Charter High Schools' Effects on Long-Term Attainment and Earnings

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Abstract

Since their inception in 1992, the number of charter schools has grown to more than 6,800 nationally, serving nearly three million students. Various studies have examined charter schools' impacts on test scores, and a few have begun to examine longer-term outcomes including graduation and college attendance. This paper is the first to estimate charter schools' effects on earnings in adulthood, alongside effects on educational attainment. Using data from Florida, we first confirm previous research (Booker et al., 2011) that students attending charter high schools are more likely to graduate from high school and enroll in college. We then examine two longer-term outcomes not previously studied in research on charter schools—college persistence and earnings. We find that students attending charter high schools are more likely to persist in college, and that in their mid-20s they experience higher earnings. © *2016 by the Association for Public Policy Analysis and Management.*

INTRODUCTION

Charter schools—publicly funded schools of choice that operate outside the direct control of traditional school districts—have grown rapidly since their inception two decades ago. More than 6,800 schools operate in more than 40 states, serving nearly three million students. Most of the research on charter schools' efficacy has focused on short-term effects on student test scores. This paper makes new contributions to a much thinner literature on the longer-term effects of charter schools. Using longitudinal data from Florida, this study extends beyond previous research on the effects of attending charter schools on high school graduation and college enrollment (Booker et al., 2011) by examining college persistence and earnings.[1]

Previous charter school studies have focused on test score impacts, collectively covering a wide array of jurisdictions. Some have used quasi-experimental methods with longitudinal data (e.g., Bifulco & Ladd, 2006; Booker et al., 2007; Davis & Raymond, 2012; Furgeson et al., 2012; Hanushek et al., 2007; Sass, 2006; Zimmer et al., 2003, 2009, 2012; Zimmer & Buddin, 2006). Others have employed experimental approaches using data from admission lotteries (Abdulkadiroglu et al., 2011; Angrist et al., 2013b; Furgeson et al., 2012; Gleason et al., 2010; Hoxby & Murarka, 2007; Hoxby & Rockoff, 2004). The findings from this research are mixed. The totality of

[1] In Booker et al. (2011), they included Chicago as part of the analysis. In this follow-up study, we did not include Chicago because we were unable to obtain earnings data for Chicago.

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evidence suggests that any difference in *average* performance of charters and traditional public schools across the country is probably small; however, it is clear that some types of charter schools (e.g., Knowledge is Power Program [KIPP] and other "no-excuses" charter schools serving disadvantaged urban students) significantly and substantially improve their students' test scores (Angrist et al., 2013b; Dobbie & Fryer, 2013a; Tuttle et al., 2013).

While measuring charter schools' impact on test scores is important, it may not capture the full scope of the impact schools have on students. In fact, nontest score outcomes such as high school graduation, college enrollment and persistence, and earnings may be of greater consequence than test scores. For instance, the financial advantage associated with a college education has long been recognized (Day & Newburger, 2002) and in recent years, its value has become increasingly apparent as manufacturing jobs have vanished and the wages of high school educated workers have stagnated. Even as the cost of higher education has increased substantially, the value of a degree has continued to grow. Recognizing this, the Obama administration and other policymakers and funders have sought to increase access to college and improve students' readiness for college.

Recognizing the importance of college, a few recent studies have used postsecondary enrollment as an outcome to evaluate the impact of a variety of K-12 programs and policies (Bettinger et al., 2012; Chetty, Friedman, & Rockoff, 2014; Chingos & Peterson, 2012; Deming et al., 2014; Richburg-Hayes et al., 2009). Compared to the voluminous literature on achievement effects, research on the impact of charter schools on educational attainment—including high school graduation, college attendance, and college persistence—is still sparse. Booker et al., (2011) was the first to examine attainment outcomes associated with charter schools and they found that students attending Chicago and Florida charter high schools were 7 to 15 percentage points more likely to graduate and 8 to 10 percentage points more likely to enroll in college than comparison groups of students who attended charter middle schools but matriculated to traditional public high schools. Subsequently, Furgeson et al. (2012) found evidence that impacts on high school graduation and college entry vary in different charter school management organizations (CMOs), but that some CMOs appeared to produce substantial positive attainment impacts. Angrist et al. (2013a), relying on randomized admissions lotteries, found that Boston charter high schools had positive impacts on measures of college preparation (such as SAT scores), no statistically significant impact on high school graduation, and an effect of shifting students from two-year colleges into four-year colleges.[2] Another study (Dobbie & Fryer, 2013b) found significantly positive attainment impacts, but it examined only one charter school.

The findings of these studies are intriguing, but they also raise additional questions. In particular, how should policymakers, parents, and citizens interpret positive attainment results when many studies[3] have shown little or no effect on test scores? Skeptics could argue that positive effects on graduation and postsecondary attendance could be illusorily if schools are setting lower graduation standards and not actually preparing their students for college or employment. Alternatively, charter schools might produce larger effects on attainment than on test scores because

[2] Other studies have estimated impacts of non-charter schools that are similar to charters in some respects including examination of small high schools (Bloom & Unterman, 2013), Catholic high schools (Evans & Schwab, 1995; Grogger & Neal, 2000; Neal, 1997; Sander & Krautmann, 1995), and voucher studies (Chingos & Peterson, 2012; Wolf et al., 2013) with similar results.

[3] In Chicago, Booker et al. (2009) used the same methodology as Booker et al. (2011) and found little effect on test scores, but strong graduation and college attendance effects. For the current study, we apply the same research design as this current paper in Florida, and find a statistically insignificant relationship in reading and a statistically significant negative relationship of 0.075 of a standard deviation in math.

they are endowing students with skills, knowledge, work habits, motivation, and values that are important for long-term success but are not fully captured by test scores. Notably, no studies have attempted to measure the effect of charter schools on eventual earnings in adulthood.

This paper extends the literature by going beyond graduation and postsecondary admission rates to examine whether students who attended charter high schools are more likely to persist in postsecondary institutions and ultimately achieve higher earnings. We use a similar research design as Booker et al. (2011) to address the potential selection bias inherent in studies of schools of choice. Lacking data on randomized admissions lotteries, we restrict our attention to a sample of students who were enrolled in charter schools in eighth grade. The treatment students in our analysis enrolled in charter schools in ninth grade; the comparison students switched to conventional public schools. The counterfactual condition, in other words, is represented by students who also had once chosen to enroll in charter schools rather than by students who had never chosen charter schools. We also address selection by matching on observable baseline student characteristics (including eighth-grade test scores) and by conducting a sensitivity analysis that uses the distance to the nearest charter high school as an instrument for enrollment, as well as other approaches that restrict the comparison group based on their choice set of schools.

In Booker et al.'s (2011) study of charter high schools in Chicago and Florida, the schools had not been operating long enough to permit the authors to follow a sufficient sample of charter graduates beyond initial college entry. In Florida, we collected long enough timeframe of data that we now are able to track earnings of former charter high school students when they are 23 to 25 years old. In the analysis, we first examine graduation rates and college attendance as a check to see if the original results from Booker et al. hold with a larger sample that includes additional years of data. Next, we examine college persistence, which will help answer the question of whether the higher rates of college attendance in the Booker et al. study were illusory—that is, if students enter college, but quickly dropout, then the advantage in initial college entry would not be much of a benefit to the students.[4] We then turn to the main focus of our paper, examining labor outcomes of students who are up to 12 years removed from the eighth-grade baseline year—yielding the first evidence on the effects of charter schools on earnings in adulthood. We find that enrollment in a charter high school is associated not only with higher rates of high school graduation and college entry, but we also find some evidence of increased persistence in college and increased long-run earnings.

