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Out-of-State Enrollment, Financial Aid and Academic Outcomes: Evidence from Wisconsin

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Scholars disagree about the effect out-of-state university students have on potential in-state students. Despite paying a premium to attend state universities, researchers argue that out-of-state students may come at a cost to in-state students by negatively affecting academic quality or by crowding out in-state students. To study this relationship, we examine the effect of a 2016 policy at a highly ranked state flagship university that removed the limit on how many out-of-state students it could enroll. We find the policy caused an increase in out-of-state enrollment by around 29 percent and increased tuition revenue collected by the university by 47 percent. We argue that this revenue was used to fund increases in financial aid disbursed at the university, particularly to students from low-income households, indicating that out-of-state students cross-subsidize lower income students. We also fail to find evidence that this increase in out-of-state students had any effect on several measures of academic quality.

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Out-of-State Enrollment, Financial Aid and Academic Outcomes: Evidence from Wisconsin

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Abstract

Scholars disagree about the effect out-of-state university students have on potential in-state students. Despite paying a premium to attend state universities, researchers argue that out-of-state students may come at a cost to in-state students by negatively affecting academic quality or by crowding out in-state students. To study this relationship, we examine the effect of a 2016 policy at a highly ranked state flagship university that removed the limit on how many out-of-state students it could enroll. We find the policy caused an increase in out-of-state enrollment by around 29 percent and increased tuition revenue collected by the university by 47 percent. We argue that this revenue was used to fund increases in financial aid disbursed at the university, particularly to students from low-income households, indicating that out-of-state students cross-subsidize lower income students. We also fail to find evidence that this increase in out-of-state students had any effect on several measures of academic quality.

Key Words: Higher Education, Out-of-State Students, Human Capital

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1 Introduction

It is commonplace in the United States to attend college outside of your home state. According to results from the Current Population Survey, nearly one third of all students travel to another state for college. For many state universities, this implies that they face high demand from out-of-state students (OSS). In the face of decreasing funding from state government sources, these out-of-state students provide key resources that can fund educational expenditures (e.g financial aid), as they pay a high premium to attend public universities relative to in-state students.

Yet many state universities also face restrictions on the number of out-of-state students they can enroll,[1] with politicians often calling for them to serve in-state students.[2] Similarly, some scholars believe that enrolling more out-of-state students may come at a cost to in-state students, either through a crowding-out mechanism or a reduction in academic quality (Jacquette, 2017; Jacquette, Curs, and Posselt, 2016; Mathias, 2020). It is therefore unclear whether greater enrollment of out-of-state students will have a net positive or negative impact on in-state students at public universities.

In this paper, we study how changes in undergraduate OSS enrollment affect financial aid and academic outcomes. To study this, we exploit a 2016 policy change at a highly ranked state flagship university that removed the existing limit on out-of-state student enrollment—the University of Wisconsin, Madison (WM). To estimate the causal effect of the policy on several key outcomes, we primarily employ a bias-corrected synthetic control estimating strategy. In this procedure, our comparison unit consists of an optimally-weighted average of control universities that are selected to best match WM. For our analysis, we use data from the IPEDS survey, which collects annual data on enrollments and several other variables from virtually every university in the country.

We find that lifting the cap resulted in greater OSS enrollment, which provided sharply higher tuition revenues that were used to increase funding for in-state low-income students. This was achieved without any clear negative effect on academic outcomes. Specifically, we find that the policy increased out-of-state enrollment by nearly 29 percent four years after implementation. As a result, overall tuition revenue increased by 46.8 percent over that same time period. We fail to find any significant effects on either in-state or out-of-state tuition rates, indicating that this increase in revenue came from these newly admitted out-of-state students.[3] As a result of the increase in tuition revenue, we find WM spent 29.4 percent more on student grant aid, particularly for in-state students from families that report making $30,000 or less.

[1] These include the University of North Carolina and the University of California systems, along with others including (until 2016) the University of Wisconsin, Madison.

[2] Hilary Clinton declared during the 2016 presidential race, “We have got to get back to using public colleges and university for what they were intended. If it is in California, for the children in California. If it is in New York, for the children in New York.”

[3] In the IPEDS data, tuition and fees is reported as a single variable. We use this variable in our main analysis. In reality, tuition and fee amounts are different for in-state and out-of-state students on average. In this paper, “tuition” is used to refer to the tuition and fees variable.

When we explore other potential effects of increased OSS enrollment, we do not find evidence of a negative impact on the academic quality of the university. We estimate that student-to-faculty ratios, retention and four year graduation rates are not impacted negatively, implying that WM may have increased spending on educational inputs (Deming and Walters, 2017). These results are robust to the use of different estimators and methods of inference, and running placebo checks based on the timing of the policy.

This paper contributes to two literatures. First, it contributes to work studying how out-of-state students affect the academic quality of universities and impact in-state students more broadly. Previous research has documented that as state appropriations to higher education have fallen, universities have increased the number of out-of-state students they enroll to make up the difference (Curs and Jacquette, 2017). These studies indicate that this increase in out-of-state students places academic burdens on universities (Jacquette, 2017; Jacquette, Curs, and Posselt, 2016; Curs and Jacquette, 2017), making the decision to enroll more out-of-state students more challenging. Further research has found that increases in OSS may also crowd-out in-state student enrollment (Curs and Jacquette, 2017), arguing that attendance at public universities is a zero-sum game between in-state and OSS. Our paper contributes to this literature by documenting that concerns about increases in out-of-state enrollments at state universities may be incorrect. While we estimate small negative effects on in-state enrollment, these results are non-significant, indicating no compelling evidence that greater OSS enrollment crowds-out in-state students. When we look at measures of academic quality, we also fail to detect significant negative effects.[4]

Second, this paper also contributes to the literature studying out-of-state student enrollment and higher education finance. Measuring the effect of increased OSS enrollment on financial outcomes is difficult as these outcomes are all likely related to other potentially endogenous factors such as student applications and enrollment, or how generous a university's or state's financial aid packages are (Winters V., 2012; Curs and Singell, 2002; Kerkvliet and Nowell, 2012). While most other papers in this literature use a fixed effects approach to study the relationship between OSS and university finances (Jaquette and Curs, 2015), our design leverages a policy that exogenously changes how many OSS the University of Wisconsin, Madison could admit. Moreover, this policy was decided by the Wisconsin Board of Regents, which operates under the approval of the governor, not the university itself. Recent work studying the relationship between states and universities finds that they are often optimizing different objective functions (Groen and White, 2004), reducing concerns that the policy choice we study was endogenous to the university.

