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

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Abstract

In the last decade, five U.S. states adopted mandates requiring high school juniors to take a college entrance exam. In the two earliest-adopting states, nearly half of all students were induced into testing, and 40-45% of them earned scores high enough to qualify for selective schools. Selective college enrollment rose by 20% following implementation of the mandates, with no effect on overall attendance. I conclude that a large number of high-ability students appear to dramatically underestimate their candidacy for selective colleges. Policies aimed at reducing this information shortage are likely to increase human capital investment for a substantial number of students.

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I. Introduction

The disparity in college attendance between children from low- and high-income families has been increasing over time (Bailey and Dynarski, 2011).¹ Meanwhile, over the past 35 years, earnings have been essentially steady among the college-educated and have dropped substantially for everyone else (The College Board, 2007; Deming and Dynarski, 2010). Earnings are correlated with not just the level of an individual's education, but also the caliber of college she attends (Hoekstra, 2009; Card and Krueger, 1992; Black and Smith, 2006).^{2,3} And while disadvantaged students, in particular, appear to gain the most from selective schools (McPherson, 2006; Dale and Krueger, 2011; Dale and Krueger, 2002; Saavedra, 2008), they are vastly underrepresented at top institutions (Bowen, Kurzweil, and Tobin, 2005; Hill and Winston, 2005; Pallais and Turner, 2006; Hoxby and Avery, 2012).^{4,5}

These trends underscore the importance of education policies that raise access to higher education, and especially highly selective schools, among disadvantaged students. Financial aid programs alone have not been able to close the educational gap that persists between socioeconomic groups (Kane, 1995). It is thus important to understand other factors, amenable to intervention, that contribute to disparities in postsecondary access and enrollment.⁶

Providing information to students at critical junctures, such as when they are finalizing their postsecondary enrollment decisions, may help address these disparities. Much research, mostly by psychologists and sociologists, has examined the profound effect a student's experiences and the expectations of those around her have on the expectations and goals she sets for herself (see Figure 1 in

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 More recently, educational attainment has been causally linked to institutional quality (Cohodes and Goodman, 2012).

⁴ Hill and Winston (2005) find that 16% 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% less likely to even apply to selective schools than their equally-high-scoring, higher-income counterparts.

⁵ These findings rely on admissions test data in which disadvantaged students are also vastly underrepresented. Only 30% of students in the bottom income quartile elect to take these exams, compared to 70% 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). Therefore, the shortage of these students at and applying to top schools is probably even larger than conventional estimates suggest.

⁶ 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% (Bettinger et al., forthcoming). Other related experiments have sought to simplify the overall college application process: one assisted disadvantaged students in selecting a portfolio of colleges and led to subsequent enrollment increases (Avery and Kane, 2004); a more recent intervention provided semi-customized information about the college application process to students and generated increased applications and admissions predominantly among the low-income high-achievers (Hoxby and Turner, 2013). 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).

Jacob and Wilder, 2010).⁷ Jacob and Wilder (2010) demonstrate that these expectations—however misaligned with ability they may be—are strongly predictive of later enrollment decisions, but that they are also malleable⁸: high school students’ future educational plans appear to fluctuate with the limited new information available in their GPAs.

To shed light on whether an intervention in this area could be successful, this paper examines whether students’ beliefs about their own suitability for selective colleges contribute to observed disparities in college attendance. I exploit recent reforms in several states that required high school students to take entrance exams necessary for admission to selective colleges. A side-effect of compulsory testing is the new availability of a salient test score—a direct measure of candidacy for selective schools—for students who would not otherwise have taken the exam. 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 information on students’ educational plans.

I demonstrate that the mandates can be linked to large increases in test participation and subsequent selective college enrollment. Many high-ability, low-income students appear to have downward-biased estimates of their probabilities of admission to selective colleges and, as a result, they do not choose to take the exam unless required to. When a mandate is introduced, they take the test, learn that they are admissible, and apply to and attend selective colleges.

This paper makes two important contributions. First, I reveal the existence of a sizable subgroup of high-ability teens interested in attending a selective school who drastically underestimate their candidacy; thus, I offer a new behavioral rationale that can help explain the widening educational attainment and earnings gap. Second, by showing that many students induced into testing by the new requirements go on to attend selective schools, I demonstrate that providing students with low-cost information at very late stages of adolescence can have a large impact on educational choices. Thus, I expose a new and inexpensive area for policy intervention that would likely increase human capital investment among a high-need population of students.

In the last decade, five U.S. states have adopted mandatory ACT testing for their public high school students.⁹ 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

⁷ 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).

⁸ See also Stinebrickner and Stinebrickner (2012), Zafar (2011), and Stange (2012).

⁹ One state, Maine, has mandated the SAT, an alternative college entrance exam.

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.¹⁰

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% 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.¹¹

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% (depending on the precise selectivity measure) relative to control states in the years following the mandate.

My results imply that about 20% of the new high scorers (or 10% of students induced into testing by the mandates) enroll 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 addresses 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. Thus, I provide the first clear

¹⁰ 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.

¹¹ For the purposes of my analysis, students who, absent a mandate, would have taken the SAT exam to satisfy their college admissions requirements are included in the “uninterested” group, as their college-going behavior is unexpected to change under the mandate.

evidence of a causal link between secondary students' perceptions of their own ability and their postsecondary educational choices, and 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.¹²

The rest of the paper proceeds as follows. Section II provides background on the ACT and the ACT mandates. Section III counts and characterizes the students induced into testing by the mandates. Section IV provides a model of information and test participation decisions. Section V presents estimates of the enrollment effects of the mandates. Section VI 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 VII 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 they 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.¹³ 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.¹⁴

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, which includes the option to send score reports to up to four colleges.¹⁵ The scores supplement the student's secondary school record in college admissions, helping to benchmark locally-normed performance measures like GPA. According to a recent ACT

¹² 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.

¹³ 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.

¹⁴ Some students might favor a particular test due to their different formats or treatment of incorrect responses.

¹⁵ The cost is \$35 if the Writing section is omitted. Additional score reports are around \$10 per school for either test.

Annual Institutional Data Questionnaire, 81% of colleges require or use the ACT 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% 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% 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% 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.¹⁶ 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.¹⁷

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

¹⁶ In addition, one state (Maine) mandates the SAT.

¹⁷ 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 2002 graduating cohort.¹⁸ Kentucky, Michigan, and Tennessee each adopted mandates more than five years later.

Figure 1 presents 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”¹⁹ 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% to just over 70% over the last 16 years. By contrast, in the early adopting states participation jumped enormously (from 68% to approximately 100%) 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 demonstrates that the mandate programs had large effects on ACT participation, that compliance with the mandates is nearly 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 effects within the other ACT mandate states, and contextualize them using the longer-term findings from Colorado and Illinois.

III. The Effect of the Mandates on Test-taking and the Score Distribution

In Appendix A, I describe the data on test-takers that I will use to identify mandate-induced test-taking increases and outcomes. In this section, I present key summary statistics demonstrating that the test-takers drawn in by the mandates were disproportionately lower-scoring, minority, and lower-income relative to pre-mandate test-takers. Then, I investigate shifts in the score distribution following the introduction of a mandate. Adjusting for cohort size, many of the new test-takers in mandate states score

¹⁸ 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.

¹⁹ 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.

within a competitive range, suggesting a large portion obtained ACT scores high enough to qualify for admission to competitive colleges.

Table 2 presents average test-taker characteristics in my sample. 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% in Illinois; in each case the neighboring states saw growth of less than 10%.

