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. This paper investigates the causal impact of participating in a specialized high school-based CTE delivery system on high school persistence, completion, earning professional certifications, and standardized test scores, with a focus on individuals from low-income families, a group that is overrepresented in CTE and high school noncompleters. Using administrative data from Massachusetts, I combine ordinary least squares with a regression discontinuity design that capitalizes on admissions data at three schools that are oversubscribed. All estimates suggest that participation in a high-quality CTE program boosts the probability of on-time graduation from high school by 7 to 10 percentage points for higher income students, and suggestively larger effects for their lower-income peers and students on the margin of being admitted to oversubscribed schools. This work informs an understanding of the potential impact of specific 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 dialog related to improving career pathways and readiness.

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1. INTRODUCTION

Nationwide, more than one in five high school students take four or more of their high school courses in a career and technical education (CTE) area, and over 90 percent of public high schools offer students access to CTE programs (Musu-Gillette et al. 2016). 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 and Katz 2008; Huff Stevens, Kurlaender, and Grosz 2015; Xu and Trimble 2016). 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 (Rumberger 2011; Murnane 2013), emphasizing the need for education policy to attend to both margins. The continued importance of ensuring high school graduation as a minimum educational certificate is also driven by the continued demand for nonroutine low-skilled and moderately-skilled jobs—the types of jobs that many high school CTE participants are prepared to take (Autor, Levy, and Murnane 2003; Autor et al. 2006; Holzer, Linn, and Monthey 2013).

Although prior work has shown that students who participate in CTE secondary school programs enjoy a wage premium over similar students who do not, the educational benefits of CTE programs have been unclear (Catterall and Stern 1986; Pittman 1991; Mane 1999; Neumark and Joyce 2001; Plank 2001; Agodini and Deke 2004; Ainsworth and Roscigno 2005; Bishop and Mane 2004, 2005; Neumark and Rothstein 2006; Meer 2007; Kemple and Willner 2008; Stone, Alfeld, and Pearson 2008; Hanushek, Woessmann, and Zhang 2011; Dalton and Bozick 2012; Page 2012; Bozick and Dalton 2013). In addition, earlier work has mainly ignored the potential effects on students from lower-income families, a group that is overrepresented in CTE and for whom CTE may have previously been used as a dumping ground (Gamoran and Mare 1989; Fraser 2008; Kelly and 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 postsecondary education for students who would not otherwise have continued their schooling (Cullen et al. 2013; Stange and 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 being 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 (RVTSs) where *all* students participate in CTE. Descriptive evidence indicates that students at RVTSs have improved their performance on the state accountability test, and increased their graduation rates (Fraser 2008; CEP 2011). To date, however, there has been no systematic 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 (Kemple and Willner 2008)—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 in an RVTS. I capitalize on a known set of criteria used for selection of students into an RVTS in Massachusetts 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 and omitted variables bias (Altonji, Elder, and Taber 2005). Capitalizing on this process and available administrative data, I first use ordinary least squares (OLS) with fixed effects for graduation cohort and town of residence to estimate the average effect on student outcomes of participating in an RVTS. I then complement state-level administrative data with actual admissions records that I obtained from three schools (not currently collected by the state) and use a regression discontinuity (RD) design to isolate the impact of participating in CTE for students on the margin of being admitted to oversubscribed RVTSs. These dual approaches complement one another, with the OLS supporting greater external validity and approximating an average treatment effect, whereas the RD analyses have strong internal validity but represent the local average treatment effect for the marginal student in an oversubscribed school.

This paper makes several important contributions to the literature. First, it focuses on whether there are different impacts of RVTS participation for low-income students, who, on average, are overrepresented in CTE and have been less likely to complete high school or enroll in postsecondary education of any kind. Second, it provides some of the first estimates of the impact of a particular form 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.

I focus on high school graduation as my primary outcome of interest because it is broadly accepted as a signal of the minimum required human capital to access full-time employment. As an intermediate measure of persistence, I also add an indicator for whether a student is still enrolled in high school in grade 11 in order to understand whether any potential effects on graduation are realized through stemming early dropout. To further understand the mechanisms of any potential effect I also include as outcomes student scores on the math and English language arts (ELA) exams in tenth grade, where passing scores on each are required to earn a diploma. These scores provide insight into whether general human capital is impacted by RVTS participation, or if most effects accrue through remaining in school. Finally, to gauge accumulation of specific forms of human capital not assessed on the general academic exams, I include as an outcome whether a student earns an industry-recognized certificate.

The rest of this paper is laid out as follows. In section 2 I provide more detail on the context of Massachusetts's 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.

2. 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 percent of CTE participants are eligible for free or reduced-price lunch (whereas only about 26 percent of students not participating in CTE are free or reduced-price lunch–eligible), make it a compelling model to study. If the system of RVTSs 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 rates 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 RVTSs where all students participate in some form of CTE.

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 nonparticipants. In contrast, at RVTSs, students alternate on a weekly basis between full-time academic coursework and full-time work in their technical area and all students in all classes take some form of CTE.

Importantly, state graduation requirements do not differ by school type, meaning that students in comprehensive high schools and RVTSs must all complete four years of English, four years of math, three years of a lab-based science, three years of history, two years of the same foreign language, one year of an arts program, and five additional "core" courses such as business education, health, and/or technology (see www.doe.mass.edu/ccr/masscore/). Taking CTE coursework in any setting can be thought of as a substitute of CTE electives for other electives, such as additional world language, arts classes, or electives in core academic areas that exceed minimum graduation requirements. The key treatment in this paper, then, is the taking of elective CTE courses in an RVTS that is structured differently than a traditional high school, arguably changing the overall educational experience in the ways described above.[1]

Access to CTE

Access to RVTSs is largely determined by the location of a student's home (see Appendix figure A.1 for a map of school locations and affiliated towns). 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, 5 are run by the city school districts where they are located (Worcester, Springfield, Lynn, Holyoke, and Boston), and there are two countywide and one statewide agricultural schools. In total, 323 of the 353 towns and cities in the Commonwealth are associated with a regional technical school. On average, RVTSs offer nearly 18 (exactly: 17.7) different CTE programs. As required by the Perkins IV act, students in the remaining towns have access to some form of CTE through their comprehensive high school (though on average comprehensive schools offer fewer than 8 [exactly: 7.6]) distinct CTE programs. 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 tuition transfers must be made by the receiving RVTS, as well as the sending home district, which is responsible for providing the per student allocation to the RVTS. About 18 percent of all high-school-aged students in Massachusetts participate in some form of CTE in high school, with about half of these students participating through an RVTS and the other half participating through a comprehensive high school.

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 the majority of programmatic offerings in RVTSs 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. Chapter 74–approved programs also require adherence to program specific student–teacher ratios and space guidelines. 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 (Gamoran and Mare 1989; Fraser 2008; Kelly and Price 2009). Despite such practices, prior research has highlighted a number of benefits of the

1. I cannot exclude the possibility that schools can set their own course content as well as their own passing thresholds for required coursework, which likely differ systematically by school. If, on average, RVTSs were more likely to set lower passing thresholds, then using graduation as an outcome could be less valid. If, however, RVTSs were no more likely than another school with students of similar prior ability to adjust course passage requirements downward, then the graduation outcome, though imperfect, would not be as biased.

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 and Willner 2008; Page 2012), that find students who participate in a CTE program have higher earnings, on average, than similar students who attended a non-CTE program (Mane 1999; Bishop and Mane 2004, 2005; Neumark and Rothstein 2006; Meer 2007; Stern, Dayton, and Raby 2010; Page 2012).

