The ACT of Enrollment: The College Enrollment Effects of State-Required College Entrance Exam Testing

Daniel Klasik

Stanford University

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Please address all correspondence to:
Daniel Klasik
Stanford University
520 Galvez Mall
Stanford, CA 94305
djklasik@stanford.edu

Abstract

Since 2001 Colorado, Illinois, and Maine have all enacted policies that require high school juniors to take college entrance exams—the SAT or the ACT. One goal of these policies was to increase college enrollment based on the belief that requiring students to take these exams would make students more likely to consider college as a viable option. Relying on quasi-experimental methods and synthetic control comparison groups, this article presents the effects of this state-mandated college entrance-exam testing. Based on both state- and individual-level analyses I find evidence that entrance exam policies were associated with increases in overall college enrollment in Illinois and that such policies re-sorted students in all three states between different types of institutions. In particular, Colorado saw an increase in enrollment at private four-year institutions, while Illinois and Maine both saw a decrease in enrollment at pubic two-year institutions. Increases in enrollment at schools that require entrance exams for admissions support the hypothesis that lack of exam scores can present barriers to college entry.

Increasing college enrollment rates has long been a goal for both the federal and state governments. For many years financial aid policies have been one of the major levers used by government to reach this end. While generally effective at dealing with the *financial* barriers to college-going, these policies are quite expensive and ignore other potential enrollment obstacles. Some states and local municipalities have recently begun trying non-aid-based, and as yet untested, methods for increasing college enrollment. These policies work to address non-financial barriers to enrollment. This article evaluates the effect of one such method: the statewide requirement of college entrance-exam testing.

College entrance exams such as the ACT or SAT are required or recommended for admission to nearly all of the nation's four-year colleges and universities and are likewise used in the admissions process at many two-year colleges. Many students interested in attending such schools, however, often fail to sit for these exams. For example, only 70 percent of students who in tenth grade expressed a desire to earn a four-year college degree had taken the SAT or ACT by their senior year of high school (NCES 2004). Failure to take college entrance exams can often mean the end of students' four-year college degree hopes, limited options for admission or advanced course placement at community colleges, or reduced eligibility for some forms of college scholarships.

Policies that require that students take a college entrance exam eliminate this failure as a reason not to enroll in college. In this article I study the effect of such college entrance-exam requirements on college enrollment in Colorado, Illinois, and Maine. Specifically, I seek to answer the following research question: Do state college entrance-exam requirements change the college enrollment choices of students in these states?

To answer this question, I use institutional enrollment data from the Integrated Postsecondary Educational Data System (IPEDS) and individual student-level data from the October supplement of the Current Population Survey (CPS). I treat the state-by-state adoption of college entrance-exam requirements as a natural experiment, allowing me to use a comparative interrupted time series model to compare the enrollment changes in states requiring these exams to synthetic control states that did not.

Background

State Policy and College Enrollment

At present, the vast majority of federally and state-funded policies designed to improve college attendance address financial barriers to enrollment. In a sample of five states, Perna and her colleagues (2008) found that over 90 percent of state policies directed toward a college enrollment outcome provided some sort of financial support for college. The remaining programs addressed either students' academic preparation for college or their knowledge about college.

This emphasis on financial barriers is not without warrant—research has consistently found links between such policies and increased college attendance. One of the more well-known state aid policies is the Georgia Helping Outstanding Pupils Educationally (HOPE) Scholarship, which provides funding to high achieving students who attend college in-state. Two studies in particular have examined how this merit-based financial assistance program has affected college enrollment among Georgia students. Dynarski (2000) found that the HOPE scholarship increased college enrollment for Georgia students by roughly 7 percentage points.

Dynarski's (2000) study addressed how the HOPE scholarship affected *whether* but not *where* students enrolled in college. This issue was subsequently addressed by Cornwell, Mustard, and Sridhar (2006). Cornwell et al. (2006) studied the HOPE effect on college enrollment by institutional type. They found large increases in enrollment in four-year private colleges and little, or negative, change in enrollment at two-year institutions. Other studies of state aid programs such as the CalGrant program in California (Kane 2003) and the District of Columbia Tuition Assistance Grant Program (Abraham & Clark 2006) found similar positive enrollment effects of state efforts to increase enrollment by providing financial assistance for college.

Though responsible for modest gains in enrollments, these programs have two main drawbacks. First, they are expensive. Georgia paid $13,650 per *new* student who enrolled who otherwise would not have. Second, efforts to alleviate financial barriers to college enrollment come relatively late in the process—usually after students have applied to, or even been accepted to, college. As a result, financial aid policies cannot help students who were discouraged from attending college before they applied.

Indeed, while the enormous cost of college attendance surely plays a large role in preventing students interested in a college degree from enrolling, it is not the only barrier to college attendance. Many students who aspire to a college degree may not enroll for simpler reasons: they fail to complete the different steps of what can be a complicated college admissions process (Klasik 2012). Many researchers have noted that groups of students who are traditionally underrepresented in college, those from minority and low-income families, fail to complete various application steps at the same rate as White or wealthy students (Avery & Kane 2004; Berkner & Chavez 1997; Cabrera & La Nasa 2001; Horn 1997; Klasik 2012).

With regard to the specific step of taking college entrance exams, Avery and Kane (2004) note that only 32 percent of a sample of largely minority urban students had taken any college entrance exam by the fall of their senior year while nearly 98 percent of a sample of wealthier suburban students had done so. Ultimately, 25 percent of the urban students enrolled in a four year college, compared to 88 percent of the suburban students. Even more directly, Klasik (2012) emphasized the specific importance of taking the SAT or ACT for college enrollment. Klasik found that few students enrolled in four-year institutions without having taken the SAT or ACT. In fact, taking the SAT or ACT was one of the steps most predictive of later college application and enrollment.

State College Entrance-Exam Requirements

Many states now recognize the obstacle presented by college entrance exams. In an attempt to raise college enrollment among their students, six states require all high school students to take one of these exams and have contracted with the ACT or the College Board (which administers the SAT) to pay for these tests. Colorado and Illinois were the first states to require the ACT for all high school juniors beginning in the spring of 2001. Maine required all of its high school juniors to take the SAT beginning in the spring of 2006. Michigan, Kentucky, and Wyoming have also begun requiring either the ACT or the SAT. Because their policies have been in place the longest and have enough post-policy data to analyze, the remainder of this article focuses on the policies in Colorado, Illinois, and Maine.

