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Community College Developmental Mathematics: Is More Better?

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Abstract

The place and prevalence of community colleges in our nation's higher educational system is difficult to overstate. The open admissions policies at community colleges that make it possible to have such inclusiveness do have curricular implications. Many students at community colleges arrive underprepared for college-level work. To address this issue, remediation (or developmental work) has become a central mission of these institutions.

A common method for distinguishing underprepared and college-ready students is by means of a standardized placement exam. The institutional policy that dictates the method for disaggregating students is referred to as the 'placement policy'. The process of setting the minimum placement scores for developmental classes is wrought with uncertainty. The task of setting the cutoff scores for placement becomes more difficult as the number of classes that comprise a developmental sequence increases.

This research employs a rich longitudinal data set of student-level data from a large multi-campus community college in the United States (hereafter called the College) that spans the period of 2000 to 2010. In 1999, the College adopted a nationally available exam known as the ACCUPLACER for developmental placement for math. A student's score on the ACCUPLACER solely determines whether remediation is needed and, when it is, the entry level in developmental sequence. In 2005, a change in placement scores at the College went into effect. This change in policy upwardly adjusted the cutoff scores for placement within the developmental math sequence. Due to the change in thresholds, many students were required to complete additional developmental coursework.

The study exploits differences in assignment within the developmental math sequence for students with very similar math placement scores. This research is unique in that it makes use of two different assignment rules, and thus is able to examine the impact of additional developmental math for students with different levels of proficiency. Unlike any other research to date, the establishment of a clear counterfactual is naturally created by using a change in a math placement threshold as the demarcation of two groups to be compared. Contrary to previous research, the focus of this study is on community college students of traditional college age who initially placed within the developmental math sequence rather than at the boundary of developmental and credit math classes.

For students in the immediate vicinity of the cutoff score between introductory algebra and intermediate algebra there is no evidence to suggest an increase in likelihood of enrolling in a credit math for those required to complete two semesters of developmental math coursework instead of one semester regardless of the cutoff scores used for placement. During the period of lower cutoff scores, there was weak evidence to suggest that students near the cutoff but on the low side (and therefore required to complete the additional class of developmental math) were less likely to become eligible for enrolling in a credit math class than those on the high side. For the short-term outcome of successfully completing a credit math, students placed using the lower cutoff scores did not experience a positive benefit from being required to complete the additional remedial math course. In the period of higher cutoff scores, there was weak evidence to suggest that students required to complete two instead of one developmental class were more likely to successfully complete a credit math class. For the short-term outcome of becoming eligible for credit math using higher cutoff scores for placement, students did not receive a benefit from completing the additional developmental math class. In short, more was not consistently better.

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1 Introduction

Community colleges play a prominent role in our nation's higher educational system. Over 7.2 million students attended community colleges in the fall of 2010 —a number that represents almost 40 percent of the total number of students attending college nationwide (Aud, S. et al., 2012 ,TA36-1). Just as tellingly, of the students who receive a baccalaureate degree in this country, half attended community college at some point along the way, according to data from the American Association of Community Colleges (AACC, n.d.). And beyond sheer volume, the community college's role as a socioeconomic stepladder is attested by the fact that many of the students educated in such institutions are those who might otherwise not have continued their education beyond the secondary level (McGrath & Spear, 1991). Community colleges provide access to higher education for many nontraditional students, minority students, first generation college students, and students of low socioeconomic status (National Center for Educational Statistics, 2008). These institutions enroll the largest number of low income and first-generation college students (Bailey, Jenkins, & Leinbach, 2005).

This inclusiveness and the open admissions policies at community colleges that make it possible do have curricular implications. The examination of the spectrum of income alone, as done by Cabrerra (2005) employing the 1980 sophomore cohort of the National Center for Education Statistics database High School and Beyond revealed that students from families of low socioeconomic status were more likely to attend community colleges and be in need of remediation ─ a reality that in turn suggests that the community colleges who open their arms to, among others, the economically disadvantaged are confronted with many students unprepared for college-level work. To address this issue, remediation (or developmental work) has become a central mission of these institutions (Bragg, 2001; Perin & Charron, 2006). McCabe (2000) equates remedial education at community colleges as a 'critical bridge' to success in life.

Based on national surveys, researchers have found that students who require remedial coursework are less likely to complete any type of credential at a community college (Bailey, Jenkins & Leinbach, 2005). Far too many of the students at community college become "stuck" and never complete their academic goals (Goldrick-Rab, 2010). The vast majority of students who enter developmental coursework never successfully complete the sequence of courses (Bailey et al., 2010). Based on data from the National Education Longitudinal Survey (NELS:88) only thirty percent of students successfully remediate in mathematics (Attewell, Lavin, Domina, & Levey, 2006). This in turn seems to be a piece of the puzzle that explains why graduation rates for community colleges are so low.

Given the long odds facing those students who find themselves subject to remediation, it is worthwhile to consider the institutional policies by which students are required or encouraged to take developmental coursework. Policies governing which students will be designated for remediation vary by institution (Perin, 2006). Assessing students for disaggregating college-ready from developmental students and placing them at time of entry is one method colleges have employed (Perin, 2006). The policy for remedial placements at community colleges may in fact disproportionately (and adversely) impact the very students who use the community college as the point of access for higher education, many for whom socioeconomic factors play a role in the outcome of the placement exam.

Bettinger and Long (2009) state that the placement exam "has become the key academic gate-keeper to postsecondary study". Further complicating the student's understanding of the process is the remedial sequence that becomes an "obstacle course" to complete before being permitted to take a college-credit math course[1] (Bailey et al., 2010). Jenkins and Cho (2012) believe that developmental students "may give up because they become discouraged with the drudgery of remedial instruction and do not see a clear pathway to success in college" (p. 11). Based on the U.S. Department of Education, National Center for Education Statistics (NCES) Beginning Postsecondary Students Longitudinal Study 2004 – 2009 (BPS:2009) data, 60% of beginning postsecondary students who entered higher education through two-year publics took at least one remedial class in their first year (NCES, 2011-275). In addition, about two-thirds of the students at community colleges who take developmental courses, spend a year or more in such courses" (Bailey, Jenkins, & Leinbach, 2005). Researchers have noted the lack of critical examination of the institutional policy related to placement in developmental courses and the effectiveness of such courses (Bailey, 2009; Bailey et al., 2010; Calcagno, 2007; Calcagno & Levin, 2008; Bahr, 2008; Oudenhoven, 2008; Grubb, 1999; Rosenbaum, Deli-Amen, & Person, 2006).

The research presented will take a step to fill the void that currently exists in the literature, by seeking to determine the impact on student success as a result of a change in institutional developmental placement policy for mathematics at a large multi-campus community college. While institutional policy for developmental classes is multi-faceted and includes numerous policies such as initial testing, re-testing, placement, and enrollment policies in the developmental courses, the aforementioned change in the policy affected the cutoff scores used for placement within the developmental mathematics sequence while maintaining the numerous other aspects of the placement policy. This characteristic of the singularity in the change in the overall institutional placement policy allows an isolation of the effects of the change in cutoffs without being entangled by other confounding factors. The policy change upwardly adjusted the placement cutoff scores for placement within the developmental mathematics sequence.

This research exploits differences in assignment within the developmental math sequence for students with very similar math placement scores. This study is unique in that it makes use of two different assignment rules, and thus is able to examine the impact of additional developmental math for students with different levels of proficiency based on the initial placement exam score. Unlike any other research to date, the establishment of a clear counterfactual is naturally created by using a change in a math placement threshold as the demarcation of two groups to be compared. Contrary to previous research, the focus of this study is on community college students of traditional college age who initially placed within the developmental math sequence rather than at the boundary of developmental and credit math classes. Among the questions answered: does more developmental coursework in math increase student success or does it simply increase the barriers barring access to higher education for the underprepared student? Simply stated, is more better?

The paper is organized as follows: Section 2 provides the background of the study institution; Section 3 discusses the creation of the analytical data set; Section 4 presents a primer on regression discontinuity

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[1] The term "credit-math" when used from this point forward will be used interchangeably with "college-credit math". College-credit math refers to college-level classes in the subject of mathematics that can be used for degree credit requirements. College-credit, credit-math, differs from classes that award institutional credits.

and specific details of its application in the empirical strategy employed; Section 5 presents student characteristics at the study institution; Section 6 presents the testing of underlying assumptions and the RD results; and Section 7 summarizes the findings of the research.

2 Background

2.1 Institutional Characteristics

The research employs a rich data set of student level data from a large community college, hereafter referred to as "The College." Its Carnegie Classification is that of Associate's – Public Suburban – serving Multi-campus. The data set for the study is derived from elements from several institutional data sets and used primarily for mandatory government reporting. The data sets employed are common to many institutional research database management arrangements. Distinct databases house student demographics records, course records, placement records, and grade records available by semester.

The College offers more than 100 programs of study that can lead to the following Associate degree types: Arts, Arts in Technology, Fine Arts, Science and Applied Science; credit certificates;[2] and non-credit career training certification and licensure. The College offers numerous state-designated degrees programs such as civil design, interpreter preparation, and mortuary science. Additionally, it offers health and workforce shortage programs such as emergency medical technology, and health informatics and information.

While the College practices open admissions, there are high-demand majors, such as nursing, that are competitive and require a separate department application beyond that of the general admissions application. The College has a large parallel enrollment program that allows students to attend classes and earn college-credit at the College while still in high school. In addition to parallel enrollment, the College has worked with the county public school system to develop bridge classes. The math bridge class is offered in high schools throughout the county and taught by a high school teacher. The bridge class in math allows a student who successfully completes the high school course and passes a College-written final exam the opportunity to bypass the placement exam at the College and register for a college-level math course.[3] The College offers numerous continuing education programs to appeal to people of all ages. These non-credit classes range from foreign language classes to home-improvement.

As is common with community colleges, the student body is diverse. The diversity is intensified by the several campuses and extension centers across the county. The College provides postsecondary education to more than half of the college-bound high school graduates within the county. In the academic year 2008, approximately 14 percent of credit students were under 20 years old and 54% were in the 20-29 age

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[2] Credit certificate programs typically require completion of approximately 30 college credits. The credits can be applied to an Associates degree. On average, an Associates degree requires successful completion of 60 to 70 credits.

[3] The math bridge class has had limited success since its inception. The average pass rate on the final exam for students participating in the bridge class has been less than 20%; thus, the majority of students who opt to attend the College, are still unable to avoid the placement exam.

bracket.[4] Approximately 60 percent of the students are female. Minority enrollment at the College exceeds 40 percent. Approximately 60 percent of the students at the College work at least 20 hours per week while attending college. Approximately 40 percent of the student body that applied for aid received financial aid with one-quarter of the student body received Pell grants. Approximately one-third of the credit students are first generation college students.[5]

Almost 75% of the entering first-time credit students in the fall of 2008 were identified with developmental education needs according to the College's Fact Book.[6] For the fall 2009 cohort of entering credit students, the percentage of students identified with developmental needs increased to 81%. The percentage of students requiring developmental education has steadily increased at the college in the study period.[7]

Using published data from the College's Research Office and focusing on the most recent cohort available,[8] in the fall 2006 cohort, less than 40 percent of students identified with developmental needs completed their developmental coursework after four years.[9] Approximately 51 percent of the fall 2006 cohort that completed the developmental sequence either graduated or transferred after four years.

The developmental noncompleters[10] in the fall 2006 cohort fared worse in measured outcomes. The graduation-transfer four-year rate for developmental non-completers is slightly above 25 percent. Successful persistence is defined as the "*percentage of first-time fall entering students attempting 18 or more hours during their first two years, who graduated, transferred, earned at least 30 credits with a cumulative grade point average of 2.0 or above, or were still enrolled, four years after entry.*"[11] Developmental noncompleters had a successful persistence rate after four years of approximately 42 percent as compared to the developmental completers' successful persistence rate of 84 percent.

The overall graduation rate for students , known as "Student Right-to-Know", available on IPEDS (Integrated Postsecondary Integrated Education Data System) is based on the number of students who began their studies as full-time, first-time, degree or certificate-seeking students to determine if they

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[4] The College experienced an increase in enrollment in the fall 2009. This 2009 increase in enrollment was a national trend.
[5] The designation of first-generation college student is applied to students for which neither parent attended college. This statistic is based on college-collected data and is calculated every even year in the spring by the Institutional Research Office. This information is submitted to the state for compliance and accountability purposes.
[6] 'Developmental need' implies placement into at least one of the three areas of developmental education – reading, English, or mathematics.
[7] A trend analysis of the percentage of students in the analysis set placing into developmental coursework is presented later in this paper.
[8] The cohort for the statistics presented is comprised of first-time credit students who enter in the fall 2006 semester. Transfer students are not included in the cohort. The students need not be full-time. The definition of the cohort for the College Fact Book is different than the Student Right-to-Know cohort definition.
[9] For the purpose of compliance reporting, a 'developmental completer' is a student who successfully completed all recommended developmental coursework or completed a college-level course in the recommended developmental area(s).
[10] 'Developmental noncompleters' are defined as students who did not complete all recommended developmental coursework.
[11] Reference not provided to maintain the anonymity of the location of the study institution.

complete a degree or award within 150% of normal time for completing the program for which they are enrolled. For the fall 2005 cohort at the study institution, the overall graduation rate was 10%. The transfer out rate, defined as the percentage of first-time full-time students who transferred to another institution, was 13%. Students who began their studies on a part-time basis or at another institution are not included in the above-mentioned statistics and thus interpreting the statistics without an understanding of the subpopulation can be misleading. At this institution, approximately one-third of first-time entering students are classified as full-time students.[12]

Disaggregating the data and examining the available data for African Americans reveal gaps between the overall student body at the institution and the subgroup examined. The successful persistence rate after four years for African American students in the fall 2006 cohort was 56% compared to the overall fall 2006 cohort rate of 66%. African American students, according to a state report, "*are disproportionately placed in developmental courses, and so they are at even greater risk of not completing a credential.*"[13] The graduation rate for African Americans of approximately 7% is less than half of that for the overall student population.

The developmental math courses are offered and taught by math faculty and coordinated within the Department by a faculty member, referred to as the Developmental Coordinator. The structure is such that the Coordinator reports to the Department Chair and ultimately to the Developmental Dean.[14] The majority of the developmental courses are taught by part-time faculty who are offered limited opportunity for professional development. The educational requirements for part-time faculty are lower than for full-time faculty. Part-time faculty are required to have earned a bachelor's degree in a non-specified field that may or not be a math-related field. Full-time faculty are required to possess, at minimum, a master's degree in math, math education, or a math-related field with a minimum number of graduate credits in math. Less than 15 percent of full-time math faculty have earned a doctoral degree.

The math developmental sequence consists of three courses: arithmetic, introductory algebra, and intermediate algebra.[15] The courses are offered in various formats: online, hybrid (a mixture of online and in-person), self-paced, and traditional lecture formats.[16]

2.2 ACCUPLACER and Institutional Placement Policy

Institutional policies determine the procedure for assessing and placing students within the developmental math sequence. The College requires mandatory assessment and placement. This policy requires the vast majority of new and transfer students to be assessed for placement in math courses. A student can qualify

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[12] This statement is based on available data from the College from the last five years.
[13] Reference suppressed to maintain anonymity of study institution.
[14] This type of organizational structure for developmental courses is referred to as "partially mainstreamed."
[15] Continuing Education courses are offered at the College for students whose math skills indicate the need for remediation in whole number arithmetic. Math faculty do not teach the Continuing Education classes nor are they coordinated by the Math Department.
[16] Students who enrolled in an online developmental course are excluded from the analysis sample. Based on previous research, online students may differ from traditional students on unobservable characteristics and therefore, inclusion of online students could bias results.

for exemption from the math placement test if he/she provides evidence of a qualifying SAT score or successful completion of a college-level math course. The placement policy of the College dictates that a student who places into a developmental class is required to successfully complete the developmental class(es) before registering for a credit math class.

A computer adaptive test (CAT) known as ACCUPLACER is used at many colleges to assess a student's ability and skill level.[17] The computer-adapted version of ACCUPLACER was adopted by the college in 1999. As stated in the Coordinator's manual for the exam, the test can be used "to assist with the determination of course placements that are appropriate for students" (CollegeBoard, 2007). CollegeBoard (2003) does not recommend using only the results of one test, such as ACCUPLACER, for placement decisions, but instead multiple measures. ACCUPLACER was developed and validated by CollegeBoard,[18] the same designer of the Advanced Placement exams and the Scholastic Aptitude Test (SAT). The exam consists of five different main/core tests and two supplementary math tests. The five main tests assess knowledge of reading comprehension, sentence skills, arithmetic, elementary algebra, and college-level math. In addition, there are two optional tests to assess math skills of students with low ability.[19] The exam is untimed. A calculator may or may not be used based on the individual institution's testing protocol.

The exam is designed using sophisticated computer algorithms based on a 3-parameter logistic model (3PL) item employing item response theory (IRT). The item parameters are difficulty, discrimination, and pseudo-chance probability. IRT allows for a set of different questions to be used for assessing the same skill and difficulty while still being able to provide a consistent scale for the outcome for the exam.

The examinee begins the exam by being presented with a midrange question in level of difficulty. Based on the outcome of the first question, the second question is adapted to be either a more difficult question or an easier question. The second question is randomly selected from a pool of questions that are of the same difficulty. Because the second question is randomly selected from the test bank of similar questions, no two tests are identical.[20] The goal of the CAT is to identify the respondent's true ability level, or latent ability. Due to the adaptive nature of the exam, the examinee has to complete fewer questions than a traditional paper-based exam.

During the administration of the math section, ACCUPLACER has the ability to offer 'seamless serial testing.' ACCUPLACER can be administered using a single subsection, such as elementary algebra, or multiple sections, such as elementary algebra and college-level math. If a college offers a single developmental math class, it may be sufficient to have the student complete a single section of the math

[17] The exam can also be administered in a traditional paper-pencil format as well; this format of the exam is referred to as the COMPANION test. The scores on the COMPANION test are typically adjusted for comparison with the CAT. Student's who completed the COMPAION test are not included in the study.
[18] CollegeBoard is a division of the Educational Testing Service, ETS.
[19] The tests for lower ability math assessment are approved for determination of the 'ability to benefit' criteria for students.
[20] Some colleges allow retesting using ACCUPLACER. The computer-adapted nature of the exam allows for the same student to take the exam without being administered an identical exam.

assessment for placement purposes.[21] However, if a college offers numerous developmental math courses, there may be the need for a better refinement of the student's ability to allow placement within the sequence. This feature of serial testing within ACCUPLACER allows an additional subsection of mathematics to be administered during the assessment phase.

A student may only need to be tested in one section and not all three, depending on the manner in which the Testing Administration has programmed ACCUPLACER. The ability to adjust the branching shortens the length of time for the exam since the maximum number of sections any test-taker will complete if the student begins in the introductory algebra section is two.[22]

From an administrative viewpoint, a CAT has advantages because it requires little in terms of man-hours because the exam is taken on the computer and offers ample flexibility in the manner in which the test is administered, including a single section or multiple sections. Hand scoring and paperwork is eliminated.

Further, a CAT's administration is only limited by the number of available computers, the exam can be adapted for site-specific use by adding questions to the test bank or collection of survey data on the respondents, the exam is relatively short in duration allowing a steady stream of test-takers to be accommodated, and scores are available immediately after completion. The College has the ability to adjust the manner in which ACCUPLACER is administered by aligning its testing protocol with the developmental sequence. The testing can be further refined by setting the starting section of the exam, as well as whether the test will allow for branching into other sections, and if so, at what scores the branching will occur.

Each test in the math portion of the core exams consists of relatively few questions compared to a traditional, paper-and-pencil exam. Specifically, the absolute number of questions per subsection is as follows: arithmetic, 17; elementary algebra, 12; college-level math, 20 questions.

The score for each exam is presented on a scale of 0 to 120.[23] The scaled score is based on the number of correct answers and the difficulty level of the questions. The score on the exam, referred to as the "Total Right Score", is a criterion-referenced measure. In other words, the score is an absolute score that is not based on a reference group of students being administered the exam and is independent of the skill-level of other test-takers. Thus, the technical manual notes the "use [of] this score [Total Score] for making student placement decisions, in computing summary statistics, in correlating test performance with other information in a student's records, and in other statistical treatments of the test data" (CollegeBoard, 2003, p. 19). The exam scores can also be presented in percentiles or as a range of values (confidence interval).

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[21] It is important to note that a college may opt to administer only the college level math assessment or only the arithmetic section for placement purposes. The section(s) to be offered is(are) determined by the college.
[22] The Administrator at the site sets the score on the elementary algebra section that prompts the branching to the college-level math section.
[23] The original number of questions in each pool for use in each core test was originally 120. The 120 no longer refers to the number of questions the student must answer per section (CollegeBoard, 2003, p. 19).

2.3 Study Institution and Policies

Under a state agreement, all public colleges within the state of the study institution follow a common cutoff for placement into developmental and college-ready courses.[24] Individual institutions within the state determine the number of courses within the developmental sequence and the cutoffs for the courses within the sequence. A student's placement scores are valid for a period of two years and by state agreement, must be accepted by all colleges in the state. If a student successfully completes the developmental math sequence at a community college, the student becomes eligible to register for a credit math course at a four-year state college or another community within the state.

In 1999, The College adopted the ACCUPLACER for developmental placement for math, reading and English. In addition, a policy was established on the minimum ACCUPLACER scores for placement within the developmental sequence. The policy included the manner in which the test would be administered and the retesting policy. Focusing on math placement policy, the testing began within the elementary algebra section. Seamless serial testing occurred for students who scored a minimum of 70 on the elementary algebra section. The subsequent section for the placement exam consisted of the college-level math sub-test. Students were allowed to retest.

In 2005, a change in placement scores at the College went into effect. The cutoff scores were modified based on the perceived 'misassignment' of the students. The Developmental Dean at the college was asked about the process for which the new placement scores were determined; the response indicated that the selection of the new cutoff scores was arbitrary.[25]

At the time of initial adoption of ACCUPLACER at the college, there was a not a minimum score, referred to as a floor, for the lowest level course in the developmental math sequence. Any student who scored below a 36 on the elementary algebra section was eligible to register for the lowest developmental math course in the sequence offered by the Math Department. In 2005, additional branching was added to the testing protocol such that the test branched to the arithmetic section once a low score[26] was received on the elementary algebra section. Thereafter, a minimum score on the arithmetic section was required to allow placement in the lowest course in the developmental math sequence. If a student scored below the floor, based on the arithmetic test score, the student was referred to Continuing Education. Once the student successfully completed the Continuing Education course, he/she could register for the lowest level developmental course offered in the Math Department without retesting.

[24] The state agreement details the required minimum score for credit math classes based on the college-math score of ACCUPLACER or the Computerized Adaptive Assessment and Support Systems Score, COMPASS. (COMPASS is a standardized math placement exam developed by Pearson.)
[25] The Institutional Research Office at the college was contacted to provide any documentation or research used to aid in the decision for the change in cutoff scores, but no report was available.
[26] The elementary algebra section score of 43 was designated as the score for which the test would branch to the arithmetic section.

Longitudinal student-level cohort data are available that straddle the policy change. Focusing on placements one and two level below credit math,[27] the policy change moved the minimum scores for placement one level below credit from 50 in the pre-policy period to 65 in the post-policy period (Tables 1 and 2). This change in policy upwardly adjusted the cutoff scores for placement two levels below credit by 15 points, which resulted in an additional semester of developmental math coursework for any student who scored within the range of 50 and 65 if the student entered the college in the post-policy period.

2.4 Retesting and Re-Enrolling

The retesting policy is another facet of the institutional policy for assessment and developmental placement. The college allows retesting for developmental math placement. A student may appeal the results of the placement test.[28] A student may opt to complete the ACCUPLACER exam a second time if a specified period of time has elapsed since the first attempt.[29] If upon receiving a second score and placement, the student is still not in agreement with the placement, he/she has the option to appeal the placement again by taking a challenge exam offered by the Math Department. The College staff has encouraged students to retest.[30] Students are not billed for placement testing. Thus, encouraging students to retest is an additional expense beyond that of the initial testing that the College must absorb.[31]

The College allows repeated attempts to complete a course in the sequence. The former policy, which spans the majority of the study period, allowed a student to continue to register for the same developmental math class. The current policy, which coincides with the post-policy period, allows a student to register for the same developmental class twice without requiring a special override for registration.[32] If a student is not successful on the second attempt, he/she must gain permission to repeat the course for the third time. A specially designated advisor for the developmental classes must grant permission for the third attempt. Only under extenuating circumstances will the College allow the student to attempt the course more than three times.

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[27] The focus of the research is at the divide of one and two levels below credit, thus, this example is limited to align with this research.

[28] The policy of retesting may be perceived by a student as a lack of confidence in the ability of the exam to correctly place the student. Given the importance of the exam, and the ability of the test results to derail a student's dreams and the opportunity to take college credits, the 'perceived message' is a dangerous one.

[29] The policy regarding the time between administrations of the exam requires an elapsed time between attempts of 24 hours.

[30] Students are made aware of the retesting option when discussing the placement scores with a college advisor. The retesting policy is provided in the written literature given to the student provided.

[31] The Testing Administrator at the College noted that ACCUPLACER billing is done by "units". If a student completes two sections on the math placement exams, the College is billed for two units. ACCUPLACER offers a discount for multiple-administration of the exam to the same student.

[32] If the student registers for the course, it is considered an attempt for the course. If the student later withdraws or stops attending the course, the attempt is still counted.

3 Analytical Set

3.1 Data

A rich data set of student level data from the College is employed for the study for the period of fall 2000 to fall 2010. The data include complete student demographic information[33], all placement scores and dates, course enrollment, course grades, faculty assignment and faculty status (full or part-time) per class, campus, and course format.[34]

The analysis focuses on traditional age students 18 to 23 years old at date of first matriculation at the college. Further requirements for inclusion in the sample used for analysis are as follows: membership in a fall cohort with an entry semester of fall 2000 to fall 2007; completion of the computerized math placement exam, ACCUPLACER[35], a minimum of once and a maximum of twice; administration of the complete placement exam must be on the same date;[36] the placement exam must be completed in the policy-period for which the student's cohort is assigned;[37] the student must be a resident of the state for which the institution is located based on the address on file at the time of registration; the student must possess a valid address such that census block data can be matched to the geocoded address; if the student is in high school, his/her graduation must occur within a 45 day window of enrollment in a class at the college to be assigned to that semester's cohort, if not, the cohort assignment is specified as the following term for which the student registers;[38] and the student must enroll in a course at the college.[39]

[33] The data sets also include students' addresses at time of first registration.
[34] Class information is provided on format of the class and is designated as traditional (in-person), hybrid (a mix of in-person and online), and online classes.
[35] Students who completed the challenge exam, a secondary method to appeal the placement for developmental math were excluded from the analysis.
[36] If a student scores 70 or above on the ACCUPLACER exam, a second exam is administered. The serial testing and branching are done seamlessly during the administration of the exam and students are not aware of the second exam. For a student to be included in the sample, both exams must be completed on the same date. Early review of the test data indicated that some students were tested on separate days for the second branched exam. After numerous failed attempts to clarify the testing anomaly with the college, the decision was made to limit the sample to students who followed the appropriate protocol for exam administration.
Branching also occurs for low scores, but this region of the scores results in a placement three or more levels below credit and therefore, is not applicable to the current study.
[37] To designate clearly a pre-policy student cohort member from a post-policy cohort member, the placement exam and placement assignment must follow the rule of the policy period.
[38] This restriction is in place to eliminate current high school students from the analysis set. Students who participated in parallel enrollment programs while in high school, who later attended the college are included in the analysis set, but assigned a cohort entry following the high school graduation date. For example, a student who has specified their high school graduation date as June 2011 and enrolled in a college class in Spring 2011 will not be assigned a cohort of Spring 2011; the student's cohort assignment will be the next term the student registers for a class at the college.
[39] Students who completed the placement exam and never enrolled at the college, commonly referred to as 'the disappearing student' are not included, but represent a group that deserves further study in the research community.

Each student is assigned membership in a fall cohort[40] in the period of fall 2000 to fall 2007 and is tracked for 15 consecutive terms (each year, the terms are as follows: fall, winter, spring, summer I, summer II). All short-term outcomes are assigned based on whether the outcome was accomplished within the 15 term limit. For students in the treatment group, defined as those assigned to the developmental class two levels below credit, the short-term outcomes could be attained within three terms.[41] For students in the control group, defined as those assigned to the developmental class one level below credit, the short-term outcomes could be attained within two terms.[42]

Because of the lack of availability of individual socioeconomic measures, such as income and parental educational attainment, census block group data as a proxy for socioeconomic status is used. The addresses of the students were converted to geocodes using ArcGIS which allowed geodata from the census (1990 and/or 2000) to be attached to the individual student observations.[43] The use of geodata for supplementing student unit records at community colleges has been outlined for researchers (Crosta, Leinbach & Jenkins, 2006; Jenkins et. al, 2010). The use of the census block data was employed for a study on developmental reading students at the community college (Jenkins et. al., 2010).

3.2 The Strength of Using Data from One Institution

There exists no universal agreement for institutional policy related to developmental coursework at the college level. Some institutions do not mandate testing and placement, others institutions mandate testing but allow the student to decide whether or not to enroll in the developmental courses, while others mandate both testing and placement. Even with a policy in place, many students are able to avoid mandatory assessment or placement. Many colleges use computer adapted tests for placement, such as Compass and ACCUPLACER, but there are no guidelines for the cutoff scores used for placement within the developmental math sequence, so institutions must set their scores. Further complicating the issue of the placement process, there is no consistency in the number of courses in the math sequence, the course designations, names of the classes, or the topics covered of the courses. Because of the wide array of issues surrounding developmental courses, it becomes difficult to aggregate data across several institutions because of the institutional variability in policy.

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[40] Preliminary analysis indicated statistically significant differences in spring, winter, and summer cohorts such that only fall cohorts are used in the analysis. Cohort fixed effects for the fall cohorts will be included to address differences within fall cohorts.

[41] The outcome eligible for enrolling in a credit class, under ideal conditions, could be accomplished by successfully completing two semesters of developmental coursework. Enrolling and successfully completing a credit math class would require another semester, under ideal situations. Thus, the minimum length to complete the requirements for success of all outcomes could be 3 semesters. Students are tracked for 15 terms, thus allowing ample time for completion of outcomes.

[42] The outcome eligible for enrolling in a credit class, under ideal conditions, could be accomplished by successfully completing one semester of developmental coursework. Enrolling and successfully completing a credit math class would require another semester, under ideal situations. Thus, the minimum length to complete the requirements for success of all outcomes could be 2 semesters. Students are tracked for 15 terms, thus allowing ample time for completion of outcomes.

[43] In a study comparing the use of 1970 and 1980 or zip code to census tract values, little difference was noted (Geronimus and Bound, 1998).

By using data from one institution, the issues that arise with aggregation of data sources are eliminated. The institutional policy mandating placements and cutoff scores are constant in each policy period (pre or post-policy) across the institution's campuses. The developmental math sequence is consistent in terms of the number of classes that comprises the sequence and the content of the courses. The strength of the dataset is that it will allow for a causal estimation of the impact of the developmental coursework and the institutional policy related to placement of developmental math students on student outcomes.