Additional research has suggested that CTE participation may also provide nonmonetary 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 (Fredricks and Eccles 2002). Because efficacy and self-worth influence a student's engagement in his learning environment, they could have an important effect on a student's decision to remain enrolled or drop out (Finn and Rock 1997; Agodini and Deke 2004: Plank, DeLuca, and Estacion 2008; Kelly and Price 2009; 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 and Willner 2008; Page 2012). Although this evaluation 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 postsecondary outcomes.[2] Although these benefits from CTE may be examples of returns on specific human capital investments (Becker 2009; Lazear 2009), we also know there are potential gains from general human capital (Becker 1962, 2009) or signaling (Spence 1973; Clark and Martorell 2014) by earning a high school diploma or the equivalent (Murnane, Willett, and Tyler 2000; Tyler, Murnane, and Willett 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 schools offering CTE have been expected to operate.

My hypothesis is consistent with the findings of Stern, Dayton, and Raby (2010), as well as those of Neumark and Rothstein (2006), suggesting the impact of CTE differs

2. 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.

depending on the structure of the CTE program itself. This also serves as motivation to extend the findings of the Career Academy experiment (Kemple and 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, Dayton, and Raby 2010). Similarly, in Massachusetts, the two methods of delivering CTE—offerings in comprehensive schools versus those in RVTSs—may produce different effects.

3. RESEARCH DESIGN

Two Approaches

The biggest challenge to estimating the causal effects of participating in CTE through an RVTS arises because students *elect* to participate, and likely differ from students who make no such choice, in both observed and unobserved ways at the time of enrollment (Heckman 1979; Imbens and Wooldridge 2009). I deal with the self-selection problem by using two approaches: (1) OLS with fixed effects for town of residence, graduation cohort, and area of technical study (or occupational cluster; e.g., culinary arts, electrical), and (2) an RD strategy in which I capitalize on a natural experiment, generated by using a quantitative ranking process and exogenous cutoff to admit students to oversubscribed schools.[3]

Each approach has strengths and weaknesses, particularly in relation to internal and external validity (Campbell 1957). For instance, my RD strategy has weaker external validity, because it applies only to those students on the margin of being admitted to an oversubscribed RVTS in the three schools for which I have data, but the ensuing causal inferences have much stronger internal validity. By contrast, my OLS approach has stronger external validity because it estimates the effects of CTE participation for a larger group of students in Massachusetts, but it has weaker internal validity.

Dataset and Sample

I use data from the comprehensive Student Information Management System provided by the Massachusetts Department of Elementary and Secondary Education, for the academic years spanning fall of 2001 through spring 2015. These fourteen cohorts include over 500,000 students in grades 1 through 12, who are followed longitudinally for as long as they remain in the Massachusetts public schools. For my OLS analysis, I include students who, if they had graduated from high school "on time," would have done so in the spring years of 2008 through 2015 (approximately 420,000 students). My sample does not include students who are eligible to take an alternative assessment based on their disability status.

For my RD analyses, I supplement the Student Information Management System with student-level, school-specific application and admissions records from three schools. These schools have been oversubscribed for at least three years during the

3. Admissions criteria are known for RVTSs and so in the Appendix I also include a set of matching estimates as a point of comparison with OLS. Though effects are slightly smaller, the substantive conclusions are not changed. Therefore, OLS is preferred based on its requiring less methodological exposition and based on the findings of Altonji, Elder, and Taber (2005).

Table 1. Summary Statistics

	No CTE (1)	Comprehensive CTE (2)	RVTS (3)
Panel A: Controls			
Male	0.496	0.585	0.579
Asian	0.053	0.045	0.031
Black	0.051	0.088	0.057
Latino/a	0.084	0.138	0.163
White	0.843	0.767	0.824
Lower income	0.197	0.375	0.372
Identified disability	0.147	0.22	0.269
English learner	0.043	0.074	0.068
Grade 8 in-school suspensions	0.008	0.022	0.005
Grade 8 out-of-school suspensions	0.012	0.016	0.006
MCAS math 8th grade	0.198	−0.267	−0.357
MCAS math 10th grade	0.194	−0.264	−0.358
4-year graduation rate	0.815	0.739	0.847
5-year graduation rate	0.828	0.764	0.865
Panel B: CTE Exposure and Credentials			
Years in CTE	0.199	2.614	3.204
Years in RVTS	0.014	0.051	3.726
Chapter 74 certificate	0.002	0.025	0.047
Private certificate	0.004	0.042	0.269
Non–Chapter 74 certificate	0.001	0.003	0
IT credential	0.001	0.021	0.046
Health credential	0.002	0.034	0.087
Engineering credential	0.003	0.066	0.153
N	342,173	31,727	43,235

Notes: Mean values of key variables are shown for all students in the 2008–14 cohorts. Inclusion in a column is defined by a student's initial status in grade 9. CTE: career and technical education; IT: information technology; MCAS: Massachusetts Comprehensive Assessment System; RVTS: regional vocational and technical high school.

last decade and were forced to admit fewer students than had applied. Though nearly thirteen 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 RD sample includes over 4,000 students from three participating RVTSs, with about 2,000 of those students just above, or just below, the admissions cutoff.

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 RVTSs (the focal treatment group in this study)—these correspond to columns 1 through 3, respectively. In each of the three columns, membership is determined by a student's status in grade 9.

Students in either CTE setting 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. Especially noteworthy is that students in comprehensive CTE and RVTSs appear similar on nearly all observables except for their mean probabilities for on-time graduation. Also noteworthy is that most students who attend an RVTS in grade 9 appear to stay for all four years of high school, and very few students in CTE programs in comprehensive high schools ever enroll in an RVTS. Graduation rates are descriptively higher for RVTS students than non-CTE participants, and lower among those in CTE programs in comprehensive high schools. Students in RVTSs are also more likely to earn industry-recognized credentials, especially in higher-wage areas like information technology, health sciences, and engineering.

Measures

My primary outcome of interest is a dichotomous indicator (*GRAD4*) of whether a student graduated from public high school in Massachusetts within four years of beginning ninth grade (= 1 if they were reported as graduating; 0 otherwise).[4] 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 11 (*ENROLL11*) as an intermediate outcome. In addition, I define an indicator of whether a student passed both required exams (*PASS*), as well as the continuous, standardized measures of performance on the math and ELA exams.[5] My final outcome of interest is an indicator of whether a student earned an industry-recognized certificate (*IRC*) while in high school. IRCs include Microsoft Office, Cisco Systems, and ServSafe certifications that signal specific skills and credentials potentially valuable to employers in specific industries.

In my OLS approach, my key treatment variable is a dichotomous indicator of whether a student enrolled in an RVTS during their ninth-grade year (*RVTS9*).[6] I argue that students in both treatment and control conditions should be equivalent in expectation of future outcomes, conditional on observing the criteria for CTE eligibility in eighth grade (Altonji, Elder, and Taber 2005).

