Universal Admission Exams and Changes in Selective U.S. College Enrollment: Evidence from a State-Level Synthetic Control Analysis

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Why do we care?

- **U.S. college undergraduate enrollment has stagnated** or declined since 2011, reversing decades of increases.
- Gaps by income and race remain large, with low-income students disproportionately underrepresented in higher education (Bailey & Dynarski 2011).
- Access barriers include *behavioral frictions*, notably the need to opt into standardized admissions testing. The process is shaped by effort costs and imperfect information (Klasik, 2013).
- Unlike in Nordic countries, **U.S. students must typically pay to take college entrance exams** (ACT/SAT) outside regular school hours.
- In 2016, **Minnesota made exams free and mandatory during school hours**, bringing the U.S. model closer to Nordic-style access.
- This created a a low-cost, universal policy "nudge," **ideal for testing modern synthetic control methods** (SCM's).

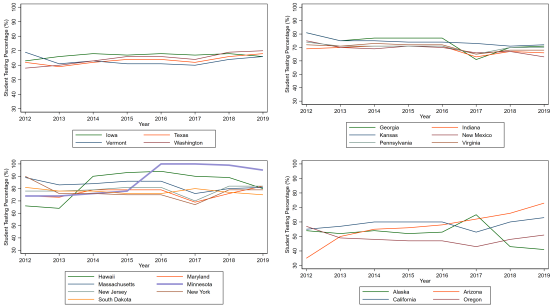
Research question

Does universal *in-school* college entrance exam testing
increase first-time, full-time undergraduate enrollment
at in-state schools and in which postsecondary sectors?

Design and Identification Strategy

- **Panel:** 21 units, state-year data from 2011–2019.
- **Treatment setup:**
  - **Treated unit**: Minnesota, *post-2015 cohort* (2016–2019).
  - **Donor pool**: 20 states that **never** reach ACT/SAT participation $\geq$ 95%.
  - **Treatment indication**: Continuous testing percentages collapsed to binary for the SCM. 1 if ACT/SAT coverage $\geq$ 95%, 0 otherwise.
- **Outcome:**
  First-time, full-time undergraduate enrollment at in-state institutions, aggregated across public and private nonprofit 2- and 4-year sectors.
- **Synthetic Control Method:**
  Constructs a *weighted combination of untreated states* from the donor pool that best matches Minnesota's pre-treatment outcomes. Post-2016 divergence of synthetic from actual then identifies the causal effect.

Figure 1: Maximum participation rates by state



Methodological Framework advancements of Abadie and Zhao (2023)

1. **Estimation:** Constructing the Synthetic Control

- Build synthetic control *by matching pre-treatment* outcomes (anchors) and covariates *using unit weights* (w matrix)
- Choose predictor weights (V matrix) to minimize discrepancy between treated and synthetic in predictor space

2. **Inference:** Design-Based Permutation Testing (Neyman–Rubin logic)

- Re-estimate w^* for each placebo to ensure valid fit comparisons
- Use *rank-based* inference: treated unit's MSPE ratio is compared to placebo ranks
- † Tested placebo inference using both positive-weight-only donors and full donor pool (filtered by pre-MSPE ≥ 2)

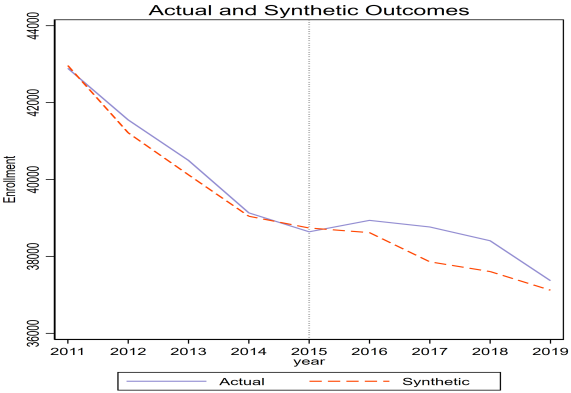
3. **Robustness:** Diagnostic Checks

- Conduct leave-one-out tests to assess sensitivity to individual donor units
- Examine resulting post/pre MSPE ratios and placebo distribution dispersion of the LOO analysis
- If full results within the band of dispersion results are robust