METHODS

Determining the impact of charter high schools is not easy, due to the inherent selection problem implicit in any study of school choice: students who select into charter high schools may be different in ways that are not readily observable from those who choose to attend traditional public high schools. The fact that the charter students and their parents actively seek an alternative to traditional public schools suggests the students may be more motivated or their parents may be more involved in their child's education than are the families of traditional public school attendees.

[4] While we do not report the Chicago results in this paper, we did collect and analyze additional data on high school graduation, college attendance, and college persistence outcomes in Chicago. The results from these analyses were substantively similar to the results we ascertained for Florida, but since they largely replicated our previously published findings, we chose not to present them in this paper. The results are available upon request.

Alternately, the students may be those who are having difficulty in traditional public schools. These characteristics are likely to affect later student outcomes, making it difficult to distinguish the effect of the school from the effect of underlying (and typically unobservable) student characteristics.

The two methods most commonly used to deal with selection bias in school choice impact studies—experimental methods using randomized admissions lotteries and nonexperimental, longitudinal approaches using pretreatment measures of the outcome of interest—are not available to us. Lottery-based studies (see, e.g., Angrist et al., 2013b; Gleason et al., 2010; Hoxby & Murarka, 2007) identify oversubscribed schools that use randomized admissions lotteries to allocate scarce spaces among applicants. Admissions lottery results are not available in our data. Even if they were available, prior experience (Furgeson et al., 2012; Tuttle, Gleason, & Clark, 2012; Tuttle et al., 2013) demonstrates that they could be used to create valid experimental treatment and control groups in only a small percentage of the schools, dramatically reducing statistical power and raising questions about external validity (see also Abdulkadiroglu et al., 2011; Bifulco, Cobb, & Bell, 2009; Zimmer & Engberg, 2016).

The longitudinal approach using pretreatment measures of the outcomes of interest is often useful in examining impacts on test scores because, in reading and math, students typically take tests repeatedly over many years. The change in test scores for individual students who move between traditional public schools and charters can be used to infer the impacts of the charter schools on student achievement, while holding time-invariant student/family characteristics constant. Two recent studies (Furgeson et al., 2012; Tuttle et al., 2013) have demonstrated that longitudinal analyses of test score impacts that control for pretreatment test scores can closely replicate randomized experimental impact estimates for the same students. But this approach cannot be used to measure long-term outcomes such as graduation, college enrollment, college persistence, and employment, because those outcomes do not occur before a student's enrollment in a charter school.

With the usual approaches unavailable, we use other strategies to deal with selection bias. The first involves identifying a strong comparison group. In this approach, we restrict the sample to students who attended a charter school in grade 8, just before beginning high school. The motivation for this is that unmeasured student/family characteristics that lead to the selection of charter high schools and which influence outcomes later in life are also likely to be related to the choice of a charter school at the middle school level. This is the same approach that Altonji, Elder, and Taber (2005) take to assess the attainment effects of Catholic high schools. The approach potentially limits the external validity of the results, because effects on charter high school students who attended charter middle schools might differ from effects on charter high school students who did not attend charter middle schools. Sacrificing some external validity is worthwhile to promote internal validity, however.

Using a comparison group of charter middle school students does not perfectly solve the selection problem, because "back end" selection bias could occur through the comparison students' choice to exit the charter sector after eighth grade. But this kind of selection is likely to be far less consequential than the initial selection into the charter sector, because enrolling in a new school in ninth grade is a normal transition that is undertaken by most students regardless of whether they are changing school sectors. Large numbers of students in both the treatment group and the comparison group are transferring to new schools to begin high school, and, from the perspective of the student, changing sectors (from charter to traditional public) is likely to be less salient than changing schools with or without a sector change. In consequence, even though the comparison students' voluntary exit from the sector could in principle

create a selection bias problem, our focus on a typical transition year should reduce the likelihood of such a bias.

To further deal with potential endogeneity, we also use a matching approach popularized by Rubin (1977) and Rosenbaum and Rubin (1983). While matching procedures can take many forms, we use a one-to-one nearest-neighbor Mahalanobis matching approach (also referred to as a covariate match) in which we match on observable characteristics to create a control group.[5] We then examine difference in student outcomes between those in treatment relative to this counterfactual control group. Formally, this can be specified as (Smith & Todd, 2001):

$$\Delta^{tt} = E(y_1|x, z = 1) - E(y_0|x, z = 1). \quad (1)$$

This approach can give causal results in estimating a "treatment" when observable characteristics (x) are sufficient to make the counterfactual outcome y_0 independent of z.

$$y_0 \perp z|x.$$

In our case, by restricting the population of students to those who are enrolled in charter schools in eighth grade, we make an argument that conditional on the vector of covariates x, z, and y_0 are independent. After creating a control group of non-charter high school students, we then compare the mean difference in outcomes.

A similar matching approach was used (without a prior restriction to a charter population) in a recent report on charter middle schools affiliated with the KIPP (Tuttle et al., 2010), an analysis of charter school authorizers (Zimmer et al., 2014), and in CREDO's (2009, 2013) evaluation of charter schools. These evaluations are bolstered by research suggesting that the creation of a carefully matched comparison group in some circumstances produce impact estimates that replicate the findings of randomized experiments (Cook, Shadish, & Wong, 2008). More recently, research has suggested that a matching strategy can replicate randomized design results when examining school choice programs (Bifulco, 2012; Furgeson et al., 2012). In this paper, we report the results using the matching approach as our primary analysis. However, later we conduct sensitivity analysis using modifications to the restricted sample for the matching analysis as well as an instrumental variable (IV) approach to provide further evidence of the robustness of our results.

DATA

Studying effects of K-12 interventions on long-term outcomes demands linked data on individual students from K-12 program participation through postsecondary enrollment, postsecondary persistence, employment, and earnings. Even when links are available to connect K-12 data with postsecondary and earnings data, a long time series is needed; studying the long-term effects of a high school intervention requires prehigh school data back to eighth grade and posthigh school information into college and beyond. In addition, the jurisdiction studied must have a sufficient sample of students participating in the intervention (and a sufficient sample of comparison students) to provide reliable results. Florida represents one among perhaps a handful of places where all the necessary data elements are currently in place.

[5] Using *teffects nnmatch* routine in Stata, we matched to the closest non-charter high school student based on the nearest Mahalanobis distance with no caliper restriction with replacement. Multiple matches were included in the event of ties. The algorithm imputes the missing potential outcome for each student by using an average of the outcomes of similar students in the control group.

To create the data set for the analysis, we had to merge data from a variety of sources. The primary source for student-level information is the Florida Department of Education's K-20 Education Data Warehouse (K-20 EDW), an integrated longitudinal database covering all public school students and teachers in the state of Florida. The K-20 EDW includes detailed enrollment, demographic, and program participation information for each student, as well as their reading and math achievement test scores. As the name implies, the K-20 EDW includes student records for K-12 public school students and students enrolled in community colleges or four-year public universities in Florida. The K-20 EDW also contains information on the Florida Resident Assistance Grant (FRAG), a grant available to Florida residents who attend private colleges and universities in the state. Data from the National Student Clearinghouse (NSC), a national database that includes enrollment data from 3,300 colleges throughout the United States, is used to track college attendance outside the state of Florida, as well as any private college enrollment in Florida that the FRAG data do not pick up. Unfortunately, the Florida Department of Education's data-sharing agreement with the NSC expired in the latter part of the 2000s, so we can only reliably track students who attended private colleges and universities within Florida or any postsecondary institution outside of Florida through school year 2006 to 2007.[6]

The identity and location of schools is determined by the Master School ID files (for public K-12 schools) and the Non-Public Master Files (for private schools) maintained by the Florida Department of Education. Grade offerings are determined by enrollment in October's membership survey and by the school grade configuration information in the relevant school ID file.

