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The Effects of Career and Technical Education: Evidence from the Connecticut Technical High School System

Eric J. Brunner, Shaun M. Dougherty, Stephen L. Ross

Abstract

We examine the effect of attending stand-alone technical high schools in Connecticut using regression discontinuity. Male students are 10 percentage points more likely to graduate from high school and have half a semester less time enrolled in college. Male students have 32% higher average quarterly earnings. Earnings effects may in part reflect general skills: male students have higher attendance rates and test scores, industry fixed effects explain less than 1/3rd of earnings gains and large earnings gains persist past traditional college going years. Attending a technical high school does not affect the outcomes of female students.

JEL Codes: I21, I26

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The Effects of Career and Technical Education: Evidence from the Connecticut Technical High School System

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I. Introduction

The past decade has witnessed a resurgence of interest in Career and Technical Education (CTE) for K-12 students (Passarella, 2018). Proponents of CTE argue that CTE improves career opportunities by providing students with hands-on training and the soft skills necessary for labor market success (Jacob, 2017). CTE may improve academic skills by increasing student engagement and school attendance. Cullen et al. (2013) argue for vocational training as a way to foster practical skills and labor market integration in currently failing schools.

Carefully identified studies of CTE typically involve a small number of schools that volunteered for evaluation. Kemple and Willner (2008) and Page (2012) examine nine career academies that agreed to randomize admissions finding an 11% increase in earnings for males, but no effect on graduation. Hemelt, Lenard and Paeplow (2019) examine a single career academy with randomized admissions in North Carolina and find improved graduation rates. Dougherty (2018) uses score based admissions to study three Vocational and Technical High Schools in Massachusetts finding a 7 to 10 percentage point increase in on-time graduation. In an exception, Bonilla (2020) evaluates a large CTE grant program in California using the threshold for grant award. Districts receiving a grant reduced high school drop-out rates, but effects could have arisen from either the CTE expansion or the additional monetary resources.[1]

[1] Also, see Cullen et al.'s (2005) study in Chicago using proximity to schools to predict enrollment, Silliman and Virtanen (2019) in Finland and Bertrand et al. (2019) in Norway.

Connecticut presents a unique opportunity to examine the impact of stand-alone delivery of CTE where the services are being delivered at scale. The Connecticut Technical High School System (CTHSS) uses a score-based admissions system supporting a regression discontinuity design, and all 16 schools are oversubscribed.[2] Our analysis includes all technical high schools in CTHSS and covers over 57,000 8th grade student applicants between spring 2006 and 2013. Annually 11,000 students attend the 16 CTHSS schools, which represents 7% of high school students in the state.

Although an admissions threshold is not recorded, the data is consistent with each school establishing a threshold each year and basing acceptances primarily on this threshold. Following Porter and Yu (2015), we estimate an admissions threshold for each school and application year. Regression discontinuity estimates show that students just above this threshold are 87 percentage points more likely to receive an acceptance letter and are 56 percentage points more likely to attend a CTHSS school. However, effects on attendance are higher for male students than for females, 58 compared to 52 percentage points, and the fall off in attendance rates as scores increase is faster for females. These patterns are consistent with underrepresentation of female students who comprise 46 percent of applicants and 41 percent of students attending and with female underrepresentation in CTE generally across the U.S. (Liu and Burns 2020).

Given gender differences in CTE program choice, we estimate treatment effect models for our full sample of students, the male subsample, and the female subsample. The estimated effects arise entirely within the male subsample. Based on 2SLS fuzzy regression discontinuity

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[2] Our analysis excludes Bristol Technical because students remain part of their original high school. The analysis also excludes Wright Technical, which did not open until 2014.

analyses, male students attending one of the technical high schools are: 1) 10 percentage points more likely to graduate from high school relative to the control mean rate of 81%; 2) have accumulated one-half fewer semesters of time enrolled in higher education; 3) have 44% higher total earnings post high school relative to a control average of $53,000; 4) have 32% higher average quarterly earnings relative to $4,500; and 5) have one additional quarter with earnings relative to an average of 10.6 quarters with earnings.[3] Effects are large: cutting the drop-out rate in half and increasing earnings by magnitudes comparable to successful jobs programs like Year-Up Boston and San Antonio Quest (Heinrich, 2012; Elliot & Roder, 2017. Similar to Page (2012), we do not find effects for female students.[4]

A key critique of CTE is that it provides specific skills at the expense of general skills and so labor market gains may be temporary (Hanushek et al., 2017, Krueger & Kumar, 2000). We provide several findings for males that suggest CTHSS earnings gains are more permanent. While effects on average quarterly earnings are higher at 43% before age 23, the effects at age 23 or later are still large at 33%. Further, the negative treatment effect for time in college is smaller after age 23, which can explain half of the decline in earnings gains between older and younger ages. Next, industry fixed effects explain less than 1/3rd of male earnings gains, and so these gains are not explained by placement into higher paying industries or industry specific skills. Finally, we find evidence of gains in general skills: a two-percentage point increase in 9th grade

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[3] Control means are means for compliers within bandwidth and above threshold minus treatment effect. For earnings, means are of the exponential of log earnings minus treatment effect.

[4] In contrast, Bonilla (2020) finds that the California Career Pathways Trust has larger effects for female students, possibly due to a focus on college readiness.

attendance rates over a base of 94%, and an increase in 10th grade test scores of 18 percent of a standard deviation. Again, we do not observe similar effects for females.

We also measure CTE offerings at counterfactual high schools overall, for trade focused courses and for human services, tourism and hospitality. The effect of attending a CTHSS school falls with overall or trade CTE offerings at the counterfactual high school.[5] Attending CTHSS has a 12 percent larger impact on quarterly earnings when the student would otherwise have faced a one standard deviation lower share of electives in CTE. However, the difference in CTE offerings between traditional and CTHSS high schools explains only 1/4th to 1/3rd of the estimated effects. Further, while CTHSS schools have higher spending, lower student-teacher ratios, and better peers than the counterfactual high schools, the treatment effects are similar regardless of counterfactual schools' spending, student-teacher ratio or peer quality. The positive effects of attending a CTHSS school are not due to the effect of additional resources, but rather something unique about the stand-alone CTE focused nature of CTHSS schools. Further, other than gender, effects are homogeneous across students.

We repeat this analysis for female students, but continue to find no effects. Male and female students tend to enroll in different programs with men focusing on building trades and manufacturing and women primarily specializing in human services and hospitality (Liu & Burns, 2020). In 2019, programs that focus on culinary arts, guest services, early child care and education, hairdressing and cosmetology, health technologies, hotel hospitality, and tourism enrolled approximately 52% of all female CTHSS students, while these same programs enroll

[5] On the other hand, the share of electives that are CTE offerings in human services and hospitality, female dominated programs, has no impact.

less than 7 percent of male students. In contrast, the trade related programs of automotive manufacturing and technology, carpentry, collision repair, heavy equipment repair, electrical, HVAC, masonry, plumbing and welding enrolled 73 percent of all male students but only 33 percent of female students.[6] While access to CTE in the trades was important for explaining the impact of CTHSS for male students, CTE offerings in human services, tourism and hospitality do not matter for female students. Perhaps lower returns in these female dominated CTE programs explain the lack of treatment effects. At the same time, the minimal effects on attendance and test scores suggest that CTHSS may not improve female student engagement in school.

The next section describes CTHSS. Section three describes our data. Sections four and five describe our methods and investigates our identification strategy, respectively. Section six presents results, section seven discusses potential mechanisms and section eight concludes.

II. Connecticut Technical High School System

The Connecticut Technical High School System (CTHSS) is a quasi-independent school district where all students participate in CTE. While students must meet the standard high school graduation requirements, CTHSS students complete CTE coursework in lieu of other electives. CTE coursework is grouped into one of 10 to 17 programs of study. Within their selected program, students take a minimum of three (often more) aligned courses. These sequences are combined with career awareness activities and opportunities for work-based learning. On the other hand, traditional high schools typically offer only 2 to 4 CTE programs from which to choose, and students may only take one or two courses, often not in the same program. At CTHSS, 9th grade students explore 3 to 6 programs of interest and at the end of the first semester

[6] See Jacob and Ricks (2020) for an analysis of selection into different CTHSS programs.

rank programs. In the spring, they are assigned a program and spend the next three and a half years completing CTE coursework with a stable cohort of peers and instructors. CTE instructors often collaborate with teachers in core academic areas to ensure overlap of content. CTHSS tends to focus on providing skills to support labor market transition, unlike some states where CTE programs focus heavily on college readiness (Bonilla, 2020).

The CTHSS schools offer a wide variety of CTE programs including manufacturing, building trades, automotive, biological and environmental, computer and information technology, hospitality services, health and child care, culinary, and beauty services, see Appendix Table A1. However, CTHSS students are strongly sorted by gender across these programs. As shown in Appendix Table A1, programs where over 80 percent of the students are male include electrical; plumbing, heating and cooling; information systems technology; and welding and metal fabrication. On the other hand, elderly care and education; hairdressing and cosmetology; and tourism, hospitality and guest services management are all programs where 90 percent or more of the students are female.

The CTHSS schools are located and serve students across all of Connecticut. 31% of total enrollment comes from the state's five, poorest central city school districts of Bridgeport, Hartford, New Haven, New London and Waterbury. However, many CTHSS school are located near and serve suburban towns or more rural regions of the state.

Eighth graders can elect to apply in the winter before they would enroll in 9th grade to attend a CTHSS high school. All 16 technical high schools are oversubscribed. Admission is coordinated by the central office of CTHSS. Each student receives an application score following a common standardized formula. For the 9th grade years of 2006-07 through 2008-09, the score is based on standardized 8th grade test scores in math and language arts (reading and writing) plus

GPA and attendance in middle school. For the 9^{th} grade years of 2009-10 through 2013-14, two additional categories were added based on points for extracurricular activities and a written statement.[7] Students can apply to as many as three schools and must rank-order their choices. Students will be admitted to the school that they ranked highest among those schools where they are above the admissions threshold. Students are never admitted to more than one school. Approximately, 67% of all 8^{th} grade student applicants are admitted to a CTHSS high school, and 40% of applicants actually attend 9^{th} grade at a CTHSS school.

Even though attendance and test scores are close to continuous, the scoring system discretizes each of these components into an ordinal set of points that are added to form the score. The discrete nature of these components when combined with their high correlation yields a distribution of raw scores that is irregular with both mass points and gaps in what otherwise might be a smooth distribution. We discuss this issue in more detail later in the paper.

School administrators describe establishing an admissions threshold every year and then sending out initial acceptance letters primarily to students whose scores lie above the threshold. However, admissions can deviate from this rule. Some students may be admitted with lower scores to increase diversity. Later waves of letters can be sent out to lower scoring students if all seats are not filled. Other students with higher scores may not be admitted because they applied late, withdrew their application prior to a second wave of admissions, or were excluded based on disciplinary information. The recording of acceptance is also imperfect: a small number of students enter the CTHSS system, even though there is no record of them receiving an acceptance letter (only 0.38%).

[7] See Appendix Table A2 for the score composition in each application year.

III. Data and Sample

The CTHSS admissions data includes students who apply to a technical high school for academic years from 2005-06 to 2012-13. The data contains each student applicant's name, date of birth, home town, middle school, the admissions score, the individual components of the score, an indicator for whether the student was admitted (date of admissions letter) and in later years the State Assigned Student Identification Number (SASID). We match the CTHSS admissions records to the Connecticut State Department of Education's (CSDE) longitudinal data system using the following criteria sequentially: SASID, exact match on first and last name plus birth year, first initial and exact match on last name plus birth year and month, and exact match on last name plus exact birth date. This sequential process handles reporting errors for birth dates, spelling errors and nicknames in the CTHSS application that was filled out by hand. Our resulting match rate was 95 percent yielding a sample of 57,658 student applications.

For our analysis sample of applications, we drop applicants who apply in 9th grade for admission to CTHSS for 10th grade enrollment and special education/IEP students because CTHSS treats these groups differently in admissions. We also drop applicants with no education outcome data, i.e. do not appear in the CSDE database in 9th grade. As noted above, students can apply to more than one school, and the sample contains one observation for every application. Students with multiple applications independently contribute to estimates based on being above or below the threshold of each school, since each application above and near a threshold creates an intent to treat for the student. Some 16% of the sample applies to two schools, but only 3% applied to three schools (maximum allowed). However, a much smaller fraction of applications are within the bandwidth of the admissions threshold for more than one school, and all results are robust to dropping students who applied to more than one school.[8] The resulting sample includes 25,072 male applications and 20,983 female applications after dropping the 3,245 male and the 1,912 female 10th grade applicants, the 6,644 male applications and 2,328 female applications who are special education students and the 1,148 male and 1,083 female applicants who are not observed in Connecticut high schools in 9th grade. Results are robust to relaxing these data filters for available outcomes.

From the CSDE data, we obtained student attributes including race, gender, free or reduced price lunch status, English learner, special education status, i.e. presence of an IEP, and 7th grade tests scores. Table 1 demonstrates the generalizability of our analysis by presenting sample means for the state overall, the CTHSS applicant pool omitting IEP students, and the sample attending CTHSS. The top panel presents means for student attributes and standardized test scores. The bottom panel presents means for the CTHSS application score and the key individual components of the score including standardized tests, grades and attendance. The CTHSS applicant sample is less female (46%) than the student population statewide (49%). African-American, Hispanic, and Free lunch eligible students are substantially over represented relative to state wide averages, 50-100% higher shares. The standardized test scores of CTHSS applicants are also about 2/3rd of a standard deviation below the statewide averages. Enrolled students are even more selected away from females (41%). On minority share, free lunch and test scores, however, enrolled students are more similar to state student composition.

In the last two columns, we present means for students who fall within the bandwidth for

[8] The first stage in the fuzzy RD adjusts for the fact that students can attend only one school, and clustering at school by cohort addresses correlations across same student applications.

a regression discontinuity analysis separately for students above and below the admissions cut-off. On average, applicants are higher performing, less disadvantaged and less likely to be black or Hispanic than the subsamples within the admissions thresholds, and these differences are largest for the subsample below the threshold. Notably, the standard deviations of score components only fall by 10 to 25% as we move from the full sample to the samples within 10 points of the cut-off due to variation in cut-offs across time and schools. Consequently, our regression discontinuity analysis should be relevant for a broad population of applicants.

Table 2 presents the CTE course offerings and the average attributes for CTHSS schools and the high schools that the applicants typically attend if they are not admitted to a CTHSS school. We organize students into cells of applicants by the town or city in which they resided in 8th grade when applying to a CTHSS school.[9] For each cell, we calculate averages based on the high schools attended by students who were not admitted to CTHSS. In most cases, students who were not admitted attended their town high school or schools, but in some larger cities the counterfactual is an average across traditional high schools and smaller magnet high schools. The course offering data comes from state data on course enrollment by year, and aggregate school attributes were drawn from state and federal public data.[10]

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[9] School district boundaries in Connecticut follow town and city boundaries except where smaller towns have been consolidated to form a combined regional high school district.

[10] Course offering data is from 2013-14 and 2016-17, 10th grade test scores are from 2007-2011, and Pupil Teacher Ratio, Share Free Lunch Students, and Share Black and Hispanic Students are based on the NCES Common Core of Data School Level Files for 2006-07 to 2013-14. Course offering and test score data was downloaded from http://edsight.ct.gov/SASPortal/main.do.

The top panel of Table 2 presents the share of high school electives that are CTE overall, trade focused, and focused on human services/hospitality.[11] The average share of CTE courses overall is twice as large in CTHSS schools as compared to our counterfactual high schools. For trades, these differences are magnified with non-CTHSS schools offering only about 5% of their electives in trades and trades representing over half of the electives in CTHSS schools. In terms of human services/hospitality, the average counterfactual high school offers more electives as compared to trades, but in CTHSS we cannot document offerings because the two most common programs, culinary and hair dressing, typically list only one or two courses that students repeat with varying content. The bottom panel shows traditional school attributes. CTHSS high schools have higher levels of spending overall and lower student-teacher ratios. Further, while CTHSS students have lower test scores and family incomes than the state average, their test scores and incomes are higher than the peers from their counterfactual schools.

The CSDE data also contains the high school attended in 9th grade, standardized test scores prior to and during high school, attendance, high school graduation, as well as college attendance drawn from the National Student Clearinghouse through May 31, 2019. Using Clearinghouse data, we calculate the number of semesters of college attended capturing all time spent in college. While the time available post-high school varies across cohorts (between 8 and 1 years for 2006 and 2013 cohorts, respectively), cohort fixed effects will assure estimates

[11] Trade programs are manufacturing, transportation, and building trades, and service programs are human services, tourism/hospitality, and family and consumer sciences. Non-CTE electives include visual and performing arts, world languages and religion.

compare individuals with similar temporal opportunities to pursue higher education.[12]

Through Connecticut's P20Win process, students in our sample are matched to Connecticut State Department of Labor (CSDOL) data. This CSDOL match is facilitated by Department of Motor Vehicle records that contain gender, birth date, and first and last name, which is matched to the CSDOL data using social security numbers. CSDOL personnel then match the resulting data to the CSDE data using an exact match on birth date and gender and a fuzzy match algorithm on name. The fuzzy match algorithm requires an estimated confidence of 70%, which yields a match rate of 72.3% between the student applicant records and the CSDOL data. The match rate increases to 84% when focusing on 8th grade applicants that we observe for at least two years post-high school, the 2006-2011 cohorts.[13] The matched data contains a record for Unemployment Insurance covered labor market earnings in each quarter of each year for which the individual had covered earnings in Connecticut and was age 16 or older.

Several factors drive the failure to match applicants in the CSDOL data including never having a driver's license in Connecticut, name changes prior to any labor earnings due to marriage or other factors, moving out of state prior to or upon completion of high school or failure to participate in the labor market after high school perhaps due to college attendance. We include quarters of earnings after allowing for four years to complete high school and six

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[12] The clearinghouse identifies spells of education by start and end dates, and in some cases these spells overlap. We accumulate all overlapping spells and calculate the number of days in each spell dividing by 112, 16 weeks times 7 days, to calculate semesters in college.