Main Results: Overall enrollment

- **Point estimate:**
  1.5% increase in levels for first-time, full-time enrollment (+568 students/year).
- **Pre-treatment fit quality:**
  Pre-RMSPE = 199; $R^2 = 0.98$ (partitioned!)
  Synthetic control tracks Minnesota well pre-policy.
- **Divergence and inference:**
  Post/pre $MSPE = 10.3$, but placebo rank $\approx 5/9$:
  Gap is visible yet not statistically significant.
  (Note: MSPE = 13.2 without 2019 where ACT coverage fell from 99% to 95%))
- **Robustness:**
  Positive treatment effect within band in all four post years and under nearly every donor exclusion test.
- **Take-away:**
  Pattern aligns with the 2016 mandate, but limited statistical power prevents firm causal claims.

Figure 2: Main Results



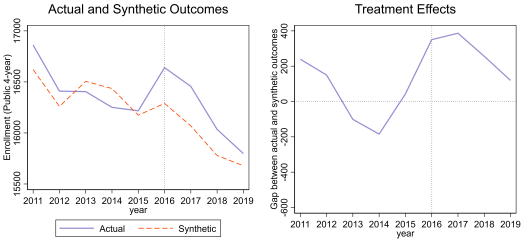
Notes: Panel shows actual enrollment versus the synthetic counterfactual for Minnesota from 2011 to 2019. The vertical dotted line marks the 2015–16 policy implementation. Estimates are derived using the synthetic control method implemented via `synth2` in Stata.

Sector Results - Enrollment by Institution Type

Summary:

- **Public 4-year**:
  - 350 in 2016 ($p = 0.18$), 387 in 2017 ($p = 0.27$)
  - Suggestive early gains aligned with policy rollout
- **Private not-for-profit 4-year**: (best model fit)
  - 272 in 2018 ($p = 0.18$), 461 in 2019 ($p = 0.09$)
  - Delayed but increasingly pronounced divergence
- **Public 2-year** (w/o Alaska and Vermont):
  - Effect fluctuates from +379 (2017) to -306 (2019)
  - All p-values ≥ 0.50: Sign reversal in last year

Figure 3: Public 4-year Institutions



Sector Results - Enrollment by Institution Type

Figure 4: Private 4-year Institutions

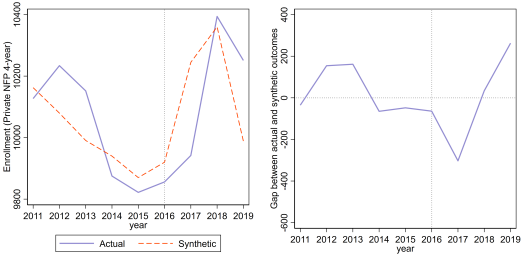
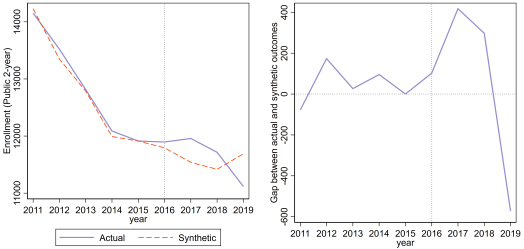


Figure 5: Public 2-year Institutions



Conclusion

- **Interpretation** A modest, enrollment increase is observed, persistent across donor exclusions but inference remains weak under rank-based placebo testing given shrinking availability of eligible control pool. SCM can struggle if effect size close to variability.
- **Comparative context**
  - Minnesota's $\approx$ 0.80 pp (1.5% in levels) estimate is modest relative to earlier mandates
  - $\approx$ 2-3 pp in Colorado Illinois (Goodman 2016)
  - $\approx$ 6-18 pp shift toward selective colleges in early SAT/ACT states (Klasik 2013)
  - $\approx$ 2-3 pp in Maine (Hurwitz 2015)
  - $\approx$ 2 pp in Michigan (Hyman 2017)
  - and $\approx$ 1.5 pp in Wisconsin (Smith and Hirschl 2022).

