The Effect of Abortion Bans on College Decisions*

Dana J. Smith[1], Huiyi Chen[2], and Molly L. Brewer[1]

[1]College of the Holy Cross
[2]James Madison University

September 29, 2026

Abstract

Following *Dobbs v. Jackson Women's Health Organization*, many U.S. states adopted new abortion restrictions. Using IPEDS data and a difference-in-differences design comparing institutions in states that adopted bans with institutions in states that did not, we examine student enrollment, institutional responses, and faculty hiring. We find no average effects on undergraduate applications or enrollment. Instead, responses are concentrated among groups with broader choice sets: women's graduate student enrollment declines, and women's applicant and graduate shares fall most at selective institutions, where women's tenure-track hiring also declines. Colleges may adjust at the margin through admissions and financial aid. Overall, abortion restrictions generated limited reallocation, with the clearest effects concentrated in more selective markets.

Keywords: abortion policy; college choice; higher education; *Dobbs*; enrollment
JEL Codes: I23; I26; J16; K38

*The authors would like to thank seminar participants from the Liberal Arts College Public and Labor Economics Group, in particular Cristina Riquelme and Caitlin Myers, for their thoughtful comments on an early version of this paper. We would also like to thank participants at the APPAM Annual Meeting in Boston. The early stages of this work were supported by the J. D. Power Center for Liberal Arts in the World and the James Madison University College of Business 2026 Summer Research Funding.

1 Introduction

In June 2022, *Dobbs v. Jackson Women's Health Organization* returned the authority to regulate abortion to the states, and thirteen states enforced total bans within months (Baden and Driver, 2023). This drastically changed the rights of otherwise similar women across states and had the potential to affect major life choices, including marriage and fertility decisions, job choices, and education. This paper examines the effect of abortion bans on college applications and enrollment at institutions across the United States. We then investigate whether institutions respond to these changes in student demand and whether abortion bans also affect faculty hiring.

Survey evidence suggests potentially large impacts of abortion bans on higher education. In the Lumina Foundation-Gallup State of Higher Education Study, 71 percent of current and prospective students said state reproductive healthcare policies affect where they would enroll; among respondents who considered those policies important, 80 percent preferred a state with greater access (Marken and Hrynowski, 2024). Another 28 percent of college-bound high school seniors reported ruling out schools because of state politics or policies (Blake, 2024). Faculty surveys similarly report that state political environments affect recruitment and retention across the South, with one report specifically citing the negative impact of abortion bans (American Association of University Professors, 2024; Langin, 2023). If these stated preferences translate into actual choices, abortion bans could redirect students and scholars away from certain institutions.

This paper asks whether the post-*Dobbs* bans changed behavior along three margins: student applications and enrollment, particularly among women; institutional admissions and financial-aid policies; and the gender composition of new faculty hiring. The question is especially important as colleges enter a period of heightened competition for students. The number of eighteen-year-old high school graduates is projected to peak in 2025 and then fall by 13 percent by 2041 (Western Interstate Commission for Higher Education, 2024; Marcus, 2025). Colleges compete for students who make decisions based on facilities and campus amenities, sports, institutional image, tuition, and financial aid (Heller, 1997; Pampaloni, 2010; McDuff, 2007; Paulsen and John, 2002; Oosterbeek et al., 1992; Drewes and Michael, 2006; Pope and Pope, 2009; Cletzer et al., 2020). We ask whether institutions use some of these levers, mainly tuition and financial aid, to overcome the negative impact of an abortion ban.

Stated preferences do not necessarily translate into choices. Eventual enrollment is based on a series of decisions. First, students choose where to apply based on the perceived costs and benefits of the institution and the likelihood of acceptance. Then, institutions choose whom to admit and students make a second enrollment decision from the offers they receive.[1] While a student may want to avoid institutions in states

[1] For more detailed models of college choice, see Chapman (1981); Litten (1982).

that ban abortion, they are constrained by the number of applications they can submit. This choice is further constrained by their (potentially few) acceptances. For these reasons, while students may state a preference for schools in states without bans, we may not observe students actually making that choice.

Second, distance is one of the strongest predictors of college choice with many students attending relatively local institutions (Griffith and Rothstein, 2009; DesJardins et al., 1999). Most American undergraduates attend a public institution in their home state and are willing to absorb sizable tuition increases before leaving it (McDuff, 2007). Students who live in a ban state face considerable path dependence in addition to the additional cost of attending school out of state.

It is also possible that stated preferences may not map to actual decisions if colleges are already adjusting on their own margins to overcome the negative amenity of an abortion ban. We argue that small changes in student behavior may be masked by institutional responses such as lowering admissions standards or increasing financial aid. We therefore examine institutional responses alongside student outcomes.

Finally, academic job candidates often apply much more broadly and receive far fewer offers compared to students, leading to an even more constrained choice set. While women may not want to work in states with bans, it is likely they have no latitude to act on this preference. Still, institutions could again offset small losses with higher salaries, which we will also investigate.

We use detailed, institution-level data from the Integrated Postsecondary Education Data System maintained by the National Center for Education Statistics. Our main difference-in-differences design compares institutions in the thirteen states that enforced total bans shortly after *Dobbs* with institutions in the twenty-six states and the District of Columbia that did not restrict abortion access. We exclude eleven states that adopted bans later or with more limited scope from the main specification and add them to the treated group in a robustness check. We treat the entering classes of 2023 and 2024 as exposed and report event studies for every outcome we estimate, using them to decide which coefficients can be read causally and which merely continue a pre-existing trend. We supplement the main design with synthetic difference-in-differences (Arkhangelsky et al., 2021) following Dench et al. (2025).

We find no detectable reallocation of undergraduate students. Women's share of applicants falls by 0.3 percentage points in ban states after *Dobbs*, women's share of enrolled students rises by 0.1 percentage points, and the female-to-male yield ratio falls by 0.1 percentage points; all three estimates are statistically indistinguishable from zero, and the share of students arriving from states without bans is unchanged as well. Women make up 58 percent of pre-period applicants and our 95 percent confidence interval rules out declines larger than 1.1 percentage points, about 1.9 percent of its pre-period mean. While the survey may have predicted more than a quarter of prospective students would rule out a ban state, essentially none of that stated response appears in aggregate application behavior. These nulls are stable across our other

measures, but they do vary with how much choice an applicant has. As theory would predict, women's shares fall relative to the baseline at the most selective institutions and at institutions whose admitted students typically hold competing offers elsewhere.

We find abortion bans impact behavior among people with the widest choice sets. Women's share of graduate enrollment falls by about 1 percentage point on average, and by 3.4 percentage points at the most selective institutions, in specifications with stable pre-treatment trends. Graduate students may be less driven by proximity, particularly for funded programs where there is no in-state student tuition advantage. Undergraduate applications show a muted version of the same trend. Women's applicant share is 1.8 percentage points lower at the most selective institutions than at the least selective, but we do not see a corresponding difference in where women actually enroll.

Institutional responses are modest and appear concentrated among institutions that may struggle to enroll students generally. Women's acceptance rate rises by 2.8 percentage points relative to men's in ban states, a change consistent with institutions attempting to preserve women's enrollment, although the annual estimates drift upward from 2019 onward, so we treat this as suggestive rather than causal. We detect no average change in the share of admitted students who are women or in the share of students receiving institutional aid. However, the least selective colleges expanded both aid distribution and women's admitted share after *Dobbs*, while the most selective did neither. Apparent declines in admitted-student test scores and average aid award continue trends that began at least six years before the decision rather than representing post-ban responses.

The faculty evidence divides along the same line. Women's shares of non-tenure-track, tenure-track, and tenured faculty show no average response, and the salary outcomes that do appear to move are contaminated by pre-existing trends. Women's share of new tenure-track hires, however, falls by 8.6 percentage points at the most selective institutions relative to the least selective, the largest single effect we estimate and it persists when we add controls, extend the panel to 2012, drop Pennsylvania from the comparison group, and drop women's colleges.

Our interpretation is that preferences and behavior diverge when the feasible choice set is narrow, and converge when it is wide. Undergraduates, most of whom attend a public institution in their home state, show no response at all, while graduate students and tenure-track candidates at the most selective institutions, who search nationally and can substitute across states at low cost, show the largest responses. The surveys may not be wrong about what students and faculty want but they do not capture what respondents are willing or able to actually forgo. The questions typically ask whether a policy matters, not the price, distance, or institutional quality a respondent would sacrifice to act on it. If students could readily sort away from ban states, institutions and local economies would bear part of the cost through lost enrollment and

talent. Our evidence instead shifts attention toward the students who attend schools in ban states despite a preference for a different state. These welfare implications are beyond the scope of this paper and data but suggest an avenue for future research.

This paper first contributes to research on college choice. Classic models emphasize that applications and enrollment are sequential, heterogeneous decisions (Chapman, 1981; Litten, 1982); subsequent work documents the importance of price and aid (Heller, 1997; McDuff, 2007), distance from home (Griffith and Rothstein, 2009; DesJardins et al., 1999), and institutional image (Pampaloni, 2010). We consider a different attribute: a salient state policy that is entirely outside an institution's control. More broadly, our analysis speaks to research on locational sorting and internal migration, which shows that households move in response to local public goods and social policy (Banzhaf and Walsh, 2008; Marcén and Morales, 2022; Jia et al., 2023). College applicants provide an interesting limit case because their feasible destination sets are unusually constrained.

Second, we contribute to research on the consequences of abortion restrictions. This literature documents the travel costs created by clinic closures (Lindo et al., 2020), educational consequences of provider regulations for teenagers (Jones and Pineda-Torres, 2024), and interstate migration following the post-*Dobbs* bans (Dench et al., 2025). Related work establishes that reproductive access shapes women's educational and economic trajectories through abortion legalization, contraceptive access, or the denial of a wanted abortion (Ananat et al., 2009; Ananat and Hungerman, 2012; Foster et al., 2022; Miller et al., 2023; Myers, 2017).

Our work is most closely related to Kane (2025), who uses Common Application data and finds that total bans reduced by 2.7 percentage points the share of high-achieving women applying to a school in a ban state. Our institution-side data complement that evidence by covering a broader applicant pool and by making enrollment, admissions, financial aid, and faculty outcomes observable. Our work similarly complements Walker et al. (2023) who find evidence of a decline in female applicant share using a narrow set of top institutions. We build on these papers by showing that a response among applicants with broad choice sets can coexist with little aggregate reallocation. Emerging evidence from another specialized labor market points to a similar distinction between totals and composition. Bergmann (2026) finds no decline in the aggregate supply of obstetricians and gynecologists in ban states but substantial sorting by gender, political affiliation, geography, and career stage.

Finally, the results clarify when stated preferences predict behavior. Post-*Dobbs* polling made unusually specific predictions about student and faculty mobility (Marken and Hrynowski, 2024; Blake, 2024; Langin, 2023; Lundberg and Startz, 2022). There were similar concerns about applicants to obstetrics and gynecology residencies and physician workforce composition (Orgera et al., 2023; Bergmann, 2026). The contrast

between survey responses and observed choices suggests that a survey's ability to forecast behavior depends on the width of a respondent's choice set. Surveys may be particularly poor predictors of aggregate behavior when choice sets vary widely, a contribution that extends beyond abortion policy.

2 Conceptual Framework

2.1 The Students

We start with a utility model describing a student's decision to attend a particular college. This is a two-step process where they first decide where to apply and then, upon receiving acceptances, decide where to enroll. The application and enrollment decisions are fairly similar. A student chooses to apply to as many schools as possible, as long as the benefits of applying outweigh the costs. The costs include application fees and other fees to send test scores and transcripts, as well as the time cost of filling out applications. We assume the costs are inversely scaled by the likelihood an applicant can get into the school. The proliferation of the CommonApp has made it easier for students to apply to many colleges with the average applicant now sending five to eight applications according to the College Board and Hughes et al. (2025). We treat each application decision as independent.

A student applies to College A if $U_a - \lambda C_a > 0$ or the utility of applying outweighs the costs where U_a is a function of prestige, academic programs, amenities including facilities, athletics, Greek life, etc., and location. Meanwhile, C_a is a function of actual application fees and effort required by the application for essays and questionnaires. These costs are multiplied by some λ that is specific to the applicant and decreases with the likelihood the student will be accepted by the school. We assume that students find it more costly to complete even relatively straightforward applications for schools to which they have almost no chance of earning admission.

Based on polling data, we assume that at least some students, particularly women, prefer locations that allow abortion, meaning that the utility of applying to schools in states that ban abortion falls after the *Dobbs* decision. This leads to the first outcome which is that students might apply to fewer colleges overall or may swap a school in a ban state for one in a no-ban state. We explore this first outcome by estimating the change in applications, particularly from female students, in states that ban abortion in 2022.

However, if λ is high enough that there are no schools a student would apply to in a no-ban state, then even if they have a preference for states that allow abortion, their application behavior will not change. As Kane (2025) shows, it is likely only women from the very top of the application pool have the ability to actually make a choice on this margin, so at the aggregate level, institutions may not notice substantial

changes. Further, even if many top female students choose not to apply to a certain school, that may not lead to significant enrollment changes.

After students decide where to apply, institutions have the opportunity to offer acceptances. We revisit the university's decision in Section 2.3 but for now assume a representative, high-achieving applicant gets into every school she applies to. Her decision is now based on maximizing U_a subject to T_a, the cost of attendance. We assume U_a is identical to her utility during the application stage but now the decision on where to enroll depends on tuition, room, and board, which the university can reduce via institutional aid.[2] Even if application behavior changes, institutions may not notice much difference on the enrollment side if those students were unlikely to attend anyway, meaning the abortion ban may not have a substantial impact on institutions.

This leads to a few interesting predictions. First, there are some, likely high-achieving, students who can "afford" to forgo applications to institutions in ban states because they are likely to get into preferred schools in no-ban states. But, because these applicants are forgoing less-preferred schools in ban states *because* they expect to get into other schools, then while this impacts applications it would have no impact on enrollment for the affected institution; the student was unlikely to attend anyway. If many applicants fall into this group then we would notice a change in the female share of applications but not enrollments. Second, some students may be unwilling to risk getting in nowhere and maintain their application behavior after *Dobbs* but after receiving multiple acceptances now choose a school in a no-ban state. If this describes student behavior then we would expect to see a decline in the female share of enrollment but not applications. This group only includes those who actually get into schools in multiple states though. Finally, the most extreme and potentially very small group of students are those who would have applied to and attended a school in a ban-state but for the ban. After *Dobbs* this student does not apply to and therefore does not enroll in the school. This would result in a decline in both applications and enrollment.

While poll data shows that many people report a distaste for abortion bans, it may not be particularly high for teenagers. They may feel confident in their ability to avoid an unwanted pregnancy; they may believe they can travel out of the state for care if they need it; and a strong present-bias may lead students to underestimate the likelihood of needing abortion care during their college years. Overall, we expect relatively small impacts on enrollment, if any, given that there is such a small number of students who fall on this margin.

