LEARNIN FROM THE TEST: RAISING SELECTIVE COLLEGE ENROLLMENT BY PROVIDING INFORMATION

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Abstract—Between 2000 and 2010, 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% to 45% of them earned scores high enough to qualify for selective colleges. Selective enrollment rose by 20% following implementation of the mandates, reflecting substitution away from noncompetitive schools. 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.

I. Introduction

This paper examines recent reforms in several states that required high school students to take 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. I link the mandates to large increases in test-taking and show that many of the new test takers score competitively. By comparing college enrollment patterns with and without compulsory testing, I show that, with a mandate, more students attend selective colleges. I interpret these results within a model of test participation and conclude students are systematically biased in their decision making, which I attribute to students underestimating their admission probability to selective programs.

Between 2000 and 2010, five U.S. states adopted mandatory ACT testing for their public high school students. The ACT is a nationally standardized test, designed to measure preparedness for college, that is widely used in selective college admissions in the United States. It has traditionally been taken only by students applying to selective colleges, and this remains the situation in most states.1 Using exam data, I show that in each of the two early-adopting states (Colorado and Illinois), between one-third and one-half of students are induced to take the ACT by the mandates. Large shares of the new test takers—40% to 45% of the total—earn scores qualifying them for competitive-admission schools. Disproportionately many—of both the new test takers and the new high scorers—are from disadvantaged backgrounds.

Yet in a simple model of test-taking under plausible parameter values, students who prefer to attend a selective college, which I define as any school that rejects applicants on the basis of quality, and think they stand a nontrivial chance of admission should take the test. This makes the large share of new test takers who score highly a puzzle unless nearly all are uninterested in attending selective schools.2

Since I cannot directly observe preferences, I examine realized enrollment outcomes. In the primary empirical analysis, I use difference-in-differences to show that mandates cause substantial increases in enrollment at selective colleges—10% to 20% (depending on the selectivity measure)—with no effect on college attendance overall (which is dominated by unselective schools; see Kane, 1998).3 My results imply that about 20% of the new high scorers (or 10% of students induced into testing) attend selective colleges when they otherwise would not have.4

My results cannot be consistent with rational decision making about test taking unless prospective test takers have downward-biased forecasts of their probabilities of being admitted to selective colleges or of their probabilities of attending if admitted. There are two potential channels for this sort of bias. First, students may misperceive their own ability, and thus underestimate their chances of achieving a high score on the exam. Second, students may not have much information about selective schools and may assume that they would choose not to attend even if they were admitted, only to learn more about them and update that

1 Traditionally, selective college-bound students in some states take the ACT, while in others the SAT is dominant. Most selective colleges require one test, but most schools that do will accept either test. At nonselective colleges, which Kane (1998) finds account for the majority of enrollment, test scores are generally not required or are used only for placement purposes.

2 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.

3 States with later mandates experience relative enrollment increases of similar magnitude.

4 If some of the new high scorers (or students induced into testing by the mandates) would have taken the SAT to satisfy their admissions requirements, these estimates represent a lower bound.
preference after taking the exam. In the discussion at the end of the paper, I argue that the former explanation is more plausible: the evidence on college enrollment decisions suggests that the kind of nonspecific information made available to test takers does not have large enough effects on their preferences to generate the enrollment impacts I find, whereas it is quite possible that a student who knows little about the ACT will be a poor judge of her or his likely performance.

Disparities in college attendance and earnings between children from low- and high-income families underscore the importance of policies that raise access to higher education, and especially selective schools, among disadvantaged students. Financial aid programs alone have not been able to close the gap that persists between socioeconomic groups (Kane, 1995). Yet it appears that small nudges, like increasing the allotment of free score sends that accompanies admissions tests, improve outcomes for low-income groups (Pallais, 2015). Identifying other factors amenable to intervention may reduce disparities in college access and enrollment.

Several such factors have been identified in previous work. For instance, the complexity of and lack of knowledge about available aid programs appear to stymie their potential usefulness (Bettinger et al., 2012). Related experiments have simplified the college application process: assisting disadvantaged students in selecting a portfolio of colleges led to subsequent enrollment increases (Avery, 2010); and mailing semicustomized information about the college application process to students generated increased applications and admissions, predominantly among low-income high achievers (Hoxby & Turner, 2013). Finally, in related work, Hurwitz et al. (2014) examine Maine’s decision to make the SAT mandatory beginning in 2007, as part of comprehensive reform to the state’s accountability system, and estimate a 4% to 6% increase in four-year college going. In their setting, they are unable to pinpoint the mechanism generating this effect: introduction of the requirement coincided with test preparation, meals on the exam day, and rewards for test taking, which could all generate score gains. In sum, students exhibit rather large postsecondary schooling responses to small shocks to their information sets when they are making their postsecondary enrollment decisions.

