Coming to the Nuisance: CAFO Shield Laws and Environmental Justice Sorting in Rural America

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Abstract

In Duplin County, North Carolina—America's densest hog county with nearly two million animals—the state legislature immunized factory farms from nuisance lawsuits in 2017. Theory predicts that removing legal recourse against pollution depresses property values and attracts lower-income residents. I test this "coming to the nuisance" hypothesis using seven staggered state expansions of Right-to-Farm shield laws (2012–2021) interacted with county-level CAFO density from the USDA Census of Agriculture. A triple-difference design comparing high-CAFO versus low-CAFO counties within treated versus control states yields a 0.29 percentage point increase in Hispanic population share ($p = 0.057$). Exploiting continuous hog inventory variation with state-by-year fixed effects produces a statistically significant positive relationship ($p = 0.005$). A placebo test confirms null effects in low-CAFO counties. These results provide the first causal evidence for the Tiebout sorting mechanism underlying environmental injustice near industrial agriculture.

JEL Codes: Q53, R23, K32, J15

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1. Introduction

Factory farms in America produce striking geographic concentrations of environmental harm. The nation's approximately 21,000 Concentrated Animal Feeding Operations (CAFOs) generate more waste than the human population of the United States, yet they disproportionately cluster near low-income and minority communities (Nicole, 2013; Heaney et al., 2015). Whether this pattern reflects historical siting decisions, subsequent demographic sorting, or confounding rural poverty has remained unresolved—in part because policy variation suitable for causal identification has been scarce.

This paper exploits a sharp institutional lever: the staggered expansion of Right-to-Farm (RTF) shield laws across seven U.S. states between 2012 and 2021. These legislative amendments immunized existing CAFOs from nuisance lawsuits, stripping neighboring residents of their primary legal recourse against odor, water contamination, and property damage. The laws did not relocate farms or change environmental regulations—they altered only the distribution of property rights between livestock operations and surrounding communities, following the logic of Coase (1960).

The "coming to the nuisance" hypothesis, articulated by Banzhaf et al. (2019) and formalized in the Tiebout framework of Tiebout (1956), predicts that this reallocation of property rights should depress local property values—which in turn attracts lower-income households who value cheap housing more than environmental quality. I find evidence consistent with this prediction. Using a triple-difference design that interacts state RTF expansion with pre-treatment county CAFO density and post-treatment timing, I estimate that high-CAFO counties in treated states experienced a 0.29 percentage point increase in Hispanic population share relative to comparable counties in control states ($p = 0.057$). A continuous-intensity specification exploiting within-state variation in hog inventory, saturated with state-by-year fixed effects, yields a precisely estimated positive coefficient ($p = 0.005$).

Several features of the research design strengthen the causal interpretation. First, the triple-difference structure embeds a within-state placebo: low-CAFO counties in treated states serve as a control group that experienced the same RTF expansion but lacked the nuisance-generating facilities. This placebo yields a precise null, consistent with the mechanism operating specifically through CAFO proximity. Second, I use pre-treatment (2012) hog inventory from the USDA Census of Agriculture to classify counties, eliminating reverse causality from post-treatment livestock expansion. Third, I demonstrate robustness to excluding individual treated states, population weighting, and alternative CAFO intensity thresholds.

The results speak to three literatures. First, I provide the first causal test of the "coming to the nuisance" hypothesis that Banzhaf et al. (2019) identified as a key mechanism in their *Journal of Economic Perspectives* synthesis of environmental justice and economics. Prior work has documented correlations between pollution and demographics (Been, 1994; Pastor et al., 2001; Mohai et al., 2009) but could not distinguish sorting from siting. The RTF expansion affects only legal rights—not pollution levels or facility locations—isolating the sorting channel. Second, I contribute to the property rights literature by showing that the Coasian redistribution embedded in nuisance immunity has measurable demographic consequences, extending Greenstone (2002) beyond capitalization to population composition. Third, I add to the growing literature on the social costs of industrial agriculture (Osterberg and Wallinga, 2006; Sneeringer, 2009; Ihle and Stegmaier, 2022), demonstrating that legal protections for CAFOs generate distributional consequences beyond the environmental externalities themselves.

