What does free community college buy? Early impacts from the Oregon Promise

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Abstract: This paper examines the Oregon Promise, a state-level program that exclusively subsidizes in-state community college attendance. I estimate impacts using a difference-in-difference design that links students in states with essentially universal 10th grade PSAT coverage to national-level postsecondary enrollment data. I find that the implementation of the Oregon Promise increased enrollment at two-year colleges by roughly four to five percentage points for the first two eligible cohorts. In the first year of the program the increase in community college enrollment comes primarily from students shifting out of four-year colleges, whereas in the second year the program predominately increases overall postsecondary enrollment.

Introduction

Rising income inequality, particularly between adults with and without a college degree, has reinvigorated a national dialogue about the role of postsecondary institutions as a driver of social mobility. One concern is the growth in postsecondary tuition; adjusting for inflation, the average published tuition and fees at two-year and four-year public institutions is 2.3 and 3.1 times as expensive in 2018 as they were thirty years prior (Ma, Baum, Pender, & Welch, 2017). Family income is a strong predictor of postsecondary success, and rising tuition will exacerbate inequality if college costs inhibit access for those most in need.

Trends in postsecondary pricing have prompted a discussion as to whether college should essentially be free, as with our public system of K-12 education. Analyzing “Promise” programs is one window into understanding the potential implications of a free college system. Promise programs are place-based scholarship programs that aim to increase college-going through two primary mechanisms. The first is sending a clear signal of college affordability, as complicated application processes and differential access to information could negatively impact disadvantaged students (Dynarski, Libassi, Michelmore, & Owen, 2018; Hoxby & Turner, 2015). The second is by subsiding postsecondary enrollment, generally at institutions proximate to the geographic entity offering the program. Promise programs increase college attendance and degree completion, though only a small number of highly publicized programs have been studied to date (Andrews, DesJardins, & Ranchhod, 2010; Bartik, Hershbein, & Lachowska, 2017; Carruthers, Fox, & Jepsen, 2018; Page, Iriti, Lowry, & Anthony, 2017). There is little consistent format to Promise programs, which vary by eligibility criteria, financial generosity, and the types of eligible postsecondary institutions (Perna, Leigh, & Carroll, 2017). Differences in program structure likely give rise to differential impacts, suggesting that researchers may want to consider multiple empirical evaluations, as well as theory, to synthesize and predict the potential positive and negative impacts of this policy lever.

This paper examines the Oregon Promise, which focuses exclusively on promoting in-state community college attendance. The Oregon Promise is one of a small number of Promise programs that operates at the state level, and became first available to the high school graduating class of 2016. The Oregon Promise covers in-state community college tuition for residents whose needs are not met by other sources. Students whose tuition and fees are covered by other sources are provided $1,000 per year.

A key question is not just whether the program increases community college enrollment, but whether in the absence of the program these students would have attended a four-year college or no college at all. Although there are significant, long-term benefits to a college education (Bhuller, Mogstad, & Salvanes, 2017; Ost, Pan, & Webber, forthcoming; Zimmerman, 2014), subsidies can induce students to attend lower-quality institutions, potentially decreasing graduation rates and other long-term outcomes (Carruthers et al., 2018; Cohodes & Goodman, 2014; Peltzman, 1973). Where one begins their postsecondary education matters, with students significantly more likely to earn a bachelor's degree if they start at a four-year rather than a two-year college (Goodman, Hurwitz, & Smith, 2017). Understanding to what extent Promise programs shift students from no college or other postsecondary sectors helps determine the long-term implications of these programs.

I estimate program impacts in a difference-in-difference analysis, using data from the College Board that links PSAT- and SAT-taking students to National Student Clearinghouse (NSC) data on postsecondary enrollments. I rely primarily on data comprising all 10^{th} grade PSAT takers, in order to handle potential endogeneity in my sample composition and ensure that my results are applicable to a large population. Oregon subsidizes 10th grade PSAT exams for public school students and offers the exam during the academic school day, minimizing the reliance on a sample of self-selected exam takers that might not generalize broadly. As a comparison group I focus on six control states that similarly subsidize the 10th grade PSAT exam. The use of 10th grade PSAT takers also eliminates potential endogeneity that might occur from shifts in later SAT participation or exam effort, as the PSAT exam provides a measure of students' academic preparation that occurred significantly prior to the point that the Oregon Promise was under discussion in the state legislature. Altering the sample composition by including students who did not take the 10th grade PSAT, adding additional states, or using alternate methods such as matching and synthetic control designs, does little to change the primary results on community college attendance, though has some impact on counterfactual outcomes of four-year college attendance or no college attendance at all.

I find that the implementation of the Oregon Promise increased enrollment at in-state, public two-year colleges between four and five percentage points. Results are robust to a variety of standard error adjustments, including new methods explicitly focused on difference-in-difference settings using individual-level data clustered within a single treatment group (Ferman & Pinto, forthcoming). Regression estimates suggest that increased community college enrollment in the first year of the program came predominately as students shifted out of four-year colleges, whereas by the second year the program induced gains in overall postsecondary attendance. There is more uncertainty in these estimates, with traditional clustering methods producing fairly precise impacts but newer methods indicating we may be unable to differentiate between students' counterfactual postsecondary enrollment status. Heterogeneous impacts, using an index based on propensity to attend a four-year college, show the largest gains in community college enrollment come from students that can be roughly considered in the "middle", with smaller impacts from students with very low and very high propensity to attend a four-year college (and conversely, high and low propensity to attend no college at all).

This paper contributes to our understanding of postsecondary financing, with a particular focus on the role that "free" college or Promise programs can play. A common concern is that many states promote last-dollar scholarships – aid programs that backfills expenses after pre-existing forms of aid are taken into account – so as not to cannibalize federal Pell dollars. Without adequate oversight these programs can be regressive in nature by moving money towards middle- or high-income earners, potentially encouraging them to attend less selective institutions (Carruthers et al., 2018). In the short-run, the Oregon Promise may have had some impact on shifting students to institutions that are less structured and provide fewer resources per student, which in other contexts has had negative impacts on degree completion (Cohodes & Goodman, 2014; Goodman et al., 2017; Scott-Clayton, 2015). Adding income eligibility criteria, which Oregon implemented in the second year of the program, is one way to counteract this issue and may be partly responsible for limiting student shifting out of four-year colleges.

Positive impacts on postsecondary attendance appear stronger in the second year of the program, and give hope that the program will lead to meaningful gains in degree completion. One implication is that the saliency of the program – the message of college as "free" and how well information is received among the target population – likely impacts postsecondary attendance as much as the monetary amount involved. Students with the lowest propensity to attend a four-year college, who likely received the least amount of state money, were ultimately responsive to the program, with delayed impacts perhaps as program awareness diffused into those communities.

Students with higher propensity of four-year attendance responded in the short-run by shifting away from four-year colleges but there was little evidence of this behavior in the second cohort, suggesting a weak role for actual financing as mattering to their ultimate decision.

This paper can only examine short-run effects, and the true test will be whether students induced to enroll are successful (Cunha, Miller, & Weisburst, 2018), or if aid increases persistence or shortens time to degree (Bettinger, Gurantz, Kawano, Sacerdote, & Stevens, 2019; Denning, 2017a). Research suggests that the largest default problems lie not with students carrying large debt loads but among community college and for-profit attendees who attend without earning a degree (Looney & Yannelis, 2015). Given U.S. trends of declining support via general state expenditures and other resources, finding the most efficient use of funding is important to helping students cross the finish line. Due to heterogeneity in Promise program implementation, more information on their effectiveness can help policy makers think through how best to design these programs moving forward.

Literature Review

The effectiveness of financial aid necessarily depends on the program's structure and how aid is allocated. Grants based on both financial need or academic merit have frequently been shown to increase postsecondary attendance and completion (Angrist, Autor, Hudson, & Pallais, 2014; Bettinger et al., 2019; Castleman & Long, 2016; Denning, Marx, & Turner, forthcoming; Dynarski, 2000, 2003, 2004, 2008; Fitzpatrick & Jones, 2016; Scott-Clayton, 2011; Scott-Clayton & Zafar, forthcoming). Yet financial aid programs do not present uniformly positive results. In Massachusetts, subsidizing in-state public colleges induced enrollment shifts away from private institutions, lowering institutional quality and decreasing degree completion (Cohodes &

Goodman, 2014). Alternate forms of aid, such as federal tax benefits, do not appear to impact enrollment, likely as they predominately target middle- or higher-income individuals, and the promised benefits are not as immediate or transparent as typical aid programs (Bulman & Hoxby, 2015; LaLumia, 2012; Long, 2004). Even the Pell Grant program may suffer from inefficiencies, as aid is captured by low-performing for-profit colleges, or when states or institutions capture aid by shifting internal programs (Cellini & Goldin, 2014; Deming, Goldin, & Katz, 2013; Turner, 2017).

One innovation in financial aid are Promise programs: place-based initiatives that aim to increase college enrollment.[1] Researchers have begun to identify the impacts of Promise programs on postsecondary outcomes, with both the Kalamazoo and Pittsburgh Promise programs exhibiting positive impacts on four-year college attendance and degree completion (Andrews et al., 2010; Bartik et al., 2017; Bozick, Gonzalez, & Engberg, 2015; Page et al., 2017). The success of these Promise programs, in conjunction with policy discussions about the potential for "free" college, has led a few states to embark on similar programs but on a state level. The earliest such work is in Tennessee, where Knox Achieves, a local last-dollar scholarship program focused on community college enrollment, helped spur the 2015 creation of the state-wide Tennessee Promise. Although Knox Achieves increased community college enrollment and credits earned, it also decreased four-year college attendance among those with the strongest academic skills, ultimately leading to positive impacts on associate's degrees but decreases in corresponding bachelor's degrees (Carruthers & Fox, 2016; Carruthers et al., 2018).

[1] "Free" college typically implies that the targeted student would be pay no tuition and fees, though extra costs like room and board are still a student's responsibility, and the program does not guarantee that students will graduate without debt.

The nascent research suggests that the effectiveness of a “free” college model is likely to be dependent on which types of behaviors they produce. As the previous examples show, a first order issue is what types of institutions are covered. A number of programs have focused on making community college free, likely as these institutions serve more low-income and first-generation students on the margins of college attendance. Yet, data show that average net price – taking into account grant aid and federal education tax credits and deductions – has remained relatively flat at two-year colleges over the last twenty years, while increasing significantly in the four-year sector (Ma et al., 2017). Lowering costs for students who are on the margins of college attendance is likely a net positive, whereas subsidies that shift students towards weaker institutions may actually weaken the likelihood a student earns a degree (Carruthers et al., 2018; Cohodes & Goodman, 2014; Goodman et al., 2017).

An additional concern is how aid is allocated. Many programs operate as “last” dollar scholarships (rather than “first” dollar), meaning that new aid is provided to students only after existing aid, such as federal Pell Grant dollars and relevant state programs, are exhausted. One criticism of these plans is a discrepancy between the touted goals and who the program actually serves. A last-dollar scholarship might require a low-income student to first use their Pell Grant on tuition whereas a first-dollar scholarship would free up the Pell Grant to be used on other expenses. New York’s Excelsior Scholarship has been criticized for just this issue, with half of award eligible students receiving no additional financial support (Gullo, 2017). As most new aid programs are last-dollar, these programs may ultimately be regressive, leading most gains to be essentially middle-class subsidies.

Yet an important final consideration is how students become aware of and interpret aid programs, irrespective of the monetary subsidies involved. An often cited benefit of Promise programs is that the simplicity of messaging "free" may be a particularly effective form of advertising (Dynarski et al., 2018; Gurantz, Hurwitz, & Smith, 2017; Thaler, Sunstein, & Balz, 2012). This is important as aid programs are complex and the distribution of information through schools and social networks may leave needy students less aware of program details. As detailed more fully below, applying for the Oregon Promise required a small number of additional administrative steps, which may disproportionately impact disadvantaged students (Page & Scott-Clayton, 2015). As a result, even the theoretically positive messaging campaign might not be enough to overcome informational or administrative barriers to applying, leading to a delayed reaction – perhaps temporary but potentially permanent – among students most in need of support.

Background on the Oregon Promise

The Oregon Promise is a free community college tuition model created by State Bill 81, passed by the legislature in July 2015, and first made available to the high school graduating cohort of 2016. For high school graduates, students are eligible if they were an Oregon resident for at least 12 months prior, had a cumulative GPA of 2.5 or higher (through the Fall semester of senior year), enrolled in at least six units in an Oregon community college within six months, and listed at least one in-state community college on a completed FAFSA (or the equivalent state-created ORSAA for undocumented students). To be eligible students were required to apply through the Oregon Office of Student Access and Completion (OSAC) portal, a previously existing website used for various state-specific scholarship applications. The application required students to submit background information, including their GPA and high school attended, at which point students could select to have their GPA verified by two methods: (1) clicking a button that would send their information to their high school registrar, who would then respond to the state via an online portal, or; (2) the student could mail a hard copy or upload a PDF of their high school transcript (in the case where no registrar in a particular high school participated in the online verification program, the website would warn the student and prohibit the selection of the online verification choice).

From over 35,000 high school graduates in the first cohort, 10,500 met the requirements and applied to a Oregon community college, and 6,745 received a Promise scholarship upon enrolling (Higher Education Coordinating Commission, 2016). The Promise grant is a "last-dollar" scholarship that covers community college tuition and fees only after all other aid programs have been exhausted. Students whose tuition and fees are covered by alternate sources of aid are provided up to $1,000 per year. Average community college tuition and fees were approximately $3,400 in 2016-17, and the Oregon Promise does not cover tuition costs for students who wish to take additional units.[2] In addition to the Pell Grant, the most significant complementary program is the Oregon Opportunity Grant (OOG). The OOG provides a fixed amount of $2,250 for eligible full-time students, with awards allocated based on a rank-ordering of Expected Family Contributions (EFC) until funds are exhausted, for families earning less than $70,000.

In short, the Oregon Promise generally offers little financial benefit to the lowest income students. In 2016-17, a student receiving the OOG of $2,250 and a Pell Grant of $1,150 (which ranges up to $5,815) received aid approximately equal to the average Oregon community college tuition, thus receiving only the minimum $1,000 payment. Internal analysis of the program's first year found that 53% of all aid went to students who did not receive a Pell Grant (Higher Education Coordinating Commission, 2016). A recent report suggests that 20% of first-year Promise recipients came from families with median adjusted gross income of $111,600 or greater (Theen, 2017).

[2] The Oregon Promise covers "average" state-level full-time tuition, so the actual amount received by the student could be slightly lower than full-time tuition at their specific community college, though these differences are small.

Survey results from the first year suggest that the program increased students' perception of college as being affordable, though as might be expected a subset of students were confused by various application and eligibility requirements (Hodara, Petrokubi, Pierson, Vazquez, & Yoon, 2017). Most pertinent to this study, the survey asked students whether they intended to enroll in community college, and would their counterfactual have been a four-year college or no college at all. Roughly 45% of survey respondents said that they planned to shift from a four-year university to a community college as a result of Oregon Promise, compared to only 25% who said that without the Promise they would have gone to no college at all.[3] This gap held for both first-generation and non-first-generation respondents.

An important issue is to what extent Oregon engaged in other policies coincident with the initiation of the Oregon Promise, particularly those that might sharply impact postsecondary attendance in the 2016 cohort and beyond. Although I cannot rule out any such possibility, there seems to be little evidence of other proposals that would have produced such a change. Most observed measures of postsecondary engagement – enrollment, degree completion, tuition changes, higher education funding (including total funding for specific programs, such as the Oregon Opportunity Grant), and the like – continued on linear trajectories beginning in 2012, a few years post-recession.[4] One potentially large scale policy proposal is Oregon's 40-40-20 plan, which began in 2011 though ultimately passed and became effective in January 2018. Under this umbrella, legislative reports list a number of projects under discussion but the few that were funded, such as the Post-Graduate Scholar Program, served very small groups of students (HECC, 2017). Although

[3] These figures are based on summing the "agree" or "strongly agree" survey responses. 8% indicated they would have attended college out-of-state, though it is not clear whether this is separate from the 45%, given the way the question is framed.
[4] Conclusions in this paragraph primarily derive from statistics and research reports found at https://www.oregon.gov/highered/research/Pages/research.aspx.

these conversations may have shaped the trajectory of Oregon over time, there do not appear to be any specific processes that would have benefitted the 2016 cohort specifically over other groups. Overall, the Oregon Promise appears to create a sharp break in higher education policy towards community college enrollment that is relatively unaffected by other meaningful policy changes that occurred during this time period.

A need-based eligibility criteria was implemented in the 2017, for the second cohort in my analysis, due to diminished state funding. Oregon implemented a maximum Expected Family Contribution (EFC) cutoff of $20,000, though the exact EFC limit was determined later and should not have affected applications. My data do not contain EFC so I have no direct measure of how many students were rejected by this criteria, but a report on the first year of the program showed that 30.1% of total Oregon Promise funding went to students in the top EFC quartile, whose cutoff was $19,645 and higher (Higher Education Coordinating Commission, 2016). Thus roughly 20% of all students with the highest income levels would have been impacted by this change, potentially limiting the extent that students shifted out of four-year colleges.

Data

Data for this project come from the College Board, who administer the PSAT (Preliminary SAT / National Merit Scholarship Qualifying Test) and SAT exams. For each exam taker I can identify gender, ethnicity, high school attended, and high school attended (which is linked to median household income), but some background characteristics, such as income, are derived primarily from self-reports that are frequently missing. I link each student's high school to the Common Core of Data (CCD), which provides school-level characteristics such as school size, free and reduced price lunch participation, and urbanicity (i.e., city, suburb, town, rural). I also use

Integrated Postsecondary Education Data System (IPEDS) data to identify distance from the high school attended to the nearest two- and four-year college.