This paper addresses two policy-relevant questions. First, do exam mandates affect enrollment? The answer to this is clearly yes: a very low-cost intervention at late stages of adolescence can have a large impact on educational choices. Second, what explains this effect? My results indicate that many students, who are likely high-need, dramatically underestimate their postsecondary opportunities, which negatively affects their attendance outcomes. This finding is important, given evidence that disadvantaged students gain the most from selective colleges and growing recognition that such students are particularly unlikely to make optimal college choices when left to their own devices.

II. ACT Mandates

The ACT is a standardized national test for high school achievement and college admissions. First administered in 1959, it contains four main sections—English, Math, Reading, and Science—and (since 2005) an optional Writing section. Students receive scores between 1 and 36 on each section and a composite score that is the average of their scores across the four main sections. The ACT competes with another exam, the SAT, in a fairly stable geographically differentiated duopoly. Traditionally, the ACT has been more popular in the South and Midwest, and the SAT on the coasts. Every four-year college and university in the United States that requires such a test will now accept either.

The ACT is generally taken in the eleventh and twelfth 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. 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 Annual Institutional Data Questionnaire, 81% of colleges

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5 The disparity in college attendance between children from low- and high-income families has been increasing over time (Bailey & Dynarski, 2011), while earnings have been essentially steady among the college educated and have dropped substantially for everyone else (Baum & Ma, 2007; Deming & Dynarski, 2010). Earnings are correlated with not just the level of an individual’s education, but the caliber of college she or he attends (Hoekstra, 2009; Card & Krueger, 1992; Black & Smith, 2006).

6 They also find that mandate compliers are rarely high scoring, so marginal students may differ from those in ACT mandate states.

7 This phenomenon is not unique to the U.S. educational system. In developing countries, experiments that inform students of the benefits of higher education have raised human capital investment along several dimensions (Jensen, 2010; Dinkelman & Martínez, 2011). A Canadian experiment found college information sessions beneficial (Oreopoulos & Dunn, 2012).

8 For example, $1,000 in grants raises college going by 3.6 percentage points (Dynarski, 2003).

9 Still, the SAT covers a smaller range of topics and, in particular, excludes a section on science. The tests also differ in format and treatment of incorrect responses.

10 The cost is $35 if the Writing section is omitted. Additional score reports are around $10 per school for either test.
require or use the ACT or the SAT in admissions. Even so, many students attend noncompetitive schools with open admissions policies. According to the Carnegie Foundation, 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.

Between 2000 and 2010, five states (Colorado, Illinois, Kentucky, Michigan, and Tennessee) began requiring public high school students to take the ACT (see the online appendix, table 1).11 Colorado and Illinois were the earliest adopters: both states have administered the ACT to all public school students in the eleventh grade since 2001 and thereby first required the exam for the 2002 graduating cohort. Kentucky, Michigan, and Tennessee each adopted mandates more than five years later.

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 national pressure on states to adopt statewide accountability measures. The act requires states to assess basic skills among students in particular grades if those states are to receive federal funding for schools. Specific provisions mandate several rounds of testing in math, reading, and science, 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, thereby avoiding a large cost of developing their own metric.12 The second motivation 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 graduation requirement.

Figure 1 presents average ACT participation rates by graduation year for mandate states, divided into two groups by the timing of their adoption, and for the twenty other "ACT states" from 1994 to 2010.13 State-level participation rates reflect the fraction of projected (public and private) high school graduates 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 sixteen 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 nonmandate states.

Due to data availability, the empirical analysis in this paper focuses on the two early adopters. However, I also 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

The 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 they attended. The data set includes a 50% sample of nonwhite students and a 25% sample of white students who took the ACT exam each year. Each student observation includes the ACT composite score, the metric most relied on in the college admissions process, and responses to a survey completed before the exam concerning enrollment, socioeconomic status, and demographics.14 The data contain high school
identifiers that can be linked to records from the Common Core of Data (CCD), an annual census of public schools that captures the size and minority share of a school’s student body. I use year-earlier CCD data describing eleventh graders, the population at risk of test taking.16

A limitation of a data set constructed from ACT test takers, as opposed to state administrative records, is that I do not observe students who do not take the ACT exam. Unobserved students either take no test or only the SAT to satisfy admissions requirements. (Appendix A examines the validity of the assumptions underlying my identification strategy.)

