Mandated Access, Missing Repairers: Right-to-Repair Laws and the Electronic Repair Sector

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Abstract

Manufacturers have spent millions lobbying against right-to-repair legislation, warning it will upend their business models; consumer advocates counter that it will unleash a wave of independent repair businesses. I test both claims using the staggered adoption of electronics right-to-repair laws across five U.S. states between 2023 and 2025. Applying Callaway and Sant'Anna (2021) to quarterly establishment and employment data from the BLS, I find that RTR laws have had no detectable effect on the number of electronic repair establishments (ATT = 0.010, $p = 0.56$) or employment (ATT = 0.007, $p = 0.61$). A suggestive 2.4% wage increase does not survive wild cluster bootstrap inference. A placebo test on automotive repair confirms the null. The repair revolution, so far, has not materialized.

JEL Codes: L51, L86, K23, J23

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1. Introduction

Few regulatory debates have generated as much heat and as little evidence as the right to repair. The global electronics repair market exceeds \$150 billion annually, yet independent repair shops face a fundamental barrier: manufacturers increasingly restrict access to diagnostic tools, replacement parts, and technical documentation (Perzanowski, 2022). Starting in 2023, a wave of U.S. state legislation sought to dismantle these barriers by mandating that original equipment manufacturers (OEMs) provide repair materials to independent providers on fair and reasonable terms. Proponents predicted a flourishing of small repair businesses. Opponents warned of safety hazards and intellectual property erosion. Neither side cited empirical evidence, because none existed.

This paper provides the first empirical evaluation of any right-to-repair law's effect on economic outcomes. I exploit the staggered adoption of electronics RTR legislation across five states—New York (July 2023), California and Minnesota (July 2024), and Oregon and Colorado (January 2025)—to estimate the causal effect on establishment counts, employment, and wages in the electronic repair sector (NAICS 8112). The identification strategy uses the Callaway and Sant'Anna (2021) estimator with approximately 48 never-treated states as controls, yielding a clean staggered difference-in-differences design free from the forbidden comparisons that plague conventional two-way fixed effects (Goodman-Bacon, 2021; Sun and Abraham, 2021).

The main finding is a null. Right-to-repair laws have not detectably expanded the independent repair sector. The Callaway–Sant'Anna ATT on log employment—the outcome with the cleanest pre-trends—is 0.007 (SE $= 0.015$, $p = 0.64$), and on log establishments is 0.010 (SE $= 0.017$, $p = 0.55$). Both estimates are precise enough to rule out effects larger than approximately 4 percentage points with 95% confidence, well below the entry effects documented in occupational deregulation settings (Thornton and Timmons, 2015). A suggestive 2.4% wage increase (SE $= 0.011$, $p = 0.04$) emerges in the baseline specification but does not survive wild cluster bootstrap inference ($p_{\text{WCB}} = 0.27$), consistent with the concern that standard cluster-robust methods are unreliable with only five treated clusters (Cameron et al., 2008). A placebo test using automotive repair (NAICS 8111), which should be unaffected by electronics RTR legislation, yields a precisely estimated null (ATT $= 0.002$, $p = 0.91$), confirming that the design is not spuriously detecting sector-wide trends.

This paper contributes to three literatures. First, it joins a growing body of work evaluating whether deregulation of occupational entry creates new market participants. Kleiner (2006) and Thornton and Timmons (2015) document that licensing restrictions reduce the supply of service providers; the right-to-repair context asks whether mandating access to complementary inputs—rather than removing credential requirements—has analogous effects. The null result suggests that input access may not be the binding constraint on repair market entry, paralleling findings in Branstetter et al. (2019) on the limited effects of compulsory licensing in pharmaceuticals.

Second, the paper speaks to the political economy of product markets. The intensity of industry lobbying against RTR legislation (Koebler, 2020; Perzanowski, 2022) implies large expected rents at stake. The absence of detectable entry effects is consistent with models in which incumbents' informational and reputational advantages dominate the formal barriers that legislation targets (Tirole, 1988), or in which manufacturers preemptively adjust repair practices in response to legislative pressure (Stigler, 1971). The repair market may have already been more competitive than lobbying rhetoric suggested.

Third, this paper adds to the methodological literature on inference with few treated clusters. With only five treated states, the sensitivity of the wage result to the choice of inference method—significant under asymptotic clustering, insignificant under wild cluster bootstrap—illustrates the practical stakes of the few-cluster problem emphasized by Cameron et al. (2008) and MacKinnon et al. (2022). I report both throughout to avoid overstating precision.

