The Effect of Career and Technical Education on Human Capital Accumulation: Causal Evidence from Massachusetts

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Abstract:

Earlier work demonstrates that career and technical education (CTE) can provide long-term financial benefits to participants, yet few have explored potential academic impacts with none in the era of high-stakes accountability. In this paper I investigate the causal impact of participating in high school-based CTE on high-school persistence, completion, and the earning of professional certifications with a focus on individuals from low-income families, a group that is overrepresented in CTE and high-school non-completers. Using administrative data from Massachusetts I combine OLS, coarsened exact matching approaches with a regression-discontinuity design that capitalizes on admissions data at two schools that are oversubscribed. All estimates suggest that participating in a high-quality CTE program boosts the probability of on-time graduation from high school by three to five percentage-points for higher income students and seven percentage points for their lower income peers, with larger effects for those on the margin of being admitted to an oversubscribed school. This work informs an understanding of the potential impact of CTE program participation on the accumulation of human capital even in a high-stakes policy environment. This evidence of a productive CTE model in Massachusetts may inform the current policy dialogue related to improving career pathways and readiness.

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INTRODUCTION

Nationwide, more than one in five high-school students takes four or more of their high-school courses in a career and technical education (CTE) area, and over ninety percent of public high schools offer access to CTE programs to their students (NCES, 2013). Despite high rates of participation in CTE by high-school aged youth, relatively little is known about what constitutes high-quality CTE and whether it allows participants to accumulate meaningful human capital. What evidence does exist is from an era prior to high-stakes accountability which has potentially shifted the incentives for schools and students to make investments away from CTE programing in favor of academic preparation for high-stakes tests and four-year colleges.

Despite shifting educational incentives, there is a recognized need to expand the percentage of adults with at least some college-level training. This stems from awareness of the loss of routine lower-skill jobs that has occurred in the workplace over the last two decades, and the rising wage premiums being paid to college graduates or those with professional certificates (Goldin & Katz 2008; Kurlaender, Huff-Stevens & Gros, 2014; Trimble & Xu, 2013). Rising college wage premiums notwithstanding, many states and cities continue to face high dropout rates among high-school students, particularly those from lower income families (Murnane, 2013; Rumberger, 2011) emphasizing the need for education policy to attend to both margins. The continued importance of ensuring high school graduation as a minimum educational credential is also driven by the continued demand for non-routine low- and moderately-skilled jobs—the types of jobs that many high-school CTE participants are prepared to take (Autor, Katz, & Kearney, 2006; Autor, Levy, & Murnane, 2003; Holzer, Linn, & Monthey, 2013).

Though much prior work has shown that students who participate in CTE secondary-school programs enjoy a wage premium over similar students who did not, the educational benefits of CTE programs have been unclear (Agodini & Deke, 2004; Ainsworth & Riscigno, 2005; Bishop & Mane, 2004, 2005; Bozick & Dalton, 2013; Caterall & Stern, 1986; Dalton & Bozick, 2012; Hanushek, Woessmann, & Zhang, 2011; Kemple & Willner, 2008; Mane, 1999; Meer, 2007; Neumark & Joyce, 2001; Neumark & Rothstein, 2006; Page, 2012; Pitman, 1991; Plank, 2001; Stone, Alfeld, & Pearson, 2008). In particular, this earlier work has underemphasized the potential effects on students from lower-income families, a group that is over-represented in CTE and for whom CTE may have previously been used as a dumping ground (Fraser, 2008; Gamoran & Mare, 1989; Kelly & Price, 2009). CTE may provide an effective pathway through secondary school for students who may not otherwise graduate from high school, or provide a bridge to meaningful post-secondary education for students who would not otherwise have continued their schooling (Cullen, Leavitt, Robertson & Sadoff, 2013; Stange & Kreisman, 2014). Alternatively, CTE programs may track students into educational programs that make them less likely to complete high school or face limited employment or schooling options in the future. Thus, understanding the impact of CTE participation on students' educational outcomes is crucial to determining its place in contemporary education policy.

Massachusetts, a state with a long history of providing CTE, presents a compelling case to analyze. It is distinctive in the pathways it offers for CTE participation, while also among the majority of states that now require a minimum passing score on state assessments to earn a high-school diploma. In addition to offering CTE at comprehensive high schools where *some* of the school's students participate in CTE, Massachusetts also has thirty-two regional vocational and technical high schools (RVTS) where *all* students participate in CTE. Descriptive evidence has indicated that students at the RVTS have improved their performance on the state test used for accountability, and increased their graduation rates (Center of Education Policy, 2011; Fraser, 2008; Massachusetts Department of Elementary and Secondary Education [MA DESE], 2011). To date, however, there has been no high-quality evaluation to estimate the causal impact of these programs, nor an exploration of the mechanisms that may drive any potential positive impact.

Among previous evaluations of the impact of CTE participation, all but one—a randomized experiment—have suffered from potential selection bias because researchers did not know which factors led some students to participate in CTE and others not. In this paper I add to the small causal literature on CTE by capitalizing on several features of the Massachusetts CTE system to provide plausibly causal impacts of CTE participation. Students who wish to participate in CTE programs in Massachusetts must submit an application that includes their grades, attendance, and discipline records from middle school. Understanding how these factors underpin admission permits an evaluation that arguably suffers less from potential selection bias. Capitalizing on this process and available administrative data, I first employ OLS and coarsened exact matching (CEM) (see Iacus, King, and Porro, 2011) to estimate the effect of participating in an RVTS on student outcomes.

In a third approach I complement state-level administrative data with actual admissions records that I obtained from two schools (not currently collected by the state) and use a regression discontinuity design to isolate the impact of participating in CTE for students who just make the cutoff score used for admission to oversubscribed regional vocational and technical schools. These multiple approaches complement one another with the matching supporting greater external validity and the regression discontinuity analyses strong internal validity.

This paper also adds to the literature in two key ways. First, it focuses on whether there are different impacts of CTE participation for low-income students, who, on average, are overrepresented in CTE and have been less likely to complete high school or enroll in post-secondary education of any kind. Second, it provides some of the first estimates of the impact of CTE on educational outcomes in the era of high-stakes accountability in a state where passage of an exam is required to earn a diploma. Specifically, I address the question: *What is the causal effect of participating in career and technical training in a regional vocational and technical high school on students' accumulation of human capital?*

I focus on high-school graduation as my primary outcome of interest since it is broadly accepted as a signal of the minimum required general human capital to access full-time employment (see, for example, Clark & Martorell, 2014; Tyler, Murnane, & Willett, 2000). As an intermediate measure of persistence I also add an indicator for whether a student is still enrolled in high school in grade 10 to understand whether any potential effects on graduation are realized through stemming early dropout. To understand how graduation is impacted I also include as outcomes indicators for whether a student took and passed both 10^{th} grade tests required to earn a diploma. Finally, to gauge accumulation of specific forms of human capital I examine whether RVTS exposure impacts the probability of students earning an industry-recognized certificate.

The rest of this paper is laid out as follows. In the next section I provide more detail on the context of Massachusetts CTE as well as a review of the extant literature on the impact of CTE participation in secondary school. I then describe my analytic strategies in greater detail, report my results, and discuss my findings. I conclude with policy recommendations and suggestions for extending this research.

BACKGROUND AND CONTEXT

Massachusetts has been providing career and technical education through its public school system for over 100 years, and has developed a system of offerings quite unique among other states' systems. The distinct structure of the programs and the fact that 38% of CTE participants are eligible for free- or reduced-price lunch, whereas only about 26% of students not participating in CTE are free- or reduced-price lunch eligible, make it a compelling model to study. If this model is successful in promoting improved graduation rates among lower income students, it may prove informative to policy makers as they consider methods to reduce high school dropout and improve labor-market outcomes for such students.

CTE Treatment in Massachusetts

Many states structure high-school CTE delivery such that students who participate in a CTE program spend part of their day in a comprehensive high school for conventional academic coursework and the remainder of their day in a technical-career center—often a different building—where they engage in CTE coursework. In Massachusetts, the structure is somewhat different. Students can participate in CTE through two primary channels with about half of all participants in each: either in specialized programs embedded in comprehensive, largely college-preparatory high schools, or through regional vocational and technical high schools (RVTS) where all students participate in some form of CTE (MA DESE, 2011).

CTE delivery differs along important dimensions for students in comprehensive school and RVTS programs. For instance, at comprehensive high schools students participate in CTE coursework as part of their typical daily schedule. This means that CTE courses are intermixed with other academic coursework, and that students in academic classes are a mix of CTE participants and non-participants. In contrast, at RVTS students alternate on a weekly basis between full-time academic coursework and full-time work ion their technical area and all students in all classes take some form of CTE.

Access to CTE

Access to regional vocational and technical schools is largely determined by the location of a student's home (see map of school locations and affiliated towns in the Appendix). Of 36 schools offering CTE in a setting where all students participate in some CTE program, 27 are run as semi-independent regional school districts, five are run by the city school districts where they are located (Worcester, Springfield, Lynn, Holyoke, and Boston), and there are two county- and one state-wide agricultural schools. In total, 323 of the 353 towns and cities in the Commonwealth are associated with a regional technical school. As required by the Perkins IV act, students in the remaining towns have access to some form of CTE through their comprehensive high school. In some cases, if particular programs are not offered in their school, students can apply for a tuition transfer to attend an RVTS in another area. Approval of such transfers must be approved by the RVTS as well as the home district, which is responsible for transferring the per student allocation.

