Help or Hindrance? The Effects of College Remediation on Academic and Labor Market Outcomes

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Abstract

Providing remedial (also known as developmental) education is the primary way colleges and universities cope with students who do not have the academic preparation needed to succeed in college-level courses. Remediation is widespread, with nearly one-third of entering freshman taking remedial courses at a cost of at least $1 billion per year. As such, it constitutes an important example of a "second-chance" intervention designed to help young people develop human capital. Despite its prevalence in American higher education, there is considerable uncertainty surrounding its short- and longer-run effects. This paper presents new evidence on this question using longitudinal administrative data from the state of Texas and a research design that exploits the sharp test score cutoffs used to assign students to remediation. Aside from weak evidence that remediation improves the grades received in college-level mathematics courses, we find little indication that students benefit from remediation. Our estimates indicate that remediation has a minimal impact on the years of college completed, academic credits attempted, receipt of an academic degree, and labor market performance.

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1. Introduction

Many new college students arrive on campus lacking the preparation to successfully pursue postsecondary education. A recent study shows that only one-third of high school graduates possess the minimum qualifications for a four-year college (Greene and Foster, 2003). This widespread phenomenon is seen as a key reason why large numbers of college students, particularly those from disadvantaged backgrounds, drop out (Venezia et al., 2003). Given the sizable earnings premium associated with college attainment (Kane and Rouse, 1995), understanding how to help under-prepared students complete college is an important question for economists and policymakers.[1]

Remediation is the most common approach used by colleges to assist students who possess weak academic skills. Remedial – also known as developmental – education consists of courses and other services (such as tutoring and supervised study) that are designed to foster skills generally acquired in high school. It is a central feature of American higher education. Eighty percent of public four-year colleges and virtually all two-year colleges (98 percent) offer remedial courses (NCES, 2003). Among freshman entering college in the fall of 2000, nearly 30 percent participated in remediation. Remediation is even more common at two-year (or community) colleges, with 42 percent of entering freshman taking a remedial course (NCES, 2003).

Despite the prevalence of remediation, substantial controversy surrounds its use. Supporters contend that it helps poorly-prepared students succeed in college by allowing them a chance to catch up to their peers. According to this view, under-prepared students are better served in remedial courses than they would be floundering in college-level courses for which they are not ready (Lazarick, 1997). In contrast, opponents argue that any benefits of remediation are outweighed by its high cost. Estimates from a decade ago suggest public colleges spent $1 billion per year on remediation (Breneman and Haarlow, 1997), and some even argue that the costs are higher (Steinberg, 1998).[2] Worries about cost partly explain why some states

[1] It is important to recognize that dropping out of college can be optimal for some students (Manski, 1989). Nonetheless, as discussed in Angrist et al. (2006), poor performance in college might be a cause for concern if students misjudge the costs and benefits of leaving school (Dominitz and Manski, 2000) or if they have time-inconsistent preferences (Oreopoulos, 2006).

[2] Breneman and Haarlow estimate that Texas alone spent at least $172 million per biennium on remediation. However, remediation proponents argue Breneman and Haarlow's estimates overstate the financial costs of remediation (Abraham, 1998).

have cut funding for remediation programs (Bettinger and Long, 2007). For instance, Texas recently placed limits on publicly funded developmental coursework.[3]

The debate over college remediation also fits into the larger discussion in labor economics about which policies most effectively increase the skills of individuals with low levels of human capital. One side argues that little can be done to improve the outcomes of low-ability individuals once they reach a certain age (Carneiro and Heckman, 2003). According to this view, "skills beget skills" and effective interventions must therefore occur very early in life. Others maintain that some "second chance" programs (especially those aimed at individuals from disadvantaged backgrounds) have been successful even though the interventions did not take place at a young age (Krueger, 2003). Since college remediation is an important example of a later-life intervention, understanding whether remediation actually helps students develop economically valuable skills is informative about which view of human capital formation is more accurate.

Currently, there is considerable uncertainty surrounding the effectiveness of remediation. Assessing the impact of remediation is difficult because remediated students would likely have worse outcomes than non-remediated students in the absence of the program. Three recent papers in the economics literature represent the first serious attempts to address this challenge, but the evidence remains mixed. Bettinger and Long (2007) and Jepsen (2006) generally find positive effects of remediation on college persistence and attainment. However, using a sample of Florida community college students and a research design similar to that used in this paper, Calcagno (2007) finds little consistent evidence that remediation has positive effects. Moreover, as we explain below, all of these studies potentially face methodological limitations that make it difficult to interpret their findings.

This paper presents new evidence on the effect of remediation using a large longitudinal dataset of Texas students. Our regression discontinuity (RD) research strategy exploits the fact that during the time period of the study, Texas used placement test score results to assign students to remediation.[4] Consistent with this policy, we find clear evidence that students who barely failed this exam were more likely to be in remediation than were students who passed. This discontinuity generates exogenous variation in the probability of remediation, and is used to generate instrumental variables (IV) estimates of the effect of remediation.

[3] In 2005, Texas law changed so that schools would no longer receive additional funding for remedial courses students take after they have accumulated 27 developmental credit hours (nearly one academic year).

[4] A similar empirical strategy is used by Jacob and Lefgren (2004) to study the effect of remedial education at the elementary school level and by Matsudaira (2005) to study the effect of English as a Second Language programs.

The administrative records that comprise our data contain rich information that we use to construct detailed measures of student success in college such as academic credit hours, years of college completed, and degree attainment. In addition, we examine the impact of remediation on labor market earnings using Unemployment Insurance (UI) earnings records. Studying the labor market effects of remediation is important because it allows us to directly test whether remediation helps students become economically successful. To the best of our knowledge, no other study has examined the labor market effects of college remediation.

Our results provide little indication that students benefit from remediation. We find some evidence that students assigned to math remediation receive better grades in their first college-level math course. However, unlike most of our results, this finding is based on a conditional sample of course-takers, and might therefore be subject to selection bias. In contrast, for a wide range of academic and labor market outcomes, and across a variety of subgroups, the estimated effects of remediation are small in magnitude and statistically insignificant.

These results are also indirectly informative about the importance of peer interactions in college classrooms. By design, students in remediation tend to have lower-ability classmates than students in conventional college courses (at least initially when they are still in remediation). Consequently, remediation might also affect student outcomes via a peer effect. The small effects that we estimate therefore suggest that one's classmates are not an important input into the educational production function at the college level.[5,6] An alternative interpretation is that negative peer effects "undo" the positive effects of remediation, leading to a negligible net effect.

Although our empirical strategy delivers credible estimates of the impact of remediation, it is important to recognize that our estimates are most pertinent for students scoring close to the remediation-placement cutoff and whose participation in remediation is affected by falling above or below the cutoff. As we explain below, the effect for this group is interesting because a large fraction of remediated students score relatively close to the passing score. In addition, the students whose remediation status is affected by failing or passing the placement exam are the students who are directly affected by the remediation placement policies used by Texas.

[5] More generally, this reasoning applies to any intervention that involves segregating individuals by ability. Matsudaira (2005) makes a similar point in the context of English as a Second Language programs, as do Jacob and Lefgren (2004) in the context of mandatory summer school for low-performing students.

[6] Studies in elementary schools find that social interactions in the classroom have strong effects on student outcomes (Graham, 2006; Boozer and Cacciola, 2001). A study that looks at peer effects in college classrooms finds no evidence that mean peer ability affects own achievement, but does find interaction effects between the mean and standard deviation of peer ability (Hoel, Parker and Rivenburt, 2006).

The paper is organized as follows. The following section reviews the existing research as well as highlights the key institutional details regarding remediation in Texas. In Section 3 we describe our data sources and explain how we construct our analytic sample. Section 4 describes our empirical and estimation strategy. We present results in Section 5. Section 6 discusses our findings in terms of the policy debate and existing research, and Section 7 concludes.

2. Background

2.1 Existing Research

Most early studies of the effect of remediation suffer from serious methodological and data limitations. Chief among these is an inability to account for the differences between remediated and non-remediated students (O'Hear and MacDonald, 1995). However, several recent studies use innovative empirical strategies that address selection bias.[7] Bettinger and Long (2007) examine a sample of Ohio students and compare those with similar observable characteristics who attend schools with differing remediation placement policies, and who therefore have different rates of participation in remediation. To get around non-random assignment of students to colleges, they use an IV strategy that exploits the association between geographic proximity and choice of college. They find that remedial education has positive effects on transferring to a more selective college and earning a college degree. One potential limitation of their approach is that colleges may differ along dimensions other than their remediation policies. In particular, the estimates of the impact of remediation might be overstated if schools with stricter remediation placement policies have other (non-remediation) features that improve the outcomes of remediated students.

Jepsen (2006) compares the outcomes of California community college students who took remedial courses to those who did not. To control for differences across these two groups, he only uses students who were referred by staff to take a basic skills course, and who presumably have similar academic backgrounds. Like Bettinger and Long (2007), he finds some evidence that remediation has positive effects on college persistence and degree completion. This approach, however, assumes that students who participate in remediation are comparable to students who were referred by staff to take developmental courses, but chose not to do so. This would not be true if, for instance, referred students who enter remediation have relatively high levels of unmeasured academic motivation.

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[7] A related study is Angrist et al. (2007) who conduct a randomized field trial of a program that offers monetary rewards for good grades as well as educational services that resemble remediation (peer advising and organized study sessions). They find no effect of offering educational services alone, but they do find that being offered monetary incentives as well as the educational services increased grades and retention for females (but not males).

Calcagno (2007) uses data on Florida community college students and a RD design based on the placement exam passing cutoffs used to assign students to remediation.[8] In contrast to Bettinger and Long (2007) and Jepsen (2006), this study's results are somewhat inconclusive. It finds that remediation improves college persistence through increasing fall-to-fall retention. However, it finds little evidence that remediation increases the likelihood of completing first college-level coursework, transferring to a four-year college or completing a degree, with the estimates sensitive to sample definitions and regression specifications. One important limitation of the data he uses is that it only has information on the student's most recent placement exam score. Because some students who initially fail appear to retake and pass the test, the assumption that placement exam barely-failers are comparable to barely-passers might be violated.[9] In contrast, we have data on all of a student's test attempts. By focusing on the initial test score, we avoid the "retesting bias" problem.[10] Another distinction between our two studies is that we are able to examine remediation at both two-year and four-year colleges, while Calcagno (2007) only looks at two-year colleges.[11] Finally, our data allow us to examine the effect of remediation on labor market outcomes.

2.2 College Remediation in Texas

Our study examines the impact of remediation on Texas college students who entered college in the 1990's. During this period, Texas law required all students pursuing academic degrees to enter remediation if they could not demonstrate college readiness. This policy, known as the Texas Academic Skills Program (TASP), stipulated that college readiness could be shown by passing the statewide TASP test or one of the state-approved alternative tests.[12] Students could also meet the TASP requirements if they did sufficiently well on the state's high school exit exam, or on the SAT or ACT. In our sample, about 10 percent of degree-seeking, entering freshman were identified as being exempt from the TASP requirements.

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[8] Three earlier studies on the effect of remediation also used a regression discontinuity design (Aiken et al. 1998; Lesik, 2006; Moss and Yeaton, 2006). However, while the regression discontinuity generally requires relatively large samples to allow for a narrow comparison at the treatment assignment threshold, all of these studies used very small samples, making it difficult to interpret their findings.

[9] Results based on a limited sample of students attending schools that appear to not allow retesting are generally, but not uniformly, suggestive that remediation has some positive benefits. However, the results are sensitive to the choice of bandwidth around the passing cutoff and the years included in the sample.

[10] As discussed in Section 5, a limitation of this approach is that it weakens the "first-stage" relationship between placement score performance and remediation, as students who initially fail the exam but retest and pass it will not be subject to the remediation requirements.

[11] Remediation in Florida is limited almost entirely to two-year colleges.

[12] We are able to locate TASP test score records for about two-thirds of the students in our sample. Although some students who did not take the TASP test took an alternative to the TASP, others may have dropped out before taking any test. On the other hand, some students who took the TASP test may have initially taken and failed one of the alternative tests.

The TASP test consists of three sections: math, reading and writing. Raw scores on each section were translated into a scale score that ranges from 100 to 300. The passing standard prior to September 1995 for all sections was a scale score of 220. Afterwards it was increased to 230 for math and reading. One important feature of the writing exam is that scores for this section take on a small number of unique values.[13] This means that students receiving the minimum passing score might be very different from students with the next lowest score, violating the assumptions needed for a valid regression discontinuity design. As explained in Section 4, we therefore focus on the math and reading exams. Failing the TASP test is fairly common. For instance in our sample, 21 percent of students initially enrolling at a four-year college failed at least one section of the TASP, while 40 percent of two-year college students failed it.

Students who failed any section of the exam were required to participate in remediation in each semester in which they were enrolled until all sections of the test were passed. Students failing multiple sections were required to enter remediation in only one of the failed subject areas (THECB, 1995). In practice, however, not all students who failed entered remediation. For example, consider a student who took the TASP test during her first semester in college, but after enrolling in college-level courses. If she failed the exam on the first try but retook it and passed before the start of the following semester, she would not have to enter remediation.[14] Alternatively, she would also appear in our data as not entering remediation if she dropped out of college following her first semester. By the same token, students who pass all sections of the TASP may nevertheless enroll in developmental education. This could occur because their advisors encourage them to do so, because they failed a local placement exam at their school, or if they initially took and failed an alternative to the TASP test.[15] As shown in Section 5, however, failing the TASP has a very strong effect on the likelihood of entering remediation.

3. Data and Descriptive Statistics

3.1 Dataset Construction

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[13] The essay portion of the writing section receives a score of 2 through 8. Passing status for students whose essays are neither a clear fail nor a clear pass is determined by their performance on 40 multiple choice questions. The scale scores in our data take on approximately 8 values in each test administration.

[14] The rules governing the timing of the TASP test changed over our study period. Starting in the 1993 fall semester students had to take the TASP test before completing the 9 academic credit hours while before that they could wait until completing 15 credit hours (approximately one academic term). In the fall of 1998, students were required to take the TASP test before enrolling in college for the first time. Thus, the type of scenario whereby a student could avoid remediation by retesting in the first semester became less common over time.

[15] Interviews with college remediation program officers suggest that retesting is encouraged primarily for students who barely failed the placement exam, with some schools even offering short review classes so that marginal students can retake the exam and pass it.

This paper uses data from the Texas Schools Microdata Panel (TSMP). The TSMP is a collection of administrative records from the state agencies that oversee K-12 public schools, public postsecondary institutions, and the state's Unemployment Insurance system. Linkages across data files can be made on the basis of an individual's encrypted social security number, which enables researchers to construct longitudinal data files.

Most of the data we use comes from Texas Higher Education Coordinating Board (THECB), which oversees postsecondary education in Texas.[16] Public postsecondary institutions in Texas are required to submit student-level reports to the THECB on an annual or semester basis. These reports contain detailed information on each student enrolled at a particular institution including participation in remediation, semester credit hours attempted, and receipt of degrees and certificates. Note that we do not observe completed credits, which will be lower than attempted credits when students receive a failing grade in a course (credits in courses that a student drops before receiving a grade are not included in the THECB's measure of attempted credits). The TSMP has all student-level data collected by the THECB from 1990 through 2005.

Information on TASP test scores comes from the database of exam results provided by the contractor the Coordinating Board used to administer the exam. This data file contains a record of every test attempt taken since the inception of the testing program in 1989. It includes the scale score received on each section of the test as well as the administration date, which enable us to determine the student's performance relative to the passing standard and identify the scores received on the initial attempt.

The primary variable of interest in this study is whether a student was in remediation. In Texas, students can be in remediation for different (and possibly multiple) subjects. In this paper we focus on whether a student is in remediation for any subject. However, we also present results where the "treatment" is defined as remediation for math or remediation for reading.[17] Because some students take the TASP exam after beginning their first semester, our definition of treatment status includes remediation in either the student's first semester or during the following semester.

[16] We are also able to link about 30% of our sample to records from Texas public high schools. We use the high school records to get information on a student's economic disadvantage status.

[17] Note that the way we define remediation in a particular subject does not depend on a student's status in any other subject area (i.e., math remediation does not depend on whether a student was also in remediation for reading or writing). As we explain below, we do not specifically examine remediation for writing because the scores for the writing placement exam take on only a handful of values, and therefore does not lend itself to a regression discontinuity analysis.

The THECB data files allow us to construct detailed measures describing a student's success in college. We examine four types of academic outcomes. The first is the number of academic credits a student attempts. We consider total credits attempted in six years. In addition, we also analyze the number of academic credits attempted during a student's first year in order to determine whether remediation crowds out degree-counting credits during the first year. Second, we look at performance in the first college-level course in mathematics.[18] Specifically, we consider whether a student ever attempted or passed a college-level math course, and for students who did attempt it, we will examine the grade received. Third, we examine whether a student initially enrolling in a two-year college "transfers up" to a four-year college and conversely, whether a student initially attending a four-year school "transfers down" to a two-year college. These measures are based on the type of school we last observe a student attending.[19] Finally, we examine measures of college attainment. One is the student's highest grade completed.[20] A second is whether a student receives an academic degree, defined for two-year college students as earning a Bachelor's or an Associate's degree and as earning a Bachelor's degree for four-year college students.

