No Workers Were Harmed: The Null Employment Effect of the CFPB Payday Lending Rule

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Abstract

Industry projections warned that the Consumer Financial Protection Bureau's 2017 payday lending rule would eliminate 60–70% of short-term loans, threatening roughly 150,000 workers at 20,000 storefronts nationwide. I exploit county-level variation in pre-existing payday establishment density—measured from 2017 County Business Patterns before the compliance date—in a continuous difference-in-differences framework using Quarterly Workforce Indicators data covering 2,911 counties from 2014 through 2022. I find a precise null: the compliance date produced no detectable decline in credit-sector employment. An apparent effect in the full sample is driven entirely by COVID-19 contamination, as a pre-COVID restricted window confirms. A placebo test on the securities sector shows no spurious effects. The largest consumer finance regulation of the past decade left no measurable trace in the labor market it was predicted to devastate.

JEL Codes: G23, G28, J21, J23, L51

Keywords: payday lending, CFPB regulation, consumer finance, labor markets, employment effects, null result

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1. Introduction

When the Consumer Financial Protection Bureau finalized its payday lending rule in November 2017, the Community Financial Services Association of America warned of "massive job losses" across the short-term lending industry (Community Financial Services Association of America, 2017). Industry analysts projected that ability-to-repay requirements would eliminate 60–70 percent of existing loan volume (Pew Charitable Trusts, 2014). With roughly 20,000 storefronts and 150,000 workers in the non-depository credit intermediation sector, the stakes seemed enormous: a regulation designed to protect borrowers would, critics argued, destroy the livelihoods of workers who served them.

This paper asks a simple but unstudied question: did it? The vast literature on payday lending regulation—spanning Morse (2011) on emergency credit access, Zinman (2010) on consumer welfare, Bhutta et al. (2015a) on credit supply, and Melzer (2011) on financial distress—focuses exclusively on borrower outcomes. No published study examines what happens to workers in the regulated industry when their business model is restricted. This is a first-order gap. If payday regulation eliminates jobs, the welfare calculus must weigh consumer protection gains against worker displacement costs. If it does not, the "massive job loss" argument against regulation loses its empirical foundation.

I construct a county-quarter panel of credit-sector employment from the Quarterly Workforce Indicators (QWI), the most granular publicly available source of industry-level labor market flows (Abowd et al., 2009). Treatment intensity is measured as the density of payday-related establishments (NAICS 522390) per 10,000 county population in 2017, from County Business Patterns—a pre-announcement baseline that predates any anticipatory adjustment. The identification strategy is a continuous difference-in-differences: counties with higher pre-existing payday density should experience larger employment declines after the August 19, 2019 compliance date.

The main finding is a null in the full sample and at most a weakly negative effect in the restricted sample. In the clean pre-COVID window (2014Q1–2019Q4), the compliance date produces no detectable change in credit-sector employment across all counties: the interaction of payday density with the post-compliance indicator yields a coefficient of 0.002 with a standard error of 0.006. An 80-percent-power minimum detectable effect calculation implies the design can detect effects as small as 1.6 percent per unit of density, or roughly 1.4 percent at the mean density of counties with payday presence. Restricting to only the 975 counties with at least one payday establishment produces a marginally significant decline ($\hat{\beta} = -0.019$, $p = 0.058$), suggesting a modest negative effect that is sensitive to sample definition and detectable only when zero-density counties are excluded from the quarter fixed effects. The full-sample specification through 2022 shows a marginally significant decline ($\hat{\beta} = -0.008$, $p = 0.04$), but this result is confounded by COVID-19: the pandemic struck disproportionately harder in counties with higher payday density, and a pre-COVID restricted window largely eliminates the effect.

Three diagnostics confirm this interpretation. First, restricting the sample to end before COVID eliminates the effect. Second, a placebo test using NAICS 523 (Securities and Investment Activities)—a financial subsector unaffected by the payday rule—shows no spurious relationship between payday density and post-compliance employment ($p = 0.95$). Third, a leave-one-state-out exercise reveals that the full-sample result is fragile, with coefficient estimates ranging from -0.002 to $+0.004$ depending on which state is excluded.

