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Zeyu Xu
Ben Backes
Dan Goldhaber

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Zeyu Xu
American Institutes for Research/CALDER

Ben Backes
American Institutes for Research/CALDER

Dan Goldhaber
American Institutes for Research/CALDER
University of Washington/CEDR

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CALDER • American Institutes for Research
1400 Crystal Drive 10th Floor, Arlington, VA 22202
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Abstract

A number of school districts and states have implemented transition intervention programs designed to help high school students graduate ready for college. This study estimates the effectiveness of a transition program implemented statewide in Kentucky for high school seniors called Targeted Interventions (TI). Using 11 years of linked panel data, this study tracks the college progression of seven cohorts of students as they move from high school into college. Using a difference-in-regression discontinuity design, we estimate the program's impact on college credit attainment and transfer as well as the extent to which the program has helped reshape pathways through college. We find that the TI program significantly increased the likelihood that students would take at least 15 credits during the first term in college, a key measure that has been shown to be predictive of college completion. These early effects, however, do not translate into statistically significant impacts on the likelihood of transfers from a 2-year to a 4-year college, or the likelihood of earning enough credits to graduate from college. We discuss some possible explanations for why the TI program did not lead to observable improvements in college transfer or credit accumulation.

1. Introduction

Many students who enroll in college do not earn a degree. Among first-time, full-time college students, only 30% graduate from 2-year colleges within 3 years of initial enrollment and only 60% graduate from 4-year colleges within 6 years.[1] Weak pre-college preparation is thought to be one of the main reasons for poor attainment (Bettinger et al, 2013). Traditionally, underprepared entering college students were placed in developmental education (DE) in college before they were able to enroll in college-level courses. However, DE has been found to be both costly and ineffective (e.g., Boatman & Long, 2018; Valentine, Konstantopoulos, & Goldrick-Rab, 2017), and some scholars and administrators consider getting students ready for college a shared responsibility between high school and college (Barnett, Fay, Pheatt, & Trimble, 2016).[2]

In response, an increasing number of states have implemented transition intervention programs for high school students designed to help them graduate high school ready for college. Between 2013 and 2017, the number of statewide programs more than doubled, up to 17, with another 22 states offering local transition intervention programs (Barnett, Chavarín, & Griffin, 2018). Transition intervention programs vary in scope, placement mechanism, delivery format, and whether successful completion automatically advances students to college-level coursework, but most include a screening assessment for college readiness, a transition curriculum for those deemed to need the intervention, and an exit evaluation (Fay, Barnett, & Chavarín, 2017).

But while high school-to-college transition intervention programs are viewed (e.g., Barnett, Fay, & Pheatt, 2016) as holding the promise of improving student college preparedness, there is limited evidence (we describe this below) on the effectiveness of this type of

[1] See https://nces.ed.gov/programs/raceindicators/indicator_RED.asp.
[2] Additional comments can be found in https://hechingerreport.org/solution-obvious-rare-making-high-school-graduates-ready-college/ and https://www.edweek.org/teaching-learning/transitional-courses-catch-on-as-college-prep-strategy/2014/02.

intervention, with existing findings mixed at best. In this paper, we explore a statewide transition intervention program in Kentucky—Targeted Interventions (TI)—and its impact on longer term college outcomes. Kentucky is an especially interesting state to investigate because there is evidence (Xu et al., forthcoming) that the Kentucky TI program has reduced the need for DE of initially low-achieving students who attend college in the state. To our knowledge, this is the first study to explore longer term college outcomes, including the likelihood of transferring from a 2-year to a 4-year college and obtaining enough credits to earn a college degree.

We find that TI significantly increased the likelihood that students would take at least 15 credits during the first term in college—a key measure of early college progress that has been shown to be predictive of college completion (e.g., Attewell & Monaghan, 2016). But we find little evidence that these initial effects led to detectible improvements in measured progress through college, and TI did not have a statistically significant effect on the likelihood of transfers from a 2-year to a 4-year college or the likelihood of completing the minimum number of credits required for graduation (60 credits for 2-year college and 120 credits for 4-year college) within 150% of the normal time for completion (i.e., 3 years for those who start in a 2-year college and 6 years for those who start in a 4-year college).

In what follows, we first summarize existing evidence about transition intervention in general and describe the design and implementation of Kentucky's TI program. This is followed by a brief description of the data, samples, and research method. The impact of TI on various student outcomes is reported in Section 4, which is followed by a discussion of why TI did not lead to observable improvements in longer-term credit accumulation and transfer.

2. Transition Intervention and Its Implementation in Kentucky

2.1. Theory and Existing Evidence

Two thirds of entering 2-year college students take at least one developmental education course, suggesting that completing a high school diploma does not necessarily adequately prepare a student for college (Chen, 2016; Bailey & Jaggars, 2016). Transition interventions are thought to bridge the disconnect between high school and college by using college placement tests and benchmarks to align expectations across education levels and to identify high school students for supplemental support (Fay et al., 2017). Transition interventions are often designed to follow the Response to Intervention (RTI) framework (Fuchs & Fuchs, 2006), using assessment data to identify specific areas of deficiency, inform instruction, and monitor progress. In doing so, transition interventions can potentially help students by targeting specific areas of need.

In addition to Kentucky, state-level transition intervention programs have been evaluated in West Virginia (Pheatt, Trimble, & Barnett, 2016), Florida (Mokher, Leeds, & Harris, 2018), and Tennessee (Kane et al., 2021). Findings in those states, all based on regression discontinuity (RD) designs, are less promising than in Kentucky. In all three states, transition interventions produced either no significant effect (Florida and Tennessee) or a negative effect (West Virginia) on students' likelihood of passing introductory college courses (a key college graduation requirement). In Tennessee, the transition intervention produced an initial effect of a 30% reduction in developmental math enrollment. But the effect was not the result of improved math skills, as the authors demonstrated using a post-intervention assessment, but due to automatic exemption from DE in college upon successful completion of a transition curriculum in high school.[3] In West Virginia and Florida, transition interventions had no detectable impact on DE enrollment.

2.2. Targeted Transitional Interventions (TI) in Kentucky

In 2009, the Kentucky General Assembly found "the continuing high rates of high school students who require remediation at the postsecondary education level totally unacceptable and an unwarranted additional expense to the state."[4] As a result, the state passed legislation intended to better align secondary and college education, including the establishment of the diagnostic cycle that would become TI. The postsecondary and secondary levels were charged to "develop a unified strategy to reduce college remediation rates by at least fifty percent by 2014 … and increase the college completion rates of students enrolled in one or more remedial classes by three percent (3%) annually from 2009 to 2014."

In terms of reducing the number of students enrolled in college remediation, the program appears to have partially achieved this goal by reducing the need for DE. Specifically, Xu, Backes, Oliveira, and Goldhaber (forthcoming) found that TI reduced the likelihood that students would enroll in college DE by 8–10 percentage points in math in both 2- and 4-year institutions among students who just missed the college-ready benchmark in 11th grade. TI also increased the rate at which these students pass introductory college math within the first year of college by four percentage points in 4-year institutions. These effects, roughly equivalent to 0.10–0.20 standard deviations, are sizeable relative to the cost of the program, estimated to be about $600 per student (Levin et al., 2020). These positive effects are even stronger among free/reduced-

[3] The Tennessee program automatically exempting students from DE in college after successful completion is a common practice across transition intervention programs (Fay et al., 2017). Kentucky's TI is one of only five (out of 17 statewide programs) in which successful completion of a transition intervention does not automatically place students out of DE in college.

[4] https://web.archive.org/web/20120113051012/http://cpe.ky.gov/NR/rdonlyres/09913AFD-9097-4374-95A1-10C96060D134/0/SenateBill1_2009RegularSession.pdf

price lunch–eligible students, reducing enrollment in developmental math courses by 11 percentage points and increasing the rate of passing college math by 9 percentage points by the end of the first year in college.

The TI program was first implemented for high school math in 2010–11 and high school English in 2011–12. Using test scores from the ACT taken by all 11th grade students in Kentucky public schools, TI uses predetermined cutoffs (19 for math and 18 for English) to identify students who may not be on track to be ready for college at the end of high school, and provides supplemental interventions to help those students meet college readiness expectations. By these standards, 59% of students were deemed not on track to be ready for college-level math and 43% were not on track to be ready for college English (Xu et al., forthcoming).[5]

Detailed student-level intervention data were not collected until the 2013–14 cohort of 11th grade students. These data, supplemented with interviews with program administrators and teachers conducted by Xu et al. (forthcoming), provide information on the format of TI delivery. Interventions were primarily delivered in the form of transition courses (65%) and extended school services (22%). These transition courses could either be a standalone course or integrated with other existing courses. On average, intervention services were provided four times a week, with each session lasting 55 minutes. Most intervention teachers used a combination of self-developed materials and online curricula such as ALEX, Dreambox, Edgenuity, and IXL.

[5] The same ACT math cutoff is used in Tennessee, where about half of students score below the cutoff. West Virginia uses its own state test to place students, and while Pheatt et al. (2016) do not report the share of students falling below the cutoff in math, a visual inspection of their density plot (Figure 3) shows well under half of students meeting the benchmark.