[13] Subgroups by gender, race, free lunch status, and test scores all have match rates over 81% in the 2006-2011 cohorts, while for English language learners the match rate is 76%.

quarters to enter the labor market. Our labor market data ends in the 1st quarter of 2018. Therefore, for our labor market analysis, we restrict the sample to 2006 to 2011 cohorts since 2011 applicants are expected to graduate in May of 2015. The 2011 cohort then has five potential quarters of labor earnings starting in quarter 1 of 2017 and running through quarter 1 of 2018.[14]

We create three labor market variables: total earnings, average earnings per quarter, and number of quarters with earnings. While we do not observe hours/days worked or wages, number of quarters with earnings captures some of the extensive margin of employment, and average quarterly earnings provides information on earnings capacity since most quarters with earnings are associated with continuous periods of employment. As with college, cohort fixed effects assure comparisons of individuals with similar opportunities for quarters with earnings.

IV. Methods

We model the relationship between outcomes and admission scores using a "fuzzy" local-linear regression discontinuity design with a uniform kernel (Imbens & Lemeiux, 2008; Murnane & Willett, 2011). However, we do not observe the threshold established for admissions, and so estimate the thresholds for each school and year by identifying the threshold yielding the largest discontinuity following Porter and Yu (2015). Specifically, we estimate linear probability models for receiving an acceptance letter (T_{isyt}) separately for each school s and application year y for the sample of applicants i from town t controlling for linear running variables in the composite admissions score (X_{isyt}) on either side of candidate thresholds or cut-offs (X^{*}_{sy}):

[14] Appendix Table A3 presents fraction applicants for which quarterly earnings are observed by quarter beginning six quarters after four years of high school. Participation rate starts below 50 percent, but from 3 quarters onward the rate of recorded earnings is reliably around 60 percent.

$$T_{isyt} = \alpha_{sy} d\left(X^*_{sy} \le X_{isyt}\right) + \theta_{11}\left(X_{isyt} - X^*_{sy}\right) + \theta_{12}\left(X_{isyt} - X^*_{sy}\right) d\left(X^*_{sy} \le X_{ist}\right) + \varepsilon_{1ist} \quad (1)$$

where $d\left(X^*_{sy} \le X_{isyt}\right)$ is a binary indicator that is one if the condition is satisfied. α_{sy} represent the parameters of interest, and θ_{kj} capture the slopes of the running variable for equation (k). Equation (1) is estimated using observations within a specified bandwidth (BW) for which:

$X_{isyt} \in [X^*_{sy} - BW, X^*_{sy} + BW]$,

and the threshold estimate is selected as:

$\widehat{X^*_{sy}} = argmax_{X^*_{sy}}\ \widehat{\alpha_{sy}}(X^*_{sy})$ over all $X^*_{sy} \in [X_{min} + BW, X_{max} - BW]$

For more details, see Section 1 of the Methodological Appendix.

We then create a centered score, $\tilde{X}_{isyt} = X_{isyt} - \widehat{X^*_{sy}}$ and pool the data across schools and application years in order to estimate models of student outcomes (y) using 2SLS:

$$y_{isyt} = \beta A_{isyt} + \theta_{21}\tilde{X}_{isyt} + \theta_{22}\tilde{X}_{isyt} d\left(0 \le \tilde{X}_{isyt}\right) + \delta_{2sy} + \gamma_{2t} + \varepsilon_{2ist} \quad (2)$$

where A_{isyt} denotes whether the individual attends the technical high school to which they applied, δ_{2sy} is a vector of school-by-application year fixed effects, and γ_{2t} is a vector of applicant town of residence fixed effects.[15] Finally, the first stage equation for A_{isyt} is:

$$A_{isyt} = \tilde{\alpha}\ d\left(0 \le \tilde{X}_{isyt}\right) + \theta_{31}\tilde{X}_{isyt} + \theta_{32}\tilde{X}_{isyt} d\left(0 \le \tilde{X}_{isyt}\right) + \delta_{3sy} + \gamma_{3t} + \varepsilon_{3ist} \quad (3)$$

where $\tilde{\alpha}$ represents the average threshold effect on attendance. The parameter of interest, β, captures the effect of attending a CTHSS school for students who are just above the threshold compared to those just below. Standard errors are clustered following our fixed effects structure: application school by application year and sending town (Kolesár & Rothe, 2018).

[15] Town fixed effects are not needed for identification, but the inclusion of town fixed effects captures the counterfactual opportunities and leads to a noticeable improvement in precision.

Finally, as shown later, the empirical distribution of admission scores contains mass points that lie above what would otherwise be a smooth unimodal distribution. To address concerns about manipulation, all models are estimated using a donut hole approach dropping observations at the cut-off, i.e. sample is selected as: $X_{isyt} \in [X_{sy}^* - BW, X_{sy}^* - 1]$ or $[X_{sy}^* + 1, X_{sy}^* + BW]$ (Barreca et al., 2011).

V. Identification

V.A. Modelling the First Stage Regression Discontinuity

As discussed above, we empirically select a threshold for each school and application year. We selected a primary bandwidth of 10 and then tested the sensitivity of our results to the use of smaller bandwidths. Using our full sample, we estimate equation (1) separately for each school and year identifying the cut-off that maximizes the discontinuity in acceptance.[16] We then estimate a first stage equation pooling data from all schools and years and imposing a donut hole specification by dropping observations at the selected threshold for each school and year.[17]

Figure 1A and Table 3 column 1 present the pooled estimates for admission using the 10-point bandwidth and donut hole sample. Figure 1B and Table 3 column 2 presents the estimates for whether a student attends one of the technical high schools. Figure 1A shows a clear discontinuity at the threshold with a probability of acceptance above 0.9 gradually approaching one as the score increases. Figure 1B illustrates a similar jump in the probability of attending

[16] Students are also coded as being accepted when no offer letter is recorded if either the system records the student's acceptance or the student is observed attending the school in 9th grade.

[17] We do not impose the donut hole when selecting thresholds because dropping of sample at each threshold could create non-convexities in the optimization function.

above 0.6, but as score increases further the probability attending falls, consistent with higher scoring students preferring other options. Table 3 indicates that being just above the threshold raises the probability of acceptance by 86 percentage points and raises the likelihood of attending by 56 percentage points. Figures 1C and 1D show results separately by male and female students. The effect of being above the threshold on attendance is larger for male students than for female students (58 versus 52 percentage points). Further, the rate of decline in attendance appears substantially faster for women. Between 40 and 50 points past the threshold, female acceptance rates lie between 20 and 25 while male acceptance lies between 25 and 35. As shown in Appendix Table A4, first stage estimates using the labor market sample are similar.

The use of the sample for selecting the threshold does not affect inference in the second stage of the 2SLS models as long as the instrument has sufficient power and the exclusion restriction is satisfied. Therefore, all results below are presented with clustered standard errors. However, the clustered standard errors in the first stage attendance model may be biased because the thresholds were selected using the same outcome and sample, and so our use of F-statistics from the first stage regression to evaluate the strength of the instrument may be misleading. To verify the power of our instrument, we draw holdout samples for re-estimating our first stage models. The resulting F-statistics are always very strong, well above 400. For details, see Section B1.1 of the Methodological Appendix.

V.B. Distribution of the Running Variable

Regression discontinuity assumes that individuals cannot adjust the running variable strategically in response to the cut-off (Lee and Lemieux, 2010). At CTHSS, students have little opportunity to strategically manipulate their score. Schools set the threshold after observing the completed applications, and so students cannot know the exact cut-off. School personnel could manipulate scores when processing each application. However, manipulation also seems unlikely here. First, school administrators have no incentive to manipulate the score given the flexibility to depart from the threshold and the lack of admission decision monitoring. Further, the key components that make up a student's score are standardized 8th grade test scores in math and language arts (reading and writing) plus GPA and attendance in middle school, objective components with little room for manipulation. There is potentially more room for the manipulation of points assigned for extracurricular activities and a student's written statement, but we observe a non-standard distribution prior to the inclusion of these components. We also verify that the components sum to the composite score for all students so manipulation would have to involve changing individual score components.

We plot raw score distributions separately for 2006-2008, 2009-2010 and 2011-2013 since points for extracurricular activities and the written statement were added in 2009 and the extracurricular and statement points were increased from 6 to 20 in 2011. The left-hand side of Appendix Figure B1 shows these score distributions. The distributions exhibit substantial mass points and holes relative to any smooth distribution one might fit to this data. In 2006-2008 when the distribution only contains objective information, the raw score distribution is just as non-smooth as later years. On the right-hand side, we present the distributions dropping students whose scores are exactly equal to their school and year cut-off (donut hole). If irregularities were driven by manipulation, most of the mass points should have disappeared when we dropped students at the cut-offs, but the distributions are relatively unchanged.

Appendix Table A5 presents the McCrary tests for a smooth distribution at the threshold separately for each school and application year. The school-year combinations that reject smoothness at the 10 percent level are shaded, but no particular pattern stands out. All but one school fails the test for some years, but none fail for all years. Similarly, multiple schools fail and multiple schools pass the test every year, and failures are no more likely after 2011 when a large weight was placed on subjective components. Later, we replicate our results using a subsample of schools/application years that do not fail the McCrary test and all results are robust.

We also conduct simulations to demonstrate that the irregular features of the score distribution arise from the processes that generate the scores. We calculate the deviation between the empirical distribution of raw scores and the distribution after dropping scores at the cut-off. We then simulate fake cut-offs for each school and year and calculate the deviation arising from dropping scores at fake cut-offs. The deviation arising from the simulated cut-offs explains between 60 and 70 percent of the deviation that arises from using the true cut-off, see Appendix Section B.2.1. We next simulate the score distributions year-by-year by randomly drawing components from their empirical distribution preserving the correlation between score components. We then plot the distribution of these fake scores. While we do not perfectly replicate the distributions, the simulations generate similar shaped distributions with significant numbers of mass points and holes, see Appendix Section B.2.1.

Finally, the centered score distributions contain cliffs where density is high above the cut-off and drops immediately below the cut-off. The process of admitting applicants until a quota has been met could create such cliffs. For each school, we draw a random number of admissions from the empirical distribution of admissions over all years. We then use simulated admissions to create fake cut-off scores. For 2006 through 2009, the simulated centered score distributions replicate the cliffs. However, beginning in 2010, the simulated distribution is smoother, while the true distribution continues to have cliffs, see Appendix Section B.2.3. Therefore, we replicate results dropping the data from 2010 and beyond and once again all our results are robust.

V.C. Balancing Tests

To further rule out manipulation and check for local randomization, we regress race and ethnicity, whether the student is free lunch eligible, whether the student is an English language learner, each student's 7th grade standardized composite test score in reading, math and writing (the running variable contains 8th grade scores), and whether the applicant is observed in the labor market sample on a dummy variable for whether the applicant's score is above the cut-off, the linear running variable for the student's score and the interaction of the running variable and being above the cut-off plus school by cohort and town of residence fixed effects. These results are shown in the top panel of Table 4. In the bottom panel, we present balance tests on counterfactual high school per pupil spending and student-teacher ratio, and on middle school's average 6th grade test scores and share of students proficient on 8th grade tests. All estimates are insignificant. Appendix Tables A6 and A7 present balancing tests separately for male and female students. We also pass balancing tests for males for the labor market sample, alternative bandwidths, and the non-donut hole sample, Appendix Tables A8, A9 and A10, respectively. Notably, donut hole size is often selected by expanding the hole until the sample passes balance, so passing balance with the non-donut hole sample strongly suggests that these irregularities are not due to manipulation.

VI. Results

Figures 2A and 2D present regression discontinuity graphs with a centered score, fitted lines to the running variable, and the outcome mean at each score on the vertical axis. Figure 2A shows the results for high school graduation. Score means form a relatively tight scatterplot around the fitted lines with a clear 4 percentage point discontinuity implying higher rates of graduation above the threshold. Figure 2D presents similar results for average quarterly earnings for the labor market sample. Again, we observe a discontinuity of 0.1 log points higher quarterly earnings just above the threshold. Next, we split the sample between male and female applications. Figures 2B and 2E present results for the male sample which mirror the results for the full sample with larger discontinuities. Figures 2C and 2F for the female sample do not show discontinuities suggesting that females do not benefit from admission to CTHSS schools.

Table 5 presents the two-stage least squares estimates for the full donut hole sample in panel 1, the male subsample in panel 2 and the female subsample in Panel 3 including the student-level covariates from the balancing tests as additional controls. As above, the full sample and male subsample results are similar with larger estimated effects for males, and the estimated effects for females are insignificant and relatively small. The treatment on the treated effects for the males are large. Attending a CTHSS high school increases high school graduation rates by 10 percentage points relative to a control mean of 81%, and reduces time enrolled in college by almost ½ a semester. While not shown, we investigated attendance at two and four-year colleges separately and find similar effects. In the labor market sample, attending a CTHSS school results in 44% higher total earnings and 32% higher average quarterly earnings for males (based on regressions of log earnings). For comparison, average quarterly earnings for male students was approximately $4,500. Attending a technical high school also results in 1.0 additional quarter with earnings relative to a sample average of approximately 10 post-high school quarters with earnings. While large, these labor market effects are similar to effects of skill intensive jobs programs (Heinrich, 2012; Elliot & Roder, 2017). For females, all estimates are relatively small ranging between 5 and 15% of the estimated treatment effects for males with the exception of college attendance, which while larger is still insignificant.

VI.A. Validation and Robustness Tests

Table 6 presents falsification tests for males moving the cut-off down 10 points or up 10, 15 or 20 points. Reduced form estimates are presented because the associated first stage will not have power. Table 6 panel 1 presents reduced form estimates for males, comparable to the 2SLS estimates in Table 5 panel 2. Panels 2-5 of Table 6 show the reduced form falsification estimates starting with the false cut-off 10 points below the true cut-off and moving to 20-points above. In panels 2-5, the point estimates are always statistically insignificant and tend to be substantially smaller than and sometimes opposite sign of the reduced form estimates in panel 1.[18]

Next, we run a series of robustness tests. Appendix Table A12 presents models from Table 5 omitting balancing test controls. Estimates are nearly identical to the estimates in Table 5 panel 2 for males, never differing by more than 2 percent, and also closely match the small, insignificant estimates for females. Appendix Table A13 presents our male estimates for bandwidths of 6 or 8 points around the discontinuity. All results are robust. With one exception, we observe no discernable pattern in estimate changes as bandwidth is narrowed: Declines in magnitude vary between 5 and 17 percent, and increases vary between 6 and 36 percent. The one exception is quarters with earnings where the treatment effect steadily increases as the bandwidth is narrowed by 44% from 10 to 8 points and by 26% from 8 to 6 points.[19] Optimal bandwidths are selected as a trade-off between precision gained and bias introduced as bandwidth is expanded (Calonico, Cattaneo and Farrell, 2020). Shrinking the bandwidth should reduce bias, and none of our estimates fall consistently as bandwidth is reduced. In Appendix Table 14, we

[18] The first stage estimates for the falsification tests are small and shown in Appendix Table A11.

[19] We present reduced form estimates so one can observe the direct responsiveness of treatment effects to changes in bandwidth, but the first stages estimates are stable over bandwidth.

replicate results for males using the non-donut hole sample and changes in estimates are modest never exceeding 18 percent and never varying in direction, inconsistent with manipulation.[20] Finally, in Appendix Table A14, male results are robust to dropping individuals who submitted more than one application, although results for college semesters and quarters with earnings lose significance given modest erosion in magnitude and larger standard errors.

Since we only observe earnings in Connecticut, we examine representation in the male labor market sample separately for towns adjacent to the boundary and for interior towns in Appendix Table A15. We observe modestly lower rates of labor market representation in boundary towns. We rerun analyses dropping male students from boundary towns (Appendix Table A16 Panel 1), and all results are robust. Finally, given concerns about outliers in earnings, we conduct quantile regressions on average quarterly earnings finding relatively stable estimates over the earnings distribution (Appendix Table A16 Panel 2).

VI.B. Treatment Effect Heterogeneity

We examine whether effects of attending CTHSS schools are heterogeneous across students' reduced or free lunch eligibility, being either African-American or Hispanic, or residing in one of Connecticut's five poorest central cities. We interact treatment, running variables and school by cohort fixed effects with the student attributes using being above the threshold and its interaction as instruments. As shown in Appendix Table A17, the vast majority

[20] We replicate findings dropping school by years that fail the McCrary test and dropping 2010 or later where simulations fail to replicate score distribution cliffs, see Appendix Table A14.

of the interaction estimates are insignificant.[21] We also find no correlation between treatment effects and the cut-off score for different schools and years, see Appendix Figure A1.

VII. Potential Mechanisms

We begin this section examining the role of general skills versus a more short-lived boost in the labor market. The first panel of Table 7 examines two subsamples: observations 6 quarters after expected high school graduation to the quarter prior to the student turning 23, and observations from the quarter the individual turns 23 until the first quarter of 2018.[22] For each subsample, we calculate average quarterly earnings for all students with at least one quarter of earnings. The first two columns show that CTHSS increases average quarterly earnings by 43% in the younger sample and by a lower, but sizable, 33% in the older sample. Columns 3 and 4 present similar estimates for semesters in college finding a significant reduction in semesters for the younger sample and an insignificant effect in the older sample, a 0.35 semester difference in point estimates. Our 22 and younger sample averages just under 11 potential quarters in the earning sample prior to age 23. If we assume that college attendance reduces labor supply substantially (over 1/3rd of a year per semester), the short-run treatment effects of college attendance can explain half of the decline in quarterly earnings gains between the samples.

Next, if the return to attending CTHSS is due to industry specific skills or arise from an advantage in entering higher paying industries, most of the earnings gains should accrue across

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[21] The one exception is the interaction of central city residence with treatment in the model for number of quarters with earnings, significant at the 99 percent confidence level.

[22] Results using balanced samples restricted to only individuals observed in both age ranges yield similar, but substantially noisier, estimates.

industry categories. Table 7 Panel 2 column 1 presents our baseline estimates for average quarterly earnings from Table 5, and column 2 presents estimates for quarterly earnings in the quarter by individual sample conditional on fixed effects for the year by quarter of earnings. Column 3 adds two-digit industry code fixed effects for each student and quarter, and column 4 controls for three-digit industry. The estimated effect on quarterly earnings of 0.31 is very similar to the average quarterly earnings effects of 0.32. The inclusion of two-digit industry codes erodes the effect somewhat with the estimate falling to 0.24, but three-digit industry codes leaves the estimate almost unchanged at 0.22. Most of the earnings returns are within industry.