For this paper the most salient differences between the RVTS and comprehensive school CTE settings are that, all students in an RVTS participate in some form of CTE, and that the majority of programmatic offerings in RVTS fall under what Massachusetts designates as Chapter 74 approved programs. In order to receive additional funding from the state, Chapter 74 approved programs must document partnerships with representatives from organized labor and local industry leaders in the program area to inform curricula, performance evaluation standards, and equipment purchases. This public-private partnership is designed to keep training relevant and to offer programs in a manner that is consistent with local labor-market needs. More than 90 percent of programs offered in RVTS settings carry this designation, whereas roughly 60 percent of programs in comprehensive settings are Chapter 74 approved. These factors suggest that RVTS settings may be different in both their structure and quality of the programs they offer.

Evidence for the Effectiveness of CTE

Historically, CTE has been thought of as a dumping ground for lower-achieving or unmotivated students (Fraser, 2008; Gamoran & Mare, 1989; Kelly & Price, 2009). Despite such practices, prior research has highlighted a number of benefits of the programs. For instance, descriptive work by Symonds, Schwartz, and Ferguson (2011) found that students who have access to a structured repertoire of skills and experiences that better prepare them for the labor market make smoother transitions into the labor force after high school. There are also numerous studies, including one randomized experiment (Kemple & Willner, 2008; Page, 2012), that find that students who participate in a CTE program have higher earnings, on average, than similar students who attended a non-CTE program (Bishop & Mane, 2004, 2005; Mane, 1999; Meer, 2007; Neumark & Rothstein, 2006; Stern, Dayton, & Raby, 2010).

Additional research has suggested that CTE participation may also provide non-monetary benefits. Research by Kelly and Price (2009) suggests that students derive positive psychological benefits (improvements in feelings of self-worth) from the success and engagement they experience while enrolled in CTE coursework, and that CTE programs may play a role in improving student efficacy along with educational and labor-market outcomes. Supporting this idea, other research has shown that feelings of efficacy and self-worth are important predictors of student success in school (Finn, 1989), and that many students enter high school with limited feelings of efficacy (Fredericks & Eccles, 2002). Because efficacy and self-worth influence a student's engagement in their learning environment, they could have an important effect on a student's decision to remain enrolled or drop out (Agodini & Deke, 2004: Finn & Rock, 1997; Kelly & Price, 2009; Plank, DeLuca, & Estacion, 2008; Rumberger, 2011).

Despite evidence that CTE participation may promote positive financial and psychological outcomes, there is no consensus on its impact on educational outcomes. The only large-scale randomized experiment to examine the effect of CTE participation comes from the MDRC evaluation of Career Academies (Kemple & Willner, 2008). Though this evaluation (as well as an updated analysis from Page, 2012) found important long-term income benefits for those randomly offered a place in a Career Academy, there were no resulting differences between the treatment and control groups in terms of high-school graduation or post-secondary outcomes.[1] While these benefits from CTE may be examples of returns on specific human capital investments (Becker, 2009; Lazear, 2009) we also know that there are potential gains from general (Becker, 1962, 2009) human capital or signaling (Clark & Martorell, 2014; Spence, 1973) by completing high school diploma or the equivalent (Murnane, Willett, & Tyler, 2000; Tyler, Willett, & Murnane, 2000).

Earlier research on the impact of CTE programs was conducted using data on cohorts of students whose educational experiences largely predated the advent of high-stakes accountability policies, including the administration of high-school exit examinations. In more recent times, the academic requirements on students have increased, evidenced by the use of high-school exit examinations and changing diploma requirements that extend to CTE participants, Thus, I hypothesize that the implementation of high-stakes testing, and in particular the use of high-school exit examinations in Massachusetts, may have changed the way that schools offering CTE have been expected to operate.

My hypothesis is consistent with the findings of Stern (2010), as well as those of Neumark and Rothstein (2006), that suggest the impact of CTE differs depending on the structure of the CTE program itself. This also serves as motivation to extend the findings of the Career Academy experiment (Kemple & Willner, 2008). Career Academies housed one or two “themed” programs that students could opt into, but not all students in a Career Academy school participated in one of these programs, nor did all teachers (see also Stern, Dayon, & Raby, 2010). Similarly, in Massachusetts, the two methods of delivering CTE—

[1] In this study, students in both the Career Academies and the traditional schools had high levels of school completion and college attendance, and so any effects might have been more difficult to detect.

offerings in comprehensive schools versus those in RVTS—may produce different effects (see Dee & Jacob, 2006 for an investigation of the effects of high-stakes tests on educational outcomes). In this research, I examine the impact of CTE participation on academic outcomes in the presence of high-stakes educational policies in regional vocational and technical high schools in Massachusetts.

RESEARCH DESIGN

The biggest challenge to estimating the causal effects of participating (versus not participating) in CTE through a regional vocational and technical school arises because students *elect* to participate, and likely differ in both observed and unobserved ways at the time of enrollment from students who make no such choice (Heckman, 1979; Imbens & Wooldridge, 2009). I deal with the self-selection problem by using three approaches: an OLS with fixed effects for town of residence, graduation cohort, and are of technical study (or occupational cluster, e.g. – culinary arts, electrical); a matching approach (CEM) that matches on actual and proxies measures used to make admission decisions to CTE programs; and a regression-discontinuity strategy in which I capitalize on a natural experiment generated by the use of a quantitative ranking process and exogenous cutoff, to admit students to oversubscribed schools.

Each approach has strengths and weaknesses, particularly in relation to internal and external validity (Campbell, 1957). For instance, my regression-discontinuity strategy has weaker external validity, since it applies only to those students on the margin of being admitted to an oversubscribed RVTS in the two schools for which I have data, but the ensuing causal inferences have much stronger internal validity. By contrast, my OLS and CEM approaches have stronger external validity because they estimate the effects of CTE participation for a larger group of students in Massachusetts, though they have weaker internal validity, because of the restrictive assumptions underlying these strategies.

Dataset and Sample

I use data from the comprehensive Student Information Management System (SIMS) provided by the Massachusetts Department of Elementary and Secondary Education (DESE), for the academic years spanning fall of 2001 through spring 2012. These eleven cohorts include over 500,000 students in grades one through 12, who are followed longitudinally for as long as they remain in the Massachusetts public schools. For both my OLS and matching analyses, I include in my analytic sample students who, if they had graduated from high school "on time," would have done so in the spring years of 2008 through 2012 (approximately 350,000 students). My sample does not include students who are eligible to take an alternative assessment based on their disability status. My sample provides me with very high power (>.95) to detect very small effects at usual levels of Type I error (α=.05).

For my RDD analyses, I supplement the SIMS with student level, school-specific application and admissions records from two schools. These schools have been oversubscribed for at least three years during the last decade and were forced to admit fewer students than had applied. Though nearly 13 schools are oversubscribed most do not maintain historical records of their admissions data. In addition, several schools have been oversubscribed for only a few years and do not yet have outcome data for their students. My RDD sample includes over 2,000 students from two participating RVTS, with about 1,000 of those students assumed to be just above, or just below, the admissions cutoff. With this sample and my RD analytic strategy, I am able to detect an effect size of 0.34 at a power of 0.80 and traditional levels of Type I error.

Descriptive Statistics

In general students who participate in any form of CTE tend to differ from their non-CTE counterparts. In Table 1, I compare descriptive statistics pertaining to student demographics and middle-school characteristics, exposure to CTE, and CTE credential attainment for students in three educational settings: non-CTE programs, CTE programs in comprehensive high schools, and CTE in regional vocational and technical schools (RVTS). The first three columns of Table 1 contain summary statistics for these settings in ex-urban areas, while columns four through six document the analogous figures for students who reside in cities. I highlight this distinction between cities and outlying areas because of clear differences in the rates of free- or reduced-price lunch eligibility – my key measure of low-income status – across these settings.

Students in any form of CTE are less academically accomplished (measured by test scores), and are more likely to be male, eligible for free- or reduced-price lunch, and to have an identified disability. Among ex-urban students, it is worth noting that students in comprehensive CTE and RVTS appear similar on nearly all observables, except their mean probabilities for on-time graduation. This distinction is not present in the cities, where students in comprehensive CTE programs appear to be of higher ability (with better graduation rates) on average than their peers in the city RVTS. Interestingly, though the city comprehensive programs have higher-ability students, they are descriptively less likely to earn an industry-recognized credential.

Measures

My primary outcomes of interest are dichotomous indicators (*GRAD4, GRAD5*) of whether a student graduated from public high school in Massachusetts within four or five years, respectively, of beginning ninth grade, (=1 if they were reported as graduating; 0 otherwise).[2] Though the five-year graduation outcomes are most relevant I favor the four-year window as it allows me to look at more cohorts. To test whether dropout occurs early in high school I include an indicator of whether a student is still enrolled in high school in grade 10 (*ENROLL10*) as an intermediate outcome. In addition, dropout from high school may be synonymous with going untested on the state test required for graduation. As a result I define my test-based outcome as an indicator of whether a student passed both required exams (*PASS*), where students with missing test scores are coded as zeros on both outcomes. My final outcome of interest is an indicator of whether a student earned an industry-recognized certificate (IRC) while in high school (*IRC*). IRCs include Microsoft Office, Cisco Systems, and Serve-safe certifications that signal specific skills and credentials potentially valuable to employers in specific industries.

In my OLS and matching approach, my key question predictor and measure of exposure to the treatment, is a dichotomous indicator of whether a student enrolled in a regional vocational and technical school during their 9th grade year (*RVTS9*).[3] I argue that students in both treatment and control conditions should be equivalent in expectation of future outcomes, when matched on the criteria for CTE eligibility in 8th grade. To explore

[2] About 3% of the sample transfer out of state before I observe whether they graduate from high school. I propose to test the sensitivity of my results by defining these students as graduates or non-graduates.
[3] I choose this definition to minimize selection bias. Conditional on having no prior formal exposure to CTE, students who experience CTE in grade 9 or not may choose to exit or enter CTE in a subsequent year. By defining exposure as a binary measure of exposure in grade 9 I seek to minimize bias related to post-grade 9 selection into or out of CTE.

potential heterogeneity in impact by urbanicity, I define a RVTS as being a city school (*CITY*) according to the definitions provided by the state.[4]

In my RDD approach, I define the forcing variable as a student's score on their application for admission (*SCORE*), and also generate a dichotomous indicator, *OFFER*, to describe whether a student was offered a seat in an oversubscribed RVTS. The variable *ENROLL* is a dichotomous indicator describing whether a student accepted the offer to attend a CTE program after participating in the admissions process.

