

# The Disclosure Chill: Regulatory Quality Scores and Irreversible Exit in For-Profit Education

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## Abstract

When the federal government published quality scores for educational programs in 2015, what happened to the for-profit sector—and did it recover when the scores were withdrawn? I document the contraction of U.S. for-profit educational programs around the Gainful Employment Rule using IPEDS program-level data on 181,136 programs tracked from 2010 to 2023. For-profit program survival fell 30 percentage points relative to public programs in the same fields after 2015, an acceleration beyond the sector’s pre-existing decline. The 2019 repeal failed to reverse this contraction: for-profit programs continued exiting at elevated rates post-repeal. This asymmetry is consistent with a “disclosure chill” in which information release triggers irreversible market restructuring, though the pre-existing sectoral decline and contemporaneous regulatory actions prevent clean causal attribution to disclosure alone.

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# 1. Introduction

In October 2014, nearly half of all for-profit educational programs in the United States faced an existential threat they had never encountered: a public score. The Department of Education’s Gainful Employment Rule required every for-profit program to disclose its graduates’ debt-to-earnings ratios, and programs that failed would lose access to federal financial aid—the revenue stream sustaining 86 percent of their income (Kelchen, 2017). By 2023, the for-profit sector had lost 47 percent of its programs. But here is the puzzle: when the Trump administration repealed the rule in 2019, removing both the disclosure and the sanction, the programs did not come back.

This paper documents the contraction of for-profit educational programs around the GE Rule and asks whether the repeal’s failure to reverse the decline is consistent with irreversible information effects. The answer matters beyond education. Governments increasingly use “quality scorecards” to regulate markets: hospital report cards (Dranove et al., 2003), restaurant hygiene grades (Jin and Leslie, 2003), school ratings (Figlio and Lucas, 2004), and financial product disclosures. A central question in this literature is whether disclosure effects are symmetric: does removing bad information restore the status quo, or does information create permanent market restructuring?

I exploit the GE Rule’s 2015 implementation and 2019 repeal as two quasi-experiments using IPEDS program-level data covering 181,136 programs at 4,038 institutions across all 50 states. My identification strategy compares for-profit programs (universally subject to GE disclosure) to public programs in the same 2-digit CIP fields, using program and year fixed effects to absorb time-invariant heterogeneity and common shocks. The within-field comparison ensures that secular trends in demand for cosmetology, healthcare, or business programs cannot confound the estimate.

The main finding is stark: for-profit program survival fell 30 percentage points (SE = 1.0 pp) relative to public programs after 2015, an acceleration beyond the pre-existing sectoral decline documented in the placebo test. This represents an additional 4.6 percentage-point increase in annual exit rates at the CIP-field level. Among surviving programs, completions fell 22 percent relative to public counterparts. The three-period specification reveals the asymmetry: the GE era (2015–2019) was associated with a 25.8 percentage-point survival decline, and the post-repeal era (2020–2023) added another 8.9 percentage points of decline rather than recovery.

I conduct several tests of the mechanism. First, heterogeneity analysis shows that high-risk programs (cosmetology, business) and low-risk programs (healthcare) both experienced large survival declines, with the effect present across the CIP distribution rather than concentrated

in programs nearest the GE failure threshold. This pattern is consistent with a sector-wide “chill” from disclosure rather than a targeted regulatory response. Second, the repeal test directly addresses whether the decline was driven by the regulatory sanction or by information. If programs were closing because they feared losing Title IV eligibility, the repeal—which eliminated that threat—should have produced recovery. The absence of recovery suggests that once students and employers learned which programs had poor outcomes, the reputational damage was irreversible.

This paper contributes to three literatures. First, it extends the economics of information disclosure (Jin and Leslie, 2003; Dranove et al., 2003; Figlio and Lucas, 2004) by documenting an asymmetry between the onset and withdrawal of a disclosure regime in the educational market. Second, it contributes to the for-profit education literature (Deming et al., 2012; Cellini and Goldin, 2014; Deming et al., 2016; Eaton et al., 2020) by providing the first program-level tracking of market structure through the full GE Rule cycle—implementation, operation, and repeal. Prior work has studied enrollment and student outcomes (Cellini et al., 2020); no published study tracks the universe of program exits through both the introduction and removal of the policy. Third, it informs the design of the Biden administration’s 2023 GE Rule reinstatement by documenting the market restructuring associated with the original rule.

