NATIONAL
CENTER for ANALYSIS of LONGITUDINAL DATA in EDUCATION RESEARCH

TRACKING EVERY STUDENT'S LEARNING EVERY YEAR

A program of research by the American Institutes for Research with Duke University, Northwestern University, Stanford University, University of Missouri-Columbia, University of Texas at Dallas, and University of Washington

AIR

Targeted Interventions in High School: Preparing Students for College

Zeyu Xu
Ben Backes
Amanda Oliveira
Dan Goldhaber

WORKING PAPER No. 232-0220 • February 2020

Targeted Interventions in High School: Preparing Students for College

Zeyu Xu
American Institutes for Research/CALDER

Ben Backes
American Institutes for Research/CALDER

Amanda Oliveira
American Institutes for Research/CALDER

Dan Goldhaber
American Institutes for Research/CALDER
University of Washington

Contents

Acknowledgments

The research reported here was supported by the Institute of Education Sciences, U.S. Department of Education, through Grant R305A160188 to the American Institutes for Research and the Kentucky Department of Education. We thank Karen Dodd, April Piper, Aaron Butler, and Hannah Poquette from the Kentucky Department of Education and Barrett Ross from the Kentucky Center for Statistics for their support. The opinions expressed are those of the authors and do not represent the views of the Institute, the U.S. Department of Education, or the Kentucky Department of Education. All opinions expressed in this paper are those of the authors and do not necessarily reflect the views of our funders or the institutions to which the authors are affiliated.

CALDER • American Institutes for Research
1000 Thomas Jefferson Street NW, Washington, DC 20007
202-403-5796 • www.caldercenter.org

Targeted Interventions in High School: Preparing Students for College
Zeyu Xu, Ben Backes, Amanda Oliveira, Dan Goldhaber
CALDER Working Paper No. 232-0220
February 2020

Abstract

This study adds to the currently limited evidence base on the efficacy of interventions targeting non-college-ready high school students by examining the impact of Kentucky's Targeted Interventions (TI) program. We focus on interventions that students received under TI in the senior year of high school based on their 11th grade ACT test scores. Using difference-in-regression discontinuity and difference-in-difference designs with seven cohorts of 11th grade students, we find that, for an average per-student cost of about $600, TI significantly reduces the likelihood that students enroll in remedial course in both 2- and 4-year postsecondary institutions by 5–10 percentage points in math and 3–4 percentage points in English. These effects are similar among students who are eligible for free-or reduced-price lunch, Black and Hispanic students, students with remediation needs in multiple subjects, and students in lower-performing schools. Evidence also shows that TI increases the likelihood that students enroll in and pass college math before the end of the first year by four percentage points in 4-year universities. However, little evidence exists for TI affecting credit accumulation or persistence.

1. Introduction

Increasing the share of adults with postsecondary credentials is high on the national education agenda. This is reflected in national initiatives such as those put forth by the Obama administration,[1] as well as in education attainment goals set by many states.[2] For example, in Kentucky, which is the focus of this study, the Kentucky Council for Postsecondary Education (CPE) has set a goal for 60 percent of working-age Kentuckians to have high-quality postsecondary degrees or credentials by 2030 (CPE, 2019).

Yet significant barriers to reaching these goals exist. Postsecondary graduation rates are low, and they have been stagnant for the last decade: Only about 30 percent of full-time community college students graduate within three years of initial enrollment and less than 60 percent of 4-year university students graduate within six years.[3] Furthermore, low graduation rates disproportionately affect disadvantaged students. For example, among community college students, Black students are 30 percent less likely to graduate than White and Asian students.

Numerous institutional and student factors may explain low postsecondary attainment (Holzer & Baum, 2017), but weak academic preparation before college matriculation is often cited as a key roadblock to student success (Bettinger, Boatman, & Long, 2013; Scott-Clayton, 2011). Estimates, for instance, suggest that only a quarter of high school students who are assessed based on ACT college benchmarks appear fully college ready (ACT, 2014); not surprisingly then, the need for remediation in public postsecondary institutions is widespread, with 68 percent of 2-year college students and 40 percent of 4-year university students taking at least one remedial course (Chen, 2016; Bailey & Jaggars, 2016).

[1] https://obamawhitehouse.archives.gov/blog/2009/07/14/investing-education-american-graduation-initiative
[2] Foundations, such as the Lumina Foundation and the Bill & Melinda Gates Foundation, have also invested heavily in programs designed to promote the share of adults with postsecondary credentials in the labor force (Bailey, 2012).
[3] https://nces.ed.gov/programs/raceindicators/indicator_RED.asp

In this study, we evaluate the effects of a high school college readiness remediation program in Kentucky on students' college outcomes using regression discontinuity (RD) and difference-in-differences (DD) methods. As the first state to adopt the Common Core State Standards (CCSS), Kentucky is also one of the earliest states to introduce statewide high school remediation for college. First implemented in the 2010–11 school year for high school math and reading and one year later for English, Kentucky's Targeted Interventions (TI) program uses 11th-grade ACT test scores to identify students for remediation. ACT tests have been mandatory for all 11th-grade students in Kentucky since 2006. Students scoring below 19 in math, 18 in English, or 20 in reading are considered not on track to be college-ready in those subjects when graduating high school. These students are required to receive intervention services or remedial support, with the goal of getting them ready to access "credit-bearing coursework without the need for developmental education or supplemental courses" in college (CPE, 2010, p.7), Kentucky's working definition of college readiness.

The rationale for addressing college-readiness gaps before college matriculation is straightforward: Traditional developmental education in college, which typically requires academically underprepared students to take up to three levels of remedial courses before they can start accumulating college credits, is both costly (Boatman & Long, 2018) and considered by many to be ineffective (e.g., Valentine, Konstantopoulos, & Goldrick-Rab, 2017). As of 2017, 17 states offer statewide college-readiness remediation in high school (sometimes also called transition interventions) and 22 states offer local high school remediation programs (Barnett, Chavarin, & Griffin, 2018). The number of statewide programs has more than doubled since 2013 (Barnett, Fay, Trimble, & Pheatt, 2013). Florida's College and Career Readiness Initiative (FCCRI) (Mokher, Leeds, & Harris, 2018) and Tennessee's Seamless Alignment and Integrated

Learning Support (SAILS) program (Kane et al., 2019) are prominent examples of statewide high school remediation.

Despite the widespread interest in high school remediation for college readiness, empirical evidence on its impact on college outcomes is relatively limited. Our study is one of four studies that look at one of the major efforts that states are adopting to help academically underprepared students succeed in higher education; moreover, the design and implementation of high school remediation programs vary considerably from state to state, and currently no evidence is available for the type of program implemented in Kentucky. As we describe in Section 3, Kentucky's high school remediation program has a unique combination of programmatic and contextual features, and its experience will add significantly to the currently limited evidence base.

Our study finds empirical evidence that Kentucky's TI program significantly reduces the remedial course enrollment rate in both 2- and 4-year postsecondary institutions by 5–10 percentage points in math and 3–4 percentage points in English. The TI program also boosts the enrollment rate in credit-bearing college math during the freshman year in 4-year universities, resulting in an overall increase of four percentage points in the likelihood of university students passing introductory college math by the end of the freshman year. The TI program reduces the likelihood of college remediation by similar magnitude among students who are eligible for free and reduced-priced lunch (FRL), Black and Hispanic students, students with remediation needs in multiple subjects, and students in lower-performing high schools. However, the TI program has no detectable effect on other measures of progress through college such as total credits earned by the end of the first two years in college or persistence.

In what follows, we first describe the theory and empirical literature on high school remediation. This is followed by a description of Kentucky's TI program. We next discuss the data, analytic samples, and methods. Impact estimates are presented in Section 5. We conclude this study with a discussion on how we may rethink high school remediation.

2. Theory and Empirical Literature

In recent years, efforts to improve the ways in which we help academically underprepared students acquire the requisite skills and knowledge for college-level instruction and contents have started to cut across postsecondary institutions and secondary schools. Traditionally, college remediation was predominantly delivered by postsecondary institutions through prerequisite developmental education models (Goudas & Boylan, 2012). Under such models, underprepared college students must take a series of remedial courses in math and English before proceeding to credit-bearing courses.

By most accounts, prerequisite remediation appears not to be working as conceived. Despite total annual spending of $4 billion (Scott-Clayton & Rodriguez, 2015), traditional remediation sequences mostly have null—and in a few cases negative—effects on subsequent student outcomes such as passing gatekeeper courses, credit accumulation, and credential attainment (Bailey, Jaggars, & Scott-Clayton, 2013; Melguizo, Bos, Ngo, Mills, & Prather, 2015; Valentine, Konstantopoulos, & Goldrick-Rab, 2017).[4] Critics argue that traditional prerequisite remediation diverts resources and efforts away from making progress toward postsecondary credential completion, and that underprepared students are further disadvantaged by the

[4] For example, one study finds that, among students who were subject to lengthy remediation sequences, only 11 percent and 29 percent of students referred to math and English remediation eventually passed introductory college courses in these subjects (Jaggars & Stacey, 2014).

additional burden of remediation that has delivered no apparent academic benefits (Scott-Clayton & Rodriguez, 2015).

The desire to minimize such "diversion effects" has led some reforms to push for remediation to be completed before college entry.[5, 6] High school remediation programs are thought to benefit students by making clearer the skills students need to succeed in college.[7] Among the 17 states that have statewide remediation programs, 13 use college placement assessments directly to screen high school seniors for intervention (Fay et al., 2017), thereby aligning expectations across the two education systems. High school remedial interventions are also frequently designed to target specific student deficiency areas identified by screener tests. Finally, by helping students become college-ready before college entry, high school remediation could potentially save students and the public costs in time and resources that college developmental education would require.

But high school remediation also raises several concerns. The first concern is the opportunity cost represented by regular high school courses and other learning opportunities that are crowded out by mandatory remediation. In states and subjects where four years of courses are required for high school diploma, remediation in the senior year tends to displace more advanced regular courses such as pre-calculus, trigonometry and English IV (Kane et al., 2019; Mokher et

[5] Widespread reforms have been underway at the postsecondary level as well. These include changes to assessment and placement practices (such as the abolishment of placement assessments in Florida); accelerated remediation by combining two sequential remedial courses into one semester (e.g., Denver's FastStart), reducing the number of remediation credit hours (e.g., Chabot's accelerated English), or offering corequisites that allow students to enroll directly in college-level courses with paired support; and the development of distinct math pathways to better target what students need in their future jobs (Jaggars & Hodara, 2011).

[6] Pre-college remediation also includes a few summer bridge programs. Existing empirical evidence is scant: whereas Bailey & Jaggars (2016) found a positive correlation between CUNY's Start program and student outcomes, Chingos, Griffiths, & Mulhem (2017) found null effects of a low-cost online summer math program using a randomized experiment.

[7] Bettinger et al. (2013) point out that the misalignment between high school graduation requirements and the expectations of postsecondary institutions is a main reason why many students finish high school unprepared for the next step in their education.

al., 2018).[8] Requirements for remediation in high school may also crowd out career technical education courses that may have higher relevance and labor market value to some students. Second and relatedly, the quality of remedial education that students receive at the expense of regular high school coursework is unclear. In both West Virginia and Florida (Mokher et al., 2018; Pheatt, Trimble & Barnett, 2016), for instance, the emphasis of transition courses leaned toward test preparation for college placement assessments. In the case of Tennessee (Kane et al., 2019), transition courses were modeled after college remedial courses. But college remedial courses have been criticized for their lack of relevant content (Bailey & Jaggars, 2016) and an instructional approach that emphasizes drilling and practice (Boatman, 2012; Grubb, 2013). In short, whether substituting regular high school coursework with remediation leads to a net gain in learning is unclear.

Despite the fast growth of high school remediation programs, we are aware of only three evaluations of statewide programs. These programs differ in how students were selected for remediation, the objective of the intervention, and the intervention curriculum (see a summary of key differences and evaluation findings in Exhibit 1). Pheatt, Trimble, and Barnett (2016) examine the effect of West Virginia's Transition Mathematics for Seniors in 2011–12 and 2012–13. Students were screened for remediation using a test administered in the junior year, and the primary goal of the math remedial course appeared to be preparing students to take and ideally to pass the COMPASS, the placement test that colleges used to refer students for developmental education. Using an RD design, the study finds no significant effect on passing the college remediation placement test and a negative effect on students' likelihood of passing a college gatekeeper math course.