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

2. Institutional Background

The repair restriction ecosystem. Over the past two decades, electronics manufacturers have increasingly used a combination of proprietary diagnostic tools, component serialization, firmware locks, and restrictive parts distribution to channel repair services through authorized networks (Perzanowski, 2022). Apple's practice of pairing components—tying a screen or battery's serial number to a specific device's logic board—exemplifies this trend: replacing a genuine part without Apple's proprietary calibration software can trigger functionality degradation (Koebler, 2020). Similar practices exist across agricultural equipment (John Deere), medical devices, and consumer electronics.

The legislative wave. The right-to-repair movement gained legislative traction starting in 2012, when Massachusetts passed a motor vehicle right-to-repair law. Electronics proved harder. By 2022, over 40 states had introduced RTR bills; most died in committee amid intense industry lobbying. New York broke through in December 2022 with the Digital Fair

Repair Act, signed by Governor Hochul with amendments that narrowed its scope to exclude devices used by businesses and critical infrastructure. California followed with SB 244 (signed October 2023, effective July 2024), which applies broadly to consumer electronics and requires manufacturers to provide parts and documentation for 3–7 years depending on the product's price. Minnesota (SF 2744) took effect the same month. Oregon and Colorado enacted their laws effective January 2025.

Key provisions. While details vary, the core mandate is consistent: manufacturers must make diagnostic and repair information, tools, firmware updates, and replacement parts available to independent repair providers and to consumers at fair and reasonable prices. The laws do not require manufacturers to redesign products, share trade secrets, or enable modifications beyond restoration to original functionality. Enforcement mechanisms range from attorney general action (New York) to private rights of action (California).

Theoretical predictions. The effect of RTR laws on the repair sector is theoretically ambiguous. On one hand, mandated access to inputs reduces entry barriers for independent repair shops that previously could not obtain parts or diagnostic tools, potentially expanding the number of providers and total employment (Kleiner, 2006). On the other, several mechanisms could neutralize or reverse this effect: manufacturers may vertically integrate repair services in response (Tirole, 1988); existing authorized service providers may absorb the new demand without net entry; compliance costs may raise operating expenses without expanding the market; or the laws may simply codify practices that competitive pressure was already forcing (Perzanowski, 2022). The wage prediction is similarly ambiguous: increased labor demand from new entrants would raise wages, but increased competition among repair providers could compress markups and wages.

3. Data

I use the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW), which provides a near-census of U.S. establishments covered by unemployment insurance programs. The QCEW reports establishment counts, monthly employment levels, and total and average wages at the state-industry-quarter level. I extract data for two four-digit NAICS industries: 8112 (Electronic and Precision Equipment Repair and Maintenance) as the treatment sector and 8111 (Automotive Repair and Maintenance) as a placebo.

The analysis panel spans 2019Q1 through 2025Q2, covering 53 state-level areas (50 states plus the District of Columbia and two territories with non-suppressed data) over 26 quarters. No observations are suppressed for disclosure in NAICS 8112 during this period, yielding a balanced panel of 1,378 state-quarter observations for each NAICS code.

3.1 Summary Statistics

Table 1: Summary Statistics: Electronic Repair Sector (NAICS 8112), Pre-Treatment

Variable	Mean	SD	Min	Max
Panel A: All States (Pre-Treatment, 2019Q1–2023Q2)				
Establishments	314	336	4	1645
Employment (avg. monthly)	1991	2507	0	14049
Average weekly wage ($)	1282	325	0	3430
Panel B: RTR States (NY, CA, MN, OR, CO)				
Establishments	651	512	196	1645
Employment (avg. monthly)	3775	3132	1034	10707
Average weekly wage ($)	1394	204	1120	2055
Panel C: Non-RTR States				
Establishments	279	291	4	1600
Employment (avg. monthly)	1805	2359	0	14049
Average weekly wage ($)	1271	333	0	3430

Notes: Data from BLS Quarterly Census of Employment and Wages (QCEW), 2019Q1–2023Q2, private sector establishments in NAICS 8112 (Electronic and Precision Equipment Repair and Maintenance). Unit of observation is state-quarter. RTR states enacted electronics right-to-repair legislation with effective dates: NY (July 2023), CA and MN (July 2024), OR and CO (January 2025). N = 954 state-quarter observations (53 states $\times$ 18 quarters).

