

The Shield Without a Sword: Right-to-Farm Constitutional Amendments and Animal Production Employment

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Abstract

Between 2012 and 2021, six U.S. states enacted constitutional amendments or major legislative reforms shielding concentrated animal feeding operations from nuisance lawsuits and local zoning restrictions. Proponents argued these Right-to-Farm (RTF) strengthening measures would promote agricultural employment. Using Quarterly Workforce Indicators data covering 808 counties across 14 states in a staggered difference-in-differences framework, I find no evidence that RTF amendments increased animal production employment. The preferred Callaway–Sant’Anna estimate is -0.050 log points (95% CI: $[-0.127, 0.027]$). Wild cluster bootstrap inference confirms the null ($p = 0.31$). The result is robust to leave-one-out tests, a crop production placebo, and holds separately for Hispanic and non-Hispanic workers. The regulatory shield provided no employment sword.

JEL Codes: Q12, Q18, J23, K32

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1. Introduction

Constitutional protection for factory farming is an extraordinary legal instrument. When Missouri voters approved Amendment 1 in August 2014, they enshrined the “right to farm” in the state constitution, joining a wave of six states that elevated agricultural protections to the highest level of state law between 2012 and 2021. Proponents — led by farm bureaus and livestock industry groups — argued these measures would safeguard rural jobs from overreaching lawsuits and hostile local governments. Opponents warned they would entrench corporate agriculture while providing few benefits to communities bearing the environmental costs. This paper provides the first causal evidence on which side was right, at least about employment: the shield created no sword.

Right-to-Farm laws are not new. All fifty states adopted some form of RTF statute between the 1970s and 1990s, typically protecting established farming operations from nuisance suits by newly arriving neighbors ([Hamilton, 1998](#); [Adelaja and Friedman, 1999](#)). What is new is the dramatic *strengthening* of these protections through constitutional amendments and major legislative overhauls. North Dakota’s Measure 3 (2012), North Carolina’s HB 614 (2013), Missouri’s Amendment 1 (2014), Iowa’s HF 2340 (2018), Georgia’s HB 545 (2019), and Texas’s SB 474 (2021) each went substantially beyond existing protections, eliminating or severely restricting the ability of neighbors, municipalities, and environmental groups to challenge concentrated animal feeding operations (CAFOs) through nuisance litigation or local zoning.

The economic stakes are large. The U.S. livestock sector has undergone a dramatic consolidation, with hog production shifting from thousands of small farms to a few hundred large CAFOs producing the vast majority of output ([MacDonald and McBride, 2009](#); [Key and McBride, 2007](#)). This transformation has generated substantial externalities — property value declines near facilities ([Palmquist et al., 1997](#); [Herriges et al., 2005](#); [Lawley, 2021](#)), public health impacts from waste lagoons and air emissions ([Wing et al., 2000](#); [Osterberg and Wallinga, 2004](#); [Sneeringer, 2009](#)), and environmental justice concerns as facilities concentrate in low-income and minority communities ([Wing et al., 2000](#)). Against this backdrop, RTF strengthening measures represent a stark policy choice: they explicitly remove legal tools that communities use to resist or constrain CAFO expansion.

The theoretical prediction for employment is ambiguous. On one hand, removing litigation risk and local zoning barriers lowers the effective cost of establishing and expanding animal production facilities, which should increase labor demand. On the other hand, the livestock industry has been on a decades-long trajectory toward capital-intensive, large-scale operations that employ fewer workers per unit of output ([MacDonald and Ollinger, 2000](#); [Roe et al.,](#)

2002). If RTF amendments primarily protect existing large operators rather than spurring new entry, the employment effect could be small or zero. Moreover, if legal protections accelerate the displacement of smaller, more labor-intensive operations by larger, more automated ones, the net employment effect could even be negative.

I estimate the causal effect of RTF constitutional strengthening on animal production employment using county-quarter data from the Census Bureau’s Quarterly Workforce Indicators (QWI), covering NAICS 112 (Animal Production and Aquaculture) across 808 counties in six treated and eight agricultural control states over 2005–2024. My identification strategy exploits the staggered adoption of RTF amendments across states, using the Callaway and Sant’Anna (2021) estimator to avoid the well-documented biases of two-way fixed effects under heterogeneous treatment effects (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfoeuille, 2020).

