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Lifting the cap on non-resident university enrollment: evidence from Wisconsin

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Abstract
The economics literature disagrees about the effects that non-resident (i.e., originating outside of the state) students have on potential resident students at public universities. Despite paying a premium to attend state universities, non-resident students are often accused of negatively affecting academic quality and crowding out resident students. We present new evidence on this relationship by exploiting the removal of an enrollment cap on non-resident students at a highly ranked state flagship university. We find this policy yielded a 29% increase in non-resident enrollment (coming almost entirely from domestic—rather than international—students), and a consequent 47% increase in tuition revenue which funded large increases in financial aid disbursed at the university, particularly for low-income resident students, indicating that non-resident students cross-subsidize lower-income students. We find no evidence of negative effects on several measures of academic quality or resident-student enrollment. These results imply that individual universities with excess capacity could in principle benefit their lower-income resident students by removing caps on non-resident-student enrollment.

Keywords Higher education finance · Non-resident students · Regional migration · Enrollment caps

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1 Introduction

Public universities in the USA face high demand from non-resident students (NRS)—international and out-of-state domestic students—who often pay a large premium relative to resident (in-state) students. Over the past 25 years, many of these institutions consequently responded to declining state financial support per pupil by dramatically increasing NRS enrollment (Snyder et al. 2017; Bound et al. 2021; Hillman 2023; National Science Board 2023). This has prompted widespread public and academic concern that resident students at public universities have been crowded out or academically hurt by these policies (Bound et al. 2020; Jacquette et al. 2016; Mathias 2020; Anelli et al. 2023). Despite evidence indicating that international student enrollment helped support domestic student enrollment (Shih 2017), political pressure led the (external) governing bodies of several public institutions to impose caps on the number of NRS who could enroll each year.[1]

In this paper, we examine how resident students are affected by the removal of such a cap. In particular, we look at the impact on undergraduate resident financial aid and academic outcomes from a 2016 policy change at a highly ranked state flagship university that removed the existing limit on NRS enrollment—the University of Wisconsin, Madison (UWM). To estimate the causal effect of the policy on several key outcomes, we employ several estimating strategies including our preferred bias-corrected synthetic control approach. In this approach, our comparison unit consists of an optimally weighted average of control universities that are selected to best match UWM during the pre-treatment period. For our analysis, we utilize data from the IPEDS survey, which collects annual data on enrollments and many other variables from virtually every university in the country.

We find that lifting the NRS cap resulted in sharply higher NRS enrollment, higher tuition revenues, greater financial aid to resident students, and no evidence of negative impacts on academic outcomes. While the *percentage* increase in NRS was largest among international students, the *level* increase was largest among non-resident *domestic* students (who comprise the lion's share of non-resident students in public universities across the USA). This sharp increase in NRS enrollment provided substantially greater tuition revenues that were used to increase funding for resident low-income students. This was achieved without any clear negative effect on academic outcomes.

Specifically, we find that the policy increased both total and domestic NRS enrollment by nearly 29% four years after implementation. As a result, overall tuition revenue increased by 47% over that same time period. We fail to find any significant effects on either resident or non-resident tuition rates, indicating that this increase in revenue

[1] For example, during the 2016 presidential race Hilary Clinton declared, "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."

came from these newly admitted NRS.[2] As a result of the increase in tuition revenue, we find UWM spent 29% more on student grant aid, particularly for resident students from families that report making $30,000 or less.

We then examine whether this increase in NRS enrollment negatively affected academic outcomes. We find no significant negative impact on resident enrollment, student-to-faculty ratios, retention, or four-year graduation rates, which may suggest that UWM increased spending on educational inputs (Deming and Walters 2017). These results are robust to the use of different estimators and methods of inference, including placebo tests on the donor pools and on the timing of the policy. Our findings are also likely generalizable to most institutions that wish to increase NRS enrollment, as we demonstrate that most comparable institutions have excess capacity, potentially due to declining resident enrollment over the past decade.

This paper makes several contributions to two literatures. First, this paper contributes in several ways to the literature studying NRS enrollment and higher education finance. Previous research has documented that as state appropriations to higher education have fallen, universities have increased the number of NRS they enroll to make up the difference (Curs and Jacquette 2017; Bound et al. 2020). Yet estimating the effect of increased NRS enrollment on financial outcomes is difficult as these outcomes are all likely related to other potentially endogenous factors such as applicant's wealth, changes in the overall economy, or how generous a university's or state's financial aid packages are (Winters 2012; Curs and Singell 2002; Kerkvliet and Nowell 2012; Lovenheim 2011; Kim 2020).

We contribute to this literature by adopting a research design which exploits the exogenous removal of a binding constraint on how many NRS could be admitted to the University of Wisconsin, Madison.[3] In this way, we are studying the effect of an exogenous, positive *supply* shock in NRS enrollment, in contrast to other papers which study changes that depend on shocks to NRS *demand* for higher education in the USA (Bound et al. 2020; Shih 2017). Our approach is more novel within the literature and also provides policy-makers with evidence about a margin (NRS enrollment caps) over which they have some control even in the absence of NRS demand shocks. We thus believe our results are relevant and of interest to universities (and their governing bodies) that face similar financial pressures and are currently constrained in their ability to admit NRS (including, for example, the many Canadian universities who have recently had caps on international students exogenously imposed).

Similar to research that specifically examines increases in *international* student enrollment (Shih 2017; Chen 2021), we find strong evidence that tuition revenue from NRS was used to subsidize resident low-income students, ultimately increasing the resources available to Wisconsin residents, in part due to the creation of the

[2] 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 resident and non-resident students on average. In this paper, "tuition" is used to refer to the tuition and fees variable.

[3] We maintain that the policy shock we exploit was exogenous, as it was decided by the Wisconsin Board of Regents which operates under the approval of the governor, rather than by university administrators (recent work studying the relationship between states and universities finds that they are often optimizing different objective functions (Groen and White 2004), supporting this conjecture). That is, the shock constituted the removal of a binding constraint on the UWM administration. The constraint need not have been binding, but if it had not been then its removal would have had no effect, contrary to our findings.

"Bucky's Tuition Promise" program three years after lifting the cap on NRS (Marifian et al. 2024). However, while those papers focus on international students, we find that essentially all of the growth in NRS caused by this policy came from higher *domestic* non-resident enrollment. This is an important result that has relevance for most public universities in the USA, where domestic NRS outnumber international students by about 6:1, on average, indicating that domestic NRS are a much larger source of non-resident demand (and, potentially, NRS tuition revenue). This contribution is, to our knowledge, novel in the higher education finance literature.

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 where students who depend on financial aid go to college. Our third contribution to the higher education NRS enrollment and finance literature is our finding that growth in NRS acts as a counter-weight to these negative consequences of constraints on tuition increases, essentially shifting out the university's budget constraint. Finally, Cook and Turner (2022), Bound et al. (2020); Shen (2016) document an increase in price discrimination at universities, such that students who have a higher ability to pay are charged more than low-income students. Our results contribute to this literature by demonstrating that the significant premia paid by NRS plays an important role in universities' ability to support low-income students.

The second broad literature to which we contribute is the work studying how non-resident students affect the academic quality of universities and experiences of resident students. Research in this area has found that increases in NRS place academic burdens on universities (Jacquette 2017; Jacquette et al. 2016; Curs and Jacquette 2017), making the decision to enroll more NRS more challenging. Additional research has found that increases in NRS may also crowd-out resident-student enrollment (Curs and Jacquette 2017); Shen (2016), and argues that attendance at universities is a zero-sum game between resident and NRS. We contribute to this literature by demonstrating that concerns about increases in NRS enrollments at state universities may be misplaced. While we estimate small negative effects on resident enrollment, these results are robustly non-significant, indicating no compelling evidence that greater NRS enrollment crowds-out resident students. When we look at measures of academic quality, we also detect zero significant negative effects.[4]

In summary, we find that removing a constraint on non-resident enrollment results in a large increase in *domestic* non-resident enrollment and sharply higher tuition revenues which are used to increase financial aid, notably to low-income resident students, all with no evidence of negative impacts on resident-student enrollment or academic outcomes.