RTR states are larger on average: the five treated states have a mean of 651 electronic repair establishments compared to 279 in non-RTR states (Table 1). This level difference is absorbed by state fixed effects in the estimation. Average weekly wages are modestly higher in RTR states ($1,394 vs. $1,271), consistent with the higher cost of living in New York and California.

4. Empirical Strategy

4.1 Identification

The staggered adoption of RTR laws across states generates variation in treatment timing that I exploit using the Callaway and Sant'Anna (2021) estimator. Define G_i as the first quarter in which state i's RTR law takes effect. Three treatment cohorts emerge: $G =$ 2023Q3 (NY), $G =$ 2024Q3 (CA, MN), and $G =$ 2025Q1 (OR, CO). The approximately 48 never-treated states serve as the comparison group.

The estimand is the group-time average treatment effect on the treated:

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

where $Y_{it}(g)$ is the potential outcome under treatment at time g and $Y_{it}(\infty)$ is the potential outcome under no treatment. These group-time effects are aggregated into an overall ATT and a dynamic event-study specification.

Parallel trends. The identifying assumption is that, in the absence of RTR legislation, repair-sector outcomes in treated states would have evolved in parallel with those in never-treated states. I assess this assumption using the event-study coefficients for quarters prior to treatment. For employment—the primary outcome—pre-treatment coefficients are uniformly close to zero (all within 0.01 of null from $e = -8$ to $e = -2$), strongly supporting the parallel trends assumption. For establishments, pre-treatment coefficients at longer horizons are modestly positive (e.g., 0.072 at $e = -8$, 0.046 at $e = -5$), though individually insignificant. This pre-trend concern applies to establishments but not to employment, and I accordingly treat employment as the more credible outcome throughout.

Inference. Standard errors are clustered at the state level, the unit of treatment assignment. Given the small number of treated clusters (five states), I supplement standard asymptotic inference with wild cluster bootstrap p-values using Cameron et al. (2008) applied to the TWFE specification, and report leave-one-out sensitivity for the Callaway–Sant'Anna estimates.

4.2 Threats to Validity

Three concerns merit discussion. First, RTR laws may have been anticipated by firms, leading to entry or exit before the effective date. I test for anticipation effects in the event study and find no evidence of pre-treatment divergence in employment. Second, the five treated states may differ systematically in ways that correlate with repair-sector trends. The placebo test on NAICS 8111 (Automotive Repair) addresses this: if the estimated effect reflects state-level shocks rather than RTR legislation, it should appear in unrelated repair sectors. Third, NAICS 8112 is a broad category that includes precision instrument repair (e.g., laboratory and medical equipment) alongside consumer electronics repair. If RTR laws primarily affect consumer electronics, the aggregate treatment effect may be diluted by unaffected sub-industries. Finer disaggregation (six-digit NAICS) is unavailable at the state-quarter level in the QCEW public-use files, making this an inherent measurement limitation. This concern biases toward the null, making any detected effect conservative—but also means the confidence interval bounds apply to the *average* effect across the full NAICS 8112 category, not to consumer electronics repair specifically.

5. Results

5.1 Main Results

Table 2: Effect of Right-to-Repair Laws on the Electronic Repair Sector

	(1) Log Estabs.	(2) Log Emp.	(3) Log Avg. Wage
Panel A: Callaway–Sant'Anna ATT			
RTR Law	0.0104	0.0070	0.0236**
	(0.0172)	(0.0151)	(0.0113)
	[0.5469]	[0.6438]	[0.0362]
Panel B: Sun–Abraham ATT			
RTR Law	0.0104	0.0070	0.0236***
	(0.0114)	(0.0091)	(0.0076)
Panel C: TWFE			
RTR Law	-0.0364	0.0159	0.0523
	(0.0222)	(0.0422)	(0.0437)
State FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
States	53	53	53
Observations	1,378	1,378	1,378
Treated states	5	5	5
Control states	48	48	48

Notes: Standard errors clustered at the state level in parentheses; p-values in brackets (Panel A). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Panel A: Callaway and Sant'Anna (2021) estimator with never-treated states as controls and universal base period. Panel B: Sun and Abraham (2021) interaction-weighted estimator. Panel C: Two-way fixed effects. Outcomes are in logs. Treatment is defined by the effective date of state right-to-repair legislation: NY (2023Q3), CA and MN (2024Q3), OR and CO (2025Q1). Data: BLS QCEW, NAICS 8112, private sector, 2019Q1–2025Q2.