Predictably, forcing all students to take the test lowers the average score: both treatment states experienced about 1½-point drops in their mean scores after their respective mandates took effect. Similarly, the mean parental income is lower among the post-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.^{22,23}

Figure 2 plots the score frequencies for the two treated states and their neighbors for the years before and after treatment.²⁴ The plots include a vertical line at a composite score of 18, reflecting a conventionally used threshold by colleges with the most liberal, but still competitive, admissions processes.²⁵

In both of the treatment states, the change in characteristics presented in the last section appeared to shift the ACT score distribution to the left. Moreover, the distributions, particularly in Colorado,

²⁰ The ACT survey asks students to estimate their parents' pretax income according to up to 9 broad income categories, which vary across years. To make a comparable measure over time, each student's selection is recoded to the midpoint of the provided ranges (or the ceiling and floor of the categories noted above, respectively), and income quantiles are calculated within each year.

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/non-Hispanic," and 0 if a test-taker selects white.

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

²³ 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. Moreover, plotting the biennial demographic and test score data in mandate states reveal patterns that mimic each of their respective composite states, with a sharp divergence between the mandate and non-mandate states in 2004 for both samples. Figures not included but available upon request.

²⁴ In order to better display the growth in the test-taking rate over time, frequencies are not scaled to sum to one. To abstract from changes in cohort size over time, pre-treatment score cells are rescaled by the ratio of the total CCD enrollment in the earlier period to that in the later period.

²⁵ 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 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% of high school graduating class (ACT: 20-23); and a "selective" admissions school tends to accept freshmen in top 25% of high school graduating class (ACT: 22-27). (See <http://www.act.org/newsroom/releases/view.php?year=2010&p=734&lang=english>.) According to a recent concordance (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.

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 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% in Colorado and 55% in Illinois, even after adjusting for changes in the size of the graduating class; at scores above 23, the growth rates were 40% and 25%, respectively.

However, since there were (small) changes in score distributions in non-mandate states as well as in mandate states, I employ a difference-in-differences strategy 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 “compliers”, respectively (Angrist, Imbens, and Rubin, 1996).²⁶

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).

The first step is to identify the number of compliers. My key assumption is that absent the policy the (voluntary) test-taking rate would have evolved in a mandate state the same way it did in its neighbors. In other 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 mandates’ average effects on test-taking – i.e. the share of students induced to take the exam by the mandates – can be estimated with the equation:

$$P_{st} = \beta_0 + \beta_1 \times (\text{mandate}_s \times \text{post}_t) + \beta_2 \times \text{post}_t + \beta_3 \times \text{mandate}_s + \varepsilon_{st} \quad (1)$$

²⁶ 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.

where P_{st} is observed test participation in a given state-year, and β_1 is the parameter of interest, representing the size of the complier group as a share of total enrollment.²⁷ I estimate (1) separately for each of the two early-adopting states and their neighbors, using the five years of matched microdata described in the appendix.

Table 3 summarizes the results. About 45% of 11th graders in Colorado, and 39% in Illinois, are “compliers.” The bottom panel of the table uses these estimates to decompose the number of test-takers into compliers and always-takers.

It is somewhat more complex to identify the score distribution of compliers and requires an additional assumption about the evolution of score distributions in my sample. My estimator relies on the fact that the fraction of all test-takers scoring at any value, r , in a mandate state can be written as a weighted average of compliers and always-takers scoring at that value, where the weights are the share of students each group represents. I recovered estimates for these weights in the last exercise; thus, estimating the share of compliers at any given score, r , only requires knowing the share of always-takers at r .

The score distribution in non-mandate neighboring states—which by definition, reflects the universe of always-takers in those states—provides a convenient counterfactual for scores among always-takers in mandate states. I assume that, absent the mandate, the likelihood that a randomly selected always-taker earns a score of r increases (or decreases) by the same amount in mandate states and their neighbors.²⁸ Repeating this exercise for each score cell, I can fully recover the complier score distribution.²⁹

Table 4 summarizes the results for test-takers scoring below 17, between 18 and 20, between 21 and 24, and above 25. The estimates are broadly consistent with Figure 2 and 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). I

²⁷ Note that an alternative specification of equation (1) is available: $\ln(A_{st}) = \beta_0 + \beta_1 \times (\text{treatment}_{st} \times \text{post}_{st}) + \beta_2 \times \text{post}_{st} + \beta_3 \times \text{treatment}_{st} + \ln(N_{st}) + \varepsilon_{st}$, where A_{st} is the number of test-takers in a state-year, 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.

²⁸ A counterfactual formed from test-taking rates would be analogous to the method used to estimate the weights. Unfortunately, this estimator is unavailable since I do not observe the population distribution of scores in non-mandate state-years. Alternatively, I could use levels instead of rates, but it is less plausible.

²⁹ An advantage of my approach is that it does not require knowing the underlying scoring potential of the at-risk population. A disadvantage is that it poses stringent requirements on the relationship between the at-risk populations and their corresponding test-taking rates. Without these, at least some of the differential changes in the score distribution among test-takers might 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). In Appendix B, I examine the plausibility of this assumption by comparing the test-taker composition to observable characteristics of 11th graders in the matched CCD schools.

estimate that around 40-45% of compliers – amounting to about 20% of all post-mandate test-takers – earned scores surpassing conventional thresholds for admission to competitive colleges.

Appendix C 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. This is true even holding ability levels constant. 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 early in their educational careers, students from these groups do not consider attending selective colleges at the same rate as other students.

IV. 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:

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

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, respectively³⁰; 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

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

³⁰ The descriptive model abstracts away from the difference between attending an unselective college and no college at all. My empirical evidence will support this abstraction.

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.

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 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%.³³ In my case, this implies that $U_S - U_U$ will average around \$80,000.

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 Appendix D, I present a second, highly conservative calculation that

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 smaller effect.

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.

In Section III, I explored the change in the test score distribution surrounding the implementation of the mandate. The results indicate that about 40-45% of compliers attained high-enough scores to qualify them for admission to selective schools, or that $E[P^*|C] \geq 0.40$. In Section V, 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 VI, 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.

V. The Effects of the Mandates on College Enrollment

In this section, I investigate the mandates' reduced-form effects on college attendance, under the assumption that the shifts in enrollment patterns I identify arise among the group of students induced into testing by the mandates described in Section III. I show that the mandates led to large increases in enrollment at selective colleges, with no discernible effect on overall college attendance. I use this evidence to argue that the students whose postsecondary plans were affected by the mandates were likely already college-bound but attended more-selective schools as a result of the new requirement. To motivate my analysis, I briefly describe the potential effects of compulsory ACT testing on enrollment.

A. Potential Effects on Enrollment

A key byproduct of required testing is the availability of an exam score for students who would not otherwise have taken the ACT.³⁴ The score may contain new information for the mandate compliers, either revealing their true ability or simply their admissibility to selective colleges. Consider two distinct decisions that might be affected by this information: one, the decision to attend college, and two, the decision to attend a selective college (as opposed to an unselective or open admissions college).

Whether to attend college at all is a complex function of individual-specific returns to college attendance and the opportunity cost of college each student faces. Further, it is unclear whether the return to college is an increasing, decreasing, or non-monotonic function of test scores. 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. Still, it is possible that students use the score as information about whether they can succeed in college (Stange, 2012), in which case the effect

³⁴ The discussion that follows in this section sets aside sticker prices of test-taking and college applications.

on their choice to attend college at all is theoretically ambiguous. Altogether, it appears that the information contained in a student's ACT score would likely have little influence on her decision to enroll in college, and the direction of influence is not obvious.

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 direct function of test scores. Therefore, the new availability of a score, on net, could positively influence her decision to attend a selective school.

B. Estimating Enrollment Effects

In Appendix A, I describe the data on freshmen enrollment that I will use to identify mandate-induced changes in college attendance. Figures 3a and 3b present suggestive evidence linking the ACT mandates to movements in these data. Figure 3a 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 3b 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% between 1994 and 2000 (in part, reflecting increased coverage of the IPEDS survey) among freshmen from the non-mandate states and by 15% among freshmen from the mandate states, while selective and more selective enrollment rose by around 15% 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% above its 2000 level, but only 9% higher among freshmen from the other states.