Although the exams were used primarily to fulfill No Child Left Behind testing requirements, state officials in all three states cited a desire to focus student attention on their postsecondary options as a motivation for their particular use of college entrance exams (Betebenner & Howe 2001; Illinois State Board of Education 2006; State of Maine 2007). Their belief was that providing students with entrance exam scores would cause college enrollment rates to increase. So great was the motivation to boost college enrollments that all three states adopted exam requirements even as they acknowledged that the exams did not fully capture state high school learning standards (Betebenner & Howe 2001; College Board 2005; Illinois State Board of Education 2006).

Conceptual Framework

There are two main ways in which college exam requirements can affect students' college choices. First, receipt of exam scores provides students with additional information on which to base their college enrollment decisions. Second, by making the exam a requirement rather than an option, states may increase students' buy-in to the college admission process by moving them further along in the process before allowing them to opt-out. Each of these mechanisms is discussed below.

Students often lack information about their own academic ability and likelihood of successful college completion (Altonji 1993; Manski 1993). This uncertainty limits students' abilities to make optimal decisions about college enrollment. College entrance exam scores are valuable pieces of information in the college search process: they provide an explicit measure that students can use to assess their likelihood of college admission by comparing it to a school's admissions criteria. Such comparisons may reveal preparedness for college that a student may not have otherwise realized. Thus college entrance-exam requirements may provide students with information that will help them make better decisions about college. Given this additional information, it is difficult to predict the schools at which students will choose to enroll.

College entrance-exam requirements may also alter students' college enrollment choices by changing students' default behavior—their path of least resistance. Before the entrance-exam requirements, a student would have to take specific action to register for an entrance exam. With the state's requirement, the default action for students becomes the taking of an entrance exam. Research has shown that such small changes in defaults can have powerful effects in a number of domains (Behsears, Choi, Laibson, & Madrian 2009). In the domain of retirement saving, employees are more likely to contribute to such plans if they are enrolled in them automatically (Choi, Laibson, Madrian, & Metrick 2002; Madrian & Shea 2001). Like retirement saving, college enrollment involves the completion of small steps that can result in large future financial rewards. Despite these clear payoffs, each has a lower than desired uptake. While more work is required to enroll in college than taking an entrance exam, changing this default clearly creates additional investment in college-going and keeps students moving down the path to college enrollment. At the most basic level, if failure to take an exam disqualifies students from consideration at some schools, then changing the default test-taking behavior automatically removes this hurdle. If this mechanism is at work, we would expect increases in enrollment at schools that require the SAT or ACT for admission.

Both of these mechanisms serve as what Thaler and Sunstein (2009) refer to as a "nudge": a small change to the way an individual approaches a choice that can affect his or her decisions. Two recent studies have shown that, despite the enormity of the college enrollment choice, nudges can have an effect on students' postsecondary enrollment decisions. Pallais (2009) found that a $6 reduction in the cost of submitting four ACT score reports caused students to submit more score reports to a wider variety of colleges. This behavior, in turn, was connected to an increase in the likelihood of submitting an additional college application. Bettinger, Long, Oreopoulos, and Sanbonmatsu (2009) demonstrated in a randomized trial that helping students complete their Free Application for Federal Student Aid resulted in a 7.7 percentage-point increase in the likelihood that those students enrolled in college. Thus, seemingly minor changes in the college enrollment process resulted in observable increases in not only the likelihood that students applied to and enroll in college, but which colleges students considered.

Empirical Strategy

Analysis of the effect of college entrance-exam requirements proceeds in two parts. The first part looks at changes in total college enrollments at the state-level while the second part considers changes in the individual likelihood that a given student enrolls in a particular type of college. A more technical and detailed description of the methods is given in the Appendix to the online version of this article.

State-Level Analysis

The state-by-state adoption of required college entrance exams allows for a straightforward comparative interrupted-time-series design.[1] The underlying strategy of this method is to compare the trend of outcomes in states with the entrance-exam requirements—the treatment—to the same trends in a set of states that did not require entrance exams before and after the treatment was in place. This method relies on the assumption that the comparison states serve as a good representation of what would have happened in the entrance-exam states in the absence of an entrance-exam requirement. If we believe this assumption holds then any difference in outcomes between the entrance-exam states and the comparison states after the enactment of an entrance-exam requirement can be attributed to the policy.

I used two main techniques to ensure the strength of this assumption. First, I selected the states to use as comparisons such that their pre-treatment characteristics and outcome trends closely aligned with the characteristics and outcome trends in the entrance-exam requirement states. I describe this method below. Second, in the regression estimation of the effect of the entrance-exam policies, I accounted for many factors that could affect the validity of the comparison group in the post-policy time period. Both state and year fixed effects work towards this goal. Respectively, these fixed-effects account for all factors of a given state that are related to an outcome that are constant over time and the characteristics of a given year that are related to an outcome, but do not vary between states.

These fixed-effects account for a large amount of potential confounding factors but are not able to account for state-by-year effects—non-policy variables that may explain changes in outcomes that change in particular states in particular years. Here we are concerned that some variable not related to an entrance-exam requirement changed in entrance-exam states in the same year the requirements went into effect and that changes in outcome may be related to this variable and not the exam policies themselves. For example, if Colorado's entrance-exam requirement implementation coincided with a notable increase in the size of the population of students graduating from high school, there may be an increase in the number of students enrolling in college, but it may not be due to the new policy. In order to avoid such problems, I controlled for as many of these potential state-by-year confounders as possible. Specifically, I controlled for the percent of college-age (18-19 years) state residents from different racial groups, number of high school seniors in the state, state high school graduation rate, average annual state unemployment, state expenditure per public school student, the percentage of a state's institutions of different types (public, private, etc.), and average state performance on the ACT or the SAT. Note that we expect average state ACT or SAT scores to change in states that require that all students take one of them simply because of the change in the composition of test-takers. To account for this as best as possible I control not for a state's actual average ACT score, but rather a state's over- or under-performance on that exam relative to what would be expected given the percent of students in that state who took the exam. In other words, I control for a state's average ACT or SAT score after adjusting for the exam participation rate in that state.[2]

We might also be concerned that the nature and magnitude of any effects of entrance-exam requirements may vary depending on state context. For example, Illinois has a higher proportion of private four-year institutions than Colorado. If one of the effects of entrance-exam requirements is to shift students' attention to private institutions, then this effect may be more apparent in Illinois than Colorado. For this reason, I perform each analysis separately for Colorado, Illinois, and Maine.

Synthetic controls.