  The similarity to Wisconsin reinforces the plausibility of a modest effect in high-participation states with limited selective capacity even if significance can not be stated here.
- **Policy relevance** Universal, in-school testing may expand access at low marginal cost, especially where baseline testing is low and selective public or nonprofit capacity exists. Gains may depend on pairing with guidance or aid information.

Questions?

Caveats

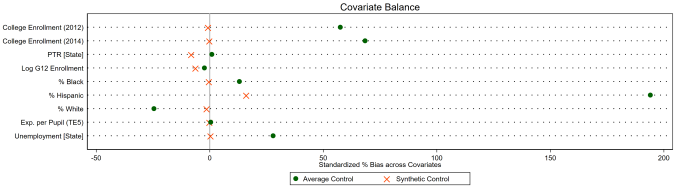
1. Mandate was part of broader "World's Best Workforce" reforms *(risk of policy bundling)*.
2. Five-year pre-treatment window limits model flexibility and precision *(weak convex hull risk)*.
3. Donor pool is small and constrained by near-universal testing states *(low treatment contrast)*.
4. In-state enrollment not residency-identified in IPEDS *(imperfect exposure measurement)*.
5. Baseline ACT participation already high (78%) *(limited intensive margin)*.
6. Predictor set heavily weighted toward lagged outcomes *(risks overfitting trends rather than matching structure)* *(only modeling latent/unobserved factors)*

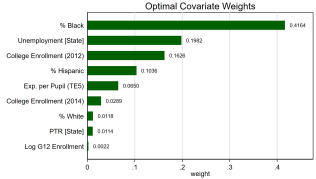
Choice of Covariates

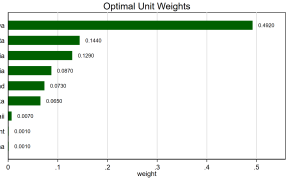
- Cohort size
- Racial demographics (Black, Hispanic & White)
- PTR, per-pupil spending (*quality and resource intensity*)
- Unemployment (*outside option*)
- *College Enrollment* (Pre-period outcome anchor/s)

3 dropped from Klasik (2013):

- High school graduation rate (not in ELSI)
- Entrance exam residual (ACT/SAT data switched to percentile reporting in period)
- Sector % of institutions in state (would make main-sector comparisons incompatable)







Tables from Thesis

Table 1: Fit measures & average outcomes

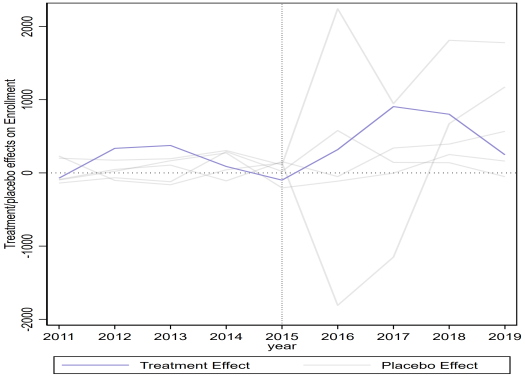
Outcome	Pre-RMSPE	MSPE Ratio	Mean ATE	R^2	Signif. Years	Placebo N
All undergraduate	198.7	10.29	568.1	0.977	0/4	9
Public 4-year	156.2	3.61	278.2	0.047	1/4	10
NFP 4-year	110.9	5.95	190.0	−0.011	2/4	10
Public 2-year	113.5	6.27	101.9	0.984	0/4	3

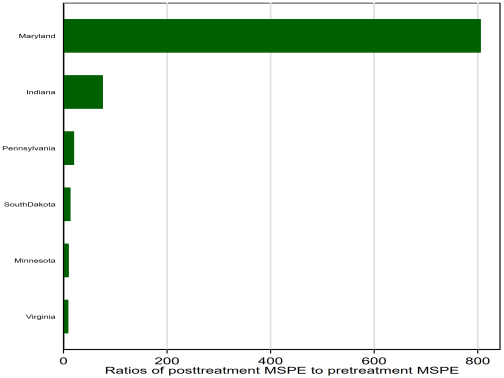
Table 2: Annual Treatment Effects with right-sided p-values

