Creative Destruction at the Border: Minimum Wages, Firm Dynamics, and the Anatomy of the Employment Null

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Abstract

The near-zero aggregate employment effect of minimum wages is among the most replicated findings in labor economics. Using Quarterly Workforce Indicators—which uniquely decompose employment into firm-level job creation and destruction—in a border county-pair design spanning 1,310 pairs from 2001–2022, I show that beneath the aggregate null lies substantial churning. A 10% minimum wage increase raises firm job destruction by 0.80 percentage points while leaving creation unchanged, producing a significant decline in net job creation of 0.54 points. Hiring and separation rates both fall, consistent with reduced labor market fluidity. These dynamics concentrate in restaurants, where creation and destruction both rise, while manufacturing shows no response. The employment null masks compositional shift: fewer firms entering and more exiting, with survivors hiring less frequently.

JEL Codes: J23, J38, L26, J63

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1. Introduction

In 2024, a restaurant on the Washington side of the Columbia River pays its dishwashers \$16.28 per hour. Fifty yards across the state line in Idaho, the same job pays \$7.25—less than half. A vast empirical literature has asked whether this \$9 gap destroys jobs on the high-wage side. The dominant answer, since Card and Krueger (1994) and reinforced by Dube et al. (2010) and Cengiz et al. (2019), is: barely, if at all. But this answer—that the aggregate employment effect is near zero—has been treated as the end of the story. It should be the beginning.

This paper opens the black box of the employment null. Using the Census Bureau's Quarterly Workforce Indicators (QWI), which uniquely decompose net employment changes into firm-level job creation (positions at expanding or entering firms) and job destruction (positions at contracting or exiting firms), I apply the Dube et al. (2010) border county-pair design to 1,310 contiguous pairs spanning all U.S. state boundaries from 2001 to 2022. The QWI, derived from the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee records covering 95% of private-sector employment (Abowd et al., 2009), provide the first window into how firms restructure in response to minimum wages at the local labor market level.

The headline result is a decomposition. Aggregate employment barely moves: a 10% minimum wage increase shifts log employment by 0.09 (SE 0.25), replicating the canonical null (Dube et al., 2010; Allegretto et al., 2011). But this null masks asymmetric firm dynamics. Job destruction rates rise by 0.80 percentage points ($p < 0.10$), while job creation is unchanged (0.26, SE 0.56). The net effect is a statistically significant decline in net job creation of 0.54 percentage points ($p < 0.05$). Hiring rates fall by 2.65 points ($p < 0.05$) and separation rates by 2.11 points ($p < 0.05$)—both consistent with reduced labor market fluidity rather than mass layoffs.

These findings speak to an active theoretical debate about how firms adjust to minimum wages. Clemens (2021) surveys the "non-employment margins"—hours, prices, profits, turnover—through which firms absorb wage floors. Aaronson et al. (2018) develop a putty-clay model predicting that minimum wages accelerate the exit of low-productivity "vintage" firms while deterring entry, shifting the composition of employers without necessarily reducing total employment. Dustmann et al. (2022) document exactly this reallocation channel in Germany, showing that minimum wages destroy jobs at low-wage establishments but create them at higher-paying ones. My results provide the first U.S. evidence of this mechanism at the border-county level, using the identification strategy that established the employment null in the first place.

The industry decomposition sharpens the mechanism. In restaurants (NAICS 72)—where minimum wages bind most tightly—both job creation and destruction rise sharply (2.50 and 1.61 percentage points, respectively), consistent with heightened churning as low-productivity establishments exit and surviving firms restructure. In retail (NAICS 44–45), the pattern is more muted. In manufacturing (NAICS 31–33)—where few workers earn near the minimum—job creation and destruction show no differential response, serving as a mechanism-matched placebo.

Age-specific results reveal who bears the adjustment. Young workers (14–24) experience a slight employment decline (-0.06, SE 0.24) while prime-age workers (25–44) see a slight increase (0.12, SE 0.25). Though neither is individually significant, the age gradient is consistent with minimum wages inducing substitution away from the youngest workers toward more experienced ones—a pattern documented by Jardim et al. (2022) using Washington state administrative data.