[8] Taking advanced courses in high school is shown to be associated with positive postsecondary outcomes (Attewell & Domina, 2008).

Mokher et al. (2018) studies the impact of Florida's high school remediation program, FCCRI, on student college outcomes. FCCRI identified students for remediation in two steps: Students who scored in the midrange of a 10th grade state standardized test (FCAT) were tested again in the 11th grade using college placement tests. The evaluated program was mandatory starting in 2011–12 but voluntary before then. Targeted students enrolled in transition courses in math and English that emphasized test preparation for college placement assessments such as the ACT or PERT. Using RD and a regression analysis, the study finds that FCCRI generally had no detectable effect on high school graduation, college enrollment, or the likelihood of enrolling in or passing college-level courses.

Finally, Kane et al. (2019) evaluates Tennessee's SAILS high school remediation program in math on student college outcomes. SAILS was launched in 2011–12, and it gradually spread to more high schools but never reached more than 60 percent of all schools during the study period. In SAILS schools, ACT scores from the junior year were used to target students for both high school remediation and college placement. Successful completion of high school remedial math automatically exempted students from developmental education in college, and so it is not surprising that high school remediation reduced the enrollment of college remedial math by around 30 percentage points. Exemption from college remediation translated into small increases in college math enrollment, but it failed to increase the passing rate of college math or accelerate college credit accumulation. A post-intervention assessment also shows no significant improvement in intervention students' math skills.

It is worth noting that in all three states reviewed above, high school remediation programs have changed in important ways over time. In West Virginia, the math remediation curriculum studied by Pheatt et al (2016) was dropped after three years of full implementation. In

Florida and Tennessee, the role of high school remediation diminished significantly after changes to developmental education at the college level. The abolishment of mandatory college placement testing in Florida in 2014 and the statewide introduction of corequisite developmental education in Tennessee in 2015 made high school remediation in those states less relevant, especially when the high school programs' goals were limited primarily to test preparation or to fulfill college remediation requirements.

3. Targeted Interventions in Kentucky

In 2009, the Kentucky General Assembly passed Senate Bill (SB) 1 calling for a complete overhaul of Kentucky's assessment and accountability system for PreK–12 education. Since then, the Commonwealth has embarked on a system of education reform—known as "Unbridled Learning"—designed to better monitor, document, and support progress toward the goal of college- and career- readiness. In response to SB1 and the strategic priorities subsequently established by the Kentucky Board of Education (KBE),[9] the Kentucky Department of Education (KDE) and the Kentucky Council on Postsecondary Education (CPE) jointly developed the Targeted Interventions (TI) program (see Kentucky Statutes KRS158.6453 and KRS158.6459, and regulation 704 KAR3:305).

The TI program is designed to monitor student progress on college- and career- readiness proficiencies, screen students who need help, and provide interventions that are targeted to fulfill those needs. This program has been in effect since the 2010–11 school year. Performance benchmarks are established for students in Grades 8, 10, and 11 using the EXPLORE, PLAN,

[9] In 2011, the KBE established four strategic priorities under Unbridled Learning: Next Generation Learners, Next Generation Professionals, Next Generation Support Systems, and Next Generation Schools and Districts. Together, these four priorities form the basis of Kentucky's new accountability model, with all of its components fully implemented starting with the 2014–15 school year.

and ACT tests, respectively.[10] College readiness is benchmarked in three subject areas: math, English and reading. The program was gradually phased in over three years, starting with high school math and reading in 2010–11 and followed by high school English in 2011–12 and middle school math, reading, and English in 2012–13.

Students scoring below the benchmarks are targeted to receive intervention services in their senior year. School districts determine the details of TI interventions, but each intervention cycle typically includes a diagnostic pretest, the development of instructional targets and their associated formative assessments, direct delivery of instruction, and a posttest. Supplemental instruction is often delivered through transition courses that are tailored toward individual students' deficiency areas and aligned with the Kentucky Core Academic Standards (KY's Common Core–aligned education standards). Transition courses can be either integrated into an existing course or a stand-alone course, and interventions can be delivered during regular school hours or as extended school services (ESS). Based on phone interviews with program administrators and surveys of teachers involved in supplemental instruction delivery, most transition courses make use of online curricula such as those developed by Dreambox, ALEX, and IXL in combination with teacher-developed materials, and test preparation does not appear to be the primary goal of these remedial interventions. More details about programmatic features and implementation details can be found in a companion cost study of the TI program conducted by Levin et al. (2020).

[10] All three assessments were developed by ACT, Inc. as an integrated series of assessment called the Educational Planning and Assessment System (EPAS). EXPLORE was designed for 8th and 9th grade and PLAN was designed for the 10th grade. Each assessment provides predicted score ranges for subsequent assessments. EXPLORE and PLAN have been replaced by the ACT ASPIRE system since 2015. See https://files.eric.ed.gov/fulltext/ED510457.pdf for more details.

Although the design of the TI program expressly states the intent to intervene early, our current study focuses on 11th-grade students. This is because intervention reporting is mandatory for targeted 11th-grade students only. Using reported intervention data, we were able to detect—as one would expect if the program had been implemented as designed—a sharp jump in TI participation rates among students who score just below the benchmarks relative to students who score just above (see more details in Section 4). By comparison, intervention reporting is not mandatory for students in the 8th and 10th grades, and we failed to detect any sharp jumps in intervention participation rates around corresponding cutoff scores based on voluntarily reported intervention data.[11] It is unclear whether the lack of discontinuity in intervention participation rates around cutoff scores is due to reporting issues, program implementation noncompliance, or both, rendering it impossible to interpret any "program impact" we may or may not find among 8th- and 10th-grade students. In addition, our study also focuses on math and English interventions only because student performance in these two subjects determines whether or not a student would be placed directly into college math and English courses that are required for graduation.

The first row of Table 1 presents the number of 11th-grade students who scored at or above the ACT benchmarks (19 for math and 18 for English) and those who scored below the benchmarks in spring 2014 and 2015.[12] More than 87,000 high school juniors took the ACT for the first time. Among those, more than 60 percent failed to meet the math benchmark and 40 percent missed the English benchmark.

About 56–57 percent of students who scored below the cutoff in either subject are documented to have participated in TI. On the other hand, between one-fifth to one-quarter of

[11] Data available upon request.
[12] These are the first two cohorts of 11th-grade students for whom TI intervention data are available.

students who scored at or above the ACT benchmarks also participated in TI (Table 1, Row 2). Unfortunately, we do not know what may have led to the observed program participation noncompliance (both "no shows" and crossovers). Communications with TI administrators suggest that other factors such as teacher input could have been considered in conjunction with ACT scores to refer students for TI. Students who failed the benchmarks on the first try might also retake the test to meet the benchmarks. Staff and parental resistance to high school remediation was also reported in our interviews with school administrators. Other possible factors like intervention exemptions and under-reporting cannot be ruled out.

The rest of Table 1 and Table 2 present summary statistics on some of the characteristics of TI in order to present a more concrete picture of what high school remediation looked like in Kentucky. The bottom half of Table 1 describes the content and type of remediation that TI participants received by student group. Students could receive remediation in multiple content areas and forms. Among all 11th-grade students who participated in TI (column 1), 79 percent received remediation in math and 48 percent received remediation in English. Most students received high school remediation in the form of transition courses (63 percent), and close to one-third of students receive services through extended school services (ESS). It is interesting to note that half of students who failed to meet the math benchmark (column 3) also participated in English remediation, and that 78 percent of students who failed ACT English (column 5) received remediation in math.

Some of the characteristics of TI remediation can be quantified using individual student-level data (Table 2). For example, an average intervention session lasted 55 minutes and was delivered four times a week. The most frequently-reported areas of deficiency included algebraic thinking, math reasoning, math computation, writing mechanics, and writing content. Around half (55 percent for math and 48 percent for English) of TI participants in our sample were judged by schools to have exited remediation successfully.

The context, design, and delivery of high school remediation vary considerably from state to state (Barnett et al., 2018), and Kentucky's TI has several unique characteristics. For example, most (12 out of 17) states that mandate statewide high school remediation automatically exempt students from developmental education in college after successful completion of high school remediation (Fay, Barnett, & Chavarin, 2017). Kentucky, by comparison, is one of five states that do not directly link high school remediation completion with developmental education exemption in college. Automatic exemption may be more efficient, but it mechanically produces an "effect" of reduction in college remedial course-taking without necessarily improving students' academic proficiency, as Kane and colleagues (2019) found in Tennessee.

In addition, high school remediation is typically designed to provide differentiated, not additional, learning opportunities for underprepared students. Because most states (e.g., Florida and Tennessee) require four years of math and English for high school graduation, remedial activities crowd out regular math and English courses that high school seniors would take. By comparison, Kentucky required only three years of math for high school graduation during the study period, and so Kentucky's math remediation displaced less advanced, elective math courses (such as algebra II and college algebra). More importantly, one-third of targeted 11th-grade students in Kentucky received intervention through extended school services that averaged over 100 minutes per week, representing net gains in learning time outside of regular school hours.

Finally, even though the ACT is used by both K–12 and postsecondary systems to assess college readiness and course placement in Kentucky, benchmarks do not align perfectly.

Students considered college-ready by high schools may still be required to undertake developmental education in college. For example, although Kentucky high schools consider students who score 19 or higher on ACT math college-ready, only students scoring above 21 can take any college-level math courses. This contrasts with some states where different placement assessments are used for remediation referral in high school and college, or where assessment and placement policies perfectly align.

4. Data and Methods

4.1. Data and Sample Statistics

We utilize longitudinal student-level administrative records provided by the Kentucky Center for Statistics (KYStats) that track student progress from high school through college between 2009 and 2018. These records include high school test scores, student background characteristics, and transcript data from both high school and postsecondary institutions in Kentucky, including both public and private postsecondary institutions. The data also include student-level records that document the interventions that remedial students received. The intervention data were first collected for the 2014 cohort of 11th-grade students, and they include information on intervention type, content area, curriculum and intensity.

The individual student outcomes we focus on include whether students take remedial courses in college, whether they pass math and English credit-bearing courses in college, and persistence and credit accumulation by the second year of college. These outcomes are examined for 2- and 4-year institutions separately.[13]

[13] One limitation of our data is that we do not have information on out-of-state postsecondary enrollment for all cohorts. To assess the potential impact of missing data on our study, we examine college enrollment behavior among the 2013 cohort of 11th-grade students for whom we do have complete information on college enrollment through the National Student Clearinghouse. We find that out-of-state postsecondary enrollment accounts for less than three

The analytic sample of this study consists of seven cohorts of 11th grade students who took the ACT for the first time in the Spring of their junior year of high school between 2009 and 2015. Each cohort consists of around 43,000 students. As depicted in Table 3, the 2010−2015 cohorts of 11th grade students are subject to math TI and the 2011−2015 cohorts are subject to English TI ("Treated" cohorts that do not have individual student level intervention data are represented by solid dots, and cohorts that have individual level intervention data are represented by squares). The 2009 cohort represents the pre-treatment group for math and the 2009 and 2010 cohorts represent the pre-treatment group for English ("Untreated" cohorts are represented by open dots in Table 3).

Descriptive statistics are presented in Table 4, with each column representing a specific sample. Among all students, 85 percent are White and 48 percent are eligible for free/reduced price lunch (FRL) (column 1). The average ACT scores are very close to the college-ready benchmarks (Math=18.8, Reading=19.3 and English=18.4), with a standard deviation of about 5-6 points. The analytic samples for the RD analysis (that is, within 3 to 5 points around the ACT cut scores) have similar characteristics, with ACT scores slightly lower than the population average by around one point.

