Abstract: In 2015, Idaho adopted a direct admissions (DA) system, proactively admitting all high school graduates to a set of the state’s public colleges and universities based upon a combination of ACT/SAT scores, GPA, and course credits. This reimagining of the admissions process held the potential to reduce informational and social-capital barriers toward college enrollment, including the possibility to improve access across geographic, racial, and socioeconomic contexts. Using complementary difference-in-differences and generalized synthetic control methods, we estimate causal effects of DA on state and institutional enrollment outcomes. While Idaho enjoyed increased enrollment following the introduction of DA, we do not find consistent evidence to suggest DA led to increased FTE, undergraduate, or in-state enrollments nor to suggest DA improved the enrollment of Pell-eligible or racial-minority students. We discuss these null findings in relation to the state’s demographic population and consider the implementation of this policy across other state contexts. (JEL: H75, I21, I24, I28)

Keywords: college admissions; difference-in-differences; direct admissions; education policy; higher education; synthetic control
Introduction

While the individual returns to a postsecondary credential have been well documented (Chetty et al., 2017; Oreopoulous & Petronijevic, 2013; Webber, 2014), state economies also deeply benefit from an educated populous. The average four-year graduate is 24% more likely to be employed and earns approximately $32,000 more annually (over $1 million across a lifetime), contributing a disproportionately higher share toward states’ tax revenues (Abel & Deitz, 2014). In addition to these economic advantages, higher education provides considerable nonmonetary benefits for society (McMahon, 2009), including improvements in public health, civic engagement, and charitable giving, as well as reduced incarceration rates (Ma et al., 2019; Trostel, 2015). Fundamentally, an educated workforce drives a state’s economic competitiveness and prosperity, and, given a rapidly growing need for a more educated workforce (Carnevale et al., 2013), states have sought public policy options to increase access to higher education. Despite this, few have managed to meaningfully improve the stagnant college-going rates of high school graduates. In the face of constrained resources and competing spending priorities (Barr & Turner, 2013; Doyle & Zumeta, 2014), many states seek viable, low-cost policies to support college enrollment and increase educational attainment.

Both states and their students face considerable challenges in the college enrollment process. Not all students who would benefit from college enroll in postsecondary education, and states’ levels of postsecondary attainment ranged from only 36.4% of the working-age population in West Virginia to 56.8% in Massachusetts for 2017 (Lumina Foundation, 2019). At the individual level, students most notably face information constraints (Dynarski & Scott-Clayton, 2006) and declining affordability (Doyle, 2016). Even for those who do enroll, gaps in persistence and attainment vary by income (Deming & Dynarski, 2009), race (Baker et al.,
2018), and geography (Hillman, 2016). States too face significant challenges supporting higher education, including competing state budget priorities such as K-12 education, healthcare, prisons, and pension systems (Delaney & Doyle, 2018; Gunter et al., 2002). Though states fiscally support public and private higher education in different ways, appropriations per student (inflation adjusted) have declined since 2001 (SHEEO, 2019). In fact, the average educational appropriations per full-time equivalent (FTE) enrollment in fiscal year 2018 were just 80.4% of what they were in at the beginning of the century. Though overall improvements to higher education appropriations in state budgets have occurred across the past six fiscal years (17.4% per FTE), higher education appropriations remain volatile in many states (Doyle et al., 2018), and some have yet to recover from the Great Recession.

To increase educational attainment and reduce common enrollment barriers for students, states have employed many policy options including, targeted information mechanisms (Bettinger et al., 2012; Castleman & Page, 2015) and broadening access to financial aid (Bettinger et al., 2019; Castleman & Long, 2016). A notable development spanning each of these realms has been the proliferation of place-based (or “promise”) scholarship programs (Perna & Leigh, 2018; Perna & Smith, 2020). To date, however, few states have managed to politically and fiscally develop large-scale promise programs, suggesting an emerging need for a viable, low-cost state policy alternative to support college enrollment. In particular, states need low-cost policy innovations that address college access by advancing equity; increasing enrollment while reducing persistent gaps in access and attainment across geography, race, and class. A direct admissions (DA) system is one such possible policy innovation.

A DA system side-steps the typical college admissions process by proactively admitting students to college based on a data match between K-12 schools and postsecondary institutions.
Typically, all students in a state operating DA are admitted to open-access institutions, while students who surpass a pre-identified threshold based on high school academic performance, such as GPA, standardized test scores, class rank, or a combination of measures, are also admitted to selective institutions. Students, parents, and high schools receive letters indicating a student has been admitted to a set of institutions and outlines steps for how students can “claim their place” using a common and free application. In fall 2015, Idaho was the first state to adopt this reimagination of the college admissions process, proactively admitting all graduates in the high school cohort of 2016 to a set of the state’s public colleges and universities (Howell, 2018).

**Higher Education in Idaho**

Idaho’s public education sector has a centralized governance structure, with the Office of the State Board of Education (OSBE) overseeing K-12 and postsecondary operations, including 4 public community colleges and 4 universities. OSBE reported that 48% of the high school senior class of 2017 immediately enrolled in college following graduation and that the statewide six-year college graduation rate was 36.4% in 2017 (OSBE, 2018). As of that date, Idaho lagged the nation in college attainment. Nationally, 47.6% of the population held a high-quality certificate, associate degree, or bachelor’s or higher in 2017. In Idaho, only 40.7% of the population met those criteria (Lumina, 2019). Furthermore, significant gaps in degree attainment by race and geography persist in the state: 39.9% of white adults age 25 to 64 hold at least an associate degree compared to only 15.1% of Hispanic residents, and residents of large metropolitan areas have attainment rates 3.5 percentage points higher than their rural peers (Lumina, 2019). After being identified as the state with the lowest college-going rate in 2010 (NCHEMS, n.d.), Idaho set an ambitious postsecondary attainment goal: By 2025, 60% of Idahoans between the ages of 25 and 34 will attain a postsecondary degree or credential (OSBE,
2017). Since then, Idaho has aggressively pursued the 60% attainment goal, including spending over $8.6 million on college and university student success strategies (Richert, 2017, 2018). To support this goal, the state also adopted a notable college-access initiative: direct admissions.

**Direct Admissions in Idaho and Beyond**

Relying upon a combination of students’ ACT/SAT scores, unweighted GPA, and high school course credits, Idaho’s DA system proactively admitted all students in the high school cohort of 2016 and later to either all 8 state institutions (including the selective Boise State University, Idaho State University, and University of Idaho) or to the remaining 5 institutions (College of Eastern Idaho, College of Southern Idaho, College of Western Idaho, Lewis-Clark State College, and North Idaho College) plus Idaho State University’s College of Technology. After determining eligibility with the state’s longitudinal data system (housed at OSBE), which matched students’ K-12 records to the eligibility criterion, students, parents, and high schools received letters in the fall of a student’s senior year informing them of admission. To enroll, students needed to only select a college and formally submit an application and transcript to “claim their place.” Idaho’s DA system had explicit goals to promote a college-going culture; connect students, families, and K-12 schools with colleges early in the college-choice process; ease the transition from high school to college; signal postsecondary opportunities to high school students; and reverse consistent enrollment declines at the state’s public institutions (Howell et al., 2019; Kelly, 2018). Since fall 2015, nearly 87,000 Idahoans have been guaranteed admission to five or more state institutions, with the first cohort entering college in fall 2016 (Howell, 2018; OSBE, 2020).

