# The Scarlet Score: Information Disclosure as Irreversible Regulation in For-Profit Higher Education

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**Abstract**

In January 2017, the U.S. Department of Education publicly labeled approximately 740 for-profit college programs as "failing" based on Debt-to-Earnings ratios. Six months later, the Trump administration paused enforcement—but the scores remained public. I exploit this two-shock design to separate reputational damage from regulatory threat. Using program-level IPEDS data for 3,429 for-profit programs, I find that labeled programs experienced a 17 percent decline in completions, with the loss *deepening* after the rollback. The "scarlet score" persists: once publicly branded as failing, reputational damage compounds without enforcement. Because for-profit programs disproportionately serve minority students, the enrollment decline produces large absolute losses for minority completers. Information disclosure alone functions as a powerful, irreversible regulatory tool.

**JEL Codes:** I23, I28, D83, L51

**Keywords:** information disclosure, for-profit colleges, gainful employment, regulatory roll-back, reputational effects, higher education accountability

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## 1. Introduction

When the government tells the public that a college program produces graduates who cannot repay their loans, does the stigma fade once the government stops enforcing? In January 2017, the Department of Education published Debt-to-Earnings (D/E) scores for over 8,600 for-profit college programs, publicly labeling roughly 740 as "failing." The label carried real regulatory consequences: programs that failed twice would lose access to federal financial aid. But the Trump administration, signaling a friendlier stance toward for-profit education, paused enforcement within six months and formally rescinded the Gainful Employment (GE) rule by 2019. The regulatory threat vanished. The published scores did not.

This paper exploits the rare two-shock structure of GE implementation—a public disclosure shock followed by a regulatory rollback—to identify whether information disclosure alone permanently alters market outcomes. The distinction matters for the design of accountability systems worldwide. If enrollment recovers after the regulatory threat is removed, score publication was merely a conduit for the threat of sanctions. If enrollment continues to decline, information disclosure is itself a regulatory tool—one that, unlike sanctions, cannot be reversed by a subsequent administration.

I construct a panel of 3,429 for-profit programs (404 "fail," 2,668 "pass") matched across the GE score publication and IPEDS completions databases, spanning 2012 to 2021. The identification strategy compares programs publicly designated as failing (annual D/E rate exceeding 12 percent) to programs that passed (D/E below 8 percent), absorbing program and year fixed effects. A two-stage decomposition separates the initial publication effect ($\beta_1$, capturing the combined reputational and regulatory channels in 2017) from the post-rollback trajectory ($\beta_2$, isolating the persistent reputational scar from 2018 onward). Within-institution comparisons—restricted to the 123 institutions that housed both failing and passing programs—further absorb all institution-level confounders.

The results are striking. In the log specification, failing programs experienced a 17.4 percent enrollment decline in the publication year ($p < 0.001$) and an additional 25.9 percent decline in the post-rollback period ($p = 0.016$). Rather than recovering, the "scarlet score" deepened. The event study shows clean pre-trends and a monotonically widening gap: by 2021, failing programs had lost 46 completions per year relative to passing programs. These patterns hold when restricting to within-institution comparisons, when excluding institutions that closed, and when measuring the extensive margin (program exit).

The effects were racially unequal. While the immediate publication shock showed no differential racial composition effect, the post-rollback period saw minority completions at failing programs decline by 11.2 students per year ($p = 0.059$), with Black completions specifically falling by 6.5 ($p = 0.085$). The scarlet score's long-run damage fell disproportionately on the students these programs predominantly serve.

This paper contributes to three literatures. First, it advances the economics of information disclosure as regulation. Greenstone (2002) showed that mandatory emissions reporting under the Toxics Release Inventory reduced pollution; Jin and Leslie (2003) demonstrated that hygiene grade cards changed restaurant revenue. I show that quality score disclosure in education creates a "scarlet score"—a permanent reputational mark that persists even after the regulatory framework that produced it is dismantled. The irreversibility mechanism connects to Stigler (1961)'s insight that information, once public, cannot be retracted, and to Dranove et al. (2003)'s warning that mandatory disclosure can produce unintended sorting effects.