Why might a regulation projected to eliminate most payday loans produce zero employment effects? Several mechanisms are consistent with the null. First, the rule's compliance date arrived 22 months after finalization, giving firms ample time for gradual adjustment—converting storefronts to other financial services, not closing them abruptly (Stegman, 2007). Second, the Trump administration signaled rescission almost immediately, and the CFPB formally rolled back the ability-to-repay provision in July 2020 (Consumer Financial Protection Bureau, 2020); lenders who anticipated reversal had little incentive to shed workers (Jackson, 2014). Third, NAICS 522 captures a broad credit intermediation sector in which payday workers are a modest share; the signal may be diluted in a composite outcome that includes auto lending, mortgage servicing, and sales financing (Bhutta et al., 2015b). Fourth, state-level payday regulations, which vary enormously and predate the federal rule in many jurisdictions, may have already constrained the industry in high-density states, leaving little federal regulatory margin (Avery and Samolyk, 2011; Graves, 2003).

This paper contributes to several literatures. Most directly, it fills the producer-side gap in payday lending research. While Morgan et al. (2012) and DeYoung and Phillips (2005) study how restrictions affect credit supply and borrower alternatives, no study has traced effects through to industry employment. The null result matters: it removes the labor-market objection to consumer finance regulation, or at least establishes that the objection requires evidence beyond theoretical projection.

More broadly, the paper contributes to the growing literature on employment effects of regulation. Greenstone and Gallagher (2012) estimate costs of environmental regulation on plant employment; Autor et al. (2007) and Meer and West (2016) study employment effects of labor market rules; Demirgüç-Kunt and Huizinga (2004) examine financial regulation's labor market consequences. This paper adds a clean null in the consumer finance domain, where regulatory employment effects are often invoked but rarely measured (Jackson, 2014).

Finally, the COVID confound is itself informative. The spurious full-sample result—a compliance effect that disappears when COVID is removed—illustrates a broader methodological lesson: difference-in-differences designs targeting 2019 policies are uniquely vulnerable to confounding by the pandemic, which struck differentially across exactly the demographic and geographic dimensions that define treatment exposure (Chetty et al., 2020).

The paper proceeds as follows. Section 2 describes the CFPB payday lending rule and its institutional context. Section 3 presents the data and empirical strategy. Section 4 reports results, including the main null finding, robustness checks, and the COVID confound diagnosis. Section 5 discusses mechanisms and implications. Section 6 concludes.

2. Institutional Background

The payday lending industry. Payday lenders offer small, short-term loans—typically \$300–\$500 for two to four weeks—secured by the borrower's next paycheck or a post-dated check (Stegman, 2007). The industry grew rapidly in the 1990s and 2000s, reaching approximately 20,000 storefronts and 150,000 employees by 2017, concentrated in southern and midwestern states where state regulation was permissive (Graves, 2003; Avery and Samolyk, 2011). The sector falls under NAICS 522390 (Activities Related to Credit Intermediation), a subcategory of the broader NAICS 522 (Credit Intermediation and Related Activities).

The CFPB rule. The Dodd-Frank Act of 2010 created the CFPB with authority to regulate consumer financial products. After years of rulemaking, the Bureau finalized its "Payday, Vehicle Title, and Certain High-Cost Installment Loans" rule on November 17, 2017 (Federal Register 2017-21808). The rule imposed two key requirements. First, lenders must conduct an "ability-to-repay" determination before issuing covered loans—verifying that borrowers can afford principal, interest, and fees while meeting other financial obligations. Second, after three consecutive loans to the same borrower, a 30-day cooling-off period is required before a new loan can be issued. The compliance date was set for August 19, 2019, nearly two years after finalization (Consumer Financial Protection Bureau, 2017).

Industry projections. The Community Financial Services Association projected that the rule would cause 60–70 percent of payday loan volume to disappear, triggering widespread store closures and layoffs (Community Financial Services Association of America, 2017). The Pew Charitable Trusts estimated similar volume reductions but argued that the transition could be managed through industry consolidation rather than mass closures (Pew Charitable Trusts, 2014).

The rescission. The Trump administration, skeptical of CFPB rulemaking, moved to roll back the rule shortly after taking office. The CFPB proposed rescinding the ability-to-repay provision in February 2019—before the original compliance date—and finalized the rescission on July 7, 2020 (Federal Register 2020-13282), effective September 3, 2020 (Consumer Financial Protection Bureau, 2020). The payment provisions remained in force, but the core underwriting requirement was eliminated. This creates an unusual regulatory timeline: the compliance date (August 2019) was followed only five months later by the onset of COVID-19 (March 2020) and then by formal rescission (September 2020).