All applicants to RVTS schools are evaluated on three elements of their middle-school experience: transcripts, attendance, and discipline record. I use the following prior measures to match the students in the treatment and control groups: (a) 8th grade attendance (total days present, *DAYS*), (b) academic performance (as measured by 8th grade MCAS score in mathematics, *MCAS_M8*), and (c) disciplinary record (total instances of in- or out-of-school suspension, *IN_SUSP* and *OUT_SUSP*, respectively). The attendance and discipline records are identical to those used in evaluating eligibility for CTE, while the test scores are an imperfect buy best available proxy for prior academic performance. In addition to the criteria used to screen applicants for CTE participation, I use several other measures on which to match treated and control students including indicators of gender, disability status, and English-language learner status. Importantly, I include a dichotomous indicator for whether a student is eligible to receive free- or reduced-price lunch (*FRPL*) as my critical indicator of poverty. Actual indicators of whether a student applied and was

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[4] I treat the RVTS in Worcester as an exurban school. It's infrastructure, offerings, and performance standout as substantially different from any of the other four city schools. My overall findings are not sensitive to this decision, though pooling Worcester with other city technical schools does partially wash out the otherwise zero impact of RVTS in cities.

denied are not available in the administrative data. In my OLS and matching approaches I rely on similarities in these other observable dimensions to make my treatment-control matches. Finally, to improve precision, I include several other exogenous demographic covariates, hypothesized to be related to my outcomes, including indicators of race and whether a student is an English-language learner.

Data Analysis

I first produce estimates using OLS by specifying the following model:

$$\mathrm{Y_{igrc}} = \alpha_0 + \alpha_1 \mathrm{RVTS9_{igrc}} + \alpha_2 \mathrm{FRPL_{igrc}} + \alpha_3 (\mathrm{FRPL} \times \mathrm{RVTS9_{igrc}})$$

$$+\mathbf{X'_i}\boldsymbol{\gamma} + \pi_\mathrm{g} + \tau_\mathrm{r} + \omega_\mathbf{c} + \varepsilon_\mathrm{igrc} \quad (1)$$

In this model, $\mathrm{Y_{igrc}}$ is the generic outcome *Y* for a student *i* in cohort *g* from town *r*, in occupational cluster *c*. In this model, the parameters π_g**,** τ_r**,** $\omega_\mathbf{c}$ represent fixed effects for cohort, town of residence, and occupational cluster, respectively.[5] The parameters of focal research interest are α_1, which represents the population relationship between CTE treatment on the probability of achieving the outcome for a student who is not low-income, and the sum of parameters α_1 and α_3 which represents the analogous relationship for a student whose family is lower income. All of my estimates use heteroskedasticity-robust standard errors clustered at the high-school level to account for a correlation of errors related to attending the same high school.

In my matching analysis I follow the analytic strategy of Iacus, King, and Porro (2011), and use coarsened exact matching (CEM) to produce plausibly causal estimates of

[5] My preferred specification does not include the fixed effects for occupational cluster as it is not possible to match on this criteria since no one in the counterfactual setting is associated with an occupational area. My results are not sensitive to the exclusion of these fixed effects.

the effect on my outcomes of participating in a CTE program in Massachusetts during high school. Choosing this non-parametric matching method favors variables known to be used in selection in a way that does not force a functional form on the selection process, in order to predict membership in treatment or control groups. Using sensible substantive judgments, I create "coarsened" categories within these covariates, and stratify participants. The intersection of these strata creates cells within which treated and control units are similar in their values of the multiple covariates and I therefore assume are also homogenous in the risk of selection (the assumption of "unconfoundedness"). I assume that the remaining variation in the outcomes is plausibly exogenous (Imbens & Wooldridge, 2009) and I can identify and estimate the causal effect of CTE participation on students' educational outcomes using CEM by re-fitting my model (1), but incorporating weights generated by the CEM algorithm to weight treatment and control units appropriately within each of the matched strata.[6]

In my regression-discontinuity approach, I estimate the causal effects of participating in CTE by capitalizing on student enrollment in RVTS that are oversubscribed. During the admissions process, student applicants to oversubscribed RVTS receive an admission ranking based on a composite score made up of multiple application criteria, and are admitted one-by-one, highest to lowest, until all seats are filled. Because the last students who are admitted differed very little in their overall admissions score from students who just missed being offered a spot in a RVTS, I posit that the similarity among students at the margins of admission made them arguably equal in expectation at the

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[6] To test the sensitivity of my estimates generated by CEM, I employ several other matching estimators to confirm that the magnitude and significance of my results are not driven by my choice of matching estimator.

admit/non-admit discontinuity on the admissions-score "forcing variable" (Imbens & Lemieux, 2008; Murnane & Willett, 2011). I capitalize on this difference in treatment among arguably equivalent groups of students to estimate the causal effect of the CTE treatment at the discontinuity, using a regression-discontinuity (RD) approach.

I implement a standard fuzzy regression-discontinuity design (Imbens & Lemiux, 2008 ; Murnane & Willett, 2011), using two-stage least squares within a local-linear regression framework in conjunction with a triangular kernel. In the first stage I model the probability that a student receiving an offer of admission takes up the offer and enrolls. In the second stage I capitalize on the exogenous variation in enrollment, carved out by my instrument (Angrist, Imbens, & Rubin, 1996), to estimate the causal effect of enrollment on student outcomes. I specify my first-stage linear-probability model, as follows:

$$\mathrm{P(ENROLL = 1)_{ics}} = \alpha_0 + \alpha_1\mathrm{OFFER_{ics}} + \alpha_2\mathrm{CSCORE_{ics}} + \alpha_3\mathrm{CSCORE} \times \mathrm{OFFER_{ics}} + \mathbf{X}'_{\mathrm{i}}\boldsymbol{\theta} + \varphi_{\mathbf{c}} + \gamma_{\mathbf{s}} + \delta_{\mathrm{ics}} \quad (2)$$

for student *i* in cohort *c* in school *s*, where φ and γ represent the fixed effects of cohort and school, and δ is a residual. In this model, I re-center the student's admissions score at the unique admissions cutoff used in their particular school and year (*CSCORE*). Standard errors are clustered on the discrete values of the forcing variable (*CSCORE*) (Lee & Card, 2008).

My second stage model, takes the form:

$$\mathrm{Y_{ics}} = \pi_0 + \pi_1\mathrm{EN\widehat{ROLL}_{ics}} + \pi_2\mathrm{CSCORE_{ics}} + \pi_3\mathrm{CSCORE} \times \mathrm{OFFER_{ics}} + \mathbf{X}'_{\mathrm{i}}\boldsymbol{\Psi} + \varphi_{\mathbf{c}} + \boldsymbol{\gamma}_{\mathbf{s}} + \varepsilon_{\mathrm{ics}} \quad (3)$$

where $\mathrm{Y_{ics}}$ represents the generic outcome. The parameter of interest is π_1, representing the population causal effect of participating in an oversubscribed CTE school on later outcomes among students at the margins of being admitted. I estimate these models using a triangular kernel and preference the optimal bandwidth suggested by Imbens and Kalyanaraman (2012) when interpreting results (see also Calonico, Cattaneo, & Titiunik, 2013 for a discussion of optimal bandwidth choice).

RESULTS

I find that students who participate in an RVTS have better graduation and enrollment outcomes, higher probabilities of earning industry-recognized credentials, and no difference in the probability of passing both exams required to earn a high-school diploma than similar peers who do not attend these schools. The effects are largest for students from lower-income backgrounds. Though the point estimates and interpretation of my results differ somewhat by approach, the positive impact of RVTS participation is confirmed in general and for those on the margin of admission at a subset of oversubscribed schools. Importantly, in the overall sample the positive effects do not appear to be driven by the oversubscribed schools , though the effects do appear to differ by whether a school is city-run or part of a regionalized semi-autonomous school.

OLS & Matching Estimates

In Table 2, I present both my OLS (columns one through four) and coarsened exact matching estimates (columns five through eight) of the impact of RVTS participation on student outcomes.[7] In all cases the appropriate reference group includes students who did not participate in CTE in an RVTS in grade 9, including nonparticipants as well as those in

[7] In the appendix Table A1 I provide evidence that my matching estimates are not sensitive to my choice of matching method.

CTE in a comprehensive setting.[8] In the first row are estimates of the aggregate effects of RVTS participation, and in rows two through five are estimates that show heterogeneity in the impact by whether, in 8th grade, a student was free-lunch eligible, and whether they resided in one of the five districts with city-wide technical high schools.

My OLS and matching estimates suggest that CTE participation in an RVTS is associated higher probabilities of graduating on time from high school, remaining enrolled in high school through grade ten, earning an industry-recognized certificate, and passing both exams required to earn a diploma.[9] Matching estimates confirm the same findings, though with more modest magnitudes. Importantly, my analysis of heterogeneity of effects by income and urbanicity suggests that students who were free-lunch eligible benefited more on all outcomes, but that the students in the five city schools likely benefited to a lesser extent though these estimates are less precise. In particular, students in the city schools are less likely to earn an industry-recognized certificate than their peers in the ex-urban areas, though I document below this may in part be due to differences in program offerings.