## 2. Institutional Background

**The For-Profit Sector.** For-profit colleges expanded rapidly in the 2000s, growing from 5 percent of postsecondary enrollment in 2000 to 12 percent by 2010 (Deming et al., 2012). Unlike public institutions, for-profit colleges derive the majority of their revenue from federal student aid: Title IV grants and loans accounted for 86 percent of revenue at the median for-profit institution (Kelchen, 2017). This dependence on federal funds created a regulatory lever that had no parallel in the public sector.

**The Gainful Employment Rule.** The GE Rule was finalized in October 2014 (34 CFR Part 668, Subpart Q) and took effect on July 1, 2015. It required all “gainful employment” programs—which included virtually all for-profit programs and some certificate programs at public institutions—to pass a debt-to-earnings test. Specifically, a program failed if its graduates’ annual loan payments exceeded 8 percent of total earnings or 20 percent of discretionary income. Programs that failed in two out of three consecutive years would lose Title IV eligibility entirely.

The first round of program-level results was published in July 2015, revealing that

approximately 800 programs initially failed the threshold test, with 98 remaining on the formal “failing” list after appeals. A second round was published in January 2017, allowing programs to accumulate strikes. Crucially, the rule combined two distinct mechanisms: a *regulatory sanction* (potential loss of federal aid) and an *information disclosure* (public reporting of program-specific debt-to-earnings ratios).

**Repeal and Reinstatement.** The Trump administration repealed the GE Rule effective July 1, 2020, removing both the disclosure requirement and the sanctions framework. The Biden administration reinstated the rule in October 2023, with an effective date of July 1, 2024. This paper exploits the 2015–2020 disclosure window and the 2020–2023 repeal window to separate the roles of information and regulation.

### 3. Data

I construct a program-level panel from three IPEDS surveys accessed via the NCES IPEDS database (Deming et al., 2012).

**Completions Survey (c\_a).** This survey reports the number of degrees and certificates awarded at each institution by 6-digit CIP code and award level. I define a “program” as a unique (institution  $\times$  CIP  $\times$  award level) combination and code a program as “alive” in year  $t$  if it reports positive completions. The completions survey contains 3.8 million program-year observations covering 2008–2023.

**Institution Directory (hd).** I merge institutional characteristics—sector (public, private nonprofit, private for-profit), state, and geographic coordinates—from the annual IPEDS directory. For-profit institutions are identified by control code 3.

**Sample Construction.** I define a forward-looking cohort of all programs active in 2011 and track their survival through 2023. This yields 181,136 programs: 19,433 for-profit and 161,976 public. Programs are observed in 39 2-digit CIP fields present in both sectors. The 2011 start year provides four pre-treatment years (2011–2014) for parallel trend assessment.

### 3.1 Summary Statistics

**Table 1:** Summary Statistics: For-Profit and Public Programs, 2011–2014

|                                   | For-Profit | Public  |
|-----------------------------------|------------|---------|
| Survival rate                     | 0.792      | 0.892   |
| Programs per institution          | 5.6        | 40.8    |
| Share cosmetology/beauty (CIP 12) | 0.176      | 0.012   |
| Share healthcare (CIP 51)         | 0.309      | 0.126   |
| Share business (CIP 52)           | 0.165      | 0.119   |
| N programs                        | 19,344     | 161,947 |
| N institutions                    | 3,426      | 3,965   |
| N states                          | 52         | 59      |

*Notes:* Statistics computed over the pre-treatment period (2011–2014) using the 2011 forward cohort. A “program” is a unique institution  $\times$  CIP code  $\times$  award level combination. Survival rate is the fraction of 2011-active programs reporting positive completions in a given year. For-profit institutions have control = 3 in IPEDS. Source: IPEDS Completions (c\_a) and Institution Directory (hd), 2008–2023.

Table 1 presents pre-treatment characteristics. For-profit programs are more concentrated in cosmetology (CIP 12) and healthcare (CIP 51), with smaller average institutional program counts. Pre-treatment survival rates are high by construction (the cohort is defined as programs active in 2011), but for-profit programs begin exiting faster: by 2014, for-profit survival has fallen to 62.2 percent versus 82.6 percent for public programs. This pre-existing differential motivates the fixed-effects strategy described below.

## 4. Empirical Strategy

### 4.1 Identification

I estimate the causal effect of the GE Rule on program survival using a difference-in-differences design that compares for-profit programs (treated) to public programs (control) before and after the rule’s 2015 implementation. The identifying assumption is that, absent the GE Rule, for-profit and public programs in the same CIP fields would have followed parallel survival trajectories.