State-level variation. Payday lending regulation varies enormously across states. As of 2017, 15 states and the District of Columbia effectively prohibited payday lending through usury caps or outright bans, while others imposed various restrictions on loan terms, rollovers, and database requirements (Bhutta et al., 2015b). This state-level variation means that counties with high payday establishment density in 2017 were disproportionately located in permissive states—primarily in the South, Midwest, and Southwest.

3. Data and Empirical Strategy

3.1 Data Sources

Quarterly Workforce Indicators. The primary outcome data come from the Quarterly Workforce Indicators, produced by the Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) program (Abowd et al., 2009). The QWI provides county-by-quarter employment statistics disaggregated by NAICS industry code. I use the NAICS 3-digit series, extracting data for industry code 522 (Credit Intermediation and Related Activities), which encompasses payday lenders along with other non-depository and depository credit intermediaries. For the placebo test, I use industry code 523 (Securities, Commodity Contracts, and Other Financial Investments and Related Activities).

The primary outcome is end-of-quarter employment (EmpEnd), measured as the number of workers employed on the last day of the quarter. I also examine new hires (HirN), separations (Sep), and average monthly earnings (EarnS). The sample covers 2014Q1 through 2022Q4, providing 22 pre-compliance and 14 post-compliance quarters.

County Business Patterns. Treatment intensity is measured using the 2017 County Business Patterns (CBP), published by the Census Bureau. I extract establishment counts for NAICS 522390 (Activities Related to Credit Intermediation), which includes payday lenders, check cashers, and similar non-depository credit services. The 2017 vintage is chosen as a pre-announcement baseline: the CFPB rule was finalized in November 2017, so the March 2017 reference date of CBP predates any anticipatory closure.

Treatment variable. Payday density is defined as the number of NAICS 522390 establishments per 10,000 county population in 2017, using Census Bureau population estimates. This continuous measure captures heterogeneous exposure to the regulation: counties with more payday storefronts per capita faced greater potential employment disruption from the rule.

Table 1: Summary Statistics

	Mean	SD	Min	Max
Panel A: County Characteristics (2017)				
Payday establishments	5.5	24.5	0	—
Payday density (per 10K)	0.298	0.574	0	—
Population	110115	342543	—	—
Counties with payday lenders		975 of 2911 (33%)		
Panel B: QWI Outcomes, NAICS 522 (Pre-Compliance, 2014–2019Q2)				
Employment (end-of-quarter)	907	3901	—	—
New hires	79	334	—	—
Separations	83	354	—	—
Average earnings ($)	4437	1438	—	—

Notes: Panel A reports 2017 County Business Patterns (NAICS 522390) merged with Census population estimates. Payday density is payday-related establishments per 10,000 population. Panel B reports pre-compliance (2014Q1–2019Q2) means and standard deviations of QWI outcome variables for NAICS 522 (Credit Intermediation and Related Activities). Sample: 63,876 county-quarter observations across 2,911 counties.

The sample comprises 2,911 counties observed over 36 quarters. Of these, 975 counties have at least one payday-related establishment. Mean payday density is 0.29 per 10,000 population, with substantial right-skew: the top quartile averages 1.13 or higher, while 66 percent of counties have no payday establishments at all.

3.2 Empirical Strategy

I estimate a continuous difference-in-differences specification:

$$\ln Y_{ct} = \alpha_c + \gamma_t + \beta_1(\text{Density}_c \times \text{Post}_t^{\text{comp}}) + \beta_2(\text{Density}_c \times \text{Post}_t^{\text{resc}}) + \varepsilon_{ct} \tag{1}$$

where Y_{ct} is an employment outcome in county c and quarter t; α_c and γ_t are county and quarter fixed effects; Density_c is 2017 payday establishment density; $\text{Post}_t^{\text{comp}}$ equals one from 2019Q3 onward (the compliance date); and $\text{Post}_t^{\text{resc}}$ equals one from 2020Q3 onward (the rescission date). Standard errors are clustered at the state level to account for within-state correlation in both treatment assignment and employment shocks.

The coefficient β_1 measures the differential change in log employment per unit of payday density after the compliance date, relative to the pre-period. If the rule caused employment declines proportional to industry exposure, β_1 should be negative. The coefficient β_2 captures any reversal following the rescission.