All PSAT takers are linked to postsecondary enrollment data from the National Student Clearinghouse (NSC), which tracks roughly 98% of all U.S. students (Dynarski, Hemelt, & Hyman, 2015). As Oregon Promise eligibility requires student enrollment within six months of high school completion, I define "college attendance" as having any postsecondary enrollment in the Fall semester of the year subsequent to graduation. (The College Board data identifies the number of days between graduation and first enrolling in a postsecondary institution, and I define enrollment as within 180 days. College Board's NSC data is restricted to the time period following high school graduation, thus ignoring postsecondary enrollment concurrent with high school attendance). I separately examine enrollment at two-year or four-year colleges by assigning each institution their IPEDS determined postsecondary sector. Two-year enrollment essentially corresponds to in-state community college attendance, as 92% of all two-year enrollment falls into this category; distinguishing between in and out of state two-year enrollment changes none of the analyses and is ignored. There is almost no observed for-profit enrollment in the sample, likely as NSC data covers only a small fraction of the for-profit postsecondary sector.

The difference-in-difference analysis relies on data from all 10th grade public school PSAT takers linked to NSC data. Beginning in Fall 2008 – which corresponds to the high school class of 2011 – the state of Oregon elected to offer the PSAT exam to all 10th grade students, by covering the associated costs and administering the exam during the school day. My primary analysis includes six comparison states: Florida, Georgia, Indiana, Maine, Nevada, and New Mexico. These states were selected as they had policies that offered the 10th grade PSAT to public school students during the school day, similar to Oregon. As three states – Indiana, Nevada, and New Mexico – adopted

PSAT participation plans that first impacted the graduating high school class of 2012, my analysis uses data from the high school graduating cohorts of 2012 through 2017. In supplementary analysis I include three more states – Maryland, Virginia, and Texas – that did not have state-level policies but do have high PSAT coverage rates, often due to similar initiatives at the district-level. I also run results for a larger sample of 48 states relying on all PSAT and SAT takers, as 10th grade PSAT coverage can be quite small in some contexts; I exclude Minnesota and Tennessee, who adopted state-level Promise programs during this time period.

Table 1 provides descriptive statistics, with the first column showing all 10th grade PSAT takers in Oregon, the second column showing the full sample of all 10th grade PSAT takers in the primary six control states, the third column including data on (i) three additional states that with strong PSAT coverage but no mandate and (ii) including students who did not take the 10th grade PSAT but took the 11th grade PSAT or SAT, and the fourth column including all 48 states (students in the third and fourth columns are used only in robustness tests). Comparing the first and second column illustrates some of the large differences between Oregon and non-Oregon students prior to the matching procedure that I employ. The Oregon sample is 65% white, a significantly higher percentage than the 47% in the full sample of control states, and only 3% African-American. Although PSAT scores are relatively similar between the two groups, Oregon students are ten percentage points less likely to take the SAT than their counterparts; this helps explain in part why Oregon's average SAT scores are significantly higher than control group students. Oregon students are also more likely to attend high schools that are smaller and more likely to be in localities classified as smaller towns rather than suburbs. Unfortunately, our best proxy for family income is self-reported data from the SAT, and even many SAT takers leave this value blank. Students in

Oregon are slightly more likely to attend two-year colleges and attend no college at all than control states.

Methodology

I first examine the impacts of the Oregon Promise in a difference-in-difference framework using the following equation:

$$Y_{ist} = \beta_0 + \beta_1 * Treatment_{ist} + \delta_s + \theta_t + X_{ist} + \varepsilon_{ist} \qquad (1)$$

Y_{ist} represents the primary outcome of interest, which is an outcome that indicates the sector of postsecondary enrollment for individual *i* in state *s* in year *t*. I examine enrollment in each sector of postsecondary enrollment (two-year, four-year, and no college) as separate regressions. I include both state (δ_s) and year (θ_t) fixed effects, and define $Treatment_{ist}$ as equal to one for any individual graduating from high school in Oregon in the 2015-16 or 2016-17 cohorts. By saturating the model with these two fixed effects, β_1 then estimates the shift in sector of postsecondary enrollment for Oregon students first exposed to the program, relative to shifts in students in control states, after accounting for level differences between these groups. X_{ist} is a vector of time-varying observable characteristics that account for minor compositional differences across cohorts, and come from three sources: (1) data on 11th grade PSAT, SAT, and AP participation and performance from the College Board, as well as student demographics, (2) CCD school-level characteristics (e.g., percent of students in a school participating in free and reduced price lunch program, urbanicity, school size, distance to closest colleges), and (3) median household income, linked through student zip code.

The difference-in-difference design suffers from two primary statistical challenges. The first is whether the control states serve as a proper counterfactual for Oregon in the absence of the Promise program. In the previous section I discuss theoretical reasons for considering the timing of program implementation exogenous to other large changes. Statistically, my first test is whether Oregon and control states exhibit similar pre-treatment trends, which I implement in equation (2) using an event study design that interacts the treatment status of living in Oregon with each year dummy in the pre-treatment period, omitting the year 2015 as the baseline year, as shown below.

$$Y_{ist} = \beta_0 + \sum_{t=2012}^{2017} (\beta_{1t} * Treatment_{ist} * \theta_t) + \delta_s + \theta_t + X_{jt} + \varepsilon_{jt} \quad (2)$$

This equation then explicitly tests for treatment effects in each year, which in a difference-in-difference design should be close to zero to support the idea of similar pre-treatment trends.

I further test my results by examining their sensitivity to sensible alterations of the sample composition, including additional control states and students (those who did not take the 10th grade PSAT but participated in College Board services, who may also be sensitive to the campaign messaging and financial subsidy). In the largest sample I use 47 control states, eliminating only Minnesota and Tennessee who implemented state-level Promise programs during this time period. I also show specifications that include high school fixed effects rather than relying on high school level covariates. I also follow recent papers that suggest matching improves the likelihood of using observational data to recover experimental treatment effects (Ferraro & Miranda, 2014, 2017). I present results from two matching protocols. In the first test I treat students living in Oregon as the treatment sample and predict the likelihood that each control student resides in that state using nearest neighbor matching, with three control observations selected with replacement and with a maximum caliper of 0.02.[5] Although this conditions on students with similar propensity scores and

[5] I use the psmatch2 command in Stata, estimating my prediction equation separately for each year and forcing exact matching on high school graduation cohort. Trimming, as suggested by Ferraro and Miranda (2014) and others, does little in this context as my prediction variables are relatively broad – many variables such as academic performance

minimizes average differences between Oregon students and controls states, the available demographic and academic background data (e.g., a white student in a low-poverty school with an average SAT) are not particularly relevant predictors of state of residence, and there is a literature suggesting that propensity scores may not be a viable way to recover treatment effects (King & Nielsen, forthcoming). I then use an alternate methodology based on a coarsened exact matching (CEM) procedure and construct a subsample that exact matches Oregon students to control state counterparts on the basis of gender, ethnicity (divided into four categories: white, Asian, underrepresented minority that includes both African-American and Hispanic students, and all other students), PSAT score binned into intervals of ten points, and quintiles of high school free and reduced price lunch participation (Iacus, King, & Porro, 2012). From these matches I then randomly select three control students for each treatment student. Finally, I estimate impacts using a synthetic control design and results broadly mirror findings from the difference-in-difference methods (Abadie, Diamond, & Hainmueller, 2010); the setup of this analysis and description of these results is placed in Appendix 1.

A second statistical challenges relates to statistical inference. Unadjusted standard errors have long been known to be insufficiently conservative, yet there are challenges in estimating clustered errors in settings with few clusters or treatment groups (Angrist & Pischke, 2008). In my main results I cluster standard errors at the state-by-year level, which are typically three to five times as large as unadjusted results, but present alternate standard errors via a number of alternate methods: clustering at the state level (seven clusters) or high school level (2249 clusters); bootstrapped errors using Stata's default bootstrapping method, and alternate methods that account for few (or one)

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or gender are not meaningful predictors of Oregon residency – and I have an extremely large number of control students to select from.

treated clusters and heteroscedasticity, including the wild cluster and pairs cluster bootstrap-t (Cameron, Gelbach, & Miller, 2008) and a new method by Ferman and Pinto (forthcoming) that uses permutation inference methods. When using the full 48 state sample I also use permutation methods that estimate pseudo treatment effects for each control state, using these range of estimates to calculate exact p-values. Unfortunately, using the full state sample does not suggest parallel pre-treatment trends, so these results should be considered secondary to other results. Although there is some variation across these methods, all distinguish positive impacts on two-year attendance as statistically meaningful, though in some versions the precision on four-year versus no college attendance becomes substantially worse.

Results

Main treatment effects

Table 2 shows that the implementation of the Oregon Promise led to a 5.3 percentage point increase in community college enrollment among 10th grade PSAT takers, relative to students living in the selected control states. Over the first two cohorts Oregon Promise had consistently positive impacts on community college enrollment, but the induced behaviors were substantially different between years. These results are provided in the bottom panel of Table 2, which uses equation (2) that allows for year-by-year treatment effects. In the first year the 4.2 percentage point increase in community college enrollment came primarily from a 2.9 percentage point enrollment decline in four-year colleges, with a statistically insignificant 1.2 percentage point shift from no college to postsecondary. In contrast, students exposed to the second year of the program were 4.3 percentage points more likely to shift from no college towards postsecondary enrollment, with a statistically insignificant -0.6 percentage point effect on four-year enrollment. For each estimate I provide corresponding p-values from the main regression but add Ferman-Pinto standard errors specifically designed for clustered difference-in-difference designs with a single treated unit (Ferman & Pinto, forthcoming). Both results confirm positive and statistically significant impacts on community college attendance, though the 2016 impact is significant at $p<0.10$. Ferman-Pinto standard errors are significantly noisier and do not clearly identify whether counterfactual outcomes on four-year college or no college attendance are different from zero, suggesting that there should be some caution in interpreting results as exactly linked to one specific sector.

To support the causal interpretation of these difference-in-difference estimates, the bottom panel of Table 2 includes placebo "treatment effects" in each of the pre-period years. These pre-period estimates are generally null and support the parallel trends assumption between treatment and control groups, with results presented graphically for two-year, four-year, and no college attendance in Figures 1 through 3, respectively. There are only three pre-treatment variables that differ from zero, with all small in magnitude and two due to small changes in the out-of-state four-year enrollment. Focusing just on in-state four-year enrollment – which is the large majority of enrollment in this sector – pre-treatment estimates range from 0.1 to 1.1 percentage points before declining a statistically significant 2.8 percentage points in 2016 (results presented graphically in Appendix Figure 1). Table 2 results show no impact on inducing out-of-state, four-year college students to enroll in in-state, two-year colleges; for this reason, I present overall impacts on four-year attendance throughout the rest of the paper, but presenting these results separately does not measurably change any of the findings.

Robustness of main treatment effects

Table 3 tests the robustness of my main results and finds a range of estimates that suggest community college enrollment increased between 4.0 and 5.3 percentage points. Thus the positive estimates are not sensitive to appropriate changes to the sample composition, though the initial 5.3 percentage point estimate falls on the upper end of the distribution. Table 3 reproduces the original results on two-year, four-year, and no college attendance in the first column, then shows results for alternate samples, including: three additional control states with strong 10th grade PSAT coverage (column 2), students who did not take the 10th grade PSAT but did take the 11th grade PSAT or SAT exam (column 3), and the larger of sample of 48 states based on 10th grade PSAT (column 4) and all students (column 5).[6] Appendix Table 1 shows results for the same samples but after removing school-level covariates and using high school fixed effects, with similar results. I then move in the other direction and shrink the size of my sample using matched comparison groups, both exact matching (column 6) and nearest neighbor matching (column 7); these choices are described in the Data section above. In Appendix 1 I show results based on a related but alternate synthetic control design. Separately, I also find no evidence of changing cohort composition across years that might explain any of the observed treatment effects.[7]

[6] I omit two states, Minnesota and Tennessee, as they implemented their own versions of state-level Promise programs at this time. A few states also added some programs in 2017, with some being smaller, pilot programs or directed towards specific groups (e.g., certificate students) that may do much to influence my estimates; when these are removed impacts on two-year attendance are essentially identical. I continue to cluster at the state-by-year level, but with the larger number of clusters I can also cluster by state, and results are similar in nature.

[7] I examine cohort composition two ways. Appendix Table 2 provides a robustness test that shows regresses students' observable characteristics on the implementation of the Oregon Promise, using my standard difference-in-difference design, with the expectation that these estimates will be null or small in magnitude. Only two small differences emerge, with neither significant at the 0.01, and the inclusion or exclusion of these values does not change the treatment estimates. I also examine whether the size of my sample might change across years due to endogenous selection into the College Board data – whether intentional or not – and find no evidence of this occurring. Appendix Figure 2 shows state-specific coverage rates when comparing observed cohort sizes in the College Board data to 10th grade enrollment in the Common Core of Data; I only include students in schools that reported positive 10th grade enrollment numbers. I also regress the high school specific ratios of observed students to 10th grade CCD enrollment estimates in my difference-in-difference design (equation (1)) and get a statistically insignificant treatment impact of -0.022 with a t-statistic of -1.1, with similar results when I separate out treatment impacts by year.

Results on sector of college attendance are generally similar across comparison groups, though there is some variation that complicates whether counterfactual outcomes derive primarily from four-year or no college attendance. Impacts on shifting from four-year colleges ranges typically range from -0.6 to -2.7 percentage points, though the largest outlier (-0.6) derives from the full 48 state sample that includes all PSAT and SAT takers, which performs poorly. In that regression five of the six pretreatment values on four-year and no college attendance statistically differ from zero, which is poor evidence of parallel pre-treatment trends desired for causal interpretation. Impacts on attending no college are more consistent, ranging from -2.3 to -3.0 percentage points and mostly statistically significant, except for the full 48 state sample. The strongest evidence of parallel trends comes from the main specification in column 1, followed by the exact matching approach (column 6). Alternative seven or ten state samples show some divergence from parallel pre-trends but perform better than the 48 state sample.

Appendix Table 3 fleshes out potential issues in the statistical inference of my treatment estimates. As described above I implement a number of methods and find that positive impacts on two-year enrollment consistently meet conventional thresholds of statistical significance. Standard errors in an unadjusted model are 0.002 but increase to 0.005 in my main state-year clustering specification, and range from 0.002 to 0.007 when clustering at the state or high school level, bootstrapping without regard to clustering, and implementing the wild cluster or pairs bootstrap methods. The Ferman-Pinto methodology increases the associated standard error to 0.02, thus producing larger uncertainty but still far from the 0.05 p-value threshold.[8] Standard errors on the four-year

[8] Ferman-Pinto is similar to prior methods, such as Conley-Taber, but relax some of the error structure assumptions in earlier work. Some methods produce standard errors whereas other produce p-values (e.g., Ferman and Pinto (forthcoming)). In the case of p-values I construct "pseudo" standard errors such that naively dividing the estimate by the standard error would produce a t-statistic equivalent to the reported p-value, in order that each method can be interpreted on the same scale. More details are in the notes of Appendix Table 3.

enrollment or no college estimates are mostly similar or smaller in magnitude when clustering at the state or high school level or using conventional or the wild cluster bootstrap. Both the pairs bootstrap and Ferman-Pinto method dramatically increase the standard error bounds on four-year and no college attendance, such that results are no longer marginally significant as observed in the main estimates; thus inference based on these values would not allow us to distinguish whether the shifts into community college derive from those who would have attended no college or those coming from the four-year sector. As one last check, I use the full sample of 48 states and show the distribution of outcomes across Oregon and all 47 controls as a method to determine the likelihood that these impacts occurred due to random change. Appendix Figure 3 shows impacts for two-year college attendance for 2016 and 2017 and Appendix Figure 4 shows results for four-year and no college attendance in those same years. Based on this method the two-year outcomes are likely not due to random chance, with Oregon having the largest observed positive impacts among 48 distinct effects, or lying outside of what might be considered the 95% confidence interval. Neither of the four-year impacts appear to be meaningfully distinct from noise, whereas the negative impact on no college attendance in 2017 is the second largest negative effect, which could be interpreted as significant at the 90% level.

Heterogeneous treatment effects

I primarily estimate heterogeneous impacts based on student propensity to attend a four-year college. To do so I run a logistic regression that estimates four-year attendance based on individual- and school-level characteristics. I then divide students into bins based on propensity for four-year attendance and separately estimate treatment impacts for each group.[9] For ease of interpretation I

[9] The distribution of predicted probabilities is shown in Appendix Figure 5. Estimating the propensity to attend a four-year college or no college at all is more straightforward than predicting two-year attendance, as both very low and very high performing students are equally unlikely to attend two-year colleges. In contrast, both four-year and no

use bins that capture the following predicted probabilities, though alternate groupings produce similar results: 0-10%, 10-25%, 20-50%, and 50-100%. Most students have a low propensity to attend a four-year college – particularly if they have not taken the SAT – and the 10%, 25%, and 50% cut points correspond to roughly the 40th, 60th, and 75th percentile of propensity to attend a four-year college).

