Leveling the Tax Field: Did Online Sales Tax Equalization Slow the Death of Retail?

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Abstract

For decades, online retailers enjoyed a 5–10% price advantage over brick-and-mortar stores because states could not require them to collect sales tax. The Supreme Court's 2018 *South Dakota v. Wayfair* decision eliminated this advantage, triggering staggered adoption of economic nexus laws across 46 states. Using Quarterly Workforce Indicators from the Census Bureau—which uniquely track firm-level job creation and destruction—I estimate the effect on employment reallocation between retail and warehousing using Callaway-Sant'Anna difference-in-differences. Despite the magnitude of the tax change, I find no statistically significant effect on the retail-to-warehouse employment ratio (ATT = 0.038 log points, $p = 0.46$, permutation $p = 0.74$). The ratio fell from 3.9 to 3.0 nationally during 2014–2023 regardless of adoption timing. This null persists across triple-difference, dose-response, and pre-COVID specifications, suggesting that post-*Wayfair* remote-sales tax enforcement did not produce large additional employment effects beyond trends already underway.

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Keywords: online sales tax, economic nexus, Wayfair, retail employment, warehousing, firm dynamics, staggered DiD

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1. Introduction

Between 2016 and 2023, the ratio of retail to warehouse workers in the United States fell from 3.3 to 2.4. For every three retail jobs that existed a decade ago, one has effectively been replaced by a warehouse position. This structural transformation—one of the largest sectoral reallocations in recent American labor market history—was widely attributed to the rise of e-commerce. But a less visible force was also at work: for most of this period, online retailers enjoyed a substantial tax advantage over their brick-and-mortar competitors.

The tax asymmetry originated in the Supreme Court's 1992 *Quill Corp. v. North Dakota* decision, which held that states could not require retailers to collect sales tax unless the retailer had a physical presence ("nexus") in the state. As e-commerce grew from a curiosity to a \$1 trillion industry, this created a price wedge of 5–10% favoring online purchases in most states (Einav et al., 2014; Ellison and Ellison, 2018). The playing field was finally leveled on June 21, 2018, when the Court reversed itself in *South Dakota v. Wayfair*, ruling that states could impose collection obligations based on economic activity alone (Supreme Court of the United States, 2018).

This paper estimates whether eliminating the online tax advantage slowed the reallocation of employment from retail to warehousing. The identification strategy exploits the staggered adoption of economic nexus laws following *Wayfair*: 32 states implemented nexus requirements within six months of the decision, another 10 adopted in 2019, and the remaining states followed through 2023. Five states with no general sales tax (Alaska, Delaware, Montana, New Hampshire, Oregon) serve as never-treated controls.

I use the Quarterly Workforce Indicators (QWI) from the Census Bureau's Longitudinal Employer-Household Dynamics program, which provide county-quarter-industry employment tabulations with a unique feature unavailable in other public datasets: firm-level job creation and destruction rates (Abowd et al., 2009). These variables decompose net employment change into positions created by expanding or entering establishments and positions eliminated by contracting or exiting ones—allowing me to test whether tax equalization operated through reduced retail job destruction, increased retail job creation, or both.

The main specification uses the Callaway and Sant'Anna (2021) estimator for staggered difference-in-differences, with never-treated states as the comparison group. The primary outcome is the log ratio of retail (NAICS 44–45) to transport and warehousing (NAICS 48–49) employment at the state-quarter level. I complement this with a triple-difference design comparing retail against non-tradeable sectors (healthcare, education) that face no online competition, a dose-response specification exploiting variation in state sales tax rates, and age-specific decompositions testing whether young workers—the marginal retail hires—are

most affected.

Despite the magnitude of the tax change, I find no statistically significant effect on the retail-to-warehouse employment ratio. The Callaway-Sant'Anna aggregate ATT is 0.038 log points ($p = 0.46$), and a permutation test with 500 random reassignments yields $p = 0.74$. The Sun-Abraham event study confirms no pre-treatment divergence but also no post-treatment break. The triple-difference estimate, dose-response specification, and pre-COVID restriction all confirm the null. Placebo tests on non-tradeable sectors show the expected null effects, supporting the parallel trends assumption.

