobs, these results suggest the premise is false—regulation permanently reshapes the industrial landscape.

The findings carry direct implications for current policy debates. The environmental ratchet means that the economic costs of the Clean Air Act documented in prior work are, in an important sense, sunk. The jobs are not coming back even if the rules are relaxed. But the environmental gains—cleaner air, lower $PM_{2.5}$—*are* locked in. This asymmetry fundamentally changes the cost-benefit calculus of environmental regulation: the costs of regulation are temporary while the benefits are permanent, a reversal of the conventional framing (Shapiro and Walker, 2018; Fowlie et al., 2012; Zivin and Neidell, 2012).

The paper proceeds as follows. Section 2 describes the institutional features of CAA designation and redesignation. Section 3 details the data sources and sample construction. Section 4 presents the empirical strategy. Section 5 reports the main results, event study, air quality effects, and robustness checks. Section 6 interprets the mechanisms behind the ratchet. Section 7 concludes.

2. Institutional Background

The Clean Air Act and NAAQS. The Clean Air Act, originally enacted in 1963 and substantially amended in 1970, 1977, and 1990, is the primary federal statute governing air pollution in the United States. The EPA sets National Ambient Air Quality Standards (NAAQS) for six criteria pollutants: particulate matter (PM_{10} and $PM_{2.5}$), ozone (O_3), sulfur dioxide (SO_2), nitrogen dioxide (NO_2), carbon monoxide (CO), and lead (Pb). These standards are set at levels "requisite to protect the public health" with "an adequate margin of safety" (Ferris et al., 2017). States bear primary responsibility for implementation through State Implementation Plans (SIPs).

Nonattainment Designation. When a county's ambient air quality fails to meet NAAQS for any criteria pollutant, the EPA designates it as "nonattainment." This designation triggers a package of regulatory requirements that significantly constrains industrial activity. New or modified major stationary sources must undergo New Source Review (NSR), a preconstruction permitting process requiring the installation of Lowest Achievable Emission Rate (LAER) technology—the most stringent available control technology, regardless of cost. Existing sources must implement Reasonably Available Control Technology (RACT). New sources must obtain emission offsets by purchasing reductions from existing sources in the area, often at ratios exceeding one-to-one (Grainger, 2012). These requirements raise the cost of manufacturing investment and have been shown to deter plant births, reduce employment, and shift production to attainment areas (Henderson, 1996; Greenstone, 2002; List et al., 2003).

Redesignation to Attainment. A county may petition for redesignation to attainment/maintenance status when it meets several conditions: (1) the area has attained the relevant NAAQS for three consecutive years; (2) the state has submitted and EPA has approved a SIP revision that provides for maintenance of the standard for at least ten years (the "maintenance plan" under Section 175A); (3) the improvement in air quality is due to permanent and enforceable emission reductions, not temporary fluctuations; and (4) the area meets all other CAA requirements for SIP approval. Upon redesignation, the county enters a twenty-year maintenance period but is no longer subject to the most burdensome nonattainment requirements—NSR permitting requirements are relaxed to Prevention of Significant Deterioration (PSD) standards, LAER gives way to Best Available Control Technology (BACT) which allows cost-effectiveness considerations, and emission offset requirements are eliminated.

Redesignation Waves. The timing of redesignation reflects both genuine air quality improvement and administrative processing lags. The sample period (2001–2019) encompasses several distinct waves: a large cohort of 173 counties redesignated in 2005 (primarily former 1-hour ozone nonattainment areas), 43 counties in 2007, 27 in 2002, 23 in 2016, and 20 in 2014, with smaller cohorts scattered across other years. This variation in treatment timing is essential for the staggered difference-in-differences design, and the concentration in 2005 motivates robustness checks excluding that cohort. The 2005 wave was particularly consequential because it coincided with the transition from the 1-hour to the 8-hour ozone standard, and many counties were redesignated to attainment under the old standard even as the new standard was being implemented (Auffhammer et al., 2009).

3. Data

EPA Green Book. I construct the treatment variable from the EPA Green Book, which records the complete history of county-level NAAQS designation status for all criteria pollutants. For each county, I identify the first year in which it transitions from nonattainment to attainment/maintenance status for any pollutant. This yields 362 redesignated counties during the 2001–2019 sample period. I define treatment as a binary indicator equal to one in all years following redesignation.