The presence of just one state receiving the entrance-exam requirement treatment in each analysis necessitates choosing a set of comparison states that match the treated state well. I found the set of comparison states by using synthetic control methods. The synthetic control method developed out of exactly the need to find an appropriate comparison group for a single treated unit. Abedie, Diamond, and Hainmueller (2007) and Abadie and Gardeazabal (2003) explain the details of the method. In short, the synthetic control method assigns weights between 0 and 1 to each of a set of candidate comparison states such that the weights sum to 1 and the weighted average of various pre-treatment characteristics in the comparison states—including values of the pre-treatment outcome—match those in the treated state as close as is mathematically possible.

Eligible comparison states included all states in the US except for those who have adopted, or are about to adopt, college entrance-exam requirements. I matched comparison units by all of the state-by-year covariates listed above, as well as the pre-treatment trend of the given enrollment outcome. Enrollment outcomes included the log of aggregated total enrollment of the colleges in a given state for: all degree granting colleges, two- and four-year schools, public or private four-year colleges, schools that require entrance exams for admission, schools that accept only full-time students, and enrollment at schools based on Carnegie-defined admissions selectivity. Log units allow for the interpretation of the effect estimates in terms of percentage change in the outcomes.

Synthetic controls are similar to, but not the same as propensity score matching. In propensity score matching, the goal is to match units based on their likelihood of receiving a given treatment. The three treated units and up to 46 comparison units in this analysis are not enough to estimate effectively the likelihood a state adopts an entrance-exam requirement. The synthetic control method instead creates weighted matches to treatment states with the primary goal of aligning the pre-treatment trends of a given outcome between treated and non-treated states.

Synthetic Fit.

To assess the quality of a synthetic control match I evaluated its root mean squared prediction error (RMSPE)—a measure of the difference between the outcome in treated and comparison units in the pre-treatment period. Overall, the RMSPE of the matches ranged from 0.013 to 0.298. Table 1 presents the results of the match for enrollment at selective colleges in Colorado, which had the median RMSPE (RMSPE=0.042). This match resulted from a synthetic control group of five states—California, Nevada, Utah, Washington, and Wisconsin—whose weights are given in Table 2.

Inference.

With the synthetic control weights in hand, I calculate the effect of entrance-exam requirements by performing a weighted linear regression with dummy variables indicating treated states in post-policy years, the fixed effects and control variables described above, and using the synthetic control weights. The coefficient of the treated-state post-policy dummy gives the treatment effect—it captures the difference in outcome between the entrance-exam state and the comparison states after enactment of the entrance-exam requirement relative to the same difference before the requirement.

This analysis differs from most regression analyses because of the small number of treated units in this analysis. As a result, the large-sample assumptions needed for traditional methods of statistical inference do not hold. Thus, the standard errors generated by typical regression estimation are not valid. Instead, I employ an exact inference test as described by Abedie, Diamond, and Hainmueller (2007). Using this technique, I first calculate the effect of a placebo treatment on all non-treated comparison states. I produce p-values for the likelihood of a Type-I error by calculating what percentage of these effects are as or more extreme than the effect I estimate for the states that did receive the treatment. I limit these comparisons to placebo units whose synthetic controls have an RMSPE no more than five times as large as that of the given treated unit. This restriction helps account for the fact that some synthetic matches are closer than others and thus helps free the p-values from bias stemming from poor matches.

Individual-Level Analysis

The state-level analysis described above estimates the effect of entrance-exam requirements on aggregate state-level college enrollment for different types of colleges. To provide another perspective on the effect of entrance-exam requirements I performed a second analysis that used individual-level data to examine the effects of entrance-exam requirements on the likelihood a particular individual enrolled in different types of colleges.

I also conducted the individual-level analysis as a comparative interrupted time-series: I compared the likelihood of enrollment in a particular type of college between individuals who lived in exam-requiring and comparison states before and after the exam requirements were enacted. State and year fixed effects control for the effect of living in a particular state, or graduating high school in a particular year. Because I evaluated the effect of a policy that occurred in a particular state in a given year, threats to the validity of the effect estimates must come from omitted variables that are also state-by-year (Angrist & Pischke 2009). Thus, I controlled for the same state-by-year covariates that were included in the state-level analysis except for the state racial composition variables: I account for race as an individual variable. Controls for individual characteristics help add precision to the estimates. In addition to race, these individual controls included a student's gender and whether the individual lived in a city.

In order to choose comparison units in a way that maximized the match between treated and control units in terms of state-by-year factors I followed the example of Fitzpatrick (2008) and used the synthetic control weights from the state-level analysis to determine comparison observations. Thus I used analysis weights that were the product of the data's original sample weights and the synthetic control weights that corresponded to an individual's state of residence. This weighting means that comparison individuals come from states with comparable pre-treatment characteristics and outcome trajectories, with greater weights given to students from more closely matching states.

Estimates of the effect of mandatory entrance-exams came from a weighted linear probability model of college enrollment with dummy variables that indicated whether a student lived in an exam requirement state after the policy was enacted (the treatment), state and year fixed effects, and controls for the covariates described above.[3] The coefficient of the treated-state post-policy dummy gives the difference in likelihood of a given outcome for students living in a treated state, relative to individuals in comparison states in the post-treatment period. For this set of analyses I examined whether individuals enrolled in college at all, enrolled at a two- or four-year college, or were enrolled full-time. In contrast to the state-level analysis, the individual-level analysis allows for the analysis of college attendance across state lines. This ability eliminates the need for the assumption I made in the state-level analysis that students affected by the entrance exam policies would attend college in-state. As in the state-level analyses, I ran the individual-level models separately for Colorado, Illinois, and Maine.

Data

State-Level Data

For the state-level analysis I use IPEDS college enrollment data for every year from 1988 to 2009 (except for 1999 when data was not collected). IPEDS is an annual survey of all postsecondary institutions that accept federal money and is gathered by the National Center for Education Statistics (NCES). The ideal data for this study would be a count of all students from each state who attend college. IPEDS only started to collect enrollment data by state-of-residence in 2000 and even then only collected it every other year. Thus, I assume that if a student is uncertain enough about her college choice that taking standardized exams will affect her decision, the student probably will attend school in-state. The reasonableness of this assumption is backed by prior research that showed that students generally attend colleges close to home (e.g. Long 2004). Although the closest college to some students may be in a neighboring state, I also assume that the cost difference of attending college in-state versus out-of-state will, on average, encourage students to attend close, in-state colleges. These assumptions allow me to analyze changes in state-wide college enrollment, which I measure by aggregating the number of first-time, first-year students reported by degree-granting institutions within each state.

