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Missed signals: The effect of ACT college-readiness measures on post-secondary decisions

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ABSTRACT

In the face of shrinking government budgets and a growing need to train a high-skilled labor force, policymakers have become increasingly interested in cost-effective measures that induce more students to apply to and enroll in college. In this paper, we use a regression discontinuity design to identify the causal effect of students receiving information about their own college-readiness after taking the ACT on their subsequent college enrollment decisions. Using data from Colorado, where all high school students are required to take the ACT, we find that students who receive information that they are college-ready are no more likely to attend college than those that do not receive this information. We discuss possible reasons for these findings.

1. Introduction

In the face of shrinking government budgets and a growing need to train a high-skilled labor force, policymakers have become increasingly interested in cost-effective measures that induce more students to apply to and enroll in college. A great deal of research has been done to understand the barriers of college entry, especially for low-income students. These barriers can be classified into three primary categories: achievement barriers, financial barriers, and information and administrative barriers. Much is known about how educational inputs affect academic achievement for students in all grades. However, these policies are often costly and must occur early and continuously throughout a student's academic career. Thus, until recently, much of the research in this area has focused on the financial barriers to higher education (see Abraham & Clark, 2006; Deming & Dynarski, 2010; Kane, 2007). However, even with the availability of financial resources for higher education, a number of qualified students choose either to not attend college or to attend lower quality schools. This tendency has raised questions about whether there are non-monetary barriers to entry into higher education and whether targeted policies can help overcome these barriers.

There is a growing number of studies assessing the role of information and administrative barriers in the decision to attend college, especially for low-income students. Bettinger, Long, Oreopoulos, and Sanbonmatsu (2009) find that simplifying the Federal Application for Financial Aid (FAFSA) and providing families with information on their aid eligibility in-

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creased the likelihood that a student will apply for aid, enroll in college, and receive more aid. Hyman (2013), Goodman (2012), Hurwitz, Smith, Niu, and Howell (2014), and Klasik (2013) assess the effects of mandatory ACT and SAT testing and find increases in college enrollment. Papay, Willett, and Murnane (2011b) look at the effects of test score labels (*Failing, Needs Improvement, Proficient, or Advanced*) from 8th and 10th grade standardized exams on college-going decisions and conclude that students update their decisions about post-secondary education based on new information about their abilities. Avery (2010) and Carrell and Sacerdote (2013) find that counseling and mentoring students through the college application process increases college enrollment, while an experiment done by Oreopoulos and Dunn (2012) shows that high school students who watched a short video on the benefits of post-secondary education updated their beliefs about the expected returns and costs of post-secondary education. Quite notably, the Expanding College Opportunities experiment of Hoxby and Turner (2013) finds that providing high-ability, low-income students with personalized information about the application process and college costs leads students to apply to more colleges that are of higher quality. With enlarged opportunity sets, students enrolled in colleges with stronger academic records, higher graduation rates, and more generous financial aid and student support resources.

This new wave of research has demonstrated that providing students with information about their higher educational opportunities late in their academic careers can impact their decisions about post-secondary education. However, in order to design effective policies, it is important to understand what types of information interventions are effective in changing students' behavior and which demographic groups are most affected by such interventions. In this study, we seek to determine whether students respond to information about their own college-readiness that is currently provided by ACT to all test-takers on their score reports. Students who take the ACT receive a score report that shows their score (out of 36) on each of the four subject tests as well as their composite score, which is the rounded average of the four subtest scores. If a student scores above a certain threshold (determined by ACT) in a given subject, they are informed on the score report that they are college ready in that subject.[1] Clearly, individuals who do not get the signal are less likely to go to college than those who do get the signal, because of the high correlation between ACT score and college enrollment. However, by comparing students just above and just below the threshold, we can identify the causal impact of receiving the signal. For this reason, we use a regression-discontinuity approach to obtain unbiased estimates of the causal effect of the college-readiness benchmarks on college enrollment, for students in the immediate vicinity of the cutoff. This approach can help policymakers understand whether telling students they are college-ready just before their senior year of high school affects the subsequent educational attainments of those whom the ACT considers to be on the margin of college-readiness. For this analysis, we utilize detailed student-level data on all Colorado public high school students who were in 11th grade in the spring of 2009 and compare two-year, four-year, and out-of-state college enrollment outcomes for those who score just above and just below the benchmark cutoffs. The student-level data also allow us to estimate heterogeneous effects across demographic groups.

The goal of implementing the college-readiness measures was to provide students with additional information about how they would fare in higher education. Ex ante, it is unclear exactly how these signals will affect student behavior. While we would ideally be able to study the effect of signal-receipt on college application decisions in addition to college enrollment, we do not have data on applications for all the students in our sample. Thus, we focus our analysis on college enrollment by type of institution. To understand how the ACT college-readiness measures may affect student behavior we look to theoretical and empirical studies on how individuals respond to information about themselves.

Evidence suggests that students indeed use performance data to update their plans about continuing in school (Jacob & Wilder, 2011). The most crucial element of any information intervention is its salience. If students do not carefully read their score reports or understand the college-readiness signals, then the information cannot influence their behavior. However, assuming that individuals carefully read the score reports and understand the meaning of the information provided, then a student's response will depend on whether the information satisfies their priors about themselves and the strength of these priors (DellaVigna & Gentzkow, 2010). If the signals the student receives satisfy their existing beliefs about themselves (either positive or negative), then the student will not change their behavior as a result of the college-readiness signals. If the college-readiness signals provide the student with *new* information about their skills or ability, then according to Bayes' rule, the students will update their beliefs about themselves and change their behavior accordingly. Specifically, if the students did not believe they were college-ready prior to receiving their score report, receipt of any of the four signals would be seen as positive news. However, if the students believed they were college-ready prior to receiving their score report, but were told they were not college-ready in some or all subjects, their beliefs about themselves may be negatively affected. These responses may be asymmetric, as individuals respond differently to positive and negative information about themselves—with individuals being less likely to update their beliefs and change their behavior when they received negative information (Eil & Rao, 2011; Howell, Kurlaender, & Grodsky, 2010; Martorell, Jr., & Xue, 2013). While the data do not provide us with explicit information about a student's beliefs about their own ability, we do have demographic characteristics about the individual as well as self-reported information on whether the student was planning on attending college at the time of the ACT exam.[2] These data can help us understand the type of information treatment the individual was facing.

Accordingly, we have four hypotheses about the potential effects of the college-readiness signals. First, there may be an increase in overall enrollment for students who receive

[1] We describe the ACT exam and the College Readiness Benchmarks in more detail in Section 2.

[2] Students must complete a questionnaire prior to taking the Colorado ACT. This questionnaire includes a question about whether the student is planning on attending college.

any signals about their college-readiness. We may see larger positive effects for those who were not planning on attending college or students from demographic groups who tend to be less informed about their college readiness. Second, we may see negative effects on enrollment for students whose beliefs about themselves were negatively affected by receipt of fewer than four college-readiness signals, namely the students who were planning on attending college at the time they took the ACT.[3] Third, a student may not change their decision to go onto higher education as a result of receiving a signal, but they may choose to attend a different *type* of institution (two-year, four-year, or out-of-state). If this is the case, we will observe no change in overall enrollment, but will see a resorting in enrollment across different types of institutions. Again, the direction of the change (from two-year to four-year or vice versa) will depend on whether the signals caused a positive or negative updating of the student's beliefs about themselves. Finally, if the college-readiness measures are not salient, not delivered early enough to allow students to adjust their plans for higher education, or are precisely in line with students' priors about themselves there will be no effect on enrollment at any of the institutions. In the course of our analysis, we will test these hypotheses to fully understand how students respond to the information provided on the ACT.

While the raw statistics show that students who receive the college-readiness signals are, on average, more likely to enroll in and attend college, our estimates suggest that there is no *causal* relationship between receipt of the college-readiness signals and college-going for most students near the threshold. The only exception is for low-income students (those who qualify for free or reduced-price lunch) who increase four-year college enrollment as a result of receiving a college-readiness signal in English. These results are important for developing optimal policies, as they show that not all information interventions cause students to change their college enrollment behavior and that certain students may benefit more from such policies.