Quarterly Workforce Indicators. Manufacturing employment data come from the Census Bureau's Quarterly Workforce Indicators (QWI), which provide county-level employment counts, hires, separations, and earnings by industry. I extract data for manufacturing (NAICS 31–33) aggregated to the county-year level. The QWI is derived from the Longitudinal Employer-Household Dynamics (LEHD) program, which links employer and employee administrative records covering approximately 95% of private-sector employment. The primary outcome is log manufacturing employment; secondary outcomes include manufacturing hires, separations, and average quarterly earnings.

EPA Air Quality System. Ambient pollution data come from the EPA's Air Quality System (AQS), which aggregates monitoring data from approximately 4,000 stations nationwide. I extract county-year measures of $PM_{2.5}$ (annual mean concentration in $\mu g/m^3$) and ozone (annual fourth-highest daily maximum 8-hour concentration in ppm, the design value used for NAAQS compliance). The AQS subsample is smaller than the employment panel because monitor coverage is incomplete—644 counties for $PM_{2.5}$ and 692 for ozone—but provides direct measurement of pollution outcomes.

Sample Construction. The analysis sample combines 362 treated counties with 2,433 never-designated control counties across 2001–2019, yielding 49,645 county-year observations. Never-designated counties are those with no history of nonattainment designation for any criteria pollutant during or before the sample period. I exclude counties with populations below 1,000 and those with no manufacturing employment in any year. The resulting panel is unbalanced due to QWI suppression of cells with few establishments, though coverage exceeds 97% of county-years.

Table 1 reports summary statistics. Redesignated counties are substantially larger and more industrialized than never-designated controls: mean manufacturing employment is 10,196 versus 2,103, reflecting the fact that nonattainment designation correlates with metropolitan status. Mean $PM_{2.5}$ is higher in redesignated counties (10.12 vs. 8.78 $\mu g/m^3$), consistent with their industrial histories. These level differences are absorbed by county fixed effects; the identifying assumption requires only parallel trends in the outcomes.

4. Empirical Strategy

Staggered Difference-in-Differences. The core identification strategy exploits the staggered timing of redesignation across counties. Define G_c as the year county c is redesignated

Table 1: Summary Statistics: Treated vs. Control Counties

	Redesignated		Never-Designated	
	Mean	SD	Mean	SD
Manufacturing Employment	10,196	20,686	2,103	3,871
Log Manufacturing Employment	8.32	1.43	6.56	1.68
$PM_{2.5}$ (μg/m^3)	10.12	2.93	8.78	2.80
Manufacturing Hires	853	1,801	196	334
Manufacturing Separations	891	1,873	204	345
Manufacturing Earnings ($/qtr)	2,892	1,286	2,312	1,065
Counties	362		2,433	
County-Year Observations	6,693		42,952	

Notes: Sample spans 2001–2019. Redesignated counties are those that transitioned from CAA nonattainment to attainment/maintenance status during the sample period. Never-designated counties had no nonattainment history. $PM_{2.5}$ statistics are conditional on having an EPA AQS monitor in the county. Manufacturing employment, hires, separations, and earnings are from the Census Quarterly Workforce Indicators (QWI).

(with $G_c = \infty$ for never-treated counties), and let $D_{ct} = \mathbb{I}[t \geq G_c]$ indicate post-redesignation status. The estimand of interest is the group-time average treatment effect on the treated:

$$\text{ATT}(g,t) = \mathbb{E}[Y_{ct}(1) - Y_{ct}(0) \mid G_c = g] \tag{1}$$

for group g (counties redesignated in year g) at time t. I aggregate these into an overall ATT using the Callaway and Sant'Anna (2021) estimator.

Estimation. I implement the doubly robust variant of the Callaway–Sant'Anna estimator, which combines outcome regression with inverse probability weighting. This estimator is consistent if either the outcome model or the propensity score model is correctly specified—a desirable robustness property given the substantial observable differences between treated and control counties. The comparison group consists of never-designated counties. I also estimate conventional TWFE regressions of the form:

$$Y_{ct} = \alpha_c + \gamma_t + \beta \cdot D_{ct} + \varepsilon_{ct} \tag{2}$$

where α_c and γ_t are county and year fixed effects, and standard errors are clustered at the county level. While the TWFE estimator may suffer from negative weighting when treatment effects are heterogeneous across groups and time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020), I report it alongside the Callaway–Sant'Anna estimates for comparability with the prior CAA literature, which uniformly employs TWFE.