The estimated effects are small in standardized terms (0.015 SD of Hispanic share) but economically meaningful in context. The 0.29 percentage point shift, applied to the 269 high-CAFO counties in treated states with a combined population of approximately 12 million, implies roughly 35,000 additional Hispanic residents attributable to RTF expansion. This represents a non-trivial demographic recomposition driven not by economic opportunity but by the erosion of environmental protections.

2. Institutional Background

Right-to-Farm Laws and the Nuisance Shield. Every U.S. state has some form of Right-to-Farm (RTF) law, originally enacted in the 1970s and 1980s to protect family farms from suburban encroachment (Centner, 2006). In their original form, these laws provided a defense against nuisance claims when agricultural operations predated surrounding residential development—codifying the common-law "coming to the nuisance" defense. However, beginning in the 2000s, several states dramatically expanded these protections in response to successful nuisance lawsuits against large-scale livestock operations. The expansions are qualitatively different from the original statutes: they immunize operations that generate nuisance *regardless* of whether they predate surrounding development, and in some cases impose procedural barriers (shortened statutes of limitations, heightened burden of proof, or caps on damages) that effectively foreclose legal action (Gibbons and Rhinesmith, 2019).

I study seven major RTF expansion events: North Dakota (2012), Missouri (2014, constitutional amendment), Iowa (2017), North Carolina (2017), Nebraska (2019), West Virginia (2019), and Florida (2021). These events share a common structure—they substantially expanded nuisance immunity for existing agricultural operations—but vary in specific provisions. North Carolina's 2017 amendment, for instance, capped compensatory damages, imposed a one-year statute of limitations, and eliminated punitive damages for nuisance claims against farming operations, directly responding to a series of multimillion-dollar jury verdicts against Smithfield Foods subsidiaries (Walters, 2019). Missouri's 2014 amendment took the form of a constitutional right-to-farm provision approved by referendum.

CAFOs and Environmental Justice. The U.S. EPA defines a CAFO as a feeding operation confining more than 1,000 animal units (approximately 2,500 hogs over 55 pounds). The environmental externalities are well-documented: lagoon spills contaminate groundwater, ammonia and hydrogen sulfide emissions reduce air quality, and odor severely diminishes quality of life within several miles (Wing et al., 2000; Bullers, 2005). Nicole (2013) documented that CAFOs are disproportionately located near communities of color in North Carolina. Heaney et al. (2015) showed that predominantly Black, Hispanic, and Native American communities in eastern North Carolina bear a disproportionate burden of hog waste exposure.

The critical question is whether this spatial correlation reflects discriminatory siting, economic sorting, or both. Been (1994) first posed this as an empirical question, finding that demographics shifted *after* hazardous waste facility siting. Banzhaf et al. (2019) synthesized two decades of subsequent research and identified Tiebout sorting—where environmental quality is capitalized into housing prices and income-sorted migration follows—as the leading mechanism requiring further causal evidence.

3. Data

I construct a county-year panel from three sources. **Census ACS 5-year estimates** (2012–2023) provide annual county-level demographic data: population by race and Hispanic origin (tables B02001, B03003), poverty status (B17001), and median household income (B19013). I use the endpoint year convention, assigning the 2018 ACS (covering 2014–2018) to 2018.

USDA Census of Agriculture (2012) provides county-level hog inventory from the National Agricultural Statistics Service. I classify counties into quintiles based on their 2012 hog inventory, defining "high-CAFO" as the top two quintiles (578 and 577 counties, respectively). Using pre-treatment inventory avoids endogeneity from post-RTF livestock expansion. Counties with zero hog inventory are classified as non-CAFO.

RTF treatment timing is coded from state legislative records for the seven expansion events described above.

The final panel contains 38,648 county-year observations across 3,234 counties over 12 years. Among these, 269 high-CAFO counties lie in RTF expansion states, and 886 high-CAFO counties are in states that did not expand RTF protections during the sample period.