These data were combined with state-year covariates drawn from other sources. Annual state unemployment data were drawn from the Bureau of Labor Statistics, while annual state twelfth-grade enrollment and total high school graduates come from the Common Core of Data collected by NCES. Data on the percentage of 18-19 year-old White, Hispanic, Asian, Black, and Native American in each state were merged from Census data. Data on state average ACT and SAT scores and participation rates came from the ACT, Inc. and the College Board.[4]

This dataset is valuable for analysis because it allows for the study of enrollment changes at the rich variety of institution types identified in IPEDS. This feature is particularly valuable for addressing questions about how entrance-exam requirements affect student preferences for different types of schools. However, aggregated state data limits statistical power because there are so few states on which to base the analysis. Synthetic controls serve to alleviate this limitation.

Individual-Level Data

Individual-level data came from the October Supplement of the CPS. I limited the sample to individuals aged 18-19—those of an age plausibly to be on-time college freshmen. The final sample contained 54,385 potential observations between 1994 and 2009. I merged this data with the state-year covariates used in the state-level analysis. Weighted descriptive statistics for the individual-level CPS observations are given in Table 3.

Because they contain many more observations than the state level analysis, analyses of these data allow for more precision in the estimate of the entrance-exam requirement effects. But these data have the drawback that they are not as rich as the state-level data in providing detail about where students attend college. The CPS only includes whether a student is enrolled in a two- or four-year college and whether the student is enrolled full- or part-time.

Power

Because of their limited observations, these two data sets are limited in their statistical power, but should work together to provide a more complete picture of the effect of state-wide test-requirement policies than either one could on its own. The methods I employ go far in improving the statistical power of the analyses. In the state level analysis I am able to detect differences in overall college enrollment rates at the $\alpha = 0.1$ level of 0.050, 0.050, and 0.098 in Colorado, Illinois, and Maine.[5] In the individual level analysis, I am able to detect differences in the likelihood of any college enrollment at the $\alpha = 0.1$ level of 0.087, 0.11, and 0.075 with 80 percent power in Colorado, Illinois, and Maine, respectively (calculated according to Bloom 1995).

Results

Results for the analysis of the state-level data are presented in Table 4 and results for the individual-level analysis are presented in Table 5. I describe each set of results in turn.

State-level outcomes

Changes in overall college enrollment.

As seen in Table 4, there were no notable changes in enrollment in Colorado or Illinois institutions that were associated with each state’s ACT requirement. Maine, however, saw roughly a 10 percent drop in the number of students enrolled in its colleges.

Two- versus four-year enrollment.

While there was some evidence for overall change in enrollment levels, it may be the case that enrollment changes were focused in two- or four-year institutions. In fact, Illinois saw a roughly 12 percent increase in enrollment in its four-year institutions. In the four-year sector, changes in enrollment appeared to disproportionately benefit private institutions. In all three test-requiring states, estimates of changes in enrollment were all greater for private four-year schools than for public ones. Colorado in particular saw just over a 10 percent increase in its private four-year enrollment.

In contrast, Maine and Illinois saw decreases in two-year college enrollment, decreases that were particularly concentrated in the state's public two-year colleges. Colorado, however, saw small, but not significant, increases in two-year college enrollment.

Enrollment based on admissions policies.

Given the prominent role college entrance exams play in admissions procedures, it is also reasonable to expect changes in college enrollment based on admissions policies. As Table 4 shows, in each state with entrance-exam requirements, Carnegie-classified "inclusive" and "selective" four-year schools saw no notable change in enrollment levels, although all effect estimates for enrollment at inclusive schools were positive. Colorado saw a statistically significant 25 percent increase in enrollment at "more selective" four-year colleges.

Estimates for enrollment changes at schools that required entrance exams for admission were also positive in Colorado and Maine, though only Maine's 14 percent enrollment increase reached statistical significance. Finally, schools that only admitted full-time students saw no significant or notable change in levels of enrollment in any of the entrance-exam-requiring states.

Individual-level outcomes

The analysis of the individual-level CPS data gives the change in likelihood that a student enrolls in a college of a particular type. As seen in Table 5, Colorado students were significantly more likely to enroll in two-year colleges or enroll full time, but no more or less likely to enroll in college overall. Illinois students were nearly 10 percentage points more likely to enroll in any college after the enactment of the entrance-exam requirement. They were also significantly more likely to enroll in four-year colleges and, like Colorado students, more likely to enroll full time. There were no statistically significant changes in the likelihood of college enrollment in Maine.

Discussion

This article presents the effects of state-mandated college entrance exam testing using quasi-experimental methods and synthetic control comparison groups. I find evidence that testing policies re-sort students between different types of institutions. Overall, Illinois students were more likely to enroll in four-year colleges. Further, Colorado saw an increase in enrollment at private and “more selective” four-year institutions, while Illinois and Maine saw a decrease in enrollment at public two-year institutions. There is also some evidence in Colorado and Maine, after the test requirements, that students were more likely to enroll at schools that required the SAT or ACT for admission. And that Colorado and Illinois students were more likely to be enrolled full time.

Interpreting the state- and individual-level results together illustrates how college enrollment changed in Colorado, Illinois, and Maine as a result of each state’s SAT or ACT requirement. State-level results allow us to see a detailed picture of changes in total enrollment in colleges of many types *within a given state*. The individual-level results, however, offer the change in likelihood of college enrollment for students from a given state regardless of the state in which they enroll. Thus a student who leaves her state to go to college will contribute to a decrease in enrollment in the state-level analysis, but will be captured as a college enrollee in the individual-level analysis.

In Illinois, for example, the significant increase in likelihood of enrollment in four-year colleges in the individual analysis confirms the finding that exam requirements were associated with an increase in four-year college enrollment in the state-level analysis. Further, we see that the individual-level analysis shows an increase in the likelihood that Illinois students enrolled in college at all, but there did not appear to be a significant overall increase in enrollment at Illinois colleges in the state-level analysis. This apparent contradiction indicates that the effect of the Illinois exam requirement was to shift students into Illinois's four-year colleges as well as to some colleges out of state.

The general increase in private four-year college enrollment in Colorado is interesting given that Bound and Turner (2007) found these institutions to be among the least responsive to changes in demand for college. There are two possible explanations for this increase despite this inelasticity. First, private colleges may be more likely to purchase student contact information from the ACT and College Board in order to build their recruitment mailing lists. Therefore increases in enrollment at these schools may have resulted from these schools sending informational material to more students in test-requiring states. Second, colleges can use entrance exam scores to determine eligibility for merit-based scholarships. Thus students who faced entrance-exam requirements may have had more access to merit-based aid to alleviate the cost of tuition at private institutions. This merit-aid argument may also help explain the increased likelihood of full-time enrolment in the individual-level data: access to greater aid may have allowed students to afford to enroll full- rather than part-time.