3. Data, Measures, and Methods

3.1. Data

In this study, we utilize individual-level administrative records provided by the Kentucky Department of Education and Kentucky Council on Postsecondary Education. We focus on seven cohorts of students who were high school juniors between the 2008–09 and 2014–15 school years. These cohorts span both pre- and post-treatment periods, with pre-treatment cohorts represented by the earliest two cohorts for math TI and the earliest three for English TI (because English TI was implemented one year before math). Each cohort consists of about 43,000 students. With 11 years of linked panel data, this study can track student progress through college until 3 years after initial enrollment in a 2-year college for all seven cohorts of high school juniors and 6 years after initial enrollment among 4-year college students for four cohorts of high school juniors (2008–09 through 2011–12).

High school data include ACT scores from the mandatory statewide administration in the spring of the 11th grade, which were used by TI to refer students for supplemental services, as well as student gender, race/ethnicity, free/reduced-price lunch (FRL) eligibility, and high school completion status. Postsecondary data cover all enrollments in Kentucky institutions (both public and private).[6] These data include records on course enrollment, grades, credits attempted and earned, programs of study, transfers, and completion.

As summarized in Xu et al. (forthcoming) and reproduced as Table 1, the study population is predominantly white (85%), with an FRL eligibility rate of close to 50%. The average ACT score is 18.8 in math and 18.5 in English. Over 90% of 11th grade students with a

[6] Less than 3 percent of students with ACT scores 2–3 points around the cutoffs enrolled in out-of-state colleges, and there is no discontinuity in the likelihood of leaving the state at the cutoffs.

valid ACT score graduated high school on time. The rate of college enrollment immediately after high school graduation varied by ACT score, ranging from about 50% among students scoring three points below the TI cutoffs to about 65% among students scoring three points above the cutoffs.

3.2. Measures

The key measure of success for this study is earning the minimum number of credits required to graduate from college. While we are able to track student transfers throughout the study period, reliable degree completion records are not available before 2014. As a result, we rely on credit accumulation as a proxy measure of college completion. Specifically, we calculate whether a student has accumulated at least 120 credits in a 4-year college or at least 60 credits in a 2-year college as an indicator of students' likely college completion status. These thresholds are based on a review of graduation requirements in academic catalogues published during the study period, and represent the minimum thresholds below which degree completion is unlikely. Students typically need to satisfy additional requirements such as minimum GPA and the completion of core credits to graduate, and some Associate of Applied Science degree programs require up to 68 credits. Because about 80% of students who enroll in 2-year institutions intend to eventually earn a bachelor's degree, we also measure transfer from a 2-year to a 4-year institution as an outcome.[7]

In order to evaluate how well credit accumulation thresholds approximate actual degree completion, we verify the relationship between credit accumulation and degree completion using available completion data from 2014 and later. As expected, falling short of the threshold almost perfectly predicts incompletion. However, earning more credits than the thresholds does not

[7] https://ccrc.tc.columbia.edu/Community-College-FAQs.html

always guarantee graduation. For example, 99% of 2-year college students who did not accumulate 60 credits 3 years after initial enrollment failed to attain any degree. On the other hand, 34% of students who had earned at least 60 credits did not graduate or transfer to a 4-year institution.

Measures of progress at critical junctures along the pathway through college can also help pinpoint where students may need more help. For example, research shows that many students start college not knowing what to study (Bettinger et al., 2013) and switch programs excessively in subsequent terms (Holzer & Xu, 2021). In Kentucky 2-year colleges, for example, 27% of first-time freshman under the age of 24 enroll without a declared major; among those who have declared a major at the start, about 20–30% switch among six broadly defined programs of study (STEM, health, business, liberal arts/social sciences, other occupational fields, and certificate/diploma programs) (Holzer & Xu, 2021). Some switches may be made as students gain more information about the field and about themselves. But there is an opportunity cost, and uncertainty about what field to pursue may explain why one third of 2-year college students in our data completed more credits than required without graduating or transferring to a 4-year college. Transition interventions like TI have the potential to improve the efficiency of program choice by exposing students to college-level content and expectations. We thus measure the likelihood of beginning college without a declared major and the number of times a student switches programs in their first year of enrollment.

Empirical evidence also suggests that carrying an adequate course load early on is a strong predictor of college completion and successful transfer from a 2-year institution to a 4-year college (e.g., Adelman, 2006; Attewell, Heil, & Reisel, 2011). National and college administrative data both suggest that a 15-credit course load during the first semester is a critical threshold that has the strongest predictive power of college attainment (Attwell & Monaghan, 2016; Belfield, Jenkins, & Lahr, 2016; Klempin, 2014). Students attempting at least 15 credits during the first semester are 9 percentage points more likely to graduate (Attewell et al., 2016), while paying 4–14% less per credit and 9–19% less per degree in tuition and fees (Belfield et al., 2016). TI significantly reduced the need for DE in college, and we investigate whether this has led to an increase in overall course load by measuring the impact of TI on the likelihood that students take at least 15 credits during the first term in college.

3.3. Research Methods

The use of predefined ACT cut scores for TI assignment lends itself to a regression discontinuity (RD) design. However, the use of these cut scores in an RD framework is complicated by these same cut scores being used at the postsecondary level for assignment to developmental courses upon arrival to college. Therefore, following Xu et al. (forthcoming), we use a difference-in-regression discontinuity (DiRD) method to estimate the effect of TI. The crucial assumption of the DiRD strategy is that college placement policies or any other factors which affect the outcomes of students just above and below the cutoffs remained unchanged over time for the 11th grade cohorts under consideration. Under this assumption, DiRD uses pre-TI cohorts to net out the college placement policy effect in order to isolate the TI effect. Specifically, we estimate the RD effect of falling just below the ACT cutoff for the pre- and post-TI periods using

$$Y_i = \alpha_0 + \alpha_1 B_i + k(S_i) + \alpha_2 B_i * k(S_i) + \varepsilon_i, \qquad (1)$$

where Y_i is the outcome for student *i* and *k(.)* is a function of the ACT score of student *i*, S_i, that is centered around the cutoff (19 for math and 18 for English) such that negative values indicate scores below cutoff. B_i is an indicator for whether student i's score falls below the cutoff. The DiRD estimate is then the difference in α_1 between the post period and the pre period:

$$\hat{\beta} = \widehat{\alpha_1}^{post} - \widehat{\alpha_1}^{pre} \quad (2)$$

As shown in Figure 1, the discontinuity of TI participation is not sharp near the cutoff. A large percentage of students who scored below the ACT cutoffs did not participate in TI in a given subject ("no shows"), and many students who scored above the cutoffs did participate ("crossovers").[8] The estimated discontinuity in program participation around the cutoff is about 40 percentage points in both subjects. Therefore, $\hat{\beta}$ estimates the intent-to-treat (ITT) effect of TI on student college outcomes. Under the assumption that the introduction of TI was the only policy change that relied on these specific ACT cut points during the study period, DiRD produces an unbiased estimate of the TI effect on student outcomes.

Our data include integer values of ACT scores and should thus be viewed as discrete rather than continuous. Due to this discreteness in the running variable—each point of difference on the ACT is equivalent to 0.17–0.20 standard deviations—we are unable to get arbitrarily close to the cutoff and have to rely on parametric assumptions about the relationship between the outcome and the ACT score near the cutoff, incurring additional uncertainty about impact estimates. As a result, we report Eicker–Huber–White (EHW) standard errors for impact estimates following McCaffrey and Bell (2002) and Kolesár and Rothe (2018). In addition, DiRD mitigates the uncertainty associated with parametric assumptions to the extent that the parametric relationship between ACT test scores and student outcomes remains unchanged before and after TI took effect.

[8] Surveys and interviews suggest several explanations for noncompliance (Xu et al., forthcoming). Teachers could consider other performance metrics in addition to ACT scores when referring students to TI. Students also had the option in many schools to test out of TI. And in some cases, staff and parental resistance to remediation also played a role in under-participation in TI.

Using the bandwidth selection procedure implemented by Calonico et al. (2020), we obtain optimal bandwidths for each college outcome, subject, and institution type. In line with prior RD studies that use ACT scores as a running variable (e.g., Boatman, 2012; Ran & Lin, 2019), optimal bandwidths range mostly between two and five points. We also investigate the robustness of key findings to different bandwidths and find that the choice of bandwidth is generally not substantively important. Finally, given the small bandwidths, $k(S_i)$ is entered into equation (1) as a linear function of test scores.

3.4. Validity

In addition to the usual assumptions of an RD—the existence of significant discontinuity in treatment receipt at the cutoff, the integrity of the running variable, no differential sample attrition at the cutoff—DiRD also assumes that the discontinuity in student outcomes at the cutoff due to college placement policies would have remained constant throughout the study period in the absence of TI. As demonstrated in Xu et al. (forthcoming), which uses the same identification approach we rely on here, there is no evidence that any of these assumptions are violated. The discontinuity in TI participation around ACT cutoffs is about 40 percentage points for both subjects. Manipulation of ACT scores is unlikely since teachers and schools were not involved in scoring and cutoffs were predetermined. We also use scores from the spring of the 11th grade, when students took the tests for the first time, to avoid potential self-selection issues stemming from test retakes. As expected, the distribution of ACT scores shows no signs of lumpiness through the cutoff.