This paper is organized as follows. First, we provide background on the ACT policy in Colorado and describe how the college-readiness benchmarks are determined. In Sections 3 and 4 we outline the data and methodology used in this analysis. Section 5 discusses the results, while Section 6 concludes.

2. Background

In response to increased testing requirements under No Child Left Behind, the Colorado State Senate passed Senate Bill 00-186 mandating that all 11th grade students in Colorado take the ACT on a statewide testing day beginning in the spring of 2001.[4] The ACT is a curriculum-based college entrance exam[5] developed by ACT, Inc. Prior to 2001, the exam was voluntary and administered by ACT, Inc. for a fee at testing centers on weekends.[6] The ACT exam consists of four subtests—English, Mathematics, Reading, and Science Reasoning. Each subtest is scored out of 36 points and a composite score, the mean of their four subject scores rounded to the nearest whole number, is reported to the colleges the students choose. Students who take the ACT (either voluntarily or on the mandated testing day) receive a score report from ACT, Inc. The score report highlights the composite score, as well as the percent rank in the state and in the U.S. Below the composite score are each of the subtest scores as well as the percent ranks of each of those scores in the U.S.[7] Fig. 1 shows an example student score report. If a student's subtest score is at or above the college-ready benchmark, there is an asterisk next to that score. Below the scores is an explanation of the meaning of the asterisk. It reads:

"**Your College Readiness: If your scores are at or above the following ACT benchmark scores, you will likely be ready for first-year college courses – English 18, Mathematics 22, Reading 21, Science 24.*" While a good amount of information about a student's relative performance on the exam and college-readiness is provided on all score reports, receipt of this information requires the student to read through the score report carefully and be able to extract information from tables.[8] The subject benchmarks are calculated by researchers at ACT, Inc. using data on student grades from four common first-year college courses: English Composition for English, College Algebra for Mathematics, Biology for Science, and Social Sciences for Reading (Allen & Sconing, 2005). For each course within each college studied, a cutoff score was chosen such that the probability of a grade B or higher in the course was 0.50. The median of these cutoff scores across all colleges is the readiness benchmark. The data in their analyses come from both the two- and four-year colleges that participated in ACT's Course Placement Service. While the sample of colleges in the dataset is not nationally representative, Allen and Sconing (2005) weight their observations to make their sample representative of colleges nationwide according to their ACT Composite scores.

While many laud the ACT for attempting to create a real world application of ACT scores, the benchmarks have faced criticism. Cordogan (2010) highlights the flaws in the research methodology used to determine the benchmarks. First, the sample used in the analysis was a convenience sample of the few institutions who chose to participate in ACT's Course Placement Services. The sample sizes varied across subject with the Math and Science tests having the least data available—33,803 and 14,136 observations, respectively. For the Science exam, that equates to approximately 1% of all the students taking the ACT in a given year. Second, Cordogan (2010) argues that only science majors taking biology were sampled

[3] Unlike those who do not receive any signals, students who receive one signal have their attention drawn to the information about the signals, and therefore may be more informed about the college-readiness signals.

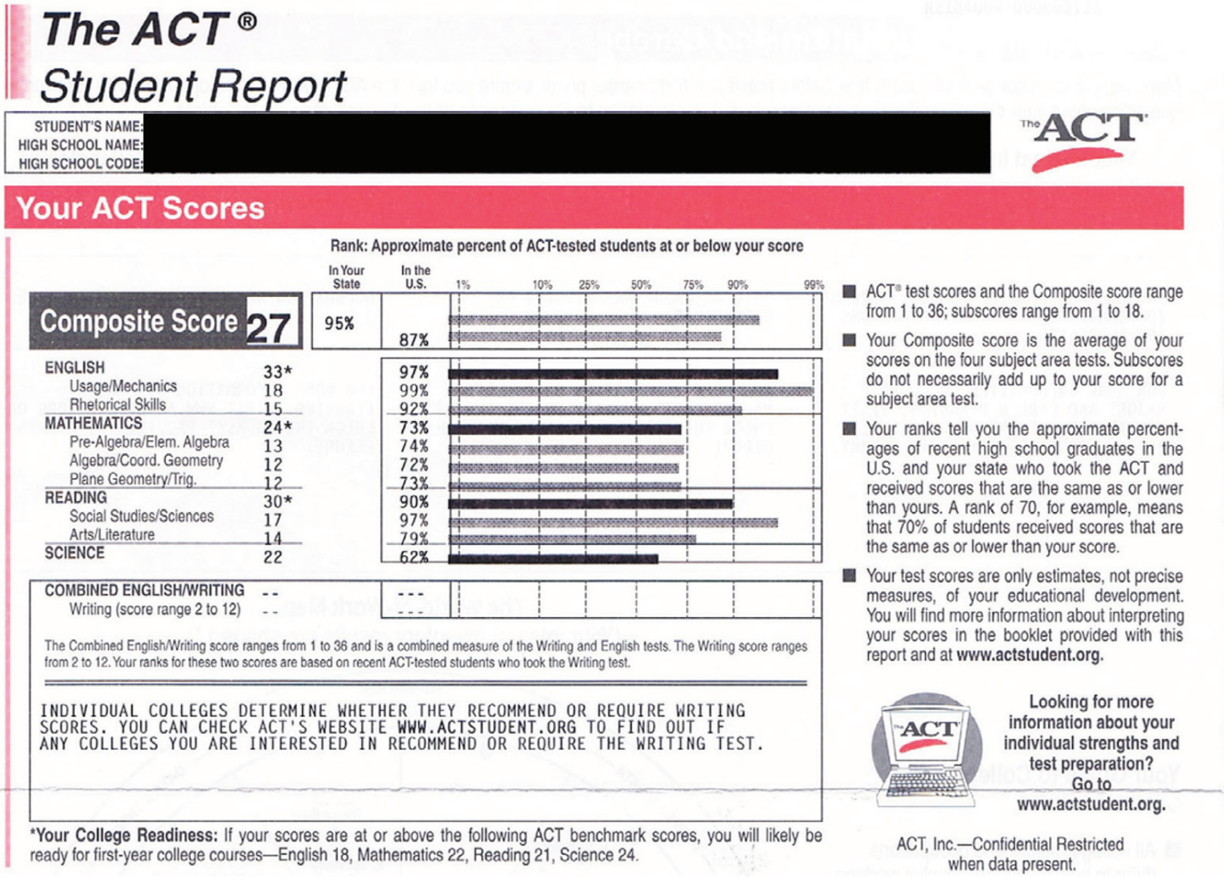
[4] The statewide testing day is determined far in advance and is always a mid-week day (often a Tuesday or Wednesday). One make-up test date is also scheduled.

[5] One of the main differences between the ACT and SAT exams is that the ACT is curriculum-based (it tests what students should have learned during high school), while the SAT is an aptitude test.

[6] Students from low-income families are eligible for fee waivers.

[7] For the English, Math, and Reading subtests, the scores and percent rank for each of subtests' components are also given. For example, the English test consists of two components, usage/mechanic and rhetorical skills.

[8] In 2003, approximately 12% and 22% of Americans had below basic and basic document literacy skills, respectively. Document literacy is the knowledge and skills needed to perform document tasks, such as searching, comprehending, and using non-continuous texts in various formats, including tables (U.S. Department of Education, 2003).


The ACT®
Student Report

STUDENT'S NAME: [redacted]
HIGH SCHOOL NAME:
HIGH SCHOOL CODE:

The ACT

Your ACT Scores

Rank: Approximate percent of ACT-tested students at or below your score

		In Your State	In the U.S.
Composite Score	27	95%	87%
ENGLISH	33*		97%
Usage/Mechanics	18		99%
Rhetorical Skills	15		92%
MATHEMATICS	24*		73%
Pre-Algebra/Elem. Algebra	13		74%
Algebra/Coord. Geometry	12		72%
Plane Geometry/Trig.	12		73%
READING	30*		90%
Social Studies/Sciences	17		97%
Arts/Literature	14		79%
SCIENCE	22		62%
COMBINED ENGLISH/WRITING	--		---
Writing (score range 2 to 12)	--		---

The Combined English/Writing score ranges from 1 to 36 and is a combined measure of the Writing and English tests. The Writing score ranges from 2 to 12. Your ranks for these two scores are based on recent ACT-tested students who took the Writing test.

