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ABSTRACT

We examine the effect of attending stand-alone technical high schools on student short- and long-term outcomes using a regression discontinuity design. Male students are 10 percentage points more likely to graduate from high school and have half a semester less time enrolled in college, although effects on college fade-out. Male students have 32% higher quarterly earnings. Earnings effects may in part reflect general skills: male students have higher attendance rates and test scores, and industry fixed effects explain less than 1/3rd of earnings gains. We find little evidence that attending a technical high school affects the outcomes of female students.

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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. In 2015, more than 7 million secondary school students were enrolled in CTE programming (Passarella, 2018). In 2018, congress reauthorized the Carl D. Perkins Career and Technical Education Act, providing $1.2 billion in funding for CTE programs and job training. As noted by Jacob (2017), proponents of CTE argue that CTE improves career opportunities providing students with hands-on training and the soft skills necessary for labor market success. Similarly, advocates stress that CTE may improve core academic skills fostering student engagement and increasing school attendance. Cullen et al. (2013) argue for an increased focus on 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 and/or examine schools that volunteered.[1] Kemple and Willner (2008) and Page (2012) examine outcomes of 1,400 students who applied for admission to nine different career academies that agreed to randomize admissions finding an 11% increase in earnings, but no effect on high school graduation. Page (2012) in her follow-up shows that all effects are concentrated among males. Hemelt, Lenard and Paeplow (2019) exploit randomized admissions to examine the effect of attending a single career academy in North Carolina and unlike Kemple and Willner (2008) find an improvement in graduation rates. Dougherty (2018) studies three Regional Vocational and Technical High Schools (RVTS) in Massachusetts that had score based admissions and volunteered to be evaluated. He finds a 7 to 10 percentage point increase in the likelihood of on-time graduation from high school. In an exception to this pattern, Bonilla (2020) evaluates a large CTE grant program in California using the threshold for grant award. She finds that

[1] A larger literature exploits longitudinal data comparing students in CTE to observationally equivalent students. Maxwell and Rubin (2002), Bishop and Main (2004), and Meer (2007) examine students surveyed in the National Educational Longitudinal Survey. Cellini (2006), Neumark and Rothstein (2006) and Kreisman and Stange (2017) examine students in the 1997 National Longitudinal Survey of Youth. Elliott, Hanser, and Gilroy (2002) and Maxwell and Rubin (2002) examine administrative student data. These studies tend to find positive effects on either educational attainment or earnings.

districts receiving a grant increased CTE spending and reduced overall high school drop-out rates, but she cannot disentangle the effects of CTE expansion from additional monetary resources.[2]

In this study, we examine the effect of admission to any one of the 16 stand-alone technical high schools within the Connecticut Technical High School System (CTHSS).[3] We exploit the fact that CTHSS has a score-based admissions system and identify the effect of attendance using a regression discontinuity design. Our analysis includes all technical high schools in CTHSS and covers over 57,000 8th grade student applicants between 2006 and 2013. The roughly 11,000 students who attend these 16 schools each year comprise more than seven percent of all high school students in the state. All 16 schools are oversubscribed receiving more applicants than they can accommodate. As a result, Connecticut presents a unique opportunity to examine the impact of stand-alone delivery of CTE where the services are being delivered at scale and a large fraction of the state's student population participates. In fact, a key motivation behind Cullen et al.'s (2013) argument for expanding CTE options was potential concerns about scaling up and replicating successful models of school reform that emphasize college preparatory work, such as magnet schools.

Students are admitted to CTHSS schools based on an application score. Although an admissions threshold is not observed in data, the pattern of admissions is consistent with each school establishing a unique threshold each year and sending out initial acceptances based primarily on this threshold. We follow Porter and Yu (2015) and estimate an admissions threshold for each school and application year. First-stage regression discontinuity estimates show that students just above this threshold are 87% more likely to receive an acceptance letter. Similarly, male and female students are respectively 58% and 52% more likely to attend the school to which they applied as compared to students just below the threshold.

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[2] Also, see Cullen, Jacob and Levitt (2005) who estimate the effects of 10 large career academies in Chicago Public schools using student residential proximity to schools as an instrument for school choice. They find large positive effects on graduation for students that attend career academies, but disentangling the effect of the career academies from any direct effects of proximity is challenging. For work outside of the U.S. context, see studies by Silliman and Virtanen (2019) in Finland and Bertrand, Mogstad and Mountjoy (2019) in Norway.

[3] Our analysis excludes Bristol Technical because students remain part of their original high school completing academic coursework and extracurricular activities at their home school. The analysis also excludes Wright Technical, which did not open until 2014.

Further, anecdotally male and female students enroll in different programs with men focusing on building trades and manufacturing and women primarily specializing in human services and tourism.[4] Given strong gender patterns in CTE, as well as traditional gender differences in the labor market, we use separate models for males and females.

Our fuzzy regression discontinuity two-stage least squares estimates imply that male students attending one of the technical high schools are approximately 10 percentage points more likely to graduate from high school relative to the control mean graduation rate of 80 percent. Male students also have accumulated ½ fewer semesters of total time enrolled in higher education. However, we find that these negative effects on college attendance fade over time, and we find no significant differences in time enrolled or whether the student ever enrolled in college for older applicants.[5] The higher high school graduation rates among male students are accompanied by a substantial increase in earnings and labor market activity soon after leaving high school.[6] Total earnings post high school increase by 44% relative to a sample average of $63,000. Similarly, average quarterly earnings increase by 32% relative to an average of $5,400, and total number of quarters with earnings increases by 1.0 quarter relative to an average of 10 quarters. These earnings effects are similar in magnitude to those observed for the Year-Up Boston and San Antonio Quest jobs programs (Heinrich 2012-13; Elliot and Roder 2017), which similarly provide intensive training in job relevant skills. We find small and statistically insignificant effects of attending a CTHSS school for female students.[7] While CTHSS schools have higher spending, lower student-teacher ratios, and better peer attributes than the counterfactual high schools these students likely would have attended, we do not find any evidence that treatment effects are larger when students come

[4] For example, in 2019 the programs of study on culinary arts, guest services, early child care and education, hairdressing and cosmetology, health technologies, hotel hospitality, and tourism captured 52 percent of female CTHSS students, but less than 7 percent of male CTHSS students. In contrast, the trade related programs of automotive manufacturing and technology, carpentry, collision repair, heavy equipment repair, electrical, HVAC, masonry, plumbing and welding contained 73 percent of all men and only 33 percent of women. See Jacob and Ricks (2020) for an analysis of patterns of selection into different CTHSS programs.
[5] Similarly, Bifulco, Fletcher, Oh and Ross (2014) show that peer effects on college attendance fade over time.
[6] We observe students in the labor market for between one and a half and eight and a half years.
[7] Estimates for a pooled sample are similar to the male results, but point estimates are somewhat smaller.

from counterfactual high schools that spend less, have higher student-teacher ratios or worse peers suggesting that the positive effects of attending a CTHSS school are not just the effect of additional resources.

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 and Kumar 2000). Several findings suggest that CTHSS earnings gains arise in part from increases in general skills. After restricting the sample to students that are 23 or older, we continue to find large earnings differences of 33 percent, and initial differences in college attendance and semesters enrolled decline substantially and become statistically insignificant by age 23. While earnings gains for quarters when students are 22 or younger are larger at 43 percent, half of the difference between the 23+ and 22- samples can be explained by the modest differences in time spent in college. Next, we find approximately a two-percentage point increase in 9th grade attendance rates over a base of 93 percent attendance, and an increase in 10th grade average test scores of 18 percent of a standard deviation.[8] Finally, we re-estimate our model of quarterly earnings including fixed effects for two- and three-digit industry codes. These fixed effects explain less than 1/3rd of the earnings gains of male students suggesting that a majority of earnings gains are not due to selection into higher paying industries via placement advantages for CTHSS students or industry specific skills gained in CTHSS. Our findings on general skills are consistent with Silliman and Virtanen (2019) in Finland and Bertrand, Mogstad and Mountjoy (2019) in Norway who find positive labor market effects of vocational training for adult men in their thirties.

We also measure the number of CTE offerings at each counterfactual high school as a share of total electives offered. The effect of attending a CTHSS school is negatively related to the extent of CTE offerings at the student's counterfactual high school.[9] For example, attending a CTHSS school has a 12 percent larger impact on quarterly earnings when the student otherwise would have faced a one standard

[8] We do not have grade point average, but even if available it is not clear that grades in CTHSS schools would be comparable to grades in traditional high school given likely changes in the pattern of course taking.
[9] On the other hand, the share of electives that are CTE offerings in human services and hospitality, female dominated programs, has no impact on the effect of CTHSS enrollment on student outcomes.

deviation lower share of electives that are CTE. However, the difference in CTE offerings between traditional and CTHSS high schools explains only 1/5th to 1/3rd of the estimated effect of attending a CTHSS school, suggesting that the stand-alone nature of these schools has an effect above and beyond more opportunities for completing CTE coursework. Other than CTE offerings at the counterfactual high school, we find no evidence of heterogeneity in CTHSS effects.[10]

We undertake a series of analyses to examine the validity and robustness of these results. First, estimates are robust across different bandwidths, the inclusion of student attributes, relaxation of sample restrictions including requiring presence in the labor market sample (for education effects), and estimation of treatment effects separately for each school and cohort (Bertanha, 2020).[11] Second, we pass balancing tests over applicant attributes, over whether the applicant is observed in the labor market sample, and over attributes of the student's likely counterfactual high school in both our main sample and our labor market sample. Third, the distribution of application scores is not smooth and contains mass points and gaps where density deviates from the density at surrounding scores. This feature of our data results in a failure of traditional McCrary test for manipulation at the boundary in some schools and years that we believe arises from structure of the running variable rather than manipulation. We show that our results are robust to dropping school by application year clusters that fail the McCrary test and to the use of a donut hole specification (McCrary 2008).[12] Finally, we conduct falsification tests creating artificial thresholds above or below the real threshold, and find no evidence of any treatment effects at these false thresholds.