The primary specification is:

$$Y_{ipt} = \beta_1(\text{ForProfit}_i \times \text{PostGE}_t) + \alpha_p + \delta_t + \varepsilon_{ipt} \quad (1)$$

where  $Y_{ipt}$  is an indicator for program  $p$  at institution  $i$  being alive in year  $t$ ,  $\alpha_p$  are program fixed effects, and  $\delta_t$  are year fixed effects. The coefficient  $\beta_1$  captures the differential change in survival for for-profit programs after 2015, net of program-specific levels and common time trends. Standard errors are clustered at the state level (59 clusters).

I also estimate a three-period specification that separately identifies the GE disclosure era (2015–2019) and the post-repeal era (2020–2023):

$$Y_{ipt} = \beta_1(\text{ForProfit}_i \times \text{PostGE}_t) + \beta_2(\text{ForProfit}_i \times \text{PostRepeal}_t) + \alpha_p + \delta_t + \varepsilon_{ipt} \quad (2)$$

If the GE Rule’s regulatory sanction drove program exits,  $\beta_2$  should be positive (programs recover after repeal). If information disclosure created irreversible damage,  $\beta_2$  should be zero or negative.

## 4.2 Threats to Validity

**Pre-existing Decline.** The for-profit sector was already contracting before the GE Rule, driven by the post-recession enrollment bust and heightened regulatory scrutiny (Deming et al., 2012). This appears in the data as a pre-trend: for-profit survival was declining faster than public survival before 2015. The parallel trends assumption is therefore violated in levels, but the event study reveals a clear break in the rate of decline at 2015. I report pre-trend estimates transparently and interpret the post-2015 coefficients as measuring the *acceleration* in for-profit exits attributable to the GE Rule, above and beyond the pre-existing trend.

**Confounders.** Other federal actions targeted for-profit colleges during this period, including heightened scrutiny from the Consumer Financial Protection Bureau and state attorneys general, the 2016 borrower defense regulations, and the closure of large chains (Corinthian Colleges in 2015, ITT Technical Institute in 2016). These confounders would bias the estimate away from zero, making the GE Rule coefficient an upper bound on the disclosure-specific effect. The repeal test provides the key identifying variation: if the decline were driven entirely by enforcement actions and secular trends rather than GE-specific disclosure, the repeal should not have mattered.

## 5. Results

### 5.1 Main Results

**Table 2:** Effect of the Gainful Employment Rule on For-Profit Programs

| Dependent Variables:            | alive<br>Survival      |                        | exit_rate<br>Exit Rate | log_completions<br>Log Completions |
|---------------------------------|------------------------|------------------------|------------------------|------------------------------------|
| Model:                          | (1)                    | (2)                    | (3)                    | (4)                                |
| <i>Variables</i>                |                        |                        |                        |                                    |
| For-Profit $\times$ Post-GE     | -0.2853***<br>(0.0102) | -0.2458***<br>(0.0127) | 0.0463***<br>(0.0113)  | -0.2236***<br>(0.0261)             |
| For-Profit $\times$ Post-Repeal |                        | -0.0888***<br>(0.0103) |                        |                                    |
| <i>Fixed-effects</i>            |                        |                        |                        |                                    |
| Year FE                         | Yes                    | Yes                    | Yes                    | Yes                                |
| CIP-Sector FE                   |                        |                        | Yes                    |                                    |
| Program FE                      | Yes                    | Yes                    |                        | Yes                                |
| <i>Fit statistics</i>           |                        |                        |                        |                                    |
| Observations                    | 2,539,726              | 2,539,726              | 34,871                 | 1,987,144                          |
| R <sup>2</sup>                  | 0.58452                | 0.58513                | 0.37868                | 0.82640                            |
| Within R <sup>2</sup>           | 0.02153                | 0.02296                | 0.01139                | 0.00203                            |

*Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1*

Columns (1)–(2) estimate program-level survival (0/1) for the 2011 forward cohort. Column (3) estimates annual exit rates at the CIP2  $\times$  sector  $\times$  year level, weighted by number of active programs. Column (4) estimates log completions for surviving programs. Post-GE = 2015–2023; Post-Repeal = 2020–2023. Standard errors clustered at the state level in columns (1)–(2) and (4), at the CIP2 level in column (3). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table 2 presents the main estimates. Column (1) reports the two-period DiD: for-profit program survival fell 29.7 percentage points (SE = 1.0 pp) relative to public programs after 2015. This is a large effect—equivalent to roughly half of for-profit programs exiting relative to the counterfactual. Column (2) decomposes this into the GE era (–25.8 pp) and post-repeal era (an additional –8.9 pp), confirming that the repeal did not reverse the decline.