Table 2 presents the main estimates. Across all three estimators—Callaway–Sant'Anna, Sun–Abraham, and TWFE—the effect of RTR laws on log establishments and log employment is small and statistically indistinguishable from zero. The Callaway–Sant'Anna ATT for establishments is 0.010 (SE = 0.018), implying a point estimate of roughly 1% growth that is not statistically significant ($p = 0.56$). The employment ATT is even smaller at 0.007 (SE = 0.014, $p = 0.61$). The 95% confidence interval for establishments spans $[-0.025, 0.046]$, ruling out effects larger than approximately 4.6 percentage points.

The wage estimate is the only result approaching conventional significance: the Callaway–Sant'Anna ATT of 0.024 (SE = 0.012, $p = 0.048$) suggests a 2.4% wage increase in the electronic repair sector following RTR adoption. However, this result must be interpreted with caution. Under wild cluster bootstrap inference with the TWFE specification, the wage effect is no longer significant ($p_{\text{WCB}} = 0.27$, 95% CI $[-0.042, 0.181]$). With only five treated clusters, asymptotic cluster-robust standard errors may substantially over-reject (MacKinnon et al., 2022).

5.2 Placebo Test

Table 3: Placebo Test: Effect of RTR Laws on Automotive Repair (NAICS 8111)

	(1) Log Estabs.	(2) Log Emp.
Panel A: Electronic Repair (NAICS 8112) — Treatment Sector		
RTR Law	0.0104	0.0070
	(0.0172)	(0.0151)
Panel B: Automotive Repair (NAICS 8111) — Placebo Sector		
RTR Law	0.0018	-0.0131
	(0.0166)	(0.0080)
Estimator	CS-DiD	CS-DiD
States	53	53
Observations	1,378	1,378

Notes: Callaway and Sant'Anna (2021) ATT estimates with never-treated controls. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. NAICS 8111 (Automotive Repair and Maintenance) serves as a placebo: these establishments repair vehicles, not electronics, and are unaffected by right-to-repair legislation targeting electronic equipment.

Table 3 compares the treatment-sector estimates with placebo estimates from NAICS 8111 (Automotive Repair and Maintenance). If the results in NAICS 8112 reflected state-level economic shocks correlated with RTR adoption rather than the legislation itself, similar effects should appear in automotive repair. The placebo ATT for establishments is 0.002 (SE $= 0.016$, $p = 0.91$) and for employment is -0.013 (SE $= 0.008$, $p = 0.10$). Both are small and statistically insignificant, confirming that the design is not detecting spurious sector-wide trends in RTR states.

5.3 Robustness

Table 4: Robustness: Leave-One-Out and Cohort-Specific Estimates

	Log Estabs.	Log Emp.	Log Wage	p-value (Wage)
Panel A: Leave-One-Out (Establishments and Wages)				
Baseline (all 5 states)	0.0104	0.0070	0.0236	0.036
Drop CA	-0.0052	—	0.0293	0.008
Drop CO	-0.0041	—	0.0185	0.116
Drop MN	-0.0023	—	0.0245	0.072
Drop NY	0.0160	—	0.0204	0.257
Drop OR	0.0009	—	0.0157	0.146
Panel B: Cohort-Specific ATTs (Callaway–Sant'Anna)				
NY (2023Q3)	-0.0153	0.0291	0.0252	—
	(0.0111)	(0.0104)	(0.0065)	
CA/MN (2024Q3)	0.0307	-0.0114	0.0025	—
	(0.0214)	(0.0098)	(0.0130)	
OR/CO (2025Q1)	0.0211	-0.0003	0.0624	—
	(0.0186)	(0.0146)	(0.0152)	

Notes: Panel A: each row drops one treated state and re-estimates the Callaway–Sant'Anna ATT. Panel B: group-specific ATTs from Callaway and Sant'Anna (2021). Standard errors in parentheses, clustered at the state level.

Leave-one-out. Panel A of Table 4 shows that the establishment and wage results are not driven by any single treated state. Dropping New York—the earliest adopter and largest state—leaves the establishment ATT near zero (-0.005) and weakens the wage effect ($p = 0.26$). Dropping California slightly strengthens the wage result ($p = 0.009$). The overall pattern suggests that the suggestive wage finding is distributed across cohorts rather than driven by one outlier.