My analyses rely on neighboring ACT states to generate a counterfactual for the experiences of test takers from the two early-adopting states.17 A counterfactual test taker constructed from surrounding states, rather than all ACT states, is likely to be more demographically and environmentally similar to the marginal test taker in a mandate state. These characteristics may be linked to experiences such as the likelihood she attends public school (and thus is exposed to the mandate) or her ambitiousness and cannot otherwise be fully accounted for in the data.18

Appendix table 4 presents average test-taker characteristics in my sample. Test taking in each treatment state jumped substantially around the timing of the mandates. The number of test takers more than doubled from the pretreatment average in Colorado and increased about 80% in Illinois. In each case, the neighboring states saw growth of less than 10%. Predictably, requiring all students to take the test lowered the average score: both treatment states experienced drops of about 1.5 points in their mean scores after their mandates took effect. Similarly, the mean parental income is lower among the posttreatment test takers than among those who voluntarily test.19 Posttreatment, test takers exhibit a more equal gender and minority balance than the group of students who opt into testing on their own.20 This is also unsurprising, since more female and white students tend to pursue postsecondary education, especially at selective schools. Finally, the posttreatment test takers more often tend to be enrolled in high-minority high schools.21

Figure 2 plots score frequencies for the two treated states and their neighbors in the years before and after treatment, adjusting for the graduating class size.22 The plots include a vertical line at 18, reflecting a conventionally used threshold by colleges with the most liberal, but still competitive, admissions processes.23 In both treatment states, the influx of new test takers shifted the score distribution to the left. Moreover, the distributions, particularly in Colorado, widened. Thus, accompanying a mandate, there is both a decline in mean scores and a considerable increase in the number of students scoring above 18: test takers scoring between 18 and 20 (inclusive) grew by 60% in Colorado and 55% in Illinois; at scores above 23, the growth rates were 40% and 25%, respectively. In the neighboring states, there were no such shifts.

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, & Rubin, 1996). Because

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16 My sample comprises test takers who can be matched to a school in the CCD sample. A fraction of students in the ACT data are missing high school codes so cannot be matched. Missing codes are more common in pretreatment than in posttreatment data, particularly for low-achieving students. This may lead me to underestimate the test score distribution for mandate compliers. I also exclude school years for which there are no students taking the ACT.

17 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.

18 Appendix figure 1 reproduces figure 1 for the matched ACT-CCD sample. Matched-sample participation rates resemble those reported for the full population by ACT. Also, participation rates in the neighboring states track those in the mandate states as closely as a composite formed from the other ACT states. Public school students apparently have a lower participation rate than private school students (who are included in the published data), but the trends are similar.

19 The ACT survey asks students to estimate their parents’ pretax income according to up to nine 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), and income quantiles are calculated within each year.

20 Minority equals 1 if a student selects a race or ethnicity other than “white/non-Hispanic.”

21 High-minority schools are those in which minorities represent more than 25% of enrollment. The differences between the pretreatment averages in the treatment states and the averages in neighbor states suggest 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. Both Colorado and Illinois have higher minority shares and slightly higher relative income among voluntary test takers than do their neighbors. 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 lends confidence that the abrupt changes observed in treatment states do in fact result from the mandates. Plotting the biennial demographic and test score data in mandate states reveals patterns that mimic each of their respective composite states, with a sharp divergence between the mandate and nonmandate states in 2004 for both samples. Figures not included but available on request.

22 To abstract from changes in cohort size over time, pretreatment score cells are rescaled by the ratio of public enrollment in the earlier period to that in the later period. Although U.S. college admissions decisions are multidimensional and typically not governed by strict test score cutoffs, ACT Inc. publishes benchmarks to help test takers understand the competitiveness of their scores. According to definitions from their website, a “liberal” admissions school accepts freshmen in the lower half of a high school’s graduating class (ACT: 18–21); a “traditional” admissions school accepts freshmen in the top 50% of a high school’s graduating class (ACT: 20–23); and a “selective” admissions school tends to accept freshmen in the top 25% of a high school’s graduating class (ACT: 22–27). (See http://www.act.org/newsroom/releases/view.php?year=2010&type=734&lang=english.) According to a recent concordance (College Board, 2009), an 18 on the ACT corresponds roughly to an 870 on the SAT (out of 1600); a 20 ACT to a 950 SAT, and a 22 ACT to a 1030 SAT.