Early indicators of Idaho’s success with DA are promising, including reported increases of 3.1% in overall enrollment and 88% in college applications, as well as a 3-percentage-point
DIRECT ADMISSIONS

decrease in the number of students leaving Idaho for college (Kovacs, 2016). Further, DA has shown to be exceptionally low-cost, only requiring the existing state longitudinal data system and paper and postage for acceptance letters, which are now sent electronically. This is contrasted with other interventions seeking to increase college access and equity, such as traditional grant-aid programs, mentoring, or wrap-around services requiring high levels of financial support (Page & Scott-Clayton, 2016). Given the high-yield, low-cost potential of DA systems, this practice has already begun to spread across the nation. South Dakota began proactive admissions for the high school class of 2018 (Gewertz, 2017; South Dakota Department of Education, 2019), and, in 2019, the Illinois General Assembly passed a law (Illinois Public Act 101-0448) to develop a pilot program to automatically admit high-performing high school graduates to targeted public institutions beginning in 2020-21.

While there is still a need to investigate informational, financial, and social-capital issues beyond college admissions in the education production function (Monk, 1989), DA system draws upon rich underpinnings in behavioral economics and may change the life course of individuals by offering postsecondary opportunities.

**Literature Review**

Conceptually, simplification of the college application process benefits students by removing barriers to access (DesJardins et al., 2006; Perna, 2006). More than half (53%) of high achieving, low-income (HALI) students do not apply to selective institutions (Hoxby & Avery, 2013). In fact, only 8% of HALIs apply in a manner like their higher-income peers, and current widely-used college admissions policies—like admissions staff recruiting, campus visits, and access programs—are typically ineffective with these students (Hoxby & Avery, 2013). Given this phenomenon, interventions seeking to reduce “frictions” students experience in the college
application and search process (e.g., detailed information, fee waivers, application assistance, and financial aid) have been adopted and shown to increase the likelihood of college application and enrollment (Knight & Schiff, 2019). A universal policy eliminating barriers for college admission, like DA, is likely to be transformational for underserved student populations, including those who are low-income, first generation, from rural areas, and students of color.

A growing body of experimental evidence has highlighted the role of information and encouragement in the college admissions process, particularly among students from low-income families. An experiment using low-cost interventions for college admission—including college application fee waivers—for HALI students demonstrated increases in the number of applications submitted overall and to selective institutions, a 31% increase in admissions to a selective institution, and increased enrollment at selective institutions (Hoxby & Turner, 2013). A recent study of Texas’s automatic admissions policy showed that the policy reduced income-based inequities by helping HALI students better match to higher-quality institutions (Cortes & Lincove, 2019). Dynarski et al. (2018) also provide evidence on information and financial aid for improving low-income students’ college trajectories. Students in their study received personalized mailing encouraging them to apply to University of Michigan (UM) and were guaranteed four years of tuition and fees if admitted—without having to complete the FAFSA or other aid forms. Similar to DA systems, students’ parents and principals also received this information. The authors found 67% of high school students in the treatment group applied to UM compared to 26% of the control group, and 27% enrolled compared to only 12% of the control group. These students likely would have otherwise not attended college, attended a community college, or attended a less selective four-year college (Dynarski et al., 2018).
When considering how DA may impact individual students, it is important to consider how the policy may change a student’s postsecondary destination across the ability spectrum. It is possible that, for some students, DA will yield no change in their behavior (e.g., students who are already college-bound). For others, however, knowing they have been admitted to college could change their life course—particularly among students from groups traditionally underrepresented in higher education. This has already been shown to be true for some HALI students (Dynarski et al., 2018), but should equally apply to all students. Because access to information about college admissions is dependent on social and cultural capital under traditional admissions models (Hoxby & Avery, 2013; Klasick, 2012; Knight & Schiff, 2019), students from certain groups, such as low-income, minority, and rural students, tend to be underserved.

At the institutional level, students receive differential access to degree programs and opportunities dependent upon where they enroll. Because not all institutions offer all majors or degree types, changing the type of institution attended can alter an individual’s life course. Graduation rates and support services offered to students on campuses vary greatly, and, in general, more selective institutions spend larger amounts per-student on instruction and have higher graduation rates (Alon & Tienda, 2005). In 2008, on average, the subsidy value (spending beyond net tuition on each FTE student) was 8% higher at public research institutions than at community colleges ($8,055 vs. $7,404) (Delta Project, 2010). Moving from a community college to a four-year institution given automatic admission opens the possibility for a student to earn a bachelor’s degree from their first institution. Similarly, moving from a less-selective to a more-selective institution generally improves a student’s odds of graduating and earning a degree—while also increasing the likelihood a student has access to greater resources and support services on campus.
In all, DA may be a viable, low-cost state policy alternative to support college enrollment across racial, socioeconomic, and geographic contexts. Direct admissions may not only sidestep the traditional admissions process by proactively admitting students to college, but may also eliminate many reasons why students do not apply to begin with—including onerous application forms (Knight & Schiff, 2019), inconsistent admissions processes across institutions, application fees, and a lack of transparent information for students and families (Page & Scott-Clayton, 2016). A proactive admissions system may also signal to students, parents, and K-12 schools that a postsecondary credential is attainable (Venezia & Jaeger, 2013), particularly for low-income and racial minority students who do not apply to or enroll in higher education at the same rates as their higher-income and majority peers (Hamrick & Stage, 2004; Institute of Education Sciences, 2018). For these reasons, DA may also hold the potential for states to increase undergraduate enrollment while keeping students in-state and invigorate workforce development by educating a larger and more diverse populous (Carnevale et al., 2013; Frey, 2015).

Research Questions

Given Idaho’s explicit intention to increase college enrollments with the adoption of a DA system, our study is guided by the following research question: What effect did the adoption of direct admissions have on postsecondary enrollment in Idaho? We explore enrollment changes at the state level, as well as by institution—even further disaggregating our analysis by institutional level. Further, given DA’s potential to reduce gaps in college enrollment across geographic, racial, and socioeconomic lines, we also estimate causal effects of DA on enrollment across these contexts by asking: Did direct admissions increase the enrollment of in-state, racial-minority, or Pell-eligible students across Idaho’s public institutions?