Figure 4 plots the treatment estimates by bins and shows the delayed effect on overall enrollment is strongly correlated with student's underlying propensity to attend a four-year college; corresponding regressions are provided in Table 4 with baseline attendance values provided under the regression estimates. (Separate treatment effects for 2016 and 2017 are plotted in Appendix Figures 6 and 7, respectively). For students in the lowest propensity bin – where 83% attend no college at all – students are predominately moving from no college to community college. For students with the highest propensity to attend a four-year college almost all the shifting is from the four-year sector, though Table 4 suggests that there are small impacts on going to college in the second cohort. Students in the middle bins – those with the highest baseline attendance rates at two-year college, at roughly 27% – exhibit the larger shifts towards two-year colleges, with impacts by year two mostly indicating that the program has increased overall enrollment.[10]

Appendix Table 4 provides additional measures of heterogeneous impacts, though given the large number of items tested I consider these results speculative. Positive impacts on community college

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college attendance are relatively linear in common measures of postsecondary success, such as PSAT scores and high school poverty. The predictive logistic regression includes indicators for student gender, ethnicity, school urbanicity (city, suburb, town, rural), and participation in Advanced Placement, along with cubics of 10th grade PSAT, 11th grade PSAT, and SAT scores, with dummies for students missing a value, and cubics for the size of the 12th grade cohort and free/reduced price lunch status. The predictive model was run on the 2012 cohort, including all students and not restricting to those who took the 10th grade PSAT, with results applied to all later years. Estimates based on models using all pretreatment years (2012 through 2015) as a baseline are identical.

[10] Ferman-Pinto standard errors for Table 4 conclude positive impacts on community college attendance but again mostly fail to find significant effects within propensity groups for four-year and no college outcomes.

enrollment are consistent across groups. The largest gains in overall enrollment were found among African-American and Hispanic students whereas the largest shifts out of four-year colleges was among Asian students. Students with lower PSAT scores and in higher poverty high schools were generally more likely to respond to the promise by increasing their postsecondary enrollment, whereas, as might be expected, students who had taken the SAT were most likely to respond by shifting out of the four-year sector.

Treatment effects on college quality

I investigate how Oregon Promise changes the characteristics of the college attended by examining: average peer PSAT and college graduation rates, two common measures of college selectivity; net price and instructional expenditures per students, which reflect student and state expenditures; and median earnings, which has implications for potential wage gains directly to the student but also to the state through tax revenues.[11] I choose to present results using all students, rather than just results based only on college attendees, though add comments on how these values might differ where appropriate.[12] When using all students I: impute a zero for net price and instructional expenditures outcomes, under the assumption they are not paying for any postsecondary training; impute average PSAT and graduation rates for students attending no college or two-year colleges (described in footnote 11), and; use zero as distance travelled to

[11] Net price and expenditures per FTE derive from IPEDS data. Median earnings are "median earnings of students working and not enrolled 10 years after entry" derived from the College Scorecard (mrc_table10). My measures of college selectivity derive from College Board data that include all students, including those who attended a two-year college or no college at all, which IPEDS cannot account for. Specifically, non-college attendees are assigned an average peer quality based on average PSAT of all students who do not attend college, and given a predicted graduation rate (again based on real College Board data) based on how often these students ultimately earn a degree within six years. Community college attendees are given a peer quality based on PSAT scores of students at that institution, which is an improvement over using IPEDS SAT/ACT data that are not given to two-year colleges.

[12] The primary concern is that changes to overall enrollment, particularly in 2017, make outcomes using only college attendees more challenging to interpret. For example, college selectivity might decline not because students are shifting into less selective colleges but because students who would have attended no college now are choosing to pursue a degree.

college. This approach has two benefits: (1) it more accurately represents state-level student and government expenditures as a result of the program, rather than the cost faced by individual students; (2) it provides a better indicator of potential impacts on future, aggregate degree completion, though is by no means conclusive.

Table 5 shows that students in the first year attended institutions with lower average graduation rates (-0.7 percentage points, or a 3% decline) and with peers of slightly lower academic preparation (-0.008 standard deviations), as these students were more likely to shift away from four-year colleges. For the second cohort, graduation rates and peer quality increase significantly, as students were less likely to be in the cohort of non-college attendees, who have very low rates of degree completion. It bears noting that the gain in predicted graduation rates is not that high relative to the gain in postsecondary enrollment, as predicted completion rates among community college attendees are relatively low.[13]

For the first cohort, estimates on net price are negative but noisy, though results suggest that instructional expenditures per student declined (percent changes are 5% and 3%, respectively). Thus at the state-level students are spending less on college and the state is spending less on instruction per student, given the four-year to two-year shifts. In the second cohort the results become positive as students and the state both pay more. Although the estimates are roughly similar in magnitude, meaning the increase in tuition payments may cover the state expenditures, estimating real transfers would require improved data, such as to what extent new students bring in Pell Grant dollars to cover costs. Overall distance travelled does not change dramatically in either cohort, as students might be about two percentages points less likely to travel more than 20

[13] Using estimates of selectivity only for college attendees consistently produce negative estimates for both cohorts, as those induced to attend community colleges bring down the state average. This approach however would not allow one to estimate impacts on overall, state-level completion rates, which I do below.

miles from home. (Descriptive statistics show that the median distance from a high school to a two- and four-year college in the sample was 6.9 and 7.2 miles, respectively, so shifts across these institutions are unlikely to change distance traveled by a meaningful amount).

If we wanted to use these results to predicted downstream impacts on bachelor's degree completion, the impacts may ultimately be relatively small. A direct translation of previous work suggests that students starting in four-year colleges are 40 percentage points more likely to earn a degree (Goodman et al., 2017), indicating a five percentage point shift in enrollment would lead to a two percentage point decline in bachelor's degree completion. Yet prior work that examines heterogeneity across student academic ability finds little evidence that initial community college enrollment leads to lower degree completion for those with higher skills (Long & Kurlaender, 2009), which may better represent the students in this sample, particularly those in the first cohort who likely shifted out of four-year colleges. Even these potential impacts do not account for the fact that students in this context are also likely to be receiving thousands of dollars of additional aid, so previous numbers likely represent the upper bound of negative impacts. Although aid has previously been shown to increase community college enrollment among those unlikely to attend college (e.g., Denning (2017b)), there is less consistent evidence of impacts on degree completion (Anderson & Goldrick-Rab, 2018; Gurantz, 2018). Overall, fairly substantial changes in attendance might not lead to large changes in observed degree completion as many students ultimately do not complete their degree, and reduced form impacts on students induced to shift enrollment by financial aid end up being relatively small.

The time frame of my data are too limited to investigate persistence across years, but I am able to engage in one supplementary analysis that examines the total terms enrolled during the first year after high school. Appendix Table 5 shows no impacts on total first-year terms for the 2016 cohort, whereas in the 2017 cohort there is an increase of 0.09 total terms, or a 4.1 percentage point increase in students enrolled in two or more terms.[14] These estimates lend some support to the idea that students induced to switch – either from a four-year college or from no college at all – are not dropping out half-way during the school year. This is slightly complicated by the fact that the aid could have independent effects on persistence among those students who were inclined to attend regardless of aid; if there was a positive effect on persistence amongst "always-attenders" and a counterbalancing negative effect on persistence among students induced to switch out of four-year colleges, it would still be possible to have a zero net effect that masks these impacts.

Discussion

This paper examines the Oregon Promise, a state-based program that induced large gains in community college enrollment by subsidizing tuition for in-state students. The program's effects appear to shift over time, moving students mostly from four-year to two-year colleges in the first year before leading to meaningful enrollment gains in the second year, though the exact counterfactual outcome may be too imprecise to definitely state how students responded.

Results suggest that the clear signal of college affordability may be an important factor in how Promise programs drive enrollment changes, as much as shifting the relative costs of different postsecondary sectors. One reason is that the Oregon Promise delivers little actual aid to low-income students, being a last-dollar scholarship layered on top of existing federal and state need-based grant programs. The mean award payment was only $653 in the first semester; a low-income student with full tuition coverage would have received significantly less (Higher Education

[14] Total terms enrolled is a simple count of NSC data rows in the year following high school graduation and generally ranges from zero to three. Very few students have larger values, likely due to measurement error or idiosyncratic enrollment patterns, such as dual enrollment (which is infrequent in the data). These regressions omit an additional year (2014) as in this year the total terms enrolled was overwritten and includes multiple years of data, and so cannot be used to identify terms separately from just the first-year enrollment.

Coordinating Commission, 2016). Applicants are now subject to a maximum EFC limit, preventing the highest-income students from receiving aid, which may partly explain why the program has smaller impacts on four-year attendance in year two. Although the College Board data does not contain students EFC or precise income data, eliminating the wealthiest 20% from eligibility does not appear capable of explaining all the shifts in sectoral enrollment between the first two cohorts. Students may also be unaware of the exact award amount, as Oregon surveys (Hodara et al., 2017), as well as previous research (Dynarski et al., 2018), posit that many students have insufficient knowledge of how financial aid programs operate. Students might overestimate the value of the award if they are not familiar with their likely award amount or the notion of "last-dollar" scholarships, and be more responsive than they would be given perfect information on the true value. Thus Oregon's promotion of community college enrollment as valued may explain some of the enrollment shifts for students sensitive to state-designated preferences or promotional campaigns.

A final question is: how should future Promise programs be structured? Promise programs come in many different formats, which may lead to important differences in how they function (Perna et al., 2017). Both the Kalamazoo and Pittsburgh Promise do not restrict the sector of institutional attendance and provide substantial grant aid, leading to large positive impacts on four-year college attendance. This holds true even thought their eligibility criteria are quite different – with no merit or income requirements in Kalamazoo and GPA and attendance requirements in Pittsburgh. Among community college focused programs, Knox Achieves has no eligibility criteria and a similar last-dollar approach to aid that led to increases in associate's degree completion at the cost of some bachelor's degrees (Carruthers et al., 2018). Across other programs there appears to be more consistent counseling services provided, which could impact how well the programs ultimately impact degree completion. One last point is that both Kalamazoo and Pittsburgh are philanthropically funded, perhaps as state programs are less willing to shoulder the burden of fully supporting four-year college enrollment. Pittsburgh in particular has been responsive to perceived programmatic shortfalls, making changes to the program format over time by, for example, altering the types of supplementary services or the amount and format by which funds are offered (Page & Iriti, 2018). Time will tell whether state-led programs will have the staying power of philanthropic programs, which may be more nimble in gathering data, engaging in decision-making, and maintaining political support.

This paper hopes to provide additional lessons for states that are considering similar Promise-type programs. A key consideration is that adding a new aid program over a multitude of disparate, existing programs could – but does not have to – lead to regressive aid policies that have the potential to induce students to choose colleges with less support and lower graduation rates. Although reducing the cost of college has been shown to help low-income students succeed (Bettinger et al., 2019; Denning et al., forthcoming), more work is needed to understand the best way to implement aid policies at scale. Work in the United Kingdom suggests that access in a free college model can result in reduced student resources, observed via lower expenditures per student or mandated enrollment caps, thus exacerbating inequality between advantaged and disadvantaged youth (Murphy, Scott-Clayton, & Wyness, 2017). This work has been supported in the U.S. context, where states have exhibited larger gains in degree completion when investing in additional resources rather than reducing tuition (Deming & Walters, 2017). Varied theories presented above highlight that quantitative research on the impacts of Promise programs should be coupled with additional insights on the politics and administration of these programs, to best understand how to most effectively promote or structure them moving forward.

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Table 1. Descriptive Statistics

	(1)	(2)	(3)	(4)
State	Oregon	Six control states (FL, GA, IN, ME, NM, NV)	Six control states plus MD, TX, and VA	48 states (excluding MN and TN)
College Board sample	10th grade PSAT	10th grade PSAT	Any PSAT or SAT	Any PSAT or SAT
N	149131	2069772	5012673	12368616
Individual				
Female	50%	51%	51%	51.7%
Asian	6%	4%	5%	7.5%
African-American	3%	20%	19%	16.1%
Hispanic	17%	21%	27%	24.5%
White	65%	47%	42%	44.3%
Other ethnicity	10%	8%	7%	7.6%
No reported income	72%	69%	67%	67.3%
Low-income	7%	10%	11%	10.0%
Middle-income	12%	13%	13%	12.3%
High-income	9%	7%	9%	10.4%
School-level (CCD)				
12th grade enrollment	287	398	412	391
% FRPL	50%	50%	48%	46.3%
City	30%	25%	31%	35.3%
Suburb	26%	46%	43%	43.9%
Town	31%	10%	8%	6.5%
Rural	14%	19%	18%	14.3%
Exam performance				
10th grade PSAT	124	120	121	125
Took 11th grade PSAT	34.4%	32.6%	50.4%	52.5%
11th grade PSAT	303	328	292	318
Took SAT	42.7%	52.9%	58.8%	41.9%
Initial SAT score	1040	975	963	989
Took AP	24.3%	37.4%	37.0%	27.0%
National Student Clearinghouse (NSC) data				
Attended two-year	20%	17%	19%	17.8%
Attended four-year	27%	33%	33%	41.3%
No college	53%	50%	48%	40.9%

Notes. Individual characteristics derived from self-reports on College Board application forms. School-level characteristics derived from the Common Core of Data (CCD). Exam performance derived from College Board academic assessments (PSAT, SAT, AP exams). National Student Clearinghouse data results from a match of all College Board participating students to NSC data, and postsecond attendance is determined by enrollment within 180 days of high school graduation.

Table 2. Impacts of Oregon Promise on postsecondary attendance

	Two-year	Four-year college			
	college	In-state	Out-state	Any	No College
Main estimates	0.053**	-0.016	-0.011	-0.027*	-0.025
	(0.005)	(0.012)	(0.006)	(0.011)	(0.013)
State-year cluster p-values	[0.000]	[0.180]	[0.067]	[0.019]	[0.062]
Ferman-Pinto p-values	[0.017]	[0.614]	[0.395]	[0.468]	[0.448]
Event study estimates					
2012	-0.011	0.011	0.010**	0.021	-0.010
	(0.008)	(0.010)	(0.004)	(0.011)	(0.015)
2013	-0.015	0.001	0.003	0.003	0.012
	(0.010)	(0.007)	(0.003)	(0.006)	(0.012)
2014	-0.002	0.005	0.008*	0.014**	-0.012
	(0.008)	(0.004)	(0.004)	(0.004)	(0.009)
2015			(omitted year)		
2016 (1st treatment year)	0.042**	-0.028**	-0.002	-0.029**	-0.012
	(0.006)	(0.010)	(0.004)	(0.007)	(0.009)
2017 (2nd treatment year)	0.049**	0.003	-0.009	-0.006	-0.043**
	(0.007)	(0.006)	(0.009)	(0.011)	(0.011)
State-year cluster p-values					
2016 (1st treatment year)	[0.000]	[0.007]	[0.670]	[0.000]	[0.170]
2017 (2nd treatment year)	[0.000]	[0.604]	[0.323]	[0.555]	[0.001]
Ferman-Pinto p-values					
2016 (1st treatment year)	[0.083]	[0.361]	[0.885]	[0.439]	[0.764]
2017 (2nd treatment year)	[0.030]	[0.907]	[0.469]	[0.881]	[0.114]

Notes. * p<0.05, ** p<0.01. Main estimates include state and year fixed effects and cluster standard errors at the state-by-year level, as specified by equation (1). P-values based on clustering and Ferman-Pinto methodologies shown in brackets below standard errors. Event study estimates interact year dummies with treatment status as specified by equation (2). All columns use 2,212,760 observations. Covariates include dummies for 10th grade and 11th grade PSAT scores (binned by 10 points), SAT scores (binned by 100 points), gender, ethnicity, self-reported income, AP participation, zip-code level median household income, and high school school characteristics (urbanicity, 12th grade size, quintiles of free- and reduced-price lunch status, distance to closest two- and four-year college).

Table 3. Impacts of Oregon Promise on postsecondary attendance, robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Two-year attendance	0.053**	0.042**	0.044**	0.040**	0.042**	0.049**	0.044**
	(0.005)	(0.006)	(0.006)	(0.004)	(0.003)	(0.005)	(0.005)
Four-year attendance	-0.027*	-0.017*	-0.014	-0.016*	-0.006	-0.026*	-0.019**
	(0.011)	(0.008)	(0.009)	(0.008)	(0.007)	(0.010)	(0.005)
No college attendance	-0.025	-0.024*	-0.030**	-0.024**	-0.036**	-0.023*	-0.025**
	(0.013)	(0.010)	(0.011)	(0.009)	(0.008)	(0.011)	(0.006)
N	2212760	4115917	5158939	8038212	12863340	595461	586539
Sample Composition							
Additional PSAT states (MD, TX, VA)		✓	✓	✓	✓		
Additional students (11th grade PSAT, SAT)			✓		✓		
All states (except MN & TN)				✓	✓		
Exact matched controls						✓	
Propensity matched controls							✓
Covariates	✓	✓	✓	✓	✓	✓	✓

Notes. * p<0.05, ** p<0.01. Regressions include state and year fixed effects and cluster standard errors at the state-by-year level, as specified by equation (1). Covariates include dummies for 10th grade and 11th grade PSAT scores (binned by 10 points), SAT scores (binned by 100 points), gender, ethnicity, self-reported income, AP participation, zip-code level median household income, and high school school characteristics (urbanicity, 12th grade size, quintiles of free- and reduced-price lunch status, distance to closest two- and four-year college).

