Learning from the Test: Raising Selective College Enrollment by Providing Information

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Abstract

In the last decade, five U.S. states have adopted mandates forcing high school juniors to take the ACT college entrance exam. Using microdata on ACT test-takers, I demonstrate that, in the two earliest-adopting states (Colorado and Illinois), between one-third and one-half of students were induced into testing by these policy changes, and that 40-45 percent of them, many from disadvantaged backgrounds, earned scores high enough to qualify for competitive-admission schools. Moreover, selective college enrollment rose by about 20 percent following implementation of the mandates in these states, with no effect on overall college enrollment. I argue that this combination of results is incompatible with unbiased decision-making about test participation and instead must reflect a large number of high-ability students who dramatically underestimate their candidacy for selective colleges. The results thus demonstrate that lack of information about one's competitiveness is an important determinant of college outcomes, and that policies aimed at reducing this information shortage are likely to be effective at increasing human capital investment for a substantial number of students.

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I. Introduction

College enrollment has risen substantially over the last 30 years in the United States. But this increase has been uneven: The disparity in college attendance between the bottom- and top-income quartiles has grown (Bailey and Dynarski, 2011).[1] Meanwhile, the importance of educational attainment for subsequent earnings has grown as well. Earnings have been essentially steady among the college-educated and have dropped substantially for everyone else (Deming and Dynarski, 2010).

Not just the level of an individual's education, but also the quality, has been shown to have important consequences for future successes (Hoekstra, 2009; Card and Krueger, 1992). At the college level, attending a higher-quality school significantly increases both an individual's lifetime earnings trajectory (Black and Smith, 2006) as well as the likelihood she graduates (Cohodes and Goodman, 2012).[2]

Disadvantaged students, in particular, appear to gain the most from attending selective colleges and universities (McPherson, 2006; Dale and Krueger, 2011; Dale and Krueger, 2002; Saavedra, 2008)—often cast as the gateways to leadership and intergenerational mobility—but, as a group, they are vastly underrepresented at these institutions. Just one tenth of enrollees at selective schools are from the bottom income quartile (Bowen, Kurzweil, and Tobin, 2005), a larger disparity than can be accounted for by standardized test performance or admission rates (Hill and Winston, 2005; Pallais and Turner, 2006).[3] In addition, these findings rely on admissions test data in which disadvantaged students are also vastly underrepresented; therefore, the shortage of these students at and applying to top schools is probably even larger than conventional estimates suggest.[4] The dearth of disadvantaged students at top schools remains an

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[1] Indeed, over the 20 years between 1980 and 2000, while average college entry rates rose nearly 20 percentage points, the gap in the college entry rate between the bottom- and top-income quartiles increased from 39 to 51 percentage points (Bailey and Dynarski, 2011).

[2] Hoxby (2009) reviews studies of the effects of college selectivity. Most studies show substantial effects. One exception is work by Dale and Krueger (2002, 2011), which finds effects near zero, albeit in a specialized sample. Even in that sample, however, positive effects of selectivity are found for disadvantaged students in particular.

[3] Hill and Winston (2005) find that 16 percent of high-scoring test-takers are low-income. Pallais and Turner (2006) find that high-scoring, low-income test-takers are as much as 15-20 percent less likely to even apply to selective schools than their equally-high-scoring, higher-income counterparts.

[4] Only 30 percent of students in the bottom income quartile elect to take these exams, compared to 70 percent of students in the top; conditional on taking the exam a first time, disadvantaged students retake it less often than other candidates, even though doing so is almost always beneficial (Bowen, Kurzweil, and Tobin, 2005; Clotfelter and Vigdor, 2003).

open and important research question, especially in light of the growing income gap described above.

These trends underscore the importance of education policies that raise postsecondary educational attainment and quality among disadvantaged students. An obvious policy response is improved financial aid. However, financial aid programs alone have not been able to close the educational gap that persists between socioeconomic groups (Kane, 1995). It is thus critically important to understand other factors, amenable to intervention, that may contribute to disparities in postsecondary access and enrollment.

Several such factors have already been identified in previous work. For instance, the complexity of and lack of knowledge about available aid programs might stymie their potential usefulness. One experiment simplified the financial aid application process and increased college enrollment among low- and moderate-income high school seniors and recent graduates by 25-30 percent (Bettinger et al., forthcoming). Another related experiment, seeking to simplify the overall college application process, assisted disadvantaged students in selecting a portfolio of colleges and led to subsequent enrollment increases (Avery and Kane, 2004). Despite its established importance, recent work has found that students are willing to sacrifice college quality for relatively small amounts of money, discounting potential future earnings as much as 94 cents on the dollar (Cohodes and Goodman, 2012). In developing countries, experiments that simply inform students about the benefits of higher education have been effective in raising human capital investment along several dimensions, including: attendance, performance, later enrollment, and completion (Jensen, 2010; Dinkelman and Martínez, 2011); a recent experiment in Canada indicated that low-income students in developed nations might similarly benefit from college information sessions (Oreopoulos and Dunn, 2012). Altogether, it appears that many adolescents are not well-equipped to make sound decisions about their human capital without policy encouragement.

This paper focuses on a related avenue for intervention that has been previously unexplored: the formation of students' beliefs about their own suitability for selective colleges. Providing secondary students with more information about their ability levels might help them develop expectations commensurate with their true abilities and thus could raise educational attainment and quality among some groups of students.

Much research, mostly by psychologists and sociologists, has examined the effect of a student's experiences and the expectations of those around her on the expectations and goals she sets for herself (see Figure 1 in Jacob and Wilder, 2010). Some authors find that students lack the necessary information to form the "right" expectations (that is, in line with their true educational prospects) and to estimate their individual-specific return to investing in higher education (Manski, 2004; Orfield and Paul, 1994; Schneider and Stevenson, 1999). Yet, Jacob and Wilder (2010) demonstrate that students' expectations, inaccurate as they may be, are strongly predictive of later enrollment decisions.

There is reason to believe that providing information to students at critical junctures, such as when they are finalizing their postsecondary enrollment decisions, may help them better align their expectations with their true abilities. Recent research has found that students indeed recalibrate their expectations with new information about their academic ability (Jacob and Wilder, 2010; Stinebrickner and Stinebrickner, 2012; Zafar, 2011; Stange, 2012). In particular, Jacob and Wilder find that high school students' future educational plans fluctuate with the limited new information available in their GPAs.

To shed light on the role of students' perceptions of their own abilities, I exploit recent reforms in several states that required high school students to take college entrance exams necessary for admission to selective colleges. In the last decade, five U.S. states have adopted mandatory ACT testing for their public high school students.[5] The ACT, short for the American College Test, is a nationally standardized test, designed to measure preparedness for higher education, that is widely used in selective college admissions in the United States. It was traditionally taken only by students applying to selective colleges, which consider it in admissions, and this remains the situation in all states without mandatory ACT policies.[6]

One effect of the mandatory ACT policies is to provide information to students about their candidacy for selective schools. Comparisons of tested students, test results, and college enrollment patterns by state before and after mandate adoption therefore offer a convenient quasi-experiment for measuring the impact of providing information to secondary school students about their own ability.

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[5] One state, Maine, has mandated the SAT, an alternative college entrance exam.

[6] Traditionally, selective college bound students in some states take the ACT, while in others the SAT is dominant. Most selective colleges require one test or the other, but nearly every school that requires a test score will accept one from either test. At non-selective colleges, which Kane (1998) finds account for the majority of enrollment, test scores are generally not required or are used only for placement purposes.

Using data on ACT test-takers, I demonstrate that, in each of the two early-adopting states (Colorado and Illinois), between $\frac{1}{3}$ and $\frac{1}{2}$ of high school students are induced to take the ACT test by the mandates I consider. Large shares of the new test-takers – 40-45 percent of the total – earn scores that would make them eligible for competitive-admission schools. Moreover, disproportionately many – of both the new test-takers and the high scorers among them – are from disadvantaged backgrounds.

Next, I develop a model of the test-taking decision, and I use this model to show that with plausible parameter values, any student who both prefers to attend a selective college and thinks she stands a non-trivial chance of admission should take the test whether it is required or not. This makes the large share of new test-takers who score highly a puzzle, unless nearly all are uninterested in attending selective schools.

Unfortunately, I do not have a direct measure of preferences. However, I can examine realized outcomes. In the primary empirical analysis of the paper, I use a difference-in-differences analysis to examine the effect of the mandates on college enrollment outcomes. I show that mandates cause substantial increases in selective college enrollment, with no effect on overall enrollment (which is dominated by unselective schools; see Kane, 1998). Enrollment of students from mandate states in selective colleges rises by 10-20 percent (depending on the precise selectivity measure) relative to control states in the years following the mandate. My results imply that about 20 percent of the new high scorers wind up enrolling in selective colleges. This is inconsistent with the hypothesis that lack of interest explains the low test participation rates of students who could earn high scores, and indicates that many students would like to attend competitive colleges but choose not to take the test out of an *incorrect* belief that they cannot score highly enough to gain admission.

Therefore, this paper answers two important, policy-relevant questions. The first is the simple question of whether mandates affect college enrollment outcomes. The answer to this is clearly yes. Second, what explains this effect? My results indicate that a significant fraction of secondary school students dramatically underestimate their candidacy for selective colleges. This is the first clear evidence of a causal link between secondary students' perceptions of their own ability and their postsecondary educational choices, or of a policy that can successfully exploit this link to improve decision-making. Relative to many existing policies with similar aims, this policy is highly cost-effective.[7]

The rest of the paper proceeds as follows. Section II provides background on the ACT and the ACT mandates. Section III describes the ACT microdata that I use to examine the characteristics of mandate compliers, and Section IV presents results. Section V provides a model of information and test participation decisions. Section VI presents estimates of the enrollment effects of the mandates. Section VII uses the empirical results to calibrate the participation model and demonstrates that the former can be explained only if many students have biased predictions of their own admissibility for selective schools. Section VIII synthesizes the results and discusses their implications for future policy.

II. ACT Mandates

In this Section, I describe the ACT mandates that are the source of my identification strategy. I demonstrate that these mandates are almost perfectly binding: test participation rates increase sharply following the introduction of a mandate.

The ACT is a standardized national test for high school achievement and college admissions. It was first administered in 1959 and contains four main sections – English, Math, Reading, and Science – along with (since 2005) an optional Writing section. Students receive scores between 1 and 36 on each section as well as a composite score formed by averaging scores from the four main sections. The ACT competes with an alternative assessment, the SAT, in a fairly stable geographically-differentiated duopoly.[8] The ACT has traditionally been more popular in the South and Midwest, and the SAT on the coasts. However, every four-year college and university in the United States that requires such a test will now accept either.[9]

The ACT is generally taken by students in the 11th and 12th grades, and is offered several times throughout the year. The testing fee is about \$50 and includes the fee for sending score

[7] For example, Dynarski (2003) calculates that it costs \$1,000 in grant aid to increase the probability of attending college by 3.6 percentage points.
[8] The ACT was designed as a test of *scholastic* achievement, and the SAT as a test of *innate* aptitude. However, both have evolved over time and this distinction is less clear than in the past. Still, the SAT continues to cover a smaller range of topics, with no Science section in the main SAT I exam.
[9] Some students might favor one test over the other due to their different testing formats and/or treatment of incorrect responses.

reports to four colleges.[10] The scores supplement the student's secondary school record in college admissions, helping to benchmark locally-normed performance measures like the grade point average. According to a recent ACT Annual Institutional Data Questionnaire, 81 percent of colleges require or use the ACT and/or the SAT in admissions.

Even so, many students attend noncompetitive schools with open admissions policies. According to recent statistics published by the Carnegie Foundation, nearly 40 percent of all students who attend postsecondary school are enrolled in two-year associate's-degree-granting programs. Moreover, according to the same data, over 20 percent of students enrolled full-time at four-year institutions attend schools that either did not report test score data or that report scores indicating they enroll a wide range of students with respect to academic preparation and achievement. Altogether, 55 percent of students enrolled in either two-year or full-time four year institutions attend noncompetitive schools and likely need not have taken the ACT or the SAT for admission.

Since 2000, five states (Colorado, Illinois, Kentucky, Michigan, and Tennessee) have begun requiring all public high school students to take the ACT. [11] There are two primary motivations for these policies. The first relates to the 2001 amendment of the Federal Elementary and Secondary Education Act (ESEA) of 1965, popularly referred to as No Child Left Behind (NCLB). With NCLB, there has been considerable national pressure on states to adopt statewide accountability measures for their public schools. The Act formally requires states to develop assessments in basic skills to be given to all students in particular grades, if those states are to receive federal funding for schools. Specific provisions mandate several rounds of assessment in math, reading, and science proficiency, one of which must occur in grade 10, 11, or 12. Since the ACT is a nationally-recognized assessment tool, includes all the requisite material (unlike the SAT), and tests proficiency at the high school level, states can elect to outsource their NCLB accountability testing to the ACT, and thereby avoid a large cost of developing their own metric.[12]

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[10] The cost is only $35 if the Writing section is omitted. Additional score reports are around $10 per school for either test.

[11] In addition, one state (Maine) mandates the SAT.

[12] ACT, Inc. administers several other tests that can be used together with the ACT to track progress toward "college readiness" among its test-takers (and satisfy additional criteria of NCLB). Recently, the College Board has developed an analogous battery of assessments to be used in conjunction with the SAT.

The second motivation for mandating the ACT relates to the increasingly-popular belief that all high school graduates should be "college ready." In an environment where this view dominates, a college entrance exam serves as a natural requirement for high school graduation.

Table 1 displays a full list of the ACT mandates and the testing programs of which they are a part. Of the five, Colorado and Illinois were the earliest adopters: both states have been administering the ACT to all public school students in the 11th grade since 2001, and thereby first required the exam for the 2002 graduating cohort.[13] Kentucky, Michigan, and Tennessee each adopted mandates more than five years later.

Figure 1 presents initial graphical evidence that ACT mandates have large impacts on test participation. It shows average ACT participation rates by graduation year for mandate states, divided into two groups by the timing of their adoption, and for the 20 other "ACT states"[14] for even numbered years 1994-2010. State-level participation rates reflect the fraction of high school students (public and private) projected to graduate in a given year who take the ACT test within the three academic years prior to graduation, and are published by ACT, Inc.

Prior to the mandate, the three groups of states had similar levels and trends in ACT-taking. The slow upward trend in participation continued through 2010 in the states that never adopted mandates, with average test-taking among graduates rising gradually from 65 percent to just over 70 percent over the last 16 years. By contrast, in the early adopting states participation jumped enormously (from 68 to approximately 100 percent) in 2002, immediately after the mandates were introduced. The later-adopting states had a slow upward trend in participation through 2006, then saw their participation rates jump by over 20 percentage points over the next four years as their mandates were introduced. Altogether, this picture is strongly suggestive that the mandate programs had large effects on ACT participation, that compliance with the mandates is near universal, and that in the absence of mandates, participation rates are fairly stable and have been comparable in level and trend between mandate and non-mandate states.

Due to data availability, the majority of the empirical analysis in this paper focuses on the two early adopters. However, I briefly extend the analysis to estimate short-term enrollment

[13] In practice, states can adapt a testing format and process separate from the national administration, but the content and use of the ACT test remains true to the national test. For instance, in Colorado, the mandatory test, more commonly known as the Colorado ACT (CO ACT), is administered only once in April and once in May to 11th graders. The state website notes that the CO ACT is equivalent to all other ACT assessments administered on national test dates throughout the country and can be submitted for college entry.

[14] These are the states in which the ACT (rather than the SAT) is the dominant test. See Figures 1a and 1b in Clark, Rothstein, and Schanzenbach (2009) for the full list.

effects within the other ACT mandate states, and contextualize them using the longer-term findings from Colorado and Illinois.

III. Test-taker Data

In this section, I describe the data on test-takers that I will use to identify mandate-induced test-taking increases and outcomes. I present key summary statistics demonstrating that the test-takers drawn in by the mandates were disproportionately minority and lower income relative to pre-mandate test-takers. I then investigate shifts in the score distribution following the introduction of the mandate. Adjusting for cohort size, I show that a substantial portion of the new mass in the post-mandate distributions is above a threshold commonly used in college admissions, suggesting that many of the new students obtained ACT scores high enough to qualify them for admission to competitive colleges.

My primary data come from microdata samples of ACT test-takers who graduated in 1994, 1996, 1998, 2000, and 2004, matched to the public high schools that they attended.[15] The dataset includes a 50-percent sample of non-white students and a 25-percent sample of white students who took the ACT exam each year.