Table 5 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.³⁵

Table 5 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 from Table 5, I turn to a regression version of the estimator, using data from 1994 to 2008:

$$E_{st} = \beta_0 + \beta_1 \times \text{mandate}_{st} + \mathbf{X}_{st}\boldsymbol{\theta} + \gamma_t + \gamma_s + \varepsilon_{st} \quad (3)$$

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.^{36,37}

\mathbf{X}_{st} represents a vector of controls that vary over time within states. For my primary analyses, I consider three specifications of \mathbf{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 3a makes clear) there is little sign of a relationship

³⁵ 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.

³⁶ 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 6a, 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 6a.

³⁷ Robust standard errors are generally smaller than clustered, except for some instances in Table 6a, 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.

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 5.

Table 6a presents the regression results for the period between 1994 and 2008, where the estimation sample includes all ACT states,³⁸ 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) 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.³⁹

In order for my strategy to identify the true impact of an ACT mandate on freshmen enrollment, college-going trends among students from mandate states would – absent the new requirement – resemble those observed in non-mandate states. To examine this, I follow Autor (2003) and estimate an event-study version of equation (3) that normalizes the mandate year to 0 and includes lead and lag terms for each year of data between 1994 and 2008 (inclusive). Following Table 6a, I estimate the event-study equation for all three categories of enrollment, with and without the two cohort controls. The omitted year is 2000, corresponding to the year just before the mandate is introduced in early-adopting states.

Table 6b displays the results. The coefficients on all of the lead terms are not significantly different from 0; thus, there is no evidence of anticipatory effects or important heterogeneity in enrollment pre-trends. When either of the selective categories is the outcome of interest, the coefficients on the lag terms are all highly significant. (They are zero when overall enrollment is the outcome.) Moreover, the effect size appears to increase sharply over the first 4 years of the policy and then gradually thereafter. I investigate this further in the next section and elaborate on possible explanations in the discussion.

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% when the mandates are introduced. Altogether, the regression results coincide with the descriptive evidence: the

³⁸ The sample omits Michigan, due to: 1) its ACT mandate potentially affecting 2008 enrollees and 2) dramatic decreases in Michigan's state college aid around the timing of mandate adoption (See: http://www.michigan.gov/mistudentaid/0,4636,7-128-60969_61002_61357-279168--,00.html). Main results are not very sensitive to its inclusion.

³⁹ I have also estimated the specifications presented in Table 8 weighting the regressions by population and total enrollment (where applicable). Results are mostly unchanged.

mandate is inducing students who would otherwise enroll in nonselective schools to alter their plans and enroll in selective institutions.

C. Robustness Checks and Extensions

This section explores several alternative specifications. To conserve space, I report results for selective enrollment, controlling for overall enrollment (Panel B, Specification (5) in Table 6a).⁴⁰

Table 7 presents the first set of results. Column (1) reviews the key results from Table 6a. 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 Section III). 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.⁴¹ However, Figures 3a and 3b suggest that the mandate effects appear gradually after the mandates are introduced, a pattern that may be 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:⁴²

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

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% in the first years after mandate implementation and grows to 20% 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% level. There are a number of possible

⁴⁰ Results using other specifications are similar (available upon request).

⁴¹ 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.

⁴² Note that the complier analysis in Section III includes test-taker data that extend only through 2004, corresponding to the period covered by the short-term effect in equation (3^{*}).

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 elaborate on these explanations in the discussion.

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

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

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 in each.⁴³

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 Colorado and Illinois from the sample.⁴⁴ 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.^{45 46}

Finally, Tables 8 and 9 present additional analyses for other measures of selectivity. The regression framework mirrors equation (3) but varies the enrollment measure.⁴⁷ For instance, Table 8 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.

⁴³ 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 6a.

⁴⁴ There is no detectable overall enrollment effect among the later-adopting states (not shown).

⁴⁵ The estimated enrollment effect is almost 50% larger when Michigan, which experienced large state aid decreases around the timing of its mandate, is omitted.

⁴⁶ Including a logarithmic control that accounts for two-year-earlier 11th grade public high school enrollment shrinks the early-adopter results to estimates more closely resembling the results I obtain in column (10); however, adding this control does not reduce the estimated effect in Kentucky and Tennessee. Since analyses of the later-adopting states rely on one year of post-treatment data, I cannot be sure whether the measure is: 1) spuriously linked to selective enrollment increases in the early-adopting states, or 2) generating the increase in the treatment effect over time recovered in columns (5) and (6). Still, without theoretical motivation for its inclusion, I omit 11th grade public high school enrollment from the main analysis. I thank Joshua Hyman for this discovery.

⁴⁷ As in Table 7, Tables 8 and 9 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.

Table 9 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.

VI. Assessing the Test-taking Decision

In the previous sections, I demonstrated that requiring 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%, on average—with no detectable effect on overall college attendance. When I separately consider the early years of implementation, corresponding to the period covered by the test-taker data, the selective enrollment impact is still around 10%. 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% of the mandate compliers in 2004, estimated as 23,000 and 53,000, respectively, in Section III. 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% of selective-college-eligible compliers wound up enrolling in such schools.

In Appendix E, I present 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 roughly 0.16. 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. In other words, many students must have systematically downwardly biased forecasts of their performance.

Figure 4 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%.⁴⁸ 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 4 depict 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).⁴⁹ 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% down to 0.03%.

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

⁴⁸ Heterogeneity in P^* among LE compliers would imply an even higher break-even point. In Appendix F, 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% are choosing not to take the test.

⁴⁹ 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.

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.

VII. Discussion

My analyses demonstrate that ACT mandates led to large increases in ACT participation, in the number of high-scoring students, and in selective college enrollment. In particular, about 40-45% of students induced to take the ACT exam under a mandate earned competitive scores and many of these new high-scorers—about 20%—ultimately enrolled in competitive schools. The fraction of all mandate compliers who achieved high scores and upwardly revised their enrollment plans—about 10%— 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%.

One concern is that the enrollment 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 6-9 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 planned to attend college.

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, 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% in 2010, similar to the estimates for Illinois and Colorado. This strongly suggests that it is the testing mandates themselves—not accountability policies that accompanied them—that account for the enrollment effects.

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

Relatedly, it remains an open question why enrollment effects appear to phase in over the first several years of compulsory testing. There are several candidate explanations: 1) selective colleges are accepting more students from mandate states over time; 2) large state schools are gradually expanding to accommodate admissible mandate compliers; 3) more students from mandate states are earning high scores on the exam over time; and 4) students are becoming better informed about their options over time, by, for example, guidance counselors becoming more knowledgeable about placement options, college recruiting efforts increasing, or successes of students in earlier cohorts from similar backgrounds carving pathways for later cohorts. Note that the first two explanations imply that there is disproportionate growth in the seats at selective colleges for students from mandate states, while the latter two imply that there is disproportionate growth in the number of candidates from these states.

Unfortunately, due to data limitations, I cannot investigate the supply-side explanations or evolutions in college admissions patterns. However, Appendix Figure 4 offers some insight into the viability of the first explanation, that large state schools are gradually increasing their slots for in-state candidates. The figure plots selective enrollment growth among students from mandate states by control and location of the college, estimated from a specification that mimics Table 7, Column (5). For example, the leftmost bars plot the estimated increases at public in-state colleges owing to the mandates, where the first column represents growth in matriculants within the first few years of compulsory testing, and the second after compulsory testing has been in effect for at least four years. There is a notable increased presence of students from mandate states at private schools in their home states and in public schools in other states, suggesting that not all of the phase-in owes to an expansion of slots for students at large in-state public schools. Further research in this area is needed.

Another concern might be the generalizability of the policy effects: what could we expect if we were to institute nationwide compulsory testing? For the most part, the estimates I present are based on two widely separated states that implemented mandates. While the total increase in selective enrollment represented about 15% of prior selective enrollment among students from those states, the new enrollees

amounted to only 1% of total national selective enrollment and 5% of selective enrollment from the mandate states and their neighbors.