Consistent with the integrity of the running variable, key baseline covariates are continuous at the cutoff. Using student gender, race/ethnicity, and FRL eligibility as the outcome, Xu et al. (forthcoming) estimated the DiRD model as described above and found no detectable discontinuity at the cutoff except for the percentage of female students, where the difference is equivalent to about 0.10 standard deviation; this table is reproduced as Table 2 here. Controlling for gender (as well as other covariates) in the DiRD model produces no meaningful change in estimated TI effects on college outcomes.

As a further test for the DiRD assumption that the college placement policy effect would have remained constant in the absence of TI, Xu et al. (forthcoming) conducted a falsification test by estimating TI "effects" away from the actual cutoff. Like the math cutoff of 19, a cutoff of 22 has also been used by colleges in Kentucky to place students in college math courses (albeit at a higher level). Unlike the cutoff of 19, the cutoff of 22 was unrelated to TI assignment. In the absence of TI, discontinuities in student outcomes around the cutoff of 22—due to college placement policies—remain constant before and after the implementation of TI, and the estimated DiRD "effects" are null.[9]

4. Findings

The estimated TI effects are presented in Table 3. The rows of the table represent different college outcomes, with Panel A consisting of early college outcomes and Panel B end-of-college outcomes. The estimated TI impact on each college outcome is reported by subject and college type as columns. For each unique regression, we show the estimated TI impact, its associated EHW standard error, optimal bandwidth, and sample size. In Table 4 we show the sensitivity of the estimates provided in Table 3 to different bandwidths and present separate pre- and post-intervention RD estimates of the effect of failing ACT cutoffs.

[9] The results in this paper are primarily obtained from the sample of students who enrolled in college. Xu et al. (forthcoming) use the DiRD model to examine the effect of TI on college enrollment and finds no significant effect on enrolling in either a 2- or 4-year college. This suggests that any TI effects on college outcomes are not achieved by altering the composition of college-going students.

4.1. Early college outcomes

As noted in Section 3.2 above, there are reasons to believe that TI might affect various early college pursuits and progress towards a degree. Estimates reported in Table 3 suggest that TI has marginal effects on some types of program choices. These effects are not consistent across subject or institution type, and some are sensitive to bandwidth choices. The findings that are consistent across bandwidth choices (Table 4) are that TI in math reduces the likelihood that 2-year college students start in a health field by 3 percentage points, and that TI in English increases the likelihood that 4-year college students start in a STEM field by 3 percentage points. TI in either subject has no effect on the frequency of program switches within the first year or first 3 years in college.

During the first term, TI in math increases the likelihood of students enrolling in at least 15 credits by 6 percentage points in 2-year colleges and 5 percentage points in 4-year colleges (Table 3). This is consistent with the reduced need for developmental education reported in Xu et al. (forthcoming), suggesting that students used the freed-up time early in their college journeys to take more college-level courses. Figure 2, which plots select college outcomes against ACT scores for the pre-treatment (circles) and post-treatment (triangles) periods, shows that the positive impact is achieved by eliminating the gap between students scoring just above and below the ACT cutoff: Before the implementation of TI, due to the same TI cutoff also being used to place students into developmental education in college, students scoring just below the ACT cutoff were significantly less likely than students scoring just above the cutoff to enroll in at least 15 credits during the first term (estimates are presented in Table 3). In the post-implementation period, by contrast, the gap in first-term course load was mostly eliminated. Results from Table 2 also show that this finding is robust to alternate bandwidths. TI in English has weaker effects on first-term course load, increasing the likelihood of students attempting at least 15 credits by 4 percentage points in 4-year colleges but having no significant impact in 2-year colleges.[10]

4.2. Long Term Credit Accumulation and Transfer

To assess whether the above impacts of TI on early college choices and progression led to changes in longer term college outcomes, we examine total credits earned and the likelihood of earning at least 60 credits at the end of 3 years in college or at least 120 credits after 6 years in a 4-year college (Table 3, Panel B). TI has no detectable effect on any of these measures. Among 4-year college students, those who were subject to math TI appear to have completed close to one additional course (2.3 credits) more than students who were not subject to math TI by the end of 3 or 6 years in college, but these are imprecisely estimated. Results in Table 3 suggest that TI has no measurable impact on helping students meet the minimum credit requirement for graduation. TI also has no detectable effect on the likelihood that 2-year college students transfer to a 4-year institution.

Figure 3 depicts the trajectory of credit accumulation by term. Cumulative credits are measured at the end of each fall, spring, and summer term for the first eight terms. Trajectories are plotted for students scoring within three points above and below the cutoff for cohorts before and after the implementation of TI separately. Gaps in credit accumulation between higher and lower scoring students emerge at the end of the first term. The higher scoring group also appears to earn credits at a faster rate, resulting in a steadily widening gap in completed credits over the course of 3 years. Notably, the gap between the higher and lower scoring groups at each point in

[10] In results available from the authors, we find that TI in math is associated with attempting more STEM credits in the first year, which is likely a consequence of being more likely to take a credit-bearing course in math instead of developmental math. However, the impact on total number of STEM credits accumulated by the end of the first year is small and not statistically significant.

time appears to remain unchanged before and after TI implementation. In other words, the lack of TI effect on accumulated credits at the end of college is not because early gains dissipated later. Rather, positive TI effects on early progress did not result in discernible gains in college progression early on.

4.3. Subgroup analysis

We investigate whether TI has similar effects on college outcomes across three student subgroups that are thought to face unique challenges in college. FRL-eligible students, for instance, may be more sensitive to the labeling effect as not being ready for college (Mokher et al., 2018). Students who missed ACT cutoffs in more than one subject and therefore were subject to intervention in multiple subjects[11] could be overwhelmed by the burden of interventions, which in turn could undermine the effectiveness of TI. Finally, high schools with a high percentage of students who need transitional intervention[12] may be overburdened by the prevalence of a need for intervention; students enrolled in these high schools may receive interventions that are less effective.

Estimated DiRD effects of TI on college outcomes, grouped by student population, are reported in Table 4. They largely mirror findings among the overall student population. Math TI improves the likelihood of students taking a 15-credit course load in the first term by 6–13 percentage points in 2-year colleges for all student subgroups. English TI has a similar effect on first-term course load among FRL-eligible students and students from high-need schools who later enrolled in 4-year colleges. However, math TI's effect on early momentum is no longer significant among these student groups in 4-year colleges, which is partially due to smaller

[11] Half of students who failed to meet the math benchmark also participated in English remediation, and 76% of students who failed to meet the English benchmark received remediation in math.
[12] Schools in the top quarter have at least 69% of their students missing the ACT cutoff in math and 54% in English.

sample sizes and the loss of statistical power. TI also appears to reduce the likelihood that 2-year college students start with short-term diploma or certificate programs by 9 percentage points for FRL-eligible students and 14 percentage points for students from high-need schools. We find no detectable TI effect on any other college outcomes.

5. Further Exploration

The direct goal of TI and other types of transition interventions is to reduce the need for DE in college. Xu et al. (forthcoming) find evidence that TI has reduced developmental course taking, and we show here that TI also affects other early college outcomes, including first-year course load. However, we do not find evidence that these initial gains put students on an accelerated trajectory towards long-term credit accumulation or successful transfer from 2- to 4-year colleges. In this section, we explore two reasons that might explain the lack of statistical significance for long-term college outcomes despite evidence that TI decreases the likelihood that students are deemed to need development (non-credit-bearing courses) in college.

The first potential explanation is that intervention activities were no more beneficial for long-run college success than the regular high school courses they crowded out. Under this hypothesis, the only channel through which TI would affect long-run college outcomes would be by moving some students out of developmental courses and into introductory courses early in their college paths, but not by increasing overall skills associated with college progression. Tabulations using transcript data show that the most frequently taken math courses by students not subject to TI were Algebra II, Algebra III, and College Algebra. Taking Algebra II or higher is considered a critical precollegiate milestone for eventually earning a college degree (Adelman, 2006). By contrast, student-level intervention data show that math TI most frequently focused on *algebraic thinking* and *math reasoning*. Although the definition of these content areas is unknown, similar terms were also used in course descriptions for developmental math courses in college.[13] This course substitution—in contrast to, for example, increasing total learning time—may impose an upper limit on the plausible long-run college impacts of TI. We explore this mechanism in three different ways: by measuring crowd-out of high school courses directly (Section 5.1), by examining college outcomes for a subset of successful TI completers (Section 5.2), and by examining a similar intervention earlier in secondary school (Section 5.3).

The second explanation is that given the effect size on early college outcomes and the strength of the relationship between early college outcomes and later college outcomes estimated in previous studies, the expected long-run effects of TI are quite small. Thus, we do not have the power to detect the expected long-run effects of TI. We provide this discussion in Section 5.4.

