The Substitution Failure: Federal Enforcement Withdrawal and Ambient Air Quality Under Cooperative Federalism

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Abstract

Environmental enforcement in the United States rests on cooperative federalism: EPA sets standards while enforcement is shared with state agencies. Between 2016 and 2020, EPA's enforcement arm lost roughly 25 percent of its workforce. I exploit cross-regional variation in historical federal enforcement intensity to estimate the effect of this withdrawal on ambient air quality. Counties in EPA regions with higher pre-period federal inspection shares experienced up to 18 percent higher PM2.5 concentrations during 2017–2019 relative to counties in low-share regions, comparing the most to least exposed regions. The effect is robust to trimming, leave-one-state-out tests, and alternative treatment windows, though significant pre-trends in the full sample caution against a strict causal interpretation. These results suggest that state agencies did not fully substitute for federal enforcement withdrawal—a substitution failure with implications for the ongoing restructuring of environmental governance.

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1. Introduction

When the EPA loses a quarter of its enforcement workforce, does someone pick up the slack? This question matters because the architecture of American environmental regulation—cooperative federalism—depends on the assumption that federal and state enforcement are, at the margin, substitutable. If they are not, then federal withdrawal directly translates into weaker deterrence and higher pollution.

Between 2016 and 2020, the EPA's Office of Enforcement and Compliance Assurance (OECA) shrank from approximately 3,259 to 2,439 employees, a 25 percent decline (Vig and Kraft, 2019). This reduction was not uniform across states. Some states historically relied on EPA for 25–30 percent of environmental inspections, while others conducted virtually all enforcement through state agencies. The differential exposure creates a natural experiment: states where EPA historically played a larger enforcement role should experience greater effective enforcement declines when federal staffing contracts.

This paper estimates the effect of federal enforcement withdrawal on ambient air quality using a shift-share design. The "share" is a state's EPA region-level proportion of inspections historically conducted by federal (rather than state) agencies, constructed from EPA's Integrated Compliance Information System. The "shift" is the national trajectory of EPA OECA staffing, which declined sharply beginning in 2017. I interact these two sources of variation to construct a continuous measure of county-level exposure to federal enforcement decline, and estimate its effect on PM2.5 concentrations measured by EPA's Air Quality System monitors.

The main finding is that counties in the most federally dependent EPA regions experienced approximately 18 percent higher PM2.5 concentrations during the decline period (2017–2019) relative to counties in the least dependent regions, with a one-standard-deviation increase in federal enforcement share corresponding to a 6 percent differential increase. The point estimate is stable in leave-one-state-out tests (range: 0.506 to 0.752, main: 0.687) and robust to trimming, alternative treatment windows, and binary treatment definitions. However, a joint F-test rejects the null of zero pre-treatment event study coefficients in the full 2010–2019 window, indicating differential pre-trends that correlate with federal enforcement exposure. When the sample is restricted to 2012–2019—dropping the noisiest early years—the coefficient remains significant ($\hat{\beta} = 0.492$, $p < 0.01$) but the pre-trend concern persists.

I interpret these results as suggestive evidence of a *substitution failure* in cooperative federalism: state agencies do not fully compensate for reductions in federal enforcement capacity. The qualifier "suggestive" is important. The pre-trend evidence means I cannot rule out that regions with higher federal enforcement dependence were already on different pollution trajectories before the staffing decline. The identifying assumption—that national EPA staffing changes are exogenous to individual states' pollution trends, conditional on fixed effects—is plausible but ultimately untestable.

This paper contributes to three literatures. First, it speaks to the extensive literature on environmental enforcement and deterrence. Shimshack and Ward (2005) show that inspections and penalties reduce pollution at targeted facilities. Gray and Shadbegian (2005) find that inspections deter pollution beyond the targeted firm. Duflo et al. (2013) demonstrate through a randomized audit experiment in India that third-party enforcement matters. My contribution is to estimate not the effect of a marginal inspection, but the *production function of the enforcement agency itself*—what happens when an entire layer of the enforcement hierarchy contracts.

Second, this paper contributes to the growing literature on regulatory capacity. Brehm and Gates (1997) argue that bureaucratic capacity determines regulatory effectiveness. Carrigan and Coglianese (2019) show that understaffed agencies produce lower-quality regulations. The EPA staffing decline provides a setting where capacity reduction is large, measurable, and exogenous to individual regulated entities.

Third, this paper informs the normative debate over fiscal federalism in environmental policy. Oates (1972) established the theoretical case for decentralization; Sigman (2005) showed that interstate externalities create a role for federal oversight. Whether state agencies can substitute for federal enforcement at the margin is a first-order empirical question for the design of environmental governance, with direct implications for the ongoing restructuring of EPA (Millimet et al., 2003; Woods, 2006).