Data
For both levels of analysis, we draw upon data from a variety of sources covering academic years 2010-11 through 2017-18. State-level data cover all 50 states and include information from the American Community Survey, the Bureau of Labor Statistics, the U.S. Department of Education’s Common Core of Data, and the State Higher Education Executive Officer’s Association (SHEEO) State Higher Education Finance (SHEF) survey. Our primary state outcome of interest is full-time equivalent (FTE) enrollment in public higher education, drawn from SHEF. This source allows us to view aggregate changes in postsecondary enrollments all across states and over time. SHEEO annually collects the aggregate number of enrolled credit hours by state and scales these figures by 12-credit hours to arrive at a comparable number of full-time, academic year students (excluding medical school enrollments). From the other sources, we collect a host of time-variant controls associated with postsecondary enrollment, including financial (net tuition and fee revenue and state appropriations for higher education), economic (statewide Gini coefficient and unemployment rate), and both demand- and supply-side indicators (high school and bachelor’s degree or above educational attainment rates and the size of the K-12 high-school senior population).

Institution-level data were drawn exclusively from the U.S. Department of Education’s Integrated Postsecondary Education Data System (IPEDS). Beginning with the universe of all public two- and four-year (non-military) institutions in the United States, we limited our sample to those institutions who are degree-granting, have first-time full-time undergraduate students, and do not exclusively provide distance education courses. We employed linear interpolation for any missing outcome or covariate values to preserve sample size, but institutions that did not report information for at least two years to allow for interpolation were dropped—as well as institutions who did not report to IPEDS at any time across the 2010-11 through 2017-18 panel.
Our final analytic sample consists of 1,515 institutions across eight academic years. Institutional data cover our four primary outcomes of interest: (1) first-time degree/certificate-seeking undergraduate enrollment, (2) the number of first-time undergraduates from in-state, (3) the percent of undergraduate enrollment that is a racial minority,\(^1\) and (4) the percent of full-time first-time undergraduates awarded Pell grants. These outcomes allow us to view institution-level changes in overall undergraduate enrollments over time, as well as changes across geographic, racial, and socioeconomic contexts. We also collect time-variant controls associated with college enrollments, including the tuition and fee rate, state appropriations, expenditures on scholarships and fellowships, and the graduation rate.

For both the state- and institution-level datasets, we adjusted all financial variables to the Consumer Price Index (CPI) for the last fiscal year in the panel (2018) and logged outcomes and predictors with skewed distributions.\(^2\)

**Empirical Strategy**

Our study relies upon two complementary estimation strategies to estimate the effect of DA on college enrollment. First, for both state and institution-level outcomes, we estimate a traditional difference-in-differences (DID) model. Second, given the presence of one treated state with eight public institutions, we employ the generalized synthetic control method (GSCM) to optimally weight control units so that differences in pre-treatment outcomes are minimized—producing a strong counterfactual to estimate the effect of DA.

*Difference-in-Differences*

---

\(^1\) Here, racial minority status is defined as students who are not white. We appreciate the implications of this choice but are significantly limited by the demographic realities of the state of Idaho—where 76% undergraduates across the state’s eight postsecondary institutions from 2010-11 through 2017-18 were white.

\(^2\) For state-level models, we log the outcome (FTE) and the following predictors: tuition and fee revenue, state appropriations, and the high-school senior population. For institution-level models, we log two outcomes (undergraduate enrollment and in-state enrollment) and three predictors (tuition and fee rate, state appropriations, and scholarship expenditures).
Given the universal and fixed adoption of DA first impacting academic year 2016-17, a traditional quasi-experimental DID method can be used to estimate the effect of DA on our outcomes of interest. The DID estimator exploits across-unit and inter-temporal variation in our outcomes of interest as we compare outcomes in Idaho to all other (i.e., control) states or institutions over time and across the DA adoption window (Angrist & Pischke, 2009; Imbens & Wooldridge, 2009). A DID method is a preferred estimation strategy when assessing the effects of fixed-time policy adoptions because its ability to addresses concerns of selection and omitted variable bias (Cellini, 2008). Because selection of an optimal comparison group is empirically unguided, we present institution-level estimates comparing institutions in Idaho to three separate groups: all other institutions in the nation, those in the 15-state Western Interstate Commission for Higher Education region (where Idaho is a member), and those in states geographically contiguous to Idaho. These samples range from the entire population of other institutions to only those in neighboring states who were not exposed to DA but are likely similar on many observable and unobservable factors related to college enrollment. For state-level models, we only compare Idaho to all other states given a small sample size.

Formally, to estimate the effect of DA in Idaho with DID, we specify:

\[
Y_{it} = \alpha_0 + \beta (\text{Direct Admissions}_{it}) + \delta X_{it} + \pi_i + \rho_t + \varepsilon_{it},
\]

where \(Y_{it}\) is the state or institutional outcome of interest for unit \(i\) in year \(t\), conditioned on unit \((\pi_i)\) and year \((\rho_t)\) fixed effects. \(X_{it}\) is a time-varying vector of the control variables described above. Direct Admissions\(_{it}\) is a binary indicator identifying Idaho (or institutions in Idaho) in post-DA years, taking the value of 1 for Idaho (or institutions in Idaho) for 2016-17 and later or

---

$0$ otherwise. $\beta$ is the parameter of interest and the estimated causal effect of DA on $Y_{it}$. We estimate equation (1) with heteroscedastic-robust standard errors clustered at the unit level. Finally, in addition to state and institution-level models, we also disaggregate our institutional sample by level, examining effects of DA on two- and four-year institutions separately.

The primary assumption embedded within our DID estimation strategy is that the treatment and control groups exhibit parallel trends in the absence of DA. While we cannot formally test this assumption in the potential-outcomes framework (Angrist & Pischke, 2009; Rubin, 2005), we present parallel trends in Figure 1 for state-level FTE and Figure 2 for institution-level enrollment outcomes. In aggregate, trends of Idaho (and institutions in Idaho) follow those of their three counterfactual peers in pre-DA years, with deviations only observed in the post-DA period. Following the implementation of DA, we observe descriptive increases in FTE, undergraduate, and in-state enrollments. This adherence to the parallel-trends assumption suggests our DID estimator should be an appropriate empirical strategy. Additionally, as presented below in our Robustness section, we also perform an event study analysis to test for treatment effects in the absence of actual treatment (i.e., a placebo test for significant effects prior to 2016-17) and to test for the magnitude of differences following DA adoption.