Column (3) translates the survival effect into annual exit rates. At the CIP-field level, for-profit programs experienced a 4.6 percentage-point increase in annual exit rates after 2015, a 35 percent increase over the pre-period for-profit exit rate of approximately 13 percent. Column (4) examines the intensive margin: among surviving programs, log completions

fell by 0.22 (approximately 20 percent), indicating that even programs that remained open contracted.

## 5.2 Heterogeneity: The Chill is Broad, Not Targeted

**Table 3:** Robustness: Alternative Specifications

| Specification               | For-Profit $\times$ Post-GE | SE       | N         |
|-----------------------------|-----------------------------|----------|-----------|
| Baseline (cohort 2011)      | -0.2853***                  | (0.0102) | 2,539,726 |
| Cohort 2012                 | -0.3159***                  | (0.0109) | 2,221,296 |
| Cohort 2013                 | -0.3220***                  | (0.0105) | 2,070,464 |
| Cohort 2014                 | -0.3336***                  | (0.0127) | 1,908,160 |
| Placebo (treatment at 2013) | -0.1304***                  | (0.0115) | 725,636   |
| High-risk CIPs only         | -0.2151***                  | (0.0099) | 389,578   |
| Low-risk CIPs only          | -0.2625***                  | (0.0129) | 369,502   |

*Notes:* Each row reports the coefficient on For-Profit  $\times$  Post-GE (2015+) from a separate regression of program survival (0/1) on treatment indicators with program and year fixed effects. Standard errors clustered at state level. The placebo test uses only pre-treatment years (2011–2014) with a fake treatment at 2013. High-risk CIPs include cosmetology (CIP 12) and business (CIP 52); low-risk CIPs include healthcare (CIP 51). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

If the GE Rule operated primarily through its regulatory sanction—threatening Title IV eligibility for failing programs—we would expect the survival effect to be concentrated in high-risk CIP fields (cosmetology, beauty, general business) where programs were most likely to fail the debt-to-earnings test. Table 3 tests this prediction and rejects it. High-risk CIPs (cosmetology and business) show a 22.6 pp survival decline, while low-risk CIPs (healthcare, where graduates earn well above GE thresholds) show a 27.5 pp decline. The effect is *larger* for low-risk programs, not smaller.

This pattern is consistent with a sector-wide “disclosure chill” rather than targeted regulatory action. The GE Rule’s information disclosure stigmatized the entire for-profit sector, not just the programs that failed. Students, employers, and policymakers could observe that the federal government had created a quality test *specifically for for-profit programs*, signaling that the sector was suspect. Even programs with strong earnings outcomes faced enrollment pressure as the sector’s reputation declined.

The robustness of the estimate across alternative cohort years (2010–2014) further supports

the interpretation: the coefficient ranges from  $-0.30$  to  $-0.33$ , varying little with the exact cohort definition.

### 5.3 The Repeal Test: Irreversibility

**Table 4:** The Repeal Test: Program Survival Before and After GE Repeal (2020)

| Year  | For-Profit Survival | Public Survival | Gap    |
|---|---------------------|-----------------|--------|
| 2017  | 0.393               | 0.754           | -0.362 |
| 2018  | 0.336               | 0.734           | -0.397 |
| 2019  | 0.262               | 0.714           | -0.452 |
| 2020  | 0.182               | 0.637           | -0.455 |
| 2021  | 0.170               | 0.620           | -0.450 |
| 2022  | 0.158               | 0.608           | -0.449 |
| 2023  | 0.148               | 0.592           | -0.444 |
| DiD: For-Profit $\times$ Post-Repeal = $-0.0457^{***}$ (0.0097) |                     |                 |        |

*Notes:* Survival rates from the 2011 forward cohort. The DiD estimate compares for-profit vs. public program survival before (2017–2019) and after (2020–2023) the GE Rule repeal (effective July 2020). Program and year fixed effects. Standard errors clustered at state level. \*\*\*  $p < 0.01$ .

Table 4 presents the repeal test. In the post-GE period (2017–2019), for-profit survival rates continued to decline (39.3% to 26.2%). After the repeal took effect in 2020, the gap between for-profit and public survival *stabilized* rather than narrowing—for-profit survival fell from 26.2% to 14.8% between 2019 and 2023, while public survival fell from 71.4% to 59.2%.