4 Regression Discontinuity Primer and Empirical Strategy

Regression Discontinuity, RD, belongs to the category of quasi-experimental designs[44] along with propensity scores and instrumental variables (Shadish, Cook,& Campbell, 2002). It has recently found favor in several social sciences, including econometrics and empirical economics (van der Klaauw, 2008), political science, sociology. RD has passed the rigorous standards of the What Works Clearinghouse (What Works Clearinghouse, 2008). Lee & Lemieux (2009) place RD as closer to the "gold standard" of a randomized experiment than other quasi-experimental designs.[45] This methodology is particularly valuable for estimating effectiveness of developmental coursework and can be used in conjunction with other techniques such as instrumental variables and discrete time-hazard modeling (Lesik, 2008).

Regression discontinuity, RD, is first noted in the literature in 1960 when applied to analysis of outcomes for National Merit finalists by Thistelwaite and Campbell. RD was described in the 1963 work of Campbell and Stanley and referred to as an 'experimental design' as opposed to a 'method of analysis'. Its original appeal was limited in the research community and not widely applied which could be attributed (at least in part) to the fact that one of the designers of the method, Campbell, noted its limited scope (Cook, 2008). In a recent paper on the history of RD, Cook (2008) notes that papers that used this method in the period following its development referred to the method by other names such as a "cutoff" or "risk-based allocation design" to avoid the label of RD.

RD analysis has become a popular choice for evaluation of educational interventions, which commonly determine treatment status by scores received on a formal assessment. The score on the assessment provides one of the necessary and defining characteristics of the method referred to as the running and/or forcing variable. The assessment score, an exogenous variable, is used for defining the treatment and control group (Trochim, 1984; van der Klaauw, 2008).

In a true experiment, there are often ethical issues of randomly assigning individuals to treatment and control groups. Subjects in many educational interventions, however are not randomized, and target only the neediest students to receive the intervention, treatment. For example, interventions for reading interventions at the elementary school-level would target students with the lowest reading ability. To access ability, a common exam is administered and a student's on performance (score) on the exam is

[44] Quasi-experimental designs are not true experiments, as the name implies. Participants are not randomly assigned as in a traditional randomized experiment.

[45] Trochim (1984), an early pioneer for the design, claimed RD to be one of the 'strongest' quasi-experimental designs in his definitive book on the topic.

used as the measure of ability (Trochim, 1984). Thus, RD is an appealing design that can be applied to evaluate educational interventions that target students at the high or low end of the spectrum based on a standard exam.

Recent advances in RD method address concerns in its application such as endogenous sorting at the cutoff and testing for manipulation of the running variable (Lee & Lemieux, 2009; Lee & Card, 2008; Calcagno & Long, 2008; Levin & Calcagno, 2007; McCrary, 2008; van der Klaauw, 2002; Shadish, Cook & Campell, 2002; Imbens & Lemieux, 2008; Lesik, 2006).

One of the appealing features of the RD methodology is that it allows for a causal estimation at the cutoff for individuals at the fringe of placement under relatively weak assumptions [46] (Hahn, Todd, van der Klaauw[47], 2001; van der Klaauw 2008; Lee and Lemieux, 2009). RD exploits an exogenous variable to determine assignment[48] which is independent of observable measures. The method focuses on estimating the difference in outcomes for individuals right above and below the cutoff. It is assumed that the individuals within the small band around the threshold, cutoff for placement, are similar on both observable and unobservable characteristics. The counterfactual is provided by the individuals to one side of the immediate cutoff who are assumed to be similar to the students on the immediate other side of the cutoff. In short, students immediately to the right and left of the cutoff for placement serve as the treatment and control group.

The results of RD can be readily communicated with accompanying visuals that make the method more transparent than other designs (Lee & Leimeux, 2009; van der Klaauw, 2008). A picture speaks a thousand words – which allows researchers to easily communicate the findings of the study to policymakers without the need to labor over the details of methodology. A discussion of the results for an RD can center on a single visual and whether or not a statistically significant discontinuity (or jump) exits at the cutoff at the control and treatment group divide. For the non-statistician, it is easy enough to understand that the jump represents the difference in the outcomes between the two groups – those who received the treatment and those who did not. A larger jump represents a greater difference in outcomes. Trochim (1984) listed three necessary assumptions for a sharp RD as follows: (1) perfect assignment based on the value of the running variable, (2) the correct specification of the functional form of the model and (3) the absence of a 'coincidental' functional form (meaning that the discontinuity at the cutoff score is attributable to the impact of the intervention itself not another covariate, which is basically the continuity assumption). Other assumptions include the need for an ample sample size to estimate the coefficients in the model and constant level of treatment across individuals in the treatment group (Trochim, 2008). An RD that is 'sharp" has the probability for treatment based on the assignment variable to be 100%, while the treatment status for those whose score dictates no treatment is 0%. In other words, a sharp RD has perfect compliance. Further clarification of variations in assumptions that make RD an option are noted by Trochim, which allows for the possibility of misassignment or noncompliance (known as a fuzzy RD) that can still result in an unbiased estimate. An incorrect

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[46] Assumptions will be presented later within this section.
[47] This reference will be referred to as HTV.
[48] Assignment to treatment status is either completely or partially determined by the running variable (also referred to as the assignment variable) is discussed later within this section.

functional form, however, produces biased estimates and estimations for discontinuities based on an observable characteristic which invalidates the use of the method (Trochim, 1984). Recent work has demonstrated that relatively weak assumptions are needed to apply RD in a fuzzy context compared to other methodologies (HTV, 2001, van der Klaauw, 2008).

An examination of the underlying assumptions for applying a sharp RD allows for justification of its use in application. Since assignment is exogenously determined by the running variable, the assumption of conditional on observables is trivial since assignment is made solely on the cutoff on the forcing variable (Lee & Lemieux, 2009). The standard assumption of 'overlap' does not apply to RD because individuals are assigned to one of two groups – treatment or control – again, based on the running variable; so while this assumption cannot be made, the continuity assumption[49] allows for RD's application. Because no values exist for both treatment and control at the cutoff score for an individual, the estimation will require an extrapolation of the functions to the left and right of the cutoff and thus, the continuity assumption allows such inference to be made.

As Lemieux explains, another assumption that is made with RD is that all factors evolve 'smoothly' with respect to the running variable (2009) which is the local continuity assumption.[50] In the case of RD, the discontinuity in the conditional expectations at the cutoff score given the value of the running variable is the average causal effect of the treatment (Imbens & Lemieux, 2008). It is assumed that there exist no other discontinuities at the cutoff other than the program's impact. This underlying assumption of whether the observable characteristics are evolving smoothly over the range of the forcing variable can be 'tested' in a visual manner as well as a quantitative manner. On each side of the cutoff, a visual display of the mean observable characteristics of individuals within specified bins of the running variable must not exhibit a jump across the range of the scores. A quantitative method employs a test of the conditional density of the mean characteristics over the range of the forcing variable. Psuedo-cutpoints can be tested for statistical significance of discontinuities by employing a regression.

Assignment may not be perfectly determined by the running variable and may be stochastically determined in terms of the assignment variables and referred to as a fuzzy RD design (Cambpell, 1969; Trochim; 1984). In terms of a randomized experiment, this is analogous to noncompliance. Bloom (1984) labels the individuals who do not follow strict adherence to treatment as either no-shows (those individuals assigned for treatment who do not receive it) or cross-overs (individuals assigned to the control groups but opt for the treatment). There is also the possibility of misassignment due to observable characteristics that are unknown to the researcher but known by the person assigning participants (van der Klaauw, 2008). The probability for treatment or control is no longer either 100% or 0%; thus the discontinuity is impacted[51] and the estimation is biased.

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[49] The continuity assumption implies that there exists a smooth function of the score as it crosses the cutoff which allows the estimation of the average causal effect of treatment by taking the limits of the function as the running variable approaches the cutoff score ($score_c$) from the left and right conditional on the assignment variable such that $Treatment\ Effect = \lim_{x \to score_c^-} E(Y_i|X_i = x) - \lim_{x \to score_c^+} E(Y_i|X_i = x)$ (Imbens and Lemieux, 2008).
[50] The assumption of ignorability is met because the assignment is based solely on the value of the forcing variables relative to the cutoff.
[51] The jump or discontinuity at the cut off would be less under the condition of noncompliance.

To compensate for the inherent bias that would ensue in the fuzzy analysis, Trochim (1984) suggested using either a percentage placed correctly or a moving average technique[52] or something akin to what is commonly referred to as an instrumental variable approach (Angrist & Imbens, 1995; van der Klaauw, 2008). The estimation procedure is no longer reliant solely on the difference in the expected values of the outcome conditional on the forcing variable, but rather an adjusted value for a subgroup. As HVT discuss this adjustment is a two stage least squares framework if the specification is linear.

Lee and Lemieux (2010) discuss a two-step estimation framework that is equivalent to the IV approach if the two steps use the same set of regressors, including assignment to treatment. However, one step uses the outcome of concern as the dependent variable, while the other step uses the outcome of concern as the dependent variable. The step that uses the outcome of concern as the dependent variable yields the intent to treat estimate (ITT) which is the effect expected given the same level of compliance as the sample. The step that uses the actual receipt of the treatment as the outcome variable provides the adjustment factor that allows a local average treatment effect to be estimated. The quotient of the ITT estimate and the adjustment factor provide the estimate for the LATE. Given that there is not complete compliance in the case of a fuzzy RD, the 'adjustment' value will be less than 1.0 and thus, the LATE will be larger than the ITT estimate.

As is true for an instrumental variable, this average treatment effect is estimated for the compliers, the individuals whose assignment is impacted by crossing the cutoff score[53] (Imbens & Lemieux, 2008).[54] The estimation of the effect of the treatment in the case of a fuzzy RD is limited in its generalizability to compliers, the students for whom a change in policy would cause a change in behavior based on the outcome of the scores on the exam. Lee and Lemieux explain that the "IV estimand is interpreted as the average treatment effect "for the subpopulation affected by the instrument." (2010, p. 301). For policy evaluations, the local average treatment effect is a useful measure.

Of particular concern with RD is the running variable ─ its ability to provide a completely observed measure of assignment, its lack of manipulation, and its classification as discrete or continuous. The early work of Goldberger on selection bias issues provides the insight necessary to better understand the running variable and the issue of endogenous sorting at the cutoff. As explained in Cook's history of RD, Goldberger's work 'proved'[55] that a completely observed score that is not psychometrically perfect will not cause biased results, while a true score that is partially observed will result in bias (Cook, 2008). If individuals can completely control or manipulate the running variable, then the use of the model is invalid.

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[52] The use of a moving average technique was later published by Spiegelman and Trochim.
[53] This is the assumption of local monotonicity.
[54] The estimated effect, local average treatment effect, is estimated by using the ratio of the expected value at the cutoff for the treatment and control groups to the expected probability of being in the treatment or control group.

$$LATE = \frac{\lim_{x \to score_c^-} E(Y_i|X_i = x) - \lim_{x \to score_c^+} E(Y_i|X_i = x)}{\lim_{x \to score_c^-} E(T_i|X_i = x) - \lim_{x \to score_c^+} E(T_i|X_i = x)}$$

[55] The 'proof' offered is not mathematical in nature but rather a statement of the argument. (Cook, 2008).

Lee and LeMieux (2009) distinguish between complete control of an individual on the running variable as opposed to either precise control or imprecise control. The distinction rests by viewing the ability of the individual to select his/her test score versus only being able to influence the score. Complete control over the running variable is equivalent to self-selection. If imprecise control is exhibited such that the person's score is partially determined by random, then the required continuity assumption may still hold and the RD design may be a viable option.[56] In the case of imprecise control, in the immediate vicinity of the cutoff, there remains a local randomized zone to allow estimation of the impact of the intervention (Lee and Lemieux, 2009).

There are other factors that must be considered when applying RD for evaluation. For educational evaluations, it is common that a qualifying test score is discrete in nature. There is an advantage of this type of measure since it allows for easy binning of values based on the assignment variable, but it does lead to other issues. Lee and Card (2008) discuss the need for use of clustered standard errors for a discrete running variable when doing an RD. Sample sizes needed for estimation purposes is much larger for RD as compared to a randomized experiment. Typically the sample size needed for RD is anywhere from 2.75 to 4 times as large as needed for a randomized experiment for the same power. (Bloom, Kemple, Gamse, & Jacob, 2005; Schochet, 2008; Shadish et al, 2003).

Misspecification of functional form is particularly deleterious to RD analysis. Its consequences are even more heightened in the case of a discrete running variable due to the extended use of extrapolation in the estimation procedures (Lee and Card, 2008). The consequence of misspecification can be either an over or underestimation of the treatment effect. Early work on RD recommended overfitting to minimize misspecification bias (Trochim, 1984). Although a linear form was used in the first application the method, newer methods employing local linear estimations can be employed. More recent work on fuzzy RD notes that identification of functional form need not be overly restrictive (HTV, 2001).

4.1 Empirical Strategy and Underlying Assumptions

Randomized experiments are based on the premise that before treatment the observed and unobserved characteristics of the treatment and control groups are similar. This is necessary such that any changes that occur can be attributed to the treatment thus allowing for a causal estimation the treatment itself. A typical model for the case of a randomized experiment with equivalent groups is as follows (assuming a linear functional form):

$$Y_i = \alpha_0 + \beta T_i + \eta \underline{X}_i + \varepsilon_i$$

for which Y_i is the outcome of interest; α is the average outcome for the control group; T_i is a dummy variable indicating treatment status assuming a value of 1 if assigned to the treatment group and 0 otherwise; $\underline{X}_{i \text{ is}}$ a vector of observable characteristics; ε_i is an error term assumed to be uncorrelated with

[56] The degree of endogenous sorting will be analyzed by the use of visuals. Since the score from a computer adapted test, ACCUPLACER, is used as the running variable for the study, it is argued that even with the provision for retesting, an individual will not have precise control.

an endogenous variables with an expected value of zero and β captures the treatment effect. If the groups are not equivalent, the estimated value of the coefficient β captures unobserved characteristics that cannot be controlled for in the model and estimates for treatment effect are biased (Calcagno & Long, 2008). Regression Discontinuity (RD) is designed such that the groups are nonequivalent. A deterministic function is used for assignment to treatment and control based on a cutoff score[57], running or forcing variable. With a sharp RD, any person who scores at or above a particular threshold is placed in the control group, while any individual who scores below the threshold will be assignment to the treatment group.[58] The probability of assignment is either 100% or 0% based on the value of the forcing variable. Further as done with an RD, by limiting the analysis close to the cutoff, the individuals within the limited range can be assumed similar in the absence of treatment (Calcagno & Long, 2008).

In other words (and symbols), the assignment for person i, T_i , who receives a score of $score_i$ with the cutoff score of $score_c$ is as follows: T_i =1 if ($score_i < score_c$) , individual is a member of the treatment group or T_i = 0 if ($score_i \geq score_c$), individual is a member of the control group.

The model is represented as the following:

$$Y_i = \alpha_0 + \beta T_i + \partial(score_i - \mathrm{score}_c) + \varepsilon_i \qquad \text{Equation (1)}$$

where Y_i is the outcome of interest and the coefficient of interest is the estimated value of β which is the unbiased estimate of the average treatment effect for individuals immediately to the right and left of the threshold;[59] α is the average outcome for the control group; T_i is a dummy variable indicating treatment status assuming a value of 1 if assigned to the treatment group and 0 otherwise; ε_i is an error term; and $(score_i - score_c)$ is the centered placement score. The score is typically centered at the cutoff by subtracting the cutoff score and it does not impact the estimation.

Since assignment is exogenously determined, the need for other statistical controls is not warranted. It is common, however, to include covariates in the model to increase power and decrease error variance (Trochim, 1984). The inclusion or exclusion of the observable characteristics should not significantly impact the estimations of the treatment effect (Lesik, 2006, 2007).

Without specifying a linear functional form and instead incorporating the continuity assumption discussed earlier in this section, the model for the estimate of the local average treatment effect, LATE, is denoted as follows:

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[57] The cutoff score can be a composite score as well, but for purposes of ease in presentation, it will be assumed to be a single score on an assessment.
[58] The assignment rules are assuming an intervention geared for students who score below a threshold based on a cutoff score. The opposite assignment rule would apply for a meritorious program.
[59] Note that the score received by the student is centered about the cutoff score. It is in the immediate vicinity of the cutoff score that the treatment and control groups are most similar. The presentation is assuming a continuous value for the running variable. Later in the paper, the method for a discrete running variable will be discussed.

$Y_i = \propto_o + \beta T_i + \delta f(score_i - score_c) + \xi age_i + \sigma female_i + \tau minority_i + \Upsilon new_i + \varphi HH\_income_i + \varepsilon_i$
equation (2)

The coefficient of the treatment variable, β, will capture the treatment effect. The $f(score_i - score_c)$ is used to denote a smooth function of the student's i score and all other variables that will be employed in the model. The variables that comprise ($score_i - score_c$) in the model are as follows: $score_c$ defined as the cutoff score for placement in the math sequence two levels below credit courses; $score_i$ defined as the algebra score used for placement in the developmental math sequence[60] ; *age* of the student at the time of entry (restricted to 18 – 23 years of age); *female*, a dummy variable; *minority*, a dummy variable based on the self-identified ethnicity on the admissions application and dichotomized as minority or non-minority[61]; *new*, a dummy variable that assumes a value of one for students who first enter the college that are coded at the time of admissions as a new student as opposed to a transfer student at the college; and *HH_income* defined as the census block median household income for a student based on the address provided at time of entry.

In order to streamline the presentation of the model, the score in the model presented in equation 1 is modified to the following format:

$$Y_i = \alpha_0 + \beta T_i + \partial(score_i) + \varphi(\underline{X}_i) + \varepsilon_i \qquad \text{equation (3)}$$

The score on the placement test is again centered, but $score_i$ will assume this from this point forward without indicating the adjustment. The vector $\underline{X}_i$ represents the all controls discussed previously, and now includes the median household incomes from the Census block. All other coefficients are as defined as discussed previously for equation (2).

4.2 Fuzzy RD

In the case of the fuzzy RD, there is a divergence between being assigned to treatment and actually receiving the treatment. Thus, the probability for receiving treatment with a score below the cutoff score is no longer 100% nor is the probability of not receiving treatment for a score above the cutoff score 0%. In the case of the sharp RD, it is assumed that there is complete compliance; this is not the case with a fuzzy RD. Non-compliance can ensue when a student assigned to treatment joins the control group or the opposite scenario, a student assigned to the control group, opts to receive the treatment.

In the case of a fuzzy RD, the coefficient β in equation (3) no longer is the LATE but rather the intent to treat estimator, ITT. This estimate is of value to policymakers and provides an estimate of the policy given the observed level of compliance within the sample (Calgano, 2007).

In order to provide the treatment effect for those who receive the treatment, an instrumental variable is used within the RD framework. The instrumental variable is employed to estimate the probability of

[60] For the analysis, the student's initial score is employed.
[61] Students who self-identified as Caucasian or Asian were classified as non-minority

treatment (T_i) to address the divergence between assignment and treatment. The assigned treatment based on the ACCUPLACER and the placement policy is used as the instrument of actual receipt of treatment (Calcagno & Long, 2008, Boatman & Long, 2011).

The two underlying assumptions to meet the exclusion requirements to apply this method are met. The first condition is as follows:(1) there must be a correlation between receiving treatment and being assigned to treatment, $corr(T_{i,}D_i) \neq 0$; this seems reasonable since a student would tend to enroll in the course if assigned to the class. Furthermore, since the study institution has a mandatory placement policy that requires a student to enroll in the assigned developmental class based on the ACCUPLACER score, this first condition is supported. This relation can be verified during the analysis. The second condition is as follows: (2) there cannot be a correlation between the instrument,D_i, and the unobserved characteristics i.e., the error term of the outcome equation. Because assignment is determined exogenously using the cutoff, this second assumption is reasonable as well.

The actual treatment received by individual i is denoted T_i . Thus, for this study the dichotomous variable assumes a value of 1 if the student enrolled in the developmental math class two levels below credit. In the first stage, the probability [or as van der Klaauw (2008) refers to as the propensity of treatment] is determined using the relationship

$$T_i = \alpha_0 + \delta D_i + \partial(score_i) + \varphi(\underline{X}_i) + \varepsilon_i$$ equation (4)

Again,($score_i$) is used to denote the score of student *i*. The vector $\underline{X_i}$ represents the controls discussed above other than the placement score; D_i is the exogenously determined assignment based on the initial ACCUPLACER algebra score ; and T_i represents the actual receipt of the treatment.

In the second stage, the estimated probabilities for treatment from the first stage ($\widehat{T}_i$) are used in the regression as opposed to the assignment to treatment.

$$Y_i = \gamma_o + \beta\widehat{T}_i + \vartheta(score_i) + \pi\underline{X_i} + u_i$$ equation (5)

Y_i is the short-term outcome of interest for which there are three – eligible to enroll in a credit math class, enrollment in a credit math class, and successful completion of a credit math class. The estimated coefficient β is the local average causal treatment effect (LATE) which provides an unbiased estimate of the impact of treatment for individuals in the immediate vicinity of the cutoff. This approach is equivalent to a two stage least squares if the functional form is linear (van der Klaauw, 2008). The interpretation of the LATE is limited to the students whose behavior was influenced by the placement policy. The focus of the estimate is the compliers (Imbens and Angrist, 1984).

RD Estimation in Study

In the last several years there have been numerous articles in econometric journals that can serve as "guides to RD" (Lesik,2006; Lee & Card, Lee & LeMieux, 2009 Imbens & LeMeiux, 2008, van der Klaauw, 2008; Calcagno & Long, 2008; Levin & Calcagano; 2008; Lee, 2008).

The correct specification of functional form is critical for estimation of the average causal impact for an RD. However, the functional form is not truly known, thus the inclusion of higher order terms is included to provide flexibility in the form. Interactions between the score and treatment status are included such that the slopes to the left and right of the cutoff are allowed to differ. The functional form employed in this study is as follows:

$$Y_i = \alpha_0 + \beta\hat{T} + \partial(score_i) + \partial_2(D_i * score_i) + \partial_3(score_i)^2 + \partial_4(D_i * score_i)^2 + \lambda\underline{X}_i + \phi_i \sum_1^n Fall_i + \varepsilon_i$$

The dependent variable, Y_i ,is dichotomous and is one of three short-term outcomes within the 15 terms for which the cohorts are tracked: (1) eligible to register for a credit math class, (2) enrollment in a credit math class, (3) successful completion of a credit math class; $\hat{T}_i$ is the estimated probability of enrollment in the developmental class two levels below credit from stage 1of the 2 SLS; D_i is the assignment to two levels below credit math based on the initial placement score; $\sum_1^n Fall_i$ represents the fixed effects for fall entry cohorts included in the study and $\underline{X}_i$ represents all other variables as discussed previously. Linear probability models with clustered standard errors by the placement score are employed.

Preliminary checks are presented to test the underlying assumptions of the RD and are detailed in the results section. Nonparametric methods were employed as suggested by Imbens and Lemieux (2008) by using local linear regressions within specified bandwidths of the cut off scores for placement one and two levels below credit math classes. Robustness examination of the LATE estimates consist of allowing the bandwidth of the RD regression to vary. The estimates for the treatment are graphed against the bandwidth.

4.3 Attrition

As outlined in Standard 2 in the Standards for Regression Discontinuity Designs from the What Works Clearinghouse (June, 2010), an “RD study must report the number of students …who were assigned to the treatment and comparison groups samples, and the proportion of students …with outcome data who were included in the impact analysis…” Attrition in an RD design leads to biased estimates because the sample is no longer representative of the population as a whole. This is of particular concern in RD, which relies on the observations close to the boundary value for estimation. If students within the specified bands for analysis are a subset of students that scored within the range because a mechanism existed that allowed some students to exit the control or treatment group, then the randomization at the cutoff no longer applies.

Attrition in the context of this study is defined in terms of the percentage of individuals who no longer are part of the data set used in the analysis beyond the exclusion restrictions discussed previously.3. This study has an additional complexity compared to the standard RD attrition analysis because of the branching mechanism for students receiving scores of 70 or higher on the algebra section of the placement exam.

In the pre-policy period, this added complexity of a second exam score for placement one level below credit and credit that occurs at an algebra score of 70 does not impact the RD estimates. The cutoff for placement one level below credit math is an algebra section score of 50. The maximum range (band) of algebra scores that can be used in the RD analysis in the pre-policy period excluding the second placement exam information from the boundary of placement two levels and one-level below credit is restricted by the cutoff for placement two levels below credit math, which is a minimum algebra placement score of 36. Using an equal-width band from the cutoff for placement one level below credit math, the range of possible scores is 36 to 65 inclusive. On a metric with a centered score of 0 for the cutoff for one level below credit (50), the maximum analysis window to the right of the cutoff is 20 (algebra score range 50 to 69, corresponding to a centered score range 0 to 19) avoids the use of the additional score of the college level math sub-test for placement in the analysis. The maximum width to the left of the centered cutoff for students placing two levels below credit is 14 (algebra score range of 36 to 50 with a centered score range of -14 to -1). Thus, a maximum analysis window using an equal distance to the right and left of the centered scores for placement one-level below credit is 14.[62]

In the post-policy period, the algebra score cutoff between one and two levels below credit was upwardly adjusted to 65. This allows the maximum bandwidth using placements based solely on the algebra score to be ±5, (60 ≤ algebra score ≤ 69 to the right of the cutoff) for the RD analysis. The limited bandwidth of 5 does not allow for ample sample size or robustness checks on estimates. An examination of the continuity of the students' placements for students placing one-level below credit math and credit math is done to justify using a larger range of algebra scores in the post-policy analysis. The algebra score can be used exclusively as the running variable in the RD analysis if there is not a discontinuity in the density of algebra placement scores.[63] To allow an examination of attrition, the continuity of placement of students at the score of 70 is analyzed in a regression framework discussed in the paragraphs that follow. To gain a better understanding of the level of attrition in the control group, the percent retained in the control group as a function of the algebra placement score (ALG) is examined. Further, the attrition in post-policy period is examined within selected bandwidths of the cutoff score for placement one level below credit math based on the algebra placement score employing the equation below where CM represents the score the student received on the college-level math subtest:

$$\%\ Attrition\ Control\ Group = \frac{\sum student\ who\ placed\ in\ credit\ class\ based\ on\ CM\ within\ designated\ band\ of\ ALG\ score}{\sum student\ who\ had\ a\ ALG\ within\ the\ designated\ band\ \ for\ the\ control\ group}$$

A regression is employed using the post-policy observations in the restricted range of algebra scores of 65 and above with a cutoff of 70. The outcome variable $placement_{initial_{credit}}$ is a dichotomous variable for placement based on the initial placement exam assuming a value of 1 for credit math and 0 for one level below credit. In other words, the outcome variable assumes a value of 1 if the student is no longer a member of the data set used for the RD estimated. The treatment and control groups will be those who

[62] Although not a requirement on a fuzzy RD, it is common practice to use an equal distance to the right and left of the cutoff.
[63] If all students who received an initial score of 70 or above were placed in developmental courses, there would be no need for an attrition study. Since this is not the case, a study of attrition is warranted.

score to the right of the cutoff of 70 and those who score to the left of a score of 70, respectively. In the attrition model, the following equation includes quadratic terms and interaction terms between the initial algebra placement score and treatment status to allow flexibility.

$$\begin{aligned}\text{placement}_{\text{initial}_{\text{credit}}} &= \alpha + \beta(\text{treatment}) + \gamma(\text{score} - 70) + \delta(\text{score} - 70) * (\text{treatment}) \\ &+ \rho(\text{score} - 70)^2 + \pi(\text{treatment}) * (\text{score} - 70)^2 + \varepsilon\end{aligned}$$

The coefficient on the treatment variable, β, is examined. A statistically significant coefficient would indicate a discontinuity in the density of placement at the score of 70 and an unacceptable level of attrition of students in the control group for the RD analysis.

5 Student Characteristics

5.1 Sample

The construction of the analysis data set has the potential for a maximum sample size of 14,838 students as detailed in Table 3. This number represents all new or transfer students who entered the college for the first time in a fall semester in the time period of 2000 to 2007, completed at least one math placement exam, at time of initial matriculation were in the age bracket of 18.0 years to less than 24.0 years old, were high school graduates or had graduated no later than 45 days after first matriculation, provided a valid address in the state of the college that could be geocoded. Approximately 6 percent of students (n=869) completed the math placement exam more than twice and were excluded from the sample.[64] As discussed previously, if a student appeals the second placement, a Departmental exam is administered. The branching of the placement exam from an algebra portion to a college level math portion is set to occur at a score of 70, whereupon a separate score for the college level math portion is issued. Students whose records indicate irregularities in the administration of the placement exam were excluded from the sample. These irregularities included students who completed the college level math portion of the placement exam more times than the student completed the algebra section; this resulted in approximately 3 percent (n=493). An additional 0.5 percent (n=79) were excluded due to an inconsistent test date, for example, a college level math placement date occurring on a date before the algebra exam date. Further, an additional 1 percent (n=141) students were excluded due to the presence of a college level math score accompanied by an algebra score of less than 70 on the same date. (The completion of the college level math section with a score below 70 on the algebra section indicates a deviation from the college policy.) Students in the pre-policy period who completed the placement exam for the first time in the post-policy period were removed from the analysis. Students in the pre-policy period who retested and completed the second placement exam in the post-policy period were removed from the analysis that used the higher placement score. The restrictions based on the alignment of the time of testing with the policy-period, eliminated an additional 3 percent (n=487) of student from the original set. The final sample size was 12,762, which resulted from a total sample attrition of approximately 14 percent.

[64] The institutional policy allows for a student to complete the ACCUPLACER placement exam twice. Since administration of the ACCUPLACER to a student three or more times is a deviation of the policy, the observations are excluded in the analysis set.

5.2 Study Period

Table 4 presents the summary statistics for selected student characteristics for the study period. The summary statistics are complemented by a set of visuals: Figures1 – 6 for the selected covariates over the study period per entering cohort. The age distribution for the students in the study remained relatively stable for the pre-policy period. The distribution of age was positively skewed with a mean age of 19.15 years. There was a slight increase in age in the post-policy period. The mean household income based on census data was $50,200 with a standard deviation of $18,300. The mean income for the cohorts in the post-policy period was $6,460 less than the pre-policy period. This difference in mean income in the policy periods is not statistically significant at the 0.05 level (p=0.056). In alignment with the College's Factbook, the majority of students were female (53 percent) and approximately 40 percent of students were minority, defined as non-white or non-Asian. The percentage of minority students increased in the post-policy period accompanied by an increase in the percentage of transfer students. The majority (91 percent) of students were classified as 'new' as opposed to transfer students in the study period.

Figure 7 summarizes mean placement scores in the study period per cohort. The mean score over the study period per cohort remained in the 50 to 55 range throughout the study period. The mean algebra placement score increased from 51.66 in the pre-policy to a mean score of 52.04 in the post-policy period. The mean increase of 0.38 points is not significant (p=0.41). A score of 50 in the pre-policy period would translate to a placement one level below credit. A score of 50 on the post-policy period would be far below the minimum score allowed for placement one level below a credit class; instead, this score would result in a placement two levels below credit.

It is the change in institutional policy related to the cutoff scores that is exploited in this research. The impact of the policy that upwardly adjusted placement cutoff scores was implemented in 2005 is illustrated in Figure 8. The green vertical line in the figure distinguishes the policy periods for this study. In addition, Figure 8 illustrates the outcome of a student's first math placement as a function of the cohort entry date. As evident by the figure, there was an increase in the proportion of students placing lower within the developmental math sequence beginning in 2005. The policy change resulted in many students who beforehand would have placed one level below credit to be placed two or three levels below credit. Referring to Figure 8, the impact of the policy on the students' placement is demonstrated by the dramatic decrease in the proportion of students placing one level below credit (green line) which is accompanied by an increase in the proportion of students placing two and three levels below credit (red and blue lines respectively). Comparing the trend for the pre- and post-policy periods, there was approximately a 16 percent decrease in the placement one level below credit accompanied by an approximately 5 percent increase in placement two levels below credit and approximately 11 percent increase in placement three or more levels below credit. The proportion of students placing into credit classes remained stable throughout the study period because the placement cutoff for credit classes was not changed.