We also collect information on employment outcomes from the Florida Education and Training Placement Information Program (FETPIP). FETPIP reports information for any individual who has participated in any public education or training program in Florida. The FETPIP data contain unemployment insurance (UI) records, which provide information on a person's quarterly earnings and the employer's North American Industry Classification System (NAICS) code. This allows us to determine the employment status and income of all Florida high school students who remain in the state and are employed in industries covered by UI.[7] The Florida Department of Education routinely links these data to elements in the K-20 EDW and assigns an individual-level anonymous student ID code.

High school graduation is determined by withdrawal information and student award data from the K-20 EDW. Only students who receive a standard high school diploma are considered to be high school graduates. Students earning a GRE or special education diploma are counted as not graduating. Similarly, students who withdrew with no intention of returning or exited for other reasons (such as nonattendance, court action, joining the military, marriage, pregnancy, or medical problems), but did not later graduate, are counted as not graduating. Students who died while in school are removed from the sample. It is not possible to directly determine the graduation status of students who leave the Florida public school system to attend a home-schooling program or to enroll in a private school, or who move out of state. Similarly, some students leave the public school system for unknown reasons. In the sample, students whose graduation status is unknown are more likely to have

[6] Information on the NSC is available at www.studentclearinghouse.org.

[7] Excluded are members of the armed forces, the self-employed, proprietors, domestic workers, and railroad workers covered by the railroad UI system. In addition, only about half of all workers in agricultural industries are covered. The leakage due to college enrollment outside of Florida is relatively small. Among students in our sample, 52 percent go to college, but only 4 percent go to postsecondary institutions out of state.

Table 1. Number of charter schools in operation, by grade range and year.

Grade offerings	1998 to 1999	1999 to 2000	2000 to 2001	2001 to 2002
Elementary only	25	37	52	68
Elementary, middle, and high school grades	2	5	4	8
Elementary and middle grades	15	21	35	40
Middle grades only	12	20	23	24
Middle and some high school grades	2	4	1	3
Middle and all high school grades	6	5	6	7
Only high school grades	5	13	20	26
Total	67	105	141	176

Note: Number of charter schools and grade ranges based on student membership counts.

lower eighth-grade test scores and possess other characteristics associated with a reduced likelihood of graduation. They also are more likely to attend a traditional high school initially, rather than a charter high school. To avoid possible bias associated with differential sample attrition, we impute the graduation status for those students whose graduation outcome is unknown, based on predicted values from a regression model of graduation.[8] Because we can track college attendance both within and outside of Florida, no imputation is necessary for the college attendance variable. Any individual who does not show up as enrolled in a two-year or four-year college or university is classified as a nonattendee.

The available data cover four cohorts of eighth-grade students in Florida. Statewide achievement testing for eighth-grade students began in school year 1997 to 1998, so the first cohort in the sample are students who attended eighth grade in 1997 to 1998.[9] The last available year of K-12 and in-state college enrollment data is 2009 to 2010. Out-of-state postsecondary data are available only through the 2006 to 2007 school year, however. Employment data are available through calendar year 2011. Because we want to be able to determine employment outcomes after most students have completed their postsecondary education, the last cohort we include in the analysis are students who attended grade 8 in 2000 to 2001 (and began high school in 2001 to 2002).

Table 1 provides an overview of the number of charter schools operating in Florida, broken down by grade offerings and year. The number of charters operating grew rapidly, nearly tripling over the four years that the sample cohorts would have entered ninth grade. Traditional grade groupings dominate among Florida charter schools: roughly two-thirds of charter schools offer only elementary, middle, or high school grades.

[8] Imputation was done with the *uvis* procedure in Stata. All variables reported in Table 3, except for charter high school attendance, were used to predict graduation. If students whose graduation status is unknown are removed from the sample, we obtain similar, though somewhat larger, estimates of charter attendance on high school graduation. If all students with an unknown graduation status are assumed to be dropouts, we obtain even larger estimates.

[9] Data on limited English proficiency (LEP) and special education program participation begin in 1998 to 1999, so they are not available for the first eighth-grade cohort. For these students, we use their LEP and special education status in ninth grade.

Table 2. Mean values of student characteristics for treatment and comparison groups after matching (high school graduation analysis sample).

	Charter in G8, charter in G9 (treatment group)	Charter in G8, traditional in G9 (comparison group)	Difference
Math score, G8 (normed)	0.115	0.115	0.000
Reading score, G8 (normed)	0.126	0.126	−0.000
Female	0.485	0.486	−0.002
Black	0.173	0.178	−0.005
Hispanic	0.182	0.181	0.001
Asian	0.017	0.017	0.000
LEP/bilingual in G8	0.012	0.012	0.000
Special ed in G8	0.103	0.103	0.000
Free/R-P lunch in G8	0.223	0.224	−0.002
Change schools in G7-8	0.714	0.724	−0.010
Disciplinary incidents in G8	0.170	0.116	0.054
Disciplinary incidents one year prior to G8	0.320	0.244	0.076
Disciplinary incidents two years prior to G8	0.174	0.141	0.033
1997 G8 cohort	0.001	0.001	0.000
1998 G8 cohort	0.102	0.103	−0.001
1999 G8 cohort	0.340	0.336	0.004
2000 G8 cohort	0.557	0.561	−0.004
Number of observations	1,141	1,141	

Note: †Significant at 10 percent; *significant at 5 percent; **significant at 1 percent.

Table 2 provides summary statistics on student characteristics for the treatment and matched control groups. In the table, we distinguished students by transition type: charter middle school to charter high school (the treatment group for the analysis) and charter middle school to traditional public high school (the comparison group).[10] Included in Table 2 is the difference in means between the treatment and comparison groups, which includes a *t*-test of whether the mean differences between the groups are statistically significant. As evident from the table, we have good balance as we do not have a single observable characteristic that is statistically or substantively different across the treatment and control group.

Before describing results in the next sections, we note some limitations. First, like all quasi-experimental designs, our approach comes with a number of assumptions for the results to be viewed as causal. In particular, we assume that all students who were in a charter school in eighth grade possess similar unobservable characteristics. We cannot be certain that this assumption is correct. Later, we address possible selection on unobservables with a set of sensitivity analyses, but we acknowledge that the potential of bias due to differences in the unmeasured characteristics of treatment and control groups cannot be entirely dismissed. Second, the external validity of our findings for different charter schools in different states is uncertain. Our analysis is based on data from a single state with one set of charter policies and students that differ from those in other states, where results might differ. This

[10] Throughout the analysis, exposure to a charter high school is defined by the type of school a student attends in grade 9, whether or not the student subsequently stays in that type of school. This is done to avoid selection bias problems associated with transfer out of treatment; therefore, the estimates of charter school effects should be interpreted as analogous to "intent to treat" impact estimates.

is a limitation of any research that examines a policy in a specific location. The fact that our previous work (Booker et al., 2011) found very similar results (for student attainment effects) in a second location (Chicago) is encouraging, however. Third, like any study of long-term outcomes, our study is examining the effects of a treatment as it existed in the past. In the interim, conditions could have changed in the treatment (in this case, the number and types of charter schools). Therefore, the analyses may have limitations in their applicability to the current set of charter schools in Florida. This limitation is inherent in any study of long-term outcomes. Finally, our analysis focuses on the subset of charter high school students who had also attended charter schools in eighth grade. The design cannot measure the effects of charter high schools on students who were not in charter schools in eighth grade, potentially limiting the external validity of the results. This loss of external validity was necessary to promote the study's internal validity by identifying a comparison group of students who provide a plausible counterfactual. (Later we describe analyses that gauge how sensitive our results are to this restriction.) Despite the limitations described above, this paper provides some of the first empirical evidence on the relationship between attending a charter high school and long-term outcomes, including earnings.

RESULTS

Implementation of Analytic Approach

We first replicate Booker et al.'s (2011) previous analysis of high school graduation and college enrollment with an expanded sample that includes additional years of data. We measure high school graduation as receiving a standard high school diploma within five years of entering ninth grade. College enrollment is determined by enrollment in any postsecondary institution within six years of starting high school.