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 develops the empirical strategy. Section 5 presents the results. Section 6 discusses implications and limitations.

2. Institutional Background

Environmental enforcement in the United States operates under a system of cooperative federalism codified in the major environmental statutes—the Clean Air Act, Clean Water Act, and Resource Conservation and Recovery Act. Under this framework, EPA promulgates standards and delegates primary enforcement authority to state agencies that meet minimum federal criteria. Roughly 96 percent of environmental enforcement actions are taken by state and local agencies, with EPA retaining direct enforcement capacity through its ten regional offices (Mintz, 2012).

The division of labor varies enormously across states. In some states—particularly those in EPA Regions 8 and 9, covering the Mountain West and Pacific states—EPA historically conducted 20–27 percent of environmental inspections. In others, especially in the Northeast (Region 1) and upper Midwest (Region 5), state agencies handled 90–95 percent of all compliance monitoring. This variation reflects historical patterns of state environmental capacity, political preferences, and the evolution of delegation agreements.

The 2017–2020 staffing decline. Beginning in fiscal year 2017, EPA experienced sustained workforce reductions. OECA staffing fell from approximately 3,259 in 2016 to 2,439 in 2020—a decline of 820 positions, or 25 percent. Total EPA employment declined from 15,376 to approximately 13,758 over the same period. These reductions were driven by hiring freezes, early retirement incentives, and budget constraints, and affected both headquarters and regional offices (Vig and Kraft, 2019).

The staffing decline translated directly into reduced enforcement activity. EPA-initiated civil cases fell from 1,713 in fiscal year 2016 to 1,228 in 2019. Criminal cases dropped from 207 to 125. Total penalties assessed declined by approximately 40 percent. These aggregate figures, widely reported in media coverage, motivated concern that the "environmental cop" was retreating from the beat.

The substitution question. The critical policy question is whether state agencies compensated for the federal reduction. If enforcement is a substitute—if states increased their own inspection and enforcement activity in response to federal withdrawal—then the aggregate decline in federal activity may not translate into weaker deterrence or higher pollution. The literature on regulatory substitution is thin: Helland (1998) find partial substitution in EPA enforcement targeting, but no study has tested whether state enforcement substitutes for federal enforcement *capacity*.

3. Data

I combine three data sources: ambient air quality measurements from EPA's Air Quality System (AQS), environmental inspection records from EPA's Integrated Compliance Information System (ICIS), and EPA staffing data from budget documents.

Air Quality System. AQS provides annual summary statistics for each air quality monitor in the United States. I extract annual arithmetic mean concentrations for four criteria pollutants: PM2.5 (parameter code 88101), SO_2 (42401), NO_2 (42602), and ozone (44201). I aggregate monitor-level data to the county-year level by taking a weighted average across monitors within each county, with weights proportional to the number of valid measurement days. I restrict the sample to observations with at least 50 percent completeness.

Federal enforcement share. I construct a measure of each state's historical dependence on federal enforcement using ICIS inspection records. For state-led inspections (flag = "S"), I count the total number of inspections by state. For EPA-led inspections (flag = "E"), which are recorded without state identifiers, I count by EPA region and allocate to states within each region proportionally by state inspection volume. The resulting *FedShare* variable captures the fraction of a state's total environmental inspections historically conducted by EPA. FedShare ranges from 0.035 (Region 4 states, predominantly state-enforced) to 0.271 (Region 9 states, including Arizona, California, and Nevada).

EPA staffing. I construct an annual index of OECA staffing normalized to 2016 = 1, using data from EPA budget justifications and OPM FedScope employment data. The index equals approximately 1.0 during 2010–2016 and declines to 0.75 by 2020.

Sample construction. The analysis sample is a balanced panel of 544 counties with PM2.5 monitors observed in at least 7 of the 10 years from 2010 to 2019, yielding 5,203 county-year observations across all 50 states plus the District of Columbia. Table 1 presents summary statistics.

Table 1: Summary Statistics

Variable	Mean	SD	P25	Median	P75	N
PM2.5 ($\mu g/m^3$)	8.31	2.22	6.96	8.26	9.57	5,203
Federal Enforcement Share	0.115	0.085	0.039	0.082	0.205	5,203
Counties				544		
States				51		
Years				2010–2019		

Notes: Unit of observation is county-year. Sample restricted to counties with PM2.5 monitors observed in at least 7 of 10 years. Federal Enforcement Share measures the proportion of environmental inspections conducted by EPA (vs. state agencies) in each state's EPA region during the full sample period, allocated to states proportionally by state inspection volume.