The remainder of this paper is structured as follows: Section 2 describes the University of Wisconsin, Madison, and the policy change which removed the limit on non-resident enrollment. Section 3 summarizes and details the data we use. Section 4 discusses our synthetic control estimation procedure and our alternative estimators. Section 5 presents our main findings and considers the robustness of our results. Section 6 concludes.

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

2 Policy environment

The University of Wisconsin, Madison, is a large public research university located in southern Wisconsin. As of 2022, it is undergraduate enrollment totaled 37,235, with an average GPA and SAT score for incoming freshman of 3.86 and 1390, respectively, making admissions to UWM competitive. UWM is also part of the University of Wisconsin system, which consists of UWM and 12 other universities. The University of Wisconsin system is overseen by a governing body known as the Board of Regents comprised of 18 members, 16 of whom are appointed by the governor of Wisconsin. Board members are subject to approval by the state senate and serve for seven-year terms. The Board of Regents decides levels of public funding from the state for each member university and dictates other features of the University of Wisconsin system, such as non-resident enrollment levels.

Prior to 2016, the University of Wisconsin Board of Regents had capped NRS enrollment at 27.5% of the student body. Exemptions from this policy included students coming from Minnesota, who were treated by the university as resident students. The rationale from the Board 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 UWM students from Wisconsin live in the state after graduation, whereas only 15% of NRS do so.

In October 2015, however, the University of Wisconsin Board of Regents voted to remove the limit on NRS enrollments at UWM beginning in the 2016 academic year. According to documents from the October meeting, the purpose of this removal was to counteract decreasing high school graduation trends in Wisconsin.[5] At the time, other public universities—including the University of North Carolina and the University of California systems, the University of Maryland, Ohio State, and others—also maintained caps on out-of-state student enrollment. In addition to this rationale, previous work has documented that NRS can also add a significant boost to revenue, as they often pay high premiums to attend out-of-state universities. For example, at UWM, resident students pay $10,720 per year, while non-resident students pay $39,427 per year for the 2022–2023 school year. The NRS enrollment policy took effect in the fall of 2016.[6] This policy removed any constraint on the number of NRS that UWM could enroll each year. We use this policy change to study three research questions; (1) whether it caused an increase in NRS enrollment, (2) what were the financial impacts of this policy and, (3) what academic impacts did this policy have on the UWM student population?

An important additional consideration in this policy environment is the introduction of UWM's "Bucky's Tuition Promise" program (BTP). BTP was implemented in Fall 2018, two years after the removal of the cap on NRS enrollment took effect.[7] Named

[5] Details on the rationale can be found in the meeting minutes for the October 2015 Board of Regents meeting: https://www.wisconsin.edu/regents/download/meeting materials/2015/october 2015/October-2015-Education-Committee-pdf-corrected-1007.pdf.

[6] 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.

[7] The much smaller "Badger Promise" program was implemented in 2017, offering tuition and fee coverage to first-generation in-state students transferring from one of UW's two-year colleges.

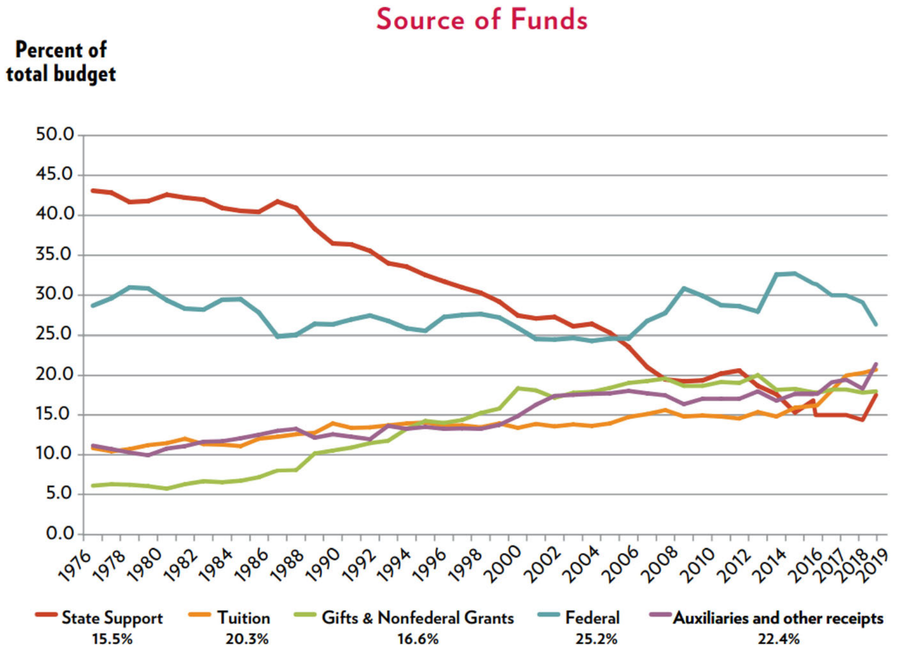

Fig. 1 University of Wisconsin, Madison, budget funding sources. *Note*: Sourced from the UWM 2019–2020 Budget Report: https://mbo.wisc.edu/wp-content/uploads/sites/194/2020/10/Budget-in-Brief BoR 2020 V6-1.pdf

after the UWM mascot, Bucky's Tuition Promise (detailed in Marifian et al. (2024)) provides four years of full tuition and fee coverage for in-state students from lower-income households. It was expected the program would benefit around incoming 800 students each year at a total annual cost of \$3.3 million once full implemented.[8]

Budget documents and public comments from UWM's then-chancellor, Rebecca Blank, indicate this program was financed through a combination of private donations and institutional resources (operating revenues, including tuition), rather than state tax revenue.[9]

One possible concern about our analysis of the financial aid impact of lifting the NRS enrollment cap may thus be that we are inadvertently capturing the effects of BTP. Furthermore, given that part of the program's funding derives from private donations/gifts, it might be argued that BTP's influence on financial aid outcomes is unrelated to UWM's internal financial resources. However, Fig. 1 makes clear that, despite substantial private donations received by the UW system during this period, the proportion of UWM's budget funded by gifts remained relatively stable from 2000 onward, actually decreasing slightly from 19% in 2010 to 18% in 2019. In contrast,

[8] See, also, https://news.wisc.edu/in-first-year-buckys-tuition-promise-provides-free-tuition-to-796-incoming-uw-madison-students/

[9] The 2019 UWM budget document can be found here: https://mbo.wisc.edu/wp-content/uploads/sites/194/2020/10/Budget-in-Brief BoR 2020 V6-1.pdf.

the budget share funded by tuition rose significantly, from 15% in 2010 to over 20% in 2019. We explore below how this increase in tuition's share is likely tied to the lifting of the cap on non-resident enrollment at UWM.

Given that the UW system faced significant financial challenges following the Great Recession (Brady 2020), given the stability of budget share funded by gifts and the sharp increase in budget share funded by tuition, given the projected annual cost of the BTP program, and given the fact the program was by-design partially funded by operating revenues, it is our conjecture that Bucky's Tuition Promise may have only become viable with increased tuition revenue realized through lifting the NRS cap.[10]

3 Data, sample, and descriptive results

3.1 Data

To study NRS enrollment and university outcomes, we use the Integrated Postsecondary Education Data System (IPEDS). IPEDS is the primary source of information on post-secondary institutions in the USA. 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 are referencing the academic calendar start year (e.g., 2015-2016 would be described as 2015, except as indicated below).