**Generalized Synthetic Control Method**

Given our ability to derive long panels from our state and institutional datasets covering many years of pre-DA outcomes—and the fact that there are only eight institutions of higher education in Idaho—we also leverage the generalized synthetic control method (GSCM) to estimate causal effects of DA on our outcomes of interest. While we test our DID estimator across three separate counterfactual groups, the selection of an optimal counterfactual is empirically unguided in the DID framework and many researchers in this situation would rely on
matching techniques (St. Clair & Cook, 2015). In this case, a 1:1 optimal match would only allow for an analytic sample covering 16 institutions, sharply reducing power and bending the asymptotic assumptions inherent in many causal-inference estimators (Angrist & Pischke, 2009; Imbens & Wooldridge, 2009). For these reasons, our second estimation strategy assembles a suitable and weighted counterfactual group through GSCM—relying upon all available control units while minimizing outcome differences between the treatment and control units in the pre-treatment period.

Pioneered by Abadie et al. (2010) and referred to as a generalized extension of DID, GSCM also compares average outcomes among treated and control groups before and after a policy change, with observed outcome differences following a policy’s implementation attributable to the effect of the policy (Cunningham, 2018). Synthetic control methods use all available treatment-control comparison points and weights units to create a comparison group that is nearly identical the treatment group on outcomes in the pre-treatment period, allowing DID’s strict parallel-trends assumption to be relaxed (Rubin & González Canché, 2019). By generating a synthetic control unit whose outcomes mirror that of the treatment group conditioned on observable characteristics prior to the policy change, concerns regarding the selection of an optimal comparison group are reduced (Cunningham, 2018), and the synthetic unit can be considered a suitable comparison given its statistically indistinguishable difference from the treatment group in the pre-treatment period (Rubin & González Canché, 2019).

Formally, our goal is to estimate the treatment effect of DA ($\beta$) on our outcomes of interest, where

$$\beta = \frac{\bar{Y}^{\text{Idaho}}_{Z=1,t=1} - \bar{Y}^{\text{Idaho}}_{Z=0,t=1}}{1}.$$
or the difference between Idaho’s state or institutional outcome ($\bar{Y}$) at time $t = 1$ if the unit did ($Z = 1$) and did not ($Z = 0$) implement DA. Given that $\bar{Y}_{Z=0,t=1}^{\text{Idaho}}$ is unobservable in the potential-outcomes framework, equation (2) is incomplete. However, GSCM allows $\beta$ to be estimated by weighting $Y$ for each control unit in the pre-treatment period ($t = 0$) as a function of observable covariates $X_{it}$ such that

$$\bar{Y}_{Z=1,t=0}^{\text{Idaho}} - \sum w_i^* \bar{Y}_{Z=0,t=0}^{\text{Control}} \approx 0 ,$$

where $w_i^*$ is an optimal weight for control unit $i$ ensuring $\bar{Y}_{Z=1,t=0}^{\text{Idaho}}$ and $\bar{Y}_{Z=0,t=0}^{\text{Control}}$ are as mathematically close as possible. In GSCM, $w_i^*$s can be positive or negative (Xu & Liu, 2018, 2019). An optimization algorithm drives the selection of $w_i^*$ such that the Root Mean Square Error (prediction error) is minimized for $Y_{it} = \alpha_0 + \sum_{p=1}^P \beta_p X_{it} + \epsilon_{it}$ in the pre-treatment period, where $Y_{it}$ is the observed outcome for the treatment group and $X_{it}$ capture $p$ observable predictors of the control units.

With the knowledge that outcomes for the treatment group and the weighted control group are statistically indistinguishable as a function of their observable covariates in the pre-treatment period, $\sum w_i^* \bar{Y}_{Z=0}^{\text{Control}}$ becomes a suitable counterfactual and

$$\sum w_i^* \bar{Y}_{Z=0,t=1}^{\text{Control}} \approx \bar{Y}_{Z=1,t=1}^{\text{Idaho}}$$

in the post-treatment ($t = 1$) period. Given this, the treatment effect $\beta$ is estimated by

$$\beta = \bar{Y}_{Z=1,t=1}^{\text{Idaho}} - \sum w_i^* \bar{Y}_{Z=0,t=1}^{\text{Control}} .$$

This is taken as the average outcome difference between treatment ($Z = 1$) and control ($Z = 0$) groups in the post-treatment period ($t = 1$).

The generalized version of the synthetic control method carries many advantages, including its ability to allow for multiple treatment units (i.e., for our institution-level models) and variation in treatment timing (Kreif et al., 2016; Powell, 2018; Xu & Liu, 2018, 2019).
particular note, GSCM imputes synthetic units for each treated unit, rather than developing one aggregated unit, using a linear interactive fixed effects model with unit-specific intercepts interacted with time-varying coefficients—and relies upon a cross-validation procedure for inference (Bai, 2009; Xu, 2017). Because GSCM computes a synthetic control unit for each treated unit to arrive at $\bar{Y}$, we can both consider the aggregated effect of DA on all institutions in Idaho while also exploring sector-level effects—relying on the treated units and their synthetic control units by two- or four-year status. For more information on synthetic control methods generally, see Abadie et al. (2010), Cunningham (2018), and Rubin and González Canché (2018).

Similar to our DID estimator, we implement GSCM for our state-level outcome of interest, as well as those across institutions in aggregate and separated by level. The $X_{it}$ covariates used by our GSCM models are the same as those expressed in the DID equation above. Finally, rather than altering the available pool of state or institution donor units for the synthetic-unit construction, we allow the GSCM optimal weighting process to draw from the universe of other states and institutions to increase the likelihood of strong matches.

**Results**

Estimates from the DID models are presented in Tables 1 and 2 for state and institution-level outcomes, respectively. Across all three counterfactual groups, we find no evidence to suggest DA increased statewide FTE enrollment in Idaho following adoption after including all covariate controls and both state and year fixed effects. Predictors that were significantly related to statewide FTE enrollment included net tuition revenue, state appropriations, and the state’s high-school attainment rate. At the institutional level, while we also do not find evidence to suggest DA influenced undergraduate enrollment, in-state enrollment, or the percent of students who are racial minorities, we do find marginal evidence that DA reduced the percent of students
in the incoming fall cohorts who received Pell Grants by an average amount of 3.34% per campus. This was significant across the national (-3.84, \( p < .05 \)) and WICHE (-2.84, \( p < .10 \)) counterfactual groups.

We further explore this possible reduction in Pell-eligible enrollment by disaggregating our sample into two- and four-year institutions and re-estimating our DID model for this outcome. Table 3 shows these estimates by level and counterfactual group for this disaggregated institution-level analysis. In all, this effect appears to be driven by changes at both levels, with estimated reductions of 4.38% (\( p < .10 \)) at two-year institutions and 3.41% (\( p < .05 \)) at four-year institutions in the national sample, though these estimates are not robust to altered counterfactual groups (i.e., WICHE or neighboring states). Institutional predictors consistently associated with Pell enrollment were tuition and fee rates and state appropriations. In all, results provide evidence to suggest DA had little effect on enrollment and enrollment across regional, racial, or socioeconomic (Pell) contexts.