II. Connecticut Technical High School System

[10] We test for differences by free and reduced-price lunch status, race and ethnicity, whether the applicant resided in one of the five central city districts in Connecticut, the cut-off for a given year and school, across application years, across CTHSS schools, and across quartiles of quarterly earnings using quantile regression.
[11] Following Bertanha (2020), we estimate a weighted average effect over all schools and cohorts given the varying admissions thresholds. High school graduation, quarterly earnings and quarters with earnings effects are all robust. The negative effects on college are eroded, but those effects do not persist over time anyway. We also find no evidence of heterogeneity when using individual school and cohort treatment effect estimates.
[12] Balancing tests and the treatment effects are very similar whether we use the entire sample or the donut hole sample. We also present several simulation analyses demonstrating that the irregular distribution appears to arise from the underlying components of the admission score, rather than manipulation at the boundary.

The Connecticut Technical High School System (CTHSS) is a quasi-independent school district comprised of 16 high schools where all students who attend the school participate in CTE. Students must meet the standard high school graduation requirements for Connecticut. However, CTHSS students complete CTE coursework in lieu of other electives. CTE coursework is grouped into one of 10 to 17 programs of study, such as information technology, health services, cosmetology, heating ventilation and air conditioning, or production processes. Within their selected program, students take a minimum of three (often many more) aligned courses. Often, these sequences are combined with career awareness activities and opportunities for work-based learning in settings outside of school. In contrast, traditional comprehensive 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 even in the same program. At CTHSS, 9th grade students explore 3 to 6 programs of interest and at the end of the first semester rank programs they wish to pursue. In the spring of 9th grade, they are assigned a program based on preferences and availability and spend the next three and a half years completing their CTE coursework with a stable cohort of peers and instructors. Further, CTE instructors often collaborate with teachers in core academic areas to ensure overlap of content, and work based learning is an important part of the curriculum. The CTE programs focus on providing skills to support transition into the labor market following high school graduation.

The CTHSS schools are located and serve students across all of Connecticut. The roughly 11,000 students who attend these 16 schools comprise more than seven percent of all high school students in the state, so services are provided at scale to a substantial share of the student population. 31 percent of total enrollment comes from the state's five poorest central city school districts of Bridgeport, Hartford, New Haven, New London and Waterbury, but in aggregate CTHSS serves only a moderately disproportionate share of students from lower-income families. The breadth of the students and environments served by this district offers a unique opportunity to understand not only how stand-alone CTE programs impact student outcomes, but also how these effects might differ by setting or student characteristics.

Eighth graders across the state can elect to apply in the winter before they would enroll in 9th grade to attend high school at one of the CTHSS schools. Students can apply to multiple schools and must rank-order their choices. All 16 of the technical high schools are oversubscribed and receive more applicants than they can accommodate. Admission is coordinated by the central office of CTHSS. Students send an application to their first-choice school, the first-choice school processes the application and issues acceptance letters, and the processed information is placed into an applicant database for all schools and stored at the CTHSS district office. 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 9th grade years of 2009-10 through 2013-14, two additional categories were added based on points for extracurricular activities and a written statement.[13] The first-choice school is responsible for scoring the application and all schools use that score. Even though the underlying attendance and standardized test scores are close to continuous, the scoring system discretizes each of these components into an ordinal set of points that are then added together to form the score. The discrete nature of these components when combined with the high correlation between them yields a distribution of raw scores that is irregular with both mass points and holes/gaps in what might otherwise appear as a smooth distribution. We discuss this issue in more detail later in the paper.

School administrators have described establishing an admissions threshold in each school every year and then sending out initial acceptance letters primarily to students whose scores lie above the threshold. However, there exists some deviation between the student's score and whether the student receives an acceptance letter. Some students may be admitted with lower scores in order to increase diversity. Later waves of letters can be sent out to lower scoring students if all seats in the school 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 information in their disciplinary file. The recording of acceptance letters is also imperfect given that some students enter the CTHSS system, even though there is no record of them receiving an acceptance letter. Therefore, the

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[13] The number of points associated with each component in each application year is shown in Appendix Table A1. Points for extracurricular activities and the written statement are based on information provided by the applicant.

admissions process results in a "fuzzy" discontinuity where the noise arises from deviations of school administrators from the scoring system, errors in the recording of acceptance letters, and imperfect take-up by applicants. Based on inspection, the discontinuity appears to arise at the bottom of the score distribution in the initial wave of acceptance letters (especially for CTHSS attendance since being in the initial wave has a large effect on take-up). Finally, special education applicants, i.e. applicants with an Individualized Educational Plan, are subject to another layer of review and evaluation prior to admission, and so are excluded from our analysis.

III. Methods

We model the relationship between outcomes and admission score using a "fuzzy" regression discontinuity design (Imbens & Lemeiux, 2008; Murnane & Willett, 2011). We employ a local-linear regression framework using a uniform kernel. However, we do not observe the threshold established for sending out admissions letters. Therefore, we identify the thresholds empirically for each school and year following Porter and Yu's (2015) recommendation that the thresholds/discontinuity be estimated as the threshold that yields the largest discontinuity in the dependent variable. 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 admissions score (X_{isyt}) on either side of candidate thresholds or cut-offs (X_{sy}^{*}):

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

where $d\left(X_{sy}^{*} \leq X_{isyt}\right)$ is a binary indicator that is one if the condition is satisfied. Equation (1) is estimated using observations that fall within bandwidth BW or for which:

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

and the threshold estimate is selected as:

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

For more details, please see Section 1 of the Methodological Appendix.[14]

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

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

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 effectively identifying the likely counterfactual high school or schools. Finally, the first stage equation for A_{isyt} is:

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

where $\tilde{\alpha}$ represents the composite or sample average effect of being above the threshold. The parameter of interest is β in equation (2) capturing the effect of attending a CTHSS school for students who are just above the admissions threshold compared to those who just missed the threshold. These estimates represent the Local Average Treatment Effect (LATE) for students near the admissions threshold (Angrist, Imbens and Rubin, 1996), but the variation in cut-offs across schools and over time yields estimates that are representative over a broader set of scores. Standard errors are clustered following our fixed effects structure: application school by application year and sending town.[15]

The estimation approach above combines applicant pools across schools and years where a different threshold arises for every school in every year. The alternative to pooling is to estimate reduced form models of student outcomes for every school by year similar to Equation (1). Following Bertanha (2020), a reduced form average treatment effect is estimated as a weighted average of individual pool estimates where weights are based on the density of observations at the discontinuity. Specifically,

[14] We empirically verify that the distribution of estimated discontinuities α are approximately unimodal over the score for each school and cohort, consistent with a single threshold of admission.
[15] Many prior studies with discrete running variables have clustered standard errors by the running variable. However, clustering by the running variable leads to confidence intervals with poor coverage properties (Kolesár and Rothe 2018). As an alternative to our two-way clustering, we also conduct inference using finite-sample exact randomization inference tests following Cattaneo et al. (2019).

$$\hat{\beta}_B = \sum_s \sum_y N_{sy} \hat{\beta}_{sy} \tag{4}$$

where N_{sy} is the number of observations within the bandwidth for school s in year y. The standard errors for the average treatment effect can be estimated by bootstrapping the individual estimates at the school by year level. Further, while we examine heterogeneity by interacting treatment with student attributes in expanded versions of Equations (2) and (3), we can also estimate models of heterogeneity using the treatment effect estimates for each school and year as the left hand side variable and controlling for the average composition of the student applicants within the bandwidth for each school and year ($\bar{z}_{sy}$).

$$\hat{\beta}_{sy} = \gamma \bar{z}_{sy} + \sigma_y + \varepsilon_{sy} \tag{5}$$

where σ_y are year fixed effects. We weight each observation by the square root of the sample size as a proxy for estimation error, rather than the inverse of the standard error, because small samples sizes within the bandwidth can lead to small sample bias in the standard error estimates.