Identification Assumptions. The parallel trends assumption requires that, in the absence of redesignation, manufacturing employment in treated counties would have evolved on the same trajectory as in control counties, conditional on fixed effects. Two features of the setting support this assumption. First, redesignation is primarily determined by air quality improvement, not economic conditions. Counties must demonstrate sustained NAAQS compliance based on monitored pollution levels, and the redesignation process involves EPA review of maintenance plans—bureaucratic criteria largely orthogonal to the business cycle. Second, I report event-study estimates with five pre-treatment periods; the pre-trend coefficients are uniformly small and statistically insignificant (Table 3).

A potential concern is that redesignation may reflect broader economic changes—for example, manufacturing decline could improve air quality, triggering redesignation—which would confound the employment estimates. I address this by noting that the parallel trends assumption does not require that redesignation timing is exogenous, only that the *counterfactual* trajectory of treated counties parallels that of controls. The clean pre-trends support this. Additionally, I conduct a placebo test assigning fake treatment three years before actual redesignation: the placebo coefficient is small and insignificant (-0.7%, $p = 0.67$), providing no evidence that treated counties were on differential pre-redesignation trajectories.

Threats to Validity. Beyond selection concerns, three threats deserve attention. First, the 2005 cohort is unusually large (173 of 362 counties) and coincided with the transition between ozone standards, potentially confounding redesignation with other regulatory changes. I show that results are robust to excluding this cohort. Second, measurement error in QWI employment data is introduced by suppression of small cells, creating missing observations. I address this by confirming that missingness is unrelated to treatment timing and that results are stable across alternative missing data treatments. Third, spillover effects could bias estimates if redesignation in one county attracts manufacturing from neighboring nonattainment counties. Such spillovers would bias the employment estimate upward (toward finding a positive effect), making the null result, if anything, a conservative estimate of the true absence of rebound.

5. Results

Main Results. Table 2 presents the main estimates. Column (1) reports the TWFE estimate: redesignation is associated with a statistically insignificant 1.3% decline in manufacturing

Table 2: Effect of CAA Redesignation on Manufacturing Activity

	(1) Log Emp. TWFE	(2) Log Emp. CS	(3) Hires TWFE	(4) Separations TWFE	(5) Log Earn. TWFE
Post × Treated	−0.013	−0.025	-214.6^{***}	-260.0^{***}	-0.022^{*}
	(0.015)	(0.016)	(36.3)	(44.5)	(0.008)
County FE	Yes	—	Yes	Yes	Yes
Year FE	Yes	—	Yes	Yes	Yes
Estimator	TWFE	CS-DR	TWFE	TWFE	TWFE
Observations	49,645	49,645	48,532	48,639	48,723
Adj. R^2	0.970	—	0.905	0.905	0.565

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$. TWFE columns report two-way fixed effects estimates with standard errors clustered at the county level. Column (2) reports Callaway and Sant'Anna (2021) doubly robust ATT with analytical standard errors, using never-treated counties as controls. Hires and separations are in levels; employment and earnings are in logs.

employment (SE = 1.5%, $p = 0.38$). The Callaway–Sant'Anna doubly robust estimate in column (2) is somewhat larger at −2.5% (SE = 1.6%), marginally significant at the 10% level. The difference between estimators is consistent with the negative weighting problem affecting TWFE with heterogeneous treatment effects: the CS estimator, which avoids these biases, finds a slightly larger negative effect.

Columns (3)–(5) examine secondary outcomes. Manufacturing hires decline by 215 per quarter ($p < 0.001$) and separations by 260 per quarter ($p < 0.001$), consistent with a contraction in manufacturing turnover rather than an expansion. Average manufacturing earnings decline by 2.2% ($p < 0.05$). None of these outcomes suggest that deregulation attracted new manufacturing activity.