Each student-observation in the ACT dataset includes several scores measuring the student's performance on the exam. In my analysis, I focus on the ACT "composite" score, which is an integer value ranging between 1 and 36 reflecting the average of the four main tested subjects. The composite score is the metric most relied upon in the college admissions process. Observations also include an array of survey questions that the student answered before the exam that provide an overview of the test-taker's current enrollment status, socioeconomic status, other demographics, and high school. My analysis omits any test-takers missing composite scores or indicating, when asked for their prospective college enrollment date on the survey response form, that they are currently enrolled.

The ACT microdata contain high school identifiers that, for most test-takers, can be linked to records from the Common Core of Data (CCD), an annual census of public schools. The CCD is useful in quantifying the size and minority share of each school's student body. I use one-year-earlier CCD data describing the 11th grade class as the population at risk of test-taking.

[15] I am grateful to ACT, Inc. for providing the extract of ACT microdata used in this analysis.

I drop any test-taker whose observation cannot be matched to a school in the CCD sample, so that my final sample is comprised of successful ACT-CCD matches.[16, 17]

The student-level analyses rely on neighboring ACT states to generate a composite counterfactual for the experiences of test-takers from the two early-adopting states.[18] In comparison to one formed from all of the ACT states, a counterfactual test-taker constructed from surrounding states is likely to be more demographically and environmentally similar to the marginal test-taker in a mandate state. This is important because these characteristics cannot be fully accounted for in the data but could be linked to particular experiences, such as the likelihood she attends public school (and thus is exposed to the mandate) or her ambitiousness. Therefore, except where otherwise noted, the sample in the remainder of this and the next section is restricted to public school test-takers from each of the two early-adopting states and their ACT-state neighbors. Appendix Figure 1 reproduces Figure 1 for the matched ACT-CCD sample and demonstrates both that matched-sample ACT participation rates track closely those reported for the full population by ACT and that average ACT participation rates in the neighboring states track those in the mandate states as closely as a composite formed from the other ACT states.[19]

Table 2 presents average test-taker characteristics in the matched sample. Means are calculated separately for the two treated states and their corresponding set of neighbors over the years before and after treatment. Note that the sample sizes in each treatment state reflect a substantial jump in test-taking consistent with the timing of the mandates. The number of test-takers more than doubled from the pre-treatment average in Colorado, and increased about 80 percent in Illinois; in each case the neighboring states saw growth of less than 10 percent.

Predictably, forcing all students to take the test lowers the average score: both treatment states experienced about 1½-point drops in their mean ACT composite scores after their respective mandates took effect. Similarly, the mean parental income is lower among the post-

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[16] A fraction of students in the ACT data are missing high school codes so cannot be matched to the CCD. Missing codes are more common in pre-mandate than in post-mandate data, particularly for low-achieving students. This may lead me to understate the test score distribution for mandate compliers.

[17] My matching method also drops school-years for which there are no students taking the ACT. For consistency, I include only school-years that match to tested students in counts and decompositions of the "at-risk" student population, such as constructed participation rates.

[18] The neighboring states include all states that share a border with either of the treatment states, excluding Indiana which is an SAT state: Wisconsin, Kentucky, Missouri, and Iowa for Illinois; Kansas, Nebraska, Wyoming, Utah, and New Mexico for Colorado.

[19] The figure demonstrates that public school students tend to have a slightly lower participation rate than public and private school students together in the published data, but that trends are similar for the two populations.

treatment test-takers than among those who voluntarily test.[20] Post-treatment, the test-taker population exhibits a more-equal gender and minority balance than the group of students who opt into testing on their own.[21] This is also unsurprising, since more female and white students tend to pursue postsecondary education, especially at selective schools. Finally, the post-treatment test-takers more often tend to be enrolled in high-minority high schools.[22]

The differences between the pre-treatment averages in the treatment states and the averages in neighbor states suggest that there are differences in test participation rates by state, differences in the underlying distribution of graduates by state, or differences brought on by a combination of the two. In particular, both Colorado and Illinois have higher minority shares and slightly higher relative income among voluntary test-takers than do their neighbors. However, the striking stability in test-taker characteristics in untreated states (other than slight increases in share minority and share from a high-minority high school) over the period in which the mandates were enacted lend confidence that the abrupt changes observed in the treatment states do in fact result from the treatment.

I next plot the score frequencies for the two treated states and their corresponding set of neighbors over the data years before and after treatment (Figure 2). In order to better display the growth in the test-taking rate over time, I do not scale frequencies to sum to one. To abstract from changes in cohort size over time, I rescale the pre-treatment score cells by the ratio of the total CCD enrollment in the earlier period to that in the later period.

Although U.S. college admissions decisions are multidimensional and typically not governed by strict test-score cutoffs, ACT Inc. publishes benchmarks to help test-takers broadly gauge the competitiveness of their scores. According to their rubric, 18 is the lower-bound composite score necessary for application to a "liberal" admissions school, 20 for "traditional",

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[20] The ACT survey asks students to estimate their parents' pretax income according to up to 9 broad income categories, which vary across years. For instance, in 2004, the lowest parental income a student can select is "less than $18,000," and the highest is "greater than $100,000," but in 1994, the top and bottom thresholds are $60,000 and $6,000, respectively. To make a comparable measure over time, I recode each student's selection as the midpoint of the provided ranges (or the ceiling and floor of the categories noted above, respectively), and calculate income quantiles for each year. In the end, each reporting student is associated with a particular income quantile that reflects her relative SES among all test-takers in all years.

[21] The minority measure consolidates information from survey questions on race/ethnicity, taking on a value of 1 if a student selects a racial or ethnic background other than "white" (these categories include Hispanic, "multirace," and "other"), and 0 if a test-taker selects white. This variable excludes non responses and those who selected "I prefer not to respond."

[22] High-minority schools are defined as those in which minorities represent more than 25 percent of total enrollment.

and 22 for “selective.”[23] The plots include a vertical line at a composite score of 18, reflecting the threshold for a liberal admissions school.

Some interesting patterns emerge between the years prior to the mandates and 2004. In both of the treatment states, the change in characteristics presented in the last section appeared to shift the testing distribution to the left. Moreover, the distributions, particularly in Colorado, broadened with the influx of new test-takers. In the neighboring states, however, where average test-taker characteristics were mostly unchanged, there were no such shifts.

New test-takers tended to earn lower scores, on average, than did pre-mandate test-takers, but there is substantial overlap in the distributions. Thus, we see both a decline in mean scores following the mandates and a considerable increase in the number of students scoring above 18. For example, the number of students scoring between 18 and 20 (inclusive) grew by 60 percent in Colorado and 55 percent in Illinois, even after adjusting for changes in the size of the graduating class; at scores above 23, the growth rates were 40 percent and 25 percent, respectively.

IV. The Effect of the Mandates on the ACT Score Distribution

There were (small) changes in score distributions in non-mandate states as well as in mandate states. In this section, I describe how a difference-in-differences strategy can be used to identify the effect of the mandates on the number of students scoring at each level, net of any trends common to mandate and non-mandate states.

Conceptually, students can be separated into two groups: those who will take the ACT whether or not they are required, and those who will take it only if subject to a mandate. Following the program evaluation literature, I refer to these students as “always-takers” and

[23] According to definitions from the ACT, Inc. website, a “liberal” admissions school accepts freshmen in the lower half of high school graduating class (ACT: 18-21); a “traditional” admissions school accepts freshmen in the top 50 percent of high school graduating class (ACT: 20-23); and a “selective” admissions school tends to accept freshmen in top 25 percent of high school graduating class (ACT: 22-27). (See http://www.act.org/newsroom/releases/view.php?year=2010&p=734&lang=english.) For reference, according to a recent concordance between the two exams (The College Board, 2009), an 18 composite ACT score corresponds roughly to an 870 combined critical reading and math SAT score (out of 1600); a 20 ACT to a 950 SAT, and a 22 ACT to a 1030 SAT.

"compliers", respectively (Angrist, Imbens, and Rubin, 1996).[24] My goal is to identify the complier score distribution.

Because my data pertain to test-takers, compliers are not present in non-mandate state-year cells. In mandate cells, by contrast, they are present but are not directly identifiable. Therefore, characterizing the complier group requires explicit assumptions about the evolution of characteristics of the "at-risk" population that I cannot observe (i.e. 11th grade students planning to graduate high school). I begin by establishing some useful notation:

- Number of test-takers by state and year: A_{st}
- Number of students at risk of taking the test (e.g., 11th graders in the CCD), by state and year: N_{st}
- Test-takers that earn a particular score, r, by state and year: A_{rst}

For simplicity, assume that there are just two states, $s = 0$ and $s = 1$, two time periods, $t = 0$ and $t = 1$, and two scores, $r = 0$ and $r = 1$. Define differencing operators, such that for any variable X:

- $D_t(X_s) = X_{s1} - X_{s0}$
- $D_s(X_t) = X_{1t} - X_{0t}$
- $DD(X_{st}) = D_t(D_s(X_t)) = D_s(D_t(X_s)) = (X_{11} - X_{10}) - (X_{01} - X_{00})$

Finally, let superscript "AT" denote always-takers and "C" denote compliers.

A. Estimating the Fraction of Compliers

The first step is to identify the number of compliers. My key assumption is that absent the policy the (voluntary) test-taking rate, $P_{st} \equiv \frac{A_{st}^{AT}}{N_{st}}$, would have evolved in s=1 the same way it did in s=0. In words, the likelihood that a randomly-selected student elects to take the exam would have increased by the same amount over the sample period, regardless of state lines. The counterfactual can be written $DD(P_{st}) = 0$. This permits me to identify the size of the complier

[24] In theory, there might be other students who do not take the exam when a mandate is in place (i.e., "never takers"), but Figure 1 shows that this group is negligible. All of the analysis below holds in the presence of never-takers, so long as there are no defiers who take the test without mandates but not with a mandate.

group (as a share of total enrollment N_{st}) via a difference-in-differences analysis of test participation rates. To see this, begin with writing out the assumption:

$$DD(P_{st}) \equiv (P_{11} - P_{10}) - (P_{01} - P_{00}) = 0$$

Rearranging the above expression yields an estimate for P_{11}:

$$P_{11} = (P_{01} - P_{00}) + P_{10}$$

Note that in the treated states I do not observe P_{11} but rather: $\frac{A_{11}}{N_{11}} \equiv \frac{A_{11}^{AT}}{N_{11}} + \frac{A_{11}^{C}}{N_{11}}$

Substituting and rearranging so that known and estimable quantities appear on the right hand side gives: $\frac{A_{11}^{C}}{N_{11}} = \frac{A_{11}}{N_{11}} - [(P_{01} - P_{00}) + P_{10}]$.

From this, it is straightforward to recover the number of compliers and always-takers.

Thus, the mandates' average effects on test-taking – i.e. the share of students induced to take the exam by the mandates – can be estimated with an equation of the form:

$$\widetilde{P_{st}} = \beta_0 + \beta_1 \times (treatment_s \times post_t) + \beta_2 \times post_t + \beta_3 \times treatment_s + \varepsilon_{st} \quad (1)$$

where $\widetilde{P_{st}}$ is observed test participation in a given state-year, and β_1 is the parameter of interest, representing the complier share $\frac{A_{11}^{C}}{N_{11}}$.[25] I estimate (1) separately for each of the two early-adopting states and their neighbors, using the five years of matched microdata described earlier. Note that the specification relies on the relevant neighboring states and the years prior to 2004 to serve as control state and period composites.

Table 3 summarizes the results. About 45 percent of 11th graders in Colorado are "compliers"; about 39 percent in Illinois. From these estimates, I decompose the number of test-takers into compliers and always takers (bottom panel of Table 3). These counts are necessary for the computations that follow.

B. Estimating the Fraction of High-Scoring Compliers

It is somewhat more complex to identify the score distribution of compliers. My estimator relies on the fact that the fraction of all test-takers scoring at any value can be written as a weighted

[25] Note that an alternative specification of equation (1) is available: $\ln(A_{st}) = \beta_0 + \beta_1 \times (treatment_{st} \times post_{st}) + \beta_2 \times post_{st} + \beta_3 \times treatment_{st} + \ln(N_{st}) + \varepsilon_{st}$, which implies a more flexible but still proportional relationship between the number of test-takers and the number of students. While I prefer the specification presented in the text for ease of interpretation, both approaches yield similar results.

average of compliers and always-takers scoring at that value, where the weights reflect the shares of each group in the population estimated in the last section.

I need an additional assumption that in the absence of a mandate, score distributions would have evolved similarly in treatment and comparison states. Formally, I assume that: $DD(S_{rst}) = 0$, where $S_{rst} \equiv \frac{A_{rst}^{AT}}{A_{st}^{AT}}$.[26] In words, this means that, absent the mandate, the likelihood that a randomly-selected always-taker earns a score of r would increase (or decrease) by the same amount in both states 0 and 1 over time.

With this additional assumption, I can fully recover the share and number of compliers earning score r. To see this, begin with writing out the assumption:

$$DD(S_{rst}) \equiv (S_{r11} - S_{r10}) - (S_{r01} - S_{r00}) = 0$$

Rearranging the above expression yields:

$$S_{r11} = (S_{r01} - S_{r00}) + S_{r10}$$

So, similar to voluntary test participation, the share of always-takers scoring at r can be computed directly from the share of test-takers scoring at r in all other treatment-periods.

Note that I do not observe S_{r11} when the mandate is in place, and instead observe:

$$\frac{A_{r11}}{A_{11}} \equiv \left(\frac{A_{11}^{AT}}{A_{11}}\right)\left(\frac{A_{r11}^{AT}}{A_{11}^{AT}}\right) + \left(\frac{A_{11}^{C}}{A_{11}}\right)\left(\frac{A_{r11}^{C}}{A_{11}^{C}}\right) \equiv \left(\frac{A_{11}^{AT}}{A_{11}}\right)(S_{r11}) + \left(\frac{A_{11}^{C}}{A_{11}}\right)\left(\frac{A_{r11}^{C}}{A_{11}^{C}}\right).$$

In words, the fraction of test-takers scoring at r is the weighted average of the fraction of always-takers at r and compliers at r, where the weights are the always-taker and complier shares of the population.

From the evaluation of equation (1), I have estimates for these weights. In addition, I have an estimate for S_{r11} from above. Therefore, I can rearrange the above expression so that known and estimable quantities appear on the right hand side and recover:

$$\frac{A_{r11}^{C}}{A_{11}^{C}} = \left(\frac{\frac{A_{r11}}{A_{11}} - \left(\frac{A_{11}^{AT}}{A_{11}}\right)[(S_{r01} - S_{r00}) + S_{r10}]}{\frac{A_{11}^{C}}{A_{11}}}\right),$$

where the estimated number of compliers at r is:

$$A_{r11}^{C} = (S_{r11}^{C})(A_{11}^{C}).$$

[26] Note that a counterfactual of $DD\left(\frac{A_{rst}^{AT}}{N_{rst}}\right) = 0$ (i.e., the likelihood that a candidate test-taker potentially scoring at r actually takes the test evolves similarly across states), which is more analogous to the previous method, is unavailable since I never observe N_{rst}. Alternatively, I could rely on a counterfactual of $DD(A_{rst}^{AT}) = 0$ for overall test-taking, but it is less plausible.

Following this procedure, I can identify the full score distribution for compliers.

An advantage of this approach is that it does not require knowing the underlying scoring potential of the at-risk population in order to decompose the set of compliers according to their scores. A disadvantage is that it poses stringent requirements on the relationship between the at-risk populations and their corresponding test-taking rates.[27] Without these, at least some of the differential changes in the score distribution among test-takers might in fact have been driven by shifts in the student population. These additional constraints underscore the importance of a comparison population that exhibits similar traits (both demographically and educationally) to the exposed population.[28] In Appendix A, I examine the plausibility of this assumption by comparing the test-taker composition to observable characteristics of 11th graders in the matched CCD schools.

I apply the above method to estimate the share of compliers within each ACT score cell. Figures 3a and 3b plot the 2004 score distributions among compliers and always-takers in each mandate state. Evidently, compliers represent a measurable fraction of test-takers at nearly every score. While the complier distribution is predictably more left skewed than the always-taker distribution—particularly so in Illinois—a substantial number of compliers achieve scores at values above the conventional thresholds used for college admissions.