Finally, we consider that the large tuition advantage in-state public schools offer may mask changes in enrollment behavior for out-of-state students. We look at changes in enrollment for students applying

[2]For simplicity we assume that all schools offer the student the same Federal Financial Aid and that schools only go above and beyond these offers to attract top students.

from no-ban states to institutions in ban states. We also separate public and private institutions assuming that the effects might be larger in private schools that hope to attract more out-of-state students, although students may consider their home-state public institutions close substitutes for an out-of-state public and find it easier to switch away from a ban state public.

One thought may be that high-ranking schools suffer the most if we assume applicants good enough to get into these schools are likely to have other offers. On the other hand, high-ranking schools may offer students a lot of utility if they value prestige heavily, meaning their distaste for abortion bans would have to be pretty high to overcome that. We also note that schools in the southeast US have been very successful in recruiting out-of-state students, particularly from the northeast US, in recent years despite the policy changes.

2.2 The Faculty

We start by considering job market candidates actively looking for a job. Like students, faculty applicants also go through a two-stage process where they first choose where to apply and then, after receiving offers, decide where to work. In the first stage, candidates weigh the utility of a job at College A (U_a), which is a function of pay, prestige, teaching load, research support, institutional classification, and location, against the costs of applying (C_a), which largely consist of the effort to apply and potentially the monetary and time costs of in-person interviews. Like students, faculty may find the costs of applying higher for jobs they have very little chance of getting but the overall tightness of the academic labor market leads to a much higher average number of applications compared to students.

As before, we assume at least some candidates will place a high enough penalty on states that ban abortion that they won't apply. We cannot observe faculty applications, only hiring, so we move on to the second stage of the process. After candidates apply, the university will make a job offer. There are two scenarios that lead to similar predictions. First, a job candidate only gets one offer and has to accept it. In this case there is either no impact on the share of women the institution hires since the applicant had no choices or there could be a decline if there is a case where a candidate would have applied to a particular school, received and accepted an offer, but for an abortion ban. We assume this group is relatively small given the wide range of applications candidates usually send. If a candidate gets multiple offers then they will choose the offer that provides higher utility. For at least some candidates, their first choice may have been a job in a ban state but their preferences shift after Dobbs. This would similarly result in a decline in the share of female new faculty hires for institutions in a ban state.

Again, we may not detect much given how tight the academic job market is. The group of people who

could possibly explore this decision is relatively small and like students, may have always preferred the job in the no-ban state for other reasons.

2.3 The University

Regarding student applications and enrollment, schools have a few options to attract more students. Given the limitations of the data we will focus on tuition and financial aid but it is also possible that schools will invest in out-of-state recruiting or improving other campus amenities to maintain applications and enrollment.

While schools rarely lower tuition and fees, they could freeze tuition or commit to smaller increases in order to attract students. If students weigh the utility of the institution against its cost, then lowering costs should impact student decisions. Financial aid provides an even more flexible way to impact students. While many colleges may follow standardized guidelines for federal student aid, many will go above and beyond to attract particularly competitive students. This could involve merit aid or blanket policies such as free tuition for all families who earn less than $100,000, for example. We would therefore expect to see a decrease in average net cost and an increase in institutional aid from schools affected by abortion bans if they are trying to overcome potential declines.

The faculty side is quite similar. If schools need to attract more or better candidates or convince them to take a job despite a distaste for an abortion ban, then the schools could raise the salary they pay.

3 Data

3.1 Abortion Ban Data

We classify each of the 50 states and the District of Columbia as either a ban state or a no-ban state in each year from 2022 through 2024, based on whether the state restricted abortion access after the Supreme Court's decision in *Dobbs v. Jackson Women's Health Organization* on June 24, 2022. Data on state abortion policy come from the Center for Reproductive Rights' state-by-state tracker of abortion laws (Center for Reproductive Rights, 2024). States that banned abortion before 20 weeks, including states with total bans, make up our ban states. States that did not tighten abortion access after *Dobbs* make up our no-ban states. These states still may restrict abortion at or near fetal viability, but permit the procedure up to around 26 weeks and may have additional exceptions where the mother's life is at risk or there are fetal anomalies. A small group also places no restrictions on abortion.

We define ban states in two ways. Our main analysis treats a narrower group of 13 states as the treated group while a robustness check described in Section 4.3 instead treats a broader group of 24 states as treated, and the results turn out to be qualitatively similar either way. Thirteen states had a total abortion ban in effect in 2022, whether through new legislation or a long-dormant statute: Alabama, Arkansas, Idaho, Kentucky, Louisiana, Mississippi, Missouri[3], Oklahoma, South Dakota, Tennessee, Texas, West Virginia, and Wisconsin. Almost all of these states constitute a clean and narrow definition of "treated" for our difference-in-differences strategy. These states enacted total or near-total abortion bans quickly, in many cases immediately in June 2022. Wisconsin is a partial exception: it is considered a ban state in 2022 because of a trigger ban dating back to 1849 but a no-ban state in 2023 and 2024, once its ban was struck down. The effects of a year-long ban on prospective students and faculty plausibly persisted beyond the ban itself, however, so we continue to classify Wisconsin as a ban state.

An additional eleven states restricted abortion less immediately or less completely than the 13 states above: Arizona, Florida, Georgia, Indiana, Iowa, Nebraska, North Carolina, North Dakota, Ohio, South Carolina, and Utah. Together with the 13 states above, these 24 states make up the ban states in our sample and form the treated group in our broader definition. Indiana's total ban did not take effect until 2023, after an earlier version was blocked in court. Florida and Georgia banned abortion after six weeks in 2022, South Carolina followed in 2023, and Iowa in 2024. Arizona and Utah adopted gestational bans between 12 and 18 weeks in 2022, and Nebraska and North Carolina adopted similar bans in 2023. Ohio and North Dakota's timing was more complicated still; we discuss both below. We also differ from other work and consider Pennsylvania always a no-ban state. Abortion is widely accessible in Pennsylvania but the state requires a number of unnecessary procedures prior to receiving an abortion leading some to classify the state as hostile.

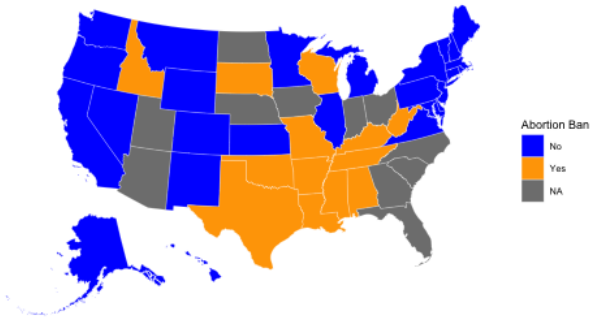
The remaining 26 states plus Washington DC never tightened abortion access after *Dobbs* and are classified as no-ban states throughout our sample. These include: Alaska, California, Colorado, Connecticut, Delaware, Hawaii, Illinois, Kansas, Maine, Maryland, Massachusetts, Michigan, Minnesota, Montana, Nevada, New Hampshire, New Jersey, New Mexico, New York, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, Wyoming, and the District of Columbia.

Figure 1 maps both definitions as of 2022, the first year in which any post-*Dobbs* restriction took effect. Panel (a) shows our main, 13-state definition, with the 11 additional states described above marked separately as they are excluded from the sample. Panel (b) shows our broader, 24-state definition, in which those same 11 states are classified as treated alongside the original 13.

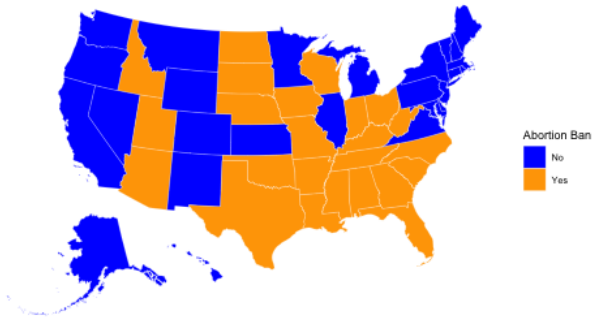
[3]Missouri's trigger ban has since been overturned by the state courts but it will appear again as a ballot measure in 2026.

Figure 1: Abortion Ban Status by State, 2022

(a) 13-State Definition



(b) 24-State Definition



Notes: Panel (a) shows our narrower, main specification, which treats 13 states as ban states; states shown in gray had bans that arrived later or were more limited than the core 13 and are excluded from this specification. Panel (b) shows our broader specification, in which these same states are classified as ban states alongside the original 13. Source: Authors' depictions using definitions from Center for Reproductive Rights (2024).

Three states' abortion laws changed enforceability within the sample period, even though our treatment coding does not vary by year within a state. Ohio enacted a six-week ban in June 2022, but a state court blocked its enforcement that September (ACLU of Ohio, 2022), leaving only Ohio's earlier viability restriction in place for the rest of our sample period. We nonetheless code Ohio as banned through 2024 but it is only included in our broader, 24-state treatment definition. North Dakota's total ban, enacted in 2023, was ruled unconstitutional in 2024 (Dura, 2025), but there are no abortion clinics left in the state (Dura, 2025). We again code North Dakota as banned in every year it appears in our broader sample. In Wisconsin, an 1849 law that predated *Roe* and had never been repealed created enough legal uncertainty after *Dobbs* that providers stopped performing abortions for roughly 15 months, until a state court ruled in September 2023 that the law applied only to feticide, not abortion, and restored legal access up to 20 weeks (NPR, 2023). In each of these three cases, we hold a state's ban status fixed for the full sample because we expect the policy's effect on prospective students and faculty to persist beyond the exact window during which the ban was in force by law.

3.2 College Data

Our institution-level data come from the Integrated Postsecondary Education Data System (IPEDS), the U.S. Department of Education's annual census of colleges and universities. The sample consists of public and private, not-for-profit, four-year institutions. We drop for-profit institutions and those that only offer two-year programs as students applying to community colleges may have very different application behaviors and preferences. We also drop institution-years with missing applicant data or zeroes for applicants, which in IPEDS is typically because of an open admissions policy rather than a missing report. We also

drop institution-years where there are fewer than 100 applicants as these are usually highly specialized institutions where an applicant may differ substantially from the traditional students and faculty we are attempting to study. We also require that institutions appear in at least seven of the nine years from 2016 to 2024 to make sure we have enough data to reliably capture trends. This leads to dropping the small institutions mentioned above that may have had 101 applicants in a particular year, for example, as well as schools that closed early in the sample period. We do not require that institutions appear in all nine years in the main specification because there are occasional reporting issues where data are missing for unknown reasons, but our synthetic difference-in-differences strategy does require a completely balanced panel.

We collect financial aid, institutional finance, graduate enrollment, and faculty staffing data from the same source at the institution-year level.

We construct two overlapping panels that differ in how broadly they define a ban state, corresponding to the two treatment definitions described in Section 3.1. The narrower panel with 13 ban states contains 1,078 institutions, 317 in ban states and 761 in no-ban states. The broader panel with 24 ban states contains 1,404 institutions, 643 in ban states and 761 in no-ban states. Our main specification uses the 2016-2024 window in both panels. Institutions appear in the data in every one of these nine years 94 percent of the time in both samples, but the effective sample for any specific regression is smaller and varies by outcome, since not every institution reports every variable in every year; test-score variables in particular are missing far more often than enrollment, admissions, and financial aid variables, so regressions using those outcomes draw on a smaller and less balanced set of institutions. Each regression table reports its own sample size. A 2012-2024 version of each panel, used for the robustness checks described in Section 4.3, requires institutions to appear in at least 11 of the 13 years and therefore contains slightly fewer institutions, 1,045 in the narrower panel and 1,356 in the broader panel, since the longer window gives more schools time to close, merge, or begin reporting late.

Our main outcomes fall into three groups. The first captures the college application and enrollment pipeline by gender: women's share of applicants, of admitted students, and of enrolled students; the women-to-men admission-yield ratio; the women-to-men acceptance-rate ratio; and women's share of total graduate enrollment. The second covers institutional financial aid: the share of students receiving institutional aid, which appears in our main table, and the average amount awarded, which we report in the appendix. The third covers faculty hiring: women's share of new hires off the tenure track and women's share of new hires on the tenure track. A further set of outcomes is reported in the appendix: women's shares of total non-tenure-track, tenure-track, and tenured faculty; average nine-month faculty salaries overall and separately for women and men; the share of enrolled students arriving from non-ban states; the international share of enrollment; and the 75th percentile SAT math, ACT math, and ACT English

scores that institutions report for entering students. We move these outcomes to the appendix because our event studies suggest their results reflect long-standing trends beginning before 2022 rather than a credible difference-in-differences result.

We additionally include net price of attendance, total institutional expenditures, and average instructional staff salary as controls, the last of which we use in place of net price when the outcome is a faculty measure. Our main tables omit these controls but Appendix Table B8 shows that adding them leaves the estimates essentially unchanged.

Figure 2 reflects state-level variation in selected outcomes. Panels (a) and (b) map, for each state, the share of students enrolling there in 2023 who came from a ban state and from a no-ban state, respectively, in both cases excluding students who stayed in their home state. Even when excluding in-state students, ban states tend to attract students from other ban states and vice versa. We also see how important in-state enrollment is in many places. It appears that large states such as California and Texas attract very few students from either ban states or no-ban states. These states just have very low shares of out-of-state students in general. Distance may explain why we see so many students from other ban states enrolling in schools in Oklahoma or Arkansas. There is a strong geographic clustering of abortion bans so students in ban states likely neighbor other ban states. Leaving ban states altogether would require overcoming both the in-state and general distance preferences as well as price and institutional quality.

Panels (c) and (d) map the other two dimensions we study. Women's share of tenure-track faculty hires varies widely across states, from roughly 45 to 60 percent, and shows no obvious regional alignment with abortion policy. Women's share of undergraduate enrollment is a bit higher, between 50 and 65 percent almost everywhere, with the ban states again represented across all values. A policy effect on these shares therefore has to be detected against a baseline that is already tightly bounded, which is one reason our confidence intervals rule out changes of the magnitude that survey evidence implied.

Table 1 reports annual means for our main outcomes and several closely related measures, separately for ban and no-ban institutions. While the annual means display little variation, we will use the data at the institution level, which displays far more variation.