Similar to effects studying increases in international student enrollment (Shih, 2017; Chen, 2021), we find strong evidence that tuition revenue from out-of-state students was used to subsidize in-state low-income students, ultimately increasing the resources available to Wisconsin residents. Focusing on how changes in revenue influence university behavior, Miller and Park (2022) find that tuition freezes and caps at public intuitions limit universities' financial aid generosity, influencing

[4] Our estimates studying student-to-faculty ratios and 4-year graduation rates actually indicate a potential positive effect of the 2016 policy, although our estimates studying these outcomes are noisy.

where students who depend on financial aid go to college. We find that increases in OSS act as a counter-weight to such effects, essentially increasing the university's budget constraint. Lastly, our results are consistent with findings in Cook and Turner (2022), who document an increase in price discrimination at universities, where students who have a higher ability to pay are charged more than low-income students. We argue that the significant premia paid by out-of-state students plays an important role in universities' ability to support low-income students.

The paper is structured as follows: section 2 describes the University of Wisconsin–Madison and the policy change which removed the limit on out-of-state student enrollment; section 3 provides more details on the data and presents descriptive results; section 4 discusses our synthetic control estimation procedure; section 5 presents our main findings and considers the robustness of our results; and section 6 concludes.

2 Policy Change Details

The University of Wisconsin, Madison is a large public research university located in southern Wisconsin. As of 2022, it's undergraduate enrollment totalled 37,235, with an average GPA and SAT score for incoming freshman of 3.86 and 1,390 respectively, making admissions to WM competitive. WM is also part of the University of Wisconsin system, which consists of WM and 12 other universities. The University of Wisconsin system is overseen by a governing body known as the board of regents, which is comprised of 18 members, 16 of which are appointed by the governor of Wisconsin. Board members are subject to approval by the state senate and serve for two-year terms. The board of regents decide levels of public funding from the state for each university and dictate other features of the University of Wisconsin system, such as enrollment levels.

In an exercise of it's authority, in 2012 the Wisconsin board of regents decided to limit the out-of-state share of students which WM could enroll, to 27.5%.[5] Exemptions from this policy included students coming from Minnesota, who were for the purposes of the university, in-state students. The rationale for this decision was to prioritize students who were most likely to live in Wisconsin after graduation. According to documents presented at the board of regents meeting, over 75% of WM students from Wisconsin live in the state after graduation, whereas only 15% of OSS do so.

In October 2015, however, the university of Wisconsin board of regents voted to remove the limit on out-of-state enrollments at WM the next year. The purpose of this removal was to counteract decreasing high school graduation trends in Wisconsin.[6] In addition to this rationale, previous work has documented that OSS can also add a significant boost to revenue, as they often pay high premiums to attend out-of-state universities. Specifically, at WM, in-state students pay $10,720 per year, while OSS pay $39,427 per year for the 2022-2023 school year. The OSS enrollment

[5] In 2012, the regents voted to increase the share from 25 to 27.5%

[6] Details on the rationale can be found in the meeting minutes for the October 2015 board of regents meeting, found here: https://www.wisconsin.edu/regents/download/meeting_materials/2015/october_2015/October-2015-Education-Committee-pdf-corrected-1007.pdf

policy took effect in the fall of 2016. [7] This policy removed any constraint on the number of OSS WM enrolled each year. We use this policy change to study three research questions; 1) whether it caused an increase in out-of-state enrollment, 2) what were the financial impacts of this policy and, 3) what academic impacts did this policy have on the WM student population?

3 Data, Sample, and Descriptive Results

3.1 Data

To study OSS enrollment and university outcomes, we use the Integrated Postsecondary Education Data System (IPEDS). IPEDS is the primary source of information on postsecondary institutions in the U.S. It includes a wealth of information on institutional characteristics, the student body, and school and student finances. We use data on basic institutional characteristics, such as type (e.g. public or private) and level of degrees offered, admission considerations, enrollments, retention and graduation rates, financial aid distribution, and school revenues and expenditures. The timing of data collection for some variables is not straightforward and we describe it in more detail below. Additionally, whenever we refer to an academic year as a single year rather than a range, we're referencing the academic calendar start year (e.g. 2015-2016 would be described as 2015, except as indicated below).

Postsecondary institutions collect fall enrollment information on October 15 or on the institution's official fall reporting date. IPEDS provides breakdowns of enrollment statistics both at the total undergraduate level and at the first-time degree/certificate-seeking undergraduate student level. We refer to the latter group as freshmen throughout the paper. Enrollment breakdowns are available by gender, race, and age group (under 25 or 25 and older) at the total undergraduate level. Enrollments by country (U.S. or non-U.S. only) and by the U.S. state of residence when the student was admitted are available for freshmen. The OSS group includes either international students or individuals from the U.S. state that is different from the state of the institution they enrolled in. For WM, we define students coming from both Wisconsin and Minnesota as in-state. Institutional reporting of this data to IPEDS is only mandatory in even years. In odd years, a lot of schools choose not to submit this data, so we see a lot of missing values. Retention rates are based on fall enrollment counts of returning full-time freshman undergraduates. [8] Student-to-faculty ratio is the count of total undergraduate full-time equivalent students divided by the count of full-time equivalent instructional staff not teaching in graduate programs.