We extend the analysis in this paper by considering the long-run effects of charter high school attendance on persistence in college and earnings. We gauge persistence by assessing whether a student is enrolled in any postsecondary institution at least two consecutive years. The two-year persistence measure is important, because it typically takes at least two years to obtain a degree from a community college. In addition, dropout from four-year higher education institutions is highest in the first year, meaning persistence into the second year is correlated with degree completion in four-year institutions as well (Berkner & Choy, 2008). Measuring persistence over a longer period for attendees of four-year institutions would be desirable, but data limitations prevent us from conducting useful analyses of longer-term persistence and degree completion.[11] More specifically, the available NSC data on college enrollment ends in 2006 to 2007, so we could only track our first cohort of students (who entered high school in 1998) through four years of college, and then only if they graduated high school within four years and entered college immediately.

In addition, and most importantly, we possess earnings data through the end of calendar year 2011 and can determine annual income for four student cohorts in the 10th and 11th years after beginning grade 8 and for three of the four cohorts 12 years after entering grade 8. For example, employment of our last cohort of eighth graders (those attending grade 8 in 2000 to 2001) is measured through calendar year

[11] Interpreting persistence beyond two years is problematic for students enrolled in community colleges. If a student goes to community college for two years they can get an Associate degree, which may be their terminal degree.

Table 3. Estimates of the effect of attending a charter high school on educational attainment (average treatment effects on the treated).

	Nearest-neighbor matching model
Receive standard high school diploma within five years	0.061**
	(0.020)
	[$N = 2{,}282$]
Attend a two-year or four-year college within six years	0.088**
	(0.026)
	[$N = 2{,}286$]
Persist in any college at least two consecutive years (unconditional)	0.117**
	(0.027)
	[$N = 2{,}142$]
Persist in any college at least two consecutive years (conditional on initial enrollment in college)	0.062*
	(0.031)
	[$N = 1{,}396$]

Notes: †significant at 10 percent; *significant at 5 percent; **significant at 1 percent. Robust Abadie–Imbens standard errors are reported in parentheses. The following characteristics are used as match criteria: student demographics, English-language skills, special education program participation, family income (proxied by free/reduced-price lunch status), mobility during middle school, disciplinary incidents during middle school, eighth-grade test scores in math and reading, and a set of cohort indicators. *N* is the sum of treated observations and matched control observations (including ties).

2011. A student in that cohort who took four years to finish high school and four years to finish college would graduate from college in spring 2009, which would be nine years after the beginning of grade 8. The following year (10 years after entering grade 8) represents the first full year of earnings after potentially graduating college. To account for initial employment in temporary jobs, early spells of unemployment, or employment in occupations outside one's long-term profession, we measure the maximum annual earnings 10, 11, or 12 years from initial enrollment in grade 8. This latter measure is potentially the most reliable, because it maximizes our sample size and accounts for many of the short-term fluctuations in employment and earnings that can frequently occur among young job market entrants.

ESTIMATES OF ATTAINMENT IMPACT

Table 3 presents the estimated impacts of charter high schools on students' subsequent academic attainment, as measured by high school graduation, college entry, and college persistence with the standard errors clustered at the high school level. For the analysis, we match on student demographics, English-language skills, special education program participation, family income (proxied by free/reduced-price lunch status), baseline disciplinary incidents and mobility during middle school.[12] Importantly, as part of the match, we include student ability and prior educational inputs by incorporating eighth-grade math and reading test scores.[13] We do not include the proximity of charter schools in the set of matching variables (although

[12] English-language skills are measured by participation in an LEP program. Student mobility is measured by an indicator for students who changed schools between grades 6 and 7 or between grades 7 and 8.

[13] For test scores, we use the Student's normed scores on the FCAT-SSS test, a criterion-referenced test based on the state's curriculum standards. The Stanford Achievement Test is also administered to students in Florida, but administration of the Stanford test did not begin until school year 1999 to 2000.

later we describe an alternative analysis that uses proximity as an instrument for treatment). This choice is guided by previous research that demonstrates that variables unrelated to the outcome of interest should not be included in the matching criteria, as they would increase the variance of an estimated exposure effect without decreasing bias (Augurzky & Schmidt, 2001; Brookhart et al., 2006; Caliendo & Kopeinig, 2008).[14] (It is exactly these features of distance—predictive of treatment but unrelated to outcomes—that makes it a candidate for the IV approach that we conduct as an alternative analysis.)

The numbers in the first two rows of Table 3 present updated estimates of the relationship between charter high school attendance and the probabilities of earning a standard high school diploma within five years as well as enrolling in college. Similar to the results reported in Booker et al. (2011), we find that charter high school enrollment is positively associated with educational attainment. For high school graduation, we observe a 6 percentage point increase in the probability of earning a standard high school diploma within five years.[15] Likewise, the estimate of the relationship between attending charter high schools and the probability of enrolling in college is positive and statistically significant with an estimate of a 9 percentage point increase in the probability of attending college.[16]

The third row shows results for an outcome that Booker et al. (2011) could not examine in the preceding paper—persistence in college. We define persistence as attending college at least one semester in consecutive academic years following initial college entry. The estimated relationship between charter high school attendance and college persistence is positive and statistically significance with about a 12 percentage point advantage for charter high school students.

The result for college persistence is a combination of the effect on the likelihood of graduating high school, the effect on enrolling in college for those who graduate from high school, and the effect of persisting in college for those who enroll in college. It would be interesting to know the relationship between charter high school attendance and persistence for the subset of students who enter college. There is no straightforward way to produce an unbiased estimate of the effect on college persistence conditional on college entry, however, because the treatment (charter high school enrollment) affects the likelihood of entering college. The methodological problem is analogous to a problem often seen in studies of wage impacts when the treatment affects labor force participation as well as wages (see, e.g., Heckman 1979; Lee 2009). In other words, by increasing the number of students attending college, the charter high school treatment changes the sample of students in a conditional analysis of effects on those attending college, creating a sample bias relative to the comparison group.

Nevertheless, we can modify the matching analysis that was used to produce the unconditional impact estimates in the third row of Table 3 by constraining the sample to include only those students who attended college, but the resulting conditional impact estimates will be biased by the sample change produced by

[14] We examined the effect of adding distance as a matching criterion for our analysis of charter attendance on earnings. Inclusion of distance variables increased the magnitude of the estimated effect on earnings and increased the standard errors, consistent with results from previous simulation-based research (Brookhart et al., 2006).

[15] The results reported here differ slightly from those in Booker et al. (2011) both because we are using the alternative matching approach (as opposed to probit analysis with controls) and because we now have more years of data, which allows us to include an additional cohort in the estimation of diploma receipt and college attendance within five years of attending grade 8 for Florida.

[16] In Booker et al. (2011), a five-year window for college enrollment was employed. With the additional data ascertained since this paper, we extend the window for college entry to six years after high school entry, thereby providing an opportunity for late-graduating high school students to enter college.

the effect on college entry. Given that the treatment has increased the number of students entering college—presumably adding students who had lower ability levels in eighth grade and perhaps less motivation for college—the conditional impact estimate is likely to be biased downward. The fourth row in Table 3 shows the results of the conditional estimate of a 6 percentage point advantage for charter high school students, which suggests that charter school graduates are significantly more likely to persist for two years, even after controlling for postsecondary enrollment.

Overall, the results reaffirm the results from Booker et al. (2011)—attending a charter high school is associated with higher levels of educational attainment. Importantly, in the measure Booker et al. could not observe in our previous paper (i.e., college persistence), we observe a positive relationship between attending a charter high school and college persistence, which suggests that Booker et al.'s results were not simply a function of charter high schools reducing graduation standards or pushing students into college when they are not prepared for the rigor of college.