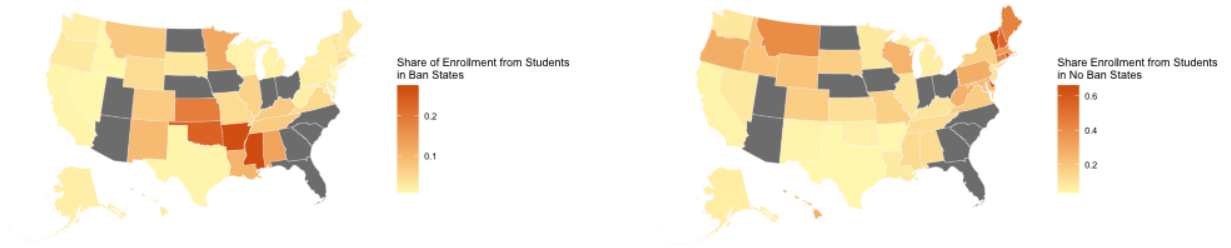
4 Empirical Strategy

We estimate the effects of post-*Dobbs* abortion bans with institution-level difference-in-differences (DiD) models. Our main analysis uses the 2016-2024 panel and compares institutions in the 13 states with immediate, near-total bans with institutions in the 26 states and the District of Columbia that did not tighten abortion access. The 11 states with later or narrower restrictions are excluded from the main sample.

Figure 2: Enrollment Flows and Women's Shares by State

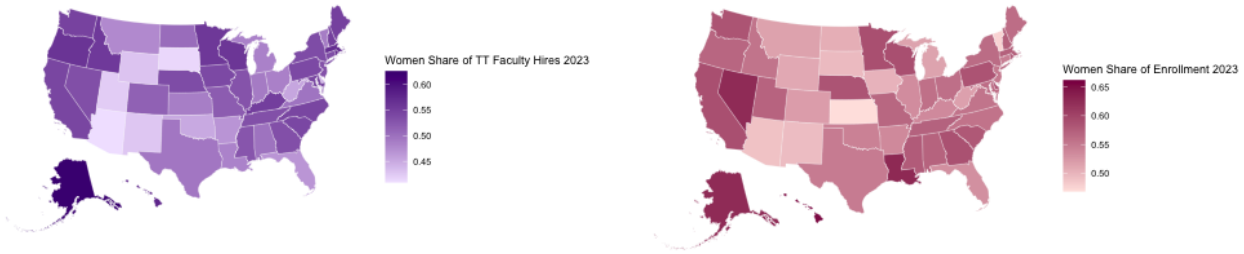
(a) Enrollment from ban-state students

(b) Enrollment from no-ban-state students



(c) Women's share of TT faculty hires

(d) Women's share of enrollment



Notes: Panels (a) and (b) use IPEDS residence-and-migration data from 2023 and report, for each state, the share of enrolled students whose state of residence was a ban state or a no-ban state under the 13-state definition; students enrolling in their own state of residence are excluded, so the maps describe cross-state flows only. Panels (c) and (d) report unweighted institution-level means within each state in 2023 of women's share of new tenure-track hires and women's share of undergraduate enrollment. Gray states in panels (a) and (b) are our excluded states and students from these states or enrolling in colleges in these states do not factor into the calculations.

4.1 Baseline Difference-in-Differences

Our preferred specification is

$$Y_{it} = \beta D_{it} + \delta_i + \delta_t + \varepsilon_{it}, \qquad D_{it} = Ban_i \times \mathbf{1}\{t \geq 2023\}, \tag{1}$$

where i indexes institutions and t indexes years. Ban_i equals one for institutions in the 13 ban states, and D_{it} switches on in 2023. We use 2023 as the first post-treatment cohort because applications, admissions, and enrollment decisions for fall 2022 were largely complete when the Supreme Court decided *Dobbs* in June 2022. The post period therefore contains the 2023 and 2024 entering cohorts.

The outcome Y_{it} measures a variety of student, institutional, or faculty outcomes in various specifications. Our main outcomes capturing student decisions are the share of undergraduate applicants and enrolled students who are women, the share of enrolled graduate students who are women, and the ratio of the enrollment yield for women to men, which captures whether women are more likely to enroll after receiving an acceptance relative to men. To capture institutional responses, we measure the share of admitted students who are women, the percent of students receiving institutional aid, and the ratio of the female acceptance rate to the male acceptance rate. Finally, to measure the faculty side, we use the share of new

Table 1: Summary Statistics by Year and Ban Status

Panel A. Applications, admissions, and enrollment

	Women Share of Applicants		Women Share of Admitted		Women Share of Enrolled	
Year	No Ban	Ban	No Ban	Ban	No Ban	Ban
2016	0.58	0.59	0.59	0.59	0.57	0.56
2017	0.57	0.59	0.59	0.59	0.56	0.56
2018	0.58	0.59	0.59	0.59	0.56	0.56
2019	0.58	0.60	0.60	0.60	0.57	0.57
2020	0.58	0.60	0.60	0.60	0.57	0.57
2021	0.58	0.59	0.60	0.60	0.56	0.57
2022	0.57	0.58	0.59	0.59	0.56	0.56
2023	0.56	0.58	0.58	0.59	0.55	0.55
2024	0.56	0.57	0.58	0.59	0.55	0.55

Panel B. Graduate enrollment and institutional aid

	Women Share of Grad. Students		% Receiving Aid		Average Amount Aid ($)	
Year	No Ban	Ban	No Ban	Ban	No Ban	Ban
2016	0.62	0.62	0.76	0.72	14,962	10,027
2017	0.62	0.63	0.77	0.73	15,732	10,517
2018	0.63	0.63	0.78	0.74	16,400	10,881
2019	0.63	0.63	0.79	0.75	17,177	11,353
2020	0.64	0.63	0.80	0.77	17,767	11,714
2021	0.64	0.63	0.82	0.79	18,211	12,060
2022	0.63	0.63	0.82	0.81	19,096	12,337
2023	0.63	0.62	0.84	0.81	19,977	12,885
2024	0.63	0.62	0.84	0.80	21,146	13,771

Panel C. Faculty hiring and tenure-track composition

	Women Share of New TT		Women Share of All Hires		Women Share of TT Faculty	
Year	No Ban	Ban	No Ban	Ban	No Ban	Ban
2016	0.51	0.47	0.52	0.50	0.51	0.50
2017	0.52	0.49	0.53	0.54	0.52	0.50
2018	0.52	0.51	0.53	0.51	0.53	0.51
2019	0.53	0.52	0.53	0.53	0.53	0.51
2020	0.53	0.51	0.55	0.53	0.54	0.51
2021	0.56	0.51	0.56	0.54	0.55	0.51
2022	0.55	0.54	0.55	0.56	0.55	0.52
2023	0.54	0.50	0.55	0.53	0.55	0.52
2024	0.53	0.50	0.55	0.54	0.55	0.52

Panel D. Tenured faculty and salaries

	Women Share of Tenured Faculty		Avg. Salary, Women ($)		Avg. Salary, Men ($)	
Year	No Ban	Ban	No Ban	Ban	No Ban	Ban
2016	0.41	0.39	74,565	60,179	82,327	67,953
2017	0.41	0.39	75,632	61,167	83,486	68,853
2018	0.42	0.39	77,048	62,027	85,088	69,601
2019	0.42	0.40	78,546	63,142	86,537	70,708
2020	0.43	0.40	79,036	63,915	86,828	71,619
2021	0.43	0.40	80,333	64,757	87,960	72,465
2022	0.43	0.41	83,263	66,527	91,005	74,330
2023	0.44	0.41	85,892	68,899	93,516	76,743
2024	0.45	0.41	89,194	70,805	97,012	78,521

Notes: Cells are unweighted means across institutions in the main 13-state, 2016-2024 panel, computed separately for institutions in ban states and in no-ban states. Share outcomes are proportions. Institutional aid receipt is expressed as a proportion of entering students; the average aid amount and faculty salaries are in nominal dollars, rounded to the nearest dollar. Panel A covers the application and enrollment pipeline, Panel B graduate enrollment and institutional aid, Panel C faculty hiring and tenure-track composition, and Panel D tenured faculty and salaries. *Dobbs* was decided in June 2022, so 2023 and 2024 are the post-treatment entering cohorts.

non-tenure-track and tenure-track hires who are women. The share outcomes are all interpreted as percentage point changes while the two ratio outcomes reflect a percentage point change for women relative to men. For example, a coefficient of 0.01 on the women/men yield ratio would indicate a 1 percentage point increase in enrollment yield for women compared to the enrollment yield for men. Ratios that are undefined because their denominator is zero, such as the women-to-men yield ratio at an institution that enrolled no men in a given year, are treated as missing and drop out of the relevant regression.

We estimate equation (1) using unweighted least squares with institution and year fixed effects. Institution fixed effects absorb time-invariant differences across colleges while year fixed effects absorb annual shocks common to all institutions. Our main tables report the specification without time-varying controls. Because treatment is assigned by state, errors may be correlated across institutions within a state and over time. We therefore cluster standard errors by state. This choice addresses the concern emphasized by Bertrand et al. (2004): conventional DiD standard errors can substantially understate uncertainty when outcomes are serially correlated, so inference should allow for arbitrary dependence within the policy-assignment unit.[4]

The data include four-year, non-profit institutions that receive more than 100 applications a year and appear in at least seven of the nine years of our analysis period. Missing outcomes further reduce the regression sample for certain models; for example, not every institution hires new faculty every year. The coefficient β is the post-2022 change at institutions in ban states relative to the contemporaneous change at institutions in no-ban states. A causal interpretation requires parallel trends in the absence of treatment. Because the main design assigns the same 2023 treatment date to all 13 ban states and excludes the other restriction states, it is a multi-period, two-group DiD rather than a staggered-adoption design.

We assess pre-trends with event-study models of the form

$$Y_{it} = \delta_i + \delta_t + \sum_{s \neq 2022} \beta_s \left(Ban_i \times \mathbf{1}\{t = s\} \right) + \varepsilon_{it}, \tag{2}$$

with 2022 as the reference year and standard errors clustered at the state level. We report event studies for every outcome that enters our tables, and we estimate them separately for the three selectivity tiers when we examine heterogeneity. Because pre-trends are what determine whether a DiD estimate can be read causally, we use the event studies to decide how to interpret each estimate. Outcomes whose pre-2022 estimates are flat support a causal reading, while outcomes already moving before 2022 do not, regardless

[4]We also considered using the distance to the nearest abortion clinic in a two-way fixed effects design; however, the theory supporting such an approach would require that undergraduate students were anticipating needing an abortion and specifically considering the distance they'd travel to get one which seems unrealistic even for graduate students and faculty. The complete bans introduced after *Dobbs* are likely much more salient to potential newcomers while the opening and closing of abortion clinics would have more impact on outcomes that affect existing residents.

of how precise the post-period coefficient is. In addition to the outcomes discussed above, we considered the overall share of women in various faculty positions, not just new hires, the average professor salary overall and by gender, the share of students (of any gender) from no-ban states, the average aid award, the share of international students, and SAT and ACT scores. We include some of these results in the Appendix but Figures A1 and A2 show that while these outcomes may be affected by *Dobbs* they are following trends laid out well before the law change so we cannot claim any effect of the abortion ban decision.

4.2 Heterogeneity by Institutional Characteristics

We define selectivity using each institution's average total acceptance rate over 2016-2022, calculated before treatment and on the full national sample so that the tier definition does not depend on ban status. Terciles of that distribution divide colleges into three groups: Most Selective, Medium, and Least Selective. Least Selective institutions are the reference group. We estimate

$$Y_{it} = \beta_0 D_{it} + \sum_{g \in \{\text{Medium, Most}\}} \beta_g \left(D_{it} \times \mathbf{1}\{\text{Tier}_i = g\} \right) + \delta_i + \delta_t + \varepsilon_{it}. \tag{3}$$

Here β_0 is the ban's DiD effect for Least Selective institutions, and each β_g is the difference between tier g and the Least Selective tier. The total effect for tier g is $\beta_0 + \beta_g$; because our tables do not report the standard error of that sum, we treat the interaction terms as the direct evidence on heterogeneity and describe the sums as implied magnitudes.

Admission rates are only one way to measure how much choice an institution's applicants have. We therefore repeat equation (3) with a second margin in the main text, replacing the tier indicators with an indicator for a high-yield institution, defined as a pre-period average admission yield of at least 35 percent. While selectivity indicates a competitive institution that would attract competitive applicants, a high-yield institution enrolls many of the students it admits, which may indicate applicants with few competing offers or a strong draw. In the appendix we use four further margins: an indicator for a top-100 institution in the US News rankings[5], an indicator for an admission rate below 26 percent, an indicator for a pre-period out-of-state enrollment share of at least 50 percent, and an indicator for a pre-period Pell-recipient share of at least 50 percent. We additionally estimate the tier specification separately for public and private, non-profit institutions.

[5]We actually include the top 100 universities and top 40 liberal arts colleges in our definition but then this sample is cut down by excluding the eleven states with limited bans.

4.3 Robustness

We vary both the treatment definition and the sample window. First, we use the full national sample and classify all 24 states with any post-*Dobbs* restriction as treated. This robustness check retains the common 2023 post indicator; it does not use each state's actual effective date. Second, we extend the 13-state analysis back to 2012. The longer panel retains institutions observed in at least 11 of the 13 years. Additional checks add the time-varying controls to the tier specification, move the post period to 2022, exclude Pennsylvania from the comparison group, and drop women's colleges, whose gender shares are mechanically fixed and could distort share-based outcomes. We also assign a placebo post period beginning in 2019 and estimate the tier specification on the pre-*Dobbs* years alone, so that any apparent treatment effect in that window signals a violation of parallel trends rather than a policy response.

As a complementary check on parallel trends, we estimate synthetic difference-in-differences models following Arkhangelsky et al. (2021). The estimator chooses institution and time weights so that the weighted comparison group tracks the treated group's pre-treatment path, then estimates the post-treatment gap. This is a useful contrast with equation (1) precisely because it does not require untreated institutions to have been on the same trend to begin with.

The synthetic-DiD models use the 13-state, 2016-2024 sample and set treatment to one for ban-state institutions in 2023 and 2024. Each outcome is estimated on its own balanced nine-year panel, which is why the synthetic-DiD sample sizes are smaller than those in the corresponding DiD tables. We estimate the models for the nine main outcomes, for the appendix outcomes, and for the subsample of Most Selective institutions, and we report jackknife standard errors.

5 Results

5.1 Average Effects on Student Decisions

Table 2 reports the post-*Dobbs* effects for our nine main outcomes, while Figure 3 plots the corresponding event study version of the same outcomes. The main result is a set of small, null effects along the undergraduate pipeline. In Panel A, we find no statistically significant changes in the gender composition of applicants or enrolled students, nor in the women-to-men yield ratio. These null effects are estimated with considerable precision. For example, the 95 percent confidence interval for women's share of applicants ranges from -1.09 to $+0.55$ percentage points, ruling out a decline larger than about 1.1 percentage points, or 1.9 percent of the pre-period mean of 58 percent female. These findings contrast with survey evidence suggesting that state politics and policies play an important role in students' college choices. In

a 2024 survey, 28 percent of college-bound high school seniors reported ruling out a college solely because of the politics or policies of the state in which it was located (Blake, 2024). Although surveys suggest that prospective students may avoid states because of abortion restrictions and other political considerations (Marken and Hrynowski, 2024; Blake, 2024), we find no evidence that post-*Dobbs* abortion bans produced a meaningful shift in women's applications or enrollment.