We also examine the labor market impacts of remediation, in particular, the effect on earnings. These analyses use administrative earnings records from the Texas Workforce Commission (TWC), which oversees the state's Unemployment Insurance (UI) system. Covered employers are required to submit reports to the TWC with information on total earnings paid out to employees in each quarter. Although not all employment in Texas is covered in the TWC data (for instance, self-employed workers may not be covered), independent estimates suggest the vast majority of workers are covered by the state's Unemployment Insurance system (Stevens, 2002; King and Schexnayder, 1999). The analyses we will conduct examine total earnings received in the 5th, 6th and 7th year after a student first enrolls in college, where annual earnings are converted to year 2000 dollars using the CPI-U.

Our dataset also includes a number of background and other baseline characteristics. These include basic demographic characteristics such as gender, race/ethnicity and date of birth. We also know whether students attending two-year colleges receive "in-district" tuition or

[18] Information on the performance in other college-level courses is not available in each year of the study.
[19] More precisely, a student is considered to have transferred up if they attempted three or more credits at a four-year college during the last semester we observe them in the data.
[20] For students who completed at least 30 academic credit hours in their final year, the highest grade is defined as the student's highest observed grade, which is included on the THECB student records. Otherwise, it is one minus the highest observed grade.

whether they pay out-of-district fees.[21] Finally, for about two-thirds of our sample, we have information available from high school records on economically disadvantaged status (mainly receipt of free or reduced lunch) as well as the distance of their high school from the college they attend.

In this paper we will examine students who first entered college between the 1991-92 and 1999-2000 school years, and did so as first-year students. We exclude earlier years because the data from this period do not appear to be complete. Later years are excluded to allow a sufficiently long follow-up period; for each student in our data, we are able to track their academic progress for 6 academic years.[22] Students were included in the sample if they: (1) were not exempt from the TASP and who took the placement exam, (2) have non-missing data for date of birth and ethnicity, (3) were pursuing academic degrees when first enrolled, and (4) took the placement exam by the end of their first semester.[23] The rationale for this last restriction is that it allows us to focus on remediation taken early on (within the first year) during a student's college career, when it is most likely to have an impact on academic outcomes.[24] Throughout this paper we refer to "four-year" and "two-year" students based on the type of school they initially attended even though transferring from one type of institution to the other is fairly common (and in fact is an outcome that we will examine).

The research design used here further requires that the sample be limited to students with valid placement exam scores. In order to examine the effect of remediation in at least one subject, the sample is limited to students with valid scores in all three subject areas. In this group, whether the minimum of the math, reading, and writing scores is high enough to pass determines whether a student is required to be in remediation. This minimum score can

[21] Texas community colleges are supported in part by local property taxes that are levied by community college taxing districts. More than one-third of students in public K-12 schools do not live in a college district and therefore face steeper tuition charges (approximately 70 percent higher) than do students living in a college district. See McFarlin (2007) for additional details concerning community college districts in Texas.

[22] Strictly speaking, we observe students who first enrolled in the spring or summer of 2000 (about 4 percent of the sample) for less than six full academic years.

[23] In our study period, there are about 1.18 million entering freshman who were registered as seeking an academic degree (possibly undecided in the case of two-year college students) and who were not listed as exempt from the TASP requirements. We were able to locate TASP test score records for about two-thirds of these students (796,509). Student might not have TASP records because they wound up not seeking an academic degree and therefore were not subject to TASP requirements, dropped out of college before taking the test, were actually exempt from the testing requirement (despite not being identified as such on the first enrollment record), or took an alternative placement exam to the TASP test. About three-quarters of the students with TASP test records took the TASP within their first semester in college.

[24] Another motivation for the restriction is that performance on the TASP is most strongly related to remedial education for these students; some students who initially took the TASP test in the second semester appear to have been assigned to remediation on the basis of local placement test results (or an alternative to the TASP test), although we cannot confirm that conjecture empirically.

therefore be used as the assignment variable in a regression discontinuity analysis. However, as noted earlier, scores on the writing section take on only a few values, making it inappropriate for use in a regression discontinuity analysis. Therefore, we further limit the sample to students who passed the writing section, and the assignment variable will be the minimum of the math and reading scores.[25] The final dataset – what we refer to as the "primary" sample – has 255,878 two-year college students and 197,502 four-year college students. In contrast, the analyses that specifically examine the effect of math or reading remediation only require that students have valid scores in the relevant subject area. These samples are therefore 14 to 25 percent larger (for two-year and four-year college students, respectively) than the primary sample. In the analyses of labor market earnings, we also exclude students who entered college after the fall of 1998. This was done because the TWC data end in the third quarter of 2004, and a six-year follow-up was only possible for students starting in the fall of 1998 (or earlier).[26]

3.2 Descriptive Statistics

Table 1 reports descriptive statistics by whether they were in remediation for at least one subject. There are three important features of the data that bear mention. First, remediation rates in Texas appear to be somewhat higher at four-year colleges than national statistics cited in Section 1. For instance, 25 percent of Texas students at public four-year colleges began their academic careers in remediation compared to only 20 percent nationally (NCES, 2003). However, in both Texas and throughout the nation, more students were in remediation for math than for either reading or writing.

Second, the baseline characteristics of students in remediation are markedly different from those of students who are not in remediation. For example, at two-year colleges, students in remediation are 14 percentage points less likely to be white than are non-remediated students. In addition, compared to non-remediated students, individuals in remediation tend to be older when first enrolling, are more likely to be economically disadvantaged, and have much lower test scores.

Finally, across all measures of college attainment and performance in the labor market, remediated students have worse outcomes. For instance, only 23 percent of remedial students

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[25] Seventy-five percent and 86 percent of two- and four-year college students, respectively, passed the writing section on the first try. We also exclude students with very low TASP scores (more than 100 scale score points away from passing – about .2% to .3% of the sample) so that values far away from the passing cutoff do not have undue influence on the estimates.

[26] In the analyses of labor market outcomes, "Year 1" begins in the quarter a student enters college, so for a student entering in the fall of 1998, "Year 6" consists of the 4th quarter of 2003 and the first three quarters of 2004. We also conduct analyses of labor market in the 7th year after entering college, where we restrict the sample to students entering in the fall of 1997 or earlier.

starting at two-year colleges earn an Associates or Bachelor's degree within six years, compared to 38 percent of non-remediated students. Likewise, among labor market participants, four-year college students in remediation earn nearly $5,800 less than non-remediated classmates, seven years after entering college.

4. Empirical Strategy and Estimation

Isolating the causal impact of remediation is difficult since students participate in developmental education precisely because their academic skills are weak. We therefore use a regression discontinuity approach based on the TASP score passing cutoffs. As we show below, the probability of being in remediation falls sharply exactly at the passing score. Provided that other determinants of student outcomes are not themselves discontinuous, variation in remediation status generated at the passing threshold can be used to identify the causal effect of participation in the program (Hahn, Todd and van der Klaauw, 2001).

In particular, we argue that, conditional on the TASP score, an indicator for passing the TASP is a valid instrumental variable (IV) for remediation status. To understand our strategy, it is helpful to consider the system of simultaneous equations that we will estimate:

(1) $Y = \theta R + f(S) + \varepsilon$

(2) $R = \pi P + g(S) + u$

where Y is the outcome of interest (e.g., graduation), R is an indicator for remediation status, S is the TASP test score – what will be the "assignment variable" in the regression discontinuity analysis, P is an indicator for passing the exam, and ε and u are mean zero random terms. The function f captures the relationship between Y and S away from the passing cutoff, and similarly the function g captures the relationship between R and S. The parameter θ in the "structural equation" represents the influence of remediation status on the outcome Y. For expositional purposes we treat the effect of remediation as constant, and we discuss the more realistic heterogeneous effects case below. Equation (2) depicts the "first stage" relationship. In this framework, P acts as a "shifter" that affects the probability of being in remediation. The parameter π is equal to the discontinuity in the remediation rate at the passing cutoff, and represents the magnitude of this shift.

Plugging Equation (2) into Equation (1) yields the "reduced form" relationship:

(3) $Y = \eta P + \theta g(S) + f(S) + \theta u + \varepsilon$

The parameter $\eta \equiv \theta\pi$ can be interpreted as the effect of passing the exam on Y. It is also equal to the magnitude in the discontinuity of the conditional expectation of Y. Moreover, the causal parameter of interest can be obtained by computing the ratio $\frac{\eta}{\pi}$. Thus, the parameter of interest can be thought of as the reduced form discontinuity in the conditional expectation of Y scaled up by the inverse of the discontinuity in the likelihood of remediation.

IV estimates of θ will be consistent if there is a discontinuity in the remediation rate at the passing threshold (i.e., $\pi \neq 0$) and passing status only affects Y through its effect on the likelihood of being in remediation. The "instrument validity" condition can be expressed formally as:

(4) $E(\varepsilon P \mid S) = 0$.

Intuitively, this condition amounts to passing status being unrelated to the other determinants of Y for students scoring just above or just below the passing threshold. This condition could be violated if students or schools could manipulate the test scores near the passing threshold in such a way that barely failers were systematically different from barely passers.[27] The nature of the testing administration makes this type of scenario unlikely. The exams are scored by the independent contractor hired by the THECB to administer the tests. Even if students had some ability to manipulate their precise score, this would be unlikely to generate non-random sorting around the passing score since students would not know the exact number of correct answers needed to pass when writing the test.[28] One of the more appealing features of our research design is that the implications of our underlying identification assumption can be tested by seeing if the distribution of test scores and the conditional expectations of other observable pre-determined characteristics behave "smoothly" at the passing cutoff (Lee, 2005a). We show evidence below that is consistent with these predictions.

The discussion thus far has assumed that the effect of remediation is constant across students, which is unlikely to be true. With heterogeneous returns to remediation, IV will consistently estimate the "Local Average Treatment Effect" (LATE; Imbens and Angrist,

[27] The condition in Equation (4) could also be violated if failing the TASP test affected students in ways other than by affecting the likelihood of remediation. For instance, students who fail might decide to delay college for a semester while they prepare to retake the test. We checked to see if the likelihood of entering college more than one semester after initially taking the TASP was discontinuous at the passing cutoff and found no evidence that was the case.

[28] As noted by Calcagno (2007), retesting could lead to non-random sorting around the passing cutoff even if students cannot manipulate their precise score on a particular test occasion. When the most recent test score is used as the "assignment variable" the set of barely passers would consist of students who either initially passed or passed on a retest and might not be comparable to barely failers who would consist of students who decided not to retest or could not pass even on a retry. Therefore we focus on the student's initial test score. This strategy avoids the retesting bias, but it diminishes the power of the "first stage" since some students who initially fail might retest and pass the exam before entering remediation.

1994).[29] In the current context, the LATE will pertain to "marginal" students who score close the passing cutoff and whose participation in remediation was manipulated by whether or not they passed the placement exam. If the effect of remediation varies with ability, then our results may not be applicable to students who score well below the passing cutoff. However, the marginal group is policy-relevant for at least three reasons. First, a large fraction of remediated students were close to the passing cutoff on their initial test attempt – 24 percent of students at two-year colleges and 30 percent of students at four-year colleges who are in remediation score within 10 scale score points of the passing cutoff. Second, policymakers clearly intend for remediation to help students who fall just below the passing level they set. In contrast, students who score well above the cutoff do not need remediation and students far below it arguably have little chance of completing college even with remediation. Finally, the results for this group inform the debate about whether remediation placement cutoffs should be raised or lowered.

Our results might also not be informative about the effect of remediation for students who fail the placement exam but avoid remediation by passing on a retest attempt, or who pass the exam but nonetheless enter remediation. Nonetheless, the LATE we estimate will be informative about the students who are directly affected by the mandatory remediation requirements of the TASP. Our results are therefore clearly relevant to policymakers.[30]

The estimation problem in this context involves obtaining estimates of the discontinuities in the conditional expectations of Y and R at the passing cutoff. Effectively this amounts to estimating the functions f and g on either side of the passing cutoff. Following a number of recent examples in the applied RD literature, we model these functions as low-order polynomials and use data away from the cutoff to estimate the parameters of the polynomial.[31] Specifically, we use a third-order polynomial specification with interactions between the polynomial terms and an indicator for passing the exam so that separate cubic functions are estimated on either side of the passing cutoff. To gauge the quality of the polynomial approximation, we will show the regression fit superimposed onto the graph of "local averages"

[29] With heterogeneous effects, the additional assumption that the instrument affects the probability in one direction for all students is needed for the IV estimates to consistently estimate the LATE (Imbens and Angrist, 1994). The "monotonicity" assumption in this context rules out the existence of students who would be in remediation if they passed the placement exam but who would not if they failed it, which we view as reasonable.

[30] Section 6 discusses how the LATE we estimate potentially differs from the overall average effect.

[31] This approach has been used by DiNardo and Lee (2004), Jacob and Lefgren (2004) and Matsudaira (2005). An alternative approach uses local polynomial regression methods (Porter, 2003). This approach has the advantage of not requiring parametric assumptions about the underlying conditional expectations, but it does require the researcher to choose the bandwidth. McCrary and Royer (2006) discuss the relative advantages of the global versus local polynomial regression in the context of implementing an IV strategy based on a RD design. See also Imbens and Lemieux (2007) for a review of applied regression discontinuity methods.

plotted as a function of *Y*; if the parametric fit performs well, it should closely track the local averages. Nonetheless, since any misspecification of the functional form will introduce a common variance component for all observations in a test score cell, standard errors for both samples are adjusted for "clustering" at the test score level (Lee and Card, 2006). In addition, to assess the sensitivity of our findings to misspecification, we also estimated the models when restricting the sample to a "narrow band" around the passing cutoff (equal to 10 scale-score points above and below the cutoff).[32] These results are reported in Appendix Tables A1-A5.

Although the discussion has been in terms of a single test score, the TASP consists of three subject-specific scores. As explained in Section 3, our primary analyses use the minimum of the math and reading score as the assignment variable. Among students who passed the writing section, the minimum of the math and reading scores determines whether a student will be required to be in remediation in at least one subject area. To analyze the effect of math remediation, *S* will simply be the math scale score, and for reading remediation *S* will be the reading scale score.

5. Results

5.1 Tests of the Validity of the Research Design

As noted in the preceding section, the assumptions required for the validity of our research design have the testable implications that (1) the test score distribution and (2) the conditional expectations of pre-determined covariates should trend smoothly through the passing cutoff. To test the first of these, Figures 1a and 1b plot the number of observations in a test score cell as a function of the test score. Note that the scale scores are re-centered to be equal to zero at the passing cutoff. We also superimpose the fitted values obtained by running a cell-level regression of the cell size on a cubic polynomial in the test score, where each observation is weighted by the cell size.[33] The upper-right of each panel contains the estimated discontinuity along with its standard error. The estimated discontinuity in the cell size is

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[32] Because there is much less data to estimate a flexible relationship between the TASP score and a given outcome in the narrow band sample, we use a more parsimonious specification in these analyses (specifically, the parametric specification is linear in the test score with an interaction between the passing indicator variable and the linear test score term). Narrow band robustness tests within the regression-discontinuity framework have also been used by Angrist and Lavy (1999) to evaluate how class size affects student achievement.

[33] The analyses examining the smoothness of the test score distribution exclude scale scores more than 80 points below the passing threshold while the rest of the results in this paper only exclude observations more than 100 scale score points below passing. The additional restriction is made here because the parametric fit of the average cell size tracks the data poorly for extremely low scores. The estimated discontinuities in the mean cell size yield qualitatively similar conclusions when the full range of test score values is used.

statistically insignificant, indicating that the test score distribution is continuous at the passing cutoff.[34,35]

To test the second implication, Table 2 reports discontinuity estimates for baseline characteristics when using the primary sample. For all of the baseline covariates, the estimates for both two-year and four-year college students are all small and statistically insignificant. These results indicate that observable pre-determined characteristics are similar for students whose lowest section score is just above or just below the passing threshold, and provides support for the validity of the research design.

5.2 Effect of Performance on the TASP Test on the Probability of Remediation

Figures 2a and 2b plot the likelihood of being in remediation in at least one subject as a function of the minimum of the math and reading TASP test score. The open circles represent the remediation rate for students in a particular test score cell. In both two- and four-year colleges, there is a sharp fall in the remediation rate at the passing cutoff. These results provide strong evidence that failing at least one section of the TASP has a strong causal effect on the likelihood of being in remediation. The point estimate for the four-year colleges is somewhat larger than it is for two-year colleges (-0.421 compared to -0.360), but both are precisely estimated. Thus, the IV estimates discussed below are not subject to the statistical problems associated with "weak instruments" (Bound et al., 1995). An important consideration when evaluating the point estimates is the degree to which the polynomial in the test score (the function f in Equation 3) accurately approximates the underlying conditional expectation, $E(R|S)$. The visual evidence in Figures 2a and 2b indicates that the polynomial fit closely "tracks" the cell means, suggesting that misspecification of f is not producing misleading estimates. Results from models that include baseline covariates are included in the top row of Table 3. Consistent with a valid regression discontinuity design, controlling for pre-determined characteristics has little effect on the point estimates.[36]

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[34] The way the NES assigned scale scores resulted in very few students receiving a scale score one point below the passing cutoff (S=-1) and a relatively large number of students receiving a scale score equal to 2 points below the cutoff (S=-2). Therefore in Figures 1a and 1b, we collapsed the data in the S=-1 and S=-2 cells and replaced it with the average of the two cell sizes. The first row of Table 2 shows the estimated discontinuity when the "unadjusted" cell size is used. The statistically significant negative discontinuity is entirely driven by the large number of observations in the S=-2 cell. When examining the unadjusted cell size and weighting all observations equally, the discontinuity is small and statistically insignificant.