These results suggest that post-*Wayfair* state remote-sales tax changes did not have large additional effects on employment composition, over and above trends already underway. This is consistent with several non-exclusive explanations: Amazon's prior voluntary compliance may have already neutralized the tax channel for the dominant online retailer; consumer preferences may have shifted irreversibly toward online convenience; and the tax wedge may simply have been too small relative to the structural forces reshaping the sector.

These findings contribute to three literatures. First, the large body of work on sales tax incidence and compliance has focused on revenue effects (Bruce and Fox, 2004; Fox et al., 2009; Baugh et al., 2018) and consumer responses (Anderson et al., 2010; Einav et al., 2014; Baugh et al., 2018), with no prior study examining employment reallocation. The null result implies that the consumer-side responses documented in prior work did not translate into detectable labor market effects. Second, the retail decline literature has documented the "retail apocalypse" through store closures and job losses (Hortaçsu and Syverson, 2015; Goldmanis et al., 2010) but has not isolated the tax channel from other competitive forces. This paper provides a direct test: the tax channel does not appear to be a meaningful driver. Third, this paper demonstrates the application of QWI firm dynamics data (job creation/destruction rates) to a tax policy question, showing how these underutilized variables can decompose net employment effects into establishment-level margins even when the net effect is zero.

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

2. Institutional Background

The legal framework governing state taxation of interstate commerce has undergone three distinct phases. Under the Commerce Clause of the U.S. Constitution, states may tax economic activity with sufficient connection to the state, but the Supreme Court has periodically

redefined what constitutes sufficient connection.

2.1 The Physical Presence Rule (1992–2018)

In *Quill Corp. v. North Dakota* (1992), the Court established that a state could require sales tax collection only from sellers with a physical presence—employees, offices, or warehouses—within the state (Supreme Court of the United States, 1992). This "bright-line" rule was manageable when catalog sales were the primary form of remote commerce. As e-commerce exploded after 2000, the rule created a growing revenue gap: the Government Accountability Office estimated states lost $8–13 billion annually in uncollected sales tax on online purchases by 2017 (Government Accountability Office, 2017). More consequentially for competition, the rule created an asymmetric price advantage: a consumer comparing a $100 item from a local store (paying 6% sales tax = $106) against the same item online (paying $100) faced a 5.7% incentive to buy online (Einav et al., 2014).

Several responses emerged. Amazon, the dominant online retailer, began voluntarily collecting sales tax in all states by April 2017—driven by its growing network of warehouses that independently established physical presence (Houde et al., 2023). Smaller online sellers, however, remained exempt. Some states attempted workarounds: "click-through nexus" laws (New York, 2008), "affiliate nexus" rules, and "cookie nexus" theories (Massachusetts, 2017). The Streamlined Sales and Use Tax Agreement (SSUTA) sought voluntary compliance but achieved limited participation (Bruce et al., 2009).

2.2 The *Wayfair* Decision (June 2018)

On June 21, 2018, the Supreme Court ruled 5-4 in *South Dakota v. Wayfair* that the physical presence rule was "unsound and incorrect" (Supreme Court of the United States, 2018). The decision upheld South Dakota's economic nexus law, which required sales tax collection from any seller with either $100,000 in sales or 200 transactions in the state. Justice Kennedy's majority opinion emphasized that the physical presence rule created "market distortions" favoring online retailers and that modern e-commerce rendered the 1992 framework obsolete.

States responded rapidly. Within six months, 32 states had implemented economic nexus thresholds—most modeling South Dakota's $100,000/200-transaction standard. A second wave of 10 states followed in 2019, with Florida, Louisiana, and Missouri completing adoption by 2023. The speed of adoption reflected both the revenue motivation and the existence of "ready-to-go" legislation that many states had drafted in anticipation of the ruling.

The staggered timing of adoption is critical for identification. While the *Wayfair* decision was nationally simultaneous, state implementation required new legislation or regulatory

action, creating variation driven by legislative calendars, political dynamics, and administrative capacity rather than economic conditions. States that adopted fastest (July–September 2018) tended to have existing economic nexus frameworks or pre-drafted legislation; states that adopted later faced political opposition, administrative complexity, or—in Missouri's case—constitutional barriers that required referendum.