Two of the six student outcomes show statistically significant changes. First, women's share of graduate enrollment falls by 1.03 percentage points, significant at the 10 percent level. Unlike the undergraduate outcomes, which capture applications, admissions, and new enrollment, women's share of total graduate enrollment reflects both entering cohorts and continuing students [6]. Graduate women are likely older than undergraduate students and are more likely to be planning for pregnancy. Even if a woman never intends to get an abortion, the restrictions on access may impact all pregnant women and thus the laws may be more salient for graduate students. Similarly, graduate students may be less tied to in-state schools as many more are international students and the tuition benefits differ at the graduate level. While we cannot actually test these mechanisms, it is not surprising that graduate students appear more sensitive to abortion bans. This is in line with findings that applicants to obstetrics and gynecology residency programs fell by 10.5 percent in states with total abortion bans, compared with 5.3 percent in other states (Orgera et al., 2023); however, Hess (2024) finds an increase in the share of female medical school applicants in ban states.

Second, the women-to-men acceptance-rate ratio rises by 2.75 percentage points, significant at the 5 percent level, while women's share of admitted students is essentially unchanged. These admissions results suggest colleges in ban states became relatively more likely to admit women than men, yet the gender composition of admitted students remained stable. This pattern is consistent with admissions offices compensating at the margin for a modest decline in women's applications.

The event studies in Figure 3 largely support this interpretation. Panels (a) through (c) show stable pre-2022 estimates centered around zero, with post-2022 estimates also remaining close to zero, providing support for the parallel-trends assumption underlying the undergraduate null results. In Panel (d), women's share of graduate enrollment is stable through 2021 and then declines by 0.93 percentage points in 2023 and 0.78 percentage points in 2024. Although neither annual estimate is individually statistically significant, their timing is consistent with a post-2022 decline. Panel (e) remains flat throughout the sample period. Panel (f), however, calls for greater caution in interpreting the acceptance-rate result. The estimates trend upward from 2019 through the 2022 reference year, suggesting that the average post-*Dobbs* effect may partly reflect a continuation of an earlier trend. Moreover, the 2023 and 2024 estimates, at 1.85 and 1.52 percentage points, are individually indistinguishable from zero. We therefore interpret the acceptance-rate

[6] IPEDS does not capture new graduate enrollment.

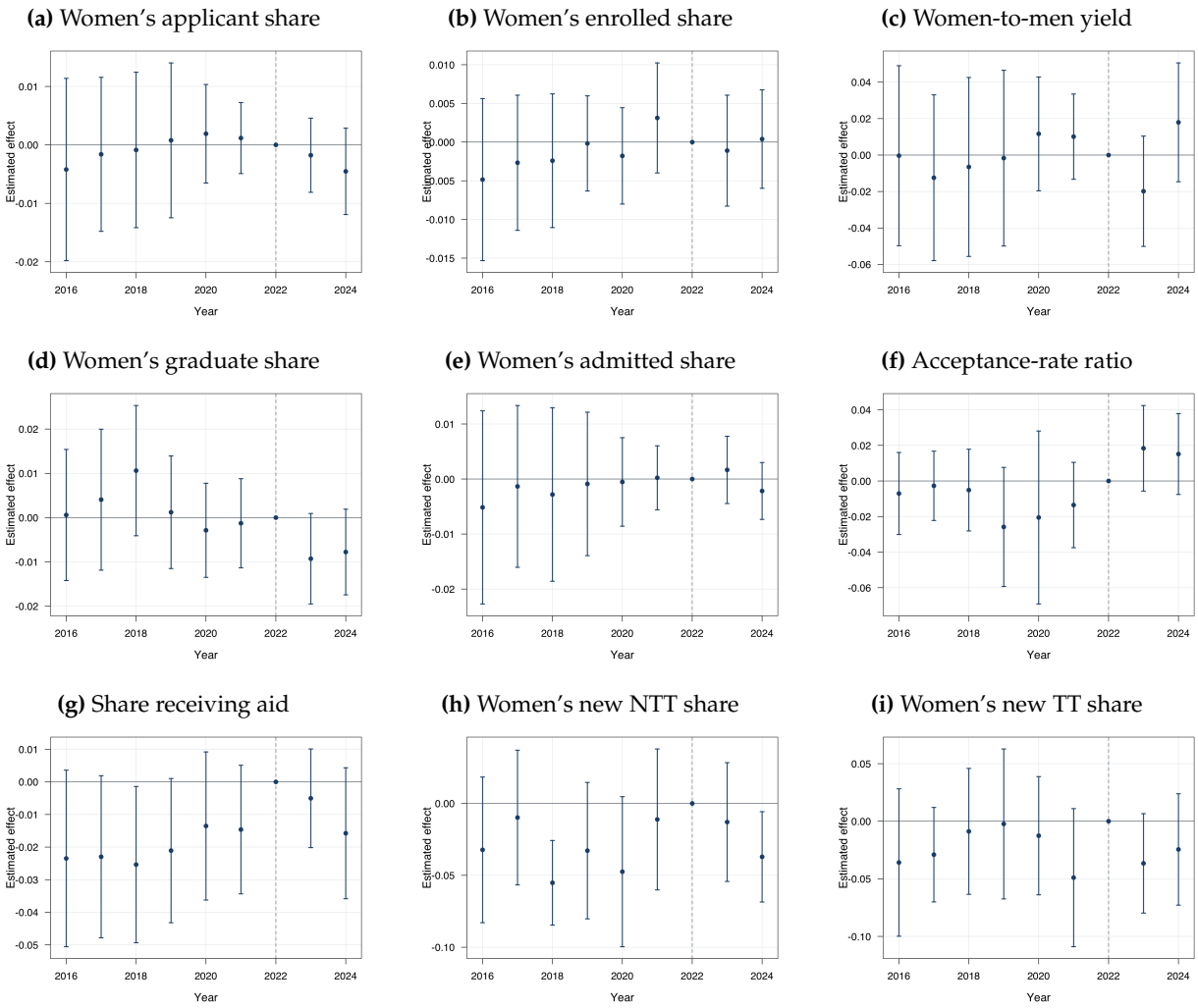
Table 2: Abortion Bans and Student, Aid, and Hiring Outcomes

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	-0.0027	0.0009	-0.0012
	(0.0042)	(0.0036)	(0.0126)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,612	9,568	9,405
R^2	0.92969	0.90525	0.42056
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0103*	0.0013	0.0275**
	(0.0056)	(0.0044)	(0.0121)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,253	9,568	9,414
R^2	0.86691	0.91753	0.37002
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0069	0.0018	-0.0112
	(0.0074)	(0.0121)	(0.0132)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,577	8,037	7,188
R^2	0.88070	0.27515	0.24412

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The sample covers 2016–2024. Treated institutions are located in the 13 states with immediate, near-total post-*Dobbs* abortion bans; institutions in states with later or narrower restrictions are excluded. Post-*Dobbs* equals one in 2023 and 2024. All models include institution and year fixed effects and cluster standard errors by state. All outcomes are proportions, so a coefficient of 0.01 corresponds to a one percentage-point change. NTT denotes non-tenure-track and TT denotes tenure-track positions.

Figure 3: Dynamic Effects of Abortion Bans, Main Outcomes



Notes: Each panel plots annual treatment effects relative to 2022 for the outcome named in its subcaption, using the main, 13-state sample. Panels (a) through (i) correspond to the nine columns of Table 2, read across Panels A, B, and C in turn. Models include institution and year fixed effects and cluster standard errors by state. Vertical bars show 95 percent confidence intervals. The vertical dashed line marks the 2022 reference year.

result as suggestive rather than conclusive.

5.2 Institutional Aid and Faculty Hiring

Panel C of Table 2 examines financial aid and hiring. While we find some suggestive evidence schools may attempt to offset application declines by admitting relatively more women, we do not find evidence that schools attempt to attract students with more widespread financial aid. Similarly, we find no statistically significant impact on faculty hiring. The share of students receiving institutional aid and women's shares of both new non-tenure-track and tenure-track hires remain statistically unchanged after *Dobbs*. The hiring estimates are less precise than the student outcomes, partly because fewer institutions report new-hire

measures and partly because institution-level hiring counts are relatively small and fluctuate considerably from year to year.

Consistent with the table estimates, Panels (g) through (i) of Figure 3 show no clear break around 2022. The two hiring series are noisy and exhibit no discernible post-*Dobbs* shift. The financial-aid series shows some evidence of pre-trends, making it difficult to isolate a post-*Dobbs* effect. We therefore find little evidence that either institutional aid or hiring responded systematically to *Dobbs*.

5.3 Heterogeneity by Selectivity and Yield

We next examine estimates by institutional selectivity because women targeting more selective colleges may have different choice sets and respond differently to abortion restrictions than those considering less selective institutions. Table 3 interacts treatment with admission-rate terciles, using Least Selective institutions as the reference group. Women's applicant share increases at Least Selective institutions but declines progressively with selectivity, with significantly lower effects at both Medium and Most Selective institutions. The implied effects are declines of 0.45 percentage points at Medium institutions and 1.11 percentage points at Most Selective institutions. In contrast, women's enrollment share does not differ meaningfully across selectivity tiers, with all estimates small and statistically insignificant. This divergence is consistent with our conceptual framework. High-achieving women considering a selective group of colleges can remove a school in a ban state from their application set at relatively low cost especially if they were unlikely to enroll there anyway. We also cannot rule out that women become less likely to apply to "reach" institutions in ban states which again are places they are unlikely to ever attend though in that case it may be because they are unlikely to get in.

The graduate enrollment and hiring effects are also concentrated among the most selective institutions. Relative to Least Selective colleges, women's share of graduate enrollment falls by 3.30 percentage points at Most Selective institutions, significant at the 5 percent level, implying a total decline of 3.37 percentage points. By contrast, the effects at Least Selective and Medium institutions are close to zero. Similarly, women's share of new tenure-track hires falls by 8.57 percentage points at Most Selective institutions relative to the reference group, significant at the 1 percent level. The estimate for Least Selective institutions is positive but statistically insignificant, while women's share of new non-tenure-track hires shows no comparable pattern. These results are consistent with graduate students and tenure-track candidates at selective institutions searching in broader national markets and having more outside options, which may make their location decisions more responsive to state abortion policy.

Admissions and financial aid also vary by institutional selectivity, further suggesting that colleges re-

Table 3: Effects by Institutional Selectivity Tier

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	0.0073*	0.0034	-0.0215
	(0.0039)	(0.0030)	(0.0153)
Ban × Post-Dobbs × tierMedium	-0.0118***	-0.0046	0.0285**
	(0.0037)	(0.0047)	(0.0114)
Ban × Post-Dobbs × tierMostSelective	-0.0184***	-0.0029	0.0330
	(0.0043)	(0.0047)	(0.0196)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,612	9,568	9,405
R^2	0.92988	0.90526	0.42075
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0007	0.0072*	0.0124
	(0.0065)	(0.0037)	(0.0117)
Ban × Post-Dobbs × tierMedium	0.0003	-0.0100***	0.0097
	(0.0078)	(0.0034)	(0.0060)
Ban × Post-Dobbs × tierMostSelective	-0.0330**	-0.0079*	0.0358***
	(0.0124)	(0.0046)	(0.0122)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,253	9,568	9,414
R^2	0.86749	0.91759	0.37011
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0273***	-0.0118	0.0237
	(0.0081)	(0.0211)	(0.0239)
Ban × Post-Dobbs × tierMedium	-0.0117	0.0191	-0.0304
	(0.0120)	(0.0239)	(0.0313)
Ban × Post-Dobbs × tierMostSelective	-0.0493***	0.0231	-0.0857***
	(0.0079)	(0.0283)	(0.0291)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,577	8,037	7,188
R^2	0.88109	0.27522	0.24511

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Institutions are grouped into terciles using their average acceptance rate over 2016-2022, computed on the full national sample before treatment. Least Selective institutions are the reference group. The Ban × Post-*Dobbs* coefficient is the effect for Least Selective institutions. Each interaction is the difference between the named tier and the Least Selective tier, so a tier's total effect is the sum of the two. The sample, outcomes, fixed effects, post period, and state-level clustering follow Table 2. Significance stars on interaction terms test differences from the reference tier, not the summed total effects.

sponded differently across tiers. Women's share of admitted students rises by 0.72 percentage points at Least Selective institutions, which is about the same size as their increase in the female share of applicants, but there is a statistically significantly different relationship at Medium and Most Selective institutions.

Compared to the Least Selective schools, women make up a smaller share of admitted students at higher tier schools, though the net effect is around zero. A similar gradient appears in financial aid: the share of students receiving institutional aid rises by 2.73 percentage points at Least Selective institutions but is 4.93 percentage points lower at Most Selective institutions, with both estimates significant at the 1 percent level. Taken together, these results suggest that less selective colleges responded by expanding aid and admitting relatively more women, whereas the most selective institutions showed little comparable adjustment. However, the women-to-men acceptance rate is also 3.58 percentage points higher at Most Selective institutions than at Least Selective institutions after *Dobbs*. This may be to offset the decline in applications. Because the table does not report standard errors for linear combinations of coefficients, we treat the interaction estimates as the primary evidence and the implied total effects as descriptive.

The tier-specific event studies in Figure A3 provide a complementary view of the heterogeneity results. Unlike Table 3, which reports interaction estimates relative to Least Selective institutions, the figure estimates the event studies separately for each selectivity subsample. The clearest differences across tiers emerge after 2022. In Panel (a), women's applicant share at Most Selective institutions declines by about 1.2 percentage points in 2023 and 1.0 percentage point in 2024, while the Least Selective series moves in the opposite direction. Importantly, all three selectivity tiers are relatively flat before 2022. In Panel (d), the decline in women's graduate enrollment share at Most Selective institutions also begins after 2022 and reaches 2.8 percentage points in 2024, with the confidence interval excluding zero. Panel (i) shows that the decline in women's share of new tenure-track hires is concentrated primarily in 2023, when it falls by roughly 12 percentage points, followed by a partial rebound in 2024. This hiring pattern is less conclusive, however, because the Medium and Most Selective series are relatively noisy even before 2022. The remaining panels show little evidence of systematic differences across selectivity tiers, broadly consistent with the patterns in Table 3.

We next consider whether popular schools are insulated from these declines by interacting the post-*Dobbs* indicator with a dummy for high enrollment yield (at least 35 percent of admitted students enroll on average in the pre-period). In Table 4 we find that at low-yield institutions, where admitted students are more likely to have attractive alternatives, women's applicant share falls by 0.85 percentage points and women's enrollment share falls by 0.26 percentage points, although neither estimate is individually statistically significant. The interactions for high-yield institutions are positive and significant, at 1.76 percentage points for applicants and 1.08 percentage points for enrollment, implying increases of about 0.9 and 0.8 percentage points, respectively. Women's share of admitted students follows a similar pattern, with a 1.61 percentage-point positive interaction for high-yield institutions. Overall, women's representation tends to decline at institutions whose admitted students have more outside options, while remaining stable or in-

creasing at institutions that have always attracted students. By contrast, the increase in the women-to-men acceptance rate does not vary meaningfully with yield. While being careful to not overstate our findings, these results are consistent with *Dobbs* having the largest impact on high-performing institutions that may not be the first choice within their peer group. This is in line with our conceptual framework suggesting high-achieving women forgo applications to schools they were not likely to attend.