Finally, as is shown in more detail later, the empirical distribution of raw admission scores contains a large number of mass points that lie above what would otherwise be a smooth unimodal distribution. Further, the centered score distribution suggests that cut-offs tend to fall at these mass points, potentially because the admission process proceeds downwards through the distribution until a desired number of applicants is reached. Such a process will be more likely to stop on a mass point than on any other specific point in the distribution. Therefore, traditional McCrary tests for manipulation at the boundary will not be able to distinguish between manipulation and the non-smooth distribution of the raw running variable. To address concerns about bias from manipulation, we estimate models using a donut hole approach dropping observations at the cut-off for the school and year, or more specifically the sample is selected based on: $X_{isyt} \in \left[X^*_{sy} - BW, X^*_{sy} - 1\right] \ or \ \left[X^*_{sy} + 1, X^*_{sy} + BW\right]$ (Barreca et al., 2011; Barreca et al., 2016; Card & Giuliano, 2014; Canaan & Mouganie, 2018).

IV. Data and Sample

Our sample contains approximately 57,000 8th graders who apply to a technical high school for academic years from 2006-07 to 2013-14. The sample contains one observation for every application so students with multiple applications independently contribute to estimates based on being above the threshold of each school. Sixteen percent of the sample applies to two schools and only three percent applied to three schools (the maximum allowed), but a much smaller fraction are within the bandwidth of the admissions threshold for more than one school. The first stage take-up rate in the fuzzy RD will adjust for the fact that students can attend only one school.[16] The CTHSS admissions data contains each student applicant's name, date of birth, home town, middle school, the total admissions score, the individual components of the score, 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. The reason for the sequential process is 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 final sample of 57,658 student applications.

From the CSDE longitudinal data system, we obtained information on each student's race, gender, free or reduced price lunch status, English learner and special education status, i.e. presence of an IEP. The CSDE data also contains short- and medium-term educational outcomes including: standardized test scores prior to and during high school, attendance, high school graduation,[17] as well as college attendance drawn from the National Student Clearinghouse. Using the Clearinghouse, we can calculate whether the individual ever attended college or attended college by a specific year,[18] as well as the number of semesters of college attended overall or during a specific time period. The Clearinghouse covers nearly 100 percent of public four-year colleges and about 93 percent of non-profit and two-year public colleges (Dynarski et al. 2015). Through Connecticut's P20Win process, students in our sample are

[16] Correlation between observations from the same student is addressed by clustering by sending town. Later in the paper, we also show that results are robust when dropping students who applied to more than one school.
[17] Unfortunately, the data only identifies the high school attended in ninth grade.
[18] We do not consider instances where the individual spell in college ends on the same day that it begins.

matched to Connecticut State Department of Labor (CSDOL) data.[19] 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.[20] Student are in the labor market sample if CSDOL observes unemployment insurance covered earnings in any quarter for which the students is 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 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 five years to complete high school and two quarters to enter the labor market. Our labor market data ends in the 1st quarter of 2018. Therefore, we restrict the sample to cohorts 2006 to 2011 so that for 2011 applicants we observe five quarters of data.[21] Below, we verify that membership in the labor market sample is not influenced by CTHSS attendance and that the labor market sample passes standard balancing tests.

We selected a primary bandwidth of 10 and then test the sensitivity of our results to the use of smaller bandwidths.[22] Table 1 is intended to demonstrate the generalizability of our analysis by presenting sample means for the state overall, the applicant pool and the portion of the applicant pool within our

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[19] We identify and add up all continuous spells of college enrollment counting total number of days for which the student was enrolled in one or more college and divide by 112, the number of calendar days in a 16 week semester.

[20] A fuzzy match criteria of 60% only yields an additional 500 matches, many of which looked erroneous upon visual inspection by CSDOL personnel.

[21] Appendix Table A2 presents the fraction of the total applicant sample for which quarterly earnings are observed by quarter beginning two quarters after taking five years to complete high school. Note that even six quarters after the standard four years of high school participation rate is below 50 percent, but by 7 quarters through the remainder of our sample the participation rate is reliably around 60 percent.

[22] Initial experiments following Calonico, Cattaneo and Farrell (2020) suggest an optimal bandwidth of 10 or higher. Such methods are not ideal, however, for discrete running variables (Cattaneo and Vazquez-Barr 2016) so as noted above we use that bandwidth as our benchmark and then test for robustness by reducing the bandwidth.

baseline bandwidth of 10 points on either side of the cut-off. The top panel presents means for student attributes and standardized test scores that are available for the state overall. The bottom panel presents means for the CTHSS application score and the key score components including standardized tests, grades and attendance. The CTHSS applicant sample is substantially less female (42%) than the student population statewide (49%). African-American, Hispanic, and Free-lunch eligible students are substantially over represented relative to state wide averages with percent African-American being 50 percent higher and percentages of Hispanic and Free-lunch eligible almost double the shares statewide, and standardized test scores are about 2/3rd of a standard deviation below the state-wide averages. The means around the estimated admissions cut-offs are quite similar on student demographics to the means of the full sample of CTHSS applicants, but standardized test scores and admissions components are modestly lower for the within bandwidth sample, differences are always less than 1/2 a standard deviation. More importantly, however, the standard deviations of demographics and score components only fall by 10 to 25 percent as we move from the full sample of applicants to the sample within 10 points of the cut-off. The modest reduction in variances within the regression discontinuity sample arises in large part because of the variation in cut-offs across schools and application years.

Table 2 provides an alternative view of the data presenting the average attributes of the CTHSS schools and the attributes of the high schools that the applicants likely would have attended if they had not attended a CTHSS school. In order to determine the counterfactual high school or schools, 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.[23] For each of these cells, we record the high schools attended by individuals who were not admitted to the CTHSS school to which they applied. In most cases, students who were not admitted to CTHSS attended their town high school or schools. [24] The top panel of Table 2 presents the share of high school electives that are CTE overall, trade focused CTE, and CTE focused on human

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[23] The focus on towns is justified because 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.
[24] However, in some larger cities, the counterfactual is an average across a combination of large traditional city high schools and smaller magnet high schools located in or around the city.

services, tourism or hospitality (services), which are programs dominated by female CTHSS students.[25] The average share of CTE courses overall is twice as larger in CTHSS schools as compared to our counterfactual high schools. When focusing on trades, these differences are magnified with on average non-CTHSS schools offering only about 5% of their electives as trade focused courses and trade offerings representing over half of the courses in CTHSS schools. On the other hand, in terms of human services and hospitality, the average CTHSS high school actually offers a somewhat smaller share of electives in these areas compared to the counterfactual schools. The bottom panel shows traditional school attributes. CTHSS high schools have higher levels of spending overall and lower pupil-teacher ratios. Further, while CTHSS students on average have lower test scores and lower family incomes than the state population, they tend to have higher test scores and income than the peers from their sending school, suggesting that CTHSS tends to draw the more advantaged students from disadvantaged high schools.

Next, we split the sample between male and female applications both because they tend to pursue very different programs in the CTHSS system and because historically men and women have different patterns of labor force participation. While program selection (unobserved in the microdata) and labor force participation choices are endogenous, gender itself is an exogenous attribute that supports stratification of the sample. We drop both applicants who apply in 9th grade for admission to the CTHSS for 10th grade enrollment and special education/IEP students given that the CTHSS system treats both of these groups quite differently in the admissions process. Finally, we also drop students who we cannot find in the CSDE database in 9th grade because we have no education outcomes for those students. 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

[25] For trade focused, we select all courses in the state data based in the categories of manufacturing, transportation, and architecture which primarily contains programs related to building trades; while for the services category we select all courses in human services, tourism/hospitality, and family and consumer sciences.

not observed in Connecticut high schools in 9th grade.[26] Later in the paper, we demonstrate that key results are robust to these data filters.

IV.A. Modelling the First Stage Regression Discontinuity

As discussed above, we empirically select a threshold for each school and application year. We estimate equation (1) separately for each school and year identifying the cut-off score that maximizes the discontinuity in the probability of receiving an acceptance letter.[27] 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.[28] Figure 1a and Table 3 columns 1 and 2 present the pooled estimates for whether a student receives an acceptance letter using our standard 10-point bandwidth. Figures 1b and 1c and the additional columns of Table 3 present first-stage estimates by gender for whether we observe the student in the school to which they applied at the end of 9th grade. All figures show a clear discontinuity with the probability of receiving an acceptance letter being above 0.9 and approaching one as the running variable increases past the cut-off. Figures 1b and 1c show a different pattern with attendance falling with the running variable to the right of the cut-off, consistent with higher scoring students having more options or coming from better school districts on average and thus being more likely to turn down the offer. The estimated first stage effect of being above the cut-off on receiving an acceptance letter is 0.86 implying an 86 percentage point increase in the likelihood of receiving a letter. The first stage for being observed in the technical high school is somewhat smaller, but still sizable, at 0.58 for men and 0.52 for women. The results are presented in pairs of columns with and without controls, and estimates are virtually identical. As shown in Appendix Tables A4 and A5, we find similar first stage estimates using alternative bandwidths and for the labor market sample.

[26] In Appendix Table A3, we present means by gender for the full sample, the sample within 10 points of the cut-off, the sample within 10 points above the cut-off, and the sample within 10 points below the cut-off.

[27] The sending of an acceptance letter is recorded in the system by the date on which the acceptance letter was sent. Students are also coded by us as having received an acceptance letter if the system records a date at which the student responded to and accepted the offer, even if no date is recorded for the sending of the acceptance letter.