Still, one hurdle to relying on the experiences in Illinois and Colorado 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).^{50,51} 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. Over the sample period national selective enrollment grew by about 2% each year. This implies that the increase in enrollment produced by a national mandate, which I estimate at around 15%, 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.

In sum, the evidence overwhelmingly suggests that students are systematically under-predicting their suitability for selective schools. I demonstrate that students on average take admissions tests far 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.

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.

⁵⁰ 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.

⁵¹ 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. However, it is worth noting that given the relatively large increases in selective out-of-state and private in-state enrollment generated by the mandates (Table 9), there is some indication mandate compliers might compete for admissions slots with always-takers if these policies were scaled up.

Recent studies have demonstrated that college quality is an important determinant of later success.⁵² 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, requiring 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 D, I can link my results to Black and Smith's findings that an additional standard deviation in college quality produces a 4.2% 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% of students who are induced to change their enrollment status by the mandate should, on average, expect a boost of 23% in their lifetime earnings.⁵³

In a separate but related line of research, Pallais and Turner (2006) establish 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 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.

⁵² 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.

⁵³ According to figures on average lifetime earnings of BA holders (Pew Research Center Social and Demographic Trends, 2011), 23% of lifetime earnings amounts to about \$760,000.

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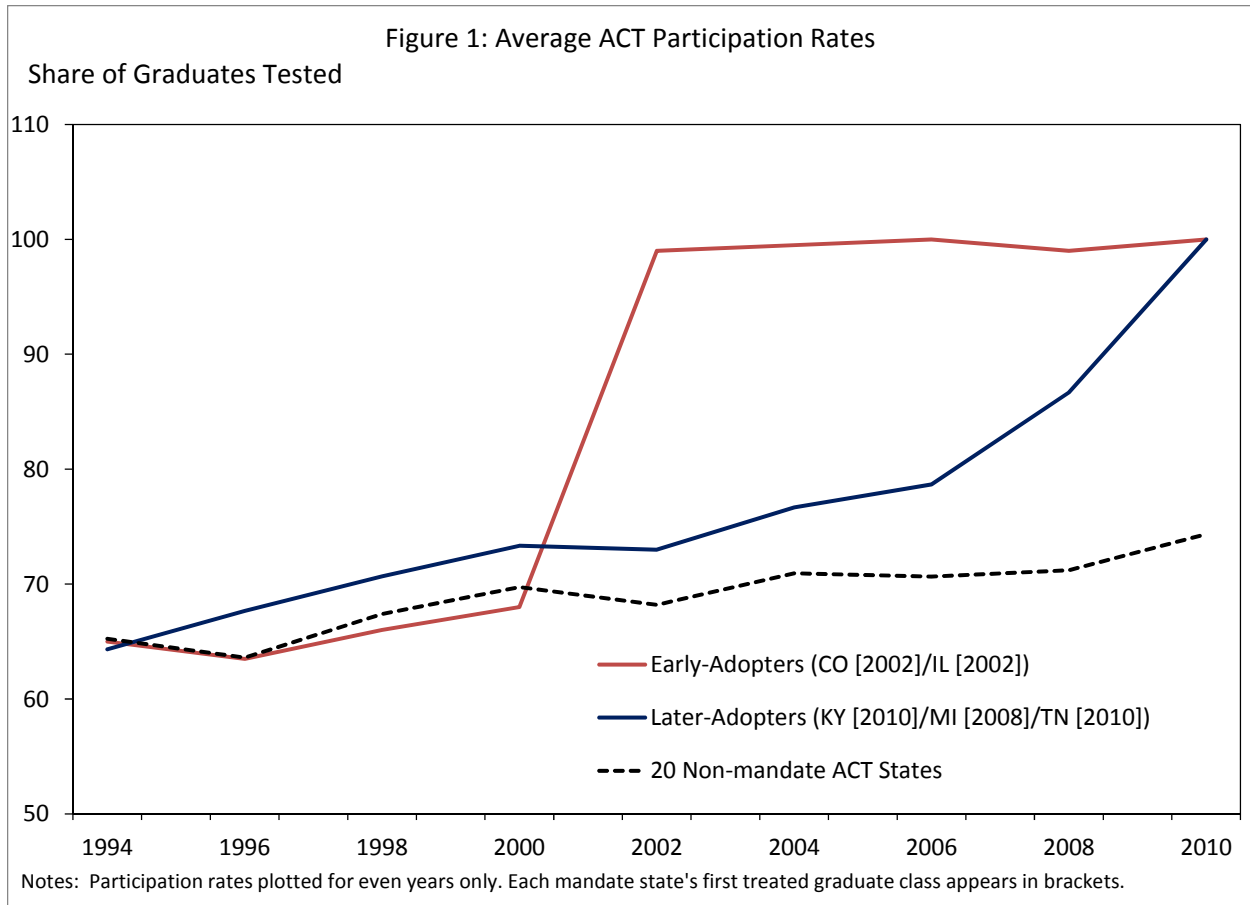
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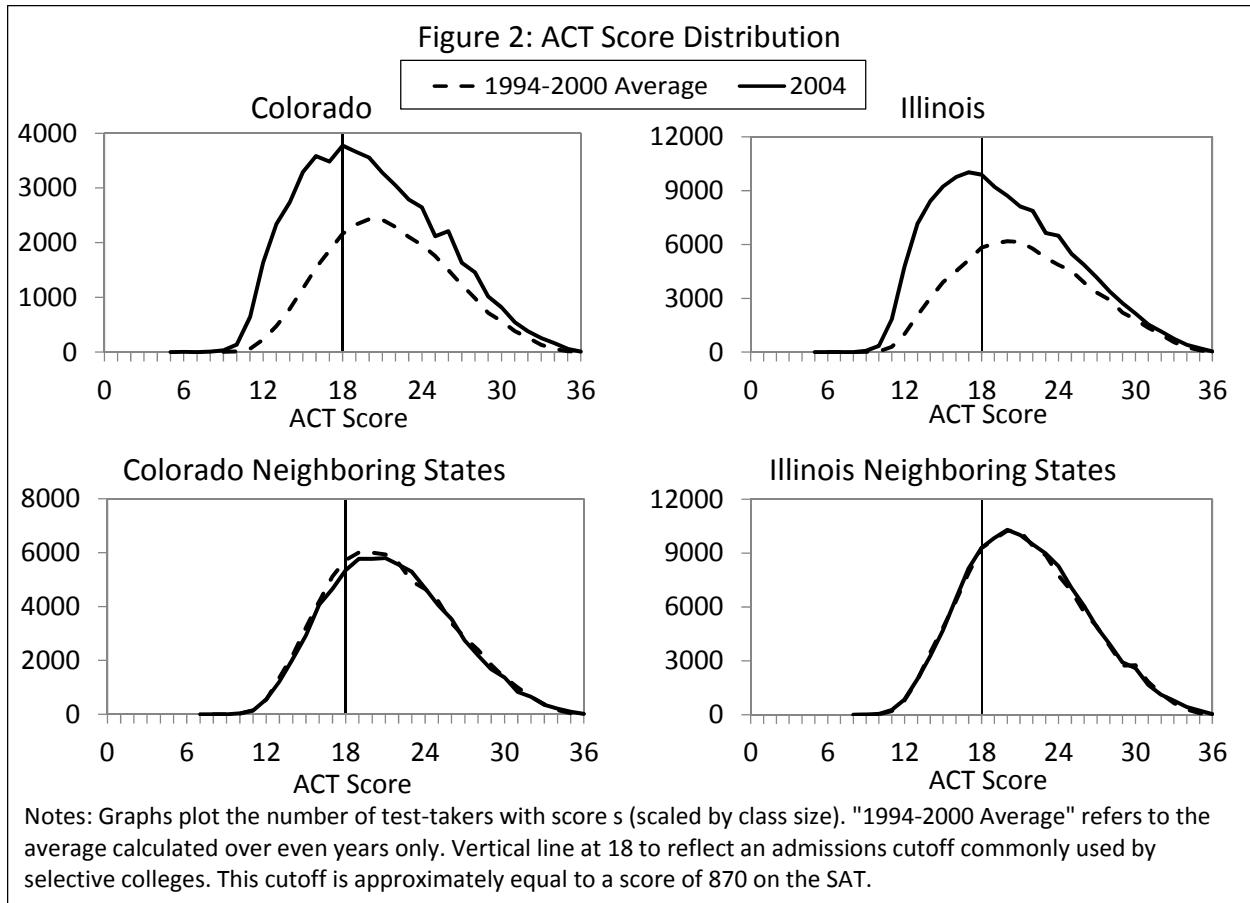