We considered other ways to delineate top schools such as using only those in the US News and World Report's Top 100 Universities and Top 40 Liberal Arts Colleges. These results are available in Table B2 and differ notably from those based on admission-rate terciles. Whereas the tercile results show declines in women's applicant and graduate enrollment shares concentrated among the Most Selective institutions, the Top 100 estimates instead point to relative increases in women's applicant, enrollment, and graduate enrollment shares. An important reason for this difference is the much smaller treated sample under the Top 100 definition. Our sample contains 119 institutions after dropping the 11 states we exclude from the sample, but only 13 are located in ban states, compared with 106 ban-state institutions among the 368 schools in the Most Selective tercile. The Top 100 estimates are therefore based on relatively few treated institutions and are more sensitive to sample composition. Table B3, which defines elite institutions using a 25% admission-rate cutoff, provides little evidence of systematic differences across most outcomes, aside from a relative decline in institutional aid but suffers from the same issue above as very few schools in general meet this definition, particularly in ban states.

We also investigated whether schools that historically attracted more out-of-state students were particularly impacted by the law change considering in-state applicants may be highly path-dependent and the margin for adjustment may be among out-of-state students. However, Table B4 shows no impact for any key outcome except for some evidence of a decline in the share of students receiving aid. We similarly thought that high-income student bodies may respond differently to *Dobbs* as wealthier women may be able to fly to other states if they needed care. Again though, we find no heterogeneous impact on any key outcomes by the percent of students receiving Pell grants (Table B5). Table B5 does show a weakly significant decline in women's graduate enrollment share at high-Pell institutions; however, we interpret this with caution as high-Pell is defined by undergraduate awardees not graduate students so this may not capture the income of graduate student families.

Finally, we find that selectivity matters most among private institutions. Tables B6 and B7 show that the selectivity gradients in women's applicant share, graduate enrollment and tenure-track hiring are concentrated among private institutions, where the Most Selective interactions are -1.04, -3.11, and -9.58 percentage points, respectively. This makes sense if we consider the strong financial incentives of attending in-state public schools. It may be easier to forgo a costly private institution. Interestingly, we find that the

Table 4: Effects by Pre-Period Admission Yield

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	-0.0085	-0.0026	0.0026
	(0.0056)	(0.0042)	(0.0141)
Ban × Post-Dobbs × High-Yield School	0.0176**	0.0108***	-0.0114
	(0.0072)	(0.0030)	(0.0181)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,612	9,568	9,405
R^2	0.92991	0.90533	0.42058
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0128	-0.0040	0.0270**
	(0.0092)	(0.0054)	(0.0126)
Ban × Post-Dobbs × High-Yield School	0.0079	0.0161**	0.0014
	(0.0142)	(0.0061)	(0.0071)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,253	9,568	9,414
R^2	0.86695	0.91772	0.37002
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	-8.1×10^{-5}	0.0060	-0.0203
	(0.0089)	(0.0141)	(0.0174)
Ban × Post-Dobbs × High-Yield School	0.0215	-0.0126	0.0295
	(0.0205)	(0.0220)	(0.0229)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,577	8,037	7,188
R^2	0.88079	0.27517	0.24428

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: A high-yield institution is one whose average admission yield over 2016-2022 was at least 35 percent. The Ban × Post-Dobbs coefficient is the effect for institutions below that cutoff; the interaction is the difference for high-yield institutions. The sample, outcomes, fixed effects, post period, and state-level clustering follow Table 2.

percent of students receiving aid is 4.93 percentage points lower at Most Selective relative to Least Selective for public colleges but is essentially zero for private nonprofit institutions. As noted above, this outcome may not have been trending parallel before 2022 so we interpret this with caution.

5.4 Other Outcomes

Several outcomes were interesting, but their event-study patterns preclude a causal interpretation. Table B1 shows declines in average nine-month faculty salaries overall and for both women and men in Panel B, a

decline in the average institutional aid award in Panel C, and lower SAT and ACT scores in Panel D. Taken at face value, these estimates would suggest that colleges in ban states reduced faculty pay and financial aid and enrolled less academically prepared students after *Dobbs*. However, the corresponding event studies show that these differences were already developing before 2022, so we do not interpret the difference-in-differences estimates as causal.

Figures A1 and A2 show why these estimates should not be interpreted causally. Across these outcomes, the event-study coefficients follow a clear downward pre-trend beginning well before 2022. For example, women's average faculty salary in ban states is roughly $2,500 higher in 2016 before converging to zero by 2022. The average institutional aid award follows a similar pattern, starting about $1,700 higher, while the 75th percentile SAT math score falls from roughly 13 points above the comparison group to zero. Because these trends were already underway for several years before *Dobbs*, the negative post-2022 difference-in-differences estimates are likely to reflect the continuation of existing trends rather than the policy change itself. We therefore report these results for completeness but do not interpret them as effects of *Dobbs*.

We also report additional results on faculty composition, student geographic origin, and international enrollment. Panel A of Table B1 shows no statistically significant changes in women's shares of non-tenure-track, tenure-track, or tenured faculty. Some of these nulls, however, are difficult to interpret because the corresponding event studies show pre-trends. For example, women's share of tenure-track faculty declines from about 1.6 percentage points above the comparison group in 2016 to zero by 2022. The remaining outcomes in Panel C provide more informative evidence. The share of students coming from no-ban states appears unchanged; however, the pre-period shows an increasing trend so it is difficult to interpret this null. If anything, it may be that *Dobbs* halted what had been a growing group of students from Northern states. International enrollment rises modestly on average, but the event study shows that this could be a continuation of earlier trends.

5.5 Robustness Checks

Our first robustness check adds time-varying controls for average net price and total institutional expenditures to the tier specification in Table B8. The results are highly stable: the applicant-share, graduate-enrollment, and tenure-track hiring patterns remain essentially unchanged, and all of the coefficients we emphasize retain the same sign, similar magnitude, and statistical significance.

Our second robustness check broadens the treatment definition to include all 24 states that adopted any post-*Dobbs* abortion restriction, with treatment beginning in 2023. As shown in Table B9, the undergraduate results remain null, while the graduate-enrollment and acceptance-rate effects become smaller and less pre-

cisely estimated. Table B10 shows a similar attenuation for faculty outcomes. This weakening is expected because restrictions across these states differed substantially in scope and timing, so assigning a common treatment date introduces additional measurement error and makes the treatment definition less precise.

Our third robustness check extends the panel back to 2012. As shown in Table B11, the declines in women's applicant share and graduate enrollment at more selective institutions remain statistically significant, and the financial-aid pattern becomes even more pronounced. The tenure-track hiring effect at the Most Selective institutions is somewhat weaker and less precisely estimated, consistent with the noisier pre-trend for that outcome.

Three additional robustness checks vary the treatment timing and sample composition. First, because some students may have adjusted their college decisions immediately after the *Dobbs* decision, we count the fall 2022 entering cohort as treated. As shown in Table B12, the applicant-share and financial-aid patterns remain largely intact, while the graduate-enrollment and tenure-track hiring effects weaken, consistent with these responses emerging primarily with the 2023 cohort. Second, because Pennsylvania is a large no-ban state and has experienced particularly contested abortion politics, we exclude it to ensure that the results are not driven by its classification as a comparison state. The estimates remain essentially unchanged in Table B13. Third, because women's colleges have unusually high female enrollment by construction and may respond differently to abortion restrictions, we exclude them from the sample. Table B14 shows that the main selectivity patterns remain largely unaffected. Together, these checks indicate that our findings are robust to alternative treatment timing and are not driven by Pennsylvania or the inclusion of women's colleges.

We also perform a placebo test to assess the parallel-trends assumption more directly. Table B15 restricts the sample to 2016–2021 and assigns a false treatment beginning in 2019. The placebo estimates for the student outcomes are small and statistically insignificant, including those for women's applicant share and graduate enrollment, providing additional support for the main student results. The financial-aid outcome, however, fails the placebo test: the placebo effects closely resemble the main estimates and are statistically significant. This confirms that the aid gradient reflects a pre-existing divergence rather than a response to *Dobbs*.

Finally, we perform a synthetic difference-in-differences analysis, which constructs a weighted synthetic comparison group that more closely matches the pre-treatment outcomes of institutions in ban states. Table B16 shows that the undergraduate outcomes remain small and statistically insignificant, while the declines in women's graduate enrollment and tenure-track hiring remain similar in direction and magnitude to the baseline difference-in-differences estimates. The acceptance-rate effect becomes notably smaller, further reinforcing our cautious interpretation of that result. Among the Most Selective institutions, Table B17

continues to show declines in women's applicant share, graduate enrollment, institutional aid receipt, and tenure-track hiring. By contrast, the salary and test-score estimates move substantially toward zero after reweighting in Table B18, consistent with our earlier conclusion that those results primarily reflect pre-existing trends rather than effects of *Dobbs*.

Overall, the evidence points to a narrow rather than broad response to abortion bans. We find no measurable changes in undergraduate applications or enrollment, and the estimates rule out shifts of the magnitude suggested by post-*Dobbs* surveys. The clearest effects are concentrated among individuals with broader choice sets, particularly graduate students and tenure-track faculty candidates at the most selective institutions, both of whom search nationally and often commit to a location for several years. Institutional responses, including higher acceptance rates for women relative to men and increased financial aid at less selective colleges, likewise suggest adjustment at the margin rather than a broad reallocation of students across states.

6 Conclusion

Post-*Dobbs* surveys made unusually specific predictions about where students and faculty would go. A large majority of prospective students told pollsters that state reproductive health policy would shape their enrollment decisions, and more than a quarter of college-bound seniors reported ruling out institutions on political grounds (Marken and Hrynowski, 2024; Blake, 2024). Using institution-side IPEDS data on four-year, nonprofit colleges across the country, we find almost none of this in undergraduate behavior. Women's shares of applicants, admitted students, and enrolled students in the thirteen states that banned abortion outright are statistically indistinguishable from their pre-*Dobbs* paths, as are the women-to-men yield ratio and the share of students arriving from states without bans. The estimates are tight enough to rule out an applicant-share decline larger than 1.1 percentage points, so this is a null result rather than an absence of statistical power.

Where we do find effects, they sit exactly where the theory of constrained choice sets predicts. Women's share of graduate enrollment falls by about one percentage point on average and by 3.4 percentage points at the most selective institutions, and women's share of new tenure-track hires falls by 8.6 percentage points at the most selective institutions relative to the least selective. Undergraduate applications show a muted version of the same pattern. Graduate students and academic job candidates capable of earning offers at top institutions search nationwide and can substitute across states at comparatively low cost; these older women may also find abortion bans more salient than teenagers who do not expect to become pregnant. Most undergraduates instead attend a public institution near home and it requires a very strong distaste

for abortion bans to actually overcome the price and location benefits. We do still find undergraduates with broader choices, those applying to top schools, do apply at lower rates following an abortion ban. While stated preferences suggest a consistent distaste for abortion bans, relatively few students and faculty members actually act on this.

The admissions results suggest modest and asymmetric institutional responses. The women-to-men acceptance rate rose by 2.8 percentage points in ban states, and less selective colleges increased women's share of the admitted class, while the most selective did not, despite seeming to bear the brunt of the losses. These admissions changes may explain part of why we see no change in enrollment behavior for any group. One should be cautious about concluding that no change in enrollment means students do not dislike bans because institutions can adjust on these margins to attract more students.

Our results also carry a methodological caution. Several outcomes that produce large and highly significant difference-in-differences coefficients, including faculty salaries, average institutional aid awards, and reported test scores, turn out on inspection to have been converging between ban and non-ban states for at least six years before *Dobbs*. The event studies make this visible, a placebo post period beginning in 2019 reproduces the aid result inside the pre-treatment window, and the synthetic estimates for these outcomes collapse toward zero once the comparison group is reweighted. Reporting the point estimates without the dynamics would have supported a dramatic and entirely unwarranted story about how colleges responded to the decision.

Two limitations bound our conclusions. First, our data end with the 2024 entering cohort, only two years after the decision. However, this does avoid more recent changes in university funding and academic programs which may further impact how students and faculty choose where to spend their educational and academic careers. Our estimates reflect short-run impacts and it is possible as the implementation and enforcement of abortion bans progress, behavior may change. Second, institution-side counts measure who went where, not what it cost them. Most undergraduate women in ban states did not, and probably could not, move, which relocates the incidence of these policies onto the students who stayed rather than onto the institutions that would have borne it through lost enrollment. Measuring what those students face is the natural next step.

References

ACLU of Ohio (2022). Ohio lower court blocks six-week abortion ban, restoring abortion access across state.

American Association of University Professors (2024). Statement on political interference in higher education.

Ananat, E. O., Gruber, J., Levine, P. B., and Staiger, D. (2009). Abortion and selection. *Review of Economics and Statistics*, 91(1):124–136.

Ananat, E. O. and Hungerman, D. M. (2012). The power of the pill for the next generation: Oral contraception's effects on fertility, abortion, and maternal and child characteristics. *Review of Economics and Statistics*, 94(1):37–51.

Arkhangelsky, D., Athey, S., Hirshberg, D. A., Imbens, G. W., and Wager, S. (2021). Synthetic difference-in-differences. *American Economic Review*, 111(12):4088–4118.

Baden, K. and Driver, J. (2023). The state abortion policy landscape one year post-roe.

Banzhaf, H. S. and Walsh, R. P. (2008). Do people vote with their feet? an empirical test of Tiebout's mechanism. *American Economic Review*, 98(3):843–863.

Bergmann, E. (2026). The impact of abortion bans on the physician labor market: Compositional and pipeline effects. *SSRN Working Paper*.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics*, 119(1):249–275.

Blake, J. (2024). 1 in 4 students nixes a college based on state politics, survey shows.

Center for Reproductive Rights (2024). After Roe fell: Abortion laws by state.

Chapman, D. W. (1981). A model of student college choice. *Journal of Higher Education*, 52(5):490–505.

Cletzer, D., Scroggs, J., Simonsen, J., and Washburn, S. (2020). Factors influencing college choice. *NACTA Journal*, 65:536–546.

Dench, D. L., Lifchez, K., Lindo, J. M., and Liu, J. L. (2025). Are people fleeing states with abortion bans? Working Paper 33328, National Bureau of Economic Research.

DesJardins, S. L., Dundar, H., and Hendel, D. D. (1999). Modeling the college application decision process in a land-grant university. *Economics of Education Review*, 18(1):117–132.

Drewes, T. and Michael, C. (2006). How do students choose a university?: an analysis of applications to universities in ontario, canada. *Research in Higher Education*, 47:781–800.