The last panel focuses on measures that proxy for the acquisition of general skills presenting estimates for 9th grade share of days attended, and standardized composite and disaggregated 10th grade reading and math test scores. We find that attendance rates improve by 1.7 percentage points relative to a control mean of 94%, and so attendance rates might proxy for student engagement (Archambault et al., 2009). Average test scores improve by 18 percent of a standard deviation, and math and reading scores improve by 13 and 16 percent of a standard deviation, respectively. These estimates suggest a role for observable general skills in explaining wage gains, and work against earnings gains arising from the provision of specific skills and connections intended to facilitate entry into specific industries. We do not find similar effects on in-school outcomes for female students.

We next examine whether the treatment effect varies by the availability of Career and Technical Education (CTE) in the high school the student would have attended if they had not attended a CTHSS school. In Table 8, we present models where we interact treatment with the share of elective courses that are CTE at a student's counterfactual high school using the 8th grade town-based definitions used for Table 2. Panel 1 presents the results using all CTE offerings, Panel 2 presents the results using the offering of courses related to trades, and Panel 3 presents results for human services and hospitality. We have relatively consistent results across graduation, college attendance and quarterly earnings with declining effects as the counterfactual high school offers an increasing share of electives as CTE overall or trade focused courses. In contrast, for human services/hospitality, CTE areas often pursued by female students, all of the interaction estimates are small in magnitude and statistically insignificant.

For comparison purposes, we examine how much of the CTHSS treatment effects are explained by standard deviation increases in CTE offerings at counterfactual high schools. In Table 2, a one standard deviation increase in share of CTE courses at non-CTHSS schools is 0.067, which is 15.3% of the gap between CTE offerings at CTHSS and non-CTHSS schools, and so eliminating 15.3% of offerings gap is associated with reducing the effects on college attendance by 5.3% and reducing the effects on quarterly earnings by 3.5%. Similarly, focusing on trade offerings within CTE, a one standard deviation increase is 0.023, which is only 4.5% of the offerings gap. Therefore, a one standard deviation change in share of electives in trades reduces treatment effects on high school graduation by 1.6% and on quarterly earnings by 1.1%. If we extrapolate out to eliminate the entire CTE offerings difference, the treatment effects on high school graduation is reduced by 36% for trade courses, and the treatment effects for quarterly earnings are reduced by 23% for CTE overall and 24% for trade.

Therefore, while a significant portion of CTHSS effects can be attributed to increased CTE offerings, these findings leave substantial room for other factors. We conduct a similar counterfactual exercise using traditional attributes of counterfactual high schools, i.e. per pupil expenditures, student-teacher ratios and average peer test scores, but find no evidence that leaving a school that had lower spending, higher student-teacher ratios or worse peers influences the CTHSS treatment effect (Appendix Table A18). Therefore, while CTHSS schools have more resources and better peers, these differences cannot explain male student gains, suggesting that benefits may arise from the integrated CTE experiences offered by CTHSS schools.

VIII. Discussion

We examine the effect of attending one of Connecticut's 16 stand-alone technical high schools on educational and labor market outcomes using regression discontinuity analysis. We find large, robust positive effects for males on high school graduation and labor market outcomes. The estimated effects are broad based accruing to males of different socio-economic backgrounds and ability. The estimates are robust to alternative bandwidths, the inclusion of controls for student attributes and a donut hole specification. Falsification tests cannot identify similar discontinuities at false thresholds above or below the true admissions cut-offs.

Several findings are consistent with earnings gains arising from general skills. First, we continue to observe large wage gains for older workers. Second, CTHSS attendance improves in-school outcomes such as high school graduation, attendance, and 10th grade test scores that are valued in the labor market. Finally, only 1/3rd of earnings gains are explained by across industry wage differences so most gains are not associated with industry specific advantages.

The impacts also differ by the CTE offerings that a student likely would have had available if they had not attended a CTHSS school. Students who likely would have attended a school with minimal CTE and CTE trade focused offerings benefit more from attending CTHSS. While the influence of counterfactual offerings on returns are sizable, closing the course offering gap only reduces the treatment effects by between 1/4 and 1/3. Also, traditional educational inputs, like spending, teacher to student ratio and peer quality do not explain differences in the return to attending CTHSS schools. These findings suggest that the integrated, standalone provision of CTE is important for explaining the large returns experienced by CTHSS students.

Unlike male students, female students admitted to the CTHSS system have very similar outcomes to non-admitted students. In terms of mechanism, we do not observe similar gains in attendance or test scores suggesting that female students do not experience increased school engagement. Another potential explanation for gender differences is that female students tend to pursue different CTE programs than male students. There is no relationship between returns to CTHSS and counterfactual offerings for female dominated programs: returns to CTHSS are zero even if CTHSS provides a large increase in hospitality/human service offerings.

The per pupil cost at a CTHSS school was over $13,000 in 2019 dollars, about $2,000 more per pupil than the state average and almost $4,000 more than the students' counterfactual high schools. Further, in the National Center for Education Statistics data, CTHSS schools report an average Student to Teacher Ratio of 10.3, compared to 13.3 in other high schools. However, even at an additional $4,000 per year, $16,000 across four years, the earnings benefits of 44% increase over mean total earnings of $53,000 would seem to easily pass any back-of-the-envelope cost-benefit test. Further, the additional costs are less than the costs of highly successful job training and placement programs like Year-Up Boston and San Antonio Quest (Heinrich, 2012; Elliot & Roder, 2017).

Our study offers some of the first quasi-experimental evidence of the treatment effects of a technical high school program offered at scale in the U.S. CTE is an important strategy for improving the economic opportunities of students who might not pursue a traditional four year college degree, and this study documents large positive effects for males. The effectiveness of CTHSS for males is especially important given declining opportunities for and declining labor force participation among non-college going, prime-age males (Abraham & Kearney, 2018;

Aguiar et al., 2017; Autor, 2019; Austin et al., 2018).

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Table 1: Summary Statistics

	State	Applied to CTHSS	Enrolled in CTHSS	Below Threshold BW 10	Above Threshold BW 10
Female	0.49	0.46	0.41	0.45	0.45
Asian	0.04	0.01	0.01	0.01	0.01
Black	0.13	0.20	0.16	0.24	0.21
Hispanic	0.17	0.32	0.30	0.36	0.34
Free Lunch	0.33	0.66	0.59	0.75	0.68
English Learner	0.06	0.06	0.05	0.09	0.06
7th Grade CMT-Reading	249	230.27	239.48	214	225.97
		(35.13)	(30.71)	(27.20)	(26.68)
7th Grade CMT-Math	263	243.70	253.76	226	238.75
		(35.64)	(31.43)	(26.31)	(23.56)
7th Grade CMT-Writing	246	230.80	235.91	220	227.93
		(28.42)	(25.77)	(23.07)	(23.61)
Total Application Score	--	64.39	71.20	53.83	62.66
		(19.26)	(14.10)	(10.65)	(10.50)
Application Grades Score	--	25.46	28.10	20.99	24.29
		(9.24)	(7.57)	(7.20)	(6.67)
Application Attendance Score	--	7.03	7.95	6.31	7.15
		(4.22)	(3.90)	(4.22)	(4.10)
Application Math Score	--	13.42	14.77	11.17	12.91
		(4.68)	(3.97)	(3.90)	(3.74)
Application Language Arts Score	--	13.67	15.09	11.18	13.22
		(5.26)	(4.40)	(4.86)	(4.50)

Notes: Table presents means and standard deviations of individual control variables. Column 1 presents mean of characteristics for the state of Connecticut overall. Columns 2 presents summary statistics for the full sample of students that applied to a CTHSS school. Columns 3 presents summary statistics for sample of students that enrolled in a CTHSS school and columns 4 and 5 present summary statistics for the sample of students within +/- 10 points of the admission score threshold.

Table 2: CTHSS versus Counterfactual H.S. Summary Statistics

	CTHSS Schools		Non-CTHSS Schools	
	Mean	Std. Dev.	Mean	Std. Dev.
	Course Offerings			
Total CTE Courses	30.25	3.821	22.027	8.162
Trade CTE Courses	19.06	3.193	2.597	1.474
Human Services/Tourism Hospitality Courses	NA	NA	4.242	1.931
Share Total CTE Courses	0.891	0.081	0.452	0.068
Share Trade CTE Courses	0.567	0.113	0.054	0.023
Share Human Services/Hospitality	NA	NA	0.087	0.026
	Test Scores and Schooling Inputs			
10th Grade Math Scores	243.669	12.646	233.84	20.77
10th Grade Reading Scores	229.289	11.214	227.13	18.38
Spending per Pupil	13,506	1,746	9,884	1,647
Pupil-Teacher Ratio	10.358	1.198	13.587	1.394
Share Free Lunch Students	0.362	0.145	0.475	0.266
Share Black	0.142	0.128	0.202	0.154
Share Hispanic	0.278	0.177	0.161	0.011

Notes: Table presents means and standard deviations of the characteristics of CTHSS schools and counterfactual high schools. Columns 1 and 2 presents summary statistics for CTHSS schools while columns 3 and 4 present summary statistics for non-CTHSS schools. The top panel presents the number and share of elective courses that are any type of CTE course, elective courses that are CTE trade courses (architecture, transportation and manufacturing) and elective courses that are human service, tourism and hospitality or Family & Consumer Sciences. Data on course offerings at the school level are based on the 2013-14 and 2016-17 school years. The bottom panel presents 10th grade test scores in reading and math along with inputs to the education production process, namely spending per-pupil, the pupil teacher ratio and the share of students that are eligible for free or reduced price lunch and share Black and Hispanic.

Table 3: First Stage Estimates (Bandwidth 10)

Outcome	Probability of Being Admitted Full Sample (1)	Probability of Attending Full Sample (2)	Probability of Attending Male Students (3)	Probability of Attending Female Students (4)
Offer	0.863***	0.555***	0.583***	0.524***
	(0.0208)	(0.0215)	(0.0229)	(0.0245)
Controls	Yes	Yes	Yes	Yes
F	1727	666	645	458
Observations	16,930	16,930	9,287	7,629

Notes: Table presents first-stage estimates of the probability of being admitted to a CTHSS school and the probability of attending a CTHSS school for the sample of all applications from 8th graders from 2006-2013. Column 1 presents first-stage estimates of the probability of being admitted to a CTHSS school where the dependent variable is an indicator for receiving an offer of admittance and the sample includes both male and female students. Column 2 presents main first-stage estimates for probability of attending a CTHSS school after receiving an offer where the dependent variable is an indicator for attendance at a CTHSS school in 9th grade. Columns 3-4 present the same information as column 2 but limit the sample to male and female students respectively. All specifications include the full set of controls listed in Table 1, namely indicators for whether an applicant is Asian, Black, Hispanic, Free or reduced price lunch eligible and whether the student is an English language learner. Columns 1 and 2 also include an indicator for whether a student is female. All specifications include CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Balancing Tests Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual-level Covariates					
Outcome	Black	Hispanic	Free Lunch	English Learner	7th Grade Test Scores	In Labor Market Sample
Offer	-0.00973	-0.00499	-0.00954	0.00774	-0.126	-0.00463
	(0.00908)	(0.00946)	(0.0109)	(0.00988)	(0.977)	(0.0151)
Observations	16,930	16,930	16,930	16,930	12,565	16,930
DV Mean CG	0.241	0.365	0.749	0.086	219.880	0.828
DV St. Dev. CG	0.428	0.481	0.433	0.281	20.477	0.377
	School / Town-level Covariates					
Outcome	Spending Per Pupil	Pupil Teacher Ratio	6th Grade Average Math Score	6th Grade Average Reading Score	Math % Proficient	Reading % Proficient
Offer	123.2	-0.0509	0.316	0.393	-0.172	-0.128
	(147.8)	(0.0594)	(0.393)	(0.380)	(0.121)	(0.112)
Observations	2,173	16,804	16,689	16,689	16,293	16,292
DV Mean CG	15,768	13.76	241.94	239.72	68.87	66.08
DV St. Dev. CG	2,739	2.42	18.51	17.17	15.67	15.39

Notes: Table presents balancing tests for the full sample of all applications from 8th graders from 2006-2013. Estimates are from a RD specification using local linear regression and a 10 point bandwidth. Top panel presents balancing tests for individual-level covariates. Columns 1-4 of the bottom panel present balancing tests for spending per-pupil, student-teacher ratio and 6th grade average test scores for sending middle schools. Columns 5 and 6 present balancing tests for sending town % proficient in math and reading. Spending per-pupil is for sending middle schools in 2017, pupil teacher ratio, average 6th grade average math and reading scores, and Math and Reading % Proficient are for 2006 - 2013. All specifications other than spending per-pupil include CTHSS school-by-year fixed effects and resident town fixed effects. Spending per pupil specification omits town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: 2SLS Estimates (Bandwidth 10) with Controls

	(1)	(2)	(3)	(4)	(5)
Outcome	Grad	Sem Col	Total Earnings	Quarterly Earnings	Quarters with Earnings
			Full Sample		
Attend	0.0642**	-0.397**	0.241***	0.184***	0.620**
	(0.0249)	(0.161)	(0.0814)	(0.0581)	(0.299)
Observations	16,925	16,930	9,981	9,981	9,981
Mean CG	0.833	2.411	10.399	8.208	11.137
St. Dev. CG	0.304	3.234	1.261	0.733	6.747
			Male Students		
Attend	0.0996***	-0.476**	0.441***	0.323***	1.138**
	(0.0328)	(0.195)	(0.0921)	(0.0640)	(0.498)
Observations	9,284	9,287	5,652	5,652	5,652
Mean CG	0.806	2.141	10.329	8.202	10.613
St. Dev. CG	0.292	3.002	1.284	0.750	6.764
			Female Students		
Attend	0.0123	-0.289	0.0238	0.0385	0.0167
	(0.0397)	(0.293)	(0.125)	(0.0771)	(0.524)
Observations	7,627	7,629	4,315	4,315	4,315
Mean CG	0.872	2.807	10.417	8.150	11.749
St. Dev. CG	0.319	3.481	1.197	0.654	6.722

Notes: Table presents 2SLS estimates for main outcomes. High school graduation and college attendance results are based on all applications from 8th graders from 2006-2013. Logarithm of total and average earnings and quarters with earnings results are based on applications from 8th graders from 2006-2011 who matched in at least one quarter to the labor market data. Mean CG is the control compiler mean of the dependent variable and is defined as E[Y|Offer=1, Attend=1] - Treatment Estimate. St. Dev. CG is the standard deviation of Mean CG and is defined as St. Dev of E[Y|Offer=1, Attend=1]. All estimates are based on a RD specification using local linear regression and a 10-point bandwidth. Controls include full set of controls listed in Table 3. All specifications include CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Falsification Test - Reduced Form (Bandwidth 10) Male Students

	(1)	(2)	(3)	(4)	(5)
Outcome	Grad	Sem Col	Total Earnings	Quarterly Earnings	Quarters with Earnings
			Reduced Form Estimates		
Offer	0.0581***	-0.278**	0.257***	0.188***	0.663**
	(0.0191)	(0.114)	(0.054)	(0.037)	(0.290)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	9,284	9,287	5,652	5,652	5,652
			Cutoff -10 Points		
Offer	0.0178	-0.103	0.0337	0.00197	0.0539
	(0.0242)	(0.117)	(0.0743)	(0.0501)	(0.284)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	5,884	5,885	3,571	3,571	3,571
			Cutoff + 10 Points		
Offer	0.0132	-0.198	0.0891	0.0543	0.194
	(0.0116)	(0.120)	(0.0552)	(0.0340)	(0.208)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	11,391	11,395	6,724	6,724	6,724
			Cutoff + 15 Points		
Offer	-0.00274	-0.0370	-0.0188	-0.00804	-0.00261
	(0.00965)	(0.125)	(0.0611)	(0.0370)	(0.229)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	10,050	10,053	5,916	5,916	5,916
			Cutoff + 20 Points		
Offer	0.00346	0.205	-0.0408	-0.0168	-0.274
	(0.00823)	(0.130)	(0.0679)	(0.0381)	(0.261)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	8,878	8,880	5,238	5,238	5,238

Notes: Table presents reduced-form RD falsification tests for main outcomes based on pseudo cutoffs where we move the actual cutoff threshold: 1) down 10 points, 2) up 10 points, 3) up 15 points, and 4) up 20 points. High school graduation and college attendance results are based on all male applications from 8th graders from 2006-2013 (omitting IEP students and students not observed in 9th grade). Logarithm of total and average earnings and quarters with earnings results are based on male applications from 8th graders from 2006-2011 who matched in at least one quarter to the labor market data. All specifications include the full set of controls listed in Table 3 and CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 7: Mechanism - 2SLS (BW 10) Male Students

	(1)	(2)	(3)	(4)
Outcome	Average Quarterly Earnings 22 or Younger	Average Quarterly Earnings 23 or Older	Semesters College 22 or Younger	Semesters College 23 or Older
Attend	0.433***	0.328***	-0.544***	-0.296
	(0.0739)	(0.0958)	(0.203)	(0.322)
Observations	5,446	3,612	9,287	5,375
Mean CG	7.963	8.409	1.068	1.822
St. Dev. CG	0.803	0.781	2.873	3.519
Outcome	Average Quarterly Earnings	Quarterly Earnings	Quarterly Earnings	Quarterly Earnings
Attend	0.323***	0.311***	0.241***	0.223***
	(0.0640)	(0.0621)	(0.0558)	(0.0531)
Industry Fixed Effects	No	No	2-digit	3-digit
Observations	5,652	63,705	63,602	63,602
Mean CG	8.202	8.317	8.388	8.406
St. Dev. CG	0.750	0.923	0.922	0.922
Outcome	9th Grade Attendance	10th Grade Composite Test Scores	10th Grade Math Score	10th Grade Reading Score
Attend	0.0172***	0.177***	0.127**	0.161**
	(0.00529)	(0.0474)	(0.0609)	(0.0759)
Observations	9,287	6,257	6,313	6,320
Mean CG	0.939	-0.052	0.099	-0.151
St. Dev. CG	0.052	0.723	0.723	0.812