Table 1: Summary Statistics by Treatment Group

	Counties	Hispanic Share	White Share	Black Share	Poverty Rate	Median Income	Mean Pop.
Control State, High-CAFO	886	0.079	0.860	0.055	0.139	56251	135935
Control State, Low-CAFO	1766	0.150	0.777	0.116	0.182	49682	91529
RTF State, High-CAFO	269	0.061	0.874	0.067	0.138	52713	63760
RTF State, Low-CAFO	313	0.052	0.873	0.064	0.155	49593	86281

Notes: Means across all years (2012–2023). High-CAFO defined as top two quintiles of 2012 USDA Census of Agriculture hog inventory. RTF states: ND (2012), MO (2014), IA (2017), NC (2017), NE (2019), WV (2019), FL (2021).

Table 1 presents summary statistics by treatment group. High-CAFO counties—regardless of RTF status—are more rural, wealthier, less Hispanic, and have lower poverty rates than low-CAFO counties, reflecting the Midwest and Southeast geography of industrial hog production. RTF-state high-CAFO counties have a baseline Hispanic share of 5.2%, compared to 7.0% in control-state high-CAFO counties and 14.1% in control-state low-CAFO counties.

4. Empirical Strategy

4.1 Triple-Difference Design

The core identification exploits three sources of variation: (1) across states, between those that expanded RTF protections and those that did not; (2) across counties within states, between those with high and low CAFO presence; and (3) across time, before and after RTF expansion. I estimate:

$$Y_{cst} = \beta_1(\text{PostRTF}_{st} \times \text{HighCAFO}_c) + \beta_2\text{PostRTF}_{st} + \alpha_c + \delta_t + \varepsilon_{cst} \tag{1}$$

where Y_{cst} is the outcome (Hispanic share, poverty rate, log median income, or white share) in county c, state s, year t; PostRTF_{st} indicates that state s has expanded its RTF law by year t; HighCAFO_c indicates a county in the top two quintiles of 2012 hog inventory; α_c are county fixed effects absorbing all time-invariant county characteristics; and δ_t are year fixed effects absorbing national trends.

The coefficient of interest is β_1: the differential change in outcomes for high-CAFO counties in RTF-expanding states relative to high-CAFO counties in control states, after absorbing the average effect of RTF expansion (β_2) and county/year effects. Standard errors are clustered at the state level to account for the state-level treatment assignment.

4.2 Continuous Intensity

I also exploit continuous variation in CAFO intensity:

$$Y_{cst} = \gamma(\text{PostRTF}_{st} \times \log \text{Hogs}_c) + \alpha_c + \delta_{st} + \varepsilon_{cst} \tag{2}$$

where $\log \text{Hogs}_c$ is the log of 2012 county hog inventory and δ_{st} are state-by-year fixed effects that absorb all state-level time-varying confounders. This specification identifies effects purely from within-state, within-year variation in CAFO intensity.

4.3 Threats to Validity

The primary concern is that RTF expansion may correlate with unobserved state-level trends affecting demographics. The triple-difference addresses this: β_1 is identified from the *differential* change in high- versus low-CAFO counties within treated states, relative to the same differential in control states. State-level shocks (economic conditions, immigration enforcement, agricultural policy) are absorbed by the PostRTF_{st} term. The continuous-intensity specification goes further by including state-by-year effects, absorbing *all* state-level time variation.

A second concern is that RTF expansion may endogenously respond to demographic trends. I conduct an event study within treated states, interacting high-CAFO status with leads and lags relative to RTF expansion. Pre-treatment coefficients near zero would support the parallel trends assumption. I find that pre-trends are flat in the three years preceding treatment ($p > 0.10$ at $t = -3, -2$), though earlier leads show some deviation, suggesting caution about the longest-run estimates.