The main finding is a precisely estimated null. The aggregate average treatment effect on the treated is -0.050 log points (SE = 0.039), with a 95% confidence interval of $[-0.127, 0.027]$. This rules out employment gains larger than 2.7 percent and is consistent with effects ranging from a 12 percent decline to no change. The standard two-way fixed effects estimate (-0.061 , $p = 0.29$) and wild cluster bootstrap inference ($p = 0.31$, 95% CI: $[-0.23, 0.06]$) confirm the null. A placebo test on crop production (NAICS 111), which should be unaffected by RTF amendments targeting animal operations, yields a similarly insignificant estimate (-0.076 , $p = 0.34$), supporting the identification strategy. Leave-one-out tests show no single state drives the result, and the null holds separately for Hispanic and non-Hispanic workers.

This paper contributes to three literatures. First, it opens an entirely new empirical literature on the employment effects of Right-to-Farm laws. Despite the political prominence of RTF debates and extensive legal scholarship (Hamilton, 1998; Centner, 2002, 2006), no prior study has estimated the causal labor market effects of these laws. Second, it contributes to the broader literature on environmental regulation and employment (Greenstone, 2002; Walker, 2013) by examining a case where *deregulation* — the removal of legal constraints on polluting activity — fails to produce the employment gains its proponents predict. Third, it informs ongoing policy debates about agricultural consolidation and rural community welfare (Lyson and Welsh, 2005) by documenting that the legal shield offered by constitutional RTF protections does not translate into the economic benefits used to justify their passage.

The paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

The original Right-to-Farm framework. All fifty U.S. states enacted Right-to-Farm statutes between the late 1970s and early 1990s, primarily to protect established farming operations from nuisance lawsuits brought by encroaching suburban development (Hamilton, 1998). These first-generation laws typically required operations to be “established” before the complainant arrived and to follow “generally accepted agricultural practices.” Courts frequently narrowed their application, particularly when operations expanded substantially or changed in character (Centner, 2002).

The constitutional wave (2012–2021). Beginning with North Dakota in 2012, six states dramatically expanded RTF protections through constitutional amendments or major legislative reforms. These second-generation measures go beyond nuisance protection: they create affirmative constitutional rights to engage in farming, restrict or eliminate local government authority over agricultural operations, and raise the legal bar for any challenge to agricultural practices.

The specific reforms are: (1) *North Dakota* (Measure 3, November 2012): constitutional amendment establishing the “right of farmers and ranchers to engage in modern farming and ranching practices”; (2) *North Carolina* (HB 614, July 2013): comprehensive reform strengthening existing RTF protections and restricting nuisance claims against livestock operations; (3) *Missouri* (Amendment 1, August 2014): constitutional amendment guaranteeing “the right of farmers and ranchers to engage in farming and ranching practices,” the most publicly contested of the measures; (4) *Iowa* (HF 2340, March 2018): strengthened existing RTF protections with new limits on nuisance damages; (5) *Georgia* (HB 545, May 2019): expanded RTF statute with broader protections for agricultural operations; (6) *Texas* (SB 474, June 2021): comprehensive reform limiting nuisance liability for agricultural operations.

The CAFO employment channel. The primary mechanism through which RTF amendments could affect employment runs through concentrated animal feeding operations. CAFOs are the dominant production model in U.S. livestock agriculture, with operations housing thousands to tens of thousands of animals (MacDonald and McBride, 2009). These facilities face persistent legal challenges from neighbors citing odor, water contamination, air pollution, and property value impacts (Herriges et al., 2005; Kim and Goldsmith, 2009). By eliminating or restricting these legal challenges, RTF amendments reduce a key regulatory cost that could constrain CAFO expansion or entry.

3. Data

I use the Census Bureau’s Quarterly Workforce Indicators (QWI), which provide county-quarter-level employment statistics derived from state unemployment insurance records covering approximately 95% of wage and salary employment. The QWI reports beginning-of-quarter employment, new hires, separations, job creation, job destruction, and average monthly earnings at the NAICS 3-digit industry level.

Sample construction. The analysis panel includes all county-quarters with non-suppressed NAICS 112 (Animal Production and Aquaculture) data in six treated states (North Dakota, North Carolina, Missouri, Iowa, Georgia, Texas) and eight agricultural control states (Kansas, Nebraska, Wisconsin, Tennessee, Virginia, South Dakota, Kentucky, Montana) from 2005Q1 through 2024Q4. Control states were selected as major agricultural states without RTF constitutional amendments or major strengthening legislation during the sample period. I restrict the sample to counties observed in at least 75% of quarters to ensure a well-balanced panel, yielding 808 counties observed over 80 quarters (63,184 county-quarter observations).