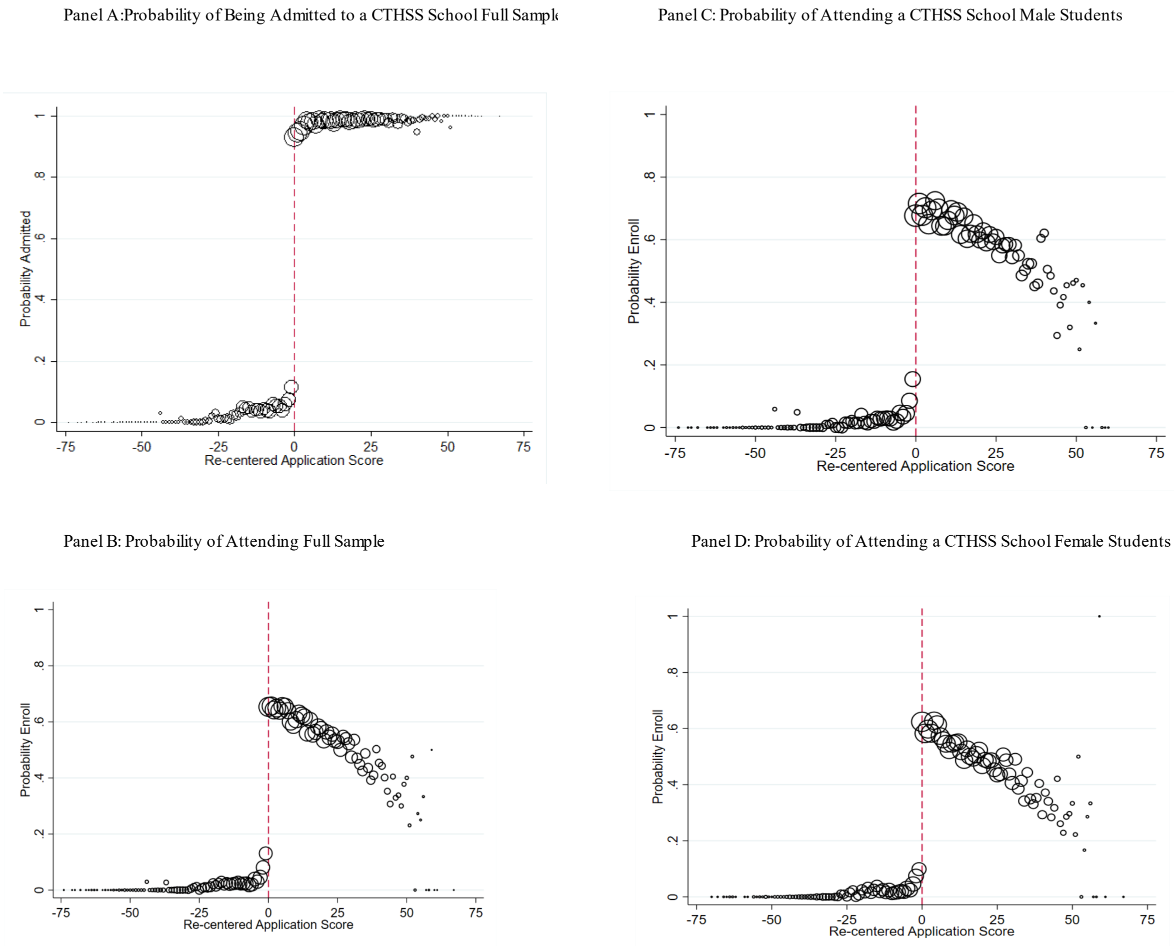
Notes: Table presents 2SLS RD estimates. High school graduation and college attendance results are based on male applications from 8th graders from 2006-2013. Logarithm of total and average earnings and quarters with earnings results are based on male applications from 8th graders from 2006-2011 who matched in at least one quarter to the labor market data. All estimates are based on local linear regression and a 10 point bandwidth. Column 1 of the top panel presents quarterly earnings estimates restricting the sample to observations of earnings 6 quarters after expected high school graduation to the quarter prior to turning 23 while column 2 restricts the sample to observations of earnings from the quarter the individual turns 23 until the end of the sample. Columns 3 and 4 present semesters of enrollment in college up through age 22 (column 3) and semesters of enrollment in college after age 23 for the restricted sample of individuals observed at age 23 or older (column 4). The second panel presents quarterly earnings estimates based on disaggregated quarterly earnings data. Column 1 replicates the average quarterly earnings estimates from Table 5. Columns 2, 3 and 4 of the second panel also include year and quarter fixed effects while column 3 adds two-digit industry fixed effects and column 4 adds three digit industry fixed effects. The last panel presents RD estimates for 9th grade days of attendance, standardized individual and composite 10th grade reading and math test scores. Mean CG is the control compiler mean of the dependent variable and is defined as E[Y|Offer=1, Attend=1] - Treatment Estimate. St. Dev. CG is the standard deviation of Mean CG and is defined as St. Dev of E[Y|Offer=1, Attend=1]. All specifications include the full set of controls listed in Table 3 and CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-application year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Counterfactual CTE 2SLS (BW 10) Male Students

Outcome	Graduation	Sem Col	Quarterly Earnings	Quarters with Earnings
	(1)	(2)	(3)	(4)
A. All CTE Courses				
Attend	0.108***	-0.447**	0.350***	1.280**
	(0.0355)	(0.183)	(0.0671)	(0.541)
Attend*CTE	-0.00958	0.382**	-0.171***	0.0628
	(0.0386)	(0.186)	(0.0580)	(0.517)
Controls	Yes	Yes	Yes	Yes
Observations	9,180	9,183	5,576	5,576
B. Trade Courses				
Attend	0.0994***	-0.418**	0.342***	1.327**
	(0.0340)	(0.202)	(0.0626)	(0.513)
Attend*Trade	-0.0717*	0.00496	-0.161**	-0.101
	(0.0363)	(0.206)	(0.0620)	(0.625)
Controls	Yes	Yes	Yes	Yes
Observations	9,180	9,183	5,576	5,576
C. Human Services/Hospitality				
Attend	0.106***	-0.469**	0.334***	1.293**
	(0.0343)	(0.200)	(0.0699)	(0.534)
Attend*HS/TH	0.00210	-0.239	0.0292	0.0320
	(0.0309)	(0.206)	(0.0723)	(0.470)
Controls	Yes	Yes	Yes	Yes
Observations	9,180	9,183	5,576	5,576

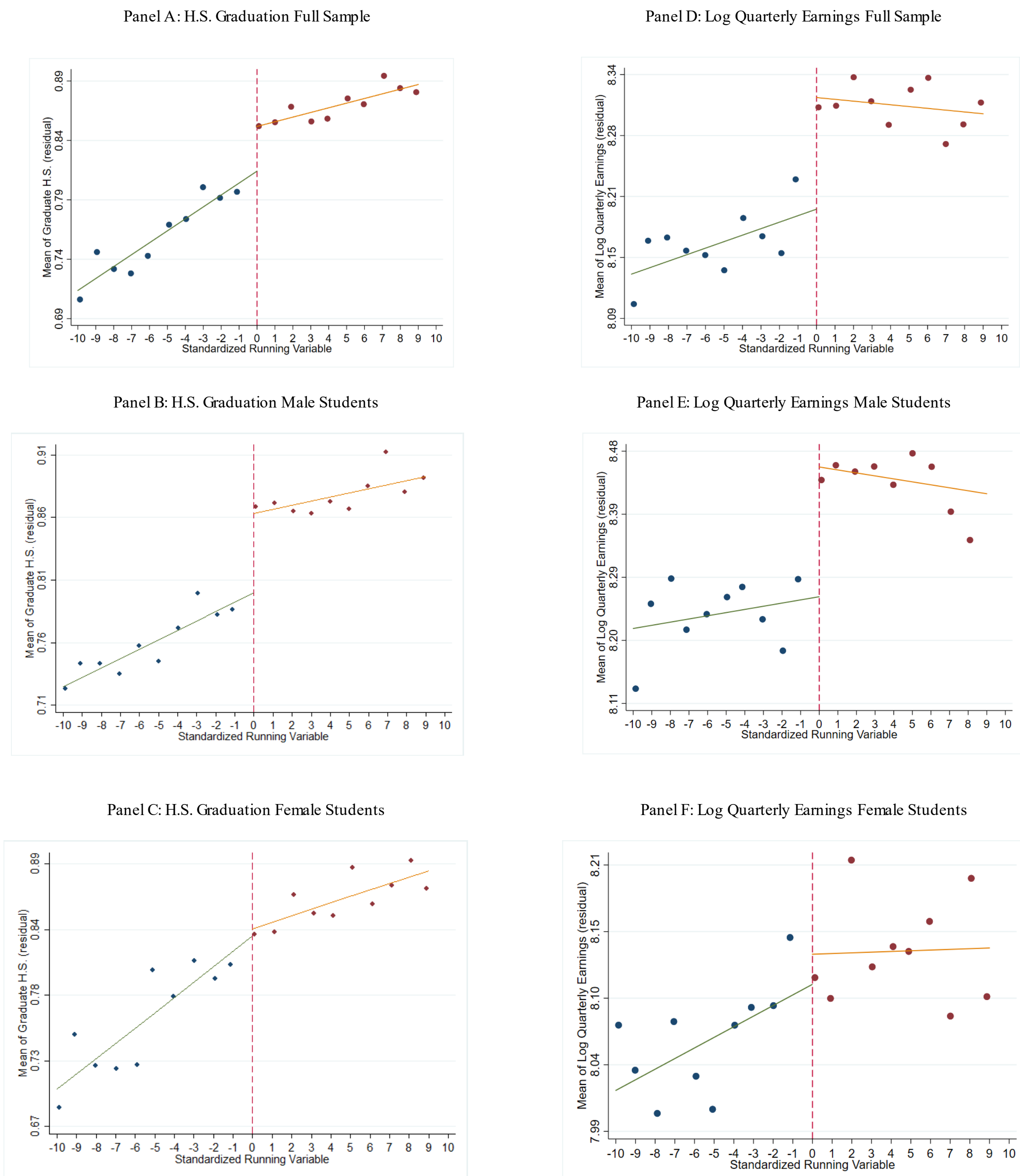
Notes: Table presents 2SLS RD estimates for main outcomes. High school graduation and college attendance results are based on all male applications from 8th graders from 2006-2013. Logarithm of total and average earnings and quarters with earnings results are based on male applications from 8th graders from 2006-2011 who matched in at least one quarter to the labor market data. Panel A interacts the attend a CTE school indicator with the share of elective courses that are CTE in a student's resident school district. Panel B interacts the attend indicator with the share of elective courses that are trade courses (architecture, transportation and manufacturing) in a student's resident school district while Panel C interacts the attend indicator with the share of elective courses that are human service, tourism and hospitality or Family & Consumer Sciences. All specifications include the full set of controls listed in Table 3, CTHSS school-by-year fixed effects and resident town fixed effects and interactions between the relevant share of elective courses and the running variable and the running variable interacted with offer. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 1: First Stage Discontinuity Plots



Notes: The scores forming the horizontal axis have been re-centered by subtracting the threshold for each school and year from the scores associated with the applicants from those schools and years. These figures document the share of students admitted to or enrolled for each discrete application score where the size of the circle indicates the relative number of applications at each score. The figures are based on all applications from 8th graders from 2006-2013 (omitting IEP students and students not observed in 9th grade). Panel A shows the results for admission, panel B shows the results for acceptance, and panels C and D show the results separately for the male and female subsamples.

Figure 2: Reduced Form Graphs H.S. Graduation and Attend College



Notes: The scores forming the horizontal axis have been re-centered by subtracting the threshold for each school and year from the scores associated with the applicants from those schools and years. These figures document the share of students graduating from high school and the average of the logarithm of average quarterly earnings for each score value. High school graduation share is calculated based on all applications from 8th graders from 2006-2013 (omitting IEP students and students not observed in 9th grade). Earnings are based on applications from 8th graders from 2006-2011 who matched in at least one quarter to the labor market data.

Technical Appendix

The Effects of Career and Technical Education: Evidence from the Connecticut Technical High School System

Eric Brunner, Shaun M. Dougherty, Stephen Ross

Empirical Appendix (Online Only)

Table A1 Gender Composition of Programs in CTHSS

CTE Program	Female	Male	Percent Male
Automated Manufacturing	25	29	53.7%
Automotive Technology	160	775	82.9%
Bioscience Environmental Technology	63	28	30.8%
Biotechnology	27	21	43.8%
Carpentry	236	610	72.1%
Collision, Repair and Refinishing	115	248	68.3%
Criminal Justice and Protective Services	7	5	41.7%
Culinary Arts	672	352	34.4%
Culinary Arts and Guest Services	21	14	40.0%
Diesel and Heavy Duty Equipment Repair	11	40	78.4%
Digital Media	29	48	62.3%
Early Care And Education	26	1	3.7%
Electrical	162	799	83.1%
Electronics Technology	62	226	78.5%
Facilities Management	0	16	100.0%
Graphic Technology	218	131	37.5%
Hairdressing and Cosmetology	820	45	5.2%
Health Technologies	570	75	11.6%
Heating, Ventilation and Air Conditioning	53	391	88.1%
Hotel Hospitality Technology	6	0	0.0%
Information Systems Technology	102	440	81.2%
Marketing, Management and Entrepreneurship	44	27	38.0%
Masonry	67	132	66.3%
Mechanical Design & Engineering Technology	146	359	71.1%
Mechatronics	21	48	69.6%
Plumbing and Heating	99	577	85.4%
Plumbing, Heating, and Cooling	3	38	92.7%
Precision Machining Technology	192	593	75.5%
Pre-Electrical Engineering and Applied Electronics	14	33	70.2%
Sound Production	9	23	71.9%
Sustainable Architecture	95	85	47.2%
Tourism, Hospitality and Guest Services Management	27	3	10.0%
Welding And Metal Fabrication	11	56	83.6%

Notes: Data are courtesy of CTHSS central office. Breakdown is districtwide and represents enrollment in grades 9 through 12 during the 2018-2019 school year.

Table A2 Application Score Components

Year	Total Score	Language Arts	Mathematics	Grades	Attendance	Leadership	Pers. Statemnt
2006							
Max Score (Weight)	100	20 (0.20)	20 (0.20)	40 (0.40)	20 (0.20)	0 (0.00)	0 (0.00)
2007							
Max Score (Weight)	100	21 (0.21)	21 (0.21)	48 (0.48)	10 (0.10)	0 (0.00)	0 (0.00)
2008							
Max Score (Weight)	100	21 (0.21)	21 (0.21)	48 (0.48)	10 (0.10)	0 (0.00)	0 (0.00)
2009							
Max Score (Weight)	106	21 (0.21)	21 (0.21)	48 (0.48)	10 (0.10)	3 (0.03)	3 (0.03)
2010							
Max Score (Weight)	106	21 (0.20)	21 (0.20)	48 (0.45)	10 (0.09)	3 (0.03)	3 (0.03)
2011							
Max Score (Weight)	120	21 (0.18)	21 (0.18)	48 (0.40)	10 (0.08)	10 (0.08)	10 (0.08)
2012							
Max Score (Weight)	120	21 (0.18)	21 (0.18)	48 (0.40)	10 (0.08)	10 (0.08)	10 (0.08)
2013							
Max Score (Weight)	120	21 (0.18)	21 (0.18)	48 (0.40)	10 (0.08)	10 (0.08)	10 (0.08)

Notes: Table presents overall admission score points and points associated with each component of the admission score for each application year in our sample. Numbers in parentheses represent the weight attached to each component when calculating the total application score.

Table A3: Labor Market Match Rate by Quarters Male Students

	Observed in Labor Market	
Quarters Count	No	Yes
1	53.57	46.43
2	42.05	57.95
3	40.32	59.68
4	40.80	59.20
5	42.42	57.58
6	38.22	61.78
7	37.26	62.74
8	38.89	61.11
9	40.58	59.42
10	37.86	62.14
11	37.26	62.74
12	37.80	62.20
13	40.06	59.94
14	37.38	62.62
15	36.89	63.11
16	37.22	62.78
17	39.50	60.50
18	37.79	62.21
19	37.83	62.17
20	38.25	61.75
21	41.37	58.63
22	39.19	60.81
23	39.23	60.77
24	39.43	60.57
25	41.67	58.33
Total	40.42	59.58

Notes: Table presents the fraction of the sample of male applicants observed in the labor market in a given quarter where quarters are enumerated starting in the first quarter of the calendar year five years after starting high school.

Table A4: First Stage Estimates Labor Market (BW 10)

Outcome	Probability of Attending Full Sample		Probability of Attending Men		Probability of Attending Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Offer	0.576***	0.577***	0.597***	0.598***	0.550***	0.551***
	(0.0233)	(0.0236)	(0.0232)	(0.0234)	(0.0246)	(0.0253)
Controls	No	Yes	No	Yes	No	Yes
F	692	691	661	653	498	475
Observations	9,981	9,981	5,652	5,652	4,315	4,315

Notes: Table presents first-stage estimates based on sample of students in labor market. Estimates are for probability of attending a CTHSS school after receiving an offer where dependent variable is an indicator for attendance at a CTHSS school in 9th grade. Columns 1 and 2 are for full sample of male and female students in labor market. Columns 3 and 4 are for sample of male students in labor market. Columns 5 and 6 are for sample of female students in labor market. All specifications include CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5: School-by-Year Running Variable Density Test P-Value All Applicants Sample

	Application Year							
School	2006	2007	2008	2009	2010	2011	2012	2013
911	0.314	0.020	0.000	0.140	0.002	0.031	0.063	0.000
912	0.035	0.002	0.324	0.700	0.622	0.444	0.691	0.820
913	0.633	0.194	0.509	0.603	0.772	0.520	0.475	0.116
914	0.212	0.001	0.000	0.083	0.003	0.189	0.045	0.860
915	0.338	0.009	0.128	0.680	0.575	0.024	0.020	0.003
916	0.285	0.487	0.635	0.403	0.760	0.092	0.858	0.372
917	0.011	0.189	0.855	0.785	0.719	0.097	0.095	0.303
918	0.465	0.000	0.006	0.024	0.332	0.634	0.829	0.162
919	0.970	0.038	0.000	0.537	0.001	0.082	0.000	0.022
920	0.204	0.271	0.074	0.392	0.086	0.109	0.323	0.924
922	0.354	0.324	0.432	0.320	0.728	0.200	0.034	0.008
923	0.015	0.421	0.900	0.258	0.485	0.017	0.370	0.080
924	0.071	0.703	0.924	0.461	0.411	0.835	0.452	0.960
925	0.809	0.015	0.604	0.590	0.007	0.014	0.938	0.205
926	0.029	0.232	0.013	0.000	0.001	0.000	0.000	0.517
927	0.706	0.867	0.137	0.225	0.042	0.200	0.810	0.376

Notes: Table presents *p*-value associated with a McCrary test for manipulation for each school and application year. Shaded cells represent cases where we fail the McCrary test at the 10 percent significance level or below.