All applicants to RVTSs are evaluated on three elements of their middle school experience: transcripts, attendance, and discipline record. To improve the internal validity of my OLS estimates, I use prior measures of (1) eighth-grade attendance (total days present, *DAYS*), (2) academic performance (as measured by eighth-grade Massachusetts Comprehensive Assessment System or MCAS scores in mathematics, *MCAS_M8*), and (3) disciplinary record (total instances of in- or out-of-school suspension, *IN_SUSP* and *OUT_SUSP*, respectively) as proxies for these known application criteria. The attendance and discipline records are identical to those used in evaluating eligibility for RVTSs, and the test scores are a proxy for prior academic performance. I

4. About 3 percent 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 nongraduates. I also examined five-year graduation probabilities and find no difference in the effects.
5. By using both the passing threshold required to earn a diploma and continuous scores, I gain greater perspective on how earning the diploma relates to levels of human capital.
6. 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.

include indicators of gender, race, disability status, and English-language learner status to improve the precision of my estimates, and a dichotomous indicator for whether a student is eligible to receive free or reduced-price lunch (*FRPL*).[7]

In my RD approach, I define the forcing variable as a student's score on her 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.

Data Analysis

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

$$\begin{aligned} Y_{igrc} &= \alpha_0 + \alpha_1 RVTS9_{igrc} + \alpha_2 FRPL_{igrc} + \alpha_3 (FRPL \times RVTS9_{igrc}) \\ &\quad + X_i'\gamma + \pi_g + \tau_r + \omega_c + \varepsilon_{igrc}, \end{aligned} \tag{1}$$

where Y_{igrc} is the generic outcome Y for student i in cohort g from town r, in occupational cluster c, and π_g, τ_r, ω_c represent fixed effects for cohort, town of residence, and occupational cluster, respectively.[8] 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.[9] All of my estimates use heteroskedasticity-robust standard errors clustered at the high school level to account for autocorrelation of errors for students in the same school.

In my regression-discontinuity approach, I estimate the causal effects of participating in CTE by capitalizing on student enrollment in RVTSs that are oversubscribed. During the admissions process, student applicants to oversubscribed RVTSs 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 differ very little in their overall admissions score from students who just miss being offered a spot in an RVTS, I posit that the similarity among students at the margins of admission make them arguably equal in expectation at the admit/nonadmit discontinuity on the admissions-score forcing variable (Imbens and Lemieux 2008; Murnane and Willett 2011).

I implement a standard fuzzy RD design (Imbens and Lemieux 2008; Murnane and Willett 2011) in conjunction with a triangular kernel, using two-stage least squares within a local-linear regression framework. 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, and Rubin 1996), to estimate the causal effect of enrollment

7. Actual indicators of whether a student applied and was denied are not available in the administrative data.
8. My preferred specification does not include the fixed effects for occupational cluster as it is not possible to match on this criterion 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.
9. In my results, I present the combined coefficients α_1 and α_3 to show the effects for lower-income students. Coefficients were combined using the *lincom* command in Stata (StataCorp, College Station, TX).

on student outcomes. I specify my general first-stage linear probability model for student i in cohort c in school s, as follows:

$$P(ENROLL = 1)_{ics} = \alpha_0 + \alpha_1 OFFER_{ics} + \alpha_2 CSCORE_{ics} + \alpha_3 CSCORE \times OFFER_{ics} + X_i'\theta + \varphi_c + \gamma_s + \delta_{ics}, \tag{2}$$

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

My second-stage model takes the form:

$$Y_{ics} = \pi_0 + \pi_1 \widehat{ENROLL}_{ics} + \pi_2 CSCORE_{ics} + \pi_3 CSCORE \times OFFER_{ics} + X_i'\Psi + \varphi_c + \gamma_s + \varepsilon_{ics}, \tag{3}$$

where 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.[10] To test for the heterogeneity of effects by lower-income status I also interact free- or reduced-price lunch eligibility with the offer indicator and add it to both the first and second stages, while allowing the relationship between the forcing variable and outcomes to also be flexible by income status.

4. RESULTS

OLS Estimates

In panel A of table 2, I present my OLS estimates of the impact of RVTSs' participation on student outcomes for higher-income students as well as their free or reduced-price lunch–eligible peers.[11] In all cases the appropriate reference group includes students who did not attend an RVTS in grade 9, including nonparticipants and those in a comprehensive CTE setting. In the first row are estimates of the aggregate effects of RVTS participation for higher-income students, and in row 2 are the estimates for students who are free or reduced-price lunch–eligible. In panel B, I report analogous results using only students in the RD sample (discussed below).

My OLS estimates suggest that CTE participation in an RVTS is associated with higher probabilities of graduating from high school on time, remaining enrolled in high school through grade 11, earning an industry-recognized certificate, and passing both exams required to earn a diploma.[12] Effects on graduation, persisting in high school, and passing both exams required for graduation are larger for low-income students and statistically different from those of their higher-income peers. There is

10. See also Calonico, Cattaneo, and Titiunik (2014) for a discussion of optimal bandwidth choice.
11. In table A.1, I provide evidence that OLS results are similar to matching estimates using a variety of matching estimators.
12. My results are not sensitive to my use of a measure of graduation in five years (results are available from the author upon request).

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

	Graduated (1)	Enrolled Grade 11 (2)	Earned Certificate (3)	Math Score (4)	ELA score (5)	Pass Both (6)
Panel A: Overall Sample						
Higher income	0.114***	0.086***	0.312***	−0.002	−0.051*	0.114***
	(0.008)	(0.006)	(0.050)	(0.013)	(0.027)	(0.008)
Lower income	0.21***	0.17***	0.30***	0.09***	0.08***	0.19***
	(0.016)	(0.011)	(0.051)	(0.020)	(0.035)	(0.013)
N	417,215	417,215	417,215	375,961	378,358	417,215
Panel B: Regression Discontinuity Sample						
Higher income	0.145***	0.059***	0.174***	0.140***	0.102***	0.013
	(0.014)	(0.010)	(0.010)	(0.025)	(0.030)	(0.015)
Lower income	0.23***	0.17***	0.21***	0.20***	0.28***	0.07***
	(0.020)	(0.015)	(0.013)	(0.029)	(0.040)	(0.017)
N	4,885	4,885	4,885	4,422	4,475	4,885

Notes: Heteroskedasticity robust standard errors clustered by school are in parentheses. Estimates are of the effects of attending an RVTS in grade 9 relative to participating in any program (CTE or not) in a comprehensive high school. The coefficients shown are generated using ordinary least squares. All specifications include fixed effects for graduation cohort and grade 8 town-of-residence. All estimates control for observable student characteristics including race, gender, income, disability, and English language-learner status (as well as observable proxies for characteristics used to admit students to RVTSs); middle school test scores, suspensions, and attendance rate. CTE: career and technical education; RVTS: regional vocational and technical high school; ELA: English language arts.

$^{*}p < 0.10$; $^{***}p < 0.01$.

no clear effect on student test scores among higher-income students, but suggestive statistical evidence exists of a positive impact on lower-income students who remain in school long enough to be tested. I cannot make strong inferences on test-score outcomes because the sample of test students is smaller than the general sample. Using common bounding techniques to account for selection out of the sample, I cannot rule out the possibility of a negative impact on higher-income students and a null effect on lower-income students.[13]

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 groups are equal in expectation on a selection of observable characteristics. I fit equation 1, replacing the outcome with each covariate, and find only one difference of note—that there appears to be fewer low-income students admitted than denied admission.[14] To establish the continuity of the forcing variable, I display a histogram of

13. Selection analyses were undertaken using Stata routines to apply a Heckman correction or Lee bounds.
14. Graphical analysis suggests this could be driven by assumptions of linearity. As I show below, including controls to account for this one potential imbalance does not affect the statistical or substantive conclusions of the results. Results are also not sensitive to removing the one school that contributes to this imbalance, though it is retained in the analysis for the added statistical power.