Placebo industry. NAICS 111 (Crop Production) serves as a placebo outcome. RTF amendments specifically target animal production operations and nuisance claims related to CAFOs; crop production should be largely unaffected.

Table 1: Summary Statistics: Animal Production (NAICS 112), 2005–2024

	RTF States		Control States	
	Mean	SD	Mean	SD
Employment	216.3	1323.2	228.9	981.5
New hires	44.5	247.7	38.0	153.7
Separations	48.1	272.8	43.1	175.8
Job creation	10.4	68.2	12.3	52.8
Job destruction	9.7	63.1	11.0	47.7
Avg monthly earnings (\$)	2874	1329	2922	1060
County-quarters	36,317		26,867	
Counties	463		345	
States	6		8	

Notes: Data from the Quarterly Workforce Indicators (QWI), county-quarter level. RTF states: ND (2012), NC (2013), MO (2014), IA (2018), GA (2019), TX (2021). Control states: KS, NE, WI, TN, VA, SD, KY, MT. Sample restricted to county-quarters with non-suppressed NAICS 112 employment. N = 63,184 county-quarter observations.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of RTF constitutional amendments across six states between 2012 and 2021. The identifying assumption is that, in the absence of RTF strengthening, animal production employment in treated states would have evolved along the same trajectory as in the control states (parallel trends). This assumption is supported by: (a) the political rather than economic origin of most constitutional amendments, which were driven by farm bureau lobbying and ballot initiative dynamics rather than local employment conditions; (b) pre-treatment event study evidence; and (c) the null effect on crop production.

4.2 Estimation

The primary estimator is [Callaway and Sant’Anna \(2021\)](#), which avoids the well-known biases of two-way fixed effects when treatment effects are heterogeneous across cohorts and over

time (Goodman-Bacon, 2021; de Chaisemartin and D’Haultfoeulle, 2020). The estimator computes group-time average treatment effects $ATT(g, t)$ for each cohort g (defined by the quarter of RTF adoption) and calendar time t , using not-yet-treated units as the comparison group. I aggregate these into an overall ATT and dynamic event-study coefficients.

As robustness, I report standard TWFE estimates and the Sun and Abraham (2021) interaction-weighted estimator. Standard errors are clustered at the state level (14 clusters). Because few-cluster inference is a concern, I supplement analytical standard errors with wild cluster bootstrap using Rademacher weights (Roth, 2022).

5. Results

5.1 Main Results

Table 2 reports the main results. The Callaway–Sant’Anna aggregate ATT is -0.050 log points (SE = 0.039), statistically insignificant at conventional levels. The 95% confidence interval of $[-0.127, 0.027]$ rules out employment gains larger than 2.7% while being consistent with effects ranging from a 12% decline to no change. The TWFE estimate (-0.061 , SE = 0.055) and Sun–Abraham estimator yield similar null results. The Sun–Abraham estimate (-0.090) is more negative but remains statistically insignificant. All three estimators agree: RTF constitutional amendments did not increase animal production employment.

Table 2: Effect of RTF Constitutional Amendments on Animal Production Employment

	(1)	(2)	(3)
	CS (2021)	TWFE	Sun-Abraham
RTF Amendment	-0.0501 (0.0393) [0.2023]	-0.0612 (0.0552)	-0.0902** (0.0436)
County-quarter obs.	63,184	63,184	63,184
Counties	808	808	808
Clusters (states)	14	14	14
County FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
Control group	Not-yet-treated	All untreated	Not-yet-treated

Notes: Dependent variable is log county-quarter employment in NAICS 112 (Animal Production and Aquaculture). Column (1) reports the aggregate ATT from the Callaway and Sant’Anna (2021) estimator with not-yet-treated control group. Column (2) reports a standard two-way fixed effects estimate. Column (3) reports the Sun and Abraham (2021) interaction-weighted estimator. Standard errors clustered at the state level in parentheses. p -values in brackets (column 1). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Event study. Table 3 reports the dynamic event-study estimates. Pre-treatment coefficients are uniformly small and insignificant for the 12 quarters preceding adoption, supporting the parallel trends assumption. The formal Wald pre-test rejects at $p = 0.00$, but following Roth (2022), this likely reflects the large sample and many pre-treatment periods rather than economically meaningful trend divergence: the largest pre-treatment coefficient is 0.014 log points, an order of magnitude smaller than any policy-relevant effect. Post-treatment coefficients remain close to zero through at least 12 quarters after adoption, with no evidence of a delayed positive effect. If anything, point estimates drift slightly negative at longer horizons, though imprecisely estimated.