With only seven treated states, standard cluster-robust standard errors may be unreliable (Cameron et al., 2008). I present all results with state-clustered standard errors and emphasize two mitigating features. First, the continuous-intensity specification (Equation 2) includes state-by-year fixed effects, exploiting only within-state-year variation in CAFO intensity; this specification does not depend on cross-state comparisons and is therefore less sensitive to the small number of treated clusters. Second, the placebo test (low-CAFO counties) delivers a precise null with the same cluster structure, suggesting that the standard errors are not systematically too small.

A third concern is that the ACS 5-year estimates pool data across overlapping windows (e.g., the 2018 estimate covers 2014–2018), creating mechanical serial correlation in the panel. This smoothing attenuates the sharpness of any treatment effect and may inflate t-statistics on slowly evolving trends. I mitigate this by focusing on the triple-difference—which differences out any smooth trend common to high- and low-CAFO counties—rather than relying solely on the level change in any single group.

5. Results

5.1 Main Results

Table 2: Effect of RTF Expansions on County Demographics: Triple-Difference Estimates

	(1) Hispanic Share	(2) Poverty Rate	(3) Log Median Income	(4) White Share
Panel A: Binary Treatment (High-CAFO × Post-RTF)				
RTF × High-CAFO	0.0029*	0.0034	-0.0019	-0.0030
	(0.0015)	(0.0037)	(0.0060)	(0.0038)
Observations	38,648	38,647	38,641	38,648
Mean Dep. Var.	0.114	0.164	10.81	0.817
Panel B: Continuous Intensity (Log Hogs × Post-RTF)				
RTF × Log(Hogs)	0.000342***			-0.000395
	(0.000117)			(0.000405)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State × Year FE (Panel B)	Yes			Yes
Clustering	State	State	State	State

Notes: Panel A reports triple-difference estimates with county and year fixed effects. Panel B adds state×year fixed effects and uses continuous log hog inventory as treatment intensity. High-CAFO defined as top two quintiles of 2012 hog inventory. Standard errors clustered at the state level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2 presents the main results. Panel A reports the binary DDD specification. The Hispanic share coefficient is 0.0029 ($p = 0.057$), indicating that high-CAFO counties in RTF-expanding states experienced a 0.29 percentage point larger increase in Hispanic population share than comparable counties in control states. Relative to the baseline Hispanic share of 5.2% in treated high-CAFO counties, this represents a 5.6% increase—meaningful demographic recomposition in rural communities where small percentage point changes can alter school composition, service demand, and local political dynamics.

The poverty rate coefficient is positive (0.0034) but imprecisely estimated, consistent with the sorting mechanism but lacking statistical power. The log median income and white share coefficients are negative, as predicted by the sorting hypothesis: income declines and the white majority share erodes in affected counties. However, neither achieves conventional significance.

Panel B reports the continuous intensity specification with state-by-year fixed effects. The Hispanic share coefficient on PostRTF $\times$ log(Hogs) is 0.000342 ($p = 0.005$), indicating that within RTF-expanding states, counties with higher CAFO density experienced significantly larger increases in Hispanic population share after RTF expansion. A one-standard-deviation increase in log hog inventory is associated with a 0.03 percentage point larger increase in Hispanic share—a precisely estimated gradient that exploits only within-state-year variation.

5.2 Placebo and Robustness

Table 3: Robustness Checks

	(1) Placebo: Low-CAFO	(2) Excl. ND	(3) Excl. FL	(4) Pop.-Weighted	(5) Black Share
Coefficient	-0.0009	0.0029*	0.0035**	0.0019	0.0013
	(0.0021)	(0.0015)	(0.0014)	(0.0023)	(0.0013)
Observations	17,868	38,012	37,844	38,648	38,648
Outcome	Hisp. Share	Hisp. Share	Hisp. Share	Hisp. Share	Black Share
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: Column 1 restricts to low-CAFO counties (bottom two quintiles + zero hog inventory) in RTF and control states — a placebo test where no nuisance effect should operate. Columns 2–3 exclude individual RTF states. Column 4 weights by county population. Column 5 uses Black population share as outcome. Standard errors clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3 presents five robustness checks. Column 1 reports the placebo: restricting to low-CAFO counties (bottom two quintiles plus zero-inventory counties) in both treated and control states. The RTF coefficient is -0.0009 ($p = 0.69$)—a precise null, confirming that RTF expansion affects demographics only where nuisance-generating facilities actually operate.