Table 3. Estimates of Differences in Observable Characteristics Between Eligible and Noneligible Regional Technical School Applicants

	Male (1)	Black (2)	Latino (3)	Asian (4)	White (5)	ELL (6)	Disability Status (7)	Low Income (8)	Grade 8 Math Score (9)
IK bandwidth	0.053	−0.024	0.015	−0.001	0.020	0.007	−0.082	-0.084^{***}	0.108
	(0.046)	(0.016)	(0.015)	(0.004)	(0.036)	(0.009)	(0.049)	(0.016)	(0.070)
N	3,037	4,229	2,291	2,291	2,108	2,451	2,108	2,291	2,689
Bandwidth = 6	0.082	−0.013	−0.027	0.001	0.041	−0.003	-0.134^{**}	-0.107^{***}	0.100
	(0.079)	(0.019)	(0.031)	(0.004)	(0.053)	(0.008)	(0.047)	(0.019)	(0.069)
N	1,023	1,023	1,023	1,023	1,023	1,023	1,023	1,023	1,001
Bandwidth = 15	0.052	−0.023	0.016	−0.003	0.026	0.007	−0.062	-0.070^{***}	0.108
	(0.050)	(0.019)	(0.014)	(0.004)	(0.030)	(0.008)	(0.047)	(0.018)	(0.071)
N	2,606	2,606	2,606	2,606	2,606	2,606	2,606	2,606	2,547
μ	0.573	0.058	0.150	0.005	0.806	0.015	0.214	0.408	−0.209

Notes: Heteroskedasticity robust standard errors clustered by application score are in parentheses. Each coefficient is the reduced form estimate of the relationship between offer of admission and the listed covariate. 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 2007–09 cohorts for which graduation outcomes are available. ELL: English language learner; IK: Imbens and Kalyanaraman 2012.

$^{**}p < 0.05$; $^{***}p < 0.01$.

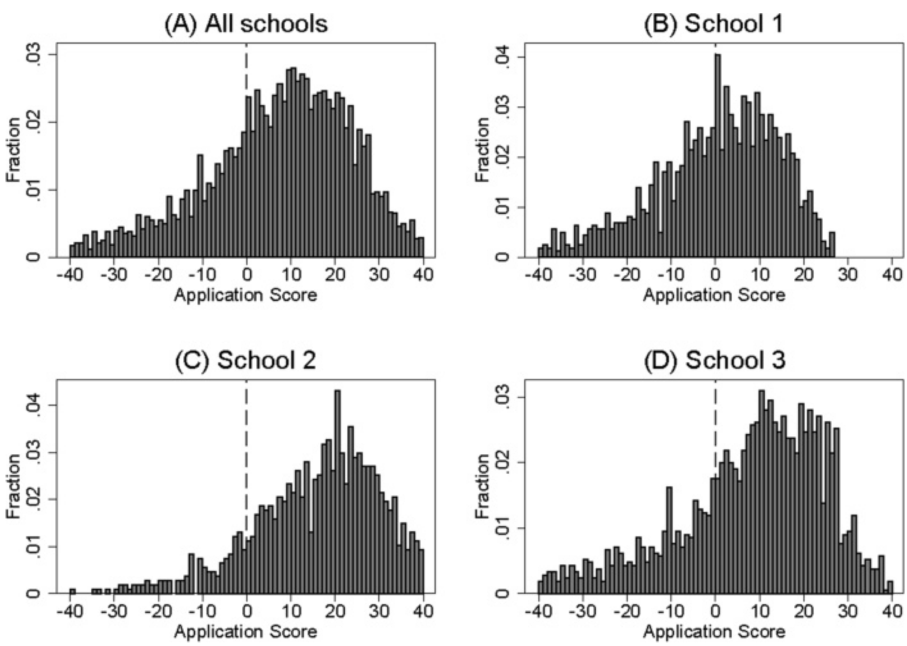

Figure 1. Distribution of Application Scores.

students by application score value on the recentered forcing variable (figure 1). Neither in the combined distribution (panel A), nor in the distributions of the forcing variable within individual schools (panels B, C, and D) do I observe evidence of manipulation.

The process used to generate the application scores also supports 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

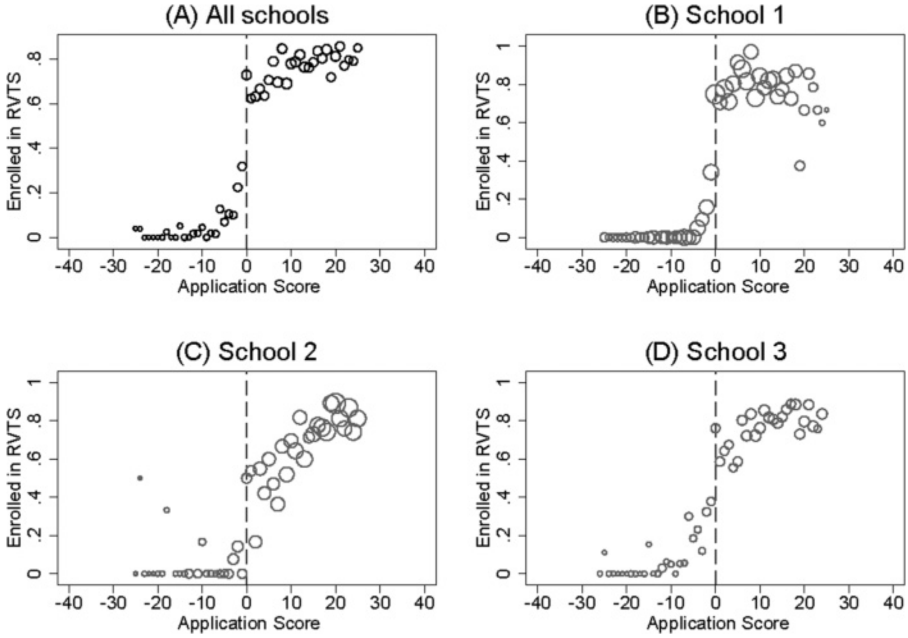

Figure 2. First-stage Probability of Enrolling as a Function of Receiving an Offer.

submitted by the middle school and without knowledge of the cutoff that was ultimately used to admit students, and therefore could 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. Nonetheless, conversations with administrators and publicly available state documents (MADESE 2010) suggest such threats 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 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.[15]

First Stage Results

The credibility of my instrumental-variables approach relies on a strong first stage that indicates having received an offer to attend an RVTS results in higher probabilities of enrollment in an RVTS for students near the admissions cutoff. This discontinuity in actual enrollment is demonstrated in figure 2, which displays the probability of having enrolled in an RVTS in ninth grade—as a function of a student's recentered application score—for all three 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 at the cutoff in probability of attending.