Colorado and Maine's positive increases in enrolment at schools that require the SAT or ACT for admission are also notable findings. This effect indicates that a lack of entrance exam scores may present a barrier to enrollment at certain colleges. This barrier disappears with exam requirements, allowing students more freedom to enroll in a school that best supports their academic needs. If students are able to optimize their choices in this way, entrance-exam requirements may have long-term benefits such as lower transfer or dropout rates and higher completion and graduation rates.

Are college entrance-exam requirements worth it?

Georgia spent about $189 million on its HOPE scholarship program in 1998-99 and saw a roughly seven percent increase in college enrollment (Dynarski 2000). This amounted to the state spending about $13,650 for every new student the program brought into college. In contrast, for example, Colorado spends about $1.6 million annually to administer the ACT. So, in order for Colorado's ACT requirement to be at least as cost-effective as the Georgia HOPE Scholarship as a method of promoting college enrollment, it would only have to induce a 0.09 percentage point increase in college enrollment rates.[6] Such results, even with perfect data, would be difficult to discern statistically, yet many of the findings presented here exceed these thresholds, even if they do not reach statistical significance. It thus seems likely that states should be able to achieve these modest increases in postsecondary enrollment. Even if they do not increase enrollment, the re-sorting of students into institutions caused by entrance exam requirements may justify the policy cost if this re-sorting is accompanied by improved college outcomes such as higher graduation rates.

Conclusion

In the past decade several states have tried to increase the percentage of their high school graduates who attend college by requiring that all students take a college entrance exam in their junior year of high school. As state budgets grow progressively tighter, state policy-makers concerned with educational outcomes should increasingly be tempted by nudges toward college enrollment. This temptation makes it vital that researchers seek to understand how state policy landscapes and existing incentives interact with these nudges to influence student college enrollment behavior. For example, the Colorado system grants automatic college admission to students who meet a given entrance exam threshold; Illinois explicitly tried to help counselors and students understand how to use ACT scores; while Maine turned to the SAT as it faced NCLB-based assessment sanctions. Each of these could be one of many reasons for the between-state differences in the results presented here.

Just as financial aid does not address the only barrier to college enrollment, so too are entrance-exam requirements only one possible nudge states can give students. Some states, for example, have considered making application to college a requirement. Given that college entrance exams are effective at causing students to re-think their college choices, it is important to think about where else in the college enrollment process nudges could affect student decisions. Further it is important to think about whether these nudges affect not only students' initial college enrollment choices, but ultimately their college success. This is a rich area for policy intervention to positively affect students' decisions about college.

Notes

[1] In this design I am only looking for a relative change in the level of the time series after the enactment of entrance-exam requirements. Alternatively, this method could be referred to as a non-parametric difference-in-difference design.

[2] Details of this adjustment are given in the methodological appendix.

[3] Models were also run as logistic regressions, but the results were not qualitatively different from the linear probability models. The results of the latter are reported for ease of interpretation while results from the former are available from the author upon request.

[4] While not publically available from the College Board website, this data has been collected from the College Board over time and is available from the NCES.

[5] Power calculations are given for the overall change in college enrollment. Power calculations for other outcomes are available from the author upon request. Note there is no formal test of power for the synthetic control method. Instead I present the minimum detectable difference at the $\alpha = 0.1$ level given the distribution of RMSPE in the placebo units.

[6] This calculation ignores the cost of providing a different NCLB accountability exam if the ACT were not used. That the ACT serves the double purpose of meeting an NCLB requirement and potentially promotes college enrollment makes it that much more valuable

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Table 1

Descriptive Statistics for Enrollment in Inclusive Colleges in Colorado and a Synthetic Colorado

Pre-treatment Covariate	Colorado	Synthetic Colorado
Enrollment in selective colleges - 1992 (ln)	8.53	8.49
Enrollment in selective colleges - 1996 (ln)	8.60	8.58
Enrollment in selective colleges - 2001 (ln)	8.80	8.82
12th grade cohort (ln)	10.62	10.40
High school gradution rate (%)	83.02	80.86
Entrance exam residual	19.77	16.97
Average state unemployment (%)	4.59	4.77
Asian (%)	2.43	3.58
Black (%)	4.72	4.47
Hispanic (%)	18.11	12.43
Native American (%)	0.94	1.54
Four-year institutions (%)	50.35	44.68
Private institutions (%)	19.25	19.54
For-profit institutions (%)	39.39	34.28
Private, four-year institutions (%)	16.57	16.54
For-profit, four-year institutions (%)	13.74	8.56
Public, two-year institutions (%)	21.33	26.49
Private, two-year institutions (%)	2.68	3.01
For-profit two-year institutions (%)	25.64	25.72
Institutions requiring entrance exam scores (%)	52.64	44.57
State expenditures per student ($)	$57,870.74	$49,954.95
RMSPE	0.042	

Note . Graduation rate is of 12th grade students. Racial composition is of the college-going (age 18-19) population. White is omitted. Postsecondary composition is of all two- and four-year degree-granting institutions. Two-year, public, and public-four-year are omitted. Entrance exam residual gives the state's average over/under performance on the ACT after adjusting for examination rate. ACT scores have been converted to the 1600 point SAT score.

Table 2

Synthetic Control Weights for Enrollment in Selective Colleges in Colorado

Control State	Synthetic Weight
Utah	0.485
Nevada	0.298
Wisconsin	0.111
California	0.077
Washington	0.028

Table 3

Weighted Descriptive Statistics of Individual Data from Treated and Synthetic Control States

	National	Colorado	Synthetic Colorado	Illinois	Synthetic Illinois	Maine	Synthetic Maine
Male	50.59	51.78	50.61	48.87	49.35	50.68	50.19
White	63.87	69.70	51.43	64.22	55.64	96.71	85.10
Black	15.11	5.15	9.86	17.41	17.00	0.56	2.46
Hispanic	15.49	19.50	31.93	14.59	20.70	0.96	6.53
Other race	6.20	6.37	9.89	4.26	8.06	1.77	6.57
City resident	6.83	4.27	14.58	21.10	6.10	0.00	0.28
Total Observations	54,385	899	13,536	1,918	14,661	801	6,078

Note. Table gives weighted percent of sample that falls in each category.