Table A6: Balancing Tests Male Students

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual-level Covariates					
Outcome	Black	Hispanic	Free Lunch	English Learner	7th Grade Test Scores	In Labor Market Sample
Offer	-0.00503	-0.00612	-0.00283	0.0158	-0.821	-0.00528
	(0.0128)	(0.0109)	(0.0154)	(0.0125)	(0.846)	(0.0169)
Observations	9,287	9,287	9,287	9,287	6,861	9,287
DV Mean CG	0.214	0.319	0.689	0.073	220.97	0.841
DV St. Dev. CG	0.410	0.466	0.463	0.260	20.97	0.366
	School / Town-level Covariates					
Outcome	Spending Per Pupil	Pupil Teacher Ratio	6th Grade Average Math Score	6th Grade Average Reading Score	Math % Proficient	Reading % Proficient
Offer	148.4	-0.000553	0.505	0.674	-0.180	-0.192
	(172.4)	(0.0779)	(0.492)	(0.449)	(0.141)	(0.127)
Observations	1,106	9,222	9,123	9,123	8,913	8,913
DV Mean CG	15,826	13.64	244.03	241.59	70.84	67.87
DV St. Dev. CG	2,611	2.34	19.19	17.77	15.96	15.74

Notes: Table presents balancing tests for the sample of male students. Estimates are from a RD specification using local linear regression and a 10-point bandwidth. Top panel presents balancing tests for individual-level covariates. Columns 1-4 of the bottom panel present balancing tests for spending per-pupil, pupil-teacher ratio and 6th grade average test scores for sending middle schools. Columns 5 and 6 present balancing tests for sending town % proficient in math and reading. Spending per-pupil is for sending middle schools in 2017, pupil teacher ratio, 6th grade average math and reading scores, and Math and Reading % Proficient are for 2006 - 2013. All specifications other than spending per-pupil include CTHSS school-by-year fixed effects and resident town fixed effects. Spending per pupil specification omits town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Balancing Tests Female Students

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual-level Covariates					
Outcome	Black	Hispanic	Free Lunch	English Learner	7th Grade Test Scores	In Labor Market Sample
Offer	-0.0155	0.00126	-0.0178	-0.00358	0.431	-0.00976
	(0.0209)	(0.0211)	(0.0157)	(0.0187)	(1.749)	(0.0196)
Observations	7,629	7,629	7,629	7,629	5,684	7,629
DV Mean CG	0.276	0.420	0.824	0.103	218.58	0.813
DV St. Dev. CG	0.447	0.494	0.381	0.304	19.80	0.390
	School / Town-level Covariates					
Outcome	Spending Per Pupil	Pupil Teacher Ratio	6th Grade Average Math Score	6th Grade Average Reading Score	Math % Proficient	Reading % Proficient
Offer	56.09	-0.104	-0.148	-0.0360	-0.0471	-0.0443
	(220.5)	(0.134)	(0.169)	(0.173)	(0.552)	(0.497)
Observations	1,038	7,567	7,370	7,368	7,555	7,555
DV Mean CG	15,705	13.91	239.42	237.46	66.49	63.92
DV St. Dev. CG	2,875	2.51	17.31	16.15	14.98	14.66

Notes: Table presents balancing tests for the sample of female students. Estimates are from a RD specification using local linear regression and a 10 point bandwidth. Top panel presents balancing tests for individual-level covariates. Columns 1-4 of the bottom panel present balancing tests for spending per-pupil, pupil-teacher ratio and 6th grade average test scores for sending middle schools. Columns 5 and 6 present balancing tests for sending town % proficient in math and reading. Spending per-pupil is for sending middle schools in 2017, pupil teacher ratio, 6th grade average math and reading scores, and Math and Reading % Proficient are for 2006 - 2013. All specifications other than spending per-pupil include CTHSS school-by-year fixed effects and resident town fixed effects. Spending per pupil specification omits town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Covariate Balancing Tests Male Students Labor Market Sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual-level Covariates					
Outcome	Black	Hispanic	Free Lunch	English Learner	7th Grade Test Scores	
Offer	-0.00726	-0.00193	0.0203	0.0141	0.201	
	(0.0168)	(0.0225)	(0.0218)	(0.0222)	(1.210)	
Observations	5,652	5,652	5,652	5,652	3,623	
	School / Town-level Covariates					
Outcome	Spending Per Pupil	Pupil Teacher Ratio	6th Grade Average Math Score	6th Grade Average Reading Score	Math % Proficient	Reading % Proficient
Offer	-33.40	0.00402	0.458	0.910*	0.106	0.0462
	(244.3)	(0.100)	(0.584)	(0.534)	(0.167)	(0.144)
Observations	793	5,612	5,566	5,566	5,444	5,444

Notes: Table presents balancing tests for sample of male students observed in the labor market. Estimates are from a RD specification using local linear regression and a 10 point bandwidth. Top panel presents balancing tests for individual-level covariates. Columns 1-4 of the bottom panel present balancing tests for spending per-pupil, pupil-teacher ratio and 6th grade average math and reading test scores for sending middle schools. Columns 5 and 6 present balancing tests for sending town % proficient in math and reading. Spending per-pupil is for sending middle schools in 2017, pupil teacher ratio, 6th grade average math and reading scores, and Math and Reading % Proficient are for 2006 - 2013. All specifications other than spending per-pupil include CTHSS school-by-year fixed effects and resident town fixed effects. Spending per pupil specification omits town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Covariate Balancing Tests Male Students Alternative Bandwidths

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual-level Covariates					
Outcome	Black	Hispanic	Free Lunch	English Learner	7th Grade Test Scores	In Labor Market Sample
	Bandwidth 6					
Offer	-0.0135	-0.00774	0.00435	0.0175	-1.681	-0.0234
	(0.0168)	(0.0192)	(0.0231)	(0.0137)	(1.164)	(0.0246)
Observations	5,801	5,801	5,801	5,801	4,246	5,801
	Bandwidth 8					
Offer	-0.0186	-0.00654	-0.000910	0.0173	-1.443	-0.0147
	(0.0134)	(0.0109)	(0.0153)	(0.0157)	(1.106)	(0.0224)
Observations	7,629	7,629	7,629	7,629	5,615	7,629
	School / Town-level Covariates					
Outcome	Spending Per Pupil	Pupil Teacher Ratio	6th Grade Average Math Score	6th Grade Average Reading Score	Math % Proficient	Reading % Proficient
	Bandwidth 6					
Offer	93.51	0.0857	0.884	1.116*	-0.0597	-0.162
	(208.6)	(0.0850)	(0.655)	(0.647)	(0.202)	(0.191)
Observations	682	5,765	5,708	5,708	5,575	5,575
	Bandwidth 8					
Offer	82.42	0.0521	0.585	0.813	-0.159	-0.222
	(218.6)	(0.0735)	(0.561)	(0.514)	(0.175)	(0.150)
Observations	905	7,582	7,505	7,505	7,337	7,337

Notes: Table presents balancing tests for sample of male students. Estimates are from a RD specification using local linear regression and the bandwidth listed at the top of each panel. Top two panels present balancing tests for individual-level covariates. Bottom two panels present balancing tests for school covariates. All specifications other than spending per-pupil include CTHSS school-by-year fixed effects and resident town fixed effects. Spending per pupil specification omits town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Covariate Balancing Tests No Donut Hole Male Students

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual-level Covariates					
Outcome	Black	Hispanic	Free Lunch	English Learner	7th Grade Test Scores	In Labor Market Sample
Offer	0.00231	-0.0111	-0.0103	0.0158	-0.958	-0.00538
	(0.0126)	(0.00824)	(0.0159)	(0.00993)	(0.814)	(0.0174)
Observations	10,039	10,039	10,039	10,039	7,426	10,039
	School / Town-level Covariates					
Outcome	Spending Per Pupil	Pupil Teacher Ratio	6th Grade Average Math Score	6th Grade Average Reading Score	Math % Proficient	Reading % Proficient
Offer	224.8	0.0365	0.429	0.559	-0.134	-0.148
	(148.4)	(0.0831)	(0.469)	(0.420)	(0.129)	(0.122)
Observations	1,189	9,966	9,867	9,867	9,646	9,646

Notes: Table presents balancing tests for the sample of male students. Estimates are from a RD specification using local linear regression and a 10 point bandwidth with no donut hole. Top panel presents balancing tests for individual-level covariates. Columns 1-4 of the bottom panel present balancing tests for spending per-pupil, pupil-teacher ratio and 6th grade average test scores for sending middle schools. Columns 5 and 6 present balancing tests for sending town % proficient in math and reading. Spending per-pupil is for sending middle schools in 2017, pupil teacher ratio, 6th grade average math and reading scores, and Math and Reading % Proficient are for 2006 - 2013. All specifications other than spending per-pupil include CTHSS school-by-year fixed effects and resident town fixed effects. Spending per pupil specification omits town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Falsification Test - First Stage (Bandwidth 10) Male Students

Outcome	(1) Cutoff -10	(2) Cutoff +10	(3) Cutoff +15	(4) Cutoff +20
A. Full Sample				
Offer	-0.00508	0.0173	0.00524	0.0220
	(0.00687)	(0.0195)	(0.0230)	(0.0188)
Controls	Yes	Yes	Yes	Yes
Observations	5,885	11,395	10,053	8,880
B. Labor Market Sample				
Offer	-0.000796	0.0111	0.0130	0.0311
	(0.00578)	(0.0245)	(0.0281)	(0.0269)
Controls	Yes	Yes	Yes	Yes
Observations	3,571	6,724	5,916	5,238

Notes: Table presents first-stage RD falsification tests based on pseudo cutoffs where we move the actual cutoff threshold: 1) down 10 points, 2) up 10 points, 3) up 15 points, and 4) up 20 points. Panel A is for the full set of male students that applied to a CTHSS school. Panel B is for sample of male students in labor market. All specifications include the full set of controls listed in Table 3 and CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A12: 2SLS Estimates with No Balancing Test Covariates (Bandwidth 10)

	(1)	(2)	(3)	(4)	(5)
Outcome	Grad	Sem Col	Total Earnings	Quarterly Earnings	Quarters with Earnings
			Full Sample		
Attend	0.0647**	-0.427***	0.248***	0.195***	0.595*
	(0.0248)	(0.163)	(0.0812)	(0.0584)	(0.302)
Controls	No	No	No	No	No
Observations	16,925	16,930	9,981	9,981	9,981
			Male Students		
Attend	0.100***	-0.479**	0.443***	0.326***	1.120**
	(0.0332)	(0.190)	(0.0933)	(0.0671)	(0.492)
Controls	No	No	No	No	No
Observations	9,284	9,287	5,652	5,652	5,652
			Female Students		
Attend	0.0125	-0.297	0.0249	0.0405	0.0147
	(0.0397)	(0.295)	(0.123)	(0.0754)	(0.531)
Controls	No	No	No	No	No
Observations	7,627	7,629	4,315	4,315	4,315

Notes: Table presents 2SLS estimates for main outcomes. All specification omit the full set of control variables. Top panel presents estimates for combined sample of male and female students. Middle panel presents estimates for sample of male students. Bottom panel presents estimates for sample of female students. All estimates are based on a RD specification using local linear regression and a 10-point bandwidth. All specifications include CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A13: First Stage and Reduced Form - Alternative Bandwidths Male Students

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Attend (First Stage)	Grad	Sem Col	Total Earnings	Quarterly Earnings	Quarters with Earnings
-	-			Bandwidth 6		
Offer	0.523***	0.0588**	-0.313**	0.318***	0.204***	1.232**
	(0.0298)	(0.0256)	(0.142)	(0.0839)	(0.0489)	(0.517)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,801	5,800	5,801	3,524	3,524	3,524
				Bandwidth 8		
Offer	0.565***	0.0553***	-0.272***	0.360***	0.246***	0.978**
	(0.0261)	(0.0200)	(0.100)	(0.0807)	(0.0450)	(0.436)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,629	7,626	7,629	4,631	4,631	4,631
				Bandwidth 10		
Offer	0.583***	0.0580***	-0.278**	0.264***	0.193***	0.681**
	(0.0229)	(0.0183)	(0.116)	(0.0561)	(0.0371)	(0.305)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,287	9,284	9,287	5,652	5,652	5,652

Notes: Table presents first-stage and reduced form estimates for the main outcomes based on various bandwidths. Sample is limited to male students. All specifications include the full set of controls listed in Table 3 and CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A14: Additional Robustness Tests Male Sample 2SLS Estimates (Bandwidth 10)

	(1)	(2)	(3)	(4)	(5)
Outcome	Grad	Sem Col	Total Earnings	Quarterly Earnings	Quarters with Earnings
			No Donut Hole		
Attend	0.108***	-0.538**	0.436***	0.320***	0.935**
	(0.0325)	(0.213)	(0.0900)	(0.0599)	(0.460)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	10,036	10,039	6,142	6,142	6,142
			Dropping School-Years that Fail Density Test for Manipulation		
Attend	0.132***	-0.456	0.436***	0.339***	1.006*
	(0.0417)	(0.344)	(0.122)	(0.0832)	(0.578)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	5,351	5,354	3,337	3,337	3,337
			Dropping 2010 and Later Cohorts		
Attend	0.104**	-0.271	0.518***	0.337***	1.740**
	(0.0417)	(0.387)	(0.124)	(0.0714)	(0.710)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	4,515	4,515	3,846	3,846	3,846
			Dropping Students that Applied to More than One School		
Attend	0.0994**	-0.390	0.328***	0.261***	0.882
	(0.0382)	(0.295)	(0.114)	(0.0752)	(0.562)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	5,864	5,865	3,849	3,849	3,849

Notes: Table presents 2SLS estimates for main outcomes based on sample of male students. All estimates are based on a RD specification using local linear regression and a 10-point bandwidth. All specifications include full set of controls and CTHSS school-by-year fixed effects and resident town fixed effects. Panel 1 presents estimates based on the full sample of male students and no donut hole. Panel 2 presents estimates based on specifications where the sample is restricted by dropping any school-year observations that fail a density test for manipulation at a significance level of 0.10 or lower. Panel 3 presents estimates based on specifications where the sample is restricted to application years 2006 - 2009. Panel 4 presents estimates based on specifications where the sample is restricted to include students that only applied to one CTHSS school. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A15: Labor Market Participation Rate for Boundary Versus Non-Boundary Towns Male Students

Towns		Observed in Labor Market No	Yes
New York	Border	46.53	53.47
	Adjacent	41.56	58.44
	Interior	40.12	59.88
Total		40.45	59.55

		Observed in Labor Market No	Yes
Massachusetts	Border	47.26	52.74
	Adjacent	38.92	61.08
	Interior	40.12	59.88
Total		40.16	59.84

		Observed in Labor Market No	Yes
Rhode Island	Border	46.08	53.92
	Adjacent	39.17	60.83
	Interior	40.12	59.88
Total		40.29	59.71

Notes: Table presents the fraction of individuals ever observed in the labor market data by whether the town lies on a state border, is adjacent to a border town, or is an interior town. Panels 1, 2 and 3 present the results for the border between Connecticut and New York, Massachusetts and Rhode Island, respectively. Towns that are on the border of another state, but not on the border of the subject state for a given panel, are omitted from the calculations.