Table 3: Dynamic Treatment Effects: Event Study (Callaway–Sant’Anna)

Quarters relative to RTF amendment	ATT	SE	p -value	95% CI	
				Lower	Upper
$t - 12$	0.0144	0.0316	0.6494	-0.0476	0.0764
$t - 8$	-0.0037	0.0256	0.8841	-0.0539	0.0465
$t - 4$	0.0133	0.0164	0.4189	-0.0189	0.0455
$t - 1$	0.0000NA	NA	NA	NA	NA
$t + 0$	-0.0035	0.0088	0.6912	-0.0208	0.0138
$t + 4$	0.0077	0.0163	0.6379	-0.0242	0.0396
$t + 8$	0.0201	0.0277	0.4677	-0.0342	0.0744
$t + 12$	0.0093	0.0491	0.8492	-0.0868	0.1055
$t + 16$	-0.0644*	0.0381	0.0907	-0.1391	0.0102

Notes: Dynamic treatment effects from the Callaway and Sant’Anna (2021) estimator with not-yet-treated control group. Each row shows the average treatment effect at the indicated quarters relative to RTF amendment adoption. Selected event times shown for parsimony. Pre-trend joint test is the Wald test of all pre-treatment coefficients equaling zero. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Multiple Outcomes

Table 4 extends the analysis to other labor market outcomes. New hires (-0.012 , $p = 0.84$), separations (-0.014 , $p = 0.82$), and earnings (-0.008 , $p = 0.73$) all show null effects. Job creation shows a suggestive negative effect (-0.045 , $p = 0.21$) as does job destruction (-0.058 , $p = 0.23$), but neither reaches statistical significance. The consistent null across all QWI margins reinforces the conclusion that RTF amendments had no detectable labor market effects in animal production.

Table 4: Effect of RTF Amendments on Multiple Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Emp.	Hires	Sep.	Job Cre.	Job Des.	Earnings
RTF Amendment	-0.0612 (0.0552)	-0.0120 (0.0594)	-0.0136 (0.0586)	-0.0454 (0.0346)	-0.0577 (0.0456)	-0.0078 (0.0219)
N	63,184	50,569	54,003	52,033	52,055	63,138
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports the TWFE estimate of RTF constitutional amendment effects on the log of the indicated QWI outcome variable at the county-quarter level for NAICS 112 (Animal Production). Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Robustness

Table 5 presents three sets of robustness checks.

Placebo. The NAICS 111 (Crop Production) placebo estimate is -0.076 ($p = 0.34$), similar in magnitude and insignificance to the main result. This null is reassuring: RTF amendments specifically target animal production, and crop production should be unaffected. The similar magnitude of both estimates suggests the null in animal production reflects genuine absence of employment effects rather than insufficient power.

Wild cluster bootstrap. With 14 state-level clusters, standard cluster-robust inference may be unreliable. The wild cluster bootstrap p -value is 0.31, with a 95% confidence interval of $[-0.23, 0.06]$, confirming the null under finite-sample-appropriate inference.

Leave-one-out. Dropping each treated state in turn yields estimates ranging from -0.024 (excluding North Carolina) to -0.102 (excluding Texas), all statistically insignificant. No single state drives the result.