Post-secondary 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 (USA or non-USA only) and by the US state of residence when the student was admitted are available for freshmen. Information stratified by student residency status in IPEDS is limited to enrollments only. The NRS group includes either international students or individuals from the US state that is different from the state of the institution they enrolled in. For UWM, we follow the practice of the university and treat students coming from both Wisconsin and Minnesota as resident. Institutional reporting of these data to IPEDS is only mandatory in even years. In odd years, a lot of schools choose not to submit this data, so we observe a lot of missing values. Retention rates are based on fall enrollment counts of returning first-time freshman

[10] We note that BTP's development was led by UWM's then-director of the Office of Student Financial Aid, Derek Kindle, who only joined UWM in July 2016, suggesting that BTP's development only properly started after UWM began realizing increased revenues from higher NRS enrollment.

undergraduates.[11] 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 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 for "in-state" and "out-of-state" students.[12] 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 freshmen. Finally, student financial aid by household income is further restricted to full-time freshmen paying resident tuition who were awarded any grant or student loan aid. Thus, our measure of financial aid for students from households earning less than $30,000 per year specifically measures aid to resident low-income students.

3.2 Primary sample and descriptive results

For our primary analysis, we restrict our sampling frame to public 4-year land–grant institutions.[13,14] 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 2 maps our final primary donor pool institutions (listed in Table 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 previous section.

[11] 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.

[12] These translate to "resident" and "non-resident" students, as in-state tuition and fees are charged only to students who meet the state's or institution's residency requirements. Everyone else is charged out-of-state tuition. Tuition and fees for a third category of students residing in the locality in which they attend school is also measured in IPEDS. While this may be a lower rate than in-state tuition offered by the institution, we do not see any differences between in-state and in-district tuition in our sample of schools, so we omit this category from our analysis.

[13] A US 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 USA and was intended to provide a broad segment of the population with a practical education.

[14] In Sect. 5.2 we relax this restriction on the sample of untreated institutions.

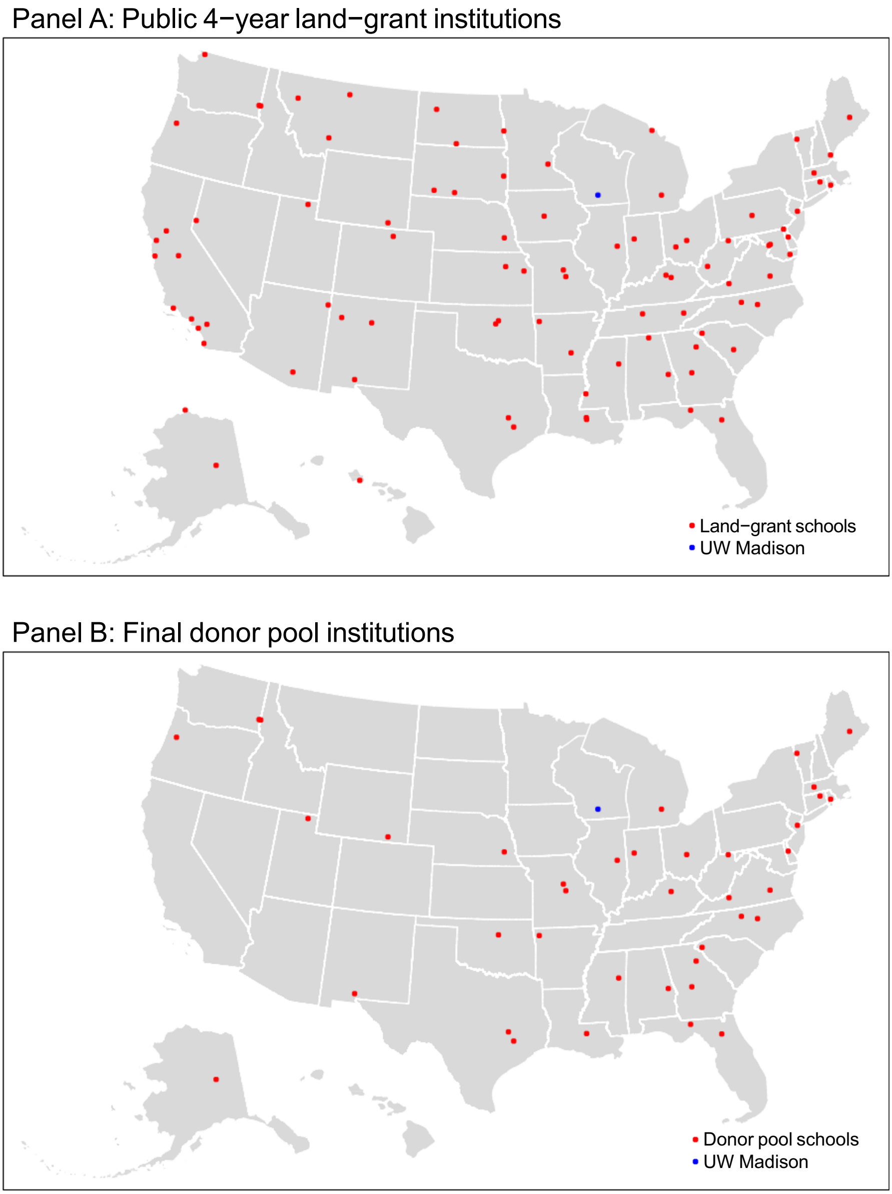

Fig. 2 Institution locations, primary sample. Panel A: Public 4-year land−grant institutions. Panel B: Final donor pool institutions. *Note*: Institution locations as reported in IPEDS. The primary donor pool consists of the 39 land–grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010 to 2019

Table 1 Primary control group/donor pool institutions and out-of-state enrollment donor weights

School	Donor weight
Auburn University	0
University of Alaska Fairbanks	0
University of Arkansas	0
University of Connecticut	0
Delaware State University	0
Florida Agricultural and Mechanical University	0
University of Florida	0
Fort Valley State University	0
University of Georgia	0
University of Idaho	0
University of Illinois at Urbana-Champaign	0
University of Kentucky	0
Louisiana State University and Agricultural & Mechanical College	0
University of Maine	0
University of Massachusetts-Amherst	0
Michigan State University	0
Mississippi State University	0
Lincoln University	0
University of Missouri-Columbia	0
University of Nebraska-Lincoln	0
Rutgers University-New Brunswick	0
New Mexico State University-Main Campus	0
North Carolina A & T State University	0.14386
North Carolina State University at Raleigh	0
Ohio State University-Main Campus	0
Oklahoma State University-Main Campus	0
Oregon State University	0
University of Rhode Island	0
Clemson University	0.31668
Prairie View A & M University	0
Texas A & M University-College Station	0
Utah State University	0
University of Vermont	0.00300
Virginia Polytechnic Institute and State University	0.52747
Virginia State University	0.00899
Washington State University	0

Table 1 (continued)

School	Donor weight
West Virginia University	0
University of Wyoming	0
Purdue University-Main Campus	0

List of institutions in our primary control group/donor pool. Includes land–grant universities outside Wisconsin and Minnesota that have annual observations for each of our covariates from 2010 to 2019 in IPEDS. The synthetic control donor weights are estimated for NRS enrollment using our preferred specification

Figure 3 provides visual evidence of the UWM policy effect on resident and non-resident freshman enrollment levels. In panel A, we see that resident (in-state) enrollment did not change across the policy threshold for UWM. It also remained constant or increased slightly over time for donor pool schools. Panel B shows NRS enrollment trends. In 2015, the number of NRS at UWM was just above 2000, increasing by 50% to over 3000 by 2019. There is some evidence of NRS enrollments trending upwards over time for donor pool schools, but nothing as drastic as seen at UWM. Online Appendix Figure A.1 plots these enrollment trends for shares of resident and non-resident students relative to all freshmen. Figure 4 then breaks out this trend in non-resident enrollment by domestic NRS (Panel A) and international NRS (Panel B). It is immediately clear that almost all of the UWM post-2015 increase in NRS comes from non-resident domestic students, and that this increase was highly unusual relative to the donor pool schools. The slight growth in international students after 2015 was not an unusual movement for UWM compared to the pre-treatment period. (The enrollment share is flat.) Online Appendix Figure A.2 further shows that non-resident domestic students grew from 25% of UWM freshmen in 2015 to 33% in 2019.