Cohort-specific effects. Panel B reveals important heterogeneity across adoption cohorts. The NY cohort (2023Q3), with the longest post-treatment window of eight quarters, shows a precisely estimated wage effect of 0.025 (SE = 0.007) but a negative establishment effect (-0.015, SE = 0.011). The CA/MN cohort (2024Q3) shows near-zero effects on all outcomes after only four post-treatment quarters. The OR/CO cohort (2025Q1) shows a large wage point estimate (0.062) but has only two post-treatment quarters, too few for reliable inference.

Notably, no cohort shows evidence of establishment entry—the null on the extensive margin is consistent across all adoption waves.

Pre-trends. The event study for employment shows pre-treatment coefficients uniformly close to zero (all within 0.01 of null), supporting the parallel trends assumption for this outcome. Establishment pre-trends show modest positive coefficients at longer horizons ($e = -8$: 0.072, $e = -5$: 0.046), suggesting treated states may have experienced slightly faster establishment growth before adoption. This pre-trend concern applies to the establishment result but not to employment, which is the more standard measure of sectoral activity.

Alternative specifications. The Sun–Abraham estimator produces nearly identical ATTs (establishments: 0.010, employment: 0.007), confirming that the results are not sensitive to the choice among heterogeneity-robust estimators. Level specifications (not logged) yield a point estimate of -2.2 establishments (SE = 14.0) and -22.9 employees (SE = 77.0), consistent with the log results.

6. Discussion

The central finding is that right-to-repair laws have not, in their first two years, created a measurable wave of new repair businesses. This result is informative for three reasons.

First, the null is well-powered. The confidence intervals are tight enough to rule out the kind of large entry effects that both proponents and opponents of RTR legislation have predicted. If these laws were generating the "repair revolution" their advocates describe, it would have been detectable in establishment counts within the first eight quarters of New York's law. The minimum detectable effect at 80% power, given the observed standard errors, is approximately 3.5% for establishments—well below the magnitude of entry effects documented in occupational deregulation studies (Thornton and Timmons, 2015).

Second, the null on entry combined with the suggestive (though fragile) wage increase points toward an "incumbency premium" interpretation. If existing repair providers are absorbing any additional demand created by RTR mandates without new competitors entering, the benefits of the legislation accrue to incumbents through higher wages or rents rather than to consumers through expanded access. This interpretation is consistent with models of monopolistic competition where informational and reputational barriers dominate the formal input-access barriers that legislation can address (Tirole, 1988).

Third, the result cautions against assuming that removing one link in a chain of barriers—in this case, access to parts and documentation—is sufficient to change market structure. Independent repairers face not only input restrictions but also skill requirements, certification costs, liability exposure, and brand-reputation disadvantages that RTR laws do not address. The policy implication is that piecemeal deregulation may be insufficient; comprehensive approaches that also address training and certification may be necessary to expand independent repair markets.

Three important caveats apply. First, the post-treatment period is short, especially for the later-adopting cohorts. Market entry responds to regulation with lags: entrepreneurs must learn about the new legal environment, secure inputs, and establish customer relationships. The null may reflect adjustment lags rather than permanent irrelevance. Second, NAICS 8112 captures a broad category of electronic repair; effects on consumer-device repair specifically may be diluted by unaffected subcategories like precision instrument repair. Third, enforcement varies across states and remains in its early stages. If manufacturers have not yet been compelled to comply—or if compliance is pro forma—the null may reflect weak implementation rather than irrelevance of the legal barrier. Future work should revisit these outcomes as enforcement matures and more post-treatment data accumulate.

7. Conclusion

Right-to-repair laws represent one of the most consequential regulatory experiments in consumer technology markets. This paper provides the first causal evidence on their economic effects, finding that mandated access to repair inputs has not expanded the independent repair sector. The null on establishment entry and employment, combined with a fragile wage effect, suggests that formal barriers to parts and documentation were not the binding constraint on repair market competition. As more states consider RTR legislation, these early results indicate that the debate's loudest participants—both industry opponents and consumer advocates—may have overestimated what input-access mandates alone can achieve.

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

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A. Data Appendix

Data source. The Quarterly Census of Employment and Wages (QCEW) is produced by the Bureau of Labor Statistics from administrative records of employers covered by state unemployment insurance laws and federal employees covered by the Unemployment Compensation for Federal Employees program. The program covers approximately 95% of U.S. jobs. Data are available at quarterly frequency by state, industry (NAICS), and ownership type. I access the data through the BLS QCEW API (https://data.bls.gov/cew/data/api/).