15. I explore this further in section 5.

Table 4. First-Stage Estimates of the Effect of an Offer of Admission on Take-up

	All Schools (1)	School 1 (2)	School 2 (3)	School 3 (4)
IK bandwidth	0.314***	0.324***	0.314***	0.251***
	(0.059)	(0.043)	(0.085)	(0.088)
F	28.2	57.1	13.6	8.1
N	1,756	463	499	1,029
Bandwidth = 6	0.278***	0.359***	0.368**	0.161
	(0.061)	(0.040)	(0.118)	(0.131)
F	20.9	79.6	9.7	1.5
N	1,023	463	156	404
Bandwidth = 15	0.384***	0.522***	0.313***	0.241***
	(0.056)	(0.065)	(0.096)	(0.084)
F	46.8	65.0	10.7	8.3
N	2,606	1,102	406	1,098
Bandwidth = IK, controls	0.309***	0.298***	0.291***	0.245***
	(0.060)	(0.059)	(0.075)	(0.088)
F	26.8	25.2	15.1	7.8
N	1,756	463	499	1,029
Bandwidth = IK, low income	0.326***	0.310**	0.372***	0.352***
	(0.085)	(0.134)	(0.064)	(0.072)
F	14.7	5.4	34.1	23.7
N	739	441	1,276	1,031
Bandwidth = IK, high income	0.294***	0.222***	0.429***	0.367***
	(0.068)	(0.044)	(0.070)	(0.071)
F	18.7	25.4	37.2	27.0
N	1,017	582	1,761	1,420

Notes: Heteroskedasticity robust standard errors clustered by score are in parentheses. First-stage estimates show the impact of receiving an offer of admission on actual enrollment in an RVTS. The estimates are generated using local linear regression in conjunction with a triangular kernel and the specified bandwidth and cohort-by-school fixed effects. An offer of admission is determined by having an admission score just above the cutoff specified for a given school and year for students in the 2007 through 2009 cohorts. The last two rows show heterogeneity in the first stage by low-income status in the IK bandwidth (Imbens and Kalyanaraman 2012). Below each coefficient is the F-statistic associated with the eligibility instrument. IK: Imbens and Kalyanaraman 2012.

$^{**}p < 0.05$; $^{***}p < 0.01$.

In table 4, I present regression-based estimates in the difference in probability of enrolling in an RVTS in ninth grade between students who received, and did not receive, an offer of admission at the 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 an RVTS as a function of receiving the offer of admission. Point estimates of this jump are relatively stable across choices of bandwidth, though they differ somewhat by school. In the combined sample, my F-statistics always substantially exceed the threshold of ten suggested by Stock, Wright, and Yogo (2002).

Causal Impact of an Offer of Admission on Student Outcomes

In figure 3, I provide visual evidence of discontinuities in my four outcomes of interest at the admissions cutoff. There is an apparent discontinuity in the probability of

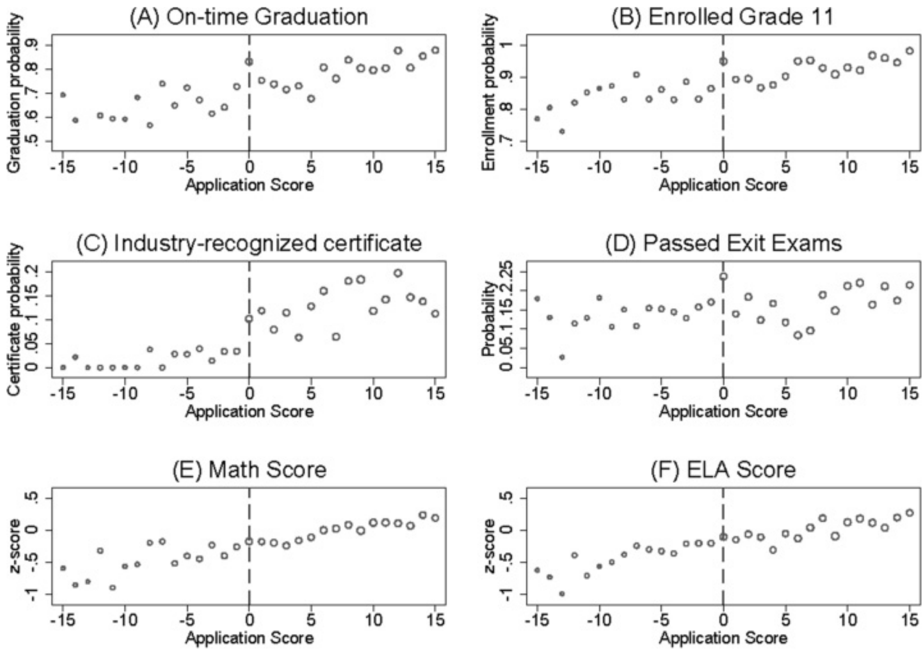

Figure 3. Reduced-form Impact of Offer of Admission for All Schools.

graduating on time for students near the cutoff who received an offer of admission. There are also similar discontinuities in remaining students enrolled by eleventh grade and in the probability of earning a certificate. Evidence is inconclusive for whether a student passes both graduation exit exams, and no real evidence of a difference in test scores. Visually, the difference in the probability of graduating from high school for students near the cutoff is about 10 percentage points.

In table 5 I present my reduced-form estimates of the effect of attending an RVTS, including heterogeneity by whether a student is free- or reduced-price lunch–eligible. My estimates suggest a large and positive impact of being offered a spot in an RVTS near the admissions threshold on high school graduation. Using the IK bandwidth (Imbens and Kalyanaraman 2012), I interpret the statistically significant point estimate of 0.13 in column 1 as a 7 percentage-point jump in the probability that a higher-income student who is offered admission to an oversubscribed RVTS graduates on time relative to his peers who just missed receiving this offer. There are also 5 percentage-point increases in the probability of remaining enrolled through eleventh grade, and earning an industry-recognized certificate for marginal students offered admission. For lower-income students the graduation benefits are slightly smaller and less precise for these outcomes, though including covariates in the last row boosts magnitude and precision. Magnitudes of outcomes fluctuate somewhat by bandwidth choice but do not change the substantive conclusions of the effects. Estimates for lower-income students are more sensitive to these choices, and such fluctuation could be related to the imbalance at the cutoff.

Table 5. Reduced-Form Estimates of the Effect of an Offer of Admission on High School Graduation Probability

	Graduated (1)	Enrolled Grade 11 (2)	Earned Certificate (3)	Math Score (4)	ELA Score (5)	Pass Both (6)
Higher income, IK bandwidth	0.133***	0.055***	0.056***	0.128**	0.122	−0.009
	(0.023)	(0.019)	(0.012)	(0.056)	(0.097)	(0.046)
Lower-income offer	0.04	0.01	0.04***	−0.02	−0.05	−0.02
	(0.034)	(0.028)	(0.014)	(0.067)	(0.082)	(0.051)
N	1,756	2,291	2,606	2,473	2,501	1,756
Higher income, bandwidth = 6	0.166***	0.064***	0.060***	0.179***	0.254**	−0.033
	(0.018)	(0.016)	(0.014)	(0.053)	(0.099)	(0.052)
Lower-income offer	0.08**	0.02	0.04***	0.02	0.03	−0.03
	(0.045)	(0.038)	(0.011)	(0.060)	(0.070)	(0.063)
N	1,023	1,023	1,023	903	913	1,023
Higher income, bandwidth = 15	0.095***	0.048**	0.057***	0.131**	0.126	−0.019
	(0.026)	(0.018)	(0.012)	(0.056)	(0.098)	(0.040)
Lower-income offer	0.01	0.01	0.04***	−0.02	−0.05	−0.03
	(0.029)	(0.025)	(0.014)	(0.068)	(0.083)	(0.043)
N	2,606	2,606	2,606	2,336	2,364	2,606
Higher income, IK bandwidth, controls	0.074***	0.027	0.045***	−0.022	−0.008	−0.033
	(0.024)	(0.028)	(0.012)	(0.048)	(0.084)	(0.042)
Lower-income offer	0.10***	0.05*	0.04***	−0.02	−0.04	−0.00
	(0.033)	(0.027)	(0.015)	(0.091)	(0.052)	(0.054)
N	1,756	2,291	2,606	2,473	2,501	1,756
μ	0.664	0.858	0.028	−0.303	−0.207	0.154

Notes: Heteroskedasticity robust standard errors clustered by score are in parentheses. The estimates shown are intent-to-treat effects generated using ordinary least squares and include school-by-year fixed effects and a triangular kernel, with standard errors clustered at the admission score level. Estimates do not include covariates unless otherwise noted. ELA: English language arts; IK: Imbens and Kalyanaraman 2012.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Because my RD sample consists of only three schools that represent thirteen (of twenty-six) such oversubscribed RVTSs, I rely on the OLS results in panel B of table 2 as evidence that the effects estimated for this subsample are somewhat comparable to their peer schools. Though the RD sample is one percent the size of the overall sample, the point estimates and statistical significance for attainment and persistence are quite similar. In the RD sample there is some evidence of a possible positive impact on test scores, especially for lower-income students. Given my limited sample, I argue that these schools—and the associated results—have reasonable though limited external validity in the context of nonurban Massachusetts RVTSs.