IPEDS reports graduation rates for student cohorts who entered the institution six years prior to current academic year. We use three sets of graduation rates - for students who completed their undergraduate degrees in four, five, and six years. IPEDS measurement timing means that the

[7] As part of the policy, the board of regents required that at least 3,500 students from the state of Wisconsin be admitted each year.

[8] This variable only measures transitions from first-year to second-year, which is only one measure of total retention. The freshman retention rate is often used as a valuable proxy for important outcomes such as graduation.

rate of students graduating in four years is based on completion counts two years prior to current academic year, the rate of students graduating in five years is based on completion counts from the previous year, and the rate of students graduating in six years is based on current year completions.

Some measures of school finances reflect statistics for the fiscal year that ended before October 1 of the current academic year. The reporting period varies slightly across institutions (fiscal year end dates in our sample range from May 31 to August 31) but can be roughly thought of as the previous academic year. Current academic year school finance variables are published tuition and fees. Fiscal year variables are revenue shares and student financial aid. Measures of student financial aid, such as Pell Grant recipient counts and average amount of aid per student, only include full-time freshmen. Student financial aid by household income is further restricted to full-time freshmen paying in-state tuition who were awarded any grant or student loan aid.

3.2 Primary Sample and Descriptive Results

For our primary analysis we restrict our sampling frame to public 4-year land-grant institutions.[9] We further restrict the donor pool to ensure we have a consistent sample for all outcomes over time. This requires a complete panel for all outcome variables and also for our covariates during the pre-treatment period (so they can all be matched on for each pre-treatment year specified). Figure A.1 in the online appendix maps our final primary donor pool institutions (listed in Table A.1). Most of the sample loss is due to the voluntary nature of reporting of enrollments by student residence in odd years, as discussed in the the previous section.

Figure 1 provides visual evidence of the WM policy effect on in- and out-of-state enrollment levels. In panel A, we see that the in-state enrollment did not change across the policy threshold for WM. It also remained constant or increased slightly over time for donor pool schools as seen in panel B. Panels C and D show OSS enrollment trends. In 2015, the number of OSS at WM was just above 2,000. This number increased by 50% to over 3,000 by 2019 (panel C). There is some evidence of OSS enrollments trending upwards over time (panel D), but nothing as drastic as seen at WM. Figure A.2 in the online appendix plots these trends for proportion of in- and out-of-state students. Figure A.3 also reports summary graphs on our model covariates and here we see that in the years before the WM policy, our treated unit generally falls within the support of the donor pool. Finally, Figures A.4–A.6 in the online appendix show that the same is true for our outcome variables: OSS enrollment, revenue from tuition and fees, financial aid, and academic quality outcomes.

[9] A U.S. land-grant college or university is an institution that has been designated by its state legislature or Congress to receive the benefits of the Morrill Acts of 1862, 1890, and 1994. The original designation of these institutions reflected a growing demand for agricultural and technical education in the U.S. and was intended to provide a broad segment of the population with a practical education. In section 5.2 we relax this restriction on the sample of untreated institutions.

4 Methodology

Our preferred estimation strategy is a bias-corrected synthetic control method (SCM). As a robustness check we also present results using a synthetic difference-in-differences (SDiD) estimator (Arkhangelsky et al., 2021), and juxtapose both sets of estimates against those from a two way fixed effects (TWFE) estimator with a difference-in-differences research design.

Synthetic control methods (SCMs) (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010, 2015) are widely-used in applied research to estimate the effects of policy interventions in cases with few or even one treated unit(s), when many regression-based approaches may be inappropriate. Unlike difference-in-differences research designs, SCMs do not rely on a parallel pre-trends assumption; and they are explicit about the contribution of each untreated unit to the counterfactual estimates, making those estimates transparent and easily interpretable (Abadie, 2021). For these reasons, we argue that a synthetic control estimating strategy is ideal for estimating the effects of increasing out-of-state enrollment.[10]

The idea underlying SCMs is that, for any "treated" unit (affected by a policy intervention), the effects of treatment can best be estimated by comparing the evolution of an outcome of interest to the combined evolution of that outcome in otherwise-similar but untreated "donor pool" units. Given a set of specified "predictors" of the outcome of interest during the pre-treatment period, SCMs estimate positive weights for a subset of donor pool units, such that the evolution/trajectory of the weighted average of untreated-unit outcome values (the "synthetic control") will be nearly identical to that of the associated treated unit during the pre-treatment period.

Under fairly general assumptions and a good pre-treatment fit, the synthetic control trajectory serves as a plausible estimate of the counterfactual trajectory for the treated unit during the post-treatment period. The difference between the trajectories of the treated unit and its synthetic control in a given post-treatment period is the estimated effect of the policy intervention. Causal inference can be conducted by permuting treatment across the donor pool units and comparing the trajectory of the estimated effect to the distribution of placebo treatment effects. We point interested readers to Abadie (2021) for a formal exposition of the synthetic control method (and to Wiltshire (2022) for practical details on implementation of the bias-correction procedure).

We normalize our outcome variables to 100 in 2015, the final pre-treatment year. To ensure our estimated synthetic controls are similar to University of Wisconsin, Madison, we include as covariates the non-normalized 2015 values of freshman out-of-state enrollment, institutional grant aid, financial aid received by full-time in-state freshmen from households earnings under $30,000/year, and the level and share of full-time freshmen receiving Pell Grants, and for each outcome include several (normalized) pre-treatment year values of the outcome as predictors. In our preferred specification, and to capture potentially important variation in student demographics and international student enrollment, we additionally include as covariates the 2015 shares of undergraduates who

[10] Our synthetic control method applies a procedure to address bias resulting from pairwise matching matching discrepancies among predictor variables.

are male, of undergraduates who are under 25 years old, and of undergraduates who are Asian (we also present estimates without these covariates as a robustness check on our results).[11]

We follow Abadie, Diamond, and Hainmueller (2010) and estimate the synthetic control weights, $w_2, ..., w_{J+1}$, on our J untreated/donor universities to minimize the distance between the synthetic control values of the specified predictors and the predictor values at the University of Wisconsin, Madison, given a separate set of weights, $v_1^i, ..., v_k^i$, that determine the relative importance of the predictors. We impose $w_j \geq 0$ and $\sum_{j=2}^{J+1} w_j = 1 \;\forall\; j \in \{2, ..., J+1\}$, which are standard restrictions in most synthetic control applications.