[35] This test is similar in spirit to the non-parametric test for a discontinuous density proposed by McCrary (2007).

[36] Additional covariates include the maximum of the math and reading scores and dummy variables for white, Hispanic, starting in fall semester, being 21 or older when enrolling in college, academic year of enrollment, academic year student initially took the TASP, economically disadvantaged, missing data on economically disadvantaged, receiving in-district tuition, missing data for in-district tuition status, distance from HS<25

Although these results show clear evidence that performance on the TASP affects the probability of entering remediation, it is important to note that there are many students who fail a section of the TASP but do not go into remediation ("never-takers" in the parlance of Imbens and Angrist, 1994) and others who enter remediation even though they passed all sections ("always-takers"). There may be always-takers because some students who pass the TASP might fail a local placement exam (which we do not observe in our data), attend a school that uses a higher cutoff score than the statewide passing threshold, or be advised by their college counselor to voluntarily enter remediation. There are also reasons why one would expect sizable numbers of never-takers to exist as well. Recall that Figures 2a and 2b use the student's initial exam score. As noted in Section 2, students who initially fail the test can retake and pass it, thereby avoiding the mandatory remediation requirement.[37] Another explanation lies in the fact that some students who fail the test might drop out of school before entering remediation (this type of student would be treated as non-remediated).[38]

5.3 Estimates of the Impact of Remediation on Academic Outcomes

We now turn to the results for academic outcomes, starting with academic credits. One of the criticisms leveled against remediation is that it reduces the time spent on earning credits that count towards a degree. To test this claim, we examined whether remediation is associated with fewer academic credits attempted during the student's first year, the time when "crowding out" would most likely occur. Figures 3a and 3b show that attempted credits during the first year are slightly higher for students who passed the TASP.[39] However, the magnitude of the crowding out is small. The IV estimates reported in Table 3 show that being in remediation reduces first-year academic credits by about 2.4 for two-year college students and 1.5 for four-year college students. As a point of reference, one full year of college is 30 credits.[40] To see whether this effect persists, we examined the total number of credits attempted over six years.

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miles, distance from HS>50 miles, distance from HS missing, and whether the TASP was taken at least 2 semesters before entering college.

[37] Note also that the downward gradient seen in Figure 3 to the left of the passing cutoff is consistent with retesting since students with higher initial scores are likely to have an easier time passing on a retry.

[38] The decision to drop out is potentially affected by failing the TASP test. If it is, then failing the TASP might affect student outcomes directly (i.e., not through its effect on the likelihood of being in remediation), thereby threatening the validity of our IV strategy. However, in results not reported, we find no evidence that failing the TASP increases the likelihood that a student leaves school at the end of their first semester. Moreover, even if such selection took place, our reduced form results would still be interpretable as estimates of the policy-relevant parameter of the impact of failing the TASP test on student outcomes.

[39] The sample in these analyses is limited to students who entered college in the fall because some schools only report credits attempted on an annual rather than on a semester basis. This makes it difficult to determine the number of credits attempted during a student's first year for those entering in the middle of an academic year.

[40] Students need to accumulate 60 credit hours to earn a two-year degree and 120 credit hours to earn a four-year degree. One course in college is 3 to 4 credits.

Figures 4a and 4b show some evidence that barely-passers attempt more credits, but the estimated discontinuity is only statistically significant for two-year college students. The IV estimates in Table 3 suggest that students at two-year colleges attempt 6 fewer academic credits when placed in remediation, with the effect for four-year college students about half as large.[41] However, these effects are small in magnitude; they correspond to only about one-fifth of a year in college.

Since remediation is designed to provide the skills needed to do well in college coursework, we next examined outcomes pertaining to the first college-level (i.e., non-remedial) math course. Remediation could affect a student's willingness to take such a course and also how well a student performs in one. To test the first possibility, the left-hand panels of Figure 5 plot the fraction of students attempting a college-level math course as a function of the TASP score.[42] The results provide no indication that passing or failing has any effect on the likelihood of taking a college math course. To determine whether performance is affected, we limited the sample to students who attempted the course for a grade. The right-hand panels of Figure 5 plot the average grade received in the first math course as a function of the math TASP score. Grades are scored on a four-point scale, with the best grade (an "A") equal to four and the lowest grade (an "F") equal to zero. The results indicate that barely-passers receive a lower grade, although this effect is not apparent in the four-year college sample. The IV estimate from the model with no covariates indicates that remediation improves the average grade received for two-year college students by about 0.24.

While this would be a sizable effect, it should be treated with some caution because it is generated from a sample restricted to students who took a college math course for a grade. This is because barely-failers might not provide a suitable comparison group for barely-passers among the students who take a college math course. To see this point, imagine the following scenario with two equally-sized groups of students who are induced to enter remediation because they failed the placement exam. The first benefits from the remedial education and are thus encouraged to take a more advanced math course. For the second group, remediation has no academic benefits and they wind up not taking a college math course because of the time devoted to remediation. Thus there may be no net effect on the probability of taking a college course (a pattern we observe in the data), but failers who decide to take the course may be very

[41] When limiting the sample to a narrow band around the passing cutoff, the estimates are actually larger for four-year college students (about a 7 credit reduction associated with remediation) than for two-year college students (a reduction of about 3 to 4 credits).

[42] We define course-taking as taking the class for a letter grade as opposed to "pass/fail". A little less than 3 percent of the students in our sample take their first college math course pass/fail.

different from those who do not.[43] Indirect support for a selection bias story can be seen by noting that adjusting for baseline covariates reduces the magnitude of the IV estimate by about one-third, which suggests that observable covariates (in the conditional sample) are not "balanced" on either side of the passing threshold. Moreover, as reported in Table 3 we find no evidence that remediation improves the likelihood of passing a college-level math course (defined as receiving a grade of "D" or better, or a "Pass" if taken pass/fail). Since these are not prone to the same kind of selection biases (they use the full sample irrespective of whether a student took a college math course), they cast further doubt on the possibility that remediation improves performance in college math courses.[44]

The next outcomes we consider are whether a two-year college student transfers up to a four-year institution. This outcome is important for evaluating the efficacy of remediation since one of the primary goals of two-year colleges is to prepare students for university-level study. Given the large numbers of students at two-year colleges in need of remediation, finding a positive effect would indicate that remediation helps community colleges fulfill one of their central missions. However, the results in Figure 6a show no evidence that transferring up is more common among students who barely fail the TASP test. The IV estimates in Table 3 actually suggest remediation makes transferring up *less* likely, although these estimates are not statistically significant. Nonetheless, we can statistically rule out positive effects larger than about 2 percentage points (or 1 percentage point when adjusting for baseline covariates that increase the precision of the estimates). Similarly, the IV estimates of the effect of remediation on the likelihood that four-year college students transfer down to a two-year college are small and statistically insignificant in the full sample. Although the estimates using the narrow band sample are larger in magnitude (suggesting that remediation increases transferring down by 4 percentage points), the visual evidence in Figure 6b provides little indication of a discontinuity in the transferring down rate at the passing cutoff.

The final set of academic outcomes describes a student's college attainment. Figure 7a plots the fraction of two-year college students that complete at least t years in college by the TASP test score and suggests that students who barely pass the exam are slightly more likely to complete at least one year than students who barely fail. The IV estimates in Table 3 indicate that remediation lowers the probability of completing at least one year in college by 6 percentage

[43] This scenario is similar to a failure of the "monotonicity" condition which is often assumed in studies with treatment non-compliance (Imbens and Angrist, 1994; Lee, 2005b).

[44] Another reason to treat the results for college math course grades cautiously is that the data do not identify what course is the student's first college-level course. If remediation causes students to take easier college-level math courses, then the grades of remediated and non-remediated students need not be comparable.

points. However, the estimates based on the narrow band sample in Table A2 are smaller and statistically significant only when controlling for baseline covariates. The estimated effects on the likelihood of completing two or more years are also fairly small and statistically insignificant. Turning to the results for four-year colleges, Figure 7b and the IV estimates in Table 3 provide little evidence that passing the TASP or placement in remediation affect the number of years completed for students in four-year colleges (although the estimate for completing at least one year is negative and marginally significant when adjusting for baseline covariates).[45]

Figures 8a and 8b show the relationship between the likelihood of earning a college degree within six years of entering college and the TASP score. There is no evidence that the graduation rate changes sharply at the passing cutoff and the estimated discontinuities are small and statistically insignificant. These results imply that remediation has little effect on eventual degree attainment. However, it may be that remediation increases the time needed to complete a degree, which would cost students time not spent earning income in the labor market. We test this possibility by examining whether remediation affects the probability of graduation within 4, 5, or 6 years of entering college. Because we find no effect of remediation on the likelihood of graduating within 6 years, if remediation delayed time to graduation, then there ought to be a negative effect on graduation within 4 (or 5) years. However, we find no evidence of such a pattern. Thus, our results do not support the hypothesis that remediation increases the time needed to finish a degree, but we also find no indication that remediation helps under-prepared students graduate from college.

5.4 Estimates of the Impact of Remediation on Labor Market Outcomes

We now discuss the effect of remediation on labor market earnings. The impact on labor market outcomes is informative about whether remediation helps students develop economically valuable skills, and should therefore be considered a key measure of the program's effectiveness. Although we found little evidence that students' academic outcomes improve if placed in remediation, it is still interesting to see if there is an effect on labor market success. Remediation may improve a student's ability to learn in college courses even if it does not affect the number of years completed or the likelihood of earning a degree. In fact, the results for the grade earned in the first college-level math course suggest this may be the case (although as explained above

[45] In contrast, the results for the narrow-band sample in Table A2 suggest that remediation may lower the number of years completed for students in four-year colleges. For instance, the estimates suggest that remediation reduces the likelihood of completing at least 2 years by 5 to 6 percentage points. The inconsistency between the estimates based on the full sample and those based on the narrow band around the passing cutoff suggest that these estimates are somewhat sensitive to the parametric specification of the test score control function.

they are potentially subject to selection biases). The findings for labor market outcomes are informative about whether any such learning has returns in the labor market.

Although understanding the effect of remediation on earnings is important, this analysis is complicated by two related issues. One is that individuals who are employed and have positive earnings will not appear in the earnings data if they moved from Texas or work in a job not covered by the state's UI system. Another issue is that earnings received while still attending college are likely to be a poor indication of long-run earnings potential. We address these problems in three ways. First, we follow students for up to 7 years after entering college, by which time most students have left college (in the 7th year, only about one-quarter of students are still enrolled). Because the earnings data end in third quarter of 2004, to analyze earnings 7 years out, we restrict the sample to students first enrolling in fall 1997 or earlier.[46] Second, to assess whether "false zeros" drive the results, we compare estimates where we impute zero earnings for individuals who do not appear in the earnings data to estimates where the sample is conditioned on having positive earnings. Finally, we also conduct analyses where the sample is limited to students who have positive earnings and who are no longer enrolled in college.

One important drawback of making these types of sample restrictions is that they could introduce selection bias. For instance, observing positive earnings might be affected by remediation if remediation makes it easier (or harder) to find a job. In that case, conditioning the sample on positive earnings could introduce sharp differences between barely failers and passers, thereby violating the assumptions underlying the research design. Similar biases might occur if the sample is restricted to individuals who are no longer enrolled in college. However, we argue against the substantive importance of such biases on two grounds. First, estimates of the discontinuity in the selection probability reported in Table 4 are small and statistically insignificant, indicating there is little differential selection into the earnings samples.[47,48] Second, as we discuss momentarily, the results are qualitatively similar with or without the sample selection restrictions.

[46] This restriction is not made for the analyses of labor market outcomes in the 5th and 6th years after enrolling in college.
[47] In the "narrow band" sample, the estimated discontinuity in the probability of having positive earnings in year 6 (for two-year college students) is 0.015 with a standard error of 0.005. However, the IV estimates of the effect of remediation on earnings when using all students and imputing zeroes for missing earnings (as well as the reduced form estimates) are qualitatively similar to the estimates based on the sample conditioned on positive earnings.
[48] As noted in the discussion of the results for the grade received in the first college-level math course, the absence of differential selection into the earnings sample guarantees the consistency of the IV estimates if passing status has a monotonic effect on selection into the earnings sample.

Figures 9a and 9b plot mean earnings received in the 7th year after first enrolling in college as a function of the TASP test score for the sample that has positive earnings and is no longer enrolled in college. Average earnings clearly increase with the test score, suggesting that the test measures skills that are valued in the labor market (or at least are strongly correlated with such skills). However, there is no discontinuous change at the passing cutoff and the IV estimates in Table 4 are all statistically insignificant. This is true for earnings received in the 5th, 6th, or 7th year after entering college. Also notable is that the magnitudes of the IV estimates tend to be fairly small and are neither overwhelmingly positive nor negative (in the models with no covariates, 11 out of 18 are negative). Because the qualitative conclusions are similar across these specifications, we conclude that missing earnings data and the possible distortions arising from observing earnings while individuals are still going to school do not cast doubt on the finding that remediation does not improve labor market outcomes.

5.5 Estimates for Subgroups

Although the findings presented thus far offer little indication that students benefit from remediation, the aggregate estimates may mask benefits for certain groups of students. To test this possibility, we examined the effect of remediation on three key academic outcomes – completing at least one year in college, the total number of credits accumulated, and receipt of a degree – separately by subgroup.

The subgroups were chosen to reflect dimensions along which the effect of remediation could vary. The first is the year a student entered college. Most students entering in 1995 or earlier were subject to the (lower) placement exam passing cutoff, which means that the LATE our instrument identifies is potentially different in the earlier and later periods of the study. The second dimension relates to the costliness of attending college. Cost is an interesting dimension because it changes the type of student attending a school.[49] Since direct and indirect costs might be different depending on the distance of the college from the student's home, we obtained estimates for students who attended a college that was more than 50 linear miles apart from the high school where they graduated, and also for students whose college was within 25 miles of their high school.[50] For two-year college students, we obtained estimates by whether they

[49] To the extent that students drop out of college after they enroll and discover their preferences or aptitude are lacking, lower costs could lead to a reduction in the ability of the marginal college student (Manski, 1989). Alternatively, if lower costs induce credit constrained students to enroll in college, the ability of the marginal student could increase (Dynarski, in press).

[50] These analyses only used students for whom we could identify the high school were they graduated (about 67 percent of the overall sample). The TSMP includes information on high school graduates who graduated from a Texas public high school between 1991 and 2002. McFarlin (2007) describes how the distances are calculated.

received "in-district" tuition. The final dimension is student demographic characteristics. We estimate results separately for non-whites, economically disadvantaged students, and individuals who were 21 or older when they first entered college.

These results are reported in Table 5.[51] Across all subgroups, we find no evidence of any positive effects of remediation and the estimates generally do not appear to differ substantially by subgroup. One interesting result is that the small negative effects found for two-year college students on the likelihood of completing at least one year and on total credits attempted are larger for students attending a school close to home. Two-year college students attending a college that is less than 25 miles from their high school are 8 percentage points less likely to complete one year of college and attempt almost 9 fewer credits if they are in remediation. In contrast, there is essentially no effect on both of these outcomes for students attending college more than 50 miles from where they went to high school. However, in the narrow-band sample (Table A4), the negative estimates for the less than 25 miles subsample are not statistically significant.[52]

5.6 Estimates of the Effect of Remediation in a Particular Subject

We now investigate whether remediation in either math or reading has an independent effect, regardless of whether one is in remediation in some other subject area. Table 6 reports the estimates for selected academic outcomes that use the math or reading score as the assignment variable in the regression discontinuity analysis.[53] Since the way we define the math (or reading) remediation treatment is not contingent upon remediation status in a different subject, it is possible that the marginal effect of assignment to math remediation on time available to take degree-counting courses could be smaller than the marginal effect of the "any subject remediation" treatment. The results in Table 6 are consistent with this conjecture. Math remediation has a smaller negative effect on attempted academic credit hours and on the likelihood of completing at least one year in college than what was seen in Table 3. The IV point estimates are statistically significant only when controlling for baseline covariates, and are never statistically significant in the narrow-band sample (Table A5). Similarly, the estimated effects of reading remediation on these outcomes are small and statistically insignificant in all cases.

[51] To be concise, Table 5 reports only estimates from models that do not adjust for other covariates aside from the test score and whether a student passed the TASP test. Estimates from models that include other covariates produce similar results.
[52] The narrow-band estimates also show negative and statistically significant effects on graduation for two-year college students who enrolled in 1995 or earlier, or who are black or Hispanic. However, the estimates using the full sample are smaller in magnitude and not statistically significant.
[53] Results for the effect of math or reading remediation on all outcomes are reported in a supplemental Appendix to this paper that will be posted online.

However, it also makes sense that remediation in math would have a particularly strong effect on performance in college math courses. In fact, the estimated effect of math remediation on the grade received in the first college-level math course (without adjusting for covariates) reported in Table 6 is 0.325 for two-year colleges and 0.217 for four-year colleges, both of which are somewhat larger than the estimates in Table 3 (0.235 and 0.122 for 2- and four-year colleges, respectively). Although these effects are fairly large, as noted earlier, they are based on a conditional sample of course-takers and therefore may be prone to selection biases. When adjusting for baseline covariates, the estimates fall by 40 percent and by 75 percent for two-year and four-year colleges, respectively. The results for attempting or passing a college-level math course tend to be small in magnitude and not statistically significant, although the estimate for two-year college students in Table A5 that adjusts for covariates indicates that math remediation increases the likelihood of passing a college-level math course by 5 percentage points.