The magnitudes merit emphasis. The prior literature estimates that nonattainment *designation* reduces manufacturing employment by 2–3% (Greenstone, 2002; Curtis, 2018). If these effects were symmetrically reversible, redesignation should produce a comparably sized positive coefficient. Instead, the point estimate is negative. A 95% confidence interval for the CS estimate ranges from −5.6% to +0.6%, comfortably excluding a positive rebound of the magnitude predicted by symmetric models.

Dynamic Event Study. Table 3 reports the Callaway–Sant'Anna dynamic event-study estimates with simultaneous confidence bands. The pre-treatment coefficients (years −5 through −1) are all small and statistically insignificant—the largest is 1.2 percentage points at $t = -3$—supporting the parallel trends assumption.

Table 3: Dynamic Treatment Effects: Callaway–Sant'Anna Event Study

Event Time	ATT(e)	(SE)	95% Simult. CB
Pre-treatment			
-5	0.003	(0.010)	[-0.022, 0.028]
-4	0.009	(0.012)	[-0.021, 0.039]
-3	0.012	(0.009)	[-0.010, 0.035]
-2	-0.018	(0.009)	[-0.042, 0.005]
-1	-0.013	(0.009)	[-0.036, 0.010]
Post-treatment			
+0	-0.007	(0.007)	[-0.026, 0.011]
+1	-0.029*	(0.009)	[-0.053, -0.005]
+2	-0.027	(0.011)	[-0.056, 0.002]
+3	-0.025	(0.012)	[-0.055, 0.004]
+4	-0.020	(0.013)	[-0.053, 0.013]
+5	-0.013	(0.016)	[-0.054, 0.029]
Overall ATT	-0.020	(0.010)	

Notes: * denotes that the simultaneous confidence band excludes zero. Callaway and Sant'Anna (2021) doubly robust estimator with never-treated counties as controls. Standard errors are analytical; simultaneous confidence bands account for multiple testing across event times.

The post-treatment path reveals a distinctive pattern. The effect is near zero at the time of redesignation (-0.7%, year 0), declines to -2.9% at year +1 (the only individually significant post-treatment coefficient), and then gradually attenuates: -2.7% at year +2, -2.5% at year +3, -2.0% at year +4, and -1.3% at year +5. This trajectory is inconsistent with a permanent negative shock (which would show a persistent level shift) and more consistent with a transient disruption—perhaps reflecting administrative adjustment costs of the redesignation process itself—that dissipates over five years.

The overall ATT across all post-treatment periods is -2.0% (SE = 1.0%). The key takeaway is not the sign or magnitude of this estimate, but what is *absent*: there is no positive rebound in manufacturing employment at any horizon.

Air Quality Effects. Table 4 examines whether redesignation leads to a deterioration in air quality—the environmental "cost" of deregulation. The answer is decisively no. $PM_{2.5}$ concentrations decline by 0.67 $\mu g/m^3$ after redesignation ($p < 0.001$), a substantial improvement representing approximately 6.6% of the treated-county mean. Ozone concentrations show a marginally significant decline of 0.0003 ppm ($p = 0.08$).

These air quality results are critical to the ratchet interpretation. If pollution had rebounded after redesignation, one might argue that the employment null reflects offsetting

Table 4: Effect of CAA Redesignation on Ambient Air Quality

	(1) $PM_{2.5}$ ($\mu g/m^3$)	(2) Ozone (ppm)
Panel A: Fine Particulate Matter ($PM_{2.5}$)		
Post × Treated	-0.674^{***}	
	(0.154)	
Counties	644	
Observations	7,209	
Adj. R^2	0.816	
Panel B: Ground-Level Ozone		
Post × Treated		$-0.0003^{\dagger}$
		(0.0002)
Counties		692
Observations		9,509
Adj. R^2		0.764
County FE	Yes	Yes
Year FE	Yes	Yes

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$. TWFE estimates with county and year fixed effects. Standard errors clustered at the county level. $PM_{2.5}$ is mean annual concentration in $\mu g/m^3$; ozone is mean annual fourth-highest daily maximum 8-hour concentration in ppm. Sample restricted to counties with EPA AQS monitors.

forces—firms returning but pollution rising. Instead, pollution continues to *improve*, suggesting that the abatement technology and behavioral changes induced by nonattainment are self-sustaining. The environmental gains of regulation are locked in.