INDIVIDUAL COLLEGES DETERMINE WHETHER THEY RECOMMEND OR REQUIRE WRITING SCORES. YOU CAN CHECK ACT'S WEBSITE WWW.ACTSTUDENT.ORG TO FIND OUT IF ANY COLLEGES YOU ARE INTERESTED IN RECOMMEND OR REQUIRE THE WRITING TEST.

*Your College Readiness: If your scores are at or above the following ACT benchmark scores, you will likely be ready for first-year college courses—English 18, Mathematics 22, Reading 21, Science 24.

- ACT® test scores and the Composite score range from 1 to 36; subscores range from 1 to 18.
- Your Composite score is the average of your scores on the four subject area tests. Subscores do not necessarily add up to your score for a subject area test.
- Your ranks tell you the approximate percentages of recent high school graduates in the U.S. and your state who took the ACT and received scores that are the same as or lower than yours. A rank of 70, for example, means that 70% of students received scores that are the same as or lower than your score.
- Your test scores are only estimates, not precise measures, of your educational development. You will find more information about interpreting your scores in the booklet provided with this report and at www.actstudent.org.

Looking for more information about your individual strengths and test preparation? Go to www.actstudent.org.

ACT, Inc.—Confidential Restricted when data present.

Fig. 1. Example student score report.
Source: Personal score report posted on colrebsez.blogspot.com.

for the science test. Since science courses are often more academically rigorous than other subjects, science majors need to have significantly higher performance levels (ACT scores) to succeed in their classes. Thus, the science benchmark is set much higher than it would have been set for the average college student. Additionally, the calculation of the benchmark scores does not take into account that many freshmen are placed in remedial courses and are, thus, not included in their sample. This would imply that the students included in the sample, particularly those with lower scores, are positively selected and not necessarily representative of the average student with the same score. A common argument against use of the benchmarks is that ACT test scores alone are relatively weak predictors of first year college performance. Bettinger, Evans, and Pope (2013) find that ACT Math and English scores are much more tightly correlated with college success than are Reading and Science scores. Additionally, they find that after controlling for Math and English scores, Reading and Science provide no predictive power regarding college outcomes. Both Cordogan (2010) and Maruyama (2012) argue that readiness measures based on a single assessment are less precise than those developed using multiple measures, such as test scores, grade point average, and experience with specific courses. They argue that many students who are not considered college-ready by ACT's benchmarks are able to succeed in college.

Despite the objections to these benchmarks from the academic community, ACT continues to provide students with this information and reports annual statistics on the college-readiness of each cohort. In this study we remain agnostic about the quality of the benchmarks in reflecting actual college readiness, especially since they seem to be neither supported nor refuted by high school staff. Rather, we focus on the students' response to being told they are college-ready, irrespective of their true college readiness.

3. Data

The data for this study were provided by the Colorado Department of Education (CDE) and the Colorado Department of Higher Education (CDHE). We have individual-level administrative data for all Colorado public high school students who took the Colorado ACT (COACT) in 2009 during 11th grade. The dataset includes their COACT scores for each subtest,[9] their test scores from the standardized state test they took

[9] While students can take the standard ACT on their own prior to or after administration of the COACT, we only observe their COACT scores.

Table 1
Summary statistics—population characteristics.

Variable	Mean	Std. deviation
ACT English score	18.6	6.9
ACT Reading score	20.0	6.9
ACT Math score	19.4	6.9
ACT Science score	19.7	5.8
ACT Composite score	19.5	5.9
Received English signal	0.55	0.50
Received Reading signal	0.45	0.50
Received Math signal	0.34	0.47
Received Science signal	0.23	0.42
Number of signals received	1.57	1.54
Received free/reduced price lunch	0.23	0.42
Female	0.50	0.50
Male	0.50	0.50
Plan to attend college	0.48	0.50
No plan to attend college	0.37	0.48
Graduated	0.85	0.36
Enrolled anywhere	0.58	0.49
Enrolled two-year college	0.19	0.39
Enrolled four-year college	0.40	0.49
Fraction of enrollees out of state	0.29	0.45
$N = 50{,}760$		

in 2008,[10] as well as their demographic characteristics, including gender, race, their free or reduced price lunch status,[11] and whether they graduated from high school. For students who enroll at a public college in Colorado, we can identify which institution they enrolled at, as we are able to link the students to enrollment records from these schools.[12] Additionally, CDHE has provided us with some basic information from the National Student Clearinghouse on the institution (if any) each student is attending, including the state, whether it is a public school, and whether it is a two- or four-year institution.[13]

Tables 1 and 2 show summary statistics from our data. The average ACT composite score is 19.5. Fifty-five percent of students receive a college readiness signal in English (the most common signal), while only 23% of students receive a signal in Science (the least common signal). Fifty-eight percent of students enroll in some post-secondary schooling; 40% of students enroll in a four-year college. Table 2 shows the fraction of students in each subgroup who receive each number of signals (zero through four). There is a great deal of variation across demographic groups in terms of how many signals the average student receives. For instance, 65% of free-lunch recipients do not receive a signal, while only 30% of non-free-lunch recipients do not receive a signal. Students who enroll in four-year colleges are much more likely to receive a high number of signals compared to students who enroll in two-year colleges or do not enroll at all.

[10] All 10th graders in Colorado are required to take a standardized state exam called the CSAP. Like the ACT, the CSAP has four subtests: Reading, Writing, Math, and Science.

[11] "Free Lunch" status is a proxy for low-income as students qualify for this subsidy based on their household income.

[12] While we are able to link the CDE records to college enrollment records, we are unable to link them to college *application* records because of inconsistencies in student identifiers.

[13] In 2010, the National Student Clearinghouse data covered over 90% of all enrollments nationally (Dynarski, Hemelt, & Hyman, 2013). See Dynarski et al. (2013) for a thorough description of the National Student Clearinghouse dataset.

Fig. 2 shows the distribution of ACT scores for our entire sample of students by subject. The red line on the graph indicates the college-readiness threshold. A few characteristics of the data emerge. First, the distribution of scores differs by subject and shows bunching at certain scores.[14] Second, the college-readiness threshold is well above the mean score for Science and Math, close to the mean for Reading, and below the mean for English. This implies that the marginal student is from a different part of the score distribution for each of the subjects. Finally, while there are a number of individuals who fall on both sides of the college-readiness thresholds in each subject, the number of observations that will identify the effect of each signal differs by subject.

4. Methodology

4.1. Traditional regression discontinuity design

The goal of this analysis is to estimate the causal effect of receiving information about one's own college-readiness on college enrollment. In order to do this, we compare individuals who score just above the threshold for college-readiness (and hence receive that information) to students who scored just below the threshold. This comparison is the set-up underlying a traditional regression-discontinuity design (RDD) with a single assignment variable. However, the ACT college-readiness treatment differs from that of the standard RDD, because the four subject test implies that there are four simultaneous discontinuities. That is, students are at "risk" of reaching more than one of the ACT college readiness benchmarks. A handful of studies have also used RDD with multiple discontinuities. We follow the methodology of Card, Chetty, and Weber (2007), but extend it from two to four discontinuities. Hemelt (2011) also employs a similar strategy when faced with multiple discontinuities.

Consider first a model in which an individual's likelihood of applying to and enrolling in a college or university is a function of whether the student receives a signal for college readiness:

$$y_i = \alpha + \beta_{\text{CR}} \times 1(\text{score}_i \geq \text{cutscore}) + \epsilon_i \tag{1}$$

where the expression $1(\text{score}_i \geq \text{cutscore})$ is equal to one if the individual's score is greater than or equal to the threshold for getting the readiness signal (cutscore). The parameter of interest in this equation is β_{CR}, which measures the effect of receiving the college-readiness signal on some outcome, y. The classic challenge of identification is that individuals who receive the signal and those who do not may be systematically different; people who do not receive the signal have lower ACT scores and are less likely to go to college than students who receive the signal for reasons unrelated to signal receipt. Therefore, estimating this simple equation will likely yield a biased result.