Table A16: Labor Market Robustness Tests

	(1)	(2)	(3)
	Dropping Students at State Boundary Male Students		
Outcome	Total Earnings	Quarterly Earnings	Quarters with Earnings
Offer	0.458***	0.325***	1.327**
	(0.100)	(0.0675)	(0.518)
Controls	Yes	Yes	Yes
Observations	5,309	5,309	5,309
	Reduced Form Quantile Regression Quarterly Earnings		
Outcome	25th Percentile	50th Percentile	75th Percentile
Offer	0.219***	0.177***	0.177***
	(0.0623)	(0.0545)	(0.0494)
Controls	Yes	Yes	Yes
Observations	5,665	5,665	5,665

Notes: Table presents reduced form quantile regression estimates for quarterly earnings based on sample of male students. All estimates are based on a RD specification using local linear regression and a 10-point bandwidth. All specifications include the full set of controls listed in Table 3, CTHSS school-by-year fixed effects and resident town fixed effects. Robust standard errors, clustered at the school-by-year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A17: Heterogeneity Reduced Form (BW 10) Male Students

Outcome	Graduation	Sem Col	Quarterly Earnings	Quarters with Earnings
	(1)	(2)	(3)	(4)
A. Free Lunch Status				
Offer	0.0559**	-0.297	0.241***	0.982**
	(0.0241)	(0.200)	(0.0658)	(0.495)
Offer*Free Lunch	0.00494	0.0249	-0.0693	-0.492
	(0.0473)	(0.369)	(0.0779)	(0.594)
Controls	No	No	No	No
Observations	9,284	9,287	5,652	5,652
B. Race/Ethnicity				
Offer	0.0703***	-0.316*	0.222***	0.393
	(0.0242)	(0.172)	(0.0587)	(0.392)
Offer*Black/Hispanic	-0.0244	0.0835	-0.0639	0.493
	(0.0363)	(0.226)	(0.0697)	(0.542)
Controls	No	No	No	No
Observations	9,284	9,287	5,652	5,652
C. Central City				
Offer	0.0596**	-0.352**	0.222***	0.347
	(0.0265)	(0.154)	(0.0523)	(0.293)
Offer*Central City	-0.00330	0.181	-0.0721	0.952**
	(0.0389)	(0.222)	(0.0582)	(0.425)
Controls	No	No	No	No
Observations	9,284	9,287	5,652	5,652

Notes: Table presents reduced form RD estimates for main outcomes based on sample of male students. Panel A interacts the offer to attend a CTE school indicator with the indicator for free lunch eligible students. Panel B interacts the offer indicator with an indicator for whether a student is Black or Hispanic. Panel C interacts the offer indicator with an indicator for whether a student resides in one of Connecticut's five central cities. All estimates are based on local linear regression and a 10-point bandwidth. All specifications include CTHSS school-by-year fixed effects and resident town fixed effects and interactions between the relevant characteristic (free lunch status, ethnicity and central city) and the running variable and the running variable interacted with offer. Robust standard errors, clustered at the school-by-year and town levels in parentheses.*** p<0.01, ** p<0.05, * p<0.1

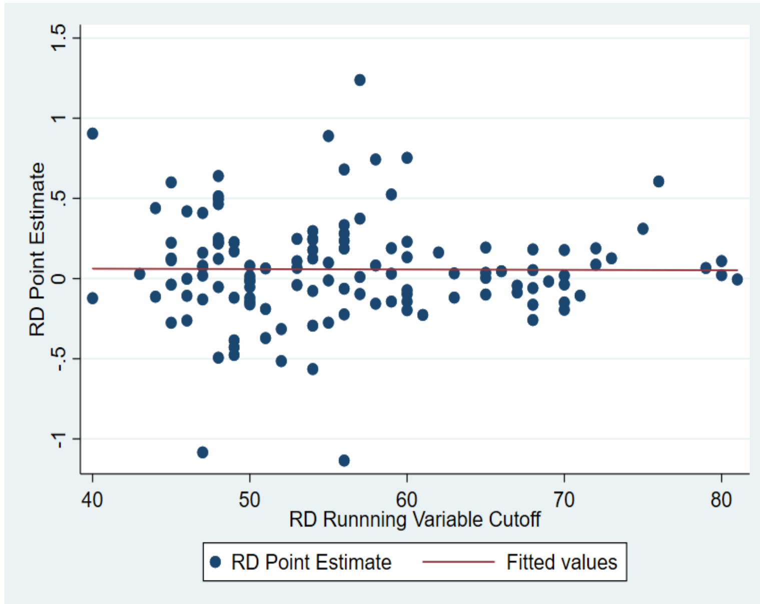
Table A18: Counterfactual CTE 2SLS (BW 10) Male Students

Outcome	Graduation	Sem Col	Quarterly Earnings	Quarters with Earnings
	(1)	(2)	(3)	(4)
A. Spending Per Pupil (SPP)				
Attend	0.103***	-0.461**	0.335***	1.109**
	(0.0341)	(0.208)	(0.0727)	(0.443)
Attend*SPP	0.0196	-0.219	0.0112	-0.690
	(0.0335)	(0.198)	(0.0732)	(0.449)
Controls	Yes	Yes	Yes	Yes
Observations	9,162	9,165	5,564	5,564
B. Pupil Teacher Ratio (PTR)				
Attend	0.0999***	-0.489**	0.332***	1.263***
	(0.0333)	(0.209)	(0.0676)	(0.422)
Attend*PTR	-0.000527	0.0643	0.0101	0.423
	(0.0360)	(0.125)	(0.0579)	(0.430)
Controls	Yes	Yes	Yes	Yes
Observations	9,180	9,183	5,576	5,576
C. Average 10th Grade Math & English Scores				
Attend	0.107***	-0.435**	0.351***	1.203***
	(0.0336)	(0.205)	(0.0688)	(0.443)
Attend*Score	0.0225	-0.0116	0.0694	-0.530
	(0.0302)	(0.155)	(0.0508)	(0.334)
Controls	Yes	Yes	Yes	Yes
Observations	9,180	9,183	5,576	5,576

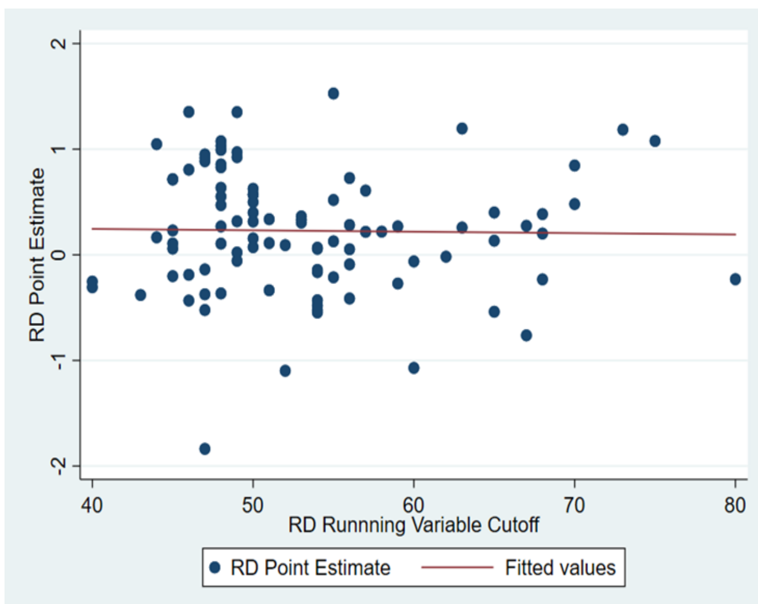
Notes: Table presents 2SLS RD estimates for main outcomes based on sample of male students. Panel A interacts the attend a CTE school indicator with average spending per pupil in a student's residential town high school(s). Panel B interacts the attend indicator with the pupil teacher ratio in a students' residential town high school while Panel C interacts the attend indicator with average standardized math and English 10th grade test scores in a student's residential town high school. All specifications include the full set of controls listed in Table 3, CTHSS school-by-year fixed effects and resident town fixed effects and interactions between either spending per pupil, pupil teacher ratio or average 10th grade math and reading scores and the running variable and the running variable interacted with offer. Robust standard errors, clustered at the school-by-year and town levels in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 1A Individual School & Year RD Point Estimates Versus Cutoffs:

Panel A High School Graduation



Panel B Average Quarterly Earnings



Notes: Figure presents plots of 2SLS estimates separately for each school and year for high school graduation in panel A and logarithm of average quarterly earnings in panel B. High school graduation results are based on all applications from 8th graders from 2006-2013 (omitting IEP students and students not observed in 9th grade). Earnings results are based on applications from 8th graders from 2006-2011 who matched in at least one quarter to the labor market data. All estimates are based on a RD specification using local linear regression and a 10-point bandwidth excluding the controls used in the balancing tests.

B. Methodological Appendix (Online Only)

As discussed in the paper, the Connecticut Technical High School System (CTHSS) admits students based on a discrete score variable that runs between zero and a maximum of between 100 and 120 depending upon the year considered. This score is the sum of individual components based on standardized state level mathematics and language arts test scores, grade point average, attendance, and for 2009 or later, points assigned based on extracurricular activities and a written statement. The weights assigned to each component vary from year to year, and the exact weights are shown in Appendix Table A2.

Two issues arise, however, that complicate our analysis relative to a simple fuzzy regression discontinuity. First, the individual schools never establish a formal cut-off score, and in fact we can find many examples of individuals receiving acceptance letters who have scores below the scores of individuals who did not receive acceptance letters for the same school and application year. Therefore, there are two sources of noise associated with the discontinuity: deviations of school administrators from a high to low score admission/acceptance rule and differences in take-up among accepted applicants; and accordingly the true location of the discontinuity is unknown. The second issue that arises is that the individual score components are both correlated with each other and discrete in nature leading to raw score distributions that have heaps or mass points at specific locations in the distribution, making it impossible to directly identify manipulation at the threshold or boundary using standard McCrary tests.

B1. Empirical Identification of Cut-offs

While receipt of an acceptance letter is not perfectly determined by a student's application score, the mass of the acceptances for each school and year appear concentrated above apparent score thresholds, and this concentration is especially true for the initial batch of acceptance letters sent out by the schools each year. Therefore, we follow Porter and Yu (2015) and select the threshold for each year and school from the empirical distribution of applications.

Porter and Yu (2015) recommend estimating the location of the threshold by selecting the threshold that maximizes the size of the discontinuity, or specifically:

$$\widehat{X^*} = argmax_{X^*}\hat{\alpha}(X^*) \qquad \text{(B1)}$$

where the treatment (T) is defined by

$$T = \begin{cases} 1 & if\ y^* \geq 0 \\ 0 & if\ y^* < 0 \end{cases},\ T^* = \begin{cases} f_1(X) + \varepsilon & if\ X < X^* \\ f_2(X) + \alpha + \varepsilon & if\ X \geq X^* \end{cases},\ f_1(X^*) = f_2(X^*) \qquad \text{(B2)}$$

where $f_1(X)$ and $f_2(X)$ are continuous and differentiable functions. The inclusion of different functions on either side of the threshold allows for differential processes for non-compliance. In practice, we estimate equation (A2) using a linear probability model and specifying $f_1(X)$ and $f_2(X)$ as linear functions of X.

Porter and Yu (2015) estimate $\widehat{X^*}$ using the discontinuity in the outcome in order to develop second stage specification tests for the existence of treatment effects at that unknown discontinuity. However, Porter and Yu's specification tests require both continuous outcomes and a smooth distribution of the population over scores X. Therefore, we take a different approach exploiting fuzzy regression discontinuity approaches. We select the thresholds applying equation (B1) to the actual school decision to send an acceptance letter to an applicant since that is the decision process that creates the discontinuity. Then, with the estimated thresholds in hand, we estimate a two stage least squares model of student outcomes (y) that incorporates the additional noise created by student take-up so that the first stage is whether we see the applicant in the technical high school the year after they applied (A). Specifically, the fuzzy RD takes the following form:

$$y = \beta A + g_1(X) + d(\widehat{X^*} \leq X)g_2(X) + \varepsilon_2 \tag{B3}$$

where A is instrumented by

$$A = \tilde{\alpha} d(\widehat{X^*} \leq X) + h_1(X) + d(\widehat{X^*} > X)h_2(X) + \varepsilon_3 \tag{B4}$$

and $d(\widehat{X^*} \leq X)$ is an indicator function that takes the value one when the condition is satisfied, and both $g_1(\widehat{X^*}) = g_2(\widehat{X^*})$ and $h_1(\widehat{X^*}) = h_2(\widehat{X^*})$.

The disadvantage of this approach is that our analysis is conditional on the estimated discontinuity and the estimated discontinuity may not represent the true discontinuity. However, in a 2SLS context, we only need to establish the power of the instrument and the validity of the exclusion restriction. The issue of whether a discontinuity exists can be determined by examining the power of the instrument. If the estimated threshold has significant power to explain student attendance A then the discontinuity exists, and the second stage estimates and standard errors from 2SLS will be consistent as long as the exclusion restriction is valid. Determining the power of the instrument, however, is not straightforward since the cutoff has been selected using the same data that is then used to estimate the first stage equation. Naturally, treatment (receiving an acceptance letter) affects attendance and so the F-test may be biased upwards providing misleading evidence on instrument power because we selected $\widehat{X^*}$ to maximize the discontinuity for T which is strongly correlated with A. We follow Card, Mas and Rothstein (2008) and address this concern using a hold-out sample.[1]

B1.1 Selection of Cutoffs using Hold-out Samples

In our case, we will divide the applicants in each school and year into equal sized analysis and hold-out samples so that we can use the analysis sample to select the thresholds and use the hold-out sample to estimate the first stage for attendance and examine the power of the instrument. Specifically, we do the following:

1) For each school and year, divide all applicants into deciles, assign each applicant a random number, place applicants in the hold-out sample if they are above the median on the random number and in the analysis sample if below, split median applicants half in hold-out/half in analysis samples, and in the case of an odd number of median applicants assign the last median applicant randomly to either the hold-out or analysis sample.
2) Using our preferred bandwidth of 10 points and linear running variables, estimate equation B1 for each school and year starting with a candidate cutoff score at 10 so that the bottom of the 20-point band is at a score of zero using the analysis sample.
3) Re-estimate these models incrementing the candidate cutoff score by 1 each time and ending 10 points away from the maximum score so that the top of the band at the maximum.
4) Select the cutoff for each school and year as the cutoff that provides the maximum estimate of α.[2]
5) Center the scores for each school and year by subtracting the cutoff and pool all years and schools.
6) Estimate equation (B4) for school attendance and calculate the F-statistic associated with the indicator $d(\widehat{X^*} \leq X)$ using the hold-out sample.

We conduct this hold-out simulation four times, and the resulting F-statistics always fall between 456 and 628 for the full sample and between 458 and 674 for the donut hole sample. We also estimate the first stage models separately for each school and year. The means of the estimated discontinuities over all schools and years range between 0.525 and 0.540 for the four simulations very close to our full sample first stage estimate of 0.582. The fraction of thresholds that are significant at the 10 percent level ranges

[1] Card, Mas and Rothstein (2008) calculate tipping points associated with racial segregation for each metropolitan area using a subset of census tracts, and then analyzed the effects of being above the tipping point using the remaining tracts.
[2] Note that we verify that the cutoffs are never anywhere near the edge of the score range considered.

between 0.795 and 0.843. Further, the average threshold size across years for any school or simulation never falls below 0.329, and the fraction of significant thresholds for any school in a simulation never falls below 0.571. However, while the empirically selected thresholds provide a very strong instrument for explaining school attendance across the sample of CTHSS schools, the magnitude of the estimated threshold varies considerably and systematically across the simulations. The correlation between the average threshold for each school across the simulations is very high ranging between 0.77 and 0.91. We observe less evidence of a systematic difference in thresholds across years with the correlation between the average threshold for each year ranging between -0.10 and 0.56. Appendix Table B1 presents the correlations of the average thresholds by school across the four simulations in panel 1, and by year in panel 2. Appendix Table B2 presents the full set of correlations for one of the simulations as an example.

B2. Examining the Empirical Distribution of Scores

We next conduct a series of analyses first intended to demonstrate that the lumpy nature of the raw distribution arises naturally from the data as opposed to manipulation around the threshold (sections B2.1 and B2.2) and then second to demonstrate how features in the centered score distribution could arise naturally from the application process (section B2.3).

B2.1 The Effect of Dropping Students at Threshold

While the underlying test scores, grade point averages and attendance data are relatively continuous, the system for assigning points divides students into more aggregated bins leading to mass points in the empirical distribution of application scores. The raw distribution of applicant admission scores are shown in the left hand side of Figure B1 separately for 2006 to 2008, 2009-2010 and 2011 to 2014. This division of the data is chosen because prior to 2008 the score did not include points from the written statement and extracurricular activities, and in 2011 those points increased from a total of 6 points to a total of 20 points. The distribution contains significant mass points as well as holes in the distribution, which would typically raise concerns about manipulation at the cutoff. However, the right-hand side of Figure B1 shows the same distributions after dropping students for each school and application year whose score was exactly at the cutoff and dropping those students has at most modest effects on the smoothness of the raw score distribution.

As shown in Figure B1, the elimination of applications who have the same score as the admissions letter threshold selected for their school and year does little to change the irregular nature of the distribution. In order assess how much of the change in the distribution we can explain, we calculate the square root of the mean squared error deviation between the full empirical distribution and the distribution minus the applications at the cutoff for each specific application school and year. These calculations are conducted separately for the periods 2006-2008, 2009-2010 and 2011-2013 as shown in Figure B1. We then conduct a simple simulation that involves the following steps:

1) Select the cutoffs chosen by all schools in a given year as a population of relevant cutoffs for that year in order to take into account the fact that the environment changes from year to year likely changing the location of any mass points.
2) For each school and year, select one cutoff randomly from the population of cutoffs for that year omitting the school's own cutoff for that year.
3) Pool the data across schools and relevant years to obtain a simulated distribution.
4) Delete all applications from the pool for that school and year at the simulated cutoff, and calculate the root mean squared error from the full distribution.
5) Repeat steps 2 through 4 and average the resulting root mean squared errors.

While the simulation cannot completely explain the deviations caused by dropping applications at the cutoff, the simulations explain much of the deviation especially for 2006-2008. Specifically, the mean root squared error from comparing the full raw distribution to the distributions after dropping applications at one of the other school's cutoffs baseline explains 71 percent of the effect of dropping applications at the school's actual cutoff for the 2006-2008 timeframe. This exercise explains 62 and 60 percent of the effect for 2009-2010 and 2011-2013 timeframes, respectively.

B2.2 Simulation of Raw Test Score Distribution

We have argued that the discrete nature of the individual components in the total score and the natural correlation between these components leads to the large number of mass points and holes in the empirical application score distribution. To demonstrate that these components can generate the patterns we observe, we next conduct the following exercise.

1) We measure the correlation between the four or six components in each year.
2) We use these correlations and draw simulated data for each of these components from a multivariate normal distribution in order to match the exact number of applications at each school and year.
3) We then map these components through the normal CDF to obtain a probability, and then assign discrete scores to each component based on the empirical frequency of each discrete score for that school and year.

4) We add the components, and the total resulting scores form the simulated distribution for each year.

The bottom panels of Figures B2-B9 present the results of this exercise for each year between 2006 and 2013. As mentioned above, the left side panel of Figures B2-B9 shows the raw distribution, and the right side panel of each figure shows a simulation of the distribution. While we cannot perfectly replicate the mass points and holes in the true distribution (likely because we have to assume a form for the unknown, underlying latent distribution), these simulations consistently generate a substantial number of sizable mass points and holes in the simulated data. Note that in 2009 we do not observe the sub-scores separately for extracurricular activities and the written statement and so conduct the simulation in 2009 using just the original four components.

B2.3 Simulation of Centered Test Score Distribution

Another unusual feature arises once we center the application score distributions by school and year. The left-hand side of Figures B10-B17 show the centered application score distribution by school and year and there is a substantial cliff to the left of the cutoff location in most cases. These cliffs could be consistent with substantial manipulation, placing students to the right of the cutoff with some noise so that an unexplained mass of students is just to the right of the cutoff. However, it is also possible that the cliffs in the centered score distribution arise naturally from the mass points and holes in the raw distribution. If a school is trying to issue a specific number of acceptances and typically works down the list from high to low scores, then the stopping point for this process is more likely to land on a mass point as opposed to a hole because the mass points contain many students and any of those students could help fill the school's capacity.