To alleviate concerns about a changing distribution of incoming freshmen as a result of the UWM policy, we present Online Appendix Figure A.3. The figure addresses the scenario of a potential drop in academic quality of students as more out-of-state students flood the school. The figure plots normalized math and verbal SAT scores of incoming freshmen at the 75th (panels A and B) and 25th (panels C and D) percentiles. For both UWM and almost all donor pool schools, and particularly for the verbal exam, there is an abrupt increase in student scores between 2016 and 2017. This is most likely an artefact that has nothing to do with student quality, as the SAT had undergone structural changes in March 2016. Despite the changes, there is no clear sign of a drop in academic student quality at UWM compared to other institutions. Past 2017, trends remain mostly flat (verbal) or even slightly positive (math). Further, to assess any changes in selectivity, Online Appendix Figure A.4 shows trends in % of students admitted at UWM and donor pool schools. At the beginning of our sample period, UWM granted admission to about 70% of applicants, which fell in the middle of the admission rate range among donor pool institutions.

Over time, UWM has become more selective and accepted about 58% of applicants as of fall 2015. This statistic puts UWM in the top quartile of the donor pool selectivity.

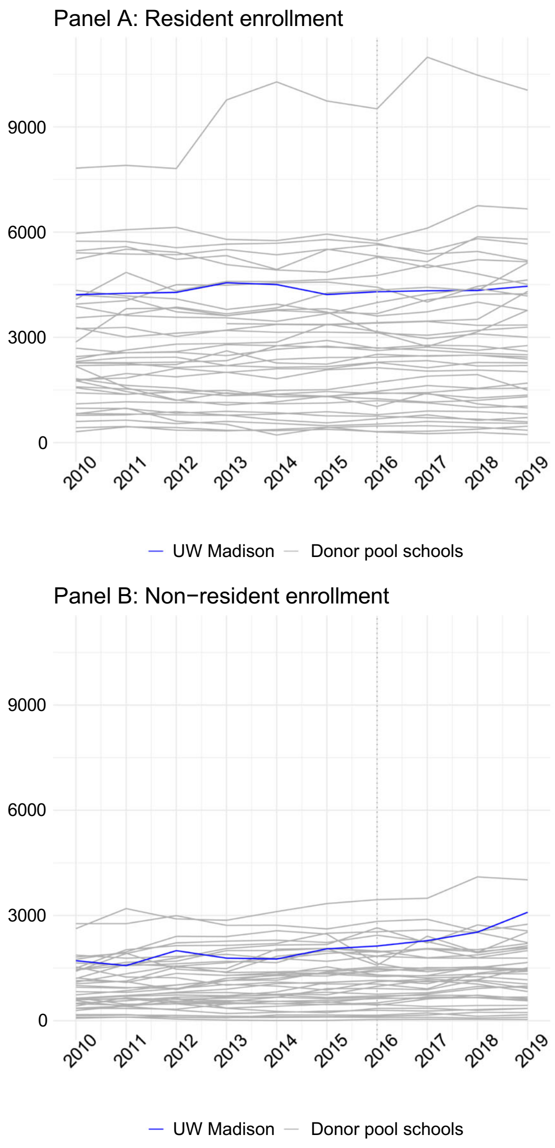

Fig. 3 Resident and non-resident freshman enrollment levels. *Note*: Values calculated using data from IPEDS. The dotted vertical line shows 2016, the first year of the UW Madison treatment. The primary donor pool consists of the 39 land–grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010 to 2019

It appears that UWM has the reputation to keep attracting high-quality students from outside the state and to maintain its selectivity of incoming freshmen.

Online Appendix Figure A.5 presents histograms for our model covariates, and demonstrates that in the years before the UWM policy the covariate values for our treated unit generally fall within the support of the donor pool. Finally, Online Appendix Figures A.6–A.8 show that the same is true for our outcome variables: NRS enrollment, revenue from tuition and fees, financial aid, and academic quality

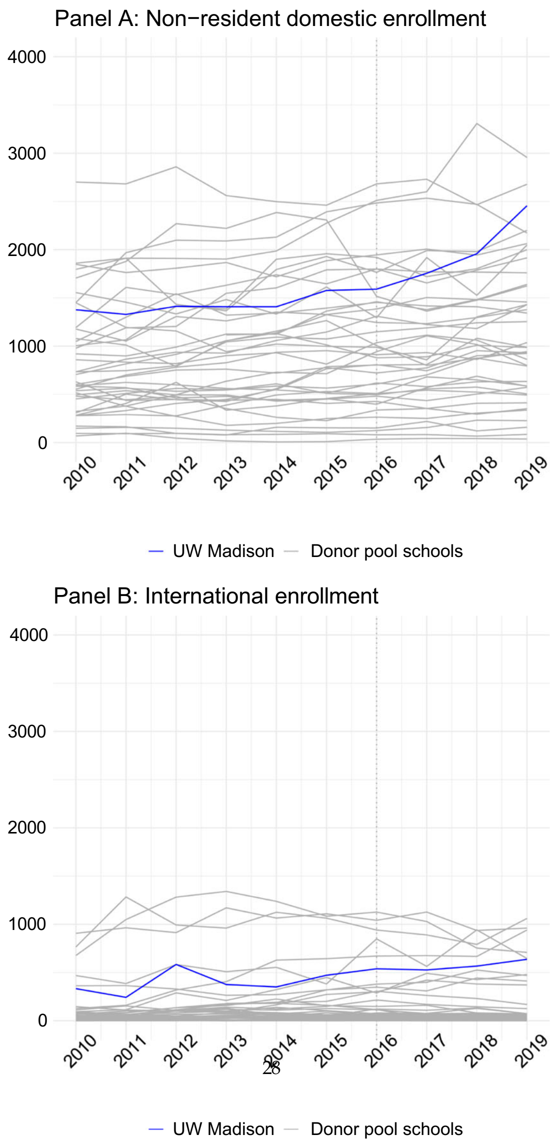

Fig. 4 Resident and non-resident freshman enrollment levels. *Note*: Values calculated using data from IPEDS. The dotted vertical line shows 2016, the first year of the UW Madison treatment. The primary donor pool consists of the 39 land–grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010 to 2019

outcomes. That is, UWM is highly representative of the land–grant universities in our primary donor pool.

One potential issue worth considering is how much capacity universities, including UWM, have to enroll additional students. To measure capacity, we note that IPEDS records the maximum number of students universities can supply with on- or off-campus residential facilities. Using the difference between this statistic and total enrollment as a measure of extra enrollment capacity, we find that in our primary sample of donors 94% of schools have the capacity to enroll more students.

For supplemental donor pool samples, between 80 (public 4-year) and 90 (R1+R2) % of universities have excess capacity to enroll students. Among schools able to enroll more students, the average excess capacity by donor pool sample ranges from 1500 to 3100. This is perhaps unsurprising as domestic enrollment in the USA has slightly decreased over the past decade, essentially leaving space for new NRS students at universities facing NRS limits.[15]

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 (DiD) research design.