The estimates of the effect of reading remediation on the grade received are small and not statistically significant The estimates that are adjusted for baseline covariates in Table 6 indicate that reading remediation might lower the likelihood of passing a college-level math course by about 7 percentage points. However, the point estimate in the narrow band sample is much smaller and not statistically significant, suggesting that this result is not robust to changes in the specification of the test score control function. Similarly some of the estimates using the narrow band sample indicate that reading remediation lowers the likelihood of taking a college-level math course or the grade received in one. However, these estimates are sensitive to the inclusion of baseline covariates, and in the full sample they are smaller in magnitude and not statistically significant.

6. Discussion

Aside from mixed evidence on the effect of remediation on performance in college-level math courses, the evidence presented in this paper does not indicate that remediation improves the outcomes of Texas students who score close to the placement exam passing cutoff and whose participation in remediation is affected by passing or failing the placement exam. This finding holds for students attending two- or four-year colleges, and across a range of student subgroups.

These results have several important implications. First, it suggests that the marginal Texas student does not benefit from mandatory remediation despite the substantial financial cost of the program. At a minimum, our results suggest that remediation is not an effective policy for students scoring close to the passing cutoff who enter remediation because they failed the TASP test. Therefore, an appropriate policy might be to lower the passing standard (which would reduce the number of students in remediation).[54] Second, our results are consistent with the view that "second-chance", later-life interventions are ineffective for improving human capital. In particular, we find no indication that remediated students have better labor market outcomes than comparable non-remediated students. Third, the absence of an effect of remediation suggests that peer ability, which is much lower in remedial courses, is not an important determinant of success in college. This may be because classroom peers are less important than peers in other college settings.[55] It might also be because there are other positive educational benefits of remediation that offset the negative effects associated with lower peer quality.

Our findings are also notable since they are in contrast with recent studies by Bettinger and Long (2007; henceforth BL) and Jepsen (2006), both of which find positive effects of remediation. There are at least three reasons why our results differ from those of other studies. Most obviously, the effect of remediation could vary across states. It may be that the remediation "treatment" in Texas is less effective than it is in California and Ohio. Moreover, there are important institutional differences that might matter. For instance, Ohio grants considerably more latitude to individual institutions to set their own remediation policies, while during our study period Texas had a number of statewide rules that applied to all schools.

Second, we look at different types of students. BL's empirical strategy requires limiting the sample to students who took the ACT or SAT. Since non-selective schools often do not use ACT and SAT scores in admissions decisions, we probably look at students of lower average ability. Jepsen's sample is composed of students who were referred to remediation. In contrast, our sample consists of students who took the statewide placement exam and who were seeking an academic degree when starting college.

Third, since our empirical strategies differ, we may not be estimating the average impact for the same type of students.[56] BL use an IV strategy that captures the effect for students whose remediation status is affected by attending a school with a more (or less) lenient remediation placement policy. Jepsen attempts to estimate the effect of "treatment on the treated", by comparing students who went into remediation to those who were referred to remediation but did not wind up enrolling in the remedial courses. In contrast, our approach identifies the effect

[54] Alternatively, the current passing standard may be set too low if it targets students who are so under-prepared that they receive little benefit from the program.
[55] For instance, strong peer effects have been found when examining random college roommate assignments (Sacerdote, 2001) and assignments to squadrons at the US Air Force Academy (Carrell et al., 2007).
[56] Calcagno's (2007) study, which uses a similar research design to the one we use, also finds little evidence of large benefits of remediation

of remediation for students whose remediation status is manipulated by passing or failing the placement exam and who score close to the passing cutoff. Although the effect for this group clearly could differ from the average effect other studies attempt to estimate, it should be noted that BL's research design also focuses on "marginal" students; their estimates pertain to the students who do well enough on the ACT to likely avoid remediation at some schools even though they would be in remediation in schools with more stringent remedial placement policies.

Although our findings provide a pessimistic assessment of the promise of college remediation, it is very important to recognize that the effect of remediation may be different among students that our research design cannot address. For instance, students with very low placement exam scores might be the ones who benefit most from remediation. Similarly, the students who enter remediation even though they passed the placement exam might do so precisely because they have very high returns to participating in the program. On the other hand, it is also possible that the benefits of remediation are even smaller among students that our empirical approach misses. For example, students who fail the placement exam but who anticipate getting very little out of remediation might be the ones most likely to attempt to avoid remediation by retesting and passing the exam.

7. Conclusion

This study assesses the impact college remediation has on academic and labor market outcomes for a large sample of Texas students. About one-third of college students across the U.S. take at least some remedial courses, and therefore remediation is an important example of a "second-chance" intervention intended to improve the human capital of young adults. To overcome the non-random nature of selection into remediation, we use a regression-discontinuity research design that exploits sharp test score cutoffs in placement exams used for assigning students into remediation.

With the exception of weak evidence that students in remediation have better grades in first college-level math courses, our findings lend little support to the view that remediation improves student outcomes. In fact, some of our results are consistent with a small negative effect on the number of academic credits attempted and the likelihood of completing at least one year of college. Importantly, we find no effect on the probability of earning a college degree or on labor market earnings for students initially attending a two- or a four-year college, suggesting that remediation does little to improve students' marketable human capital.

Interpreting our results, it is important to bear in mind that our findings pertain to students in a particular state and subject to the particular institutional features of the Texas remediation policies. It is possible that remediation programs in other states, perhaps because they are better designed or because they are targeted more effectively, have stronger positive effects. An important area for future research is to determine whether better-designed and possibly more intensive remedial interventions can be effective.

Moreover, the nature of our research design implies that our estimates capture the average impact for a particular subset of Texas students – namely, those on the margin whose participation in remediation is affected by passing or failing the placement exam. We maintain that this is a crucial policy parameter since it is informative about the students most directly affected by mandatory remediation policies such as the one in Texas. Furthermore, the effect on these students is important for evaluating policies that seek to change the cutoff used to assign students to remediation. However, it is important to recognize that there are other policy-relevant parameters that our approach does not address. An important example is that our design might not be informative about whether remediation is beneficial for students who would seek out such a program regardless of their placement exam performance. Another direction for future research is to determine whether remediation benefits these students.

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Figure 1a: Cell Size by TASP Scale Score, 2-Year Colleges

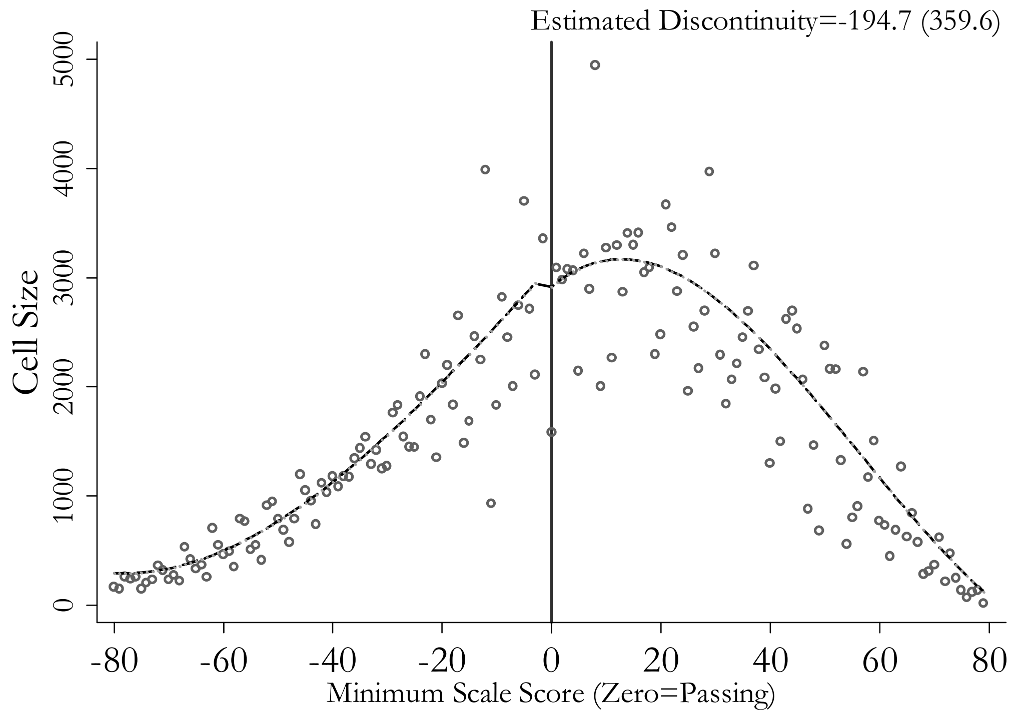
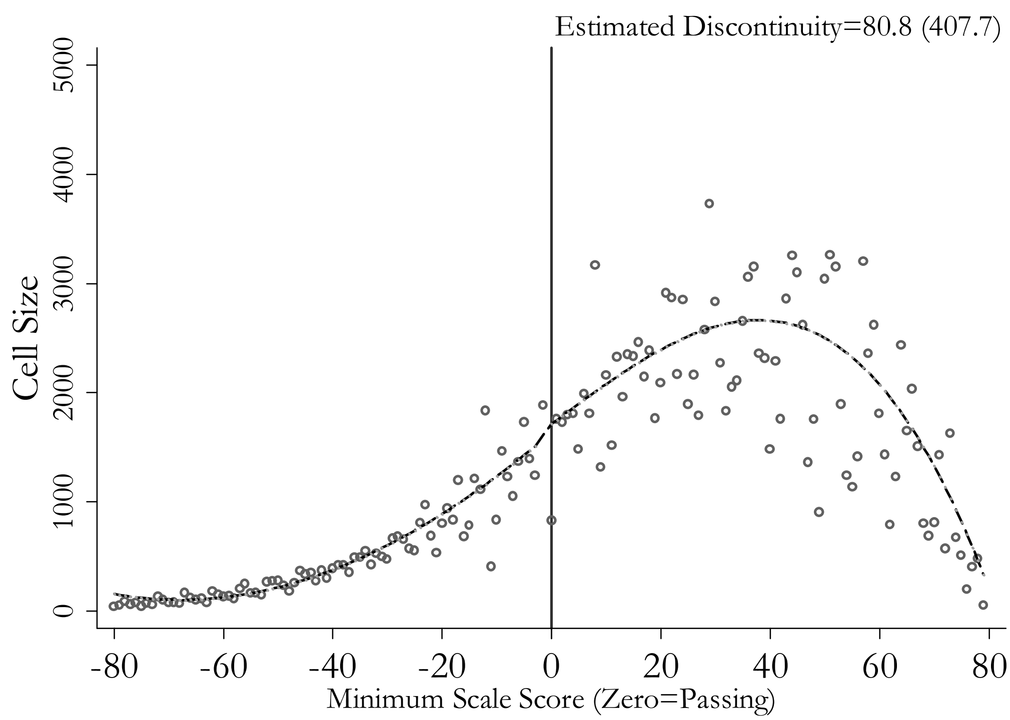


Figure 1b: Cell Size by TASP Scale Score, 4-Year Colleges



Note: Cell size of cells S=-2 and S=-1 replaced with the average number of observations in these two cells, and is assigned a value of S=-1.5

Figure 2a: Probability of Remediation in Any Subject by TASP Scale Score, 2-Year Colleges

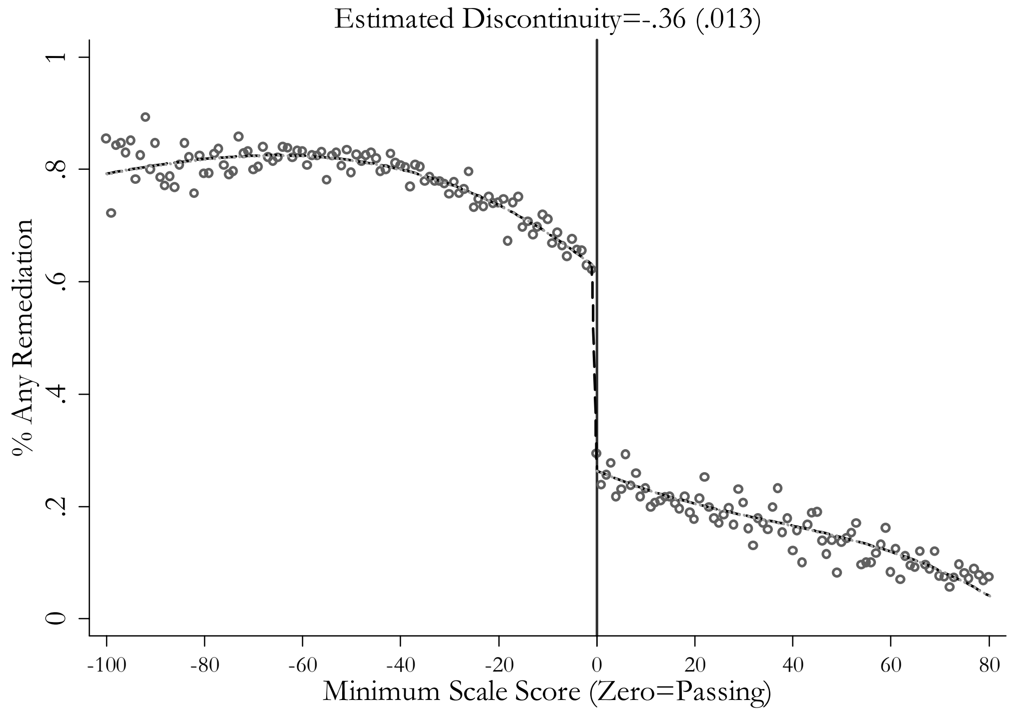


Figure 2b: Probability of Remediation in Any Subject by TASP Scale Score, 4-Year Colleges

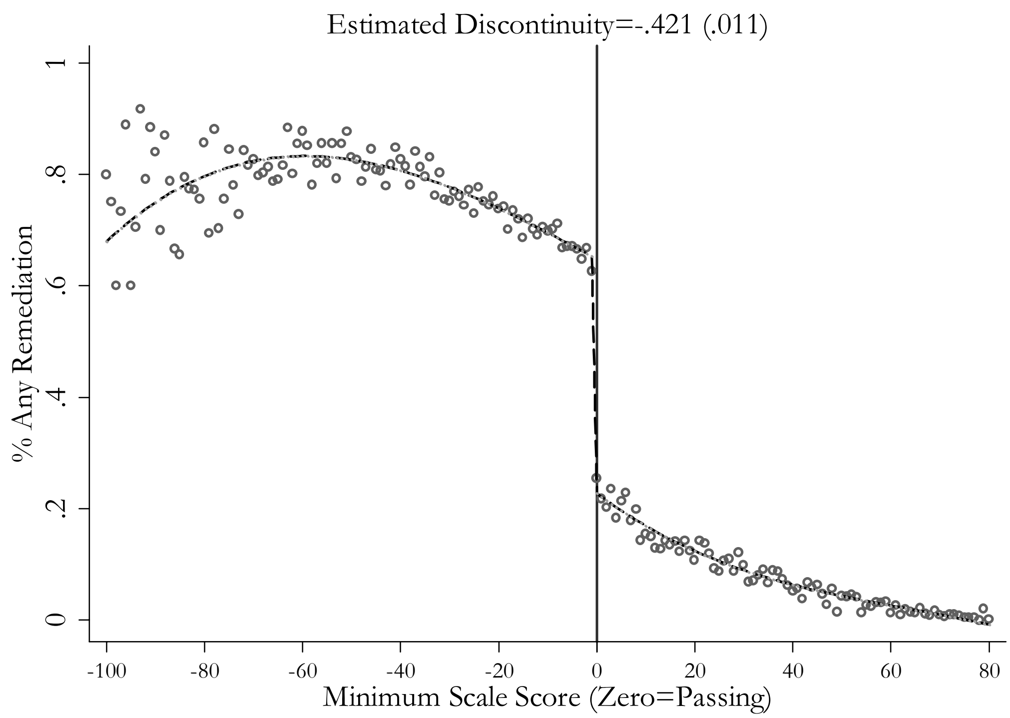


Figure 3a: Academic Credit Hours Attempted in First Year by TASP Scale Score, 2-Year Colleges

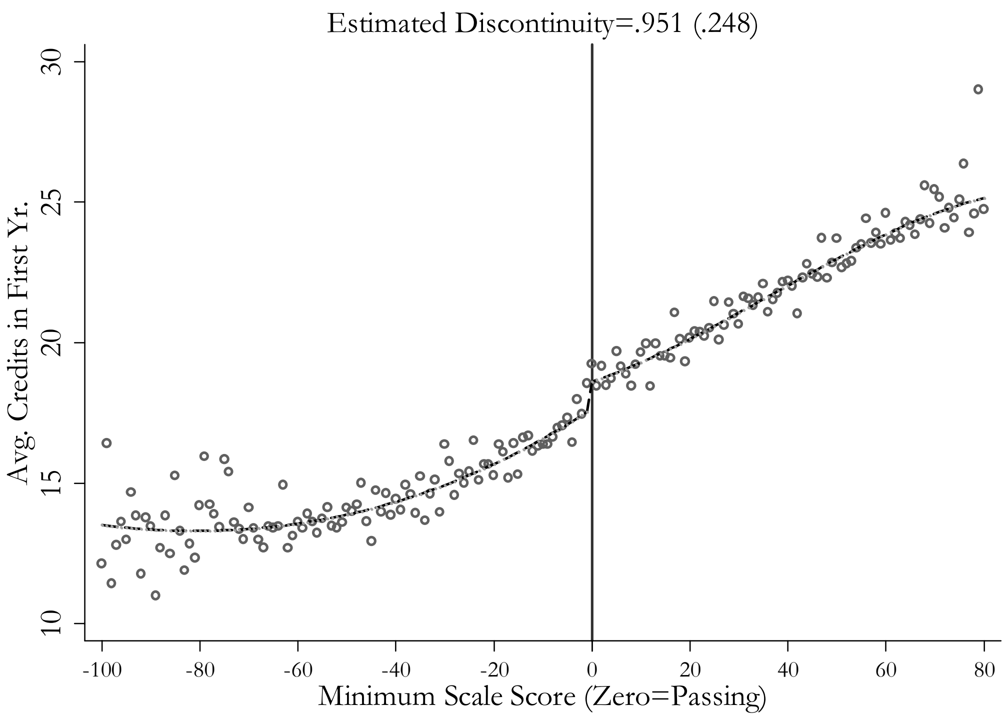


Figure 3b: Academic Credit Hours Attempted in First Year by TASP Scale Score, 4-Year Colleges