We then apply the synthetic control bias-correction proposed by Abadie and L'Hour (2021) and Ben-Michael, Feller, and Rothstein (2021), to mitigate potential bias resulting from differences in predictor variable values between the University of Wisconsin, Madison and its synthetic control donors. This bias-correction procedure is detailed in Wiltshire (2022), which describes the Stata package, `allsynth`, that we use to estimate our synthetic control results.

The most widely examined and adopted inferential approach for synthetic controls, developed in Abadie, Diamond, and Hainmueller (2010, 2015), generates p-values based on distributions of the ratios of the mean squared prediction error (RMSPE) calculated by permuting treatment across untreated units and estimating placebo treatment effects. We primarily adopt this inferential approach, and to ensure our p-values are conservative, we do not remove any donor pool units with a poor pre-treatment fit. Given this choice, and given we have a single treated university and just 39 donor pool universities, our tests are underpowered. To help mitigate this issue, where appropriate we adopt one-sided tests which can substantially increase the statistical power (Abadie, 2021). Specifically, we posit that any detectable effect of the policy on OSS enrollment, tuition revenues, and financial aid awarded will be positive, and conduct one-sided tests for those outcomes. We are agnostic about the sign of any effect on the remaining outcomes of interest, and so conduct two-sided tests for those outcomes. In all cases we view an RMSPE p-value of ≤ 0.1 as indicative of statistical significance given the relatively few donor pool units and given we construct our p-values to be conservative and never approach zero (e.g. even when WM has the largest RMSPE relative to the empirical distribution of placebos, the associated p-value will be $rank(RMSPE_{WM})/N = \frac{1}{N} > 0$).

We also present estimates of the treatment effects on the (normalized) outcomes of interest using the Synthetic Difference-in-differences (SDiD) estimator (Arkhangelsky et al., 2021) along with p-values from the prescribed placebo variance procedure, implemented using the `sdid` Stata package (Clarke et al., 2023). Tests are again one- or two-sided as with the synthetic control results. The synthetic control covariates are included but have little effect on the SDiD estimates as they are pre-treatment averages observed in each institution, and as such are effectively controlled for by unit fixed effects.[12]

[11] Including the share that are Asian may also help capture differences in the size of the international student body, which may also have implications for tuition revenues and student outcomes.

[12] This selection of covariates maintains consistency with the synthetic control specifications and also ensures there is

Finally, for comparative purposes, we use OLS to estimate the model:

$$Y_{it} = \gamma_i + \lambda_t + \sum_{s=2016}^{2019} \beta_s \mathbb{1}[s = t] \times D_i + \varepsilon_{it}$$

where Y_{it} is the (normalized) outcome value of interest for institution i at time t, γ_i and λ_t are respectively institution and year fixed effects, D_i is a dummy indicating whether i is the University of Wisconsin, Madison, treated in $t = 2016$, and β_s are the coefficients of interest we present.[13] We note that, with a single treated unit, OLS is not consistent for the β_s (Conley and Taber, 2011). Thus while we present the associated asymptotic p-values for reference, they should be interpreted with caution.[14]

5 Results

To study the causal effect of the 2016 policy, we present results from each of our estimators—including two synthetic control specifications, three different donor pool samples, and several post-treatment years—for each of our outcomes of interest.

5.1 Main Results

Our preferred estimates are the synthetic control estimates using the complete set of matching covariates, presented in Table 1 and Figures 2–4. We examine the effect of the policy on 2019 outcomes.[15] We first present results on our "first-stage" outcomes, including out-of-state student enrollment and tuition revenues, in Figure 2 and section 1 of Table 1. This allows us to check whether the policy had the intended effect of increasing institutional revenues by increasing OSS enrollment. In Figure 3 and section 2 of Table 1 we then examine effects on financial outcomes, including institutional grant aid, financial aid to students from households earning $\leq \$30,000$, and published tuition fees for in-state and OSS, separately. This serves to check whether any effects seen in our first-stage resulted in other policy changes that would have directly affected students financially. Finally, in Figure 4 and section 3 of Table 1 we consider the effects on academic outcomes including in-state freshman enrollment, the retention rate of full-time students, the student-to-faculty ratio, and the 4-year graduation rate. This serves to check whether any first-stage effects had consequences for academic policies that could impact access or academic quality for students.

no bias from using "bad controls".

[13] The inclusion of the synthetic control covariates makes no difference to our coefficients of interest as they are pre-treatment averages observed in each institution, and are thus effectively controlled for by the unit fixed effects.

[14] We again conduct one- or two-sided tests consistent with our approach for our other estimators.

[15] This year was chosen because it offers us the most recent effect of the policy, while remaining free from the effects of the COVID-19 pandemic. All enrollment decisions were made in the fall 2019 term. We have confidence that measurement of our outcome variables was unaffected by the pandemic. However, as one of several robustness checks we also present estimates focusing on the 2018-2019 year in the robustness section.