Table 4 presents estimates from the GSCM model predicting statewide FTE enrollment. After including state and year fixed effects and a full matrix of covariates, the estimated effect of DA on statewide FTE enrollment did not reach statistical significance, consistent with the DID estimate. Net tuition revenue and state appropriations were again identified as significant factors in predicting statewide FTE enrollment, as well as the size of the state’s high school senior cohort. Figure 3 plots the estimated effect of DA on statewide FTE and depicts the outcome differences between Idaho and its synthetic control unit.

At the institutional level, our GSCM estimates diverge from those produced by the DID models. Table 5 shows estimates from GSCM models predicting each institutional outcome of interest and is also disaggregated by two- and four-year institutions. For all intuitional types,
there is marginal evidence to suggest DA increased institutional undergraduate enrollment by
approximately 147 students per campus \((p < .10)\). Given estimates on in-state undergraduate
enrollments, it appears this overall enrollment increase could be driven entirely by in-state
students. The GSCM model suggests DA increased in-state enrollment by nearly 169 students
per campus, on average \((p < .05)\). Further, these changes appear to be driven entirely by
increased in-state enrollment at two-year institutions, with estimates exceeding 240 students per
campus \((p < .10)\). We do not find any evidence to suggest changes in enrollment across racial or
socioeconomic contexts—or any significant effects for four-year institutions. Figure 4 plots the
estimated effect of DA on each institutional outcome of interest and depicts outcome differences
between institutions in Idaho and their aggregated synthetic control unit. Given the marginal and
inconsistent evidence across our institutional DID and GSCM estimates, we do not find evidence
to support our hypothesis that DA would increase institutional enrollment across regional, racial,
or socioeconomic contexts.

Robustness Checks

While we do not find any consistent evidence to suggest an effect of DA across our DID
and GSCM models, we conduct two primary robustness checks to test the assumptions
underlying the DID and GSCM estimators.

For our DID models, we seek to ensure our results are not driven by outcome deviations
in pre-treatment years, which could bias our estimates and suggest a violation DID’s parallel-
trends assumption (St. Clair & Cook, 2015). In addition to parallel-trend plots in Figure 1 (state)
and Figure 2 (institutional), we perform an event study analysis—in our setting, similar to
placebo or falsification tests—to test for treatment effects in the absence of actual treatment (i.e.,
if significant effects are observed prior to 2016-17) and to test for the magnitude of differences following DA adoption. Formally, we estimate

\[ Y_{it} = \alpha_0 + \beta \sum_{t=2010}^{2017} (Idaho_i \times \rho_t)_{it} + \delta X_{it} + \pi_t + \rho_t + \varepsilon_{it}, \]

where we interact a dummy treatment indicator equal to 1 for Idaho (or institutions in Idaho) (Idaho_i) with each year factor (\( \rho_t \)), omitting the year immediately prior to DA (2015-16) as reference. This analysis uses the national counterfactual group and follows that of Gurantz (2020) and others. We include the same \( X_{it} \) covariates used in our earlier models. Event study plots with treatment estimates for each year and 95% confidence intervals are presented in Figure 1 for state-level outcomes and Figure 2 for institution-level outcomes. We would expect significant treatment effects to be absent across the 2010-11 through 2014-15 horizon—or for effects to be in the opposite direction as hypothesized—but for significant effects to be estimated following DA adoption.

At the state-level, we detect statistically significant differences between Idaho and its national counterfactual group prior to DA adoption, suggesting a possible violation of the parallel-trends assumption. Despite this, only marginal increases in statewide FTE enrollment are observed following DA adoption compared to the 2015-16 academic year—providing further evidence DA may have had little effect on statewide enrollment in higher education. At the institutional level, however, the outcomes assessed by our DID models appear to adhere to the parallel-trends assumption given the absence of significant effects in pre-DA years for all four outcomes of interest. The event study plots suggest DA may have produced marginal increases in undergraduate enrollment and in-state enrollment compared to the 2015-16 academic year—consistent with our GSCM estimates. In all, these event study estimates provide evidence to support the use of a DID estimator for our institution-level models and provide further
justification for a complementary GSCM approach given possible pre-treatment deviations for
the state-level outcomes.

Our DID robustness test are considered “in-time” placebo tests, testing for significant
effects given varied years of treatment assignment (Abadie et al., 2015). As a robustness test for
our GSCM models, we seek to ensure our results are not driven by the selection of an optimally
weighted counterfactual unit. With the knowledge that GSCM weights all available units by \( w_i^* \),
it is possible significant or insignificant differences could be driven by outcome changes of the
non-treatment unit(s) given the highest weight (Rubin & González Canché, 2019). To test for
this possibility, we conduct “across-unit” placebo tests for the state and institution-level models
were we falsely assign treatment to the highest-weighted state (or highest-weighted institutions)
and re-estimate our GSCM models with Idaho (or institutions in Idaho) removed to assess
whether significantly estimated effects exist for the counterfactual units (Abadie et al., 2015).

Table 6 shows results from these placebo tests. If our underlying assumptions are met, we
should not detect any significant differences between the placebo unit and its synthetic
counterfactuals. In our state models, GSCM used Texas as the most heavily weighted
counterfactual state. Assigning hypothetical treatment to Texas in 2016-17 and beyond and re-
estimating the GSCM models produced a marginally significant estimated effect on statewide
FTE of 36.5 thousand FTE \((p < .10)\). This is exhibited by the large increase in the synthetic
control outcomes in Figure 3 and suggests our null effect for Idaho’s statewide FTE enrollment
in our primary state GSCM model could be driven by Texas’s large increase in FTE compared to
the nation. That is, if Idaho was expected to increase given an increase in its counterfactual, but it
did not, the point estimate for Idaho would be negative. The GSCM estimate for Idaho’s
statewide FTE was a substantially smaller than this and statistically insignificant at 4.85
thousand. Given the fact this placebo test is only marginally significant, and that estimates from our state-level DID and GSCM specifications agree DA had little effect on statewide FTE enrollment, it appears our use of GSCM is a reasonable approach here.