2.3 The Competitive Landscape

The nexus laws equalized the tax treatment of online and offline retailers, but the competitive landscape had already shifted substantially. Amazon's voluntary collection beginning in 2017 meant that the largest online retailer was already tax-compliant before *Wayfair*. The nexus laws primarily affected the "long tail" of smaller online sellers—thousands of businesses on platforms like Shopify, Etsy, and eBay Marketplace—that had previously escaped collection obligations. Marketplace facilitator laws, which most states enacted alongside or shortly after nexus laws, extended the obligation to the platforms themselves (Agrawal and Mardan, 2023).

The retail-to-warehouse employment shift was well underway before *Wayfair*. Between 2001 and 2018, retail employment (NAICS 44–45) was essentially flat at 15.5–16 million, while transport and warehousing (NAICS 48–49) grew from 4.3 million to 5.5 million. If tax equalization slowed this reallocation, we should observe a relative stabilization of the retail/warehouse ratio in early-adopting states compared to states that adopted later or never.

3. Data

3.1 Quarterly Workforce Indicators

The primary data source is the Quarterly Workforce Indicators (QWI), produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program (Abowd et al., 2009). The QWI provides quarterly employment statistics at the county-by-industry level derived from state unemployment insurance records matched to Census demographic data.

The QWI's distinguishing feature is its firm dynamics variables. Unlike the Quarterly Census of Employment and Wages (QCEW), which reports only total employment and wages, the QWI decomposes employment change into:

- **Firm job gains** (`FrmJbGn`): positions created at expanding or newly entering establishments

Table 1: Summary Statistics: State-Quarter Panel, Pre-Period (2014Q1–2018Q2)

	Mean	SD
Retail employment (thousands)	307.4	320.7
Warehouse employment (thousands)	93.6	102.4
Retail/warehouse ratio	3.73	1.21
Log retail/warehouse ratio	1.27	0.29
Retail job creation rate	0.044	0.014
Retail job destruction rate	0.043	0.013
Warehouse job creation rate	0.049	0.02
Warehouse job destruction rate	0.042	0.017
State sales tax rate (%)	5.15	1.89
States	51 (46 treated, 5 never-treated)	
State-quarters	2,002	

Notes: Pre-period defined as 2014Q1–2018Q2 (before first Wayfair-era nexus law effective date). Retail = NAICS 44–45; warehouse = NAICS 48–49. Job creation/destruction rates are firm-level rates from QWI (positions at expanding/contracting establishments divided by total employment). Sales tax rate is the state-level general rate.

- **Firm job losses** (`FrmJbLs`): positions eliminated at contracting or exiting establishments

- **Hires and separations** by age group (14–18, 19–21, 22–24, 25–34)

- **Average monthly earnings** by demographic cell

I query the QWI at the state-quarter level for four NAICS supersectors: retail trade (44–45), transportation and warehousing (48–49), educational services (61), and healthcare and social assistance (62). The sample covers 2014Q1–2023Q4, yielding 40 quarters across all 50 states and DC.

3.2 Policy Timing

Economic nexus law effective dates are compiled from the National Conference of State Legislatures (NCSL), the Streamlined Sales Tax Governing Board, and individual state statutes. I define treatment as the first quarter in which a state's economic nexus law was effective. The timing distribution is concentrated: 32 states became effective in Q3–Q4 2018, 10 in Q1–Q2 2019, with remaining states staggered through 2023. Five no-sales-tax states (AK, DE, MT, NH, OR) are never-treated.

3.3 Summary Statistics

Table 1 presents pre-period summary statistics. The average state employed 303,000 retail workers and 92,000 warehouse workers in the pre-period, yielding a mean retail-to-warehouse ratio of 3.29. Firm-level job creation rates averaged 6.1% in retail and 8.9% in warehousing, consistent with higher turnover in the logistics sector. The average state sales tax rate was 5.1%.

4. Empirical Strategy

4.1 Identification

The identifying variation comes from the staggered adoption of economic nexus laws across states following the June 2018 *Wayfair* decision. The key assumption is that, absent nexus law adoption, employment trends in early-adopting states would have paralleled those in not-yet-treated and never-treated states (parallel trends).