Figure 2 shows the evolution of synthetic control "gaps" for both OSS enrollment (Panel A) and overall tuition revenue (Panel B), for WM (in blue) and all donor schools (in gray) in each post-treatment year. Both graphs show large increases beginning in 2016, the year the policy took effect. Table 1 quantifies the magnitude of those increases in 2019, showing the policy caused an increase in OSS enrollment of 28.9 percent (RMSPE p = 0.05) and increase in overall tuition revenue collected by WM of 46.8 percent (RMSPE p = 0.075).[16]

To examine whether this increase in tuition comes from changes at the intensive or extensive margin, we then examine the impact of the policy on both in and out-of-state published tuition fees. Panels A and B of Figure 3 present SC gaps for these outcomes. We see that there is no published impact on in-state tuition, although there visually appears to be a modest increase in published out-of-state tuition. Table 1 confirms there no significant impact on either, with a point estimate of -0.52 percent on in-state tuition fees (RMSPE p-value of 0.775). The estimated impact on OSS tuition is 11.6 percent, but RMSPE p-value of 0.25 indicates this estimate is not statistically significant. This leads us to conclude that increases in tuition revenue which we estimate were caused by the 2016 policy come from changes in OSS enrollment.

We next study how this increase in revenue impacted the amount of financial aid dispersed at WM. Previous work has shown that public universities use increases in tuition to subsidize low-income students (Shih, 2017; Cook and Turner, 2022). Figure 3 presents the synthetic control gaps for institution grant aid disbursed, with Panel C showing overall institutional grant amounts awarded to freshman and Panel D showing financial aid awarded to low-income students whose families earn less than \$30,000 a year. Both panels capture a large, distinct increase after the 2016 policy. Table 1 shows significant treatment effects of 29.4 percent (RMSPE p = 0.075) and 24.1 percent (RMPSE p = 0.10), respectively. We therefore conclude that the the increase in tuition allowed WM to support more students, and particularly to provide more financial support to low-income students.

Previous work has reported that OSS students place a burden on universities' academic quality, negatively impacting in-state students (Jacquette, 2017). We thus next examine effects on outcomes that might be impacted by increased OSS enrollment, with a focus on measures that capture elements of academic quality. While we admit that these variables are very coarse and may not represent perfectly accurate measures of academic life at the university, we view them as important proxies of academic quality during this period.

Figure 4 presents the SC gaps for each of our academic outcomes. We see no indication that the 2016 policy impacted retention at WM (Panel B). The plots for in-state enrollment (Panel A), student-to-faculty ratios (Panel C), and 4-year graduation rate (Panel D) all show some mild movement post-treatment. However, Table 1 shows that the 2019 estimated effects of -2.6 percent (RMSPE p = 0.275), -9.7 percent (RMSPE p = 0.85), and 7.4 percent (RMSPE p = 0.45), respec-

[16]The across-the-board zero gaps in 2012 and 2013 for all results in Figures 2–4 follow mechanically from the bias-correction procedure given the inclusion of outcomes in those years as predictors. See Wiltshire (2022).

tively, make clear that none of these estimates are statistically significant. Panel B of Figure 4 and the values in Table 1 (-0.2 percent, RMSPE *p*-value of 0.45) show no change in the retention rate. If we focus particularly on the plot for in-state enrollment, we can see a small u-shape in the post-treatment period, but in fact none of these point estimates from any year are statistically significant. In summary, we conclude that the 2016 policy did not place a negative burden on WM or on its students' academic outcomes.

5.2 Robustness

To test the robustness of our main findings, in Tables A.2–A.4 of the online appendix we present results based on various tests and alternative specifications and donor pool samples. These include: re-running our synthetic control estimation for 2019, excluding the covariates for student sex and age and international enrollment (column 1 of Tables A.2–A.4); estimating treatment effects in 2018 to demonstrate that our main findings are not contingent on selecting 2019 to measure our outcomes (column 2); changing our donor pool sample to R1 and R2 universities (column 3) and all public four-year universities (column 4), both subject to the restriction that a complete panel is observed for all included institutions; using a two way fixed effects (TWFE) estimator (column 5); and using a synthetic difference-in-differences (SDiD) design (column 6). We note that while we present TWFE estimates for completeness, the validity of the standard errors for these treatment effects assumes homoskedasticity across units and normality of the estimand (Arkhangelsky et al., 2021), and therefore should be interpreted with a degree of caution. For the SDiD estimates, we present *p*-values estimated using the placebo variance (Arkhangelsky et al., 2021).

Looking at our first stage outcomes in Table A.2, the magnitudes and significance levels appear similar to those from our primary specification. The point estimates generally grow larger as we expand the donor pool to R1 and R2 universities and to all public four-year universities, though we note that for the R1+R2 analysis, only, the estimates for OSS enrollment is no longer significant (p = 0.362). When we expand the donor pool even more, to include all public four-year universities, the point estimates for OSS enrollment grow even larger and regain significance (p = 0.01). The estimated effects on tuition revenues are significant across the board. Looking at our financial outcomes in Table A.3, we find a similar pattern: most point estimates resemble those from our primary specification. The estimated effect on average institutional grant aid awarded loses significance for the R1+R2 analysis, only (p = 0.246) and regains significance for the analysis using all public four-year universities in the donor pool (p = 0.02). The point estimates on average student aid awarded to students from low-income households are all similar in size to our primary estimates, though they are somewhat noisier, with the *p*-values in columns (1) and (4) slipping to p = 0.13 and p=0.12, respectively, and that for the SDiD estimate reaching p = 0.29, suggesting a degree of caution is in order. Additionally, the SDiD estimated effect on out-of-state tuition and fees positive and marginally significant despite being smaller than our preferred estimate. Lastly, all of the estimates looking at our academic outcomes, presented in Table A.4, are similar to those from our primary specification.

6 Discussion and Conclusion

In this paper, we investigate a 2016 policy at the University of Wisconsin, Madison that removed the limit on the number of out-of-state students it could enroll. Using a synthetic control approach, we estimate the policy led to a significant increase in the proportion of out-of-state students admitted to the university by 28.9 percent as well as an increase in tuition revenue by nearly 50 percent. We also find an increase in the amount of financial aid distributed to students—importantly for those whose families earn less than $30,000 a year—by 29.4 and 24.1 percent, respectively. We fail to detect significant effects on several educational outcomes that measure academic quality at universities, including retention, student-to-faculty ratios and graduation rates. Given these results, we view concerns about the negative impacts on academic quality from out-of-state students as misplaced.