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my data pertain to test takers, compliers are not present in nonmandate state-year cells. In mandate cells, they present but 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 (eleventh-grade students planning to graduate from high school).

Since there were (small) changes in the number of test takers and the score distributions in nonmandate states as well as in mandate states, I employ a difference-in-differences strategy to identify the number of compliers scoring at each level, net of any common trends (which will be attributed to the always-taker group). The first step is to estimate the number of new test takers. 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’ effects on test taking—the share of students induced to take the exam by the mandates—can be estimated with

$$P_{st} = \beta_0 + \beta_1 \times \text{mandate}_{st} + \gamma_1 + \gamma_s + \epsilon_{st},$$

where $P_{st}$ is test participation in a state-year and $\beta_1$ represents the estimated size of the complier group as a share of total enrollment.25 The $\gamma$’s represent state and time effects that absorb any permanent differences between states and any time series variation that is common across states.

I estimate equation (1) separately for the two early-adopting states and their respective neighbors and decompose the number of test takers into compliers and always-takers.26 About 45% of eleventh graders in Colorado and 39% in Illinois, are compliers (table 1).

It is more complex to identify the score distribution of compliers as it requires an assumption about the evolution of score distributions. 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

25 Note that an alternative specification of equation (1) is available: $\ln(A_{st}) = \beta_0 + \beta_1 \times \text{mandate}_{st} + \beta_2 \times \ln(N_{st}) + \gamma_1 + \gamma_s + \epsilon_{st}$, 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. The two approaches yield similar results.

26 I investigate the sensitivity of my results to specifications that address the small number of states in these regressions. Heteroskedastic-robust and wild cluster bootstrap-t standard error calculations do not affect inferences (Cameron, Gelbach, & Miller, 2008). Results are not statistically different if estimated using synthetic control composite states derived from the set of ACT states and demographic controls from equation (2) (Abadie & Gardeazabal, 2003).
recovered estimates for these weights in the last exercise; thus, estimating the share of compliers at any given score, \( r \), requires the share of always-takers at \( r \). The score distribution in nonmandate neighboring states, which by definition reflects the universe of always-takers in those states, provides a convenient counterfactual for always-takers in mandate states.\(^{27}\) Thus, I assume that absent the mandate, the likelihood a randomly selected always-taker earns a score of \( r \) increases (or decreases) by the same amount in both groups.\(^{28}\) Repeating this exercise for each score cell, I recover the full complier score distribution.\(^{29}\)

Table 2 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 earned high scores (column 3). As a consequence, a substantial portion of the high scorers in mandate states came from the induced group (column 5). I estimate that around 40% to 45% of compliers, amounting to about 20% of all postmandate test takers, earned scores surpassing conventional thresholds for admission to competitive colleges.

Appendix B shows how I can link the above methodology to test taker characteristics to estimate complier shares in subgroups of interest (e.g., high-scoring minority students). Compliers come from poorer families and high-

\(^{27}\) I derive a composite counterfactual distribution from neighboring states and pre-2004 years.

\(^{28}\) Were I able to observe the population distribution of scores in nonmandate state-years, a counterfactual formed from test-taking rates would be analogous to the method used to estimate the weights. 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 A, I examine the plausibility of this assumption by comparing the test-taker composition to observable characteristics of eleventh graders in the matched CCD schools.

IV. The Effects of the Mandates on College Enrollment

In this section, I explore how and whether the ACT mandates affect college attendance, under the assumption that the shifts in enrollment I identify arise among the group of students induced into testing by the mandates described in the previous section.

A key by-product of required testing is the availability of an exam score for students who would not otherwise have taken the ACT.\(^{30}\) The score may contain new information for the mandate compliers, revealing either their true ability or simply their admissibility to selective colleges. Two distinct decisions may be affected by this information: the decision to attend college and to attend a selective college.

Whether to attend college 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 nonmonotonic function of test scores. 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 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, returns are higher to attending a selective college, and acceptance at a selective college is a direct function of test scores. Thus, the new availability of a score, on net, could positively influence a student’s decision to attend a selective school.

To identify mandate-induced changes in college attendance, I use information on college matriculation available in the Integrated Postsecondary Education Data System (IPEDS).\(^{31}\) IPEDS surveys are completed annually by 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, disag-

\(^{30}\) The discussion that follows sets aside sticker prices of test taking and college applications.