4.2. Methods

The TI program relies on ACT test scores to refer students for supplemental and remedial support. In a world with perfect compliance, all students with scores S below the cutoff c would be assigned to treatment (T=1), while those scoring at or above would not (T=0 if $S \geq c$). The treatment status would be a discontinuous function of ACT score: $T=1(S<c)$. Under the assumptions that population average potential outcomes Y are continuous functions of the

---

percent of students in the cohort around the ACT cutoffs, and there is no apparent discontinuity at the cut point in the likelihood of leaving the state.

running variable (i.e., the ACT test score) at the cutoff, and that no determinants of the potential outcomes change abruptly at the cutoff other than treatment assignment, the causal effect of TI on students within the immediate vicinity of cutoff c could be reliably estimated using RD. This can be represented by regression equation (1), where *k(.)* is a function of the ACT score of student i, S_i, that is centered around the cutoff such that negative values indicate scores below cutoff.

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

Not all students scoring below the cutoff participate in TI (no-shows) and some students scoring above the cutoff receive treatment (cross-overs). Figure 1 depicts these two types of noncompliance using math intervention data from the 2014 and 2015 cohorts. Although the ACT score does not predict TI participation deterministically, it still holds that the probability of assignment to treatment changes discontinuously (by about 40 percentage points) at the cutoff. When the intended and actual assignment to TI deviate from each other, α_1 estimates the intent-to-treat (ITT) effect of high school remediation on student outcomes Y_i instead of the treatment effect of the treated (TOT).

Because ITT reflects the overall effect that a state could realistically expect from similar programs, and because we have intervention data for only two cohorts of 11th grade students, our study focuses on the ITT effect instead of the TOT effect that would be estimated using a fuzzy RD.[14] The decision to focus on ITT is also related to two additional challenges in the current setting that have led us to use difference-in-RD as the preferred method to estimate the TI impact.

[14] The TOT effect of TI on student outcomes is equivalent to the ITT effect scaled by the difference in remediation participation rates between students scoring just above and below the cutoff.

The first challenge is related to the discreteness of the running variable and the resulting uncertainty associated with potential specification errors in the RD model. ACT scores are typically reported in whole number scores in a range of 1-36. Even with finer-grained underlying scores, the distribution of ACT scores is clearly lumped at several discrete score points (see Kane et al. [2019] which obtained finer-grained ACT scores for one cohort). When the running variable is discrete, it becomes infeasible to compare averages within arbitrarily small neighborhoods around the cutoff (Card & Lee, 2008). Impact estimates would instead have to rely on parametric assumptions about the relationship between the outcome and the running variable near the cutoff. Following the recommendation of Card and Lee (2008), researchers typically widen the confidence interval around the impact estimate to account for the additional uncertainty brought about by parametric assumptions. This is typically achieved by clustering the standard error at each distinct value of the running variable (CRV).[15] With additional, pre-treatment data, however, we can also estimate a similar RD "effect" in the pre-treatment sample and take the difference between the pre- and post-RD estimates. To the extent that the underlying relationship between ACT scores and student outcomes in the absence of TI has not changed over time, difference-in-RD can produce specification-robust estimate of high school remediation on student outcomes.[16]

The second challenge has to do with the alignment between high school college-readiness benchmarks and course placement policy at the postsecondary level in Kentucky public

[15] The clustered variance estimator is known to be biased when the number of clusters is small (McCaffrey & Bell, 2002), and more recent research provides convincing empirical and theoretical evidence that CRV standard errors generally understate the uncertainty related to specification errors and lead to substantially worse coverage properties than Eicker-Huber-White (EHW) heteroskedasticity-robust standard errors (Kolesár and Rothe, 2018). Because of the issues associated with CRV standard errors, we follow Kolesár and Rothe's advice and use EHW standard errors when making inferences about estimated TI impacts on student outcomes.

[16] Difference-in-RD, sometimes also called difference-in-discontinuity, has been implemented to study a wide range of public policy topics. See, for example, Aucejo, Romano, & Taylor (2019); Dantas, Duarte, Neto, & Sampaio (2018); Jackson (2017); Grembi, Nannicini, & Troiano (2016); Xie (2015); Bettendorf, Folmer, & Jongen (2014).

institutions. Because students scoring above and below the math and English cut scores on the ACT also face different college remediation requirements, it is clear that TI participation is not the only "treatment" that differentiates students around the cutoffs. For example, in math, students scoring in the 16-18 range must (if they fail to retake and pass college placement assessments) take lower level remedial math courses in college (e.g., Basic Algebra and Mathematical Literacy) than students scoring in the 19-21 range, who are able to directly enroll in certain credit-bearing math courses with paired supplemental support (e.g., College Algebra Workshop). Any estimated RD effect on student college outcomes by Equation (1) would reflect the combined effect of both high school and college remediation. This is the type of scenario (i.e., the presence of confounding treatments around the cutoff) that typically calls for the use of difference-in-RD (Eggers, Freier, Grembi, & Nannicini, 2018). Because placement policies into college remedial sequences have remained constant for all 11th grade cohorts during the study period, we can use difference-in-RD to net out the effect that is due to differential college remediation requirements under the assumption that such effect is constant across all student cohorts.

We conduct the usual validity checks on RD setup. The critical identifying assumption in an RD design is the continuity assumption: that absent the treatment, expected student outcomes would remain smooth through the cutoff. This assumption implies that determinants of potential outcomes other than treatment status are also smooth around the cutoff. Given these assumptions, any discontinuity in student outcomes can only be caused by the treatment.

One way the continuity assumption may be violated is through the manipulation of the running variable such that students of different characteristics sort themselves into treatment and control groups. If TI were viewed as an extra burden, for example, more students might be expected to show up just at or above the cutoff than just below to avoid TI. This type of manipulation is unlikely because ACT tests are assessed without any teacher, student or principal influence and it is almost impossible for students to precisely target scores in the close vicinity of the cutoff.[17] The distribution of ACT test scores, centered around the cutoff, indeed shows no sign of score manipulation as it is smooth through the cutoff (Figure 2).[18]

Because students are unable to precisely achieve a score at or below the cutoff, we should not expect student attributes to change sharply around the cutoff. While this assumption cannot be definitively proven, as many individual and family characteristics are unobservable to the researchers, we can test for continuity of observable characteristics such as student demographic characteristics and FRL eligibility. As shown Table 5, within a bandwidth of three points around the cutoff, we do not generally detect significant discontinuity in student attributes around the cutoff scores with the exception of fewer females among students who scored just at or above the math benchmark than just below. However, controlling for gender does not meaningfully change our estimated TI effects.

In addition to difference-in-RD, we also estimate a DD model in order to explore the potential effect of TI on students further away from the cutoff. The DD model:

$$Y_{it} = \alpha_0 + \alpha_1 T_{it} + \alpha_2 Post_{it} + \alpha_2 T_{it} * Post_{it} + \varepsilon_{it} \qquad (2)$$

compares the differences in student outcomes between students in different ACT score groups after the implementation of TI with similar differences before TI. α_2 estimates the DD impact on student outcomes and T_{it} can accommodate various student groups depending on their score

[17] Some students took the ACT more than once before the 12th grade. Unobserved factors, such as motivation, that influence test re-taking behavior may affect both the likelihood of passing the threshold after retesting and future education outcomes. To avoid this type of endogenous sorting, we use the required sitting for ACT in March of students' junior years scores as the running variable when all students take the test at the same time.
[18] We rely on graphical inspection because McCrary's test for distributional discontinuity (2008) relies on local linear regressions that might lead to incorrect inferences when the running variable is discrete (Card & Lee, 2008).

range, with the reference group defined as students scoring just above the benchmarks to mimic the RD samples (i.e., ACT math 19−21 and ACT English 18−20).[19] The validity of DD estimates of the TI effect relies on the assumption that, in the absence of TI, pre-treatment differences across student groups would have remained the same in the post-treatment period. In our analysis, we include two "treatment group" indicators in the vector T_{it}: students scoring just below the benchmarks (ACT math 16–18 or ACT English 15–17) and students scoring further below the benchmarks (ACT math 13–15 or ACT English 12–14).

5. Findings

Empirical findings are presented in four sets of tables. Tables 6a-6b display group means of student outcomes that we will refer to from time to time when discussing the estimated TI effects in order to put those estimates into context. Tables 7−9 present the estimated TI effects on high school completion and college enrollment rates. Although TI was not designed to affect high school-to-college transition outcomes, it is important to examine the extent to which the program may have altered the composition of entering college students, a potential mechanism through which TI may affect student college outcomes. Next, conditional on college enrollment Tables 8–9 present the estimated TI effects on various college outcomes. Finally, Table 10–12 present findings for student subgroups as well as an exploratory analysis of the correlation between intervention type and student outcomes.

[19] The "just below" group resembles the RD sample that uses a bandwidth of three, and the α_2 estimate for this group can be used as a further check on the robustness of difference-in-RD estimates. The main difference between the DD and difference-in-RD estimates is that our DD model assumes the student outcome to be constant within each student score group, whereas the RD framework allows for flexible relationship between the student outcome and the test score within each score group. The DD model is more restrictive, but it may also be less risky in situations where a limited number of data points are available to estimate a relationship. See a related discussion in Bloom (2003).

5.1. High School-to-College Transition

The high school graduation rate among 11th grade students, at more than 90 percent, is high for all student groups in the analytic sample, which is broken down by ACT test score range, subject, and pre- and post-treatment periods in Tables 6a and 6b. The graduation rate in our samples is higher than the published high school graduation rate for Kentucky,[20] likely because Kentucky's compulsory school leaving age at the time was 16 and most high school dropouts would likely have occurred before the spring of the 11th grade. Between one-third to two-thirds of 11th grade students in our sample enrolled in college immediately after high school graduation. Unsurprisingly, higher ACT scores are associated with higher high school graduation rate and college enrollment rate, and the association appears the strongest between ACT scores and the rate of enrollment in 4-year institutions.

In order to understand whether the implementation of TI induced more marginal students to graduate high school and enroll in college, we present the estimated TI impact on these outcomes in Table 7. The estimated effects of math TI are shown in the top panel of the table, and the estimated effects of English TI are in the bottom panel. Difference-in-RD estimates, presented in the first three columns of Table 7 with varying bandwidths, show no detectable effect of high school math remediation on high school completion or college enrollment. High school English remediation, on the other hand, appears to lower the overall college enrollment immediately after high school graduation by 3–4 percentage points. However, the estimated impact becomes statistically insignificant when enrollment is examined for 2- and 4-year institutions separately. These results are largely consistent with DD estimates (the last two

[20] During the study period, Kentucky graduated 88 percent of all its high school students, with nearly 70 percent of the state's school districts reporting graduation rates of 90 percent or higher. See http://www.americaspromise.org/resource/all-kids-how-kentucky-closing-high-school-graduation-gap-low-income-students

columns of Table 7) for students scoring just below the benchmark scores (16–18 in math and 15–17 in English). Among students with much lower scores (13–15 in math and 12–14 in English), participation in high school remediation in either subject may have lowered their 2-year college enrollment by 2–3 percentage points, whereas participation in high school math remediation seems to have improved the chances of enrolling in 4-year universities. Overall, TI does not appear to have changed the composition of entering college students in significant ways.

5.2. College Outcomes

Figure 3 depicts the time trends of the rate of remedial course enrollment in college, the enrollment rate in introductory credit-bearing courses and the passing rate in credit-bearing courses. The trends are separately presented by ACT score range, subject and institution type (2- and 4-year). Among college students who scored below the 11th-grade ACT benchmarks, including those scoring far below the benchmarks, there is a clear decrease in the likelihood of remedial course enrollment in the first year in college. For example, among 2-year college students, even though students scoring just above the ACT benchmarks have been taking college remedial courses at a relatively steady rate over a seven-year period (about 10–14 percent), the percentage of college students scoring just below the ACT benchmarks (16–18 in math and 15–17 in English) who enroll in remedial courses has decreased from 50 percent to 30 percent in math and from 40 percent to 20 percent in English.[21] We observe similar time trends in 4-year universities, where the remedial course enrollment rates of students scoring above and below the

[21] As the time between the original policy change and the cohort in question increases, it becomes more difficult to attribute change in developmental course-taking patterns in college to the introduction of TI as they could reflect other simultaneous efforts to reduce remediation at the postsecondary level. Alternatively, it is possible that the effects of TI increased over time as the program matured and schools had more experience with implementation. Because the biggest declines in developmental course-taking are in the later years, omitting them from our results yields attenuated estimates. Nonetheless, our estimates are still statistically significant when restricting the sample to smaller windows after the policy change.