5.1. Crowd-Out of Other High School Courses

We begin by investigating the extent to which TI may have crowded out regular high school courses. Although some TI students received interventions as extended school services, most interventions were delivered as transition courses during the normal school day. Time students spend on transition interventions during the normal school day implies less time spent on what students otherwise would have been doing.

To investigate how course-taking changed after the introduction of TI for students below the cutoff relative to above the cutoff, we divide courses into four categories based on coding conventions stipulated by the Kentucky Uniform Academic Course Codes: transition courses in math or English, regular non-transition courses in math or English, career technical education

[13] See, for example, the course description for Mathematical Literacy (MAT 075) in the academic catalogue of the Kentucky Community and Technical College System.

(CTE) courses, and other non-transition courses.[14] Results are presented in Table 6 in the same format as in Tables 3 and 5, with each coefficient representing the DiRD estimate on a given outcome for a given subject. As expected, we see that the introduction of TI led students below college readiness benchmarks to take more transition math courses by 12 percentage points and more transition English courses by 2 percentage points. For math TI, we also see evidence that the increase in transition math course-taking largely came at the expense of regular math courses (an 8-percentage-point drop in enrollment) rather than CTE courses or other courses, although the latter is very imprecisely estimated. For English, we see smaller reallocation effects.

5.2. Substitution of Curriculum

As another test of whether TI content was geared more toward the skills needed to avoid DE in college than toward the more advanced skills needed for later progression through college, we examine the relationship between successful TI completion and later college outcomes. In particular, we estimate the difference in college outcome *Y* between successful completers of TI (*S_TI*) and students who failed to complete TI (*F_TI*) relative to students who did not receive any intervention, the reference group, in the following OLS regression:

$$Y_i = \beta_0 + \beta_1 S\_TI_i + \beta_2 F\_TI_i + COV * \Theta + S_s + u_i. \qquad (3)$$

Equation (3) also controls for ACT score fixed effects *S* and a vector of covariates COV that include student race/ethnicity, gender, FRL eligibility, and whether a student was eligible for TI in more than one subject.

Estimates reported in Table 7 show that among students with the same ACT score and similar background characteristics, successful completion of TI is associated with lower

[14] We examine CTE course-taking as a separate course category because low-achieving students are more likely to focus on CTE, so there is significant overlap between the target student population of transition interventions and students who are likely to benefit from CTE.

likelihood of DE enrollment, but not with other measures of progress later on (i.e., the likelihood of passing credit-bearing college courses during the first year, taking at least 15 credits in the first term, or completing at least 60 credits by the end of three years). Unsuccessful exit from TI, on the other hand, is associated with higher likelihood of DE enrollment and weaker college outcomes. These findings are not causal due to selection into TI and into completion of TI, but taken together, they suggest that the standards used by high schools to judge student progress towards college readiness are largely consistent with the skills needed to place out of developmental courses, but not sufficient to better prepare students for college-level instruction.

5.3. Intervention and Student Skills

As an additional test for whether TI might be plausibly expected to lead to measurable changes in college-ready skills, we examine the relationship between falling below the Grade 8 cutoff (and thus being discontinuously likely to receive TI) and ACT scores in 11th grade. While this paper focuses on Grade 12 interventions because this is the grade that the TI program has been continuously focusing on, TI was implemented as early as Grade 8 for a short period between 2012–13 and 2014–15. Using these Grade 8 cohorts allows for a direct test of student knowledge gains due to TI that we cannot perform for Grade 12 interventions due to a lack of access to post-intervention test scores. If TI did not lead to an appreciable change in long-run student skills, we might expect to find little relationship between TI in Grades 8–9 and outcomes years later.

Because reporting was not mandatory for interventions in the eighth and ninth grades, we are limited to examining the impact of TI among “complier” districts where reported TI participation is strongly associated with failing the cutoffs.[15] As shown in Figure 4, students in these districts participated in TI discontinuously at the cutoffs. The estimated first stage (Table 8) finds that students scoring just below the cutoffs were 40–60 percentage points more likely than students scoring just above the cutoffs to receive interventions in a given subject.

We use five cohorts of eighth grade students to estimate the ITT effect using the same DiRD model as in our main analysis, with 2014–15 representing the post-intervention period and 2009–2012 the pre-intervention period. Results are shown in Table 9. While estimates for some outcomes are imprecise due to the limitations noted above, we do not find any evidence that falling just below the college-readiness threshold in eighth grade led to students "catching up" by meeting ACT benchmarks in 11th grade, or, further down the road, taking credit-bearing math or English courses. In particular, we are able to rule out a positive effect on ACT math scores any larger than 0.80 ACT points (-0.19 + 1.96 * 0.51), and the point estimates on ACT scores and the likelihood of meeting ACT benchmarks are negative. While this is consistent with TI not moving the needle on long-run student outcomes, possibly due to the lack of an increase in overall instruction time, there are two important caveats: First, it is possible that the "complier" districts are unrepresentative of districts in the state. Second, this program was terminated only 3 years later, and it is possible it was not implemented with high fidelity.

5.4. Statistical power

Although Kentucky's transition intervention has strengthened early college outcomes that are shown to be predictive of later attainment, the effects may be too small to produce

[15] We use equation (1) to estimate the discontinuity of reported TI participation around EXPLORE cutoffs district by district. The optimal bandwidth is 3 for math and 4 for English. Districts with an estimated discontinuity of 0.20 and at least 50 students within bandwidth were selected. This results in nine districts for math and six districts for English. We also used alternative criteria for district selection (i.e., a discontinuity of 0.15 and 0.25, with and without sample size restrictions, and alternating the bandwidth), but the findings on earlier intervention's impact on student outcomes did not materially change.

statistically detectable changes in credit accumulation and transfer rates. Take first-term course load as an example. The empirical literature on early progress in college has found that taking 15 or more credits during the first term increases the likelihood of college completion by 9 percentage points (Attewell et al., 2016). Since TI in math is estimated to have increased the likelihood that students take at least 15 credits by 5–6 percentage points, a direct extrapolation suggests that we can expect the average graduation rate to increase by roughly half of a percentage point (i.e., 0.09 x 0.05 ≈ 0.005). Converting the percentage point change to an effect size using Cox index conversion (Sanchez-Meca, Marin-Martinez, & Chacon-Moscoso, 2003):

$$d_{Cox} = \omega \frac{LOR}{1.65},$$

where $\omega = [1 - \frac{3}{4N-9}]$ with N denoting total sample size, $LOR = \ln(Odds_i) - \ln(Odds_j)$, and assuming 30% of students graduate before the intervention, we estimate the expected impact of TI on college graduation rate to be roughly equivalent to 0.01 standard deviations. Given a sample size of 54,000 students within a bandwidth of 3 around the cutoff who are about evenly split between the pre- and post-TI samples, a typical RD design is expected to have a minimum detectable effect size (MDES) of 0.05 standard deviations.[16] The estimated MDES for a DiRD design, assuming independence between the pre- and post-TI samples, would be $\sqrt{2} * 0.05 = 0.07$ standard deviations. Thus, the research design does not have sufficient power to detect a change of 0.01 standard deviations (i.e., a 0.5 percentage point change in graduation rate).

Even if a study were powered to detect a 0.5 percentage point change—which would require at least 2 million observations split between the pre- and post-intervention periods—it

[16] MDES is estimated following Schochet (2008) for a two-tailed test with a significance level of 0.05 and statistical power of 0.80. Estimation assumes the following parameters: Students within a bandwidth of 3 around the cutoff are evenly divided between treatment and control groups. Covariates explain 15 percent of the variation in the outcome variable.

would still raise the question of whether such a relatively small effect is substantively important. The Kentucky Council for Postsecondary Education (CPE) has set out a goal of raising the percentage of Kentuckians with a postsecondary degree from 45.5% in 2017 to 60% in 2030 (CPE, 2019). Thus, even if it led to a 0.5 percentage point increase in the rate at which students earned a postsecondary degree, TI alone would not be sufficient to move the needle towards this college completion goal.

6. Conclusion

In this study, we investigate the potential impact of Kentucky's high school transition intervention program on the likelihood of college completion and transfer as well as on some of the key predictors of college success. The key finding is that the TI program achieved its direct goal by reducing the number of students enrolling in developmental education in college, which in turn allowed more students to build early momentum by taking 15 or more credits during the first term. However, the early benefits of the TI program did not lead to detectable improvements in measured proxies of college attainment.

As discussed in Section 5, one possible explanation for the findings is that time constraints led to the substitution of regular high school courses with intervention activities and as a result, there was no increase in the time that students spent on academic pursuits in high school. Because the immediate mandate for TI was to get students ready to access "credit-bearing coursework without the need for developmental education or supplemental courses" in college (CPE, 2010, p. 7), it is not surprising that TI chose to focus on skills required to place out of DE in college, explaining the changes in early college outcomes. However, the transition curriculum, while helping students avoid the need for college developmental courses, does not appear to have helped a measurable share of students to develop the necessary skills to progress through college relative to what they would have otherwise taken.