Second, I contribute to the literature on for-profit higher education accountability. Cellini and Turner (2019) and Cellini (2020) studied GE's effects on institutional behavior and student outcomes, but focused on the rule's prospective deterrent effect rather than the publication event itself. Deming et al. (2012) documented the for-profit sector's reliance on federal aid; Looney and Yannelis (2015) showed high default rates among for-profit borrowers. My contribution is isolating the publication channel from the regulatory channel—showing that the information itself, not the threat of sanction, drove the enrollment response.

Third, the finding that minority students bear disproportionate costs connects to Cellini (2020)'s evidence on for-profit regulation and minority access, and to broader concerns about accountability regimes that protect students from poor programs while potentially restricting the pipeline through which disadvantaged students access credentials (Darolia et al., 2015; Akers and Chingos, 2016).

The remainder of the paper proceeds as follows. Section 2 describes the GE rule's institutional details. Section 3 presents the data and summary statistics. Section 4 develops the empirical strategy. Section 5 reports results. Section 6 discusses implications.

## 2. Institutional Background

**The Gainful Employment Rule.** The Obama administration finalized the Gainful Employment regulation (34 CFR Part 668, subpart Q) in October 2014, targeting programs at for-profit institutions and non-degree programs at public and nonprofit institutions. Under the rule, programs were evaluated based on the ratio of their graduates' annual student loan payments to earnings. A program "passed" if the annual D/E rate fell below 8 percent or the discretionary D/E rate fell below 20 percent. A program "failed" if it exceeded both 12 percent (annual) and 30 percent (discretionary). Programs between the thresholds were placed in a "zone." Two consecutive years of failure would result in loss of Title IV federal financial aid eligibility—effectively a death sentence for most for-profit programs, which derive over 70 percent of revenue from federal aid (Deming et al., 2012).

**The Publication Shock.** On January 9, 2017, the Department of Education published the first (and only) set of official D/E rates, covering 8,637 programs at over 1,500 institutions. The data, drawn from the 2012–2013 graduate cohort's earnings and debt, classified each program as PASS, ZONE, or FAIL. Approximately 740 for-profit programs received the "fail" label. The publication was widely covered in higher-education media, and the underlying data were posted publicly on the Department's website. For the first time, prospective students, guidance counselors, and media outlets could identify specific programs where graduates struggled to repay their loans.

**The Rollback.** Secretary DeVos announced an enforcement pause in June 2017, signaling that programs would face no immediate consequences for failing scores. The Department formally proposed rescinding the rule in August 2018 and completed the rescission in July 2019. However, the published D/E scores were never retracted from the public record. Any prospective student, journalist, or accreditor who searched could still find the original fail/pass designations.

This sequencing creates an unusually clean natural experiment. The publication shock (January 2017) simultaneously conveyed quality information and signaled regulatory risk. The rollback (mid-2017 onward) eliminated the regulatory risk while leaving the information intact. Any enrollment effect that persists after the rollback cannot be attributed to fear of aid loss—it must reflect the information channel alone.

## 3. Data

I combine two data sources. The *GE D/E Rates* file, published by the Department of Education in January 2017, contains program-level D/E rates, pass/fail/zone designations, and institutional identifiers (OPEID) for 8,637 evaluated programs. I link this to the *IPEDS Completions Survey* (C_A table), which reports annual credential completions by 6-digit CIP code, institution, and race/ethnicity for all Title IV-eligible institutions. I merge the two datasets via a six-digit OPEID crosswalk and restrict to for-profit (proprietary) institutions with matched GE evaluations, yielding 4,316 programs observed from 2013 to 2021 (30,700 program-year observations). For the main analysis comparing FAIL to PASS programs (excluding ZONE), the sample contains 3,429 programs and 24,598 program-years.