Regression-Discontinuity Estimates

To establish the internal validity of my RD estimates, I demonstrate that students immediately on either side of the discontinuity for admissions are similar on observable characteristics, and verify that the forcing variable is smooth and continuous at the cutoff, to satisfy the assumption that an applicant's position cannot be manipulated relative to the offer threshold. In Table 3, I present evidence to suggest that my treatment and control

[8] See Table A2 for evidence that the effects I estimate hold up even when matching to similar students who were in CTE in a comprehensive setting, suggesting the effects are about delivery method, not just curriculum.
[9] My results are not sensitive to my use of a measure of graduation in five years (see Appendix Table A3).

groups are equal in expectation on a selection of observable characteristics. I fit equation (1) replacing the outcome with each covariate, and find there are no meaningful differences in the mean values of these covariates across the threshold.[10] To establish the continuity of the forcing variable, I display a scatterplot of the density of students at each application score value on the re-centered forcing variable (Figure 1), and overlay that scatterplot with a smoothed trend in the density on either side of the exogenous cutoff used for admissions. Neither in the combined distribution (Panel A), nor in the distributions of the forcing variable within individual schools (Panels B and C) do I observe evidence of manipulation.

The process used to generate the application scores also support the internal validity of the research design. First, students received points (according to a fixed set of rules) based on their middle-school academic performance, attendance, and disciplinary records. In addition, the fourth criteria, middle-school counselor rating, was submitted by the middle school and without knowledge of the cutoff that was ultimately be used to admit students, and could therefore not be used to manipulate a student's position relative to the cutoff.

The most credible threat to internal validity stems from the admissions interview where administrators interviewed applicants however, conversations with administrators and publicly available state documents (MA DESE, 2011), suggests such threat are minimal. First, interviewers did not know what score was to be used as the admissions cutoff, and so could not have reliably manipulated a student's position relative to that cutoff. Second, interviewers followed set protocols with predetermined questions and processes for

[10] In the appendix I include a graphic to demonstrate that my conclusion of equality of expectation is not likely dependent on functional form.

awarding points. Thus, the interview score, and a student's position relative to the ultimate admissions cutoff, should not have been subject to manipulation by either the applicant or the school.

First Stage Results

The potential credibility of my instrumental-variables approach relies on a sufficiently strong first-stage that must indicate that having received an offer of a seat at a regional vocational and technical school results in higher probabilities of enrollment in a RVTS for students near the admissions cutoff. I demonstrate this discontinuity in actual enrollment in Figure 2 where I display the sample probability of having enrolled in a RVTS in 9th grade as a function of a student's re-centered application score, for both schools combined, as well as individually. I indicate the point at which students first receive an offer ($CSCORE$>=0) by the vertical dashed line and demonstrate the clear jump in probability of attending at the cutoff.

In Table 4, I include regression-based estimates in the difference in probability of enrolling in a RVTS in 9th grade between students who received, and did not receive, an offer of admission at the discontinuity in cut-score. The parameter estimate of interest represents the jump in the average probability of enrolling in an RVTS for students who are just eligible to receive the offer relative to those who just missed receiving an offer. My estimates of the first-stage discontinuity suggest a clear jump in the probability of attending a RVTS as a function of receiving the offer of admission. Point estimates of this jump are stable across choices of bandwidth as well as across the combined and individual school samples. In the combined sample my F statistics always exceed the threshold of 10 suggested by Stock, Wright, and Yogo (2002), however, at smaller bandwidths the instrument is weaker in the individual schools.

Causal Impact of an Offer of Admission on Student Outcomes

In Figure 2, I provide visual evidence of discontinuities in my four outcomes of interest at the admissions cut-off. There is an apparent discontinuity in the probability of graduating on time for students near the cutoff who received an offer of admission, with similar discontinuities in remaining enrolled by 10th grade and in the probability of earning a certificate, and inconclusive evidence for whether a student passes both graduation exit exams. This graphical evidence suggests an impact of receiving the offer to attend an RVTS.[11]

Building on my first-stage results and the visual evidence of an intent-to-treat effect, I present in Table 5 my IV estimates of the effect of attending a RVTS. My estimates show a large and positive impact of RVTS participation near the admissions threshold. Using the IK bandwidth I interpret the statistically significant point estimate of 0.15 in column one as a 15 percentage point jump in the probability that a student who is offered admission and attends an oversubscribed RVTS graduates on time relative to their peers who just missed receiving this offer. I find similar boons to students who are just admitted and attend an RVTS on their probability of remaining enrolled through grade 10, and earning an industry-recognized credential.[12] My RD estimates of the effect of RVTS participation on passing both required exit exams is much less precise. Across outcomes, the statistical significance

[11] For brevity I omit the regression-based evidence of these reduced-form intent-to-treat effects in the main body of the paper, but they are shown in Table A4 as part of the specification check for non-linearity.
[12] In addition to not being highly sensitive to bandwidth choice these results are also not sensitive to the inclusion of a quadratic term in the forcing variable (Gelman & Imbens, 2014), see Table A4.

of my IV estimates is not sensitive to the choice of bandwidth, however, narrower bandwidths do produce larger (and in some instances less plausible) magnitudes. However, the clear visual evidence of the effect suggests this is likely do to the smaller sample sizes at narrow bandwidths.

Since my RD sample consists of only two schools that represent 13 (of 26) such oversubscribed RVTS, I present evidence in Table 6 that these schools are somewhat comparable to their peer schools. While school 1 is less racially diverse and higher achieving than the average peer school, school 2 is a clear counterweight in both of these dimensions. Given my limited sample, I argue that the schools—and the associated results—have reasonable though limited external validity in the context of non-urban Massachusetts regional vocational and technical schools.

DISCUSSION

The results from all three of my analytic strategies suggest a clear benefit of attending an RVTS in grade 9 as measured by indicators of both general (graduation) and specific (certificate completion) forms of human capital. I find larger effects on graduation for students in suburban and rural areas, and those who are free- or reduced-price lunch eligible. The benefits are similar for my intermediate measure of persistence, enrollment in grade 10, as well as for the earning of an industry-recognized certificate. Less clear is whether RVTS participation impacts the probability that a student passes both exams required for graduation, since the estimates in the large sample are comparable in magnitude to the RD estimates, but the RD estimates are not significant in any specification. Also noteworthy is the lower rate of certificate completion among students in city schools, which have a higher density of students with lower incomes. Below I consider the policy relevance of these findings, and explore potential explanations for this heterogeneity in effects.

While there are relatively few high-quality evaluations of systematic high-school interventions that show effects, my estimates of the effects of attending an RVTS in Massachusetts are comparable to existing evidence on these other interventions. The aggregate sample average treatment effect using my CEM approach produced an estimate of 2.5 percentage points whereas my RD estimate was closer to 15 percentage points. These estimates are not entirely inconsistent with Deming and colleagues (2011), who demonstrated that changes in school-choice options in Charlotte-Mecklenburg resulted in a 5.5 percentage point boost in the probability that lottery winners graduated from high school. Similarly, MDRC's study of Talent Development High schools (Kemple, Herlihy & Smith, 2005) found a boost in the probability of high school completion of eight percentage points for students who were randomly admitted to the schools. The New York small schools program (Bloom & Unterman, 2014) saw increased probabilities of graduation on the order of 9 percentage points over a baseline probability of graduation of 59 percent. In addition, Rodriguez-Planas (2012) finds a bump of five percentage points in the probability of graduating high school as a result of a comprehensive mentoring program in grades 9 through 12. While my IV estimates suggests a larger effect than these other interventions, the standard errors of my estimates suggest that I cannot rule out comparable effects.

The primary policy concern generated by these findings is that the effects for both lower and higher income students outside of the cities do not extend to the city technical schools. One potential explanation for these heterogeneous effects may relate to access to and quality of the programs available to students in lower income communities. Other literature that explores the impact of choice-based educational settings suggests that choice can produce positive impacts provided that the outside option represents an improvement in quality over a student's assigned school (Angrist, Pathak, & Walters, 2013; Deming, 2011; Deming et al. 2011; Fryer & Dobbie, 2013). Therefore, if RVTS in the cities don't offer a higher quality environment than a student's assigned school we may not expect to find positive effects, even if enrolling in and attending an RVTS has the potential to improve a student's match to an academic or technical program of interest. Said another way, if peer effects and environment don't improve concurrent with an improved match, the potential benefits may not be realized (Hastings, Kane, & Staiger, 2010; Holzer, Linn, & Monthey, 2013). My descriptive evidence from Table 1 suggests that in city RVTS students are lower performing on average than their peers in comprehensive CTE and non-CTE programs, whereas this is not true in ex-urban settings. This fact may support the hypothesis that fundamental differences in quality across these two settings could drive differences in effects.

To explore whether the overall effect of attending an RVTS is related to the quality of the environment, I estimated heterogeneous effects of RVTS attendance by whether a school is oversubscribed using oversubscription as a proxy for quality revealed by the applicants. In Table 7 I present evidence of heterogeneous effects by oversubscription status for all four outcomes These estimates suggests that, on average, I cannot rule out that the association between student outcomes in oversubscribed schools is similar to that of students in schools that were not oversubscribed. In the cities being in an oversubscribed school does seem to indicate better outcomes, but this is driven by the one oversubscribed school among the five, Worcester Tech. From these point estimates I conclude that simply being oversubscribed does not explain overall effects and may not indicate systematic differences in the quality of the RVTS.