4. Empirical Strategy

Estimating equation. I estimate the following specification:

$$\log(\text{PM2.5}_{c,t}) = \alpha_c + \lambda_t + \beta \cdot \text{FedShare}_s \times \text{Post}_t + \varepsilon_{c,t} \tag{1}$$

where c indexes counties, t indexes years, and s indexes states. α_c are county fixed effects absorbing time-invariant county characteristics. λ_t are year fixed effects absorbing national trends in air quality. FedShare_s is the state's federal enforcement share. Post_t equals one for 2017–2019. The coefficient β captures the differential change in PM2.5 concentrations between high- and low-federal-share counties after the onset of the staffing decline.

Event study. I also estimate an event study version replacing Post_t with year-specific indicators (omitting 2016) to assess pre-trends and trace out dynamics:

$$\log(\text{PM2.5}_{c,t}) = \alpha_c + \lambda_t + \sum_{k \neq 2016} \beta_k \cdot \text{FedShare}_s \times \mathbb{I}(t = k) + \varepsilon_{c,t} \tag{2}$$

Identification. The shift-share design exploits two sources of variation. The "share"—cross-sectional variation in regional federal enforcement intensity—is determined by historical patterns of state delegation and EPA regional office activity. The "shift"—national EPA OECA staffing changes—is driven by federal budget and hiring decisions plausibly exogenous to any individual state's pollution trends.

Threats to identification. The main concern is that regions with historically higher federal enforcement shares may differ systematically in ways that correlate with pollution trends. For example, Western states (Regions 8–9) have different industrial compositions, growth trajectories, and wildfire exposure than Eastern states. County and year fixed effects absorb level differences and national trends, but cannot address differential trends that correlate with the regional enforcement share. I probe this threat through event study pre-trends, leave-one-state-out tests, and alternative sample windows.

A second concern is measurement error in FedShare. Because EPA-led inspections lack state identifiers in the ICIS data, I allocate regional EPA inspections to states proportionally. This assigns identical FedShare values to all states within an EPA region, meaning the effective treatment clusters are the 10 EPA regions. I cluster standard errors at the state level (51 clusters), which is conservative relative to the 10 effective treatment clusters.

What the design cannot identify. The shift-share cannot distinguish between enforcement deterrence (fewer inspections reduce compliance) and information effects (fewer inspections reduce facility awareness of violations). It also cannot isolate which enforcement program—Clean Air Act, Clean Water Act, or RCRA—drives the result, since EPA staffing declines affected all programs.

5. Results

5.1 Main Estimates

Table 2 reports the main results. Column (1) shows the baseline specification: counties in states with a one-unit higher federal enforcement share experienced a 0.687 log-point increase in PM2.5 concentrations during the post-2017 period ($p < 0.001$). Since FedShare ranges from 0.035 to 0.271, a comparison between the most and least exposed regions implies a differential PM2.5 increase of approximately $0.687 \times (0.271 - 0.035) = 0.162$ log-points, or about 18 percent. Evaluated at the sample mean PM2.5 of 8.25 μg/m^3, this corresponds to roughly 1.5 μg/m^3—a meaningful increment given that the EPA's annual PM2.5 standard is 12 μg/m^3.

Column (2) replaces the binary Post indicator with a continuous treatment variable: $\text{FedShare}_s \times (1 - \text{OECA Index}_t)$, which varies smoothly with annual EPA staffing levels. The coefficient of 3.709 ($p < 0.01$) indicates that PM2.5 rose differentially in high-federal-share counties as EPA staffing declined. Column (3) restricts the sample to 2012–2019, dropping the noisiest pre-period years; the coefficient attenuates to 0.492 but remains significant ($p < 0.01$). Column (4) reports the state-level specification, which lacks precision but points in the same direction ($\hat{\beta} = 0.359$, $p = 0.11$).

