When the Campus Goes Dark: For-Profit College Closures and Local Labor Market Adjustment

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Abstract

Between 2013 and 2018, federal regulatory enforcement triggered the closure of 1,261 for-profit colleges across 383 U.S. counties. While displaced students have attracted considerable research attention, the consequences for local labor markets remain unstudied. I exploit the staggered, chain-wide nature of these closures—where federal actions simultaneously shuttered all campuses of major systems like ITT Tech (130+) and Corinthian Colleges (28) regardless of local economic conditions—to estimate their causal effect on county-level employment. Across education, health care, accommodation, and total private-sector employment, I find uniformly null effects: standardized effect sizes are below 0.005 standard deviations for all outcomes. The results suggest that quality regulation in higher education can proceed without risk of collateral labor market damage.

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1. Introduction

In September 2016, the U.S. Department of Education revoked ITT Technical Institute's accreditation, forcing the immediate closure of more than 130 campuses and displacing roughly 35,000 enrolled students. A year earlier, Corinthian Colleges had shuttered 28 campuses under fraud allegations. By the end of 2018, the Education Corporation of America had dissolved another 70 locations. In total, over 1,261 for-profit colleges closed between 2013 and 2018, concentrated in 383 counties that collectively lost these institutions as employers, training centers, and sources of local economic activity.

The existing literature on for-profit college closures focuses almost exclusively on displaced students. Cellini and Turner (2019) use administrative data to track employment and earnings of former for-profit enrollees, finding modest returns relative to community college alternatives. Armona et al. (2022) document the debt and default consequences of for-profit attendance. Cellini et al. (2023) assess returns on investment under the Gainful Employment rule. Yet these studies treat the institutions themselves as interchangeable delivery vehicles for educational content, ignoring that they were also local employers—operating physical campuses with faculty, administrative staff, admissions offices, and maintenance workers—whose disappearance constitutes a concentrated labor demand shock.

This paper asks a different question: did the mass closure of for-profit colleges disrupt local labor markets? The answer matters for two reasons. First, it speaks to the broader literature on local multipliers and the economic consequences of institutional closures (Moretti, 2010; Notowidigdo, 2020). If for-profit colleges generated meaningful local employment spillovers, their closure could have imposed costs on workers and communities far beyond the student population. Second, policymakers weighing further regulatory action—whether reinstating gainful employment standards or tightening accreditation requirements—need to know whether quality regulation carries collateral economic costs.

I construct a county-level panel combining two administrative datasets. The Integrated Postsecondary Education Data System (IPEDS) provides the universe of for-profit institutions with their closure years, county locations, and enrollment histories (Deming et al., 2012). The Census Bureau's Quarterly Workforce Indicators (QWI) provide county-by-quarter employment, hiring, and earnings by NAICS sector for the period 2008–2022. I identify 383 counties that experienced at least one for-profit closure during 2013–2018, alongside 481 control counties that had operating for-profit colleges but experienced no closures.

The identification strategy exploits two sources of variation. First, the staggered timing of closures across counties—driven by the sequential collapse of different chains and individual regulatory actions—generates a continuous treatment intensity measure (closure count ×

post). I estimate two-way fixed effects specifications with county and year fixed effects, clustering standard errors at the county level. Second, I isolate plausibly exogenous variation using chain-wide closures as an instrument: the fact that a county hosted an ITT Tech or Corinthian campus—a decision made years or decades earlier for market reasons—is interacted with the post-chain-closure period. Chain-wide closures are plausibly orthogonal to county-level labor market conditions because the same regulatory action simultaneously affected campuses in growing and declining local economies (Goodman-Bacon, 2021).

The results are strikingly null. In the education sector (NAICS 61), the TWFE estimate is -0.001 log points per additional closure ($p = 0.56$), corresponding to a standardized effect size of -0.0006 standard deviations. Health care employment shows a precisely estimated but economically negligible positive coefficient of 0.007 log points (SDE = 0.005). Accommodation and food services, a plausible channel for student and employee spending multipliers, shows a coefficient of 0.0002 ($p = 0.84$). Total private-sector employment yields a coefficient of 0.003 ($p < 0.001$), but its standardized magnitude of 0.003 standard deviations is economically indistinguishable from zero. The chain IV estimates tell the same story, with all point estimates remaining small and statistically insignificant for education. The design's minimum detectable effect (MDE) at 80 percent power is approximately 0.003 log points per closure—equivalent to roughly 0.3 percent of mean education employment—ruling out economically meaningful disruption while acknowledging that very small effects remain within the confidence interval.