5.3 Pre-Policy Descriptives

Table 5 presents the summary statistics for all students in the pre-policy period disaggregated by the initial placement score. For students who took the placement test twice, only the initial score is used in the table. Table 6 further details the students in the pre-policy period based on the initial placement and includes course enrollment and passing rates. All values in the table are conditional probabilities based on the initial assignment. Unlike the flowcharts for student progression, (Figures 9, 10, 14, 14), Tables 5 and 6 include students using the initial placement only without regard to compliance to the policy for progression through the developmental sequence. Thus, a student who initially placed two levels below credit math and was somehow able to register for a credit math class without successfully completing the two developmental math classes is retained in the subsample for enrolling and passing a credit math class.

A detailed depiction of how the group of students who placed two levels below credit progressed through the developmental sequence is presented in flowchart form in Figure 9. The flowchart is repeated for the pre-policy period for students who placed one level below credit math classes in Figure 10. Further, the flowchart details the path of students who follow the policy of the College by transcending from one class to the next without skipping courses within the developmental sequence and achieving the necessary milestones for progression. The reader will note that the proportions stated for the flowchart differ from Table 6, which uses only the milestones without regard to the compliance of policy through the developmental sequence. The flowcharts may indicate a student beginning at a lower level than placed, which could occur under the label of ‘compliance’ since the policy allows a student to start lower within the sequence. There was a small percentage, approximately 2 percent, of students who opted to begin lower in the sequence than the original placement. Further, the flowcharts are based on the initial placement, which may differ from the second placement. Students who begin at a class higher than the initial placement may or may not appear to be in complete compliance with the policy, but still be included in the flowchart. For the “may not” students, these students retested and scored at a higher level than the initial test and thus changed placement to a higher starting point in the sequence of remediation. However, if a student placed, for example, two levels below credit and somehow was able to enroll in the developmental class one level below credit without retesting, he/she was not complying with the placement designated by the policy and therefore not included in the flowchart.

The flowchart illustrates the number of intermediate steps a student had to successfully navigate before eventually enrolling in the credit math class. At each juncture, the sample size decreases or at best stays unchanged. As evident from the flowchart, for students who originally placed two levels below credit and complied with the policy related to progression, approximately 28 percent became eligible to enroll in a credit class. For students who reached this important milestone, eligibility to register for a credit math class, approximately 19 percent completed the credit math class. For students who placed one level below credit, approximately 49 percent became eligible to register for a credit class and 40 percent successfully completed the credit math class.

Examination of the tables and figures presented indicates that on average students who placed lower within the developmental math sequence lived in households with lower mean incomes. The proportion of female students in the development math sequence increased as the distance from credit classes increased. The greatest subgroup percentage of placement in credit classes was transfer students. The percentage of students classified as minority was inversely related to the placement score.

Focusing on just students who placed within the developmental math sequence (Table 6), the outcomes were bleaker as the distance from credit to the developmental classes increased. The proportion of students enrolling in any math courses within 15 terms of the cohort assignment decreased the further the placement was from credit math classes. The difference in math enrollment proportions, defined as ever enrolling for a math class within 15 terms of matriculation, for students placing one level below credit to students placing three or more levels below credit was approximately 13 percent. While the developmental math classes are designed to prepare students for success in credit math, only a small percentage of students actually reached the credit math state through this formal avenue of remediation within the 15 terms for which the cohorts are tracked. Based on the analysis, approximately one-half of students who placed one level below credit ever enrolled in a credit math class. For students placing three or more levels below credit, approximately 14 percent of students reached the intermediate milestone of enrolling in a credit math course. The overall success of students in the credit math class ranged from 41 percent for those who initially placed one level below credit to approximately 10 percent for those who placed three or more levels below credit.

Figure 11 provides the percentage of students who attained the short-term outcomes that this study focuses on based on the initial placement. Since retesting is allowed at the institution and students do not always comply with the placement received, there exists a difference in the proportions between students who are eligible for registering for a credit class and those who actually register for the credit class. This difference in proportions is most evident for students who place one level below credit based on the results of the first placement exam.

Focusing only on students who enrolled in a credit math class, Figure 12 provides the success rates in credit math based on the student's initial placement. For students who do reach the milestone of eligibility for enrolling in a credit math class and then registering for a credit math class, the success rates vary based on the initial placement. Approximately 85 percent of students who place into credit math class successfully pass the class. Lower rates of success in the credit class are realized for students who place lower in the sequence: 74% for students who place one level below credit, 70% for students who placed two levels below credit and 72 percent for students who place three or more levels below credit.

5.4 Post-Policy Descriptives

A parallel analysis to the pre-policy analysis is presented for the post-policy period. Table 7 presents the summary statistics for all students in the post-policy period disaggregated by placement score. As before, for students who took the placement test twice, just their first score is used in the table. Table 8 further details the students in the post-policy period based on the course enrollment and passing rates, again disaggregated by the first placement test assignment. All values in the table are conditional probabilities based on initial assignment.

A detailed depiction of how the group of students who placed two levels below credit progressed through the developmental sequence is presented in flowchart form in Figure 13. These flowcharts are repeated for the post-policy period for students who placed one level below credit math classes in Figure 14. As noted in the pre-policy discussion, the flowcharts detail the path of students who followed the policy at the College by progressing from one class to the next without skipping courses within the developmental sequence. Students who had not achieved the necessary milestone for progression through the developmental sequence based on the first course are noted as exiting the pathway at the juncture where the movement did not align with policy.

The percentage of students in the post-policy period who opted to begin lower in the sequence than the original placement is about half of that in the pre-policy period. The same pattern depicted in the pre-policy period emerges for post-policy students as the progression through the developmental sequence is examined. As evident from the flowchart, for students who originally placed two levels below credit, approximately 39 percent became eligible for enrolling in a credit class upon successful completion of the developmental sequence, and approximately 24 percent successfully completed a credit class. For students who originally placed one level below credit, these percentages were 51 and 39, respectively.

Examination of the tables and figures presented in the post-policy period indicates that on average students who placed lower within the developmental math sequence lived in households with lower mean incomes. The proportion of females who placed lower in the sequence increased as the placement score decreased. Of the students placed into credit classes, transfer students had the largest percentage. The percentage of students classified as minority was inversely related to the placement score.

Focusing only on students who placed within the developmental math sequence, Table 8, the outcomes were again bleaker as the distance from credit to the developmental classes increased. The proportion of students enrolling in any math courses within 15 terms of the cohort assignment decreased the further the placement was from credit math classes. The difference in math enrollment proportions between students placing one level below credit and students placing three or more levels below credit was again approximately 13 percent. Based on the analysis, approximately 53 percent of students who placed one level below credit ever enrolled in a credit math class. For students placing three or more levels below credit, approximately 12 percent of students reached the intermediate milestone of enrolling in a credit math course. The overall success of students in the credit math class ranged from 39 percent for those who initially placed one level below credit to approximately 9 percent for those who placed three or more levels below credit.

Figure 15 provides the percentage of students who attained the short-term outcomes that this study focuses on based on the initial placement. Figure 16 provides the success rates in credit math classes for students who enrolled in a credit math class. For students who do reach the milestone of eligibility for enrolling in a credit math class and registering for a credit math class, the success rates vary based on the initial placement. Approximately 80 percent of students who place into credit math class successfully pass the class. Lower rates of success are realized for students who place lower in the sequence: 73% for students who place one level bellow credit, 68% for students who placed two levels below credit and 68 percent for students who place three or more levels below credit.

5.5 Pre and Post-Policy Outcomes

Figure 17 provides a comparison of the overall success rates of the short-term outcomes in the two periods examined in this study, again disaggregated by the initial placement. Figure 18 provides a comparison of the rates of passing a credit math class for the students who enroll in a credit math class in the two periods. The horizontal axis notes the initial placement of the student in the developmental math sequence. The rates of success in the credit math class are higher for every subgroup based on initial placement in the pre-policy period.

5.6 Representativeness of Study Institution

While the use of a single institution's data made this research possible, generalizability is a concern. In order to extend the results beyond that of the institution, the comparability of the general observable characteristics of the study institution and students are examined.

In terms of campus location and setting, approximately one-quarter of all community colleges are located in suburban settings. The different campuses that comprise the College are typical in size and fall within the average size of over 70% of all community colleges, based on National Center for Educational Statistics Community Colleges Special Supplement – The Condition of Education, 2008.

The College's student body has overall characteristics that are typical of a community college. Tables 9 and 10 provide the details for which the comparisons are made. The first column contains the statistics for the study institution. The institution's statistics are pulled from a variety of sources including the data set for analysis, the college's website and IPEDS. The second column (pink) represents publically available statistics from the Digest of Education (2009). The last column (pink) provides the table from the Digest of Education from which the statistics was found. The purple column is taken from a previous study by Bailey, Jeong, and Cho (2010) for which they discussed the comparability of the Achieving the Dream data set with NELS88. Not all statistics from the comparisons sets are available or available using the same limits (such as age bands). Any deviations in the comparison units are noted in the table.

Given the comparability of the study institution with nationally available statistics for community college students, the results of this study can be extended beyond the one institution.

6 Results

6.1 Pre-Policy Period

6.1.1 RD Validity and Underlying Assumptions

As noted by Lee and Lemieux (2010), the assumption of local randomization can be evaluated by examining the level of control an individual has over his/her test score. If one can exert only 'imprecise control', then the assignment variable will result in randomized variation in the vicinity of the threshold. McCrary's work outlines a partial method to further examine the manipulation of the running variable by analyzing the density of the running variable.[65] McCrary's test of manipulation examines the marginal density of the running variable by employing a regression analysis (McCrary, 2008). If the results warrant rejection of the continuous density assumption of the running variable, then the application of the RD design is questionable.

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[65] The word "partial' is purposely used for the statement provided in the text. A failure to reject the hypothesis of continuous density at the threshold is not complete evidence of the lack of manipulation of the running variable.

Examination of the manner in which the placement exam is administered and scored reveals an inability for human tampering with the scoring mechanism. A student is administered the exam in a proctored environment. A student must provide acceptable documentation of identity before beginning the exam. The ACCUPLACER exam is scored electronically; scores are downloaded to a database for use by the college administrators. The exam consists of multiple-choice questions and grading is not subjective, the answer is either right or wrong.

The distribution of the ACCUPLACER algebra scores, the running variable, is presented in Figure 19. The examination of the distribution is a critical 'test' to justify the imprecise control of the individuals on the placement score. The visual inspection of the distribution should indicate no discontinuities. The vertical scale represents the percentage of students in the analysis sample that received the score indicated on the horizontal axis. Keeping in mind that the placement scores are discrete in nature, each possible score is indicated on the horizontal axis. The vertical lines indicate the cutoff scores for placement within the developmental sequence. The algebra section score is the sole determinant for placement two levels below credit math, and three levels below credit math in the pre-policy period. Placement one level below credit math is determined by the algebra score if the score is in the range of 50 to 69, or by the college level math score if the student scores a 70 or above on the algebra section. A vertical line is shown at a value of 70 to designate the use of the secondary score, the college math score for placement. Placement into credit math courses is then determined solely by the college level math score for students who score a 70 or above of the algebra section. As explained previously in the Background Section, the exam is programmed such that seamless branching to the college level math section occurs after the completion of the algebra section if a student scores a 70 or above on the section.

Focusing on the threshold for placement one and two levels below credit, one would expect that if a student is able to exert control over his/her score, the distribution would have many more students scoring just above the threshold for placement one level below credit as opposed to two levels below credit. Visual inspection at the aforementioned threshold does not provide evidence of sorting at the threshold. Further, the distribution of the running variable does not exhibit a jump or discontinuity within the range for placement in the developmental math classes (Figure 20).

A statistical analysis complements the visual inspection of the distribution of the running variable. A modified version of McCrary's test is employed. The test begins with binning the running variable. With a discrete forcing variable, the natural selection for bin width is one. The number of observations per bin, or in this case the number of students per score, is used as the dependent variable in a regression. The independent variables are treatment status based on the placement score, interactions and quadratic terms to allow a flexible form. Cohort fixed effects are included in the regression. The results of the regression are presented in Table 11. The coefficient of interest is the treatment variable that will estimate the change in density at the threshold between the treatment and control group. The null hypothesis is no change in density of the running variable at the threshold. The regression result ($p=0.279$) does not yield evidence to reject the null and thus, the test does not suggest evidence of manipulation of the running variable.

To examine further the underlying assumption of local randomization, the baseline covariates are examined. Figures 21 - 25 display for each covariate the mean baseline covariate conditional on the placement score over the range of placement scores. The figures do not reveal jumps or discontinuities over the range of the placement scores. In other words, the visual inspection of the figures indicates the covariates seem to evolve smoothly over the range of scores, a result that is expected if local randomization was realized.

Further, regressions were run within specified bands of the threshold. The regressions used the mean characteristic per score as the dependent variable in least squares regression[66] and were examined to identify the presence of jumps at the placement cutoff. The results of the analyses are presented in Tables 12 through 16 for the tested bandwidths (5 through 14).

In addition, Levene's test for equality of variance was followed by a two-sample t test to examine the balance of the baseline covariates to determine whether the treatment and control groups had similar means. While RD does not require global balance of the covariates, the comparability of the treatment and control group's observable characteristics can be assessed.[67] The results are presented in Table 17. The examination reveals no significant imbalance for the covariates of age, female, student-type for the bandwidths of ten or less. For the larger bands (greater than 10), there is some imbalance, but this is not of concern since global randomization is not expected over the complete range of scores. Even in randomized experiments, there may not be complete balance of the covariates between the treatment and control groups. Furthermore, the covariates examined are controlled in the RD analysis and can compensate for the slight imbalances that are observed.

The T-tests for the observable characteristics of household income are significant across all bandwidths. Further examination for household income included the examination of parallel boxplots in Figure 26. Given the influence of the outliers on the mean, the median household income per score was examined. Again, referring to Figure 26, it is evident there is a great comparability of the median per score for the household income. A 5% trimmed mean for all cases within the specified bands was employed and the t tests were rerun. All tests were found to be not significant (Table 18). The outliers were not removed for the other analyses. The household incomes were assigned using census block information and thus, there is not a justification for their removal in the analysis.

The results for the variable minority indicate that there is a statistically significant difference in the proportion of minority students placing in the treatment group as compared to the control group for all bands except seven. The models control for minority status. As stated in the introduction, it is hypothesized that minority students may disproportionally be impacted by the change in policy being examined. The results of the t tests support such a claim and align with the state's findings for minority students.

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[66] The regressions were run using a weighted least squares using the weights designated as the number of observations per score. Results did not change and thus, are not presented.
[67] The RD validity checks on the covariates need to access whether the distribution of each of the characteristics is continuous at the selection threshold. This aforementioned validity test is of more importance of the T-Test.

6.1.1.1 Level of Compliance

As noted previously, a fuzzy RD framework is employed. In the case of perfect policy compliance, each individual assigned to treatment would enroll in the developmental math course two levels below credit. Likewise, each individual assigned to the control group would enroll in the development math course one level below credit. Figure 27 provides a visual of the level of compliance based on the initial placement score in the pre-policy period.[68] The percentage of students enrolling in the treatment group drops to near zero at the cutoff of 50.

6.1.1.2 RD Visual Inspection

As discussed in the RD primer section, Lee and Lemieux (2010) state one of the advantages of the RD method is its "transparency". To aid in this "transparency" the data is presented in a series of visuals. All the RD graphs are set-up as illustrated in Figure 28. The outcome is indicated at the top of the visual. Underneath the outcome, the results of the RD analysis are presented. The placement score used for the analysis is indicated on the horizontal axis. The scores are centered by subtracting the cutoff score at the boundary. The vertical axis provides the mean proportion of students in the sample attaining the outcome by the initial placement score. The treatment group is to the left of the vertical reference line at the cutoff for placement between one and two levels below credit. The control group is thus to the right of the red line. In Figure 28 a linear function is superimposed to aid in the visualization of the two regressions to the left and right of the cutoff. The RD graphs based on this analysis include quadratic terms and interactions. Three possible scenarios can exist for treatment effect – positive, none, and negative. The regression to the left of the cutoff does not extend to the red line because the lowest centered score for the treatment group is -1. The function needs to be visualized by extending it to the red line. If the function to the left is above the right sided-function, a positive effect is suggested (function A). If the function to the left extended to the red line is below the left-handed function, a negative effect is suggested. If the functions to the right and left of the cutoff score meet at the same vertical height then no effect is inferred. Focusing on the location of the cutoff for one and two levels below credit, indicated by a vertical red line, the graphs are using a starting point as to whether a discontinuity, an effect may exist.

Figures 29 through 31 provide the visuals for the mean short-term outcome per initial placement scores in the pre-policy period.

6.2 Pre-Policy RD Results

6.2.1 Outcome: Eligible to Register for Credit Math Class

The analysis included a detailed examination of numerous regressions for both the ITT and LATE with and without statistical controls for varying bandwidths. Table 19 outlines the process employed. Referring to Table 19, the first column indicates the covariates. The variable *assign* is the treatment

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[68] Students who retest may test higher within the sequence and this accounts for lower probabilities than perhaps expected for enrollment based on the initial placement scores.

status based on the placement score (0/1); *score* is the placement score centered by the cutoff for one and two levels below credit; *square_score* is the squared value of the centered placement score; *score_assign* is the interaction between the treatment status and the centered placement score; *sq_score_assign* is the interaction between the square of the centered placement score and the treatment status; *female-dum* is a dichotomous variable for gender; *minority_dummy* is the indicator for minority status, Asian and Caucasian are classified as non-minority; *new_st* is a dichotomous variable for whether the student was classified as a new, first time entrant to the college, or a transfer student. The value assigned to *new_st* is based on the institutional classification at time for enrollment; *age* is defined at time of first entry at the college is in units of years. Interactions between score and treatment and quadratic terms were included in the models. Cohort fixed effects with cluster robust standard errors based on the centered placement score are used in all models excepted noted otherwise. All standard errors are indicated below the coefficient values within parentheses. Significant results are indicated with starred values. A single star indicates significance at $p < 0.05$, two stars indicates a significance levels of $p < 0.01$ and three stars indicates a significant levels with $p < 0.001$. The coefficient of interest is the value of the *assign* or the *enroll* variable. For the 2SLS results, the coefficient on the assign variable will be blank and the coefficient of interest is the enroll variable.

The columns for the regressions are organized as follows:

<u>Regression 1</u>: ITT No Controls No Cohort provides the intent to treat estimate with no controls or cohort fixed effects.

<u>Regression 2</u>: RD-IV No Controls No Cohort provides the local average treatment estimates without statistical controls or cohort fixed effects.

<u>Regression 3</u>: ITT with Cohort Controls provides a modification to the first regression by the addition of cohort fixed effects.

<u>Regression 4</u>: RD-IV No Controls provides the local average treatment estimates with the addition of cohort fixed effects.

<u>Regression 5</u>: ITT with All Controls provides a modification of the first regression by the addition of the full set of statistical controls and cohort fixed effects. The coefficient of the *assign* variable represents an estimate of the impact of the effect of placement in the developmental math class for students on the short-term outcome examined at the observed level of compliance to the placement policy.

<u>Regression 6</u>: RD-IV with All Controls provides the local average treatment estimates with the addition of the full set of statistical controls and cohort fixed effects. The coefficient of the *enroll* variable provides the local average treatment effect for the subpopulation of students whose treatment was induced by the placement policy. The LATE is an adjusted value based on the intent-to-treat estimate as discussed previously. The summary visuals are based the coefficient of the *enroll* variable from the sixth regression.

Outcome: Eligible to Register for a Credit Math Class

Figure 32 provides the summary of the local average treatment effects (LATE) as a function of the bandwidth for the RD. The horizontal scale is the distance from the centered algebra placement scores used in the regression. For example, the bandwidth of 10 indicates that the treatment group range of scores employed were -10 to -1 while the control group scores ranged from 0 to 9 inclusive. Any value in the horizontal scale that has a triangle around it indicates a statistically significant result ($p<0.05$) for the enroll coefficient. A significant result was realized for the enroll coefficient for the bandwidths of 6, 7 and 9. The vertical scale is the LATE from the two stage least squares regressions (Regression 6 in Table 19) that included fixed effects for cohort entry, interaction and quadratic terms, and the full set of covariates (age, minority, household income, student type , gender). Table 20 provides the numerical statistical summaries of the LATEs illustrated in Figure 32 in the blue-shaded region. The estimates remained relatively stable over the bandwidths. The signs of all LATE estimates for the bandwidths are negative. Based on the results of the regressions, results are interpreted as weak evidence of a negative effect, thus indicating that the students who are required to complete two as opposed to one developmental math class are less likely to become eligible to complete the remedial math sequence and become eligible to enroll in a credit math class

6.2.2 Outcome: Enroll in a Credit Math Class

Figure 33 presents the summary of the LATE estimates for the outcome of registering for a credit-level math class as a function of the bandwidth employed in the analysis. Table 20 provides the numerical statistical summary of the LATE illustrated in Figure 32 in the pink-shaded region. No significant results were observed for the tested bandwidths. Based on the results of the regressions, no effect is indicated for the outcome register for a credit math class for students who placed near the divide of placement of two levels and one level below credit math classes. Students who are required to complete the additional developmental math class are no more likely to enroll in a credit math class than those who are required to complete a single developmental course.

6.2.3 Outcome: Successfully Complete a Credit Math Class

Figure 33 presents the summary of the LATE estimates for the outcome of successfully completing a credit math as a function of the bandwidth employed in the analysis. Table 20 provides the numerical statistical summary of the LATE illustrated in Figure 33 in the purple-shaded region. The estimates were found to be not statistically significant although the stability for the estimates was less than those realized for the other measures. Based on the results of the regressions, results indicate no effect for the outcome successfully completing a credit math class for students who placed near the divide of placement of two levels and one level below credit math classes. Students who are required to complete the additional developmental math class are no more likely to successfully complete a credit math class.

6.2.4 Pre-Policy RD Discussion

Developmental classes are supposed to prepare students for success in the credit math class. There is weak evidence to support the idea that students who are required to complete an additional semester of developmental math class may be less likely to become eligible to enroll in a credit math class. There is no evidence to suggest that students in the pre-policy period near the cutoff who are required to take an additional course of developmental coursework realize a statistically significant benefit for the short-term outcome of enrolling in a credit math class from the additional semester of coursework. Further, the results indicate there is no difference in the success rates in the credit math class for student who originally placed two as opposed to one level below credit math classes.

6.3 Post-Policy Period

6.3.1 RD Validity and Underlying Assumptions

Focusing on the threshold for placement one and two levels below credit in Figures 35 and 36, the distribution of the running variable does not exhibit a jump or discontinuity within the range for placement in the developmental math classes. There is no visual evidence to suggest that students are exerting complete control over their placement. Further, it is noted that more students score to the left of the cutoff, placing two levels below credit than one level below credit

A statistical analysis complements the visual inspection of the distribution of the running variable as discussed previously. The results of the regression are presented in Table 21. The coefficient of interest is the treatment variable that will estimate the change in density at the threshold between the treatment and control group. The regression result (p=0.201) does not yield evidence to reject the null, which indicates that a lack of manipulation of the running variable.

To examine further the underlying assumption of local randomization, the baseline covariates are considered. Figures 37 through 41 allow the visualization of the mean baseline covariate per placement score within each placement score over the range of placement scores for each covariate. The figures do not reveal jumps or discontinuities over the range of the placement scores. In other words, the visual inspection of the figures indicates the covariates seem to evolve smoothly over the range of scores, a result that is expected if local randomization is realized.

Further, regressions were run within specified bands of the threshold (Tables 22 – 27). The regressions used the mean characteristic per score as the dependent variable in least squares regression[69] and were examined to identify the presence of jumps at the placement cutoff., Table 22 provides the results for each of the covariates using the analysis window of the centered score of -20 to 13 (inclusive). Tables 23 -27 provide the results for each of the covariates using the varying bandwidths. Although the coefficient on

[69] The results of a weighted least squares (WLS) regression was using the number of observations per score for the weights were compared to the unweighted models. Results did not change and thus, the WLS models are not presented.

treatment is significant in the regression for age for the full range, no other tested bandwidth produced in a significant result. Further, results for discontinuity for the mean proportion of minority students does yield significant results at the majority of the bandwidths tested. Minority status is controlled in the RD analysis. The regression results indicate a higher proportion of minority students placed lower in the developmental math sequence.

The level of compliance in the post-policy period is presented in Figure 42. The graphical analysis based on mean outcomes without controls is presented in Figures 43 through 45.

6.3.2 Attrition Study

Because of the upward movement of the cutoff scores for placement one and two levels below credit classes in the post-policy period, an added complexity is present in the RD analysis. If a student receives a score of 70 on the algebra subsection, the test branches to college math section for the placement. To examine the possibility of relying solely on the algebra score for the RD, the level of attrition from the control groups is analyzed as a function of the algebra section score.

Figure 6.28 presents the percentage of the students who are retained in the control group, defined as students who place one level below credit math classes, as a function of the algebra placement score. For students who receive a score of 65 to 69 on the algebra section, the student is assigned to one level below credit math classes. For students who receive a score of 70 or above, the college level math exam is administered and the student must score a 45 or above to place in credit math classes. As evident from Figure46, as the algebra score increase, the percentage of students being placed into credit math increases. The range of particular interest for the attrition study is within the range of 65 to78, which corresponds to a maximum bandwidth of 14 for the RD analysis in the post-policy period.

To examine further the impact of the college level math score on placement of students within specified distances from the cutoff score of 65, Table 28 is presented. The attrition rate within the distance of 13 from the centered score or less is below 5%.[70]

To provide quantitative evidence beyond that of descriptives, a regression is employed to determine if a discontinuity in the density at the cutoff value of 70 exists. Table 29 presents the results of the regression. The score variable is denoted as *score_at* for the attrition study to indicate that the centered score is relative to cutoff score of 70 instead of using the centered score relative to the cutoff for placement one and two levels below credit math. The assign variable assumes the value of 1 if the *score_at* is 0 or above (which is equivalent to a score of 70 or above). Of interest is the coefficient on the variable assign. There are significant results for the maximum range. Since the entire range of algebra scores will not be employed for the RD, the significant result for this range is not of concern. A significant result was also realized for the bandwidths of 14 and 9. The significant results for these bandwidths coincide with higher rates of attrition in the control group (Table 27). For a bandwidth of 8 or below, the size of the sample

[70] An attrition rate of 5% is used an upper limit exclude participants and still obtain a reasonable estimate for an RD. (Shadish, Cook, Campbell reference to Judd and Kenny, 1991)

becomes limited and the significant results may be more of a function of the sample size than a true discontinuity and will be interpreted with caution.

In summary, it seems reasonable to use the algebra section for the running variable in the post-policy period for the RD analysis using a limited bandwidth. Interpretation of the RD results with bandwidths of less than 9 will be interpreted with caution due to the smaller sample sizes. Bandwidths of 9 or greater were run, but due to the higher rates of attrition in the larger bandwidths, the summary interpretation will not rely on the larger bandwidths.

6.4 Post-Policy RD Results

6.4.1 Outcome: Eligible to Register for Credit Math Class

Figure 47 provides the summary of the local average treatment effects (LATE) as a function of the bandwidth for the outcome eligible to register for a credit math class. Table 30 provides the numerical statistical summary of the LATE illustrated in Figure 47 in the blue-shaded region. The post-policy period results indicate no difference in eligibility for credit math for students at the margin of one and two levels below credit using a level of significance of 0.05.

6.4.2 Outcome: Enroll in a Credit Math Class

Figure 48 provides the summary of the local average treatment effects (LATE) as a function of the bandwidth for the outcome register for a credit math class. Table 30 provides the numerical statistical summary of the LATE illustrated in Figure 48 in the pink-shaded region. Based on the results of the regressions, there is no effect on the outcome of registering for a credit math class for students who placed near the divide of placement of two levels and one level below credit math in the post-policy period using a level of significance of 0.05.

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6.4.3 Outcome: Successfully Complete a Credit Math Class

Figure 49 provides the summary of the local average treatment effects (LATE) as a function of the bandwidth for the outcome successfully complete a credit math class. Table 30 provides the numerical statistical summaries of the LATEs illustrated in Figure 49 in the purple-shaded region. Significant results were realized for the bandwidths of 5 and 8 ($p<0.05$) and the bandwidths of 6 and 7 ($p<0.10$). Overall, results for the post-policy period indicate weak evidence that students may experience a slightly positive benefit for successful completion of a credit math class if an additional remedial class is completed.

6.4.4 Post-Policy RD Discussion

Students near the cutoff who were required to take an additional course in developmental math did not experience success rates that were different than their comparison group of students who completed a single course in developmental math for the outcome of eligible to enroll in a credit math class and enrolling in a credit math class. There is weak evidence that students may experience a benefit in the short-term outcome of successfully completing a credit math class if an additional course in developmental math is required.

7 Final Results and Discussion

This study adds to the discussion of developmental math courses at community colleges in a manner that previous research has not addressed. The focus of the study is unique in that it examines students who place within the developmental sequence rather than at the divide of credit and non-credit classes. There is little known about this group of students outside of the research of Boatman and Long (2012). By exploiting the change in placement scores at the study institution, a discussion of the effect of placement on short-term student outcomes based on two distinct sets of cutoff scores is possible. Given the current discussion of the placement test, this study is able to quantify whether more stringent placement criteria for developmental math classes at community colleges results in better outcomes.

Each year many students who enter community colleges are required by institutional policy to complete a placement exam for math. By setting cutoff scores high, more students are required to complete additional remedial work. For the underprepared student, the outcome of the placement test can result in a delay to entry to college-credit coursework.

At the study institution, minority and female students generally scored lower on the placement exam, and as a result were required to complete more remedial work as compared to non-minority or male students. Students who reside in areas of lower socioeconomic status, based on census track data, are more likely to place lower within the developmental math sequence. Under both placement polices, minorities and females were impacted, which resulted in additional remediation.

The students placed in remedial math must navigate through a sequence of developmental courses to prepare for a credit-level math class. Examining the flow by the students at the study institution through the sequence indicates that many students are lost on the journey to the credit math class. By upwardly adjusting the cutoff scores, there was a greater chance for students to lose sight of their pathway to credit level math classes. Compared to the students who placed higher in the developmental sequence, the students who initially placed two levels below credit math and eventually enrolled in a credit math class had a lower probability of being successful. For those students initially placing two levels below credit, approximately 22% of students using the lower cutoff scores and 24% of students using higher cutoff scores eventually passed the credit math class. For those students initially placing one level below credit, slightly more than 40% of students using the lower cutoff scores and slightly less than 40% of students using higher cutoff scores eventually passed the credit math class.