Kentucky's strategic agenda for postsecondary education set out a goal of increasing the share of Kentuckians with a postsecondary degree from 44% in 2015 to 60% in 2030.[17] If much of this increase is going to come from students who currently do not appear prepared for college, our evidence suggests that more intensive intervention may be required. Although no direct evidence is available about the relationship between the "dosage" of transition intervention and college outcomes, empirical findings from elementary and middle school grades consistently suggest that increased instruction time has positive effects on student test scores (e.g., Taylor, 2014; Cortes, Goodman, & Nomi, 2015; Figlio, Holden, & Özek, 2018). While we cannot definitively say whether TI's lack of statistically identifiable impacts on longer-run college outcomes is because of substitution among high school courses or because the effect is too small to detect, it appears that high school-to-college transition interventions that do not increase total instruction time have a limited scope for impact on long-run college success.

In addition to expanded opportunities for students to acquire the requisite academic skills for college, another potential way to increase the impact of transition intervention programs is to add support for non-academic college readiness. Students on the margin of being deemed academically ready for college face so many additional challenges that they are unlikely to complete college in the first place: For example, only about 1 in 4 students near the ACT college readiness math cut score obtain 60 credits within 3 years in 2-year institutions (Figure 2). These additional challenges include behavioral, informational, and financial constraints (Page & Scott-Clayton, 2016). These barriers often overlap, and academically vulnerable students could benefit

[17] http://cpe.ky.gov/ourwork/60x30.html

from non-academic support (Karp, 2011). Our analysis shows, for example, that many students found it difficult to decide on what to study in college, and that TI did not reduce either the prevalence of students starting college with an undeclared program of study or excessive program switching in subsequent years. This suggests potential efficiency to be gained if students were able to make informed choices early on, and high school transition interventions could help by providing information on the values of majors in the labor market, personalized advising, and career counseling.

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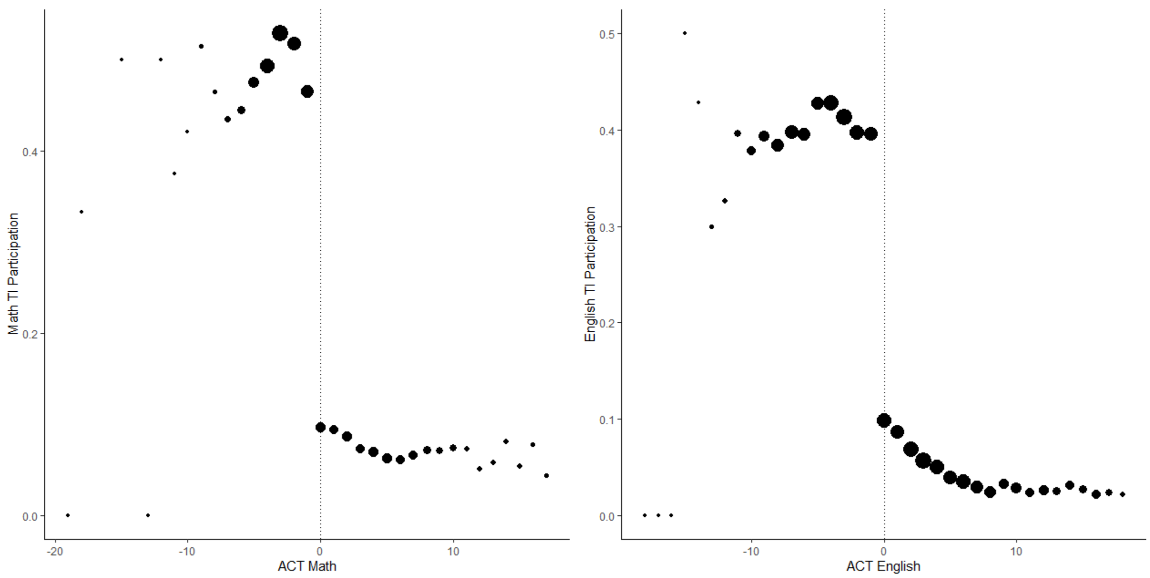
Figures and Tables

Figure 1. Targeted Intervention (TI) Participation Rate by ACT Score, by Subject.



Note: Based on ACT math and English tests taken by high school juniors in 2014, 2015, and 2016. Test scores are centered around 19 for math and 18 for English.

Figure 2. College Outcomes by ACT Test Scores, by Subject and Institution Type

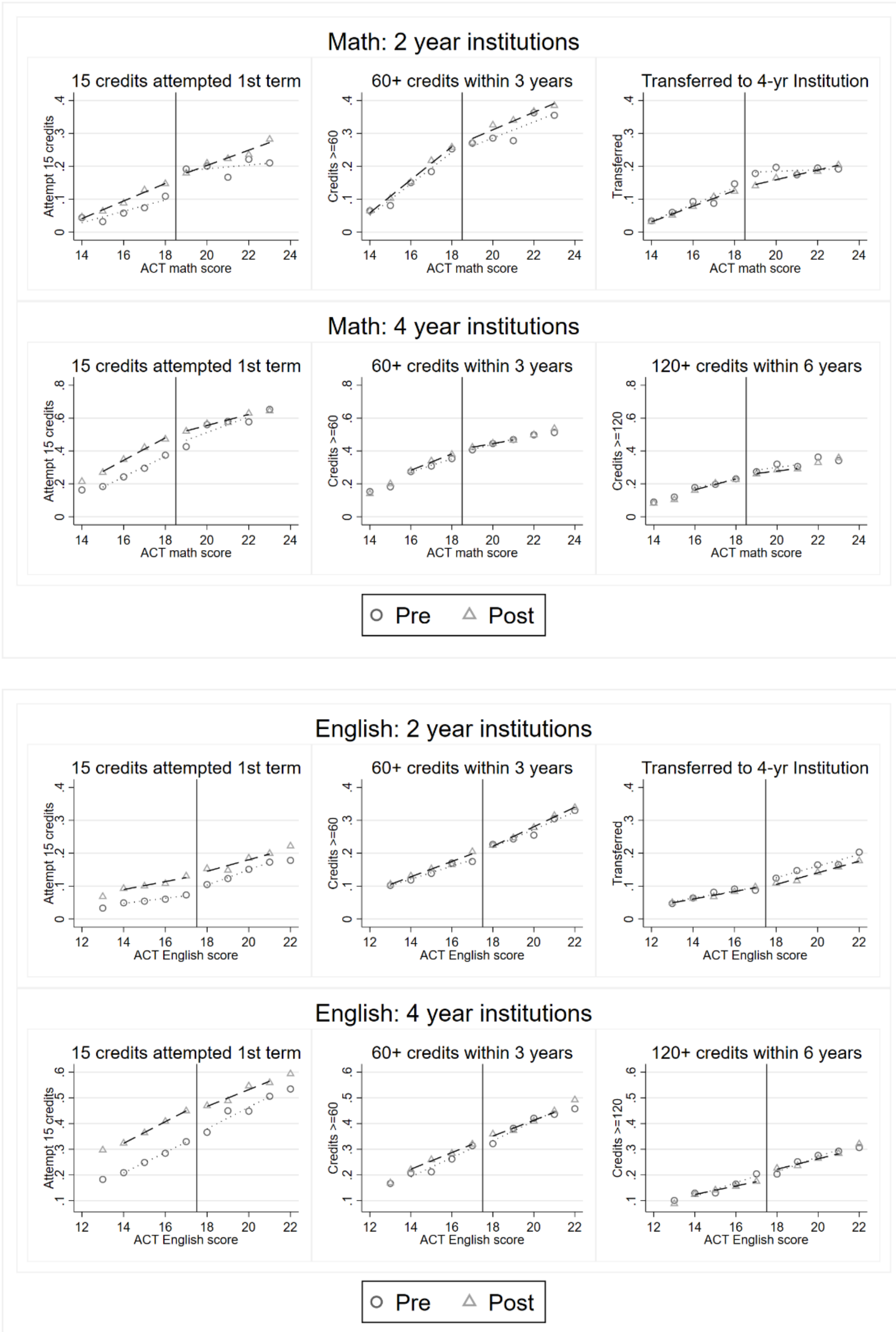
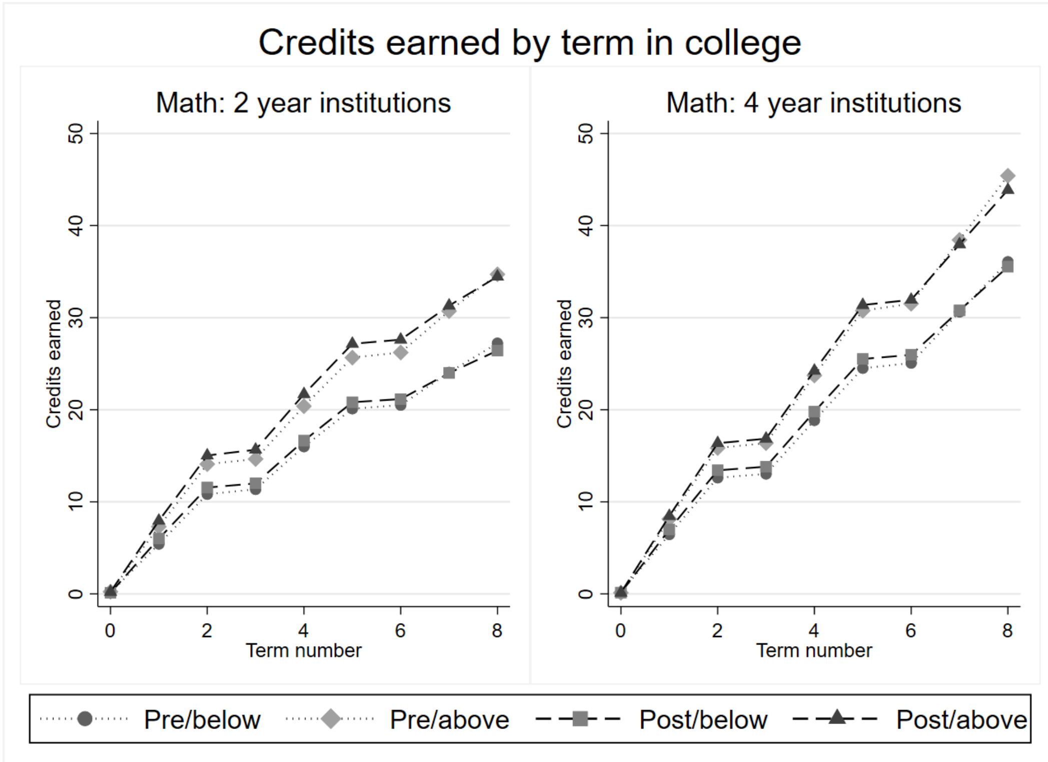