[28] We do not impose the donut hole when selecting the thresholds because the dropping of sample at each threshold could create non-convexities in the function that we are maximizing in selecting the optimal threshold.

The use of the sample for selecting the threshold does not affect inference in the second stage of the 2SLS models because those models simply require that the instrument have sufficient power and that the exclusion restriction be satisfied conditional on the running variable, along with the standard correction for using a predicted regressor. 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 the dummy variable based on that estimated threshold is included as a regressor. If this bias is severe, our use of *F*-statistics from the first stage regression to evaluate the strength of the instrument may be misleading. To address this concern, we follow Card, Mas and Rothstein (2008) and draw a holdout sample, using the non-holdout sample for each school and year to estimate the threshold. We use the pooled holdout samples to estimate the first stage regression and repeat the entire exercise for four different draws of the holdout sample. The resulting *F*-statistics are always very strong, ranging 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 using the hold out samples. The means of the estimated discontinuities over all schools and years range between 0.525 and 0.540, respectively, and the fraction of thresholds that are significant at the 10 percent level ranges between 0.795 and 0.843. Further, the average discontinuity at the threshold 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. Therefore, the empirically selected thresholds together provide a very strong instrument for explaining school attendance. For details, see Section B1.1 of the Methodological Appendix.

IV.B. Distribution of the Running Variable

A key assumption in RD is that individuals cannot precisely control the value of the running variable and so cannot adjust that variable strategically in response to the cut-off value (Lee and Lemieux, 2010). In the current context, it is highly unlikely that students could strategically manipulate their position along the running variable, given the admission process used by CTHSS. Recall that schools set the threshold after observing the pool of applicants, all their scores and all their provided materials. As a result, it is nearly impossible for students to know the exact position of the cut-off. On the other hand, there is potentially more room for manipulation by school personnel who process and calculate each applicant's total score. However, manipulation also seems unlikely in this situation. First, school administrators have no incentive to manipulate the score given that they are free to depart from any threshold that they set and that their admission decisions are not monitored. Further, recall that the four primary components that make up a student's total score are standardized 8th grade test scores in math and language arts (reading and writing) plus GPA and attendance in middle school. All of these components are clear and objective with little to no room for manipulation. There is potentially more room for the manipulation of the final two components of the total score, namely the points assigned for extracurricular activities and a student's written statement for why they want to attend a technical high school, but students applying to CTHSS most likely never interacted with school officials and we observe a non-standard distribution prior to the consideration of the additional components. We also verify that the components sum to the composite score for all students in our sample so any manipulation would have to involve school administrators manipulating components to get a desired result on the total score.[29]

To illustrate the issues with the score distribution, we plot the raw score distribution separately for 2006-2008, 2009-2010 and 2011-2013 since 2009 is when the points for extracurricular activities and the written statement were added and in 2011 the number of points assigned to those criteria was increased from 6 to 20.[30] The left hand side of Appendix Figure B1 shows these unconditional score distributions. The distributions exhibit substantial mass points and some gaps or holes relative to any smooth distribution one might fit to this data. In 2006-2008 when the distribution is based entirely on objective information, the raw score distribution is just as non-smooth as the other years. On the right hand side of the figure, we repeat these raw distributions except that we drop students whose scores are exactly equal to the cut-off for their school and application year (donut hole distribution). If the

[29] Unfortunately, while we have the contribution of each component to the total score, we do not have the raw information like GPA that is used to calculate the contribution for each component or the precise rules used each year to calculate the contribution from the raw data.

[30] Note that the scores in 2009-2013 are above 100 because the maximum score was increased when the extra two components were added.

irregularity of the distribution was driven primarily by manipulation, we would expect that most of the mass points in the distributions should have disappeared when we dropped students at these cut-offs, but the score distribution appear relatively unchanged by this sample restriction.

Appendix Table A6 presents the results of the McCrary test for a smooth running variable across the threshold separately for each school and application year. The school-year combinations that reject the null of the McCrary test 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, in every year, multiple schools fail, but also multiple schools pass the test in every year, and failures are no more likely after 2011 when a larger weight was placed on the subjective components. Later in the paper, we replicate our key results restricting estimation to the subsample of school/application year clusters that do not fail the McCrary test, and the key findings are all robust.

We also conduct a series of simulations to ensure that the irregular features of the score distribution arise from the processes that generate the distribution, rather than manipulation around the boundary. The first two simulations focus on the raw score distribution. We first simulate the cut-offs for each school and year drawing from the distribution of observed school specific cut-offs in each year for all other schools. This simulation allows us to assess how much of the deviation caused by dropping students at cut-offs is explained explicitly by the location of the cut-off for that school in that year, as opposed to being explained by dropping observations at natural mass points in the data where cut-offs might tend to arise. Specifically, we calculate the root mean squared error arising from differencing the distribution shown in the left and right hand sides of Figure B1 separately for each subsample, and then calculate a similar root mean squared error where we draw the cut-off randomly based on selecting from cut-offs chosen by other schools in the same year. The deviation arising from the simulated cut-offs explains 71, 62 and 60 percent of the deviation that arises from using the cut-off itself for the periods of 2006-2008, 2009-2010 and 2011-2013, respectively. In our opinion, these fractions are substantial given that application pools differ substantially across schools likely leading to differences in mass points. See Appendix Section B.2.1 for a description and results.

We next simulate the raw score distributions year-by-year by measuring the correlation between the score components and randomly drawing components from the empirical distribution of those components in a manner that preserves this correlation. This allows us to create populations of fake total scores that are not influenced by manipulation around specific thresholds. We then plot the distributions of these simulated scores. While this simulation does not perfectly replicate the raw distributions, the simulations do consistently generate similar shaped distributions for each year with significant numbers of both mass points and gaps. See Appendix Section B.2.2 for a description and results.

Finally, the centered score distributions sometimes contain cliffs where the density is high above the cut-off and then drops substantially immediately below the cut-off. We speculate that the process of admitting applicants until a quota has been met could lead to cut-offs that tend to fall on or just after mass points. To examine that possibility, for each school, we draw a random number of admissions letters to be sent out from the empirical distribution of the number of admissions each year over all years. We then use this simulated number of admissions to create a simulated cut-off score. For 2006 through 2009, the simulated centered score distributions match the actual distribution quite well, including replicating cliffs observed in the data. However, beginning in 2010, the simulated distribution becomes much smoother and the empirical distribution continues to have significant cliffs. Therefore, we replicate core results dropping the data from 2010 and beyond and all our core results are robust. See Appendix Section B.2.3.

IV.C. Balancing Tests

To further rule out manipulation at the boundary, we conduct balancing tests across the cut-off boundaries using our donut hole sample. For both the male and female samples pooled across years and schools, we regress student attributes including 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 (since the running variable contains 8th grade test score), and notably 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 that running variable with the dummy for being above the cut-off.[31] These results are shown in the top panel of Table 4. In the bottom panel of Table 4, we present balance tests on counterfactual high school attributes including per pupil spending, pupil-student ratio, average 8th grade test scores and share of students proficient based on 8th grade test scores. None of the individual variables are significant. Appendix Table A7 presents the balancing test for alternative bandwidths, Appendix Table A8 presents the balancing test for the female sample, Appendix Table A9 presents the balancing test for the male, labor market sample and Appendix Table A10 presents the balancing test for the full, male, non-donut hole sample. All of these samples exhibit balance at the cut-off.[32] Further, the typical data driven approach to selecting the size of the donut is to expand the donut whole until the sample passes balance. Therefore, passing the balancing tests with the full, non-donut hole sample provides further support of our supposition that the non-standard distribution is not due to manipulation.

V. Results

Figure 2 presents traditional reduced form regression discontinuity graphs with a centered score, fitted lines to the running variable, and the outcome mean at each score on the vertical axis. The top half of each figure presents results for male students, and the bottom half presents results for female. High school graduation rates are shown on the left hand side, and semesters of enrollment in college are shown on the right. For male students, the score means form a relatively tight scatterplot around the fitted lines with a clear discontinuity implying higher rates of high school graduation and lower semesters of college enrollment for applicants above the threshold. In contrast, for female students, there is little evidence of a discontinuity for either outcome. Figure 3 presents similar results for both average quarterly earnings and number of quarters with earnings for the labor market sample. Again, for the sample of male students only, we observe a discontinuity at the threshold with higher quarterly earnings and the number of quarters with earnings just above the threshold.

[31] As with our main RD models, these balancing tests include school by application year fixed effects and applicant town of residence fixed effects.
[32] As shown in Table A8 we fail balance at the 10% level in just two cases for the female sample, namely 8th grade average reading and math scores.