Furthermore, under increasingly tight budget constraints experienced by universities, we see these results as confirmation that out-of-state students represent a much needed financial resource. This is especially true if universities are trying to fund higher education for low-income students (Cook and Turner, 2022) and those who might be constrained by how high of a tuition level they can set (Miller and Park, 2022).

A clear policy implication from this study is that universities should reconsider limits on out-of-state enrollment, especially for institutions facing decreases in state support for higher education. One limitation of this study is that we cannot observe these outcomes in a scenario where limits on out-of-state enrollment are removed from each university. While previous research has found that this would be efficient from a national perspective (Knight and Schiff, 2019), public universities exist for the benefit of individual states and treatment effects may look different under a system where limits on OSS are removed entirely.

A further limitation of this study is that it focused on a selective public university in the Midwest. It may be the case that demand for university admission at the University of Wisconsin, Madison is higher than most other public universities both in the Midwest and other regions. While increasing out-of-state enrollment is likely to increase revenue, as out-of-state students are often charged more than in-state students, it is unclear whether this increase in out-of-state students would impact academic quality at other universities. Therefore, we caution our results against extrapolation to other settings. This warrants further research to estimate impacts on universities with different characteristics.

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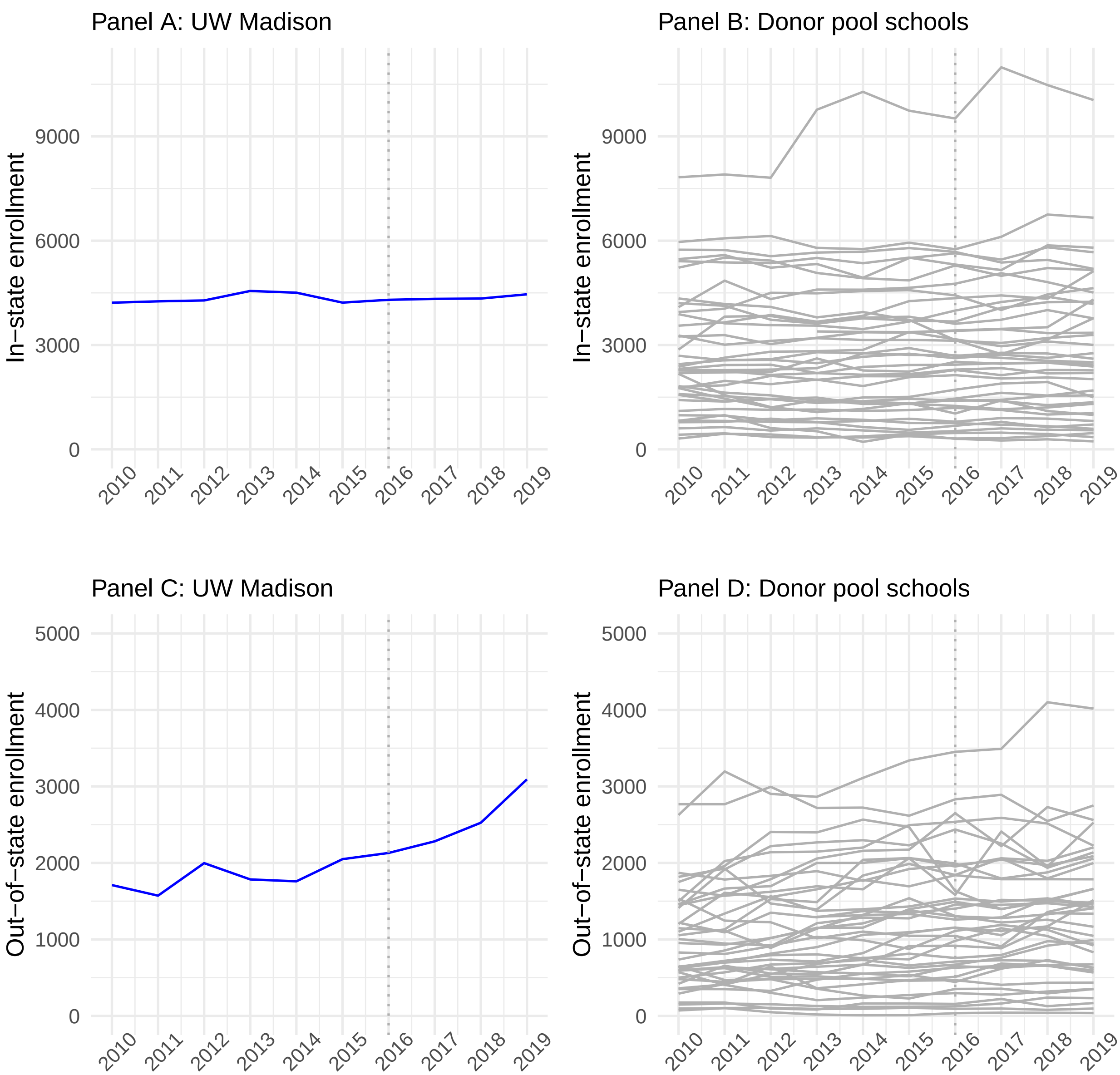
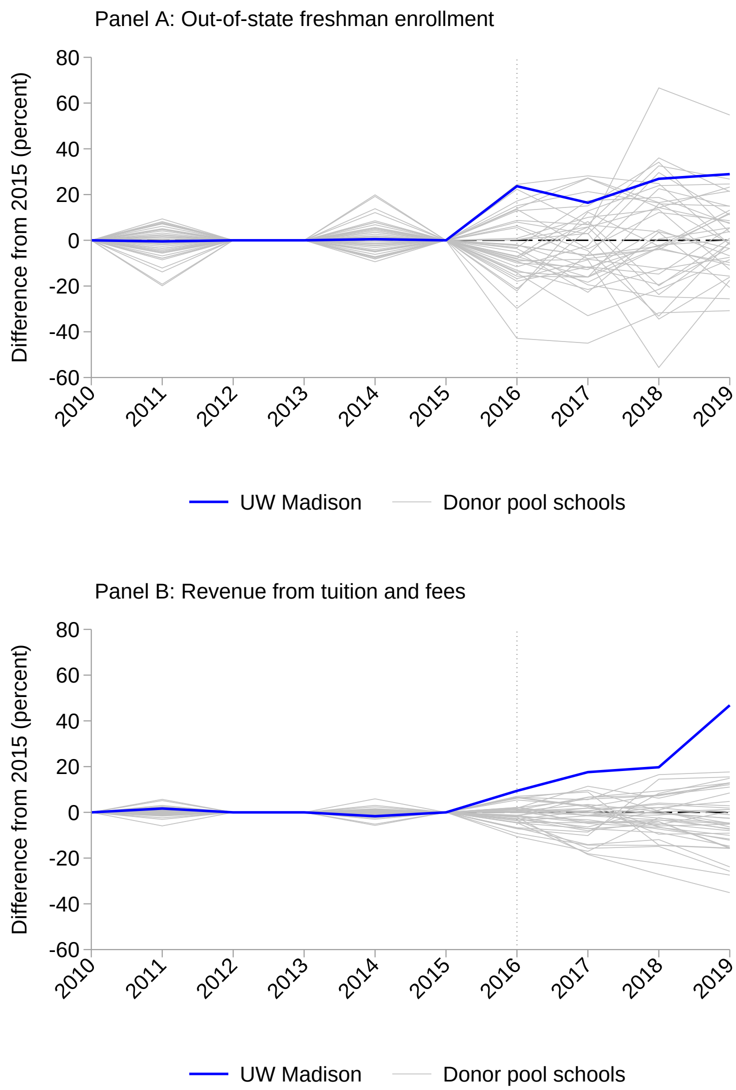
Graphs and Tables