For students in the immediate vicinity of the cutoff score between introductory algebra and intermediate algebra there is no evidence to suggest an increase in likelihood of enrolling in a credit math for those required to complete two semesters of developmental math coursework instead of one semester regardless of the cutoff scores used for placement. During the period of lower cutoff scores, there was weak evidence to suggest that students near the cutoff but on the low side (and therefore required to complete the additional class of developmental math) were less likely to become eligible for enrolling in a credit math class than those on the high side. For the short-term outcome of successfully completing a credit math, students placed using the lower cutoff scores (i.e., the pre-policy period) did not experience a positive benefit from being required to complete the additional remedial math course. In the period of higher cutoff scores (i.e., post-policy period), there was weak evidence to suggest that students required to complete two instead of one developmental class were more likely to successfully complete a credit math class. For the short-term outcome of becoming eligible for credit math using higher cutoff scores for placement, students did not receive a benefit from completing the additional math class. In short, more is not consistently better.

Table 1: Pre-Policy Developmental Math Placement Scores

Math Placement (D) = Developmental (C)= College Credit	Arithmetic Section Score	Elementary Algebra Section Score	College-Level Math Section Score
(D) Arithmetic		Less Than 36	
(D) Introductory Algebra	This test was not administered for general math placement testing. There was no branching to this section.	36 - 49	
(D) Intermediate Algebra		50 – 69 **OR**	Less Than 45
(C) Credit Math*		At Least 70 **AND**	45– 69

The yellow enclosed region is the focus of the research.

*The State agreement requires a minimum score of a 45 on the ACCUPLACER college-level math section for placement in a college-credit general education math course or an equivalent score on COMPASS. This minimum score does not allow entry to credit courses that have prerequisites – for example, a student who scores a 45 on the college-level math section cannot register for pre-calculus if they have not completed the college algebra course.

Table 2: Post-Policy Developmental Math Placement Scores

Math Placement (D) = Developmental (C) = College Credit	Arithmetic Section Score	Elementary Algebra Section Score	College-Level Math Section Score
(D) Arithmetic	At Least 32 (Floor)	**AND** Less than 44	
(D) Introductory Algebra		44 - 64	
(D) Intermediate Algebra		At least 65 – 69 **OR**	Less Than 45
(C) Credit Math*		At Least 70 **AND**	45 – 69*

The yellow enclosed region is the focus of the research.

**Students who score above a 69 on the college-level math section can register for a higher-level credit math course.*

Table 3: Resulting Sample Size Due to Restrictions

Description for Maximum Sample Size		Maximum Sample Size
Students in the age range of 18 to 23 years old that rtook at least one math placement exam, provided a valid in-state address, enrolled in a class at the institution, and belonged to a fall cohort		14838
Exclusion Condition	**Number Excluded**	**Sample Size**
Completed placement exam more than twice	869	13969
Completed college level math section more times than algebra section	493	13476
Invalid code for term of placement exam	7	13469
Completed college level section on a different date than algebra section	72	13397
Completed college level math section without receiving a qualifying score on algebra section to prompt branching	141	13256
Completed placement exam in post-policy, but assigned to pre-policy cohort	487	12769
Census Block Match but income not available	7	12762
Total Sample Size for Study	**2076**	**12762**

Table 4: Student Mean Characteristics in Study Period Fall 2000 to Fall 2007

Fall Cohorts Fall 2000 - Fall 2007	N	min	max	mean	median	std
Age (years)	12762	18.00	24.00	19.15	18.61	1.32
HH_inc (per $10000)	12762	0.66	20.00	5.02	4.77	1.83
Female	12762			0.53		0.50
Minority	12595			0.39		0.49
Student Type (new)	12762			0.91		0.28

Table 5: Pre-Policy Period Mean Student Characteristics Based on the Initial Placement Score

Placement	Variable	N*	min	max	mean	sd
Credit	Age (years)		18.00	23.94	19.11	1.29
N=577	HH_inc (per $10,000)		1.50	20.00	5.46	2.04
	Female	574			0.43	0.50
	Minority	564			0.17	0.38
	New				0.90	0.30
One Level Below Credit	Age (years)		18.00	23.89	19.00	1.20
Full Range	HH_inc (per $10,000)		0.93	20.00	5.25	1.80
N=3032	Female	3006			0.52	0.50
	Minority	2983			0.26	0.44
	New				0.93	0.26
Two Levels Below Credit	Age (years)		18.00	23.98	18.98	1.19
N=1739	HH_inc (per 10,000)		0.66	14.70	5.03	1.77
	Female	1724			0.55	0.50
	Minority	1713			0.35	0.48
	New				0.96	0.20
Three Levels Below Credit	Age (years)		18.00	23.98	19.32	1.42
	HH_inc (per 10,000)		0.70	13.55	4.67	1.66
	Female	2825			0.59	0.49
	Minority	2804			0.48	0.50
N=2847	New				0.96	0.19
Total Sample	Age (years)		18.00	24.00	19.12	1.30
	HH_inc (per 10,000)		0.66	20.00	5.04	1.82
	Female	8129			0.54	0.50
	Minority	8064			0.35	0.48
N=8195	New				0.94	0.23

Table 6: Pre-Policy Placement Statistics

placement_ea	variable	min	max	mean	sd
Credit	CPTG Score	20	119	69.80	13.18
	Centered CPTG	0	69	52.07	13.18
	Math_Class_Enrollment			0.88	0.33
	Enrollment:Three Levels Below			0.00	0.00
	Enrollment: Two Levels Below Credit			0.00	0.00
	Enrollment: One Level Below Credit			0.01	0.10
	Enrollment: Credit Class			0.88	0.33
	Pass Three Levels Below Credit			0.00	0.00
	Pass Two Levels Below Credit			0.00	0.00
	Pass One Level Below Credit			0.01	0.10
N=577	Pass Credit Class			0.74	0.44
One Level Below Credit Complete Analysis Set	CPTG Score	50	119	69.80	15.58
	Centered CPTG	0	69	19.83	15.58
	Math_Class_Enrollment			0.85	0.36
	Enrollment:Three Levels Below			0.00	0.07
	Enrollment: Two Levels Below Credit			0.02	0.14
	Enrollment: One Level Below Credit			0.67	0.45
	Enrollment: Credit Class			0.55	0.50
	Pass Three Levels Below Credit			0.00	0.06
	Pass Two Levels Below Credit			0.01	0.11
	Pass One Level Below Credit			0.46	0.50
N=3032	Pass Credit Class			0.41	0.49
One Level Below Credit Subset less than 70 cptg	CPTG Score	50	69	58.91	5.46
	Centered CPTG	0	19	8.91	5.46
	Math_Class_Enrollment			0.85	0.35
	Enrollment:Three Levels Below			0.01	0.01
	Enrollment: Two Levels Below Credit			0.03	0.16
	Enrollment: One Level Below Credit			0.71	0.45
	Enrollment: Credit Class			0.51	0.50
	Pass Three Levels Below Credit			0.01	0.07
	Pass Two Levels Below Credit			0.02	0.12
	Pass One Level Below Credit			0.46	0.50
N=1797	Pass Credit Class			0.37	0.48
Two Levels Below Credit	CPTG Score	36	49	41.77	4.04
	Centered CPTG	-14	-1	-8.23	4.04
	Math_Class_Enrollment			0.81	0.39
	Enrollment:Three Levels Below			0.02	0.15
	Enrollment: Two Levels Below Credit			0.62	0.49
	Enrollment: One Level Below Credit			0.45	0.50
	Enrollment: Credit Class			0.31	0.46
	Pass Three Levels Below Credit			0.02	0.13
	Pass Two Levels Below Credit			0.39	0.49
	Pass One Level Below Credit			0.28	0.45
N=1739	Pass Credit Class			0.22	0.41
Three Levels Below Credit	CPTG Score	20	35	28.13	4.60
	Centered CPTG	-30	-15	-21.87	4.50
	Math_Class_Enrollment			0.72	0.45
	Enrollment:Three Levels Below			0.50	0.50
	Enrollment: Two Levels Below Credit			0.42	0.49
	Enrollment: One Level Below Credit			0.25	0.43
	Enrollment: Credit Class			0.14	0.35
	Pass Three Levels Below Credit			0.32	0.47
	Pass Two Levels Below Credit			0.25	0.43
	Pass One Level Below Credit			0.15	0.36
N=2847	Pass Credit Class			0.10	0.50
Total of Analysis Set for PrePolicy	CPTG Score	36	69	50.48	9.83
	Centered CPTG	-14	19	0.48	9.83
	Math_Class_Enrollment			0.83	0.37
	Enrollment:Three Levels Below			0.01	0.12
	Enrollment: Two Levels Below Credit			0.32	0.47
	Enrollment: One Level Below Credit			0.59	0.49
	Enrollment: Credit Class			0.41	0.49
	Pass Three Levels Below Credit			0.01	0.10
	Pass Two Levels Below Credit			0.20	0.40
	Pass One Level Below Credit			0.37	0.48
N=3536	Pass Credit Class			0.29	0.46

Table 7: Post-Policy Period Mean Student Characteristics Based on the Initial Placement Score

Placement	Variable	N*	min	max	mean	std
Credit	Age		18.00	23.93	19.24	1.48
N=313	HH_inc		0.88	16.48	5.59	2.03
	Female				0.44	0.50
	Minority	312			0.18	0.38
	New				0.75	0.44
One Levels Below Credit	Age		18.00	23.85	19.24	1.36
N=943	HH_inc		0.95	14.58	5.41	2.00
	Female	936			0.49	0.50
	Minority	935			0.31	0.46
	New				0.78	0.41
Two Levels Below Credit	Age		18.00	23.96	19.06	1.21
N=1190	HH_inc		0.71	14.33	5.12	1.78
	Female	1187			0.50	0.50
	Minority	1178			0.39	0.49
	New				0.86	0.35
Three Levels Below Credit	Age		18.00	24.00	19.26	1.42
N=2121	HH_inc		0.70	13.55	4.61	1.72
	Female	2112			0.56	0.50
	Minority	2108			0.59	0.49
	New				0.91	0.29
Total	Age		18.00	24.00	19.21	1.36
N=4567	HH_inc		0.70	16.48	4.98	1.86
	Female	4548			0.52	0.50
	Minority	4533			0.46	0.50
	New				0.86	0.35

* Sample size indicated if all observations are not included in the calculation due to missingness.

Table 8: Post-Policy Students Characteristics by Placement

Placement	Variable	Min	Max	Mean	SD
Credit Class	CPTG Score	70	119	102.00	13.38
	Centered CPTG	5	54	37.00	13.38
	Math_Class_Enrollment			0.85	0.36
	Enrollment:Three Levels Below			0.00	0.06
	Enrollment: Two Levels Below Credit			0.01	0.08
	Enrollment: One Level Below Credit			0.03	0.16
	Enrollment: Credit Class			0.83	0.37
	Pass Three Levels Below Credit			0.00	0.00
	Pass Two Levels Below Credit			0.01	0.08
	Pass One Level Below Credit			0.02	0.14
N=313	Pass Credit Class			0.67	0.47
One Level Below Credit	CPTG Score	65	116.00	79.58	12.25
	Centered CPTG	0	51.00	14.58	12.25
	Math_Class_Enrollment			0.82	0.38
	Enrollment:Three Levels Below			0.00	0.05
	Enrollment: Two Levels Below Credit			0.01	0.12
	Enrollment: One Level Below Credit			0.71	0.45
	Enrollment: Credit Class			0.53	0.49
	Pass Three Levels Below Credit			0.00	0.03
	Pass Two Levels Below Credit			0.01	0.11
	Pass One Level Below Credit			0.51	0.50
N=943	Pass Credit Class			0.39	0.49
Two Levels Below Credit	CPTG Score	44	64.00	53.91	6.21
	Centered CPTG	-21	-1.00	-11.10	6.21
	Math_Class_Enrollment			0.83	0.37
	Enrollment:Three Levels Below			0.01	0.11
	Enrollment: Two Levels Below Credit			0.59	0.49
	Enrollment: One Level Below Credit			0.58	0.49
	Enrollment: Credit Class			0.35	0.48
	Pass Three Levels Below Credit			0.01	0.09
	Pass Two Levels Below Credit			0.43	0.50
	Pass One Level Below Credit			0.39	0.49
N=1190	Pass Credit Class			0.24	0.43
Three or More Levels Below Credit	CPTG Score	21	43.00	31.37	6.50
	Centered CPTG	-44	-22.00	-33.63	6.50
	Math_Class_Enrollment			0.69	0.46
	Enrollment:Three Levels Below			0.56	0.49
	Enrollment: Two Levels Below Credit			0.42	0.49
	Enrollment: One Level Below Credit			0.24	0.43
	Enrollment: Credit Class			0.12	0.33
	Pass Three Levels Below Credit			0.36	0.48
	Pass Two Levels Below Credit			0.25	0.43
	Pass One Level Below Credit			0.15	0.36
N=2121	Pass Credit Class			0.09	0.28

Table 8 continued

Total Sample Size	CPTG Score	21	119.00	52.04	24.50
	Centered CPTG	-44	54.00	-12.96	
	Math_Class_Enrollment			0.77	0.42
	Enrollment:Three Levels Below			0.27	0.44
	Enrollment: Two Levels Below Credit			0.35	0.48
	Enrollment: One Level Below Credit			0.41	0.49
	Enrollment: Credit Class			0.31	0.46
	Pass Three Levels Below Credit			0.18	0.38
	Pass Two Levels Below Credit			0.23	0.42
	Pass One Level Below Credit			0.28	0.45
N=4567	Pass Credit Class			0.23	0.42

Table 9: Study Institution National Comparisons

	Percentage of Student-body with Characteristic				
Observable Characteristic	Current Study*	NCES-IES	Achieving the Dream (Bailey et al., 2010)	ELS 2002 (Bailey,et al., 2010)	Year and Table of NCES
Male percentage	40	43.0	44	45	Fall 2009, Table 196
Female percentage	60	57.0	56	55	Fall 2009, Table 196
On financial aid, part time students	26 received Pell Grants; 38 received loans, scholarships, or financial aid	44.5			2007-2008 year, Table 360
On financial aid, full time students		65.4			2007-2008 year, Table 358
First time college	33				
Minority (black & Hispanic)	31	32.7	39	23	2010 year, Table 238
Developmental Completers in 2006 cohort	39				
Never enrolled in a math class	17 (Pre) 23 (Post)		20		
Assessed with Developmental Needs	73				
Enrolled in math remedial course in 1st year	58.9 (Pre-Policy) 61.2 (Post-Policy)	60 (any remedial)			BPS 04:09**
Full time student	36	55.3			2009 year, Table 202
Part time student	64	44.7			2009 year, Table 202
Employed full time	60 (at least 20 hours per week)	28.6			2003-2004, Table 347
Employed part time		39.9			2003-2004, Table 347
Full time permanent faculty	30	30.2			Fall 2009, Table 259
Part time adjunct faculty	70	69.8			Fall 2009, Table 259
Age	19.2 years			19.1	
Age 18-24 of full time student		71.6			Fall 2009, Table 202
Age 18-24 of part time students		39.5			Fall 2009, Table 202
Ages 18-24 of all students		52.6			Fall 2009, Table 202

*Year 2009 unless noted otherwise **NCES, 2011:2011-275

Table 10: Comparison with Achieving the Dream (ATD) Developmental Rates*

Distance from Credit Math Class	Never Enrolled in a Math Class			Enrolled in Referred Developmental Class			Completed Developmental Math Sequence			Enrolled in Credit Math Class based on Initial Referral		Passed Credit Math Class			Passed Credit Math Conditional on Enrolling In Credit Math		
	Pre	Post	ATD	Pre	Post	ATD	Pre	Post	ATD	Pre*	Post	Pre	Post	ATD	Pre	Post	ATD
One Level	15	18	24	67	71	76	46	51	45	55	53	41	39	27	74	73	78
Two Levels	19	17	22	62	59	78	28	39	32	31	35	22	24	20	70	68	81
Three Levels	28	31	17	50	56	83	15	15	17	14	80	10	9	10	72	68	78

*(Bailey, et al, 2010)

Table 11: Pre-Policy Initial Score Density Distribution

Variable: Count	Unstandardized coefficient	se	p-value
score	4.626^{**}	1.585	0.008
treatment	12.51	11.288	0.279
square_score	-0.336^{***}	0.081	0.000
sq_score_treatment	0.783^{**}	0.222	0.002
score_treatment	-3.577	3.580	0.328
Constant	109.1^{**}	1.585	0.008
Observations	34		

p-values in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
*Score Range -14<score<20.

Table 12: Pre-Policy Density Testing for Age

Dependent Variable: Age								
Bandwidth:	Full	14	12	10	9	8	7	6
score	0.0211	0.0152	-0.0150		-0.0294	0.0110	0.0211	0.0152
	(0.0127)	(0.0199)	(0.0234)	(0.0470)	(0.0583)	(0.0737)	(0.0127)	(0.0199)
treatment	-0.0600	-0.0890	-0.0356	-0.146	-0.176	-0.0868	-0.0600	-0.0890
	(0.144)	(0.150)	(0.166)	(0.184)	(0.214)	(0.231)	(0.144)	(0.150)
square_score	-0.000968	-0.000534	0.00302	0.00533	0.00478	-0.00191	-0.000968	-0.000534
	(0.000654)	(0.00139)	(0.00218)	(0.00537)	(0.00725)	(0.0108)	(0.000654)	(0.00139)
sq_score_treatment	0.00131	0.000371	0.00157	-0.00355	-0.00566	0.00659	0.00131	0.000371
	(0.00249)	(0.00311)	(0.00463)	(0.00702)	(0.0100)	(0.0163)	(0.00249)	(0.00311)
score_treatment	-0.0437	-0.0451	0.0361	0.0255	-0.00123	0.00328	-0.0437	-0.0451
	(0.0397)	(0.0465)	(0.0551)	(0.0798)	(0.109)	(0.105)	(0.0397)	(0.0465)
f2001	0.318	0.701	0.205	0.217	0.326	0.300	0.318	0.701
	(0.641)	(0.703)	(0.680)	(0.724)	(0.828)	(1.042)	(0.641)	(0.703)
f2002	0.179	0.802	0.617	0.364	0.453	0.0657	0.179	0.802
	(0.704)	(1.044)	(1.076)	(1.152)	(1.211)	(1.838)	(0.704)	(1.044)
f2003	0.144	0.647	0.331	0.771	0.807	1.064	0.144	0.647
	(0.492)	(0.850)	(1.188)	(1.355)	(1.437)	(1.866)	(0.492)	(0.850)
f2004	0.405	0.793	0.0416	-0.598	-0.384	-0.224	0.405	0.793
	(0.825)	(1.370)	(1.061)	(1.110)	(1.370)	(1.383)	(0.825)	(1.370)
Constant	18.62^{***}	18.25^{***}	18.63^{***}	18.74^{***}	18.65^{***}	18.62^{***}	18.62^{***}	18.25^{***}
	(0.387)	(0.642)	(0.649)	(0.663)	(0.752)	(1.012)	(0.387)	(0.642)
Number of Clusters	34	28	24	20	18	16	14	12

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13: Pre-Policy Density Testing for Female

Dependent Variable: Female								
Bandwidth:	Full	14	12	10	9	8	7	6
score	-0.00186	0.0142	0.00332	-0.0128	-0.0164	0.0305	0.0282	0.0490
	(0.0123)	(0.0175)	(0.0229)	(0.0348)	(0.0417)	(0.0370)	(0.0453)	(0.0672)
treatment	-0.0338	-0.00847	-0.0413	-0.0359	-0.00352	0.0708	0.0938	0.126
	(0.0598)	(0.0654)	(0.0827)	(0.0959)	(0.0981)	(0.0962)	(0.106)	(0.116)
square_score	0.000118	-0.00124	-0.0000383	0.00219	0.00280	-0.00495	-0.00498	-0.00999
	(0.000538)	(0.00108)	(0.00166)	(0.00314)	(0.00446)	(0.00401)	(0.00592)	(0.0125)
sq_score_treatment	-0.000569	0.00143	-0.00121	-0.00187	0.000399	0.0100	0.0126	0.0211
	(0.000774)	(0.00122)	(0.00251)	(0.00412)	(0.00509)	(0.00496)	(0.00665)	(0.0167)
score_treatment	-0.00796	-0.0165	-0.0218	0.00903	0.0381	0.0113	0.0288	0.0254
	(0.0187)	(0.0214)	(0.0299)	(0.0413)	(0.0451)	(0.0390)	(0.0518)	(0.0801)
f2001	0.0261	-0.0731	-0.0487	-0.303	-0.422	-0.675*	-0.579	-0.362
	(0.293)	(0.325)	(0.365)	(0.333)	(0.336)	(0.311)	(0.285)	(0.667)
f2002	-0.313	-0.578	-0.382	-0.601	-0.698	-1.383**	-1.082*	-0.778
	(0.345)	(0.473)	(0.495)	(0.504)	(0.543)	(0.341)	(0.499)	(1.155)
f2003	-0.265	-0.273	-0.351	-0.439	-0.478	-0.650	-0.813	-0.777
	(0.240)	(0.282)	(0.352)	(0.345)	(0.334)	(0.417)	(0.565)	(0.860)
f2004	0.0102	-0.377	-0.195	-0.477	-0.711	-0.768**	-0.636	-0.301
	(0.336)	(0.401)	(0.526)	(0.476)	(0.463)	(0.258)	(0.362)	(0.956)
Constant	0.647**	0.773**	0.721*	0.905**	1.004**	1.204***	1.134***	0.946
	(0.198)	(0.268)	(0.310)	(0.279)	(0.277)	(0.216)	(0.214)	(0.579)
Number of Clusters	34	28	24	20	18	16	14	12

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 14: Pre-Policy Density Testing for Household Income

Dependent Variable: Household Income								
Bandwidth:	Full	14	12	10	9	8	7	6
score	-0.0259	-0.0217	0.0307	0.0733	0.0762	0.116	0.182	0.139
	(0.0294)	(0.0404)	(0.0539)	(0.0881)	(0.107)	(0.152)	(0.213)	(0.328)
treatment	-0.456	-0.426	-0.394	-0.583	-0.417	-0.328	-0.498	-0.380
	(0.231)	(0.261)	(0.272)	(0.331)	(0.374)	(0.466)	(0.546)	(0.806)
square_score	0.00135	0.000964	-0.00510	-0.0114	-0.0116	-0.0181	-0.0292	-0.0187
	(0.00139)	(0.00270)	(0.00471)	(0.00977)	(0.0127)	(0.0207)	(0.0339)	(0.0627)
sq_score_treatment	-0.00297	-0.00261	0.00479	-0.000807	0.0122	0.0242	0.0138	0.0273
	(0.00369)	(0.00457)	(0.00821)	(0.0131)	(0.0170)	(0.0321)	(0.0442)	(0.116)
score_treatment	-0.00724	-0.0104	-0.0482	-0.205	-0.0962	-0.0910	-0.301	-0.138
	(0.0612)	(0.0703)	(0.0844)	(0.160)	(0.205)	(0.239)	(0.308)	(0.457)
f2001	-1.146	-1.187	-0.829	0.457	-0.0535	-0.0734	0.00970	1.350
	(1.231)	(1.286)	(1.350)	(1.403)	(1.896)	(2.322)	(3.017)	(5.663)
f2002	-0.445	-0.750	-1.202	-0.435	-0.820	-1.193	-2.100	-0.332
	(1.376)	(2.075)	(2.017)	(2.244)	(2.247)	(3.192)	(5.824)	(8.910)
f2003	1.294	1.442	2.495	3.412	3.260	3.522	4.395	5.448
	(0.890)	(1.517)	(2.006)	(2.765)	(2.606)	(3.358)	(4.051)	(6.053)
f2004	-1.108	-0.463	-0.568	0.0860	-0.660	-0.498	-0.526	2.086
	(1.293)	(1.481)	(1.701)	(1.876)	(2.345)	(2.686)	(3.798)	(7.637)
Constant	5.619^{***}	5.522^{***}	5.294^{***}	4.536^{**}	4.889^{**}	4.854^{*}	4.805	3.475
	(0.712)	(0.943)	(1.026)	(1.207)	(1.430)	(1.870)	(2.635)	(4.903)
Number of Clusters	34	28	24	20	18	16	14	12

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 15: Pre-Policy Density Testing for Minority

Dependent Variable: Minority								
Bandwidth:	Full	14	12	10	9	8	7	6
score	0.00224	-0.0103	-0.00984	0.00149	-0.00642	-0.0360	-0.0133	-0.0486
	(0.00660)	(0.00818)	(0.0127)	(0.0199)	(0.0228)	(0.0343)	(0.0401)	(0.0564)
treatment	0.130*	0.116	0.0896	0.0468	0.0152	-0.0512	-0.0962	-0.106
	(0.0634)	(0.0697)	(0.0768)	(0.0849)	(0.0812)	(0.0875)	(0.101)	(0.130)
square_score	-0.000412	0.000641	0.000523	-0.000986	0.000228	0.00511	0.000994	0.00953
	(0.000360)	(0.000619)	(0.00122)	(0.00231)	(0.00301)	(0.00516)	(0.00709)	(0.0107)
sq_score_treatment	0.00104	0.0000922	-0.00131	-0.00155	-0.00448	-0.0136	-0.0154	-0.0223
	(0.00111)	(0.00118)	(0.00227)	(0.00337)	(0.00424)	(0.00679)	(0.00884)	(0.0186)
score_treatment	0.0128	0.0271	0.00958	-0.0188	-0.0256	-0.0300	-0.0943	-0.0508
	(0.0180)	(0.0164)	(0.0258)	(0.0437)	(0.0501)	(0.0458)	(0.0579)	(0.0734)
f2001	0.275	-0.0174	0.0851	0.246	0.309	0.321	0.426	0.489
	(0.346)	(0.333)	(0.367)	(0.416)	(0.410)	(0.482)	(0.426)	(0.858)
f2002	0.404	0.110	0.233	0.186	0.222	0.497	0.394	0.456
	(0.486)	(0.436)	(0.495)	(0.612)	(0.633)	(0.728)	(0.791)	(1.339)
f2003	-0.343	-0.746*	-0.691	-0.384	-0.371	-0.575	-0.378	-0.166
	(0.308)	(0.349)	(0.534)	(0.643)	(0.641)	(0.711)	(0.786)	(0.984)
f2004	0.445	0.132	0.380	0.140	0.127	0.00160	0.0938	0.324
	(0.322)	(0.337)	(0.451)	(0.512)	(0.631)	(0.670)	(0.571)	(1.062)
Constant	0.148	0.432*	0.326	0.280	0.267	0.296	0.224	0.127
	(0.222)	(0.199)	(0.264)	(0.334)	(0.373)	(0.412)	(0.362)	(0.728)
Number of Clusters	34	28	24	20	18	16	14	12

Standard errors in parentheses * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 16: Pre-Policy Density Testing for New Students

Dependent Variable: New Student								
Bandwidth:	Full	14	12	10	9	8	7	6
score	0.000632	0.00190	0.00545	-0.00392	-0.00816	-0.0257	-0.0293	-0.0238
	(0.00402)	(0.00654)	(0.00899)	(0.0108)	(0.0120)	(0.0137)	(0.0161)	(0.0200)
treatment	0.0122	0.0221	0.0279	0.0234	0.0128	-0.00769	-0.0129	0.00996
	(0.0248)	(0.0290)	(0.0327)	(0.0427)	(0.0455)	(0.0310)	(0.0335)	(0.0401)
square_score	-0.0000961	-0.000181	-0.000565	0.000679	0.00134	0.00424*	0.00520*	0.00389
	(0.000218)	(0.000447)	(0.000721)	(0.00108)	(0.00145)	(0.00194)	(0.00240)	(0.00383)
sq_score_treatment	-0.000195	0.000211	0.000759	-0.000403	-0.00149	-0.00396	-0.00528*	-0.000603
	(0.000470)	(0.000662)	(0.00107)	(0.00142)	(0.00200)	(0.00233)	(0.00238)	(0.00597)
score_treatment	-0.00483	-0.00176	-0.00322	0.00679	0.00744	0.0255	0.0284	0.0397
	(0.00714)	(0.00898)	(0.0110)	(0.0170)	(0.0203)	(0.0168)	(0.0237)	(0.0307)
f2001	0.0647	-0.0438	-0.0295	-0.0947	-0.0806	0.0704	-0.0174	0.181
	(0.155)	(0.135)	(0.145)	(0.175)	(0.180)	(0.124)	(0.151)	(0.339)
f2002	0.137	-0.132	-0.169	-0.172	-0.168	0.149	-0.0293	0.239
	(0.248)	(0.194)	(0.205)	(0.193)	(0.198)	(0.142)	(0.329)	(0.591)
f2003	0.0483	-0.0435	-0.0198	-0.0708	-0.0704	0.112	0.176	0.274
	(0.116)	(0.121)	(0.163)	(0.211)	(0.207)	(0.167)	(0.213)	(0.279)
f2004	0.115	0.00973	-0.0255	-0.0129	-0.0509	0.0312	-0.0782	0.269
	(0.120)	(0.138)	(0.177)	(0.190)	(0.225)	(0.191)	(0.220)	(0.622)
Constant	0.864***	0.977***	0.980***	1.010***	1.017***	0.884***	0.947***	0.763*
	(0.110)	(0.0929)	(0.107)	(0.115)	(0.121)	(0.0860)	(0.103)	(0.322)
Number of Clusters	34	28	24	20	18	16	14	12

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 17: Pre-Policy T-Test for Covariates

Bandwidth	**Bandwidth +/- 5**			**Bandwidth +/- 6**			**Bandwidth +/- 7**			**Bandwidth +/- 8**			**Bandwidth +/- 9**			**Bandwidth +/- 10**			**Bandwidth +/- 12**		
	T	**C**	**t-test**	**T**	**C**	**t-test**	**T**	**C**	**t-test**	**T**	**C**	**t-test**	**T**	**C**	**t-test**	**T**	**C**	**t-test**	**T**	**C**	**t-test**
Age	18.83	18.87	0.63	18.83	18.86	0.34	18.85	18.86	0.25	18.87	18.86	-0.17	18.87	18.88	0.11	18.9	18.9	-0.01	18.97	18.92	-1.06
n																					
HH_inc	5	5.34	2.85	5.05	5.31	2.48	5.06	5.27	2.19	5.06	5.29	2.6	5.08	5.28	2.4	5.04	5.26	2.89	5.05	5.26	2.88
n																					
Female	0.51	0.53	0.8	0.53	0.53	0.03	0.54	0.53	-0.43	0.55	0.53	-0.68	0.55	0.54	-0.55	0.55	0.54	-0.21	0.54	0.54	0
n	*490*	*472*		*607*	*574*		*712*	*652*		*836*	*770*		*967*	*880*		*1109*	*986*		*1402*	*1173*	
Student_Type*	0.96	0.93	-1.57	0.96	0.92	-1.9	0.96	0.94	-1.73	0.96	0.94	-1.56	0.95	0.94	-1.44	0.96	0.94	-1.63	0.96	0.94	-2.2
n																					
Minority*	0.39	0.3	-2.87	0.38	0.32	-2.24	0.36	0.31	-1.83	0.36	0.31	-2.09	0.35	0.3	-2.27	0.35	0.3	-2.49	0.34	0.29	-2.96
n	*490*	*464*		*607*	*565*		*711*	*642*		*834*	*757*		*964*	*868*		*1104*	*976*		*1392*	*1159*	
Observations	495	472		613	574		720	652		845	770		977	883		1120	991		1413	1179	
Total within Band			967			1187			1372			1615			1860			2111			2592

Pre-Policy T-Tests for Comparison of Means (continued)