Table 5 presents the two-stage least squares estimates for the sample of male students showing the impact of attending a technical high school. The resulting treatment on the treated effects are large. In the top panel, attending a technical high school increases high school graduation rates by 10 percentage points relative to a sample average of 83%, and reduces number of semesters enrolled in college by almost ½ of a semester. While not shown, we investigated attendance at two and four year colleges separately and find similar effects for both types of schools. In the bottom panel (labor market sample), attending a technical high school results in 44 percent higher total earnings and 32 percent higher quarterly earnings. For comparison, the average quarterly earnings in the sample of male students was approximately $6,000. Attending a technical high school also results in 1.0 additional quarters with earnings relative to a sample average of approximately 10 post-high school quarters with earnings. While large, these labor market effects are reasonable given higher graduation rates and labor supply and similar to effects of skill intensive jobs programs (Heinrich 2012-13; Elliot and Roder 2017).[33]

We also examine several outcomes restricting our sample to individuals observed at age 23 or older. As shown in the top panel, by age 23, the effect on total semesters of enrollment in college decline sharply in magnitude and become insignificant. We also examine whether the student was ever enrolled in college for this sample finding near zero estimates, while estimates are negative 8 percentage points for the full sample (not reported). Initial differences in college attendance and pursuit fade-out over time as some CTHSS students who initially entered the labor market return to college. We also re-estimate quarterly earnings effects restricting our sample to quarters where the individual was 23 years or older, and find very similar effects: attending a technical high school raises quarterly earnings by 33 to 35 percent. As noted above, inference is conducted using two-way clustering at the CTHSS school by application year and at the sending town. As an alternative, we also conduct inference using finite-sample

[33] Appendix Table A11 presents the small and statistically insignificant estimates for the female sample. Appendix Table A12 presents the results for the combined sample of male and female students. Result are similar in sign to the results in Table 5 and statistically significant, but naturally smaller in magnitude.

exact randomization inference tests following Cattaneo et al. (2019). The resulting *p*-values are substantially smaller than the ones presenting in the paper (See Appendix Table A13).

V.A. Validation and Robustness Tests

In Table 6, we present reduced form estimates for selected outcomes based on alternative bandwidths. The first two panels present results based on 6-point and 8-point bandwidths, while the third panels present results for the baseline bandwidth of 10-points. The effects for most outcomes are robust across the bandwidths either very stable or bouncing up and down with bandwidth changes so that the estimates differences between the 10 and 6 point bandwidth range between no change and 20 percent increases. However, the effects for number of quarters of earnings and average quarterly earnings at age 23 or later strengthen for smaller bandwidths increasing by 62 and 81 percent, respectively. We verify that estimates for female students remain small and insignificant across bandwidths.[34]

In general, regression discontinuity results should typically be viewed as robust if they are stable or increase as the bandwidth is narrowed. Optimal bandwidths are selected as a trade-off between precision that is gained and the bias introduced as the 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. The effects for quarters of earnings and for quarterly earnings age 23 do increase considerably, 81 and 62 percent, respectively. However, as we reduce bandwidth, the estimated slope of the running variable is based on a smaller and smaller number of discrete points raising concern that the estimated slope of the running variable could be heavily influenced by one or two points. In fact, the standard errors for quarters of earnings and for quarterly earnings age 23 also have the largest increases of 45 and 70 percent respectively, so these increases may simply represent sampling error. Therefore, in order to be conservative, we continue to focus our attention on the 10-point bandwidth estimates.

[34] We present reduced form estimates so one can observe the direct responsiveness of treatment effects to changes in bandwidth. The first stage estimates for different bandwidths are shown in Appendix Table A4.

In Table 7, we present falsification tests where we move the cut-off down 10 points or up 10, 15 or 20 points using the 10-point bandwidth so that the true discontinuity is excluded from the sample.[35] Panels 1-4 of Table 7 show the estimates for different falsification tests starting with the false cut-off being set 10 points below the true cut-off and moving to 20-points above the true cut-off. In all four panels, the point estimates are substantially smaller than the reduced form estimates reported in Panel 3 of Table 6, and are always statistically insignificant.[36]

We also successfully replicate our results using the non-donut hole sample, consistent with the non-standard distribution arising from the construction of the admissions score rather than manipulation (Appendix Table A15). Further, we replicate all results in a sample dropping school by application years where the data in that cluster fail the McCrary test (Appendix Table A16), and we replicate all results for the 2006-2009 subsample due to the failure of our simulations to replicate the centered score distribution cliff in the later years (Appendix Table A17). Results are also robust to a subsample that drops applicants who applied to more than one school (Appendix Table A18), which represents about 20 percent of our sample, as well as adding back in special education students, i.e. with an Individualized Educational Program, or applied to CTHSS in 10th grade (Appendix Table A19). Finally, we re-estimate our models following Bertanha (2020) estimating separate treatment effects by school and year and calculating a weighted average. These results are shown in Appendix Table A20, and all results except for the short-run effects on semesters in college are robust. We also demonstrate that the treatment effect estimates are uncorrelated with the cut-off, see Appendix Table A20 Panel 3 and Figure A1.

For the labor market analysis, we replicate our quarterly earnings result restricting the workers in the sample to having at least two or at least four quarters of earnings (Appendix Table A21). Given concerns about outliers in earnings, we conduct quantile regressions on average quarterly earnings finding relatively stable estimates over the earnings distribution (Appendix A22). We also replicate our high

[35] The data is too thin to move the threshold down by more than 10 points.
[36] Note that we must present reduced form estimates because the first stage has no power at the false cutoffs The first stage estimates for the falsification tests are very small and shown in Appendix Table A14.

school graduation results and our finding of no effect on college in the long-run restricting the education analysis to the labor market sample only (Appendix Table A23). Similarly, since the education outcomes are unobserved for students not in Connecticut public high schools in ninth grade, we rerun our labor market sample adding those students back when they are observed in the labor market sample (Appendix Table A24). Since we only observe labor market data in Connecticut, we examine representation in the labor market sample for boundary towns, towns adjacent to the boundary towns or interior towns in Appendix Table A25. We observe modestly lower rates of being present in the labor market sample in boundary towns with all three neighboring states. Therefore, we rerun the analysis dropping observations in boundary towns (Appendix Table A26), and all results are robust.

VI. Potential Mechanisms

We begin this section attempting to isolate the role of general skills versus a more specific, short lived boost in the labor market. The first panel of Table 8 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 end of the sample. The first two columns present treatment effect estimates on quarterly earnings over each period where column 2 replicates results from Table 5. CTHSS increases average quarterly earnings by 43 percent on average in the younger sample and by a lower, but still sizable, 33% in the older sample consistent with a declining treatment effect. However, columns 3 and 4 present similar estimates for semesters in college completed during the same time periods finding 0.4 semester reduction in the younger sample and no effect in the older sample. 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 for a single semester reduces labor supply substantially over about 1/3rd of a year, the short-run treatment effects of college attendance can explain 5 percentage points of the 10 point decline in the effect of CTHSS on average quarterly earnings between the younger and older sample.

The second panel of Table 8 presents quarterly earnings estimates based on disaggregated quarterly earnings data so that we can observe the industry associated with each quarter of earnings. If the return to attending CTHSS is due to industry specific, rather than general, skills or arise because CTHSS provides students with an advantage in entering higher paying industries, most of the earnings gains should accrue across industry categories. Column 1 presents our baseline estimates for average quarterly earnings from Table 5, and column 2 presents estimates for the quarterly earnings sample where the model is expanded to include a vector of fixed effects for the year and quarter of earnings. Column 3 adds two-digit industry code fixed effects, and column 4 adds three digit industry codes. The estimated effect of CTHSS on quarterly earnings of 0.31 is very similar to the effects on average quarterly earnings of 0.32. The inclusion of two digit industry codes erodes the effect somewhat with the estimate falling to 0.24, but the further inclusion of detailed three digit industry codes has almost no effect with an estimate of 0.22. Therefore, most of the earnings returns do not arise across industries.

The last panel focuses on measures that capture or 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 sample mean rate of 93 percent. Improvements in attendance rates can be considered as a proxy for student engagement.[37] Average test scores improve by 18 percent of a standard deviation, while individually math and reading scores improve by 13 and 16 percent of a standard deviation, respectively. Note that we find some evidence (significant at 10 percent level) that attending a CTHSS school improves attendance but no evidence that it improves test scores for female students (Appendix Table A27). To quantify these effects for males, we estimate a reduced form model of log of average quarterly earnings within the bandwidth sample regressed upon attendance share, composite test score, and high school graduation plus the individual demographic controls listed in Table 1 and CTHSS application school by application year fixed effects (Appendix Table A28). Multiplying these estimates by the effect estimates for each outcome implies a 6 percent increase in quarterly earnings. Together, these estimates suggest a

[37] Students who exhibit lower engagement early in high school are more likely to become less engaged over time, learn less, and are more susceptible to dropping out (Archambaukt et al. 2009). Improving relationships with peers, teachers, and families can improve engagement at the point of transition from middle to high school (Wang and Eccles, 2012), and that increased engagement can spillover among peers (Mendoza and King, 2020).

role for observable general skills in explaining wage gains, and work against an interpretation that most of the wage gains are short-term arising from an advantage provided by the provision of skills and connections intended to facilitate entry into specific industries.