Table 4. Impacts of Oregon Promise on postsecondary attendance, by propensity to attend four-year college

	Propensity to attend four-year college:			
	0-10%	10-25%	20-50%	50-100%
Two-year college				
Main estimates	0.041**	0.072**	0.091**	0.044**
	(0.007)	(0.011)	(0.012)	(0.005)
Event Study estimates				
2016 (1st treatment year)	0.033**	0.049**	0.073**	0.037**
	(0.005)	(0.008)	(0.012)	(0.005)
2017 (2nd treatment year)	0.049**	0.060**	0.090**	0.038**
	(0.006)	(0.011)	(0.014)	(0.008)
Four-year college				
Main estimates	-0.009	-0.033	-0.051**	-0.056**
	(0.005)	(0.016)	(0.014)	(0.016)
Event Study estimates				
2016 (1st treatment year)	-0.012**	-0.033**	-0.045**	-0.045**
	(0.004)	(0.011)	(0.013)	(0.009)
2017 (2nd treatment year)	-0.009	0.001	-0.034*	-0.010
	(0.007)	(0.014)	(0.014)	(0.011)
No college				
Main estimates	-0.032**	-0.039	-0.039*	0.012
	(0.009)	(0.021)	(0.016)	(0.016)
Event Study estimates				
2016 (1st treatment year)	-0.021**	-0.016	-0.027*	0.008
	(0.007)	(0.012)	(0.013)	(0.010)
2017 (2nd treatment year)	-0.040**	-0.061**	-0.057**	-0.029**
	(0.009)	(0.019)	(0.016)	(0.010)
Baseline statistics, Oregon students in 2015				
Attended four-year	2.5%	17.1%	41.9%	68.8%
Attended two-year	15.5%	25.9%	27.3%	13.4%
No college	82.0%	57.0%	30.8%	17.8%
Female	41.2%	56.0%	56.8%	58.0%
African-American or Hispanic	27.2%	17.3%	15.6%	7.0%
10th grade PSAT	102.9	127.4	128.6	153.6
Took SAT	1.7%	26.9%	85.8%	99.7%
Initial SAT score	653.1	791.2	924.6	1133.3

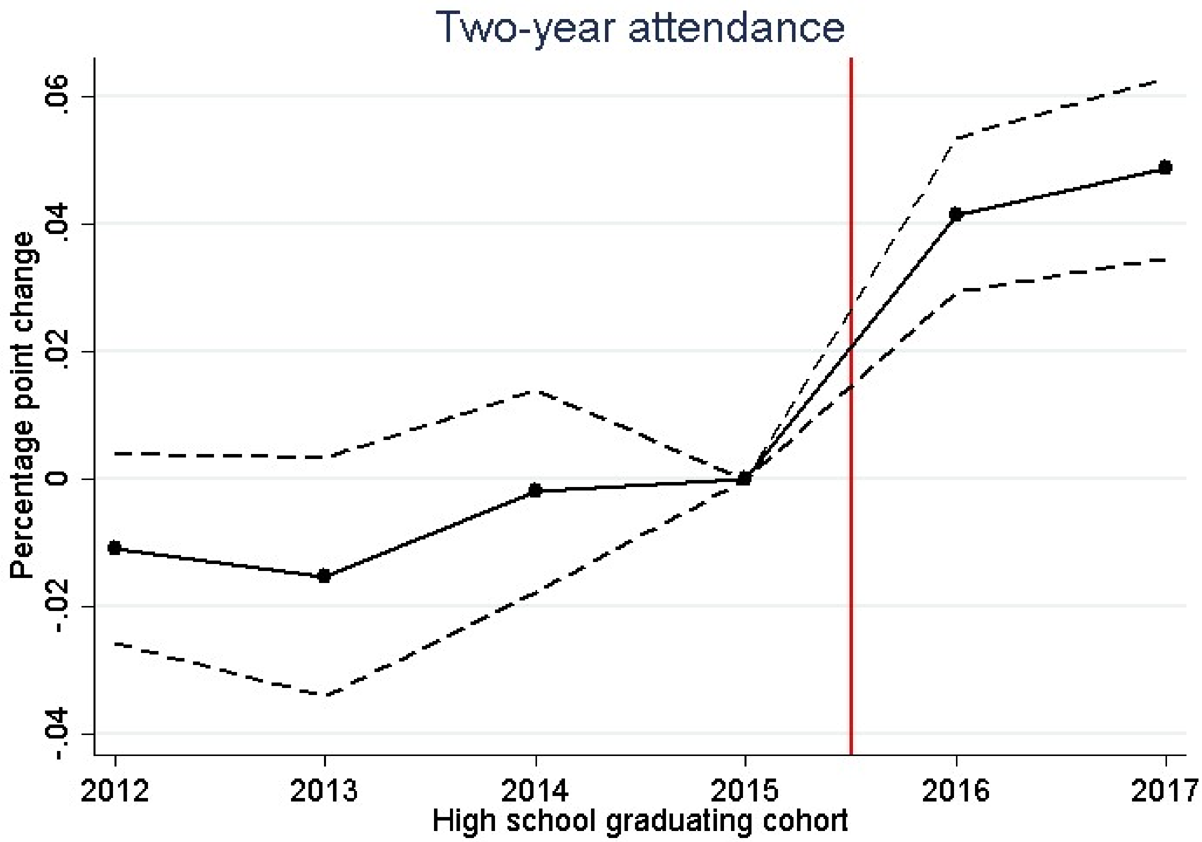
Notes. * p<0.05, ** p<0.01. Main estimates include state and year fixed effects and cluster standard errors at the state-by-year level, as specified by equation (1). Event study estimates interact year dummies with treatment status as specified by equation (2). Bins contained 801,902, 396,648, 351,055, and 663, 165 observations from the lowest to highest propensity bins, respectively. Covariates include dummies for 10th grade and 11th grade PSAT scores (binned by 10 points), SAT scores (binned by 100 points), gender, ethnicity, self-reported income, AP participation, zip-code level median household income, and high school school characteristics (urbanicity, 12th grade size, quintiles of free- and reduced-price lunch status, distance to closest two- and four-year college).

Table 5. Impacts of Oregon Promise on characteristics of postsecondary institutions

	Average peer PSAT	Average peer graduation rate	Median Earnings	Net price	Instructional expenditures per student	Distance travelled to college		
						0-20 miles	20-100 miles	100+ miles
2016 (1st treatment year)	-0.008	-0.007**	-328.240**	-275.181	-103.459*	0.007	-0.019*	-0.006
	(0.005)	(0.003)	(49.781)	(182.865)	(40.917)	(0.009)	(0.008)	(0.003)
2017 (2nd treatment year)	0.027**	0.009*	-29.245	138.892	134.755*	0.019	-0.013	0.007
	(0.006)	(0.004)	(89.544)	(193.390)	(58.084)	(0.011)	(0.008)	(0.003)
Control mean	-0.133	26.1%	$38,424	$5,560	$3,568	21.8%	13.6%	15.6%
N	2176631	2176631	1113190	2212760	2212760	2212760	2212760	2212760

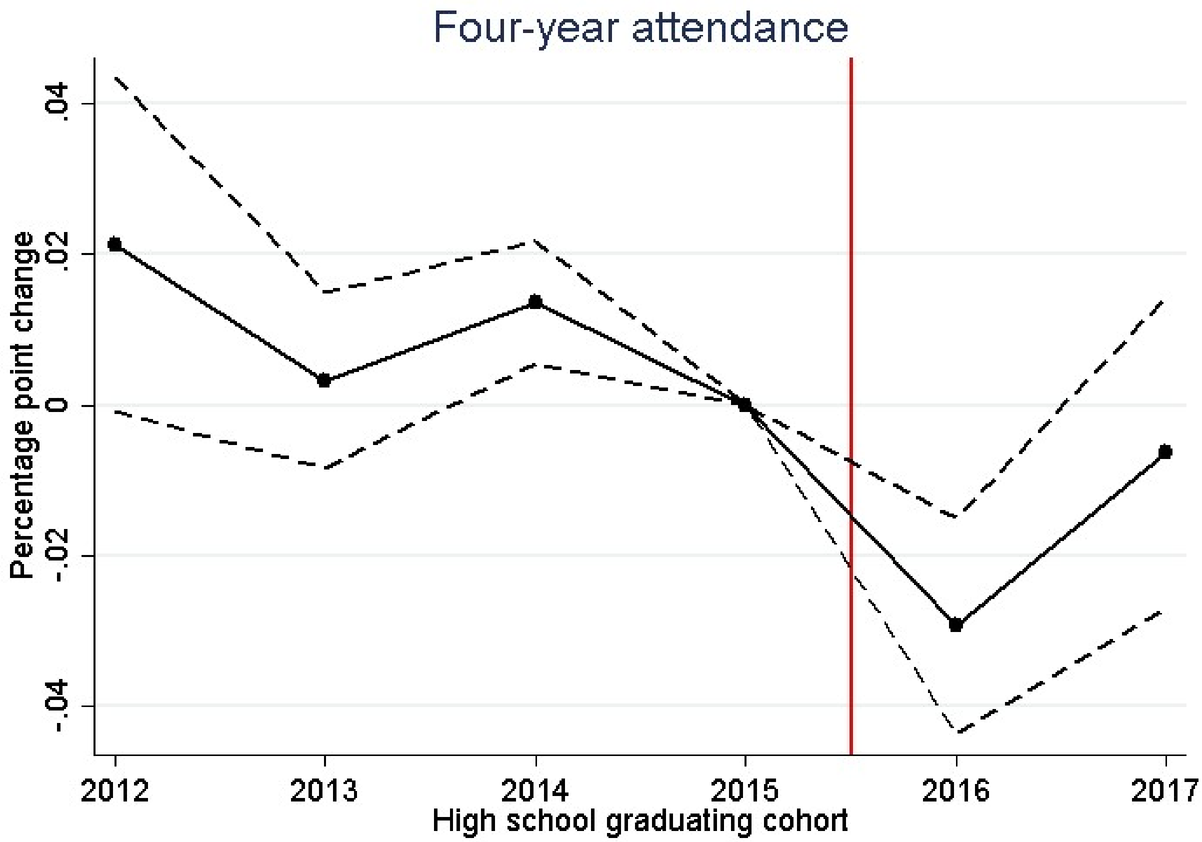
Notes. * p<0.05, ** p<0.01. Event study estimates interact year dummies with treatment status as specified by equation (2). Distance from high school to college, net price, and institutional expenditures per FTE derive from publicly available IPEDS data, median earnings from publicly available College Scorecard data, and average peer PSAT and graduation rate from College Board data. Measures include all students by: assigning non-college attendees a zero for IPEDS data and assigning an average peer quality and graduation rate based on College Board averages for students attending two-year colleges or no college. Covariates include dummies for 10th grade and 11th grade PSAT scores (binned by 10 points), SAT scores (binned by 100 points), gender, ethnicity, self-reported income, AP participation, zip-code level median household income, and high school school characteristics (urbanicity, 12th grade size, quintiles of free- and reduced-price lunch status, distance to closest two- and four-year college).

Figure 1. Difference-in-difference event study impacts of Oregon Promise on two-year college enrollment



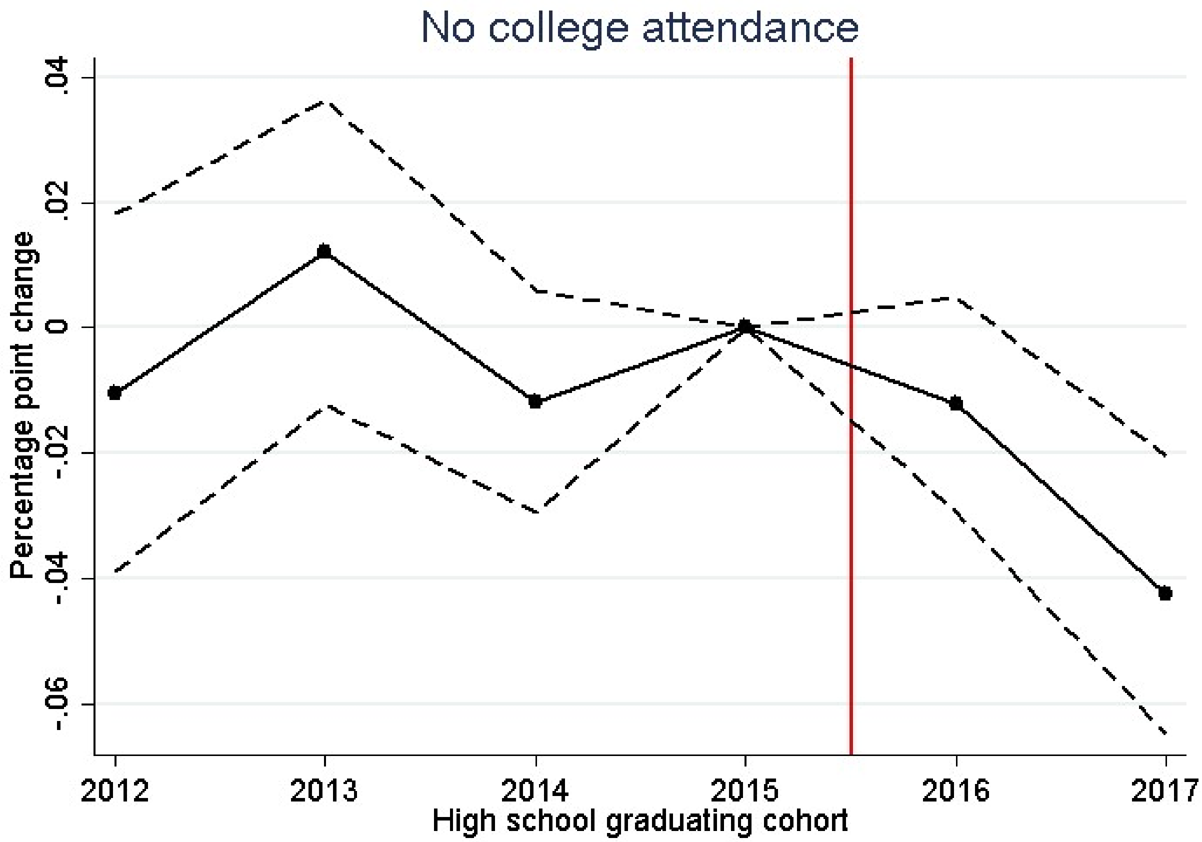
Notes. Point estimates are year-specific treatment effects that compare two-year college enrollment in Oregon versus six control states with mandatory 10th grade PSAT policies, as specified by equation (2). Dotted lines indicate 95% confidence intervals, with all errors clustered at the state-year level.

Figure 2. Difference-in-difference event study impacts of Oregon Promise on four-year college enrollment



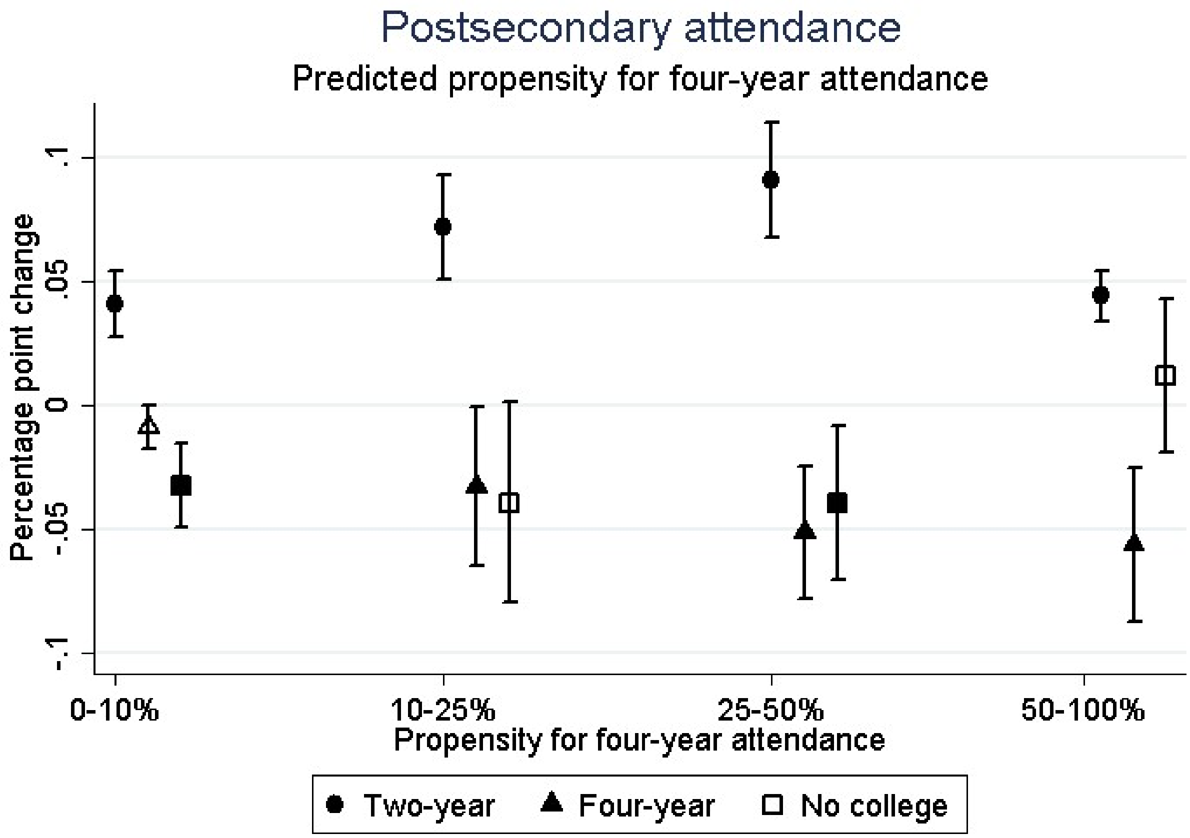
Notes. Point estimates are year-specific treatment effects that compare four-year college enrollment in Oregon versus six control states with mandatory 10th grade PSAT policies, as specified by equation (2). Dotted lines indicate 95% confidence intervals, with all errors clustered at the state-year level.

Figure 3. Difference-in-difference event study impacts of Oregon Promise on no college enrollment



Notes. Point estimates are year-specific treatment effects that compare no college enrollment in Oregon versus six control states with mandatory 10th grade PSAT policies, as specified by equation (2). Dotted lines indicate 95% confidence intervals, with all errors clustered at the state-year level.