Bandwidth	Bandwidth +/- 14			BandwidthFull Range for Analysis			BandwidthFull Range for Placement		
	T	C	t-test	-14<=score<20			-14<=score<70		
Age	18.98	18.92	-1.5	18.98	18.91	-1.63	18.98	19.01	0.69
n									
HH_inc	5.03	5.27	3.62	5.03	5.26	3.71	5.03	5.32	5.16
n									
Female	0.55	0.53	-0.95	0.55	0.54	-0.61	0.55	0.52	-2.16
n	*1724*	*1354*		*1724*	*1783*		1805	2925	
Student_Type*	0.96	0.94	-2.2	0.96	0.93	-3.18	0.96	0.92	-4.75
n									
Minority*	0.35	0.29	-3.87	0.35	0.28	-4.63	0.35	0.26	-6.74
n	*1713*	*1342*		*1713*	*1773*		1713	*2983*	
Observations	1739	1363		1739	1797		1739	3032	
Total within Band			3102			3536			4771

*Unequal Variance

Table 18: Pre-Policy Modified T-Test for HH_Income with Trimmed Mean

Bandwidth	5% Trimmed Mean			All cases
	treatment	control	t-test	
5	5.00	5.17	1.87	2.85
	446	*431*		
6	5.03	5.15	1.47	2.48
	548	*521*		
8	*5.06*	*5.11*	*0.87*	2.6
	756	*700*		
10	5.07	5.1	0.56	2.89
	872	*804*		
12	5.05	*5.08*	0.53	2.88
	1268	*1066*		
14	5.02	*5.09*	1.27	3.62
	1556	*1235*		

Table 19: Sample Regressions in Analysis*

Variable	Regression1	Regression 2	Regression 3	Regression 4	Regression 5	Regression 6
	ITT No Controls NoCohort	RD-IV No Controls NoCohort	ITT with Cohort Controls	LATE with Cohort Controls	ITT with All Controls	LATE with All Controls
assign	-0.0420		-0.0610		-0.0532	
	(0.0360)		(0.0398)		(0.0447)	
score	-0.0193	-0.0208	-0.0188	-0.0211	-0.0201	-0.0219
	(0.0153)	(0.0149)	(0.0149)	(0.0142)	(0.0160)	(0.0154)
square_score	0.00374^{*}	0.00386^{*}	0.00356^{*}	0.00375^{*}	0.00368	0.00383^{*}
	(0.00165)	(0.00158)	(0.00162)	(0.00152)	(0.00177)	(0.00167)
score_assign	0.0397	0.0383	0.0300	0.0281	0.0301	0.0289
	(0.0221)	(0.0209)	(0.0234)	(0.0218)	(0.0252)	(0.0234)
square_score_assign	-0.00226	-0.00248	-0.00290	-0.00323	-0.00323	-0.00345
	(0.00213)	(0.00212)	(0.00225)	(0.00220)	(0.00246)	(0.00237)
enroll		-0.106		-0.154		-0.133
		(0.0856)		(0.0946)		(0.103)
female_dum					0.108^{***}	0.114^{***}
					(0.0190)	(0.0193)
minority_dummy					-0.0821^{***}	-0.0779^{***}
					(0.0182)	(0.0177)
HH_inc					0.0132	0.0118
					(0.00937)	(0.00939)
new_st					-0.0139	-0.0103
					(0.0470)	(0.0467)
age					-0.0330^{***}	-0.0362^{***}
					(0.00829)	(0.00817)
Constant	0.398^{***}	0.405^{***}	0.219^{***}	0.230^{***}	0.749^{***}	0.820^{***}
	(0.0221)	(0.0245)	(0.0206)	(0.0208)	(0.149)	(0.145)
Observations	2111	2111	2111	2111	2064	2064

**Example table based on a bandwidth of 10 in the Pre-Policy Period for the outcome eligible for enrolling in a credit math class*

Table 20: Pre-Policy Summary of LATE for Bandwidths

Policy Period: Pre							
Outcome	Bandwidth	B	SE	P	Lower Limit 95% CI	Upper Limit 95% CI	N
Eligible	5	-0.121	0.068	0.073	-0.253	0.011	949
	6	-0.156	0.076	0.041	-0.305	-0.006	1166
	7	-0.295	0.082	0.000	-0.455	-0.134	1345
	8	-0.130	0.067	0.053	-0.261	0.002	1582
	9	-0.163	0.076	0.033	-0.312	-0.013	1819
	10	-0.133	0.103	0.198	-0.334	0.069	2064
	11	-0.097	0.088	0.271	-0.269	0.075	2281
	12	-0.150	0.079	0.057	-0.305	0.004	2534
	13	-0.121	0.065	0.064	-0.248	0.007	2786
	14	-0.042	0.069	0.541	-0.177	0.093	3031
Enrolled	5	0.041	0.138	0.768	-0.230	0.312	949
	6	-0.097	0.139	0.484	-0.369	0.175	1166
	7	-0.107	0.108	0.321	-0.319	0.105	1345
	8	0.035	0.075	0.641	-0.113	0.183	1582
	9	-0.030	0.065	0.643	-0.159	0.098	1819
	10	-0.021	0.075	0.776	-0.169	0.126	2064
	11	-0.024	0.068	0.725	-0.158	0.110	2281
	12	0.015	0.071	0.832	-0.124	0.154	2534
	13	0.013	0.067	0.843	-0.119	0.146	2786
	14	0.051	0.066	0.441	-0.079	0.181	3031
Completed	5	0.441	0.253	0.081	-0.054	0.936	949
	6	0.138	0.272	0.611	-0.395	0.672	1166
	7	0.019	0.228	0.934	-0.428	0.466	1345
	8	0.096	0.171	0.576	-0.240	0.432	1582
	9	0.009	0.158	0.956	-0.302	0.319	1819
	10	-0.039	0.157	0.805	-0.346	0.268	2064
	11	-0.023	0.146	0.875	-0.310	0.264	2281
	12	-0.025	0.140	0.859	-0.299	0.249	2534
	13	0.063	0.119	0.593	-0.169	0.296	2786
	14	0.112	0.115	0.331	-0.114	0.338	3031

Table 21: Post-Policy Density Testing for Initial Score

Dependent variable: Frequency per Score			
Variable	Unstandardized Coefficient	se	p-value
score	-4.029	2.297	0.091
treatment	8.026	9.434	0.403
square_score	0.174	0.171	0.320
sq_score_treatment	-0.064	0.186	0.735
score_treatment	6.202*	2.719	0.031
Constant	55.073***	14.999	0.001
Observations	34 clusters		

$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Table 22: Post-Policy Summary Density Tests for Covariates for Initial Score

	Age	Female	HH_inc	Minority	New_st
score	0.119	0.0171	-0.0157	0.0243	0.00286
	(0.0711)	(0.0173)	(0.0526)	(0.0192)	(0.0123)
treatment	0.324*	0.0324	-0.288	0.116	0.0520
	(0.155)	(0.0657)	(0.171)	(0.0570)	(0.0462)
square_score	-0.00667	-0.00146	0.000297	-0.00150	-0.000527
	(0.00613)	(0.00128)	(0.00403)	(0.00154)	(0.000952)
sq_score_treatment	0.00753	0.00120	-0.00298	0.00203	0.000807
	(0.00616)	(0.00131)	(0.00427)	(0.00164)	(0.00102)
score_treatment	-0.0976	-0.0217	-0.00772	-0.0181	0.00257
	(0.0742)	(0.0195)	(0.0629)	(0.0213)	(0.0144)
(mean) f2006	-0.0596	-0.184	-1.917**	-0.204	-0.148
	(0.628)	(0.153)	(0.603)	(0.187)	(0.115)
(mean) f2007	-0.150	0.396*	0.682	-0.0371	-0.0709
	(0.665)	(0.162)	(0.910)	(0.170)	(0.128)
Constant	18.91***	0.384**	5.980***	0.336***	0.895***
	(0.392)	(0.114)	(0.406)	(0.0788)	(0.0655)
Observations	34	34	34	34	34

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 23: Post-Policy Density Tests for Age

Dependent Variable: Mean Age						
Bandwidth	14	12	10	9	8	7
score	0.118	0.0742	0.0611	-0.0853	-0.144*	-0.126
	(0.0733)	(0.0625)	(0.0897)	(0.0626)	(0.0668)	(0.0936)
treatment	0.252	0.0177	0.0198	-0.0933	-0.266	-0.351
	(0.170)	(0.141)	(0.187)	(0.208)	(0.177)	(0.224)
square_score	-0.00662	-0.00257	-0.000912	0.0209*	0.0308**	0.0277
	(0.00637)	(0.00524)	(0.0107)	(0.00852)	(0.00918)	(0.0157)
sq_score_treatment	0.00582	-0.00370	-0.00470	-0.0187	-0.0415**	-0.0490
	(0.00680)	(0.00607)	(0.0138)	(0.0131)	(0.0128)	(0.0255)
score_treatment	-0.123	-0.149	-0.129	0.0658	0.0287	-0.0668
	(0.0845)	(0.0888)	(0.114)	(0.124)	(0.111)	(0.146)
f2006	0.00183	1.070	1.020	2.045**	1.824**	1.949*
	(0.788)	(0.679)	(0.737)	(0.594)	(0.585)	(0.811)
f2007	-0.0259	0.626	0.540	1.423	1.533*	1.282
	(0.733)	(0.789)	(1.206)	(0.749)	(0.702)	(0.823)
Constant	18.85***	18.34***	18.40***	17.89***	17.98***	18.00***
	(0.466)	(0.466)	(0.556)	(0.357)	(0.354)	(0.435)
Observations	28	24	20	18	16	14

Table 24: Post-Policy Density Tests for Female

Dependent variable Mean Proportion of Females							
Bandwidth	14	12	11	10	9	8	7
score	0.0178	0.0150	0.0200	0.0304	0.0410	-0.00744	-0.0271
	(0.0176)	(0.0215)	(0.0264)	(0.0340)	(0.0450)	(0.0395)	(0.0412)
treatment	0.0496	0.0294	0.0745	0.0977	0.140	0.0354	-0.0774
	(0.0734)	(0.0881)	(0.108)	(0.129)	(0.152)	(0.191)	(0.152)
square_score	-0.00152	-0.00132	-0.00208	-0.00366	-0.00537	0.00368	0.00835
	(0.00125)	(0.00187)	(0.00248)	(0.00394)	(0.00555)	(0.00647)	(0.00943)
sq_score_treatment	0.00179	0.00121	0.00412	0.00629	0.0111	-0.00193	-0.0221
	(0.00156)	(0.00275)	(0.00401)	(0.00616)	(0.00814)	(0.0121)	(0.0138)
score_treatment	-0.0155	-0.0182	-0.00154	-0.00543	0.00744	0.0210	-0.0525
	(0.0250)	(0.0354)	(0.0492)	(0.0618)	(0.0814)	(0.110)	(0.0893)
(mean) f2006	-0.247	-0.0909	-0.140	-0.221	-0.192	0.0990	0.0171
	(0.173)	(0.206)	(0.248)	(0.345)	(0.396)	(0.488)	(0.565)
(mean) f2007	0.313	0.396	0.297	0.249	0.185	0.637	0.641
	(0.183)	(0.222)	(0.282)	(0.339)	(0.495)	(0.646)	(0.564)
Constant	0.431^{**}	0.356^{*}	0.400^{*}	0.434	0.436	0.225	0.262
	(0.129)	(0.149)	(0.172)	(0.217)	(0.305)	(0.349)	(0.355)
Observations	28	24	20	18	16	14	12

Table 25: Post-Policy Density Tests for Household Income

Dependent variable Mean Household Income

Bandwidth	14	12	11	10	9	8	7
score	-0.0145	0.0396	0.0127	-0.0967	0.00607	0.0231	-0.0917
	(0.0550)	(0.0631)	(0.0823)	(0.0782)	(0.0967)	(0.112)	(0.0999)
treatment	-0.301	-0.415	-0.319	-0.501*	-0.385	0.0679	-0.00201
	(0.212)	(0.269)	(0.252)	(0.221)	(0.338)	(0.264)	(0.302)
square_score	0.000162	-0.00547	-0.00234	0.0121	-0.00329	-0.00352	0.0176
	(0.00425)	(0.00521)	(0.00920)	(0.00901)	(0.0114)	(0.0143)	(0.0149)
sq_score_treatment	-0.00334	-0.00563	-0.00199	-0.0207	-0.00803	0.0248	0.0116
	(0.00667)	(0.00877)	(0.0128)	(0.0128)	(0.0213)	(0.0171)	(0.0276)
score_treatment	-0.0152	-0.161	-0.0631	0.00740	-0.105	0.138	0.299
	(0.0962)	(0.114)	(0.134)	(0.128)	(0.198)	(0.164)	(0.180)
(mean) f2006	-2.068**	-1.958	-2.337*	-2.620**	-3.359***	-3.707***	-3.351*
	(0.735)	(0.956)	(0.906)	(0.706)	(0.827)	(0.863)	(1.174)
(mean) f2007	0.649	0.705	0.945	1.456	0.874	-0.820	0.370
	(1.001)	(1.028)	(1.222)	(1.425)	(1.278)	(1.066)	(1.224)
Constant	6.042***	5.914***	6.001***	6.047***	6.399***	7.025***	6.606***
	(0.476)	(0.546)	(0.538)	(0.564)	(0.522)	(0.629)	(0.694)
Observations	28	24	22	20	18	16	14

Table 26: Post-Policy Density Tests for Minority

Dependent variable Proportion Minority							
Bandwidth	14	12	11	10	9	8	7
	(0.0199)	(0.0310)	(0.0321)	(0.0409)	(0.0467)	(0.0478)	(0.0523)
treatment	0.189**	0.243*	0.264*	0.288*	0.282*	0.215	0.382*
	(0.0669)	(0.0939)	(0.102)	(0.105)	(0.106)	(0.130)	(0.155)
square_score	-0.00156	-0.000695	-0.00166	-0.00287	-0.00606	-0.00577	-0.0164
	(0.00160)	(0.00268)	(0.00302)	(0.00432)	(0.00553)	(0.00693)	(0.00920)
sq_score_treatment	0.00440*	0.00580	0.00785	0.0101	0.0104	0.00526	0.0250
	(0.00211)	(0.00361)	(0.00412)	(0.00507)	(0.00692)	(0.00808)	(0.0128)
score_treatment	0.0122	0.0473	0.0503	0.0504	0.00668	-0.0278	-0.0102
	(0.0257)	(0.0308)	(0.0413)	(0.0545)	(0.0594)	(0.0690)	(0.0883)
(mean) f2006	-0.279	-0.507	-0.514	-0.472	-0.612	-0.569	-0.994
	(0.217)	(0.300)	(0.344)	(0.357)	(0.444)	(0.561)	(0.666)
(mean) f2007	-0.0884	-0.216	-0.0711	-0.103	-0.256	-0.0189	-0.515
	(0.165)	(0.213)	(0.263)	(0.291)	(0.357)	(0.593)	(0.324)
Constant	0.377***	0.505***	0.454**	0.441**	0.519*	0.435	0.701*
	(0.0907)	(0.127)	(0.128)	(0.130)	(0.183)	(0.318)	(0.278)
Observations	28	24	22	20	18	16	14

Table 27: Post-Policy Density Tests for New Student

Dependent variable: Mean Proportion of Student Type								
Bandwidth	14	12	11	10	9	8	7	6
score	0.00393	0.00679	0.0191	0.0299	0.0319	0.0213	0.0161	0.0839
	(0.0129)	(0.0195)	(0.0204)	(0.0256)	(0.0312)	(0.0337)	(0.0466)	(0.0413)
treatment	0.0723	0.0909	0.0854	0.0821	0.0911	0.155	0.150	0.193
	(0.0540)	(0.0671)	(0.0658)	(0.0620)	(0.0773)	(0.0821)	(0.122)	(0.117)
square_score	-0.000619	-0.000863	-0.00239	-0.00381	-0.00412	-0.00171	0.000766	-0.0175
	(0.00102)	(0.00178)	(0.00203)	(0.00272)	(0.00364)	(0.00495)	(0.0084))	(0.00799)
sq_score_treatment	0.00128	0.00192	0.00273	0.00301	0.00379	0.00585	0.00505	0.0211
	(0.00129)	(0.00240)	(0.00266)	(0.00306)	(0.00494)	(0.00568)	(0.0124)	(0.0115)
score_treatment	0.00773	0.0104	-0.0103	-0.0314	-0.0291	0.0188	0.0245	-0.0393
	(0.0174)	(0.0243)	(0.0321)	(0.0369)	(0.0439)	(0.0447)	(0.0601)	(0.0602)
(mean) f2006	-0.253	-0.350	-0.265	-0.278	-0.297	-0.434	-0.414	-0.641
	(0.125)	(0.179)	(0.183)	(0.195)	(0.256)	(0.320)	(0.441)	(0.314)
(mean) f2007	-0.186	-0.252	-0.192	-0.268	-0.273	-0.645	-0.593	-0.416
	(0.134)	(0.175)	(0.196)	(0.195)	(0.194)	(0.360)	(0.346)	(0.475)
Constant	0.967***	1.017***	0.955***	0.973***	0.979***	1.148***	1.129***	1.123***
	(0.0687)	(0.0977)	(0.0863)	(0.0843)	(0.102)	(0.187)	(0.214)	(0.253)
Observations	28	24	22	20	18	16	14	12

Table 28: Post-Policy Attrition Rates

Distance from Cutoff for Control Group	Percent Attrition in Control Group
5	0
6	1.2
7	1.0
8	0.8
9	2.1
10	2.5
11	2.7
12	3.0
13	4.0
14	5.2

Table 29: Attrition Regression Results

Outcome Variable: Placed in Credit Math								
Bandwidth for RD	65-119	15	14	13	12	11	10	9
Algebra Score Range	Full Range	65-79	65-78	65-77	65-76	65-75	65-74	65-73
assign	***0.0583****	***0.0308***	***0.0673****	***0.0647***	***0.0535***	***0.0525***	***0.0615***	***0.0885******
	0.0223	0.0463	0.0265	0.0303	0.0369	0.0378	0.0295	0.00436
score_at	0.00155	0.00165	0.00137	0.00033	0.000812	0.00131	0.00163	0.000454
	0.00207	0.00226	0.00236	0.00226	0.00178	0.00171	0.00249	0.00108
square_score_at	0.000156	0.000165	0.000137	0.000033	0.0000814	0.000131	0.000164	0.0000455
	0.000286	0.000309	0.000323	0.000341	0.000252	0.00024	0.000383	0.000157
sq_score_at_assign	0.000214	0.000243	0.00596**	0.00488	0.00158	0.00187	0.00944	0.0497***
	0.000286	0.00362	0.00189	0.00288	0.00424	0.00639	0.00772	0.00261
score_at_assign	0.000141	0.0113	0.0288	0.0211	0.00558	0.00723	0.0339	0.135***
	0.00317	0.0275	0.0153	0.0201	0.0273	0.0353	0.0353	0.00726
f2006	0.0102	0.0116	0.00415	0.0102	0.000786	0.0119	0.0236	0.00117
	0.0237	0.0276	0.0295	0.0271	0.0264	0.0242	0.023	0.0148
f2007	-0.0386	-0.0402	-0.0386	-0.0197	-0.0259	-0.0295*	-0.0286	-0.0151
	-0.0211	-0.0222	-0.0232	-0.013	-0.0126	-0.0131	-0.014	-0.011
Constant	0.019	0.0202	0.0165	0.00327	0.00955	0.0162	0.0208	0.00529
	-0.0161	-0.0186	-0.0197	-0.0151	-0.0143	-0.0134	-0.0137	-0.00781
Observations	1257	583	559	528	494	471	435	389

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 30: Summary of Post-Policy Period LATE for Bandwidths

Policy Period: Post							
Outcome	Bandwidth	B	SE	P	Lower Limit 95% CI	Upper Limit 95% CI	N
Eligible	5	0.235	0.273	0.390	-0.300	0.769	532
	6	0.220	0.272	0.419	-0.313	0.752	606
	7	0.151	0.293	0.607	-0.423	0.724	691
	8	0.141	0.192	0.464	-0.236	0.518	773
	9	0.045	0.153	0.766	-0.254	0.345	854
	10	-0.137	0.182	0.450	-0.494	0.219	958
	11	-0.100	0.163	0.540	-0.420	0.220	1053
	12	-0.168	0.178	0.345	-0.516	0.180	1113
	13	-0.121	0.136	0.371	-0.387	0.145	1199
	14	-0.041	0.135	0.761	-0.305	0.223	1298
Enrolled	5	0.000	0.131	0.999	-0.258	0.258	532
	6	0.245	0.216	0.257	-0.178	0.667	606
	7	0.316	0.293	0.280	-0.257	0.890	691
	8	0.196	0.199	0.325	-0.194	0.585	773
	9	0.094	0.169	0.577	-0.236	0.425	854
	10	0.031	0.178	0.861	-0.318	0.380	958
	11	0.006	0.176	0.974	-0.338	0.350	1053
	12	-0.048	0.189	0.799	-0.419	0.322	1113
	13	-0.038	0.156	0.807	-0.344	0.268	1199
	14	-0.026	0.159	0.871	-0.338	0.286	1298
Completed	5	0.495	0.175	0.005	0.151	0.839	532
	6	0.402	0.206	0.051	-0.002	0.805	606
	7	0.400	0.209	0.055	-0.009	0.809	691
	8	0.359	0.174	0.040	0.017	0.701	773
	9	0.205	0.163	0.207	-0.114	0.524	854
	10	0.088	0.182	0.630	-0.269	0.444	958
	11	0.023	0.186	0.900	-0.341	0.388	1053
	12	-0.043	0.202	0.830	-0.440	0.353	1113
	13	-0.009	0.156	0.955	-0.314	0.297	1199
	14	-0.026	0.163	0.875	-0.346	0.295	1298