ACT benchmarks have converged as a result of decreasing likelihood of students scoring below the cutoff enrolling in college remedial courses.

Low-scoring students appear to use some of the extra time gained through reduced remediation needs to take credit-bearing courses in the freshman year, leading to an increase in college course enrollment rate in math and, to a lesser extent, in English. However, the passing rate of college math has not changed much for students scoring below the math benchmark relative to those scoring above the benchmark, suggesting that not all students who were induced to take college math were ready for those courses.

We present the causal estimates of the TI effect on student college outcomes in Table 8 for math and in Table 9 for English. The columns are organized by institution type (2-year vs. 4-year) and model specification (difference-in-RD with three bandwidths, followed by DD estimates for students just below and far below the ACT benchmarks). The rows are organized by college outcomes grouped into course-taking outcomes in college (the top two panels) and more general outcomes that describe student progress through college (the bottom panel).

Difference-in-RD estimates in Table 8 show that, among students scoring in close vicinity around the ACT math benchmark, being referred to math remediation in high school reduces the likelihood of college math remedial course enrollment by 5–10 percentage points in 2-year institutions and by seven percentage points in 4-year institutions. High school English remediation has similar, albeit weaker, effects, reducing the likelihood of college remedial English enrollment by 3–4 percentage points (Table 9). The difference-in-RD estimates can be better understood by an inspection of Figure 4, which plots student outcomes on the y-axis against ACT scores on the x-axis for students around the cutoff and compares their relationship in the pre-treatment (represented by circles) period with the relationship in the post-treatment (represented by triangles) period. It reveals that TI helps reduce college remediation needs by significantly narrowing the remediation gap that existed before the introduction of TI between students just above and below the ACT cutoffs.

The next six rows of Tables 8 and 9 present the estimated TI impact on the enrollment and passing rate of college courses. Difference-in-RD estimates show that, among 4-year university students scoring just below the ACT benchmark, TI increases the likelihood of enrollment in college math courses in the freshman year by four percentage points (Table 8). For these same students, TI also increases the overall passing rate of college math by a similar, four percentage points by the end of both the first and the second year in college. By comparison, there is no consistent evidence that TI has any impact on the enrollment and passing rates of college math among 2-year college students. Instead, math TI increases the first-year enrollment rate in college English by 3–7 percentage points, suggesting that 2-year college students may have opted to spend the extra time saved from math remedial courses to fulfill college English requirements. In contrast to math TI, there is no evidence that English TI has any detectable effects on the likelihood of enrolling in or passing college courses in either English or math in any types of institutions (Table 9).

The bottom panel in Tables 8 and 9 show the estimated TI effects on credit accumulation by the end of the first two years in college and college persistence. There is no detectable TI effect on any of these measures of progress through college. The lack of statistical significance is not entirely surprising because the expected impact of TI on credit accumulation is much smaller than what statistical power would allow us to detect. Take math TI among 4-year university students as an example. Difference-in-RD estimates suggest a reduction of math remedial course enrollment rate of seven percentage points (Table 8) among students scoring just below the ACT threshold (i.e., 16–18 if we assume a bandwidth of three), representing a 18 percent drop in remedial math enrollment in this student group.[22] These students, who would have had to take remedial math and earned an average of 23 credits by the end of the freshman year before the introduction of TI (Table 6a), can now earn three additional credits. Even if all of them took and successfully completed three additional college credits, the average number of earned credits for this student group (i.e., ACT math 16–18) would now be 0.18*(23+3) + 0.82*23=23.54, a mere half-credit increase (equivalent to 0.06 standard deviation in earned credits by the end of freshman year, which is 9.09).

The jump in the TI participation rate around the ACT benchmarks is 40 percentage points, and so the treatment effects among the treated (TOT) are likely larger than the intent-to-treat (ITT) effects reported above. A back-of-the-envelope calculation (i.e., dividing ITT estimates by 0.40) shows that, among students who participated in interventions, TI reduced the likelihood of remedial course enrollment by 13–25 percentage points in math and 8–13 percentage points in English; it also increased the likelihood that treated students enrolled in and passed college math in 4-year universities by 10 percentage points.

Tables 8 and 9 also present DD estimates of the TI impact for the same set of student outcomes as those investigated using difference-in-RD. As discussed in the methods section, here we compare the pre-post changes in student outcomes across student groups, with students scoring just above the ACT benchmarks (ACT math 19–21 or English 18–20) serving as the reference group. Each model includes indicators for two separate treatment groups: students scoring just below the benchmarks (ACT math 16–18 or English 15–17) and students scoring far below the benchmarks (ACT math 13–15 or English 12–14). The estimated TI impacts for both

[22] Forty percent of students in the ACT math 16–18 group took remedial math in the pretreatment period (Table 6a), and so a seven percentage points reduction is equivalent to a 100*7/40=18 percent change.

groups of low-scoring students are presented next to each other in Tables 8 and 9. The main purpose of the DD estimates for the "just below" students is to check the robustness of the difference-in-RD estimates of the TI effect, and these two sets of estimates are mostly consistent: DD estimates confirm that, among students who score just below benchmarks, being identified for high school remediation leads to significant decreases in the likelihood of college remediation in both English and math. It also increases the likelihood of college math course enrollment in 4-year universities. However, unlike difference-in-RD estimates, DD estimates fail to detect significant improvement in the passing rate of college math among 4-year university students.

Results from DD also suggest that the estimated TI benefits are not limited to marginal students who just miss the ACT benchmarks. We find similar effects among lower-scoring students as well. For students scoring well below the benchmarks, TI reduces the likelihood of remediation in college by 10 or more percentage points in both math and English, and it increases the enrollment rate of credit-bearing math and English by 6–7 percentage points in both 2- and 4-year institutions.[23]

5.3. Student and School Characteristics as Moderators

In this section, we investigate possible variation in TI impact on student college outcomes among students who are underprepared in multiple subjects, minorities students, FRL-eligible students, and students in low-performing schools. There are reasons to believe that high school remediation may have differential impacts among these student subgroups. For example, the

[23] If high school remediation was successful and it reduced the need for remediation in college, as is the case with TI, it would also change the mix of students enrolled in college remedial courses simply because those who still need remediation despite having received interventions in high school are likely the lowest-performing. Consequently, a possible unintended consequence of a successful high school remediation program is a lower passing rate of college remedial courses. A quick calculation based on group statistics in Tables 6a and 6b supports this possibility: Before the implementation of TI, 65 percent of remedial math students in two-year colleges scoring just below the benchmark passed the course compared to 57 percent after the introduction of TI.

summary statistics in Table 1 show that many students who fail the ACT benchmark in one subject also tend to receive interventions in another subject. The demand for time and resources is likely higher for students who need remediation in multiple subjects than students who miss the benchmark in only one subject, potentially lowering the effectiveness of intervention in any single subject. On the other hand, since academic proficiency (and deficiency) is often correlated across subjects and cross-subject benefits of interventions are widely documented (e.g., Master, Loeb & Wyckoff, 2017), interventions in multiple subjects may reinforce one another and strengthen the effects of each intervention.

TI may also have differential impact among minority and FRL-eligible students. This is because part of the effect of high school remediation is thought to be achieved through labeling students as not on track to be college-ready (Mokher et al., 2018). However, being labelled as not ready for college will motivate higher levels of student effort *only when* success is considered a goal that is attainable and when resources to help those students to catch up are available (Ou, 2012; Papay et al., 2011). These conditions are often lacking for FRL and minority students. In addition, research suggests that Black students may be particularly susceptible to discouragement effect from being labelled as not college-ready (Dougherty, 2015).

Finally, the effectiveness of TI could potentially vary by school performance level. The percentage of students failing to meet the ACT benchmark at the school level ranges from 0 to 100. It is plausible that schools with large numbers of students failing to meet benchmarks could become overwhelmed by the need to tailor interventions to each student, so the intervention would become less targeted. On the other hand, higher demand for remediation may create economies of scale that allow schools to secure resources that are unaffordable to schools with few remediation students.

In Tables 10 and 11, we present results that investigate the potential heterogeneity of TI impact on student outcomes by the four student and school characteristics discussed above. To limit the size of the tables, we choose to focus on three first-year college outcomes that are most directly targeted by the TI program: the likelihood of students enrolling in college remedial courses and the likelihood of enrolling in and passing credit-bearing college math and English. These tables are structured in a way similar to Tables 8 and 9, with columns representing model specifications. We test for statistical equivalence between subgroup estimates and their corresponding population estimates, and bold and italicize cells where estimates are significantly different.

Most subgroup estimates of the TI effects are qualitatively similar to the average effects reported in Tables 8 and 9. The most consistent finding is that TI significantly reduces the need for college remediation in nearly all student subgroups (with the exception of remedial English enrollment in 2-year colleges among FRL-eligible students and students who have failed benchmarks in multiple subjects). In very few instances, subgroup estimates are statistically different from population estimates. Specifically, math TI raises the enrollment rate of credit-bearing math during the freshman year among FRL-eligible students in 4-year universities by about 10 percentage points (compared to four percentage points on average), and English TI increases the enrollment rate of credit-bearing English among Black and Hispanic students in 2-year colleges by at least 12 percentage points (compared to an estimated zero effect on average).

In some other instances, apparent differences in point estimates between student subgroups and the whole sample are not statistically significant due to large standard errors. Such differences concentrate mainly among the estimated TI impact on the likelihood of passing college-level courses. Notably, for students who fail benchmarks in multiple subjects and students from high schools with the highest concentration (top quarter) of remedial students, being identified for math TI has a null effect on the likelihood of passing college math in 4-year universities (where the average effect is a four-percentage-point increase) and a negative effect in 2-year colleges (where the average effect is null). Among FRL-eligible students, TI has a stronger than average positive effect (nine percentage points) on the passing rate of college math in 4-year universities, but a statistically significant negative effect (-7 to -8 percentage points) in 2-year colleges. For Black and Hispanic students, math TI fails to produce any detectable effects on college outcomes in 4-year universities where the population average effects are all significant. In contrast, in 2-year colleges where TI has null effects in the full sample, TI increases in the likelihood of college course enrollment of Black and Hispanic students by more than 10 percentage points in both math and English. Although the increase in the college English enrollment rate leads to a 6–13 percentage points increase in the passing rate of college English, a similar increase in college math enrollment fails to translate into discernible gains in the passing rate of college math.

5.4. Intervention Type

TI interventions were reported under three categories: transition courses, extended school services (ESS) and "other" interventions. Of particular interest to us is the impact of TI delivered through ESS and how it compares to the effect of transition courses. This is because ESS interventions, which average more than 100 minutes per week and affecting one-third of TI students, provides additional learning time for students who need help. This contrasts with other high school remediation programs (such as those evaluated in West Virginia, Florida, and Tennessee), which typically replaces regular high school coursework with remediation without adding additional learning opportunities. It is hardly conceivable that, without spending more time to study, underprepared students would be able to catch up with their more advanced peers. Indeed, abundant evidence shows positive causal effects of additional instruction and learning on student outcomes.[24]

Unfortunately, the investigation of potential heterogeneous effect by intervention type is hampered by two factors. First, our reliance on difference-in-RD to identify TI effects prevents us from estimating separate TI effects for each intervention type. Moreover, a fundamental concern is that the choice of intervention type is likely endogenous: Extra learning opportunities may have been provided to students after regular hours for those who needed more help based on factors that researchers cannot observe.

With these concerns in mind, we conducted an exploratory analysis by estimating an ordinary least squares regression for each college outcome, TI subject and postsecondary institution type, with standard errors clustered at the school level. Student outcome is regressed on indicator variables for ESS and other interventions (with transition courses as the reference intervention type), ACT test scores, FRL eligibility, gender and race/ethnicity. The analytic samples include the 2014 and 2015 cohorts of 11th-grade students who participated in the TI program and have available intervention data. Estimated coefficients on intervention type indicators are reported in Table 12. Results are mixed. Relative to transition courses, ESS is associated with larger increases (by four percentage points) in the passing rate of college math in 4-year universities. For English, however, ESS students are more likely than students who take transition courses to enroll in college remedial English (three percentage points in 4-year

[24] For example, Jacob and Lefgren (2004) find that summer school classes targeting low-performing students in Chicago elementary schools increase academic achievement in reading and mathematics and that these positive effects remain substantial at least 2 years following the completion of the program. Taylor (2014) and Cortes, Goodman and Nomi (2015) both find that adding a remedial math course to a regular math course in middle school has positive effects on test scores, though the immediate effects decayed quickly in a couple of years. And Figlio, Holden and Özek (2018) find significant benefits of additional reading instruction provided through extended school days on reading test scores.

institutions and five percentage points in 2-year colleges); ESS students are also four percentage points less likely to take introductory English in 2-year institutions, but the difference is not statistically significant.