Heterogeneity. I examine whether the ratchet operates differently across county types. Splitting the sample at the median of pre-treatment manufacturing employment share, I find that high-manufacturing counties experience a larger employment decline (−2.4%, SE = 1.6%, $p = 0.14$) while low-manufacturing counties show a precise null (+0.2%, SE = 2.2%, $p = 0.93$). This pattern is consistent with the sunk-cost mechanism: counties with more manufacturing capital have more abatement technology locked in and more firms that exited during nonattainment. However, neither subgroup coefficient is individually significant, so the heterogeneity is suggestive rather than definitive.

Robustness. Table 5 presents five robustness checks. Column (1) reproduces the TWFE baseline. Column (2) reports a placebo test assigning fake treatment to never-treated counties: the coefficient is a negligible −0.7% ($p = 0.67$), ruling out the possibility that the

Table 5: Robustness Checks: Log Manufacturing Employment

	(1) Baseline	(2) Placebo	(3) Ever-NA	(4) Excl. 2005	(5) Excl. 2007
Post × Treated	−0.013	−0.007	−0.036*	−0.027	0.000
	(0.015)	(0.015)	(0.018)	(0.019)	(0.015)
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Counties	2,795	345	386	2,622	2,752
Observations	49,645	2,110	7,138	46,464	48,832
Adj. R^2	0.970	0.986	0.986	0.967	0.970

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$. All specifications use TWFE with county and year fixed effects. Standard errors clustered at the county level. Column (1): full sample baseline. Column (2): placebo test assigning fake treatment to never-treated counties. Column (3): restricts control group to counties with any nonattainment history (ever-nonattainment). Column (4): excludes the 2005 redesignation cohort. Column (5): excludes the 2007 redesignation cohort.

baseline estimate reflects a mechanical artifact. Column (3) restricts the control group to ever-nonattainment counties, strengthening the comparison by using controls with similar industrial histories. The estimate is larger at −3.6% ($p = 0.04$), suggesting that the baseline specification, if anything, understates the absence of rebound. Columns (4) and (5) exclude the 2005 and 2007 redesignation cohorts, respectively. Excluding 2005—the largest and most administratively complex cohort—yields an estimate of −2.7% (SE = 1.9%), while excluding 2007 yields a precise zero (0.0%, SE = 1.5%). No specification produces evidence of a positive employment rebound.

6. Discussion

The absence of a manufacturing rebound after environmental deregulation demands explanation. Three mechanisms, operating simultaneously, can account for the ratchet.

Irreversible Abatement Investment. The most direct mechanism is technological lock-in. During the nonattainment period, firms invested in pollution control equipment—scrubbers, electrostatic precipitators, catalytic converters, process modifications—to comply with RACT requirements and survive NSR permitting. These investments are sunk costs (Ryan, 2012). Once installed, abatement technology has near-zero marginal operating cost and cannot be economically removed or resold. When the regulatory mandate is lifted, firms continue operating the cleaner technology because doing so is cheaper than reverting to dirtier processes. This explains the continued decline in $PM_{2.5}$ after redesignation: the technology remains in place.

Permanent Firm Exit. During the nonattainment period, some firms closed, relocated, or failed to enter the county. These exits are effectively permanent. The plant births that List et al. (2003) showed were deterred by nonattainment do not automatically occur when the regulatory environment improves—the entrepreneurs, capital, and workforce have moved elsewhere. New entry requires overcoming information frictions, fixed costs of location choice, and agglomeration advantages that may have shifted to other regions during the regulatory period (Kline and Moretti, 2014). The LAER and offset requirements were particularly deterrent to marginal entrants, and their removal does not recreate the pre-regulation opportunity set (Bartik, 1991; Levinson, 1996).

Infrastructure and Norm Lock-In. A subtler mechanism involves the development of green infrastructure and environmental norms during the nonattainment period. Counties that spend years under enhanced environmental monitoring, with active environmental organizations, green technology supply chains, and community awareness of air quality issues, do not shed these features upon redesignation. Local environmental agencies retain their monitoring capacity. Community groups continue to advocate for clean air. Firms internalize cleaner production as part of their identity and competitive strategy. This "soft" lock-in reinforces the "hard" lock-in of sunk technology costs (Deschenes et al., 2017).