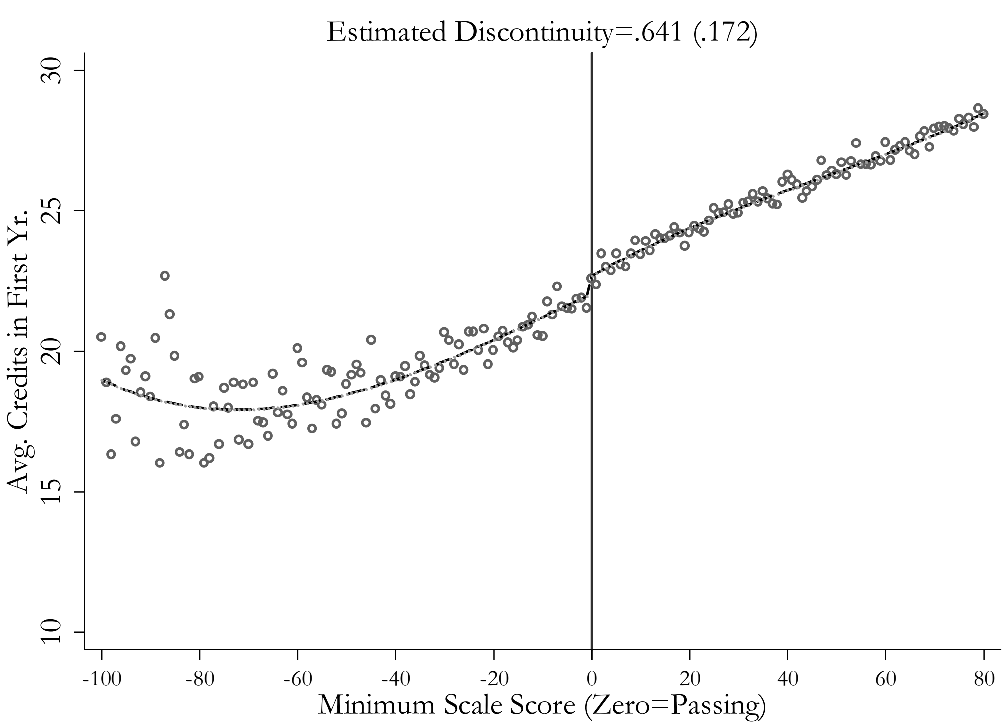


Figure 4a: Total Attempted Credits by TASP Scale Score, 2-Year Colleges

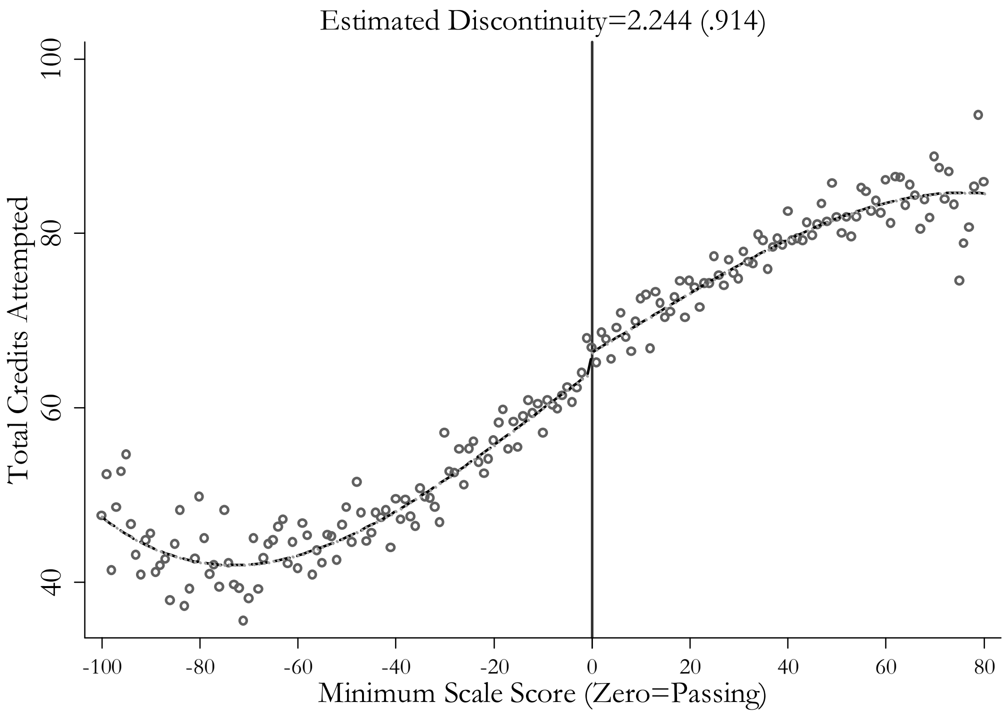


Figure 4b: Total Attempted Credits by TASP Scale Score, 4-Year Colleges

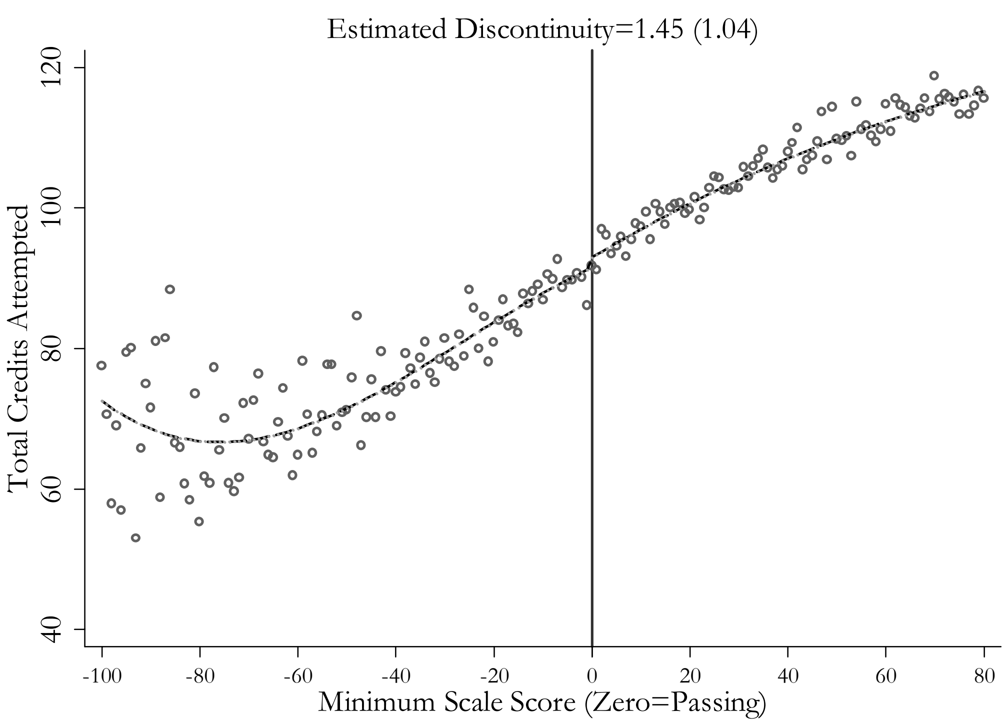


Figure 5: Outcomes for First College-Level Math Course by TASP Scale Score

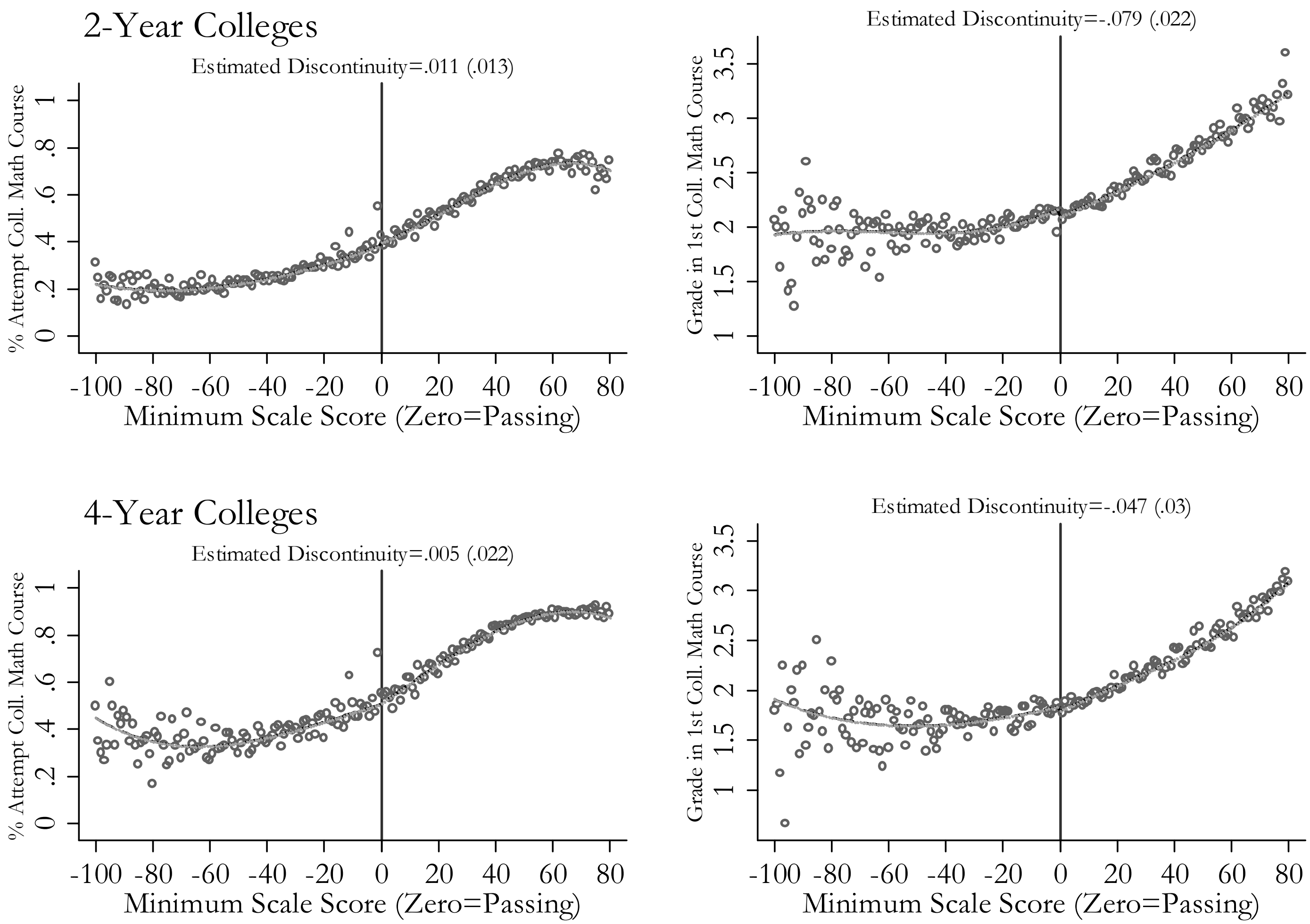


Figure 6a: Fraction of Initial 2-Year College Students who Transfer to a 4-Year College by TASP Scale Score

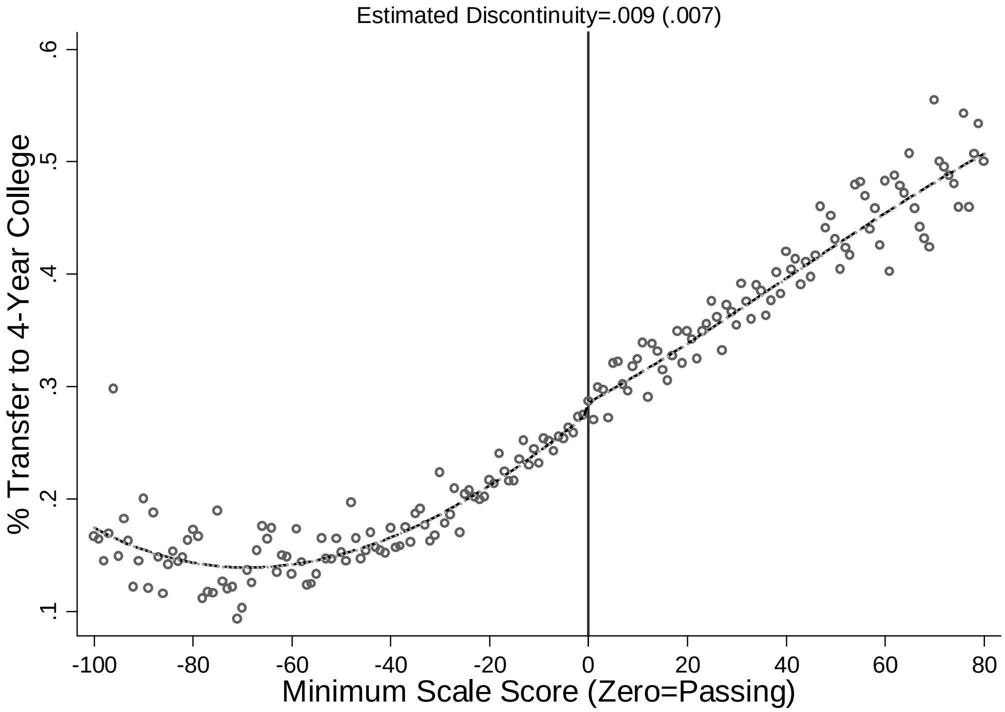


Figure 6b: Fraction of Initial 4-Year College Students who Transfer to a 2-Year College by TASP Scale Score

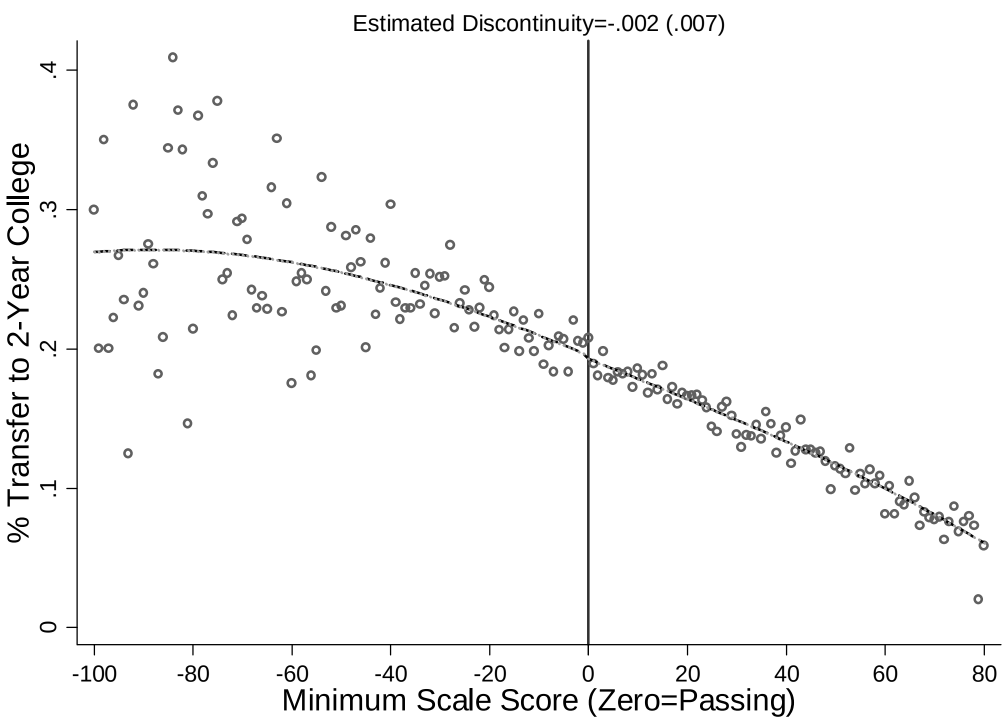


Figure 7a: Years of College Completed by TASP Scale Score, 2-Year College Students