6. Discussion and Conclusion

This study is part of a growing body of evidence that explores linkages from high school to college. Understanding these linkages is especially important given that high school and college are increasingly treated as a continuum as opposed to distinct educational processes in preparing students for college-level education. On the other hand, postsecondary institutions frequently admit students deemed not college ready, but react to the preparation level of students with various initiatives, including the developmental course requirements used as a dependent variable here.[25] On the other hand, getting all students ready for college has become the mantra for K-12 schools so college outcomes are an important indicator for the success of the K-12 system.

This study adds to the limited evidence about the extent to which high school remediation can help academically underprepared students succeed in college. We find evidence consistent with the goal set forth by Kentucky for the TI program: Being assigned to TI is estimated to lead to reductions in the likelihood that students take remedial courses in college and increases in the likelihood that 4-year university students will take and pass college math.

The TI program findings are sizable. The intent-to-treat estimates suggest that TI reduces the likelihood of remedial course enrollment by 5–10 percentage points in math and 3–4

[25] The co-requisite model is another strategy by which colleges react to students who are deemed not college ready. Under this strategy, students enroll directly in college-level courses with a paired support course. In 2016, over a-third of community colleges across the nation were offering co-requisite options in English and 16% in math (Scott-Clayton, 2017). The adoption of the co-requisite model has continued to grow at a quick pace, and in 2018, 15 states recommended or mandated co-requisite reforms (Rutschow, 2019). Kentucky introduced the co-requisite model in its community college system in 2019.

percentage points in English, and the program is estimated to increase the likelihood that 4-year university students take and pass college math during the freshman year by four percentage points. Importantly, only half of the students who scored below the ACT benchmarks were treated, suggesting treatment-on-the treated effects that are roughly twice as large. The estimated effects are similar in terms of reducing the need for college remediation among disadvantaged students, students in low-performing schools as well as students scoring far below college-ready benchmarks.

These effect estimates are also sizeable relative to the cost of the program and remedial courses (as an assumed necessity if the program did not exist). The resources associated with developing and implementing TI amounted to about $600 per treated student (Levin et al., 2020). By comparison, each 3-credit remedial course costs between $830 (in community colleges in Tennessee, Belfield, Jenkins, & Lahr [2016]) and $1,500 (national average across all institution types, Barry & Dannenberg [2016]).

On the whole, our findings suggest that the TI program helps address college readiness issues at the front end of the college pipeline. We did not find significant evidence that it had positive effects on progress through college (credit accumulation and college persistence). But, these findings should be interpreted with caution given that the estimated impact on early college outcomes may be too small to produce discernible changes in later college outcomes.

References

Angrist, J. D., Imbens, G. W., & Rubin, D. B. (1996). Identification of causal effects using instrumental variables. *Journal of the American statistical Association*, *91*(434), 444-455.

Attewell, P., & Domina, T. (2008) Raising the bar: Curricular intensity and academic performance. *Educational Evaluation and Policy Analysis*, *30*(1), 51–71.

Aucejo, E., Romano, T., & Taylor, E. S. (2019). *Does Evaluation Distort Teacher Effort and Decisions? Quasi-Experimental Evidence from a Policy of Retesting Students*. CEP Discussion Paper No. 1612. Centre for Economic Performance.

Bailey, T. (2012). Can community colleges achieve ambitious graduation goals? In A. Kelly & M. Schneider (Eds.), *Getting to graduation: The completion agenda in higher education* (pp. 73–101). Baltimore, MD: The Johns Hopkins University Press.

Bailey, T. & Jaggars, S. (2016). *When college students start behind*. The Century Foundation.

Bailey, T., Jeong, D. W., & Cho, S. W. (2010). Referral, enrollment, and completion in developmental education sequences in community colleges. *Economics of Education Review*, *29*(2), 255-270.

Barnett, E., Fay, M., & Pheatt, L. E. (2016). *Implementation of high school-to-college transition courses in four states*. New York, NY: Community College Research Center, Teachers College, Columbia University.

Barnett, E. A., Fay, M. P., Trimble, M. J., & Pheatt, L. (2013). *Reshaping the College Transition: Early College Readiness Assessments and Transition Curricula in Four States. A State Policy Report*. New York, NY: Community College Research Center, Teachers College, Columbia University.

Barnett, E. A., Chavarín, O., & Griffin, S. (2018). Math Transition Courses in Context: Preparing Students for College Success. CCRC Research Brief. *Community College Research Center, Teachers College, Columbia University*.

Barry, M. N., & Dannenberg, M. (2016). *Out of pocket: The high cost of inadequate high schools and high school student achievement on college affordability*. Education Reform Now.

Belfield, C., Jenkins, P. D., & Lahr, H. E. (2016). *Is corequisite remediation cost-effective? Early findings from Tennessee*. (CCRC Research Brief 62). New York, NY: Columbia University, Teachers College, Community College Research Center.

Bettendorf, L. J., Folmer, K., & Jongen, E. L. (2014). The dog that did not bark: The EITC for single mothers in the Netherlands. *Journal of Public Economics*, *119*, 49-60.

Bettinger, E. P., Boatman, A., & Long, B. T. (2013). Student supports: Developmental education and other academic programs. *The Future of Children, 23*(1): 93–115.

Bloom, H. S. (2003). Using "short" interrupted time-series analysis to measure the impacts of whole-school reforms: With applications to a study of accelerated schools. *Evaluation Review*, *27*(1), 3-49.

Boatman, A. (2012). *Evaluating Institutional Efforts to Streamline Postsecondary Remediation: The Causal Effects of the Tennessee Developmental Course Redesign Initiative on Early Student Academic Success*. An NCPR Working Paper. National Center for Postsecondary Research, Columbia University.

Boatman, A., & Long, B. T. (2018). Does Remediation Work for All Students? How the Effects of Postsecondary Remedial and Developmental Courses Vary by Level of Academic Preparation. *Educational Evaluation and Policy Analysis*, *40*(1), 29-58.

Boylan, H. R., & Trawick, A. R. (2015). Contemporary developmental education: Maybe it's not as bad as it looks. *Research & Teaching in Developmental Education*, *31*(2), 26.

Card, David and David S. Lee. (2008). Regression Discontinuity Inference with Specification Error. *Journal of Econometrics, 142* (2): 655-674.

Calcagno, J. C., & Long, B. T. (2008). *The impact of postsecondary remediation using a regression discontinuity approach: Addressing endogenous sorting and noncompliance* (No. w14194). National Bureau of Economic Research.

Chao, J. C., & Swanson, N. R. (2005). Consistent estimation with a large number of weak instruments. *Econometrica*, *73*(5), 1673-1692.

Chen, X. (2016). Remedial Coursetaking at U.S. Public 2- and 4-Year Institutions: Scope, Experiences, and Outcomes (NCES 2016-405). U.S. Department of Education. Washington, DC: National Center for Education Statistics. Retrieved [10/11/2019] from http://nces.ed.gov/pubsearch.

Chingos, M. M., Griffiths, R. J., & Mulhern, C. (2017). Can low-cost online summer math programs improve student preparation for college-level math? Evidence from randomized experiments at three universities. *Journal of Research on Educational Effectiveness*, *10*(4), 794-816.

Cortes, K. E., Goodman, J. S., & Nomi, T. (2015). Intensive math instruction and educational attainment long-run impacts of double-dose algebra. *Journal of Human Resources*, *50*(1), 108-158.

Dantas, R. N., Duarte, G., Neto, R. D. M. S., & Sampaio, B. (2018). Height restrictions and housing prices: A difference-in-discontinuity approach. *Economics Letters*, *164*, 58-61.

Dougherty, S. M. (2015). Bridging the discontinuity in adolescent literacy? Mixed evidence from a middle grades intervention. *Education Finance and Policy*, *10*(2), 157–192.

Edgecombe, N. (2011). Accelerating the Academic Achievement of Students Referred to Developmental Education. CCRC Working Paper No. 30. *Community College Research Center, Columbia University*.

Eggers, A. C., Freier, R., Grembi, V., & Nannicini, T. (2018). Regression discontinuity designs based on population thresholds: Pitfalls and solutions. *American Journal of Political Science*, *62*(1), 210-229.

Fay, M. P., Barnett, E., & Chavarín, O. (2017). *How states are implementing transition curricula: Results from a national scan.* New York, NY: Columbia University, Teachers College, Community College Research Center.

Figlio, D., Holden, K. L., & Ozek, U. (2018). Do students benefit from longer school days? Regression discontinuity evidence from Florida's additional hour of literacy instruction. *Economics of Education Review*, *67*, 171-183.

Flory, M., & Cramer, E. (2017). Participation in Kentucky's College Preparatory Transition Courses: An Update. REL 2017-211. *Regional Educational Laboratory Appalachia*.

Goudas, A. M., & Boylan, H. R. (2012). Addressing flawed research in developmental education. *Journal of Developmental Education, 36*(1), 2–13.

Grembi, V., Nannicini, T., & Troiano, U. (2016). Do fiscal rules matter? *American Economic Journal: Applied Economics*, 1-30.

Grubb, W. N. (2013). *Basic skills education in community colleges: Inside and outside of classrooms*. New York City, NY: Routledge.

Hahn, J., Todd, P., & Van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression ‐ discontinuity design. *Econometrica*, *69*(1), 201-209.

Imbens, G., & Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of economic studies*, *79*(3), 933-959.

Jackson, C. K. (2017). *The Effect of Single-Sex Education on Test Scores, School Completion, Arrests, and Teen Motherhood: Evidence from School Transitions*. NBER Working Paper No. 22222. National Bureau of Economic Research.

Jacob, B. A., & Lefgren, L. (2004). Remedial education and student achievement: A regression discontinuity analysis. *Review of economics and statistics*, *86*(1), 226-244.

Jaggars, S. S., & Hodara, M. (2011). The Opposing Forces that Shape Developmental Education: Assessment, Placement, and Progression at CUNY Community Colleges. CCRC Working Paper No. 36. *Community College Research Center, Columbia University*.

Kentucky Department of Education and Kentucky Council on Postsecondary Education. (2010). *Unified strategy for college and career readiness: Senate Bill 1 (2009)*. Retrieved from http://education.ky.gov/educational/CCR/Documents/CCRUnifiedPlan_draft.pdf.

Kolesár, M., & Rothe, C. (2018). Inference in regression discontinuity designs with a discrete running variable. *American Economic Review*, *108*(8), 2277-2304.

Levin, J., Carbuccia, M., Adelman-Sil, E., & Danks. A. (2020). *Kentucky targeted intervention program cost study*. Washington, DC: American Institutes for Research.

Master, B., Loeb, S., & Wyckoff, J. (2017). More Than Content: The Persistent Cross-Subject Effects of English Language Arts Teachers' Instruction. *Educational Evaluation and Policy Analysis, 39*(3), 429-447.

McCaffrey, D. F., & Bell, R. M. (2002). Bias reduction in standard Errors for linear and generalized linear models with multi-stage samples. In *Proceedings of Statistics Canada Symposium* (pp. 1-10).

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, 142(2), 698–714.

Melguizo, T., Bos, J. M., Ngo, F., Mills, N., & Prather, G. (2016). Using a regression discontinuity design to estimate the impact of placement decisions in developmental math. *Research in Higher Education*, *57*(2), 123-151.

Mokher, C. (2014). *Participation and pass rates for college preparatory transition courses in Kentucky* (REL 2014–009). Washington, DC: U.S. Department of Education, Institute of Education Sciences, National Center for Education Evaluation and Regional Assistance, Regional Educational Laboratory Appalachia. Retrieved from http://ies.ed.gov/ncee/edlabs.