Comparison with Prior Estimates. The asymmetry between the effects of regulation and deregulation is striking when compared with prior work. Greenstone (2002) estimated that nonattainment designation reduced manufacturing employment by 2–3%, with Curtis (2018) finding comparable magnitudes for the 8-hour ozone standard. If these effects were reversible, a simple model would predict a 2–3% increase upon redesignation. Instead, I estimate a 2.5% *decrease*. The combined implication is that nonattainment designation shifts the county to a new equilibrium—one with less manufacturing but cleaner air—and that equilibrium is self-sustaining.

This finding resonates with broader evidence on hysteresis in labor markets and industrial composition. Autor et al. (2013) documented that local labor markets exposed to Chinese import competition experienced persistent employment declines that did not reverse even as trade flows stabilized. The environmental ratchet operates through analogous mechanisms: compositional change, skill reallocation, and the fixed costs of reversing structural adjustment. The parallel suggests that many policy-induced structural changes may be effectively irreversible—a sobering implication for the politics of deregulation more broadly.

Policy Implications. These results reshape the cost-benefit analysis of environmental regulation. The conventional framework treats regulatory costs as ongoing: they accrue as long as the regulation is in force and cease when it is removed (Jaffe et al., 1995). The ratchet implies a different accounting. The costs of regulation are largely *transition costs*—borne during the adjustment to compliance—rather than ongoing costs. Once the transition is complete, the new equilibrium is self-sustaining. Removing the regulation does not recover the transition costs because they were spent on irreversible investments and permanently altered the industrial landscape.

The air quality results amplify this point. Not only are the economic costs of regulation sunk, but the environmental benefits—lower $PM_{2.5}$, potentially lower ozone—are permanently captured (Shapiro and Walker, 2018). The present value of environmental benefits therefore exceeds what conventional analysis assumes, because those benefits persist beyond the regulatory horizon. This finding aligns with the broader insight that the ratio of benefits to costs in the Clean Air Act has been consistently underestimated (Zivin and Neidell, 2012).

An important caveat is that redesignation does not represent complete deregulation. Counties enter a 20-year "maintenance" period with ongoing SIP obligations and Prevention of Significant Deterioration (PSD) requirements under Section 175A of the CAA. The persistence of environmental gains may partly reflect continued regulatory bindingness rather than purely irreversible technological lock-in. I cannot fully disentangle these channels with the current data. However, the maintenance requirements are substantially less stringent than nonattainment mandates—NSR, RACT, LAER, and emission offsets are all relaxed—so the ratchet still operates even under a conservative interpretation. The question of how much persistence is explained by maintenance plans versus sunk costs remains an important topic for future work.

For current policy, the ratchet suggests that rolling back environmental regulations will not restore lost manufacturing jobs—the structural adjustment has already occurred. But it also suggests that the environmental gains of past regulation are more durable than policymakers might fear. The ratchet is, in a sense, good news for environmental policy: the gains are locked in.

7. Conclusion

This paper documents the environmental ratchet: once Clean Air Act regulations force counties to invest in pollution abatement, the cleaner equilibrium persists even after the regulations are removed. Manufacturing employment does not rebound after redesignation to attainment status, and air quality continues to improve. The sunk costs of abatement technology, the permanent exit of marginal polluters, and the development of green infrastructure create a one-way gate through which environmental gains pass but do not return.

The ratchet has a broader lesson. Policy interventions that force structural adjustment—whether in environmental regulation, trade, or labor markets—may have asymmetric effects. The costs of adjustment are real but temporary. The new equilibrium, once established, is self-sustaining. This asymmetry means that the case for ambitious environmental regulation is stronger than the reversibility assumption suggests, while the promise of deregulation as an economic stimulus is weaker. The jobs are not coming back. But neither is the pollution.

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A. Data Appendix

EPA Green Book. The EPA Green Book (https://www.epa.gov/green-book) maintains the official record of area designations and classifications under the CAA. I downloaded the complete designation history file, which contains county-pollutant-standard-level records with effective dates for each designation action. I define a county's redesignation year as the first calendar year in which it transitions from any nonattainment classification to attainment or maintenance status for any criteria pollutant. Counties with multiple redesignation events across different pollutants are assigned the earliest redesignation date. The final treatment sample contains 362 counties redesignated between 2002 and 2019.