\(^{31}\) Data were most recently accessed June 11, 2012.
I merge the IPEDS data to classifications of schools into nine selectivity categories from the Barron’s College Admissions Selector (appendix table 7). 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”) tend to score below 21. I create six summary enrollment selectivity measures: overall (any institution, including those not ranked by Barron’s), 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). In the appendix, I explore analyses cross-classifying institutions by selectivity and institutional characteristics, constructed from IPEDS.

Figure 3 presents suggestive evidence linking ACT mandates to enrollment patterns. The top panel plots overall enrollment over time for freshmen from the ACT states. The solid line represents students from Illinois and Colorado, and the dashed line is students from the remaining 23 states. For comparability, the series are indexed to 1994 levels. The bottom panel presents the same construction for selective and more selective enrollment. There is a series break 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 (reflecting, in part, increased coverage of the IPEDS survey) among freshmen from the nonmandate states and by 15% among freshmen from the mandate states, while selective and more selective enrollment rose by around 15% over this period from both groups. Second, after 2002, the rate of increase of each series slowed somewhat among freshmen from the nonmandate states. The mandate states experienced a similar slowing in overall enrollment growth for much of that period, but the growth of selective and more

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32 IPEDS also releases counts for the number of first-time, first-year enrollees who have graduated from high school or obtained an equivalent degree in the last twelve months (i.e., immediate enrollment), but these are less complete. Specifically, there are 203 selective schools (out of 1,262) that report nonzero immediate enrollment in 1998 and 2006 but have 0 immediate enrollment reported for either 1996 or 2008. When I correct for this underreporting, either by restricting attention to a balanced panel of schools (schools that report nonzero immediate enrollment in each year) or by using first-time, first-year enrollment to fill in the missing values, I obtain results quite similar to those that I report in the main analysis (available on request).

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 primarily focuses 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.

33 Barron’s selectivity rankings are constructed from admissions statistics describing the year-earlier first-year class. About 40% 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 noncompetitive. The Barron’s data were generously provided to me by Lesley Turner.

34 Year-to-year changes in the Barron’s designations are uncommon. I follow Pallais (2015) and rely on Barron’s data from a single base year (2001 edition). Appendix figure 2 depicts the distribution of enrollment by institutional selectivity in 2000. Together, the solid colors (35% of the pie) represent “more selective” enrollment, and the solid colors plus the hatched slice (45%) represent “selective.” The majority of students attend noncompetitive institutions. These shares are consistent with the Carnegie Foundation statistics.
selective enrollment from these states accelerated. By 2010, selective enrollment from the mandate states grew almost 30% from its 2000 level, compared with 9% growth in the other states. Finally, there is a discrete jump in selective enrollment from these states around the mandate.35 Comparing average selective enrollment in 1994–2000 to 2002–2010, the top five institutions absorbing the growth in students from Colorado, from most to least, were Colorado State University; University of Colorado, Boulder; Metropolitan State College of Denver; University of Colorado, Denver; and University of Colorado at Colorado Springs.36 Each contributed at least 6% to selective enrollment growth. The top five institutions absorbing Illinois growth were: DePaul University; Southern Illinois University Edwardsville; University of Illinois at Urbana-Champaign; Indiana University-Bloomington; and University of Iowa. Each contributed at least 3.5%. All ten require an admissions test, reject at least 20% of applicants, and report that at least 75% of their students score above 18 on the ACT.

Appendix table 8 summarizes enrollment by mandate status in 2000 and 2002, denominated as a share of the at-risk population of 18-year-olds, and population characteristics from the Current Population Survey. College going increased around 5 percentage points within both groups, whereas attendance at selective and more selective colleges grew around 2 percentage points among students from mandate states and was essentially flat among those from nonmandate states.37 The population in mandate states differs somewhat from nonmandate states, but the differences are stable over time and thus unlikely to confound identification in my estimation strategy. I will present specifications controlling for these observables.