Other differences in quality across RVTS may be represented by program offerings, specifically those that are Chapter 74 approved and receive additional state funding, as well as those in high-growth new technology fields. In Table 8 I present a summary of CTE offerings in both cities and ex-urban areas to explore systematic differences in quality by program type. The first ten rows display counts of program offerings and identify those that are Chapter 74. Columns one and three present the total number programs offered for city and ex-urban schools, respectively. Because Worcester Technical High School was the sole oversubscribed city school, column three documents the number of total offerings in each cluster for city schools, less Worcester. In the bottom two rows I present the average number of programs per school as well as the share of programs that I define as "new tech" programs, or programs that correspond to industries that are projected to have high job demand or growth. While the average number of programs offered in city and exurban schools appears comparable, city RVTS have a smaller share of program offerings that are Chapter 74 approved in "new tech" areas. Just one in four overall program offerings are in new tech areas, compared to exurban schools that have more than one-third of programs. These differences suggest that heterogeneity in the effect of RVTS participation by urbanicity may, at least in part, be due to differences in program quality.

Conditional on schools of similar quality, there are several other elements of the RVTS that may drive their apparent positive impacts on student persistence through high school. For instance, substantial exposure to the same instructors across multiple years provides the potential for informal mentoring which has been shown in other settings to improve student's attachment to school (Black, Grenard, Sussman, Rohrbah, 2010; DuBois, et al, 2011). This is also consistent with other work that has shown that CTE participation can positively impact students' attachments to school (Plank, DeLuca, & Estacion, 2008).

Conversations with administrators at RVTS and in CTE programs in comprehensive high schools suggested that the RVTS structure may allow educators to address preparation for exit exams in a different manner. Administrators at the RVTS reported fewer instances of students being pulled out of their CTE coursework to remediate learning needs in tested subjects, relative to administrators in comprehensive schools. Though students in all school settings must earn a passing score on the MCAS to graduate, the administrators in RVTS do not describe this practice of substituting time away from technical training in favor of additional academic instruction. In fact, RVTS students spent effectively half the same total days in ELA and math instruction as their comprehensive school peers based on their alternating week schedule. Notably, though RVTS administrators fewer pullouts from CTE coursework for remedial math and ELA instruction, they indicated awareness that the prior test-score performance of their incoming students was considerably lower than those at the comprehensive schools (see Table 1). In response they described a practice of administering low-stakes reading and math diagnostic tests to incoming students to determine placement in ELA and math coursework. Of the nine schools I visited all described some version of this practice to ensure that students scored at or above the state-mandated levels to earn a diploma.

Finally, the apparent benefits of the RVTS setting may originate from the reduced stigma associated with participating in these programs relative to a comprehensive school setting. While my empirical evidence cannot wholly support this claim, prior work does suggest that students have historically been negatively selected into CTE (Donahoe, D., & Tienda, M., 1999). Such negative selection could increase the opportunity for stigma if CTE participation was synonymous with lower academic performance or misbehavior. Since all students in an RVTS participate in a CTE program, the risk for stigma associated with CTE in general is not possible, though between-program stigmatization could still occur.

Cost & Payoff

The cost of providing CTE education through RVTS and general labor market demands for students with vocational and technical skills are also important considerations when interpreting my findings. Though my results suggest that expansion of CTE offerings could benefit students in the Bay State, these likely diminishing benefits from expansion must be weighed against the reality that CTE programs are more expensive to offer (nearly $19K per pupil/year, compared to about $13K in non-CTE settings).

Findings from earlier research and available labor-market data suggest that there may be net gains from making the additional investment in CTE for students. For instance, we know that on average high-school graduates in Massachusetts have expected annual earnings that exceed those of non-graduates by about $9K (Massachusetts Budget Office, 2011). If students who are admitted to a RVTS in the current admissions framework are 10 percentage points more likely to graduate than their peers who graduate at a rate of about 60%, then the expected salary for a student who is admitted is about $500 per year higher for the student who is admitted to the RVTS. Based on increased earnings across the lifecycle alone, the additional cost associated with providing access to an RVTS could be justified. Additional public benefits may be realized through the reduction of social costs via increased graduation rates. Existing evidence suggests that graduating from high school and winning a lottery through a school choice system can reduce the social costs associated with crime (Deming, 2011; Lochner & Moretti, 2004; Petit & Western, 2004).

Conclusion

Using rich administrative data from Massachusetts, I provide plausibly causal estimates of the benefits to high-school persistence and graduation and the earning of industry-recognized credentials for students who participate in a unique model of CTE where all students take some form of CTE. My estimates are among the first to capitalize on knowledge of how students opt into a specific form of CTE delivery in high school as a means to understand the impact of CTE participation on human capital accumulation in high school.

As policymakers consider ways to offer a breadth of educational pathways for their students, the Massachusetts model of regional vocational and technical schools likely presents a novel and potentially effective delivery mechanism for CTE specifically, and high-school curricula in general. Importantly, these effects are derived in a policy environment that requires evidence of basic competencies for all graduates, suggesting that the benefits of earning a diploma through an RVTS in Massachusetts do not require the sacrifice of accumulating a minimally acceptable level of general human capital.

The descriptive evidence that I provide surrounding potential differences in access to these programs as a function of student's eligibility for free- or reduced-price lunch is also novel in that it focuses on the students who may most benefit from skills and certifications that carry value on the labor market. Furthermore, because students from lower income families are more likely to drop out of high school, demonstrating the benefits of CTE participation on this outcome is particularly compelling from both an educational and social policy perspective.

Despite working with a rich dataset and several identification strategies, my analyses remain limited in their respective internal and external validity. However, the robustness of my findings to multiple identification strategies bolsters my claim that the relationships I observe are real. Given the renewed policy focus on CTE, as well as preparing students to be college and career ready, states and districts should consider how the Massachusetts regional vocational and technical school models may be adapted to their own educational settings. Finally, there is ongoing work suggesting that there are clear labor-market returns to some forms of certificates that can be earned in community-college settings (Kriesman, et al, 2013; Kurlaender, Huff-Stevens & Gros, 2014; Trimble & Xu, 2013). These papers suggest, unsurprisingly, that returns are higher in some areas (health services) than in others. Policymakers and researchers should continue to explore the potential for alignment in CTE-related offerings across the secondary-post-secondary threshold.

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Figure 1: Distribution of Application Scores

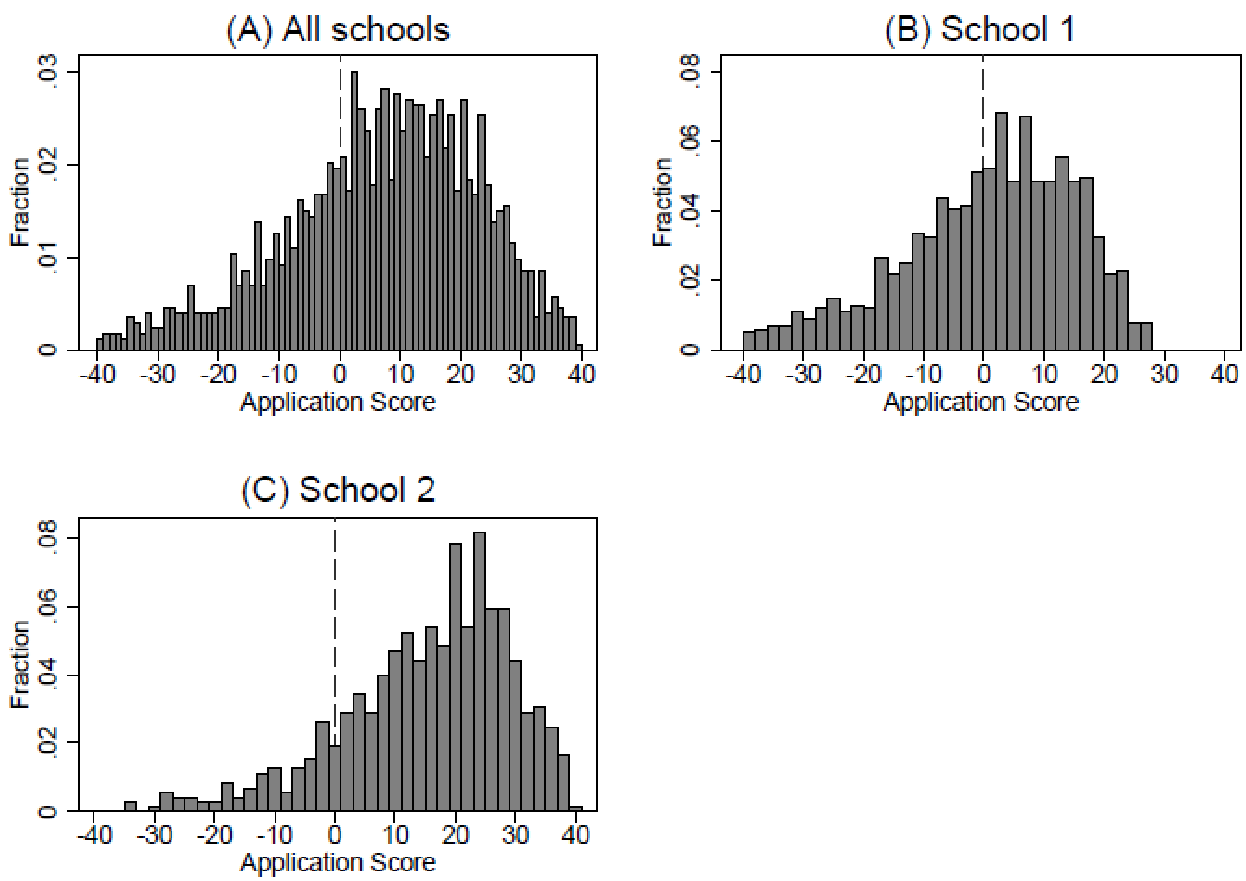

Figure 2: First-stage Probability of Enrolling as a Function of Receiving an Offer