Ou, D. (2010). To leave or not to leave? A regression discontinuity analysis of the impact of failing the high school exit exam. *Economics of Education Review, 29*(2), 171–186.

Papay, J. P., Murnane, R. J. & Willett, J. B. (2011). How performance information affects human-capital investment decisions: The impact of test-score labels on educational outcomes. *NBER Working Paper No. 17120*. Cambridge, MA: National Bureau of Economic Research.

Pheatt, L. E., Trimble, M. J., & Barnett, E. (2016). *Improving the transition to college: Estimating the impact of high school transition courses on short-term college outcomes*. New York, NY: Community College Research Center, Teachers College, Columbia University.

Rutschow, E. Z. (2019). *Developmental mathematics reforms*. The National Academies of Sciences, Engineering, and Medicine Workshop on Understanding Success and Failure of Students in Developmental Mathematics. Washington, DC: Board on Science Education.

Scott-Clayton, J., & Rodriguez, O. (2015). Development, discouragement, or diversion? New evidence on the effects of college remediation policy. *Education Finance and Policy*, *10*(1), 4-45.

Scott-Clayton, J. (2017). *Evidence-Based Reforms in College Remediation are Gaining Steam—And So Far Living Up to the Hype*. Washington DC: Brookings Institution.

Sparks, D. & Malkus, N. (2013). First-year undergraduate remedial coursetaking: 1999-2000, 2003-04, 2007-08. *Statistics in Brief (NCES 2013-013)*. Washington, DC: National Center for Education Statistics, Institute of Education Sciences, U.S. Department of Education.

Taylor, E. (2014). Spending more of the school day in math class: Evidence from a regression discontinuity in middle school. *Journal of Public Economics*, 117, 162-181.

Xie, E. (2015). The Influences of New Rural Pension Scheme on Elderly Labor Supply and Well-being in Rural Areas. *Journal of Finance and Economics*, *8*, 39-49.

Figures and Tables

Figure 1. Math Targeted Intervention (TI) Participation by ACT Math Score

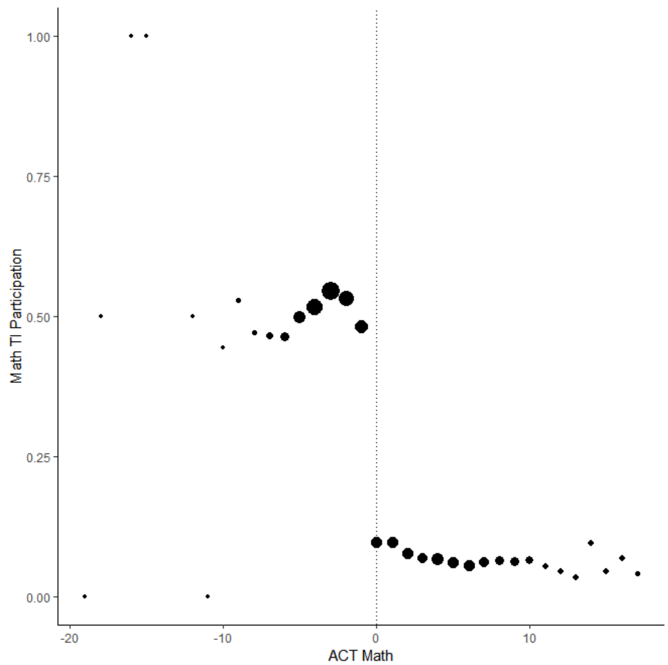

Figure 2. Distribution of ACT Test Scores in Kentucky, by Subject

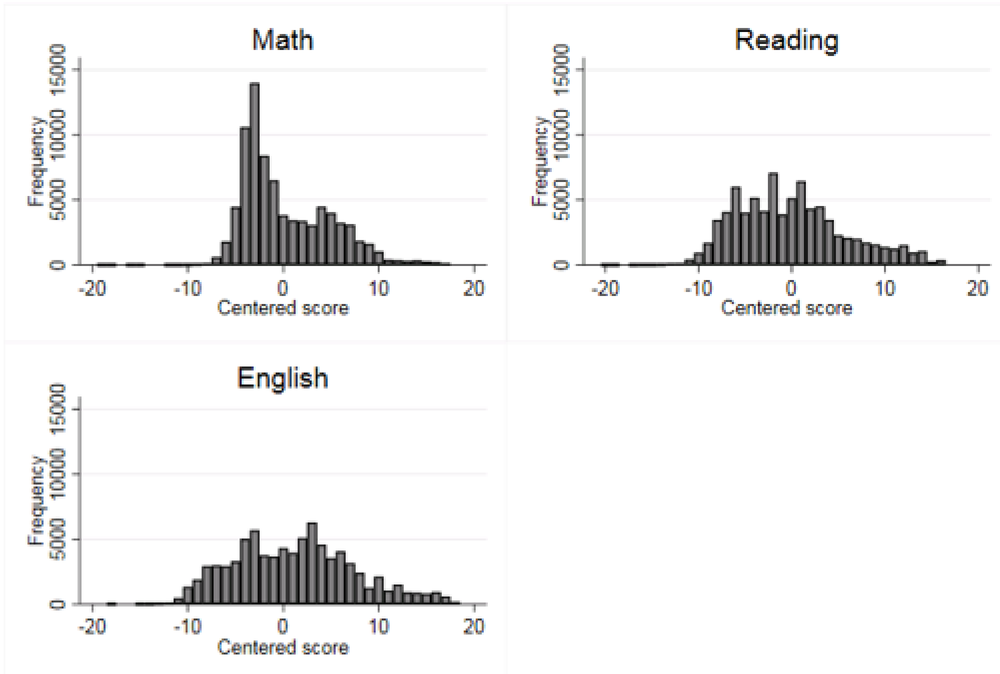

Figure 3. Trend in College Outcomes, by Institution Type, Subject, and ACT Scores: 2009-2015

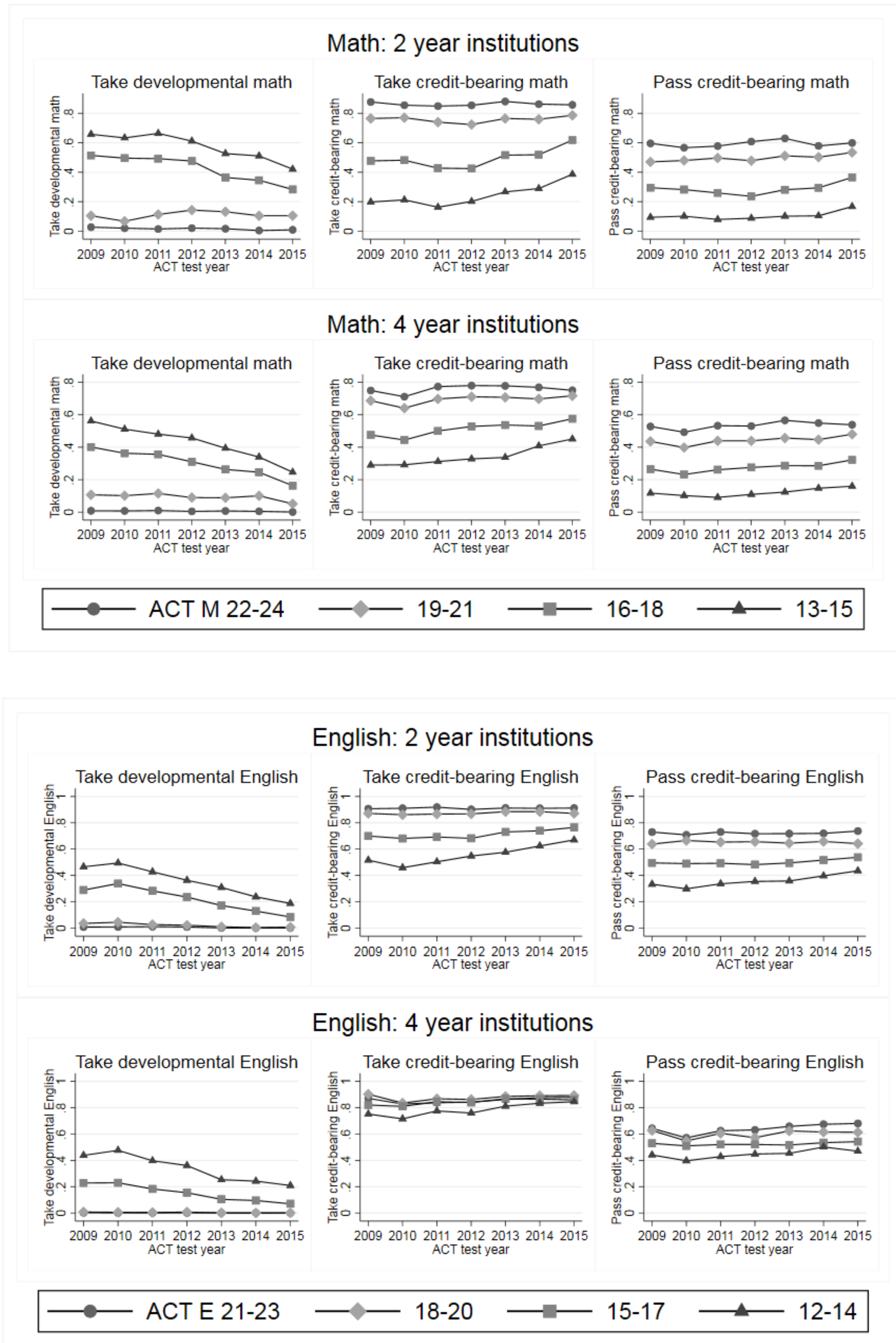

Figure 4. College Outcomes by ACT Test Scores, by Subject and Institution Type

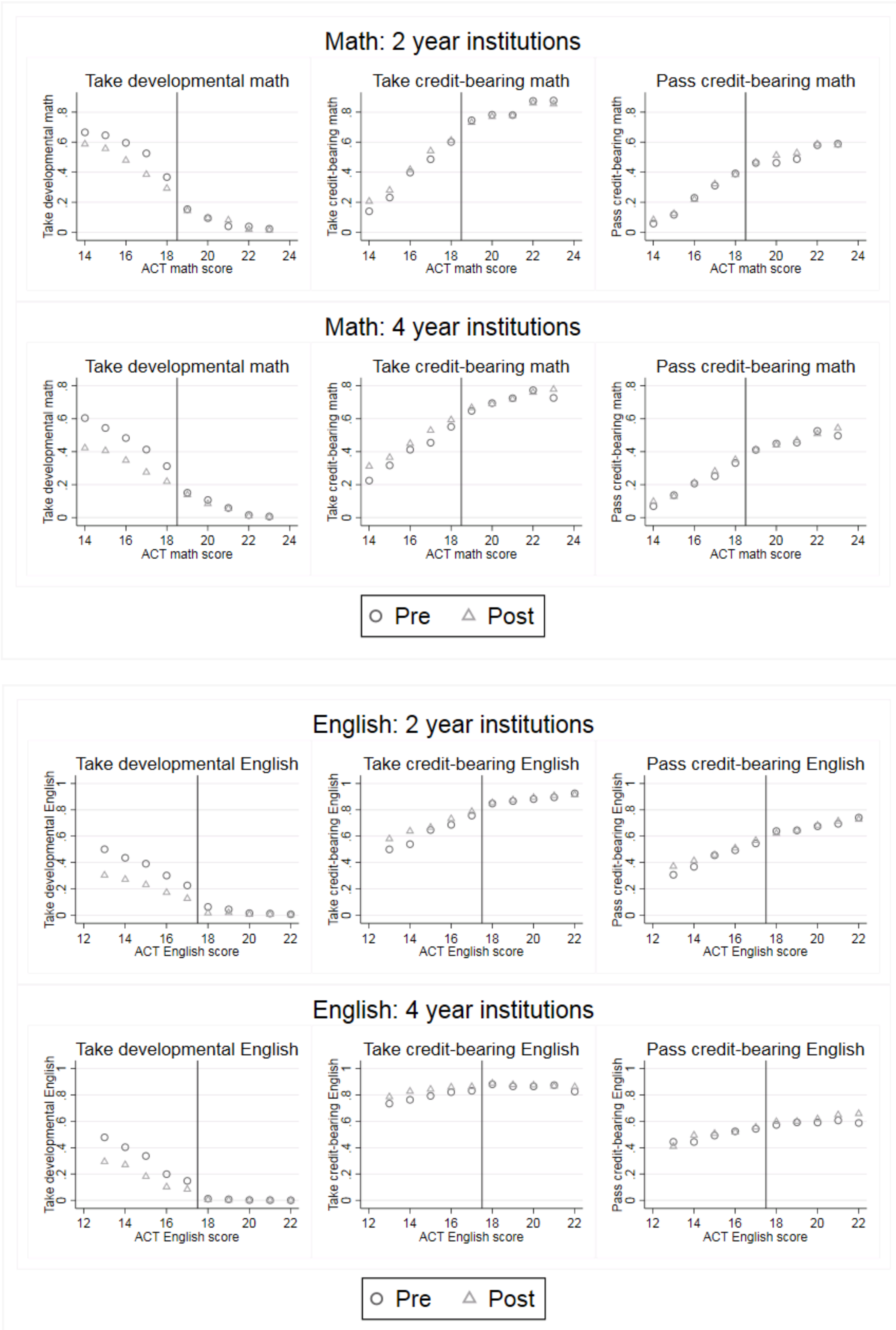

Exhibit 1. Existing Studies of Statewide High School Remediation: Program Features and Evaluation Findings