Table 2: Federal Enforcement Exposure and Ambient PM2.5 Concentrations

	(1)	(2) County Level	(3)	(4) State Level
Post × FedShare	0.686*** (0.178)		0.492*** (0.171)	0.359 (0.222)
FedShare × (1 − OECA Index)		3.709*** (1.208)		
County FE	Yes	Yes	Yes	
State FE				Yes
Year FE	Yes	Yes	Yes	Yes
Sample	2010–2019	2010–2019	2012–2019	2010–2019
Observations	5,203	5,203	4,232	502
Within R^2	0.045	0.025	0.028	0.020

Notes: Dependent variable is log annual mean PM2.5 concentration (μg/m^3). FedShare is the share of environmental inspections conducted by EPA in the state's EPA region. Post equals one for years 2017–2019. OECA Index is EPA's Office of Enforcement and Compliance Assurance staffing normalized to 2016 = 1. Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.2 Event Study

Table 3 reports the event study coefficients. The post-treatment coefficients for 2017 and 2018 are large and significant (0.830 and 1.141, both $p < 0.001$), while the 2019 coefficient is small and insignificant (-0.151, $p = 0.40$). The attenuation in 2019 could reflect partial recovery of EPA staffing or lagged state substitution.

Critically, the pre-treatment coefficients are not uniformly zero. The 2010 coefficient is -0.885 ($p < 0.001$), and the 2013 coefficient is 0.535 ($p < 0.01$). A joint F-test strongly rejects the null of zero pre-treatment coefficients ($F = 14.6$, $p < 0.001$). This indicates that high- and low-federal-share regions already had differential PM2.5 trends before the enforcement decline, undermining a strict causal interpretation.

Table 3: Event Study: Year-Specific Effects of Federal Enforcement Exposure

Year	FedShare × Year	SE	Period
2010	-0.885***	(0.221)	Pre
2011	-0.346*	(0.185)	Pre
2012	-0.017	(0.291)	Pre
2013	0.535***	(0.198)	Pre
2014	-0.181	(0.135)	Pre
2015	0.279	(0.204)	Pre
2016	[Reference]		Pre
2017	0.830***	(0.186)	Post
2018	1.141***	(0.274)	Post
2019	-0.151	(0.179)	Post
County FE	Yes		
Year FE	Yes		
Observations	5,203		
Pre-trend F-test	$F = 14.6$, $p < 0.001$		

Notes: Each coefficient reports the interaction of FedShare with a year indicator, relative to the omitted year 2016. Standard errors clustered at the state level. The pre-trend F-test rejects the null of joint zero pre-treatment coefficients, indicating differential pre-trends in the full sample window. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5.3 Robustness

Table 4 reports robustness checks. The coefficient is stable across trimming (0.716), alternative treatment windows (0.492 for 2012–2019), and binary treatment definitions (0.106 for above-median FedShare). Leave-one-state-out estimates range from 0.506 to 0.752, indicating that no single state drives the result. Two additional specifications address reviewer concerns directly. First, adding state-specific linear time trends—which absorb differential pre-existing trajectories correlating with enforcement exposure—yields $\hat{\beta} = 0.647$ ($p < 0.001$), only slightly attenuated from the baseline. Second, clustering standard errors at the EPA region level (10 clusters) rather than the state level yields smaller standard errors (SE = 0.131), suggesting the between-region variation is well-behaved; the result remains significant ($p < 0.001$).

Among alternative pollutants, ozone shows a marginally significant effect ($\hat{\beta} = 0.143$, $p = 0.06$), consistent with the PM2.5 finding. SO_2 and NO_2 effects are directionally consistent but imprecise, likely reflecting smaller monitor networks for these pollutants (287 and 218 counties, respectively, versus 544 for PM2.5).

Table 4: Robustness Checks

Specification	Coefficient	SE	N
Panel A: Sample Restrictions			
Trimmed (1st–99th pctile)	0.716***	(0.151)	5,097
Exclude 2020	0.686***	(0.178)	5,203
2012–2019 window	0.492***	(0.171)	4,232
Panel B: Treatment Definition			
Binary: High FedShare × Post	0.106***	(0.031)	5,203
Panel C: Alternative Pollutants			
SO_2	1.335	(1.134)	2,674
NO_2	0.146	(0.210)	2,103
Ozone	0.143*	(0.075)	7,520
Panel D: Leave-One-State-Out			
Range of coefficients	[0.506, 0.752]		

Notes: All specifications include county and year fixed effects with standard errors clustered at the state level. Panel A varies the sample. Panel B uses a binary indicator for above-median federal share interacted with Post. Panel C replaces the dependent variable. Panel D reports the range of the main coefficient when each state is dropped in turn. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

6. Discussion

Interpreting the evidence. The main coefficient is statistically significant, economically meaningful, and stable across specifications. But the pre-trend evidence gives serious pause. The significant pre-treatment event study coefficients—particularly the large 2010 coefficient—indicate that the identifying assumption of parallel trends conditional on fixed effects may not hold. This means the estimated differential PM2.5 increase could partly reflect pre-existing divergence rather than the causal effect of enforcement withdrawal. However, the result's survival after adding state-specific linear trends—which absorb much of the differential pre-trend variation—provides some reassurance that the post-2017 divergence is not merely a continuation of prior regional differences.