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 using the 8th grade town based counterfactual high school definitions defined earlier for Table 2. In Table 9, we present models where we interact treatment with the share of elective courses at a student's counterfactual high school that are CTE. Panel 1 presents the results using all CTE offerings, Panel 2 presents the results using the offering of CTE courses related to the trades, and Panel 3 presents results related to Human Services and Tourism. We have relatively consistent results across graduation, college attendance and quarterly earnings with declining effects on outcomes as the counterfactual high school offers an increasing share of electives as CTE or as trade focused CTE courses. In contrast, for human services or tourism, all of the point estimates on the interaction terms are relatively small in magnitude and statistically insignificant. We conduct similar estimates using other attributes of the counterfactual high school, i.e. per pupil expenditures, pupil-teacher ratio and average peer test scores, but do not find any evidence that leaving a school that had lower spending, higher-student teacher ratios or worse peers influences the treatment effect of attending a CTHSS high school (Appendix Table A29).

For comparison purposes, we examine how much of the gap between CTHSS and non-CTHSS schools is eliminated by a standard deviation increase in either CTE offerings overall or CTE trade offerings at the counterfactual high school. Looking back to Table 2, a one standard deviation increase in share of CTE courses at non-CTHSS schools is 0.067, which is 15.3 percent of the gap between CTE offerings at CTHSS and non-CTHSS schools, and so eliminating 15.3 percent of that gap in course offerings is associated with reducing the effects on college attendance by 5.3 percent and reducing the effects on quarterly earnings by 3.5 percent. Similarly, focusing on trade offerings within CTE, a one standard deviation increase is 0.023, which is only 4.5 percent of the gap in trade offerings between CTHSS and non-CTHSS schools. Focusing on the significant interactions in Panel 2 of Table 10, a one standard deviation change in share of electives in the trades reduces the effect of treatment on high school graduation by 1.6 percent and on quarterly earnings by 1.1 percent. If we extrapolate out to eliminate 100% of the difference in course offerings, the treatment effects on high school graduation is only reduced by 36 percent for trade courses, and the treatment effects for quarterly earnings are only reduced by 23% for CTE courses overall and 24% for trade courses.[38] Therefore, while a significant part of the gain from attending a CTHSS school can be attributed to increased CTHSS offerings, these findings leave substantial room for other features arising from the integrated CTE experience to affect outcomes.

Finally, we examine whether the effects of attending a technical high school are heterogeneous across different students based on reduced or free lunch eligibility, being either African-American or Hispanic, or residence in one of Connecticut's five poorest central cities. We estimate models in which we interact treatment, the running variables and the school by cohort fixed effects with the student attributes and focus on the 2SLS models in order to conduct inference on the interactions, where we use being above the threshold and its interaction with the attribute as instruments. We also estimate models using the individual treatment effect estimates for each school and year and controlling for the share of students in the applicant pool for each student attribute. As shown in Appendix Table A30, the vast majority of the interaction estimates are insignificant.[39] We also find no evidence of heterogeneous effects across schools or cohorts using the models based on individual treatment effects by school and year.[40]

VII. Discussion

In this study, we examine the effect of attending one of Connecticut's 16 stand-alone technical high schools on student educational and labor market outcomes. We find large, robust positive effects for

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[38] A second explanation could be that the return to CTE courses is non-linear and rises as more CTE offerings are provided. In those circumstances, the marginal effects of a change in traditional CTE offerings would be below the effects created by the larger share of courses in CTHSS schools. We re-estimated models incorporating the interaction of the square of the CTE variable with treatment using the interaction with being above the threshold as an instrument, but we found no evidence of increasing returns as the number of offerings increase.

[39] Only the interaction of central city residence with treatment in the model for number of quarters with earnings is significant, and this result is significant at the 99 percent confidence level. Therefore, even with the large number of tests for heterogeneity, it appears that most of the labor supply effects on the extensive margin are concentrated among students from one of Connecticut's poorest central cities.

[40] For the school fixed effects, the p-values for an F-test on the set of fixed effects range between 0.20 and 0.81 across the student outcomes. Similar tests for the year fixed effects yield p-values between 0.36 and 0.73.

male students on high school graduation and post-high school labor market outcomes. These estimates are robust to alternative bandwidths, the inclusion of controls for student attributes and a donut hole specification. We also demonstrate that the sample exhibits balance on student socio-demographic attributes. Falsification tests cannot identify similar discontinuities at false thresholds that are set above or below the true cut-offs, further bolstering the validity of the fuzzy regression discontinuity design.

Several findings are consistent with the wage gains arising from the development of general skills. First, the negative effects on college enrollment of attending a technical high school erode as we focus on older applicants, and we continue to observe large wage gains for older applicants. Second, CTHSS attendance improves key in-school outcomes such as high school graduation, attendance and 10th grade test scores that are broadly valued within the labor market. Finally, when we condition on industry fixed effects, we find that only 1/3rd of the earnings gains are explained by across industry wage differences, consistent with wage gains arising from general rather than industry specific skills.

We also examine whether these impacts differ by the CTE offerings that a student likely would have had available if they had not attended a CTHSS school. Specifically, we identify a counterfactual high school based on students who attended the same middle school and applied to the same CTHSS school, but were not admitted. We then measure the number of CTE offerings in each counterfactual school as a share of total elective course offerings at that high school. We find that students who likely would have attended a school with minimal CTE offerings benefit more in terms of quarterly earnings from attending one of the CTE schools. When focusing on CTE offerings in the trades, we find that fewer trade offerings at the counterfactual high school increases the effects of attending CTE for both high school graduation and quarterly earnings. While the influence of counterfactual offerings on returns to attending CTHSS schools are sizable, we find that closing the gap in course offering would only reduce the estimated positive treatment effects by between 1/4 and 1/3. Therefore, these findings leave substantial room for returns arising from the integrated provision of CTE in a standalone setting as provided by the CTHSS in the State of Connecticut.

The per pupil cost of educating a student at a CTHSS school was over 13 thousand dollars in real 2019 dollars, which is about $2,000 more per pupil than the state average and almost $4,000 more than the counterfactual high schools these students likely would have attended. These differences are in part explained by the smaller Student-Teacher Ratio (STR). In the National Center for Education Statistics data for the same year, the CTHSS schools report an average STR of 10.3, compared to 13.3 in other high schools, which may begin to explain part of the difference in the educational setting provided in CTHSS schools. Even at a cost of an additional $4,000 per year per student, or about $16,000 per student across four years, the high school graduation and earnings benefits would seem to easily pass any back-of-the-envelope cost-benefit test. Further, we find no evidence of larger treatment effects when student's counterfactual high schools have less resources so the treatment effect likely arises from how these resources are allocated within CTHSS schools, rather than a simple budget increase.

Our study offers some of the first quasi-experimental evidence of the treatment effects of a technical high school program that is being offered at scale in the U.S. and involves the evaluation of all schools in the program, not just schools that volunteer. Career and technical education 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 male participants programs. The evidence of the CTE effectiveness, especially in terms of labor force participation, is especially important given the declining opportunities for non-college educated workers and the declining labor force participation among non-college going prime-age males (Abraham and Kearney 2018; Aguiar, Bils, Charles, and Hurst 2017; Autor 2019; Austin, Glaeser, and Summers 2018).

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Table 1: Summary Statistics

	State	CTHSS Full Sample		Around Threshold (BW 10)	
	Mean	Mean	Std. Dev.	Mean	Std. Dev.
Female	0.49	0.46	0.50	0.45	0.50
Asian	0.04	0.01	0.11	0.01	0.10
Black	0.13	0.20	0.40	0.22	0.41
Hispanic	0.17	0.32	0.47	0.35	0.48
Free Lunch	0.33	0.66	0.47	0.71	0.46
English Learner	0.06	0.06	0.24	0.07	0.26
7^{th} Grade CMT-Reading	249	230.27	35.13	221	27.43
7^{th} Grade CMT-Math	263	243.70	35.64	234	26.99
7^{th} Grade CMT-Writing	246	230.80	28.42	225	23.64
Total Application Score	--	64.39	19.26	59.09	11.24
Application Grades Score	--	25.46	9.24	22.94	7.02
Application Attendance Score	--	7.03	4.22	6.81	4.17
Application Math Score	--	13.42	4.68	12.19	3.89
Application Language Arts Score	--	13.67	5.26	12.39	4.74

Notes: Table presents means and standard deviations of individual control variables, 7th grade standardized test scores, total application score and components of application score. Column 1 presents mean of characteristics for the state of Connecticut overall. Columns 2 and 3 present summary statistics for the full sample of students that applied to a CTHSS school. Columns 4 and 5 present summary statistics for the sample of students within +/- 10 points of the admission score threshold.

Table 2: Summary Statistics Above and Below Threshold

	CTHSS Schools		Non-CTHSS Schools	
	Mean	Std. Dev.	Mean	Std. Dev.
	Course Offerings			
Share Total CTE Courses	0.891	0.081	0.453	0.067
Share Trade CTE Courses	0.567	0.113	0.054	0.023
Share Human Services/Tourism Hospitality	0.067	0.027	0.088	0.027
	Test Scores and Schooling Inputs			
10th Grade Math Scores	243.669	12.646	235.32	19.73
10th Grade Reading Scores	229.289	11.214	228.51	17.41
Spending per Pupil	13,506	1,746	9,892	1,590
Pupil-Teacher Ratio	10.358	1.198	13.547	1.306
Share Free Lunch Students	0.362	0.145	0.463	0.257
Share Black	0.142	0.128	0.199	0.152
Share Hispanic	0.278	0.177	0.242	0.155

Notes: Table presents means and standard deviations of the characteristics of CTHSS schools and counterfactual high schools. The top panel presents the share of elective courses that are CTE courses. Columns 1 and 2 presents summary statistics for CTHSS schools while columns 3 and 4 present summary statistics for non-CTHSS schools. The first row shows the share of elective courses that are any type of CTE course. The second row shows the share of elective courses that are CTE trade courses (architecture, transportation and manufacturing). The third row shows the share of elective courses that are human service, tourism and hospitality or Family & Consumer Sciences. 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. See appendix for definitions of various CTE courses.