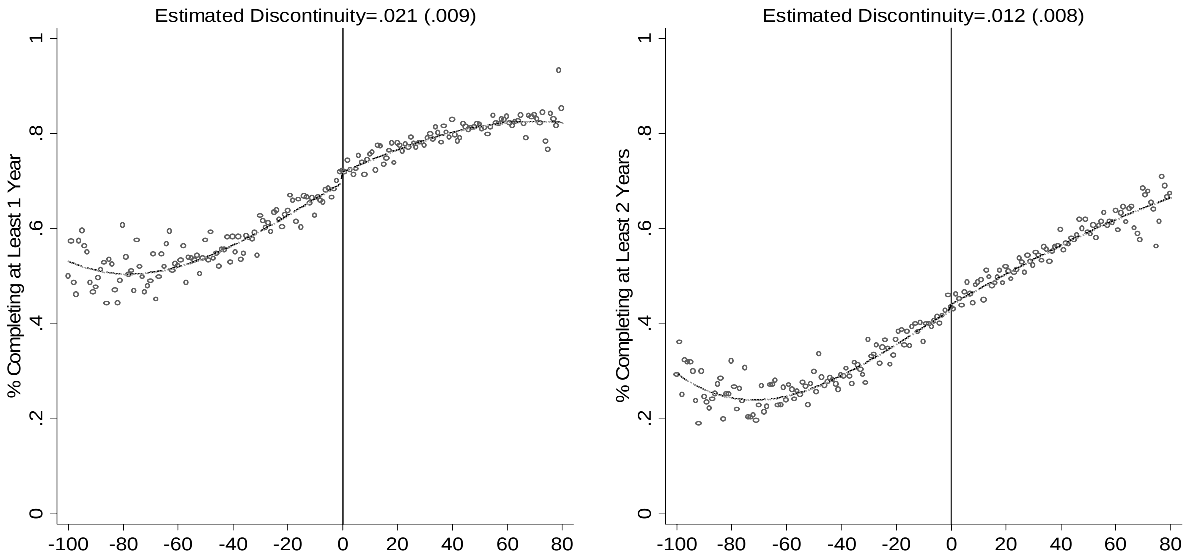


Figure 7b: Years of College Completed by TASP Scale Score, 4-Year College Students

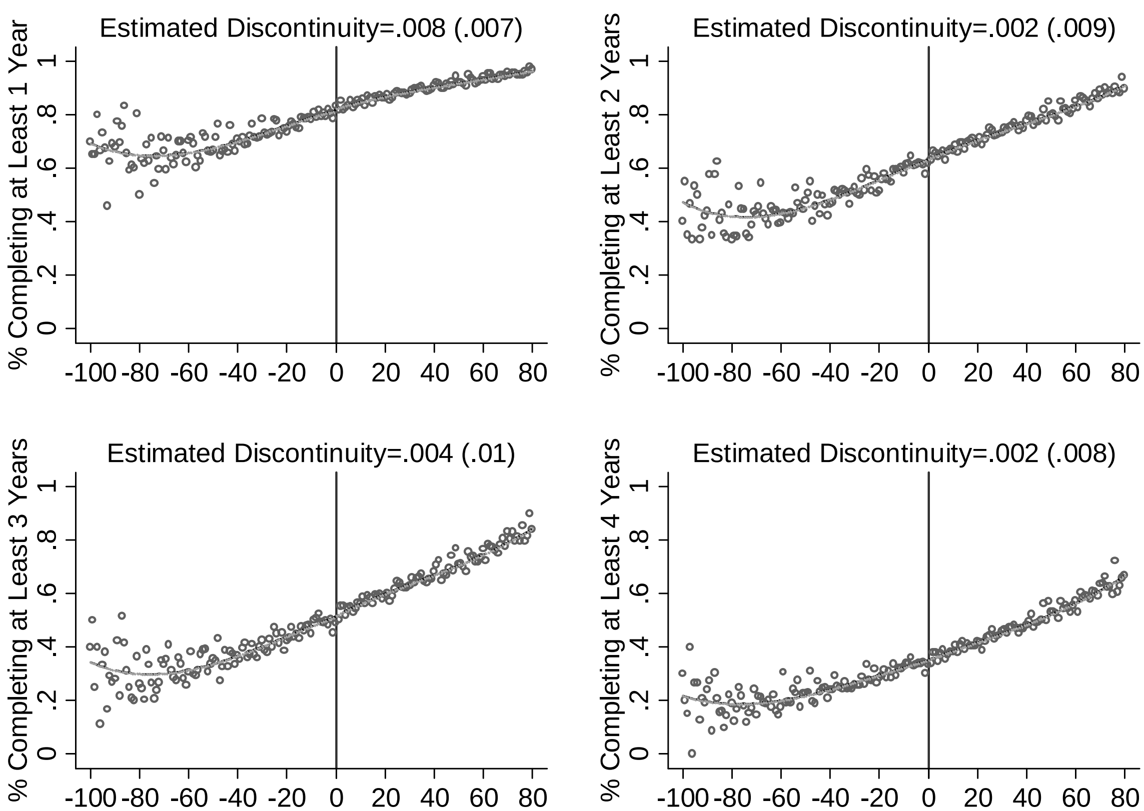


Figure 8a: Fraction Graduating Within 6 Years by TASP Scale Score, 2-Year Colleges

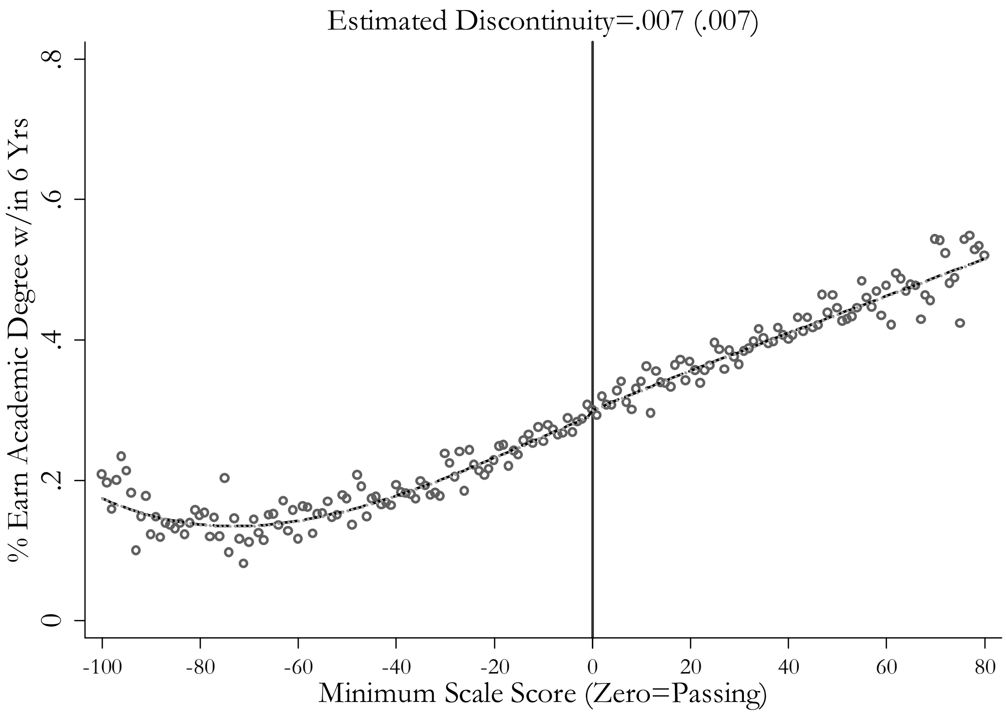


Figure 8b: Fraction Graduating Within 6 Years by TASP Scale Score, 4-Year Colleges

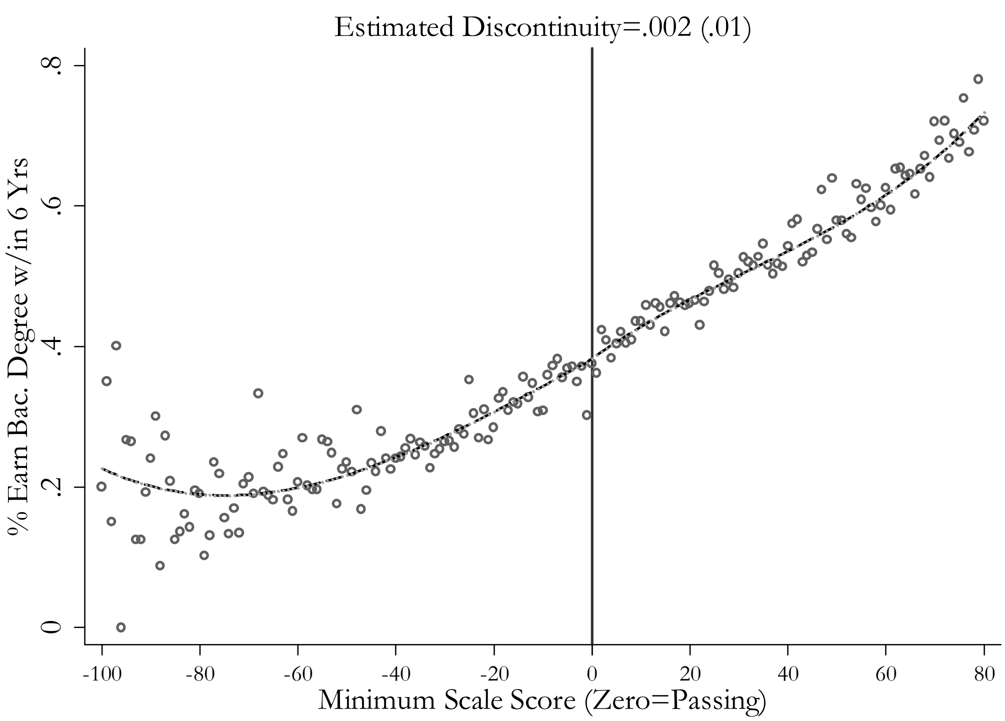


Figure 9a: Average Earnings in 7th Year After Entering College by TASP Test Score (Conditional on Positive Earnings and not Enrolled), 2-Year Colleges

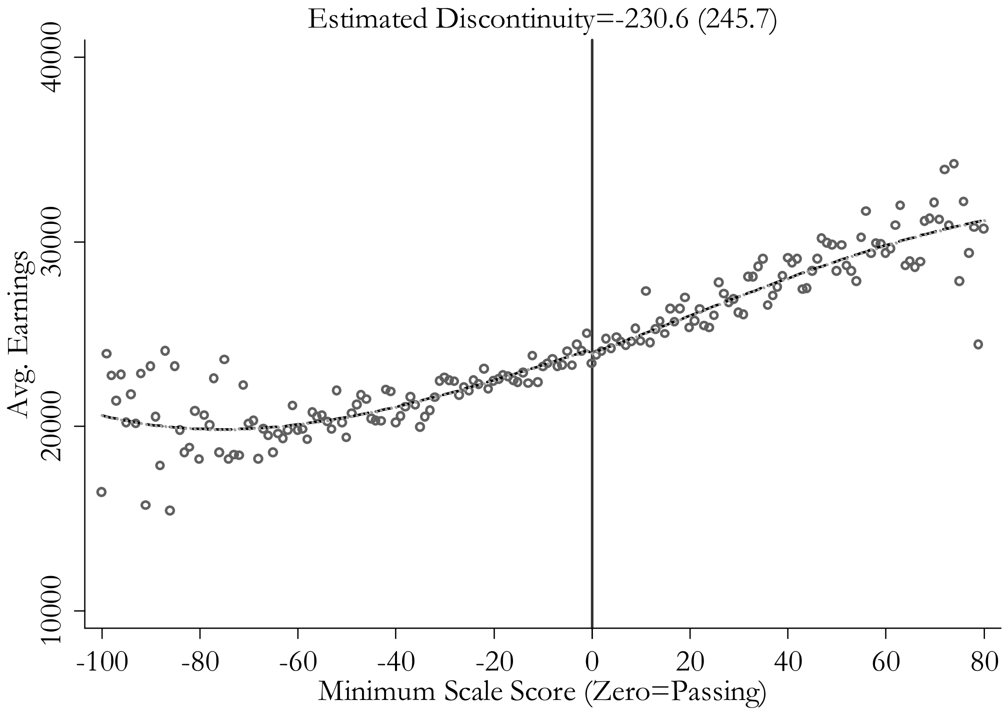


Figure 9b: Average Earnings in 7th Year After Entering College by TASP Test Score (Conditional on Positive Earnings and not Enrolled), 4-Year Colleges

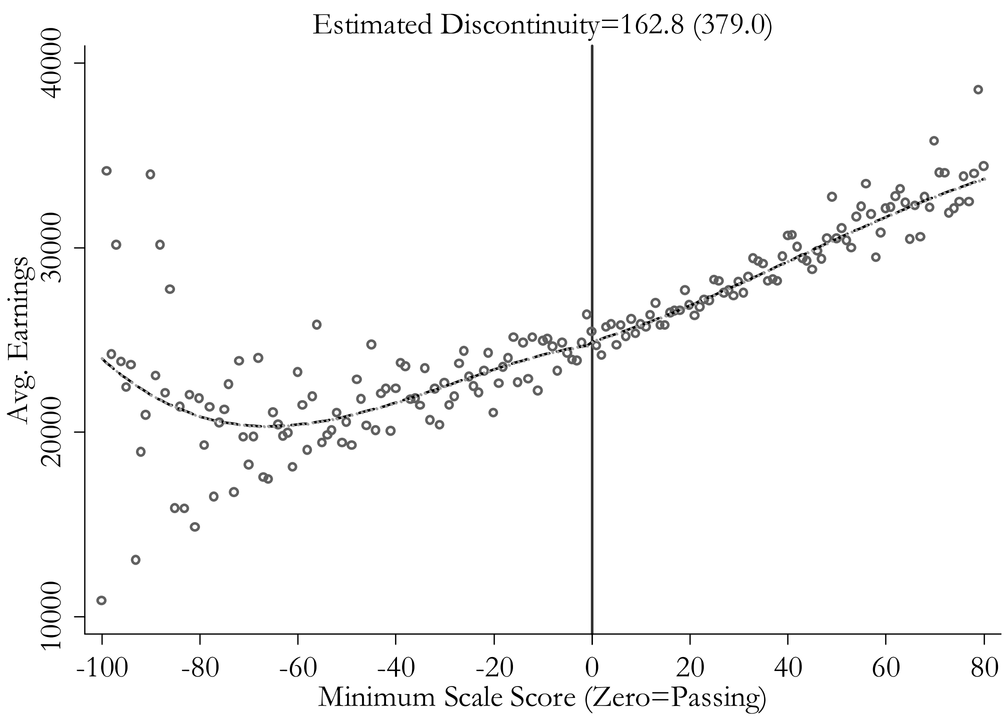


Table 1: Sample Means by Remediation Status

	2-Year Colleges			4-Year Colleges		
	All	Remed.	Non-Remed.	All	Remed.	Non-Remed.
Math Remediation	0.35	0.85	0.00	0.19	0.85	0.00
Reading Remediation	0.11	0.28	0.00	0.06	0.25	0.00
Writing Remediation	0.08	0.20	0.00	0.03	0.12	0.00
Pass TASP	0.60	0.28	0.83	0.79	0.30	0.93
Attempted Academic Credits	18.63	16.07	20.52	24.42	20.44	25.52
in First Year*	(10.32)	(9.49)	(10.50)	(7.59)	(7.81)	(7.15)
Total Attempted Academic	66.82	55.83	74.40	100.40	83.83	105.23
Credits	(48.81)	(44.94)	(49.91)	(47.55)	(51.25)	(45.28)
Attempt College Math Course	0.46	0.35	0.53	0.69	0.48	0.75
Pass College Math Course	0.41	0.31	0.49	0.62	0.39	0.68
Grade in 1st College-Level	2.37	2.12	2.49	2.24	1.81	2.32
Math Course*	(1.26)	(1.28)	(1.24)	(1.29)	(1.28)	(1.27)
Transfer Up/Down	0.30	0.20	0.37	0.16	0.22	0.14
Complete at Least 1 Yr.	0.71	0.64	0.76	0.86	0.76	0.89
Complete at Least 2 Yrs.	0.45	0.36	0.52	0.71	0.55	0.76
Complete at Least 3 Yrs.	0.28	0.17	0.35	0.61	0.43	0.66
Complete at Least 4 Yrs.	0.18	0.10	0.23	0.44	0.30	0.48
Graduate within 4 Yrs.	0.17	0.12	0.20	0.16	0.07	0.18
Graduate within 5 Yrs.	0.26	0.18	0.31	0.38	0.22	0.43
Graduate within 6 Yrs.	0.32	0.23	0.38	0.48	0.30	0.53
Earnings in Year 7*	25353	22918	26917	28085	23450	29211
	(17057)	(14794)	(18193)	(17460)	(14228)	(17980)
Prob(Earnings in Year 7 > 0)*	0.56	0.56	0.56	0.55	0.53	0.56
White	0.68	0.60	0.74	0.62	0.49	0.66
Economically Disadvantaged	0.10	0.12	0.08	0.09	0.16	0.08
Econ. Disadvantaged Missing	0.34	0.39	0.31	0.25	0.29	0.24
Age ≥ 21Years in 1st Semester	0.21	0.27	0.18	0.06	0.11	0.05
Distance from HS < 25 Miles	0.68	0.70	0.67	0.34	0.43	0.31
Distance from HS > 50 Miles	0.15	0.15	0.15	0.55	0.43	0.58
Distance from HS Missing	0.37	0.43	0.34	0.22	0.25	0.21
Receive In-District Tuition	0.58	0.60	0.56	1.00	1.00	1.00
Enrolled in 1995 or Earlier	0.44	0.40	0.47	0.53	0.37	0.58
Started College Fall Semester	0.63	0.66	0.61	0.86	0.82	0.87
Rescaled Max(Reading,Math)	33.91	22.23	41.97	45.24	24.65	51.25
Score	(26.01)	(27.25)	(21.72)	(24.05)	(25.47)	(19.95)
Rescaled Min(Reading, Math)	6.35	-13.49	20.03	23.21	-9.86	32.86
Score	(33.00)	(30.80)	(26.97)	(31.19)	(28.52)	(24.64)
Sample Size*	255,878	104,405	151,473	197,502	44,617	152,885

* - Analyses that use attempted credits in first year, grade in first college-level math course, and earnings in year 7 require additional sample restrictions and use fewer observations than listed in the Table. "Remediated" and "Non-Remediated" columns refer to remediation in any subject area. See text for additioanl details.