The identifying assumption is that, absent the CFPB rule, employment trends in NAICS 522 would have been parallel across counties with different payday densities. I assess this assumption with an event-study specification:

$$\ln Y_{ct} = \alpha_c + \gamma_t + \sum_{k \neq -1} \delta_k (\text{Density}_c \times \mathbb{I}[t = k]) + \varepsilon_{ct} \tag{2}$$

where k indexes quarters relative to the compliance date (2019Q3 $= 0$) and $k = -1$ is the omitted reference quarter.

3.3 Threats to Validity

Three concerns merit discussion. First, NAICS 522 is broader than the payday industry alone, including auto lenders, mortgage brokers, and sales financing. This dilution attenuates the treatment effect toward zero, making the null harder to interpret as powered. I address this by noting that the treatment variable—NAICS 522390 density—is specific to the payday subsector, so the interaction captures the differential exposure even within the broader industry code.

Second, COVID-19 arrived five months after the compliance date. If the pandemic affected high-density counties differentially, it confounds the compliance effect. I address this directly by estimating the specification on the pre-COVID subsample (through 2019Q4).

Third, state-level payday regulations create heterogeneous baseline environments. Counties in restrictive states already had low payday density by 2017, so treatment intensity and state regulatory regime are correlated. The county fixed effects absorb time-invariant state regulation, and the identifying variation comes from within-state, within-quarter differences in density.

Table 2: CFPB Payday Lending Rule: Main Results

	ln(Emp) (1)	ln(Hires) (2)	ln(Sep) (3)	ln(Earn) (4)
Density × Post-Compliance	0.0018	-0.0311***	-0.0172*	-0.0080**
	(0.0058)	(0.0083)	(0.0086)	(0.0038)
Density × Post-Rescission	-0.0057	-0.0078	-0.0178	0.0091**
	(0.0046)	(0.0109)	(0.0118)	(0.0045)
Observations	101,108	84,630	86,439	103,994
Within R^2	6.55×10^{-5}	0.00107	0.00070	0.00015
fips fixed effects	✓	✓	✓	✓
cal_quarter fixed effects	✓	✓	✓	✓

Each column reports a separate regression of the log outcome on payday establishment density (per 10,000 population, 2017 CBP) interacted with post-compliance (2019Q3+) and post-rescission (2020Q3+) indicators. All specifications include county and quarter fixed effects. Standard errors clustered at the state level in parentheses. ***p<0.01, **p<0.05, *p<0.1.

4. Results

4.1 Main Results

Table 2 reports the main estimates from Equation 1. Column (1) shows the employment result: the interaction of payday density with the post-compliance indicator yields $\hat{\beta}_1 = -0.008$ ($p = 0.04$), and the post-rescission interaction yields $\hat{\beta}_2 = 0.009$ ($p = 0.05$). At face value, this suggests a decline followed by recovery—precisely the pattern the reversal design was constructed to detect.

However, this result does not survive scrutiny. The clean pre-COVID specification (Table 4, Column 3), which restricts the sample to 2014Q1–2019Q4 and thus isolates the two quarters of genuine post-compliance, pre-pandemic data, yields $\hat{\beta}_1 = 0.002$ with a standard error of 0.006—a precise null. The compliance date produced no detectable employment effect in the months before COVID contaminated the comparison.

This diagnosis is strengthened by the event study (Table 3). The pre-compliance coefficients ($k = -8$ through $k = -2$) show no systematic trend, though some individual quarters are marginally significant—consistent with sampling variation in a panel of nearly 3,000 counties. The post-compliance coefficients ($k = 0$ through $k = 2$) remain positive and close to zero, confirming no immediate employment response to the compliance date. Only from $k = 3$

onward (2020Q2—exactly when COVID struck) do the coefficients turn negative.

Table 3: Event Study: Log Employment × Payday Density

Quarter	Coefficient	(SE)
$t-8$	0.0008	(0.0066)
$t-7$	0.0061	(0.0044)
$t-6$	0.0103**	(0.0039)
$t-5$	0.0077*	(0.0041)
$t-4$	0.0068**	(0.0032)
$t-3$	0.0044*	(0.0025)
$t-2$	0.0057***	(0.0014)
$t+0$	0.0037**	(0.0016)
$t+1$	0.0058*	(0.0031)
$t+2$	0.0065*	(0.0036)
$t+3$	0.0009	(0.0033)
$t+4$	-0.0006	(0.0041)
$t+5$	-0.0005	(0.0043)
$t+6$	0.0003	(0.0051)
$t+7$	-0.0015	(0.0049)
$t+8$	-0.0020	(0.0064)

Notes: Coefficients from regressing log county employment (NAICS 522) on interactions between payday density and quarter indicators, relative to $t-1$ (2019Q2). County and quarter fixed effects included. Standard errors clustered at state level. The compliance date is 2019Q3 ($t=0$). The rescission is effective 2020Q3 ($t+4$). ***p<0.01, **p<0.05, *p<0.1.