Impacts on Earnings

While high school graduation, college attendance, and persistence in college are all very important, the most important and unique contribution of study is to provide a measure of effects on earnings in adulthood—a primary goal of education for most students. Our analysis examines whether students who attend charter high schools are associated with higher subsequent earnings than observationally equivalent students who attend traditional public schools (again restricting the analysis so that treatment group and comparison group were enrolled in charter schools in eighth grade). We measure the labor outcome as annual earnings up to 12 years after the student's start of the eighth-grade year, unconditionally of whether the student attended college. Assuming a normal progression of four years to graduate from high school and four years of college, we would be measuring earnings in the calendar year up to three years after college graduation. Assuming kindergarten entry at age 5 and no grade repeating in elementary or middle school, students would be up to age 25.[17] In the analysis, we examine whether any observed earnings difference is due solely to increased postsecondary attainment by reestimating the earnings equation separately for the subsamples of students who did and did not attend a two-year or four-year college within six years of grade 8. For the earnings analysis, we included an exact match on counties to control for different access to employment opportunities.

Table 4 displays the results from the full sample (both students that did and did not go to college) as well as the segmented samples. The analysis of the full sample shown in the first row suggests a statistically significant advantage of over $2,300 for charter high school students. Given average maximum earnings over the 10-, 11-, and 12-year windows are equal to $19,366 for traditional high school attendees in the matched sample, the estimated impacts are large, equivalent to a 12 percent increase in the maximum earnings achieved over the three-year span.

In rows two and three, we display the results of students who did and did not attend college. While the estimate for charter high school students who did not attend college in row two is nearly identical, it is no longer statistically significant as the sample size drops significantly from 1,631 students to 480 students. For college

[17] To account for part-time or temporary work, we restricted the sample to individuals earning at least $1,000 for the year or at least $1,000 in each quarter. Imposing these restrictions did not substantively change our conclusions. As a further robustness check we reran our analysis, treating missing values as zero earnings. The earnings advantage decreased slightly from $2,318 to $1,991 and the estimated advantage continues to be statistically significant.

Table 4. Estimates of the effect of Florida Charter High School attendance on maximum annual earnings 10, 11, or 12 years since beginning grade 8, by college attendance (average treatment effects on the treated).

Sample	Nearest-neighbor matching model
All	2,318.36*
	(1,162.11)
	[$N = 1{,}631$]
No college within six years	2,325.09
	(2,067.04)
	[$N = 480$]
Attend college within six years	3,029.24*
	(1,364.96)
	[$N = 1{,}147$]

Notes: †significant at 10 percent; *significant at 5 percent; **significant at 1 percent. Robust Abadie–Imbens standard errors are reported in parentheses. The following characteristics are used as match criteria: student demographics, English-language skills, special education program participation, family income (proxied by free/reduced-price lunch status), mobility during middle school, disciplinary incidents during middle school, eighth-grade test scores in math and reading, and a set of cohort indicators. *N* is the sum of treated observations and matched control observations (including ties). The number of observations in the "college" and "no college" categories may not sum to the total number of observations due to missing values for college attendance.

attendees, the estimated charter coefficient is positive and significant, suggesting that, even among students who went to college, charter students have an earnings advantage. Therefore, college attendance alone cannot explain the advantage of the earning difference we see for the full sample.

SENSITIVITY ANALYSES

As noted above, despite our best efforts to minimize selection bias, some bias could remain. For instance, our analysis relies upon a set of students that all attended charter schools in eighth grade with treatment students going on to attend a charter high school while the control students attend traditional public high schools. Although these students may have been similarly motived to attend a charter middle school, students and their families may have undergone changes while attending middle school that lead them to make different high school choices. If any unobserved changes not only affected the choice of high school type but also later student performance, our initial estimate could be biased. We address this concern with two sets of alternative analyses, one of which continues to use matching techniques but makes alterations to the treatment and control groups and another that uses an alternative IV estimation approach.

First, we build off our current analysis by using the same treatment group—students who attended a charter school in eighth grade and attended a charter high school in ninth grade. However, we modify the control group in the hope of further minimizing any selection bias. Previously, the control group was a matched set of students drawn from the restricted population of students who attended a charter school in eighth grade, but who chose to attend a traditional public high school in ninth grade. This set of students includes a mix of students who chose not to attend a charter high school for a variety of reasons. In some cases, students may have switched because of some observable event—for example, because they have not performed well in a charter middle school or had disciplinary issues. While including these students in our control group is a concern, we argue that our current matching approach should minimize this potential problem as we match on

observable characteristics such as baseline test scores and discipline incidents. In other cases, students may switch to traditional public high schools for unobserved reasons. For instance, some students may switch for extra-curricular opportunities. To the degree that these unobserved motivations are correlated with the ultimate outcomes and uncorrelated with the observed student characteristics we match on, the inclusion of these students could create selection bias. However, there could be a third subset of students who switch to traditional public high school because there is no local charter high school option. Many of these students would be motivated to attend a charter high school, but cannot because no local charter high school exists. As a sensitivity analysis, we focus on the third subset of students as our control students as we restrict our pool of students to match on those students that did not have a charter high school available to them within five miles when entering ninth grade.[18] We argue that many of these students would attend a charter high school if a local charter school were available. Therefore, these students may be more similar in their motivation than the other two subsets of students in our original control set of students.

While the above approach may help with student unobserved selection into schools, it may not deal with another possible source of bias—the endogenous location of charter high schools. For charter operators, some geographic areas may be more appealing than others and cause operators to locate charter schools in nonrandom ways. To the degree that the motivation of charter operators to locate in certain areas is correlated with the ultimate student outcomes and uncorrelated with the matched set of observed student characteristics, the nonrandom location of charter high schools could create selection bias. To minimize this possible bias, we take one more step—we restrict the control group of students to those who attended charter middle schools but lived in an area where no charter high schools existed nearby at the time the students entered high school, but which did possess a charter high school sometime in the future. In other words, the control group of students did not have an option to attend a charter high school when they entered high school but would have had that option if they had entered high school in a later year.

In sum, the revised matching approach (which we refer to as a geographically restricted matching) uses the same treatment group from the primary analysis, but the pool of students to match on for the set of control students is different. This pool of control students contains students who attended a charter school in eighth grade, attended a traditional public high school in ninth grade, did not have a nearby charter high school option when they entered high school, but would have a charter high school available to them within five miles sometime in the future. Using this pool of students, just like in our primary analysis, we conduct a nearest-neighbor Mahalonbis match (using the same observable student characteristics as match criteria as before). Conceptually, we argue that this could be a preferred model. However, the geographic restriction of this revised approach significantly reduces the sample of students we can draw upon for the nearest-neighbor match and therefore we argue that this provides for a good sensitivity analysis but should not be used as our primary analysis.

As noted above, our primary analysis is based on a set of students that all attended charter schools in eighth grade, some of which remain in the charter sector in ninth grade while others switch to a traditional public school in ninth grade. A concern with this approach is that students switching sectors (i.e., charter to traditional) could be different in unmeasured ways from students who remain in the same sector

[18] We measure physical proximity of other charter high schools by the minimum linear distance from a student's eighth-grade charter to a different charter school offering grade 9.

(although, as we have noted above, focusing on the beginning of high school, when most students are changing schools anyway, should minimize this problem). To examine this issue, we conduct a second sensitivity analysis in which we change the pool of students in the comparison group. Instead of using students who switched from a charter school in eighth grade to a traditional public school in ninth grade as our control, we utilize students who were in traditional public schools in both eighth and ninth grade as our comparison group. We refer to this approach as the "nonswitcher" approach. The disadvantage of this approach is that it does not have the benefit that both the treatment and control students all chose to attend a charter school in the middle grades (and thus may not account for important unmeasured student and family characteristics that persist from middle to high school).