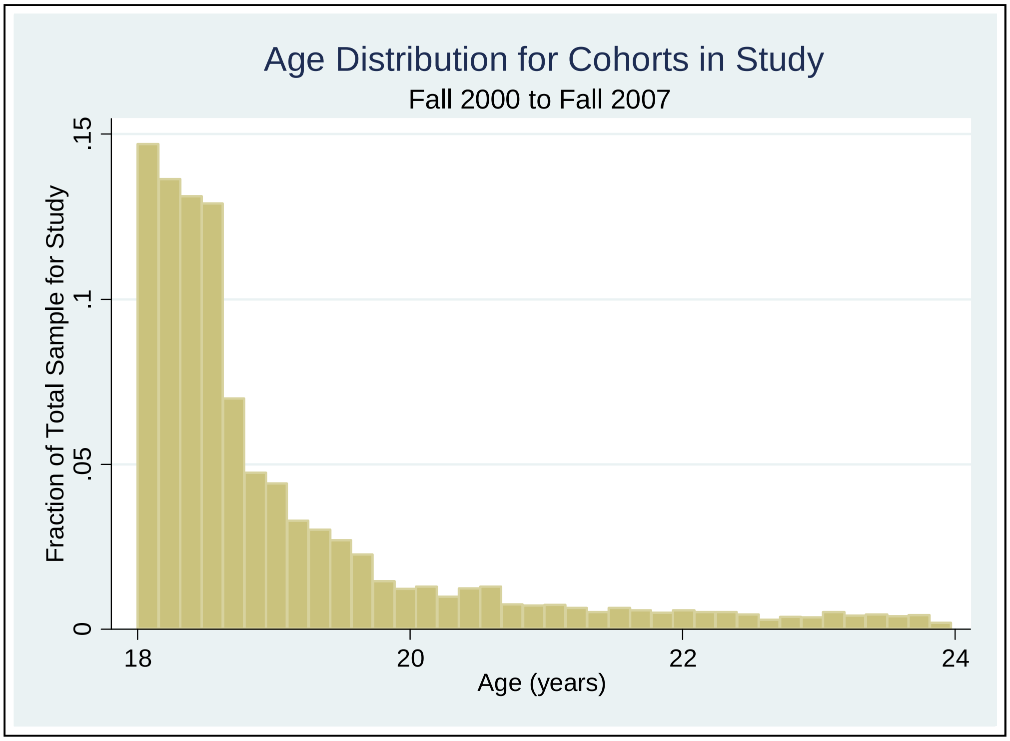

Figure 1: Age Distribution in Study Period

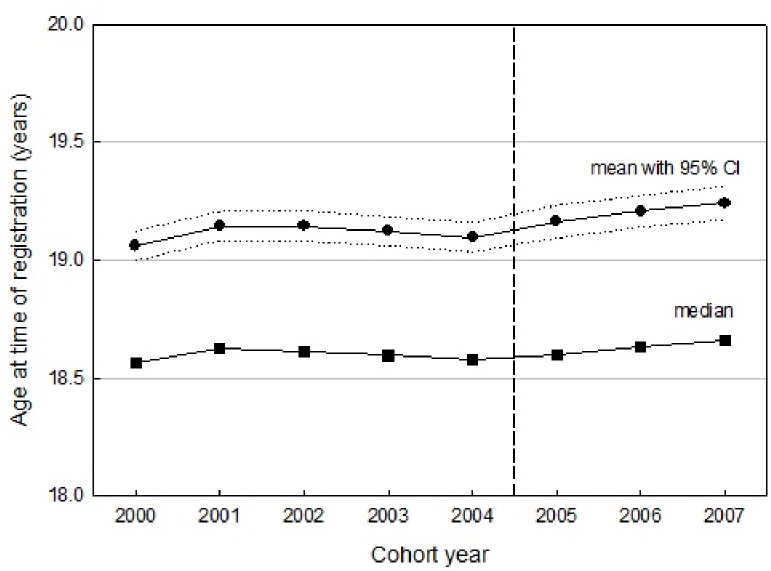

Figure 2: Mean and Median Age Distribution per Cohort Entry in Study Period

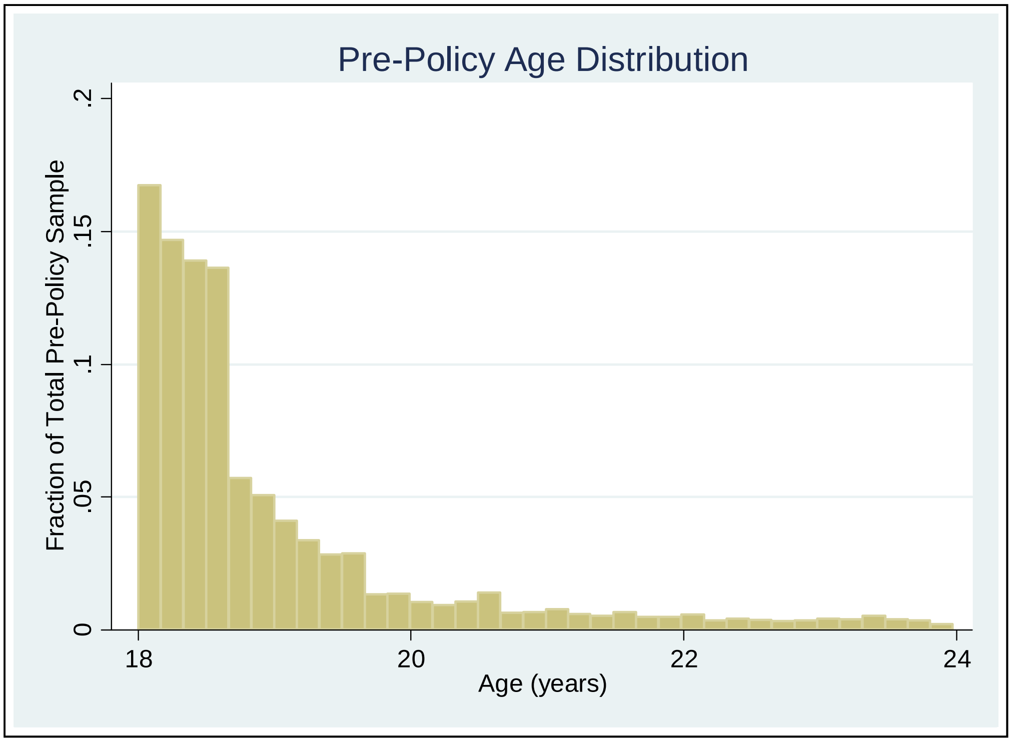

Figure 3: Age Distribution in Pre-Policy Period

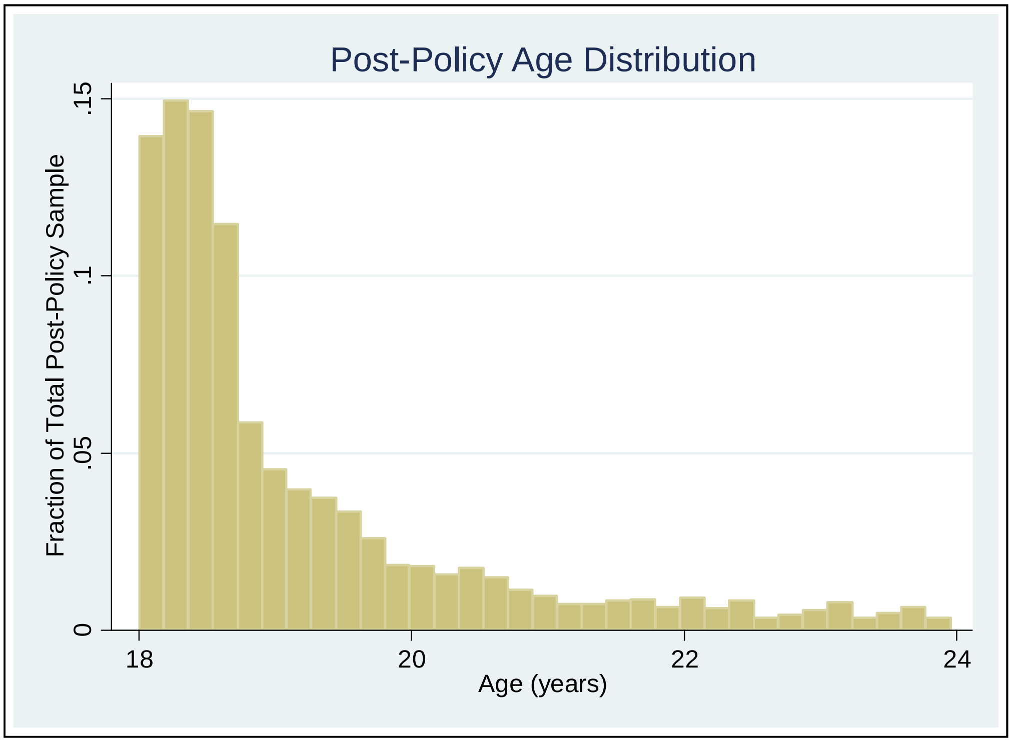

Figure 4: Age Distribution in Post-Policy Period

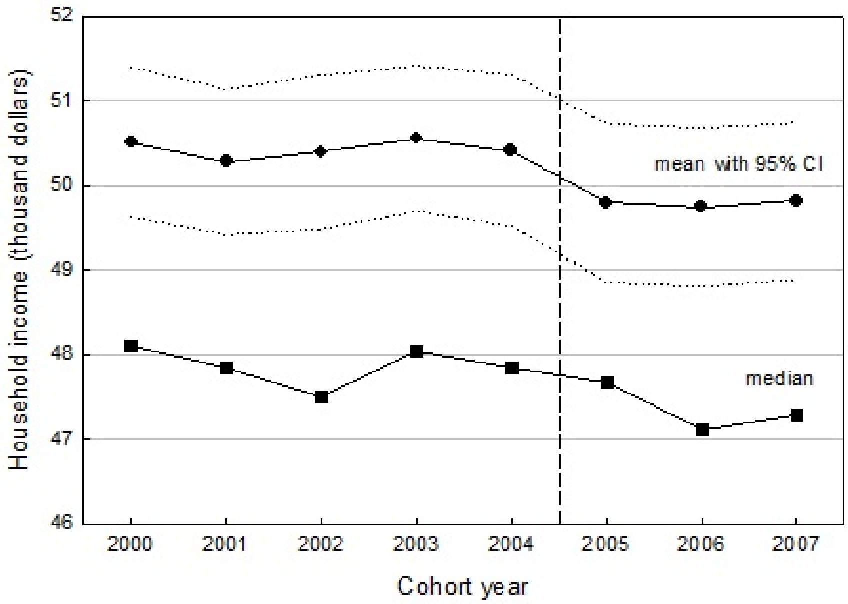

Figure 5: Mean Household Income per Cohort

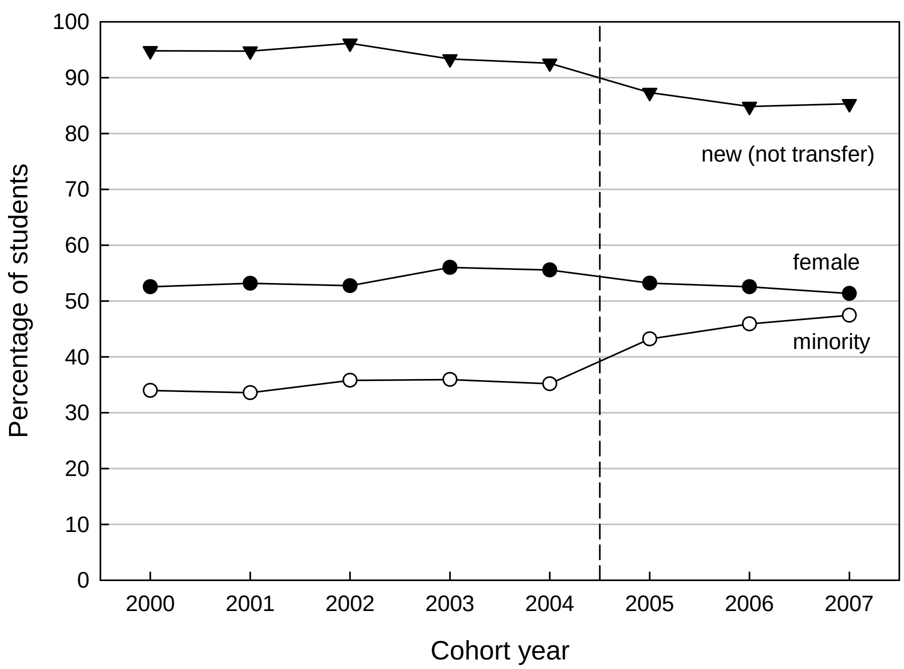

Figure 6: Proportion of New, Female, and Minority Students per Cohort Entry

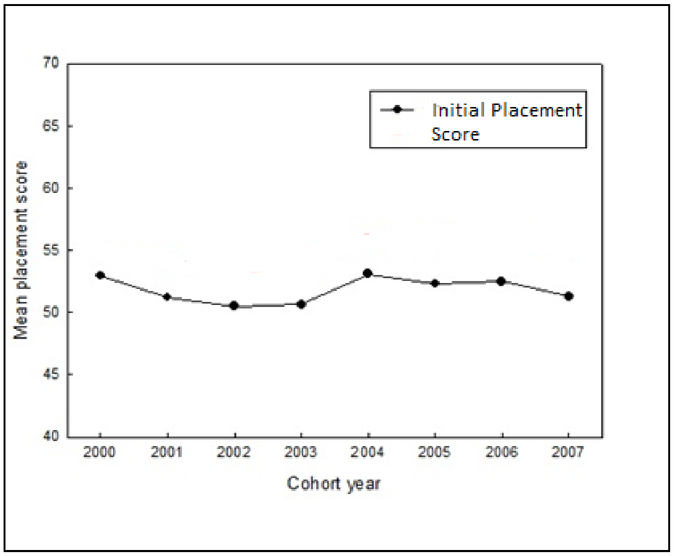

Figure 7: Mean Initial Algebra Placement Score

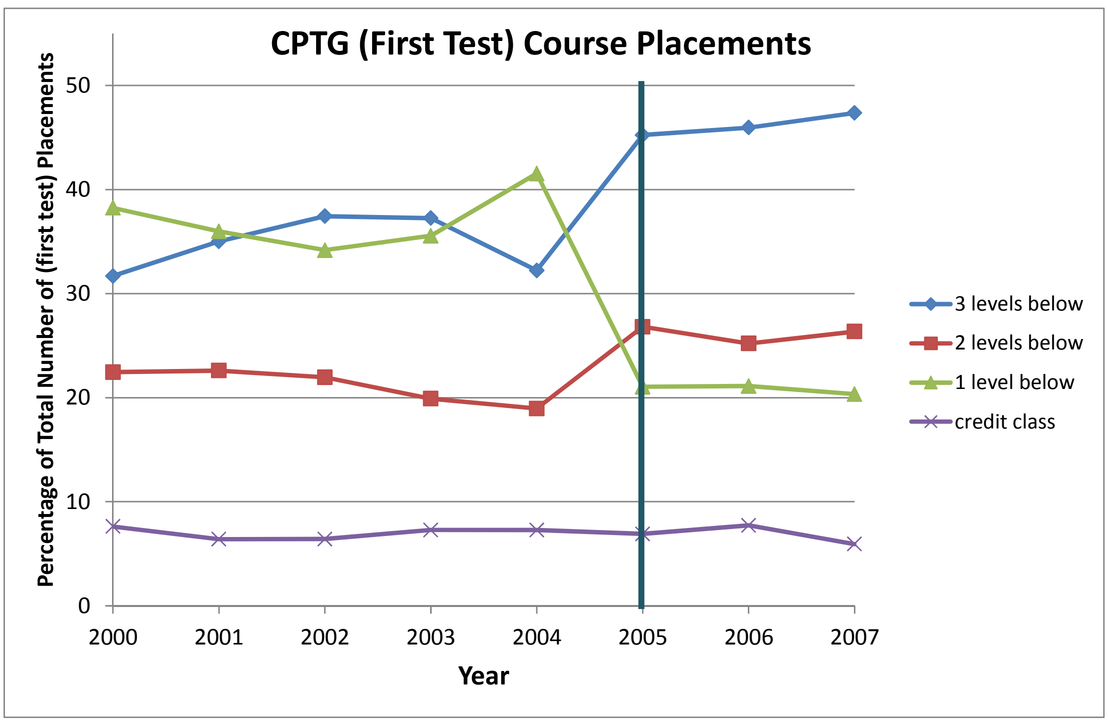

Figure 8: Developmental Math Placement per Cohort Entry

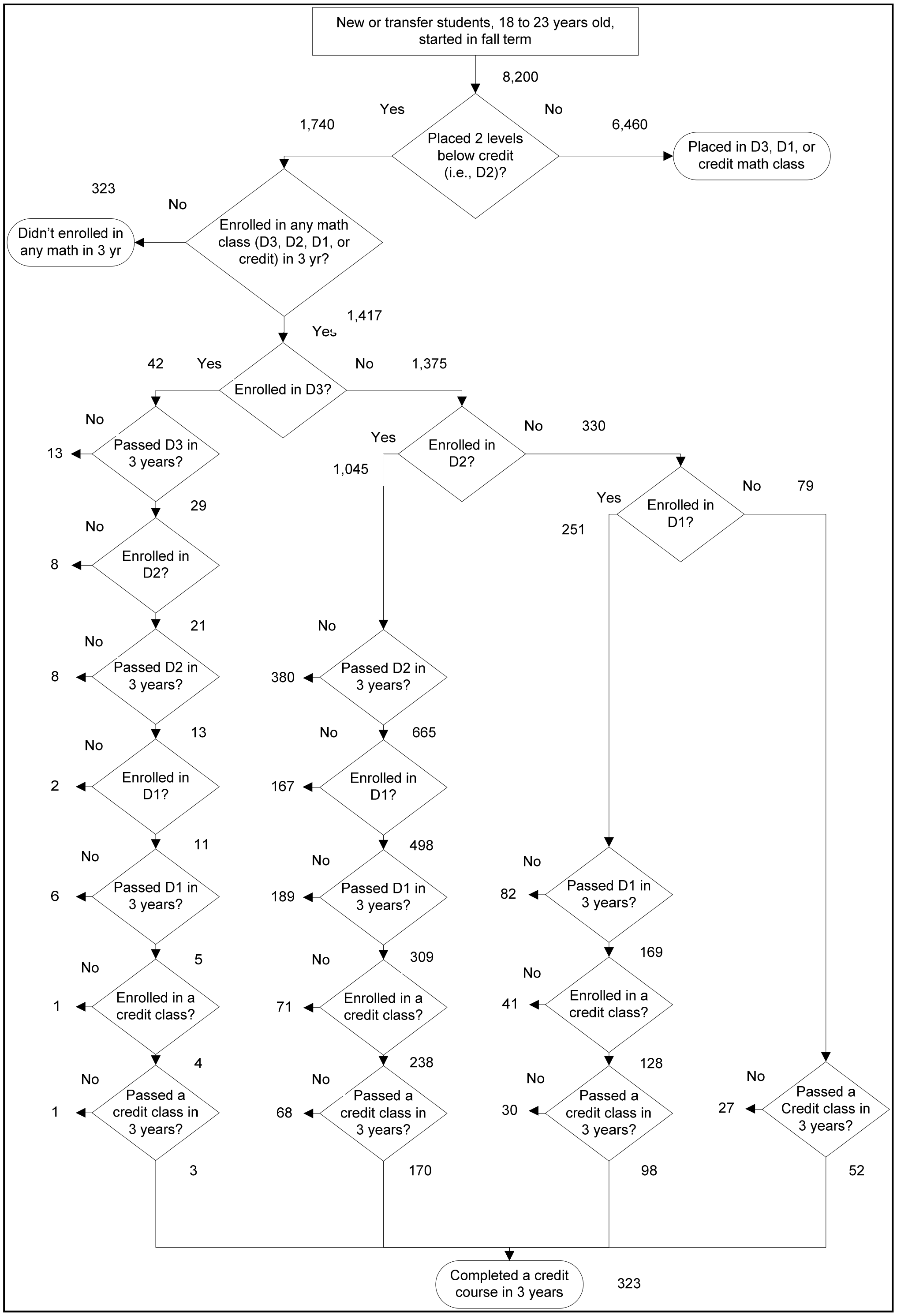

Figure 9: Flow of Students in Pre-Policy Period for Students Placing Two Levels Below Credit

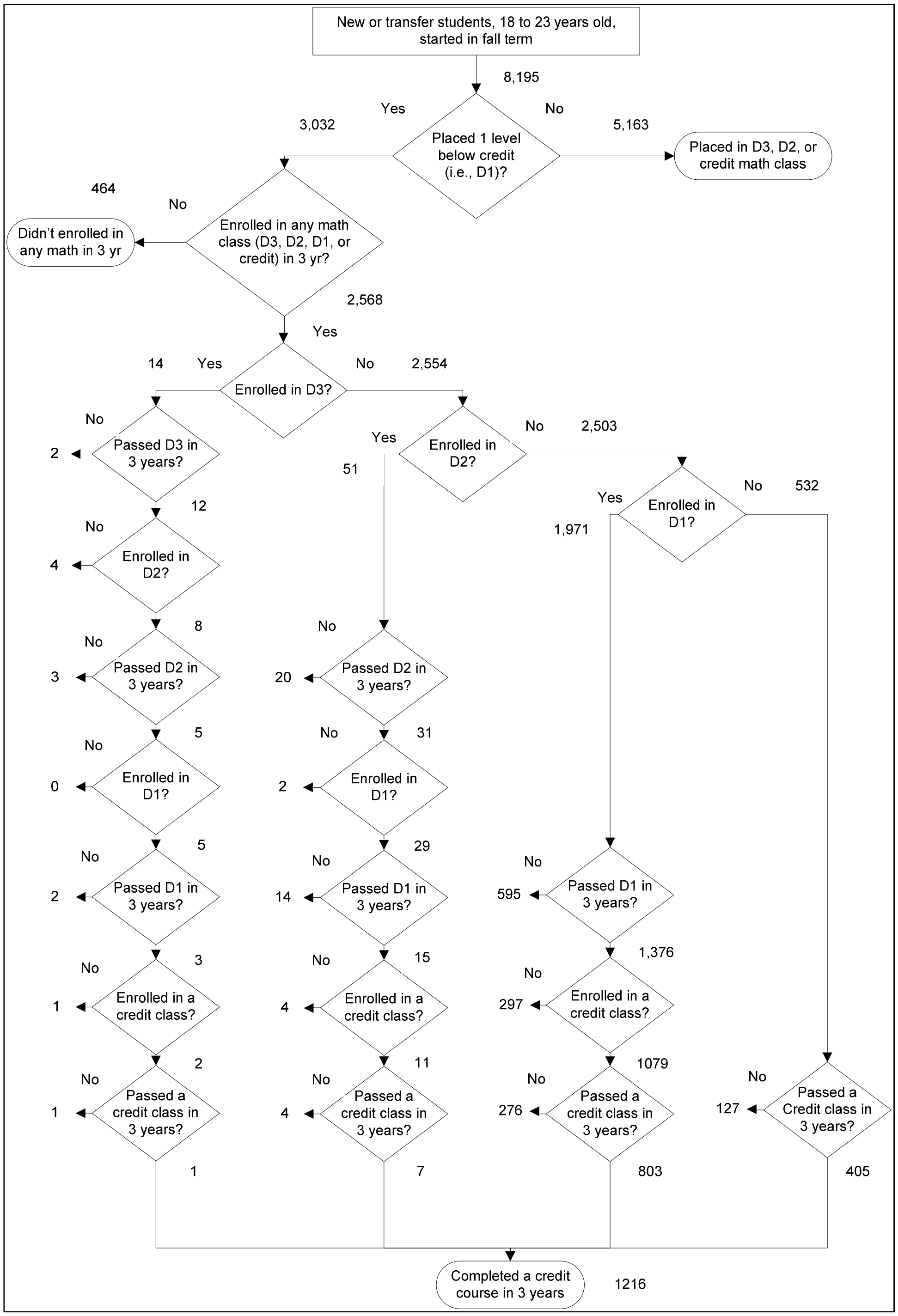

Figure 10: Flow of Students in Pre-Policy Period for Students Placing One Level Below Credit

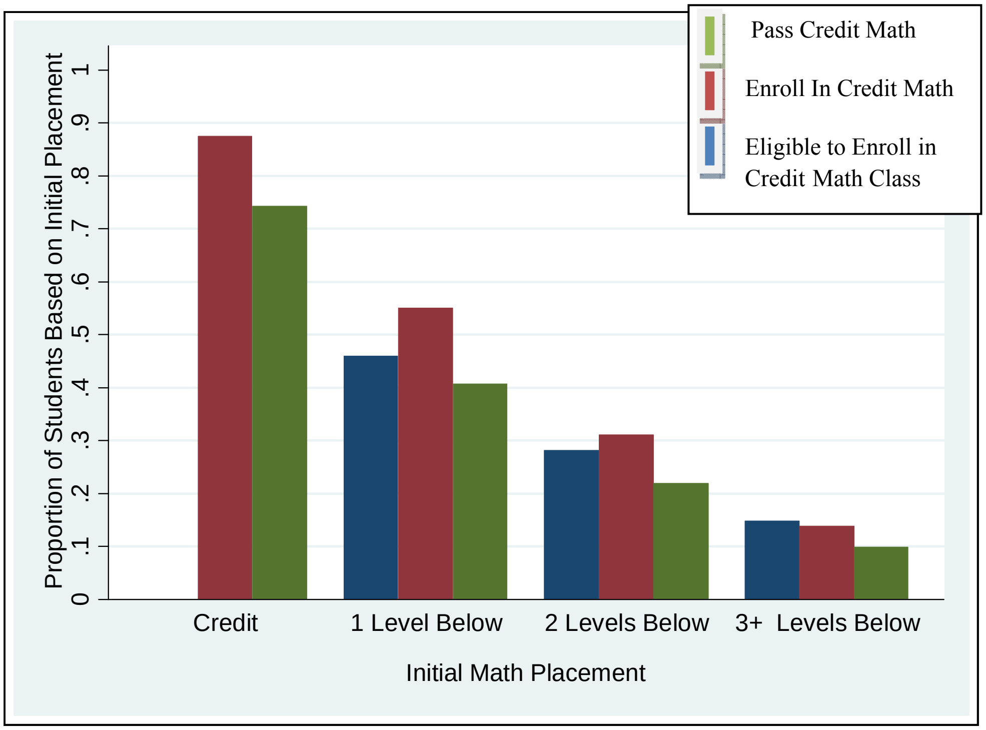

Figure 11: Pre-Policy Short-Term Outcome Success Rate

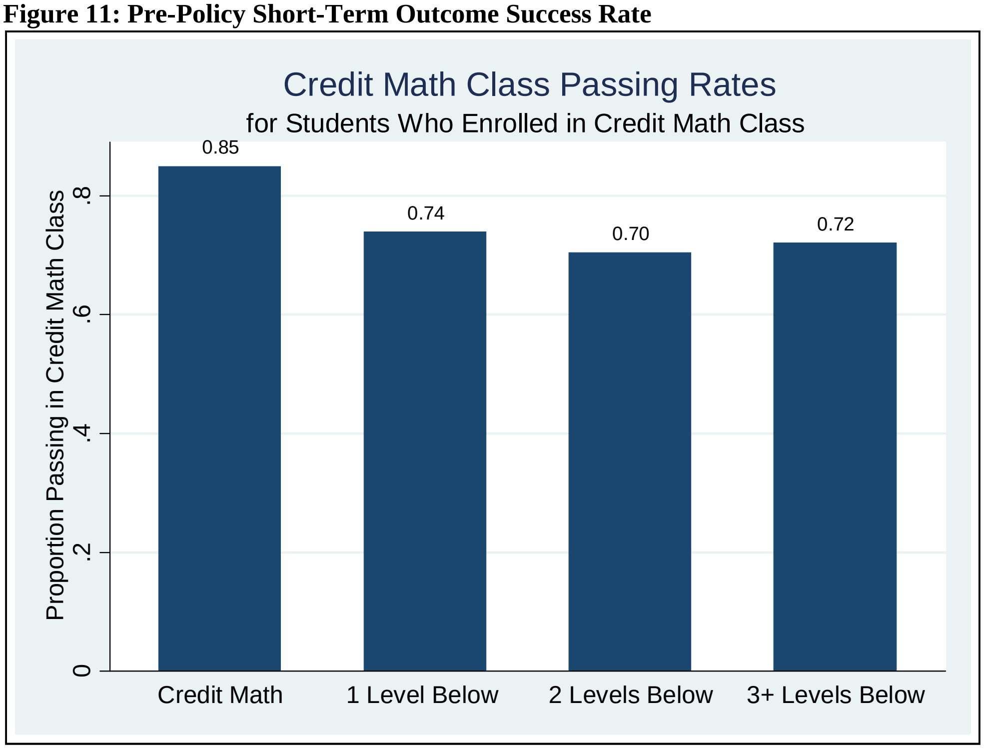

Figure 12: Pre-Policy Success Rates in Credit Math Conditional on Enrolling in Credit Class based on Initial Placement

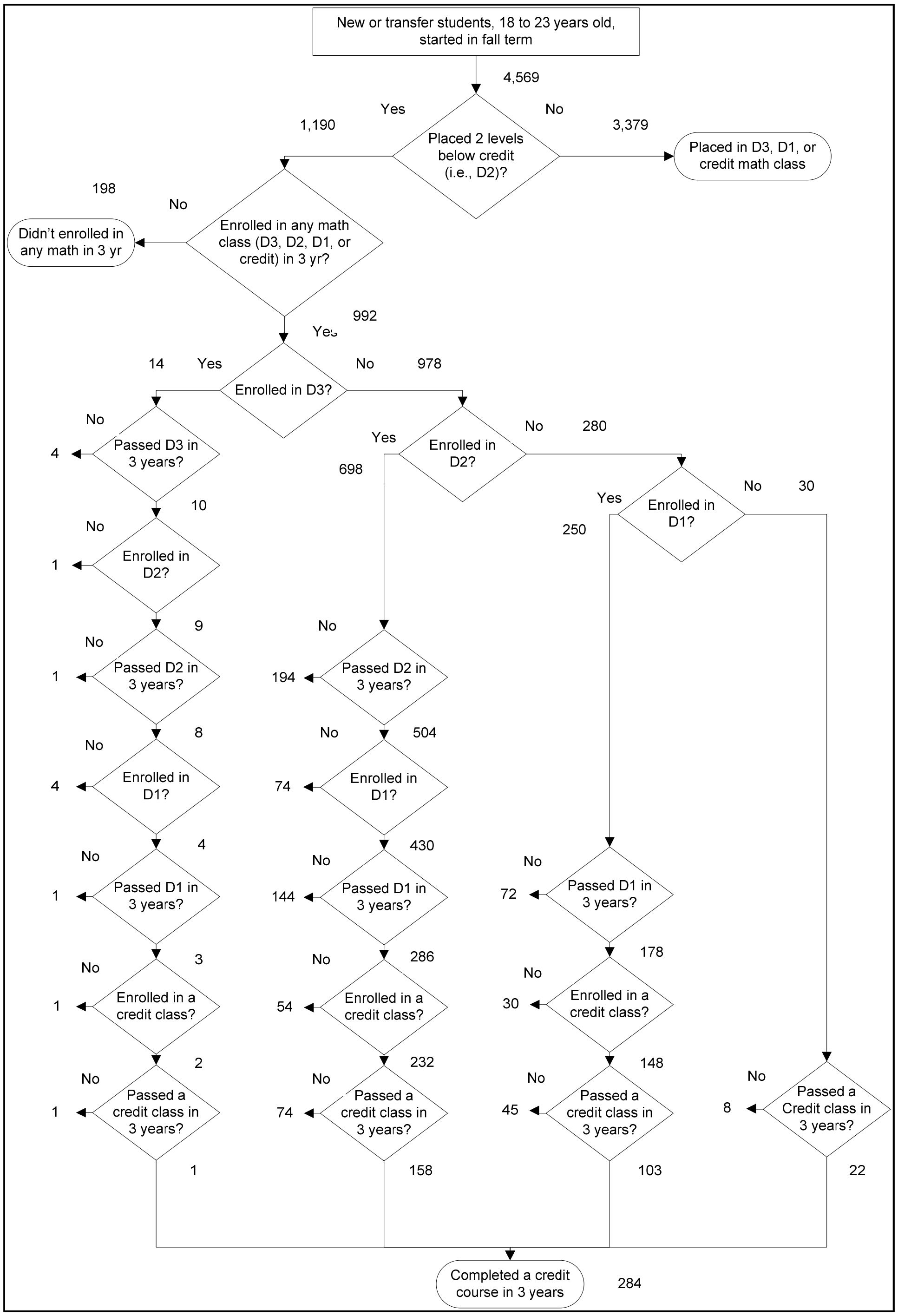

Figure 13: Flow of Students in Post-Policy Period for Students Placing Two Levels Below Credit

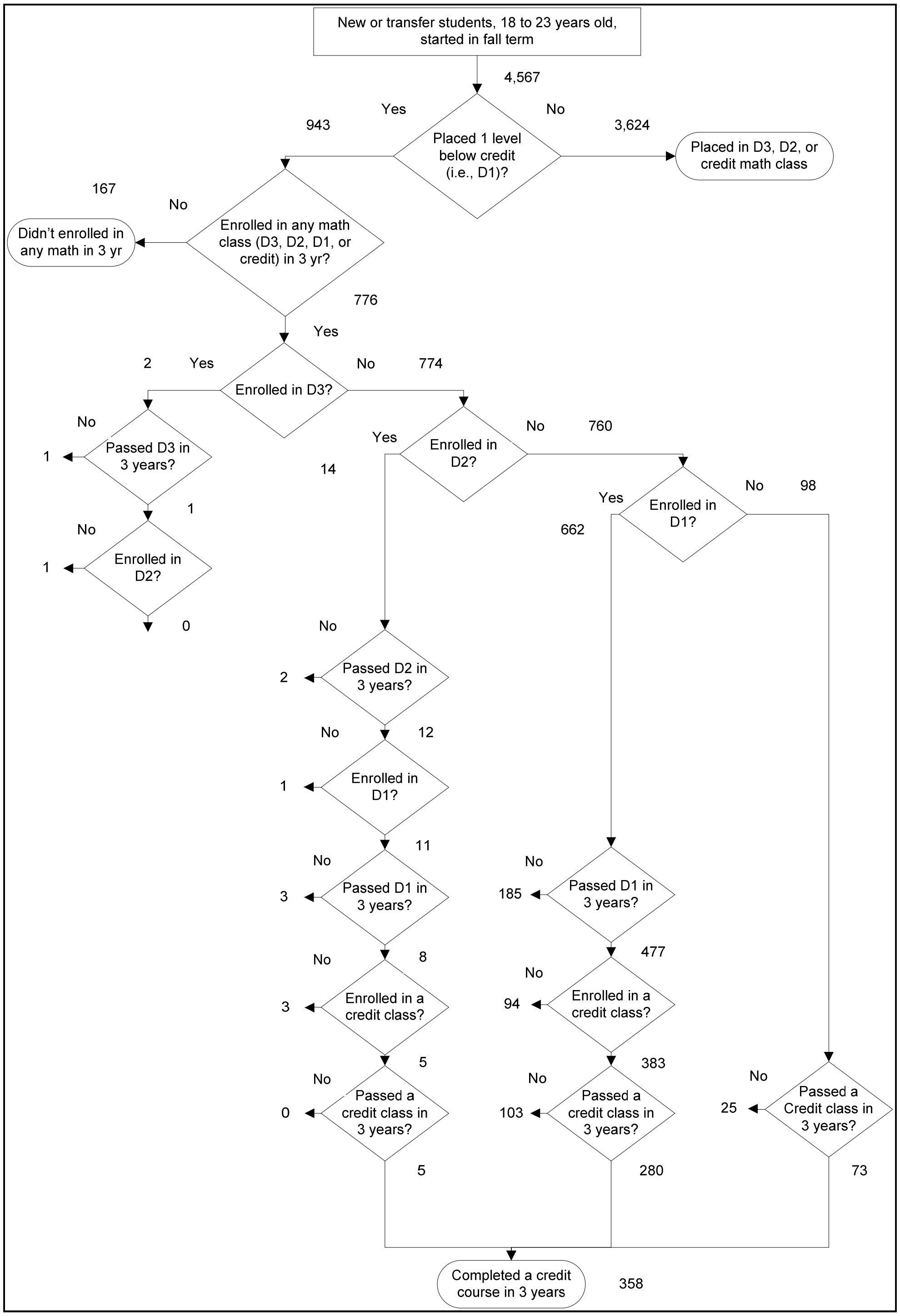

Figure 14: Flow of Students in Post-Policy Period for Students Placing One Level Below Credit

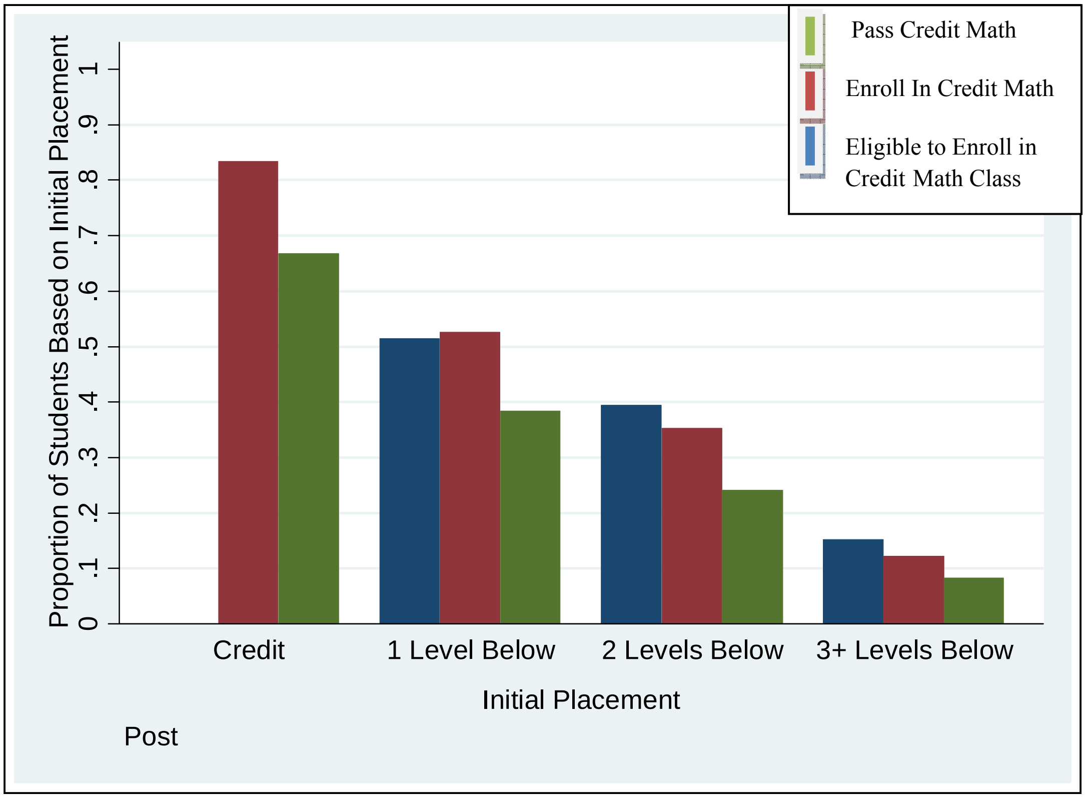

Figure 15: Post-Policy Short-Term Outcome Success Rate

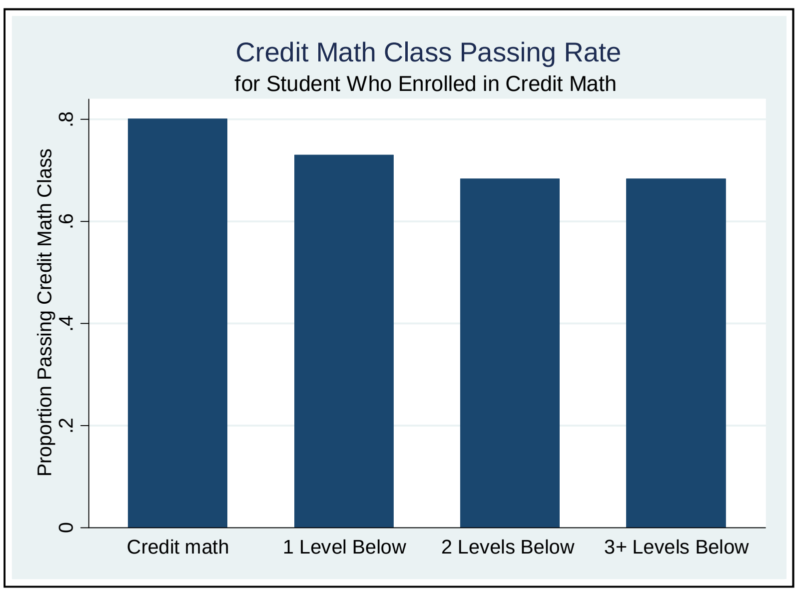

Figure 16: Post-Policy Success Rates in Credit Math Conditional on Enrolling in Credit Class Based on Initial Placement