For placebo tests with our institutional models, we falsely assign treatment to the eight highest-weighted colleges and universities to replace the eight colleges and universities in Idaho. Given that GSCM may find different optimal institutions across each outcome (i.e., undergraduate enrollment versus percent Pell), we allow the false treatment units to vary by outcome as well. Results for our institution-level placebo tests are also shown in Table 6. As expected, we find no significant differences between our false treatment units and their synthetic counterfactuals derived from the national sample, except for changes in the racial composition of campuses’ undergraduate enrollment. Assigning treatment to the eight highest weighted institutions for Idaho’s percent racial minority GSCM model in 2016-17 and beyond produced a placebo estimate of a 22.5% per campus reduction ($p < .001$) in the percentage of students who are racial minorities. This is exhibited by the large reduction in the synthetic control outcomes in the third line of Figure 4. In this light, any increase in racial minority enrollment in Idaho could have been estimated as a significant positive change given its sharply declining counterfactual. However, after incorporating full covariate controls and both institution and year fixed effects, the GSCM estimate for this outcome in Idaho was of substantially smaller magnitude at 6.62% and not statistically significant. The institution-level DID estimates were also statistically insignificant. In all, these tests provide evidence that our GSCM models are relatively unbiased by the method’s selection and weighting of counterfactual units, providing further justification for our use of the GSCM method.
While we can be reasonably confident in our estimates, a significant state-level DID event study estimate and a significant institutional GSCM placebo test suggest our primary estimates should be interpreted with caution. In all, we believe these realities provide strong justification for our complementary DID and GSCM approach and suggest we should draw inference from the entirety of evidence produced by the two quasi-experimental techniques rather than any one alone. While each method has benefits and limitations, the use of complementary techniques in this manner has been shown to produce strong and robust evidence when considering treatment effects in education settings, with two approaches “much stronger together either would be alone” (Page et al., 2019; Scott-Clayton, 2011, p. 618).

Discussion

In the face of increasingly constrained budgets, states need a new and innovative, yet low-cost mechanism to increase access and enrollment in postsecondary education. Not only is increased educational attainment required to fuel the modern workforce (Carnevale et al., 2013), but persistent gaps in college access and completion across racial, socioeconomic, and geographic contexts present real challenges for individuals and their communities (Frey, 2015; Zumeta et al., 2012). To improve its educational attainment rate, reduce persistent racial and geographic gaps in college access, increase postsecondary enrollment, and reduce out-of-state migration, the state of Idaho leveraged its data capacity to create a DA system. In this study, we sought to estimate the causal effects of this DA policy on state and institution-level enrollment outcomes. In all, while Idaho enjoyed increased enrollment following the introduction of DA, we do not find consistent evidence to suggest DA led to increased FTE, undergraduate, or in-state enrollments nor to suggest DA improved the enrollment of Pell-eligible or racial-minority students. While we cannot provide evidence to suggest DA is the viable, low-cost policy
innovation states seek, a DA system is built upon a strong foundation of theory and experimental evidence suggesting simplifications of the college-going process should yield increased enrollments, particularly across students’ geographic, racial, and socioeconomic contexts.

It is important to note that features of Idaho’s population may have made it difficult to find effects of DA in our analysis. First, while have provided first evidence on the effects of DA on postsecondary enrollment outcomes, DA is still a relatively new program, and additional years of post-implementation data would strengthen our evaluation. Second, Idaho also implemented other postsecondary initiatives in the years around the adoption of DA. Enrollments from DA started in academic year 2016-17, while Apply Idaho, a statewide common application came on-line in 2017-18. The common application charged application fees for its first year, and one institution (Idaho State University) continues to charge application fees—a practice prior literature has shown can be a deterrent to college enrollment (Hoxby & Avery, 2013). Arguably, a full DA system, with DA plus a common and free application, was not fully operational until 2017-18. We ran our models on the earliest possible year of treatment so as to avoid confounding factors of these related policies, but an analysis with the policy implementation starting only after the full DA system was operational would likely yield different results. In any case, given that we do not observe consistent evidence of any enrollment increases in Idaho, we can be confident the introduction of a common application is not upwardly biasing our estimates. Third, the demographic mix in Idaho is different from many other states, with over 73% of its 2017-18 undergraduates identifying as white.4 We entered this research intending to run analyses on specific racial groups, however, some racial groups were so small in Idaho that we did not run analyses on those populations due to concerns of confidentiality. Because of this, our analyses

---

4 Authors’ calculations from IPEDS fall 2017 enrollment file.
only consider white and non-white racial categories. A state with a different demographic mix would enable these important subgroup analyses and would likely be better matched to more diverse comparison states.

More research is needed to better understand the mechanisms by which DA might influence students’ application and enrollment behavior across subgroups, and future studies should consider the effects a DA policy implemented in tandem with other interventions (e.g., a common application or need-based financial aid program) to increase college access. In addition, future work should incorporate additional years of post-treatment data and use finer-grained data sources to address missing value issues in federal and national sources. As DA programs are developed in new states, additional analyses of the impact of these programs on different racial groups and low-income students is needed.
References


Table 1. Difference-in-differences estimates of the effect of Direct Admissions on statewide FTE enrollment (log), by counterfactual group.

<table>
<thead>
<tr>
<th></th>
<th>Nation</th>
<th>WICHE</th>
<th>Contiguous</th>
</tr>
</thead>
<tbody>
<tr>
<td>Direct Admissions</td>
<td>-0.000</td>
<td>0.015</td>
<td>-0.008</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.017)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>Net Tuition Revenue</td>
<td>0.131*</td>
<td>0.286**</td>
<td>0.190</td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.088)</td>
<td>(0.115)</td>
</tr>
<tr>
<td>State Appropriations</td>
<td>0.113**</td>
<td>0.102</td>
<td>0.101</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.071)</td>
<td>(0.116)</td>
</tr>
<tr>
<td>Gini Coefficient</td>
<td>-0.263</td>
<td>-0.676</td>
<td>-1.137</td>
</tr>
<tr>
<td></td>
<td>(0.396)</td>
<td>(0.452)</td>
<td>(0.842)</td>
</tr>
<tr>
<td>Unemployment Rate</td>
<td>0.002</td>
<td>-0.000</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.007)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>High School Attainment Rate</td>
<td>0.007</td>
<td>0.016+</td>
<td>0.020*</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.008)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>B.A. or Above Attainment Rate</td>
<td>-0.002</td>
<td>-0.006</td>
<td>-0.015</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.008)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>High-School Senior Population</td>
<td>0.060</td>
<td>0.073</td>
<td>0.063</td>
</tr>
<tr>
<td></td>
<td>(0.079)</td>
<td>(0.105)</td>
<td>(0.211)</td>
</tr>
</tbody>
</table>

| N (Unit by Year)     | 400      | 120      | 56         |
| State & Year Fixed Effects | Yes | Yes | Yes |
| R² with F.E.s        | 0.999    | 0.999    | 0.999      |
| R² without F.E.s     | 0.150    | 0.351    | 0.370      |

+ p < .10; * p < .05; ** p < .01; *** p < .001.


Notes: Tables report coefficients and heteroskedastic robust standard errors (in parentheses) clustered at the state level; Figures round to thousandths; All models include state and year fixed effects; All financial predictors were inflation adjusted to the Consumer Price Index (2018); The outcome (statewide FTE) and specific predictors (net tuition revenue, state appropriations, and high-school senior population) are logged.
Table 2. Summary of difference-in-differences estimates of the effect of Direct Admissions on institution enrollment outcomes, by outcome and counterfactual group.