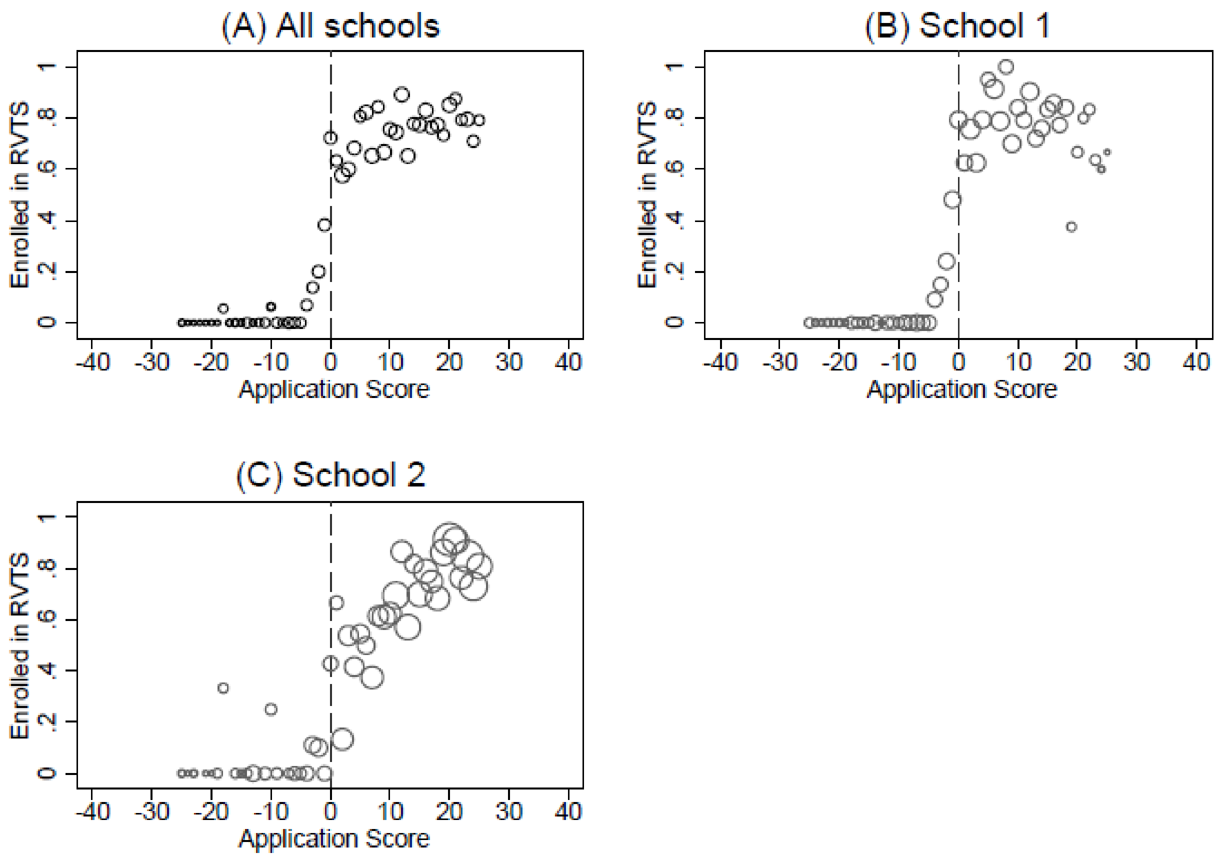

Figure 3: Reduced-form Impact of Offer of Admission by School

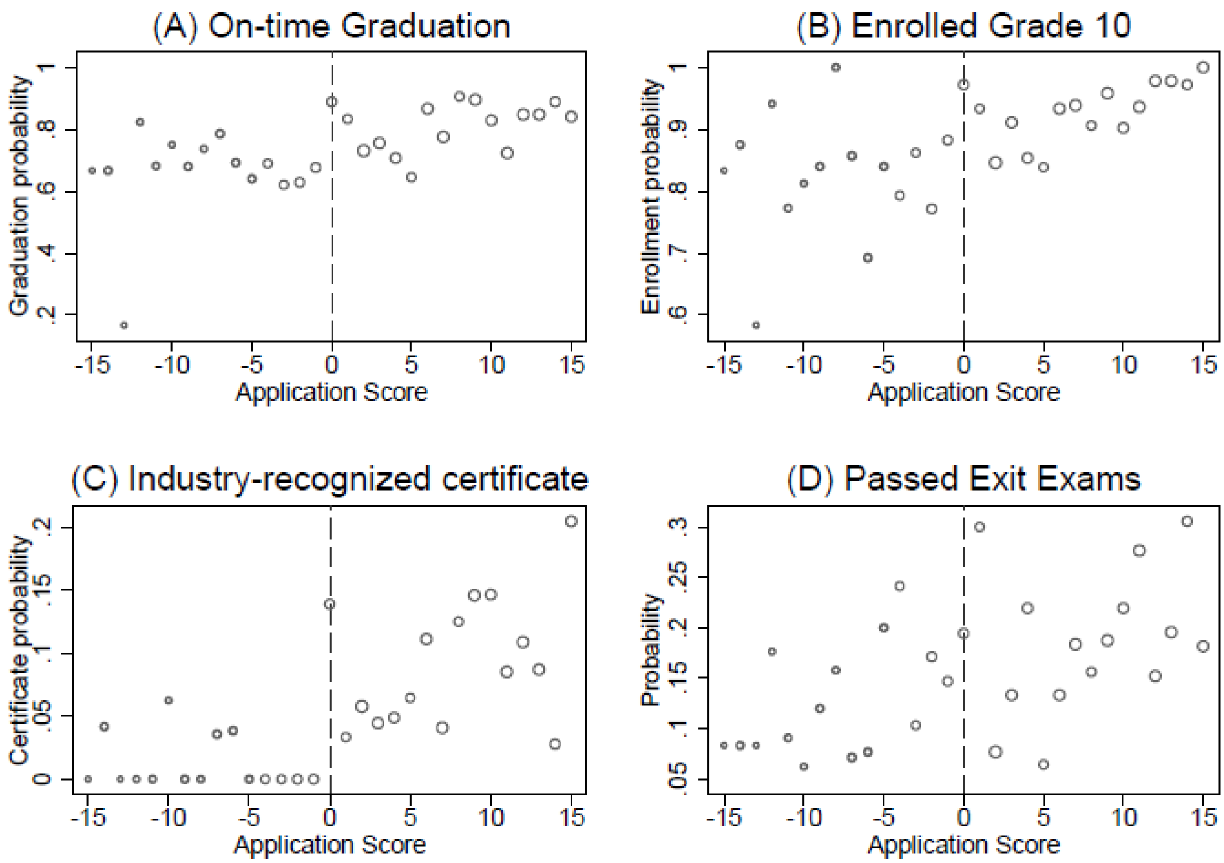

Table 1: Summary Statistics

	(1)	(2) Ex-urban	(3)	(4)	(5) Cities	(6)
	No CTE	Comprehensive CTE	RVTS	No CTE	Comprehensive CTE	RVTS
(A) Controls						
Male	0.497	0.586	0.585	0.49	0.553	0.536
Asian	0.052	0.042	0.032	0.085	0.062	0.033
Black	0.052	0.091	0.057	0.373	0.263	0.281
Latino/a	0.081	0.133	0.158	0.367	0.392	0.599
White	0.857	0.787	0.835	0.258	0.362	0.141
Lower Income	0.184	0.352	0.356	0.676	0.7	0.85
Identified Disability	0.154	0.229	0.265	0.208	0.236	0.273
English Learner	0.049	0.082	0.08	0.204	0.208	0.359
Grade 8 In-school Suspensions	0.008	0.023	0.006	0.008	0.015	0.018
Grade 8 Out-of-school Suspensions	0.013	0.016	0.007	0.022	0.023	0.027
MCAS Math 8	-0.024	-0.67	-0.772	-0.854	-1.247	-1.597
MCAS Math 10	0.222	-0.249	-0.323	-0.266	-0.54	-0.981
On-time Graduation	0.84	0.751	0.871	0.639	0.622	0.550
5-year Graduation	0.853	0.776	0.888	0.671	0.659	0.591
(B) CTE exposure and credentials						
Years in CTE	0.208	2.701	3.861	0.222	2.615	3.301
Years in an RVTS	0.015	0.177	3.855	0.008	1.105	3.415
Chapter 74 Certificate	0.001	0.023	0.019	0	0.008	0.017
Private Certificate	0.002	0.039	0.248	0.001	0.009	0.036
IT Certificate	0.001	0.019	0.047	0	0.01	0.044
Health Certificate	0.002	0.029	0.087	0	0.023	0.113
Engineering Certificate	0.002	0.064	0.155	0.002	0.098	0.068
N	250,481	33,338	32,762	39,278	13,159	6,544

Notes: Mean values of key variables are shown for all students in the 2008-2012 cohorts.