To that point, Table 4 summarizes the information from the figures according to test-takers scoring below 17, between18 and 20, between 21 and 24, and above 25. The estimates suggest that, while a majority of compliers earned low scores (more than twice as often as their always-taker counterparts from earlier years), many still scored within each of the selective scoring ranges (column 3). As a consequence, a substantial portion of the high scorers in mandate states came from the induced group (column 5). Altogether, I estimate that around 40-45 percent of compliers – amounting to about 20 percent of all post-mandate test-takers – earned scores surpassing conventional thresholds for admission to a competitive college.

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[27] For example, assume $P_{rst} = P_{st}$ and $DD\left(\frac{N_{rst}}{N_{st}}\right) = 0$ – i.e., the participation rate among potentially-r-scoring students within a state-year matches that of that state-year's overall participation rate, and there are no differential changes between the treatment and control state in the potentially-r-scoring fraction of students—so that the potentially-r-scoring share of the population equals the r-scoring share of always-takers. Then, any observed changes in the score distribution among test-takers can be attributed to the policy change (rather than changes in the underlying population).

[28] For this reason, I restrict the complier analysis to only the treatment states and their neighboring ACT states.

Appendix B shows how I can link the above methodology to test-taker characteristics to estimate complier shares in subgroups of interest (such as, e.g., high-scoring minority students). I demonstrate that in both treatment states, compliers tend to come from poorer families and high-minority high schools, and are more often males and minorities, than those who opt into testing voluntarily.

Table 5 summarizes the other key results. Generally, a majority of students from disadvantaged backgrounds are compliers with the mandates – that is, they would not have taken the exam in the absence of the mandate.[29] Turning to the scoring distribution, we see that a substantial portion of compliers in every subgroup wind up with competitive scores. Compliers account for around 40 percent of high-scoring students from high-minority high schools as well as low-income and minority students overall, while they comprise around 30 percent of high-scorers from other student groups. Thus, even conditioning on scoring ability, students from disadvantaged backgrounds are more likely to be compliers—i.e. less likely to take the ACT voluntarily—than are other students. Finally, in the Appendix, I also calculate the share of high-scoring compliers (and always-takers) with particular characteristics. High-scoring compliers are disproportionately likely to be from disadvantaged backgrounds, relative to students with similar scores who take the test voluntarily.

Altogether, these test-taking and -scoring patterns are consistent with previous literature that finds these same groups are vastly underrepresented at selective colleges, suggesting that as early as high school, students from these groups do not aspire to attend selective colleges at the same rate as other students.

The rest of my paper asks why there are so many high-scoring students in the complier group, when one might think that students with the potential to score so highly would have taken the test even without a mandate. In particular, I investigate whether those who did not voluntarily take the test simply had no interest in attending a selective college, or whether a substantial number of compliers were interested in attending a selective school but underestimated their ability to qualify. I show that mandates led to large increases in enrollment at selective colleges. I then argue that this is consistent only with substantial underestimation among many students of

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[29] This is a true majority for all three categories that proxy for disadvantage in Colorado. In Illinois, however, just below half of the minority students and students from high-minority high schools would not take the test, absent the mandate. A majority of low-income students in both states are compliers.

their potential exam performance, leading them to opt out of the exam when they would have opted in had they had unbiased estimates.

V. The Test-taking Decision

In this section, I model the test-taking decision a student faces in the non-mandate state. I assume that all students are rational and fully-informed. Such a student will take the exam if the benefits of doing so exceed the costs.

The primary benefit of taking the exam is potential admission to a selective college, if the resulting score is high enough. The test-taking decision faced by a student in a non-mandate state can be fully characterized by:

$$take\ the\ exam\ iff\ P \times \max\{0, U_S - U_U\} > T, (2)$$

where T is the cost of taking the exam; U_S and U_U represent utility values accrued to the student from attending a selective or unselective school, respectively[30]; and P is the (subjective) probability that the student will "pass" – earn a high-enough score to qualify her for a selective school – if she takes the exam.[31] Note that this condition can be rewritten as:

$$take\ the\ exam\ iff\ P > \frac{T}{U_S - U_U}\ and\ U_S - U_U > 0. (2^*)$$

The expression captures several important dimensions of the testing decision. A student who prefers to attend the unselective school — for whom $U_S - U_U \leq 0$ — will not take the exam regardless of the values of T and P. A student who prefers the selective school — for whom $U_S - U_U > 0$ — will take the exam only if she judges her probability of passing to be sufficiently large, $P > \frac{T}{U_S - U_U}$. Finally, note the relevant P is not the objective estimate of a student's chance of earning a high score. The objective estimate, which I denote P^*, governs the optimal test-taking decision but might not be a particularly useful guide to the student's actual decision. Rather, the student forms her own subjective expectation and decides whether to take the exam on that basis. Thus, under P, a high-ability student might choose not to take the exam because she underestimates her own ability and judges her probability of passing to be small. If

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[30] The descriptive model abstracts away from the difference between attending an unselective college and no college at all.

[31] I assume that the probability of admission is zero for a student who does not take the exam; if this is incorrect, I could instead simply redefine P to be the increment to this probability obtained by taking the exam.

students are rational in their self-assessments, $E[P^*|P] = P$, in which case there should be no evidence that such underestimation is systematically occurring.

This framework allows me to enumerate two exhaustive and mutually exclusive subcategories of mandate compliers. There are those who abstain from the exam in the non-mandate state because they simply prefer the unselective college to the selective college, and there are those who abstain from the exam even though they prefer the selective college, because they judge $P < \frac{T}{U_S - U_U}$.[32] I refer to the former as the "not interested" (NI) compliers and the latter as the "low expectations" (LE) compliers.

The LE group is of particular interest here because if these students have incorrectly low expectations, then a mandate may lead substantial numbers of them to enroll in selective schools. It is thus useful to attempt to bound the ratio $\frac{T}{U_S - U_U}$. I sketch out an estimate here, and provide more details in Appendix C.

I begin with the test-taking cost, T. There are two components to this cost: the direct cost of taking the test – around $50 – and the time cost of sitting for an exam that lasts about 4 hours. A wage rate of $25 would be quite high for a high school student. I thus assume T is unlikely to be larger than $150.

It is more challenging to estimate $U_S - U_U$. Given the magnitudes of the numbers involved in this calculation – with returns to college attendance in the millions of dollars – it would be quite unlikely for the choice between a selective and an unselective college to be a knife-edge decision for many students. I rely on findings from the literature on the return to college quality to approximate the difference between the return to attending a selective and a non-selective school. In the most relevant study for this analysis, Black and Smith (2006) estimate that the average treatment-on-the-treated effect of attending a selective college on subsequent earnings is 4.2 percent.[33] In my case, this implies that $U_S - U_U$ will average around $80,000.

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[32] In reality, a handful of students might indeed prefer the selective college, but plan to take only the SAT exam. In my setup, these students are part of the "NI" complier group, since they would not have taken the ACT without a mandate and, outside of measurement error between the two tests, their performance on the ACT will not affect their enrollment outcomes.

[33] I follow Cohodes and Goodman (2012) in my reliance on the Black and Smith (2006) result due to their broad sample and rigorous estimation strategy. Dale and Krueger (2011), studying a narrower sample, find a much smaller effect.

Combining these estimates, the ratio of $\frac{T}{U_S - U_U}$ is likely to be on the order of 0.0019 for a large share of students for whom $U_S > U_U$. In the appendix, I present a second, highly conservative calculation that instead estimates $\frac{T}{U_S - U_U}$ at around 0.03, so that students opt not to take the test unless $P > 0.03$. Then the average subjective passage rate among low-expectations compliers must be below 0.03 ($E[P|LE] = E[P|P < 0.03] < 0.03$), most likely substantially so.

As noted above, this framework does not incorporate the decision of whether to attend college at all, which is a complex function of individual-specific returns to college attendance and the opportunity cost of college each student faces. Note that the ability to attend college does not depend on test scores, as the majority of American college students attend open-enrollment colleges that do not require test scores. Further, it is unclear whether the return to college is an increasing, decreasing, or non-monotonic function of test scores. While it is possible that students use the score as information about whether they can succeed at a non-competitive college (Stange, 2012)—in which case it could affect their choice to attend a non-competitive college rather than no college—my empirical evidence will not support this possibility. Thus, the information contained in a student's ACT score is expected to have little influence on her ability to enroll in college and has no clear effect on her interest in doing so. By contrast, conditional on attending college, it is clear that returns are higher to attending a selective college, and acceptance at a selective college is a function of test scores.

In the previous section (Section IV), I explored the change in the test score distribution surrounding the implementation of the mandate. The results indicate that about 40-45 percent of compliers attained high-enough scores to qualify them for admission to selective schools, or that $E[P^*|C] \geq 0.40$. In the next section (Section VI), I will investigate the effect of the mandates on selective college enrollment, which will identify the share of compliers who both score highly and are interested in attending a selective college. In Section VII, I use these two results to place a lower bound on $E[P^*|LE]$ and shed light on whether these compliers' low expectations are indeed rationally-formed.

VI. The Effects of the Mandates on College Enrollment

A. Enrollment Data Description

The test-taker data discussed above are collected at the time of the test administration, and do not describe where students ultimately matriculate. Thus, to study enrollment effects I turn to an alternative data set, the Integrated Postsecondary Education Data System (IPEDS).[34] IPEDS surveys are completed annually by each of the more than 7,500 colleges, universities, and technical and vocational institutions that participate in the federal student financial aid programs. I use data on first-time, first-year enrollment of degree- or certificate-seeking students enrolled in degree or vocational programs, disaggregated by state of residence, which are reported by each institution in even years.[35] The number of reporting institutions varies over time. To obtain the broadest snapshot of enrollment at any given time, I compile enrollment statistics for the full sample of institutions reporting in any covered year.[36]

I merge the IPEDS data to classifications of schools into nine selectivity categories from the Barron's "College Admissions Selector."[37] A detailed description of the Barron's selectivity categories can be found in Appendix Table 5. Designations range from noncompetitive, where nearly 100 percent of an institution's applicants are granted admission and ACT scores are often not required, to most competitive, where less than one third of applicants are accepted. Matriculates at "competitive" institutions tend to have ACT scores around 24, while those at "less competitive" schools (the category just above "noncompetitive") generally have scores below 21. I create six summary enrollment measures, corresponding to increasing degrees of selectivity, in order from most to least inclusive: overall (any institution, including those not ranked by Barron's), selective ("less competitive" institutions and above), more selective ("competitive" institutions and above), very selective ("very competitive" institutions and above), highly selective ("highly competitive" institutions and above), and most selective ("most

[34] Data were most recently accessed June 11, 2012.

[35] IPEDS also releases counts for the number of first-time first-year enrollees that have graduated high school or obtained an equivalent degree in the last 12 months, but these are less complete.

[36] The number of reporting institutions grows from 3,166 in 1994 to 6,597 in 2010. My analysis will primarily focus on the 1,262 competitive institutions in my sample, of which 99 percent or more report every year, so the increase in coverage should not affect my main results. The 3,735 institutions in the 2010 data that do not report in 1994 represent around 15 percent of total 2010 enrollment and 3 percent of 2010 selective enrollment.

[37] Barron's selectivity rankings are constructed from admissions statistics describing the year-earlier first-year class, including: median entrance exam scores, percentages of enrolled students scoring above certain thresholds on entrance exams and ranking above certain thresholds within their high school class, the use and level of specific thresholds in the admissions process, and the percentage of applicants accepted. About 80 percent of schools in my sample are not ranked by Barron's. Most of these schools are for-profit and two-year institutions that generally offer open admissions to interested students. I classify all unranked schools as non-competitive. The Barron's data were generously provided to me by Lesley Turner.

competitive" institutions, only).[38] As discussed above, there is little reason to expect mandates to affect overall enrollment, since the marginal enrollee enrolls at a non-competitive school that generally does not require the ACT. By contrast, if mandates provide information about ability to those who prefer selective to unselective colleges but underestimate their candidacy for selective schooling, they may affect the distribution of enrollment between non-competitive and competitive schools.

Figure 4 depicts the distribution of enrollment by institutional selectivity in 2000. Together, the solid colors represent the portion of enrollment I designate "more selective", and the solid colors *plus* the hatched slice represent the portion I designate "selective." More than half of enrolled students attend noncompetitive institutions, a much larger share of students than in any other one selectivity category. Around 35 percent of enrollment qualifies as "more selective", and 45 percent as "selective." These shares are broadly consistent with the Carnegie Foundation statistics described earlier.

I also explore analyses that cross-classify institutions by selectivity and other institutional characteristics, such as program length (4-year vs. other), location (in-state vs. out-of-state), control (public vs. private), and status as a land grant institution,[39] constructed from the IPEDS.

B. Estimating Enrollment Effects

Figures 5a and 5b present suggestive graphical evidence linking the ACT mandates to college enrollment. Figure 5a plots overall enrollment over time by 2002 mandate status for freshmen from all of the ACT states. Students from Illinois and Colorado are plotted on the left axis, and those from the remaining 23 states are on the right. Figure 5b presents the same construction for selective and more selective enrollment. There is a break in each series between 2000 and 2002, corresponding to the introduction of the mandates.

The graphs highlight several important phenomena. First, there are important time trends in all three series: overall enrollment rose by about 30 percent between 1994 and 2000 (in part, reflecting increased coverage of the IPEDS survey) among freshmen from the non-mandate

[38] Year-to-year changes in the Barron's designations are uncommon. I follow Pallais (2009) and rely on Barron's data from a single base year (2001).

[39] Per IPEDS, a land-grant institution is one "designated by its state legislature or Congress to receive the benefits of the Morrill Acts of 1862 and 1890. The original mission of these institutions, as set forth in the first Morrill Act, was to teach agriculture, military tactics, and the mechanic arts as well as classical studies so that members of the working classes could obtain a liberal, practical education." Many of these institutions – including the University of Illinois at Urbana-Champaign and Colorado State University – are now flagships of their state university systems.

states and by 15 percent among freshmen from the mandate states, while selective and more selective enrollment rose by around 15 percent over this period from each group of states. Second, after 2002, the rate of increase of each series slowed somewhat among freshmen from the non-mandate states. The mandate states experienced a similar slowing in overall enrollment growth for much of that period, but if anything, the growth of selective and more selective enrollment from these states accelerated after 2002. For instance, by 2010, selective enrollment from the mandate states was almost 30 percent above its 2000 level, but only 9 percent higher among freshmen from the other states.

Table 6 summarizes levels and changes in average enrollment figures according to mandate status using data from 2000 and 2002. The bolded rows indicate the primary enrollment measures I consider in my baseline regressions, denominated as a share of the at-risk population of 18 year olds. (Note that the mandate states are larger than the average non-mandate state.) The share of 18 year olds attending college increased around 5 percentage points within both groups between 2000 and 2002, whereas attendance at selective and more selective colleges grew around 2 percentage points among students from mandate states but was essentially flat for those from non-mandate states. Appendix Table 6 shows that the same general pattern—relatively larger growth among the students from mandate states—holds for an alternative measure of institutional selectivity, schools that primarily offer four-year degrees, as well as across a wide variety of subgroups of selective institutions, in particular those both public and private and both in-state and out-of-state.

Table 6 also summarizes key characteristics derived from the Current Population Survey that might affect college enrollment: namely, the minority and in-poverty shares, the fraction of adults with a B.A., and the June-May unemployment rate. While mandate states differ somewhat from non-mandate states in these variables, the change over time is similar across the two groups of states. This suggests that differential time trends in these measures are unlikely to confound identification in the difference-in-differences strategy I employ. Nonetheless, I will present some specifications that control for these observables as a robustness check.

To refine the simple difference-in-differences estimate of the college-going effects of the mandates from Table 6, I turn to a regression version of the estimator, using data from 1994 to 2008:

$$E_{st} = \beta_0 + \beta_1 \times mandate_{st} + \boldsymbol{X_{st}\theta} + \gamma_t + \gamma_s + \varepsilon_{st} \quad (3)$$

Here, E_{st} is the log of enrollment in year t among students residing in s, aggregated across institutions (in all states) in a particular selectivity category. The γ's represent state and time effects that absorb any permanent differences between states and any time-series variation that is common across states. The variable $mandate_{st}$ is an indicator for a treatment state after the mandate is introduced; thus, β_1 represents the mandate effect: the differential change in the mandate states following implementation of the mandate. Standard errors are clustered at the state level.[40, 41]

$\boldsymbol{X_{st}}$ represents a vector of controls that vary over time within states. For my primary analyses, I consider three specifications of $\boldsymbol{X}$ that vary in how I measure students at-risk of enrolling. In the first set of analyses, I do not include an explicit measure of cohort size. In the second, I include the size of the potential enrolling class (measured as the log of the state population of 16-year-olds in year t-2). And in the third, just of selective and more selective enrollment, I instead use total postsecondary enrollment in the state-of-residence/year cell as a summary statistic for factors influencing the demand for higher education. Because (as I show below and as Figure 5a makes clear) there is little sign of a relationship between mandates and overall enrollment, this control makes little difference to the results. I estimate each specification with and without the demographic controls from Table 6.