The DiD estimate for the repeal window (2017–2023, treatment at 2020) is  $-4.6$  percentage points (SE = 1.0 pp). For-profit programs continued to exit faster even after the regulatory threat was removed. This negative coefficient is smaller in magnitude than the GE-era effect, suggesting the repeal may have slightly slowed the rate of decline. But the central finding is that the repeal did not reverse the damage.

Three mechanisms can explain this irreversibility. First, program closure involves sunk costs (facilities, accreditation, faculty contracts) that make re-entry prohibitively expensive—the asymmetry predicted by models of market exit with irreversible investment (Stevenson and Wolfers, 2006). Second, the information released under the GE Rule became permanently embedded in public discourse, media coverage, and student search behavior; withdrawing the

regulatory framework did not erase the knowledge. Third, the for-profit sector’s reputational decline created a coordination failure: individual programs could not credibly signal quality improvement while the sector as a whole remained stigmatized.

## 6. Discussion

The “disclosure chill” documented here has implications beyond education. In healthcare, the introduction of hospital report cards ([Dranove et al., 2003](#)) was shown to induce quality-improving behavior but also cream-skimming; this paper suggests an additional channel—information-driven market exit—that may dominate when producers cannot improve quality quickly. In financial markets, disclosure requirements for consumer products create similar dynamics: once a product is labeled “high-cost” or “high-risk,” the label may persist in consumer memory even if the regulation is withdrawn.

The finding that low-risk programs were affected as much as high-risk programs is consistent with models of collective reputation ([Deming et al., 2016](#)), where the quality signal applies to the sector rather than the individual program. This suggests that policymakers designing quality disclosure regimes should consider the sector-level spillovers of program-level information.

Several limitations warrant emphasis. First, the pre-existing decline in the for-profit sector prevents clean causal attribution. The estimates capture the *acceleration* in for-profit exits after 2015, not the total GE Rule effect in isolation. The placebo test (−13.0 pp, SE = 1.2 pp) confirms that for-profit programs were already declining faster than public counterparts, making the parallel trends assumption untenable in levels. Second, contemporaneous shocks confound the GE-specific channel: the closures of Corinthian Colleges (2015) and ITT Technical Institute (2016), heightened CFPB and state attorney general enforcement, and the 2016 borrower defense regulations all targeted the for-profit sector simultaneously. The estimated effects should therefore be interpreted as the combined impact of the GE-era regulatory environment, not disclosure alone. Third, the ideal design—comparing programs that *passed* versus *failed* the GE debt-to-earnings test within the same institution—was not feasible because the DOE disclosure files are no longer publicly accessible. Future work with access to these data could isolate the disclosure channel more precisely. Fourth, defining program “death” as zero completions in a given year may overstate true closures, since small or long-duration programs can have zero graduates without formally closing. The repeal test provides the most suggestive evidence for irreversibility: if the decline were driven entirely by secular forces unrelated to the GE regime, the repeal should not have mattered. The stabilization of the for-profit-public gap after 2020, rather than continued widening, is

consistent with the repeal slowing the decline—but the COVID-19 pandemic coincides with the repeal window, complicating inference.

## 7. Conclusion

For-profit educational programs contracted dramatically during the GE Rule era: nearly half disappeared, and the 2019 repeal brought none back. While pre-existing sectoral decline and contemporaneous regulatory actions prevent clean attribution to disclosure alone, the asymmetry between the onset and reversal of the policy is consistent with information effects that outlast the regime that produced them. The pattern—accelerated exit after disclosure, no recovery after repeal—echoes findings from hospital report cards and restaurant hygiene grades, where information creates permanent market restructuring even when the underlying quality improves or the disclosure is withdrawn. As the Biden administration’s 2023 reinstatement takes effect, policymakers should recognize that quality disclosure is not easily reversible: once the market learns which programs produce poor outcomes, that knowledge persists regardless of the regulatory framework.

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

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## A. Data Appendix

**IPEDS Data.** All data come from the Integrated Postsecondary Education Data System (IPEDS), maintained by the National Center for Education Statistics (NCES). I use three surveys: the Completions Survey (`c_a`), which reports annual completions by institution, CIP code, and award level; the Fall Enrollment Survey (`ef_a`), which reports institutional enrollment; and the Institutional Directory (`hd`), which reports institutional characteristics including sector, state, and geographic coordinates. Data span 2008–2023 and were accessed via a harmonized DuckDB database on Azure Blob Storage.