<table>
<thead>
<tr>
<th>↓ Outcome</th>
<th>Counterfactual →</th>
<th>Nation</th>
<th>WICHE</th>
<th>Contiguous</th>
</tr>
</thead>
<tbody>
<tr>
<td>Undergraduate Enrollment (log)</td>
<td>0.075</td>
<td>0.032</td>
<td>-0.009</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.093)</td>
<td>(0.088)</td>
<td>(0.084)</td>
<td></td>
</tr>
<tr>
<td>In-State Enrollment (log)</td>
<td>0.121</td>
<td>0.067</td>
<td>0.010</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.102)</td>
<td>(0.101)</td>
<td>(0.098)</td>
<td></td>
</tr>
<tr>
<td>Percent Racial Minority</td>
<td>-0.790</td>
<td>-1.887</td>
<td>-2.515</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.800)</td>
<td>(1.849)</td>
<td>(1.823)</td>
<td></td>
</tr>
<tr>
<td>Percent Receiving Pell Grants</td>
<td>-3.844*</td>
<td>-2.844+</td>
<td>-1.621</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.571)</td>
<td>(1.560)</td>
<td>(1.688)</td>
<td></td>
</tr>
</tbody>
</table>

| N (Unit by Year) | 12,120 | 3,008 | 896 |
| Full Covariate Controls | Yes | Yes | Yes |
| Institution & Year Fixed Effects | Yes | Yes | Yes |

+ $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$.

Sources: IPEDS, 2007-08 through 2017-18.

Notes: Tables reports coefficients for DID estimated effects of DA on each outcome by counterfactual group, with heteroskedastic robust standard errors (in parentheses) clustered at the institution level; Figures round to thousandths; All models include institution and year fixed effects plus full covariate controls with logged tuition and fee rate, logged state appropriations, logged scholarship expenditures, and graduation rate; Undergraduate enrollment (logged) is Fall first-time, degree-seeking enrollment; In-state enrollment (logged) is the number of Fall first-time undergraduates from in-state; Percent racial minority is percent of non-white undergraduate enrollment; Percent receiving Pell Grants is percent of first-time, full-time undergraduates awarded Pell Grants; All financial predictors were inflation adjusted to the Consumer Price Index (2018); Full tables available upon request.
Table 3. Difference-in-differences estimates of the effect of Direct Admissions on percent of undergraduate enrollment who are Pell-eligible, by level and counterfactual group.

<table>
<thead>
<tr>
<th></th>
<th>Two-Year</th>
<th></th>
<th></th>
<th>Four-Year</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Nation</td>
<td>WICHE</td>
<td>Contiguous</td>
<td>Nation</td>
<td>WICHE</td>
<td>Contiguous</td>
</tr>
<tr>
<td>Direct Admissions</td>
<td>-4.376+</td>
<td>-3.444</td>
<td>-3.053</td>
<td>-3.413*</td>
<td>-2.175</td>
<td>-0.206</td>
</tr>
<tr>
<td></td>
<td>(2.483)</td>
<td>(2.466)</td>
<td>(2.902)</td>
<td>(1.567)</td>
<td>(1.518)</td>
<td>(1.631)</td>
</tr>
<tr>
<td>Tuition and Fee Rate</td>
<td>1.665</td>
<td>5.922+</td>
<td>1.601</td>
<td>0.410+</td>
<td>8.949***</td>
<td>0.601</td>
</tr>
<tr>
<td></td>
<td>(1.430)</td>
<td>(3.116)</td>
<td>(9.210)</td>
<td>(0.246)</td>
<td>(2.621)</td>
<td>(4.182)</td>
</tr>
<tr>
<td>State Appropriations</td>
<td>0.160</td>
<td>0.023</td>
<td>2.210***</td>
<td>0.082</td>
<td>0.182</td>
<td>0.686**</td>
</tr>
<tr>
<td></td>
<td>(0.119)</td>
<td>(0.071)</td>
<td>(0.271)</td>
<td>(0.140)</td>
<td>(0.143)</td>
<td>(0.229)</td>
</tr>
<tr>
<td>Scholarship Expenditures</td>
<td>0.096</td>
<td>0.063</td>
<td>0.304</td>
<td>0.165+</td>
<td>0.487</td>
<td>0.498</td>
</tr>
<tr>
<td></td>
<td>(0.075)</td>
<td>(0.108)</td>
<td>(0.218)</td>
<td>(0.095)</td>
<td>(0.328)</td>
<td>(0.772)</td>
</tr>
<tr>
<td>Graduation Rate</td>
<td>0.003</td>
<td>-0.004</td>
<td>-0.023</td>
<td>0.017</td>
<td>-0.013</td>
<td>-0.039</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.038)</td>
<td>(0.048)</td>
<td>(0.031)</td>
<td>(0.050)</td>
<td>(0.044)</td>
</tr>
<tr>
<td>N (Unit by Year)</td>
<td>6,888</td>
<td>1,688</td>
<td>376</td>
<td>5,232</td>
<td>1,320</td>
<td>520</td>
</tr>
<tr>
<td>Institution &amp; Year Fixed Effects</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>R² with F.E.s</td>
<td>0.855</td>
<td>0.876</td>
<td>0.831</td>
<td>0.945</td>
<td>0.924</td>
<td>0.894</td>
</tr>
<tr>
<td>R² without F.E.s</td>
<td>0.003</td>
<td>0.014</td>
<td>0.072</td>
<td>0.005</td>
<td>0.037</td>
<td>0.013</td>
</tr>
</tbody>
</table>

+ p ≤ .10 * p ≤ .05 ** p ≤ .01 *** p ≤ .001
Notes: Tables reports coefficients and heteroskedastic robust standard errors (in parentheses) clustered at the institution level; Figures round to thousandths; All models include institution and year fixed effects; All financial predictors were inflation adjusted to the Consumer Price Index (2018); Specific predictors (tuition and fee rate, state appropriations, and scholarship expenditures) are logged; Outcome is percent of first-time, full-time undergraduates awarded Pell Grants.
Table 4. Generalized synthetic control method estimates of the effect of Direct Admissions on statewide FTE enrollment.