However, if we assume that ACT scores are imperfectly manipulable and there is no selection into taking the exam, then comparing students very close to the college-readiness benchmark cutoffs may yield an unbiased estimate of β_{CR}.

[14] We have verified that this is the true distribution of test scores for students in Colorado in 2009 by cross-referencing our statistics with those from the official ACT summary statistics (ACT, 2010).

Table 2
Summary—fraction of each subgroup receiving each number of signals.

Subgroup	0 Signals	1 Signal	2 Signals	3 Signals	4 Signals
Free lunch recipient	64.97	14.35	10.97	5.08	4.63
Non-free lunch recipient	30.00	15.18	18.06	13.93	22.84
Female	34.94	16.41	19.23	12.81	16.61
Male	40.83	13.59	13.71	11.09	20.79
Plan to attend college	36.08	16.39	18.00	12.08	17.46
No plan to attend college	32.56	15.08	17.13	12.74	22.48
Not enrolled	60.33	14.84	12.35	5.89	6.60
Enrolled any college	22.43	15.09	19.26	16.13	27.09
Enrolled four-year college	11.69	13.12	19.86	19.52	35.81
Enrolled two-year college	45.55	19.39	18.02	8.82	8.22
Enrolled out of state	11.09	10.02	16.75	18.53	43.61

Note: Free lunch recipients are students who qualify for free or reduced-price lunch.

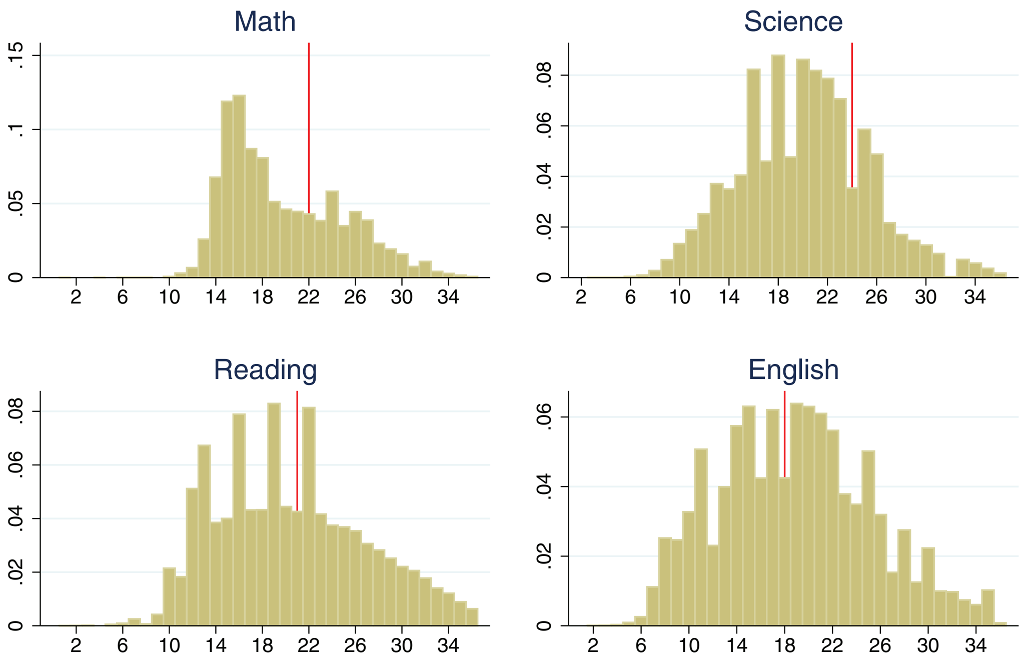


Fig. 2. Distribution of ACT subject test scores.
Note: The vertical lines denote the score at which students are considered to be college-ready by ACT in that subject.

This bias is overcome because students do not have perfect control over their ACT score. The underlying distribution in unobserved characteristics for those who just marginally did and did not receive the signal should be the same. Formally,

$$\lim_{\Delta\to 0^+} E[\epsilon|\text{score} = \text{cutscore} + \Delta] = \lim_{\Delta\to 0^-} E[\epsilon|\text{score} = \text{cutscore} + \Delta] \quad (2)$$

In keeping with the literature, we can then augment Eq. (1) with a control function, $f(\text{score})$. The key observation is then that if $f(\text{score})$ is continuous through the threshold, while CR is not, then β_{CR} is identified. Therefore, any discontinuity at the cutoffs can be asserted as the causal impact of receiving a college-readiness signal on the outcome of interest.

Consider first a simple setting in which there was only one subject on the ACT, and individuals received a signal for college readiness if they exceeded the college-readiness benchmark in that subject. Then we can estimate the effect of the signal using a regression discontinuity (RD) design, where we estimate $f(\text{score})$ by a flexible polynomial function, as below.

$$y_i = \alpha + \beta_{CR} \times 1(\text{score}_i \geq \text{cutscore}) + f(\text{score}_i) + \epsilon_i \quad (3)$$

where $f(\text{score})$ includes cubic polynomials in each subject and interactions between the polynomials and the indicator for being above the cutoff. However, our setting is not as simple as this single college-readiness signal case. Students that take the ACT can receive any of four college-readiness signals (one for each subject). Because individuals can be close to multiple cutoffs, it is unlikely that $f(\text{score}_i)$ is continuous through the cutoff. To address this issue, we augment the control function by flexibly controlling for the other scores. In order to do so, we include all possible discontinuities in one estimating equation:

$$y_i = \alpha + \sum_{s=1}^{4} [\beta_{CR,s} \times 1(\text{score}_{is} \geq \text{cutscore}_s) + f(\text{score}_{is})] + \epsilon_i \quad (4)$$

where $1(\text{score}_s \geq \text{cutscore}_s)$ is an indicator for whether an individual's score on subject s exceeds the cutoff for subject s. Then $\beta_{CR,s}$ is the causal impact of receiving a signal of college readiness in subject s on outcome y. For each outcome that we consider, we run four different estimations of Eq. (4), one for each subject, where the control function is the polynomial in that subject's scores, as well as the flexible controls for the other three scores. For each subject's regression, we restrict the sample to students close to that subject's benchmark (a bandwidth of five).[15] We then report $\beta_{CR,s}$ for that subject. Again following Card et al. (2007), we cluster standard errors on individual.[16]

An alternative strategy is to estimate Eq. (4) for individuals that are close to *all four* cutoffs, and report all four $\beta_{CR,s}$s from the same equation. However, this would estimate a local average treatment effect for a very selected group. The two strategies yield nearly identical results in our setting.[17]

In order for the RD to be valid, two things need to be true. First, the distribution of scores needs to be smooth through the cutoff; if it is not, that is evidence of sorting, which would invalidate the RD design. Second, there cannot be a discontinuity in observable, pre-treatment characteristics at the cutoff. If that is the case, then the students just below the cutoff are a poor control group for those who receive the treatment. In order to verify the first requirement, we perform a McCrary test (McCrary, 2008). We find no evidence of a discontinuity in the score distribution at the cutoff for English, Math, and Reading, which we take as evidence that individuals are unable to sort on either side of the cutoff. However, the McCrary test shows that there is evidence of sorting just below the Science threshold. This may be because the Science college-readiness threshold is at the top of the distribution and/or because relatively few students have exactly the threshold score relative to scores just below the threshold. Because of this, we focus our discussion on the other three signals. To address the second concern, we estimate Eq. (4) with various demographic characteristics as the outcome. Some of our results are given in Figs. A.8 and A.10, and show no evidence of a discontinuity.[18]

This analytic approach certainly has its merits—namely its direct parallels with the traditional RDD and computational simplicity which allow us to analyze the effects for subgroups of the student population. However, this approach requires us to assume that the signals have independent effects on enrollment decisions. That is, we assume that each signal is a completely separate piece of information and that the effect of getting all four signals is simply the sum of those estimated coefficients. In order to relax this assumption, we extend the empirical model of Papay, Willett, and Murnane (2011a), which allows for the signals to interact with one another. We briefly discuss the methodology, and then present estimates using this less restrictive approach.