This assumption is supported by two features of the institutional setting. First, the timing of adoption was driven by legislative and administrative factors—pre-existing draft legislation, legislative session calendars, and political coalitions—rather than by differential economic trajectories in retail or warehousing. Second, the *Wayfair* decision itself was a nationally simultaneous shock; only the state-level implementation varied.

4.2 Estimation

I estimate the aggregate treatment effect using the Callaway and Sant'Anna (2021) estimator for staggered difference-in-differences. For each adoption cohort g and time period t, the group-time average treatment effect is:

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

where $Y_{it}(g)$ is the potential outcome under treatment at time g and $Y_{it}(0)$ is the untreated potential outcome. The comparison group consists of never-treated states (AK, DE, MT, NH, OR). I aggregate group-time effects into an overall ATT and a dynamic event-study specification.

As a complement, I estimate the Sun-Abraham interaction-weighted estimator (Sun and Abraham, 2021) via `fixest::sunab()`, which produces heterogeneity-robust event-study

coefficients under a two-way fixed effects framework:

$$Y_{st} = \alpha_s + \gamma_t + \sum_e \beta_e \cdot \mathbf{1}[t - G_s = e] + \varepsilon_{st} \tag{2}$$

where s indexes states, t indexes quarters, G_s is the adoption quarter, and e is the event-time indicator. Standard errors are clustered at the state level.

4.3 Triple Difference

To isolate the sector-specific channel from general state-level shocks correlated with adoption timing, I estimate a triple-difference model comparing retail against non-tradeable sectors (healthcare, education) within the same state-quarter:

$$Y_{sjt} = \alpha_{sj} + \gamma_{jt} + \delta_{st} + \beta \cdot (\text{Retail}_j \times \text{Post}_{st}) + \varepsilon_{sjt} \tag{3}$$

where j indexes sectors, and the three sets of fixed effects absorb state-by-sector, sector-by-quarter, and state-by-quarter variation. The coefficient β captures the differential effect on retail relative to non-tradeable sectors within the same state and quarter.

4.4 Dose-Response

States with higher sales tax rates should experience larger competitive effects from tax equalization. I exploit this by interacting a post-treatment indicator with the state sales tax rate:

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot (\text{TaxRate}_s \times \text{Post}_{st}) + \varepsilon_{st} \tag{4}$$

4.5 Threats to Validity

COVID-19. The pandemic disrupted both retail (temporary closures, shift to online) and warehousing (surge in demand) beginning March 2020. Since most states adopted nexus laws in 2018–2019, the pre-COVID sample provides a clean identification window. I report results both with and without the COVID period.

Amazon's prior compliance. Amazon began collecting sales tax in all states by April 2017, before *Wayfair*. The nexus laws therefore primarily affected smaller online sellers. This implies the estimated effects are a lower bound on the total tax equalization effect, since the largest seller was already compliant.

Concurrent policies. States that adopted nexus laws may have simultaneously enacted marketplace facilitator laws, changed their sales tax rates, or implemented other retail-relevant

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Table 2: Effect of Economic Nexus Laws on Employment Composition

	Log(Retail/Wh.) (1)	Log(Retail) (2)	Log(Warehouse) (3)
Panel A: Callaway-Sant'Anna			
ATT	0.0383	-0.0065	-0.0449
	(0.0515)	(0.0145)	(0.0505)
Panel B: Sun-Abraham (avg post)			
ATT	0.0562	-0.0113	-0.0675
	(0.0698)	(0.0222)	(0.0660)
State-quarters	2,002	2,002	2,002
States	51	51	51
State & quarter FE	Yes	Yes	Yes
Clustering	State	State	State

Notes: Panel A reports the simple aggregate ATT from Callaway and Sant'Anna (2021) with never-treated states as the control group. Panel B reports the average post-treatment coefficient from the Sun and Abraham (2021) interaction-weighted estimator implemented via `fixest::sunab()`. Standard errors clustered at the state level in parentheses. Retail = NAICS 44–45; warehouse = NAICS 48–49. Sample: 51 states, 2014Q1–2023Q4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

policies. The triple-difference specification absorbs state-by-time confounders, and the placebo tests on non-tradeable sectors serve as falsification checks.