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

$$E_{st} = \beta_0 + \beta_1 \times \text{mandate}_{st} + X_{st}\theta + \gamma_s + \gamma_t + \epsilon_{st}.$$  \hspace{1cm} (2)

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 $\gamma$’s are state and time effects that absorb any stable differences between states and any time series variation that is common across states. The variable $\text{mandate}_{st}$ is an indicator for a treatment state after the mandate is introduced; thus, $\beta_1$ represents the mandate effect, the differential change in the mandate states following implementation of the mandate. Standard errors are clustered at the state level.38 $X_{st}$ represents a vector of controls that vary over time within states. For my primary analyses, I consider three specifications of $X$ 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, for only selective and more selective enrollment, I use total postsecondary enrollment as a summary statistic for factors influencing the demand for higher education. Because (as I show below and as figure 3 makes clear), there is little sign of a relationship between mandates and overall enrollment, this control makes little difference to

35 The jump in selective enrollment among students from the three later-adopting states (relative to those from states without a mandate) was almost exactly the same magnitude.
36 Institutions in bold are state flagships. Italicized institutions are located out of state.
37 Appendix table 9 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, including those both public and private and both in-state and out-of-state.
38 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 3, 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 0 in each of the specifications marked as significant in table 3.
the results. I estimate each specification with and without demographic controls.

Table 3 presents the regression results for the period between 1994 and 2008, where the estimation sample includes all ACT states and the treatment states are Colorado and Illinois. Each panel reflects a different enrollment outcome, with the definition of selectivity increasing in stringency from the top to the bottom of the table. Within each panel, I present up to six variations of my main equation: Specification 1 includes no additional controls beyond the state and time effects, specification 2 adds 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 columns 3 and 4 with total college enrollment.

Results are quite stable across specifications. There is no sign that the mandates affect overall enrollment probabilities. However, they do influence selective enrollment: when mandates are introduced, selective and more selective college enrollment each increase between 10% and 20%. The regression results coincide with the descriptive evidence: a mandate induces students who would otherwise enroll in nonselective schools to instead enroll in selective institutions.

Still, in order for my strategy to identify the effect of an ACT mandate on enrollment of freshmen, college-going trends among students from mandate states would be clear, the new requirement, resemble those observed in nonmandate states. To examine this, I follow Autor (2003) and estimate an event-study version of equation (2) that normalizes the mandate year to 0, omits the year just before the mandate is introduced (2000), and includes lead and lag terms for each year of data between 1994 and 2008 (inclusive) (appendix table 10). The coefficients on the mandate lead terms are not significantly different from 0; thus, there is no evidence of anticipatory effects or important heterogeneity in enrollment pretrends. When either of the selective categories is the outcome of interest, the lag terms are highly significant. (They are 0 for overall enrollment.) Estimated effects increase sharply over the first four years of the policy and gradually after. I examine this further in table 4 and provide a discussion in the appendix.

Table 4 explores specifications that examine the robustness of my findings. (More extensions are presented in appendix C.) I report results for selective enrollment, controlling for overall enrollment (panel B, specification 5 in table 3). Column 1 reviews results from table 3. In column 2, I add state-specific time trends. The mandate effect vanishes in this specification. Figure 3 and the analysis in appendix table 10 suggest that the mandate effects grow over time, a pattern that may be absorbed with a linear trend and a single step-up mandate effect. Therefore, I explore another specification that allows the treatment effect to phase in:

\[ E_{st} = \alpha_0 + \alpha_1 \times (\text{treatment}_{s} \times (\leq 4\text{years of policy}_{t})) + \alpha_2 \times (\text{treatment}_{s} \times (\geq 4\text{years of policy}_{t})) + \alpha_3 \times \text{overall}_{st} + \gamma_t + \gamma_s + \psi_s \times t + \epsilon_{st}. \] (2')

The results are presented without state-specific trends in column 3 and with them in column 4. Column 3 indicates

39 The main analysis omits Michigan due to dramatic decreases in Michigan’s state aid around its mandate (see www.michigan.gov/misdendentaid/0,4636,7-128-60969_61002_61357-279168--,00.html, accessed June 6, 2013). Headline results are not very sensitive to Michigan’s inclusion as a control through 2007 and treatment state thereafter. Michigan is included in some robustness checks.

40 I have also estimated the specifications presented in table 3 weighting the regressions by population and total enrollment (where applicable). Results are mostly unchanged.

41 Results using the other specifications from table 3 are similar (available on request).

42 In columns 2, 4, and 6, I present robust standard errors, as they are more conservative here than the clustered standard errors of 0.016, 0.017, 0.033, and 0.031, respectively.