Table 4

Estimated Effect of Entrance-Exam Requirements on State Enrollment Changes, by State and College Type

	All Colleges	Four-year	Two-year	Private Four-year	Public Four-year	Public Two-year	Inclusive	Selective	More Selective	Test Required	100% Full-time students
Colorado											
ACT requirement	0.015	-0.070	0.068	0.104 +	-0.022	0.154	0.231	-0.036	0.247 *	0.112	-0.016
RMSPE of control	0.065	0.042	0.089	0.108	0.032	0.089	0.298	0.042	0.089	0.075	0.031
# of valid placebos	45	42	40	41	41	39	40	45	37	42	40
Illinois											
ACT requirement	0.003	0.127 *	-0.167	0.088	-0.099 +	-0.214 +	0.015	-0.068	-0.071	-0.040	-0.073
RMSPE of control	0.032	0.013	0.063	0.024	0.032	0.063	0.040	0.020	0.026	0.023	0.016
# of valid placebos	42	32	39	32	41	38	28	37	32	35	39
Maine											
SAT requirement	-0.102 +	0.005	-0.004	0.074	-0.027	-0.149 +	0.015	0.044	-0.044	0.142 *	0.021
RMSPE of control	0.055	0.041	0.092	0.042	0.049	0.107	0.063	0.071	0.041	0.053	0.035
# of valid placebos	46	43	41	39	44	42	37	46	32	42	41

Note . Estimates of the effect of the given state's testing requirement on the log of total state enrollment at the given college type. Covariate coefficients not reported. Estimation includes outcome data from 1994 to 2009 . Number of valid placebos counts the number of placebo cases where the synthetic match had RMSPE at most five times as high as that of the given case. Indications of statistical significance are based the percent of placebo cases with estimated effects as or more extreme than the given case, as described in the text.

+ $p<0.1$, * $p<0.05$.

Table 5

Estimated Effect of SAT or ACT Requirements on Individual Likelihood of College Enrollment, by State

	Any College	Four-year College	Two-year college	Full-time
Colorado				
ACT requirement	0.101	0.099	0.127 **	0.125 *
	(0.087)	(0.114)	(0.035)	(0.055)
N	12,816	7,131	9,892	17,249
Illinois				
ACT requirement	0.096 *	0.180 **	-0.072	0.191 **
	(0.044)	(0.053)	(0.044)	(0.047)
N	14,809	17,405	12,800	15,838
Maine				
SAT requirement	0.007	0.066	0.017	0.067
	(0.049)	(0.040)	(0.030)	(0.048)
N	6,302	6,517	6,363	5,470

Note. All regressions include controls for individual and state-year covariates as well as state and year fixed-effects. Standard error in parentheses.

+ $p<0.1$, * $p<0.05$, ** $p<0.01$.

Appendix

This appendix provides more technical details of the methods used for both the state-level and individual-level analysis that are described in the main paper. In the state-level analysis, I look at changes in the aggregated college enrollment within a state at colleges of different types. In the individual-level analysis I examine changes in the likelihood that any individual student enrolled in different types of colleges. I discuss the strategy used in each analysis in turn below.

State-Level Analysis

The state-by-state adoption of required college entrance exams allows for a straightforward comparative interrupted-time-series design. In this design I am only looking for a relative change in the level of the outcome trend after the enactment of entrance-exam requirements. Alternatively, this method could be referred to as a non-parametric difference-in-difference design. With this method I compare the trend over time of an outcome in a treated state—one that enacted an entrance-exam requirement—to the trend of the same outcome in non-treated states both before and after treatment.

To determine this difference I employ a model that predicts the log of first-year, first-time student enrollment summed across all colleges of a given type within a state. This model accounts for state and year fixed effects and a vector of various state-by-year controls for factors that may be correlated both with the enactment of college entrance exam requirements and college enrollment trends. This vector includes average state unemployment, state expenditure per public school student, lagged number of high school seniors in the state, high school graduation rate, percent of residents aged 18-19 from different racial groups, the percentage of the state's set of postsecondary institutions that are two- or four-year, or public or private, and average state performance on the ACT or SAT (depending on which the treated state in question required). Specifically, I model:

$$\ln E_{it} = \alpha + \beta T_{it} + X_{it}\varphi + \gamma_i + \delta_t + \varepsilon_{it}$$

Where E_{it} is the college enrollment outcome of state i in year t, X_{it} is the vector of the state-by-year covariates, and $\gamma_i$and δ_t represent state and year fixed effects. Treatment in the estimating equation is represented by the dummy variable T_{it}, which is equal to one for states and years where college entrance exams are required of recent high school graduates. The treatment effect is thus estimated by disentangling the enrollment changes in treated states from general trends in enrollment and idiosyncratic year and state effects. Students who faced entrance exam requirements would have taken the exam in the spring of their junior year, applied to high school in the subsequent fall—at the start of their senior year—and enrolled in college the following fall. Thus, the first students required to take college entrance exams in Colorado or Illinois would have been in a position to enter college in the fall of 2002, while Maine students would have entered starting in 2007. These years represent the first years of treatment in each state.

The use of a comparative interrupted time series to determine the effects of the policy is valid in the absence of random assignment as long as we can assume that the outcome trend of the non-treated states serves as valid comparison for the treated states after policy enactment. The use of state and year fixed effects helps support this assumption by controlling, respectively, for all factors in a given state that are constant over time and all factors unique to a particular year that are not specific to a particular state. I use year fixed effects in particular in order not to impose any particular shape on the time-trend of the outcome variables. In other words, this estimating equation does not require me to assume that the trend in enrollment outcomes was, for example, linear leading up to the enactment of test requirements.

State-by-year covariates.

The state-by-year covariates help to alleviate concerns about remaining variables that might make non-treated states poor comparisons or that might bias my results by being correlated with both the treatment and given outcome. Here we are worried about factors that might change in states at the same time that the test-requirement policies were enacted that would make it appear that changes in enrollment outcomes are the result of the new policies when in fact outcomes are changing along with this other variable.

One set of such covariates controls for state demographics of the college age population. Here we are concerned with changes in both the size and composition of this population. First, I control in each state and year for the number of high school seniors in the state as well as the percent of students who graduate from high school. If one or both of these factors were to change dramatically in the same year the test-requirement went into effect, college enrollment levels might also change in the same direction, but not as a result of the policy. Thus controlling for these variables alleviates this concern. Second, I control for the racial and ethnic composition of the population of college-entry age (18-19 years old) individuals in each state and each year. These controls allow me to account for well documented variation in college attendance rates within different race/ethnicity groups.

A second set of covariates concerns the composition of the population of a state's postsecondary institutions. In other words, I control for the percent of institutions within a state that are four-year schools, private, for-profit, etc. These controls allow me to ensure that changes in enrollment levels at, for example, private four-year colleges are not the result of a sudden increase in the population of private four-year colleges in a particular state.