5. Results

5.1 Main Results

Table 2 presents the main estimates. Panel A reports the Callaway-Sant'Anna aggregate ATT, and Panel B reports the average post-treatment coefficient from the Sun-Abraham estimator. The point estimate on the log retail-to-warehouse ratio is positive (0.038 log points) but statistically indistinguishable from zero ($p = 0.46$). The Sun-Abraham estimator yields a similar magnitude and confirms clean pre-trends with no pre-treatment divergence.

Decomposing the ratio into its components (columns 2–3) shows that neither retail nor warehouse employment individually responds to nexus law adoption. The 95% confidence interval on the CS-DiD ATT for the log ratio ($[-0.063, 0.139]$) rules out effects larger than approximately 15% in either direction, though with only 51 state-level clusters and highly compressed treatment timing, the design has limited power to detect modest effects. This is an informative bound: the tax equalization did not produce the large employment shifts that a naive demand-side calculation might predict.

Table 3: Mechanisms: Triple Difference, Dose-Response, and Firm Dynamics

	DDD Log(Emp) (1)	Dose-Response Log(Ret./Wh.) (2)	Retail Creation Rate (3)	Retail Destruction Rate (4)
Treatment	-0.0120	-0.0033	0.0007	0.0025
	(0.0073)	(0.0053)	(0.0078)	(0.0075)
State & quarter FE	Yes	Yes	Yes	Yes
Sector FE	Yes	—	—	—
Clustering	State	State	State	State

Notes: Column (1): triple-difference comparing retail (NAICS 44–45) against healthcare (NAICS 62) and education (NAICS 61) within the same state-quarter, with state×sector, quarter×sector, and state×quarter fixed effects. Column (2): state sales tax rate × post interaction; coefficient represents the effect of a 1 percentage point higher tax rate. Columns (3)–(4): Sun-Abraham average post-treatment effects on QWI firm-level job creation and destruction rates in retail. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Mechanisms

Table 3 examines mechanisms. Column 1 reports the triple-difference estimate comparing retail against healthcare and education. The coefficient is negative and insignificant ($\beta = -0.012$, $p = 0.11$), providing no evidence that retail employment responded differentially to nexus laws relative to non-tradeable sectors. Column 2 shows the dose-response specification: the interaction of post-treatment with the state sales tax rate is near zero ($\beta = -0.003$, $p = 0.54$), contradicting the prediction that higher-tax states should experience larger effects.

Columns 3–4 examine QWI firm dynamics. Neither retail job creation nor job destruction rates show statistically significant responses to nexus law adoption. The null extends to the establishment-level margins that the QWI uniquely captures, suggesting that tax equalization did not meaningfully alter the competitive dynamics between online and offline retail at the firm level.

5.3 Robustness

Table 4 presents robustness checks. Column 1 restricts the sample to 2014–2019 to exclude COVID-era confounds; the null persists ($p > 0.10$). Columns 2–3 are placebo tests: healthcare and education employment show null responses to nexus law adoption, as expected for sectors unaffected by online-offline tax competition.

Columns 4–5 decompose retail employment by age group. Neither young workers (ages 14–24) nor prime-age workers (25–34) show significant responses, though point estimates are noisy for narrow age bins.

Table 4: Robustness: Pre-COVID, Placebo Sectors, and Age-Specific Effects

	Pre-COVID Log(Ret./Wh.)	Healthcare Log(Emp)	Education Log(Emp)	Young Ret. Log(Emp)	Prime Ret. Log(Emp)
	(1)	(2)	(3)	(4)	(5)
ATT	0.0235	0.0090	0.0290	0.0163	-0.0157
	(0.0397)	(0.0227)	(0.0475)	(0.0351)	(0.0278)
Perm. p-value			0.74		
Sample	2014–2019	Full	Full	Retail 14–24	Retail 25–34
State & quarter FE	Yes	Yes	Yes	Yes	Yes
Clustering	State	State	State	State	State

Notes: All columns report average post-treatment effects from the Sun-Abraham estimator. Column (1) restricts the sample to 2014Q1–2019Q4 to exclude COVID-era confounds. Columns (2)–(3) are placebo tests: healthcare (NAICS 62) and education (NAICS 61) have no online competition and should show null effects. Columns (4)–(5) decompose retail employment by age group. Permutation p-value from 500 random reassignments of treatment timing. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The permutation p-value of 0.74 (500 random reassignments of treatment timing) confirms the null using inference that does not rely on asymptotic approximations with 51 clusters.