Year	All	Public 4-year	NFP 4-year	Public 2-year[1]
2016	+318 (0.50)	+350 (0.18)	+67 (0.36)	+44 (0.75)
2017	+906 (0.33)	+387 (0.27)	−40 (0.36)	+379 (0.50)
2018	+801 (0.33)	+256 (0.36)	+272 (0.18)	+290 (0.75)
2019	+247 (0.67)	+120 (0.55)	+461 (0.09)	−306 (0.75)

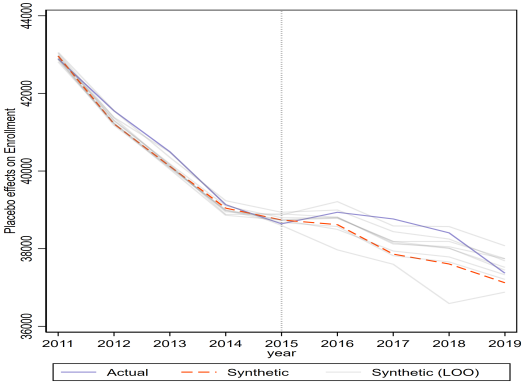
[1] Alaska and Vermont are excluded from the donor pool due to missing outcome data in key years, with Alaska showing post-treatment gaps and Vermont consistently failing data checks.