4.2. Regression discontinuity with multiple assignment variables

By generalizing the standard RDD model to include multiple assignment variables, Papay et al. (2011a) and Papay, Willett, and Murnane (2014) (hereafter PWM) are able to simultaneously model discontinuities that arise when there are multiple criteria that determine placement into different treatment conditions. To clearly illustrate the multi-dimensional RDD approach, we use the test score labeling application used in Papay et al. (2011b). In this setting, students take an exam with two different subjects, English and Math, and are assigned treatment based on their scores in each subject.

To estimate their RDD model, they define four key variables. X_{ei} and X_{mi} are the two assignment variables, the student's score on each of the subject tests, and c_e and c_m define the respective cutoffs. For each individual, they define the treatment indicators W_e and W_m as follows:

$$W_{ei} = 1(X_{ei} \geq c_e) \text{ and } W_{mi} = 1(X_{mi} \geq c_m)$$

Thus, an individual can fall into one of four "treatment" conditions: passing both English and Math, passing only English, passing only Math, and failing both subjects. The four treatment conditions define four separate regions in the two-dimensional space spanned by the assignment variables, X_e and X_m. Similar to the case with a single assignment variable, the parameters of interest are the conditional mean probabilities for individuals in each treatment condition, at the cutoff. For example, the causal effect of passing the Math exam instead of failing it for individuals who have passed the English exam.

They estimate the causal effect of passing the exams using local linear regression analysis. They fit the requisite regression models in each region simultaneously by specifying a single statistical model with 16 parameters—an intercept and slope parameter to accompany all 15 possible interactions among X_{ei}, X_{mi}, W_{ei}, and W_{mi}. In order to do so, they estimate the following model with the two assignment variables (X^c_{ei} and X^c_{mi}) centered on their respective cut-points:

$$\begin{aligned} y_i = {} & \beta_0 + \beta_1 W_{ei} + \beta_2 W_{mi} + \beta_3 (W_{ei} \times W_{mi}) \\ & + \beta_4 X^c_{ei} + \beta_5 X^c_{mi} + \beta_6 (X^c_{ei} \times X^c_{mi}) \\ & + \beta_7 (X^c_{ei} \times W_{ei}) + \beta_8 (X^c_{mi} \times W_{mi}) + \beta_9 (X^c_{ei} \times W_{mi}) \\ & + \beta_{10} (X^c_{mi} \times W_{ei}) + \beta_{11} (X^c_{ei} \times X^c_{mi} \times W_{ei}) \\ & + \beta_{12} (X^c_{ei} \times X^c_{mi} \times W_{mi}) \\ & + \beta_{13} (X^c_{ei} \times W_{ei} \times W_{mi}) + \beta_{14} (X^c_{mi} \times W_{ei} \times W_{mi}) \\ & + \beta_{15} (X^c_{ei} \times X^c_{mi} \times W_{ei} \times W_{mi}) \end{aligned} \tag{5}$$

The above equation allows one to quantify the effect of each treatment, separately and together with the other. For example, the effect of passing the English test only is β_1 and effect of passing the Math test only is β_2. The effect of passing both English and Math is $\beta_1 + \beta_2 + \beta_3$. Thus, β_3 is the additional effect of passing both tests. This model allows for

[15] We show that results are robust to alternate bandwidth choices in Table A.7 of the supplementary material.

[16] We also tried clustering on score bin as in Lee and Card (2008) and it had no meaningful impact on the standard errors.

[17] Technically, these two samples identify two different local average treatment effects, one for students close to one cutoff, another for students close to *all* cutoffs. However, because we are controlling for all scores, in practice these should be almost identical.

[18] Full results of the McCrary test and the background characteristics regressions are available upon request.

a richer definition of treatment, and for the effect of each individual treatment to differ based on the other treatments. It also allows for the size of the treatment effect of passing one subject to differ by the score of the other subject (β_9 through β_{15}).

We extend the approach taken by PWM to four assignment variables and utilize eight key variables. The four running variables that determine treatment are X_{ei}^c, X_{mi}^c, X_{si}^c, and X_{ri}^c, the centered scores for English, Math, Science, and Reading, respectively. Additionally, our treatment indicators for each subject are defined as $W_{ji} = 1(X_{ji}^c > 0)$, for each subject j. Therefore, we estimate an augmented version of Eq. (5), as shown below.

$$\begin{aligned} y_i = \ & \beta_0 + \beta_1(W_{ei}) + \beta_2(W_{mi}) + \beta_3(W_{si}) + \beta_4(W_{ri}) \\ & + \beta_5(W_{ei} \times W_{mi}) + \beta_6(W_{ei} \times W_{si}) + \beta_7(W_{ei} \times W_{ri}) \\ & + \beta_8(W_{mi} \times W_{si}) + \beta_9(W_{mi} \times W_{ri}) + \beta_{10}(W_{si} \times W_{ri}) \\ & + \beta_{11}(W_{ei} \times W_{mi} \times W_{si}) + \beta_{12}(W_{ei} \times W_{mi} \times W_{ri}) \\ & + \beta_{13}(W_{ei} \times W_{si} \times W_{ri}) \\ & + \beta_{14}(W_{mi} \times W_{si} \times W_{ri}) \\ & + \beta_{15}(W_{ei} \times W_{mi} \times W_{si} \times W_{ri}) + \Gamma_i + \epsilon_i \end{aligned} \quad (6)$$

where Γ_i includes all 240 additional interactions between subject scores and treatment indicators. Our analysis focuses only on the interactions between the treatment indicators (signal). From the above equation, we can estimate the effect of each combination of treatment indicators. The added dimensionality complicates both implementation and interpretation. To aid the reader in interpreting the coefficients, we show how each of the 16 treatment conditions is estimated in Table 4. Table 4 displays the combined effect of receiving the subject signals shown in each row. For instance, the combined effect of receiving the English, Math, and Reading signals is $\beta_1 + \beta_2 + \beta_4 + \beta_5 + \beta_7 + \beta_9 + \beta_{12}$.

A challenge in implementing our approach comes in choosing the appropriate bandwidths (h_1^*, h_2^*, h_3^*, h_4^*) for our analysis. In order to do so, we again generalize the approach of PWM. For each observation, we use a local linear regression analysis – within an arbitrary bandwidth (h_1, h_2, h_3, h_4) – to estimate a fitted value of the outcome at that point.

$$\begin{aligned} & \hat{\mu}(X_{ei}^c, X_{mi}^c, X_{si}^c, X_{ri}^c, h_1, h_2, h_3, h_4) \\ & = \hat{\gamma}_0 + \hat{\gamma}_1 X_{ei}^c + \hat{\gamma}_2 X_{mi}^c + \hat{\gamma}_3 X_{si}^c + \hat{\gamma}_4 X_{ri}^c \\ & \quad + \hat{\gamma}_5(X_{ei}^c \times X_{mi}^c) + \hat{\gamma}_6(X_{ei}^c \times X_{si}^c) + \hat{\gamma}_7(X_{ei}^c \times X_{ri}^c) \\ & \quad + \hat{\gamma}_8(X_{mi}^c \times X_{si}^c) + \hat{\gamma}_9(X_{mi}^c \times X_{ri}^c) + \hat{\gamma}_{10}(X_{si}^c \times X_{ri}^c) \\ & \quad + \hat{\gamma}_{11}(X_{ei}^c \times X_{mi}^c \times X_{si}^c) + \hat{\gamma}_{12}(X_{ei}^c \times X_{mi}^c \times X_{ri}^c) \\ & \quad + \hat{\gamma}_{13}(X_{ei}^c \times X_{si}^c \times X_{ri}^c) \\ & \quad + \hat{\gamma}_{14}(X_{mi}^c \times X_{si}^c \times X_{ri}^c) + \hat{\gamma}_{15}(X_{ei}^c \times X_{mi}^c \times X_{si}^c \times X_{ri}^c) \end{aligned} \quad (7)$$

In each case, we attempt to mirror the regression-discontinuity approach by only using observations that fall within the appropriate region, and estimating $\hat{\mu}(X_{ei}^c, X_{mi}^c, X_{si}^c, X_{ri}^c, h_1, h_2, h_3, h_4)$ as if it were a boundary point. For a given bandwidth (h_1, h_2, h_3, h_4), we thus estimate a fitted probability of enrollment in any college for each observation. For computational feasibility, we must require that the bandwidths around each subject score are equal. That is, we constrain the set of possible bandwidths such that the following expression is satisfied: $h_1 = h_2 = h_3 = h_4$.