Table 3: First Stage Estimates (Bandwidth 10)

	Probability of Being Admitted Full Sample		Probability of Attending Men		Probability of Attending Women	
Outcome	(1)	(2)	(3)	(4)	(5)	(6)
Offer	0.863***	0.863***	0.583***	0.583***	0.523***	0.524***
	(0.0208)	(0.0208)	(0.0230)	(0.0229)	(0.0238)	(0.0245)
Controls	No	Yes	No	Yes	No	Yes
F	1718	1727	645	645	485	458
Observations	15,339	15,339	9,287	9,287	7,629	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. Columns 1 and 2 present first-stage estimates of the probability of being admitted to a CTHSS school where dependent variable is an indicator for receiving an offer of admittance and the sample includes both male and female students. Columns 3-4 present main first-stage estimates 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 and the sample is limited to male students. Columns 5 and 6 present the same information as columns 3 and 4 for the sample of female students. Specifications with controls 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. 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 Male Students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Individual-level Covariates						
Outcome	Asian	Black	Hispanic	Free Lunch	English Learner	7th Grade Test Scores	In Labor Market Sample
Offer	-0.00215	-0.00503	-0.00612	-0.00283	0.0158	-0.821	-0.00528
	(0.00416)	(0.0128)	(0.0109)	(0.0154)	(0.0125)	(0.846)	(0.0169)
Observations	9,287	9,287	9,287	9,287	9,287	6,861	9,287
	School / Town-level Covariates						
Outcome	Spending Per Pupil	Pupil Teacher Ratio	8th Grade Average Math Score	8th Grade Average Reading Score	Math % Proficient	Reading % Proficient	
Offer	148.4	-0.000553	0.447	0.800	-0.180	-0.192	
	(172.4)	(0.0779)	(0.846)	(0.857)	(0.141)	(0.127)	
Observations	1,106	9,222	3,317	3,317	8,913	8,913	

Notes: Table presents balancing tests for sample of male students. Estimates are from a RD specification using local linear regression and a bandwidth of 10. 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 8th 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 is for 2006-2013, 8th grade average math and reading scores are from 2009 -2011, Math and Reading % Proficient are measured at the town level and 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) Male Students

Outcome	(1) Grad	(2) Grad	(3) Sem Col	(4) Sem Col	(5) Sem Col 23+	(6) Sem Col 23+	(7) Any Coll 23+	(8) Any Coll 23+
Attend	0.100***	0.0996***	-0.479**	-0.476**	-0.297	-0.296	-0.0247	-0.0242
	(0.0332)	(0.0328)	(0.190)	(0.195)	(0.309)	(0.322)	(0.0419)	(0.0438)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	9,284	9,284	9,287	9,287	5,375	5,375	5,375	5,375
Outcome	Total Earnings	Total Earnings	Quarterly Earnings	Quarterly Earnings	Quarters with Earnings	Quarters with Earnings	Quarterly Earnings 23+	Quarterly Earnings 23+
Attend	0.443***	0.441***	0.326***	0.323***	1.120**	1.138**	0.345***	0.328***
	(0.0933)	(0.0921)	(0.0671)	(0.0640)	(0.492)	(0.498)	(0.101)	(0.0958)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	5,652	5,652	5,652	5,652	5,652	5,652	3,612	3,612

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. Columns with 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: Reduced Form - Alternative Bandwidths Male Students

Outcome	(1) Grad	(2) Sem Col	(3) Total Earnings	(4) Quarterly Earnings	(5) Quarters with Earnings	6) Quarterly Earnings 23+
				Bandwidth 6		
Offer	0.0588**	-0.313**	0.318***	0.204***	1.232**	0.316***
	(0.0256)	(0.142)	(0.0839)	(0.0489)	(0.517)	(0.0819)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,800	5,801	3,524	3,524	3,524	2,256
				Bandwidth 8		
Offer	0.0553***	-0.272***	0.360***	0.246***	0.978**	0.261***
	(0.0200)	(0.100)	(0.0807)	(0.0450)	(0.436)	(0.0602)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,626	7,629	4,631	4,631	4,631	2,963
				Bandwidth 10		
Offer	0.0580***	-0.278**	0.264***	0.193***	0.681**	0.195***
	(0.0183)	(0.116)	(0.0561)	(0.0371)	(0.305)	(0.0569)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,284	9,287	5,652	5,652	5,652	3,612

Notes: Table presents reduced-form RD estimates for 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 7: Falsification Tests - Reduced Form (Bandwidth 10) Male Students

Outcome	(1) Grad	(2) Sem Col	(3) Total Earnings	(4) Quarterly Earnings	(5) Quarters with Earnings	(6) Quarterly Earnings 23+
			Cutoff -10 Points			
Offer	0.0178	-0.103	0.0337	0.00197	0.0539	-0.0434
	(0.0242)	(0.117)	(0.0743)	(0.0501)	(0.284)	(0.0575)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,884	5,885	3,571	3,571	3,571	2,282
			Cutoff + 10 Points			
Offer	0.0132	-0.198	0.0891	0.0543	0.194	-0.0140
	(0.0116)	(0.120)	(0.0552)	(0.0340)	(0.208)	(0.0410)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,391	11,395	6,724	6,724	6,724	4,218
			Cutoff + 15 Points			
Offer	-0.00274	-0.0370	-0.0188	-0.00804	-0.00261	0.0345
	(0.00965)	(0.125)	(0.0611)	(0.0370)	(0.229)	(0.0444)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,050	10,053	5,916	5,916	5,916	3,650
			Cutoff + 20 Points			
Offer	0.00346	0.205	-0.0408	-0.0168	-0.274	-0.0531
	(0.00823)	(0.130)	(0.0679)	(0.0381)	(0.261)	(0.0518)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,878	8,880	5,238	5,238	5,238	3,183

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. 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 8: Mechanism - 2SLS (BW 10) Male Students

	(1)	(2)	(3)	(4)
Outcome	Ave Quarterly Earnings 22 or Younger	Ave Quarterly Earnings 23 or Older	Semesters College 22 or Younger	Semesters College 23 or Older
Attend	0.433***	0.328***	-0.401	0.0398
	(0.0739)	(0.0958)	(0.327)	(0.0902)
Controls	Yes	Yes	Yes	Yes
Observations	5,446	3,612	3,612	3,612
Outcome	Average Quarterly Earnings	Quarterly Earnings	Quarterly Earnings	Quarterly Earnings
Attend	0.323***	0.311***	0.237***	0.217***
	(0.0640)	(0.0621)	(0.0558)	(0.0545)
Controls	Yes	Yes	Yes	Yes
Industry Fixed Effects	No	No	2-digit	3-digit
Observations	5,652	63,705	63,602	63,601
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)
Controls	Yes	Yes	Yes	Yes
Observations	9,287	6,257	6,313	6,320

Notes: Table presents 2SLS RD estimates based on the sample of male students. All estimates based on local linear regression and a 10 point bandwidth. All specifications include the full set of controls listed in Table 3 and CTHSS school-by-year fixed effects and resident town fixed effects. The first panel examines two subsamples restricting to observations of earnings 6 quarters after expected high school graduation to the quarter prior to turning 23 and restricting to observations of earnings from the quarter the individual turns 23 until the end of the sample. The first two columns presents treatment effect estimates on quarterly earnings over each of these periods and columns 3 and 4 present the treatment effect on semesters in college during these periods. 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 codes The last panel presents RD estimates for 9th grade days of attendance, standardized individual and composite 10th grade reading and math test scores. 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 9: 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/Tourism 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
		(0.206)	(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 based on sample of male students. 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 1a: Probability of Being Admitted to a CTHSS School Full Sample