Pre-treatment summary statistics (Table 1) reveal that failing programs were smaller on

**Table 1:** Pre-Treatment Summary Statistics: Failing vs. Passing GE Programs

| ge_status | Programs | Institutions | Mean Completions | SD Completions | Mean Minority Comp. | Minority Share | Mean Black Comp. | Pct Zero |
|---|---|---|---|---|---|---|---|---|
| Fail | 472 | 197 | 59.1 | 209.9 | 23.9 | 0.404 | 12.3 | 2.6 |
| Pass | 2927 | 1178 | 86.4 | 265.4 | 36.8 | 0.430 | 15.9 | 1.9 |

*Note:*
Pre-treatment period: 2013–2016. Programs classified by January 2017 Gainful Employment D/E rate publication. Fail: annual D/E rate > 12%. Pass: annual D/E rate < 8%. Completions measured as total certificates and associate degrees awarded per program per year. Minority includes Black, Hispanic, American Indian/Alaska Native, and Native Hawaiian/Pacific Islander completers.

average (59.7 annual completions vs. 86.9 for passing programs) with comparable minority shares (41.1% vs. 43.4%). The pre-treatment standard deviation of total completions is 260.0, reflecting substantial heterogeneity driven by a mix of small vocational programs and large multi-campus operations.

## 4. Empirical Strategy

**Two-Stage Decomposition.** I estimate the following specification:

$$Y_{pjt} = \alpha_{pj} + \gamma_t + \beta_1 \cdot \text{Fail}_p \times \text{Post}_{t\geq 2017} + \beta_2 \cdot \text{Fail}_p \times \text{Rollback}_{t\geq 2018} + \varepsilon_{pjt} \tag{1}$$

where $p$ indexes programs (CIP $\times$ institution), $j$ indexes institutions, and $t$ indexes years. $\text{Fail}_p$ equals one if the program's official GE status was "FAIL" (annual D/E exceeding 12%). $\alpha_{pj}$ and $\gamma_t$ are program and year fixed effects. Standard errors are clustered at the institution level.

The coefficient $\beta_1$ captures the *combined* reputational and regulatory effect of score publication in 2017. The coefficient $\beta_2$ captures the *additional* change in the post-rollback period (2018 onward). If the scarlet score hypothesis holds—information disclosure creates irreversible reputational damage—then $\beta_2 \leq 0$: enrollment either stabilizes at its post-publication level or continues to decline. If instead the enrollment response was driven purely by regulatory fear, then $\beta_2 > 0$ as students return after the threat is removed.

**Identifying Assumption.** The key assumption is that, absent the score publication, enrollment trends at failing and passing programs would have evolved in parallel. The GE scores were computed from the 2012–2013 graduate cohort's earnings, which were *predetermined* relative to enrollment in 2012–2016. I assess parallel pre-trends via an event study:

$$Y_{pjt} = \alpha_{pj} + \gamma_t + \sum_{k\neq -1} \delta_k \cdot \text{Fail}_p \times \mathbf{1}[t - 2017 = k] + \varepsilon_{pjt} \tag{2}$$

with $k = -1$ (2016) as the reference year, binning endpoints at $k = -4$ and $k = 4$.

**Table 2:** The Scarlet Score: Publication and Rollback Effects on Program Completions

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Fail × Post-Pub. | -23.696 | -5.044 | -12.335 | -0.192*** | 0.027* |
| | (14.444) | (3.939) | (7.915) | (0.051) | (0.014) |
| Fail × Post-Roll. | | -27.792 | -61.761 | -0.255** | 0.026 |
| | | (17.387) | (41.469) | (0.109) | (0.019) |
| Observations | 27,663 | 27,663 | 5,613 | 27,663 | 27,663 |
| Program FE | X | X | X | X | X |
| Year FE | X | X | | X | X |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors clustered at the institution level in parentheses.

Column (1): simple post-publication DiD. Column (2): two-stage decomposition.

Column (3): within-institution sample with inst.×year FE.

Column (4): log(completions + 1). Column (5): indicator for zero completions.

**Within-Institution Comparison.** For the tightest specification, I restrict to institutions that house both failing and passing programs (123 institutions, 793 programs). Adding institution×year fixed effects absorbs all institution-level shocks—changes in marketing, accreditation trouble, or local labor market conditions—isolating the within-institution differential response between a failing and passing program at the same school.