State	Subject	Screener	Main objective	Findings
WV (Pheatt et al. 2016)	Math	Not directly aligned with college placement	Test preparation for college placement assessments	No effect on passing the placement test and negative effect on passing college math.
FL (Mokher et al., 2018)	Math and English	2-stage screening, with 2nd stage using college placement tests	Test preparation for college placement assessments	No effect on the likelihood of college remediation placement, or the enrollment and passing rates of college-level courses.
TN (Kane et al., 2019)	Math	Same as college placement tests	Automatic exemption from college remediation upon successful completion of high school remediation	Lowered college remediation placement rate by 30 percentage points, small increase in enrollment rate in college math, but no impact on the passing rate of college math or credit accumulation.

Table 1. Targeted Intervention (TI) Participation Rate, by Test Subject, Intervention Content and Type: 2014 and 2015 11th Grade Students

	All	Math: At or above cut score	Math: Below cut score	English: At or above cut score	English: Below cut score
Total students with valid ACT score	87,811	35,040	52,771	49,373	38,438
Percent with record of TI	41	18	56	28	57
Among TI participants:					
CONTENT					
Math (%)	79	47	85	80	78
English (%)	48	39	50	18	67
TYPE					
Extended school services (ESS) (%)	31	54	26	37	27
Course (%)	63	38	69	55	68
Other (%)	20	14	22	17	23

Note: Less than 1.5 percent of TI participants have missing information on TI type. Percentages add up to more than 100 percent because students can receive TI in more than one content area and type.

Table 2. Characteristics of Targeted Interventions (TI), by Subject: 2014 and 2015 11th Grade Students

	Math	English
Deficiency area (% of intervention by subject)		
Algebraic Thinking	36	—
Math Reasoning	32	—
Math Computation	32	—
Writing Mechanics	—	40
Writing Content	—	32
Other TI characteristics		
Duration (Minutes/Session)	55	55
Frequency (Number of Sessions/Week)	4	4
Teacher-developed materials (%)	79	81
In-person delivery (%)	83	81
Successful exit of TI (%)	55	48

— Not applicable

Table 3. Pre- and Post-Treatment 11th Grade Cohorts

	2008-09	2009-10	2010-11	2011-12	2012-13	2013-14	2014-15	2015-16
Math	○	●	●	●	●	□	□	□
English	○	○	●	●	●	□	□	□

Note: Years refer to the time when a student was in the 11th grade.

○ Pre-treatment

● Post-treatment without intervention data

□ Post-treatment with intervention data

Table 4. Descriptive Statistics of Study Samples: 2009−2015

	Full Sample	RD Sample Math			RD Sample English		
		BW=3	BW=4	BW=5	BW=3	BW=4	BW=5
		ACT-M 16-21	ACT-M 15-22	ACT-M 14-23	ACT-E 15-20	ACT-E 14-21	ACT-E 13-22
Demographics							
Female (%)	50	52	52	51	51	51	51
White (%)	85	85	84	84	85	85	85
Black (%)	10	9	11	11	10	10	10
Hispanic (%)	5	5	5	5	5	5	5
FRL (%)	48	49	52	52	50	50	49
Test Scores							
ACT Math	18.8	17.6	17.3	17.3	17.7	17.8	17.9
	(4.5)	(1.6)	(2.1)	(2.6)	(2.8)	(3.0)	(3.1)
ACT Reading	19.3	18.7	18.2	18.1	18.0	18.1	18.2
	(5.9)	(4.6)	(4.7)	(4.9)	(3.6)	(3.8)	(4.0)
ACT English	18.4	17.9	17.2	17.0	17.4	17.5	17.6
	(6.3)	(4.5)	(4.9)	(5.1)	(1.8)	(2.4)	(2.8)

Note: Standard deviation in parentheses.

Table 5. Covariate Balance Check

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

Note: * and ** denote statistical significance at the 0.10 and 0.05 levels, respectively. Standard errors in parentheses. RD estimates assume linear association between the running variable and outcomes with a bandwidth of three. Results are robust to other bandwidth choices.

Table 6a. Group Means of Student Outcomes, by Treatment Period and ACT Math Score Range: 2009−2015

Student Outcomes	Pre-Treatment Period (2009) ACT 19-21	Pre-Treatment Period (2009) ACT 16-18	Pre-Treatment Period (2009) ACT 13-15	Post-Treatment Period (2010–2015) ACT 19-21	Post-Treatment Period (2010–2015) ACT 16-18	Post-Treatment Period (2010–2015) ACT 13-15
High school-to-college transition						
High school graduate (%)	98	96	92	98	96	92
Enroll in college immediately after high school (%)	71	57	34	67	52	30
2-year institution (%)	22	26	22	23	26	20
4-year institution (%)	49	31	12	44	26	9
Two-year college outcomes						
Take remedial math by end of 1st year (%)	12	52	60	12	42	54
Pass remedial math by end of 1st year (%)	9	34	31	7	24	27
Take credit-bearing math by end of 1st year (%)	71	40	15	71	45	21
Pass credit-bearing math by end of 1st year (%)	42	24	7	46	25	9
Take credit-bearing math by end of 2nd year (%)	78	51	24	78	54	30
Pass credit-bearing math by end of 2nd year (%)	51	34	13	53	33	15
Total credits earned by end of 1st year	22	17	12	24	19	13
Total credits earned by end of 2nd year	31	24	15	31	24	16
Return for 2nd year of college (%)	66	61	51	64	57	48
Four-year university outcomes						
Take remedial math by end of 1st year (%)	11	40	54	9	29	41
Pass remedial math by end of 1st year (%)	7	25	30	5	18	23
Take credit-bearing math by end of 1st year (%)	67	46	28	68	51	33
Pass credit-bearing math by end of 1st year (%)	42	25	11	43	27	11
Take credit-bearing math by end of 2nd year (%)	76	60	43	77	64	47
Pass credit-bearing math by end of 2nd year (%)	51	36	20	52	37	20
Total credits earned by end of 1st year	27	23	18	28	24	19
Total credits earned by end of 2nd year	43	36	28	43	36	27
Return for 2nd year of college (%)	83	77	69	79	73	65

Table 6b. Group Means of Student Outcomes, by Treatment Period and ACT English Score Range: 2009−2015

	Pre-Treatment Period (2009–10)			Post-Treatment Period (2011–2015)		
Student Outcomes	ACT 18-20	ACT 15-17	ACT 12-14	ACT 18-20	ACT 15-17	ACT 12-14
High school-to-college transition						
High school graduate (%)	97	95	94	97	96	94
Enroll in college immediately after high school (%)	66	55	40	62	49	35
2-year institution (%)	26	28	25	26	26	23
4-year institution (%)	40	27	16	35	23	13
Two-year college outcomes						
Take remedial English by end of 1st year (%)	5	30	43	2	18	29
Pass remedial English by end of 1st year (%)	3	19	27	1	11	18
Take credit-bearing English by end of 1st year (%)	82	63	44	85	68	54
Pass credit-bearing English by end of 1st year (%)	59	43	28	62	47	34
Take credit-bearing English by end of 2nd year (%)	86	71	54	88	74	62
Pass credit-bearing English by end of 2nd year (%)	64	51	37	66	53	42
Total credits earned by end of 1st year	21	17	13	22	18	15
Total credits earned by end of 2nd year	28	22	17	28	22	18
Return for 2nd year of college (%)	64	59	53	61	55	50
Four-year university outcomes						
Take remedial English by end of 1st year (%)	1	23	44	0	13	29
Pass remedial English by end of 1st year (%)	0	15	28	0	8	18
Take credit-bearing English by end of 1st year (%)	86	80	72	87	84	79
Pass credit-bearing English by end of 1st year (%)	58	51	41	60	52	45
Take credit-bearing English by end of 2nd year (%)	90	87	82	91	89	85
Pass credit-bearing English by end of 2nd year (%)	64	58	50	65	58	51
Total credits earned by end of 1st year	25	22	19	27	23	20
Total credits earned by end of 2nd year	39	34	29	40	34	28
Return for 2nd year of college (%)	79	75	71	75	71	65

Table 7. Estimated Effect of Targeted Interventions (TI) on High School to College Transition Outcomes, By Intervention Subject and Model

	Difference-in-RD estimates			DD estimates	
	BW=3	BW=4	BW=5		
Based on ACT math cutoff				ACT 16-18	ACT 13-15
High school graduate	0.01	0.01	0.01*	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Enroll in college immediately after high school	0.00	0.00	-0.00	-0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Enroll in 2-year institutions	0.00	-0.00	0.00	-0.01*	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Enroll in 4-year institutions	0.00	0.00	-0.00	0.01	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Based on ACT English cutoff				ACT 15-17	ACT 12-14
High school graduate	0.00	0.01	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Enroll in college immediately after high school	-0.04**	-0.04**	-0.03*	-0.02***	-0.02***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Enroll in 2-year institutions	-0.02	-0.02	-0.01	-0.02***	-0.03***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Enroll in 4-year institutions	-0.02	-0.02	-0.01	-0.00	0.01
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes. DD estimates use students scoring up to three points above the cutoff as the comparison and control for student ACT scores, FRL-eligibility, gender and race/ethnicity.

Table 8. Estimated Effect of Targeted Interventions (TI) in Math on College Outcomes

	2 year					4 year				
	Difference-in-RD			DD		Difference-in-RD			DD	
	BW=3	BW=4	BW=5	ACT 16-18	ACT 13-15	BW=3	BW=4	BW=5	ACT 16-18	ACT 13-15
College math outcomes										
Take remedial math by end of 1st year	-0.05*	-0.09***	-0.10***	-0.12***	-0.10***	-0.07***	-0.07***	-0.07***	-0.10***	-0.14***
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Take credit-bearing math by end of 1st year	0.04	0.03	0.02	0.04**	0.07***	0.04	0.04*	0.04**	0.04***	0.06***
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)
Take credit-bearing math by end of 2nd year	0.04	-0.00	-0.00	-0.00	0.05***	0.03	0.01	0.02	0.03**	0.06***
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Pass credit-bearing math by end of 1st year	-0.00	-0.02	-0.03	-0.03	-0.01	0.04*	0.04*	0.04*	0.01	0.00
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Pass credit-bearing math by end of 2nd year	0.01	-0.03	-0.04*	-0.04***	-0.00	-0.00	-0.02	-0.02	-0.02	-0.01
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Cross-subject outcomes										
Take credit-bearing English by end of 1st year	0.07***	0.05**	0.03*	0.03***	0.06***	-0.00	0.00	0.01	0.03***	0.05***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Pass credit-bearing English by end of 1st year	0.05	0.03	0.03	0.02	0.03*	0.01	0.01	0.01	0.01	0.03*
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
General college outcomes										
Total credits earned by end of 1st year	-0.03	-0.19	0.12	-0.38	-0.21	0.43	0.58	0.57	-0.04	-0.27
	(0.69)	(0.60)	(0.56)	(0.37)	(0.38)	(0.44)	(0.39)	(0.37)	(0.23)	(0.30)
Total credits earned by end of 2nd year	1.35	0.08	0.60	-0.69	0.12	0.94	1.16	1.12	0.46	-0.04
	(1.59)	(1.40)	(1.30)	(0.88)	(0.89)	(1.17)	(1.04)	(0.97)	(0.60)	(0.80)
Return for 2nd year of college	0.01	-0.01	-0.00	-0.02	-0.01	0.00	-0.00	-0.00	0.00	0.00
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes. DD estimates use students scoring up to three points above the cutoff as the comparison and control for student ACT scores, FRL-eligibility, gender and race/ethnicity.