43 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 (2').
that the treatment effect is 10% in the first years after mandate implementation and then grows to 20%. This specification is much more robust to the inclusion of state-specific trends (column 4) than was the version with a single treatment effect. The hypothesis that both treatment coefficients are 0 is rejected at the 1% level. There are a 
gle treatment effect. The hypothesis that both treatment specific trends (column 4) than was the version with a sin-
ification is much more robust to the inclusion of state-
mandate implementation and then grows to 20%. This spec-
that the treatment effect is 10% in the first years after

Finally, I estimate the effects of more recent mandates in Kentucky, Michigan, and Tennessee over the full sample period, omitting Colorado and Illinois from the sample (column 5).46 Because I observe only one postmandate year in Kentucky and Tennessee and two in Michigan, based on column 3, we should expect a smaller treatment effect than was seen for Illinois and Colorado. This is indeed what we see.45 Due to dramatic decreases in Michigan’s state college aid around the timing of its mandate, I reestimate the enrollment effect in the late-adopting states, excluding Michigan entirely (column 6). The coefficient is almost 50% larger and much closer in magnitude to the comparable column 3 estimate for the early-adopting states.

V. Assessing the Test-Taking Decision

In appendix E, I present a simple model of test taking to show that the combination of results above—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. The pass rate among compliers who would attend a selective college if admitted (LE compliers) is roughly 0.16. Because a rate well below that threshold would be sufficient to justify the cost of taking the exam, there must be a divergence between the actual rate at which such students pass the exam (P*) and their subjective judgments of their likelihood of passing (P). Many students have systematically downwardly biased forecasts of their scores.

Figure 4 graphs the probability that a student will earn a competitive score against the return to selective colleges. The solid curve represents the locus of points (return to selective college, probability of admission) at which a student will be indifferent between taking the test or not. For a given anticipated 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 passing rate among LE compliers, 16%.46 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 $1,000. The vertical lines on figure 4 are derived returns estimates from the literature, ranging from $5,000 (Black & Smith, 2006, adjusting for Cohodes & Goodman, 2012; see appendix E for the derivation of this estimate) to $507,000 (Dale and Krueger, 2011, for their minority subsample; in

46 Heterogeneity in $P^*$ among LE compliers would imply a higher break-even point. In appendix F, an alternative calculation using the full score distribution and external estimates of the ACT’s reliability implies that students with $P^*$ as high as 40% to 45% are choosing not to take the test.
the full sample, Dale and Krueger estimate a return of zero.⁴⁷ All are well above the value consistent with the estimated passing rate, implying much lower decision thresholds for student test taking, ranging from 3% down to 0.03%.

These calculations do not account for psychic costs of test taking or differences in financial costs of attendance. Such costs could offset the wage benefits for some students, but it is unlikely that very many students view the choice as a knife-edge decision, so removing $150 from the marginal cost of selective enrollment makes the benefit exceed the cost. A more plausible explanation is that many compliers held biased estimates of their chances of earning high scores.

VI. Conclusion

I show that ACT mandates led to large increases in ACT participation, the number of high-scoring students, and selective college enrollment. About 40% to 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%.

In the paper, I interpret the results as suggesting students would like to attend competitive colleges but choose not to take the test out of an incorrect belief that they cannot score high enough to gain admission. Much research, mostly by psychologists and sociologists, has examined the profound effect students’ experiences and the expectations of those around them have on the goals they set themselves for (see figure 1 in Jacob & Wilder, 2010).⁴⁸ These goals, whether aligned with ability or not, are strongly predictive of later enrollment decisions, but they are also malleable.⁴⁹ High school students’ future educational plans appear to fluctuate with the limited new information available in their GPAs (Jacob & Wilder, 2010). In an environment of compulsory testing, students may upwardly revise their college plans because they had otherwise lacked information about their admissibility to particular schools.

Another compatible explanation is that students underestimate the utility of going to a selective school (before taking the test). Agencies that administer admissions tests may cooperate with college campuses by providing test taker contact information for marketing purposes, and students’ perceptions of colleges may update with information they receive after taking the ACT. Because I do not observe beliefs, I cannot distinguish a student underestimating her or his ability from an upward revision to her or his perceived returns to a selective college after testing. I can only truly conclude that students systematically underestimate their abilities if their ex ante and ex post valuations of the returns to a selective college are roughly equal.