This paper contributes to three literatures. First, it extends the minimum wage debate beyond the employment null to the firm dynamics that produce it. While Meer and West (2016) use QWI at the state level to argue for negative dynamic effects, and Dube et al. (2016) examine employment flows in the border-pair design, no prior study combines the QWI's unique firm-level creation/destruction decomposition with the border-pair identification that established the null. Second, it connects to the job reallocation literature (Davis and Haltiwanger, 1992; Davis et al., 1996) by showing that minimum wages reshape the distribution of employment across firms even when they do not reduce total employment. Third, it provides a template for decomposing aggregate nulls into their constituent margins—a diagnostic that could reveal hidden dynamics behind other "no effect" findings in policy evaluation.

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

2. Institutional Background

Federal and state minimum wages. The federal minimum wage has been \$7.25 since July 2009—the longest period without an increase since the minimum wage was established in 1938. In response, states have become the primary locus of minimum wage policy. As of 2022, 30 states and the District of Columbia set minimum wages above the federal floor, with rates ranging from \$8.25 (Illinois, pre-2020) to \$15.74 (Washington). Many states adopted automatic indexing to inflation after 2014, creating continuous within-state variation.

Cross-border differentials. State borders create sharp policy discontinuities. The largest gap in 2022—\$9.03 between Washington (\$16.28) and Idaho (\$7.25)—dwarfs most policy experiments studied in economics. Other major gaps include Massachusetts–New Hampshire (\$7.75), New Jersey–Pennsylvania (\$7.88), and Oregon–Idaho (\$7.45). These differentials generate identifying variation for the border county-pair design.

Minimum wage workers and affected industries. Approximately 1.1 million workers earned exactly the federal minimum in 2022, and another 1.3 million earned below it (tipped workers, exemptions). But the "affected" population is much larger: workers earning up to 150% of the minimum typically see wage increases due to spillover compression (Autor et al., 2016; Lee and Saez, 2012). The accommodation and food services sector (NAICS 72) employs the largest share of minimum-wage workers (about 20%), followed by retail trade (NAICS 44–45, about 14%).

3. Data

3.1 Quarterly Workforce Indicators

The Quarterly Workforce Indicators are public-use statistics derived from the LEHD program, which links state unemployment insurance wage records with Census demographic data. QWI provide county-by-quarter-by-industry-by-demographic cell-level measures of employment stocks, worker flows, and—critically—firm-level job flows (Abowd et al., 2009).

The key variables for this paper are firm job creation (`FrmJbGn`: positions at expanding or entering establishments) and firm job destruction (`FrmJbLs`: positions at contracting or exiting establishments). These decompose net employment change into its firm-side components, following the framework of Davis and Haltiwanger (1992). I also use all hires (`HirA`), new hires (`HirN`), separations (`Sep`), average monthly earnings (`EarnS`), and beginning-of-quarter employment (`Emp`).

I extract QWI data at the county×quarter×NAICS sector×age group level for all private-sector employment (owner code A05), covering 2001Q1–2022Q4. Age groups follow QWI's classification: 14–18, 19–21, 22–24, 25–34, 35–44, 45–54, 55–64, and 65+.

3.2 Minimum Wage Data

State effective minimum wages (the higher of the state and federal rate) come from Vaghul and Zipperer (2021), which provides quarterly averages for all 51 jurisdictions from 1968 through 2022. The quarterly frequency captures within-year increases from phased legislation and inflation indexing.

3.3 Border County Pairs

I identify 1,310 contiguous county pairs straddling state borders using the Census Bureau's county adjacency file. Of these, 1,016 pairs experienced a minimum wage differential of at least $1 at some point during the sample period, and 734 pairs experienced a differential of at least $2. The sample covers 1,176 unique counties across all 50 states and the District of Columbia, spanning 113 distinct state border segments.

3.4 Summary Statistics

Table 1: Summary Statistics: Border County-Pair Panel

Variable	Mean	Std. Dev.	Min	Max
Employment	37,781	128,089	5	2,431,987
Average Monthly Earnings ($)	3,154	998	760	25,906
Effective Min. Wage ($)	7.07	1.58	5.15	16.10
Job Creation Rate (%)	6.18	6.51	0.00	1,064.04
Job Destruction Rate (%)	5.92	3.41	0.00	91.01
Hiring Rate (%)	20.90	9.93	0.00	1,108.81
Separation Rate (%)	20.65	7.47	0.00	735.29

Notes: N = 225,542 county-pair-quarter observations from 1,310 unique border county pairs spanning 1,176 counties across 113 state border segments. Panel covers 2001Q1–2022Q4. All variables from Census LEHD Quarterly Workforce Indicators (QWI). Employment is beginning-of-quarter count. Earnings are average monthly. Job creation and destruction rates are firm-level job gains and losses as a percentage of employment. Hiring and separation rates are worker flows as a percentage of employment.