Instrumental Variables Results

In table 6, I present my instrumental variables (IV) estimates of the effect of the treatment-on-the-treated, or the effect of being admitted and enrolling in an RVTS versus not. These estimates are simply my reduced-form estimates by income status scaled by their respective first stage. These results suggest clear, large, positive effects on graduation and earning an IRC, with less precise estimates on persistence and no effects on test scores for the marginal student induced into an RVTS. Across specifications the magnitudes differ, but in all cases there appear to be positive and similar effects for both higher- and lower-income students.

Table 6. Instrumental Variables Estimates of the Effect of Attending an RVTS on Student Outcomes

	Graduated (1)	Enrolled Grade 11 (2)	Earned Certificate (3)	Math Score (4)	ELA Score (5)	Pass Both (6)
IK BW high income	0.349***	0.047*	0.136***	0.253	0.225	−0.035
	(0.047)	(0.026)	(0.028)	(0.157)	(0.253)	(0.139)
IK lower income	0.21***	0.05*	0.11***	0.04	−0.02	−0.05
	(0.049)	(0.028)	(0.033)	(0.170)	(0.227)	(0.153)
N	1,756	3,833	2,606	2,473	2,501	1,756
BW = 6 high income	0.528***	0.079	0.204***	0.577***	0.801**	−0.115
	(0.077)	(0.061)	(0.019)	(0.169)	(0.387)	(0.189)
BW = 6 lower income	0.38***	0.06	0.18***	0.31*	0.42	−0.11
	(0.095)	(0.084)	(0.033)	(0.163)	(0.308)	(0.207)
N	1,023	1,023	1,023	903	913	1,023
BW = 15 high income	0.202***	0.082***	0.135***	0.263	0.235	−0.054
	(0.063)	(0.028)	(0.028)	(0.161)	(0.257)	(0.105)
BW = 15 lower income	0.08	0.08***	0.11***	0.05	−0.01	−0.07
	(0.063)	(0.033)	(0.033)	(0.173)	(0.231)	(0.114)
N	2,606	2,606	2,606	2,336	2,364	2,606
IK BW high, controls	0.233***	0.019	0.114***	−0.052	−0.029	−0.087
	(0.057)	(0.037)	(0.032)	(0.100)	(0.173)	(0.131)
IK BW lower, controls	0.32***	0.09***	0.13***	−0.05	−0.09	−0.04
	(0.059)	(0.034)	(0.034)	(0.179)	(0.113)	(0.164)
N	1,756	3,833	2,606	2,473	2,501	1,756

Notes: Heteroskedasticity robust standard errors clustered by application score are in parentheses. IV estimates show the impact of attending an oversubscribed RVTS on each of the outcomes stated in the column heading, where attending an RVTS 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-by-school fixed effects. The sample consists of those members of the 2007 through 2009 cohorts who are present in the data in eighth grade. BW: bandwidth; ELA: English language arts; IK: Imbens and Kalyanaraman 2012.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

5. DISCUSSION

The results from my analyses suggest three important points. First, both of my analytic strategies suggest a clear benefit of attending an RVTS in grade 9 as measured by indicators of attainment (graduation, passing required exams) and specific forms of human capital (certificate completion). My OLS results suggest these effects exist for the average student, and the RD estimates suggest that similar effects may hold for the marginal student admitted to an oversubscribed RVTS. Second, effects on attainment are larger for lower-income students in the OLS, but not as clearly larger in the RD sample, suggesting that average effects might be larger for lower-income students on average, rather than when they are the marginal student in a more selective school. Third, effects appear to accrue through persistence in school and acquisition of specific human capital (IRCs) rather than a measurable improvement in general math and reading skills. Below, I briefly address concerns about threats to validity, situate the effects on attainment in the relevant literature on high school interventions, and consider the mechanisms through which these effects might accrue.

Threats to Validity

Both my OLS and RD analytic approaches are susceptible to concerns about internal validity. In both cases I have outlined why these concerns should be minimized by the fact that the two approaches offer corroborating evidence of the effects of participating in CTE, particularly as it relates to persistence, high school graduation, and earning an IRC. In OLS the main concern is omitted variables bias—specifically, that even conditioning on known proxies for admission to an RVTS, as well as observable student characteristics and town of residence, students in RVTSs may be self-selected on unobserved characteristics. Evidence from my OLS models on the RD sample should substantially address these concerns because that sample consists entirely of students who choose to apply to an RVTS, which should be correlated with most of the unobservable characteristics that could undermine causal inference in this setting. Thus, although strong ignorability (Rosenbaum and Rubin 1984) cannot be guaranteed, it may be a more reasonable assumption under these circumstances. I argue that the OLS estimates from the RD sample, which are nearly identical to the larger OLS sample, are less prone to omitted variables bias, and their similarity to the general results provides support—though imperfect—for interpreting the larger OLS estimates as causal.[16]

In my RD approach two concerns arise: (1) the potential imbalance in the share of students who are free- or reduced-price lunch eligible at the margin of admission, and (2) the potential endogeneity of the interview score for the total admissions score. I deal with the first concern by including covariates in one specification and showing that it does not change the overall set of findings. Although this is not perfect, it at least establishes that accounting for this imbalance statistically does not undermine the core results.

To address the second concern, I capitalize on the fact that one of the three oversubscribed schools retained both overall admissions scores and subscores.[17] I then test the sensitivity of my core results by removing the potentially endogenous interview subscores and replacing them with the average interview score for that application year so that all students were pulled to the mean on this element (while maintaining the original cutoff score to determine the offer of admission). Though this introduces more fuzziness to the first stage, there is still a discontinuity and statistically significant reduced-form estimates of scoring just above the admissions threshold (see figure A.2 and table A.2). This suggests that even removing the potential benefit of an above-average interview score does not undermine the positive effect of being offered admission to an RVTS on the other elements of the application materials.

Plausibility of Effect Magnitudes

Although there are relatively few high-quality evaluations of systematic high school interventions that show effects, my reduced-form and OLS 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 OLS approach in both samples produced an estimate of about 10 percentage points, whereas my RD estimate of the intent-to-treat effect was closer to 7 percentage points. These estimates

16. My nonparametric and parametric matching estimators also yield similar evidence (see table A.1).
17. The state does not require that historical records of application scores be stored, nor whether records of individual subscores be maintained. Thus, this specification check is only possible in this one case in my data.

are roughly in line with Deming et al. (2014), 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, and Smith 2005) found a boost in the probability of high school completion of 8 percentage points for students who were randomly admitted to the schools. The New York small schools program (Bloom and Unterman 2014) saw increased probabilities of graduation on the order of 9 percentage points over a baseline 59 percent probability of graduation. In addition, Rodríguez-Planas (2012) finds a bump of 5 percentage points in the probability of graduating high school as a result of a comprehensive mentoring program in grades 9 through 12.