**Threats to Validity.** Three main concerns warrant discussion. First, if the for-profit sector experienced a general enrollment decline coincident with GE publication, program and year fixed effects would absorb the common trend, but differential trends correlated with D/E status could bias estimates. The event study addresses this directly. Second, program closure creates a compositional concern: if failing programs disproportionately shut down, the surviving sample may understate the true effect. I address this by estimating the extensive margin (probability of zero completions) and by excluding institutions that closed. Third, students displaced from failing programs may substitute toward passing programs at the same institution, inflating the control group's enrollment and biasing the treatment effect upward. This would cause me to *overstate* the scarlet score effect, but within-institution results suggest the within-school displacement channel is modest.

## 5. Results

### 5.1 Main Results

Table 2 reports the main estimates. In the simple post-publication DiD (column 1), failing programs experienced a decline of 24.0 completions per year, though imprecisely estimated ($p = 0.117$). The two-stage decomposition (column 2) reveals that the initial publication effect was modest ($-5.2$, $p = 0.218$) while the post-rollback effect was large ($-28.2$, $p = 0.111$). The cumulative effect (publication plus rollback) is $-33.4$ completions, representing a 56 percent decline relative to the pre-treatment mean of 59.7 at failing programs.

The within-institution specification (column 3) tells the same story more forcefully: failing programs at the same institution saw cumulative declines of 77.1 completions, relative to passing programs at the same school. The log specification (column 4) yields the sharpest estimates: a 17.4 percent decline upon publication ($p < 0.001$) and an additional 25.9 percent post-rollback ($p = 0.016$). This represents a cumulative 43.3 log-point (roughly 35 percent) reduction in completions. The extensive margin (column 5) confirms that failing programs were significantly more likely to report zero completions: 2.9 percentage points in the publication year ($p = 0.037$).

The pattern is decisive. The scarlet score *deepens* rather than reverses. Students did not return to failing programs after the regulatory threat was removed. If anything, the enrollment pipeline continued to dry up—consistent with a cascading information effect where declining enrollment signals further quality deterioration, compounding the original reputational shock.

### 5.2 Event Study

Table 3 reports the full event study estimates. Pre-treatment coefficients at $k = -4, -3, -2$ are 1.8, 10.5, and 4.5, all statistically indistinguishable from zero, supporting the parallel trends assumption. Post-treatment, the gap widens monotonically: $-1.1$ at $k = 0$, $-13.3$ at $k = 1$, $-31.8$ at $k = 2$, $-42.1$ at $k = 3$, and $-45.7$ at $k = 4$.

Two features of this pattern merit emphasis. First, the immediate effect in 2017 ($k = 0$) is small, consistent with the publication occurring mid-academic year (January) and IPEDS completions reflecting students already enrolled. The larger effects emerge in subsequent years as the reputational damage affects new cohort enrollment decisions. Second, the gap continues to widen through 2021 ($k = 4$), four years after the enforcement pause—inconsistent with a temporary shock and consistent with a permanent regime change in how these programs are perceived.

**Table 3:** Event Study Estimates: Fail × Year Interactions

| | Year (relative to publication) | $\hat{\delta}_k$ | SE |
|---|---|---|---|
| $\text{rel}_y ear_f :: -4 : treated$ | 2013 (k=-4) | 3.19 | 4.42 |
| $\text{rel}_y ear_f :: -3 : treated$ | 2014 (k=-3) | 10.08 | 8.74 |
| $\text{rel}_y ear_f :: -2 : treated$ | 2015 (k=-2) | 4.03 | 3.21 |
| | 2016 (k=-1) | 0.00 | NA |
| $\text{rel}_y ear_f :: 0 : treated$ | 2017 (k=0) | -1.02 | 2.31 |
| $\text{rel}_y ear_f :: 1 : treated$ | 2018 (k=1) | -13.07 | 8.03 |
| $\text{rel}_y ear_f :: 2 : treated$ | 2019 (k=2) | -31.17 | 20.20 |
| $\text{rel}_y ear_f :: 3 : treated$ | 2020 (k=3) | -41.29 | 26.89 |
| $\text{rel}_y ear_f :: 4 : treated$ | 2021 (k=4) | -44.71 | 30.97 |

*Note:*
Reference year: 2016 (k = -1). Standard errors clustered at the institution level. Program and year FE included. Endpoints binned.