Dura, J. (2025). Court rules north dakota's overturned abortion ban won't be in effect during appeal.

Foster, D. G., Biggs, M. A., Ralph, L., Gerdts, C., Roberts, S., and Glymour, M. M. (2022). Socioeconomic outcomes of women who receive and women who are denied wanted abortions in the united states.

American Journal of Public Health, 112(9):1290–1296.

Griffith, A. L. and Rothstein, D. S. (2009). Can't get there from here: The decision to apply to a selective college. *Economics of Education Review*, 28(5):620–628.

Heller, D. E. (1997). Student price response in higher education: An update to leslie and brinkman. *Journal of Higher Education*, 68(6):624–659.

Hess, J. H. (2024). The impact of dobbs v. jackson on medical school applications. *Economics Letters*, 241:111815.

Hughes, R., Armstrong, E., Freeman, M., Herring, W., Kajikawa, T., Kim, B. H., Nolan, S., and Park, S. (2025). End-of-season report, 2024–2025: First-year application trends. Technical report, Common App, Data Analytics and Research.

Jia, N., Molloy, R., Smith, C., and Wozniak, A. (2023). The economics of internal migration: Advances and policy questions. *Journal of Economic Literature*, 61(1):144–180.

Jones, K. M. and Pineda-Torres, M. (2024). TRAP'd teens: Impacts of abortion provider regulations on fertility and education. *Journal of Public Economics*, 234:105112.

Kane, S. B. (2025). In the wake of Dobbs: The effect of state abortion bans on women's college choices. EdWorkingPaper 25-1126, Annenberg Institute at Brown University.

Langin, K. (2023). Abortion laws are driving academics out of some u.s. states—and keeping others from coming. *Science*. Science, Vol. 381, Issue 6655.

Lindo, J. M., Myers, C., Schlosser, A., and Cunningham, S. (2020). How far is too far? new evidence on abortion clinic closures, access, and abortions. *Journal of Human Resources*, 55(4):1137–1160.

Litten, L. H. (1982). Different strokes in the applicant pool: Some refinements in a model of student college choice. *Journal of Higher Education*, 53(4):383–402.

Lundberg, S. J. and Startz, D. (2022). The end of roe creates new challenges in higher education. *Brookings*. Accessed: 2026-06-24.

Marcén, M. and Morales, M. (2022). The effect of same-sex marriage legalization on interstate migration in the USA. *Journal of Population Economics*, 35:441–469.

Marcus, J. (2025). A looming "demographic cliff": Fewer college students and ultimately fewer graduates. *NPR*. Published by NPR and produced by The Hechinger Report.

Marken, S. and Hrynowski, Z. (2024). State reproductive policies important to enrollment decisions.

McDuff, D. (2007). Quality, tuition, and applications to in-state public colleges. *Economics of Education Review*, 26(4):433–449.

Miller, S., Wherry, L. R., and Foster, D. G. (2023). The economic consequences of being denied an abortion. *American Economic Journal: Economic Policy*, 15(1):394–437.

Myers, C. K. (2017). The power of abortion policy: Reexamining the effects of young women's access to reproductive control. *Journal of Political Economy*, 125(6):2178–2224.

NPR (2023). Abortion services resume in wisconsin after fifteen months.

Oosterbeek, H., Groot, W., and Hartog, J. (1992). An empirical analysis of university choice and earnings. *De economist*, 140(3):293–309.

Orgera, K., Mahmood, H., and Grover, A. (2023). Training location preferences of U.S. medical school graduates post Dobbs v. Jackson Women's Health.

Pampaloni, A. M. (2010). The influence of organizational image on college selection: What students seek in institutions of higher education. *Journal of Marketing for Higher Education*, 20(1):19–48.

Paulsen, M. B. and John, E. P. S. (2002). Social class and college costs: Examining the financial nexus between college choice and persistence. *The Journal of Higher Education*, 73(2):189–236.

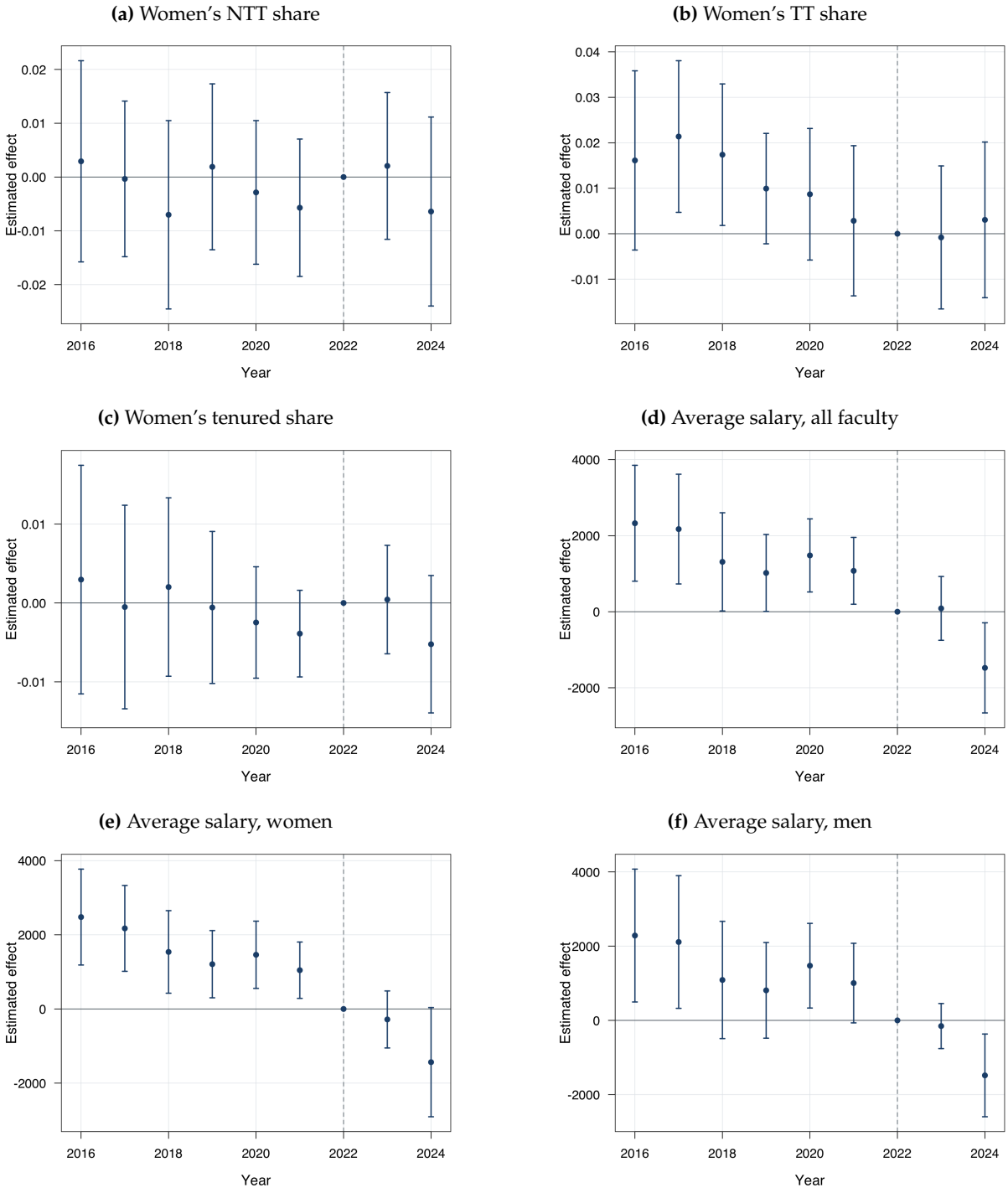
Pope, D. G. and Pope, J. C. (2009). The impact of college sports success on the quantity and quality of student applications. *Southern Economic Journal*, 75(3):750–780.

Walker, B., Wisniewski, J., Torres, J., and Sharma, R. (2023). Anticipatory impacts of the repeal of roe v. wade on female college applicants. *Economics Letters*, 233:111379.

Western Interstate Commission for Higher Education (2024). Knocking at the college door: Projections of high school graduates. Technical report, Western Interstate Commission for Higher Education.

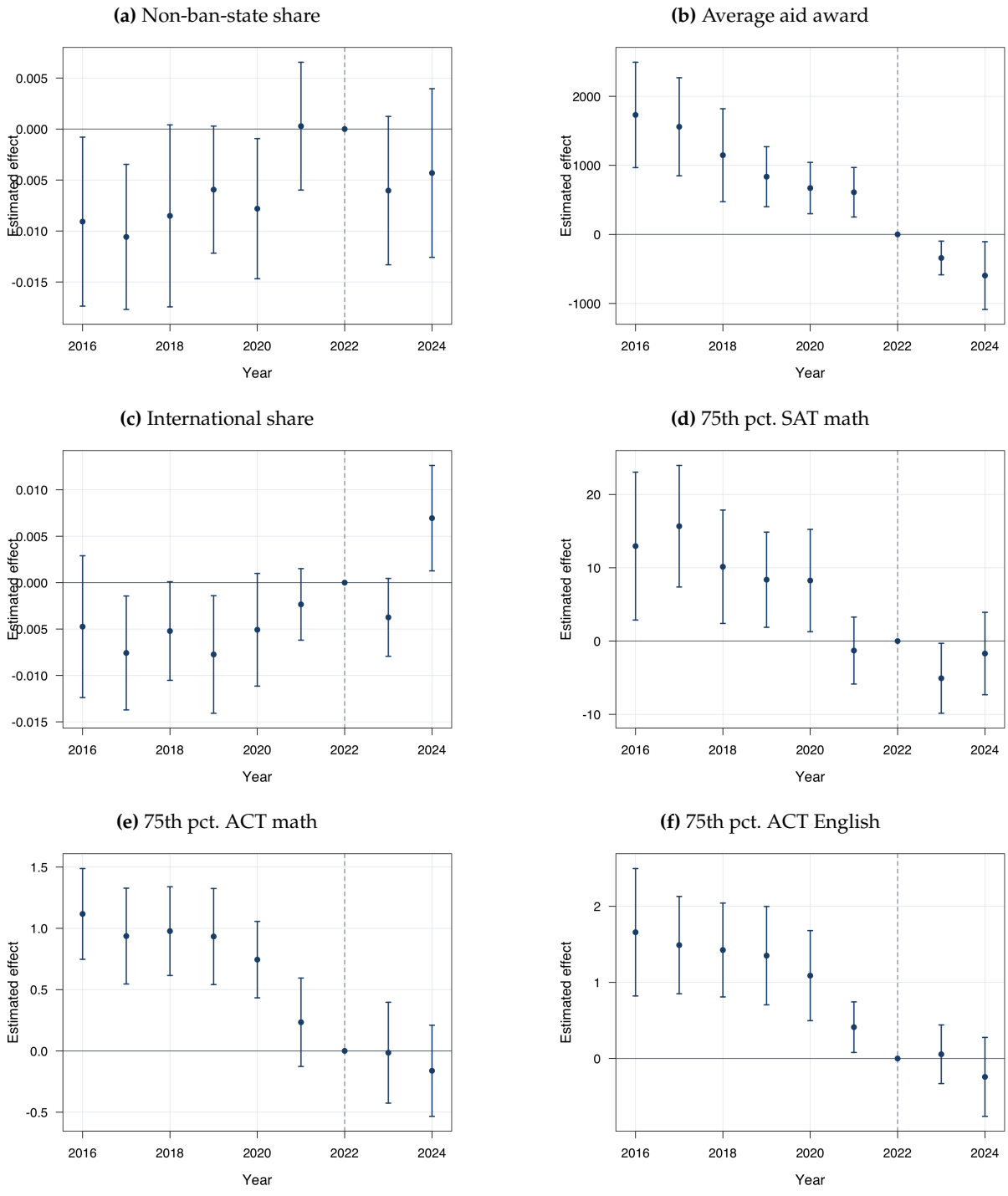
Appendix A: Figures

Figure A1: Dynamic Effects on Faculty Composition and Salaries



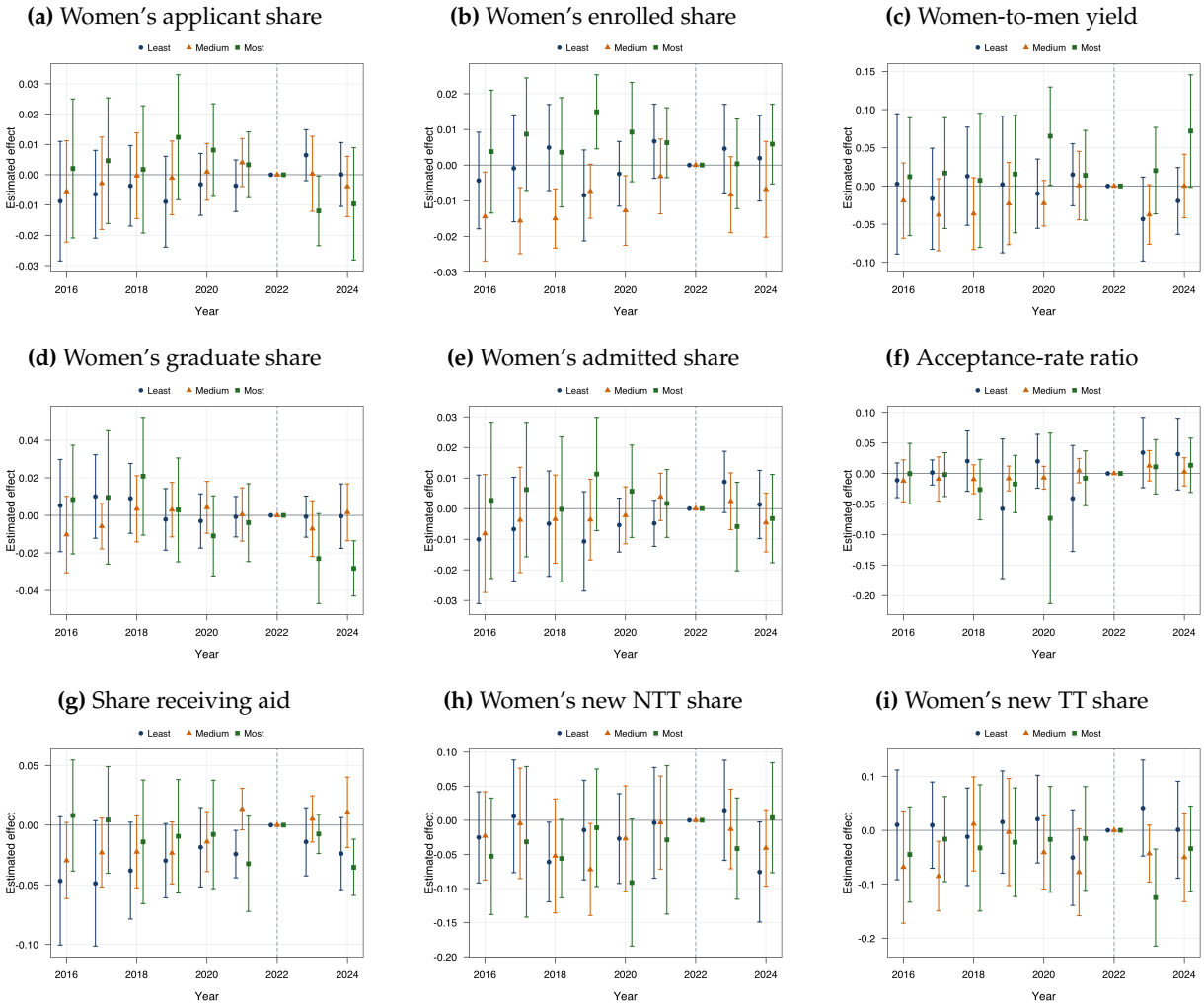
Notes: Each panel plots annual treatment effects relative to 2022 using the main 13-state sample, and corresponds to a column of Panels A and B of Table B1. Models include institution and year fixed effects and cluster standard errors by state. Vertical bars show 95 percent confidence intervals. Share outcomes are proportions and salary outcomes are in nominal dollars. The three salary series and women's share of tenure-track faculty converge toward zero from significantly positive values well before 2022, which is why the corresponding table estimates cannot be read as effects of *Dobbs*.