Table 2: Estimated Discontinuities in Baseline Characteristics

Baseline Characteristics:	2-Year	4-Year
Test Score Cell Size*	-1774.1	-929.3
	(384.1)	(355.1)
Adjusted Test Score Cell Size*	-194.7	80.8
	(359.6)	(407.7)
Predicted Prob(Graduation |X)	-0.001	-0.007
	(0.002)	(0.004)
Start in Fall Semester	-0.004	-0.001
	(0.013)	(0.010)
Non-Hispanic White	0.002	-0.003
	(0.005)	(0.009)
Age $\geq$ 21Years in First Semester	-0.006	-0.002
	(0.006)	(0.006)
Max Subject Score	-0.174	0.337
	(0.593)	(0.854)
Economically Disadvantaged	-0.001	0.009
	(0.004)	(0.005)
Missing Economically Disadvantaged	-0.004	-0.012
	(0.006)	(0.010)
Entered College in 1995 or Earlier	0.069	0.058
	(0.056)	(0.056)
College <25 Miles from HS	0.015	0.012
	(0.008)	(0.008)
College >50 Miles from HS	-0.005	-0.006
	(0.004)	(0.012)
Distance from HS Mising	-0.006	-0.007
	(0.006)	(0.009)
Receive In-District Tuition	<0.001	
	(0.007)	
Sample Size	255,878	197,502

Notes: Cell entries are least squares estimates of discontinuity in the conditional expectation of a given covariate. The test score used in all models is the minimum of the math and reading scores and the remedi-ation variable is an indicator for being in remediation for any subject. Standard errors adjusted for clustering at the test score level in parentheses. See text for additional details concerning regression specification. *Estimated discontinuity in the mean cell size obtained from a test score cell-level regression weighted by the cell size using the same parametric specification as that used for the other covariates (N=158)

Table 3: Reduced-Form and IV Estimates for Academic Outcomes

	Reduced Form				IV			
	No Covariates		With Covariates		No Covariates		With Covariates	
Dependent Variable	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year
Remediation (Any Subject)	-0.360	-0.421	-0.371	-0.426				
	(0.013)	(0.011)	(0.010)	(0.009)				
Academic Credit Hours:								
Attempted in First Year*	0.951	0.641	0.987	0.704	-2.407	-1.500	-2.421	-1.625
	(0.248)	(0.172)	(0.167)	(0.146)	(0.636)	(0.402)	(0.394)	(0.343)
Total Attempted	2.244	1.450	2.249	1.908	-6.239	-3.447	-6.068	-4.475
	(0.914)	(1.040)	(0.689)	(0.857)	(2.589)	(2.463)	(1.890)	(1.998)
Performance in First College Math Course:								
Attempt Course	0.011	0.005	0.005	-0.005	-0.030	-0.012	-0.013	0.012
	(0.013)	(0.022)	(0.006)	(0.011)	(0.035)	(0.051)	(0.017)	(0.027)
Pass Course	0.005	0.000	0.001	-0.007	-0.015	0.001	-0.002	0.016
	(0.012)	(0.018)	(0.006)	(0.010)	(0.033)	(0.044)	(0.017)	(0.024)
Grade Conditional on	-0.079	-0.047	-0.055	-0.025	0.235	0.122	0.160	0.063
Attempting Course*	(0.022)	(0.030)	(0.025)	(0.033)	(0.069)	(0.078)	(0.072)	(0.086)
Transferring:								
Up to 4-Yr (from 2 Yr.) or	0.009	-0.002	0.010	-0.006	-0.025	0.006	-0.027	0.013
Down to 2-Yr. (from 4 Yr.)	(0.007)	(0.007)	(0.006)	(0.007)	(0.021)	(0.017)	(0.018)	(0.016)
College Attainment:								
At Least 1 Year	0.021	0.008	0.022	0.011	-0.060	-0.019	-0.059	-0.025
	(0.009)	(0.007)	(0.007)	(0.006)	(0.024)	(0.017)	(0.018)	(0.014)
At Least 2 Years	0.012	0.002	0.013	0.007	-0.034	-0.004	-0.034	-0.017
	(0.008)	(0.009)	(0.007)	(0.008)	(0.023)	(0.022)	(0.018)	(0.019)
At Least 3 Years	0.009	0.004	0.010	0.012	-0.025	-0.010	-0.028	-0.028
	(0.007)	(0.010)	(0.006)	(0.009)	(0.019)	(0.025)	(0.016)	(0.021)
At Least 4 Years	0.006	0.002	0.007	0.006	-0.018	-0.005	-0.018	-0.015
	(0.005)	(0.008)	(0.004)	(0.007)	(0.014)	(0.020)	(0.012)	(0.017)
Graduate Within 4 Years	0.005	-0.007	0.007	-0.001	-0.013	0.016	-0.018	0.003
	(0.005)	(0.007)	(0.004)	(0.006)	(0.014)	(0.018)	(0.012)	(0.014)
Graduate Within 5 Years	0.004	0.000	0.006	0.009	-0.012	0.001	-0.017	-0.021
	(0.006)	(0.011)	(0.005)	(0.008)	(0.017)	(0.025)	(0.015)	(0.018)
Graduate Within 6 Years	0.007	0.002	0.009	0.010	-0.020	-0.004	-0.023	-0.023
	(0.007)	(0.010)	(0.006)	(0.009)	(0.020)	(0.024)	(0.016)	(0.020)

Notes: Estimated discontinuities in the conditional expectation of a given outcome in columns 1-4. 2SLS IV estimates of the effect of remediation on a given outcome are reported in remaining columns. Standard errors adjusted for clustering at the test score level in parentheses. The test score used in all models is the minimum of the math and reading scores and the remediation variable is an indicator for being in remediation for any subject. Models with covariates include controls for the score on the "opposite" subject, dummies for white, Hispanic, starting in fall semester, being 21 or older in first semester, academic year of first semester, academic year student initially took the TASP, economically disadvantaged, missing economically disadvantaged, receiving in-district tuition, missing data for in-district tuition status, distance from HS <25 miles, distance from HS>50 miles, distance from HS missing, starting college more than 1 semester after initially taking the TASP test. *-Additional sample restrictions made for these outcomes (see text for details).

Table 4: Reduced-Form and IV Estimates of Impact on Earnings

		2-Year Colleges			4-Year Colleges		
	Sample	All	Positive Earnings	Positive Earnings & Not Enrolled	All	Positive Earnings	Positive Earnings & Not Enrolled
				No Covariates			
Year 5	Prob(Selection)		0.004	0.003		-0.004	-0.006
			(0.005)	(0.008)		(0.006)	(0.007)
	Reduced Form	-74.0	-176.0	-15.0	-64.7	3.3	-645.4
		(153.2)	(177.4)	(261.5)	(183.8)	(223.2)	(372.4)
	IV	218.8	522.3	43.2	160.1	-8.3	1613.1
		(453.4)	(528.1)	(752.9)	(455.3)	(564.2)	(937.5)
Year 6	Prob(Selection)		0.007	0.001		-0.007	-0.005
			(0.004)	(0.006)		(0.009)	(0.010)
	Reduced Form	80.1	-53.5	98.8	32.9	204.4	-129.7
		(183.8)	(227.8)	(269.6)	(253.0)	(319.6)	(429.9)
	IV	-236.8	159.7	-294.7	-81.5	-514.8	324.4
		(545.6)	(677.9)	(803.2)	(625.7)	(808.2)	(1076.8)
Year 7*	Prob(Selection)		0.007	0.004		0.012	0.009
			(0.006)	(0.007)		(0.010)	(0.010)
	Reduced Form	13.3	-173.7	-230.6	507.6	333.2	162.8
		(194.9)	(217.9)	(245.7)	(335.4)	(360.8)	(379.0)
	IV	-43.0	562.3	766.6	-1306.0	-860.4	-412.9
		(630.9)	(698.2)	(812.6)	(866.9)	(949.6)	(968.3)
				With Covariates			
Year 5	Prob(Selection)		0.003	0.001		-0.005	-0.006
			(0.005)	(0.006)		(0.005)	(0.007)
	Reduced Form	44.0	-1.1	192.1	5.8	76.0	-356.2
		(143.6)	(153.1)	(213.3)	(228.5)	(252.6)	(391.5)
	IV	-126.5	3.2	-535.3	-14.2	-190.8	882.6
		(413.6)	(442.3)	(592.4)	(560.1)	(633.0)	(973.2)
Year 6	Prob(Selection)		0.005	-0.001		-0.008	-0.003
			(0.004)	(0.005)		(0.007)	(0.010)
	Reduced Form	144.9	70.5	246.7	118.5	303.5	37.4
		(168.1)	(186.7)	(234.4)	(257.3)	(270.2)	(338.4)
	IV	-416.9	-205.1	-712.8	-290.5	-758.7	-92.5
		(487.6)	(544.3)	(676.5)	(628.8)	(675.2)	(836.6)
Year 7*	Prob(Selection)		0.006	0.002		0.011	0.009
			(0.005)	(0.006)		(0.008)	(0.009)
	Reduced Form	27.3	-136.2	-150.6	552.0	389.5	233.4
		(189.2)	(216.7)	(263.1)	(358.0)	(359.8)	(378.4)
	IV	-86.1	431.8	488.8	-1413.9	-1001.8	-588.4
		(597.3)	(684.3)	(849.8)	(912.8)	(930.3)	(953.7)

Notes: Cell entries are estimated effects on labor market outcomes. Year refers to the number of years since first enrolling in college. For a given year, entries in the first row denote the probability of selection into the sample (where the sample is denoted in the column heading and consists of either all observations, individuals with positive earnings in a given year, or individuals with positive earnings who are also not enrolled in school during a given year), entries in the second row are estimated discontinuities in average earnings, and entries in the third row are IV estimates of the effect of remediation on earnings. Standard errors adjusted for clustering at the test score level in parentheses. The test score used in all models is the minimum of the math and reading scores and the remediation variable is an indicator for being in remediation for any subject. See text for additional details concerning regression specification. Models with covariates include the controls listed in the notes to Table 3.
*-Year 7 estimates restricted to students enrolling in college in fall of 1997 or earlier

Table 5: Reduced-Form and IV Estimates for Selected Outcomes by Subgroup

Outcome:	Complete 1 Year of College				Total Academic Credits Attempted				Graduate Within 6 Years			
	Reduced Form		IV		Reduced Form		IV		Reduced Form		IV	
Subgroup	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year
Year Started College												
1995 or earlier	0.011	-0.012	-0.041	0.039	0.896	-0.263	-3.268	0.851	0.008	0.018	-0.028	-0.057
	(0.010)	(0.011)	(0.035)	(0.038)	(1.066)	(1.652)	(3.743)	(5.384)	(0.009)	(0.013)	(0.031)	(0.040)
1996 or later	0.032	0.023	-0.077	-0.047	4.032	2.799	-9.710	-5.855	0.015	0.005	-0.035	-0.010
	(0.014)	(0.012)	(0.034)	(0.026)	(1.534)	(1.684)	(3.670)	(3.534)	(0.012)	(0.016)	(0.030)	(0.033)
Tuition												
In-district	0.024		-0.067		2.216		-6.097		0.015		-0.040	
	(0.011)		(0.029)		(1.086)		(2.970)		(0.008)		(0.022)	
Out-of-district	0.017		-0.049		2.294		-6.459		-0.003		0.010	
	(0.010)		(0.028)		(1.231)		(3.577)		(0.010)		(0.027)	
Distance From HS												
25 Miles or less	0.029	0.001	-0.078	-0.002	3.192	1.174	-8.742	-2.827	0.016	-0.009	-0.045	0.022
	(0.010)	(0.014)	(0.028)	(0.034)	(0.995)	(1.947)	(2.764)	(4.682)	(0.009)	(0.014)	(0.024)	(0.035)
50 Miles or greater	0.000	0.018	-0.001	-0.043	0.306	2.994	-0.744	-7.329	-0.023	0.022	0.056	-0.054
	(0.013)	(0.010)	(0.032)	(0.024)	(1.926)	(1.374)	(4.691)	(3.307)	(0.016)	(0.015)	(0.039)	(0.037)
Demographics												
Black or Hispanic	0.028	0.005	-0.073	-0.012	2.103	0.758	-5.429	-1.759	0.013	-0.002	-0.034	0.005
	(0.010)	(0.012)	(0.028)	(0.027)	(1.142)	(1.632)	(3.045)	(3.789)	(0.009)	(0.014)	(0.023)	(0.033)
Econ. disadvantaged	0.011	0.002	-0.026	-0.004	1.195	3.656	-2.762	-8.564	0.019	0.014	-0.043	-0.032
	(0.017)	(0.017)	(0.040)	(0.040)	(1.963)	(2.179)	(4.571)	(5.202)	(0.018)	(0.020)	(0.042)	(0.048)
Age $\geq$ 21 Yrs. when	0.028	0.027	-0.080	-0.065	2.051	0.528	-5.866	-1.269	0.004	-0.003	-0.012	0.008
started college	(0.015)	(0.029)	(0.044)	(0.068)	(1.115)	(2.590)	(3.339)	(6.208)	(0.011)	(0.023)	(0.032)	(0.056)

Notes: Cell entries in the "Reduced Form" columns are estimated discontinuities in the outcome and entries in the "IV" column are 2SLS estimates of the effect of being in remediation for at least one subject. Sample limited to students in the indicated subgroup. The test score used in all models is the minimum of the math and reading score (these estimates are not adjusted for other covariates). Standard errors adjusted for clustering at the test score level in parentheses. See text for details concerning regression specification.

Table 6: Estimates for Selected Academic Outcomes by Remediation Subject Area

	Reduced Form				IV			
	No Covariates		With Covariates		No Covariates		With Covariates	
Dependent Variable	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year
	Math Remediation							
Math Remediation	-0.363	-0.437	-0.379	-0.446				
	(0.017)	(0.014)	(0.013)	(0.011)				
Academic Credit Hours:								
Total Attempted	1.375	0.302	1.784	1.091	-3.788	-0.692	-4.704	-2.446
	(1.213)	(1.275)	(.833)	(.915)	(3.293)	(2.925)	(2.162)	(2.071)
First Math Course								
Attempt Course	0.023	0.024	0.008	-0.011	-0.062	-0.055	-0.020	0.024
	(0.012)	(0.015)	(0.007)	(0.010)	(0.034)	(0.034)	(0.018)	(0.023)
Pass Course	0.012	0.011	-0.001	-0.015	-0.033	-0.026	0.001	0.034
	(0.011)	(0.012)	(0.007)	(0.009)	(0.032)	(0.028)	(0.017)	(0.021)
Grade Conditional on	-0.109	-0.087	-0.068	-0.022	0.325	0.217	0.197	0.055
Attempting Course	(0.029)	(0.031)	(0.026)	(0.024)	(0.089)	(0.081)	(0.074)	(0.060)
College Attainment:								
At Least 1 Year	0.015	0.004	0.018	0.009	-0.043	-0.010	-0.048	-0.020
	(0.012)	(0.009)	(0.008)	(0.007)	(0.031)	(0.021)	(0.021)	(0.017)
Graduate Within 6 Years	0.009	-0.015	0.012	-0.007	-0.025	0.035	-0.032	0.015
	(0.009)	(0.011)	(0.007)	(0.008)	(0.023)	(0.025)	(0.017)	(0.017)
	Reading Remediation							
Reading Remediation	-0.295	-0.355	-0.284	-0.352				
	(0.012)	(0.018)	(0.010)	(0.016)				
Academic Credit Hours:								
Total Attempted	-0.535	0.454	0.330	1.008	1.813	-1.279	-1.159	-2.864
	(1.803)	(2.453)	(1.229)	(1.569)	(6.116)	(6.883)	(4.317)	(4.411)
First Math Course								
Attempt Course	-0.030	-0.051	0.015	0.015	0.101	0.144	-0.053	-0.041
	(0.022)	(0.028)	(0.008)	(0.011)	(0.074)	(0.079)	(0.029)	(0.032)
Pass Course	-0.021	-0.048	0.019	0.009	0.071	0.134	-0.067	-0.025
	(0.021)	(0.025)	(0.009)	(0.011)	(0.070)	(0.070)	(0.030)	(0.030)
Grade Conditional on	0.049	-0.002	0.029	-0.026	-0.164	0.006	-0.101	0.071
Attempting Course	(0.029)	(0.033)	(0.028)	(0.031)	(0.093)	(0.092)	(0.095)	(0.086)
College Attainment:								
At Least 1 Year	-0.017	0.005	-0.011	0.006	0.057	-0.014	0.040	-0.017
	(0.016)	(0.016)	(0.012)	(0.011)	(0.054)	(0.044)	(0.041)	(0.032)
Graduate Within 6 Years	-0.001	0.011	0.002	0.012	0.004	-0.031	-0.007	-0.033
	(0.013)	(0.020)	(0.009)	(0.011)	(0.045)	(0.056)	(0.031)	(0.031)

Notes: Cell entries in first four columns are estimated discontinuities for a given outcome. IV estimates of the effect of remediation are reported in remaining columns. Regression discontinuity and IV estimates use the math test score for the "Math Remediation" panel and reading test scores for the "Reading Remediation" panel. The "treatment" for the IV estimates is remediation in math for the "Math Remediation" panel and remediation in reading for the "Reading Remediation" panel. Standard errors adjusted for clustering at the test score level in parentheses. See text for additional details concerning regressio specification. *-Additional sample restrictions made for these outcomes (see text for details).