Figure 3a: Overall College Attendance in the United States by State of Residence

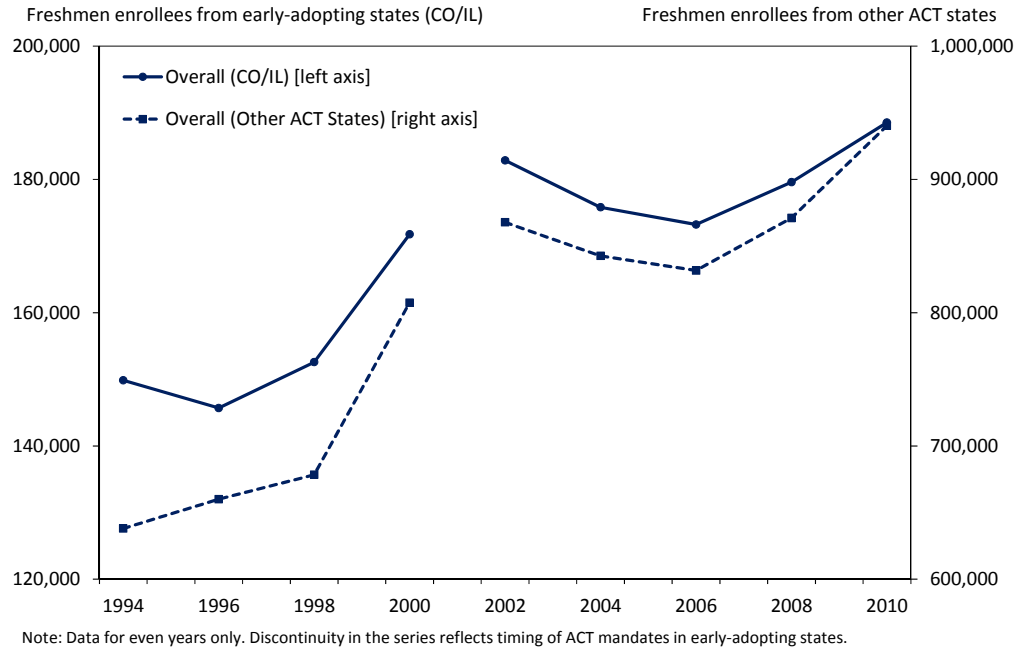
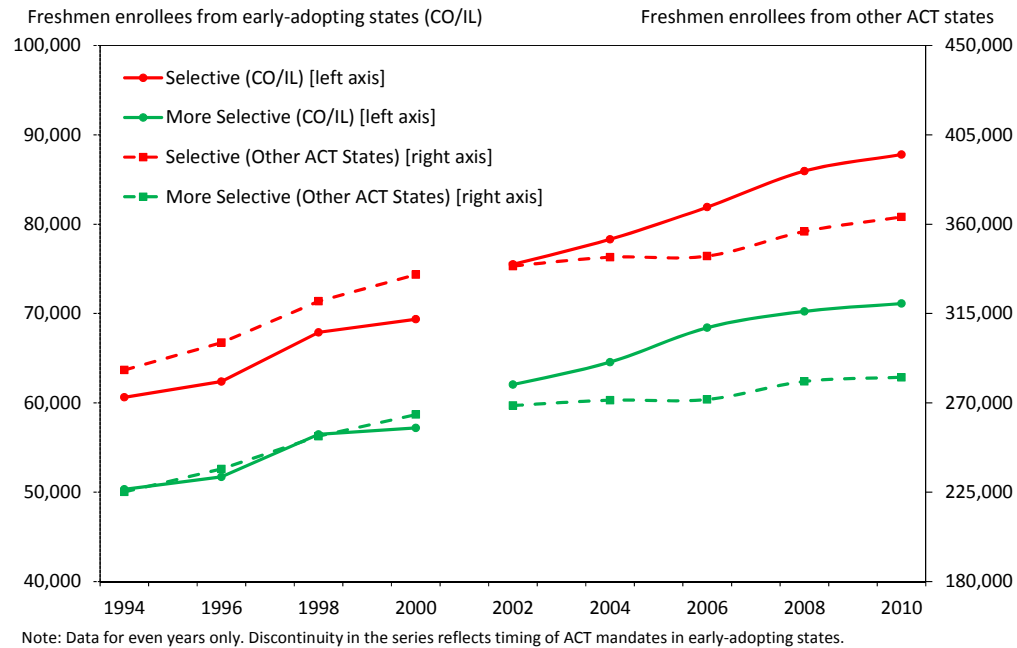


Figure 3b: Selective College Attendance in the United States by State of Residence



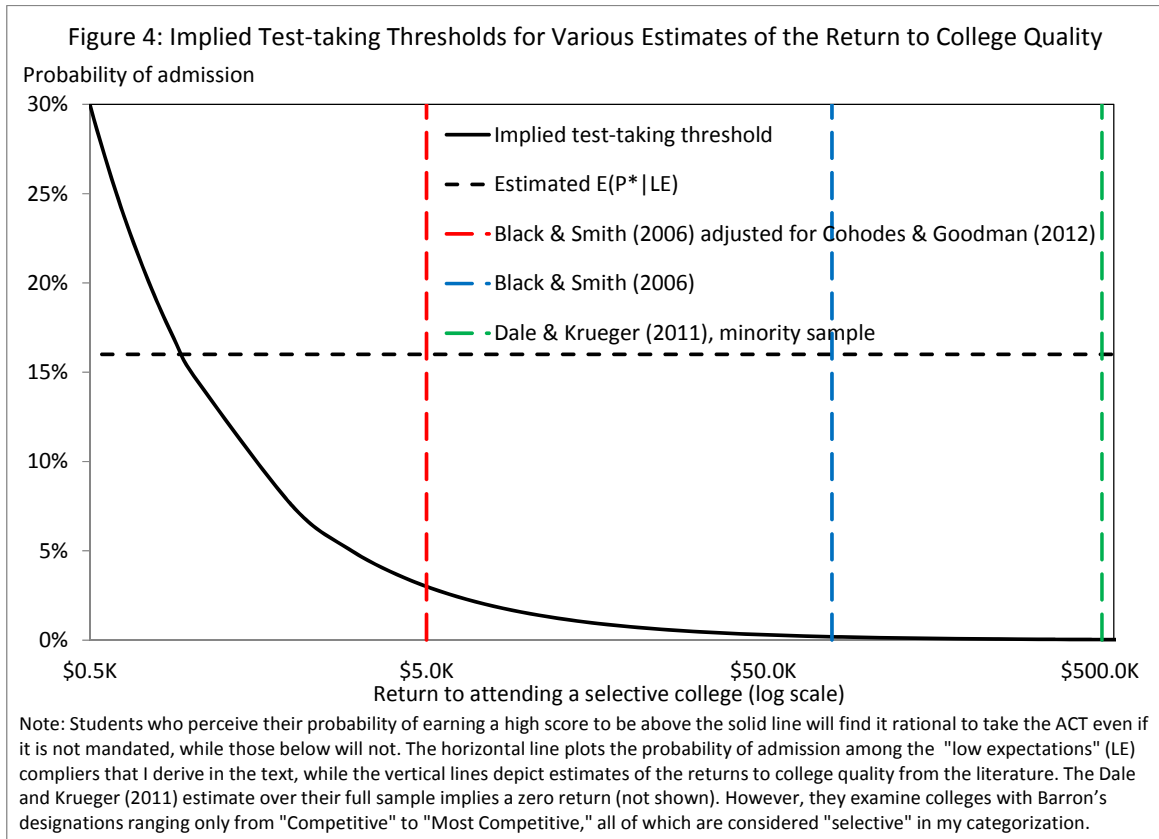


Table 1: State ACT Mandate Timing

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

Period	State or State Group	Number of States	Number of Test-takers	Average		Share 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. Count of test-takers in neighboring states reflect the average across states in each group.