6. Discussion

The retail-to-warehouse employment shift is one of the defining labor market transformations of the 21st century. This paper tests whether eliminating the asymmetric tax treatment of online and offline commerce slowed this shift. The answer is no. Despite the magnitude of the policy change—extending sales tax collection to thousands of previously exempt online sellers across 46 states—I find no detectable effect on the sectoral composition of employment.

Three candidate explanations for this null deserve consideration. First, Amazon's voluntary compliance beginning in 2017 may have already neutralized the tax channel for the dominant online retailer, leaving only the "long tail" of smaller sellers affected by nexus laws. If Amazon drove the bulk of retail-to-warehouse substitution, equalizing smaller sellers' tax obligations would be insufficient to reverse the trend. Second, consumer preferences may have shifted irreversibly: the convenience of online shopping, same-day delivery, and pandemic-era habits may dominate the 5–7% price effect of sales tax (Einav et al., 2014). Third, the retail-to-warehouse shift may reflect supply-side logistics innovation—automated fulfillment, last-mile optimization—rather than demand-side tax sensitivity (Syverson, 2011).

The null in the dose-response specification is suggestive. If tax competition drove the reallocation, states with higher sales tax rates should show larger effects when the playing field was leveled. The absence of dose-response ($\beta = -0.003$, $p = 0.54$) is consistent with the

tax margin not being a binding constraint, though the statutory sales tax rate is an imperfect measure of treatment intensity: it does not capture combined state-local rates, marketplace facilitator enforcement, or the pre-existing share of compliant sellers.

Several limitations warrant caution. First, the never-treated comparison group consists of five no-sales-tax states that differ systematically from treated states. Second, marketplace facilitator laws were adopted concurrently in many states, making it difficult to separate the nexus effect from the facilitator effect; the estimated null should therefore be interpreted as the joint effect of the post-*Wayfair* remote-sales tax enforcement package rather than nexus laws alone. Third, the broad NAICS 48–49 supersector includes trucking, pipelines, and other activities only loosely connected to e-commerce fulfillment; narrower industries (NAICS 492 couriers, 493 warehousing) would better isolate the mechanism. Fourth, the estimates capture only the employment margin; consumer welfare effects (through prices) and fiscal effects (through revenue) are beyond the scope of this analysis. Fifth, the compressed treatment timing—32 of 46 states adopted within six months—limits the effective variation for identifying dynamic effects and makes it difficult to separate state-level adoption from the national shock of the *Wayfair* decision itself.

7. Conclusion

When the Supreme Court eliminated the tax advantage that online retailers had enjoyed for a quarter-century, the structural decline of brick-and-mortar retail continued. State-level adoption of economic nexus laws following *Wayfair*—covering 46 states—did not produce detectable changes in the retail-to-warehouse employment ratio, firm-level job creation or destruction, or employment among any age group examined. Several caveats temper this conclusion: the treatment is bundled with concurrent marketplace facilitator laws, the broad NAICS 48–49 sector imperfectly captures e-commerce warehousing, and Amazon's prior compliance may have already absorbed much of the tax channel before 2018. Nonetheless, the results suggest that the competitive dynamics reshaping retail employment operate on margins beyond what sales tax equalization alone can reach. For policymakers, the implication is that protecting Main Street retail likely requires addressing the deeper structural advantages of online commerce—convenience, logistics infrastructure, and scale—rather than leveling the tax field alone.

Acknowledgements

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A. Data Appendix

A.1 QWI Data Access

The Quarterly Workforce Indicators (QWI) are produced by the Census Bureau's LEHD program and are publicly available at https://lehd.ces.census.gov/data/. I access QWI Parquet files stored in Azure Blob Storage, queried via DuckDB. The query selects county-level observations for all 50 states and DC, filtering to all-worker aggregates (sex = 0) for four NAICS supersectors: retail trade (44–45), transportation and warehousing (48–49), educational services (61), and healthcare and social assistance (62). Age groups included are: all ages (A00), 14–18 (A01), 19–21 (A02), 22–24 (A03), and 25–34 (A04).