The evidence from the literature is more consistent with my interpretation. First, in their Boston experiment, Avery and Kane (2004) find that exam-taking rates are systematically lower among disadvantaged students, which cannot be explained by differences in perceived returns. In fact, students, including those from disadvantaged backgrounds, tend to overestimate returns, and their perceived returns do not have much impact on the selectivity of the school they choose to attend. The authors surmise it more likely that disadvantaged students discount their own qualifications than the returns to college. In addition, while prior work demonstrates that high school students respond to information about colleges and the college application process through mentoring and tailored recommendations, non-streamlined information like brochures and pamphlets might be difficult for students and families to distill on their own (Avery, 2010; Carrell & Sacerdote, 2013; Hoxby & Turner, 2013, 2015). Bettinger et al. (2012) found large effects of informing students of their aid eligibility only when they had assistance with forms (and none otherwise). Hoxby and Turner’s experiment produced large enrollment effects from targeted information even though their treat-
ment and control groups were both eligible to receive marketing materials, suggesting that brochures alone might not be very helpful; as confirmation, their survey found that untreated students dramatically underused information on college quality. Still, one might worry that mandate compliers in my setting receive additional materials that improve their outcomes (e.g., scholarship offers), but Hoxby and Turner’s evidence suggests that they do not. In sum, nonspecific information that test takers receive does not appear to affect their preferences enough to generate the enrollment effects I find.

One concern is that the effects I detect derive not from the mandates themselves but from the accountability policies of which they 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 directly improved student achievement or admissions qualifications, the reduced-form analyses would attribute those effects to the mandates. There are several reasons, however, to believe that the results derive from the mandates themselves. The first 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 eighth-grade students so do not reflect the same cohorts. The stability of NAEP scores is 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. Beginning in spring 2008, Tennessee mandated 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 equation (2) estimates that selective enrollment from Tennessee 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 large effects on selective college enrollment appear immediately after the implementation of the mandates.

Another concern might be the generalizability of the policy effects: What could we expect if we were to institute nationwide compulsory testing? Most of the estimates I present rely 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, it was to only 1% of total national selective enrollment and 5% of selective enrollment from the mandate states and their neighbors. Still, one hurdle to generalizing based on the experiences in Illinois and Colorado is that national policies may create congestion in selective colleges, leading either to crowd-out of always-takers by compliers or to reduced enrollment of compliers (Bound & Turner, 2007; Card & Lemieux, 2000). 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, suggesting that projected growth under a national ACT mandate could be absorbed with under eight years of normal growth. The supply of selective college seats nationwide is likely elastic enough to avoid large or long-lasting crowd-out.

In sum, the evidence indicates that students are systematically underpredicting their suitability for selective schools. Many students are learning important information when they are required to take the ACT, 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 I cannot directly estimate the characteristics of these students, it seems likely that many come from low-income families. Low-income students are much less likely to take the test in the absence of a mandate than their peers, and low-SES mandate compliers are no less likely to earn high scores than high-SES compliers.

Recent studies have demonstrated that college quality is an important determinant of later success. Black and Smith (2006) and Cohodes and Goodman (2012), for example, find that students at lower-quality schools earn less over

50 Most of their control students reported they did not weight college quality heavily when deciding where to apply. Also, they experiment among students taking admissions exams, who are thus somewhat engaged with the competitive higher education landscape. Such students are more likely than disengaged students to try to distill broad-based information from promotional mailings. Their work reveals that even the engaged group is systematically underinformed and undermatches to selective colleges on receiving such promotional mailings.

51 The share of the population from untreated states enrolled in selective schools did not fall after the early mandates were introduced, suggesting that they did not produce meaningful crowd-out. Since mandate compliers from two states represent such a small share of national selective enrollment and are fairly evenly distributed across types of schools, this experiment offers limited insight into the extent of crowd-out we might anticipate from broader policies. Had effects been larger or more concentrated, there might be more evidence of crowd-out. Given the large increases in selective out-of-state and private in-state enrollment (appendix table 13), mandate compliers might compete for admissions slots with always-takers if these policies were scaled up.
their lifetimes and are less likely to graduate. Since my results show that compulsory ACT testing leads many students to enroll in more competitive, higher-quality colleges, mandating the ACT likely enables many students to vastly improve their lifetime earnings trajectories. Using the continuous quality measure I describe in appendix E, 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. The roughly 4.5% of students who are induced to change their enrollment status by the mandate could, on average, expect a boost of 23% in their earnings.52

Pallais and Turner (2006) establish that underprivileged groups are underrepresented at top schools, which they attribute to a combination of information constraints, credit constraints, and precollegiate underachievement. My analysis provides support for their first explanation: that lack of information can explain some missing low-income students. Increasing information flow between universities and students from underrepresented groups so that the potential high scorers know they are indeed suitable candidates could erase some of the enrollment gap found at top schools. This is a potentially fruitful area for policy. 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 According to figures on average lifetime earnings of B.A holders (Pew Research Center Social and Demographic Trends, 2011), 23% of lifetime earnings amounts to about $760,000.

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