Table 3: Estimated Mandate Effect on Test-taking

Dependent variable	Participation Rate (0 to 1)	
	Colorado	Illinois
Treatment state		
$\text{treatment}_s * \text{post}_t$	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

Score Range	(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)
	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: Differences in Key Characteristics between 2000 and 2002

	Mandate Status in 2002				Difference in Difference
	Mandate: CO and IL		No Mandate: Other ACT States		
	Average (2000)	Difference (2002–2000)	Average (2000)	Difference (2002–2000)	
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.
<i>States in Group</i>		2	23		
ACT Participation Rate (published)	68%	31 p.p.	70%	-1 p.p.	32 p.p.

Note: Enrollment categories are cumulative.

Table 6a: 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 6b: Event-Study Estimates of the Effect of Mandates on Log First-time Freshmen Enrollment, 1994-2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrollment Measure	Overall		Selective		More Selective			
Treatment state x 8 years prior	-0.018	-0.002	-0.016	-0.002	-0.011	-0.003	-0.009	0.006
s.e.	(0.177)	(0.151)	(0.053)	(0.034)	(0.025)	(0.026)	(0.072)	(0.048)
Treatment state x 6 years prior	-0.073	-0.063	-0.022	-0.013	0.001	0.02	-0.005	0.005
s.e.	(0.156)	(0.145)	(0.049)	(0.040)	(0.020)	(0.025)	(0.059)	(0.049)
Treatment state x 4 years prior	-0.033	-0.028	-0.005	0.000	0.006	0.026	0.015	0.02
s.e.	(0.112)	(0.107)	(0.036)	(0.031)	(0.013)	(0.021)	(0.031)	(0.027)
Treatment state x 2 years prior	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
s.e.	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treatment state x mandate	0.015	0.006	0.067***	0.060***	0.063***	0.065***	0.070***	0.062***
s.e.	(0.035)	(0.030)	(0.012)	(0.019)	(0.019)	(0.015)	(0.014)	(0.017)
Treatment state x 2 years after	0.016	-0.022	0.129***	0.096***	0.124***	0.126***	0.132***	0.095***
s.e.	(0.039)	(0.025)	(0.035)	(0.022)	(0.024)	(0.026)	(0.037)	(0.023)
Treatment state x 4 years after	0.039	-0.023	0.187***	0.132***	0.174***	0.212***	0.226***	0.166***
s.e.	(0.054)	(0.040)	(0.038)	(0.027)	(0.025)	(0.043)	(0.058)	(0.041)
Treatment state x 6 years after	0.024	-0.046	0.212***	0.150***	0.204***	0.218***	0.227***	0.158***
s.e.	(0.034)	(0.032)	(0.046)	(0.034)	(0.039)	(0.053)	(0.061)	(0.043)
adjusted R ²	0.988	0.990	0.995	0.996	0.996	0.996	0.995	0.996
controls								
ln(population)		X		X			X	
ln(overall enrollment)					X			X

Notes: Each column 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. All regressions include state and year effects. 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 7: Effect of Mandates on Log First-time Freshmen Enrollment at Selective Colleges, Robustness and Specification Checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Extended Sample Period: 1994-2010	Alt. Control Sample: Neighboring States	State Time Trends	Phased-in Treatment	State Time Trends w/ Phased-in Treatment	Placebo Policy Four Years Earlier	Colorado only	Illinois only	Late-adopting States
Specification	Table 6a, Panel B, Spec. (5)									
mandate _{st}	0.142*** (0.018)	0.163*** (0.019)	0.134*** (0.020)	0.043 (0.044)			0.139*** (0.018)	0.156*** (0.025)	0.129*** (0.016)	
s.e.					0.095*** (0.014)	0.065** (0.029)				0.070** (0.030)
treatment _s *(<4 years of policy) _t					0.190*** (0.029)	0.140*** (0.034)				
s.e.										
treatment _s *(≥4 years of policy) _t										
s.e.							0.007 (0.018)			
treatment _s *(post1998) _t										
s.e.										
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 6a, 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 8: 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 6, 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 9: 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 6, 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).

For Online Publication

Appendix A: Data

A. *Test-taker Data*

My primary test-taker 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.⁵⁴ The dataset includes a 50% sample of non-white students and a 25% sample of white students who took the ACT exam each year.

Each student-observation in the ACT dataset includes an 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 student survey responses completed before the exam that provide an overview of the test-taker’s enrollment status, socioeconomic status, other demographics, and high school.⁵⁵

The ACT microdata also contain high school identifiers that 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.⁵⁶

The student-level analyses in the text rely on neighboring ACT states to generate a composite counterfactual for the experiences of test-takers from the two early-adopting states.⁵⁷ 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.⁵⁸

B. *Enrollment Data*

⁵⁴ I am grateful to ACT, Inc. for providing the extract of ACT microdata used in this analysis.

⁵⁵ My analysis omits any test-takers missing scores or indicating that they are currently enrolled in college.

⁵⁶ 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. 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. 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.

⁵⁷ 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.

⁵⁸ Appendix Figure 1 reproduces Figure 1 for the matched ACT-CCD sample and demonstrates 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. It appears 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.

To study enrollment effects, I use information on college matriculation available in the Integrated Postsecondary Education Data System (IPEDS).⁵⁹ 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.^{60,61}

I merge the IPEDS data to classifications of schools into nine selectivity categories from the Barron's "College Admissions Selector."⁶² A detailed description of the Barron's selectivity categories can be found in Appendix Table 1. Designations range from noncompetitive, where nearly 100% 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 competitive" institutions, only).^{63,64}

⁵⁹ Data were most recently accessed June 11, 2012.

⁶⁰ 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 (i.e. "immediate enrollment"), but these are less complete. Specifically, there are 203 selective schools (out of 1,262) that report non-zero immediate enrollment in 1998 and 2006 but have zero immediate enrollment reported for either 2002 or 2004. This is not normal year-to-year variation: there are only 10 schools that have non-zero immediate enrollment in 1998 and 2006 but are missing it in either 1996 or 2008. When I correct for this underreporting, either by restricting attention to a balanced panel of schools (i.e., schools that report non-zero immediate enrollment in each year) or by using first-time, first-year enrollment to fill in the missing values, I get results quite similar to those that I report in the main analysis (available upon request).

⁶¹ 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. 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% 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% of total 2010 enrollment and 3% of 2010 selective enrollment.

⁶² 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% 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.

⁶³ 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).

⁶⁴ Appendix Figure 2 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% of enrollment qualifies as "more selective", and 45% as "selective." These shares are consistent with the published Carnegie Foundation statistics described in Section II.

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,⁶⁵ constructed from the IPEDS.

⁶⁵ 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.

Appendix B: Comparing Test-taker Composition to the At-Risk Population

Appendix Tables 2 and 3 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 2004.⁶⁶

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.

⁶⁶ Colorado did not report demographic data to the CCD in 1998-99. I use data on 12th graders in 1999-2000 instead.