4.1 Synthetic control

Synthetic control methods (SCMs) (Abadie and Gardeazabal 2003; Abadie et al. 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 NRS enrollment.[16]

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 highly similar to that of the associated treated unit during the pre-treatment period. Under fairly standard assumptions (discussed below) including 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 (Abadie 2021). 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.

[15] Results on enrollments are collected by the National Center for Education Statistics and can be accessed at https://nces.ed.gov/fastfacts/display.asp?id=98.

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

Formally, we observe an outcome of interest, $Y_{j,t}$, and k predictors of the outcome observed in the pre-treatment period, $X_{h,j} = (X_{1,j}, ..., X_{k,j})'$, for $J + 1$ units indexed by j and observed over all calendar years $t = \{2010, ..., 2019\}$. The University of Wisconsin, Madison, is $j = 1$, treated in $T0_1 = T_0 = 2016$, and the remaining $j = \{2, ..., J + 1\}$ units are a judiciously selected set of untreated, donor pool universities (each with $T_{0j} = \infty$ for $j > 1$). For each j, we normalize $Y_{j,t}$ such that $Y_{j,2015} = 100$ (where 2015 is the final pre-treatment year).[17]

For each j and t, define $Y_{j,t}^N$ as the potential outcome with *No* intervention by time t, and for $j = 1$ and $t \geq T_0$ define $Y\_\{1,t\}^{\wedge I}$ as the potential outcome under *I*ntervention.

Then, the marginal treatment effect of interest in $j = 1$ and $t \geq T_0$ is

$$\tau_{1,t} = Y_{1,t}^I - Y_{1,t}^N \tag{1}$$

We observe all $Y\_\{1,t\}^{I} = Y\_\{1,t\}$ in $t \geq T_0$, but we cannot observe what would have happened at the University of Wisconsin, Madison, in the absence of treatment. That is, we do not and cannot observe the counterfactual outcome values, $Y\_\{1,t\}^{N}$, in $t > T_0$. Thus, to estimate the treatment effect $\tau_{1,t}$, we must estimate $Y\_\{1,t\}^{N}$. Given weights $\mathbf{W} = (w_2, ..., w_{J+1})'$ on the donor pool units, the synthetic control estimators for $Y\_\{1,t\}^{N}$ and $\tau_{1,t}$ are, respectively:

$$\hat{Y}_{1,t}^N = \sum_{j=2}^{J+1} \omega_j Y_{j,t} \, \forall t \tag{2}$$

and

$$\hat{\tau}_{1,t} = Y_{1,t} - \hat{Y}_{1,t}^N \tag{3}$$

We follow Abadie et al. (2010) and select the weights 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 set of weights v_1^{1}, ..., v_k^{1} predictors that determine their relative importance.[18] That is, the synthetic control, $\hat{\boldsymbol{W}} = \left(\hat{\omega}_2, \ldots, \hat{\omega}_{J+1}\right)'$, is selected to minimize:

$$\left(\sum_{h=1}^{k} \upsilon_h \left(X_{h,1} - \omega_2 X_{h,2} - \cdots - \omega_{J+1} X_{h,J+1}\right)^2\right)^{1/2} \tag{4}$$

We impose $\omega_j \geq 0$ and $\sum_{j=2}^{J+1} \omega_j = 1 \forall j \in \{2, \ldots, J+1\}$, which are standard constraints in most synthetic control applications.

[17] For two robustness checks, we instead remove the pre-treatment mean from the level values and estimate the effects in level terms.

[18] We select the v^1 weights using the regression-based method (Kaul et al. 2022).

We then apply the synthetic control bias-correction proposed by Abadie and L'Hour (2021) and Ben-Michael et al. (2021) to mitigate potential bias resulting from pair-wise matching discrepancies in predictor values between the University of Wisconsin, Madison, and its synthetic control donors. This bias-correction procedure, which effectively residualizes the outcome variable values of the effects of the predictors in each period, is formally detailed in Wiltshire (2025a), which describes the Stata package, `allsynth` (written by one of us), that we use to estimate our results.[19] To clarify, if the reader is concerned about potential bias resulting from differences in the values of the predictors (including the covariates), this bias-correction procedure directly addresses this concern and mitigates the consequent bias by, in each period, adjusting the outcome values in UWM and each of its donor pool schools to account for those matching discrepancies.

The most widely examined and adopted inferential approach for synthetic controls, developed in Abadie et al. (2010, 2015), generates p-values for each outcome based on the relative position of the University of Wisconsin, Madison, in distributions of the ratios of the mean squared prediction error (RMSPE), calculated by first 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 (in our preferred specification), our tests are underpowered. To help mitigate this issue, where appropriate we adopt one-sided tests which can substantially increase the statistical power for synthetic control methods with relatively few donor pool units (Abadie and L'Hour 2021; Abadie 2021; Yan and Chen 2023).[20] Specifically, we posit that any detectable effect of the policy on NRS 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 RMSPE p-values to be conservative and never be zero (e.g., even when our treated unit, UWM, has the largest RMSPE relative to the empirical distribution of the J placebos, the associated p-value will be $\frac{R}{J+1} = \frac{1}{J+1} > 0$, where R is the rank of UWM's RMSPE relative to the empirical distribution of the placebos).[21,22] These p-values are also estimated using the `allsynth` Stata package.

[19] See Wiltshire (2025b) and Wiltshire et al. (ming) for examples of this procedure in action.

[20] Abadie (2021) observes "One-sided inference may result in a substantial of gain of power. This is an important consideration in many comparative case study settings, where samples are considerably small."

[21] With only 39 donor pool units in our preferred donor pool sample, our conservative algorithm would find $p < 0.05$ only in cases where $R = 1$, which we view as incredibly restrictive.

[22] It is unfortunately relatively common in the applied synthetic control literature to see reported *RMSPE* -ranked p-values based on algorithms (e.g., that implemented by Galiani and Quistorff 2017) which instead use $p = \frac{R-1}{J}$, which will always yield smaller p values than our approach and yields $p = 0$ regardless of how small J might be when $R = 1$. When J is large and $R \neq 1$, the differences are small. But R is rarely 1 and most environments in which synthetic controls are appropriate (including ours) involve smaller J, meaning the difference matters. E.g., with our preferred donor pool sample, $R = 3$ would yield $p = 0.075$ using our approach but $p \approx 0.05$ using the less-conservative algorithm. Removing placebos with

Identification of causal treatment effects essentially boils down to (i) the treated and synthetic control units being highly similar in the pre-treatment period, and (ii) the treatment being responsible for observed post-treatment differences. Abadie (2021) notes that the "contextual requirements" for synthetic control applications are largely necessary for any research design in a situation with relatively few treated units: (1) relatively-low volatility of the outcome variable; (2) availability of a group of untreated units that are similar to the treated unit; (3) no anticipation of treatment; (4) no interference across units (effectively the stable unit treatment value assumption, or SUTVA); (5) the convex hull condition (effectively, discrepancies between the treated unit and the synthetic control are small for all variables used to predict the synthetic control); and (6) the post-treatment time horizon is sufficient to allow treatment effects to be realized. Abadie and Vives-i Bastida (2022) prescribe additional "guiding principles" for synthetic control estimation, including (7) achieving a good pre-treatment fit (the quality of which can be partially assessed against pre-treatment periods left out of the calculation of the donor pool unit weights) over an extended pre-treatment period; (8) using a judiciously selected donor pool that is not too large (to reduce interpolation bias and the chance of overfitting); (9) achieving a sparse matrix of donor pool unit weights (allowing synthetic controls to be meaningfully interpreted); and (10) including relevant covariates.