To examine whether we can replicate the cliffs we observe in the centered application score distributions, we conduct another simulation. For each school, we capture the number of students admitted each year, and view these numbers as an empirical distribution of the number of students the school might admit. For each school, we also calculate the fraction of students over all years who received an admission letter whose test score falls above the threshold, and the fraction of students over all years who are above the threshold, but did not receive an admission letter as a share of the number of students above the threshold. We then use these ratios to scale the potential numbers of students admitted up, based on some students not being admitted even though they were above the threshold, and down based on students being admitted whose scores were below the threshold. We then conduct the following exercise:

1) For each year and school, we select a scaled number of admitted students from the empirical distribution over all years for that school.
2) We then count down the distribution from high to low test scores for the year and school until we have admitted the scaled number of students. The score of the last student admitted is the simulated threshold cutoff test score.
3) We then use the simulated cutoffs for every year and school to center the test score distribution on the simulated cutoff and then pool the centered distributions over all schools.

The result of this exercise is shown on the right-hand side of Figures B10-B17. Specifically, as noted above, for each year, the left-hand side of Figures B10-B17 show the actual empirical distribution of the re-centered application score and the right-hand side shows the simulated distribution based on the process described above. The resulting simulated distributions are relatively similar to the empirical distributions for the years of 2006-2009, but starting in 2010, and especially in 2011, the simulated centered distributions become much smoother while we continue to see mass points and cliffs in the empirical distribution. While pooling centered distributions should in principle smooth over mass points, we have no explanation for the time pattern of this phenomenon. However, given our inability to generate data that looks similar to the actual centered distribution after 2009, we rerun all our analyses dropping the years between 2010 and 2013. These results are shown in Appendix Table A14 and as noted in the paper, with the exception of college attendance, our core results are robust to this sample restriction.

Table B1
Correlations between Estimated Mean Threshold Sizes

Correlations by School over Sample of Years			
Simulation #'s	1	2	3
2	0.871916		
3	0.773716	0.819969	
4	0.840485	0.912581	0.89444

Correlations by Year over Sample of Schools			
Simulation #'s	1	2	3
2	0.136102		
3	0.557161	0.470346	
4	0.402145	0.051942	-0.10463

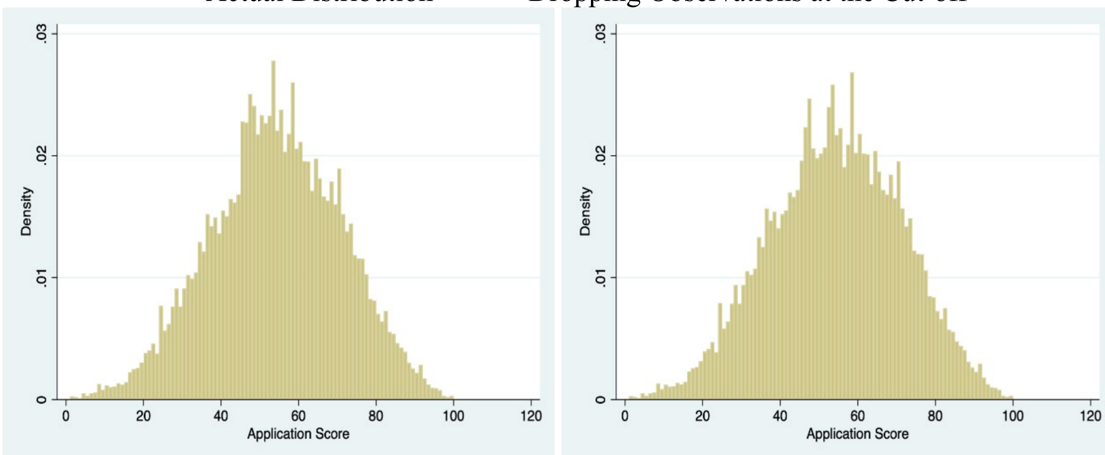
Table B2
Estimated Thresholds for one Simulated Set of Hold-out Samples

Application School/Year	2006	2007	2008	2009	2010	2011	2012	2013	Mean Threshold	Significant at 0.10
911	0.871	0.266	0.881	0.586	0.921	0.831	0.903	0.704	0.745	1.000
912	-0.053	0.411	0.432	0.499	0.450	0.426	0.558	0.652	0.422	0.750
913	0.333	0.493	0.529	1.116	0.374	1.021	-0.299	0.924	0.561	0.750
914	0.506	0.610	0.652	0.271	0.648	0.726	0.818	0.695	0.616	0.875
915	0.563	0.658	0.520	0.602	0.337	0.858	0.647	0.425	0.576	1.000
916	0.674	0.646	0.563	-0.001	0.172	0.228	0.073	0.605	0.370	0.625
917	0.706	0.693	0.100	0.791	0.805	0.581	NA	0.596	0.610	0.750
918	0.592	0.836	0.506	0.697	0.515	0.013	-0.729	0.266	0.337	0.750
919	0.375	0.745	0.821	0.795	0.594	0.582	0.556	0.461	0.616	1.000
920	0.747	0.297	0.341	0.138	0.726	0.461	0.109	0.475	0.412	0.750
922	0.763	0.756	0.359	0.901	-0.094	0.734	0.620	0.152	0.524	0.750
923	0.652	0.606	0.322	0.117	0.810	0.291	0.670	0.436	0.488	0.875
924	0.790	0.474	0.576	-0.692	0.728	0.559	0.560	-0.362	0.329	0.875
925	0.436	0.281	0.528	0.447	0.802	0.865	0.870	0.747	0.622	0.875
926	0.777	0.487	0.665	0.476	0.723	0.599	0.544	0.348	0.577	1.000
927	1.077	0.560	0.681	0.002	0.587	0.582	0.875	0.465	0.604	0.750
Mean Threshold	0.613	0.551	0.530	0.422	0.569	0.585	0.452	0.474	0.525	
Fraction Significant at 0.10	0.938	0.875	0.938	0.688	0.813	0.938	0.733	0.813		0.843

Figure B1: Uconditional Distribution of Running Variable

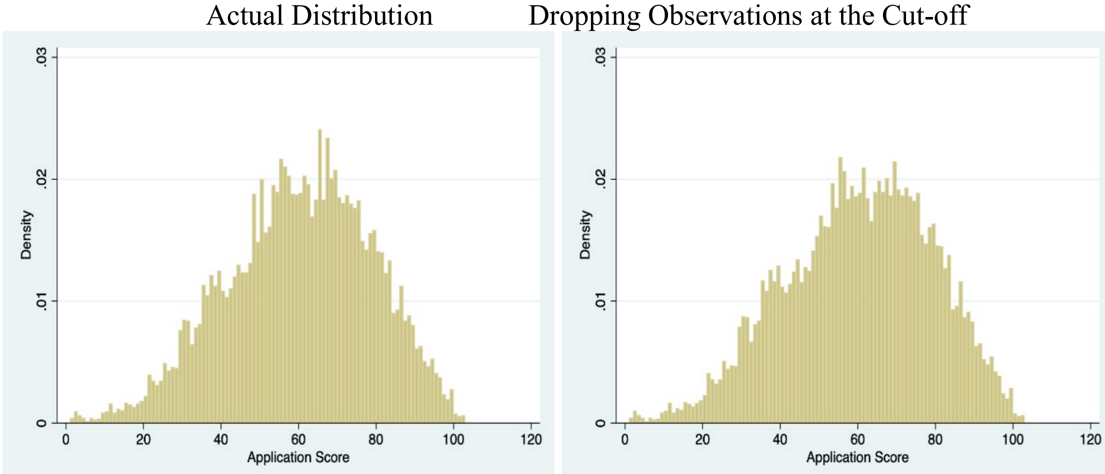
2006-2008

Actual Distribution Dropping Observations at the Cut-off



2009-2010

Actual Distribution Dropping Observations at the Cut-off



2011-2013

Actual Distribution Dropping Observations at the Cut-off

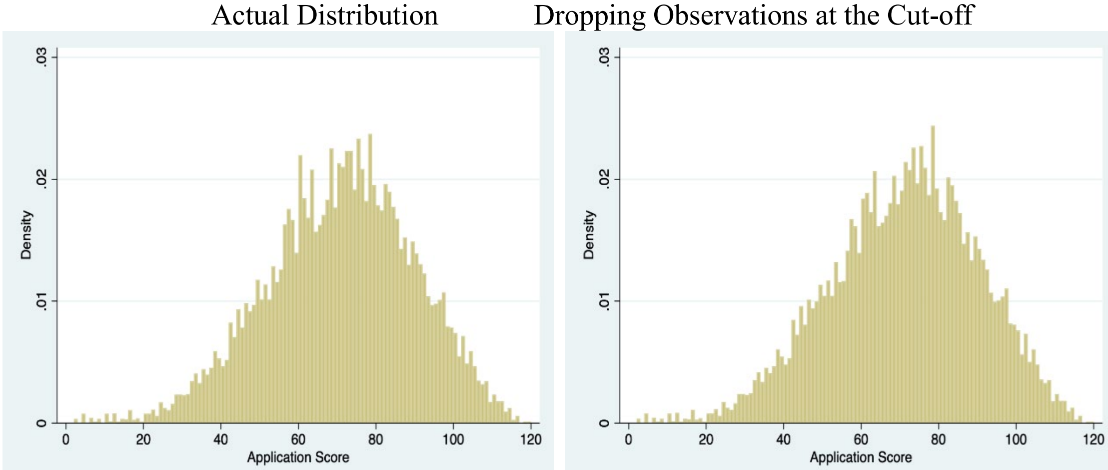


Figure B2
Application Score Distributions: Application Year 2006

Full Sample | Simulated Sample

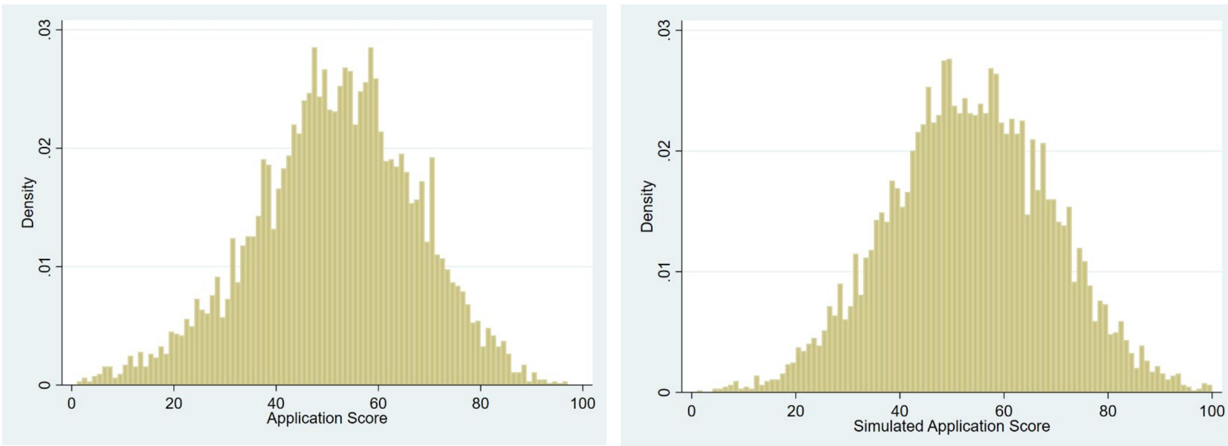


Figure B3
Application Score Distributions: Application Year 2007

Full Sample | Simulated Sample

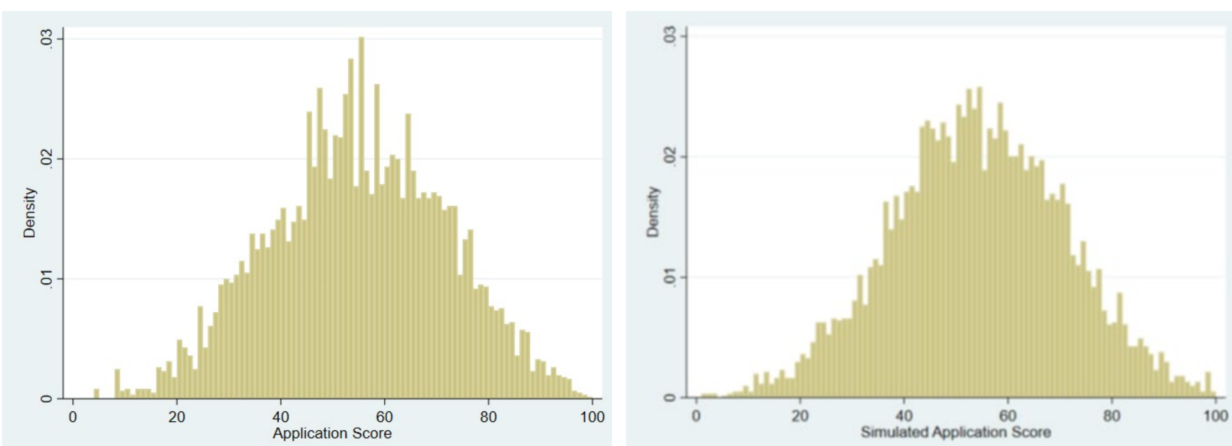
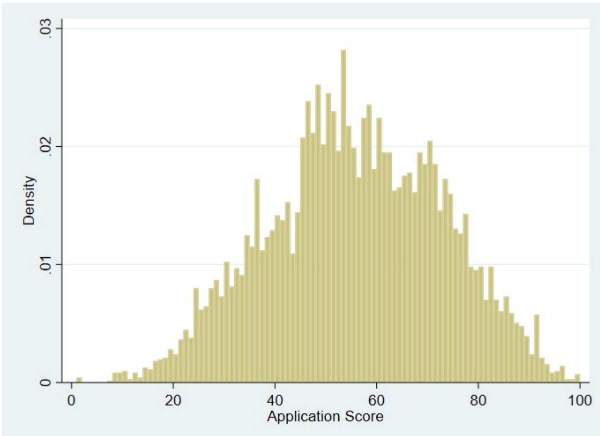


Figure B4
Application Score Distributions: Application Year 2008

Full Sample



Simulated Sample

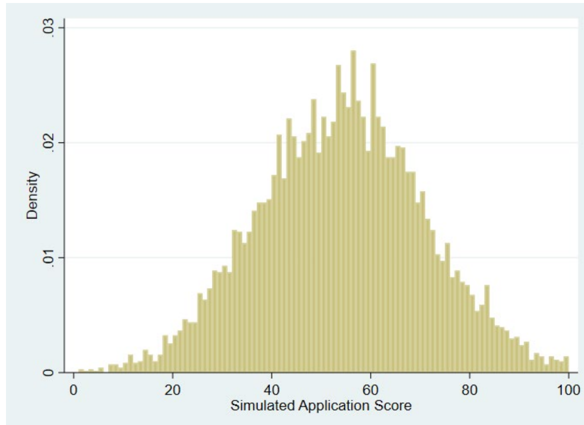
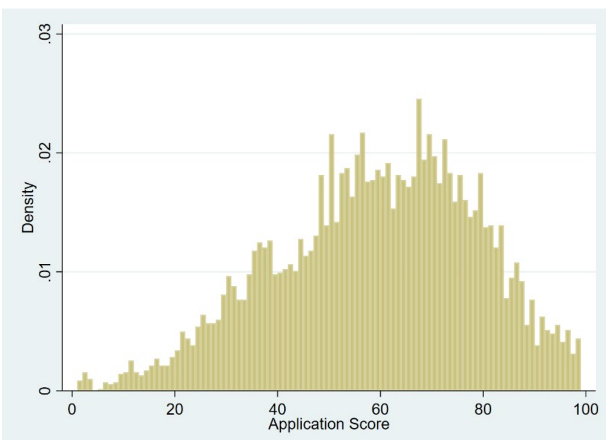


Figure B5
Application Score Distributions: Application Year 2009

Full Sample



Simulated Sample

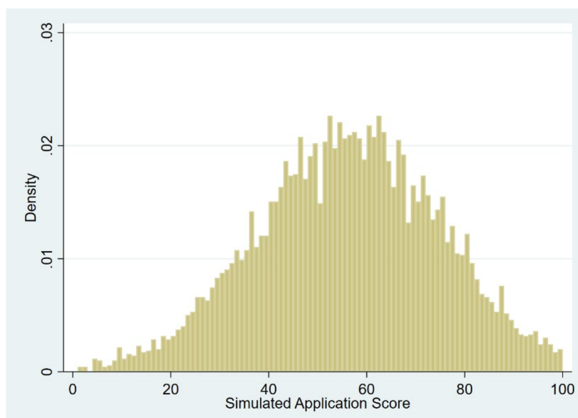
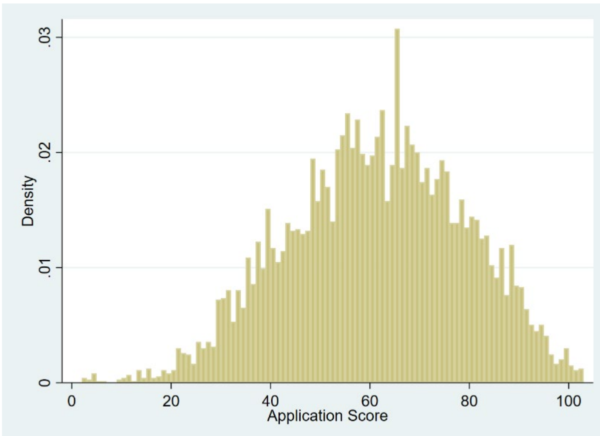


Figure B6
Application Score Distributions: Application Year 2010

Full Sample



Simulated Sample

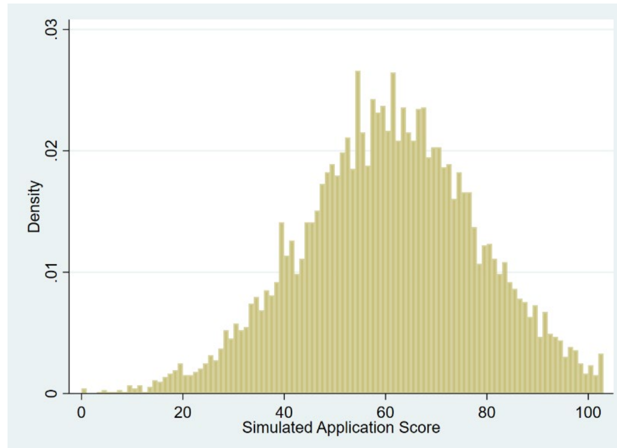
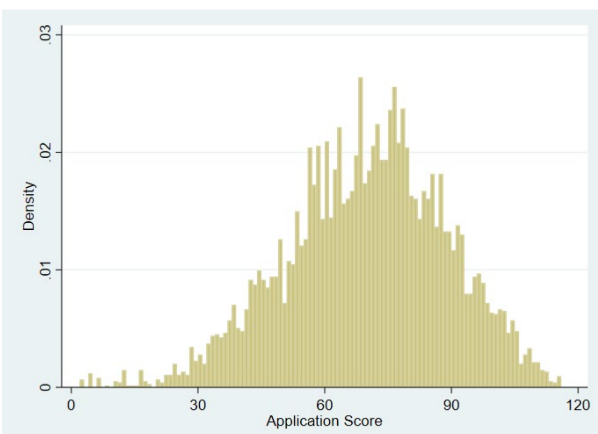