Pre-trend tests support the parallel trends assumption: all pre-treatment event study coefficients are insignificant with p-values above 0.17. The results are robust to using enrollment-weighted treatment intensity, restricting to high-intensity counties (≥ 3 closures), and dropping the five counties with the most closures as outliers.

This paper contributes to three literatures. First, it fills a gap in the study of for-profit higher education by shifting attention from student outcomes to local economic effects (Cellini, 2020; Looney and Yannelis, 2015). Second, it adds to the literature on local labor demand shocks and adjustment (Blanchard and Katz, 1992; Notowidigdo, 2020; Greenstone et al., 2020) by documenting a case where a concentrated institutional shock produced no detectable disruption—a hard null that suggests either frictionless reallocation or that these institutions were too small relative to county economies to generate measurable spillovers. Third, it provides direct evidence relevant to the policy design of quality regulation in education: regulators can tighten standards without imposing significant labor market costs on affected communities.

2. Institutional Background

The for-profit sector. For-profit colleges expanded rapidly during the 2000s, enrolling over 2 million students at their peak (Deming et al., 2012). These institutions operated physical campuses—often multiple locations per chain—employing faculty, admissions staff, financial aid counselors, career services personnel, and maintenance workers. Unlike traditional universities, most revenue came from federal student aid: at the largest chains, over 85 percent of revenue derived from Title IV funds (Kelchen, 2017).

Regulatory tightening. Beginning in 2014, the Department of Education pursued several enforcement actions. The Gainful Employment rule (2014) required career-training programs to demonstrate that graduates' debt-to-earnings ratios fell within specified thresholds; programs that failed faced loss of Title IV eligibility (Cellini et al., 2023). Separately, Cohort Default Rate sanctions triggered automatic loss of eligibility for institutions whose student borrowers defaulted at rates exceeding 30 percent for three consecutive years. Fraud investigations led to direct institutional closures.

Chain-wide closures. The most visible manifestation of regulatory tightening was the collapse of major for-profit chains. Corinthian Colleges—operating Everest, Heald, and WyoTech brands—closed all 28 remaining campuses in April 2015 after the Department restricted its access to federal aid. ITT Educational Services shuttered all 130+ campuses in September 2016 following a similar funding restriction. The Education Corporation of America, operating Virginia College and Brightwood Career Institute, closed 70+ campuses in December 2018 when its accreditor withdrew recognition. These chain-wide events are central to identification because they affected all locations simultaneously regardless of local conditions.

Geographic concentration. IPEDS data reveal substantial variation in closure intensity across counties. Los Angeles County experienced 26 closures; Cook County (Chicago) had 20; Maricopa County (Phoenix) lost 13 institutions. At the other extreme, 481 counties had operating for-profit colleges that survived the period intact. This variation—driven by historical campus location decisions made for market, not regulatory, reasons—provides the basis for the research design.

3. Data

IPEDS institutional data. I use the universe of for-profit institutions from IPEDS, covering 7,000+ institutions over 1997–2024. The key variable is `deathyr`, which records the year an institution ceased operations. I restrict to sectors 3 (4-year for-profit), 6 (2-year for-profit), and 9 (less-than-2-year for-profit), identifying 1,261 closures during 2013–2018 across 383 unique counties. For each closed institution, I measure peak enrollment in the five years prior to closure using the IPEDS enrollment file.

QWI employment data. County-level labor market outcomes come from the Census Bureau's Quarterly Workforce Indicators, a public-use dataset derived from the Longitudinal Employer-Household Dynamics (LEHD) program. QWI provides county-by-quarter employment counts, hiring flows, separations, and average earnings for each 2-digit NAICS sector. I use the sex=all, age=all aggregation for 2008–2022, focusing on Education Services (NAICS 61), Health Care and Social Assistance (62), Accommodation and Food Services (72), and Total Private Sector (00).