Our approach meets each of these conditions: (1) In Sect. 3 we demonstrated the relative stability of the outcome variables; (2) to test various notions of comparability, we present results from a several different restrictions on the donor pool sample of untreated institutions; (3) anticipation is essentially impossible in our design as the policy decision was made in October 2015, *after* the beginning of the academic year immediately preceding enactment, and in any case was addressing a policy that prevented advance action; (4) we eliminate the possibility of interference by removing all institutions from Wisconsin and Minnesota from our donor pools;[23] (5) the convex hull condition is met for our primary estimates after we apply the bias-corrected synthetic control approach prescribed by Abadie and L'Hour (2021) and Ben-Michael et al. (2021) to mediate discrepancies in pre-treatment outcome values; (6) while we would have certainly preferred to estimate results over a longer post-treatment period, the COVID pandemic restrictions severely impacted cross-state enrollments, confounding possible estimates. Nonetheless, as we demonstrate in Sect. 5, the horizon was long enough for us to find large, statistically significant effects even when considering a year before the end of our post-treatment period.

(7) As we demonstrate in Sect. 5, we achieve a good pre-treatment fit for UWM for each of our outcomes of interest, including in the "left out" pre-treatment years of 2011 and 2014. This pre-treatment period includes six years, which is not particularly long but is also not unusually short in the applied synthetic control literature. While a longer pre-treatment period is generally preferable if feasible for synthetic control applications, we do not extend our pre-treatment period farther back in time in response to

Footnote 22 continued
poor pre-treatment fits, as is also sometimes done in this literature, would reduce J and thus exacerbate the differences. We thank an anonymous referee for requesting we offer these details to clarify why our p-values are comparatively conservative.

[23] As a reminder, the two states had an agreement which treated students from Minnesota as resident students at state schools in Wisconsin, including at UWM.

the caution of Abadie (2021) that accuracy may be substantially affected by structural breaks in the data pre-treatment data series, and given the known impact of the great recession on post-secondary demand and supply (Long 2014) which almost-certainly resulted in such structural breaks. We also note the conclusion of Ferman and Pinto (2021) that synthetic control methods can offer substantial improvements over DiD methods even when T_0 is not large provided appropriate modifications are made. The modification they consider is a demeaned synthetic control (see, also, Doudchenko and Imbens 2016) which is particularly appropriate when outcomes are expressed in levels and estimates are expressed as level changes. Where we estimate outcomes in levels, we thus first demean them. When we estimate % changes, we apply an analogous transformation to normalize outcomes to their 2015 values. For all these reasons, we thus view our six years of pre-treatment observations as appropriate given the context.

(8) Our preferred donor pool sample was intentionally selected for comparability along a dimension exogenous to the policy change we examine, and the size of our preferred donor pool is nearly identical to that of the donor pool from the seminal Abadie et al. (2010) paper on synthetic control methods; (9) our matrices of donor pool unit weights using our primary donor pool sample are indeed sparse for each outcome, offering a clean interpretation of our results;[24] and (10) to ensure our estimated synthetic controls are similar to the University of Wisconsin, Madison, we include as covariates the 2015 values of freshman NRS enrollment, institutional grant aid, financial aid received by full-time resident 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 three (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 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.)[25]

Additional technical details on synthetic controls are included in Online Appendix B.

4.2 Synthetic difference-in-differences

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

[24] Donor pool unit weight tables are available for each outcome on request.

[25] 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.

observed in each institution, and as such are effectively controlled for by unit fixed effects.[26]

4.3 Two-way fixed effects

Finally, for comparative purposes, we use ordinary least squares to estimate the two-way fixed effects (TWFE) model:

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

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.[27] 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.[28]

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 (land–grant, R1 and R2 and public 4-year universities), and several post-treatment years—for each of our outcomes of interest.[29] For each outcome variable, covariate list, and donor pool sample, we estimate unique synthetic controls as convex combinations of strict subsets of the donor pool schools.

5.1 Main results

Our preferred estimates are the synthetic control estimates using the complete set of matching covariates, presented in Table 2 and Figs. 5, 6, and 7. We examine the effect of the policy on 2019 outcomes.[30] We first present results on our "first-stage"

[26] This selection of covariates maintains consistency with the synthetic control specifications and also ensures there is no bias from using "bad controls."

[27] 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.

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

[29] As we show in Sect. 5.2, the choice of sample does not influence our results.

[30] 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 Sect. [31] Associated donor pool weights from an equivalent OLS regression are presented in Online Appendix Section B.1

Table 2 Main results

	Estimates
1. First-stage outcomes	
All Non-resident Freshman Enrollment	
Treatment Effect (%)	28.878
Ranked-RMSPE-based *p*-value*	0.025
Domestic Non-resident Freshman Enrollment	
Treatment Effect (%)	28.888
Ranked-RMSPE-based *p*-value*	0.075
Revenue from Tuition and Fees	
Treatment Effect (%)	46.709
Ranked-RMSPE-based *p*-value*	0.075
2. Financial Outcomes	
Published Resident Tuition Fees	
Treatment Effect (%)	− 0.587
Ranked-RMSPE-based *p*-value	0.775
Published Non-resident Tuition Fees	
Treatment Effect (%)	11.623
Ranked-RMSPE-based *p*-value	0.225
Average Institutional Grant Awarded	
Treatment Effect (%)	29.338
Ranked-RMSPE-based *p*-value*	0.075
Average Financial Aid Awarded to In-state Students from Households Earnings $< \$30\,k$	
Treatment Effect (%)	23.904
Ranked-RMSPE-based *p*-value*	0.100
3. Academic Outcomes	
Resident (in-state) Freshman Enrollment	
Treatment Effect (%)	− 2.309
Ranked-RMSPE-based *p*-value	0.275
Full-time Retention Rate	
Treatment Effect (%)	− 0.206
Ranked-RMSPE-based *p*-value	0.425
Student-to-faculty Ratio	
Treatment Effect (%)	− 9.885
Ranked-RMSPE-based *p*-value	0.850
4-year Graduate Rate	
Treatment Effect (%)	7.396
Ranked-RMSPE-based *p*-value	0.450
N	40
T	10
$N \times T$	400

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) estimates from our preferred model specification—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

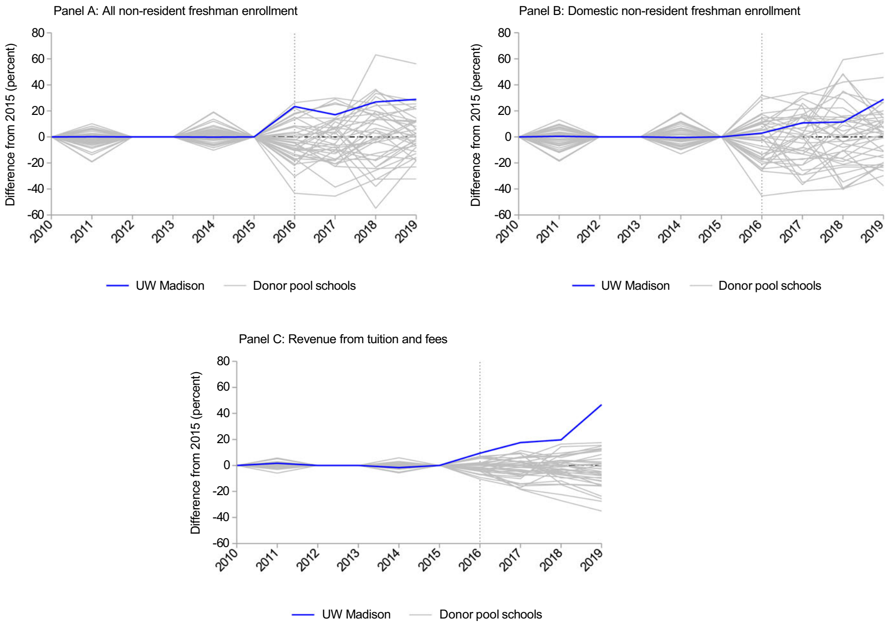

Fig. 5 First stage—estimated treatment effects. *Note*: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land–grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010 to 2019. The y-axis shows the % difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The thick, dark line shows the estimated effect at the University of Wisconsin, Madison. The gray 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