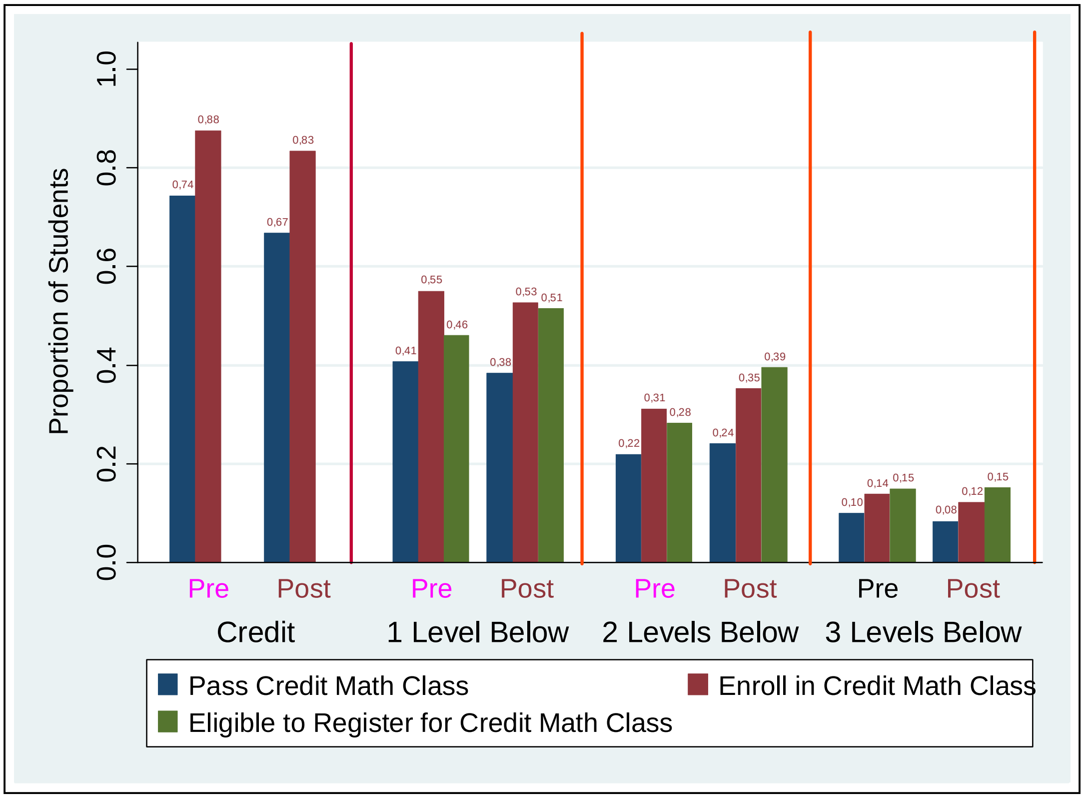

Figure 17: Pre- and Post-Policy Short Term Outcomes

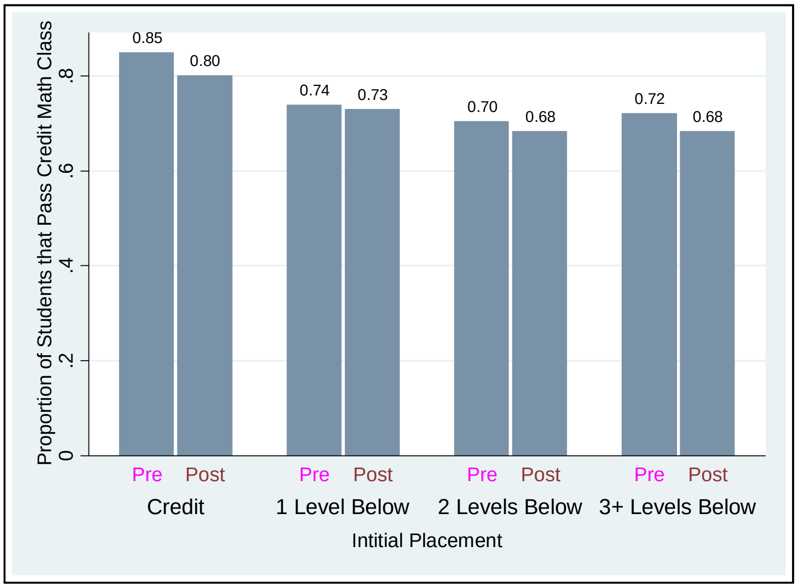

Figure 18: Pre and Post-Policy Passing Credit Math Class Based on Initial Placement Conditional on Enrolling in Credit Math Class

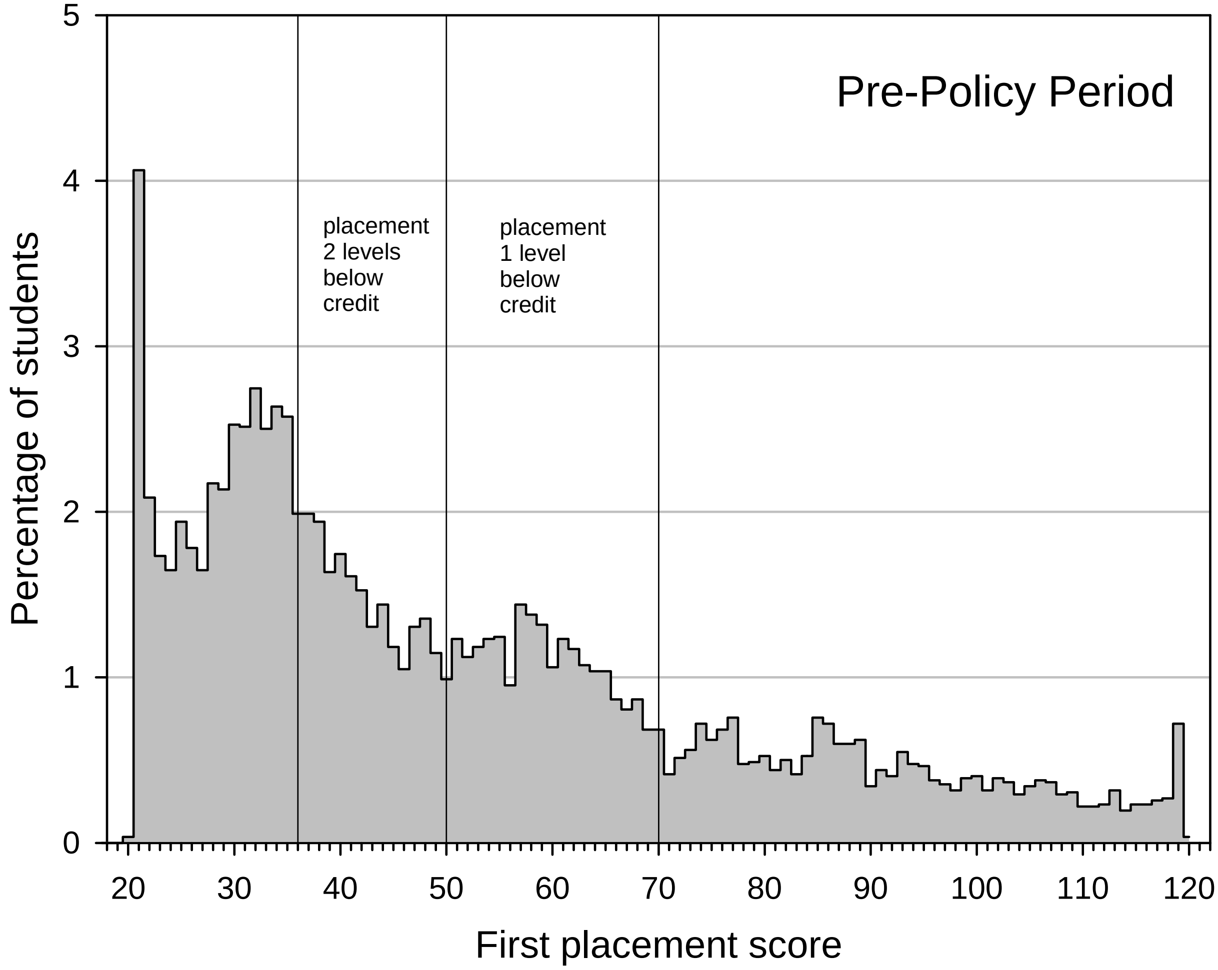

Figure 19: Pre-Policy Distribution of Initial Placement Score

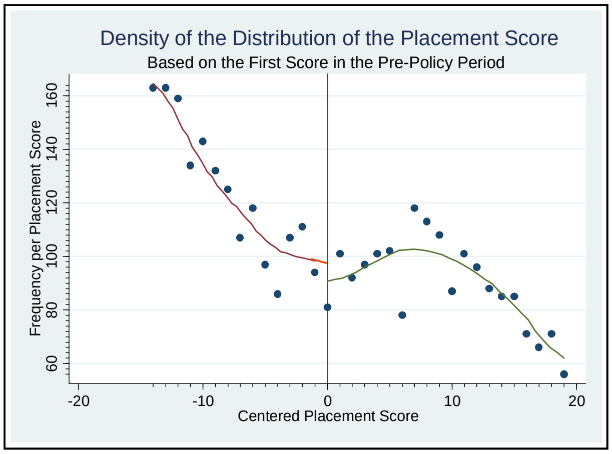

Figure 20: Pre-Policy Density Test of Initial Placement Score

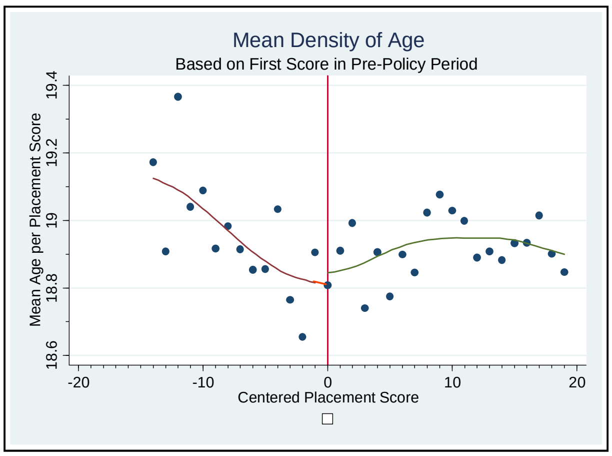

Figure 21: Pre-Policy Mean Covariate Age as a Function of the Initial Placement Score

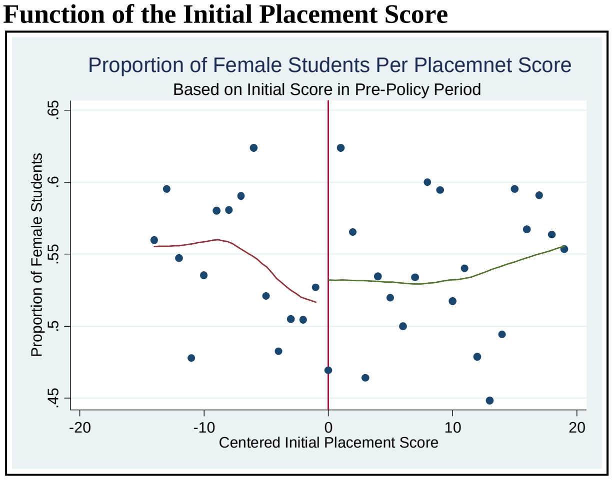

Figure 22: Pre-Policy Mean Covariate Female as a Function of the Initial Placement Score

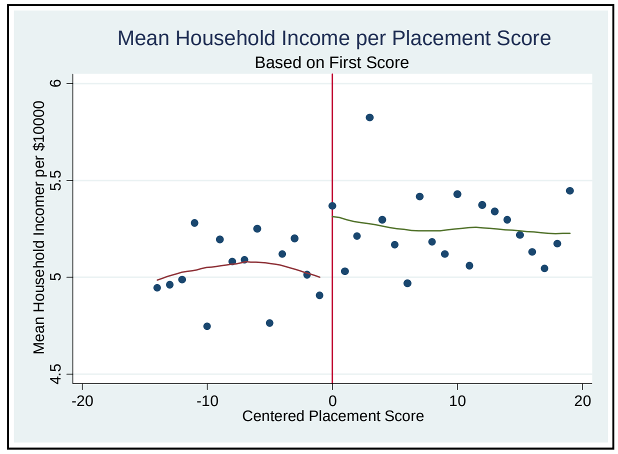

Figure 23: Pre-Policy Mean Covariate Household Income as a Function of the Initial Placement Score

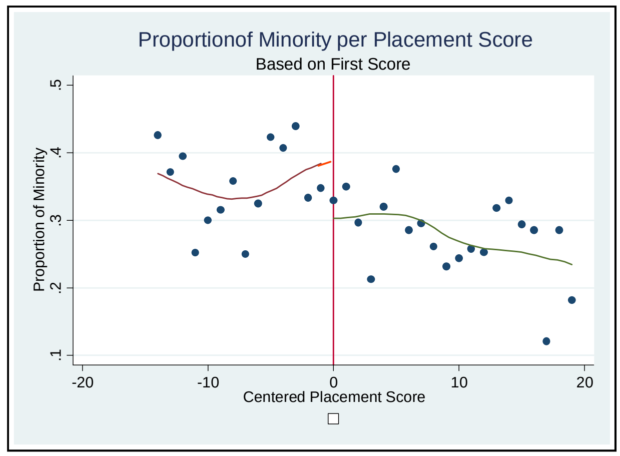

Figure 24: Pre-Policy Mean Covariate Minority as a Function of the Initial Placement Score

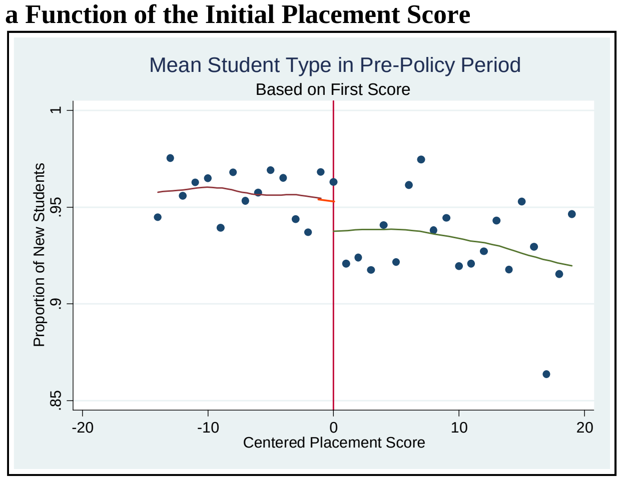

Figure 25: Pre-Policy Mean Student Type as a Function of the Initial Placement Score

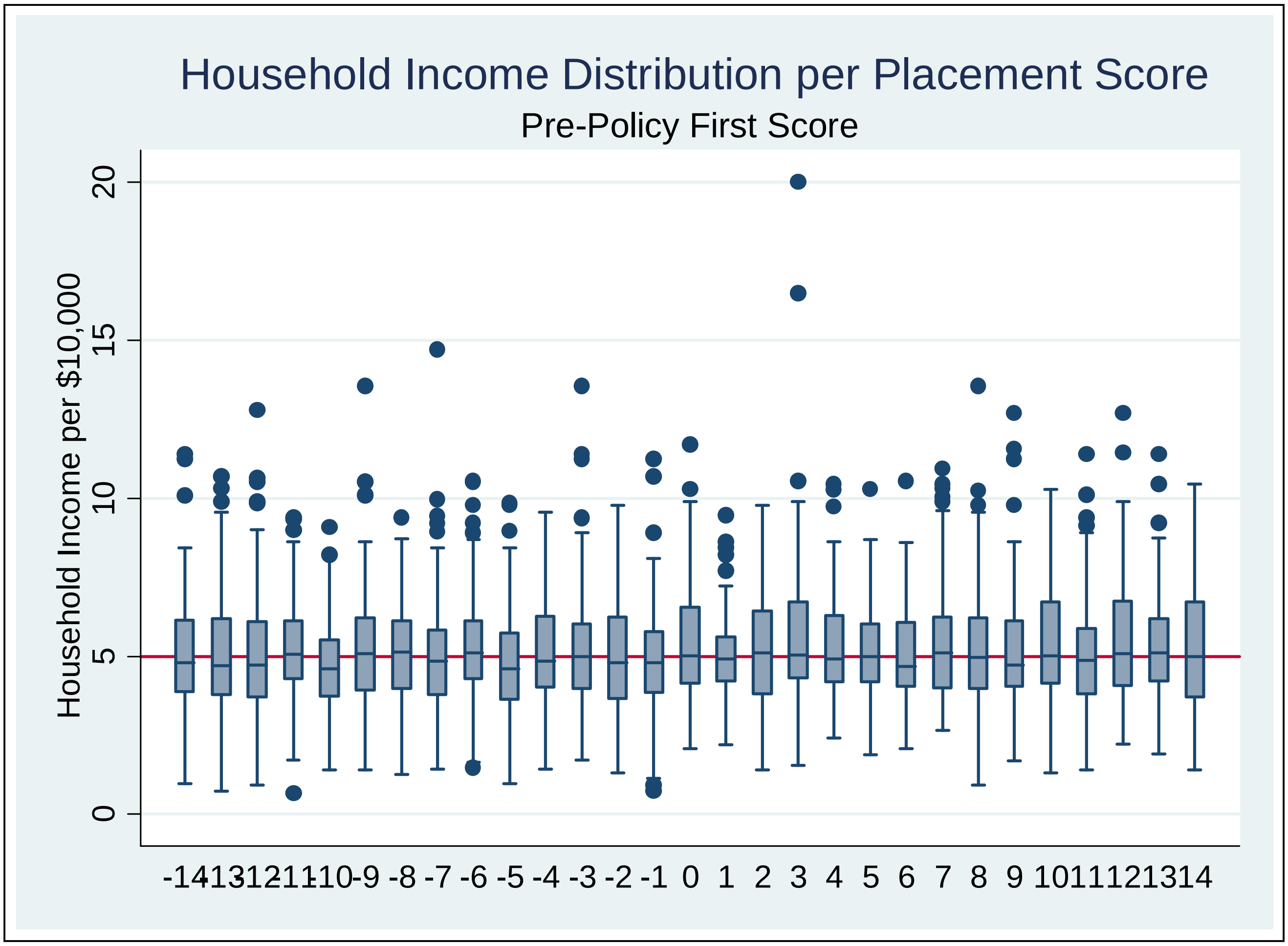

Figure 26: Pre-Policy Boxplots for Household Income per Initial Placement Score

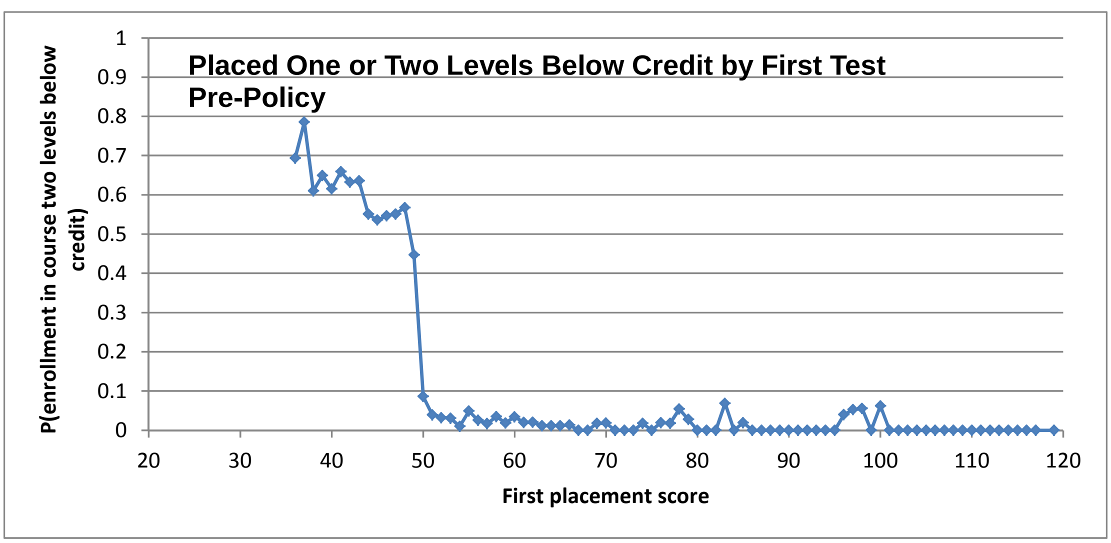

Figure 27: Pre-Policy Percentage of Enrollment in Treatment Group

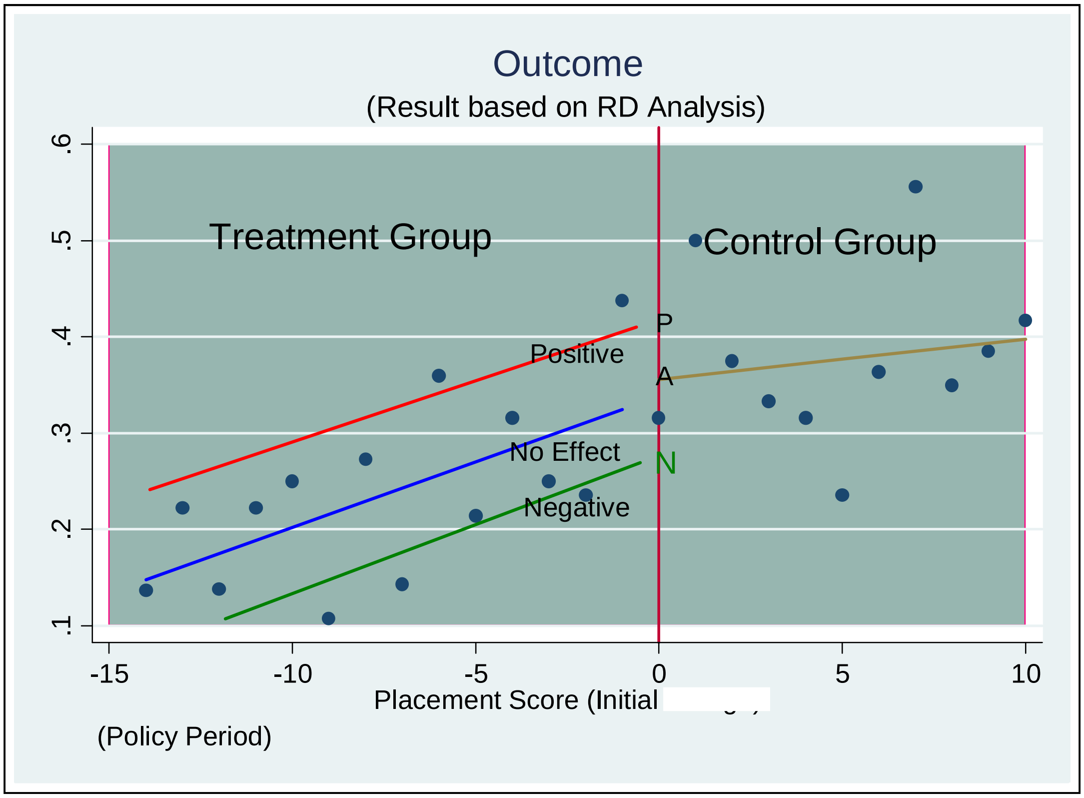

Figure 28: RD Generic Visual

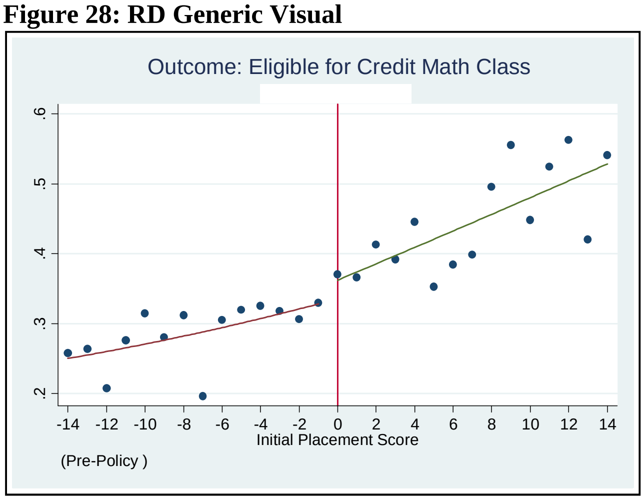

Figure 29: Pre-Policy RD Eligible for Credit Math Class

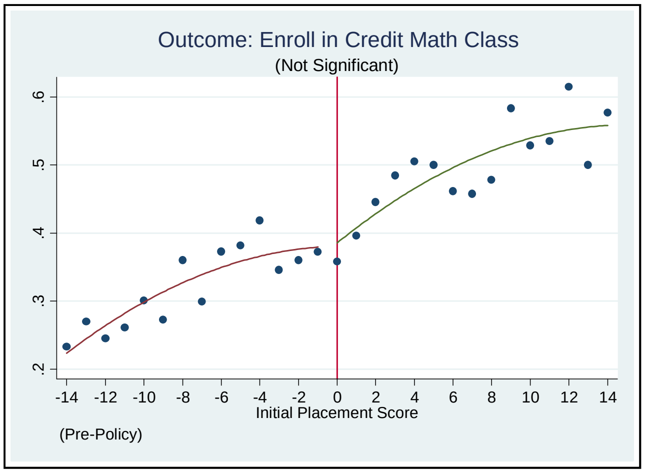

Figure 30: Pre-Policy RD Enroll in Credit Math Class

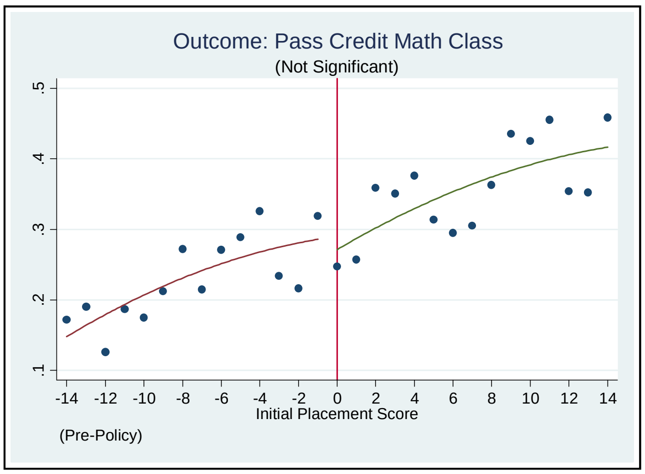

Figure 31: Pre-Policy RD Pass Credit Math Class

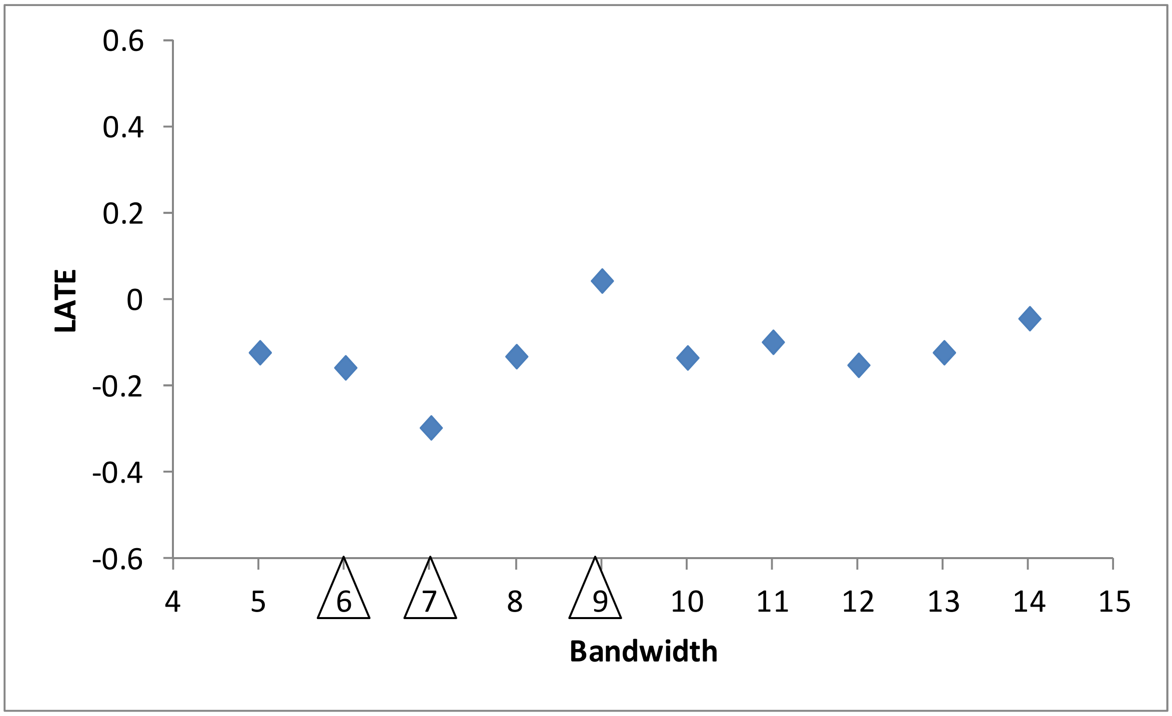

Figure 32: Summary LATE Pre-Policy Outcome: Eligible to Register for Credit Level Math Class Based on Initial Score

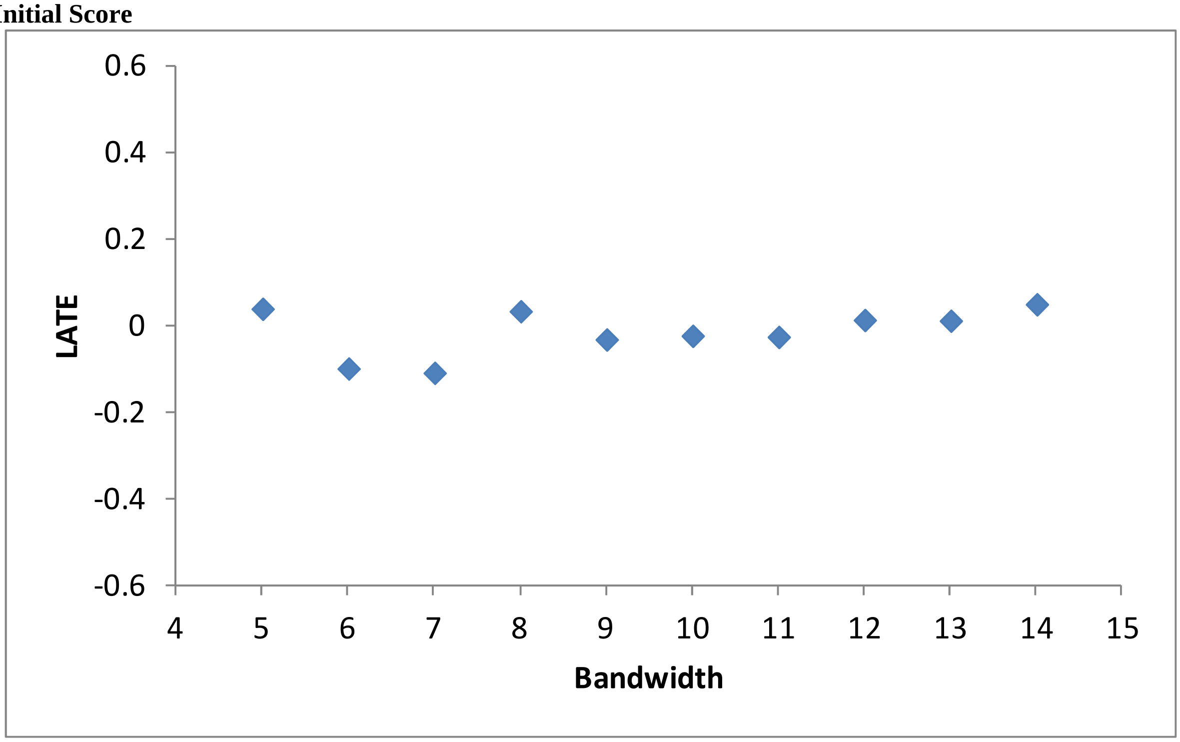

Figure 33: Summary of LATE for Pre-Policy Period Outcome: Enroll in Credit Math Class Based on Initial Score