Figure 4. Impacts of Oregon Promise on sector of college enrollment, by propensity to enroll in a four-year college



Notes: Point estimates are treatment effects that compare sector of college enrollment in Oregon versus six control states with mandatory 10th grade PSAT policies, as specified by equation (1). Bins classify students into propensities for likelihood of attending a four-year college, using data on Oregon students in the 2012 cohort as measured by characteristics described in footnote 9, with the distribution of propensities shown in Appendix Figure 5. Solid symbols indicate an estimate that is statistically significant at the p<0.05 level; hollow symbols indicate non-significant estimates. Regression results shown in Table 4.

Appendix Table 1. Impacts of Oregon Promise on postsecondary attendance, robustness checks with high school fixed effects

	(1)	(2)	(3)	(4)	(5)
Two-year attendance	0.051**	0.041**	0.043**	0.048**	0.043**
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Four-year attendance	-0.025**	-0.016**	-0.012**	-0.025**	-0.019**
	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
No college attendance	-0.026**	-0.024**	-0.030**	-0.023**	-0.025**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
N	2218903	4148735	5204245	596464	586431
Sample Composition					
Additional states (MD, TX, VA)		✓	✓		
Additional students (11th grade PSAT, SAT)			✓		
Exact matched controls				✓	
Propensity matched controls					✓
Covariates	✓	✓	✓	✓	✓
High school fixed effects	✓	✓	✓	✓	✓

Notes. * p<0.05, ** p<0.01. Regressions include state and year fixed effects and cluster standard errors at the state-by-year level, as specified by equation (1). Covariates include dummies for 10th grade and 11th grade PSAT scores (binned by 10 points), SAT scores (binned by 100 points), gender, ethnicity, self-reported income, AP participation, zip-code level median household income, and high school school characteristics (urbanicity, 12th grade size, quintiles of free- and reduced-price lunch status, distance to closest two- and four-year college).

Appendix Table 2. Impacts of Oregon Promise on sample composition

	Female	White	Asian	Black	Hispanic	High-income
Individual characteristics	0.000	0.002	-0.001	0.000	-0.001	0.006*
	(0.001)	(0.004)	(0.001)	(0.001)	(0.004)	(0.003)
	Grade 12 size	Lunch status	City	Suburb	Town	Rural
School characteristics	7.203	0.000	0.001	0.006	-0.004	-0.003
(Common Core of Data)	(6.614)	(0.001)	(0.017)	(0.011)	(0.007)	(0.004)
	Average 10th grade PSAT	Took 11th grade PSAT	Average 11th grade PSAT	Took SAT	Average SAT	Took AP
Test characteristics	0.121	-0.016	-1.180	-0.035	3.876	0.010*
	(0.212)	(0.019)	(4.881)	(0.020)	(4.889)	(0.004)

Notes. * p<0.05, ** p<0.01. Regressions include state and year fixed effects and cluster standard errors at the state-by-year level, as specified by equation (1). All columns use 2,212,760 observations, except those that condition on having an exam score.

Appendix Table 3. Impacts of Oregon Promise on postsecondary attendance, variation in standard error estimates

	Main sample			CEM sample		
	Two-year	Four-year	No college	Two-year	Four-year	No college
Point estimates	0.053	-0.027	-0.025	0.049	-0.026	-0.023
Standard error or pseudo-standard error estimates						
Unadjusted	**(0.002)**	**(0.002)**	**(0.003)**	**(0.002)**	**(0.002)**	**(0.003)**
Cluster: state-by-year	**(0.005)**	**(0.010)**	(0.011)	**(0.005)**	**(0.010)**	**(0.011)**
Cluster: state	**(0.004)**	**(0.015)**	**(0.011)**	**(0.005)**	**(0.015)**	**(0.011)**
Cluster: high school	**(0.005)**	**(0.004)**	**(0.005)**	**(0.005)**	**(0.004)**	**(0.005)**
Regular bootstrap	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.002)**	**(0.003)**
Wild Cluster bootstrap	**(0.004)**	**(0.012)**	**(0.010)**	**(0.004)**	**(0.013)**	**(0.010)**
Pairs bootstrap	**(0.007)**	(0.038)	(0.038)	**(0.007)**	(0.044)	(0.043)
Ferman-Pinto	**(0.020)**	(0.036)	(0.031)	**(0.021)**	(0.036)	(0.029)

Notes. * p<0.05, ** p<0.01. Standard error estimates derive from equation (1) with standard error corrections described in the first column. All columns use 2,212,760 observations. Covariates include dummies for 10th grade and 11th grade PSAT scores (binned by 10 points), SAT scores (binned by 100 points), gender, ethnicity, self-reported income, AP participation, zip-code level median household income, and high school school characteristics (urbanicity, 12th grade size, quintiles of free- and reduced-price lunch status, distance to closest two- and four-year college). All bootstraps used 500 repetitions. Wild cluster and pairs bootstrap performed by 'clustse' in Stata. Ferman-Pinto calculated using public code provided by the authors (Ferman & Pinto, forthcoming), adjusted for this analysis.

Appendix Table 4. Heterogeneous impacts of Oregon Promise on postsecondary attendance

		Main estimates		
	N	Two-Year	Four-year	No College
Male	1092314	0.050**	-0.019	-0.031*
		(0.006)	(0.011)	(0.013)
Female	1120446	0.054**	-0.034**	-0.020
		(0.006)	(0.012)	(0.013)
White	1065950	0.045**	-0.031**	-0.014
		(0.006)	(0.011)	(0.012)
Black/Hispanic	882023	0.069**	-0.017	-0.052**
		(0.009)	(0.009)	(0.015)
Asian	84412	0.051**	-0.064**	0.014
		(0.010)	(0.020)	(0.024)
Other	180375	0.051**	-0.014	-0.038**
		(0.007)	(0.010)	(0.010)
1st PSAT tercile	738643	0.051**	-0.011	-0.039**
(Low PSAT)		(0.008)	(0.007)	(0.012)
2nd PSAT tercile	749354	0.064**	-0.028*	-0.036*
		(0.008)	(0.011)	(0.015)
3rd PSAT tercile	724763	0.045**	-0.036*	-0.008
		(0.004)	(0.014)	(0.014)
1st FRPL tercile	739558	0.048**	-0.036*	-0.012
(Low poverty HS)		(0.007)	(0.014)	(0.016)
2nd FRPL tercile	739839	0.055**	-0.023*	-0.032*
		(0.006)	(0.010)	(0.013)
3rd FRPL tercile	733363	0.051**	-0.021	-0.031**
		(0.006)	(0.011)	(0.011)
Took SAT	1154319	0.071**	-0.061**	-0.010
		(0.010)	(0.015)	(0.017)
Did not take SAT	1058441	0.041**	-0.015*	-0.026**
		(0.006)	(0.006)	(0.010)

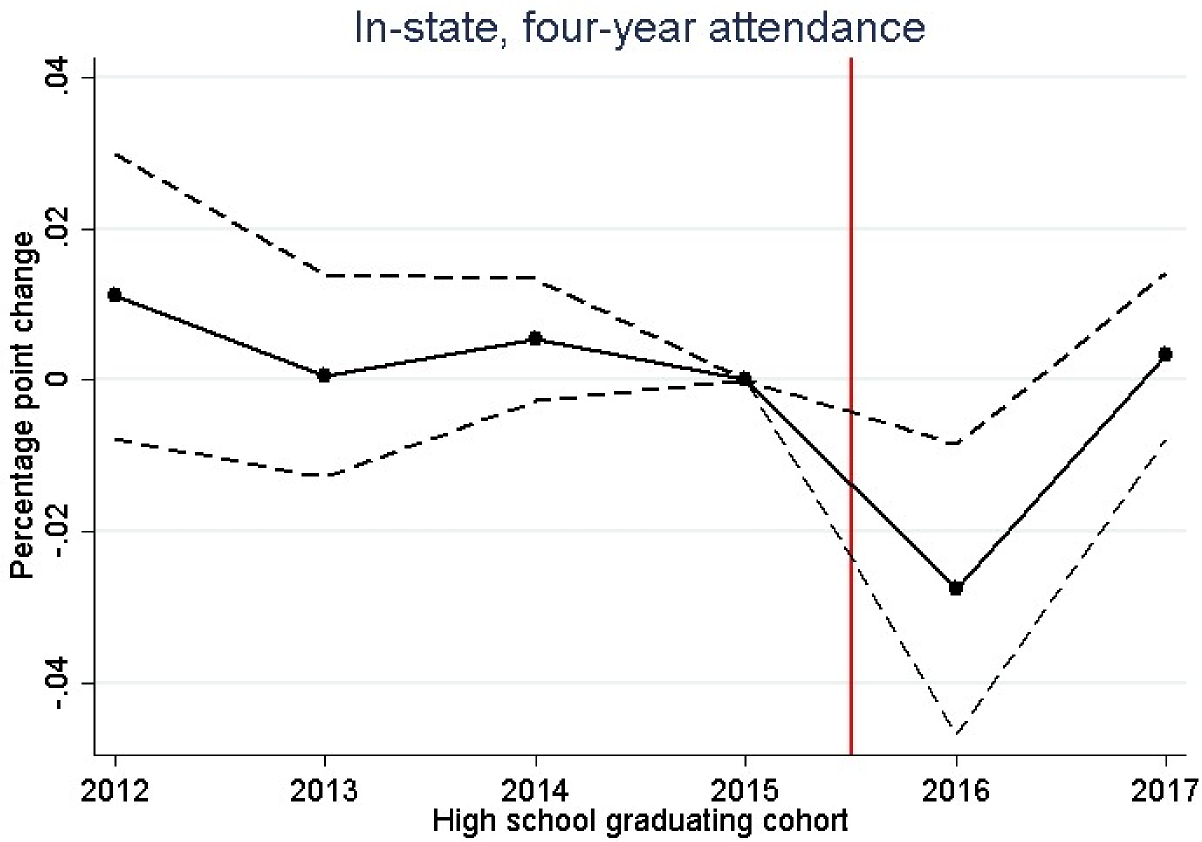
Notes. * p<0.05, ** p<0.01. Main estimates include state and year fixed effects and cluster standard errors at the state-by-year level, as specified by equation (1). Covariates include dummies for 10th grade and 11th grade PSAT scores (binned by 10 points), SAT scores (binned by 100 points), gender, ethnicity, self-reported income, AP participation, zip-code level median household income, and high school school characteristics (urbanicity, 12th grade size, quintiles of free- and reduced-price lunch status, distance to closest two- and four-year college).

Appendix Table 5. Impacts of Oregon Promise on total terms enrolled in first year after high school graduation

	Total terms enrolled in first year of potential postsecondary attendance	Enrolled two or more terms in first year of potential postsecondary attendance (binary variable)
Main estimates	0.051	0.024
	(0.032)	(0.014)
Event study estimates		
2012	0.061**	0.010
	(0.017)	(0.008)
2013	-0.015	-0.005
	(0.032)	(0.011)
2014	(omitted year - poor data)	
2015	(omitted year)	
2016 (1st treatment year)	0.038	0.010
	(0.026)	(0.009)
2017 (2nd treatment year)	0.094**	0.041**
	(0.027)	(0.014)

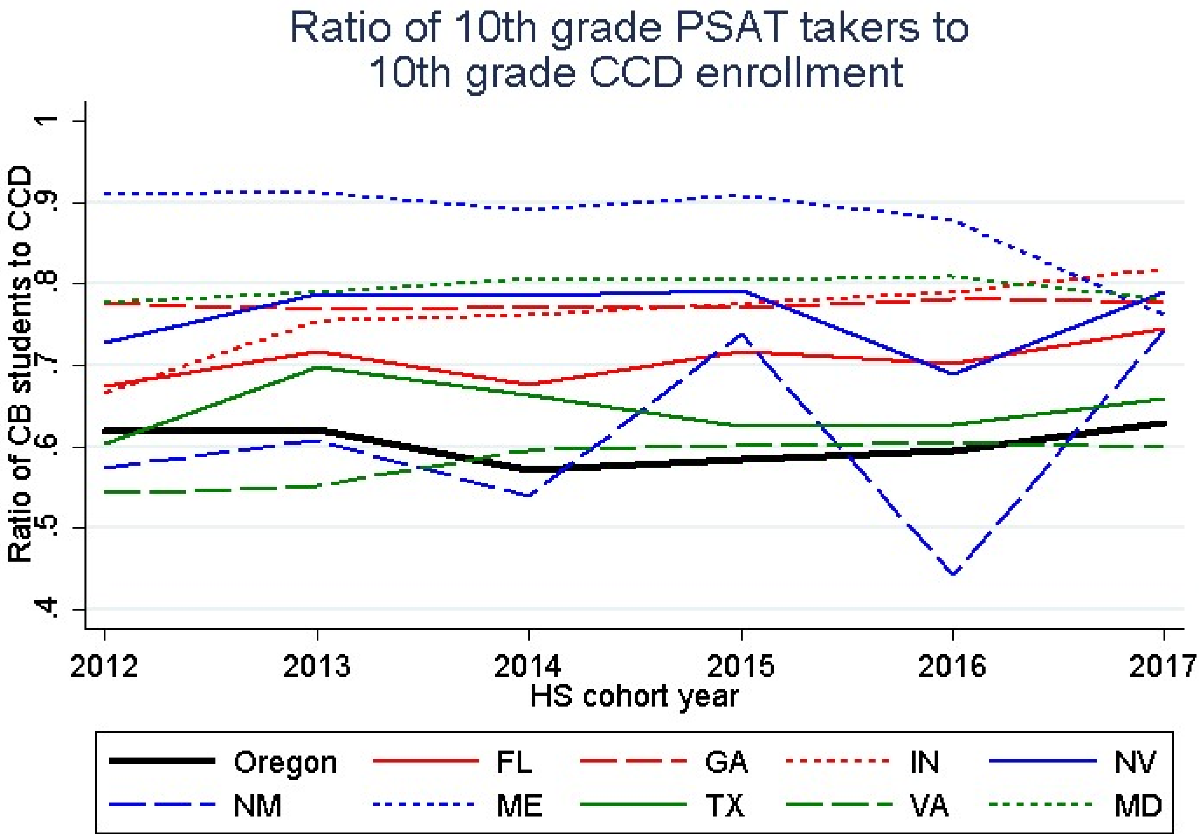
Notes. * p<0.05, ** p<0.01. Main estimates include state and year fixed effects and cluster standard errors at the state-by-year level, as specified by equation (1). Event study estimates interact year dummies with treatment status as specified by equation (2). All columns use 1,837,904 observations, as data in 2014 were omitted to due mistakes in College Board's coding of these outcomes. Covariates include dummies for 10th grade and 11th grade PSAT scores (binned by 10 points), SAT scores (binned by 100 points), gender, ethnicity, self-reported income, AP participation, zip-code level median household income, and high school school characteristics (urbanicity, 12th grade size, quintiles of free- and reduced-price lunch status, distance to closest two- and four-year college).

Appendix Figure 1. Difference-in-difference event study impacts of Oregon Promise on in-state four-year college enrollment



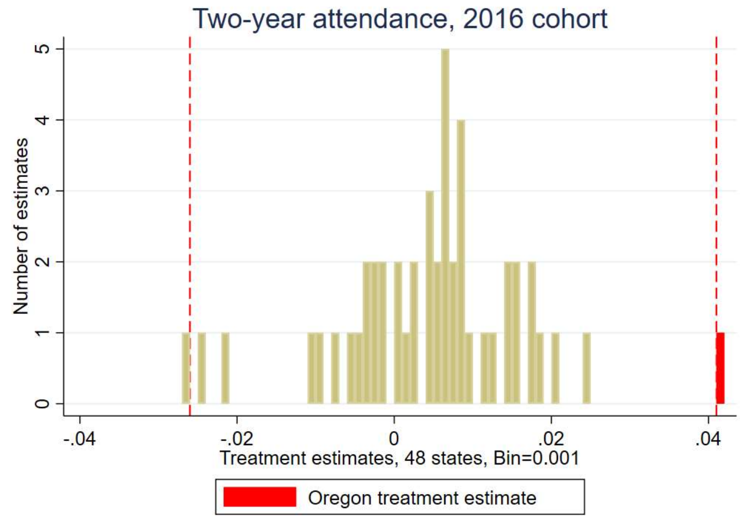
Notes. Point estimates are year-specific treatment effects that compare in-state four-year college enrollment in Oregon versus six control states with mandatory 10th grade PSAT policies, as specified by equation (2). Dotted lines indicate 95% confidence intervals, with all errors clustered at the state-year level.

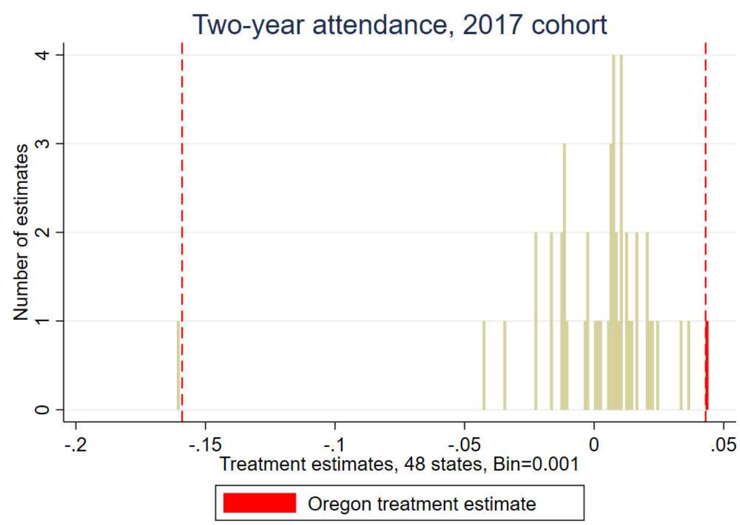
Appendix Figure 2. Representation of state-level public school students in College Board PSAT and SAT data



Notes. Estimates include all students attending public high schools with positive values for 10th grade enrollment. The numerator is state-level observed College Board observations and the denominator is state-level 10th grade CCD enrollment figures two years prior (e.g., 2013 CCD 10th grade data and 2015 cohort College Board data). Red lines represent the three largest states in the primary estimation strategy, blue lines represent the three smallest states, and green lines represent the three extra states with strong PSAT coverage used in robustness checks. Estimates may be inflated due to reporting errors to either the CCD or the College Board.