Table 7 presents the regression results for the period between 1994 and 2008, where the estimation sample includes all ACT states,[42] and the treatment states are Colorado and Illinois. Each panel reflects a different dependent variable measuring selective enrollment, with the definition of selectivity increasing in stringency from the top to the bottom of the table. Within each panel, I present up to 6 variations of my main equation: Specification (1) includes no additional controls beyond the state and time effects, specification (2) adds only the demographic controls, specification (3) controls only for the size of the high school cohort, specification (4)

[40] Conley and Taber (2011) argue that clustered standard errors may be inconsistent in difference-in-differences regressions with a small number of treated clusters, and propose an alternative estimator for the confidence interval. Conley-Taber confidence intervals are slightly larger than those implied by the standard errors in Table 7, but the differences are small. For instance, in Panel B, Specification (5), the Conley-Taber confidence interval is (0.059, 0.219), while the clustered confidence interval is (0.104, 0.180). Conley-Taber confidence intervals exclude zero in each of the specifications marked as significant in Table 7.

[41] Robust standard errors are generally smaller than clustered, except for some instances in Table 7, specifications (5) and (6) where they are slightly larger but not enough as to affect inferences. A small-sample correction for critical values using a *t*-distribution with 23 degrees of freedom (i.e. $G-1$), as recommended by Hansen (2007), does not affect inferences.

[42] The sample omits Michigan, due to its ACT mandate potentially affecting 2008 enrollees. Results are not very sensitive to its inclusion.

adds the demographic controls, and specifications (5) and (6) replace the size of the high school cohort in (3) and (4) with total college enrollment.[43]

Results are quite stable across specifications. There is no sign that mandates affect overall enrollment probabilities. However, the mandate does appear to influence enrollment at selective schools: selective and more selective college enrollment each increase by between 10 and 20 percent when the mandates are introduced. Altogether, the regression results coincide with the descriptive evidence: the mandate is inducing students who would otherwise enroll in nonselective schools to alter their plans and enroll in selective institutions.

C. Robustness Checks and Falsification Tests

This section explores several alternative specifications. To conserve space, I report results only for selective enrollment, controlling for overall enrollment (as in Panel B, Specification (5) in Table 7). Results using other specifications are similar (available upon request).

Table 8 presents the first set of results. Column (1) reviews the key results from Table 7. The specification in column (2) extends the sample to include 2010. To do so, I remove Kentucky and Tennessee from the sample, since their ACT mandates potentially affect 2010 enrollment. The treatment coefficient strengthens a bit with the additional year of coverage.

In column (3), I reduce the sample to just the two mandate states and their nine neighbors (as discussed in Sections III and IV). Given the demonstrated similarity in test-taking rates and demographic characteristics across state borders, it is plausible that the marginal competitive college-goer within treatment states is better represented by her counterpart in a neighboring state than in the full sample of ACT states. The results are quite similar to those in column (1).

The implicit assumption so far is that, all else equal, the underlying enrollment trends in treatment and control states are the same. In column (4), I add state-specific time trends. The mandate effect vanishes in this specification.[44] However, Figures 5a and 5b suggest that the mandate effects appear gradually after the mandates are introduced, a pattern that may be

[43] I have also estimated the specifications presented in Table 8 weighting the regressions by population and total enrollment (where applicable). Results are mostly unchanged.

[44] In columns (4) and (6), I present robust standard errors, as they are more conservative here than the clustered standard errors of 0.016, 0.017, and 0.033, respectively.

absorbed in a specification with a linear trend and a single step-up mandate effect. So I also explore another specification that allows the treatment effect to phase in:[45]

$$E_{st} = \alpha_0 + \alpha_1 \times (treatment_s \times (< 4\, years\ of\ policy)_t) + \alpha_2 \times (treatment_s \times (\geq 4\, years\ of\ policy)_t) + \alpha_3 \times overall_{st} + \gamma_t + \gamma_s + \psi_s \times t + \varepsilon_{st} \quad (3^*)$$

The results are presented without state-specific trends in column (5) and with them in column (6). Column (5) indicates that the treatment effect is 10 percent in the first years after mandate implementation and grows to 20 percent thereafter. Turning to column (6), we see that this specification is much more robust to the inclusion of state specific trends than was the version with a single treatment effect. The hypothesis that both treatment coefficients are zero is rejected at the 1 percent level. There are a number of possible explanations for the growing treatment effect in column (6), including changes in student aspirations over time and/or better preparation for testing by both schools and students. I discuss these explanations more fully toward the end of this section.

Column (7) presents a simple falsification test that extends the treatment period four years earlier to 1998. I estimate:

$$E_{st} = \beta_0 + \beta_1 \times (treatment_s \times post2002_t) + \beta_{1'} \times (treatment_s \times post1998_t) + \beta_2 \times overall_{st} + \gamma_t + \gamma_s + \varepsilon_{st} \quad (3^{**})$$

In effect, this specification simulates additional effects from a placebo testing mandate affecting the two cohorts prior to the treatment group. The coefficient on the placebo term is not statistically different from zero, while the average impact of the mandates on the exposed cohorts remains essentially unchanged.

Columns (8) and (9) present separate estimates of the mandate effect in Illinois and Colorado. The increase in students attending selective schools is essentially the same across treatment states.[46]

In the last column, I use a similar specification to estimate the effects of more recent ACT mandates in Kentucky, Michigan, and Tennessee. Column (10) presents the results of estimating equation (3) over the full sample period for the late-adopting states, omitting

[45] Note that the complier analysis in Sections III and IV includes test-taker data that extend only through 2004, corresponding to the period covered by the short-term effect in equation (3^*).

[46] When I estimate overall college enrollment effects, I find a decrease in Illinois and an increase in Colorado. The selective enrollment effects in the two states are similar with the alternative set of controls from Table 7.

Colorado and Illinois from the sample.[47] An important limitation is that I have only one post-mandate year of data in Kentucky and Tennessee and only two years in Michigan. Thus, based on column 5 we should expect a smaller treatment effect than was seen for Illinois and Colorado with the same specification. This is indeed what we see.

Finally, Tables 9 and 10 present additional analyses for other measures of selectivity. The regression framework mirrors equation (3) but varies the enrollment measure.[48] For instance, Table 9 examines the effects of the mandate on enrollment in each of the six Barron's selectivity categories, treated as mutually exclusive rather than as cumulative. The enrollment effect is large and mostly comparable in magnitude across each of the five selective tiers, but negative for non-competitive enrollment.

Table 10 further probes variation across types of institutions. Mandates appear to increase enrollment at all schools primarily offering four-year degrees, as well as enrollment within several subcategories of selective institutions, including land grant schools, both public and private schools, and both in-state and out-of-state schools. The size of the effect (in percentage terms) is larger at private than at public schools, and at out-of-state than at in-state schools; however, taking into account baseline enrollment in each category, attendance levels actually increased more at public and in-state institutions with the mandate.

Rows 8 and 9 try to zero in on "flagship" schools, which are difficult to define precisely. I find large effects for selective in-state land grant schools, but not particularly large effects for an alternative definition that includes all in-state public schools. Applying the estimated increases to baseline enrollment, it appears that state flagships absorb some, but not all, of the estimated in-state increase; a bit more than half of the increase owes to increased enrollment at private schools in Colorado and Illinois. Since the effect for out-of state enrollment (row 7) and in-state selective private enrollment (row 10) are each quite large, any possible public sector responses—which might conceivably have been part of the same policy reforms that led to the mandates (although I have found no evidence, anecdotal or otherwise, of any such reforms)—do not appear to account for the observed boost in enrollment.

[47] There is no detectable overall enrollment effect among the later-adopting states (not shown).
[48] As in Table 8, Tables 9 and 10 reflect the specification including a control for log overall enrollment, but results are mostly robust to its exclusion. I do not report robust standard errors, though they are nearly always smaller than clustered standard errors and none of the significant treatment coefficients would be insignificant using robust standard errors for inference.

One concern is that the effects I find might derive not from the mandates themselves but from the accountability policies of which the mandates were a part. In each of the mandate states but Tennessee, the mandates were accompanied by new standards of progress and achievement imposed on schools and districts. If those reforms had direct effects on student achievement or qualifications for selective college admissions, the reduced-form analyses in Tables 6-10 would attribute those effects to the mandates. There are several reasons, however, to believe that the results indeed derive from the mandates themselves.

The first reason is the absence of an overall enrollment effect of the mandate policies. One would expect that accountability policies that raise students' preparedness would lead some to enroll in college who would not otherwise have. There is no sign of this; effects appear to be concentrated among students who would have attended college in any case.

Second, National Assessment of Educational Progress (NAEP) state testing results for math and reading in both Colorado and Illinois mirror the national trends over the same period. These results are based on 8th grade students, so do not reflect the same cohorts. Nevertheless, I take the stability of NAEP scores as evidence that the school systems in mandate states were not broadly improving student performance.

Third, one would expect accountability-driven school improvement to take several years to produce increases in selective college enrollment. But I show above that important effects on selective college enrollment appear immediately after the implementation of the mandates.

Finally, the policies in the later-adopting states and the different ways in which they were implemented provide some insight into the causal role of the testing mandate alone. Beginning in Spring 2008, Tennessee mandated that its students take the ACT as part of a battery of required assessments, but did not implement other changes in accountability policy at the same time. A specification mirroring the baseline equation in this section estimates that enrollment of Tennessee students in selective schools rose by 15 percent in 2010, similar to the estimates shown above for Illinois and Colorado. This strongly suggests that it is testing mandates themselves—not accountability policies that accompanied them in the two early-adopting states—that account for the enrollment effects.

D. Generalizability

The above estimates are based on two widely-separated states that implemented mandates. While the total increase in selective enrollment represented about 15 percent of prior selective enrollment among students from those states, the new enrollees amounted to only 1 percent of total national selective enrollment and 5 percent of selective enrollment from the mandate states and their neighbors. One hurdle to generalizing based on the results of the Illinois and Colorado mandates to encourage similar policies nationwide is that national policies may create significant congestion in the selective college market, leading either to crowd-out of always-takers by compliers or to reduced enrollment of compliers (Bound and Turner, 2007; Card and Lemieux, 2000).[49]

Since the estimated effects from mandates in just two states represent such a small share of national selective enrollment and are fairly evenly distributed across types of schools, this experiment offers only limited insight into the extent of crowd-out we might anticipate from broader policies. Had the main effect been larger or more concentrated within a particular type of school, there might be more direct evidence for concern about crowd-out. Still, given the relatively large increases in selective out-of-state enrollment and private in-state enrollment generated by the mandates, there is some indication mandate compliers might compete for these types of admissions slots with always-takers if these policies were scaled up.

This kind of crowd-out would be particularly likely if the selective college sector was unable to expand quickly enough to accommodate new mandate-induced demand. However, over the sample period national selective enrollment has grown by about 2 percent each year. This implies that the increase in enrollment produced by a national mandate, which I estimate at around 15 percent, would be absorbed with under eight years of normal growth. Thus, while crowd-out remains a possibility from a national mandate, it seems likely that the supply of selective college seats is elastic enough to avoid large or long-lasting crowd-out effects.

VII. Assessing the Test-taking Decision

I have demonstrated that forcing students to take the ACT leads test-taking rates to rise by about 40-45 percentage points and produces substantial increases in selective college enrollment—between 10 and 20 percent, on average—with no detectable effect on overall college

[49] The share of the population from untreated states enrolled in selective schools did not fall after the Illinois and Colorado mandates were introduced, suggesting that these smaller shocks at least did not produce meaningful crowd-out.

attendance. When I separately consider the early years of implementation, corresponding to the period covered by the test-taker data used in Sections III and IV, the selective enrollment impact is still around 10 percent. This implies that the mandates induced about 2,000 students in Colorado and 5,000 students in Illinois to enroll in selective schools in each of the early post-mandate years of these mandates. This corresponds to about 10 percent of the mandate compliers in 2004, estimated as 23,000 and 53,000, respectively, in Section IV. About half of the compliers earned scores above 18, roughly corresponding to the threshold for admission to selective schools. Thus, the enrollment effects indicate that about 20 percent of selective-college-eligible compliers wound up enrolling in such schools.

In this section, I work through a simple calculation to show that the combination of results above – in particular, the high share of mandate compliers who earn high scores and the large effect of the mandates on selective college enrollment – are incompatible with a view in which students make rational test-taking decisions based on unbiased forecasts of their probabilities of earning high scores. In particular, I show that the true passing probability among compliers who would attend a selective college if admitted (referred to previously as the LE compliers) is well above 0.03. Because a passing probability well below that threshold would be sufficient to justify the cost of taking the exam for an LE student, this implies an important divergence between P^*, the actual rate at which such students pass the exam, and P, the students' subjective judgments of their likelihood of passing.

Using the decomposition outlined in Section V, I can write:

$$E(P^*|\mathrm{C}) = \frac{f_{NI}}{f_{NI}+f_{LE}} \times E(P^*|\mathrm{NI}) + \frac{f_{LE}}{f_{NI}+f_{LE}} \times E(P^*|\mathrm{LE}), (4)$$

where f_{NI} and f_{LE} are the shares of students in the NI (not interested) and LE (low expectations) groups, respectively. Moreover, note that $E(P^*|\mathrm{LE})$ is simply the pass rate among LE students. Thus, the second term of equation (4) can be rewritten as:

$$\frac{f_{LE}}{f_{NI}+f_{LE}} \times E(P^*|\mathrm{LE}) = \Pr(\mathrm{LE}|\mathrm{C}) \times \Pr(\mathrm{pass}|\mathrm{LE}) = \Pr(\mathrm{pass} \cap \mathrm{LE}|\mathrm{C}).$$

The expression on the right hand side is the share of mandate compliers who would attend a selective school if admitted *and* who earn high enough scores to enroll in a selective school. In other words, it equals the share of compliers who ultimately enroll in a selective school. Under the assumption that test mandates do not affect the preferences, scores, or behaviors of the always-takers, I can interpret the effect of the mandate on selective college enrollment as an estimate of this share.[50] From Section VI, it is approximately 10 percent. Moreover, in Section IV, I demonstrated that roughly 40 percent of compliers earn scores high enough to be admitted to a selective college; that is, $E(P^*|\mathrm{C}) \cong 40\%$.

Substituting these into (4) and rearranging, we find that:

$$\frac{f_{NI}}{f_{NI}+f_{LE}} \times E(P^*|\mathrm{NI}) = E(P^*|\mathrm{C}) - \frac{f_{LE}}{f_{NI}+f_{LE}} \times E(P^*|\mathrm{LE}) \cong 40\% - 10\% = 30\%$$

Next, I attempt to bound $E(P^*|\mathrm{NI})$. It seems reasonable to assume that the NI compliers are no more able than the group of always-takers. From Section IV, $E(P^*|\mathrm{AT}) \cong 80\%$. Assuming that $E(P^*|\mathrm{NI}) < 80\%$, we can conclude that $\frac{f_{NI}}{f_{NI}+f_{LE}} > 37.5\%$. This in turn implies $\frac{f_{LE}}{f_{NI}+f_{LE}} < 62.5\%$. Returning to equation (4), this implies that $E(P^*|\mathrm{LE}) \cong 16\%$. In other words, the actual pass rate for mandate compliers who want to attend selective schools is roughly 16%.

Using the rationale established by the model in Section V, these calculations imply that many high-scoring compliers thought that their chances of passing were lower than their actual chances. Formally, $E(P|\mathrm{LE}) \ll E(P^*|\mathrm{LE})$ so that these students must have systematically downwardly biased forecasts of their performance.