Figure B7
Application Score Distributions: Application Year 2011

Full Sample



Simulated Sample

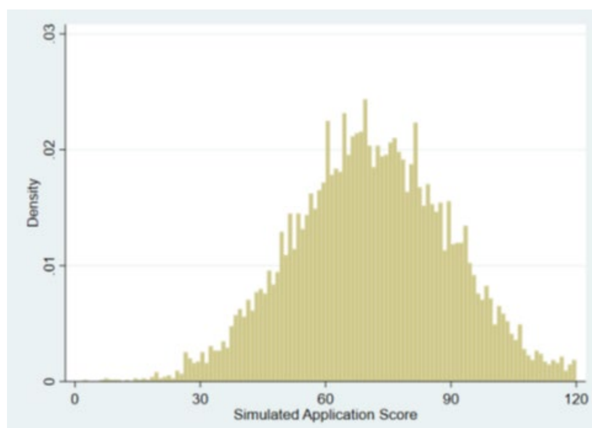
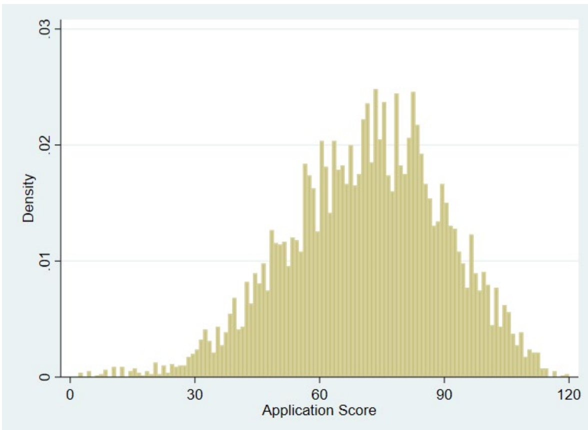


Figure B8
Application Score Distributions: Application Year 2012

Full Sample



Simulated Sample

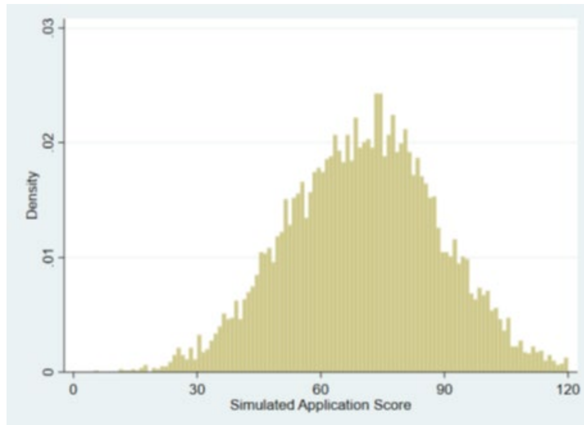
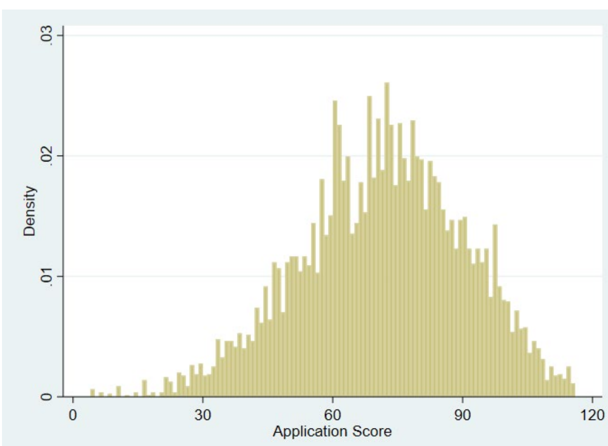


Figure B9
Application Score Distributions: Application Year 2013

Full Sample



Simulated Sample

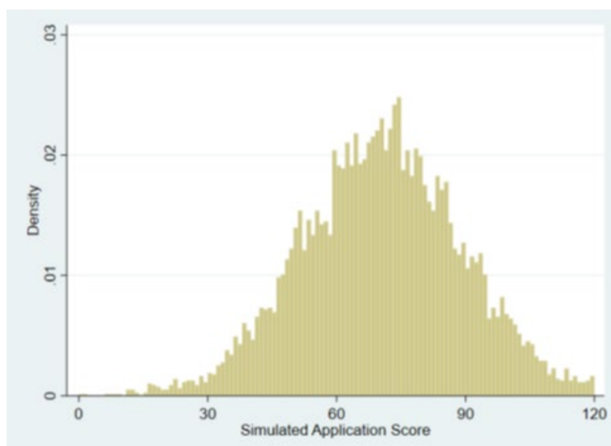


Figure B10
Centered Score Distributions: Application Year 2006

Full Sample | Simulated Sample

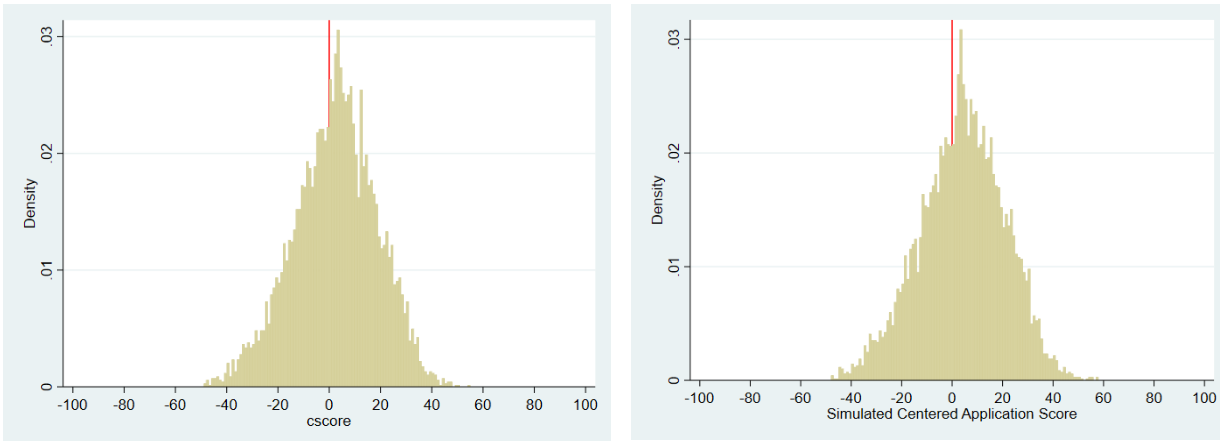


Figure B11
Centered Score Distributions: Application Year 2007

Full Sample | Simulated Sample

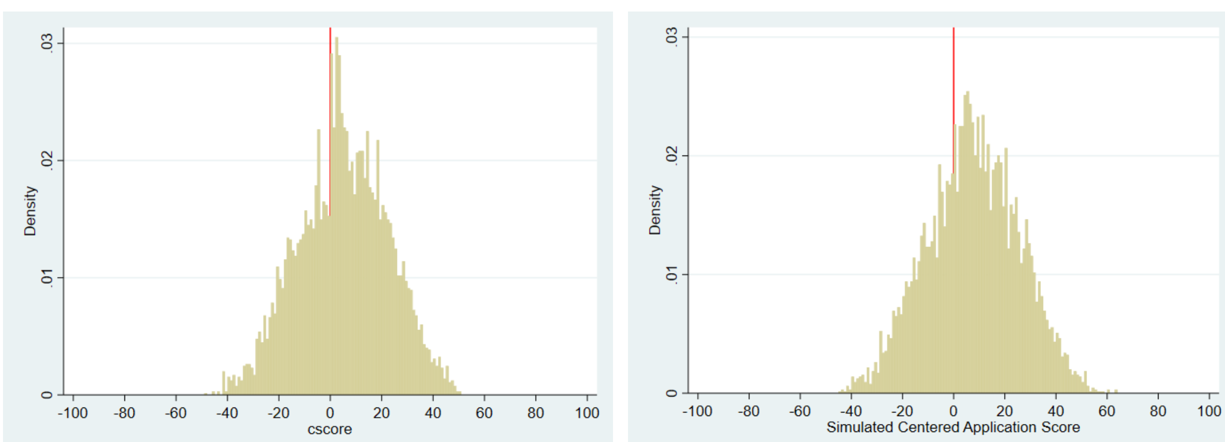
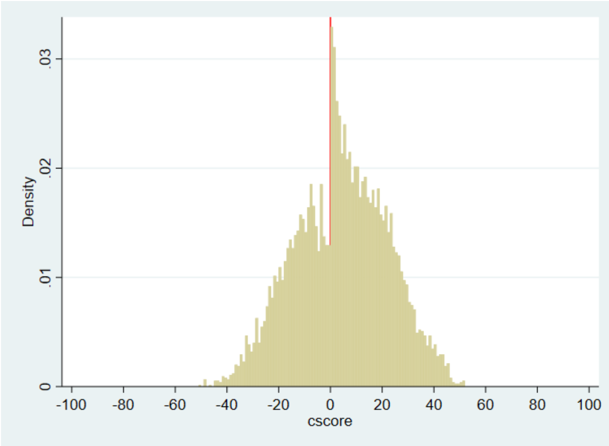


Figure B12
Centered Score Distributions: Application Year 2008

Full Sample



Simulated Sample

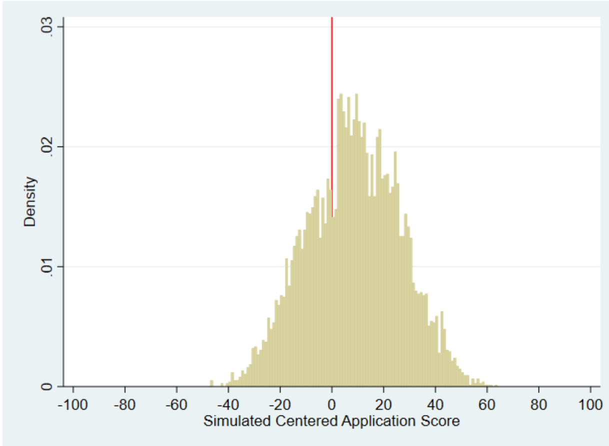
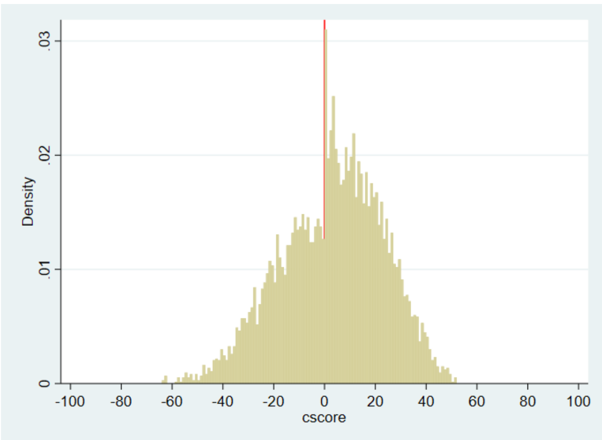


Figure B13
Centered Score Distributions: Application Year 2009

Full Sample



Simulated Sample

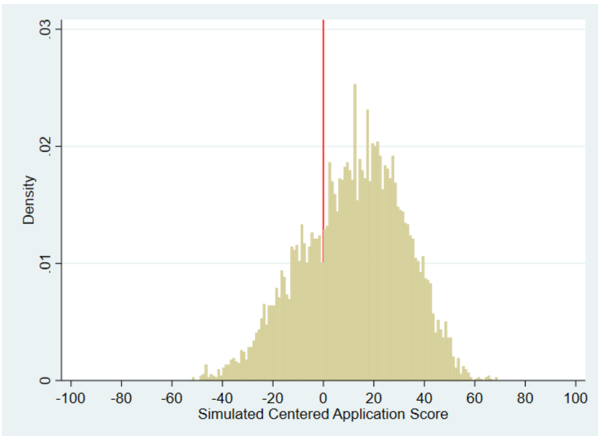
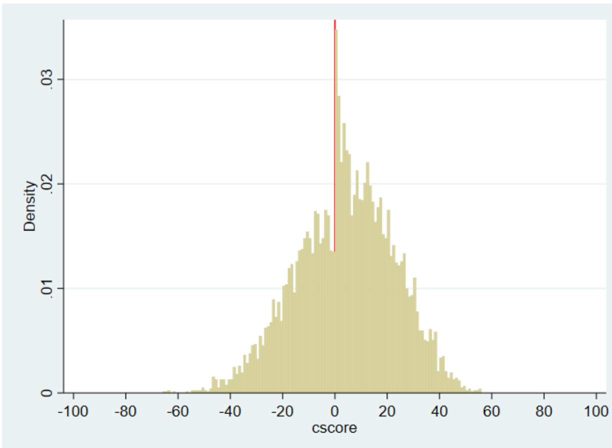


Figure B14
Centered Score Distributions: Application Year 2010

Full Sample



Simulated Sample

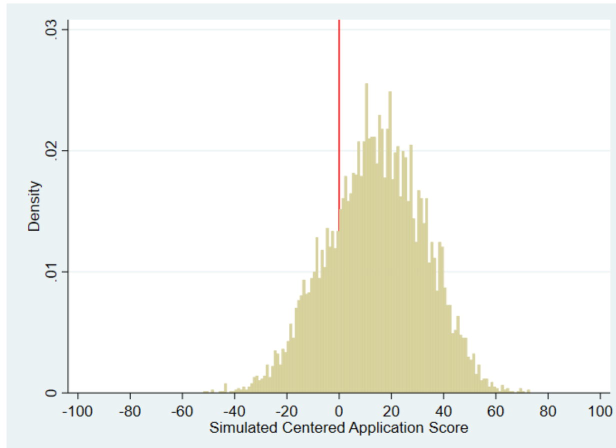
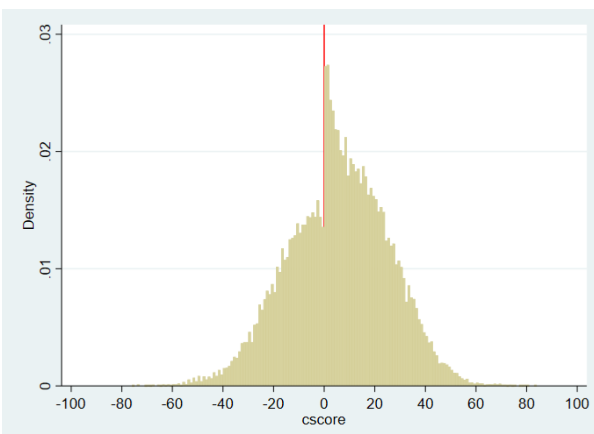


Figure B15
Centered Score Distributions: Application Year 2011

Full Sample



Simulated Sample

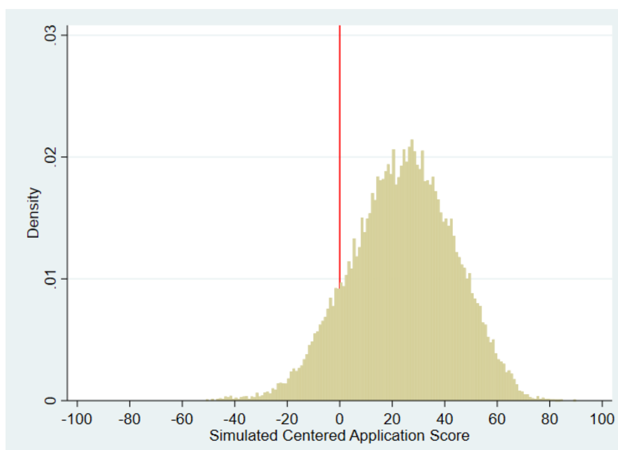


Figure B16
Centered Score Distributions: Application Year 2012

Full Sample　　　　　　　　　　Simulated Sample

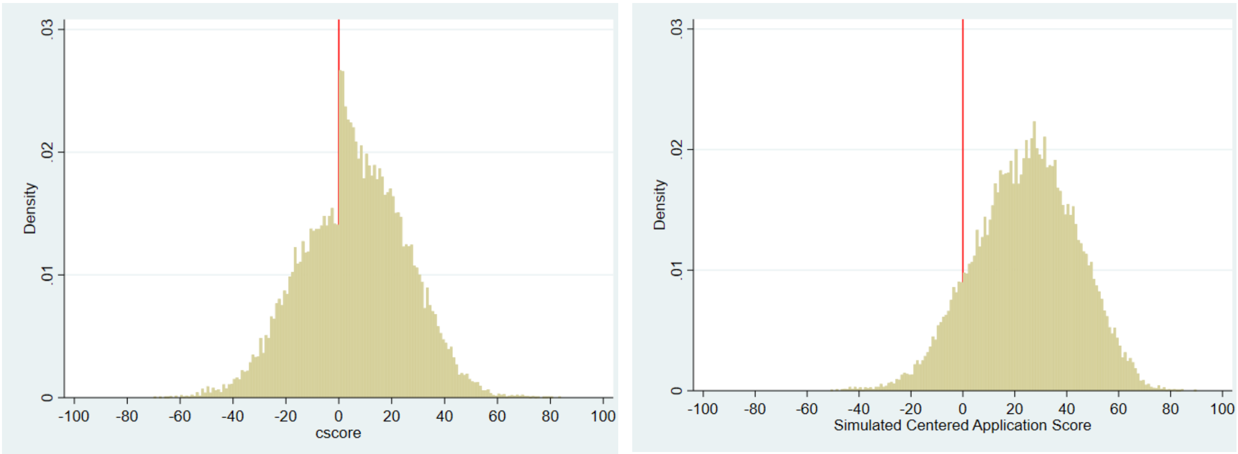


Figure B17
Centered Score Distributions: Application Year 2013

Full Sample　　　　　　　　　　Simulated Sample