We compare these fitted values to the observed values, across the entire sample, using a generalized version of the Imbens and Lemieux (2008) cross-validation criterion:

$$\begin{aligned} & CV_{\text{ENROLL}}(h_1, h_2, h_3, h_4) \\ & = \frac{1}{N} \sum_{i=1}^{N} \left(\text{ENROLL}_i - \hat{\mu}(X_{ei}^c, X_{mi}^c, X_{si}^c, X_{ri}^c, h_1, h_2, h_3, h_4)\right)^2 \end{aligned} \quad (8)$$

Our optimal joint bandwidth, h_1^*, h_2^*, h_3^*, and h_4^*, is the set of bandwidths that minimize the CV criterion. We calculate that the CV criterion is minimized at a bandwidth of nine.[19]

5. Results

Table 3 shows the results from the traditional RD analysis, while Fig. 3 shows the graphical representations of this analysis for the math signal. The figures for the other three subjects are available in the supplementary material.[20] These estimates allow us to test Hypotheses 1, 3, and 4 from Section 1. While most of the estimates are not statistically different from zero, a few results emerge. First, we find no evidence to support our first hypothesis that students receiving a signal would be more likely to attend college and that the effect might be larger for those students who were not planning to attend college or who had less information about their own ability. There are no statistically significant results for the “enroll in college” outcome, displayed in the first panel of Table 3, either for the full sample or for the subgroups that we might have expected to respond more.

We do, however, observe some evidence of resorting between types of institutions (Hypothesis 3). For the full population of students, receipt of the English signal decreases the likelihood of enrolling in a two-year college by about 9% and increases out-of-state enrollment by 11%. A similar result emerges for students who receive free or reduced-price lunch for whom receiving the English signal makes them less likely to enroll in a two-year college, but more likely to enroll in a four-year college. Two-year and out-of-state college enrollment for females is negatively affected by receipt of the Math signal.

Overall, the lack of strong effects leads us to believe that, for most of the students, receiving college-readiness information in this capacity does not affect their college-going decisions. This may due to any of the reasons listed in our fourth hypothesis; that is, either the measures are not salient, not delivered early enough to allow students to adjust their plans, or are precisely in line with their priors about themselves.

As mentioned in Section 4, the benefit of using the Multiple Assignment RD approach is that we are able to allow the receipt of signals to interact with one another. One benefit this provides is that we are able to discuss the effect of receiving multiple signals at the same time. However, it also means estimating a regression with a large number of control variables in a relatively small dataset. This becomes even

[19] This bandwidth selection criteria is the same used in Papay et al. (2011a).

[20] Additionally, we ensure that our results are not sensitive to the inclusion of controls, which we show in Table A.6.

Table 3
Effect of college readiness signals on enrollment, by subgroup.

	(1) All	(2) Free lunch	(3) Female	(4) Male	(5) Plan college	(6) No plan college
Enroll in college						
English	−0.0433	−0.0538	−0.0987	−0.0069	−0.0920	0.0175
	(0.0477)	(0.0911)	(0.0707)	(0.0661)	(0.0669)	(0.0781)
Reading	0.0297	−0.1451	0.0708	−0.0338	−0.0009	−0.1205*
	(0.0454)	(0.1048)	(0.0625)	(0.0671)	(0.0615)	(0.0718)
Math	−0.0506	0.1315	−0.0841	0.0043	0.0484	−0.0346
	(0.0454)	(0.1183)	(0.0596)	(0.0694)	(0.0613)	(0.0733)
Science	−0.0030	0.0694	−0.0327	0.0381	0.0187	−0.0260
	(0.0364)	(0.1064)	(0.0482)	(0.0556)	(0.0493)	(0.0574)
Enroll in two-year college						
English	−0.0882**	−0.1837**	−0.0833	−0.0954	−0.1170*	−0.0482
	(0.0427)	(0.0784)	(0.0644)	(0.0586)	(0.0613)	(0.0714)
Reading	0.0326	−0.1385	−0.0078	0.0569	−0.0077	−0.0229
	(0.0400)	(0.0910)	(0.0567)	(0.0579)	(0.0571)	(0.0639)
Math	−0.0657	0.0983	−0.0935*	−0.0244	−0.0499	−0.0707
	(0.0403)	(0.0987)	(0.0563)	(0.0589)	(0.0578)	(0.0651)
Science	0.0042	0.0561	0.0014	0.0240	0.0198	0.0652
	(0.0319)	(0.0897)	(0.0434)	(0.0476)	(0.0461)	(0.0502)
Enroll in four-year college						
English	0.0503	0.1326*	−0.0054	0.0870	0.0255	0.0625
	(0.0411)	(0.0726)	(0.0613)	(0.0564)	(0.0591)	(0.0687)
Reading	−0.0072	−0.0114	0.0735	−0.0944	−0.0009	−0.1007
	(0.0435)	(0.0877)	(0.0613)	(0.0627)	(0.0604)	(0.0703)
Math	0.0155	0.0394	0.0037	0.0356	0.0986	0.0385
	(0.0480)	(0.1101)	(0.0680)	(0.0683)	(0.0668)	(0.0777)
Science	−0.0098	0.0008	−0.0323	0.0093	−0.0059	−0.0973
	(0.0392)	(0.1033)	(0.0543)	(0.0572)	(0.0546)	(0.0623)
Enroll out of state						
English	0.1151**	0.0716	0.0821	0.1015	0.0339	0.1735*
	(0.0518)	(0.0742)	(0.0809)	(0.0676)	(0.0650)	(0.0898)
Reading	−0.0045	0.0332	0.0617	−0.0582	0.0275	−0.0461
	(0.0626)	(0.0972)	(0.0920)	(0.0848)	(0.0794)	(0.1059)
Math	−0.1185	0.1664	−0.2524**	−0.0057	0.0096	−0.0593
	(0.0727)	(0.1227)	(0.1149)	(0.0913)	(0.0968)	(0.1170)
Science	−0.0865	−0.1247	−0.0382	−0.1109	−0.0271	−0.1181
	(0.0625)	(0.1340)	(0.0911)	(0.0849)	(0.0885)	(0.1015)

Each coefficient is a separate estimate of $\beta_{CR,s}$ from Eq. (4). Each panel in the table is for a different outcome variable, which is listed at the top of the column. Each column is for a different subgroup, which is listed at the head of each column. Standard errors in parentheses are clustered on individual.
* $p < 0.10$,
** $p < 0.05$.

more problematic once we move to our subgroup analysis. For this reason we focus primarily on the full sample in the text, and for transparency, display the subgroup results in the supplementary material.

We begin our Multiple Assignment RD analysis by assessing how overall college enrollment is affected by receiving a college-readiness signal. These results are shown in the first column of Table 5. The estimate in each cell shows the combined effect of receiving all the subject signals shown in the leftmost column in that row and is constructed according to the linear combination of βs in the corresponding cell of Table 4. To give readers a better idea of how many individuals are close to those given cutoffs and experience the outcome of interest (and thus are identifying each estimate), we show the number of individuals with scores that fall within two points of those subject thresholds but do not receive any of the other signals. For example, for the Math and English signals, we show the number of individuals who are within two points of receiving both the Math and English signals but were below the Science and Reading thresholds. Columns (2)–(4) display the results for two-year enrollment, four-year enrollment and out of state enrollment.