Table 2: OLS and Matching Estimates of the Effect of Attending a Regional Vocational and Technical School

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				Matching			
	Graduated	Enrolled grade 10	Earned certificate	Passed both exit exams	Graduated	Enrolled grade 10	Earned certificate	Passed both exit exams
Enrolled RVTS Grade 9	0.083***	0.053***	0.123***	0.075***	0.025***	0.019***	0.139***	0.034***
	(0.019)	(0.012)	(0.037)	(0.020)	(0.004)	(0.003)	(0.002)	(0.004)
Ex-Urban Non-poor	0.08***	0.04***	0.17***	0.08***	0.05***	0.03***	0.16***	0.05***
	(0.015)	(0.008)	(0.048)	(0.014)	(0.005)	(0.003)	(0.002)	(0.004)
Ex-Urban Poor	0.17***	0.09***	0.17***	0.14***	0.12***	0.06***	0.15***	0.11***
	(0.019)	(0.011)	(0.043)	(0.017)	(0.005)	(0.003)	(0.002)	(0.005)
City Non-poor	0.01	0.02	-0.01	-0.01	0.02**	0.03***	-0.02***	0.02
	(0.052)	(0.021)	(0.019)	(0.050)	(0.012)	(0.007)	(0.004)	(0.011)
City Poor	0.00	0.05	-0.02	-0.01	0.01	0.05***	-0.02***	0.01**
	(0.046)	(0.032)	(0.017)	(0.047)	(0.006)	(0.004)	(0.002)	(0.006)
N	358,238	358,238	358,238	358,238	355,308	355,308	355,308	355,308

Notes: Heteroskedasticity robust standard errors clustered by school are in parentheses (* p<.10 ** p<.05 *** p<.01). Estimates are of the effects of attending an RVTS in grade nine relative to participating in any program (CTE or not) in a comprehensive high school. The coefficients shown are generated using OLS (columns 1 through 4) and coarsened exact matching (columns 5 through 8). All specifications include fixed effects for graduation cohort and grade 8 town-of-residence. All estimates control for or match on observable student characteristics including race, gender, income, disability, and language-learner status, as well as observable proxies for characteristics used to admit students to RVTS; middle-school test scores, suspensions, and attendance rate.

Table 3: Test of covariate balance at admission threshold

	(1) Male	(2) Black	(3) Latino	(4) Asian	(5) White	(6) ELL	(7) Disability status	(8) Low Income	(9) Grade 8 Math score
IK bandwidth	0.069	-0.016	0.019	-0.007	0.019	0.001	-0.014	-0.010	0.101
	(0.069)	(0.028)	(0.022)	(0.007)	(0.025)	(0.011)	(0.052)	(0.021)	(0.076)
N	1,522	1,453	1,734	1,647	1,660	1,775	1,865	1,716	1,555
Bandwidth = 9	0.122	-0.036	0.041	0.005	-0.019	0.014	-0.084	-0.029	0.125
	(0.102)	(0.030)	(0.029)	(0.013)	(0.023)	(0.014)	(0.096)	(0.022)	(0.084)
N	586	586	586	586	586	586	586	586	573
Bandwidth = 12	0.088	-0.033	0.038	-0.001	-0.015	0.015	-0.045	-0.037	0.080
	(0.087)	(0.031)	(0.029)	(0.012)	(0.025)	(0.012)	(0.093)	(0.024)	(0.082)
N	785	785	785	785	785	785	785	785	768
Bandwidth = 15	0.074	-0.027	0.038	-0.004	-0.005	0.015	-0.021	-0.034	0.086
	(0.081)	(0.030)	(0.029)	(0.011)	(0.026)	(0.012)	(0.085)	(0.023)	(0.082)
N	966	966	966	966	966	966	966	966	944
Bandwidth = 18	0.069	-0.023	0.035	-0.007	0.002	0.010	-0.007	-0.039*	0.084
	(0.078)	(0.029)	(0.028)	(0.010)	(0.026)	(0.013)	(0.079)	(0.022)	(0.078)
N	1,134	1,134	1,134	1,134	1,134	1,134	1,134	1,134	1,112
μ	0.700	0.057	0.057	0.014	0.900	0.014	0.243	0.271	-0.318

Notes: Heteroskedasticity robust standard errors clustered by application score are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). Each coefficient is the reduced form estimate of the relationship between offer of admission and the listed covariates. The coefficients shown are generated by local linear regression using a triangular kernel and specified bandwidth and include cohort and school fixed effects. Also listed is the mean of the covariate for students just below the threshold for receiving an offer of admission. The sample includes the 2007-2008 cohorts for which graduation outcomes are available.

Table 4: First-stage estimates of the effect of an offer of admission on take up

	(1) Both schools	(2) School 1	(3) School 2
IK bandwidth	0.356***	0.337***	0.292***
	(0.077)	(0.098)	(0.089)
F	21.5	11.7	10.8
N	1,022	643	443
Bandwidth = 9	0.249***	0.227**	0.305*
	(0.078)	(0.098)	(0.144)
F	10.2	5.4	4.5
N	586	437	149
Bandwidth = 12	0.313***	0.310***	0.291**
	(0.078)	(0.098)	(0.127)
F	16.2	10.0	5.2
N	785	566	219
Bandwidth = 15	0.356***	0.376***	0.277**
	(0.077)	(0.098)	(0.113)
F	21.4	14.6	6.0
N	966	689	277
Bandwidth = 15, controls	0.347***	0.368***	0.276**
	(0.072)	(0.098)	(0.108)
F	23.4	14.0	6.6
N	966	689	277

Notes: Heteroskedasticity robust standard errors clustered by score are in parentheses (* p<.10 ** p<.05 *** p<.01). First-stage estimates show the impact of receiving an offer of admission on actual enrollment in a regional vocational and technical school. The estimates are generated using local linear regression in conjunction with a triangular kernel and the specified bandwidth and fixed effects for graduation cohort. Full sample estimates also include fixed effects for school. An offer of admission is determined by having an admissions score just above the cutoff specified for a given school and year. Below each coefficient is th F-statistic associated with the eligibility instrument.

Table 5: Instrumental variables estimates of the effect of attending an RVTS on student outcomes

	(1) Graduated	(2) Enrolled grade 10	(3) Earned certificate	(4) Passed both exit exams
IK Bandwidth	0.154**	0.103**	0.125**	0.051
	(0.069)	(0.042)	(0.051)	(0.058)
N	1,628	1,660	1,364	1,762
Bandwidth = 9	0.667***	0.258**	0.410***	0.099
	(0.118)	(0.126)	(0.090)	(0.209)
N	586	586	586	586
Bandwidth = 12	0.427***	0.191**	0.261***	-0.019
	(0.111)	(0.096)	(0.085)	(0.163)
N	785	785	785	785
Bandwidth = 15	0.325***	0.170**	0.208***	-0.040
	(0.102)	(0.076)	(0.067)	(0.133)
N	966	966	966	966
Bandwidth = 18	0.273***	0.157**	0.167***	-0.023
	(0.093)	(0.063)	(0.059)	(0.111)
N	1,134	1,134	1,134	1,134
Bandwidth = 15, controls	0.367***	0.182**	0.204***	-0.055
	(0.111)	(0.075)	(0.072)	(0.128)
N	966	966	966	966

Notes: Heteroskedasticity robust standard errors clustered by application score are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). Instrumental variables estimates show the impact of attending an oversubscribed RVTS on on-time graduation probability, remaining enrolled in grade 10, the probability of earning an industry-recognized certificate, and whether a student passed both exams required for graduation, where attending is instrumented by an offer of admission. The coefficients shown are generated by local linear regression using a triangular kernel of the listed bandwidth, including cohort and school fixed effects. The sample consists of those members of the 2007 and 2008 cohorts who are present in the data in 8th grade.

Table 6: Summary Statistics - Regression Discontinuity Design Sample

	(1) School 1	(2) School 2	(3) Other Ex-urban RVTS	(4) Other CTE
Male	0.531	0.593	0.585	0.581
White	0.954	0.571	0.831	0.700
Black	0.300	0.267	0.057	0.128
Latino/a	0.019	0.115	0.158	0.179
Asian	0.400	0.500	0.032	0.047
Lower Income	0.154	0.0.560	0.357	0.417
Identified Disability	0.096	0.257	0.265	0.231
English Learner	0.007	0.100	0.065	0.083
N	1,033	837	11,684	12,862

Notes: Mean values of key variables are shown for all students in the 2007-2008 cohorts and demonstrate the representativeness of the oversubscribed schools in the sample relative to all similar schools.

Table 7: Heterogeneity of the Effect of Attending a Regional Vocational and Technical School by Oversubscription Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Graduated on Time		Enrolled Grade 10		Earned Certificate		Passed Both Exams	
	Statewide	City Only	Statewide	City Only	Statewide	City Only	Statewide	City Only
Not Oversubscribed	0.075***	0.047	0.058***	0.092**	0.108**	0.021	0.068***	0.021
	(0.024)	(0.056)	(0.015)	(0.040)	(0.051)	(0.013)	(0.026)	(0.055)
Oversubscribed	0.02	0.11***	0.02*	0.03	0.21***	0.06***	0.03	0.10***
	(0.018)	(0.045)	(0.010)	(0.020)	(0.048)	(0.014)	(0.021)	(0.038)
N	358,238	52,365	358,238	52,365	358,238	52,365	358,238	52,365

Notes: Heteroskedasticity robust standard errors clustered by school are in parentheses (* p<.10 ** p<.05 *** p<.01). Estimates in each pair of columns illustrate the impact of participating in an RVTS in grade 9 on the listed outcome for students in the whole state, and cities only respectively.The coefficients shown are generated using OLS with fixed effects for cohort and town-of-residence.Heterogeneity of the impact of CTE participation by school overenrollment status is indicated in the table as a linear combination of the relevant interaction terms with the indicator of treatment exposure.

Table 8: Program Offerings by School Location and Occupational Cluster

	(1)	(2) City	(3)	(4)	(5) Ex-urban	(6)	(7)
	Total Programs	Chapter 74	Total Less Worcester	Total Programs	Chapter 74	Total Chapter 74 Approved	Total
Transportation	11	11	9	57	54	65	68
Agriculture	4	4	1	24	22	26	28
Arts & Communication	9	9	8	35	33	42	44
Business Services	12	11	7	52	51	62	64
Construction	21	19	12	121	110	129	142
Early Child Education	3	3	2	13	13	16	16
Hospitality/ Tourism	8	8	6	33	32	40	41
Health Services	8	7	6	59	57	64	67
Information Technology	6	5	4	29	25	30	35
Manufacturing, Engineering, Technology	19	11	6	95	84	95	114
Programs per school	20.2	17.6	15.3	16.7	15.5	15.8	17.2
Share New Tech	32.7%	26.1%	26.2%	35.3%	34.5%	33.2%	34.9%

Notes: Program offerings disaggregated by geographic region, program type, and occupational cluster. The sample consists of those members of the 2008 through 2012 cohorts. Chapter 74 approved programs are subject to state approval and receive additional funding. Health services, information technology, and manufacturing, engineering, and technology are defined as "new tech" in that they deviate from the traditional trades and represent areas of rapid labor-market expansion.