Sample construction. I extract all state-level (aggregation level code 56), private-sector (ownership code 5) records for NAICS 8112 (Electronic and Precision Equipment Repair and Maintenance) and NAICS 8111 (Automotive Repair and Maintenance) for the period 2019Q1 through 2025Q2. This yields 1,378 state-quarter observations per NAICS code across 53 state-level areas. No observations are suppressed for disclosure.

Variable construction. Employment is measured as the average of three monthly employment levels reported in each quarter. All continuous outcomes are analyzed in logs (using $\ln(x + 1)$ to accommodate potential zeros, though no zeros exist in the state-level data). The treatment indicator equals one for state-quarters at or after the effective date of that state's RTR law.

Treatment coding. States are coded as treated beginning in the quarter their RTR law takes effect: New York in 2023Q3 (Digital Fair Repair Act, effective July 1, 2023); California (SB 244) and Minnesota (SF 2744) in 2024Q3 (both effective July 1, 2024); Oregon (SB 1596) and Colorado (HB 24-1121) in 2025Q1 (both effective January 1, 2025). All remaining states are coded as never-treated ($G_i = 0$ in the Callaway–Sant'Anna framework).

B. Identification Appendix

The event-study estimates for employment show pre-treatment coefficients at all horizons from $e = -8$ to $e = -2$ within 0.01 of zero, with no systematic trend. For establishments, pre-treatment coefficients at longer horizons ($e = -8$: 0.072, $e = -6$: 0.059) are modestly positive, though individually insignificant. This pattern suggests that RTR-adopting states may have had slightly faster establishment growth trajectories prior to adoption. Because the employment results—the more standard measure of sectoral scale—do not exhibit this pre-trend concern, I interpret the establishment null with appropriate caution.

C. Robustness Appendix

Wild cluster bootstrap p-values for the TWFE specification are 0.100 (establishments), 0.725 (employment), and 0.274 (wages). These are uniformly larger than the asymptotic p-values, consistent with over-rejection in the standard cluster-robust framework with five treated states.

Level (non-log) specifications yield ATTs of -2.2 establishments (SE = 14.0) and -22.9 employees (SE = 77.0), confirming the null in untransformed units.

D. Heterogeneity Appendix

I split states into those with above-median and below-median pre-treatment electronic repair employment (median: 1,299 workers). The wage ATT is 0.015 (SE = 0.012) in large-sector states and 0.007 (SE = 0.014) in small-sector states. Neither subgroup estimate is individually significant, and the difference between them is not statistically distinguishable from zero.

E. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Log Establishments	CS-DiD ATT	0.0104	1.0392	0.0100	0.0166	Small positive
Log Employment	CS-DiD ATT	0.0070	1.5174	0.0046	0.0100	Null
Log Avg. Weekly Wage	CS-DiD ATT	0.0236	0.8856	0.0266	0.0127	Small positive
Panel B: Heterogeneous (Wages by Pre-Treatment Sector Size)						
Wage, Large Sector	CS-DiD ATT	0.0149	0.8856	0.0168	0.0136	Small positive
Wage, Small Sector	CS-DiD ATT	0.0071	0.8856	0.0080	0.0159	Small positive

Notes: **Country:** United States. **Research question:** Do state-level electronics right-to-repair (RTR) laws, which mandate manufacturer provision of diagnostic tools and parts to independent repair shops, affect repair-sector establishment counts, employment, and wages? **Policy mechanism:** RTR laws require original equipment manufacturers to make diagnostic and repair information, tools, firmware, and parts available to independent repair providers and consumers on fair and reasonable terms, thereby reducing barriers to independent repair market entry. **Outcome definition:** Log quarterly establishments (NAICS 8112), log average monthly employment (NAICS 8112), and log average weekly wages (NAICS 8112) from BLS Quarterly Census of Employment and Wages. **Treatment:** Binary; state has an effective electronics RTR law. Five treated states with staggered adoption: NY (2023Q3), CA and MN (2024Q3), OR and CO (2025Q1). **Data:** BLS QCEW, 2019Q1–2025Q2, state-quarter level, private sector NAICS 8112; N = 1,378 state-quarter observations across 53 states/territories. **Method:** Staggered difference-in-differences using Callaway and Sant'Anna (2021) with never-treated states as controls; state-clustered standard errors. **Sample:** All 50 states plus DC and territories with non-suppressed QCEW data for NAICS 8112 (Electronic and Precision Equipment Repair and Maintenance); pre-treatment period 2019Q1–2023Q2. 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).