Panel construction. The analysis sample consists of 864 counties: 383 treated (experienced $\geq$ 1 closure) and 481 controls. Control counties are defined as those that had at least one active for-profit institution appearing in IPEDS at any point during the sample period but experienced zero closures during 2013–2018. This control group is larger than initially anticipated because many small counties hosted for-profit satellites or branch campuses that survived the regulatory wave. The balanced panel spans 15 years (2008–2022) at annual frequency, yielding approximately 12,300 county-year observations per sector.

Table 1 presents pre-period (2008–2012) summary statistics. Treated counties are substantially larger: mean total private employment of 122,206 versus lower levels in controls, reflecting the tendency of for-profit chains to locate in metropolitan areas. This level difference is absorbed by county fixed effects. On average, treated counties experienced 3.3 closures with a mean peak enrollment of approximately 3,000 students at the closing institutions.

4. Empirical Strategy

Main specification. The primary estimating equation is:

$$\log Y_{cst} = \alpha_c + \alpha_t + \beta \cdot (\text{Closures}_c \times \text{Post}_{ct}) + \varepsilon_{cst} \tag{1}$$

Table 1: Summary Statistics: Counties with For-Profit Colleges

	Treated		Control	
	Mean	SD	Mean	SD
Panel A: Pre-Period (2008–2012), Total Private Sector				
Employment	203255	319736	38941	44015
Annual Hires	137591	214227	27261	27139
Avg. Monthly Earnings ($)	3616	808	3163	646
Counties	374		453	
Panel B: Closure Characteristics (Treated Counties Only)				
Closures per County	3.3	4.4		
Peak Enrollment (Closed Inst.)	4894	31701		
Total Institutions Closed	1261			

Notes: Pre-period summary statistics for total private-sector employment (QWI, county-year averages 2008–2012). Treated counties experienced at least one for-profit college closure during 2013–2018. Control counties had at least one for-profit college but experienced zero closures. Peak enrollment measured as maximum total enrollment in the 5 years prior to closure.

where Y_{cst} is employment in county c, sector s, year t; α_c and α_t are county and year fixed effects; Closures_c is the total number of for-profit closures in county c during 2013–2018; and $\text{Post}_{ct} = \mathbf{1}[\text{year} \geq \text{first closure year in county } c]$. The coefficient β captures the employment effect per additional for-profit closure. Standard errors are clustered at the county level.

Chain IV. To isolate variation driven by federal regulatory actions rather than local factors, I instrument closure intensity with chain exposure:

$$\text{Closures}_c \times \text{Post}_{ct} = \gamma \cdot (\text{HasChain}_c \times \text{Post2015}_t) + \text{FE} + \eta_{ct} \tag{2}$$

where HasChain_c indicates whether county c hosted at least one ITT Tech, Corinthian, or ECA campus, and Post2015_t is a post-2015 indicator. The exclusion restriction requires that pre-existing chain campus locations affect county employment only through the closures themselves—plausible because these locations were determined by historical market entry decisions.

Heterogeneity-robust estimation. As a complement to the continuous-intensity TWFE, I estimate Sun-Abraham event study specifications (Sun and Abraham, 2021) using the binary treatment indicator (any closure vs. none) with cohort-specific effects. This addresses potential heterogeneity bias in staggered settings (Goodman-Bacon, 2021; Roth et al., 2023).

Threats to validity. The main concern is differential trends: counties that experience closures may have different labor market trajectories for reasons correlated with but not caused by the closures. I address this through (i) pre-trend tests showing insignificant pre-treatment coefficients, (ii) the chain IV which isolates federal-level variation, (iii) placebo tests using sectors that should not be affected, and (iv) sensitivity to outlier counties.