My IV estimates of the effect of the treatment-on-the-treated suggest a larger effect than these other interventions (table 6), although these effects are for the marginal student admitted (LATE), rather than the average student who experiences the treatment (ATE). Using optimal bandwidths at each stage of my IV approach I estimate the local average treatment effect at between 17 and 35 percentage points. As I show in the last row of table 5, the mean share of students who graduate from high school for those who just miss an offer of admission to an oversubscribed RVTS in my sample was 0.66. This suggests that the marginal student was at high risk for not completing high school and so is perhaps less alarming. This low probability of graduating for the marginal student not receiving an offer might also explain why the LATE is not different for lower- and higher-income students. Presumably students on this margin of admission are facing multiple impediments to their potential graduation from high school and so family income may not be a differentiating characteristic on the admissions margin, though it appears to be in the general population.

Mechanisms

Though my analytic approaches do not allow for strong causal identification of the mechanisms through which attending an RVTS affects student outcomes, my choice of outcomes, the policy context, and related extant literature present some insights into how the effects I estimate might be realized. First, if the RVTSs simply represent a higher-quality educational environment relative to a student's residentially assigned school, the effects may accrue through that mechanism. Earlier literature that explored the impact of choice-based educational settings suggests that choice can produce positive impacts, provided the outside option represents an improvement in quality over a student's assigned school (Angrist, Pathak, and Walters 2011; Deming 2011; Dobbie and Fryer 2013; Deming et al. 2014). Therefore, if the RVTSs offer a higher-quality environment than a student's assigned school we might expect to find positive effects, even if enrolling in and attending an RVTS also improves a student's match to an academic or technical program of interest. Said another way, peer effects and environment might positively augment the positive impact of an improved match (Holzer, Linn, and Monthey 2013; Deming et al. 2014). My descriptive data (table 1) show lower-average middle and high school test scores for students who attend RVTSs relative to students in comprehensive schools. As a result, it seems implausible that differences in quality—as solely defined by peer academic performance—would drive the attainment effects I find. Rather, other mechanisms may be more likely and offer more specific pathways for these effects. Below, I consider whether other elements of quality, specific to the RVTS treatment, might generate effects.

Second, it appears the overall story is more about impact on persistence and completion than on influencing math and ELA learning (as measured by test scores). Evidence that the overall effects are driven by persistence and completion is supported by the clear positive impact on high school graduation, as well as the evidence of better persistence through eleventh grade. These effects are clear in both analytic approaches, though effects on persistence for higher-income students in the RD analysis is less apparent. The absence of an effect on test scores further suggests that the main benefit of attending an RVTS is accruing to students through their higher probability of completing high school, rather than higher general human capital (Becker 2009; Lazear 2009). Much higher rates of successful completion of IRCs also points to benefits accruing via accumulation of specific human capital (Becker 1962, 2009), although there are no reliable measures in the administrative data of other soft or noncognitive skills that may also accrue.

Effects appear to operate through persistence and earning of IRCs, which may suggest that students (or schools) understand the importance of the signaling (Spence 1973; Clark and Martorell 2014) value associated with completing a high school diploma or its equivalent (Murnane, Willett, and Tyler 2000; Tyler, Murnane, and Willett 2000). It may be that RVTS participation facilitates the development of specific human capital—as measured by IRCs—which may also influence persistence based on perceived labor market benefits.

Finally, the structure of the RVTS's learning environment may make learning more relevant and engaging, while simultaneously reducing the stigma associated with participating in CTE, and providing better mentorship opportunities, even if the general education in math and English is comparable to another school setting. In addition to offering clear connections between formal education and applied learning, RVTSs provide substantial exposure to the same instructors across multiple years—providing the potential for informal mentoring that has been shown in other settings to improve students' attachment to school (Black et al. 2010; DuBois et al. 2011). This is also consistent with work that has shown that CTE participation can positively impact students' attachment to school (Plank, DeLuca, and Estacion 2008). In addition, RVTSs also offer the potential for reduced stigma associated with participating in these programs relative to a comprehensive school setting. Although my empirical evidence cannot wholly support this claim, prior work does suggest that students have historically been negatively selected into CTE (Donahoe and Tienda 1999). Such negative selection could increase the opportunity for stigma if CTE participation were synonymous with lower academic performance or misbehavior. Because 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.

6. CONCLUSIONS

Using rich administrative data from Massachusetts, I provide plausibly causal estimates of the benefits to high school persistence and graduation, and the earning of

IRCs, for students who participate in a unique model of CTE (where all students engage in 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.

Although more is known about intensive models of CTE delivery internationally (Iannelli and Raffe 2007; Malamud and Pop-Eleches 2010; Busemeyer, Cattaneo, and Wolter 2011; Hanushek, Woessmann, and Zhang 2011; Van de Werfhorst 2011), as U.S.-based policy makers consider ways to offer a breadth of educational pathways for their students, the Massachusetts model of RVTSs 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 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 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. Neverthless, 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 there are clear labor-market returns to some forms of certificates that can be earned in community college settings (Kriesman et al. 2013; Huff Stevens, Kurlaender, and Grosz 2015; Xu and Trimble 2016). These papers suggest, unsurprisingly, that returns are higher in some areas (health services) than in others. Policy makers and researchers should continue to explore the potential for alignment in CTE-related offerings across the secondary–postsecondary threshold.

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APPENDIX

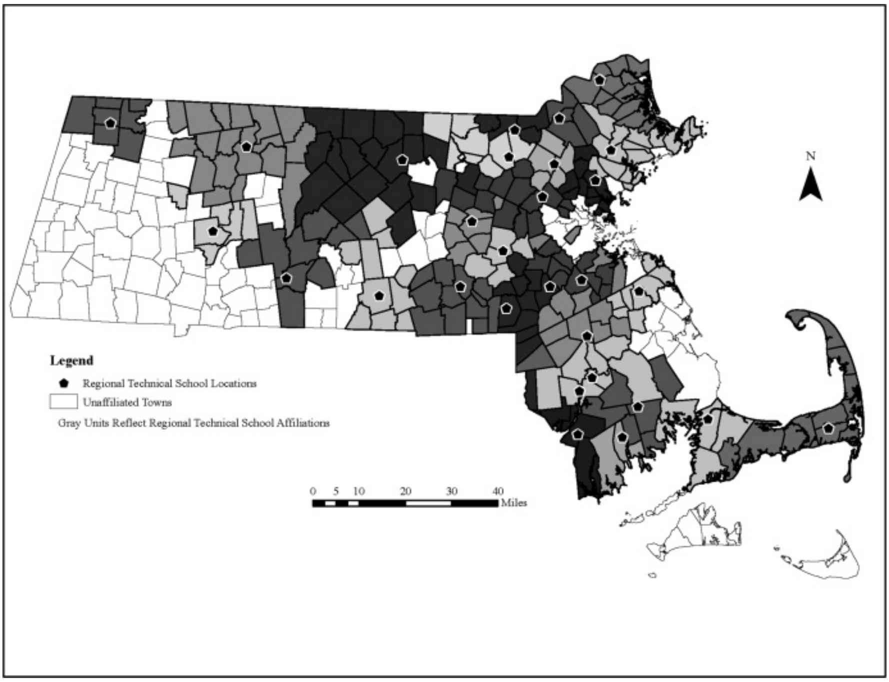

Figure A.1. Regional Vocational and Technical School Locations with Affiliated Towns.