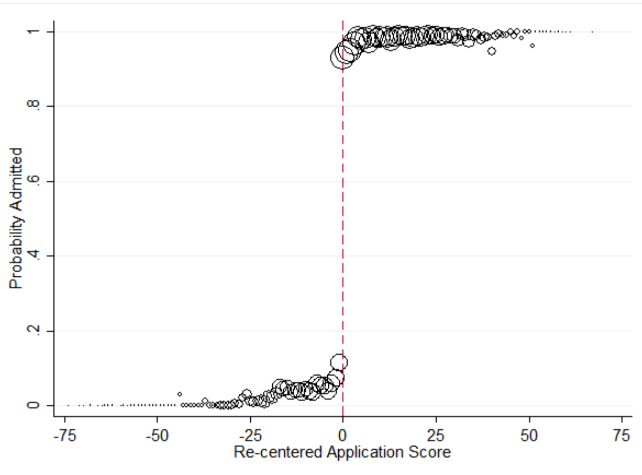


Figure 1b: Probability of Attending a CTHSS School Male Students

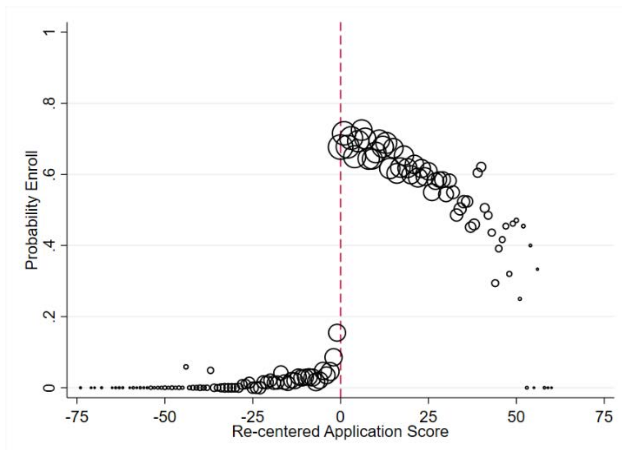


Figure 1c: Probability of Attending a CTHSS School Female Students

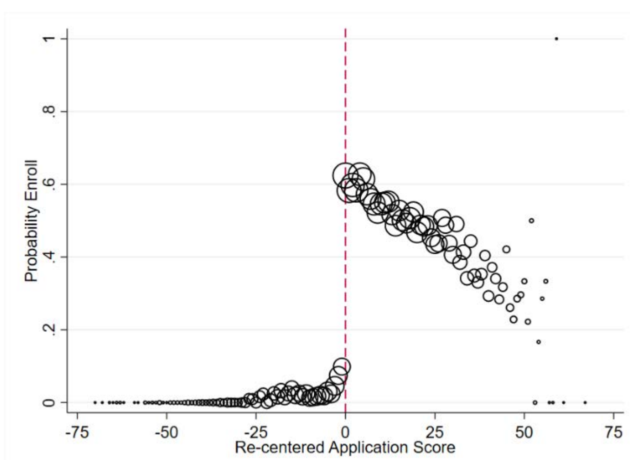
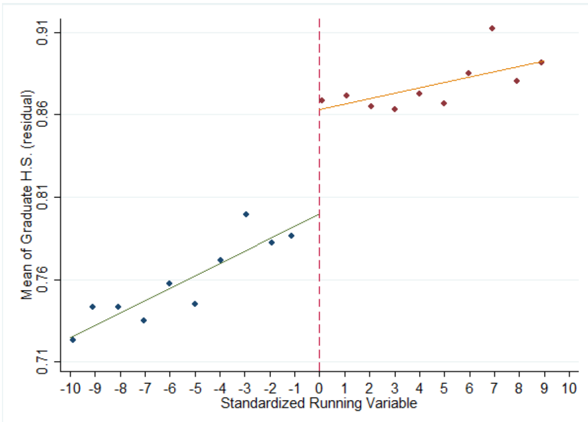
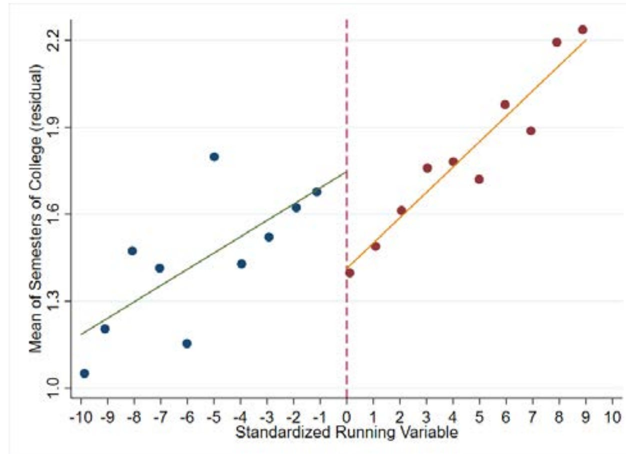


Figure 2: Reduced Form Graphs H.S. Graduation and Attend College

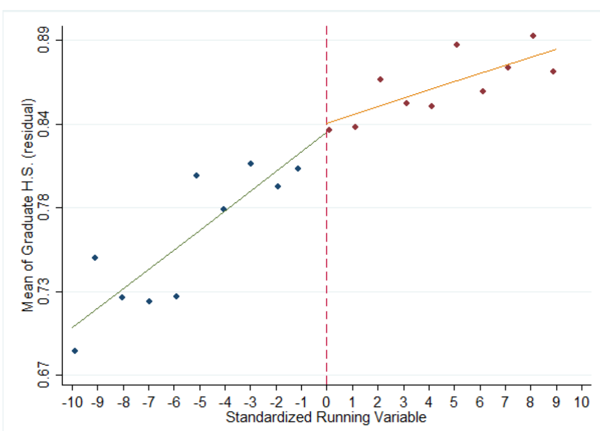
High School Graduation Male Students



Semesters College Male Students



High School Graduation Female Students



Semesters College Female Students

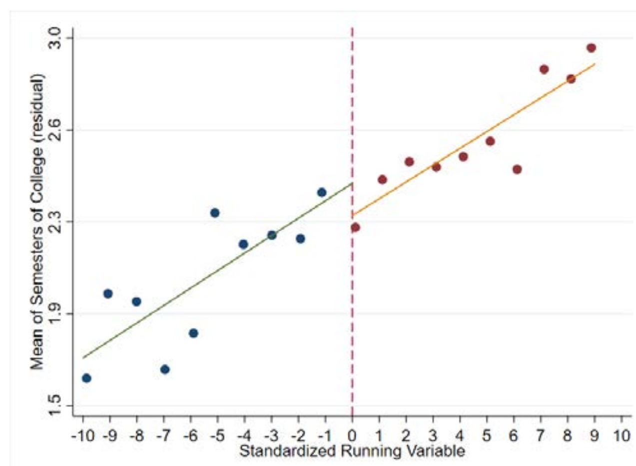
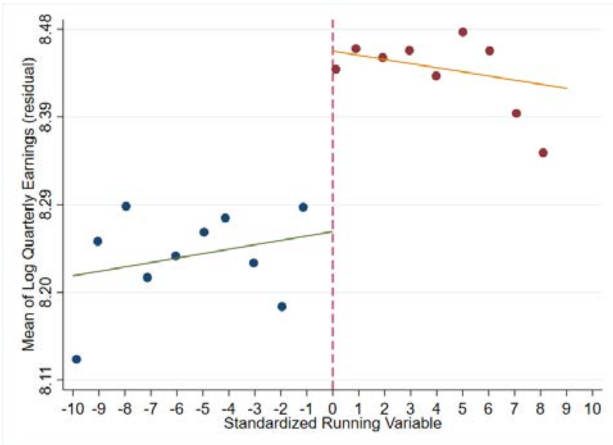
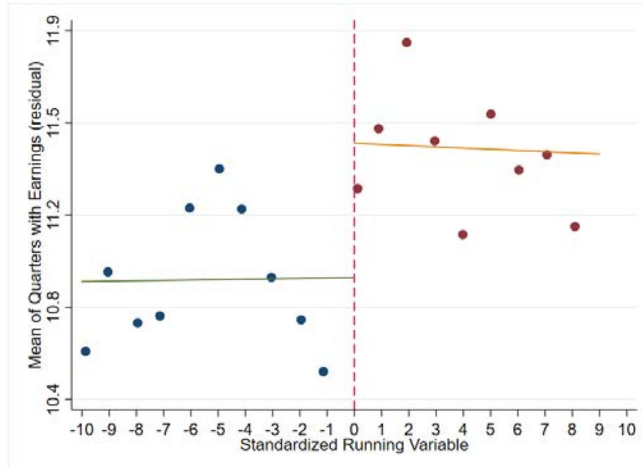


Figure 3: Reduced Form Quarterly Earnings and Quarters with Earnings

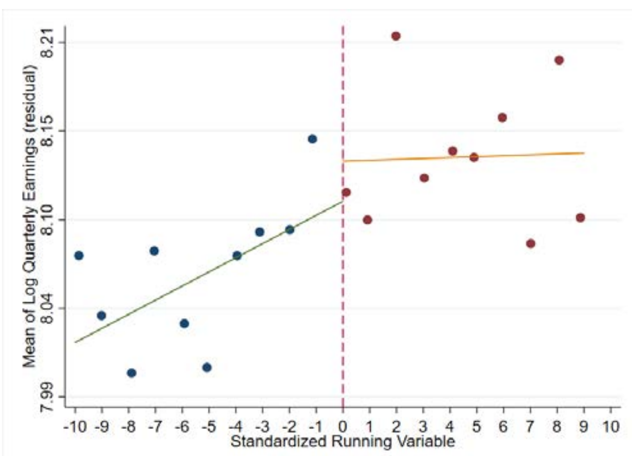
Log Quarterly Earnings Male Students



Quarters with Earnings Male Students



Log Quarterly Earnings Female Students



Quarters with Earnings Female Students