Figure 6 graphs the probability that a student will earn a competitive score against various potential values of the return to selective college attendance. The solid curve represents the locus of points {return to selective college, probability of admission} at which individual students will be indifferent between taking the test or not. For a given anticipated selective college return, students who perceive their probability of earning a high score as above this curve will find it rational to take the ACT even if it is not mandated, while students who perceive their probability of earning a high score as below the line will not. The dashed horizontal line represents my lower-bound estimate for the average passing rate among LE compliers, 16

[50] This assumes that the 80 percent of high-scoring compliers who were not induced to enroll in selective schools are from the NI group. But there are other possibilities: some might have enrolled in the absence of the mandate (e.g., by electing the SAT test), while others might have been ineligible for admission for other reasons (such as bad grades). If any were from the latter group, I am understating $\Pr(\mathrm{pass} \cap \mathrm{LE}|\mathrm{C})$, and my bound on the break-even point for the test-taking decision is a lower bound.

percent.[51] This crosses the decision threshold at \$963, indicating that the observed decisions can be consistent with full information only if students perceive the return to attending a selective college as under \$1000.

The vertical lines on Figure 6 show estimates of the returns to attending a selective college from the literature. These range from \$5,000 (Black and Smith [2006] with an adjustment from Cohodes and Goodman [2012]) to \$507,000 (Dale and Krueger [2011] for their minority subsample; in the full sample, Dale and Krueger estimate a return of zero).[52] All of these are well above the value consistent with the estimated passing rate. They thus imply much lower decision thresholds for students to take the test, ranging from 3 percent down to 0.03 percent.

This of course does not count the psychic costs, if any, of attending a more selective school, nor any difference in tuition or other financial costs. It is conceivable that such costs almost exactly offset the wage benefits for some students, but it is extremely unlikely that very many students view the choice as a knife-edge decision, so that removing \$150 from the marginal cost of selective college enrollment is decisive in making the benefit exceed the cost. A more plausible explanation for the results is that many compliers held downward-biased estimates of their chances of earning high scores.

VIII. Discussion

The analyses above demonstrate that ACT mandates lead to large increases in ACT participation, in the number of high-scoring students, and in selective college enrollment. In particular, about 40-45 percent of students induced to take the ACT exam under a mandate earned competitive scores and many of these high-scoring students—about 20 percent—ultimately enrolled in competitive schools. The fraction of all mandate compliers who achieved high scores and upwardly revised their enrollment plans—about 10 percent— is well beyond the fraction that can be accommodated by an unbiased model of the test-taking decision. Under extremely conservative assumptions, such a fraction would not be higher than 3 percent. Altogether, the

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[51] Heterogeneity in P^* among LE compliers would imply an even higher break-even point. In Appendix D, I present an alternative calculation based on the full distribution of test scores and external estimates of the ACT's reliability. This implies that students with P^* as high as 40-45 percent are choosing not to take the test.

[52] These conversions are approximate, since each estimate derives from a unique sample of students and colleges and a unique measure and definition of college quality. In particular, the return to selective college estimated by Dale and Krueger relies on a sample of colleges quite limited in scope. They examine colleges with Barron's designations ranging only from "Competitive" to "Most Competitive," all of which are considered "selective" in my categorization.

evidence overwhelmingly suggests that students are systematically under-predicting their suitability for selective schools.

Recent studies have demonstrated that college quality is an important determinant of later success.[53] Black and Smith (2006) and Cohodes and Goodman (2012), for example, find that students at lower-quality schools earn less over their lifetimes and are less likely to graduate than their counterparts at higher-quality schools. Since my results demonstrate that mandatory ACT testing leads many students to enroll in more-competitive, higher-quality colleges, forcing students to take the ACT likely enables many students to vastly improve their lifetime earnings trajectories. For instance, using the continuous quality measure I describe in Appendix C, I can link my results to Black and Smith's findings that an additional standard deviation in college quality produces a 4.2 percent increase in lifetime earnings. The increase in selective enrollment that I estimate translates into a 0.247 standard deviation increase in average college quality. Thus, the roughly 4.5 percent of students who are induced to change their enrollment status by the mandate should, on average, expect a boost of 23 percent in their lifetime earnings.[54]

In a separate but related line of research, Pallais and Turner (2006) recently established that underprivileged groups are underrepresented at top schools. They attribute their finding to a combination of information constraints, credit constraints, and pre-collegiate underachievement, but are unable to distinguish among them. My analysis provides additional support for their first explanation – that lack of information can explain at least some of the missing low-income students.

I demonstrate that students on average take the test less often than they should: mandate compliers earn competitive scores at remarkably high rates and attend more-selective schools with significant probability when they do earn competitive scores. Thus, many students are learning important information from these tests, and these better-informed students, all else equal, are enrolling in better schools. This pattern is not consistent with a model of rational ignorance – students who can collect valuable information at low cost by taking the ACT frequently opt not to do so.

[53] Cohodes and Goodman (2012) suggest that students are willing to accept very low compensation in exchange for substantial reductions in college quality and therefore must apply extraordinarily high discount rates to potentially-severe lifetime earnings penalties. Even after accounting for this extremely-myopic behavior, students in my sample are *still* electing the exam much less than optimal.

[54] According to figures on average lifetime earnings of BA holders (Pew Research Center Social and Demographic Trends, 2011), 23 percent of lifetime earnings amounts to about $760,000.

Framed differently, a substantial share of high-ability students prematurely and incorrectly rule out selective colleges from their choice sets. Although my data do not permit me to estimate the characteristics of these students, it seems likely that many of them come from low-income families. Students from low-SES families are much less likely to take the test in the absence of a mandate than are their more advantaged peers, and when the mandate is implemented the low-SES compliers are no less likely to earn high scores than are compliers from high-SES families.

I conclude that increasing information flow between universities and students from these underrepresented groups—so that the potential high scorers know they are indeed suitable candidates—will likely erase some of the enrollment gap found at top schools. This is a potentially fruitful area for further policy development – expanding mandates appears to be a desirable policy on its own, but there may be additional policies that would complement mandates in targeting students' information shortages.

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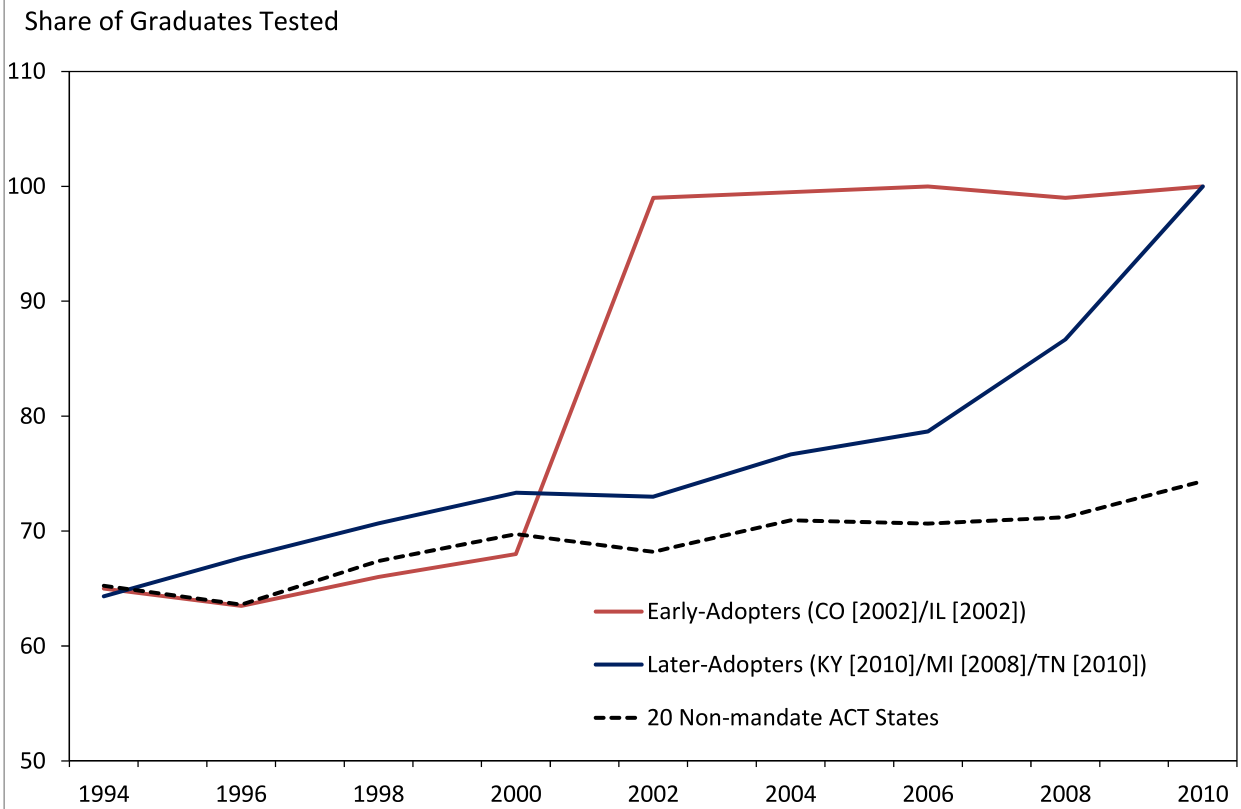
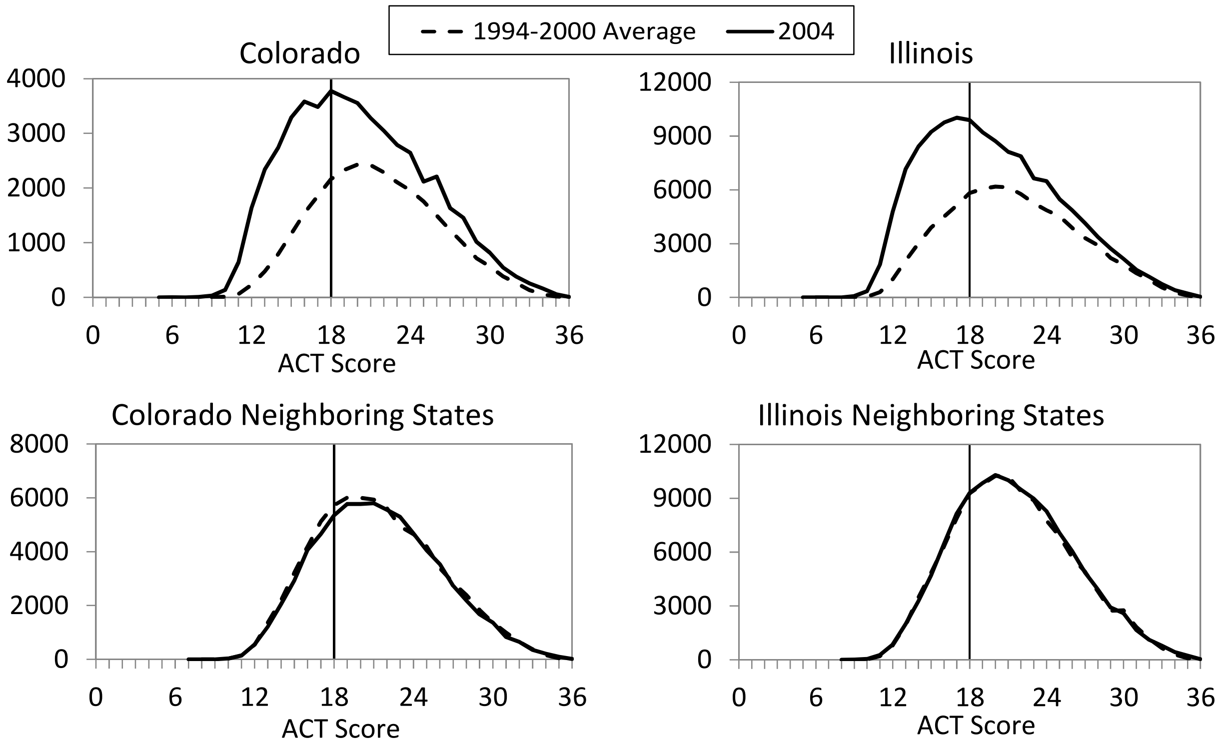
Figure 1: Average ACT Participation Rates



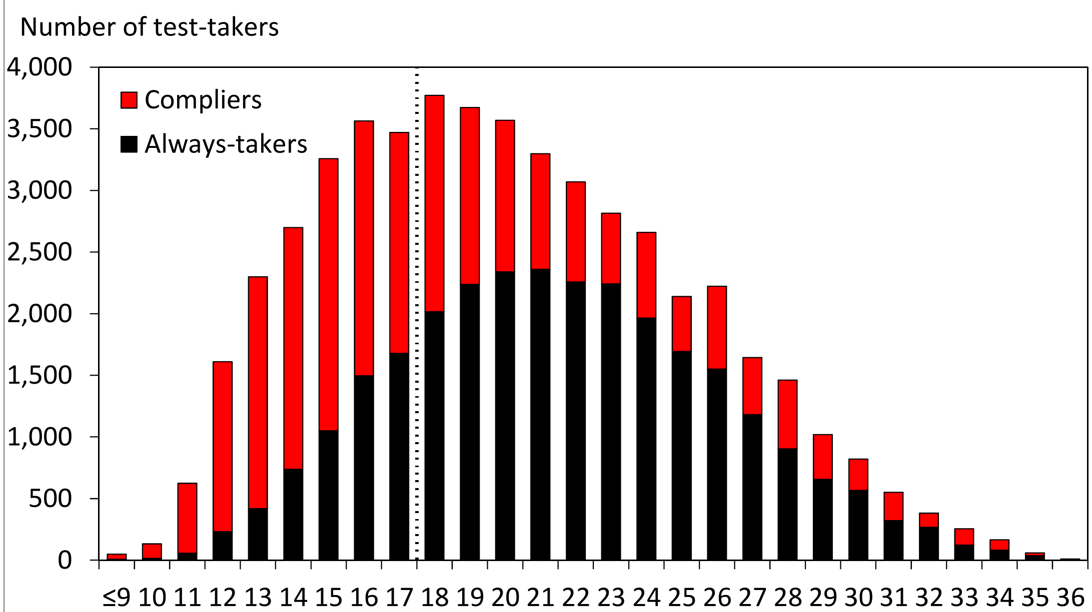
Notes: Participation rates plotted for even years only. Each mandate state's first treated graduate class appears in brackets.

Figure 2: ACT Score Distribution



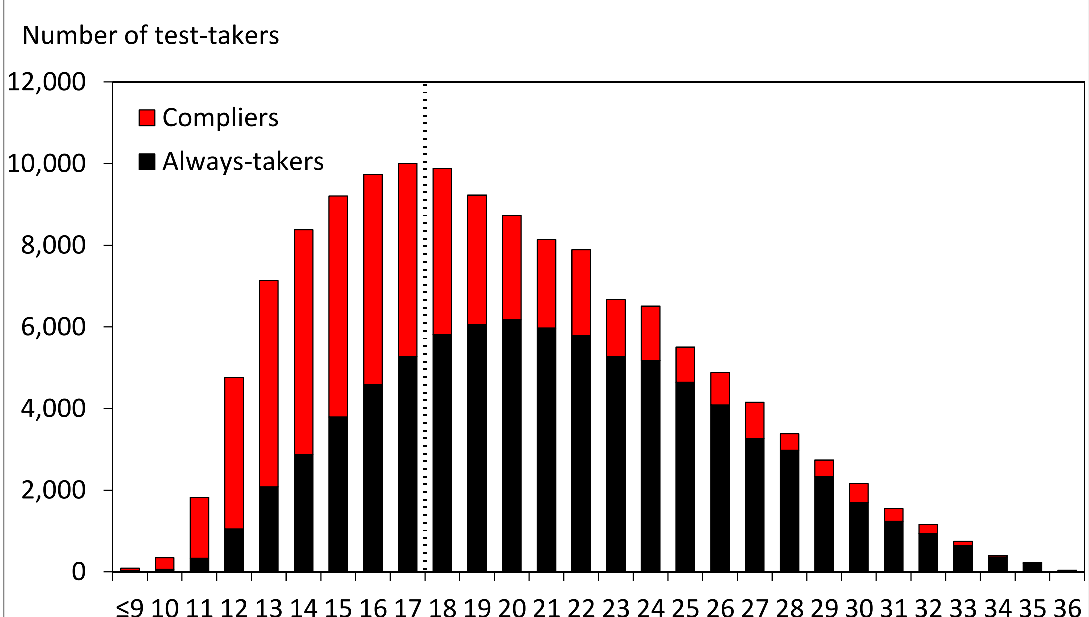
Notes: Graphs plot the number of test-takers with score s (scaled by class size). "1994-2000 Average" refers to the average calculated over even years only. Vertical line at 18 to reflect an admissions cutoff commonly used by selective colleges. This cutoff is approximately equal to a score of 870 on the SAT.