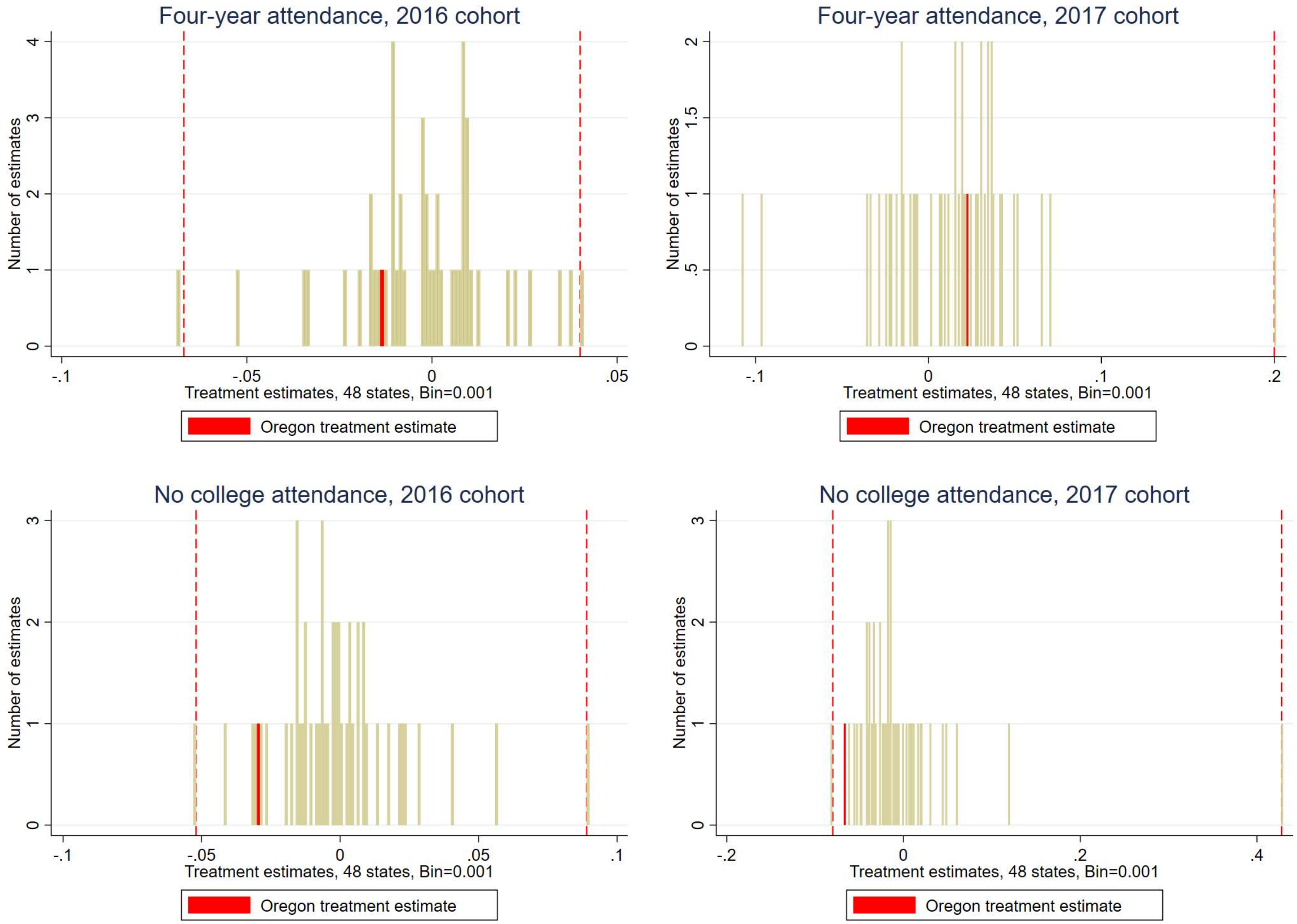
Appendix Figure 3. Distribution of treatment effects for two-year attendance, full sample of 48 states including all PSAT/SAT exam takers





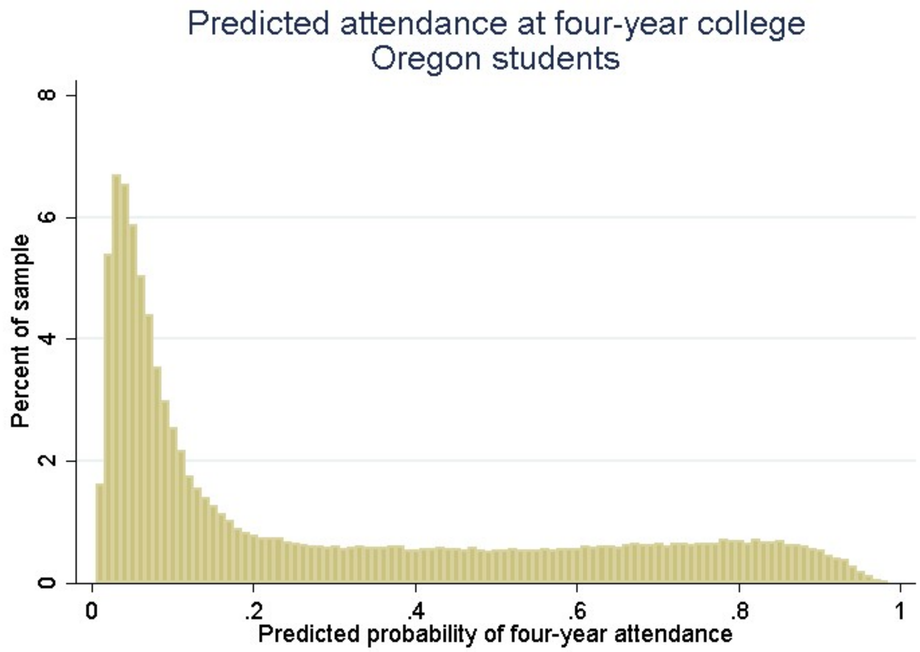
Notes. Each bar represents a treatment estimate for one of 48 states (excluding MN and TN), with Oregon in red. Red dashed lines indicate the highest and lowest treatment effects, which correspond to the 95% confidence interval.

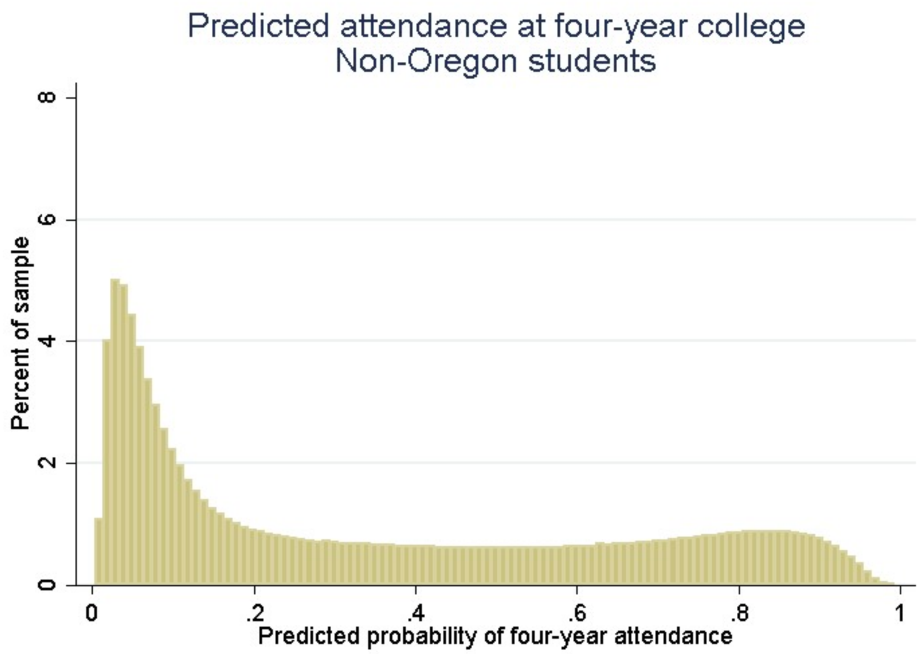
Appendix Figure 4. Distribution of treatment effects for four-year and no college attendance, full sample of 48 states including all PSAT/SAT exam takers



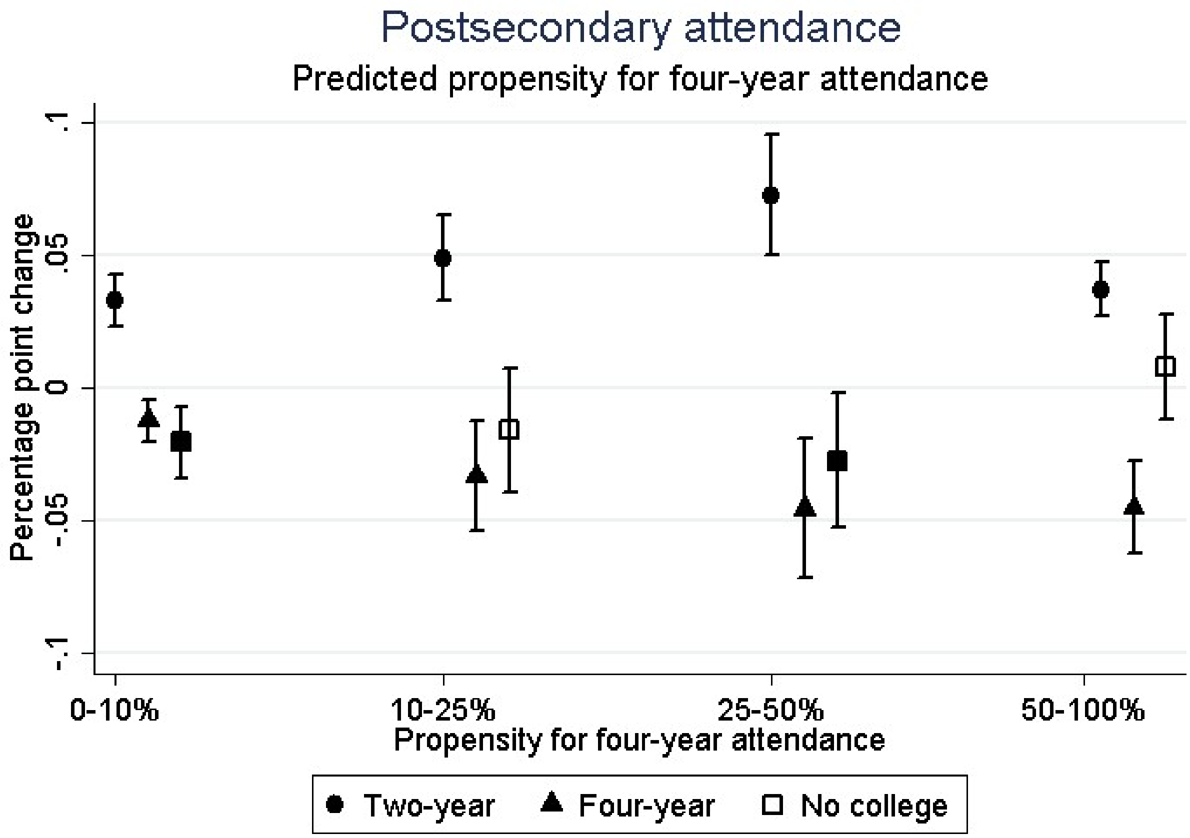
Notes. Each bar represents a treatment estimate for one of 48 states (excluding MN and TN), with Oregon in red. Red dashed lines indicate the highest and lowest treatment effects, which correspond to the 95% confidence interval.

Appendix Figure 5. Predicted attendance at four-year institution for 10^{th} grade PSAT takers, Oregon and control states



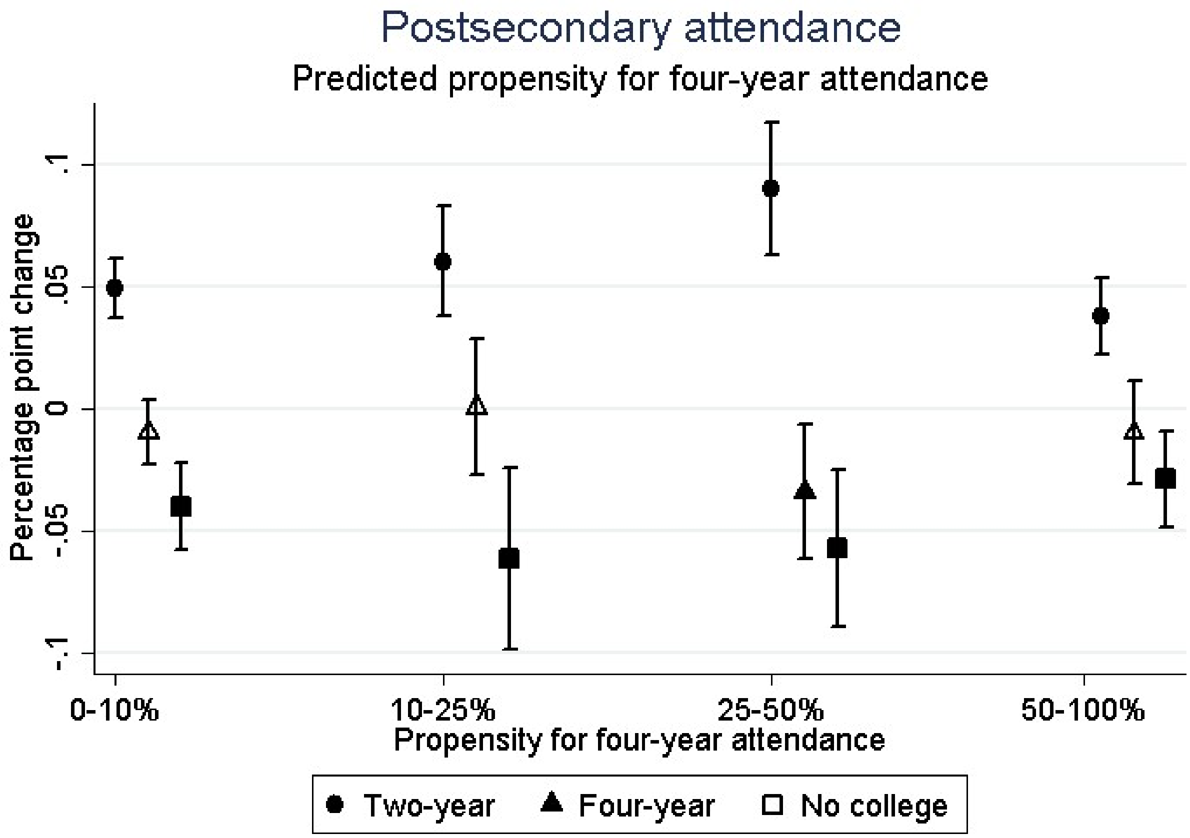


Appendix Figure 6. Impacts of Oregon Promise on sector of college enrollment, by propensity to enroll in a four-year college, 2016 treatment effects



Notes: Point estimates are treatment effects that compare sector of college enrollment in Oregon versus six control states with mandatory 10th grade PSAT policies, as specified by equation (1). Bins classify students into propensities for likelihood of attending a four-year college, using data on Oregon students in the 2012 cohort as measured by characteristics described in footnote 9, with the distribution of propensities shown in Appendix Figure 5. Solid symbols indicate an estimate that is statistically significant at the $p<0.05$ level; hollow symbols indicate non-significant estimates. Regression results shown in Table 4.

Appendix Figure 7. Impacts of Oregon Promise on sector of college enrollment, by propensity to enroll in a four-year college, 2017 treatment effects



Notes: Point estimates are treatment effects that compare sector of college enrollment in Oregon versus six control states with mandatory 10th grade PSAT policies, as specified by equation (1). Bins classify students into propensities for likelihood of attending a four-year college, using data on Oregon students in the 2012 cohort as measured by characteristics described in footnote 9, with the distribution of propensities shown in Appendix Figure 5. Solid symbols indicate an estimate that is statistically significant at the $p<0.05$ level; hollow symbols indicate non-significant estimates. Regression results shown in Table 4.

Appendix 1. Synthetic Control design estimates

In this appendix I estimate impacts of the Oregon Promise using a synthetic control design (Abadie et al., 2010). All estimates are produced using the 'synth' command in Stata 14.2. Results broadly mirror findings from the difference-in-difference (DD) methods presented in the paper but differences in methodology and changes to the sample composition are discussed below.

As outcome measures I create state-level averages of two- and four-year college attendance rates among the College Board sample, as well as the percent who attended no college (all definitions provided in the main paper). The DD results restrict the main analysis to a set of seven states – Oregon as the treated state and six control states – that fully subsidize the 10th grade PSAT. The data begin with the 2011-12 high school cohort as all seven states had fully implemented the program by this point, though Oregon first fully implemented the program for the 2010-11 cohort. The data end with the 2016-17 high school cohort simply based on data availability.