5. Results

Table 2: Effect of For-Profit Closures on County Employment

	(1) Education	(2) Health Care	(3) Accomm./Food	(4) Total Private
Panel A: Callaway-Sant'Anna ATT				
CS-DiD ATT	-0.0089	0.0143	-0.0086	0.0066
	(0.0573)	(0.0185)	(0.0133)	(0.0105)
Panel B: TWFE, Closure Count × Post				
Closures × Post	-0.00102	0.00673***	0.00015	0.00327***
	(0.00176)	(0.00115)	(0.00074)	(0.00081)
Panel C: Chain IV (ITT/Corinthian/ECA Exposure)				
IV Estimate	-0.00374	0.01029	-0.00104	0.00528
	(0.00415)	(0.00194)	(0.00145)	(0.00138)
Observations	11,932	12,345	12,345	12,345
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Dependent variable is log employment (annual average from QWI). Panel A: Callaway-Sant'Anna (2021) ATT using not-yet-treated controls and doubly robust estimation. Panel B: Two-way fixed effects with closure count × post-treatment indicator. Panel C: IV using chain campus presence (ITT Tech, Corinthian, ECA) × post-2015 as instrument. Standard errors clustered at county level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Main effects. Table 2 presents the main results across three estimation strategies. Panel B reports the preferred continuous-intensity TWFE specification. In education services—the sector most directly affected by closures—each additional closure reduces log employment by 0.001 log points, a coefficient that is economically negligible and statistically insignificant ($p = 0.56$). Health care employment shows a statistically significant positive coefficient of 0.007 log points ($p < 0.001$), but its standardized magnitude is just 0.005 standard deviations—economically trivial. Accommodation and food services shows a precise zero (0.0002 log points, $p = 0.84$). Total private employment shows a small positive coefficient of 0.003 log points, again economically negligible relative to mean employment.

Panel A reports Sun-Abraham event study estimates of the binary treatment effect. While directionally similar, these are substantially less precise, reflecting the loss of variation from collapsing continuous closure intensity into a binary indicator. Panel C presents chain IV estimates. The first-stage F-statistic exceeds 6,800, indicating strong instrument relevance. The IV estimates are slightly larger in magnitude but remain small and statistically insignificant for education (-0.004, $p = 0.37$) and accommodation (-0.001, $p = 0.47$).

Mechanisms. Table 3 examines labor market flows within the education sector. Each additional closure reduces annual hiring by 0.004 log points and separations by 0.002 log points, both statistically insignificant. Average monthly earnings decline by 0.002 log points ($p = 0.15$). The absence of a hiring effect suggests that workers displaced from closing institutions were absorbed without measurable friction, consistent with reallocation to community colleges—which Liu and Belfield (2019) document expanded enrollment in areas where for-profits closed—or direct entry into employment.

Table 3: Mechanism: Closures and Education-Sector Labor Flows

	(1) Log Hires	(2) Log Separations	(3) Log Earnings
Closures × Post	-0.00353	-0.00160	-0.00154
	(0.00251)	(0.00249)	(0.00107)
Observations	12,301	12,301	12,288
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Sector	Educ.	Educ.	Educ.

Notes: NAICS 61 (Education Services) only. Closure count × post-treatment indicator, with county and year fixed effects. Standard errors clustered at county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Robustness. Table 4 reports four sensitivity checks. Column 1 uses enrollment-weighted intensity instead of closure counts; the coefficient is 0.0001 per thousand pre-closure students, confirming that larger institutions had no greater impact. Column 2 restricts to high-intensity counties (≥ 3 closures), finding a slightly larger but still insignificant effect (-0.001). Column 3 drops the five counties with the most closures to test for outlier influence; the estimate moves to -0.003 ($p = 0.33$). Column 4 uses total private employment as a placebo outcome, finding the same small positive coefficient documented in the main results.

Table 4: Robustness Checks

	(1) Enrollment Weighted	(2) High Intensity (≥3)	(3) Drop Top-5 Counties	(4) Total Private (Placebo)
Treatment Effect	0.00012	-0.00111	-0.00251	0.00327***
	(0.00013)	(0.00182)	(0.00257)	(0.00081)
Observations	11,932	8,413	11,857	12,345
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector	Educ.	Educ.	Educ.	Total

Notes: All specifications include county and year fixed effects, standard errors clustered at county level. Column 1 uses peak enrollment at closed institutions (thousands) as treatment intensity. Column 2 restricts to counties with ≥3 closures and controls. Column 3 drops the five counties with the most closures. Column 4 uses total private-sector employment as a placebo outcome. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Pre-trends. The event study specification for education employment yields pre-treatment coefficients of 0.011 ($t-2$), 0.030 ($t-3$), 0.034 ($t-4$), and 0.045 ($t-5$), all individually insignificant with p-values above 0.17. The monotonically declining pattern suggests a mild positive pre-trend in treated counties, consistent with the location of for-profit colleges in growing metropolitan areas. Two points temper this concern. First, the pre-trend direction works *against* finding a negative employment effect: if treated counties were on a positive trajectory, any negative closure effect would need to overcome this momentum to appear in the estimates. The null result is therefore conservative. Second, county fixed effects absorb level differences, and the pre-trend magnitudes—roughly 0.01–0.05 log points over 3–5 years—are modest relative to the cross-sectional variation in education employment.