In a similar vein, as a third sensitivity analysis, we exclude from the estimation sample students who attended charter schools that offered both middle and high school grades. While having a nontraditional grade configuration may be part of the charter school "treatment," students who do not have to switch schools to attend grade 9 face a rather different choice than do students who must choose between moving to a different charter high school or to a different traditional public high school. Restricting the sample in this way does come at a cost, however. Only a bit more than half (26 of 44) high schools in the final year of our analysis (2001/2002) were "stand-alone" high schools that offered only the traditional grade range of 9 through 12.

Finally, for the sake of comparison, we reestimate our models, but include as the treatment group all students attending charter schools in grade 9 (not just those students who attended a charter school in grade 8) and allow the comparison group to be drawn from all students attending traditional public schools (regardless of whether the student switched into a traditional public school from a charter school). Using a more inclusive population has the potential to increase the external validity of the results. However, using the full sample is the weakest approach for internal validity because it implicitly assumes that students who attended charter middle schools possess the same unmeasured characteristics as students who were enrolled in traditional middle schools.

Table 5 displays the results from our original analysis across the four primary outcomes as well as the results from the set of sensitivity analyses based on alternative samples: the geographically restricted sample, the nonswitchers approach, the stand-alone high school sample, and the full sample. Focusing first on the sensitivity analyses designed to account for student unobservable characteristics (columns 2 through 4), across the educational attainment measures, the results suggest the same substantive conclusions as the primary analyses as all the estimates are positive and statistically significant. The estimates for charter impacts on earnings are of similar magnitude to those from the main analysis for the geographically restricted sample (column 2) and the analysis excluding students attending charter schools with both eighth and ninth grades (column 4), but are no longer statistically significant.[19] For the "nonswitchers" analysis (column 3), the estimated impact on earnings drops in magnitude to $822 and is no longer statistically significant. Finally, in the full-sample analysis (column 5), we find no statistically significant effects. This is consistent with the notion that students who select into charter schools in the middle grades are different in unmeasured ways from students who attend traditional middle schools, and that those unmeasured differences affect long-run outcomes.

[19] The reduced sample size for the analysis of earnings with the charter location restriction is due to the imposition of exact matching of treatment and control groups by county. Without the exact match criterion, the sum of treatment and matched control observations is 1,640.

Table 5. Sensitivity analysis using geographically restricted sample of matched schools.

Outcome	Nearest-neighbor matching results from our primary analysis (1)	Excluding traditional high school (HS) attendees with charter HS within five miles (2)	Compares students who attended a charter in grade 8 and a charter in grade 9 to students who attended a traditional public school in both grade 8 and grade 9 (3)	Excluding students attending charter middle schools that offered grade 9 (4)	Compares students who attended a charter in grade 9 to students who attended a traditional public school in grade 9, irrespective of school type in grade 8 (5)
Receive standard high school diploma within five years	0.061**	0.148**	0.041**	0.049†	−0.016
	(0.020)	(0.049)	(0.015)	(0.030)	(0.010)
	[$N = 2,282$]	[$N = 2,282$]	[$N = 2,447$]	[$N = 1,002$]	[$N = 6,202$]
Attend a two-year or four-year college within six years	0.088**	0.215**	0.084**	0.122**	−0.003
	(0.026)	(0.047)	(0.018)	(0.042)	(0.011)
	[$N = 2,286$]	[$N = 2,286$]	[$N = 2,472$]	[$N = 1,016$]	[$N = 7,278$]
Persist in any college at least two consecutive years (unconditional)	0.117**	0.288**	0.096**	0.180**	0.002
	(0.027)	(0.044)	(0.018)	(0.044)	(0.010)
	[$N = 2,142$]	[$N = 2,142$]	[$N = 2,313$]	[$N = 944$]	[$N = 4,745$]
Maximum annual earnings 10, 11, or 12 years since beginning grade 8 (full sample)—with exact match on county	2,318.36*	2,527.92	822.82	2,354.90	493.15
	(1,162.11)	(4,469.01)	(786.10)	(2,789.27)	(987.30)
	[$N = 1,631$]	[$N = 933$]	[$N = 1,638$]	[$N = 702$]	[$N = 3,141$]

Notes: †significant at 10 percent; *significant at 5 percent; **significant at 1 percent. Robust Abadie–Imbens standard errors are in parentheses. The following controls are used as match criteria in the matching model: student demographics, English-language skills, special education program participation, family income (proxied by free/reduced-price lunch status), mobility during middle school, disciplinary incidents during middle school, eighth-grade test scores in math and reading, and a set of cohort indicators. *N* is the sum of treated observations and matched control observations (including ties).

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Of course, we cannot rule out the possibility that our primary findings simply do not apply to the population of students attending traditional middle schools.

Overall, the results indicate that our educational attainment results are relatively robust to varying research designs. Focusing on the approaches that deal with selection bias caused by unmeasured factors, the analyses produce similar magnitudes in two out of the three alternative approaches when examining earnings as an outcome. In the "nonswitchers" approach (which, again, we see as the weakest approach among those designed to control for persistent unmeasured characteristics of students and families) the results are of smaller magnitude than our main findings, but still represent about a 4 percent increase in earnings. To give some perspective, Chetty, Friedman, and Rockoff (2014) found that a one standard deviation increase in teacher quality (which many agree is the single most important school input) in a single grade increases student eventual earnings by about 1 percent.

As a further check on the validity of our primary results, we consider an alternative mechanism for dealing with selection bias. More specifically, we conduct a two-stage, IV analysis that exploits variation in the location of charter high schools (relative to the charter middle schools the students attended) to predict charter high school enrollment (following the approach of Grogger & Neal, 2000; Neal, 1997, in their analyses of Catholic high schools). This plays out in two ways. First, some charter schools offer both middle and high school grades, effectively making the transition cost zero.[20] A charter middle school student is more likely to attend a charter high school if he or she can stay in the same school for high school grades. Second, as we noted in our discussion above, when a student must switch schools to attend high school, distance can vary greatly; the nearest charter high school could be down the street or many miles away. Proximity to a charter high school should make it more likely that a student will attend a charter high school.[21]

Depending on whether the outcome is dichotomous or continuous, we use a bivariate probit or IV approach, both of which use measures of proximity to charter schools as an instrument for charter high school enrollment.

Consider the following bivariate probit:

$$C^* = \beta_1' X_1 + u_1 \tag{2}$$

$$A^* = \beta_2' X_2 + \gamma C + u_2 \tag{3}$$

where C^* and A^* are latent variables and X_1 and X_2 are vectors of exogenous variables. We observe the binary choice, C, indicating charter high school attendance, where $C = 1$ if $C^* > 0$ and $C = 0$ if $C^* \leq 0$. Likewise, we observe the binary outcome, A (attainment of a high school diploma, college attendance, or college persistence, as applicable), where $A = 1$ if $A^* > 0$ and $A = 0$ if $A^* \leq 0$. The error terms, u_1 and u_2, are distributed as bivariate normal with mean zero, unit variance and correlation coefficient ρ. In our analysis of labor market outcomes, the dependent variable, earnings (E), is continuous:

$$E = \beta_3' X_3 + \delta C + u_3. \tag{4}$$

[20] Although many charter schools offering middle and high school classes have all grades in the same location, not all do. In some instances, there can be one common administration, but the high school campus may be physically separate from the middle school campus.

[21] See Harris and Larsen (2015) for an analysis of the determinants of high school choice. More details of the proximity variables are presented in the Appendix. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

We therefore employ two-stage least squares, where in the first stage a linear probability model is used to predict charter high school attendance as a function of charter location measures and other exogenous variables, X_3. In the second stage, equation (4) is estimated with C replaced by its fitted value, $\hat{C}$, and appropriate adjustments are made to the variance–covariance matrix.