Figure 1: In- and Out-of-State Freshman Enrollment Levels



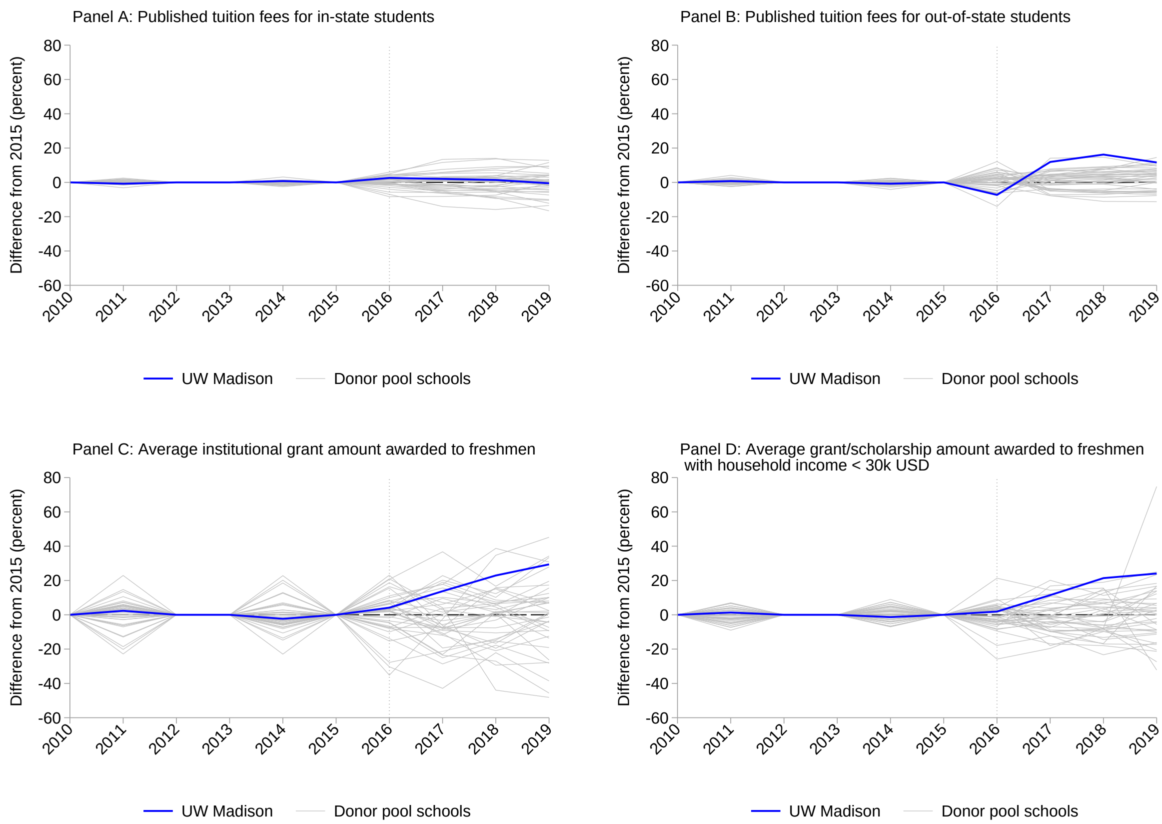
Note: The dotted vertical line shows 2016, the first year of treatment. Values calculated using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019.