Figure A2: Dynamic Effects on Enrollment, Aid, and Test Scores



Notes: Each panel plots annual treatment effects relative to 2022 using the main 13-state sample, and corresponds to a column of Panels C and D of Table B1. Models include institution and year fixed effects and cluster standard errors by state. Vertical bars show 95 percent confidence intervals. Aid amounts are in nominal dollars and test scores are in score points. Panel (a) is flat throughout and supports a well-identified null; panels (b), (d), (e), and (f) converge toward zero from significantly positive values before 2022; panel (c) places its entire post-period increase in 2024.

Figure A3: Dynamic Effects by Selectivity Tier, Main Outcomes



Notes: Each panel reports event-study estimates run separately within the Least, Medium, and Most Selective tiers rather than as interaction terms, so the confidence intervals differ from those implied by Table 3. The omitted reference year is 2022. The nine panels correspond to the nine columns of Table 3, read across Panels A, B, and C in turn. Models include institution and year fixed effects, and cluster standard errors by state. Vertical bars show 95 percent confidence intervals.

Appendix B: Tables

Table B1: Faculty, Enrollment Composition, and Test Scores

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of NTT Faculty	Women Share of TT Faculty	Women Share of Tenured Faculty
Variables			
Ban × Post-Dobbs	-0.0005	-0.0098	-0.0020
	(0.0068)	(0.0078)	(0.0039)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,294	8,014	8,120
R^2	0.75489	0.67032	0.90098
Panel B			
Dependent Variables:	Avg. Faculty Salary (9-month)	Avg. Faculty Salary, Women (9-month)	Avg. Faculty Salary, Men (9-month)
Variables			
Ban × Post-Dobbs	-2,028.3**	-2,270.4***	-2,066.7**
	(836.4)	(834.7)	(811.8)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,513	9,362	9,393
R^2	0.97084	0.96078	0.97200
Panel C			
Dependent Variables:	Share Enrolled From Non-Ban States	Average Amount Aid	International Share of Enrolled
Variables			
Ban × Post-Dobbs	0.0010	-1,402.5***	0.0070**
	(0.0021)	(326.6)	(0.0029)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,100	9,542	6,705
R^2	0.99686	0.96053	0.85524
Panel D			
Dependent Variables:	75th Percentile SAT Math	75th Percentile ACT Math	75th Percentile ACT English
Variables			
Ban × Post-Dobbs	-11.06***	-0.7804***	-1.132***
	(3.456)	(0.2386)	(0.3164)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	6,941	6,492	6,488
R^2	0.92448	0.91299	0.91231

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The sample covers 2016–2024 and uses the main 13-state treatment definition, with the post period set to 2023-2024. Models include institution and year fixed effects, and cluster standard errors by state. Panels A and C report share outcomes as proportions, Panel B reports salaries in nominal dollars, the average aid award in Panel C is in nominal dollars, and Panel D reports 75th percentile scores in score points. Figures A1 and A2 show that the salary, aid-amount, and test-score series in Panels B, C, and D were converging toward zero for at least six years before 2022, so these coefficients should not be read as effects of *Dobbs*.

Table B2: Effects by Top-100 Status

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	-0.0033	0.0001	-0.0016
	(0.0042)	(0.0036)	(0.0131)
Ban × Post-Dobbs × High US News Ranking	0.0135**	0.0184***	0.0092
	(0.0050)	(0.0041)	(0.0179)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,612	9,568	9,405
R^2	0.92972	0.90529	0.42056
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0115**	0.0006	0.0268**
	(0.0054)	(0.0044)	(0.0122)
Ban × Post-Dobbs × High US News Ranking	0.0295***	0.0157***	0.0156
	(0.0044)	(0.0041)	(0.0273)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,253	9,568	9,414
R^2	0.86700	0.91756	0.37002
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0088	0.0045	-0.0114
	(0.0076)	(0.0119)	(0.0130)
Ban × Post-Dobbs × High US News Ranking	-0.0450***	-0.0520***	0.0045
	(0.0145)	(0.0107)	(0.0116)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,577	8,037	7,188
R^2	0.88077	0.27524	0.24413

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This specification uses the main 13-state, 2016–2024 sample and interacts Ban × Post-*Dobbs* with an indicator for a top-100 institution in the US News rankings. The main coefficient is the effect for institutions outside the top 100; the interaction is the difference for top-100 institutions. Models include institution and year fixed effects and cluster standard errors by state.

Table B3: Effects by Elite Status

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	-0.0028	0.0007	-0.0017
	(0.0042)	(0.0036)	(0.0129)
Ban × Post-Dobbs × Elite (Top Schools)	0.0055	0.0117*	0.0314
	(0.0033)	(0.0065)	(0.0243)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,612	9,568	9,405
R^2	0.92969	0.90525	0.42057
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0105*	0.0012	0.0276**
	(0.0056)	(0.0044)	(0.0122)
Ban × Post-Dobbs × Elite (Top Schools)	0.0198*	0.0055	-0.0068
	(0.0109)	(0.0051)	(0.0259)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,253	9,568	9,414
R^2	0.86693	0.91753	0.37002
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0080	0.0020	-0.0117
	(0.0075)	(0.0120)	(0.0135)
Ban × Post-Dobbs × Elite (Top Schools)	-0.0645**	-0.0087	0.0310
	(0.0276)	(0.0466)	(0.0428)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,577	8,037	7,188
R^2	0.88075	0.27515	0.24414

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This specification uses the main 13-state, 2016–2024 sample and interacts Ban × Post-*Dobbs* with an indicator for an institution whose average admission rate over 2016-2022 was below 26 percent. The main coefficient is the effect for all other institutions. Models include institution and year fixed effects and cluster standard errors by state.

Table B4: Effects by Pre-Period Out-of-State Share

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	-0.0016	0.0021	-0.0009
	(0.0043)	(0.0036)	(0.0120)
Ban × Post-Dobbs × High Out-of-State Share	-0.0066	-0.0056	0.0074
	(0.0060)	(0.0033)	(0.0258)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,463	9,419	9,256
R^2	0.93078	0.90675	0.41919
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0123*	0.0022	0.0265**
	(0.0065)	(0.0043)	(0.0124)
Ban × Post-Dobbs × High Out-of-State Share	0.0129	-0.0061	0.0054
	(0.0117)	(0.0061)	(0.0169)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,253	9,419	9,265
R^2	0.86697	0.91877	0.36995
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0123*	-0.0024	-0.0111
	(0.0072)	(0.0104)	(0.0138)
Ban × Post-Dobbs × High Out-of-State Share	-0.0329**	0.0275	-0.0006
	(0.0138)	(0.0319)	(0.0319)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,428	8,037	7,188
R^2	0.88012	0.27522	0.24412

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The main 13-state, 2016-2024 specification interacts treatment with an indicator for institutions whose pre-period out-of-state enrollment share is at least 50 percent. Institutions below the cutoff are the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B5: Effects by Pre-Period Pell Share

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	-0.0037	0.0005	-0.0060
	(0.0049)	(0.0037)	(0.0119)
Ban × Post-Dobbs × High Pell Share	0.0036	0.0013	0.0175
	(0.0058)	(0.0038)	(0.0173)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,594	9,550	9,387
R^2	0.92908	0.90419	0.42055
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0073	0.0001	0.0300**
	(0.0058)	(0.0051)	(0.0119)
Ban × Post-Dobbs × High Pell Share	-0.0109*	0.0043	-0.0098
	(0.0057)	(0.0051)	(0.0132)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,253	9,550	9,396
R^2	0.86697	0.91638	0.36956
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0057	-0.0068	-0.0054
	(0.0073)	(0.0140)	(0.0133)
Ban × Post-Dobbs × High Pell Share	0.0044	0.0317	-0.0242
	(0.0198)	(0.0213)	(0.0235)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,577	8,020	7,183
R^2	0.88070	0.27485	0.24447

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The main 13-state, 2016-2024 specification interacts treatment with an indicator for institutions whose pre-period Pell-recipient share is at least 50 percent. Institutions below the cutoff are the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B6: Selectivity Tiers, Private Institutions

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	0.0001	-0.0063*	-0.0328
	(0.0054)	(0.0035)	(0.0209)
Ban × Post-Dobbs × tierMedium	-0.0206***	-0.0078	0.0421*
	(0.0072)	(0.0047)	(0.0228)
Ban × Post-Dobbs × tierMostSelective	-0.0104*	0.0092	0.0499
	(0.0054)	(0.0065)	(0.0306)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	6,034	5,990	5,827
R^2	0.93010	0.89810	0.41331
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0106	0.0006	0.0224
	(0.0132)	(0.0050)	(0.0175)
Ban × Post-Dobbs × tierMedium	-0.0048	-0.0191***	0.0080
	(0.0148)	(0.0069)	(0.0137)
Ban × Post-Dobbs × tierMostSelective	-0.0311**	-0.0003	0.0296
	(0.0149)	(0.0067)	(0.0179)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	4,969	5,990	5,836
R^2	0.84244	0.91651	0.36145
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0032	-0.0228	0.0198
	(0.0115)	(0.0415)	(0.0589)
Ban × Post-Dobbs × tierMedium	-0.0049	0.0743*	-0.0150
	(0.0144)	(0.0408)	(0.0746)
Ban × Post-Dobbs × tierMostSelective	-0.0008	0.0371	-0.0958**
	(0.0111)	(0.0532)	(0.0421)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	6,034	4,924	3,907
R^2	0.89394	0.28288	0.23298

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The tier specification of Table 3 estimated on private not-for-profit institutions only. Least Selective institutions remain the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B7: Selectivity Tiers, Public Institutions

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	0.0021	0.0032	-0.0052
	(0.0038)	(0.0039)	(0.0205)
Ban × Post-Dobbs × tierMedium	0.0009	0.0028	0.0165
	(0.0059)	(0.0038)	(0.0197)
Ban × Post-Dobbs × tierMostSelective	-0.0066	-0.0056	-0.0135
	(0.0041)	(0.0068)	(0.0161)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	3,577	3,577	3,577
R^2	0.94349	0.93211	0.46458
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	0.0012	0.0021	-0.0007
	(0.0080)	(0.0042)	(0.0073)
Ban × Post-Dobbs × tierMedium	0.0082	0.0027	0.0099
	(0.0071)	(0.0062)	(0.0090)
Ban × Post-Dobbs × tierMostSelective	-0.0072	0.0025	0.0445
	(0.0129)	(0.0054)	(0.0272)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	3,284	3,577	3,577
R^2	0.93066	0.93355	0.64631
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	-0.0054	-0.0040	0.0200
	(0.0175)	(0.0247)	(0.0207)
Ban × Post-Dobbs × tierMedium	-0.0035	-0.0194	-0.0372
	(0.0168)	(0.0279)	(0.0309)
Ban × Post-Dobbs × tierMostSelective	-0.0493***	0.0042	-0.0475
	(0.0176)	(0.0333)	(0.0399)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	3,542	3,113	3,281
R^2	0.80174	0.25872	0.24917

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The tier specification of Table 3 estimated on public institutions only. Least Selective institutions remain the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B8: Selectivity Tiers with Time-Varying Controls

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	0.0084**	0.0039	-0.0235
	(0.0039)	(0.0030)	(0.0155)
Average Net Price	-1.2×10^{-7}	-4.9×10^{-7}	-2.14×10^{-6}**
	(3.47×10^{-7})	(3.59×10^{-7})	(7.97×10^{-7})
Institution Expenses	7.24×10^{-12}***	8.87×10^{-12}	1.08×10^{-11}**
	(1.17×10^{-12})	(1×10^{-8})	(5.17×10^{-12})
Avg. Faculty Salary (9-month)	7.69×10^{-7}***	7.12×10^{-7}**	-1.43×10^{-7}
	(2.22×10^{-7})	(2.72×10^{-7})	(7.95×10^{-7})
Ban × Post-Dobbs × tierMedium	-0.0109***	-0.0036	0.0289**
	(0.0038)	(0.0046)	(0.0115)
Ban × Post-Dobbs × tierMostSelective	-0.0167***	-0.0018	0.0318
	(0.0041)	(0.0047)	(0.0191)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,374	9,330	9,167
R^2	0.93057	0.90660	0.42088
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	0.0005	0.0083**	0.0075
	(0.0065)	(0.0036)	(0.0084)
Average Net Price	7.78×10^{-7}**	2.02×10^{-8}	8.08×10^{-7}
	(3.54×10^{-7})	(3.55×10^{-7})	(7.99×10^{-7})
Institution Expenses	7.15×10^{-12}**	5.95×10^{-12}***	1.18×10^{-11}
	(3.06×10^{-12})	(1.8×10^{-12})	(1×10^{-8})
Avg. Faculty Salary (9-month)	4.88×10^{-7}**	7.37×10^{-7}***	-1.81×10^{-6}
	(2.27×10^{-7})	(2.71×10^{-7})	(2.31×10^{-6})
Ban × Post-Dobbs × tierMedium	0.0007	-0.0091**	0.0074
	(0.0076)	(0.0036)	(0.0065)
Ban × Post-Dobbs × tierMostSelective	-0.0325***	-0.0065	0.0317**
	(0.0119)	(0.0045)	(0.0133)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,177	9,330	9,176
R^2	0.86895	0.91800	0.40777
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0284***	-0.0110	0.0227
	(0.0088)	(0.0213)	(0.0238)
Average Net Price	-7.53×10^{-7}	2.31×10^{-7}	5.45×10^{-7}
	(1.16×10^{-6})	(1.64×10^{-6})	(1.13×10^{-6})
Institution Expenses	-2.37×10^{-11}***	1.04×10^{-12}	-1.59×10^{-12}
	(6.85×10^{-12})	(1×10^{-8})	(1.16×10^{-11})
Avg. Faculty Salary (9-month)	2.05×10^{-6}***	-1.97×10^{-7}	3.46×10^{-7}
	(6.92×10^{-7})	(7.43×10^{-7})	(5.95×10^{-7})
Ban × Post-Dobbs × tierMedium	-0.0093	0.0188	-0.0300
	(0.0123)	(0.0240)	(0.0315)
Ban × Post-Dobbs × tierMostSelective	-0.0472***	0.0230	-0.0920***
	(0.0084)	(0.0286)	(0.0270)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,375	7,945	7,117
R^2	0.88194	0.27198	0.24386

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The tier specification of Table 3 with average net price and total institutional expenditures added as time-varying controls. Least Selective institutions remain the reference group. Models include institution and year fixed effects, include time-varying controls, and cluster standard errors by state.