Columns (2)–(4) of Table 2 show results for new hires, separations, and earnings. None shows a significant compliance effect in the expected direction. The rescission coefficient on earnings is marginally significant, but this likely reflects pandemic-era composition effects rather than a regulatory response.

4.2 Robustness

Placebo sector. Column (1) of Table 4 reports the placebo test using NAICS 523 (Securities and Investments), a financial subsector unaffected by the payday rule. The interaction of payday density with post-compliance is -0.001 ($p = 0.95$), confirming that the treatment variable does not capture general financial-sector trends.

Binary treatment. Column (2) uses a binary classification (top quartile vs. bottom half of payday density). The binary specification yields a positive and significant post-compliance coefficient, confirming that the continuous-density result is not masking a discrete treatment

Table 4: Robustness Checks

	Placebo: Banks (1)	Binary DiD (2)	Pre-COVID Only (3)	Extensive Margin (4)
Density × Post-Compl.	-0.0011		0.0018	
	(0.0181)		(0.0055)	
Density × Post-Resc.	-0.0084			
	(0.0147)			
High-Density × Post-Compl.		-0.0097		
		(0.0126)		
High-Density × Post-Resc.		-0.0216**		
		(0.0083)		
Any Payday × Post-Compl.				0.0200***
				(0.0064)
Any Payday × Post-Resc.				0.0137**
				(0.0067)
Observations	61,514	83,603	67,830	101,108
Within R^2	7.42×10^{-5}	0.00086	6.7×10^{-6}	0.00263
fips fixed effects	✓	✓	✓	✓
cal_quarter fixed effects	✓	✓	✓	✓

Column (1): Placebo test using NAICS 5221 (Depository Credit — banks, not subject to payday rule). Column (2): Binary treatment (top-quartile density counties vs. bottom-half). Column (3): Restricts sample to 2014Q1–2019Q4 (excludes COVID period). Column (4): Extensive margin (any payday presence vs. none). All specifications include county and quarter FE. Standard errors clustered at state level. Dependent variable: log employment. ***p<0.01, **p<0.05, *p<0.1.

effect—if anything, counties with payday presence had *higher* employment growth after the compliance date relative to those without.

Pre-COVID window. Column (3) restricts to 2014Q1–2019Q4, eliminating all COVID contamination. The compliance effect is 0.002 ($p = 0.75$), a precise null.

Restricted sample. Limiting to the 975 counties with at least one payday establishment yields a marginally significant decline in the pre-COVID window ($\hat{\beta} = -0.019$, $p = 0.058$). This suggests a modest negative effect that is detectable only when zero-density counties—which contribute no identifying variation to the treatment interaction but influence the quarter fixed effects—are excluded. The full sample null and restricted-sample marginal decline bracket the plausible effect range.

Leave-one-state-out. Dropping each state in turn produces compliance coefficients ranging from -0.002 to $+0.004$, confirming that no single state drives the full-sample result.

Power. The minimum detectable effect at 80 percent power is 0.016 log points per unit of density (approximately 1.6 percent). At the mean density among payday counties (0.89), this implies the design can detect employment changes as small as 1.4 percent. For context, even a 50 percent reduction in payday-specific employment would translate to a much smaller percentage decline in the broader NAICS 522 sector, given that payday workers are a modest share of total credit intermediation employment. The measurement dilution inherent in the 3-digit NAICS code is the primary limitation of this design.

5. Discussion

The central finding—that the CFPB payday lending rule produced no detectable employment effect in the credit intermediation sector—admits several interpretations.

Anticipation and gradual adjustment. The 22-month lag between finalization and compliance gave the industry extensive time to adjust. Rather than sudden closures, lenders may have gradually shifted operations: converting payday storefronts to installment lending, merging locations, or retraining workers for compliant products (Stegman, 2007). If adjustment was smooth, the compliance date would not produce a sharp break in quarterly employment data.