APPENDIX:

Figure A.1: Regional Vocational and Technical School Locations with Affiliated Towns

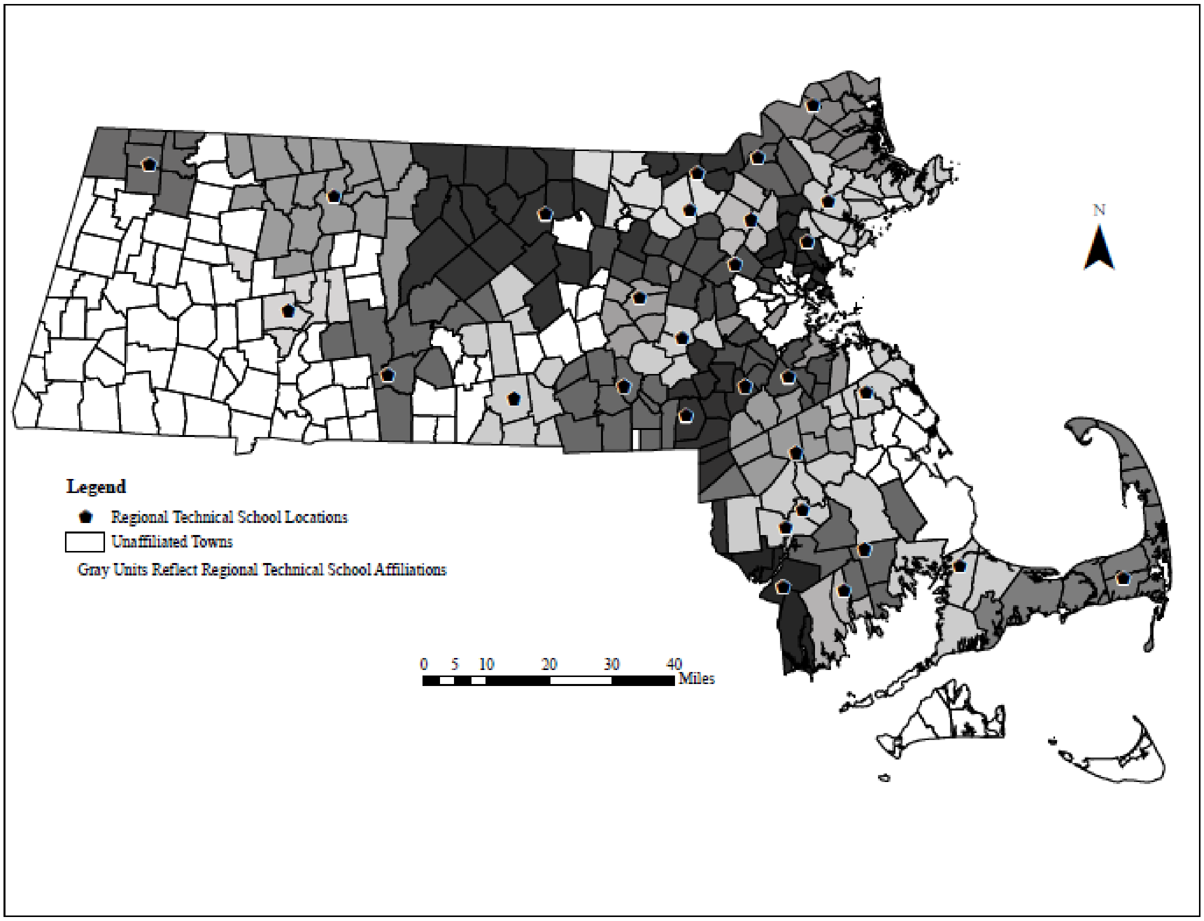

Table A.1: Estimates Using Multiple Matching Estimators

	(1) CEM	(2) Propensity score	(3) Inverse propensity weights	(4) Nearest neighbor Mahalinobis
On-time Graduation	0.04***	0.055***	0.039***	0.051***
	(0.003)	(0.003)	(0.005)	(0.003)
N	355,308	358,238	358,238	358,238

Notes: Heteroskedasticity robust standard errors are in parentheses (* p<.10 ** p<.05 *** p<.01). Estimates of the sample average treatment effect of experiencing an RVTS in grade 9 on the probability of graduating on time from high school using multiple matching estimators. The sample consists of those members of the 2008 through 2012 cohorts who are present in the data in 8th grade.

Table A.2: Coarsened Exact Matching Estimates of the Effect of Attending a Regional Vocational and Technical School Versus Participating in Career and Technical Education in a Comprehensive School

	(1) Graduated	(2) Enrolled grade 10	(3) Earned certificate	(4) Passed both exit exams
Enrolled RVTS Grade 9	0.056***	0.037***	0.156***	0.038***
	(0.007)	(0.004)	(0.005)	(0.006)
Ex-Urban Non-poor	0.05	0.04	0.20	0.04
	(0.007)	(0.004)	(0.005)	(0.007)
Ex-Urban Poor	0.09	0.06	0.19	0.08
	(0.008)	(0.005)	(0.006)	(0.007)
City Non-poor	0.11	0.04	-0.02	0.03
	(0.017)	(0.009)	(0.012)	(0.015)
City Poor	0.10	0.07	-0.03	0.04
	(0.010)	(0.006)	(0.007)	(0.009)
N	68,121	68,121	68,121	68,121

Notes: Standard Errors are in parentheses (* p<.10 ** p<.05 *** p<.01). Estimates are of the effects of attending an RVTS in grade nine relative to participating in any CTE program in a comprehensive high school. The coefficients shown are generated using coarsened exact matching. All specifications include fixed effects for graduation cohort and grade 8 town-of-residence. Matching is done using observable observable proxies for characteristics used to admit students to RVTS; middle-school test scores, suspensions, and attendance rate, as well as student gender, income, and disability status. Also included are controls for student race, and language-learner status.

Table A.3: OLS Estimates of the Effect of Attending a Regional Vocational and Technical School on Graduation from High School Within Five Years

	(1) Graduated in 5 years
Enrolled RVTS Grade 9	0.079***
	(0.020)
Ex-Urban Non-poor	0.06***
	(0.015)
Ex-Urban Poor	0.18***
	(0.019)
City Non-poor	-0.01
	(0.057)
City Poor	-0.00
	(0.045)
N	287,417

Notes: Heteroskedasticity robust standard errors clustered by school are in parentheses (* $p<.10$ ** $p<.05$ *** $p<.01$). Estimates are of the effects of attending an RVTS in grade nine relative to participating in any program (CTE or not) in a comprehensive high school. The coefficients shown are generated using OLS and all specifications include fixed effects for graduation cohort and grade 8 town-of-residence. Estimates also include controls for observable student characteristics including race, gender, income, disability, and language-learner status, as well as observable proxies for characteristics used to admit students to RVTS; middle-school test scores, suspensions, and attendance rate.

Table A.4: Testing sensitivity of reduced-form estimates to the use of both linear and quadratic specifications of the forcing variable.

	(1)	(2)	(3)	(4)
	Graduated	Enrolled grade 10	Earned certificate	Passed both exit exams
Linear, BW=IK	0.076**	0.052**	0.053**	0.025
	(0.031)	(0.020)	(0.023)	(0.032)
N	1,628	1,660	1,364	1,762
Up to quadratic, BW=IK	0.124***	0.061**	0.091***	-0.006
	(0.039)	(0.028)	(0.032)	(0.043)
N	1,628	1,660	1,364	1,762
Linear, BW=9	0.150***	0.061*	0.097**	0.018
	(0.034)	(0.033)	(0.037)	(0.053)
N	586	586	586	586
Up to quadratic, BW=9	0.205***	0.045	0.114***	0.108*
	(0.023)	(0.036)	(0.030)	(0.060)
N	586	586	586	586
Linear, BW=12	0.127***	0.058*	0.077**	-0.011
	(0.039)	(0.030)	(0.034)	(0.049)
N	785	785	785	785
Up to quadratic, BW=12	0.180***	0.060	0.115***	0.068
	(0.028)	(0.039)	(0.036)	(0.057)
N	785	785	785	785
Linear, BW=15	0.116***	0.061**	0.072**	-0.019
	(0.036)	(0.027)	(0.029)	(0.046)
N	966	966	966	966
Up to quadratic, BW=15	0.172***	0.055	0.100**	0.029
	(0.042)	(0.037)	(0.040)	(0.054)
N	966	966	966	966
Linear, BW=18	0.108***	0.063**	0.065**	-0.015
	(0.035)	(0.025)	(0.026)	(0.042)
N	1,134	1,134	1,134	1,134
Up to quadratic, BW=18	0.157***	0.057	0.096**	-0.005
	(0.045)	(0.036)	(0.036)	(0.055)
N	1,134	1,134	1,134	1,134

Notes: Heteroskedasticity robust standard errors clustered by score are in parentheses (* p<.10 ** p<.05 *** p<.01). The reduced form estimates reported here were generated using OLS with an indicator for whether a student received an offer of admission from an oversubscribed regional vocational and technical school. Estimates are reported across multiple bandwidths with both quadratic and linear specifications of the forcing variable included at each bandwidth. All models include individual-level covariates to improve precision, as well as fixed effects for graduation cohort and school.