Figure 3a: Colorado ACT Score Distribution, 2004



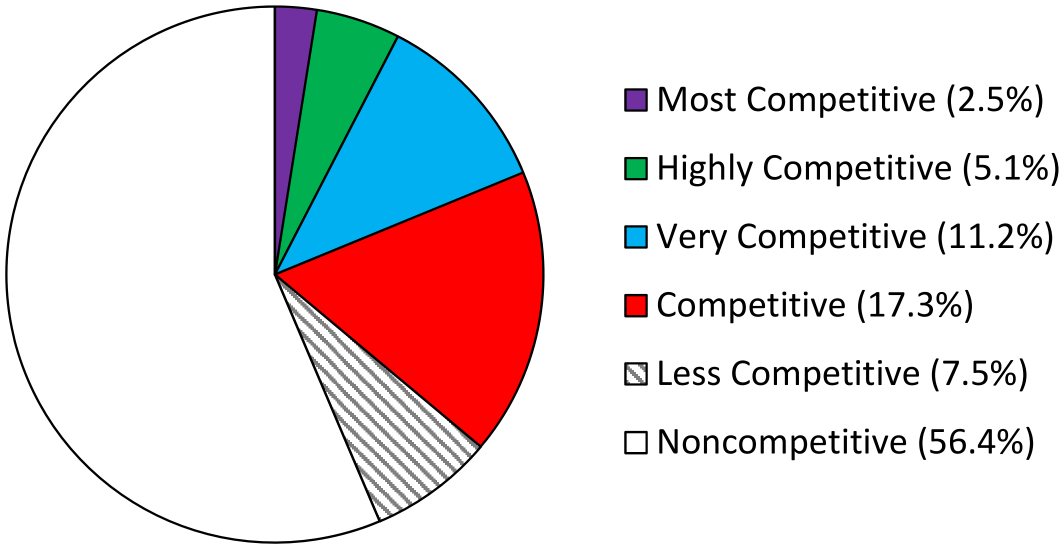
Note: Always-takers are students who take the ACT whether or not they are required; compliers take it only if subject to a mandate. Dashed line at 18 to reflect an admissions cutoff commonly used by selective colleges. This cutoff is approximately equal to a score of 870 on the SAT.

Figure 3b: Illinois ACT Score Distribution, 2004



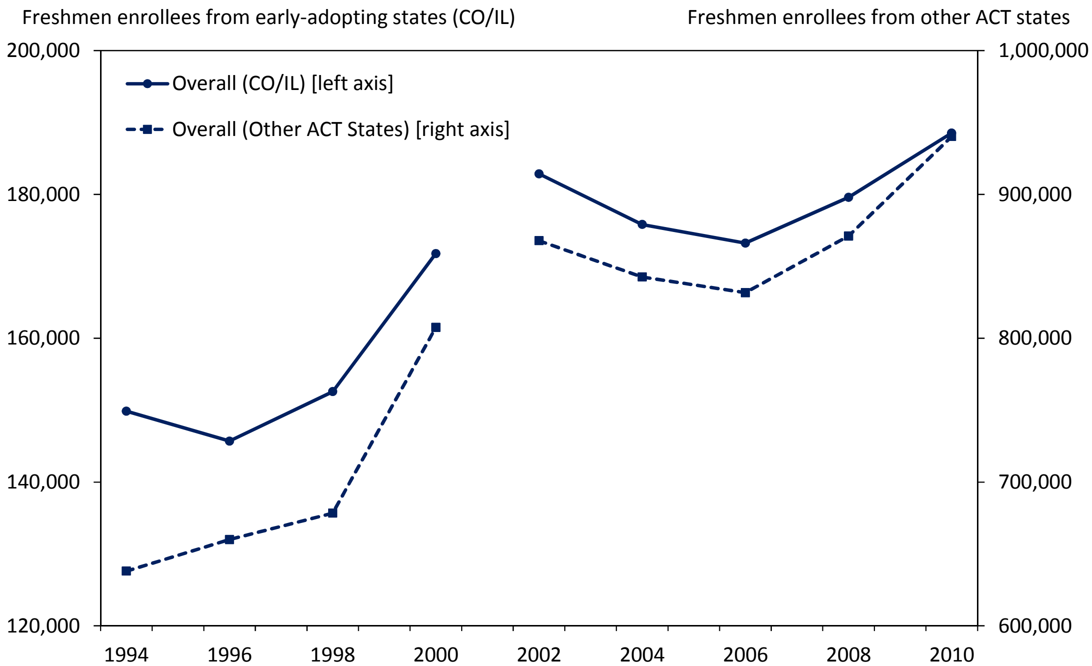
Note: Always-takers are students who take the ACT whether or not they are required; compliers take it only if subject to a mandate. Dashed line at 18 to reflect an admissions cutoff commonly used by selective colleges. This cutoff is approximately equal to a score of 870 on the SAT.

Figure 4: 2000 Enrollment Distribution by Selectivity



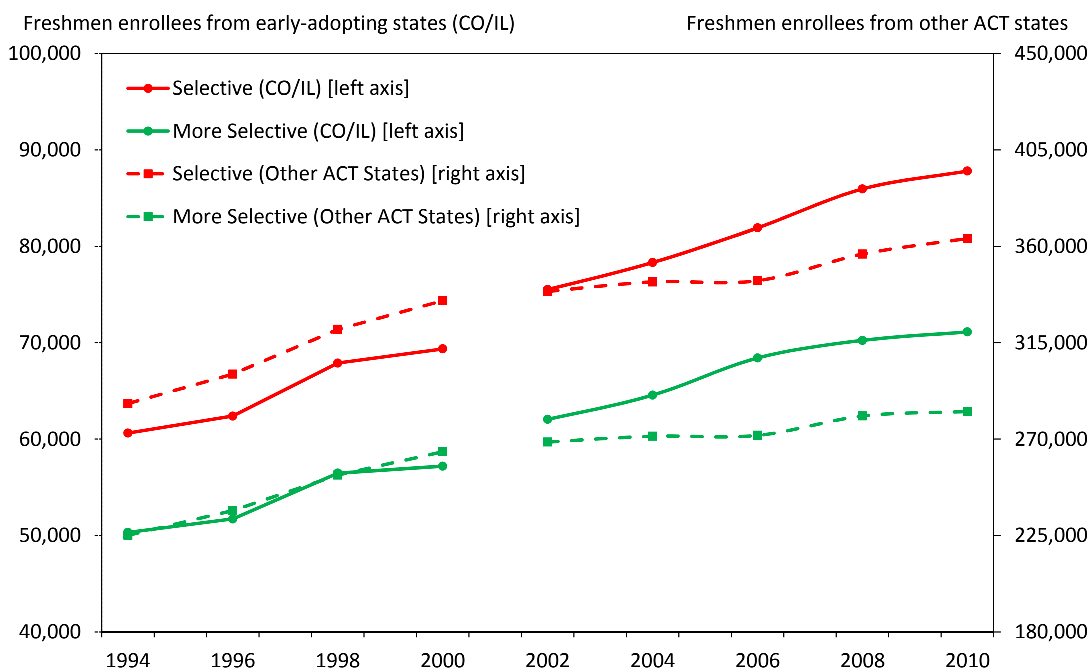
Note: Sample is first-time freshmen enrollees in all U.S. institutions in 2000. "Noncompetitive "enrollment includes students attending schools designated by Barron's as noncompetitive as well as all schools without Barron's designations.

Figure 5a: Overall College Attendance in the United States by State of Residence



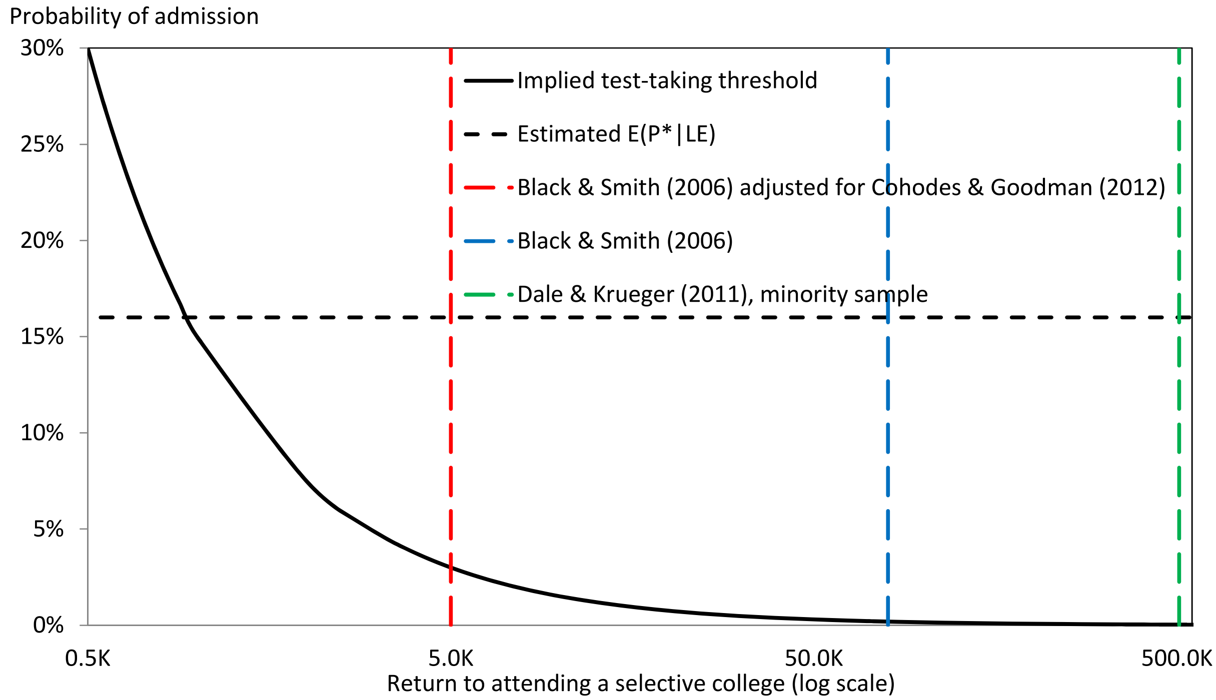
Note: Data for even years only. Discontinuity in the series reflects timing of ACT mandates in early-adopting states.

Figure 5b: Selective College Attendance in the United States by State of Residence



Note: Data for even years only. Discontinuity in the series reflects timing of ACT mandates in early-adopting states.

Figure 6: Implied Test-taking Thresholds for Various Estimates of the Return to College Quality



Note: Students who perceive their probability of earning a high score to be above the solid line will find it rational to take the ACT even if it is not mandated, while those below will not. The horizontal line plots the probability of admission among the "low expectations" (LE) compliers that I derive in the text, while the vertical lines depict estimates of the returns to college quality from the literature. The Dale and Krueger (2011) estimate over their full sample implies a zero return (not shown). However, they examine colleges with Barron's designations ranging only from "Competitive" to "Most Competitive," all of which are considered "selective" in my categorization.

Table 1: State ACT Mandate Timing

State	Program Name or Details	First Affected Graduating Class / Enrollment Year
Colorado	Colorado ACT	2002
Illinois	Prairie State Achievement Exam	2002
Kentucky	Kentucky Work and College Readiness Examination	2010
Michigan	Michigan Merit Exam	2008
Tennessee	Tennessee Code Annotated 49-6-6001(b), amended by Senate Bill No. 2175	2010

Notes: All states—with the exception of Tennessee—began mandating the ACT as part of a statewide accountability system overhaul. Program inception is denominated in even years to match enrollment data.

Table 2: Summary Statistics

				Average		Share of Test-takers		
Period	State or State Group	Number of States	Number of Test-takers	ACT Score	Income Quintile	Minority	Attended High-Minority HS	Female
1994-2000 Average	Colorado	1	23,421	21.5	3.0	25%	30%	55%
2004	Colorado	1	51,300	20.1	2.7	34%	42%	50%
1994-2000 Average	CO neighbors	5	69,887	21.3	2.8	18%	21%	54%
2004	CO neighbors	5	71,604	21.3	2.8	21%	26%	54%
1994-2000 Average	Illinois	1	73,799	21.3	2.9	29%	36%	55%
2004	Illinois	1	135,468	19.8	2.6	36%	46%	51%
1994-2000 Average	IL neighbors	4	114,207	21.4	2.8	12%	12%	56%
2004	IL neighbors	4	123,684	21.5	2.8	14%	15%	56%

Note: "1994-2000 Average" refers to the average calculated over even years only.

Table 3: Estimated Mandate Effect on Test-taking

Dependent variable	Participation Rate (0 to 1)	
Treatment state	Colorado	Illinois
$treatment_s * post_t$	0.455***	0.393***
	(0.03)	(0.02)
$post_t$	-0.019	0.005
	(0.03)	(0.01)
$treatment_s$	-0.036	0.025*
	(0.02)	(0.01)
adj R^2	0.649	0.908
N	*30*	*25*
Implied Number of Test-takers in Each Group		
Compliers	22,803	52,718
Always Takers	28,497	82,750
Total ACT Test-takers	51,300	135,468

Notes: Each column in the top panel reports coefficients from an OLS regression, with robust standard errors in parentheses. The participation rate is the share of potential public high school graduates who take the ACT exam in each state-time cell. The estimation sample in each column is one of two early-adopting states - treated beginning in 2002 - and its neighboring states in 1994, 1996, 1998, 2000, and 2004, where the set of neighboring states and the years prior to 2004 serve as control state and period composites. See text for explanation of bottom panel.

Table 4: Summary Table of Complier Testing Statistics

	(1)		(2)		(3)	(4)	(5)
	Pre ("1994-2000 Average")		Post (2004)		Share of Compliers (2004)	Compliers (2004)	Complier Share of Test-takers (4)/(2) (2004)
Score Range	n	%	n	%	%	n	%
				Colorado			
0-17	4,880	21%	17,884	35%	53%	12,016	67%
18-20	5,507	24%	10,992	21%	19%	4,417	40%
21-24	6,973	30%	11,750	23%	13%	3,021	26%
25+	6,060	26%	10,674	21%	15%	3,349	31%
All	23,421	100%	51,300	100%	100%	22,803	44%
				Illinois			
0-17	17,984	24%	51,656	38%	60%	31,394	61%
18-20	16,256	22%	27,826	21%	19%	9,797	35%
21-24	19,817	27%	29,130	22%	13%	6,975	24%
25+	19,741	27%	26,856	20%	9%	4,554	17%
All	73,799	100%	135,468	100%	100%	52,718	39%

Note: Compliers are students who take the ACT only if subject to a mandate. "1994-2000 Average" refers to the average calculated over even years only.

Table 5: Complier Characteristics and Scores by Characteristics

	Complier Share within Group	Share of High-Scorers who are Compliers	Share of Compliers who Earn High Scores
		Colorado	
All	45%	32%	47%
Female	39%	27%	47%
Male	50%	37%	48%
From a High-Minority HS	53%	37%	37%
Not From a High-Minority HS	38%	29%	58%
Minority	54%	40%	37%
Non-minority	40%	33%	64%
Bottom Income Quintile	57%	43%	37%
2nd - 4th Quintiles	41%	30%	49%
Top Income Quintile	34%	29%	74%
		Illinois	
All	39%	25%	40%
Female	35%	24%	43%
Male	43%	28%	39%
From a High-Minority HS	47%	34%	35%
Not From a High-Minority HS	32%	20%	47%
Minority	46%	37%	39%
Non-minority	35%	28%	61%
Bottom Income Quintile	53%	46%	38%
2nd - 4th Quintiles	34%	19%	36%
Top Income Quintile	29%	25%	75%

Note: Compliers are students who take the ACT only if subject to a mandate. A high-scorer earns a score greater than or equal to 18, reflecting an admissions cutoff commonly used by selective colleges.