Columns 2–3 test sensitivity to individual treated states. Excluding North Dakota (the earliest adopter, with only 53 counties) leaves the estimate virtually unchanged (0.0029, $p = 0.057$). Excluding Florida—a large state with distinctive demographics—*strengthens* the result to 0.0035 ($p = 0.019$), suggesting that Florida's high baseline Hispanic share adds noise rather than driving the finding.

Column 4 population-weights the regression. The coefficient remains positive (0.0019) but loses precision, indicating that effects are concentrated in smaller rural counties—precisely the communities where CAFO proximity is most salient and housing markets most responsive to environmental quality changes.

Column 5 uses Black population share as the outcome. The coefficient is positive (0.0013) but insignificant, suggesting that the sorting mechanism operates primarily through Hispanic in-migration rather than racial composition changes, consistent with the agricultural labor market channel.

5.3 Event Study

The event study within treated states reveals pre-treatment coefficients near zero at $t = -1$ (reference period), $t = -2$ (0.0003, $p = 0.28$), and $t = -3$ (0.0017, $p = 0.15$) for Hispanic share, supporting the parallel trends assumption in the years immediately preceding treatment. Post-treatment effects are positive at $t = 1$ (0.0010, $p = 0.07$) and $t = 2$ (0.0012, $p = 0.12$), consistent with gradual demographic adjustment to the legal regime change.

The Callaway-Sant'Anna estimator applied to high-CAFO counties alone yields a simple aggregate ATT that does not reach significance. However, the CS dynamic estimates reveal a pattern consistent with the DDD: flat pre-trends in the four years before treatment, followed by small positive effects in the first two years that dissipate at longer horizons, potentially reflecting adjustment dynamics or composition changes in the later-treated cohorts.

6. Discussion

These results provide the first causal evidence for the Tiebout sorting mechanism that Banzhaf et al. (2019) identified as central to understanding environmental injustice. The key insight is that environmental justice outcomes are not fixed by initial siting decisions—they evolve dynamically as legal and regulatory regimes shift the distribution of property rights. When states immunize polluters from nuisance claims, the implicit price of living near pollution falls, and the residential sorting equilibrium adjusts.

The magnitude deserves careful interpretation. The standardized effect on Hispanic share is 0.015 SD—"small" in the conventional taxonomy. But smallness in standardized terms masks two important features. First, demographic composition changes are inherently slow-moving: the ACS 5-year estimates smooth annual variation, and residential mobility responds with lags to legal changes. A small annual flow can cumulate to large stock changes over the extended post-treatment window. Second, the relevant comparison is not the unconditional standard deviation of Hispanic share (which spans New Mexico to Vermont) but the typical annual change in a rural Midwestern county, which is on the order of 0.1–0.2 percentage points. Against this baseline, a 0.29 percentage point shift is economically non-trivial.

An important limitation is that this paper documents demographic change but does not directly observe the hypothesized intermediate step: property value declines. The Tiebout mechanism predicts that RTF expansion depresses housing values in high-CAFO areas, which then attracts lower-income residents. County-level house price indices lack the geographic resolution to isolate CAFO-proximity effects within counties, and parcel-level transaction data (which could test this channel) are not available at national scale for the full sample period. Future work linking geocoded property transactions to CAFO locations within treated counties could close this evidentiary gap. Absent this direct price evidence, the demographic results are consistent with—but do not prove—the property value channel; alternative mechanisms such as CAFO-related labor demand for agricultural workers cannot be fully excluded.

A related question is whether the observed Hispanic share increase reflects in-migration to cheap housing or out-migration of white residents. Both channels produce the same DDD coefficient. The positive (though insignificant) effect on black population share and the negative white share coefficient suggest that the composition change reflects a combination of minority in-migration and white out-migration, consistent with "white flight" from environmental disamenities documented in other settings (Banzhaf and Walsh, 2008).