Figure 2: First Stage Results



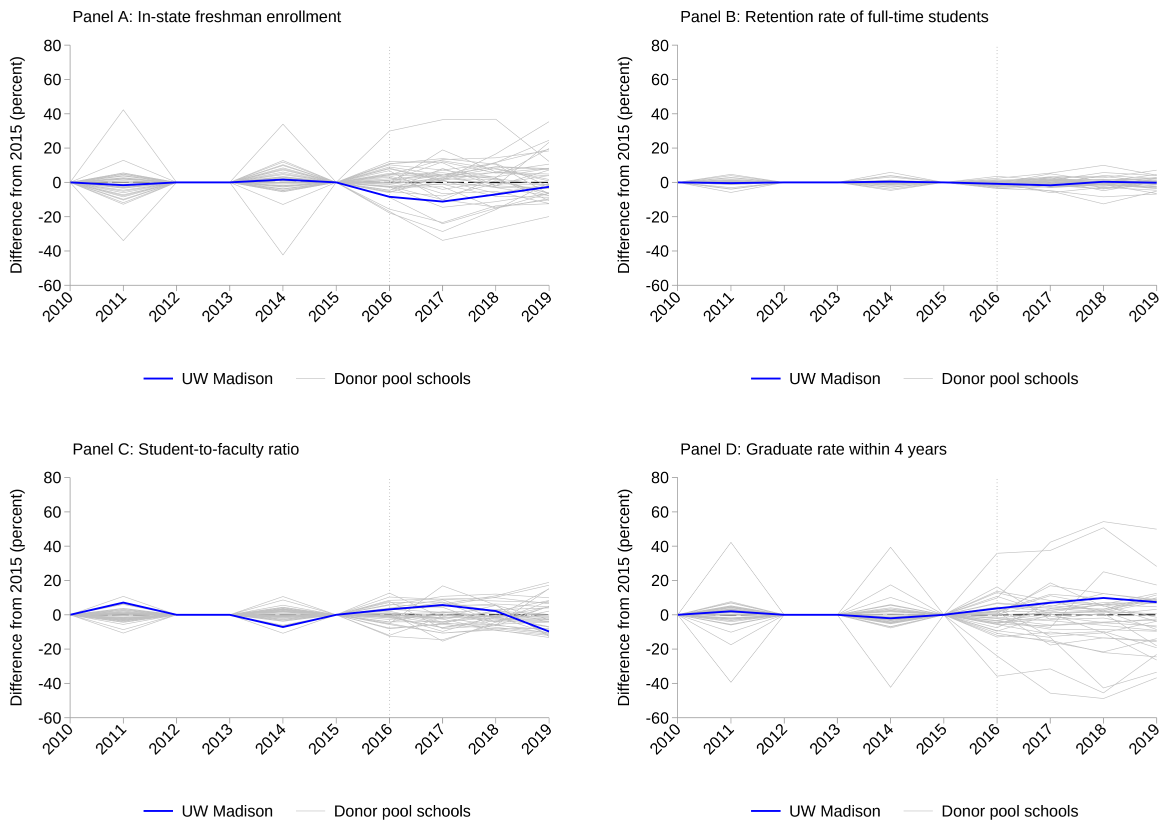
Note: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019. The y-axis shows the percent difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The solid blue line shows the estimated effect at the University of Wisconsin, Madison. The dark grey lines show the 39 placebo treatment effects, estimated by permuting treatment "in space", across the donor pool universities, then taking the difference between the outcome values of the placebo treated unit and those of its synthetic control. The vertical dotted line shows 2016, the first year of treatment. The results are corrected for bias from matching discrepancies.

Figure 3: Financial Outcomes Results



Note: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019. The y-axis shows the percent difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The solid blue line shows the estimated effect at the University of Wisconsin, Madison. The dark grey lines show the 39 placebo treatment effects, estimated by permuting treatment "in space", across the donor pool universities, then taking the difference between the outcome values of the placebo treated unit and those of its synthetic control. The vertical dotted line shows 2016, the first year of treatment. The results are corrected for bias from matching discrepancies.

Figure 4: Academic Outcomes Results



Note: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019. The y-axis shows the percent difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The solid blue line shows the estimated effect at the University of Wisconsin, Madison. The dark grey lines show the 39 placebo treatment effects, estimated by permuting treatment "in space", across the donor pool universities, then taking the difference between the outcome values of the placebo treated unit and those of its synthetic control. The vertical dotted line shows 2016, the first year of treatment. The results are corrected for bias from matching discrepancies.

Table 1: Main results

	Estimates
1. First Stage Outcomes	
Out-of-state Freshman Enrollment	
Treatment Effect (%)	28.945
Ranked-RMSPE-based p-value*	0.050
Revenue from Tuition and Fees	
Treatment Effect (%)	46.820
Ranked-RMSPE-based p-value	0.075
2. Financial Outcomes	
Published In-state Tuition Fees	
Treatment Effect (%)	-0.521
Ranked-RMSPE-based p-value	0.775
Published Out-of-state Tuition Fees	
Treatment Effect (%)	11.636
Ranked-RMSPE-based p-value	0.250
Average Institutional Grant Awarded	
Treatment Effect (%)	29.407
Ranked-RMSPE-based p-value*	0.075
Average Financial Aid Awarded to Students from Households Earning $< \$30k$	
Treatment Effect (%)	24.063
Ranked-RMSPE-based p-value*	0.100
3. Academic Outcomes	
In-state Freshman Enrollment	
Treatment Effect (%)	-2.584
Ranked-RMSPE-based p-value	0.275
Full-time Retention Rate	
Treatment Effect (%)	-0.192
Ranked-RMSPE-based p-value	0.450
Student-to-faculty Ratio	
Treatment Effect (%)	-9.737
Ranked-RMSPE-based p-value	0.850
4-year Graduation Rate	
Treatment Effect (%)	7.379
Ranked-RMSPE-based p-value	0.450
N	40

Note: Estimated effects in 2019 using data from IPEDS, with the set of control universities restricted to those with a complete panel for the full set of covariates. Section 1 contains first stage outcomes. Section 2 contains financial outcomes. Section 3 contains academic outcomes. Column (1) presents our preferred estimates—the bias-corrected synthetic control estimates using the full set of covariates. For each outcome, Row (1) presents estimated treatment effects and Row (2) presents p-values from ranking the RMSPEs of the empirical distribution of in-space placebo treatment effects through 2019 (for the synthetic control estimates). p-values marked with a * are one-sided; the remainder are two-sided.