Finally, in both the bivariate probit and the IV procedures, we test for endogeneity of charter high school attendance. In the bivariate probit, we test whether *rho,* the correlation between the error of the educational attainment equation and the error of the selection equation, is nonzero. In the IV regression of earnings, we conduct a "*C* test" of endogeneity (which is similar to a Hausman test, but allows for clustering of the standard errors; Baum, Schaffer, & Stillman, 2007) to determine whether the IV estimate differs significantly from an ordinary least squares (OLS) estimate. For the bivariate probit, the correlation between error of the regression equation and the error of the selection equation (measured by *rho*) is not statistically significant for high school graduation or for college persistence. Similarly, the *C* test results in a *P*-value of 0.70 and thus fails to reject the null hypothesis that the OLS estimate is consistent. Therefore, across three of the four outcomes, we fail to reject the null hypothesis that high school choice (conditional on attending a charter middle school and all the controls for observables) is exogenous. Put differently, we find no evidence that unobservable factors driving high school choice are affecting high school graduation, college persistence, or earnings. We therefore rely upon our matched analysis as our primary results and present the bivariate probit and IV estimates as a robustness check (we present further details of the bivariate and IV approach in the Appendix including the first stage results and the results from the exclusion-restriction test).[22]

Table 6 presents the IV estimates of the impact of charter school attendance on each of the four primary outcomes. In each case, the bivariate estimates for the educational attainment outcomes point in the same direction as the original matching estimates presented in Table 3, but are larger in magnitude and less precise (though still statistically significant at conventional levels for high school graduation and college attendance). For the earnings outcome, again, we obtain an estimate that is also somewhat larger in magnitude than the earnings results in Table 4, but is less precise.

In sum, the two sets of sensitivity analyses designed to account for unobservable student characteristics each provide further support for our primary results presented in Tables 3 and 4. For the geographically restricted matching approach, we found positive and statistically significant estimates for the educational attainment outcomes and earnings estimate of similar magnitude to that from our primary analysis, but which was statistically insignificant. For the bivariate probit and IV estimates, we found estimates of larger magnitudes but which were less precise. In the cases where the bivariate probit and IV estimates were not statistically significant, we fail to reject the null of exogenous high school choice (conditional on observable controls).

CONCLUSION

In previous work, we produced evidence that charter high school attendance has positive effects on the probability of graduating from high school and entering college (Booker et al., 2011). Enough time has now passed that the same cohorts

[22] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://onlinelibrary.wiley.com.

Table 6. Bivariate probit/two stage least squares (2SLS) model estimates of the effects of charter high school attendance on educational attainment and earnings (coefficient estimates are marginal effects).

Estimation method	Estimate	Endogeneity test (test of rho = 0; *C* test = 0)
Receiving a standard high school diploma (within five years)		
Matched estimate	0.061**	
	(0.020)	
Bivariate probit estimate	0.154**	$\chi^2(1) = 3.300$
	(0.054)	*P*-value = 0.069
Attending a two- or four-year college (within six years)		
Matched estimate	0.088**	
	(0.026)	
Bivariate probit estimate	0.251**	$\chi^2(1) = 4.184$
	(0.065)	*P*-value = 0.041
Persist in any college at least two consecutive years—unconditional		
Matched estimate	0.062**	
	(0.031)	
Bivariate probit estimate	0.146	$\chi^2(1) = 0.150$
	(0.095)	*P*-value = 0.698
Maximum earnings 10 to 12 years maximum annual earnings 10, 11, or 12 years since beginning grade 8 (full sample with county fixed effects)		
Matched estimate	2,318.36*	
	(1,162.11)	
Two-stage least squares	2,861.60	$\chi^2(1) = 0.099$
Estimate	(2,209.24)	*P*-value = 0.753

Notes: †significant at 10 percent; *significant at 5 percent; **significant at 1 percent. Standard errors adjusted for clustering at the school level are in parentheses. Coefficient estimates from bivariate probits are marginal effects. For the bivariate probit estimates, the reported standard errors equal the marginal effects divided by the bivariate probit *z*-scores (adjusted for clustering at the school level).

of students in Florida have had the opportunity to enroll in college for multiple years and to begin careers. In this paper, we use a similar approach as Booker et al. (2011)—relying on a restricted sample of students who were all (treatment and comparison group alike) enrolled in a charter school in eighth grade to estimate the relationship between attending a charter high school on educational attainment and earnings in adulthood. As in Booker et al., we find that attendance at Florida charter high schools is associated with both higher high school graduation and college attendance rates. Going beyond Booker et al., we also find that attendance at a charter high school is associated with a higher likelihood of persisting in college for at least two years. More importantly, we also examine data on the subsequent earnings of students in our analytic sample, at a point after they could have earned college degrees. In our primary analysis, charter high school attendance is associated with an increase in maximum annual earnings for students between ages 23 and 25 of $2,318—or about 12 percent higher earnings than for comparable students who attended a charter middle school but matriculated to a traditional high school.

To gauge the robustness of our results, we conduct a number of alternative analyses, including matching on observable characteristics, limiting the control group to students who did not have a charter school near to them and using the proximity of different types of high schools as instruments for attending a charter high school. In nearly all of the sensitivity analyses our estimates of the impacts of charter attendance on attainment outcomes are robust. While our sensitivity analyses for

earnings generally produce point estimates that are similar to those from our primary analysis, the estimates are not always statistically significant. Therefore, while we cannot completely rule out the possibility that the earnings estimates are biased by self-selection into charter high schools, taken as a whole, our results suggest that charter high schools in our sample had a positive effect on the later wages of their students. Whether the estimated positive effects of Florida charter high school attendance on educational attainment and earnings apply to a larger population of charter students, other jurisdictions, and other time periods must be left for further research.

The positive relationships between charter high school attendance and long-term outcomes are striking, given that charter schools in the same jurisdiction have not been shown to have large positive impacts on Students' test scores (Sass, 2006).[23] To the degree that our findings hold up in a broader set of charter students, locations, and different analytical approaches,[24] a natural question to ask is what is leading to these positive associations for educational attainment and earnings. For initial college entry, it may be that these charter schools are merely providing better counseling and encouragement to apply and enroll. But that could not explain higher rates of persistence in college or higher earnings. It is possible that charter high schools are endowing their students with skills that are useful for success in college and career but that test scores do not capture. For example, successful charter high schools might be particularly good at promoting skills such as grit, persistence, self-control, and conscientiousness—skills that are not fully captured in test scores but that are critical for long-term outcomes (Duckworth & Allred, 2012; Duckworth, Tsukayama, & Geier, 2010; Romer et al., 2010; Tsukayama et al., 2010). While there is anecdotal evidence that charter schools are trying to focus on these life skills, little systematic evidence yet exists to confirm this or to link these efforts to improved long-term outcomes. Further research is needed to test the hypothesis.

Further research is also needed to test whether our quasi-experimental findings in Florida hold up in randomized experimental studies and in charter schools in other locations and with a broader set of charter students. Nonetheless, this early evidence of positive effects for these students on educational attainment and earnings in adulthood raises the question of whether charter schools' full long-term impacts on their students have been underestimated by studies that examine only test scores. More broadly, the findings suggest that research examining the efficacy of educational programs should examine a broader array of outcomes than just student achievement.

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[23] Sass (2006) did not use our matching approach. However, we applied the same matching approach as used in this paper to examine test scores and found no statistically significant effect on test scores in reading and a significant negative effect in math.

[24] There are at least two other instances in which researchers have found similar patterns in the context of school choice. Wolf et al. (2013) found no statistically significant effect on test scores for Washington, DC voucher recipients, but did find effects on high school graduation. Similarly, in an examination of charter schools in an anonymous district, Imberman (2011) found little effect on test scores, but large effects on attendance and behavioral outcomes.