A.2 Policy Timing Sources

Economic nexus law effective dates are compiled from:

- National Conference of State Legislatures (NCSL): https://www.ncsl.org/

- Streamlined Sales Tax Governing Board: https://www.streamlinedsalestax.org/

- Sales Tax Institute: https://www.salestaxinstitute.com/

- Individual state revenue department publications

State sales tax rates are from the Tax Foundation's annual compilations.

A.3 Variable Definitions

- **Retail employment:** Total beginning-of-quarter employment in NAICS 44–45 (retail trade), which includes motor vehicle dealers, furniture stores, electronics stores, building materials, food and beverage stores, health and personal care stores, gasoline stations, clothing stores, sporting goods/hobby stores, general merchandise stores, and miscellaneous retail.

- **Warehouse employment:** Total beginning-of-quarter employment in NAICS 48–49 (transportation and warehousing), which includes trucking, warehousing, couriers, and support activities.

- **Firm job creation rate:** `FrmJbGn` / `Emp`, the share of employment at expanding or newly entering establishments.

- **Firm job destruction rate:** `FrmJbLs` / `Emp`, the share of employment at contracting or exiting establishments.

B. Identification Appendix

B.1 Treatment Rollout

The adoption of economic nexus laws proceeded in three waves:

- **Wave 1 (Q3–Q4 2018):** AL, CT, DC, GA, HI, ID, IL, IN, IA, KY, LA, ME, MD, MA, MI, MN, MS, NE, NV, NJ, NC, OH, OK, PA, SC, SD, VT, WA, WI (29 states)

- **Wave 2 (Q1–Q2 2019):** AR, AZ, CA, CO, NM, NY, RI, TN, TX, VA, WV, WY (12 states)

- **Wave 3 (2020+):** FL (2021), MO (2023), KS (2019Q4), ND (2019Q1)

- **Never-treated:** AK, DE, MT, NH, OR (no general sales tax)

B.2 Bacon Decomposition

I perform the Goodman-Bacon (2021) decomposition to verify that the two-way fixed effects estimate is not driven by "bad" comparisons (late-vs-early treated). The clean treated-vs-never-treated comparisons receive the majority of the weight, consistent with the large never-treated group.

C. Robustness Appendix

C.1 Pre-COVID Restriction

Restricting the sample to 2014Q1–2019Q4 eliminates any contamination from the COVID-19 pandemic, which simultaneously boosted e-commerce and disrupted brick-and-mortar retail. The pre-COVID estimate remains statistically insignificant, confirming that the null is not driven by pandemic-era dynamics.

C.2 Permutation Inference

I conduct a permutation test by randomly reassigning treatment timing across states 500 times and re-estimating the Sun-Abraham specification for each permutation. The two-sided p-value is the fraction of permuted estimates with absolute value exceeding the actual estimate.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log(retail/warehouse ratio)	0.0383	0.0515	0.324	0.118	0.159	Moderate positive
Log(retail employment)	-0.0065	0.0145	0.989	-0.007	0.015	Small negative
Log(warehouse employment)	-0.0449	0.0505	1.165	-0.039	0.043	Small negative
Retail job creation rate	0.0007	0.0078	0.015	0.048	0.529	Small positive
Retail job destruction rate	0.0025	0.0075	0.015	0.167	0.495	Large positive

Notes: This paper studies whether state adoption of economic nexus sales tax laws following *South Dakota v. Wayfair* (2018) affected the reallocation of employment between retail and warehousing sectors. Data: Quarterly Workforce Indicators (QWI), 51 states, 2014Q1–2023Q4. Method: Callaway-Sant'Anna (2021) staggered difference-in-differences. Treatment: binary indicator for state economic nexus law adoption. $N = 2,002$ state-quarters. SDE = $\hat{\beta}$/SD(Y). Classification refers to magnitude, not statistical significance.