outcomes, including NRS enrollment and tuition revenues, in Fig. 5 and Sect. 1 of Table 2. This allows us to check whether the policy had the intended effect of increasing institutional revenues by increasing NRS enrollment. In Table 1, we also provide the synthetic control donor weights for NRS enrollment.[31] In Fig. 6 and Sect. 2 of Table 2, 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 resident and non-resident students, 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 Fig. 7 and Sect. 3 of Table 2 we consider the effects on academic outcomes including resident freshman enrollment, the retention rate of full-time students, the student-to-faculty ratio, and the 4-year

[31] Most of the donor pool schools had very low baseline international freshman enrollment in 2015, and saw small declines in level terms through 2019. This translated into large declines in percentage terms in the donor pool schools, resulting in an apparent very large *relative* % increase in UWM international freshman enrollment which is not robust to the various alternative specifications and estimators or to estimation in (demeaned) level terms.

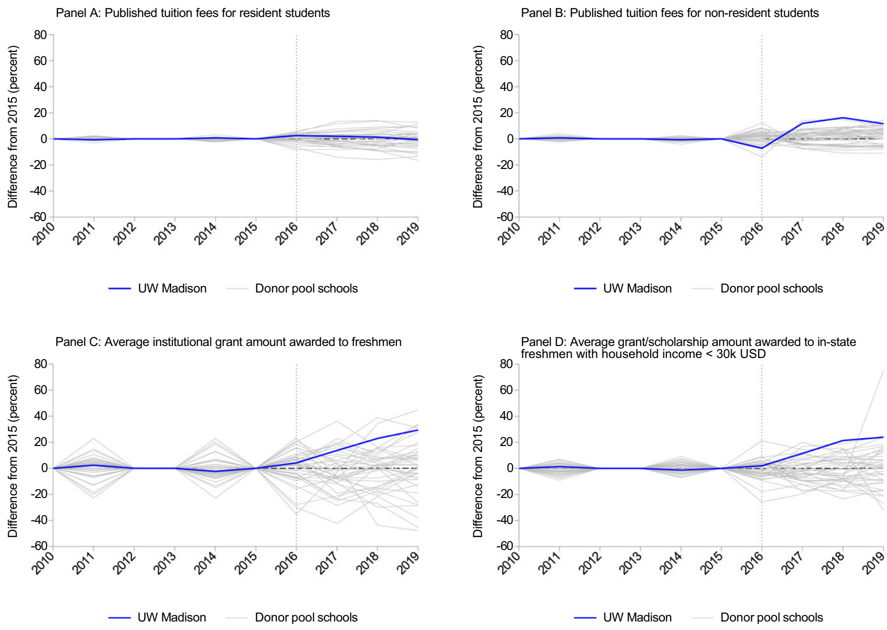

Fig. 6 Financial outcomes—estimated treatment effects. *Note*: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land–grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010 to 2019. The y-axis shows the % difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The thick, dark line shows the estimated effect at the University of Wisconsin, Madison. The gray 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

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.

Figure 5 shows the evolution of synthetic control "gaps" for all NRS freshman enrollment (Panel A), domestic NRS freshman enrollment (Panel B), and overall tuition revenue (Panel C), for UWM (in blue) and all donor schools (in gray) in each post-treatment year. All three graphs show large increases from 2016 to 2019, the period over which the policy was in effect. Table 2 quantifies the magnitude of those increases in 2019, showing the policy caused an increase in all NRS enrollment and domestic NRS enrollment, each of 28.9% (RMSPE $p = 0.025$ and 0.075, respectively). That is, all of the increase in NRS freshman enrollment came from non-resident domestic students, to the tune of 685 more of them in 2019 (RMSPE p-value $= 0.1$. See Online Appendix Table A.1)—estimated from a (demeaned) level change specification (% change analyses on international student numbers are not robust, but in level terms there is no significant change in international enrollment. See Online Appendix

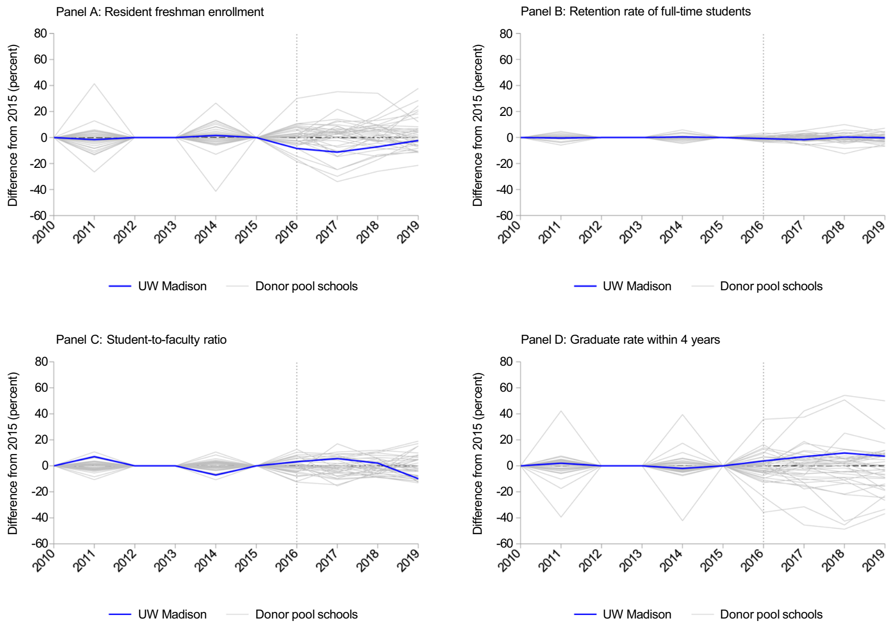

Fig. 7 Academic outcomes—estimated treatment effects. *Note*: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land–grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010 to 2019. The y-axis shows the % difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The thick, dark line shows the estimated effect at the University of Wisconsin, Madison. The gray 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

Tables A.1 and A.3).[32] Table 2 also quantifies the resulting increase in overall tuition revenue collected by UWM at 46.7% (RMSPE $p = 0.075$).[33]

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 resident and non-resident published tuition fees.[33] Panels A and B of Fig. 6 present SC gaps for these outcomes. We see that there is no published impact on resident tuition, although there visually appears to be a modest increase in published non-resident tuition. Table 2 confirms there no significant impact on either, with a point estimate of − 0.59% on resident tuition fees (RMSPE p-value of 0.775). The estimated impact on NRS tuition is 11.6%, but RMSPE p-value of 0.225 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 (domestic) NRS enrollment.

32 The across-the-board zero gaps in 2012 and 2013 for all results in Figs. 5–7 follow mechanically from the bias-correction procedure given the inclusion of outcomes in those years as predictors. See Wiltshire (2025a).

33 Most public universities charge common non-resident tuition fees to non-resident domestic and international students alike.