Table 6: Differences in Key Characteristics between 2000 and 2002

	Mandate Status in 2002				
	Mandate: CO and IL		No Mandate: Other ACT States		
	Average (2000)	Difference (2002–2000)	Average (2000)	Difference (2002–2000)	Difference in Difference
Enrollment (as Share of Population)					
Most Selective	1.1%	0.1 p.p.	0.6%	0.0 p.p.	0.1 p.p.
Highly Selective	4.3%	0.3 p.p.	1.8%	0.0 p.p.	0.3 p.p.
Very Selective	12.8%	1.2 p.p.	8.0%	0.1 p.p.	1.1 p.p.
More Selective	**23.9%**	**1.8 p.p.**	**20.8%**	**0.3 p.p.**	**1.6 p.p.**
Selective	**30.4%**	**2.3 p.p.**	**26.9%**	**0.4 p.p.**	**1.9 p.p.**
Overall	**74.9%**	**5.7 p.p.**	**65.8%**	**4.5 p.p.**	**1.1 p.p.**
Key Demographics					
18-year-old Population	118,114	278	53,196	-129	407
Minority Share (Fr. of All Residents)	27.4%	2.3 p.p.	18.9%	1.3 p.p.	1.0 p.p.
Poverty Share (Fr. of All Residents)	9.4%	0.2 p.p.	12.6%	-0.1 p.p.	0.3 p.p.
Unemployment Rate (Fr. of Residents in the Labor Market, Ages 16+)	3.7%	1.8 p.p.	4.0%	0.8 p.p.	0.9 p.p.
Share with a B.A. (Fr. of Residents, Ages 25+)	30.8%	0.7 p.p.	23.0%	0.3 p.p.	0.3 p.p.
States in Group	*2*		*23*		
ACT Participation Rate (published)	68%	31 p.p.	70%	-1 p.p.	32 p.p.

Note: Enrollment categories are cumulative.

Table 7: Effect of Mandates on Log First-time Freshmen Enrollment, 1994-2008

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Overall			
$mandate_{st}$	0.054	0.032	0.003	-0.006		
s.e.	(0.142)	(0.138)	(0.117)	(0.115)		
adjusted R^2	0.988	0.988	0.990	0.990		
			B. Selective			
$mandate_{st}$	0.159***	0.138**	0.111***	0.102***	0.142***	0.128***
s.e.	(0.057)	(0.053)	(0.034)	(0.033)	(0.018)	(0.021)
adjusted R^2	0.995	0.995	0.996	0.996	0.996	0.996
			C. More Selective			
$mandate_{st}$	0.163**	0.140*	0.110**	0.100**	0.145***	0.130***
s.e.	(0.000)	(0.012)	(0.045)	(0.046)	(0.025)	(0.029)
adjusted R^2	0.995	0.995	0.996	0.996	0.996	0.996
controls						
ln(population)			X	X		
ln(overall enrollment)					X	X
demographic controls		X		X		X

Notes: Each column in each panel represents a separate OLS regression. The dependent variable in each regression is the log of first-time freshmen enrollment of students from state s in year t at a subset of schools, with the definition of selectivity increasing in stringency from the top to the bottom of the table. All regressions include state and year effects. Demographic controls include poverty and minority share, the June-May unemployment rate, and the share of residents over 25 with a B.A.. The estimation sample is all ACT states (excl. Michigan) in even years between 1994-2008 (inclusive). Two states are treated beginning in 2002. Standard errors, clustered on the state, are in parentheses. *, **, and *** reflect significance at the 10%, 5%, and 1% levels.

Table 8: Effect of Mandates on Log First-time Freshmen Enrollment at Selective Colleges, Robustness and Specification Checks

Specification	(1) Table 7, Panel B, Spec. (5)	(2) Extended Sample Period: 1994-2010	(3) Alt. Control Sample: Neighboring States	(4) State Time Trends	(5) Phased-in Treatment	(6) State Time Trends w/ Phased-in Treatment	(7) Placebo Policy Four Years Earlier	(8) Colorado only	(9) Illinois only	(10) Late-adopting States
$mandate_{st}$	0.142***	0.163***	0.134***	0.043			0.139***	0.156***	0.129***	
s.e.	(0.018)	(0.019)	(0.020)	(0.044)			(0.018)	(0.025)	(0.016)	
$treatment_s$*(<4 years of policy)$_t$					0.095***	0.065**				0.070**
s.e.					(0.014)	(0.029)				(0.030)
$treatment_s$*(≥4 years of policy)$_t$					0.190***	0.140***				
s.e.					(0.029)	(0.034)				
$treatment_s$*(post1998)$_t$							0.007			
s.e.							(0.018)			
Joint Significance Test										
F-statistic					30.962***	9.426***				
p-value					0.000	0.000				
adjusted R^2	0.996	0.995	0.997	0.997	0.996	0.997	0.996	0.995	0.996	0.995
control states	*22*	*20*	*9*	*22*	*22*	*22*	*22*	*21*	*21*	*20*
N	*192*	*198*	*88*	*192*	*192*	*192*	*192*	*184*	*184*	*207*

Notes: Each column represents a separate OLS regression. Sample and specification for columns (1)-(9) are as in Table 7, Panel B, Specification (5), except as noted. Sample for (2) excludes Kentucky and Tennessee; sample for (10) excludes Colorado and Illinois and includes Michigan. In (10), one state is treated beginning in 2008 and two states are treated in 2010. Standard errors, clustered on the state (except in columns (4) and (6), which use more conservative robust standard errors), are in parentheses . *, **, and *** reflect significance at the 10%, 5%, and 1% levels, respectively.

Table 9: Effects of Mandates on Log First-time Freshmen Enrollment in Detailed Selectivity Categories, 1994-2008

Enrollment Category	Specification	Share of Enrollment (2000)	$mandate_{st}$
Most Competitive	(1)	1.5%	0.139***
			(0.024)
Highly Competitive	(2)	4.4%	0.205***
			(0.029)
Very Competitive	(3)	11.3%	0.236***
			(0.057)
Competitive	(4)	14.8%	0.097***
			(0.021)
Less Competitive	(5)	8.5%	0.159**
			(0.064)
Noncompetitive	(6)	59.4%	-0.122***
			(0.032)

Notes: Each row represents a separate OLS regression. Sample and specification as in Table 7, Panel B, Specification (5). Categories are exhaustive and mutually exclusive. Standard errors, clustered on the state, are in parentheses. *, **, and *** reflect significance at the 10%, 5%, and 1% levels. Figures in the third column denote the share of freshmen enrollment in each category in the treatment states in 2000 (before mandates were introduced).

Table 10: Effects of Mandates on Log First-time Freshmen Enrollment in Various Subcategories, 1994-2008

Enrollment Category	Specification	Share of Enrollment (2000)	$mandate_{st}$
Four-year	(1)	47.6%	0.137***
			(0.020)
Subcategories of Selective Enrollment			
Selective-Land Grant	(2)	6.6%	0.169**
			(0.077)
Selective-Public	(3)	27.8%	0.104***
			(0.032)
Selective-Private	(4)	12.7%	0.230***
			(0.040)
Selective-Private Not-for-Profit	(5)	11.8%	0.227***
			(0.041)
Selective-In-State	(6)	29.6%	0.116***
			(0.031)
Selective-Out-of-State	(7)	10.9%	0.212***
			(0.043)
More-refined Subcategories			
Selective-Land Grant-In-State	(8)	4.9%	0.167*
			(0.093)
Selective-Public-In-State	(9)	23.1%	0.084**
			(0.036)
Selective-Private-In-State	(10)	6.5%	0.311***
			(0.095)
Selective-Land Grant-Out-of-State	(11)	1.6%	0.240***
			(0.059)
Selective-Public-Out-of-State	(12)	4.8%	0.228***
			(0.049)
Selective-Private-Out-of-State	(13)	6.2%	0.210***
			(0.067)

Notes: Each row represents a separate OLS regression. Sample and specification for (1)-(7) and (11)-(13) as in Table 7, Panel B, Specification (5); Specifications (8) and (9) exclude Kansas and specification (10) excludes Wyoming, as there are none of the relevant institutions in these states. Standard errors, clustered on the state, are in parentheses. *, **, and *** reflect significance at the 10%, 5%, and 1% levels. Figures in the third column denote the average share of freshmen enrollment in each category in the treatment states in 2000 (before mandates were introduced).

Appendix A: Comparing Test-taker Composition to Characteristics of the At-Risk Population

Appendix Tables 1 and 2 compare the gender and racial composition of test-takers, both before and after mandates, to those of the high school student population. I draw data for the latter from the CCD, focusing on 11th grade students in public schools that enroll at least one test-taker in the corresponding year – either 2000 or 2004.[55]

Not surprisingly, the composition of high school students changes little over time within states. The composition of test-takers is stable as well in non-mandate states and in mandate states before the mandates are introduced. However, in 2004 the female share of test-takers falls, and the minority share rises, in Illinois and Colorado.

Note that even after the mandates, there are small differences in the minority share of test-takers and high school students in the mandate states. This may reflect differences in the way that race is reported and counted in the two data sets. Gender shares are quite comparable across the two groups.

[55] Colorado did not report demographic data to the CCD in 1998-99. I use data on 12th graders in 1999-2000 instead.

Appendix B: Describing the Compliers

In this Appendix, I generalize the methods developed in Section IV to characterize the demographic composition of mandate compliers. Assume c is an indicator variable taking on a value of 1 if a test-taker indicates that she has a particular characteristic (i.e., low income, male, minority), and 0 if not.

Though the ACT-CCD matched dataset enables me to approximate a limited number of characteristics describing the at-risk population, I cannot observe the full range of student demographics or data years available in the ACT survey. Thus, to best approximate the characteristics of the complier population, I assume, analogous to the main text, that test-taker *composition* would evolve similarly across states over time in the absence of mandates: $DD(S_{cst}) = 0$, where $S_{cst} \equiv \frac{A_{cst}^{AT}}{A_{st}^{AT}}$. This assumption is supported by the estimates in Appendix Tables 1 and 2. With this assumption and a derivation parallel to that in Section IV, I can express $\frac{A_{cst}^{C}}{A_{st}^{C}}$ in terms of observable quantities.

Appendix Table 3 estimates complier characteristics using school reports of their minority enrollment and student reports of their gender, minority status, and parental income bracket. The middle columns reveal that in both treatment states, compliers are from poorer families, and are more often males and minorities, than those who opt into testing voluntarily. The final column presents these same statistics from a different perspective: a majority of low-income and minority students would not have taken the exam if they had not been forced.

The first columns of Appendix Table 4 present the share of high-scorers who are compliers and always-takers in each demographic. Across groups, a substantial portion of the compliers from every subpopulation wind up with competitive scores, and generally about 30-40 percent of students with scores above 18 would not have taken the test in the absence of a mandate. Compliers account for around 40 percent of competitive scoring within groups typically associated with disadvantage (low income and minority students and students from high-minority high schools), and around 30 percent of competitive scoring within other student groups. Thus, students who can earn high scores are less likely to take the ACT if they are from minority groups.

The last columns of Appendix Table 4 present the share of always-takers and compliers who are high scorers in each demographic. These statistics are useful in calculating the share of high-scoring compliers (or always-takers) with particular characteristics. Using Bayes' Rule, $Pr(minority|high\ scoring\ complier) = \frac{\Pr(high\ scoring\ complier|minority)\times\Pr(minority)}{\Pr(high\ scoring\ complier)}$. Substituting in values from Appendix Tables 3 and 4, I estimate that 30 percent of high-scoring compliers in Colorado, and 40 percent in Illinois, are minority students. These figures can be compared with the 20 percent of high-scoring always-takers in each mandate state that are minorities. A similar series of calculations demonstrates that between 30 and 40 percent of high-scoring compliers are from the bottom income quintiles, compared to just 15 percent of high-scoring always-takers.

Appendix C: Bounding P for LE Compliers

This exercise generates a rough estimate for the upper bound of P for the LE compliers. Recall from the text that these students would like to enroll in selective schools but perceive that their probabilities of achieving a high score are below some threshold, $P < \frac{T}{U_S - U_U}$. Define $\bar{p} \equiv \frac{T}{U_S - U_U}$.

First, I bound the numerator, T – the cost to the student of taking the test. $25/hour would be an implausibly large time cost, incorporating the disutility of sitting for the test. (Note that this is well above minimum wage, and teenage unemployment rates are high.) The test is about three hours in length, so allowing an additional hour for transportation and administration time, the full amount a student would need in exchange for taking the exam is $100. To this must be added the direct cost of signing up for the test, $35 in 2012 (for the version of the exam without the writing portion). Therefore, under extremely conservative assumptions, the total cost of taking the exam is $150. Thus, any student who perceives the net present value of taking the exam to be $150 or more will take the test even without a mandate.

Next, I consider the denominator, the additional value accrued from attending a selective school. I model the calculation after Cohodes and Goodman (2012). Black and Smith (2006) find that a one standard deviation increase in quality causes earnings to rise by 4.2 percent. To use this, I need to convert the difference in quality a student in my sample experiences from electing the selective school over the unselective school into standard deviation units. Following Cohodes and Goodman (2012), I first construct a continuous measure of "college quality" as the first component of a principal components analysis of available college characteristics; specifically, I use each college's student-faculty ratio, detailed Carnegie classification, dichotomous Barron's competitiveness measure, and open enrollment status.[56] The gain in college quality associated with moving from an unselective to selective college—estimated by scaling the average mandate-induced change in college quality by the fraction of students induced to take the test—is 0.60 standard deviation. Average lifetime earnings for college graduates are approximately $3.3 million (Pew Research Center Social and Demographic Trends, 2011).[57] So, according to the

[56] I weight the principal component analysis by first-time, first-year enrollment figures to give colleges with more students more significance, and then standardize the resulting quality measure to have mean zero and standard deviation of one.

[57] By using earnings of graduates, I am assuming that the student in question will graduate. Note, however, that graduation rates are higher in selective than in unselective colleges, and while the simple difference may overstate

Black and Smith result, a student stands to gain $0.042 \times \$3{,}300{,}000 = \$139{,}000$ in lifetime earnings for every standard deviation increase in quality, or around \$80,000 by switching from unselective to selective enrollment among the colleges in my sample.

Therefore, for any rational student not to take the exam, $P < \frac{\$150}{\$80{,}000}$, or $P < 0.0019$. Hence, a student interested in attending college must believe she has a less-than-0.19 percent probability of passing in order to opt out of the exam, or $E[P|LE] < 0.0019$.

The above calculation assumes that all students value attending a selective college (relative to an unselective college) at \$80,000. This might be too high, either due to heterogeneity in the returns to selectivity or to understatement of the discount rate that some students apply to long-run earnings changes. For instance, Cohodes and Goodman (2012) find that students value differences in college quality with an extremely large discount rate. By their estimates, students are willing to sacrifice about \$110,000 of lifetime income for about \$7,000 in free tuition. Adjusting my estimates for these low values of selective schooling, reduces the value of a high score to about \$5,000 ($\$0.06 \times \$80{,}000$), and thus increases the estimate of $\bar{p}$ to 0.03, so that $E[P|LE] < 0.03$.

the causal effect, it appears to be positive (Cohodes and Goodman, 2012). Thus, my calculation probably understates the benefit of matriculating at a selective college.

Appendix D: Simulating Luck

In the main text, I assigned an upper bound on $E[P^*|LE]$ that could be consistent with the estimated share of compliers who were observed to earn "passing" scores and with the effect of the mandate on college-going. An even tighter bound that would be consistent with rational decision-making can be obtained by assuming that potential test-takers know their own ability and are uncertain only about how lucky they will be on the test day. In this Appendix, I ask whether this "luck" effect is large enough to account for the effect I find of the mandates on enrollment, given the full distribution of test scores I observe (rather than only the passing rate).

Under an assumption that students make rational test-taking decisions based on accurate estimates of P_i^*, I demonstrate that, in order to match the 10 percent share of all compliers that go on to competitive schools, it would have to be the case that students opt out of the exam whenever their pass rates are below 40 to 45 percent. This far exceeds the threshold implied by any plausible estimate of the ratio of the costs of test-taking to the benefits of attending a selective school.

To estimate this, I assume that students are fully informed about their own ability, as measured by the ACT. We can write the ACT test score, s_i as the sum of ability, A_i^*, and a luck component, ε_i, which reflects idiosyncratic influences on the test (e.g., the student has a cold on test day). Assume that A_i^* and ε_i are normally distributed and independent, with:

- $A_i^* \sim \mathrm{N}(\mu_{A^*}, \sigma_{A^*}^2)$
- $\varepsilon_i \sim \mathrm{N}(0, \sigma_\varepsilon^2)$

Suppose there is a known threshold, $\bar{s}$, that a student's s_i must meet or surpass in order to enroll in the selective college. The condition for a "passing" test score is thus: $s_i \geq \bar{s}$. Student i's probability of passing, given her ability, can be written:

$$P_i^* = P^*(A_i^*) = \Pr(A_i^* + \varepsilon_i \geq \bar{s}|A_i^*) = \Pr(\varepsilon_i \geq \bar{s} - A_i^*|A_i^*) = 1 - F_\varepsilon(\bar{s} - A_i^*|A_i^*),$$

where F_ε is the cumulative distribution function of ε.