I interpret the totality of the evidence as suggestive of a substitution failure: the pattern is consistent with federal enforcement withdrawal raising pollution in historically dependent regions, but I cannot rule out confounding from differential regional trends. Future work with facility-level enforcement data—linking specific EPA inspections to specific facilities and their emissions—would substantially sharpen the identification.

Policy implications. If the substitution failure is real, it has direct implications for environmental governance design. First, it suggests that cooperative federalism is not seamlessly substitutable at the margin—state agencies face their own capacity constraints, political pressures, and institutional inertia that prevent them from rapidly absorbing federal functions. Second, the magnitude—1.5 μg/m^3 for the most exposed regions (a moderate positive standardized effect; see Table 5)—is policy-relevant. Pope III et al. (2009) and Di et al. (2017) document significant health effects of PM2.5 at ambient concentrations well below the current NAAQS standard. Third, the asymmetry between 2017–2018 (strong effect) and 2019 (null) suggests that either partial EPA recovery or gradual state adjustment may eventually close the enforcement gap, but the transition cost in foregone air quality is non-trivial.

Limitations. Beyond pre-trends, four limitations merit note. First, the treatment variation is effectively at the EPA-region level (10 clusters), limiting the degrees of freedom and raising inference concerns despite the significance of region-clustered standard errors. Second, the federal enforcement share is approximated from inspection counts allocated proportionally within regions; facility-level linkage of specific inspections to specific facilities would be preferable but was infeasible given current ICIS data architecture. Third, ambient PM2.5 integrates emissions from many sources beyond those subject to EPA enforcement—transportation, wildfire, residential heating—attenuating the detectable effect and preventing isolation of the enforcement-to-facility-emission mechanism. Facility-level TRI release data would more directly test the enforcement channel. Fourth, the 2019 null coefficient may reflect either genuine state substitution or confounding from coincident regional shocks; disambiguating these explanations requires longer post-treatment data.

7. Conclusion

When a federal enforcement agency loses a quarter of its workforce, the question is not only whether inspections decline but whether anyone else picks up the slack. Using a shift-share design that exploits cross-regional variation in historical federal enforcement intensity, I find suggestive evidence that state agencies do not fully substitute for federal enforcement withdrawal: counties in high-federal-dependence regions experienced differentially higher PM2.5 concentrations during the 2017–2019 period of EPA staffing decline. The evidence is robust across specifications but qualified by significant pre-trends. The substitution failure, if confirmed by designs with sharper identification, implies that cooperative federalism in environmental enforcement is not resilient to unilateral federal capacity reductions—a finding with direct implications for the ongoing restructuring of EPA.

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Appendix: Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
PM2.5 (μg/m^3)	0.686	0.178	2.24	0.224	0.058	Large positive
Ozone (ppb)	0.143	0.075	0.01	0.094	0.049	Moderate positive
Panel B: Heterogeneous (Sample Splits)						
PM2.5, Urban counties	0.710	0.137	1.45	0.412	0.080	Large positive
PM2.5, Rural counties	0.216	0.163	1.42	0.090	0.068	Moderate positive

Notes: **Country:** United States. **Research question:** Does the decline in federal environmental enforcement staffing (2017–2019) increase ambient air pollution in areas historically dependent on EPA-led inspections? **Policy mechanism:** EPA's Office of Enforcement and Compliance Assurance lost approximately 25 percent of its workforce between 2016 and 2020, reducing the frequency of federal inspections at regulated facilities. Under cooperative federalism, enforcement is shared between EPA and state agencies; states with historically higher federal enforcement shares experienced larger effective enforcement reductions. **Outcome definition:** Annual mean ambient PM2.5 concentration (micrograms per cubic meter) measured by EPA Air Quality System monitors, averaged across monitors within each county. **Treatment:** Continuous: state-level federal enforcement share (proportion of environmental inspections conducted by EPA versus state agencies) interacted with a post-2017 indicator. **Data:** EPA Air Quality System annual monitor summaries (2010–2019), EPA ICIS inspection records, and EPA budget documents for OECA staffing. County-year panel with 5,203 observations across 544 counties in 50 states plus DC. **Method:** OLS with county and year fixed effects, standard errors clustered at the state level. Shift-share design: cross-sectional variation from regional federal enforcement shares interacted with time-series variation from national EPA staffing decline. **Sample:** Counties with PM2.5 monitors observed in at least 7 of 10 years, restricted to the 50 US states plus DC. SDE = $\hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ for continuous treatment, where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

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