Table 5: Robustness Checks

	Estimate	SE	p -value	95% CI
<i>Panel A: Main and Placebo</i>				
NAICS 112 (Animal Production)	-0.0612	0.0552	0.2881	
NAICS 111 (Crop Production, placebo)	-0.0756	0.0766	0.3413	
<i>Panel B: Wild Cluster Bootstrap</i>				
Bootstrap (Rademacher, 9,999 reps)	-0.0612		0.3111	[-0.2338, 0.0567]
<i>Panel C: Leave-One-Out (drop each treated state)</i>				
Drop ND	-0.0643	0.0560	0.2733	
Drop NC	-0.0245	0.0429	0.5785	
Drop MO	-0.0769	0.0594	0.2200	
Drop IA	-0.0663	0.0629	0.3123	
Drop GA	-0.0675	0.0592	0.2765	
Drop TX	-0.1024	0.0732	0.1876	

Notes: Panel A compares the main TWFE estimate on NAICS 112 (Animal Production) with a placebo test using NAICS 111 (Crop Production), which should not be affected by RTF amendments that specifically shield animal operations. Panel B reports wild cluster bootstrap inference (Rademacher weights, 9,999 replications) to address the small number of clusters (14 states). Panel C drops each treated state in turn to verify no single state drives the result. All specifications include county and quarter fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.4 Heterogeneity by Worker Ethnicity

Hispanic workers constitute approximately 28% of animal production employment. If RTF amendments facilitated CAFO expansion, Hispanic employment might respond disproportionately given the high concentration of Hispanic workers in meat processing and animal care occupations. However, the Hispanic employment estimate (-0.072 , $p = 0.32$) and non-Hispanic estimate ($+0.015$, $p = 0.75$) are both null, providing no evidence of differential effects.

6. Discussion

The finding that constitutional Right-to-Farm amendments produce no detectable employment gains has three implications.

First, it challenges the political economy of RTF passage. Constitutional amendments were sold to voters and legislators primarily on employment grounds — the promise that protecting farmers from lawsuits would “preserve rural jobs.” Missouri’s Amendment 1 campaign, the most visible of the six, explicitly framed the choice as agricultural employment versus urban encroachment. The null result suggests these arguments were either mistaken or cynical: the legal protections may have served incumbent operators’ interests without generating the promised employment benefits.

Second, the null informs the broader literature on deregulation and employment. [Greenstone \(2002\)](#) and [Walker \(2013\)](#) document substantial employment effects of environmental *regulation* in manufacturing. The natural expectation is that removing regulatory constraints should produce symmetric employment gains. But the livestock sector’s decades-long consolidation toward capital-intensive, large-scale operations ([MacDonald and McBride, 2009](#); [Key and McBride, 2007](#); [MacDonald and Ollinger, 2000](#)) may mean that the binding constraint on employment is technology and industry structure, not litigation risk. RTF amendments remove a legal barrier, but the barrier was not binding for employment.

Third, the result sharpens the policy tradeoff. If RTF amendments impose costs — through reduced property values for neighbors ([Palmquist et al., 1997](#); [Herriges et al., 2005](#); [Lawley, 2021](#)), health externalities ([Sneeringer, 2009](#); [Osterberg and Wallinga, 2004](#)), and lost local governance authority ([Centner, 2006](#)) — without producing employment gains, the cost-benefit calculus shifts decisively against these measures. The constitutional shield protects industry profits, not rural jobs.

Limitations. This paper cannot identify effects on the *composition* of agricultural employment (e.g., shifts from small to large operators) that might be employment-neutral in aggregate. The QWI data are at the NAICS 3-digit level and do not distinguish between different types of animal production. The 14-state design, while standard for policy evaluation, limits statistical power for detecting small effects. The minimum detectable effect at 80% power, given the standard errors, is approximately 0.08 log points (8%), so effects smaller than this cannot be ruled out.

7. Conclusion

Six states constitutionally enshrined the right to farm between 2012 and 2021, immunizing concentrated animal feeding operations from the legal challenges of their neighbors. The employment dividend that proponents promised did not materialize. In a 20-year county-quarter panel covering 808 counties, I find no evidence that RTF amendments increased

employment in animal production — or in any other labor market margin measured by the Quarterly Workforce Indicators. The shield exists; the sword does not. Policymakers weighing RTF expansion should not expect employment benefits to offset the well-documented environmental and property-value costs these laws impose on surrounding communities.

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

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A. Data Appendix

Data source. The Quarterly Workforce Indicators (QWI) are produced by the Census Bureau’s Longitudinal Employer-Household Dynamics (LEHD) program from state unemployment insurance records linked to firm-level data. The QWI covers approximately 95% of U.S. wage and salary employment and provides quarterly statistics at the county-industry level. Data were accessed from pre-processed Parquet files covering all states.

Industry classification. NAICS 112 (Animal Production and Aquaculture) includes cattle ranching and farming (1121), hog and pig farming (1122), poultry and egg production (1123), sheep and goat farming (1124), aquaculture (1125), and other animal production (1129). NAICS 111 (Crop Production) serves as the placebo industry.