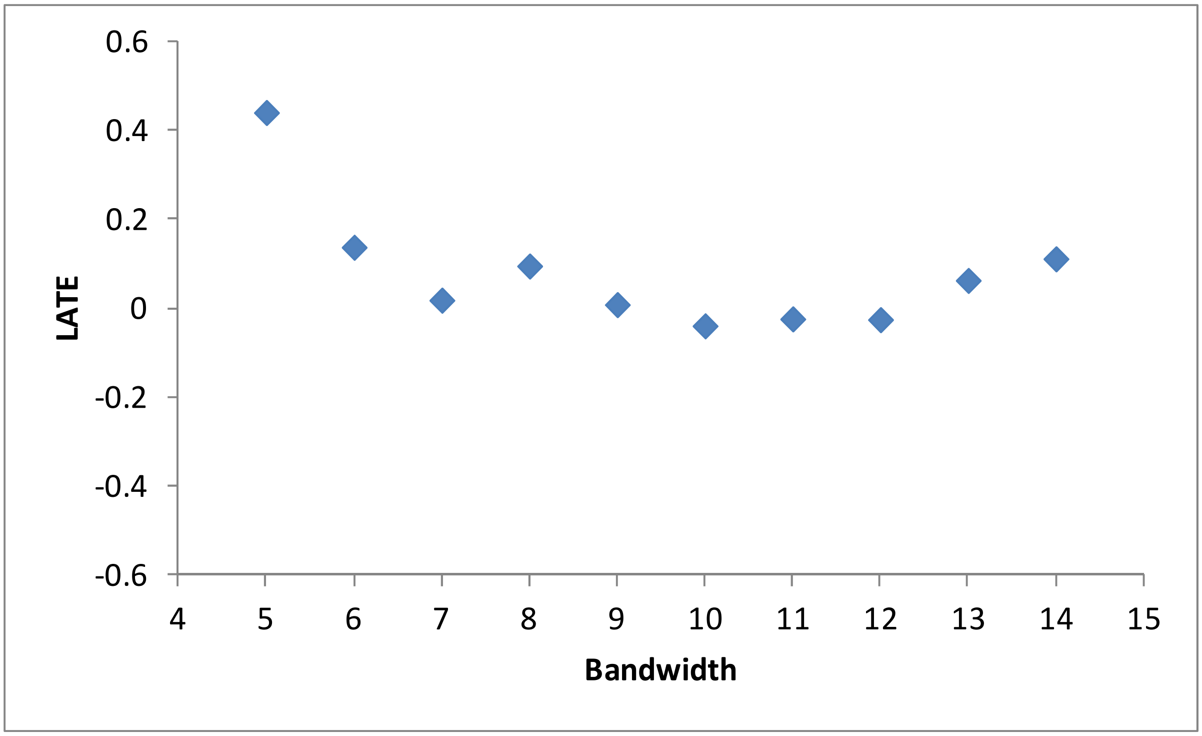

Figure 34: Summary of LATE for Pre-Policy Period Outcome: Successful Completion of a Credit Math Class Based on Initial Score

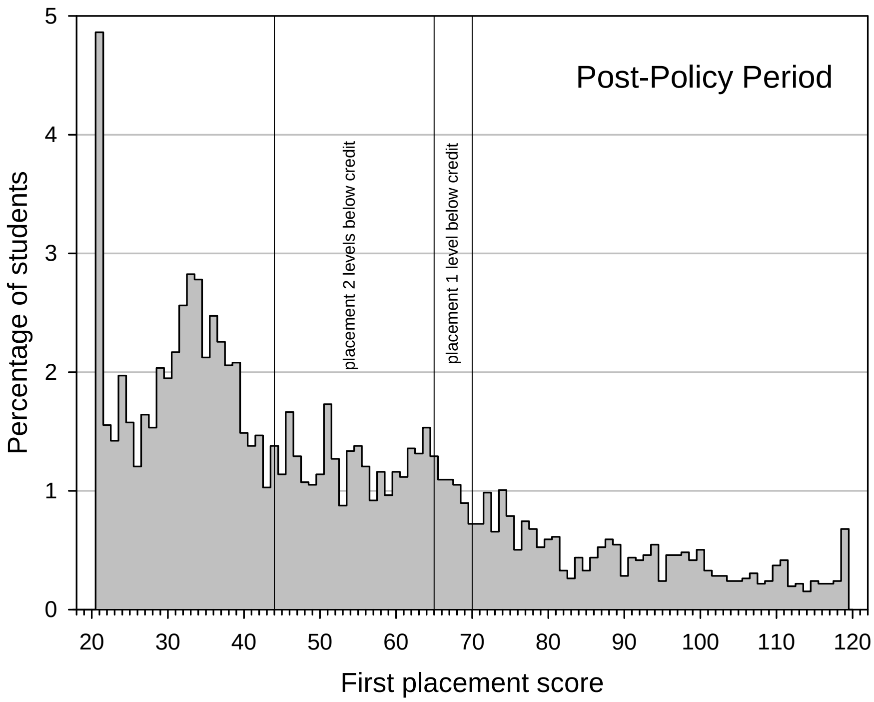

Figure 35: Post-Policy Distribution of Initial Placement Score

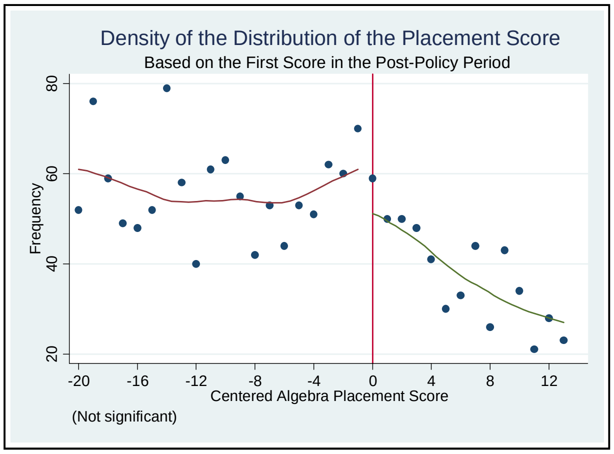

Figure 36: Post-Policy Density Test for Initial Placement Score

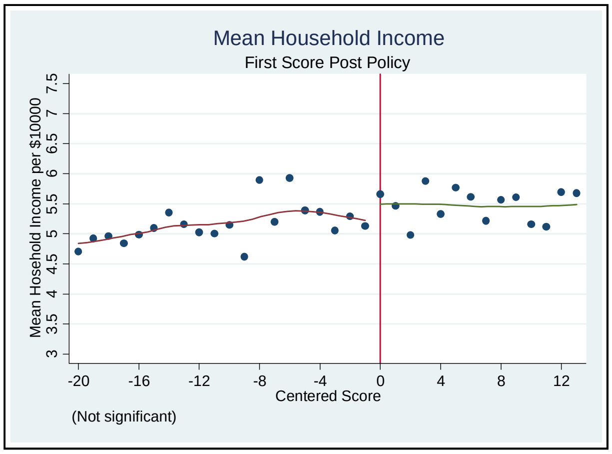

Figure 39: Post-Policy Mean Covariate Household Income as a Function of the Initial Placement Score

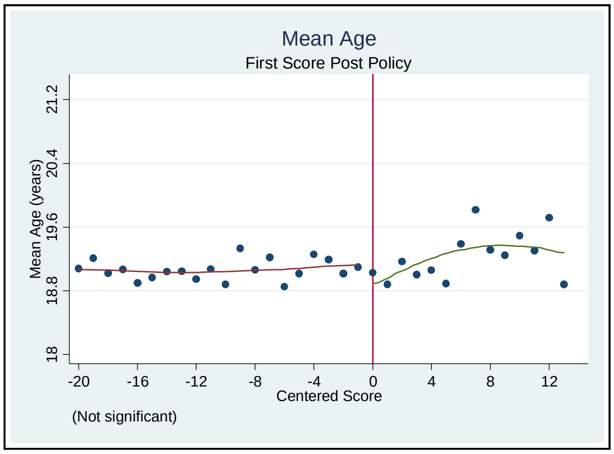

Figure 37: Post-Policy Mean Covariate Age as a Function of the Initial Placement Score

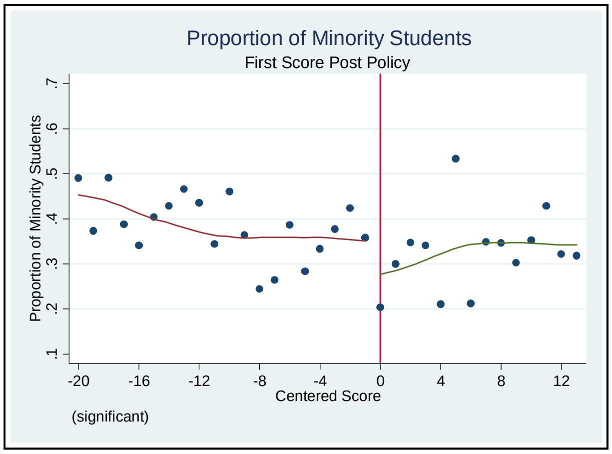

Figure 40: Post-Policy Proportion of Minority Students as Function of Initial Placement Score

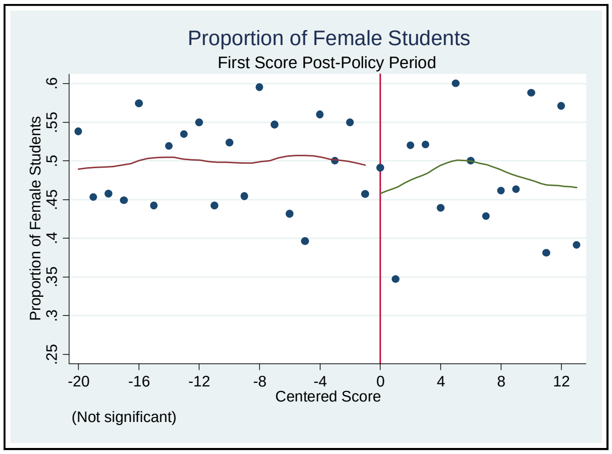

Figure 38: Post-Policy Proportion of Females as Function of Initial Placement Score

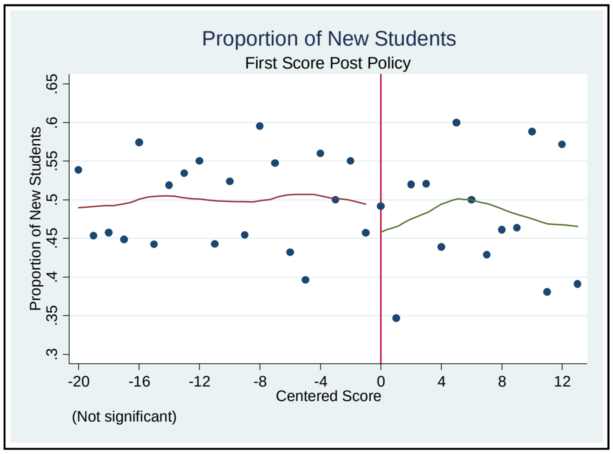

Figure 41: Post-Policy Proportion of New Students as Function of Initial Placement Score

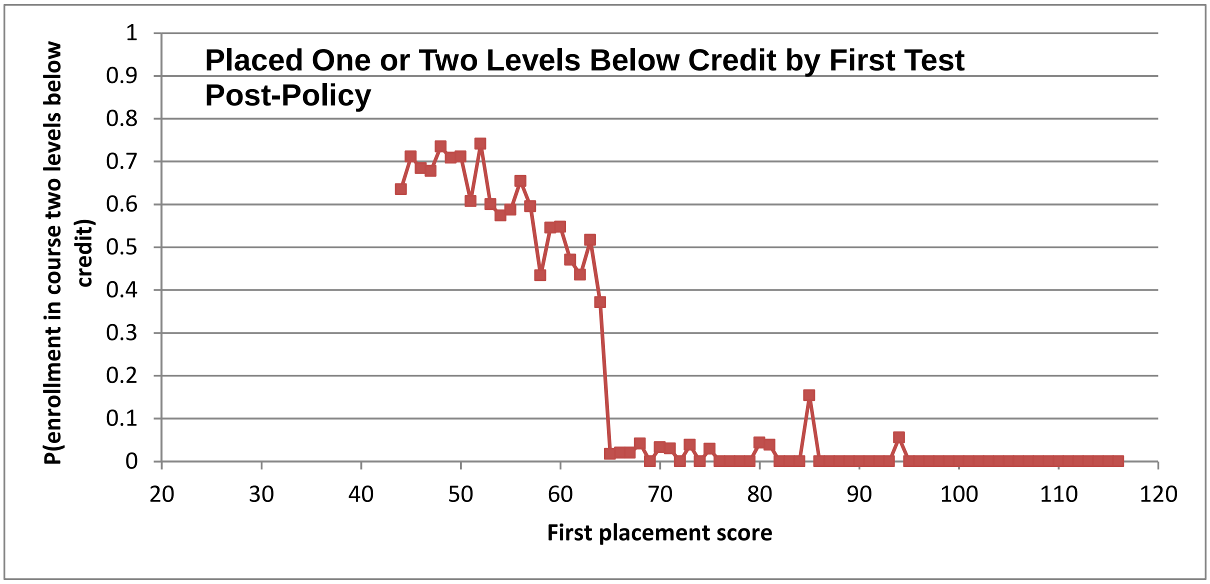

Figure 42: Post-Policy Percentage of Enrollment in Treatment Group

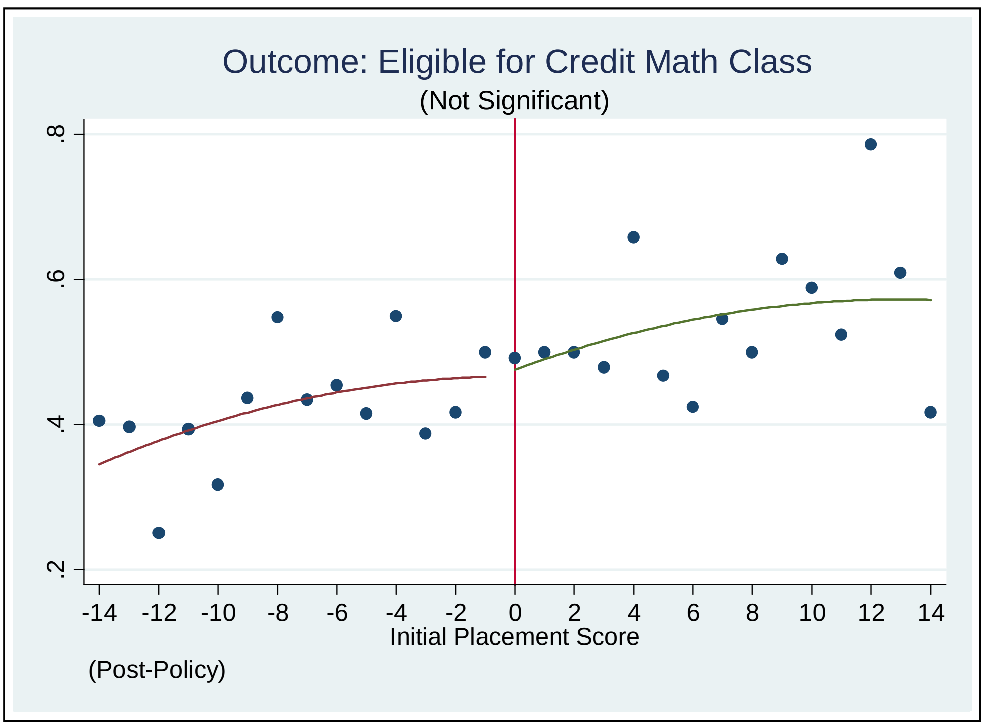

Figure 43: Post-Policy RD Eligible for Credit Math Class

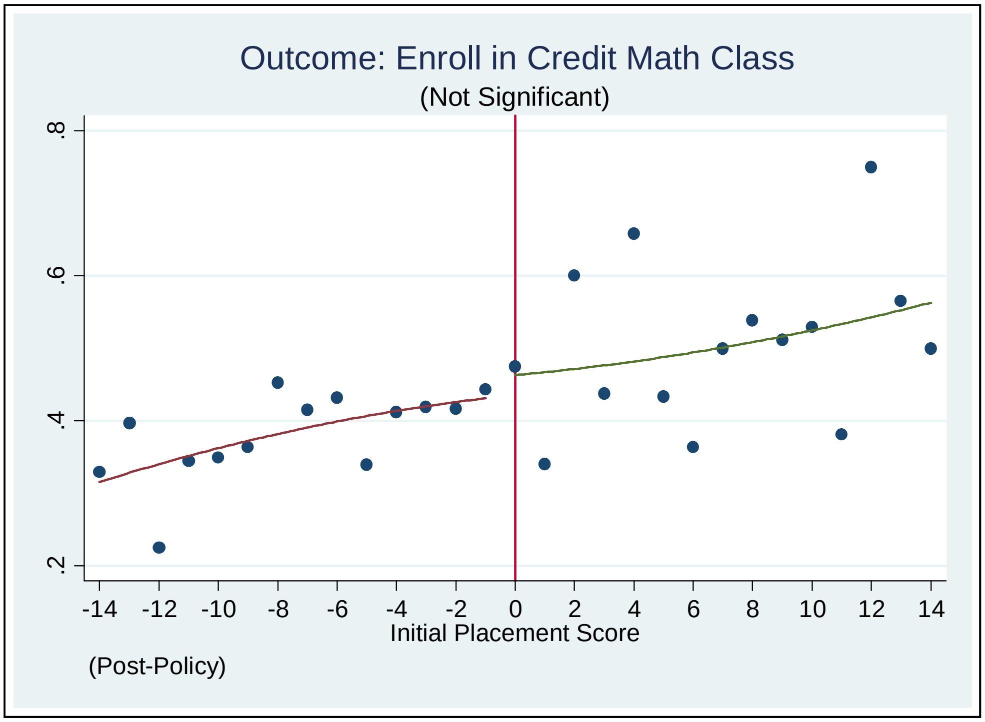

Figure 44: Post-Policy RD Enroll in Credit Math Class

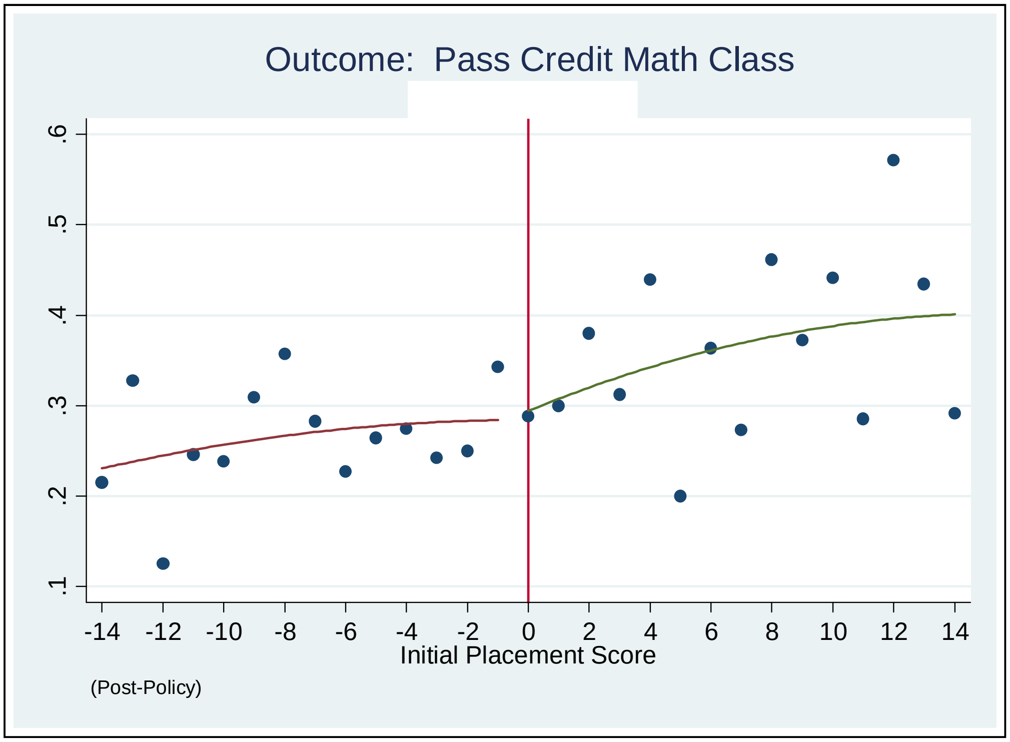

Figure 45:Post-Policy RD Pass Credit Math Class

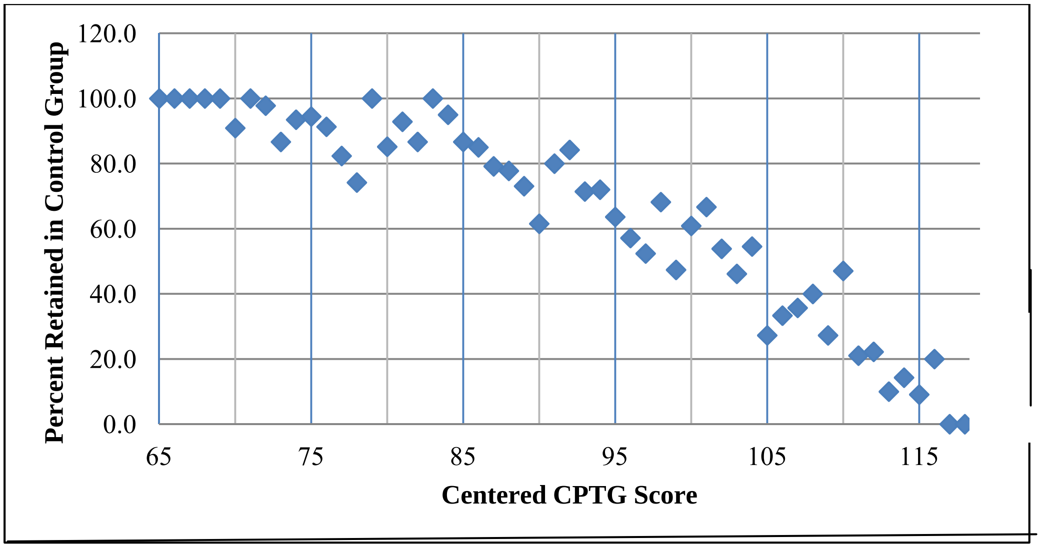

Figure 46: Percent Retained in Control Group in Post-Policy Period as a Function of the Initial Score

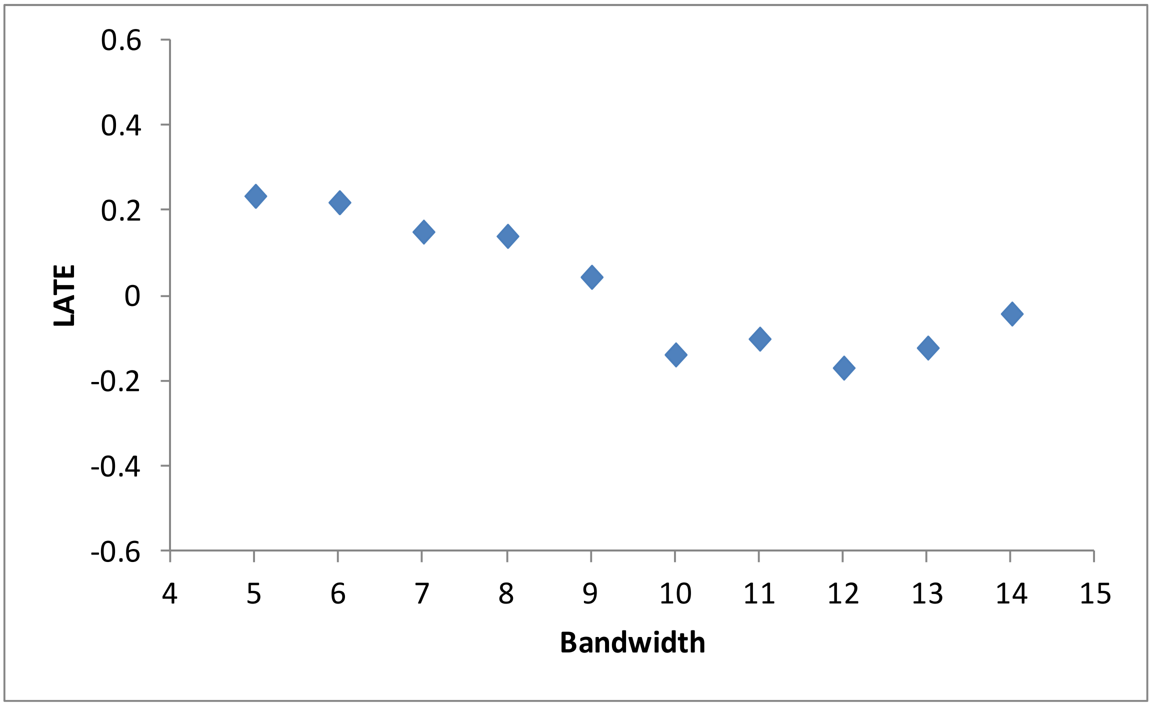

Figure 47: Summary of LATE for Post-Policy Outcome: Eligible to Register for Credit Math Based on Initial Score

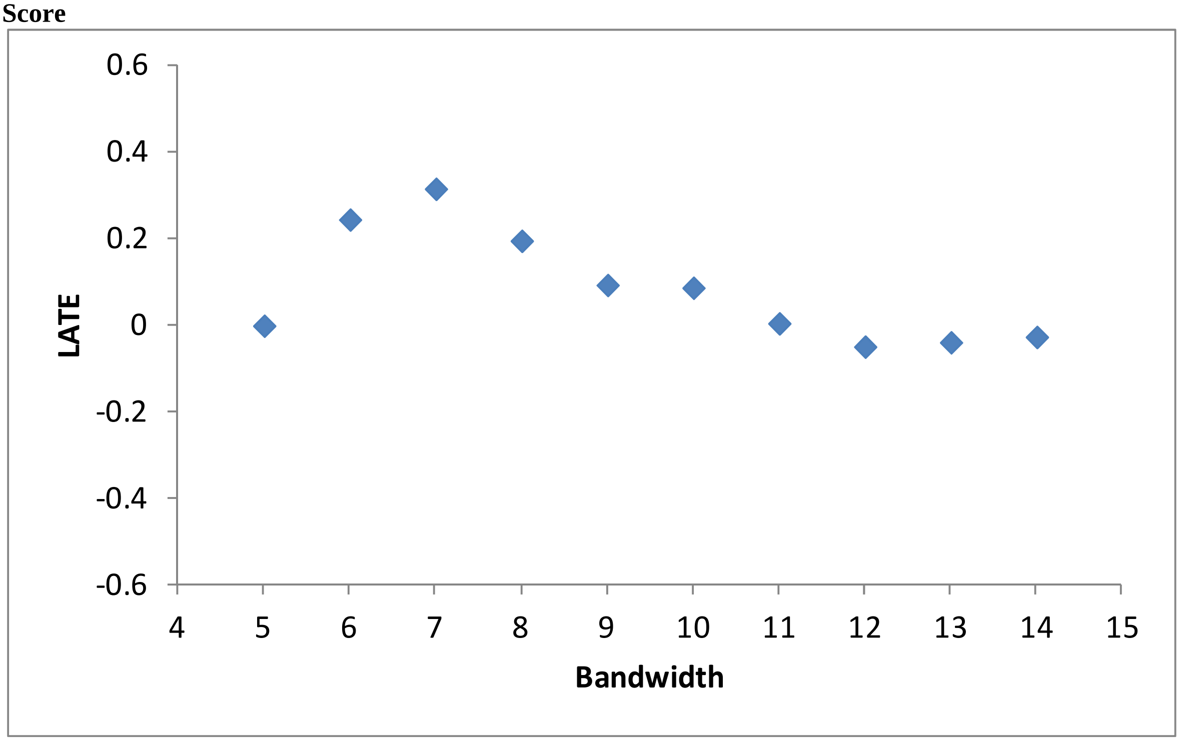

Figure 48: Summary of LATE for Post-Policy Outcome: Enroll in Credit Math Class Based on Initial Score

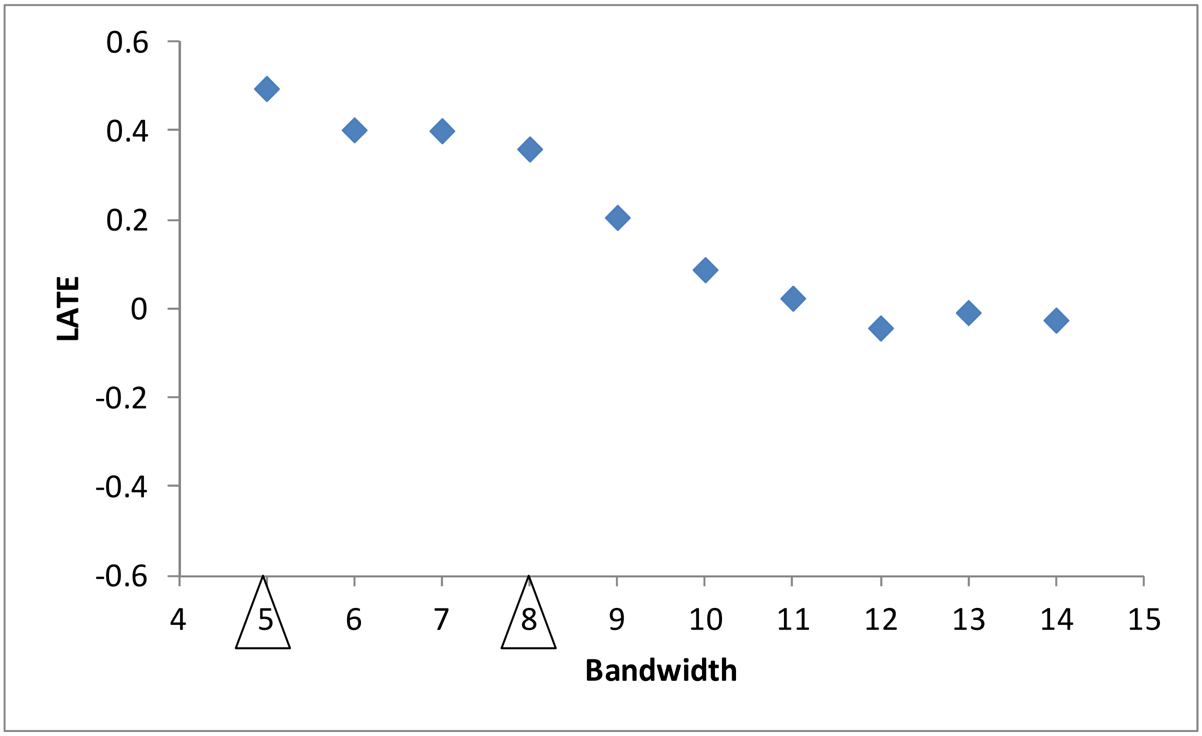

Figure 49: Summary of LATE for Post-Policy Outcome: Successfully Pass Credit Based on Initial Score

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