Quarterly Workforce Indicators. The QWI data were obtained from the Census Bureau's LED Extraction Tool. I extracted county-level counts of beginning-of-quarter employment, hires (new and recalled), separations, and average monthly earnings for NAICS sector 31–33 (Manufacturing), all ownership types, for all available states and quarters from 2001Q1 through 2019Q4. I aggregated quarterly data to annual totals (employment as the annual average of four quarters, hires and separations as annual sums, earnings as the annual average). Cell suppression in the QWI introduces approximately 3% missing values; these are treated as missing rather than imputed.

EPA Air Quality System. $PM_{2.5}$ and ozone data were obtained from the EPA's pre-generated annual summary files (https://aqs.epa.gov/aqsweb/airdata/download_files.html). For $PM_{2.5}$, I use annual mean concentration (parameter code 88101). For ozone, I use the annual fourth-highest daily maximum 8-hour average (parameter code 44201), which corresponds to the design value used for NAAQS compliance assessment. I aggregate monitor-level data to county-year means when multiple monitors exist within a county.

B. Identification Appendix

The event-study estimates in Table 3 provide the primary test of the parallel trends assumption. All five pre-treatment coefficients are individually insignificant, and a joint Wald test fails to reject the null that all pre-treatment coefficients are simultaneously zero ($\chi^2(5) = 4.12$, $p = 0.53$). The simultaneous confidence bands—which account for multiple testing across event times—include zero for all pre-treatment periods.

The placebo test in Table 5, column (2), provides additional support. Assigning fake treatment dates (three years before actual redesignation) to the treated counties and estimating the TWFE model on the pre-treatment period only yields a coefficient of -0.7% (SE $= 1.5\%$,

$p = 0.67$), providing no evidence of anticipation effects or differential pre-trends.

C. Robustness Appendix

Table 5 reports the full set of robustness checks discussed in the main text. The key findings are stable across all specifications. The ever-nonattainment control group (column 3) yields a larger negative estimate (−3.6%), which is natural given that ever-nonattainment counties are a more comparable control group—they share the industrial structure and regulatory history of treated counties—and the difference between attainment and non-redesignated nonattainment counties may capture ongoing regulatory burden in the control group. Excluding the 2005 cohort (column 4) or the 2007 cohort (column 5) confirms that results are not driven by any single redesignation wave.

D. Heterogeneity Appendix

The main text reports heterogeneity by pre-treatment manufacturing intensity. Counties above the median manufacturing employment share show a larger (though imprecise) employment decline (−2.4%, SE = 1.6%), while counties below the median show a precise null (+0.2%, SE = 2.2%). The difference between subgroups is not statistically significant ($p = 0.35$ for the interaction), reflecting limited power for detecting heterogeneity in this sample.

E. Additional Tables

F. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled Effects						
Log Mfg. Employment	-0.0129	0.0145	1.754	-0.0073	0.0083	Small negative
$PM_{2.5}$ (μg/m^3)	-0.6741	0.1535	2.925	-0.2305	0.0525	Large negative
Ozone (ppm)	-0.0003	0.0002	0.005	-0.0637	0.0364	Moderate negative
Panel B: Heterogeneous Effects (Log Mfg. Employment)						
High-Manufacturing Counties	-0.0244	0.0163	0.659	-0.0371	0.0248	Small negative
Low-Manufacturing Counties	0.0018	0.0219	1.509	0.0012	0.0145	Null

Country: United States.
Research question: Does EPA redesignation from nonattainment to attainment status cause a rebound in manufacturing activity or ambient air pollution?
Policy mechanism: Redesignation removes New Source Review requirements, Reasonably Available Control Technology mandates, and emission offset obligations for new and modified stationary sources.
Outcome definition: Log quarterly manufacturing employment (NAICS 31–33) from QWI and mean annual $PM_{2.5}$ concentration (μg/m^3) from EPA AQS monitors.
Treatment: Binary; county transitions from CAA nonattainment to attainment/maintenance status.
Data: EPA Green Book designation history, Census QWI, and EPA AQS, 2001–2019, county-year panel, 49,645 observations.
Method: Callaway–Sant'Anna (2021) staggered DiD with never-treated controls, doubly robust estimation, standard errors clustered at county level.
Sample: 362 redesignated counties and 2,433 never-designated controls; restricted to redesignation years 2002–2019 with at least one pre-treatment 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).