Figure 3. Cumulative Credits Earned by Term



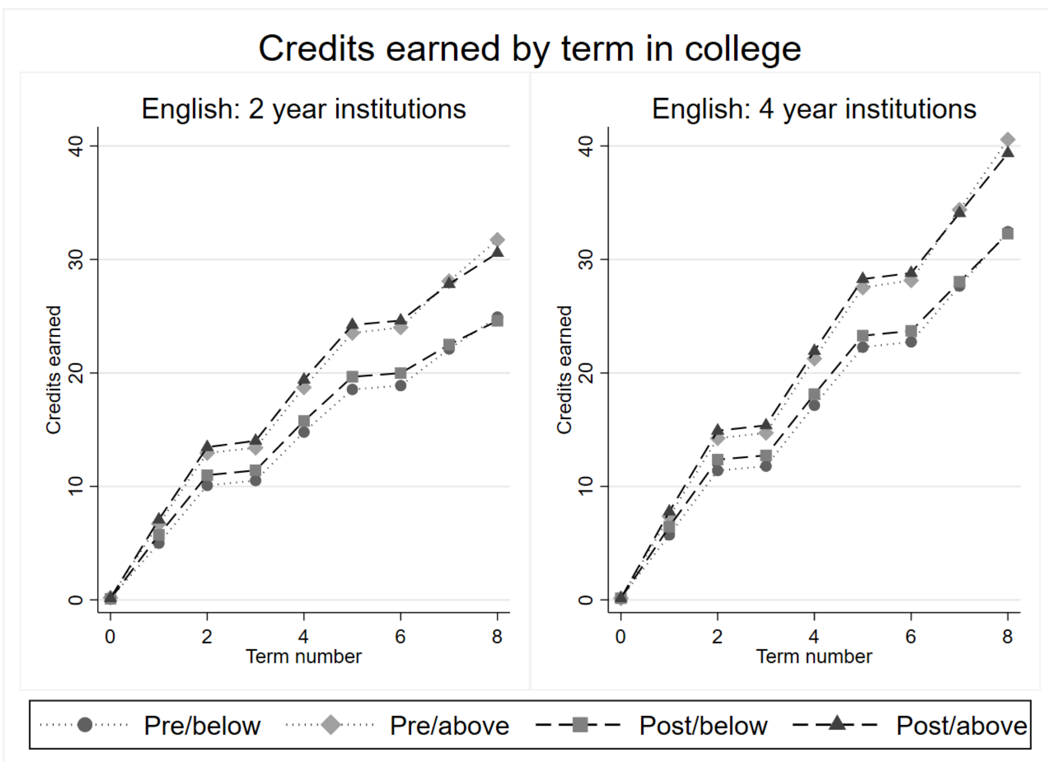


Figure 4. Intervention Participation Rate by Eighth Grade EXPLORE Score, Select School Districts, 2014–15

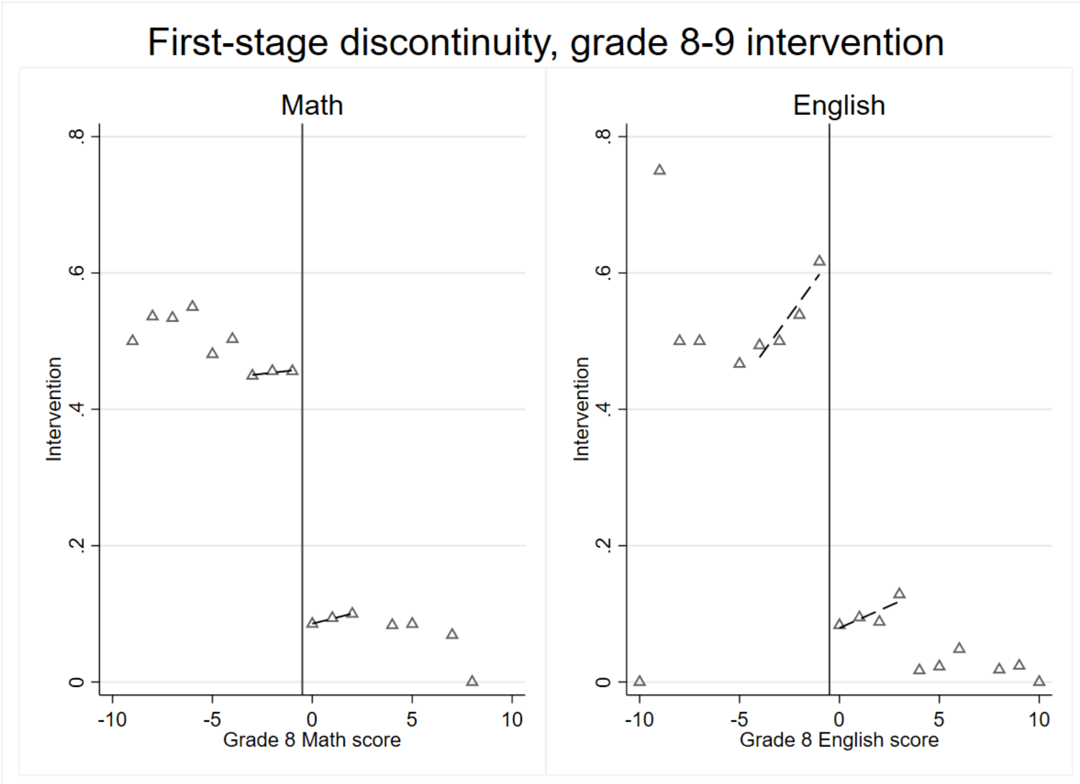


Table 1. Descriptive Statistics of Study Samples: 2009−2016 (reproduced from Xu et al., forthcoming)

	Full Sample	Math RD Sample (ACT-M 16-21)	English RD Sample (ACT-E 15-20)
Demographics (%)			
Female	50	52	51
White	84	85	85
Black	10	10	10
Hispanic	5	5	5
FRL	48	50	51
Test Scores (Mean and standard deviations)			
ACT Math	18.8	17.6	17.7
	(4.5)	(1.6)	(2.8)
ACT Reading	19.4	18.7	18.1
	(5.9)	(4.6)	(3.6)
ACT English	18.5	17.9	17.4
	(6.3)	(4.6)	(1.8)

Table 2. Covariate Balance Check (reproduced from Xu et al., forthcoming)

	Math				**English**			
	All	College	2 year	4 year	All	College	2 year	4 year
Female	-0.04***	-0.05***	-0.07**	-0.04*	0.01	0.01	0.04	-0.02
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
White	0.01	0.01	-0.02	0.03*	0.00	-0.00	0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Black	-0.01	-0.01	0.01	-0.02	-0.01	-0.02	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)
Hispanic	0.00	-0.00	0.00	-0.01	-0.01*	-0.01	-0.02	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
FRL	-0.01	-0.01	-0.01	0.00	-0.01	-0.02	-0.04	-0.00
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Eicker-Huber-White standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes with a bandwidth of three. Results are robust to other bandwidth choices. “All” column contains sample of all cohorts with ACT scores, “College” a sample of students who enrolled in college the year after graduating high school, “2 year” a sample of students who enrolled in a 2-year institution the year after graduating high school, and “4 year” a sample of students who enrolled in a 4-year institution the year after graduating high school.