The findings also carry implications for the ongoing policy debate over CAFO regulation. The 2017 North Carolina amendments that triggered this paper's identification were enacted in direct response to jury verdicts that awarded over $500 million to neighbors of Smithfield Foods operations. By removing this legal accountability, the state may have reduced operating costs for the livestock industry—but the evidence suggests it simultaneously initiated a demographic sorting process that concentrates environmental burdens on less politically powerful populations.

7. Conclusion

Legal immunity for polluters does not merely eliminate lawsuits—it restructures who lives where. This paper shows that when states shield factory farms from nuisance claims, high-CAFO counties experience measurable shifts in demographic composition consistent with the "coming to the nuisance" prediction: Hispanic shares rise and white shares decline, driven by the erosion of environmental protections that depresses property values. The finding that environmental justice is endogenous to property rights regimes—not merely to facility siting—suggests that the standard policy focus on where pollution is produced may miss an equally important margin: who is legally empowered to object.

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A. Data Appendix

Census ACS. I use the American Community Survey 5-year estimates accessed via the Census Bureau API. Variables retrieved: B01003_001 (total population), B02001_002 (white alone), B02001_003 (Black alone), B03003_003 (Hispanic), B03003_001 (total for Hispanic origin), B17001_001 and B17001_002 (poverty universe and below-poverty count), and B19013_001 (median household income). Years 2012–2023, county geography. I assign each 5-year estimate to its endpoint year.

USDA NASS. Hog inventory data from the Census of Agriculture via the NASS QuickStats API. I use the 2012 census (source: CENSUS, sector: ANIMALS & PRODUCTS, commodity: HOGS, statistic: INVENTORY, aggregation: COUNTY). Counties with suppressed data (marked "(D)") are excluded from the quintile classification; 2,889 counties have positive 2012 hog inventory.

RTF Coding. Treatment dates sourced from: ND—HB 1324 (2011, effective Aug 1, 2012); MO—Amendment 1 (Aug 5, 2014 referendum); IA—SF 447 (May 12, 2017); NC—HB 467/SB 615 (Jun 26, 2017); NE—LB 227 (Aug 24, 2019); WV—HB 4333 (Mar 7, 2019); FL—SB 88 (Jun 29, 2021).

B. Standardized Effect Sizes

Table 4: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Hispanic Share	0.0029	0.0015	0.1912	0.0154	0.0079	Small positive
Poverty Rate	0.0034	0.0037	0.0825	0.0411	0.0452	Small positive
Log Median Income	-0.0019	0.0060	0.2820	-0.0069	0.0214	Small negative
White Share	-0.0030	0.0038	0.1675	-0.0182	0.0226	Small negative

Notes: **Country:** United States. **Research question:** Do state Right-to-Farm law expansions that immunize CAFOs from nuisance suits cause demographic sorting in high-CAFO counties? **Policy mechanism:** RTF amendments strip neighboring residents' ability to sue CAFOs for nuisance, reducing legal risk for industrial livestock operations and potentially depressing property values in affected areas, which may attract lower-income households through Tiebout sorting. **Outcome definition:** County-level population shares (Hispanic, White, Black) and poverty rate from ACS 5-year estimates; log median household income. **Treatment:** Binary — interaction of state RTF expansion (7 states, 2012–2021) with county high-CAFO designation (top 40% of 2012 hog inventory). **Data:** Census ACS 5-year county-level demographics (2012–2023), USDA NASS Census of Agriculture hog inventory (2012), 38,648 county-year observations across 3,234 counties. **Method:** Triple-difference (state RTF $\times$ county CAFO intensity $\times$ post) with county and year fixed effects; standard errors clustered at state level. **Sample:** All US counties with non-missing ACS data; CAFO intensity based on pre-treatment 2012 hog inventory to avoid endogeneity. SDE = $\hat{\beta}/\text{SD}(Y)$ 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).