Table 9. Estimated Effect of Targeted Interventions (TI) in English on College Outcomes

	2 year					4 year				
	Difference-in-RD			DD		Difference-in-RD			DD	
	BW=3	BW=4	BW=5	ACT 15-17	ACT 12-14	BW=3	BW=4	BW=5	ACT 15-17	ACT 12-14
College English outcomes										
Take remedial English by end of 1st year	-0.02	-0.04**	-0.04**	-0.11***	-0.14***	-0.00	-0.03*	-0.03**	-0.10***	-0.15***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Take credit-bearing English by end of 1st year	0.03	-0.01	-0.01	0.02*	0.07***	0.01	0.00	0.02	0.03***	0.06***
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Take credit-bearing English by end of 2nd year	0.03	0.00	-0.00	0.00	0.03**	-0.03	-0.04*	-0.01	-0.02*	-0.02
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)
Pass credit-bearing English by end of 1st year	0.04	0.01	-0.00	0.01	0.04***	-0.01	-0.03	-0.00	-0.01	0.02
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)
Pass credit-bearing English by end of 2nd year	0.01	0.00	-0.01	-0.00	0.01	-0.02	-0.02	-0.01	-0.01	0.00
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)
Cross-subject outcomes										
Take credit-bearing math by end of 1st year	0.03	0.00	-0.01	0.02	0.05***	0.02	0.02	0.03	0.01	-0.01
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)	(0.02)
Pass credit-bearing math by end of 1st year	-0.03	-0.04	-0.04	-0.02	-0.02	0.01	0.01	0.02	-0.01	-0.03**
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)
General college outcomes										
Total credits earned by end of 1st year	0.62	0.05	-0.04	-0.10	0.28	-0.40	-0.42	0.07	-0.31	-0.20
	(0.63)	(0.52)	(0.46)	(0.27)	(0.29)	(0.52)	(0.43)	(0.38)	(0.22)	(0.27)
Total credits earned by end of 2nd year	0.85	0.08	-0.23	-0.28	0.28	-2.26	-2.29**	-0.77	-0.98*	-0.91
	(1.45)	(1.21)	(1.07)	(0.64)	(0.66)	(1.39)	(1.16)	(1.03)	(0.58)	(0.71)
Return for 2nd year of college	0.02	0.01	0.00	-0.01	-0.01	-0.03	-0.03	-0.01	-0.02*	-0.03*
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Standard errors in parentheses. Difference-in-RD estimates assume linear association between the running variable and outcomes. DD estimates use students scoring up to three points above the cutoff as the comparison and control for student ACT scores, FRL-eligibility, gender and race/ethnicity.

Table 10. Estimated Effect of Targeted Interventions (TI) in Math on College Outcomes, by Student and School Characteristics

	2 year					4 year				
	Difference-in-RD			DD		Difference-in-RD			DD	
	BW=3	BW=4	BW=5	ACT 16-18	ACT 13-15	BW=3	BW=4	BW=5	ACT 16-18	ACT 13-15
Students who missed ACT cutoff in addition to math										
Take remedial math by end of 1st year	-0.06	-0.08***	-0.09***	-0.09***	-0.08***	-0.07**	-0.08***	-0.08***	-0.11***	-0.15***
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.04)	(0.01)	(0.02)
Take credit-bearing math by end of 1st year	0.00	-0.01	-0.02	0.01	0.04**	0.04	0.03	0.04	0.04**	0.07***
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)
Pass credit-bearing math by end of 1st year	-0.03	-0.04	-0.05	-0.03	-0.01	-0.02	-0.01	-0.02	-0.02	-0.02
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Free/reduced price lunch eligible students										
Take remedial math by end of 1st year	-0.02	-0.10**	-0.11***	-0.13***	-0.11***	-0.09**	-0.11***	-0.11***	-0.14***	-0.15***
	(0.05)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.02)	(0.03)
Take credit-bearing math by end of 1st year	-0.04	-0.02	-0.00	0.07**	0.10***	0.09**	***0.12****	***0.12****	***0.10****	0.08***
	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)	(0.05)	***(0.04)***	***(0.04)***	***(0.02)***	(0.03)
Pass credit-bearing math by end of 1st year	-0.08*	-0.08*	-0.07*	-0.05*	-0.03	0.09**	0.09***	0.09***	0.05**	0.03
	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)
Black or Hispanic students										
Take remedial math by end of 1st year	-0.04	-0.10	-0.07	-0.11**	-0.11**	0.03	-0.02	-0.03	-0.10***	-0.10***
	(0.10)	(0.08)	(0.07)	(0.05)	(0.04)	(0.06)	(0.05)	(0.05)	(0.03)	(0.03)
Take credit-bearing math by end of 1st year	0.12	0.12	0.11	0.11**	0.13***	0.02	0.02	0.02	0.04	0.08**
	(0.10)	(0.08)	(0.07)	(0.05)	(0.04)	(0.06)	(0.06)	(0.05)	(0.03)	(0.04)
Pass credit-bearing math by end of 1st year	0.00	0.01	0.03	0.03	0.04	-0.02	0.01	0.00	0.01	0.01
	(0.10)	(0.09)	(0.08)	(0.06)	(0.05)	(0.06)	(0.05)	(0.05)	(0.03)	(0.03)
Top quartile school in % students below cutoff										
Take remedial math by end of 1st year	-0.07	-0.16***	-0.17***	-0.15***	-0.08***	-0.09*	-0.07*	-0.06	-0.12***	***-0.21****
	(0.06)	(0.05)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)	(0.04)	(0.02)	***(0.03)***
Take credit-bearing math by end of 1st year	-0.01	-0.01	0.01	0.05	0.09***	-0.05	-0.00	-0.00	0.05*	0.07**
	(0.06)	(0.05)	(0.05)	(0.03)	(0.03)	(0.06)	(0.05)	(0.05)	(0.03)	(0.03)
Pass credit-bearing math by end of 1st year	-0.07	-0.08	-0.08*	-0.05	-0.02	-0.04	-0.02	-0.04	0.01	0.04
	(0.06)	(0.05)	(0.05)	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Standard errors in parentheses. Bold and italic estimates are significantly different from the corresponding effects among all students. Difference-in-RD estimates assume linear association between the running variable and outcomes. DD estimates use students scoring up to three points above the cutoff as the comparison and control for student ACT scores, FRL-eligibility, gender and race/ethnicity.

Table 11. Estimated Effect of Targeted Interventions (TI) in English on College Outcomes, by Student and School Characteristics

	2 year					4 year				
	Difference-in-RD			DD		Difference-in-RD			DD	
	BW=3	BW=4	BW=5	ACT 15-17	ACT 12-14	BW=3	BW=4	BW=5	ACT 15-17	ACT 12-14
Students who missed ACT cutoff in addition to English										
Take remedial English by end of 1st year	-0.02	-0.04*	-0.04**	-0.10***	-0.14***	0.00	-0.03*	-0.03**	-0.10***	-0.15***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Take credit-bearing English by end of 1st year	0.04	-0.01	-0.00	0.02*	0.07***	-0.00	-0.00	0.01	0.03***	0.06***
	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Pass credit-bearing English by end of 1st year	0.04	0.01	-0.00	0.01	0.05***	-0.03	-0.04	-0.01	-0.01	0.03
	(0.03)	(0.03)	(0.02)	(0.01)	(0.01)	(0.03)	(0.03)	(0.02)	(0.01)	(0.02)
Free/reduced price lunch eligible students										
Take remedial English by end of 1st year	-0.01	-0.01	-0.02	-0.10***	-0.14***	-0.04	-0.11***	-0.09***	-0.14***	-0.16***
	(0.04)	(0.03)	(0.03)	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.01)	(0.02)
Take credit-bearing English by end of 1st year	0.07*	0.04	0.02	0.04**	0.08***	0.03	0.02	0.04	0.03*	0.05**
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)
Pass credit-bearing English by end of 1st year	0.04	0.02	0.01	0.01	0.03	-0.00	-0.03	0.01	-0.02	0.00
	(0.05)	(0.04)	(0.03)	(0.02)	(0.02)	(0.05)	(0.04)	(0.04)	(0.02)	(0.02)
Black or Hispanic students										
Take remedial English by end of 1st year	-0.18**	-0.19***	-0.14***	-0.14***	-0.11***	0.05	0.00	0.01	-0.07***	-0.11***
	(0.07)	(0.06)	(0.05)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)
Take credit-bearing English by end of 1st year	***0.24****	***0.13***	***0.12***	***0.09****	0.12***	-0.01	-0.05	-0.02	0.00	0.07***
	(0.08)	***(0.06)***	***(0.06)***	***(0.03)***	(0.03)	(0.05)	(0.04)	(0.04)	(0.02)	(0.03)
Pass credit-bearing English by end of 1st year	0.13	0.08	0.06	0.04	0.09**	0.06	0.02	0.07	0.01	0.04
	(0.09)	(0.07)	(0.07)	(0.04)	(0.04)	(0.07)	(0.06)	(0.05)	(0.03)	(0.03)
Top quartile school in % students below cutoff										
Take remedial English by end of 1st year	-0.05	-0.07*	-0.05	-0.12***	-0.15***	-0.00	-0.04	-0.06**	-0.12***	-0.14***
	(0.05)	(0.04)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)
Take credit-bearing English by end of 1st year	0.02	0.01	0.02	0.03	0.06***	0.07	0.06	0.07**	0.05**	0.07***
	(0.05)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)
Pass credit-bearing English by end of 1st year	-0.02	-0.03	-0.01	0.01	0.05*	-0.04	-0.02	-0.00	-0.02	-0.00
	(0.06)	(0.05)	(0.04)	(0.03)	(0.03)	(0.06)	(0.05)	(0.04)	(0.03)	(0.03)

Note: *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. Standard errors in parentheses. Bold and italic estimates are significantly different from the corresponding effects among all students. Difference-in-RD estimates assume linear association between the running variable and outcomes. DD estimates use students scoring up to three points above the cutoff as the comparison and control for student ACT scores, FRL-eligibility, gender and race/ethnicity.

Table 12. Correlation Between College Outcomes and Intervention Type, by Institution Type and Intervention Subject: 2014 and 2015 Cohorts of 11th Grade Students

	2 year		4 year	
Dependent Variable	ESS	Other	ESS	Other
Math intervention in high school				
Take remedial math by end of 1st year	0.02	0.00	-0.02	-0.02
	(0.02)	(0.03)	(0.02)	(0.02)
Take credit-bearing math by end of 1st year	0.01	0.01	0.03	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Pass credit-bearing math by end of 1st year	0.00	0.02	0.04*	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
English intervention in high school				
Take remedial English by end of 1st year	0.05*	0.02	0.03*	-0.02*
	(0.02)	(0.02)	(0.02)	(0.02)
Take credit-bearing English by end of 1st year	-0.04	0.01	0.00	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)
Pass credit-bearing English by end of 1st year	-0.01	0.04*	0.01	-0.02
	(0.02)	(0.02)	(0.03)	(0.03)

Note: Estimated coefficients on intervention type (ESS and other) reported in table with standard errors in parentheses. Standard errors are clustered at the school level. The regression samples include all TI participants from the 2014 and 2015 cohorts of 11th grade students for whom intervention data are available. College outcomes are regressed on indicator variables for intervention type (ESS or other, with transition courses as the reference intervention type) and control variables that include student ACT scores, FRL eligibility, gender and race/ethnicity. ** and *** denote statistical significance at the 0.05 and 0.01 levels, respectively.