Table 3. Estimated Effect of Targeted Interventions (TI) on College Outcomes

	Math				English			
	2 year		4 year		2 year		4 year	
Outcome	Coefficient/ S.E.	*Bandwidth/ N*	Coefficient/ S.E.	*Bandwidth/ N*	Coefficient/ S.E.	*Bandwidth/ N*	Coefficient/ S.E.	*Bandwidth/ N*
Panel A: Early college outcomes								
Started out in a non-degree program	-0.07**	3			0.01	4		
	(0.03)	20304			(0.02)	20195		
Started with undeclared program	0.02	4	-0.03*	4	-0.00	5	0.00	3
	(0.04)	27473	(0.02)	32380	(0.02)	23406	(0.02)	17840
Started in STEM	0.02*	5	0.01	5	-0.00	4	0.03**	4
	(0.01)	54400	(0.01)	66175	(0.01)	35707	(0.02)	41895
Started in Health	-0.03*	4	0.01	5	0.03*	5	0.00	4
	(0.02)	48470	(0.01)	66175	(0.01)	41648	(0.01)	41895
Program switch frequency within 1 year	0.01	6	-0.00	5	-0.00	4	-0.01	4
	(0.01)	57639	(0.01)	66175	(0.01)	35707	(0.01)	41895
Program switch frequency within 3 years	0.00	6	-0.02	5	-0.00	5	0.03	5
	(0.04)	57639	(0.04)	66175	(0.03)	41648	(0.04)	50708
Attempted ≥15 credits in first term	0.06***	5	0.05**	4	0.02	5	0.04*	4
	(0.02)	54400	(0.02)	57524	(0.01)	41648	(0.02)	41895
Panel B: End-of-college Outcomes								
Credits earned at the end of 3 years	1.08	3	2.33	3	-0.36	5	-1.37	4
	(1.81)	36637	(1.75)	46036	(1.20)	41648	(1.62)	41895
Earned >=60 credits by year 3	-0.00	5	0.03	3	0.01	4	-0.01	4
	(0.02)	54400	(0.02)	46036	(0.02)	35707	(0.02)	41895
Transfer within 3 years	0.02	5			0.02	5		
	(0.02)	54398			(0.01)	41646		
Credits earned at the end of 6 years			2.34	3			0.17	5
			(2.73)	46036			(2.24)	50708
Earned >=120 credits by year 6			0.04	3			-0.02	5
			(0.02)	27336			(0.02)	29889

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Eicker-Huber-White standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes. Bandwidth represents the bandwidth for a given subject and outcome, chosen using optimal bandwidth selection. N is the number of students.

Table 4. Robustness of Results to Alternate Bandwidths

	Math						English					
	2 year			4 year			2 year			4 year		
	BW	BW + 1	BW + 2	BW	BW + 1	BW + 2	BW	BW + 1	BW + 2	BW	BW + 1	BW + 2
Attempted ≥15 credits in first term												
Disc. pre	-0.07***	-0.07***	-0.07***	-0.03*	-0.04**	-0.05***	-0.03***	-0.03***	-0.04***	-0.02	-0.03*	-0.03**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Disc. post	-0.01*	-0.02***	-0.02***	0.01	0.01	0.00	-0.01	-0.01	-0.01	0.02	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in RD	0.06***	0.05***	0.04***	0.05**	0.05**	0.05***	0.02	0.02*	0.03**	0.04*	0.04**	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Earned >=60 credits by year 3												
Disc. pre	0.01	0.01	0.01	-0.03	-0.02	-0.02	-0.02	-0.02	-0.02	0.00	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Disc. post	0.01*	0.00	0.00	-0.00	0.01	0.01	-0.01	-0.01	-0.01	-0.00	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in RD	-0.00	-0.00	-0.00	0.03	0.02	0.03	0.01	0.01	0.01	-0.01	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Started with undeclared program												
Disc. pre	-0.03	-0.02	-0.03	0.02	0.02	0.02	-0.00	0.01	0.01	0.01	0.01	0.02
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Disc. post	-0.01	-0.01	-0.01	-0.01	-0.00	-0.00	-0.01	-0.00	-0.01	0.01	0.01	0.01*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in RD	0.02	0.01	0.02	-0.03*	-0.02	-0.02	-0.00	-0.01	-0.01	0.00	-0.00	-0.01
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Start with STEM												
Disc. pre	-0.01	-0.01	-0.01	0.00	0.00	0.00	0.00	0.00	-0.00	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Disc. post	0.01**	0.01***	0.01***	0.01**	0.01***	0.01***	-0.00	0.00	0.00	0.02**	0.02***	0.02***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in RD	0.02*	0.02	0.02*	0.01	0.01	0.01	-0.00	0.00	0.00	0.03**	0.03**	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)

Start with health												
Disc. pre	0.03	0.03*	0.02	-0.01	-0.01	-0.01	-0.02	-0.01	0.00	-0.00	-0.01	-0.01*
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Disc. post	-0.01	-0.01	-0.00	0.00	0.00	0.00	0.01	0.01*	0.01	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Difference in RD	-0.03*	-0.04**	-0.03*	0.01	0.01	0.02	0.03*	0.02	0.00	0.00	0.01	0.02*
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Started out in a non-degree program												
Disc. pre	0.06**	0.03	0.02				-0.01	-0.01	-0.01			
	(0.03)	(0.02)	(0.02)				(0.02)	(0.02)	(0.01)			
Disc. post	-0.00	-0.01	-0.01				-0.00	-0.00	-0.01			
	(0.01)	(0.01)	(0.01)				(0.01)	(0.01)	(0.01)			
Difference in RD	-0.07**	-0.04	-0.03				0.01	0.01	0.00			
	(0.03)	(0.02)	(0.02)				(0.02)	(0.02)	(0.02)			

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Eicker-Huber-White standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes. Bandwidth represents the bandwidth for a given subject and outcome, chosen using optimal bandwidth selection. N is the number of students. BW+1 and BW+2 columns display results from wider choses of bandwidth. “Discontinuity pre” represents the estimated discontinuity in the pre period, “Discontinuity post” the different in the post period, and “Difference in RD” the difference between the two, which are the results displayed in other tables.

Table 5. Estimated Effect of Targeted Interventions (TI) on College Outcomes, by Student and School Characteristics

	Math				English			
	2 year		4 year		2 year		4 year	
	Coefficient/ Standard error	*Bandwidth/ N*	Coefficient/ Standard error	*Bandwidth/ N*	Coefficient/ Standard error	*Bandwidth/ N*	Coefficient/ Standard error	*Bandwidth/ N*
Students who missed ACT cutoff in addition to subject								
Attempted >=15 credits in first term	0.07***	5	0.03	4	0.02	5	0.03	4
	(0.02)	39206	(0.03)	29630	(0.01)	35681	(0.02)	31656
Earned >=60 credits by year 3	0.04	5	-0.05	3	0.01	4	-0.02	4
	(0.03)	39206	(0.03)	23725	(0.02)	31168	(0.02)	31656
Started with undeclared program	0.05	4	-0.03	4	0.00	5	-0.01	3
	(0.04)	21217	(0.02)	18908	(0.03)	20806	(0.02)	14846
Started out in a non-degree program	-0.05	3			0.01	4		
	(0.03)	15037			(0.02)	18162		
Started in STEM	0.02	5	0.01	5	-0.00	4	0.03*	4
	(0.02)	39206	(0.02)	32498	(0.01)	31168	(0.02)	31656
Started in health	-0.04**	4	0.02	5	0.02	5	0.02	4
	(0.02)	34917	(0.02)	32498	(0.02)	35681	(0.01)	31656
Free/reduced-price lunch eligible students								
Attempted >=15 credits in first term	0.06**	5	0.04	4	0.01	5	0.06*	4
	(0.03)	26589	(0.04)	21690	(0.02)	19618	(0.04)	16070
Earned >=60 credits by year 3	0.02	5	0.03	3	0.02	4	0.03	4
	(0.03)	26589	(0.04)	17260	(0.03)	16728	(0.03)	16070
Started with undeclared program	0.04	4	-0.04	4	-0.03	5	-0.01	3
	(0.06)	13665	(0.03)	12811	(0.04)	11304	(0.03)	7213
Started out in a non-degree program	-0.09*	3			-0.03	4		
	(0.05)	9695			(0.03)	9669		
Started in STEM	0.03	5	0.02	5	-0.00	4	0.04	4
	(0.02)	26589	(0.02)	24469	(0.02)	16728	(0.02)	16070
Started in health	-0.02	4	0.02	5	0.03	5	0.01	4
	(0.03)	23428	(0.02)	24469	(0.02)	19618	(0.02)	16070
Top quartile school in % students below cutoff								
Attempted >=15 credits in first term	0.13***	5	0.07	4	0.01	5	0.11**	4
	(0.04)	16362	(0.05)	12656	(0.03)	9965	(0.05)	9497
Earned >=60 credits by year 3	0.01	5	-0.06	3	-0.03	4	-0.03	4
	(0.04)	16362	(0.05)	10041	(0.04)	8541	(0.04)	9497