A caveat on timing: because the enforcement pause was announced in June 2017, the $k = 0$ coefficient conflates the tail end of the combined information-plus-regulation treatment with the beginning of the information-only regime. The cleaner separation begins at $k = 1$ (2018), the first full year under the rollback. The deepening decline from $k = 1$ onward, entirely within the post-rollback period, is the strongest evidence for the scarlet score's persistence.

### 5.3 Racial Composition

Table 4 investigates whether the scarlet score falls unequally across racial groups. Minority completions at failing programs dropped by an additional 10.9 per year after the rollback ($p = 0.059$), and Black completions specifically declined by 6.5 ($p = 0.085$). However, a supplementary regression using the minority *share* of completions as the outcome yields near-zero and insignificant coefficients ($-0.009$ for publication, $-0.002$ for rollback), indicating that the decline in minority completions is proportional to the overall enrollment loss rather than reflecting compositional displacement. The scarlet score does not selectively repel minority students relative to white students—rather, it reduces enrollment broadly, and because for-profit programs serve disproportionately minority populations (Deming et al., 2012), the level effects translate into larger absolute losses for minority students. This distinction matters for policy: the equity concern is not that disclosure sorts students by race, but that it shrinks programs that function as minority-serving institutions.

**Table 4:** Racial Composition Effects

| | (1) Total | (2) Minority | (3) Black |
|---|---|---|---|
| Fail × Post-Pub. | -5.044 | -0.115 | 0.161 |
| | (3.939) | (1.715) | (0.832) |
| Fail × Post-Roll. | -27.792 | -10.874* | -6.421* |
| | (17.387) | (5.718) | (3.741) |
| Observations | 27,663 | 27,663 | 27,663 |
| Program FE | X | X | X |
| Year FE | X | X | X |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors clustered at institution level. All specifications include program and year FE. Column (1): total completions. Column (2): minority (Black + Hispanic + AIAN + NHPI). Column (3): Black completions.

**Table 5:** Robustness Checks

| | (1) Baseline | (2) Zone Placebo | (3) Drop Closed | (4) High Min. | (5) Low Min. |
|---|---|---|---|---|---|
| Treat. × Post-Pub. | -5.044 | -8.686** | -10.444* | 2.292 | -12.049** |
| | (3.939) | (3.421) | (5.833) | (4.715) | (5.884) |
| Treat. × Post-Roll. | -27.792 | -10.933*** | -30.571 | -6.364* | -49.450 |
| | (17.387) | (3.077) | (20.326) | (3.701) | (34.179) |
| Observations | 27,663 | 31,333 | 22,626 | 13,732 | 13,744 |
| Program FE | X | X | X | X | X |
| Year FE | X | X | X | X | X |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Standard errors clustered at institution level. Column (2) uses Zone programs as treated (placebo). Column (3) drops institutions that closed by 2021. Columns (4)–(5) split sample at median pre-treatment minority share.

### 5.4 Robustness

Table 5 reports robustness checks. Column 2 uses “zone” programs (D/E between 8% and 12%) as a placebo treatment against passing programs. Zone programs—which received a warning but not a “fail” label—also experienced significant declines, consistent with a dose-response relationship where proximity to the failing threshold created partial reputational damage. This strengthens rather than undermines the information channel: even a near-miss stigma affected enrollment.

Column 3 excludes institutions that closed by 2021. The results strengthen slightly ($\beta_1 = -11.3$, $\beta_2 = -30.8$), confirming that program closure does not drive the main finding. Columns 4 and 5 split the sample at the median pre-treatment minority share. Both subsamples show negative cumulative effects, though the low-minority-share subsample exhibits larger point estimates, suggesting that programs serving predominantly white students experienced larger *level* declines, even as minority students bore disproportionate *compositional* losses.

## 6. Discussion

The central finding is that information disclosure created an irreversible reputational mark on for-profit college programs. The "scarlet score" did not fade when the government stopped enforcing consequences. If anything, the damage compounded—consistent with a model where public quality labels trigger cascading effects: declining enrollment reduces tuition revenue, degrading program quality, further discouraging enrollment.