Both in the main paper but also in this appendix I relax some of these data restrictions. In this appendix I show results using all College Board exam takers (i.e., 10th and 11th grade PSAT takers and SAT takers) with the following restrictions:

1. Using a set of 48 states that begins with the 2012 high school graduating cohort.
    a. I remove both MN and TN, which implemented state-level Promise programs during this time period.
2. Using 48 states but beginning with a longer pre-period that starts in 2008.
    a. The largest potential problem with this approach is that prior to 2012 some states had significantly lower coverage rates (i.e., low ratio of observed College Board data to the actual state-level student population), and there is differential selection into coverage over time as states adopt different policies. In general I do not favor this approach, though recognize that the synthetic control design method may still be able to optimally select an appropriate counterfactual.
3. Using ten states – the primary seven states (OR and GA, FL, IN, NM, NV, ME) with mandated 10th grade PSAT coverage, plus MD, TX, and VA, which all had strong 10th grade PSAT coverage.
4. Using just the primary seven states (OR and GA, FL, IN, NM, NV, ME).

I then show results for each of these four groups using two methods:

1. By exact matching on college enrollment in each per-period year. For example, results for two-year college attendance separately match two-year college attendance in each pre-period year prior to 2016; I use the exact same approach for the four-year college and no college outcomes.
2. Using the first method but also including a set of covariates that includes: average SAT and PSAT scores (both 10th and 11th grade separately); percent of students who are African-American, Hispanic, or Native American; the percent of observed students whose high

school is located in suburbs or town/rural areas, and; the average free- and reduced-price lunch participation rate of high school attended.

Each method produces a weighted control unit using multiple states that then serves as the counterfactual to what would have occurred in Oregon in the absence of the policy change. Each analysis presented below uses all PSAT and SAT takers in a state, but results based only on 10th grade PSAT takers shows similar results. As some states have very low representation of these students, I omit these results for brevity.

The benefits of the synthetic control method, as compared to the difference-in-difference estimator, include: (1) the researcher does not get to arbitrarily choose control states, as these are selected by a purely data-driven procedure; (2) a non-parametric model that does not require extrapolating from pre-treatment trends to estimate future outcomes (or deviations from these outcomes). Of course, these benefits can also be drawbacks as, for instance, there may be theoretically justified reasons for selecting specific controls. For instance, I omit Minnesota and Tennessee knowing that they added state-level Promise programs during this time period, but opt to leave all other states in so as not to preclude potential matches.

One last issue with synthetic control is that it does not produce standard errors in a manner similar to typical regression analysis. I follow the permutation-based approach to inference, and use the remaining 47 states as "placebo" treatment effects, and then compare my point estimate to the distribution of these other 47 estimates, with the expectation that any statistically significant results from my regression will be in the tails of these distributions. In short, these distributions allow me to estimate how likely my result would have happened by random chance.

Appendix Table T1 shows that the range of point estimates of the Oregon Promise on two-year, four-year, and no college attendance. Results closely match difference-in-difference (DD) estimates, with positive impacts on two-year college attendance ranging from 3.4 to 5.4 percentage points in the first year and 3.2 to 6.9 percentage points in the second. It bears noting that the impacts in the 7 state sample is very similar to those in the DD results presented throughout the paper.

As in the DD, there are large negative impacts on four-year college enrollment for the 2016 cohort with almost no impact on no college attendance, again suggesting that most movement was strictly away from four-year colleges. In contrast, estimates for the 2017 cohort mostly point toward increases in overall college attendance, with much smaller impacts on four-year college enrollment.

Appendix Table T1. Impact of Oregon Promise, synthetic control design estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of states	48	48	10	7	48	48	10	7
Start year	2012	2008	2008	2008	2012	2008	2008	2008
Covariates					✓	✓	✓	✓
	Synthetic Control Design impact estimates							
Two-year attendance								
2016	4.1%	3.4%	4.4%	5.1%	4.4%	3.4%	3.5%	5.4%
2017	4.5%	6.9%	4.1%	6.0%	4.6%	6.9%	3.2%	4.8%
Four-year attendance								
2016	-3.1%	-4.4%	-4.4%	-4.3%	-4.4%	-4.4%	-4.6%	-4.3%
2017	-1.3%	0.8%	0.8%	0.8%	-2.7%	0.8%	0.8%	0.7%
No college attendance								
2016	0.5%	-0.4%	-0.2%	-0.2%	0.5%	-0.4%	0.3%	-0.8%
2017	-2.4%	-2.4%	-3.4%	-3.4%	-0.5%	-2.4%	-2.5%	-4.3%

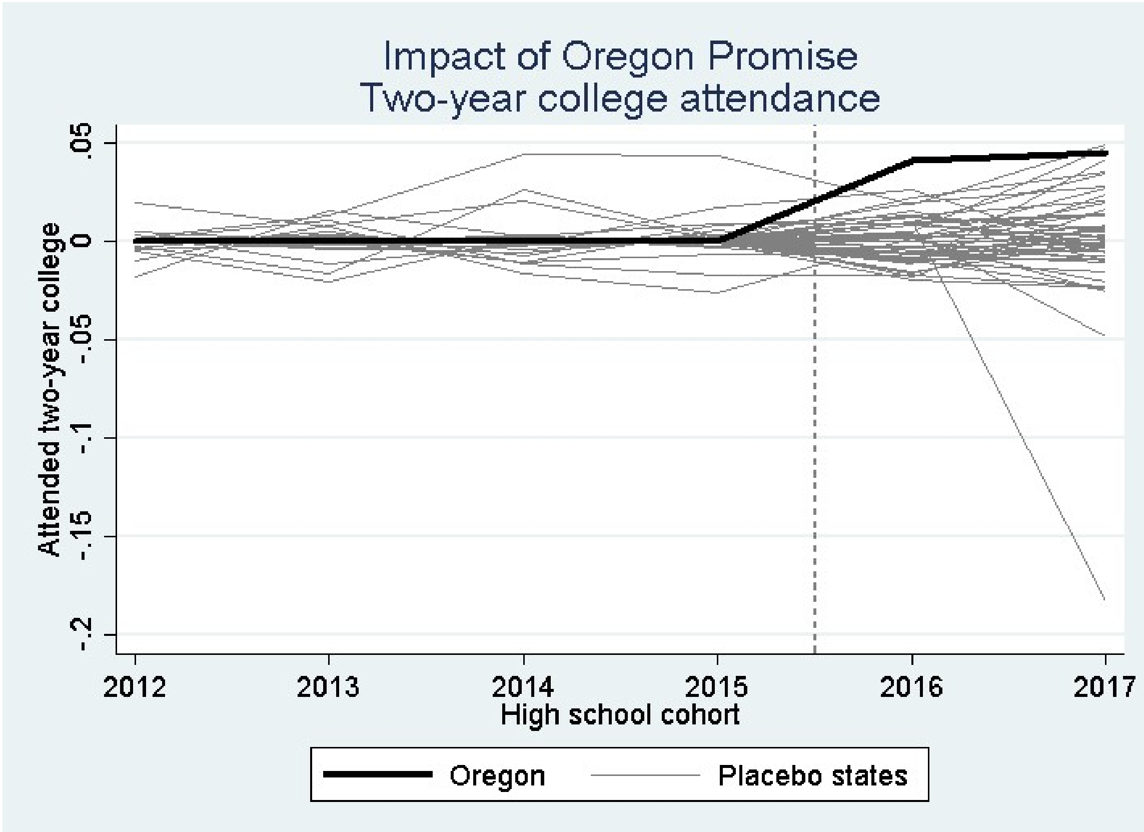
Notes. Each value derives from a synthetic control design estimate that controls for an exact match of postsecondary enrollment (e.g., two-year enrollment for two-year estimates, four-year enrollment for four-year estimates) in each year prior to 2016. Each estimate varies by the number of states or pre-period years used. Covariates include: 10th and 11th grade PSAT scores, SAT scores, percent of students who are underrepresented minorities (African-American, Hispanic, Native American), high-school level percentages of free- and reduced-price lunch participation and residence in suburban or rural schools.

In Appendix Figures F1, F3, and F5, I show the synthetic control results for Oregon on two-year, four-year, and no college attendance, respectively, based on the sample in column 1 of Appendix Table T1. Each figure shows Oregon as a solid black line and each placebo treatment effect as a gray line. Some researchers would omit states for which the set of control states suggests a poor counterfactual, but I choose to leave this in for simplicity and transparency. In Appendix Figures F2, F4, and F6 I show the full distribution of treatment effects for the two-year, four-year, and no college outcomes, respectively, with separate histograms for the 2016 and 2017 cohorts.

Appendix Figures F1 and F2 show that Oregon's increase in community college enrollment does appear to be a strong outlier, with the positive treatment effect in 2016 by far larger than any other placebo state, and the positive treatment effect in 2017 similar among the largest five estimates (with the largest negative effect clearly a result of poor fit). Appendix Figures F3 and F4 show the negative impact on four-year attendance is strong but only resides in the top quintile of treatment effects before going closer to zero in 2017. Similarly, Appendix Figures F5 and F6 show a null effect on no college attendance in 2016 before a negative effect in 2017, though the overall set of estimates are noisy, and often not well matched in the pre-period.

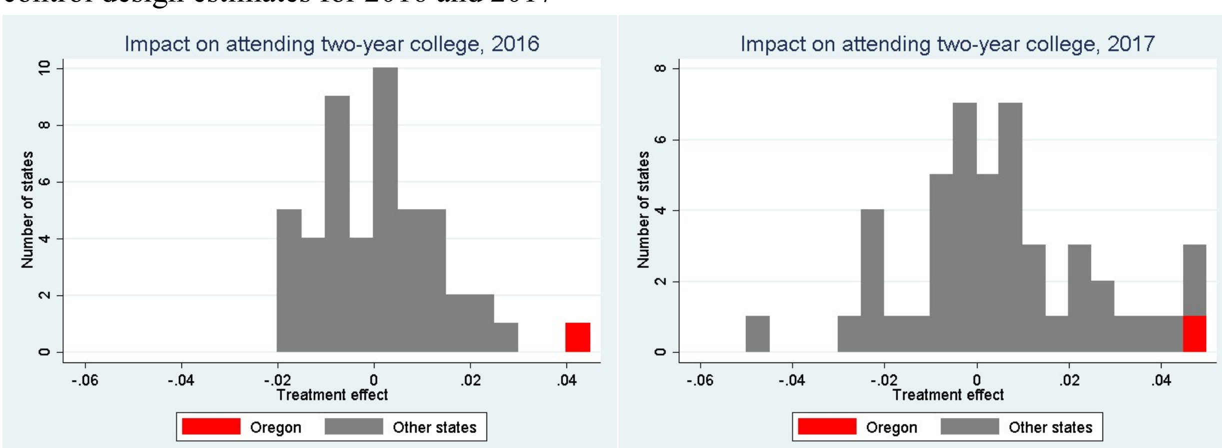
In Appendix Figures F7 through F14 I show Oregon's pathway for the three college-going outcomes, with a dashed line indicating the counterfactual control states. Each of these figures corresponds to one of the eight columns in Appendix Table T1.

Appendix Figure F1. Impacts of Oregon Promise on two-year college attendance



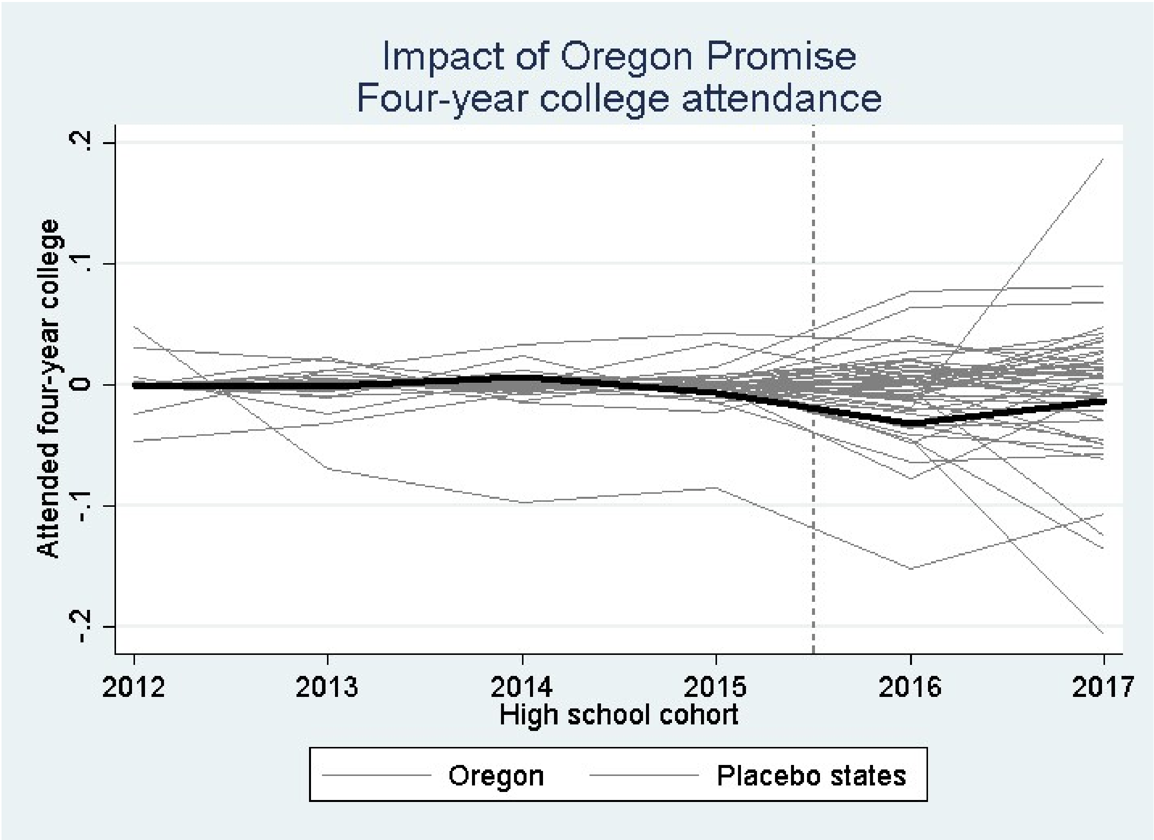
Notes. Black line indicates synthetic control design estimates for Oregon, with gray lines representing impacts on other 47 states as placebo estimates. Results are based on specification (1) in Appendix Table T1, which: eliminates MN and TN; begins in 2012; matches each pre-period year on two-year enrollment, and; does not include additional covariates.

Appendix Figure F2. Histogram of treatment and placebo effects on two-year attendance, synthetic control design estimates for 2016 and 2017



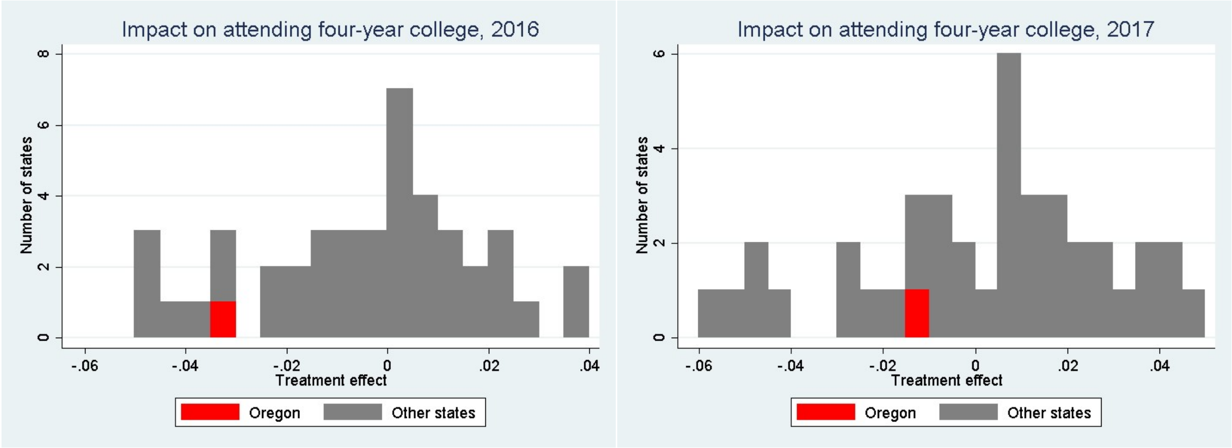
Notes. Histogram shows full distribution of 48 treatment effects (all states except MN and TN), separately for 2016 and 2017, as based on specification (1) in Appendix Table T1. Red bar indicates treatment effect for Oregon, which implemented the Oregon Promise, an in-state community college tuition program in 2016.