Table A1: Estimated Discontinuities in Baseline Characteristics (Narrow Band Sample)

Baseline Characteristics:	2-Year	4-Year
Test Score Cell Size*	-6832.3	-3768.5
	(4113.1)	(2463.2)
Adjusted Test Score Cell Size*	6944.0	1629.7
	(8862.3)	(5726.2)
Predicted Prob(Graduation |X)	-0.003	-0.005
	(0.003)	(0.005)
Start in Fall Semester	-0.018	0.005
	(0.012)	(0.013)
Non-Hispanic White	0.003	-0.006
	(0.007)	(0.010)
Age ≥ 21Years in First Semester	-0.003	-0.006
	(0.008)	(0.006)
Max Subject Score	-0.039	0.153
	(0.872)	(1.402)
Economically Disadvantaged	-0.003	0.012
	(0.005)	(0.007)
Missing Economically Disadvantaged	0.003	-0.004
	(0.006)	(0.013)
Entered College in 1995 or Earlier	0.075	0.037
	(0.090)	(0.093)
College <25 Miles from HS	0.008	0.011
	(0.012)	(0.009)
College >50 Miles from HS	-0.007	-0.004
	(0.005)	(0.016)
Distance from HS Mising	0.004	-0.010
	(0.008)	(0.013)
Receive In-District Tuition	0.002	
	(0.008)	
Sample Size	59,344	33,910

Notes: Sample is limited to students scoring within 10 scale score points of the passing cutoff. Cell entries are least squares estimates of discontinuity in the conditional expectation of a given covariate. The test score used in all models is the minimum of the math and reading scores and the remediation variable is an indicator for being in remediation for any subject. Standard errors adjusted for clustering at the test score level in parentheses. See text for additional details concerning regression specification. *Estimated discontinuity in the mean cell size obtained from a test score cell-level regression weighted by the cell size using the same parametric specification as that used for the other covariates (N=21)

Table A2: Reduced-Form and IV Estimates for Academic Outcomes (Narrow Band Sample)

	Reduced Form				IV			
	No Covariates		With Covariates		No Covariates		With Covariates	
Dependent Variable	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year
Remediation (Any Subject)	-0.360	-0.409	-0.376	-0.425				
	(0.015)	(0.014)	(0.013)	(0.013)				
Academic Credit Hours:								
Attempted in First Year*	0.883	0.682	0.869	0.739	-2.166	-1.654	-2.042	-1.717
	(0.317)	(0.270)	(0.184)	(0.220)	(0.810)	(0.659)	(0.418)	(0.530)
Total Attempted	1.153	2.828	1.486	3.226	-3.208	-6.906	-3.956	-7.588
	(1.124)	(1.398)	(0.812)	(1.147)	(3.151)	(3.428)	(2.151)	(2.712)
Performance in First College Math Course:								
Attempt Course	-0.004	0.022	-0.008	0.002	0.010	-0.053	0.022	-0.004
	(0.020)	(0.031)	(0.008)	(0.014)	(0.056)	(0.075)	(0.020)	(0.033)
Pass Course	-0.009	0.020	-0.013	0.005	0.026	-0.050	0.034	-0.011
	(0.019)	(0.026)	(0.008)	(0.012)	(0.052)	(0.062)	(0.021)	(0.028)
Grade Conditional on	-0.062	0.021	-0.042	0.033	0.186	-0.057	0.124	-0.089
Attempting Course*	(0.028)	(0.043)	(0.026)	(0.044)	(0.086)	(0.118)	(0.076)	(0.119)
Transferring:								
Up to 4-Year/Down to	0.003	-0.015	0.005	-0.018	-0.008	0.037	-0.013	0.043
2-Year College	(0.009)	(0.008)	(0.008)	(0.007)	(0.024)	(0.020)	(0.020)	(0.016)
College Attainment:								
At Least 1 Year	0.010	0.014	0.013	0.016	-0.029	-0.035	-0.035	-0.038
	(0.010)	(0.010)	(0.006)	(0.008)	(0.027)	(0.023)	(0.017)	(0.019)
At Least 2 Years	0.000	0.020	0.003	0.025	-0.001	-0.049	-0.008	-0.060
	(0.009)	(0.010)	(0.007)	(0.008)	(0.025)	(0.024)	(0.019)	(0.018)
At Least 3 Years	0.006	0.016	0.009	0.023	-0.017	-0.040	-0.023	-0.054
	(0.007)	(0.016)	(0.006)	(0.012)	(0.021)	(0.040)	(0.016)	(0.029)
At Least 4 Years	0.001	0.013	0.002	0.018	-0.003	-0.031	-0.005	-0.042
	(0.006)	(0.011)	(0.005)	(0.009)	(0.017)	(0.027)	(0.014)	(0.023)
Graduate Within 4 Years	0.001	-0.004	0.002	0.001	-0.002	0.009	-0.004	-0.002
	(0.005)	(0.010)	(0.005)	(0.008)	(0.015)	(0.025)	(0.013)	(0.019)
Graduate Within 5 Years	0.005	0.003	0.008	0.012	-0.015	-0.008	-0.020	-0.028
	(0.006)	(0.016)	(0.005)	(0.011)	(0.017)	(0.040)	(0.014)	(0.026)
Graduate Within 6 Years	0.009	0.009	0.011	0.017	-0.024	-0.021	-0.029	-0.040
	(0.008)	(0.016)	(0.006)	(0.012)	(0.022)	(0.039)	(0.017)	(0.028)

Notes: Sample is limited to students scoring within 10 scale score points of the passing cutoff. Estimated discontinuities in the conditional expectation of a given outcome in columns 1-4. 2SLS IV estimates of the effect of remediation on a given outcome are reported in remaining columns. Standard errors adjusted for clustering at the test score level in parentheses. See notes to Table 3 for additional details. *-Additional sample restrictions made for these outcomes (see text for details).

Table A3: Reduced-Form and IV Estimates of Impact on Earnings (Narrow Band Sample)

		2-Year Colleges			4-Year Colleges		
Sample		All	Positive Earnings	Positive Earnings & Not Enrolled	All	Positive Earnings	Positive Earnings & Not Enrolled
				No Covariates			
Year 5	Prob(Selection)		0.008	0.013		-0.009	-0.014
			(0.006)	(0.010)		(0.008)	(0.009)
	Reduced Form	127.3	-9.1	125.7	-185.9	-76.0	-559.3
		(217.8)	(260.9)	(357.5)	(217.9)	(240.8)	(305.6)
	IV	-373.5	26.5	-350.9	466.5	195.6	1469.6
		(638.0)	(763.0)	(990.7)	(552.0)	(619.2)	(827.9)
Year 6	Prob(Selection)		0.015	0.008		-0.008	-0.011
			(0.005)	(0.007)		(0.011)	(0.012)
	Reduced Form	138.5	-193.6	68.4	-25.4	167.3	-170.0
		(237.1)	(270.4)	(312.3)	(350.9)	(450.8)	(592.3)
	IV	-406.5	569.4	-200.5	63.9	-428.5	427.5
		(704.8)	(786.3)	(914.0)	(880.6)	(1158.6)	(1494.8)
Year 7*	Prob(Selection)		0.012	0.015		0.010	0.002
			(0.007)	(0.010)		(0.014)	(0.015)
	Reduced Form	102.0	-198.6	-512.9	357.3	170.3	245.8
		(266.1)	(257.3)	(250.2)	(421.2)	(489.0)	(503.0)
	IV	-325.4	632.8	1705.9	-931.0	-448.7	-637.7
		(858.7)	(799.9)	(809.1)	(1103.6)	(1307.6)	(1330.6)
				With Covariates			
Year 5	Prob(Selection)		0.009	0.011		-0.010	-0.013
			(0.007)	(0.008)		(0.007)	(0.009)
	Reduced Form	245.5	124.0	278.5	-189.7	-112.5	-488.5
		(180.3)	(203.1)	(242.9)	(303.2)	(313.7)	(351.3)
	IV	-697.1	-349.6	-749.8	466.6	281.7	1240.2
		(516.7)	(571.4)	(643.9)	(749.2)	(787.0)	(896.9)
Year 6	Prob(Selection)		0.015	0.007		-0.009	-0.009
			(0.005)	(0.006)		(0.010)	(0.011)
	Reduced Form	246.0	-73.9	234.4	-28.3	130.3	-177.1
		(207.0)	(222.6)	(239.6)	(366.2)	(403.3)	(463.9)
	IV	-698.5	209.4	-657.9	69.6	-326.3	430.3
		(602.4)	(628.7)	(671.4)	(901.5)	(1009.4)	(1134.9)
Year 7*	Prob(Selection)		0.012	0.012		0.008	0.002
			(0.005)	(0.008)		(0.011)	(0.014)
	Reduced Form	151.1	-142.9	-392.3	326.5	157.0	278.3
		(237.8)	(271.5)	(337.8)	(493.1)	(522.4)	(500.2)
	IV	-469.1	445.3	1269.4	-858.3	-414.2	-717.8
		(749.2)	(839.0)	(1063.8)	(1299.2)	(1384.3)	(1296.7)

Notes: Sample is limited to students scoring within 10 scale score points of the passing cutoff. Year refers to the number of years since first enrolling in college. See notes to Table 4 for additional details.
*-Year 7 estimates restricted to students enrolling in college in fall of 1997 or earlier

Table A4: Reduced-Form and IV Estimates for Selected Outcomes by Subgroup (Narrow Band Sample)

Outcome:	Complete 1 Year of College				Total Academic Credits Attempted				Graduate Within 6 Years			
	Reduced Form		IV		Reduced Form		IV		Reduced Form		IV	
Subgroup	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year
Year Started College												
1995 or earlier	-0.003	-0.012	0.010	0.045	-0.568	0.016	2.127	-0.058	0.015	0.010	-0.056	-0.036
	(0.010)	(0.014)	(0.040)	(0.054)	(1.336)	(1.565)	(5.212)	(5.764)	(0.009)	(0.010)	(0.028)	(0.035)
1996 or later	0.018	0.029	-0.041	-0.061	2.199	4.597	-5.191	-9.618	0.007	0.013	-0.017	-0.027
	(0.016)	(0.015)	(0.038)	(0.032)	(1.771)	(2.130)	(4.119)	(4.463)	(0.013)	(0.020)	(0.030)	(0.042)
Tuition												
In-district	0.011		-0.031		0.986		-2.713		0.015		-0.041	
	(0.012)		(0.033)		(1.315)		(3.583)		(0.010)		(0.026)	
Out-of-district	0.009		-0.025		1.374		-3.874		0.001		-0.002	
	(0.011)		(0.033)		(1.667)		(4.813)		(0.010)		(0.028)	
Distance From HS												
25 Miles or less	0.018	0.012	-0.049	-0.032	2.176	4.992	-5.867	-12.897	0.010	0.025	-0.028	-0.064
	(0.012)	(0.017)	(0.033)	(0.042)	(1.218)	(1.854)	(3.325)	(4.858)	(0.009)	(0.013)	(0.026)	(0.033)
50 Miles or greater	-0.018	0.017	0.044	-0.043	-1.744	2.494	4.273	-6.296	-0.011	0.007	0.026	-0.018
	(0.014)	(0.013)	(0.033)	(0.032)	(2.940)	(1.592)	(7.170)	(3.935)	(0.022)	(0.023)	(0.054)	(0.058)
Demographics												
Black or Hispanic	0.020	0.015	-0.047	-0.036	1.545	3.169	-3.752	-7.534	0.027	0.004	-0.066	-0.009
	(0.012)	(0.016)	(0.030)	(0.038)	(1.482)	(1.901)	(3.652)	(4.556)	(0.012)	(0.020)	(0.030)	(0.047)
Econ. disadvantaged	0.015	0.007	-0.033	-0.016	1.851	6.781	-4.004	-16.061	0.009	0.022	-0.019	-0.052
	(0.020)	(0.022)	(0.043)	(0.051)	(2.570)	(2.625)	(5.601)	(6.822)	(0.023)	(0.026)	(0.050)	(0.062)
Age ≥ 21 Yrs. when	0.007	0.014	-0.023	-0.031	0.245	-3.393	-0.742	7.621	-0.003	-0.013	0.009	0.030
started college	(0.016)	(0.045)	(0.050)	(0.099)	(1.176)	(3.847)	(3.599)	(9.086)	(0.014)	(0.032)	(0.044)	(0.074)

Notes: Sample is limited to students scoring within 10 scale score points of the passing cutoff. Cell entries in the "Reduced Form" columns are estimated discontinuities in the outcome and entries in the "IV" column are 2SLS estimates of the effect of being in remediation for at least one subject. Sample limited to students in the subgroup indicated in the column heading. The test score used in all models is the minimum of the math and reading score (these estimates are not adjusted for other covariates). Standard erros adjusted for clustering at the test score level in parentheses.

Table A5: Estimates for Selected Academic Outcomes by Remediation Subject Area (Narrow Band Sample)

	Reduced Form				IV			
	No Covariates		With Covariates		No Covariates		With Covariates	
Dependent Variable	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year	2-Year	4-Year
	Math Remediation							
Math Remediation	-0.349	-0.434	-0.383	-0.460				
	(0.025)	(0.019)	(0.017)	(0.013)				
Academic Credit Hours:								
Total Attempted	0.125	-0.644	1.219	1.240	-0.359	1.483	-3.187	-2.698
	(1.501)	(2.049)	(1.017)	(1.391)	(4.288)	(4.690)	(2.614)	(3.053)
First Math Course:								
Attempt Course	0.002	0.030	-0.012	-0.005	-0.006	-0.070	0.031	0.012
	(0.014)	(0.018)	(0.006)	(0.011)	(0.041)	(0.042)	(0.016)	(0.023)
Pass Course	-0.009	0.018	-0.021	-0.008	0.026	-0.042	0.054	0.017
	(0.013)	(0.015)	(0.007)	(0.009)	(0.038)	(0.036)	(0.017)	(0.019)
Grade Conditional on	-0.166	-0.060	-0.127	-0.004	0.527	0.151	0.369	0.010
Attempting Course*	(0.029)	(0.037)	(0.029)	(0.038)	(0.096)	(0.093)	(0.082)	(0.093)
College Attainment:								
At Least 1 Year	-0.001	-0.002	0.007	0.010	0.003	0.004	-0.019	-0.022
	(0.013)	(0.015)	(0.009)	(0.011)	(0.037)	(0.034)	(0.022)	(0.024)
Graduate Within 6 Years	0.004	-0.018	0.011	0.000	-0.011	0.042	-0.030	0.000
	(0.011)	(0.017)	(0.008)	(0.011)	(0.031)	(0.038)	(0.021)	(0.025)
	Reading Remediation							
Reading Remediation	-0.311	-0.332	-0.277	-0.309				
	(0.014)	(0.029)	(0.013)	(0.021)				
Academic Credit Hours:								
Total Attempted	-0.961	1.489	0.138	0.945	3.096	-4.479	-0.497	-3.058
	(2.800)	(3.551)	(1.751)	(2.155)	(9.015)	(10.514)	(6.324)	(6.909)
First Math Course:								
Attempt Course	-0.056	-0.064	0.000	0.034	0.181	0.192	0.001	-0.110
	(0.038)	(0.046)	(0.011)	(0.012)	(0.122)	(0.134)	(0.038)	(0.038)
Pass Course	-0.041	-0.059	0.008	0.015	0.132	0.178	-0.027	-0.049
	(0.036)	(0.042)	(0.012)	(0.013)	(0.114)	(0.123)	(0.042)	(0.041)
Grade Conditional on	0.107	0.053	0.075	0.003	-0.349	-0.159	-0.275	-0.010
Attempting Course*	(0.038)	(0.044)	(0.043)	(0.043)	(0.122)	(0.125)	(0.153)	(0.137)
College Attainment:								
At Least 1 Year	-0.029	0.016	-0.023	0.007	0.092	-0.050	0.081	-0.023
	(0.025)	(0.024)	(0.017)	(0.016)	(0.080)	(0.070)	(0.063)	(0.052)
Graduate Within 6 Years	0.001	0.017	0.006	0.004	-0.004	-0.050	-0.021	-0.013
	(0.017)	(0.028)	(0.012)	(0.017)	(0.056)	(0.083)	(0.043)	(0.055)

Notes: Sample is limited to students scoring within 10 scale score points of the passing cutoff. Cell entries in first four columns are estimated discontinuities for a given outcome. IV estimates of the effect of remediation are reported in remaining columns. Regression discontinuity and IV estimates use the math test score for the "Math Remediation" panel and reading test scores for the "Reading Remediation" panel. The "treatment" for the IV estimates is remediation in math for the "Math Remediation" panel and remediation in reading for the "Reading Remediation" panel. Standard errors adjusted for clustering at the test score level in parentheses. See text for additional details concerning regression specification. *-Additional sample restrictions made for these outcomes (see text for details).