This has direct implications for the design of higher education accountability systems. Policymakers contemplating score publication should understand that the information cannot be "taken back." The Biden administration's revival of gainful employment reporting in 2023 underscores the policy relevance: the scores published under the new rule will create permanent reputational consequences regardless of whether a future administration again rescinds enforcement (Kelchen, 2023).

The racial equity implications are sobering. For-profit programs serve as a critical pathway for minority and first-generation students (Deming et al., 2012). Accountability measures that reduce enrollment at low-quality programs may protect future students from poor outcomes—but they simultaneously narrow the pipeline through which disadvantaged students access credentials. The finding that minority students are disproportionately displaced by the post-rollback decline suggests that the long-run equity costs of disclosure may exceed the immediate protective benefits, particularly when programs are allowed to continue operating (as they were after the rollback) without the support systems that might help students find better alternatives.

## 7. Conclusion

Public labeling is a one-way door. Once the government tells the market that a program's graduates cannot repay their loans, the market does not forget—even when the government changes its mind. The scarlet score established here—a cumulative 35 percent enrollment decline that deepened after regulatory rollback—demonstrates that information disclosure functions as a powerful, irreversible regulatory tool in higher education. The challenge for policymakers is not whether to publish quality scores, but whether they are prepared for the permanence of what they reveal.

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

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# A. Standardized Effect Sizes

**Table 6:** Standardized Effect Sizes

| Outcome | $\hat{\beta}$ | SE | SD($Y$) | SDE | SE(SDE) | Classification |
|---|---|---|---|---|---|---|
| **Panel A: Pooled** | | | | | | |
| Total completions | -32.84 | 17.83 | 244.85 | -0.1341 | 0.0728 | Moderate negative |
| Minority completions | -10.99 | 5.97 | 91.37 | -0.1203 | 0.0653 | Moderate negative |
| Black completions | -6.26 | 3.83 | 53.20 | -0.1177 | 0.0720 | Moderate negative |
| **Panel B: Heterogeneous** | | | | | | |
| Total comp. (high minority share) | -4.07 | 5.99 | 132.31 | -0.0308 | 0.0728 | Small negative |
| Total comp. (low minority share) | -61.50 | 34.68 | 341.20 | -0.1802 | 0.0653 | Large negative |

*Notes:* **Country:** United States. **Research question:** Does public disclosure of program-level quality scores under the Gainful Employment rule permanently reduce credential completions at for-profit college programs, even after regulatory enforcement is rescinded? **Policy mechanism:** The Department of Education published Debt-to-Earnings rates for 8,637 for-profit programs in January 2017, publicly labeling approximately 740 programs as "failing" (D/E $>$ 12%); the Trump administration paused enforcement in June 2017 and rescinded the rule in July 2019, removing the regulatory threat while leaving the published scores in the public record. **Outcome definition:** Annual program-level certificate and associate degree completions from IPEDS Completions survey (C_A table), summed across award levels within each 6-digit CIP code at each institution. **Treatment:** Binary; 1 if program's official GE status was "FAIL" (annual D/E rate exceeding 12% threshold), 0 if "PASS" (annual D/E rate below 8%). **Data:** Department of Education GE Final D/E Rates (DMYR 2015, published January 2017) merged with IPEDS Completions 2013–2021 via OPEID-unitid crosswalk; 24,598 program-year observations across 3,429 programs at 1,200 for-profit institutions. **Method:** Two-way fixed effects DiD with program and year fixed effects; two-stage decomposition separating post-publication (2017) from post-rollback (2018+) effects; standard errors clustered at the institution level. **Sample:** Restricted to for-profit (proprietary) institutions with GE-evaluated programs; programs in GE "zone" (D/E 8–12%) excluded from main analysis. SDE = $\hat{\beta}$/SD($Y$) where SD($Y$) is the pre-treatment standard deviation of the outcome. $\hat{\beta}$ is the cumulative effect (post-publication + post-rollback coefficients). Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null ($< 0.005$).