Appendix Figure F3. Impacts of Oregon Promise on four-year college attendance



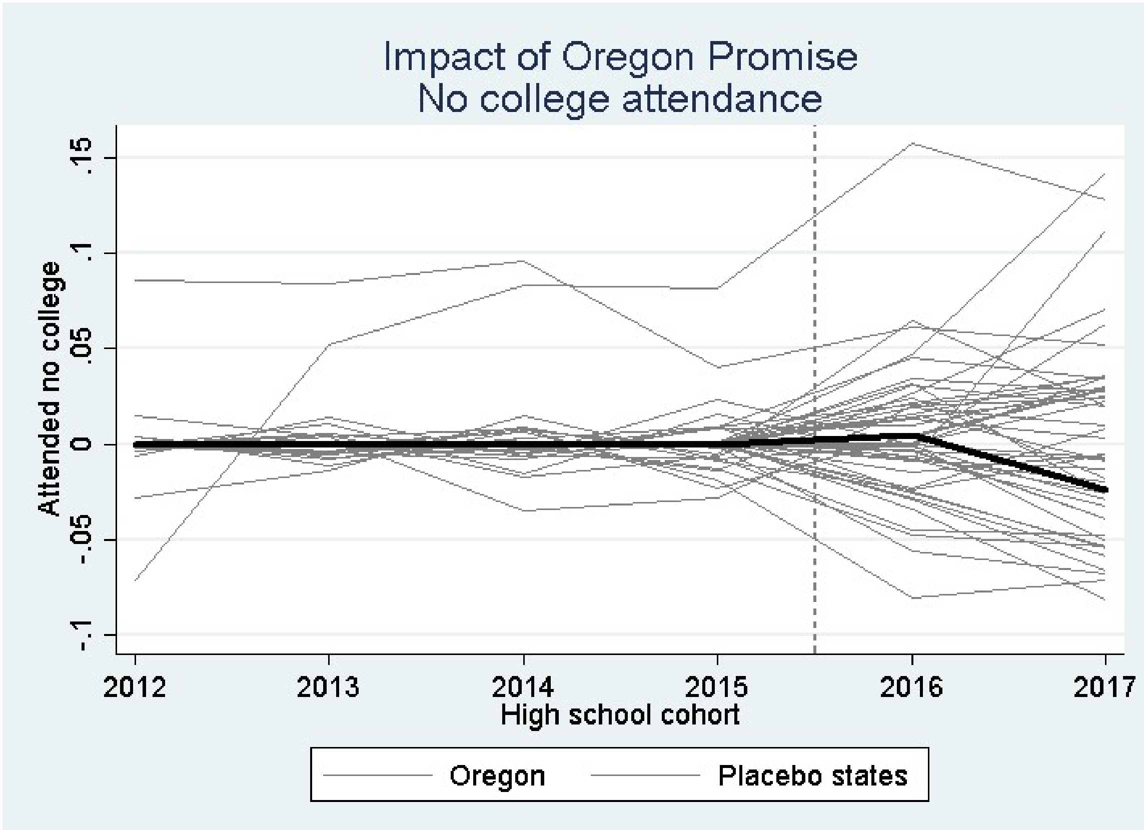
Notes. Black line indicates synthetic control design estimates for Oregon, with gray lines representing impacts on other 47 states as placebo estimates. Results are based on specification (1) in Appendix Table T1, which: eliminates MN and TN; begins in 2012; matches each pre-period year on two-year enrollment, and; does not include additional covariates.

Appendix Figure F4. Histogram of treatment and placebo effects on four-year attendance, synthetic control design estimates for 2016 and 2017



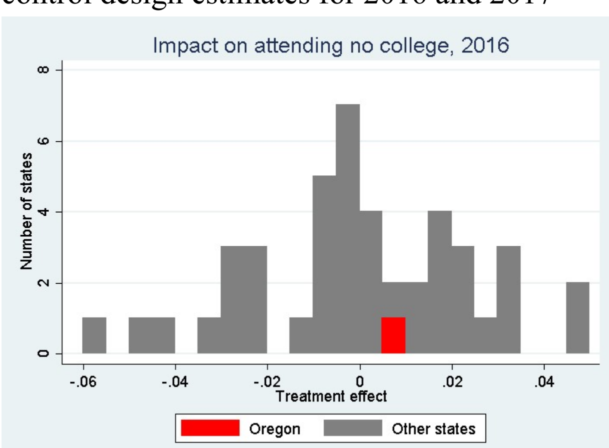
Notes. Histogram shows full distribution of 48 treatment effects (all states except MN and TN), separately for 2016 and 2017, as based on specification (1) in Appendix Table T1. Red bar indicates treatment effect for Oregon, which implemented the Oregon Promise, an in-state community college tuition program in 2016.

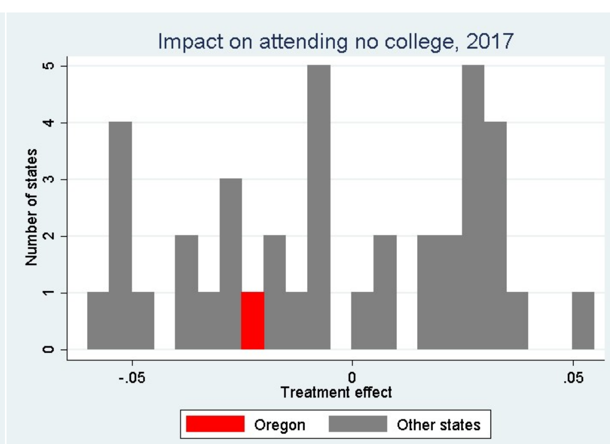
Appendix Figure F5. Impacts of Oregon Promise on no college attendance



Notes. Black line indicates synthetic control design estimates for Oregon, with gray lines representing impacts on other 47 states as placebo estimates. Results are based on specification (1) in Appendix Table T1, which: eliminates MN and TN; begins in 2012; matches each pre-period year on two-year enrollment, and; does not include additional covariates.

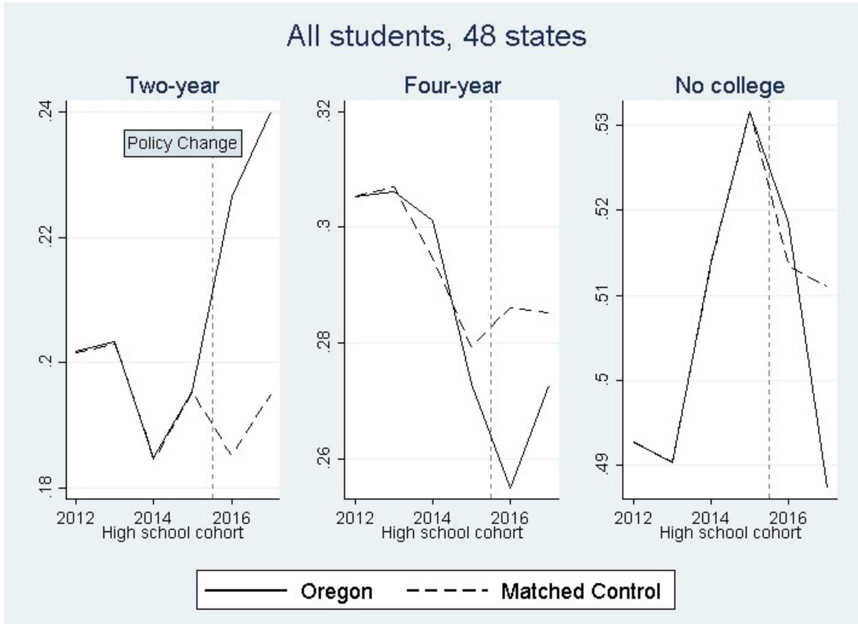
Appendix Figure F6. Histogram of treatment and placebo effects on two-year attendance, synthetic control design estimates for 2016 and 2017





Notes. Histogram shows full distribution of 48 treatment effects (all states except MN and TN), separately for 2016 and 2017, as based on specification (1) in Appendix Table T1. Red bar indicates treatment effect for Oregon, which implemented the Oregon Promise, an in-state community college tuition program in 2016.

Appendix Figure F7. 48 state sample, 2012 start year, no covariates



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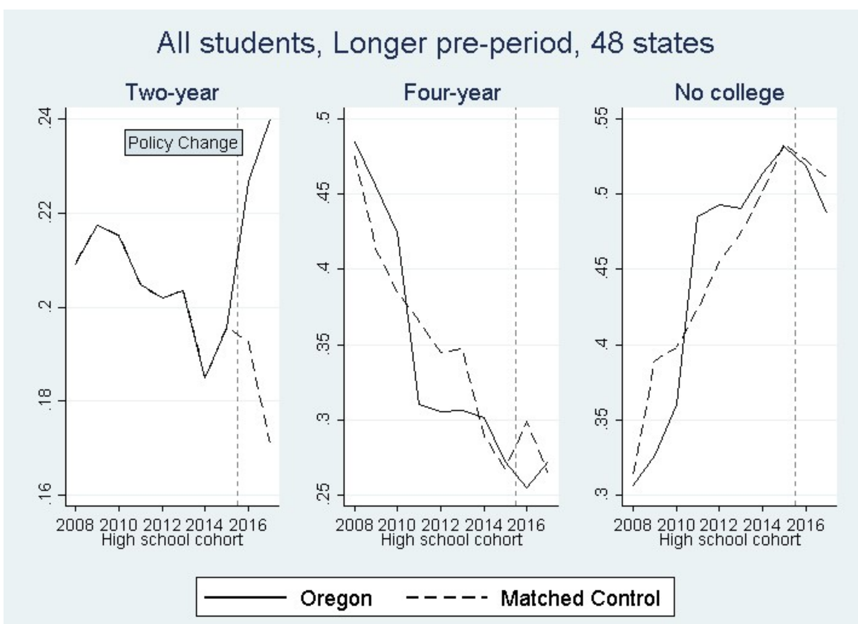
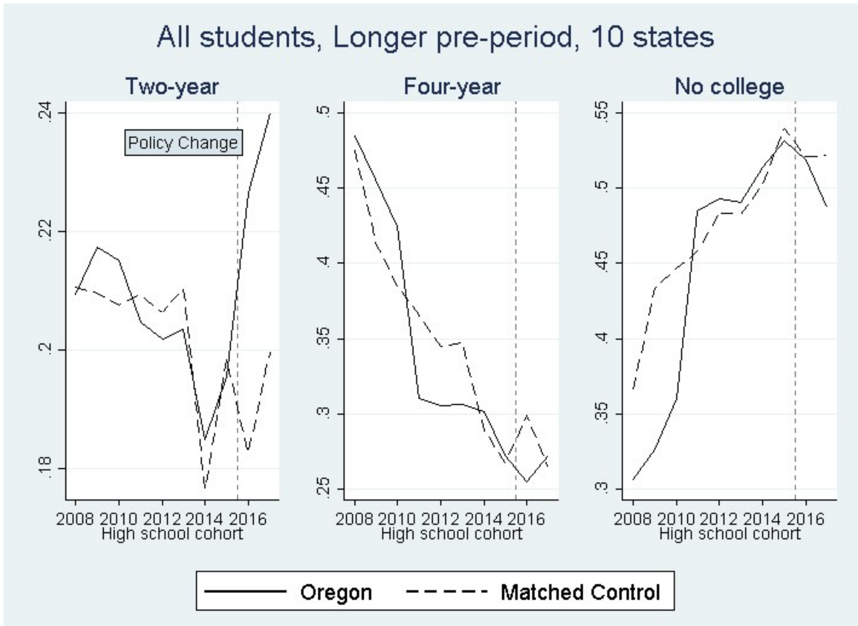
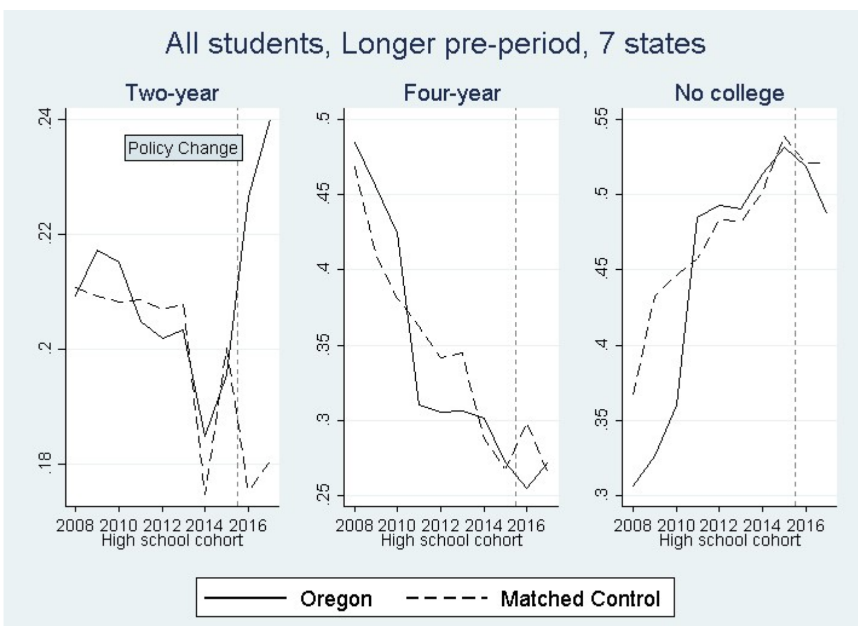
Appendix Figure F8. 48 state sample, 2008 start year, no covariates



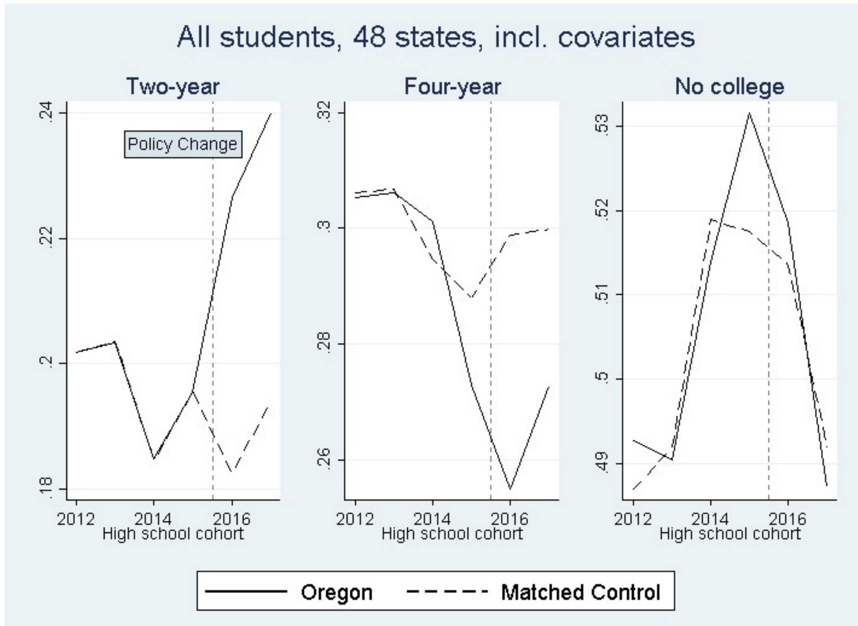
Appendix Figure F9. 10 state sample, 2008 start year, no covariates



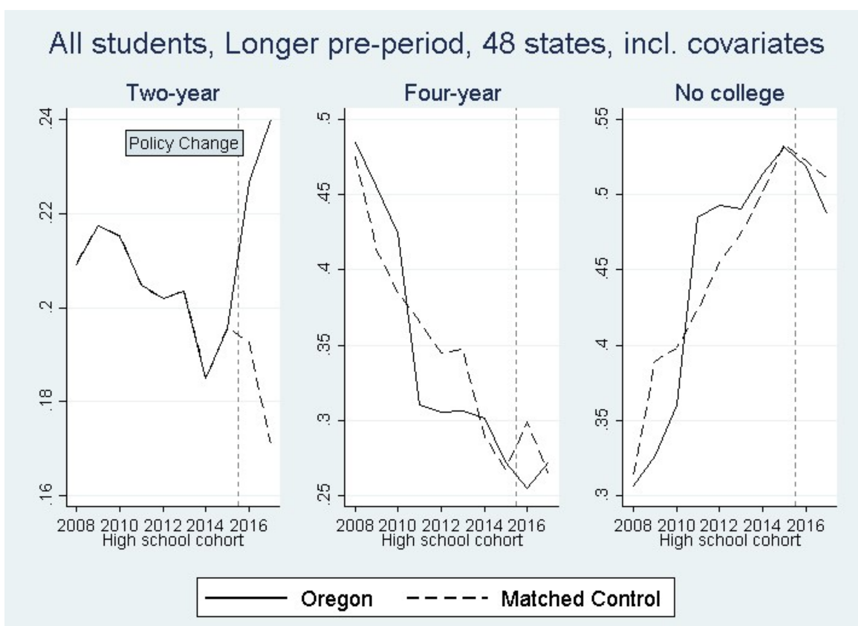
Appendix Figure F10. 7 state sample, 2008 start year, no covariates



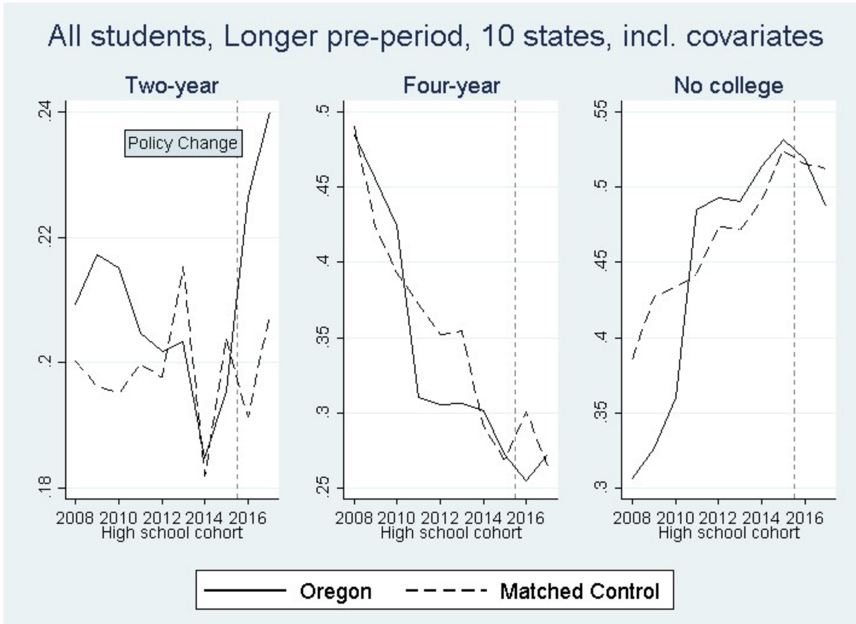
Appendix Figure F11. 48 state sample, 2012 start year, with covariates



Appendix Figure F12. 48 state sample, 2008 start year, with covariates



Appendix Figure F13. 10 state sample, 2008 start year, with covariates



Appendix Figure F14. 7 state sample, 2008 start year, with covariates