We next study how this increase in revenue impacted the amount of financial aid dispersed at UWM. Previous work has shown that public universities use increases in tuition to subsidize low-income students (Shih 2017; Cook and Turner 2022). Figure 6 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 2 shows significant treatment effects of 29.3% (RMSPE $p = 0.075$) and 23.9% (RMPSE $p = 0.10$), respectively. We therefore conclude that the increase in tuition allowed UWM to support more students, and particularly to provide more financial support to low-income students.[34]

Previous work has concluded that NRS students place a burden on universities' academic quality, negatively impacting in-state (resident) students (Jacquette 2017). We thus next examine effects on outcomes that might be impacted by increased NRS 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 7 presents the SC gaps for each of our academic outcomes. We see no indication that the 2016 policy impacted retention at UWM (Panel B). The plots for resident 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 2 shows respective 2019 estimated effects of - 2.3% (RMSPE $p = 0.275$), - 9.9% (RMSPE $p = 0.85$), and 7.4% (RMSPE $p = 0.45$), making clear that none of this apparent movement can be interpreted as causally related to the policy change. A negative point estimate on the student-to-faculty ratio may sound counterintuitive given the dramatic increase in NRS freshman enrollments, even despite noisy inference. We attribute this to two causes. First, though not as drastic as the NRS freshman enrollment increase, there was a simultaneous increase in instructional staff (as well as in tenured and tenure-track faculty) employed at WM. Second, the student-to-faculty ratio is based on the count of all undergraduates, and not just freshmen. Our positive causal point estimate on the 4-year graduation rate means that there was likely a decrease in senior student enrollment through faster graduation. Since we do not see any definitive changes in freshman retention rate, we do not attribute a lack of growth in the student-to-faculty ratio to first-year students dropping out. Panel B of Fig. 7 and the values in Table 2 (- 0.2%, RMSPE p-value of 0.425) show no change at all in the retention rate. If we focus particularly on the plot for resident freshman 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. Lastly, Appendix Table A.6 parallels the descriptive

[34] Expenditure share on scholarships and fellowships at UWM is only about 2–3% across our sample years. As this is a very small share, we do not expect there to be large ripple effects on other expenditure categories because of the extra funds allocated to student grant aid. (By contrast, typical expenditure shares on instruction and research are 22 and 37%.) Furthermore, in 2019, revenue from tuition and fees at UWM was $661.3 million, while expenditure on student scholarships and fellowships was only $61.4 million. Given that the expenditure on student aid was only 10% of the revenue collected from tuition and fees paid by all students, most of the extra revenue collected post-policy change must have gone to other purposes.

results and shows that a changing academic quality of incoming freshmen was not a factor. In summary, we find no evidence that the 2016 policy placed a negative burden on UWM or on its students' academic outcomes.

5.2 Robustness tests

To test the robustness of our main findings, in Online Appendix Tables A.2–A.5 we present results based on various tests and alternative specifications and donor pool samples. These include: rerunning our synthetic control estimation for 2019, excluding the covariates for student sex and age and international enrollment (column 1 of Online Appendix Tables A.2–A.5); 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 Online Appendix 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 estimate on all NRS 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 NRS enrollment grow even larger and regain significance ($p = 0.011$). Growth of the donor pool to include these less-comparable schools may threaten the validity of the synthetic control estimation strategy and introduce the possibility of overfitting, which is why we prefer the more-judicious donor pool of public 4-year land–grant institutions. Still, these results broadly show the robustness of our preferred estimates, as do the TWFE and SDiD estimates, which is highly reassuring. The estimated % change effects on domestic NRS enrollment follow a highly similar pattern (as do the estimated level change effects, in Online Appendix Table A.3), further supporting our finding that essentially all of the increase in NRS enrollment came from non-resident domestic students. The estimated effects on tuition revenues are again similar in magnitude to our preferred estimates and significant across the board.

Turning to our financial outcomes, in Online Appendix Table A.4, we find a similar pattern: Most point estimates resemble those from our primary specification. The synthetic control estimated effect on average institutional grant aid awarded loses significance for the R1+R2 analysis, only ($p = 0.246$), and significant for the analysis using all public four-year universities in the donor pool ($p = 0.016$). The point estimates on average student aid awarded to students from low-income households are all similar in magnitude to our primary estimates, though they are somewhat noisier, with the p-values in columns (1) and (4) slipping to $p = 0.125$ and p=0.121, respectively, and that for the SDiD estimate reaching $p = 0.230$, though the p-values in columns (2) and (3) are 0.10 and 0.072, respectively. Lastly, all of the estimates looking at our academic outcomes, presented in Online Appendix Table A.5, are similar to those from our primary specification, supporting our finding that increasing the number of non-resident students yields no discernible negative effect on academic outcomes. As an additional test of this conclusion, in Online Appendix Table A.6 we also present the estimated effect on freshman SAT scores at the 75th percentile (additionally, 25th for math), and find no evidence of any effect.

6 Discussion and conclusion

In this paper, we investigate the impact of a 2016 policy change at the University of Wisconsin, Madison, that removed an enrollment cap on the number of non-resident (domestic and international) students. The removal of this binding constraint constituted a positive supply shock even as demand from non-resident students remained constant. Therefore, this policy change admits a straightforward strategy to identify the impact on university and student finances and academic outcomes.

Using a synthetic control estimating strategy, we estimate the policy change led to a significant 29% increase in the number of NRS enrolled, which helped drive tuition revenue 47% higher. We also demonstrate that these non-resident-student enrollment effects are almost entirely driven by the non-resident domestic students, rather than international students. We find this resulted in significantly higher amounts of financial aid being distributed to students (a 29% increase), including financial aid for resident students from families earning less than $30,000 a year (a 24% increase). We fail to detect any significant effects on various educational outcomes that measure academic quality at universities, including retention, student-to-faculty ratios, and graduation rates.

These results are robust to alternative estimators, specifications, and control groups, and indicate that increasing the number of NRS does not negatively impact resident students. On the contrary, given increasingly tight budget constraints experienced by universities, our results suggest that NRS can be a source of substantial financial support for universities which have the capacity to enroll them and which are constrained in their ability to raise resident-student tuition (e.g., through legislated caps).

Our study has certain limitations. For one, the relative uniqueness of this policy change means the study is focused on this one selective public university in the Midwest. While we show that resident and non-resident enrollment levels and trends at the University of Wisconsin, Madison, were entirely unremarkable in the years before the policy change, it may be the case that demand for admission at this university is in some way different than at other public or land–grant universities, which would limit the generalizability of our conclusions. Similarly, if a policy to remove NRS caps had been implemented nationwide rather than just at this one institution, competition for NRS among schools might reduce NRS demand faced by any particular school (Canche 2014), limiting the local impact of such a policy change. Finally, while administrators at the University of Wisconsin, Madison, chose to use the elimination of this constraint to increase NRS enrollment and to use the resulting increased tuition revenue to improve financial supports for resident low-income students, potentially through the creation of the Bucky's Tuition Promise program (Marifian et al. 2024), other administrators at other universities might use a similar tuition revenue windfall differently—for example, by increasing faculty numbers or salaries, for capital investments, or even for expanding administrative staff numbers or salaries. Absent additional constraints on the use of increased tuition revenues, there is no guarantee that the benefits we find for resident students would hold if NRS caps were lifted elsewhere.

These considerations notwithstanding, our results imply that individual universities could financially benefit from reconsidering limits on NRS enrollment, and that the resulting financial windfall could in principle be used to benefit their low-income resident students. This is especially true for institutions that have excess capacity and which face declining state support for higher education.

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Data availability The IPEDS data are available at https://nces.ed.gov/ipeds/help/complete-data-files.

Declarations

Conflict of interest Not applicable.

Materials availability Supplemental material availability is detailed in the Supplementary information section.

Code availability The replication package for this package is available at https://doi.org/10.7910/DVN/OXATRB.

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