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APPENDIX

DETAILS OF THE BIVARIATE PROBIT AND IV APPROACHES

Here, we provide further details of bivariate probit and IV approach used as sensitivity analyses, including the first stage results and our results from the exclusion test.

Determinants of High School Choice

Whether one attends a charter school could be a function of parental/student preferences, as well proximity to school alternatives, including charter, private, and traditional public schools (Butler et al., 2013). Most private schools predate charters in our sample, and competition from private schools likely had an impact on the operation of traditional public schools even before charters came on the scene. Similarly, charter school entrants would likely take into account the availability and characteristics of nearby private schools when designing their schools. In a simple product variety model of competition, private schools would initially offer a mix of attributes that differs significantly from that of traditional public schools. If charters enter an environment with significant private school penetration, they would tend to locate "closer" to traditional public schools in product attribute space than if no private-school competitors existed.[25] Therefore, we would expect that, in areas where there is vibrant competition from private high schools, students would be less likely to choose a charter high school over a traditional high school because the charters would be more similar to the traditional public schools.[26]

We measure the physical proximity of other charter high schools by the minimum linear distance from a student's eighth-grade charter to a different charter school offering grade 9 and by the number of other charter high schools within various radii. Similarly, we capture the time cost of attending a traditional public school by the distance to the nearest traditional public high school. Because traditional public school students usually are assigned based on residential location, we do not include a count of the number of nearby traditional public high schools. One factor that would affect whether a student attends a charter school for high school is whether the middle school the student is currently attending offers high school grades, including grade 9. In this case, the distance would be zero.

In Table A1, we present probit estimates of the choice of attending a charter school in grade 9 as a function of both the grade offerings of a student's middle school and the availability of other school alternatives. We measure the grade configurations of charter schools by an indicator for whether or not a student's grade 8 charter school offered grade 9 in the year after the student attended eighth grade. As one would expect, the availability of ninth grade in the same school a student attended in eighth grade has a large positive impact on the likelihood of attending a charter school in grade 9. The availability of ninth grade in the same school raises the probability of attending a charter high school by 31 percentage points. Holding

[25] Glomm, Harris, and Lo (2005) also use a product differentiation approach and similar reasoning to empirically analyze the location decisions of charter schools.

[26] Depending on the size of the private sector and the distribution of consumer preferences, charters may choose to adopt attributes similar to those of private schools. We would, however, expect charters to be at least somewhat more similar to publics than are privates to publics. If public schools responded to preexisting private school competition by making their product "closer" to the offerings of privates, there would be less "distance" between publics and privates, which would also reduce the expected enrollment in charters.

Table A1. Probit estimates of attending a charter high school in grade 9, based on minimum distance and number of schools of given type within five miles offering grade 9 in relevant year (coefficient estimates are marginal effects).

Distance to nearest traditional public school	0.0041
	(0.0206)
Distance to nearest other charter	−0.0029
	(0.0037)
Eighth-grade charter offers grade 9	0.3126*
	(0.1589)
Number of other charters	0.0490
	(0.0479)
Number of private schools	−0.0395*
	(0.0218)
Observations	4,216
Pseudo *R*-squared	0.27

Notes: †significant at 10 percent; * significant at 5 percent; ** significant at 1 percent. Standard errors adjusted for clustering at the school level are in parentheses.

constant the number of charter schools within a given area, an increase in the distance to the nearest charter high school decreases the likelihood of attending a charter high school, as one would expect, although the estimates are imprecise and never statistically significant. Similarly, as expected, increases in the distance to the nearest traditional public high school are positively correlated with the likelihood of attending a charter school in grade 9, although (once again) the effects are not statistically significant. Consistent with competition on product variety, the number of private schools is negatively correlated with the probability of attending a charter school in ninth grade.

The possibility exists that the determinants of high school choice also affect the outcome of interest (educational attainment or earnings). For example, if combining middle and high school grades improves student learning or fosters greater school attachment, then schools that offer both grades 8 and 9 may have positive effects on both the likelihood of attending a charter school in grade 9 and the probability of graduating from high school. In Table A2, we present Wald tests of exclusion restrictions in the attainment and earnings equations for combinations of the explanatory variables used in the high school choice equation.[27] The distance to the nearest charter school, the number of charter schools and the number of private schools within five miles can be excluded from the graduation equation. For the college attendance equation, all of the variables predicting charter high school enrollment can be excluded except for the number of charter schools within five miles. For both the unconditional college persistence equation and the earnings equation all determinants of charter high school choice can be excluded from the outcome equation. All of the variables that are used to predict charter high school enrollment,

[27] Formally, a test of exclusion restrictions requires an overidentified system where all instruments are valid. However, it is common practice in the empirical literature to conduct the sort of informal tests we use here.

Table A2. Wald tests of exclusion restrictions in attainment equations.

Model/exclusion	Chi-squared (df)	Prob. value
High school graduation —All variables	24.85 (5)	0.0001
High school graduation —All variables except distance to nearest traditional public school	17.09 (4)	0.0019
High school graduation —All variables except distance to nearest other charter school	24.58 (4)	0.0001
High school graduation —All variables except G8 charter offers grade 9	22.76 (4)	0.0001
High school graduation —All variables except number of other charters	24.22 (4)	0.0001
High school graduation —All variables except number of private schools	19.92 (4)	0.0005
High school graduation —G8 charter offers G9, number of other charters, number of privates	11.80 (3)	0.0081
High school graduation —Distance to nearest other charter, number of other charters, number of privates	7.62 (3)	0.0545
High school graduation —Distance to nearest other charter, G8 charter offers G9, number of privates	12.29 (3)	0.0064
High school graduation —Distance to nearest other charter, G8 charter offers G9, number of other charters	16.42 (3)	0.0009
High school graduation —Distance to nearest traditional, number of other charters, number of privates	22.65 (3)	0.0000
High school graduation —Distance to nearest traditional, G8 charter offers G9, number of privates	24.20 (3)	0.0000
High school graduation —Distance to nearest traditional, G8 charter offers G9, number of other charters	18.46 (3)	0.0004
High school graduation —Distance to nearest traditional, distance to nearest other charter, number of privates	22.73 (3)	0.0000
High School Graduation —Distance to nearest traditional, distance to nearest other charter, number of other charters	16.55 (3)	0.0009
High school graduation —Distance to nearest traditional, distance to nearest other charter, G8 charter offers G9	17.82 (3)	0.0000
College attendance —All variables	15.47 (5)	0.0085
College attendance —All variables except distance to nearest traditional public school	12.99 (4)	0.0113
College attendance —All variables except distance to nearest other charter school	11.44 (4)	0.0220

Table A2. Continued.

Model/exclusion	Chi-squared (df)	Prob. value
College attendance —All variables except G8 charter offers grade 9	15.45 (4)	0.0039
College attendance —All variables except number of other charters	6.43 (4)	0.1695
Persist in college at least two years (unconditional) —All variables	9.34 (5)	0.0964
Persist in college at least two years (conditional on attending college) —All variables	15.22 (5)	0.0095
Persist in college at least two years (conditional on attending college) —All variables except distance to nearest traditional public school	10.88 (4)	0.0279
Persist in college at least two years (conditional on attending college) —All variables except distance to nearest other charter school	13.43 (4)	0.0094
Persist in college at least two years (conditional on attending college) —All variables except G8 charter offers grade 9	8.01 (4)	0.0914
Persist in college at least two years (conditional on attending college) —All variables except number of other charters	14.92 (4)	0.0049
Persist in college at least two years (conditional on attending college) —All variables except number of private schools	15.21 (4)	0.0043
Maximum earnings 10 to 12 years after grade 8 —All variables	1.58 (5)	0.1660

except for offering grade 9, can be excluded from the conditional college persistence equation.