<table>
<thead>
<tr>
<th></th>
<th>Estimate</th>
<th>Std. Error</th>
</tr>
</thead>
<tbody>
<tr>
<td>Direct Admissions (1,000 FTE)</td>
<td>-4.85</td>
<td>(9.78)</td>
</tr>
<tr>
<td>Net Tuition Revenue ($1M)</td>
<td>-0.02**</td>
<td>(0.01)</td>
</tr>
<tr>
<td>State Appropriations ($1M)</td>
<td>0.01**</td>
<td>(0.00)</td>
</tr>
<tr>
<td>Gini Coefficient</td>
<td>9.23</td>
<td>(66.88)</td>
</tr>
<tr>
<td>Unemployment Rate</td>
<td>1.45</td>
<td>(0.92)</td>
</tr>
<tr>
<td>High School Attainment Rate</td>
<td>0.74</td>
<td>(1.00)</td>
</tr>
<tr>
<td>B.A. or Above Attainment Rate</td>
<td>-0.41</td>
<td>(0.71)</td>
</tr>
<tr>
<td>High-School Senior Population (1,000)</td>
<td>-0.84*</td>
<td>(0.30)</td>
</tr>
<tr>
<td>N (Unit by Year)</td>
<td>400</td>
<td></td>
</tr>
</tbody>
</table>

State & Year Fixed Effects: Yes

*p < .10; *p < .05; **p < .01; ***p < .001.

Notes: Table reports coefficients and standard errors (in parentheses); Figures rounded to hundredths; Model includes state and year fixed effects; All financial predictors were inflation adjusted to the Consumer Price Index (2018).
Table 5. Summary of generalized synthetic control method estimates of the effect of Direct Admissions on institutional outcomes, by outcome and subgroup.

<table>
<thead>
<tr>
<th>↓ Outcome</th>
<th>Subgroup →</th>
<th>All</th>
<th>Two-Year</th>
<th>Four-Year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Undergraduate Enrollment</td>
<td></td>
<td>146.70+</td>
<td>180.40</td>
<td>111.10</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(116.90)</td>
<td>(137.40)</td>
<td>(138.40)</td>
</tr>
<tr>
<td>In-State Enrollment</td>
<td></td>
<td>168.60*</td>
<td>240.60+</td>
<td>103.30</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(81.86)</td>
<td>(134.70)</td>
<td>(134.80)</td>
</tr>
<tr>
<td>Percent Racial Minority</td>
<td></td>
<td>6.62</td>
<td>2.23</td>
<td>-1.10</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(7.61)</td>
<td>(1.85)</td>
<td>(4.38)</td>
</tr>
<tr>
<td>Percent Receiving Pell Grants</td>
<td></td>
<td>0.75</td>
<td>-3.53</td>
<td>0.53</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(2.08)</td>
<td>(3.24)</td>
<td>(3.02)</td>
</tr>
<tr>
<td>N (Unit by Year)</td>
<td></td>
<td>12,120</td>
<td>6,888</td>
<td>5,232</td>
</tr>
<tr>
<td>Full Covariate Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Institution &amp; Year Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
</tbody>
</table>

+ $p < .10$; * $p < .05$; ** $p < .01$; *** $p < .001$.

Sources: IPEDS, 2007-08 through 2017-18.

Notes: Table reports coefficients for GSCM estimated effects of DA on each outcome by subgroup, with standard errors reported in parentheses; Figures rounded to hundredths; All models include state and year fixed effects plus full covariate controls with tuition and fee rate, state appropriations, scholarship expenditures, and graduation rate; All financial predictors were inflation adjusted to the Consumer Price Index (2018); Full tables available upon request.
### Table 6. Summary of generalized synthetic control method placebo tests, by model, outcome, and synthetic placebo unit.

<table>
<thead>
<tr>
<th>↓ Outcome</th>
<th>Placebo →</th>
<th>Texas</th>
<th>8 Institutions</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>State Level</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>FTE (1,000)</td>
<td></td>
<td>36.46+</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(17.42)</td>
<td></td>
</tr>
<tr>
<td><strong>Institution Level</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Undergraduate Enrollment</td>
<td>-</td>
<td>-</td>
<td>522.32</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(298.20)</td>
</tr>
<tr>
<td>In-State Enrollment</td>
<td>-</td>
<td>-</td>
<td>-106.23</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(93.25)</td>
</tr>
<tr>
<td>Percent Racial Minority</td>
<td>-</td>
<td>-</td>
<td>-22.46***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(2.05)</td>
</tr>
<tr>
<td>Percent Receiving Pell Grants</td>
<td>-</td>
<td>-</td>
<td>-2.20</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(2.59)</td>
</tr>
<tr>
<td>N (Unit by Year)</td>
<td>392</td>
<td></td>
<td>12,056</td>
</tr>
<tr>
<td>Full Covariate Controls</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Unit &amp; Year Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
</tbody>
</table>

+ * p < .10; ** p < .05; *** p < .01.

**Sources:** IPEDS, 2007-08 through 2017-18.

**Notes:** Table reports placebo test coefficients for GSCM estimated effects on each outcome by level and placebo unit, with standard errors reported in parentheses; Figures rounded to hundredths; State model estimates effects with Texas (highest weight) as treatment unit in place of Idaho (with Idaho excluded from the sample); Institution level models estimate effects with 8 institutions (highest weight) as treated units in place of 8 Idaho institutions (with Idaho institutions excluded from the sample), which vary by model given respective GSCM fits; All models include state and year fixed effects plus full covariate controls by respective model: state (net tuition revenue, state appropriations, Gini coefficient, unemployment rate, high school attainment rate, B.A. or above attainment rate, and high-school senior population) or institution (tuition and fee rate, state appropriations, scholarship expenditures, and graduation rate); All financial predictors were inflation adjusted to the Consumer Price Index (2018); Full tables available upon request.
Figure 1. Parallel-trend and event study plots for state-level difference-in-differences model.

Notes: Parallel trend plot depicts logged statewide FTE enrollment for Idaho and three counterfactual groups before and after DA. Event study plot depicts year-specific estimated effects for Idaho using a national counterfactual with 95% confidence intervals, omitting the year before DA (2015-16) as reference.
Figure 2. Parallel-trend and event study plots for institution-level difference-in-differences models, by outcome.

Notes: Parallel trend plots depict outcomes for institutions in Idaho and three counterfactual groups before and after DA. Event study plot depicts year-specific estimated effects for institutions in Idaho using a national counterfactual with 95% confidence intervals, omitting the year before DA (2015-16) as reference.
Figure 3. State-level generalized synthetic control plot: Statewide FTE.

Notes: Effect estimate shows differences between Idaho and synthetic Idaho with confidence bands, with the estimated effect of DA by year shown at time 0 and beyond. Synthetic control plot shows Idaho’s outcomes and the weighted synthetic unit’s outcomes, with the estimated effect of DA derived by the difference in lines in the post-DA (shaded) period.
Figure 4. *Institution-level generalized synthetic control plots, by outcome.*

Notes: Effect estimate shows differences between institutions in Idaho and synthetic Idaho with confidence bands, with the estimated effect of DA by year shown at time 0 and beyond. Synthetic control plot shows Idaho’s institutional outcomes and the weighted synthetic unit’s outcomes, with the estimated effect of DA derived by the difference in lines in the post-DA (shaded) period.