While the table allows us to display the effect of each subject signal separately, we also want to assess the average effect of each number of signals to see if any additional patterns emerge. To do so, we graph the weighted averages of the estimates for each number of signals, where the weights are the number of individuals near the thresholds of interest (as shown on the tables). Fig. 4 has four panels – one for each outcome – and shows how receiving each number of signals affects that outcome. For instance, the top left panel displays the estimates shown in the first column of Table 5. The first data point on the graph is the weighted average of receiving just one subject signal (the first four estimates in the

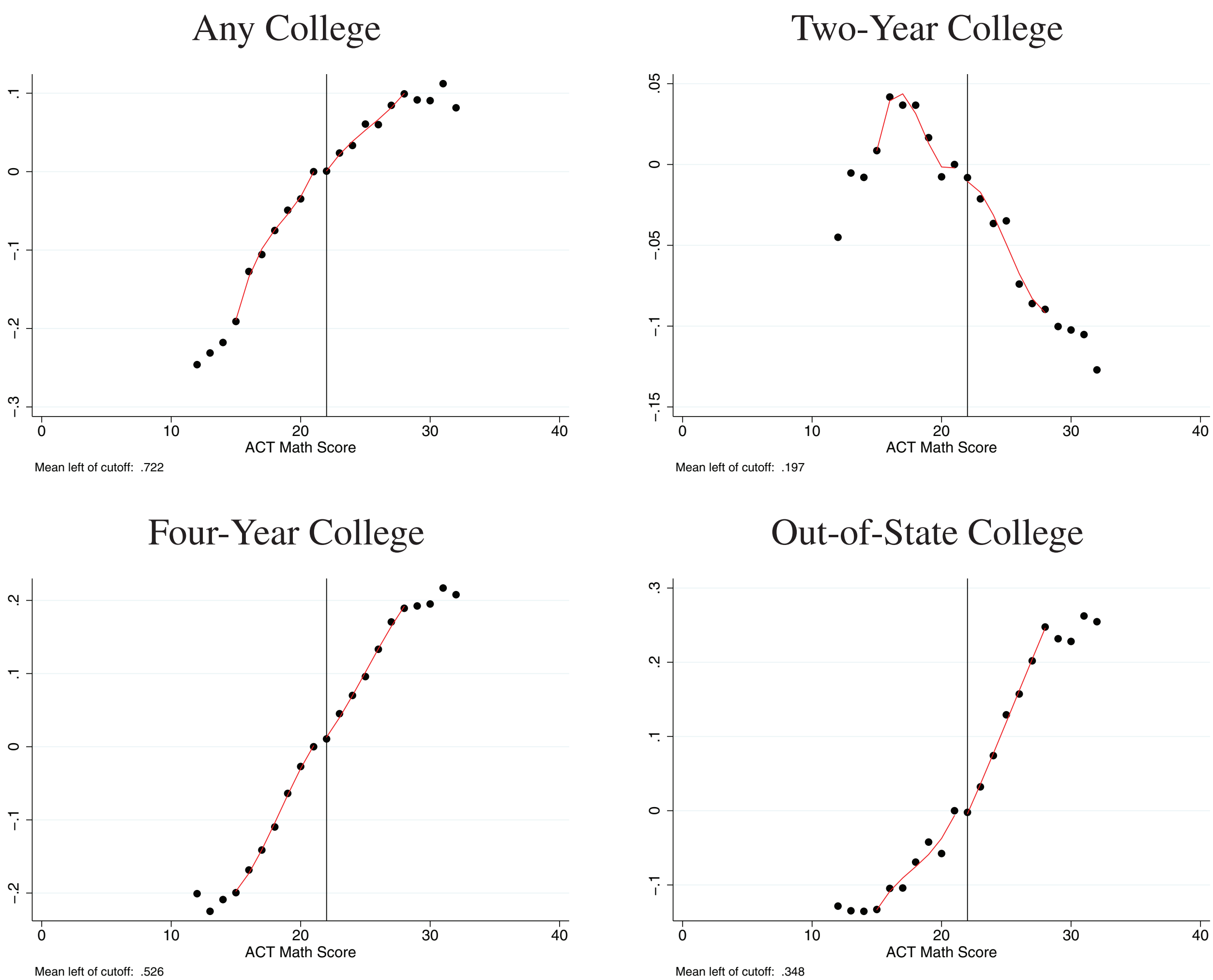


Fig. 3. Receipt of Math signal on college enrollment.
Note: Following Card et al. (2007), Fig. 3 plots the βs from an equation of the form: $y_i = \alpha + \sum_{s=0}^{36}[\beta_s \times 1(\text{score}_i \geq \text{cutscore})] + f(\text{score}_i) + \epsilon_i$ along with the fitted polynomial estimated in Eq. (3). The values can be interpreted as the difference in probability for the group of individuals at each score and the group that scored one point below that subject's cutoff.

Table 4
Combined effects of the subject signals.

Number of total signals	Subject signals received	Coefficients
1	English	β_1
1	Math	β_2
1	Science	β_3
1	Reading	β_4
2	Math and Science	$\beta_2 + \beta_3 + \beta_8$
2	Math and Reading	$\beta_2 + \beta_4 + \beta_9$
2	Math and English	$\beta_1 + \beta_2 + \beta_5$
2	Science and Reading	$\beta_3 + \beta_4 + \beta_{10}$
2	Science and English	$\beta_1 + \beta_3 + \beta_6$
2	Reading and English	$\beta_1 + \beta_4 + \beta_7$
3	Math, Science, and Reading	$\beta_2 + \beta_3 + \beta_4 + \beta_8 + \beta_9 + \beta_{10} + \beta_{14}$
3	Math, Science, and English	$\beta_1 + \beta_2 + \beta_3 + \beta_5 + \beta_6 + \beta_8 + \beta_{11}$
3	Math, Reading, and English	$\beta_1 + \beta_2 + \beta_4 + \beta_5 + \beta_7 + \beta_9 + \beta_{12}$
3	Science, Reading, and English	$\beta_1 + \beta_3 + \beta_4 + \beta_6 + \beta_7 + \beta_{10} + \beta_{13}$
4	Math, Science, Reading, and English	$\beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5 + \beta_6 + \beta_7$ $\beta_8 + \beta_9 + \beta_{10} + \beta_{11} + \beta_{12} + \beta_{13} + \beta_{14} + \beta_{15}$

Note: Coefficients are estimated using Eq. (6).

Table 5
All outcomes for the full sample.

Number of signals	Subjects	Enroll	Enroll four-year	Enroll two-year	Enroll out-of-state
1	Math	−0.1886	−0.1321	−0.0591	−0.1488
		(0.1508)	(0.1468)	(0.1287)	(0.1798)
		953	953	953	384
	Science	−1.0482***	−0.5110	−0.5283*	−1.5590**
		(0.3876)	(0.4186)	(0.3137)	(0.6493)
		546	546	546	206
	Reading	−0.2748	−0.4356*	0.1636	0.1509
		(0.2258)	(0.2301)	(0.2169)	(0.3438)
		2455	2455	2455	1029
	English	0.1110	−0.0253	0.1341	0.0902
		(0.1833)	(0.1870)	(0.1627)	(0.2801)
		3870	3870	3870	1622
2	Math and Science	−0.4154	−0.2875	−0.1307	0.8905
		(0.3231)	(0.3439)	(0.2516)	(0.6024)
		193	193	193	64
	Math and Reading	0.0540	−0.0039	0.0590	0.2810
		(0.3075)	(0.2648)	(0.2420)	(0.4350)
		361	361	361	129
	Math and English	−0.0776	−0.1575	0.0750	−0.0586
		(0.2781)	(0.2759)	(0.2450)	(0.3733)
		700	700	700	264
	Science and Reading	−0.3120	−0.7757**	0.4610	−0.0939
		(0.3705)	(0.3400)	(0.3640)	(0.4774)
		259	259	259	92
	Science and English	−0.6129*	−0.0111	−0.6038**	−0.9567**
		(0.3330)	(0.3739)	(0.2791)	(0.4329)
		403	403	403	149
	Reading and English	−0.2751	−0.3367*	0.0605	0.0356
		(0.1749)	(0.1746)	(0.1525)	(0.2380)
		1521	1521	1521	613
3	Math, Science, and Reading	−0.8139*	−0.2708	−0.5419**	1.1743
		(0.4301)	(0.3942)	(0.2672)	(0.8633)
		92	92	92	25
	Math, Science, and English	−0.5437	−0.7486**	0.2031	−0.4627
		(0.3518)	(0.3588)	(0.3004)	(0.4835)
		168	168	168	52
	Math, Reading, and English	0.0537	0.0950	−0.0441	−0.3144
		(0.3677)	(0.3361)	(0.3120)	(0.4450)
		327	327	327	108
	Science, Reading, and English	−0.3421	−0.4410	0.0924	0.3030
		(0.4647)	(0.4489)	(0.3978)	(0.3066)
		226	226	226	81
4	Math, Science, Reading, and English	−0.2824	−0.3004	0.0119	−0.3003
		(0.3096)	(0.3144)	(0.2734)	(0.4510)
		106	106	106	29