Started with undeclared program	-0.05	4	0.01	4	0.01	5	0.03	3
	(0.07)	7779	(0.05)	6480	(0.05)	5635	(0.04)	3861
Started out in a non-degree program	-0.14**	3			0.01	4		
	(0.06)	5345			(0.05)	4830		
Started in STEM	0.02	5	-0.01	5	0.00	4	0.06**	4
	(0.02)	16362	(0.03)	14258	(0.02)	8541	(0.03)	9497
Started in health	0.00	4	0.02	5	0.03	5	-0.01	4
	(0.03)	14379	(0.02)	14258	(0.03)	9965	(0.02)	9497
Male								
Attempted >=15 credits in first term	0.06**	5	0.01	4	0.01	5	0.05	4
	(0.03)	23590	(0.03)	22711	(0.02)	18179	(0.03)	19260
Earned >=60 credits by year 3	0.00	5	0.09**	3	0.02	4	-0.00	4
	(0.03)	23590	(0.04)	17990	(0.03)	15549	(0.03)	19260
Started with undeclared program	0.05	4	-0.01	4	0.03	5	0.00	3
	(0.05)	12609	(0.03)	14185	(0.04)	10710	(0.03)	8700
Started out in a non-degree program	-0.08*	3			0.04	4		
	(0.04)	9467			(0.04)	9168		
Started in STEM	0.05**	5	-0.03	5	0.00	4	0.04	4
	(0.03)	23590	(0.02)	26526	(0.03)	15549	(0.03)	19260
Started in health	0.00	4	0.00	5	0.01	5	0.02**	4
	(0.02)	20883	(0.01)	26526	(0.02)	18179	(0.01)	19260
Female								
Attempted >=15 credits in first term	0.07***	5	0.07**	4	0.03*	5	0.03	4
	(0.03)	30810	(0.03)	34813	(0.02)	23469	(0.03)	22635
Earned >=60 credits by year 3	-0.00	5	-0.00	3	0.01	4	-0.01	4
	(0.03)	30810	(0.03)	28046	(0.03)	20158	(0.03)	22635
Started with undeclared program	0.01	4	-0.05**	4	-0.03	5	-0.00	3
	(0.05)	14864	(0.02)	18195	(0.03)	12696	(0.03)	9140
Started out in a non-degree program	-0.06*	3			-0.03	4		
	(0.03)	10837			(0.02)	11027		
Started in STEM	-0.01	5	0.04**	5	-0.01	4	0.02	4
	(0.01)	30810	(0.01)	39649	(0.01)	20158	(0.02)	22635
Started in health	-0.07**	4	0.02	5	0.04*	5	-0.02	4
	(0.03)	27587	(0.02)	39649	(0.02)	23469	(0.02)	22635

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Eicker-Huber-White standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes. Bandwidth represents the bandwidth for a given subject and outcome, chosen using optimal bandwidth selection. N is the number of students.

Table 6. Effect of Targeted Interventions (TI) on 12th Grade Course-Taking, by Intervention Subject

	Math		English	
	Coefficient/ S.E.	*Bandwidth/ N*	Coefficient/ S.E.	*Bandwidth/ N*
Within-subject substitution				
Transition courses in subject	0.12***	2	0.02***	3
	(0.02)	93,243	(0.01)	114,410
Non-transition courses in subject	-0.08***	2	-0.01	4
	(0.03)	93,243	(0.03)	157,382
Other course substitution				
CTE courses taken	-0.02	3	0.02	3
	(0.05)	168,262	(0.07)	114,410
Other courses taken	-0.06	2	-0.02	3
	(0.12)	93,243	(0.12)	114,410

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Eicker-Huber-White standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes. Bandwidth represents the bandwidth for a given subject and outcome, chosen using optimal bandwidth selection. N is the number of students.

Table 7. Correlation Between College Outcomes and Intervention Completion Status

College type and subject	Take developmental course	Pass credit-bearing course in subject	15+ credits in first term	At least 60 credits in 3 years
2-year college, math				
No successful exit (b_2)	0.083***	-0.038***	-0.023***	-0.021***
	(0.01)	(0.01)	(0.01)	(0.01)
Successful exit (b_1)	-0.087***	0.01	0.01	-0.016*
	(0.01)	(0.01)	(0.01)	(0.01)
p-value ($b_2 = b_1$)	0.000	0.000	0.002	0.541
Observations	11292	11292	11292	11292
R-squared	0.054	0.088	0.038	0.066
4-year college, math				
No successful exit (b_2)	0.083***	-0.071***	-0.053***	-0.060***
	(0.01)	(0.01)	(0.01)	(0.01)
Successful exit (b_1)	(0.02)	-0.051***	0.024*	-0.030***
	(0.01)	(0.01)	(0.01)	(0.01)
p-value ($b_2 = b_1$)	0.000	0.098	0.000	0.019
Observations	9296	9296	9296	9296
R-squared	0.032	0.075	0.041	0.089
2-year college, English				
No successful exit (b_2)	0.041***	-0.007	-0.017**	-0.007
	(0.01)	(0.01)	(0.01)	(0.01)
Successful exit (b_1)	-0.082***	0.02	0.01	(0.01)
	(0.01)	(0.02)	(0.01)	(0.01)
p-value ($b_2 = b_1$)	0.000	0.072	0.051	0.856
Observations	7694	7694	7694	7694
R-squared	0.081	0.091	0.044	0.057
4-year college, English				
No successful exit (b_2)	0.038***	0.030*	-0.064***	-0.023
	(0.01)	(0.02)	(0.02)	(0.02)
Successful exit (b_1)	-0.054***	0.01	0.02	(0.02)
	(0.01)	(0.02)	(0.02)	(0.02)
p-value ($b_2 = b_1$)	0.000	0.314	0.000	0.941
Observations	4789	4789	4789	4789
R-squared	0.117	0.046	0.055	0.072

Notes: ACT test year 2014 and 2015. Fixed effect for ACT test score. Additional controls for cubic function of test scores in other ACT subjects, race, gender, FRL eligibility, and whether the student was below cut points in any other subject. Only below cut score students included. Robust standard errors. Omitted group is students not in intervention. P-values indicate p-value of F-test of equality of the "No successful exit" and "Successful exit" coefficients. *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Table 8. First-Stage Discontinuity in Complier Districts: Grade 8–9 Intervention

	Math			English		
	BW=2	BW=3	BW=4	BW=2	BW=3	BW=4
Discontinuity in TI participation	0.37***	0.38***	0.36***	0.60***	0.57***	0.55***
	(0.06)	(0.04)	(0.04)	(0.10)	(0.07)	(0.06)
F-statistic	26.2	36.1	39.1	21.6	28.8	30.5
N	1127	1623	1782	391	577	730

Notes: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Eicker-Huber-White standard errors in parentheses. Discontinuity in TI participation is estimated by regression TI participation on EXPLORE score, an indicator of falling below cutoffs, and the interaction of the two terms. Complier districts defined as districts with an estimated district-specific discontinuity of 0.20 with at least 50 students within bandwidth. Columns show bandwidths of 2, 3, and 4 points around the cutoff in each subject. The F-statistic tests the joint significance of the indicator variable of falling below the cutoff and its interaction with the EXPLORE score. The critical value is 11.59 based on Stock and Yogo (2005).

Table 9: Estimated Effect of Targeted Interventions (TI) in Grades 8 and 9 on Later Outcomes

	Take ACT	Above ACT threshold	ACT score	Graduate high school	Attend college	Take DE course in subject in first year in college	Take credit-bearing college course in subject in first year in college
Math							
Discontinuity pre	-0.01	-0.05	-0.22	-0.00	-0.02	-0.08***	0.05
	(0.01)	(0.03)	(0.23)	(0.01)	(0.02)	(0.03)	(0.03)
Discontinuity post	-0.05*	-0.06	-0.42	-0.06*	-0.07	0.00	-0.04
	(0.03)	(0.07)	(0.47)	(0.03)	(0.04)	(0.04)	(0.06)
Difference in RD	-0.05	-0.01	-0.19	-0.05	-0.05	0.08*	-0.09
	(0.03)	(0.08)	(0.51)	(0.03)	(0.05)	(0.05)	(0.07)
Bandwidth	4	2	2	4	4	3	4
N	8612	5213	5213	8612	8612	3875	4184
English							
Discontinuity pre	0.03	-0.07	-0.33	-0.00	0.07*	-0.04	0.04
	(0.03)	(0.04)	(0.30)	(0.03)	(0.04)	(0.03)	(0.05)
Discontinuity post	0.02	-0.19**	-0.90	-0.07	0.06	0.04	-0.20*
	(0.06)	(0.08)	(0.59)	(0.06)	(0.07)	(0.04)	(0.12)
Difference in RD	-0.02	-0.12	-0.57	-0.07	-0.01	0.08	-0.24
	(0.07)	(0.09)	(0.62)	(0.06)	(0.08)	(0.06)	(0.14)
Bandwidth	4	3	4	4	4	4	4
N	3415	2304	2927	3415	3415	1377	1377

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Eicker-Huber-White standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes. Bandwidth represents the bandwidth for a given subject and outcome, chosen using optimal bandwidth selection. N is the number of students.