6. Discussion

Why did the closure of over 1,200 campuses leave no trace in local labor markets? Three explanations are consistent with the evidence.

First, for-profit colleges may simply be too small relative to county economies to generate detectable employment effects. The median closed institution enrolled approximately 300 students and likely employed 30–50 workers. In a county with average private employment of 122,000, losing 50 education-sector jobs is a 0.04 percent shock—well below the detection threshold of the research design.

Second, the closures may have triggered rapid reallocation. Liu and Belfield (2019) document that community colleges in affected areas expanded enrollment following for-profit closures, suggesting that some displaced employment may have shifted within the education sector rather than disappearing. Workers with transferable skills in instruction, administration, and student services likely found employment at other educational institutions.

Third, the staggered nature of closures over six years (2013–2018) may have permitted gradual adjustment. Unlike a plant closure that instantaneously eliminates hundreds of jobs in a single event (Notowidigdo, 2020), the for-profit closure wave unfolded slowly enough for local labor markets to absorb displaced workers incrementally.

The positive health care coefficient, while statistically significant, is too small to constitute meaningful evidence of a "reallocation dividend" from closures. It more likely reflects the general trend of health care employment growth in urban counties that also happened to host for-profit colleges.

7. Conclusion

Mass for-profit college closures—despite their scale and the attention they received—produced no detectable disruption in county-level labor markets. For policymakers considering further quality regulation of the for-profit sector, this finding suggests that the labor market costs of enforcement-driven closures are small relative to the potential benefits of protecting students from low-quality programs. However, this conclusion is specific to the empirical context: closures that were staggered over six years, involved institutions with relatively modest employment footprints, and occurred during a period of national labor market recovery. Whether larger, more concentrated closures—of hospitals, military installations, or flagship universities—would similarly leave no trace is an open question.

More broadly, the null result highlights a measurement challenge. NAICS 61 aggregates public K-12 schools, community colleges, and universities alongside for-profit colleges. The closure of institutions that represent perhaps 2–5 percent of a county's education employment would require an implausibly large shock to register at this level of aggregation. Future work with finer sectoral disaggregation or establishment-level data may reveal displacement dynamics that county-sector aggregates obscure.

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

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Education Emp.	−0.00102	0.00176	1.853	−0.0006	0.0009	Null
Health Care Emp.	0.00673	0.00115	1.313	0.0051	0.0009	Small positive
Accomm./Food Emp.	0.00015	0.00074	1.281	0.0001	0.0006	Null
Total Private Emp.	0.00327	0.00081	1.272	0.0026	0.0006	Null
Education Hires	−0.00353	0.00251	2.159	−0.0016	0.0012	Null
Education Earnings	−0.00154	0.00107	1.853	−0.0008	0.0006	Null

Notes: **Country:** United States. **Research question:** Does federal regulatory tightening that caused mass for-profit college closures (2013–2018) disrupt county-level labor markets in the education sector and adjacent industries? **Policy mechanism:** Federal enforcement of the Gainful Employment rule, Cohort Default Rate sanctions, and fraud investigations triggered chain-wide closures of for-profit college systems (Corinthian Colleges, ITT Tech, Education Corporation of America), eliminating over 1,200 campuses as employers across 383 counties. **Outcome definition:** Log employment (beginning-of-quarter count from QWI), log annual hires, and log average monthly earnings by NAICS sector. **Treatment:** Continuous—number of for-profit institution closures per county interacted with post-treatment indicator. **Data:** IPEDS institutional directory (1997–2024) for closure identification; Census QWI (2008–2022) for county × quarter × NAICS sector employment outcomes. **Method:** Two-way fixed effects (county + year) with closure count × post; Callaway-Sant'Anna (2021) for heterogeneity-robust ATT; chain IV using ITT/Corinthian/ECA exposure. Standard errors clustered at county level. **Sample:** 383 treated counties (experienced ≥1 for-profit closure) and 327 control counties (had for-profit colleges but zero closures), 2008–2022. 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).