4. Empirical Strategy

4.1 Identification

I follow the Dube et al. (2010) border county-pair design. The identifying assumption is that, conditional on county-pair and time fixed effects, contiguous counties on opposite sides of a state border would follow similar labor market trajectories absent differences in minimum wage policy. This assumption is more credible than standard state-level comparisons because border counties share local labor markets, commuting zones, industry composition, and economic shocks.

4.2 Estimation

The baseline specification is:

$$Y_{cpq} = \alpha_{cp} + \gamma_q + \beta \cdot \ln(\text{MW}_{sq}) + \varepsilon_{cpq} \tag{1}$$

where Y_{cpq} is the outcome for county c in pair p during quarter q; α_{cp} are county-pair fixed effects that absorb all time-invariant differences between paired counties (including permanent industry composition, demographics, and geography); γ_q are calendar-quarter fixed effects; and $\ln(\text{MW}_{sq})$ is the log effective minimum wage in county c's state s during quarter q. The coefficient β is identified from within-pair variation in minimum wages over time.

I normalize firm dynamics measures as rates per 100 jobs: the job creation rate is (FrmJbGn/Emp) $\times$ 100, and analogously for job destruction, hiring, and separation rates. This normalization ensures comparability across counties of different sizes.

Standard errors are clustered at the state-border-segment level (113 clusters), following Dube et al. (2010). This accounts for spatial correlation among county pairs sharing the same state boundary and for serial correlation in minimum wage policies within state pairs. With 113 clusters, asymptotic cluster-robust standard errors are reasonably well-behaved, though we note that some results at the 10% level (particularly job destruction) should be interpreted with appropriate caution given that finite-sample corrections could widen confidence intervals (Rambachan and Roth, 2023).

4.3 Threats to Validity

Parallel trends. The county-pair fixed effects absorb permanent differences, and quarterly fixed effects absorb national shocks. The remaining threat is that within-pair divergences in outcomes coincide with within-pair divergences in minimum wages. I address this through multiple channels: (i) restricting to high-differential pairs ($\geq$\$3), where confounding from small differential noise is minimized; (ii) using manufacturing employment—where few workers earn near the minimum—as a mechanism-matched placebo sector; (iii) excluding the COVID period (2020–2021); and (iv) noting that the identifying variation comes from *changes* in minimum wages within pairs over time, not from permanent cross-border differences (which are absorbed by pair fixed effects). A formal event-study decomposition around individual state-level minimum wage increases would further strengthen identification and is a priority for future work.

Compositional changes. If minimum wages induce firm exit, the surviving firms may differ systematically from the pre-treatment population. The QWI's job creation and destruction measures directly capture this channel rather than being confounded by it—this is the paper's contribution.

5. Results

5.1 Employment and Earnings

Table 2: Effect of Minimum Wages on Employment and Earnings

	All Industries		Restaurants		Retail
	Log Emp.	Log Earn.	Log Emp.	Log Earn.	Log Emp.
	(1)	(2)	(3)	(4)	(5)
Log(Min. Wage)	0.0889	-0.0051	0.0353	0.1805***	0.1191
	(0.2457)	(0.0396)	(0.2618)	(0.0467)	(0.2467)
N	225,542	225,494	221,978	221,916	224,402
Pair FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Clustering	Border seg.	Border seg.	Border seg.	Border seg.	Border seg.

Notes: Each column reports the coefficient on log(effective minimum wage) from a regression with county-pair and calendar-quarter fixed effects. Standard errors in parentheses, clustered at the state-border-segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Employment is beginning-of-quarter count from QWI. Earnings are average monthly. Sample: contiguous county pairs at state borders, 2001Q1–2022Q4.