Table B9: Main Outcomes, 24-State Treatment Definition

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	-7.24×10^{-5}	0.0013	0.0047
	(0.0033)	(0.0027)	(0.0139)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	12,521	12,462	12,219
R^2	0.93156	0.90617	0.42487
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0063	0.0018	0.0209*
	(0.0043)	(0.0033)	(0.0117)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	10,737	12,468	12,237
R^2	0.86315	0.92105	0.37569
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	-0.0033	0.0003	-0.0073
	(0.0069)	(0.0108)	(0.0105)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	12,486	10,593	9,204
R^2	0.89001	0.28118	0.23700

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The sample covers 2016-2024 and classifies all 24 states with any post-*Dobbs* restriction as treated, retaining a common 2023 post indicator rather than each state's actual effective date. Models include institution and year fixed effects and cluster standard errors by state.

Table B10: Faculty Outcomes, 24-State Definition

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of New NTT	Women Share of New TT	Women Share of All Hires
Variables			
Ban × Post-Dobbs	0.0003	-0.0073	-0.0025
	(0.0108)	(0.0105)	(0.0096)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	10,593	9,204	11,729
R^2	0.28118	0.23700	0.30276
Panel B			
Dependent Variables:	Women Share of NTT Faculty	Women Share of TT Faculty	Women Share of Tenured Faculty
Variables			
Ban × Post-Dobbs	-0.0012	-0.0106*	-0.0003
	(0.0043)	(0.0057)	(0.0030)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	12,155	10,316	10,468
R^2	0.75817	0.66936	0.89346
Panel C			
Dependent Variables:	Avg. Faculty Salary (9-month)	Avg. Faculty Salary, Women (9-month)	Avg. Faculty Salary, Men (9-month)
Variables			
Ban × Post-Dobbs	-1,869.0**	-2,027.7**	-1,912.3**
	(791.1)	(770.7)	(808.5)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	12,415	12,242	12,273
R^2	0.97137	0.96104	0.97222

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The sample covers 2016-2024 and classifies all 24 states with any post-*Dobbs* restriction as treated. Panels A and B report share outcomes as proportions and Panel C reports salaries in nominal dollars. Models include institution and year fixed effects and cluster standard errors by state.

Table B11: Selectivity Tiers, 2012-2024

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	0.0112***	0.0064	-0.0212
	(0.0040)	(0.0039)	(0.0142)
Ban × Post-Dobbs × tierMedium	-0.0137***	-0.0061	0.0271**
	(0.0048)	(0.0052)	(0.0120)
Ban × Post-Dobbs × tierMostSelective	-0.0181***	-0.0035	0.0258
	(0.0061)	(0.0047)	(0.0238)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	13,499	13,484	13,234
R^2	0.91078	0.89305	0.41245
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0034	0.0098**	0.0053
	(0.0074)	(0.0042)	(0.0125)
Ban × Post-Dobbs × tierMedium	0.0026	-0.0112**	0.0106
	(0.0094)	(0.0049)	(0.0075)
Ban × Post-Dobbs × tierMostSelective	-0.0271**	-0.0076	0.0358***
	(0.0126)	(0.0057)	(0.0119)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	11,526	13,484	13,246
R^2	0.84451	0.90276	0.38029
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0397***	-0.0036	0.0107
	(0.0117)	(0.0214)	(0.0241)
Ban × Post-Dobbs × tierMedium	-0.0193	0.0074	-0.0201
	(0.0155)	(0.0250)	(0.0313)
Ban × Post-Dobbs × tierMostSelective	-0.0731***	0.0239	-0.0640*
	(0.0097)	(0.0285)	(0.0317)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	13,448	10,702	9,831
R^2	0.86883	0.22140	0.20954

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This specification extends the main 13-state analysis to 2012–2024 and retains institutions observed in at least 11 of the 13 years. Least Selective institutions are the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B12: Selectivity Tiers with a 2022 Post Period

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	0.0072	0.0020	-0.0174
	(0.0044)	(0.0029)	(0.0180)
Ban × Post-Dobbs × tierMedium	-0.0115***	-0.0014	0.0284***
	(0.0026)	(0.0029)	(0.0102)
Ban × Post-Dobbs × tierMostSelective	-0.0150***	-0.0009	0.0216
	(0.0043)	(0.0044)	(0.0226)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,612	9,568	9,405
R^2	0.92985	0.90525	0.42073
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0014	0.0062	0.0064
	(0.0062)	(0.0047)	(0.0104)
Ban × Post-Dobbs × tierMedium	0.0010	-0.0088***	0.0132**
	(0.0066)	(0.0027)	(0.0065)
Ban × Post-Dobbs × tierMostSelective	-0.0228*	-0.0053	0.0390***
	(0.0118)	(0.0043)	(0.0097)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,253	9,568	9,414
R^2	0.86723	0.91759	0.37015
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0353***	-0.0004	0.0198
	(0.0105)	(0.0173)	(0.0239)
Ban × Post-Dobbs × tierMedium	-0.0187	0.0246	-0.0150
	(0.0129)	(0.0167)	(0.0282)
Ban × Post-Dobbs × tierMostSelective	-0.0475***	0.0223	-0.0423
	(0.0100)	(0.0222)	(0.0350)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,577	8,037	7,188
R^2	0.88125	0.27541	0.24437

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The tier specification with Post-*Dobbs* set to one from 2022 rather than 2023, so that the fall 2022 entering cohort is treated. Least Selective institutions remain the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B13: Selectivity Tiers Excluding Pennsylvania

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	0.0060	0.0024	-0.0172
	(0.0038)	(0.0030)	(0.0152)
Ban × Post-Dobbs × tierMedium	-0.0118***	-0.0046	0.0285**
	(0.0037)	(0.0047)	(0.0114)
Ban × Post-Dobbs × tierMostSelective	-0.0184***	-0.0029	0.0330
	(0.0043)	(0.0047)	(0.0196)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,787	8,743	8,601
R^2	0.93006	0.90562	0.42185
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0015	0.0061	0.0110
	(0.0067)	(0.0036)	(0.0131)
Ban × Post-Dobbs × tierMedium	0.0003	-0.0100***	0.0097
	(0.0078)	(0.0034)	(0.0060)
Ban × Post-Dobbs × tierMostSelective	-0.0330**	-0.0079*	0.0358***
	(0.0124)	(0.0046)	(0.0122)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	7,582	8,743	8,608
R^2	0.87301	0.91812	0.44432
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0264***	-0.0084	0.0233
	(0.0085)	(0.0213)	(0.0241)
Ban × Post-Dobbs × tierMedium	-0.0117	0.0192	-0.0304
	(0.0120)	(0.0239)	(0.0313)
Ban × Post-Dobbs × tierMostSelective	-0.0493***	0.0231	-0.0857***
	(0.0079)	(0.0283)	(0.0291)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,752	7,383	6,597
R^2	0.88094	0.27911	0.25419

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The tier specification with Pennsylvania institutions dropped from the comparison group. Least Selective institutions remain the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B14: Selectivity Tiers Excluding Women's Colleges

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-Dobbs	0.0065	0.0025	-0.0188
	(0.0040)	(0.0030)	(0.0149)
Ban × Post-Dobbs × tierMedium	-0.0119***	-0.0048	0.0285**
	(0.0037)	(0.0049)	(0.0114)
Ban × Post-Dobbs × tierMostSelective	-0.0182***	-0.0030	0.0240
	(0.0046)	(0.0049)	(0.0164)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,415	9,371	9,370
R^2	0.91646	0.88254	0.42951
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-Dobbs	-0.0002	0.0063*	-0.0027
	(0.0065)	(0.0037)	(0.0058)
Ban × Post-Dobbs × tierMedium	-0.0004	-0.0101***	0.0097
	(0.0075)	(0.0035)	(0.0060)
Ban × Post-Dobbs × tierMostSelective	-0.0345**	-0.0077	0.0372***
	(0.0131)	(0.0049)	(0.0115)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	8,090	9,371	9,371
R^2	0.86284	0.89907	0.65185
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-Dobbs	0.0265***	-0.0143	0.0226
	(0.0083)	(0.0214)	(0.0238)
Ban × Post-Dobbs × tierMedium	-0.0125	0.0198	-0.0305
	(0.0120)	(0.0243)	(0.0313)
Ban × Post-Dobbs × tierMostSelective	-0.0500***	0.0223	-0.0859***
	(0.0083)	(0.0302)	(0.0289)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	9,381	7,871	7,037
R^2	0.88027	0.26946	0.23783

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The tier specification with women's colleges dropped, identified as institutions reporting no male applicants in 2016 or in any later sample year. Least Selective institutions remain the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B15: Placebo Test: Tiers with a 2019 Post Period

	(1)	(2)	(3)
Panel A			
Dependent Variables:	Women Share of Applicants	Women Share of Enrolled	Women/Men Yield
Variables			
Ban × Post-2019	0.0034	0.0012	0.0146
	(0.0043)	(0.0021)	(0.0198)
Ban × Post-2019 × tierMedium	0.0006	0.0032	0.0029
	(0.0043)	(0.0023)	(0.0209)
Ban × Post-2019 × tierMostSelective	-0.0002	0.0045	-0.0057
	(0.0049)	(0.0031)	(0.0235)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	6,435	6,408	6,292
R^2	0.95004	0.92346	0.45555
Panel B			
Dependent Variables:	Women Share of Grad. Students	Women Share of Admitted	Women/Men Acceptance Rate
Variables			
Ban × Post-2019	-0.0078	0.0009	-0.0199**
	(0.0065)	(0.0047)	(0.0091)
Ban × Post-2019 × tierMedium	0.0099	0.0017	0.0041
	(0.0082)	(0.0047)	(0.0070)
Ban × Post-2019 × tierMostSelective	-0.0072	0.0036	0.0164**
	(0.0122)	(0.0051)	(0.0080)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	5,450	6,407	6,299
R^2	0.90140	0.93888	0.47961
Panel C			
Dependent Variables:	% Receiving Aid	Women Share of New NTT	Women Share of New TT
Variables			
Ban × Post-2019	0.0270**	0.0079	-0.0110
	(0.0120)	(0.0139)	(0.0203)
Ban × Post-2019 × tierMedium	-0.0152	-0.0092	0.0121
	(0.0141)	(0.0240)	(0.0221)
Ban × Post-2019 × tierMostSelective	-0.0435**	-0.0062	0.0285
	(0.0194)	(0.0265)	(0.0227)
Fixed-effects			
University ID	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	6,413	5,378	4,811
R^2	0.92869	0.33400	0.28787

Clustered (State) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The sample is restricted to 2016-2021, entirely before *Dobbs*, and Post-2019 equals one from 2019 onward. Any coefficient that reproduces the pattern of the main estimates signals a violation of parallel trends rather than a policy response. Least Selective institutions remain the reference group. Models include institution and year fixed effects and cluster standard errors by state.

Table B16: Synthetic DiD, Main Outcomes

Outcome	Estimate	Observations
Women Share of Applicants	-0.005 (0.003)	9117
Women Share of Enrolled	-0.002 (0.002)	9108
Women/Men Yield	0 (0.009)	8910
Women Share of Grad Students	-0.007 (0.004)	7722
Women Share of Admitted	-0.002 (0.002)	9099
Women/Men Acceptance Rate	0.011 (0.006)	8919
% Receiving Aid	-0.006 (0.006)	9081
Women Share of New NTT	0.01 (0.015)	5211
Women Share of New TT	-0.025 (0.015)	4734

Notes: Synthetic difference-in-differences estimates following Arkhangelsky et al. (2021), using the 13-state, 2016-2024 sample with treatment switched on in 2023. Each outcome is estimated on its own balanced nine-year panel, which is why the observation counts are smaller than in Table 2. Jackknife standard errors are in parentheses. All outcomes are proportions.

Table B17: Synthetic DiD, Most Selective Institutions

Outcome	Estimate	Observations
Women Share of Applicants	-0.011 (0.006)	3123
Women Share of Enrolled	-0.003 (0.004)	3114
Women/Men Yield	0.019 (0.017)	3015
Women Share of Grad Students	-0.016 (0.009)	2430
Women Share of Admitted	-0.008 (0.005)	3114
Women/Men Acceptance Rate	0.016 (0.017)	3015
% Receiving Aid	-0.021 (0.01)	3078
Women Share of New NTT	0.045 (0.026)	1809
Women Share of New TT	-0.054 (0.03)	1710

Notes: Synthetic difference-in-differences estimates restricted to institutions in the Most Selective admission-rate tier. Because the estimator cannot accommodate interaction terms, heterogeneity is examined by estimating on the subsample. Jackknife standard errors are in parentheses. All outcomes are proportions.

Table B18: Synthetic DiD, Appendix Outcomes

Outcome	Estimate	Observations
Women Share of NTT Faculty	-0.005 (0.01)	7191
Women Share of TT Faculty	0 (0.005)	7668
Women Share of Tenured Faculty	-0.002 (0.002)	7794
Avg. Faculty Salary (9-month)	-337.821 (292.433)	9090
Avg. Faculty Salary, Women (9-month)	-490.043 (275.725)	9036
Avg. Faculty Salary, Men (9-month)	-607.947 (292.591)	9045
Share Enrolled From Non-Ban States	-0.001 (0.003)	4149
Average Amount Aid	-300.013 (130.864)	9036
International Share of Enrolled	0.004 (0.002)	2736
75th Percentile Math SAT Scores	-3.318 (2.197)	3933
75th Percentile Math ACT Scores	-0.644 (0.116)	3699
75th Percentile English ACT Scores	-0.711 (0.136)	3708

Notes: Synthetic difference-in-differences estimates for the outcomes reported in Table B1. Share outcomes are proportions; salaries and aid amounts are in nominal dollars; test scores are in score points. Jackknife standard errors are in parentheses. The salary and test-score estimates are an order of magnitude smaller here than in Table B1, which is what should happen when the difference-in-differences coefficient is generated by a pre-existing trend rather than by the policy.