Appendix C: Describing the Compliers

In this Appendix, I rely on the methods proposed in Section III 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. This assumption is supported by the estimates in Appendix Tables 2 and 3. With this assumption and reasoning parallel to that in Section III, I can express the share of compliers with any characteristic c in terms of observable quantities.

Appendix Table 4 estimates complier characteristics using school reports of their minority enrollment and student reports of their gender, minority status, and parental income bracket. 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.⁶⁷ 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 5 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% of students with scores above 18 would not have taken the test in the absence of a mandate. Compliers account for around 40% of competitive scoring within groups typically associated with disadvantage (low income and minority students and students from high-minority high schools), and around 30% 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 5 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,

⁶⁷ 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.

$Pr(minority|high\ scoring\ complier) = \frac{Pr(high\ scoring\ complier|minority) \times Pr(minority)}{Pr(high\ scoring\ complier)}$. Substituting in

values from Appendix Tables 4 and 5, I estimate that 30% of high-scoring compliers in Colorado, and 40% in Illinois, are minority students. These figures can be compared with the 20% of high-scoring always-takers in each mandate state that are minorities. A similar series of calculations demonstrates that between 30 and 40% of high-scoring compliers are from the bottom income quintiles, compared to just 15% of high-scoring always-takers. Thus, high-scoring compliers are disproportionately likely to be from disadvantaged backgrounds, relative to students with similar scores who take the test voluntarily.

Appendix D: 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%. 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.⁶⁸ 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).⁶⁹ So, according to the 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.

⁶⁸ 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.

⁶⁹ 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 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.

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% 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 $E[P|LE] < 0.03$.

Appendix E: Bounding P^* for LE Compliers

I can express the fraction of compliers who are high-scoring as a weighted sum:

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

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 $E(P^*|LE)$ is simply the pass rate among LE students.

Thus, the second term can be rewritten as:

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

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.⁷⁰ From Section III in the main text, it is approximately 10%. Moreover, in Section V, I demonstrated that roughly 40% of compliers earn scores high enough to be admitted to a selective college; that is, $E(P^*|C) \cong 40\%$.

Substituting these into the original expression and rearranging, we find that:

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

Next, I attempt to bound $E(P^*|NI)$. It seems reasonable to assume that the NI compliers are no more able than the group of always-takers. From Section III, $E(P^*|AT) \cong 80\%$. Assuming that $E(P^*|NI) < 80\%$, 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 $E(P^*|LE) \cong 16\%$. 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 IV, my calculations imply that many high-scoring compliers thought that their chances of passing were lower than their actual chances. Formally, $E(P|LE) \ll E(P^*|LE)$ so that these students must have systematically downwardly biased forecasts of their performance.

⁷⁰ This assumes that the 80% 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 $\Pr(\text{pass} \cap LE|C)$, and my bound on the break-even point for the test-taking decision is a lower bound.

Appendix F: Simulating Luck

In Appendix E, 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% 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%. 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:

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

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:

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

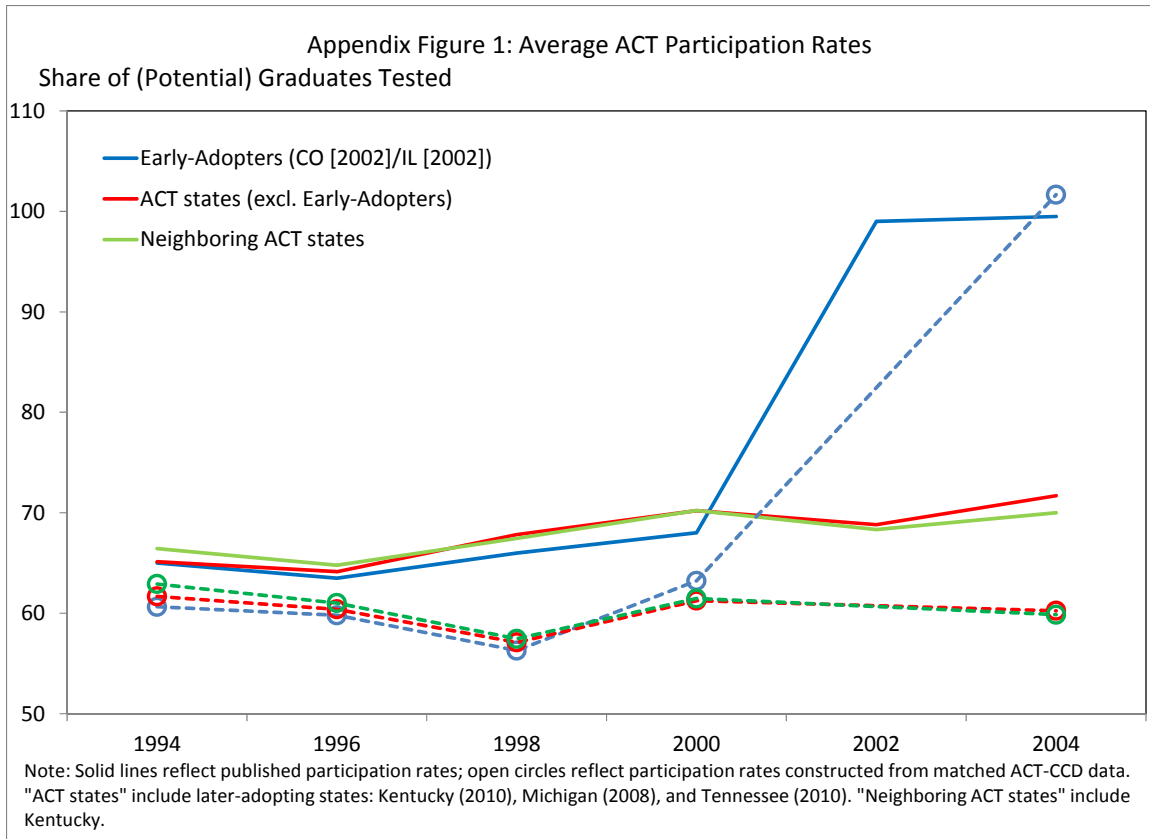
I assume that the ACT’s test-retest reliability is 0.9 and compute σ_ε as: $\sigma_\varepsilon = \sigma_s \sqrt{1 - 0.9}$.⁷¹ I estimate σ_{A^*} and $\sigma_{A^*}^2 + \sigma_\varepsilon^2$ from the empirical score distributions in each state in 2004. For each, I

⁷¹ 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.

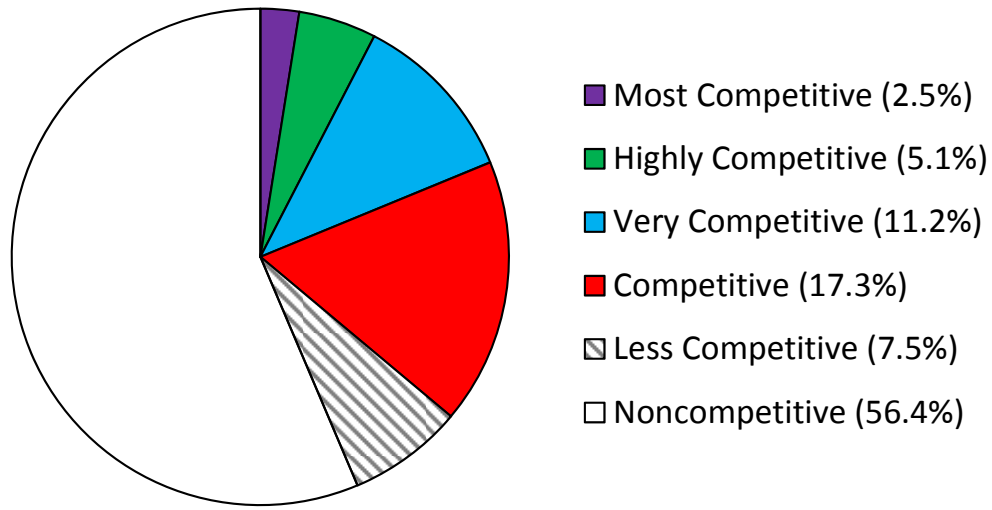
generate 1,000,000 draws of A_i^* , which together with my estimate of σ_ε and an assumed value of \bar{s} , yields 1,000,000 observations of P_i^* . Incrementing values of c by hundredths for values between 0 and 1, I calculate the mean passing rate among test-takers who have $P_i^* < c$. Appendix Figure 3 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.