I assume that students take the test voluntarily if $P_i^* > c$, so compliers consist of those students for whom $P_i^* < c$. Then the complier pass rate is:

$$E[P_i^*|P_i^* < c] = \int_0^{P^{*-1}(c)} [1 - F_\varepsilon(\bar{s} - A_i^*)] dF_{A_i^*}$$

I assume that the ACT's test-retest reliability is 0.9 and compute σ_ε as: $\sigma_\varepsilon = \sigma_S\sqrt{1-0.9}$.[58] I estimate σ_{A^*} and $\sigma_{A^*}^2+\sigma_\varepsilon^2$ from the empirical score distributions in each state in 2004. For each, I generate 1,000,000 draws of A_i^*, which together with my estimate of σ_ε and an assumed value of $\bar{s}$, yields 1,000,000 observations of P_i^*. Incrementing values of c by hundredths for values between 0 and 1, I calculate the mean passing rate among test-takers who have $P_i^* < c$. Appendix Figure 2 shows the implied mean passing rate among compliers, assuming $\bar{s} = 18$, as a function of c, for Illinois and Colorado.

The graph also plots dashed horizontal lines at the estimated competitive attendance rate among compliers within each state. This can be taken as a lower bound to the actual pass rate – if there are any high-scoring compliers who are not interested in attending selective colleges, the true pass rate exceeds this rate.

The horizontal lines fall quite high in the distribution. They indicate that one would expect to see the observed effects on selective college enrollment if students take the test only when their anticipated probabilities of passage exceed 40 percent (in Colorado) or 45 percent (in Illinois). The calculations in Section V would support this kind of a decision rule only if the benefit of enrolling in a selective college rather than a nonselective one were smaller than $330.

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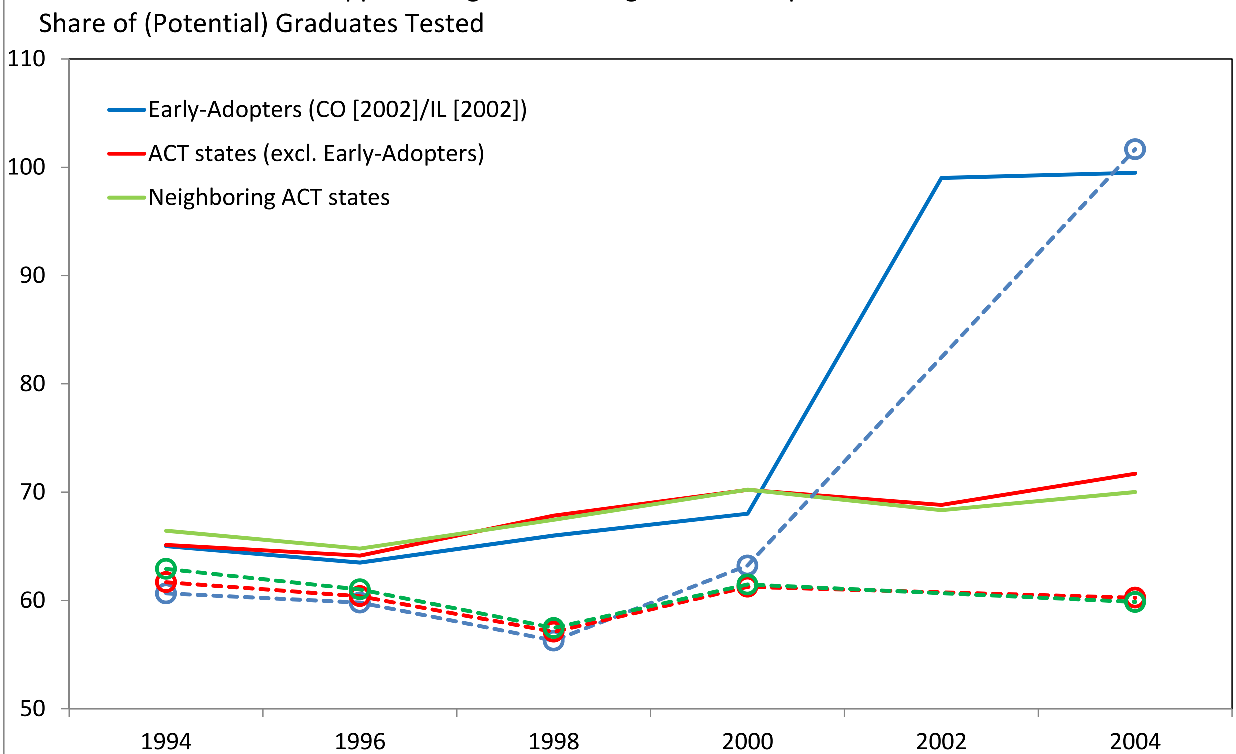
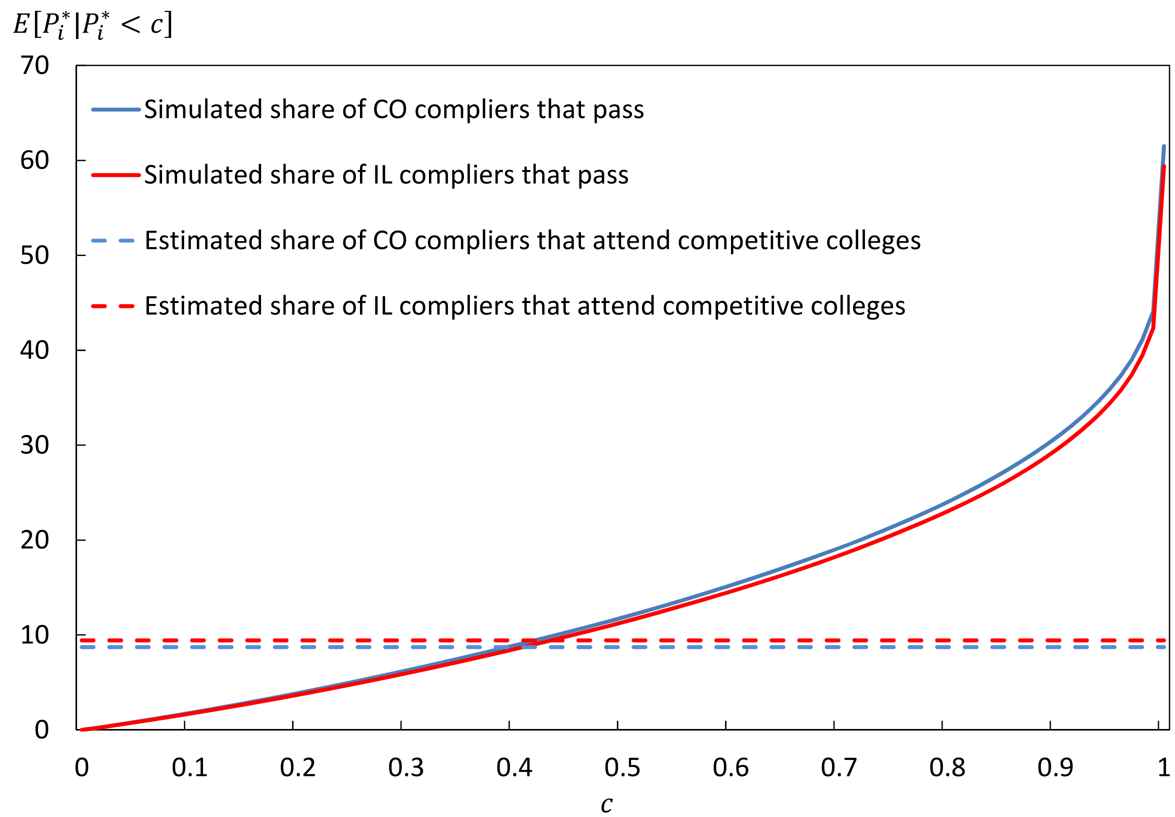
[58] The test-retest reliability is derived from two years of published estimates for the ACT composite score—reliability was 0.97 for test dates in 1995 and 0.96 in 2005-6. (See: http://www.act.org/aap/pdf/ACT_Technical_Manual.pdf). Reliability estimates are similar for the SAT.

Appendix Figure 1: Average ACT Participation Rates



Note: Solid lines reflect published participation rates; open circles reflect participation rates constructed from matched ACT-CCD data. "ACT states" include later-adopting states: Kentucky (2010), Michigan (2008), and Tennessee (2010). "Neighboring ACT states" include Kentucky.

Appendix Figure 2: Simulated Pass Rates for Compliers



Appendix Table 1: Female Share

	ACT Data					CCD Data	
	1994	1996	1998	2000	2004	2000	2004
Iowa	54	55	56	55	54	49	49
Kansas	54	54	53	55	54	49	49
Kentucky	56	56	57	57	57	50	50
Missouri	55	56	57	58	56	49	49
Nebraska	53	53	54	54	54	49	49
New Mexico	55	56	55	57	57	49	49
Utah	53	53	54	54	53	49	50
Wisconsin	55	56	57	57	56	49	49
Wyoming	55	55	58	56	54	49	49
Colorado*	54	55	56	55	50	50	49
Illinois	54	55	55	55	51	50	50

Note: Colorado CCD data for 2000 uses 1999-2000 12th graders rather than 1998-1999 11th graders.

Appendix Table 2: Minority Share

	ACT Data					CCD Data	
	1994	1996	1998	2000	2004	2000	2004
Iowa	7	7	7	7	9	6	8
Kansas	14	15	15	16	18	15	18
Kentucky	12	13	13	12	15	10	11
Missouri	14	14	15	17	18	16	16
Nebraska	9	9	10	11	13	11	14
New Mexico	53	55	57	58	61	58	61
Utah	8	9	9	9	13	9	13
Wisconsin	10	10	11	11	13	12	14
Wyoming	11	11	11	11	12	9	10
Colorado*	23	24	25	26	34	22	27
Illinois	29	28	29	30	36	31	33

Note: Colorado CCD data for 2000 uses 1999-2000 12th graders rather than 1998-1999 11th graders.

Appendix Table 3: Complier Characteristics

Colorado

Characteristic	Pre-reform Test-takers	2004 Test-takers			Complier Share of 2004 Test-Takers
	Always-Takers	All	Always-Takers	Compliers	Compliers
Female	55%	50%	55%	45%	39%
Male	45%	50%	45%	55%	50%
From a High-Minority HS	30%	42%	35%	50%	53%
Not From a High-Minority HS	70%	58%	65%	50%	38%
Minority	25%	34%	28%	41%	54%
Non-minority	75%	66%	72%	59%	40%
Bottom Income Quintile	22%	26%	20%	33%	57%
2nd - 4th Quintiles	60%	61%	65%	57%	41%
Top Income Quintile	18%	13%	15%	10%	34%

Illinois

Characteristic	Pre-reform Test-takers	2004 Test-takers			Complier Share of 2004 Test-Takers
	Always-Takers	All	Always-Takers	Compliers	Compliers
Female	55%	51%	55%	46%	35%
Male	45%	49%	45%	54%	43%
From a High-Minority HS	36%	46%	39%	56%	47%
Not From a High-Minority HS	64%	54%	61%	44%	32%
Minority	29%	36%	32%	42%	46%
Non-minority	71%	64%	68%	58%	35%
Bottom Income Quintile	24%	29%	22%	39%	53%
2nd - 4th Quintiles	58%	58%	62%	51%	34%
Top Income Quintile	18%	13%	16%	10%	29%

Appendix Table 4: Scores by Characteristics

Characteristic	Share of High-Scorers who are Always-takers	Share of High-Scorers who are Compliers	Share of Always-takers who Earn High Scores	Share of Compliers who Earn High Scores
Colorado				
All	68%	32%	79%	47%
Female	73%	27%	80%	47%
Male	63%	37%	80%	48%
From a High-Minority HS	63%	37%	70%	37%
Not From a High-Minority HS	71%	29%	86%	58%
Minority	60%	40%	64%	37%
Non-minority	67%	33%	86%	64%
Bottom Income Quintile	57%	43%	64%	37%
2nd - 4th Quintiles	70%	30%	81%	49%
Top Income Quintile	71%	29%	92%	74%
Illinois				
All	75%	25%	76%	40%
Female	76%	24%	74%	43%
Male	72%	28%	77%	39%
From a High-Minority HS	66%	34%	62%	35%
Not From a High-Minority HS	80%	20%	85%	47%
Minority	63%	37%	55%	39%
Non-minority	72%	28%	85%	61%
Bottom Income Quintile	54%	46%	50%	38%
2nd - 4th Quintiles	81%	19%	81%	36%
Top Income Quintile	75%	25%	92%	75%

Note: A high-scorer earns a score greater than or equal to 18.

Appendix Table 5. Gradations of Selectivity According to the Barron's College Admissions Selector

Category	Criteria	Share Admitted	Example Schools
Most Competitive	HS rank: top 10-20%	<1/3	Amherst College, MA
	GPA: A to B+		Brown University, RI
	Median SAT: 1310-1600		Middlebury, VT
	Median ACT: 29+		Tufts University, MA
Highly Competitive	HS rank: top 20-35%	1/3 - 1/2	Brandeis University, MA
	GPA: B+ to B		George Washington University, DC
	Median SAT: 1240-1308		SUNY Binghamton, NY
	Median ACT: 27-28		Vanderbilt University, TN
Very Competitive	HS rank: top 35-50%	1/2 - 3/4	Alfred University, NY
	GPA: B- and up		American University, DC
	Median SAT: 1146-1238		Fordham University, NY
	Median ACT: 24-26		George Mason University, VA
Competitive	HS rank: top 50-65%	75% - 85%	Hofstra University, NY
	GPA: C and up		Quinnipiac University, CT
	Median SAT: 1000-1144		SUNY Buffalo, NY
	Median ACT: 21-23		UC Davis, CA
Less Competitive	HS rank: top 65%	85% or more	San Francisco State University, CA
	GPA: below C		SUNY Farmingdale, NY
	Median SAT: below 1000		UT Arlington, TX
	Median ACT: below 21		UWisconsin/Milwaukee, WI
Noncompetitive	HS graduate	98% or more	CUNY York, NY
			UT El Paso, TX
			UT San Antonio, TX
			Wilmington College, DE

Source: Barron's Profiles of American Colleges 2001

Appendix Table 6: Differences in Shares of Additional Types of Enrollment between 2000 and 2002

	Mandate Status in 2002				
	Mandate: CO and IL		No Mandate: Other ACT States		
	Average (2000)	Difference (2002–2000)	Average (2000)	Difference (2002–2000)	Difference in Difference
Subcategories of Enrollment (as Share of Population)					
Four-Year	35.7%	2.7 p.p.	36.8%	1.2 p.p.	1.5 p.p.
Selective and					
Land Grant	4.9%	0.4 p.p.	7.2%	0.2 p.p.	0.2 p.p.
Public	21.0%	1.5 p.p.	20.1%	0.5 p.p.	1.0 p.p.
Private	9.3%	0.7 p.p.	6.8%	-0.1 p.p.	0.9 p.p.
Private Not-for-Profit	8.7%	0.7 p.p.	6.6%	-0.1 p.p.	0.8 p.p.
In-State	22.3%	1.4 p.p.	20.4%	0.5 p.p.	0.9 p.p.
Out-of-State	8.1%	0.9 p.p.	6.6%	-0.1 p.p.	1.0 p.p.
In-State and					
Selective-Land Grant	3.7%	0.3 p.p.	6.3%	0.2 p.p.	0.1 p.p.
Selective-Public	17.5%	1.1 p.p.	17.6%	0.4 p.p.	0.7 p.p.
Selective-Private	4.7%	0.3 p.p.	3.7%	0.0 p.p.	0.2 p.p.
Out-of-State and					
Selective-Land Grant	1.2%	0.1 p.p.	1.2%	0.0 p.p.	0.1 p.p.
Selective-Public	3.5%	0.4 p.p.	3.3%	0.1 p.p.	0.3 p.p.
Selective-Private	4.6%	0.4 p.p.	3.2%	-0.2 p.p.	0.6 p.p.
18-year-old Population	118,114	278	53,196	-129	407
States in Group	*2*		*23*		
ACT Participation Rate (published)	68%	31 p.p.	70%	-1 p.p.	32 p.p.