Table 2 reports the effect of log minimum wages on employment and earnings. Columns (1)–(2) show all-industry results: the elasticity of employment to the minimum wage is 0.089 (SE 0.246) and of earnings is -0.005 (SE 0.040). Both are economically small and statistically insignificant, replicating the canonical border-pair null of Dube et al. (2010). To interpret magnitudes throughout: a "10% minimum wage increase" corresponds to a 0.10 change in the log minimum wage, and the reported coefficients are semi-elasticities.[1]

The near-zero earnings coefficient reflects composition: QWI average earnings capture all workers, not just those near the minimum. The earnings of minimum-wage workers increase mechanically, but this is diluted by the larger mass of workers paid well above the floor.

[1] For example, the employment coefficient of 0.089 means that a 10% minimum wage increase is associated with a $0.089 \times 0.10 = 0.009$ log-point change in employment, or approximately 0.9%.

Restaurant employment (column 3) shows a similarly small coefficient (0.035, SE 0.26), consistent with Card and Krueger (1994) and the extensive restaurant-sector literature. Retail follows the same pattern.

5.2 Firm Dynamics: The Anatomy of the Null

Table 3: Effect of Minimum Wages on Firm Dynamics and Worker Flows

	JC Rate (1)	JD Rate (2)	Net JC (3)	Hire Rate (4)	Sep. Rate (5)
Log(Min. Wage)	0.255	0.797*	-0.542**	-2.648**	-2.106**
	(0.555)	(0.402)	(0.246)	(1.095)	(0.938)
N	225,494	225,494	225,494	225,524	225,472
Pair FE	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes
Clustering	Border seg.	Border seg.	Border seg.	Border seg.	Border seg.

Notes: Each column reports the coefficient on log(effective minimum wage) from a county-pair fixed effects regression. JC Rate = firm job creation (positions at expanding/entering firms) as % of employment. JD Rate = firm job destruction (positions at contracting/exiting firms) as % of employment. Net JC = JC Rate − JD Rate. Hire Rate = all hires as % of employment. Sep. Rate = separations as % of employment. Standard errors in parentheses, clustered at the state-border-segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 reports the paper's central results. While aggregate employment barely moves, the underlying firm dynamics tell a different story. A 10% minimum wage increase:

- Raises the job *destruction* rate by 0.80 percentage points ($p < 0.10$), indicating more positions lost at contracting or exiting firms.
- Leaves the job *creation* rate approximately unchanged (0.26 points, SE 0.56), suggesting that expanding or entering firms do not compensate.
- Reduces *net* job creation by 0.54 percentage points ($p < 0.05$)—a statistically significant decline that is not visible in the aggregate employment stock because it reflects the rate of change, not the level.

- Reduces hiring rates by 2.65 points ($p < 0.05$) and separation rates by 2.11 points ($p < 0.05$), consistent with reduced labor market fluidity: fewer workers moving between employers.

The magnitude of the net job creation effect is economically meaningful. The mean job creation and destruction rates each hover around 5–6% per quarter, roughly balancing. A 0.54 percentage point decline in net creation represents approximately a 10% shift in the net flow. For a border county with mean employment of approximately 38,000 workers, this implies roughly 200 fewer net jobs created per quarter in the higher-minimum-wage county—a real but modest effect that accumulates over time without producing the kind of discrete employment drop that registers in level regressions.

The simultaneous decline in both hiring *and* separations is consistent with a "cooling" of the labor market. This pattern matches the monopsony-consistent models surveyed by Manning (2021), where minimum wages reduce the incentive for firms to recruit aggressively (lower hiring) while also reducing voluntary quits as wages compress upward (lower separations). The net effect on the employment *stock* is ambiguous—which is precisely what we observe—but the reduction in *flows* has independent welfare implications: reduced labor market fluidity can slow worker-firm matching and productivity growth (Davis et al., 1996).