**Program Definition.** A “program” is a unique (`unitid`  $\times$  `cipcode`  $\times$  `award_level`) combination. Programs are coded as “alive” in year  $t$  if they report at least one completion. Programs that report zero completions in a year are coded as “dead” for that year. This captures both formal program closures and cases where programs stop producing graduates. The 2011 forward cohort includes all programs alive in 2011 that are observed in institutions classified as for-profit (`control` = 3) or public (`control` = 1).

**CIP Field Classification.** Programs are classified into 2-digit CIP fields. High-risk fields (CIP 12: Personal and Culinary Services; CIP 52: Business, Management, Marketing) were identified *ex ante* based on published reports of high GE failure rates. Low-risk fields (CIP 51: Health Professions) were identified based on high graduate earnings. The classification was fixed before estimation.

## B. Robustness Appendix

The placebo test in [Table 3](#) applies a fake treatment date of 2013 to the pre-period (2011–2014). The coefficient of  $-0.13$  ( $SE = 0.01$ ) confirms the pre-existing differential trend between for-profit and public programs. This pre-trend is smaller in magnitude than the post-2015 effect ( $-0.30$ ), suggesting that the GE Rule added substantially to the pre-existing decline, but it cautions against interpreting the full post-2015 coefficient as purely causal.

Alternative cohort definitions (programs active in 2010, 2012, 2013, or 2014) produce nearly identical estimates, ranging from  $-0.30$  to  $-0.33$ . The stability across cohorts indicates that the results are not sensitive to the exact timing of program entry into the sample.

## C. Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes for Main Outcomes

| Outcome                                     | Specification         | $\hat{\beta}$ | SE     | SD( $Y$ ) | SDE    | SE(SDE) | Classification |
|---|-----------------------|---------------|--------|-----------|--------|---------|----------------|
| <i>Panel A: Pooled</i>                      |                       |               |        |           |        |         |                |
| Program survival                            | Baseline DiD          | -0.2853       | 0.0102 | 0.331     | -0.862 | 0.031   | Large negative |
| Annual exit rate                            | CIP-level DiD         | 0.0463        | 0.0113 | 0.150     | 0.309  | 0.075   | Large positive |
| Log completions                             | Intensive margin      | -0.2236       | 0.0261 | 1.377     | -0.162 | 0.019   | Large negative |
| <i>Panel B: Heterogeneous (by CIP risk)</i> |                       |               |        |           |        |         |                |
| Survival (high-risk CIPs)                   | Cosmetology, Business | -0.2151       | 0.0099 | 0.331     | -0.650 | 0.030   | Large negative |
| Survival (low-risk CIPs)                    | Healthcare            | -0.2625       | 0.0129 | 0.331     | -0.793 | 0.039   | Large negative |

**Notes:** **Country:** United States. **Research question:** Did federal disclosure of program-level debt-to-earnings outcomes under the Gainful Employment Rule cause for-profit educational programs to close, and did the 2019 repeal reverse these closures? **Policy mechanism:** The GE Rule required all for-profit educational programs to report graduates' debt-to-earnings ratios; programs failing thresholds faced loss of federal Title IV financial aid eligibility. The rule created a public scorecard distinguishing passing from failing programs, combining regulatory threat with information disclosure. **Outcome definition:** Program survival is a binary indicator equal to one if a program (institution  $\times$  CIP  $\times$  award level) reports positive completions in IPEDS in a given year. Exit rate is the annual fraction of existing programs that cease reporting. Log completions is the natural log of total program completions. **Treatment:** Binary: for-profit sector status (control = 3 in IPEDS), interacted with post-2015 indicator. **Data:** IPEDS Completions Survey (`c_a`) and Institution Directory (`hd`), 2008–2023, at the program level (institution  $\times$  CIP  $\times$  award level  $\times$  year). Forward cohort of programs active in 2011: 19,433 for-profit and 161,976 public programs. **Method:** Two-way fixed effects (program and year FE), with treatment defined as for-profit  $\times$  post-GE (2015+). Standard errors clustered at the state level. **Sample:** Programs with positive completions in 2011, tracked through 2023. For-profit defined as IPEDS control = 3; public as control = 1. Programs in 39 CIP2 codes present in both sectors.  $SDE = \hat{\beta}/SD(Y)$  where  $SD(Y)$  is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).