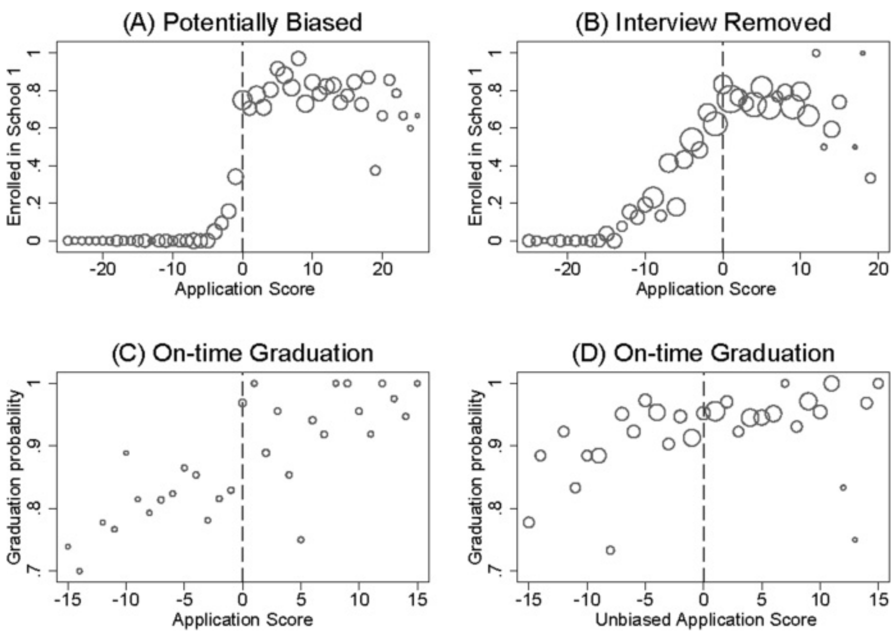

Figure A.2. Differences in First Stage When Interview is Removed for One School Where Subscores Are Available.

Table A.1. Estimates Using Multiple Matching Estimators

	CEM (1)	Propensity Score (2)	Inverse Propensity Weights (3)	Nearest Neighbor Mahalanobis (4)
On-time graduation	0.04***	0.053***	0.037***	0.049***
	(0.003)	(0.003)	(0.005)	(0.003)
N	410,176	417,215	417,215	417,215

Notes: Heteroskedasticity robust standard errors are in parentheses. 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 2015 cohorts who are present in the data in eighth grade. CEM: coarsened exact matching (see Iacus, King, and Porro 2012).

$^{***}p < 0.01$.

Table A.2. Reduced-form Estimates of the Effect of Attending an RVTS on Student Outcomes When Removing Potentially Endogenous Interview Score from Admissions Score

	Graduated (1)	Earned Certificate (2)	Math Score (3)	ELA Score (4)	Pass Both (5)
IK bandwidth	0.051***	−0.058	0.152**	0.118	−0.022
	(0.013)	(0.035)	(0.067)	(0.072)	(0.06)
N	560	715	661	661	759
Bandwidth = 6	0.051***	−0.065	0.212***	0.154**	0.007
	(0.013)	(0.036)	(0.049)	(0.064)	(0.058)
N	560	560	560	560	560
Bandwidth = 9	0.036**	−0.051	0.048	−0.005	−0.022
	(0.016)	(0.035)	(0.09)	(0.099)	(0.059)
N	759	759	759	759	759
Bandwidth = 12	0.012	−0.052	−0.014	−0.069	−0.034
	(0.02)	(0.035)	(0.092)	(0.104)	(0.053)
N	1,029	1,029	1,029	1,029	1,029
Bandwidth = 9, controls	0.034**	−0.056	0.012	0.011	−0.017
	(0.015)	(0.037)	(0.062)	(0.06)	(0.055)
N	758	758	758	758	758

Notes: Heteroskedasticity robust standard errors clustered by application score are in parentheses. Reduced-form estimates show the impact of attending an oversubscribed RVTS on student outcomes. In these models only one school is used. The forcing variable is the admissions scores purged of the interview component, and with the mean interview score imputed so that the initial cutoff score for admission could be retained to define eligibility for admission. This increases the fuzziness of the discontinuity, but arguably removes the only potentially endogenous element of application scores. The coefficients shown are generated by local linear regression using a triangular kernel of the listed bandwidth, including cohort fixed effects. The sample consists of those members of the 2007 through 2009 cohorts who are present in the data in eighth grade. ELA: English language arts; IK: Imbens and Kalyanaraman 2012.

$^{**}p < 0.05$; $^{***}p < 0.01$.

Table A.3. Testing Functional Form Assumptions

	Graduated (1)	Enrolled Grade 10 (2)	Earned Certificate (3)	Math Score (4)	ELA Score (5)	Pass Both (6)
Linear, BW = IK	0.087***	0.021	0.045***	−0.019	−0.022	−0.020
	(0.023)	(0.013)	(0.011)	(0.041)	(0.052)	(0.044)
N	1,756	3,833	2,606	2,473	2,501	1,756

Table A.3. Continued.

	Graduated (1)	Enrolled Grade 10 (2)	Earned Certificate (3)	Math Score (4)	ELA Score (5)	Pass Both (6)
Up to quadratic, BW = IK	0.141***	0.040**	0.048***	−0.027	0.060	−0.054
	(0.041)	(0.017)	(0.013)	(0.040)	(0.076)	(0.077)
N	1,756	3,833	2,606	2,473	2,501	1,756
Linear, BW = 6	0.117***	0.017	0.050***	−0.080	−0.006	−0.038
	(0.026)	(0.022)	(0.008)	(0.074)	(0.045)	(0.051)
N	1,023	1,023	1,023	903	913	1,023
Up to quadratic, BW = 6	0.120***	−0.005	0.027	−0.075	0.024	−0.083
	(0.033)	(0.043)	(0.016)	(0.089)	(0.076)	(0.091)
N	1,023	1,023	1,023	903	913	1,023
Linear, BW = 12	0.076**	0.028	0.046***	−0.021	−0.002	−0.021
	(0.022)	(0.017)	(0.011)	(0.040)	(0.053)	(0.043)
N	2,108	2,108	2,108	1,885	1,906	2,108
Up to quadratic, BW = 12	0.128***	0.018	0.048***	−0.046	0.084	−0.037
	(0.030)	(0.024)	(0.013)	(0.051)	(0.073)	(0.071)
N	2,108	2,108	2,108	1,885	1,906	2,108
Linear, BW = 15	0.057**	0.030**	0.045***	−0.018	−0.020	−0.027
	(0.023)	(0.014)	(0.011)	(0.042)	(0.052)	(0.038)
N	2,606	2,606	2,606	2,336	2,364	2,606
Up to quadratic, BW = 15	0.127***	0.020	0.047***	−0.030	0.061	−0.023
	(0.029)	(0.022)	(0.014)	(0.040)	(0.076)	(0.063)
N	2,606	2,606	2,606	2,336	2,364	2,606

Notes: Heteroskedasticity robust standard errors clustered by score are in parentheses. The reduced form estimates reported here were generated using ordinary least squares 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. BW: bandwidth; ELA: English language arts; IK: Imbens and Kalyanaraman 2012.

$^{**}p < 0.05$; $^{***}p < 0.01$.