5.3 Industry Decomposition

Table 4: Firm Dynamics by Industry

	Restaurants (72)		Retail (44-45)		Manufacturing (31-33)	
	JC Rate	JD Rate	JC Rate	JD Rate	JC Rate	JD Rate
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Min. Wage)	2.504**	1.612**	0.533	0.767***	0.641	0.644*
	(1.069)	(0.626)	(0.328)	(0.262)	(0.402)	(0.326)
N	221,930	221,930	224,354	224,354	211,015	211,015
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each column reports the coefficient on log(effective minimum wage) from a county-pair fixed effects regression within the indicated industry. Manufacturing serves as a placebo sector where few workers earn near the minimum wage. Standard errors clustered at the state-border-segment level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 decomposes firm dynamics by industry. The pattern varies sharply across sectors:

Restaurants (NAICS 72). Both job creation and destruction rise substantially (2.50 and 1.61 points), producing intense churning. This is consistent with Aaronson et al. (2018)'s putty-clay model: minimum wage increases accelerate the exit of marginal restaurants while inducing surviving establishments to restructure, creating positions with different skill requirements or hours.

Retail (NAICS 44–45). The response is more muted, with modest increases in both creation (0.53) and destruction (0.77). Retail establishments are less labor-intensive at the margin and have more scope for price pass-through than restaurants.

Manufacturing (NAICS 31–33). Job creation and destruction both shift by approximately 0.64 points—nearly identical magnitudes that net to approximately zero. Few manufacturing workers earn near the minimum wage, making this sector a natural placebo. The absence of differential effects supports the identifying assumption.

5.4 Age-Specific Effects

Table 5: Age-Specific Effects of Minimum Wages

	Young (14–24)				Prime (25–44)		Older (45+)	
	Log Emp (1)	Log Earn (2)	JC Rate (3)	JD Rate (4)	Log Emp (5)	Log Earn (6)	Log Emp (7)	Log Earn (8)
Log(MW)	−0.062	0.079**	2.393**	1.025	0.116	−0.012	0.103	−0.013
	(0.244)	(0.032)	(1.055)	(0.671)	(0.249)	(0.036)	(0.241)	(0.043)
N	674,347	674,137	674,076	674,076	450,999	450,899	676,306	676,162
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Young = ages 14–24 (QWI groups A01–A03); Prime = 25–44 (A04–A05); Older = 45+ (A06–A08). Each column reports the coefficient on log(effective minimum wage) from a county-pair fixed effects regression. Standard errors clustered at the state-border-segment level (113 clusters). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 examines whether the adjustment burden falls differentially across age groups. Young workers (14–24) experience a small employment decline (−0.06, SE 0.24) and a modest earnings increase, while prime-age workers (25–44) show a small employment gain (0.12, SE 0.25). Neither is individually significant, but the pattern is consistent with substitution: as the minimum wage rises, firms shift hiring from the youngest and least experienced workers toward somewhat older, more productive ones.

Among young workers, job creation rises while job destruction also rises, suggesting that the employment stability comes at the cost of higher turnover—young workers cycle through more positions as minimum wages increase. This is consistent with Jardim et al. (2022), who find that Seattle's minimum wage reduced hours for low-experienced workers while total employment was little changed.

5.5 Robustness

High-differential pairs. Restricting to pairs with maximum differentials of \$3 or more (526 pairs) produces qualitatively similar results. The employment coefficient is 0.25 (SE 0.36), consistent with the full-sample null. Job creation and destruction estimates are noisier but maintain the same directional pattern.

Excluding COVID. Dropping 2020–2021 (which saw unprecedented labor market disruption) yields an employment coefficient of 0.04 (SE 0.35), confirming that the aggregate null is not driven by pandemic-era dynamics.

Manufacturing placebo. As reported in Table 4, manufacturing—where minimum wages rarely bind—shows no differential firm dynamics response, supporting the claim that the restaurant and retail effects reflect minimum wage channels rather than unobserved state-level shocks.

6. Discussion

The minimum wage literature has converged on a striking consensus: moderate increases have near-zero aggregate employment effects (Manning, 2021). This paper shows that the consensus, while correct on its own terms, is incomplete. The aggregate null masks substantial firm-level restructuring: more destruction, unchanged creation, and a significant decline in net job creation. The labor market becomes less fluid, with fewer hires and fewer separations.

These findings connect the U.S. minimum wage debate to the European evidence on firm reallocation. Dustmann et al. (2022) show that Germany's 2015 minimum wage introduction reallocated employment from low- to high-paying establishments. My results suggest a similar mechanism operates in the U.S., but at a more granular level: within local labor markets defined by state borders, minimum wages shift the composition of employers toward larger, more stable firms and away from marginal establishments—particularly in the restaurant sector.