Notes: Each estimate is constructed according to the formula listed in the corresponding cell in Table 4. The value for *N* gives the number of individuals with scores that fall within two points on either side of the cutoff for the signal(s) listed in column 2 and *did not* receive any additional signals.
* $p < 0.10$,
** $p < 0.05$,
*** $p < 0.01$.

column) while the second data point is the weighted average of receiving two subject signals. The advantage of showing the weighted averages is that the estimates identified by relatively few individuals (such as receipt of the science signal) do not receive as much weight.[21]

In general, the Multiple Assignment RD does not provide any consistent evidence that signal receipt changes students' behavior. Many of the statistically significant estimates are the ones that include science, which was the subject that failed the McCrary test. Because of this, we caution interpretation of these results, but include them for transparency. This analysis allows us to test Hypothesis 2, whether there are any negative effects on enrollment from receiving only a couple of the signals. Fig. 4 shows that there is no more of a negative response for one or two signals than there are for three or four. We also look to our subgroup analysis for those planning on going to college to see if they provide any additional insight (Fig. A.14) and again find no evidence that students' college-going behavior is negatively affected by receiving fewer signals than anticipated.

[21] Fig. 4 and Figs. A.11 through A.15 show these graphs for each of the subgroups.

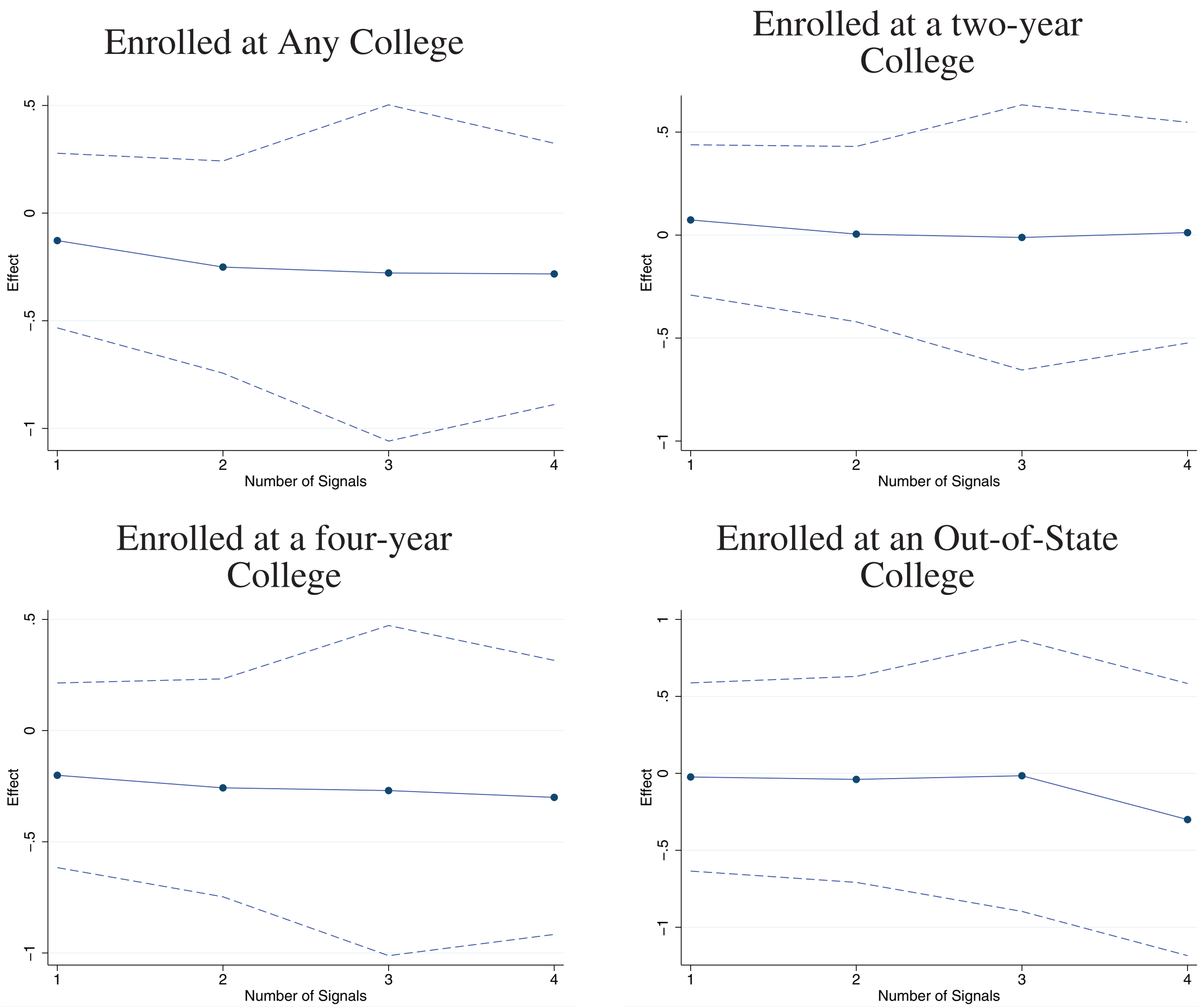


Fig. 4. Average effect of the college-readiness signals for all students.

Note: Fig. 4 plots the weighted average of the effect of college-readiness signals estimated using Eq. (6) for the subgroup listed in the title. The point plotted for "1 signal" is the weighted average of the four single-subject effects described in the first four rows of Table 4, the point plotted for "2 signals" is the weighted average of the six two-subject effects, the point plotted for "3 signals" is the weighted average of the four three-subject effects and the point plotted for "4 signals" is the estimated effect of receiving all four signals. The dotted lines show the 95% pointwise confidence interval for the weighted average.

6. Conclusion

Using administrative data from the state of Colorado, we utilize two variations of the regression discontinuity design to determine the effect of the ACT's college readiness benchmarks on student behavior regarding college enrollment. By exploiting the existence of the state-mandated ACT, we overcome the issue of selection into exam-taking. With the exception of suggestive evidence for low-income students, we find no causal effect of receiving a college-readiness signal on the decision to enroll in college. While recent studies suggest that students respond to information interventions regarding higher education, we find no such response on the particular margin we study, which raises questions about how to design effective information interventions.

There are a number of explanations for our findings. First, students may already know how college-ready they are and these signals provide no additional information to the students. Thus, students who are considered by ACT to be college-ready but do not attend college either have preferences that lead them away from higher education or may be dissuaded because of costs. Second, the college-readiness signal may not be presented in a clear and/or salient manner. Although ACT is a reputable source of information and score reports sent directly from ACT are likely to be opened and at least cursorily reviewed, students may not be reading their score report carefully enough to extract the information about their college-readiness. The college-readiness signals are certainly not highlighted in any way and may be missed by many students. Finally, the signal may come too late for students to make major changes that would allow them to alter their college trajectory. While a number of studies have found that interventions late in one's high school career can have a positive effect on educational outcomes

(Avery, 2010; Carrell & Sacerdote, 2013; Hoxby & Turner, 2013), the one studied in this paper does not appear to affect college-going behavior. This may be because of the population we are studying (those on the margin of college-readiness) or that this information was not paired with any assistance in undertaking the college application process.

We do find limited evidence that low-income students who receive the English signal (the lowest of the benchmark scores) are more likely to enroll in a four-year college and less likely to enroll in a two-year college. While these estimates are too sensitive to specification and bandwidth choice to be conclusive, they do suggest that information interventions may be more impactful for students who have the lowest beliefs or information about their college-readiness.

With the growing interest in using information interventions to alter students' college-going behavior, more research is needed to understand how marginal college students respond to information and how to provide this information most effectively. This work should focus on the salience of information treatments, the content and quality of the information, and the timing of it. As a highly reputable organization and key player in the college application process, ACT should better seize its opportunity to provide information to over 1.8 million students each year that can help them make better-informed choices about their higher education (ACT, 2014).

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Supplementary materials

Supplementary material related to this article can be found in the online version, at 10.1016/j.econedurev.2015.02.002.

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