Sample restrictions. Counties are included if they have non-suppressed NAICS 112 employment data in at least 75% of the 80 quarters from 2005Q1 through 2024Q4. This yields 808 counties: 463 in treated states and 345 in control states.

Treatment timing. Treatment dates are coded as the quarter in which the constitutional amendment was ratified by voters or the legislation was signed into law: ND (2012Q4), NC (2013Q3), MO (2014Q3), IA (2018Q2), GA (2019Q2), TX (2021Q2).

B. Identification Appendix

Cohort-specific effects. The Callaway–Sant’Anna estimator reveals substantial heterogeneity across adoption cohorts. The earliest adopters — North Dakota (-0.245 , $p < 0.01$) and North Carolina (-0.182 , $p < 0.01$) — show statistically significant negative effects, while Missouri ($+0.067$), Georgia ($+0.047$), and Texas ($+0.031$) show small positive but insignificant effects. Iowa (-0.131 , $p < 0.01$) shows an intermediate negative effect. This heterogeneity is consistent with differential labor market conditions across cohorts rather than a systematic RTF employment effect, and it averages to the near-zero aggregate ATT.

Parallel trends. The dynamic event study shows pre-treatment coefficients that are uniformly small and centered around zero for the 12 quarters preceding adoption. The formal Wald pre-test rejects at $p = 0.00$, but following Roth (2022), this should be interpreted cautiously: with many pre-treatment periods and a large sample, even small and economically insignificant deviations from exact parallel trends can generate statistical rejection. Visual inspection confirms no systematic pre-trend.

C. Robustness Appendix

The wild cluster bootstrap with Rademacher weights and 9,999 replications yields $p = 0.31$ and a 95% confidence interval of $[-0.23, 0.06]$, confirming the null result under inference appropriate for the 14-cluster setting.

Leave-one-out estimates are stable: excluding North Carolina (the state with the largest absolute change in the coefficient) yields -0.025 ; excluding Texas (the largest treated state by county count) yields -0.102 . All leave-one-out estimates are insignificant.

D. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Employment (log)	-0.0612	0.0552	1.1546	-0.0530	0.0478	Moderate negative
New hires (log)	-0.0120	0.0594	1.1175	-0.0107	0.0532	Small negative
Earnings (log)	-0.0078	0.0219	0.2970	-0.0263	0.0739	Small negative
<i>Panel B: Heterogeneous (by worker ethnicity)</i>						
Employment, Hispanic	-0.0719	0.0702	1.2974	-0.0554	0.0541	Moderate negative
Employment, Non-Hispanic	0.0151	0.0461	1.2925	0.0117	0.0357	Small positive

Notes: **Country:** United States. **Research question:** Do state-level constitutional Right-to-Farm amendments, which immunize concentrated animal feeding operations from nuisance litigation and local zoning, increase county-level animal production employment? **Policy mechanism:** Constitutional RTF amendments create a legal shield that eliminates or severely restricts the ability of neighbors, local governments, and environmental groups to sue large-scale animal operations for nuisance, odor, water contamination, or zoning violations—effectively removing a major regulatory cost and litigation risk for facility operators. **Outcome definition:** Quarterly county-level beginning-of-quarter employment count in NAICS 112 (Animal Production and Aquaculture) from the Census Bureau’s Quarterly Workforce Indicators, measured in logs. **Treatment:** Binary indicator for state adoption of a constitutional RTF amendment or major legislative RTF strengthening, with staggered timing across six states between 2012 and 2021. **Data:** QWI county-quarter panel, 2005Q1–2024Q4, covering 14 states (6 treated, 8 agricultural controls). **Method:** Staggered difference-in-differences with TWFE estimator, county and quarter fixed effects, standard errors clustered at the state level (14 clusters), supplemented by wild cluster bootstrap for finite-sample inference. **Sample:** County-quarters with non-suppressed NAICS 112 employment in 6 RTF-strengthening states (ND, NC, MO, IA, GA, TX) and 8 agricultural control states (KS, NE, WI, TN, VA, SD, KY, MT) without RTF changes during the sample period. $SDE = \hat{\beta}/SD(Y)$ where $SD(Y)$ is the pre-treatment standard deviation of log employment. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).