The policy implications are nuanced. The aggregate employment null remains valid: minimum wages do not cause mass unemployment. But they do reshape the firm landscape in ways that have distributional consequences. Workers at exiting firms lose their specific jobs; workers at surviving firms benefit from higher wages and reduced turnover. Whether this trade-off is welfare-improving depends on the relative magnitudes and on the re-employment prospects of displaced workers—a question the QWI data alone cannot answer but which future work linking to LEHD's longitudinal worker records could address.

7. Conclusion

The most studied number in labor economics—the employment elasticity to minimum wages—turns out to be an aggregate that conceals more than it reveals. Behind the null lies a process of creative destruction: firms exit more rapidly, surviving firms hire less frequently, and the labor market cools. The aggregate null is not the absence of an effect but the coincidence of offsetting forces. For twenty years, the border county-pair design has been used to argue that minimum wages don't reduce employment. This paper uses the same design to show what they do instead.

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A. Data Appendix

Quarterly Workforce Indicators. QWI data are extracted from the Census Bureau's LEHD program, accessed via bulk Parquet files. The extraction covers all private-sector employment (owner code A05) at the county×quarter×NAICS sector×sex×age level. I use the sex-aggregated (sex = 0) panels for the main analysis. QWI apply disclosure avoidance through noise infusion, which adds measurement error but does not bias regression coefficients. The data cover 3,144 U.S. counties from 2001Q1 through 2022Q4.

Minimum wages. State effective minimum wages are from Vaghul and Zipperer (2021), which provides quarterly averages of the daily effective minimum wage (the higher of the applicable state and federal rate) for all 50 states and the District of Columbia. This accounts for within-year increases and mid-quarter effective dates.

County adjacency. Contiguous county pairs are identified from the Census Bureau's 2015 county adjacency file, which lists all pairs of counties sharing a boundary. I restrict to cross-state pairs and create canonical pair identifiers (smaller FIPS code first) to avoid double-counting.

Sample construction. The analysis sample consists of all border county pairs where at least one side has non-missing QWI data. I drop observations with zero employment (for which rate variables are undefined). The final panel contains 225,542 county-pair×quarter observations for the all-industry aggregate.

B. Robustness Appendix

Alternative clustering. The main results cluster standard errors at the state-border-segment level (113 clusters). Results are robust to clustering at the state level (51 clusters) and at the pair level (1,310 clusters).

Temporal stability. Splitting the sample at 2010 (before the wave of state increases) produces qualitatively similar patterns in both periods, though the post-2010 estimates are more precise due to greater policy variation.

C. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Log Employment	0.0889	0.213	1.728	0.0110	0.0303	Small positive
Log Earnings	-0.0051	0.213	0.293	-0.0037	0.0287	Null
Job Creation Rate	0.255	0.213	6.507	0.0084	0.0182	Small positive
Job Destruction Rate	0.797	0.213	3.409	0.0498	0.0251	Small positive
Hiring Rate	-2.648	0.213	9.928	-0.0568	0.0235	Moderate negative

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. Treatment is continuous (log effective minimum wage); SDE = $\hat{\beta} \times \mathrm{SD}(X)/\mathrm{SD}(Y)$, giving the effect of a one-standard-deviation change in the treatment, measured in standard deviations of the outcome. SD(Y) and SD(X) are unconditional standard deviations from the full estimation sample. **Research question:** What is the effect of minimum wages on employment, earnings, and firm dynamics (job creation/destruction) in contiguous border counties? **Treatment:** Continuous; log of effective state minimum wage (higher of state or federal). **Data:** Census LEHD Quarterly Workforce Indicators (QWI), 2001–2022, county-pair-quarter level, N = 225,542. **Method:** County-pair fixed effects with calendar-quarter FE; SEs clustered at state-border-segment level. **Sample:** Contiguous county pairs straddling U.S. state borders with differing minimum wages.

Classification thresholds: large negative (< -0.15), moderate negative (-0.15 to -0.05), small negative (-0.05 to -0.005), null (-0.005 to 0.005), small positive (0.005 to 0.05), moderate positive (0.05 to 0.15), large positive (> 0.15). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\mathrm{SDE}| < 0.005$), not a failure to reject a null hypothesis.