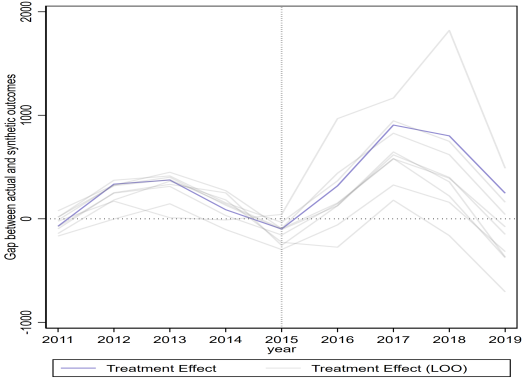
Main - Placebo MSPE ratios



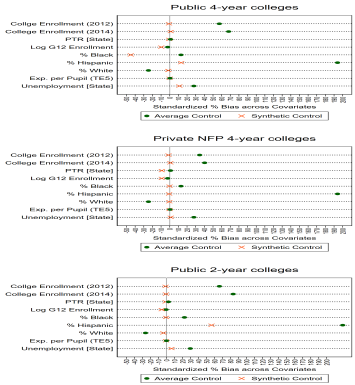


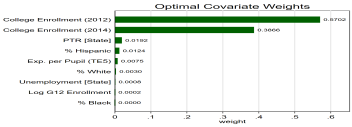
Main - Leave-one-out robustness

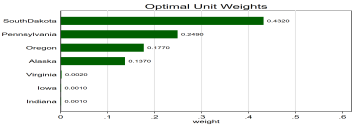


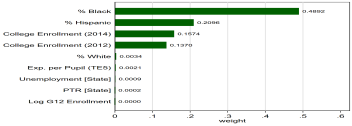


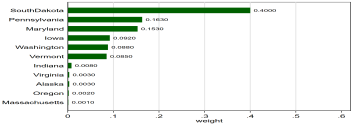
Sector - Covariate Balance Table

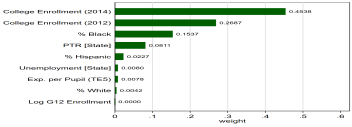


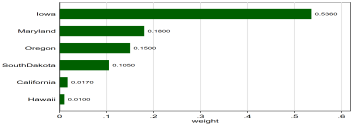




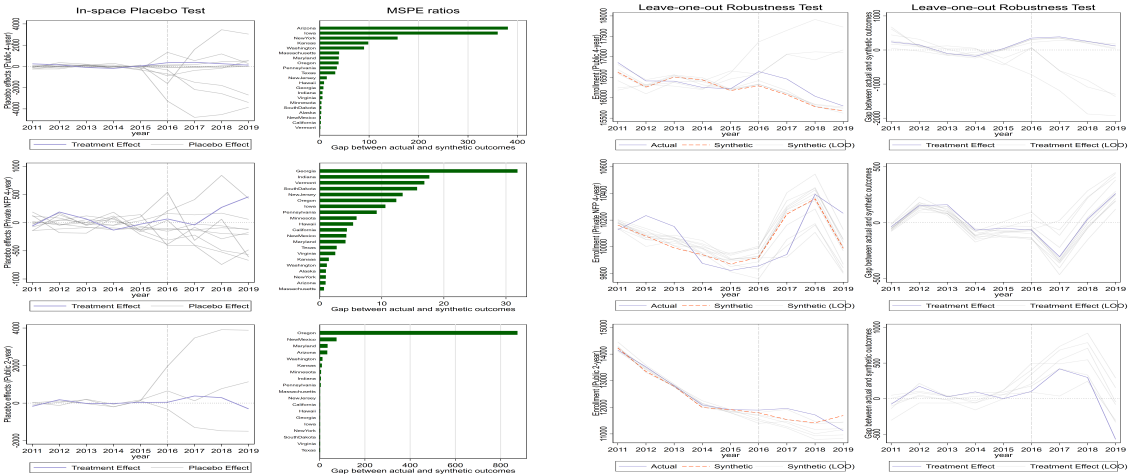








Sector - ISP and LOO



Treatment Effect Converted to PP

- While this study reports effects as level percent changes in enrolled undergraduate students, following standard practice in the synthetic control literature, the table below converts these to percentage point (pp) changes to facilitate comparison with prior studies.

Table 3: Estimated Treatment Effects: Levels and Percentage Points

Year	Grade 12 Students	Real College Enrollment	Effect (Level)	Effect (pp)
2016	69,441	38,938	318.32	0.46
2017	70,215	38,766	905.69	1.29
2018	70,833	38,408	801.29	1.13
2019	72,172	37,369	247.19	0.34
Mean	70,665	38,370	568.12	**0.80**

Equations (a) - Methods of Estimation

Synthetic control weights (unit weights)

$$w^*(V) = \arg\min_{w} \|x_1 - X_0 w\|_V \quad \text{s.t.} \quad w \geq 0, \quad 1'w = 1 \tag{4.1}$$

Predictor weight selection (covariate weights)

$$V^* = \arg\min_{V} \|Z_1 - Z_0 w^*(V)\|_2^2 \tag{4.2}$$

Synthetic control treatment effect estimate

$$\widehat{\Delta}_{1t} = y_{1t} - \widehat{y}_{1t}^0 = y_{1t} - \sum_{j=2}^{N+1} w_j^* y_{jt} \tag{4.3-4.4}$$

Equations (b) - Methods of Inference

Pre-treatment fit (Mean Squared Prediction Error)

$$\text{MSPE} = \frac{1}{T_0}\sum_{t=1}^{T_0}\left(y_{1t} - \widehat{y}_{1t}^{0}\right)^2 = \frac{1}{T_0}\sum_{t=1}^{T_0}\left(y_{1t} - \sum_{j=2}^{N+1} w_j^* y_{jt}\right)^2 \tag{A.1}$$

Right-sided p-values

$$p_t^{\text{right}} = \frac{1}{N^\dagger + 1}\sum_{i=1}^{N^\dagger+1} 1\left(\hat{\Delta}_{it} \geq \hat{\Delta}_{1t}\right) \tag{A.2}$$

MSPE ratio-based permutation p-value

$$p^{\text{MSPE}} = \frac{1}{N^\dagger + 1}\sum_{i=1}^{N^\dagger+1} 1\left(\frac{\text{MSPE}_{i,\text{post}}}{\text{MSPE}_{i,\text{pre}}} \geq \frac{\text{MSPE}_{1,\text{post}}}{\text{MSPE}_{1,\text{pre}}}\right) \tag{A.3}$$

Equations (c) Structural Assumption of the SCM: Linear Factor Model

Abadie and Zhao (2023): Core Identifying Assumption

$$Y_{jt}^{N} = \delta_t + \theta_t' Z_j + \lambda_t' \mu_j + \varepsilon_{jt} \tag{1}$$

- Z_j: observed covariates (e.g., demographics, inputs)
- μ_j: unobserved latent factors (e.g., motivation, culture)
- θ_t, λ_t: time-varying factor loadings
- δ_t: common time effect, ε_{jt}: idiosyncratic shock

Adapted from Abadie (2020), "Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects." Equation 10, p.12.