Third, I control for state-level economic variables. These include the average state expenditure per public school student and the average annual state unemployment rate. If state expenditure-per-student changed in tandem with entrance-exam requirements, then changes in college enrollment may reflect changes in the quality of pre-college education a student received rather than changes due to the exam requirement. Additionally, students may make college enrollment choices based on their likelihood of getting a job with and without a college degree. The ease of getting a job in a given state is reflected in the state's unemployment rate. Thus I control for yearly average state unemployment to remove this concern as a confounding factor.

I also control for average state performance on college entrance exams Here I wanted to control for any changes in the academic ability level of each state's cohort of students—I may attribute apparent changes in college enrollment behavior to test policies if policy enactment coincided with a general statewide change in student test performance. The challenge here is that the composition of students who take an entrance exam changes when that exam is required, so we expect average state exam scores to change along with the policy regardless of any difference in the academic ability of students in that state in that year. Because of this potential problem, I instead control for participation-adjusted state average exam scores. I made this adjustment by predicting the average exam score in each state in each year based on the percentage of students in that state that took the exam. This regression took the basic form

$$S_{it} = \beta_0 + \beta_1 P_{it} + \varepsilon_{it}$$

Here, S_{it} is the average exam score in a given state in a given year, and P_{it} is the percent of high school seniors in that year in that state who took that exam. Thus, ε_{it} is the participation-adjusted exam score in each state and year. It represents the difference between a state's actual average score and what average score is expected given the percent of students who took the exam. For Colorado and Illinois, since their states mandated the ACT, I controlled for participation-adjusted ACT scores. ACT exam scores were converted to the SAT score scale using the College Board's score concordance chart. This made results comparable across testing regimes. Because the adjustment is essentially linear it should not impact the estimation of results other than changing the scale of the coefficients. In Maine, I used SAT scores as Maine's policy required students to take the SAT.

Note that the argument can be made that exam requirements may alter college enrollment patterns by increasing student effort (and thus student scores) on the exams. This turns out not to be the case. In all states with the entrance exam policy, post-policy state performance on the exam was all below what would have been expected given participation rates. Therfore any increases in certain types of college enrollment are in spite of this post-policy underperformance on the entrance exams.

There may be other state-by-year variables that are correlated to college enrollment and that change with the enactment of entrance-exam requirements that I am not able to capture. These variables may bias my results. I believe, however, that these concerns are minimal. Some omitted variables may be specifically related to the entrance exam policies. For example, entrance exam policies may be accompanied by a statewide effort on the part of school counselors to help students use their exam scores to make better college decisions. I consider such variables to be part of the treatment, so I am not concerned that the omission of controls for such variables may be reflected in my estimate of entrance exam requirement effects. To the extent that such ancillary treatment activities may occur in, for example, Colorado and not

Illinois, I may see heterogeneous treatment effects across the three treated states. To accommodate for potentially heterogeneous treatment effects, I ran the analyses separately for each of the three states.

There are, of course, other omitted variables we may not be aware of. These variables give more motivation to finding comparison states that match the pre-treatment trends of treated states. If pre-treatment trends match between treated and controlled states then we know that, at least in these pre-treatment years, these states responded similarly to changes in omitted variables and can assume they are likely to do so in post-treatment years.

Synthetic controls.

As noted above, a second way to ensure the validity of the non-treatment states is to choose the comparison states carefully to match the pre-treatment outcome trend of treated states as closely as possible. Thus the challenge for these analyses, which include only one treated unit each, is to find the best comparison group. The synthetic control method serves exactly this purpose (Abedie, Diamond, & Hainmueller 2007; Abadie & Gardeazabal 2003). This method functions by creating weights for a comparison group of states (the "synthetic control") that matches the treated state on a set of observable, pre-treatment covariates according to the equation:

$$\min_{\mathrm{W} \in \mathbf{R}_{+}^{\mathrm{N}}} \|\mathrm{X}_{\mathrm{T}} - \mathrm{X}_{\mathrm{U}}\mathrm{W}\| \text{ s.t. } \sum_{\mathrm{i}=1}^{\mathrm{N}} \mathrm{W}_{\mathrm{i}} = 1$$

In other words, this method calculates a weighting vector W that minimizes the difference between a set of pre-treatment covariates in the treated unit, X_T, and the N untreated candidate comparison states, X_U. Eligible comparison states included all states in the US except for those who have adopted, or are about to adopt, college entrance exam requirements. Comparison units were matched by all of the state-year covariates listed above, as well as the pre-treatment trend of the given enrollment outcome.

Enrollment outcomes included the sum of all students attending college within a given state at: all degree granting colleges, two- and four-year schools, public or private four-year colleges, schools that require entrance exams for admission, schools that accept only full-time students, and enrollment at schools based on admissions selectivity as defined by the Carnegie Classifications. The Carnegie Foundation classifies schools' selectivity according to the 25th percentile scores of the students they admit. "Inclusive" schools either do not require standardized test scores, or 25 percent of their admitted students scored lower than 18 on the ACT (and 870 on the 1600 point SAT scale). "Selective" schools have a bottom 25 percent scoring between 18 and 21 (between 870 and 990 on the 1600 point SAT scale) and "more selective" schools have a bottom 25 percent scoring above 21 on the ACT (above 990 on the 1600 point SAT scale).

States were matched over all years of available pretreatment data starting in 1988. Because average state exam scores and participation rates are only available from 1994, the regression estimations of policy effects span the years 1994 to 2009.

Figure A1 gives an example of the synthetic match of enrollment at private four-year colleges in Illinois. The solid line represents the trend in log enrollment in Illinois, while the dashed line gives the weighted trend in log enrollment in the "synthetic control" for Illinois.

The quality of a synthetic control match is evaluated by its root mean squared prediction error (RMSPE)—the square root of the mean squared difference between the outcome in treated and synthetic control units in the pre-treatment period. Tables 1 and 2 of the main body of this article give the full description of the match with the median RMSPE across all synthetic matches made for each outcome in each treated state.

It is important to note that synthetic control methods are different from propensity score matching. In propensity score matching, many variables are used to predict the likelihood a given observation receives a given treatment. Observations with similar likelihood of treatment can then be compared to determine the treatment effect. The main goal of synthetic controls, however, is to match pre-treatment trends as closely as possible. This strategy is necessary in cases like the analysis in this article where there are very few observations that receive the treatment. Here, there are not enough observations available to effectively estimate the likelihood of receiving treatment so true propensity score matching is not possible.

Inference.