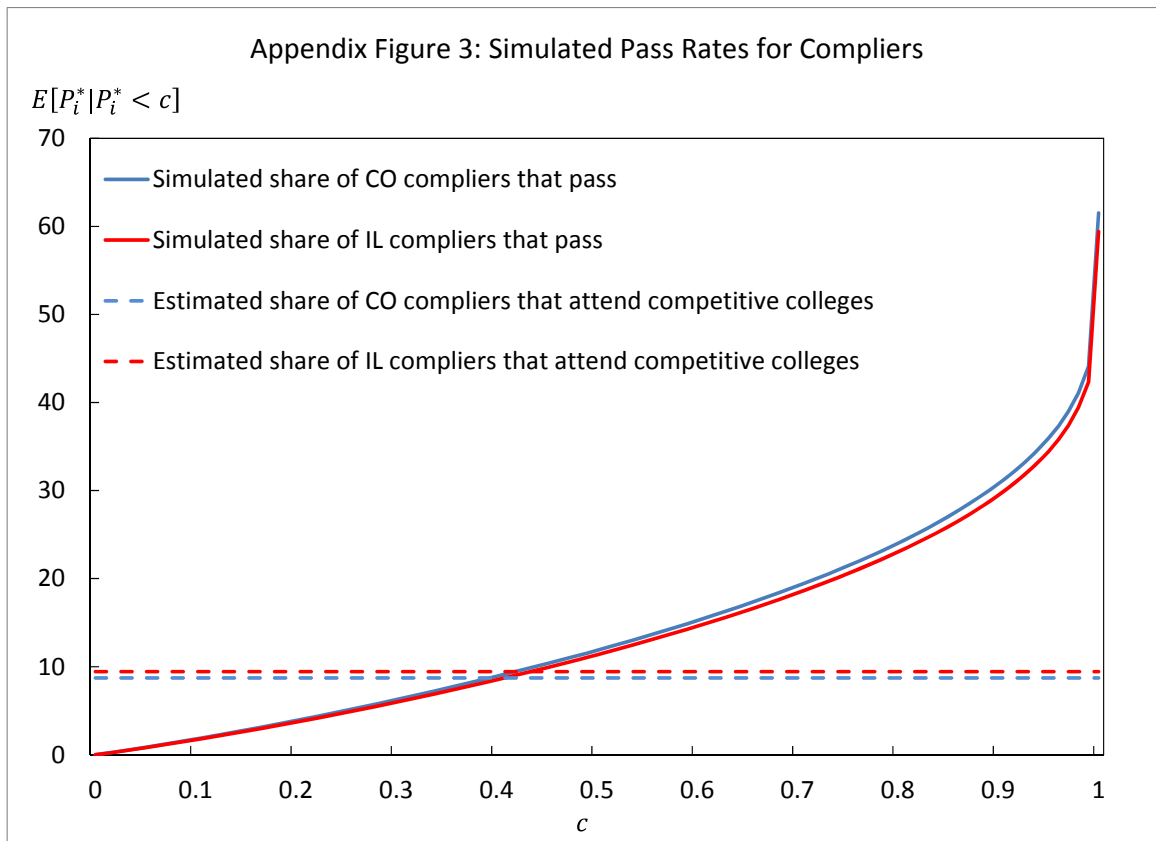
The horizontal lines fall quite high in the distribution. They indicate that one would expect to see the observed effects on selective college enrollment if students take the test only when their anticipated probabilities of passage exceed 40% (in Colorado) or 45% (in Illinois). The calculations in Section IV 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.



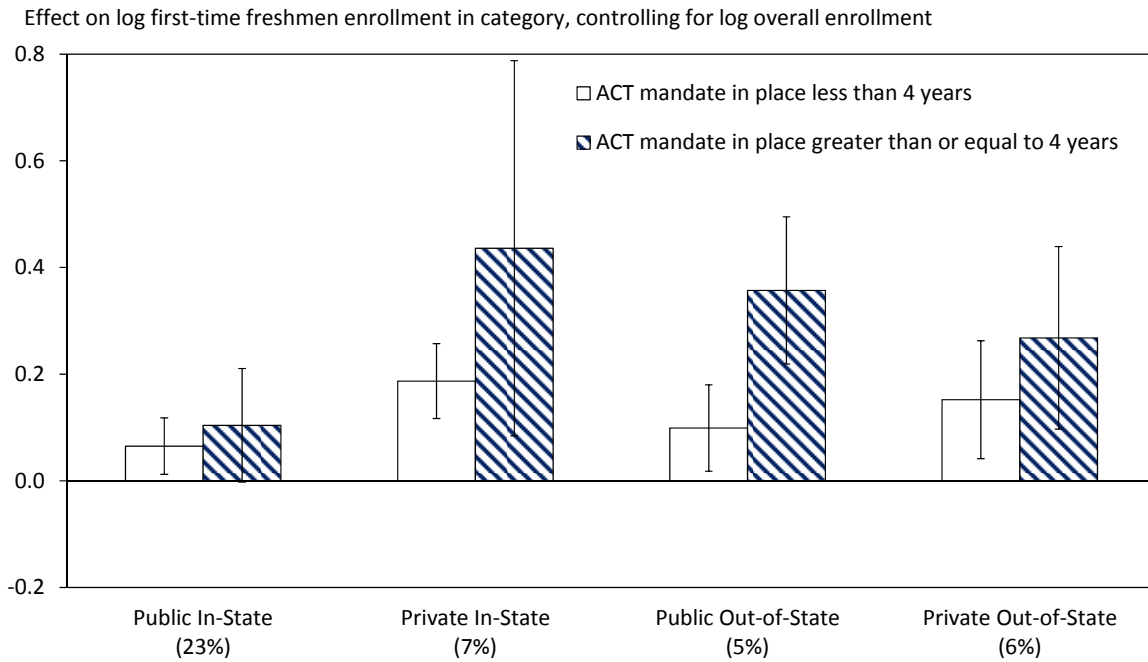
Appendix Figure 2: 2000 Enrollment Distribution by Selectivity



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.



Appendix Figure 4: Effect of Mandates on Subcategories of Selective Enrollment, Allowing Phase-In



Note: Categories are mutually exclusive. Estimation sample is all ACT states (excl. Michigan) in even years between 1994 and 2008 (inclusive). Two states are treated beginning in 2002. Specification controls for log overall enrollment, and state and year effects. Bars represent 95% confidence intervals with standard errors clustered on the state. Figures in parentheses denote the share of freshmen enrollment in each category in the treatment states in 2000 (before mandates were introduced).

Appendix Table 1. Gradations of Selectivity According to the Barron's College Admissions Selector

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

Note: * indicates a state flagship university in one of the early-adopting mandate states.

Source: Barron's Profiles of American Colleges 2001.

Appendix Table 2: 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 3: 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 4: Complier Characteristics

Colorado					
Characteristic	Pre-reform Test-takers	2004 Test-takers			Complier Share of 2004 Test- Takers
		All	Always- Takers	Compliers	Compliers
	Always- Takers				
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
		All	Always- Takers	Compliers	Compliers
	Always- Takers				
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 5: Scores by Characteristics

Colorado				
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
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 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		Difference in Difference
	Average (2000)	Difference (2002–2000)	Average (2000)	Difference (2002–2000)	
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
<i>States in Group</i>	2		23		
ACT Participation Rate (published)	68%	31 p.p.	70%	-1 p.p.	32 p.p.