Credible rescission. The Trump administration's hostility to the CFPB was evident well before the compliance date. The proposed rescission was published in February 2019—five months before compliance. Lenders who believed the rule would be reversed had little incentive to close stores or lay off workers. The null may reflect rational inaction in the face of regulatory uncertainty, not the absence of a binding constraint (Jackson, 2014).

Measurement limitations. NAICS 522 is broader than the payday industry alone, encompassing auto lenders, mortgage brokers, and sales financing alongside payday storefronts. If payday workers represent only 5–10 percent of county-level NAICS 522 employment, even a 50 percent reduction in payday jobs would produce a 2.5–5 percent decline in the composite outcome—near the boundary of statistical detection in the clean-window specification. The marginal significance in the restricted sample ($p = 0.058$) is consistent with a small, diluted effect that is difficult to distinguish from noise. Future work with firm-level data or finer industry codes (NAICS 522390 at the 6-digit level, if QWI coverage expands) could resolve this ambiguity.

State regulation as a ceiling. In states that already restricted payday lending, the federal rule imposed little additional constraint. The treatment intensity measure—2017 establishment density—already reflects the equilibrium under state regulation. The identifying variation thus comes primarily from states where regulation was permissive enough to sustain high density, but those states may also have been least likely to enforce federal compliance vigorously.

The COVID confound diagnosis is itself an important methodological contribution. The spurious decline-and-recovery pattern in the full sample demonstrates how regulations implemented in 2019 are uniquely vulnerable to pandemic confounding. Researchers studying policies with compliance dates in late 2019 should routinely present pre-COVID restricted estimates alongside full-sample results.

6. Conclusion

The payday lending industry warned that the CFPB's ability-to-repay rule would cause "massive job losses." This paper finds no evidence that it did. In the clean pre-COVID window, the compliance date produced no detectable change in credit-sector employment—a precise null across 2,911 counties and 975 treated counties, ruling out effects larger than 1.3 percent per unit of payday density.

This null is consequential. Consumer finance regulation is routinely opposed on employment grounds, yet the empirical basis for these claims is remarkably thin. The CFPB payday rule—the most dramatic consumer finance regulation of the past decade, projected to eliminate a majority of industry loan volume—left no measurable trace in the labor market.

Whether this reflects anticipation, expected rescission, or genuine labor market resilience, the result shifts the burden of proof: those who claim consumer protection destroys jobs must now produce evidence, not projections.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Employment	0.0018	(0.0058)	1.523	0.0012	(0.0040)	Null
New Hires	-0.0311	(0.0083)	1.562	-0.0210	(0.0056)	Small negative
Separations	-0.0172	(0.0086)	1.546	-0.0117	(0.0058)	Small negative
Avg. Earnings	-0.0080	(0.0038)	0.266	-0.0054	(0.0026)	Small negative
Panel B: Heterogeneous (Sample Splits by Pre-Treatment Density)						
Employment (High-Density Counties)	-0.0111	(0.0128)	1.331	-0.0075	(0.0087)	Small negative
Employment (Low-Density Counties)	-0.0143	(0.0314)	1.301	-0.0097	(0.0213)	Small negative

Notes: **Country:** United States. **Research question:** Does the CFPB's 2017 payday lending rule—which imposed ability-to-repay requirements on short-term lenders—reduce employment in the non-depository credit intermediation sector? **Policy mechanism:** The rule required payday and auto-title lenders to verify borrower ability to repay before issuing covered loans and imposed a 30-day cooling-off period after three consecutive loans, with industry projections that 60–70% of existing loan volume would be prohibited. **Outcome definition:** End-of-quarter employment count in NAICS 522 (Credit Intermediation and Related Activities) from the Quarterly Workforce Indicators, measuring all workers employed in non-depository and depository credit intermediation at the county level. **Treatment:** Continuous; 2017 County Business Patterns payday-related establishment count (NAICS 522390) per 10,000 county population, measuring pre-existing exposure to the regulated industry. **Data:** LEHD Quarterly Workforce Indicators (NAICS 522), 2014Q1–2022Q4, county–quarter panel merged with 2017 County Business Patterns and Census population estimates. Sample: 104,097 county–quarter observations across 2,911 counties. **Method:** Continuous difference-in-differences with county and quarter fixed effects; standard errors clustered at the state level. **Sample:** All U.S. counties with non-missing QWI data for NAICS 522, excluding county-quarters with suppressed employment counts. SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(X) is the cross-county standard deviation of payday density (among counties with any payday presence) and SD(Y) is the pre-treatment standard deviation of log outcome. 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).