As a consequence of the small number of treated units in this analysis, the large-sample assumptions needed for traditional methods of statistical inference do not hold. Instead I employ an exact inference test by applying the synthetic control method to all available comparison units as described by Abedie, Diamond, and Hainmueller (2007). This method works by first calculating the effect of a placebo treatment on all non-treated comparison states. In other words, I look at the estimated "effect" of a hypothetical test requirement in each state that did not actually have such a requirement. I produce p-values for the likelihood of a Type-I error by calculating what percentage of these effects are as or more extreme than the effect I estimate for the states that did receive the treatment. Because the quality of the synthetic control matches can vary from state to state, I limit these comparisons to placebo units to those whose synthetic controls have an RMSPE no more than five times as large as that of the given treated unit. In this way there are enough placebos to make comparisons, but poor synthetic matches do not bias the calculation of p-values. The results I present hold if I further limit comparisons to placebos with RMSPE less than twice as large as the RMSPE for the treated unit.

Individual-Level Analysis

I also analyze changes in individuals' likelihood of college enrollment due to the entrance exam requirements. This approach adds a valuable second dimension to the state-level analysis. Like the analysis above, this individual-level analysis can be considered either a comparative interrupted time series (allowing for a change in levels) or a non-parametric difference-in-difference. In this analysis I compared individuals in treated states to those in non-treated states before and after the entrance-exam requirements went into effect. The linear probability model took the form

$$\mathrm{P(Enroll_i)} = \beta_1 \mathrm{T_i} + \mathrm{X_i}\beta_2 + \mathrm{W_{st}}\beta_3 + \gamma_s + \delta_t + \varepsilon_i$$

Here γ_s and δ_t are state and year fixed effects, respectively. X_i is a vector of individual characteristics and W_{st} is a vector of state by year covariates. T_i indicates that individual i was from a treated state after entrance-exam requirements began. Thus β_1 gives the effect of entrance exam policies on the likelihood of college enrollment. I clustered the error term ε_i at the state-year level. In this individual-level analysis I looked specifically at the likelihood a student enrolls in any college, enrolls in a four-year college, enrolls in a two-year college, and whether a student enrolls full-time. Similar to the state-level analyses, the individual-level models were run separately for Colorado, Illinois, and Maine to accommodate potential heterogeneous treatment effects.

Individual and state-by-year controls.

State and year fixed-effects, like in the state-level analysis, control respectively for all factors in a state that affect a student's likelihood of enrolling in college that do not vary over time and all factors in a given year that impact a student's likelihood of enrolling in college that do not vary by state. What is left, again as in the state-level analysis, is to control for other factors that may be correlated both with the enactment of entrance-exam policies and the likelihood of our college enrollment outcomes. In this analysis, any potential confounding omitted variable must affect the average college enrollment likelihood of all students in the given treated state only after the enactment of the entrance-exam requirement. In other words, concern about omitted variable bias in this analysis of a state level policy change comes only from state-by-year variables (Angrist & Pischke 2009). So, in addition to the state and year fixed-effects, I include nearly all of the state-by year covariates described in the state-level analysis. I do not, however, include the variables describing the racial composition of a state's college-age population. I make this omission because I deal with differences in college enrollment rates by race by controlling for the race of individual students.

Other individual covariates serve to improve the precision of my estimates rather than ameliorate omitted variable bias. In addition to race, I include individual controls for gender and whether a student lives in a city.

Comparison units.

Researchers who faced similar analytic challenges with the same data have assumed that individuals from states that border treated states serve as good comparisons (see, for example, Dynarski's (2000) study of the effect of the Georgia HOPE Scholarship on college enrollment rates). This assumption is somewhat arbitrary and synthetic controls allow us to construct a more precise comparison group. Therefore I follow the model of Fitzpatrick (2008) and create analytical weights that are the product of the original CPS sample weights and the synthetic control weights according to the state in which in individual lives. This weighting means that comparison individuals come from states with similar pre-treatment characteristics and outcome trajectories.

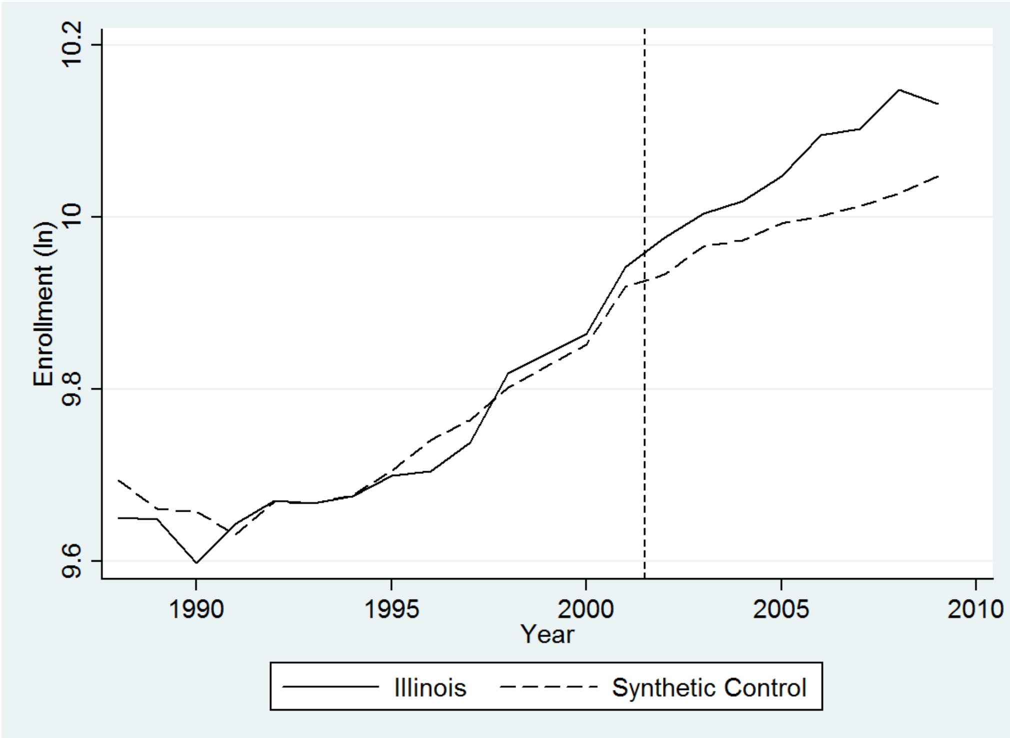

Figure A1. Private four-year college enrollment in Illinois and a synthetic control for Illinois. Students who faced entrance exam requirements in Illinois entered college beginning in 2002, just after the vertical dashed line in the figure.