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Social Returns to Private Choice? Effects of Charter Schools on Behavioral Outcomes, Arrests, and Civic Participation

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Social Returns to Private Choice? Effects of Charter Schools on Behavioral Outcomes, Arrests, and Civic Participation

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Abstract

The vast majority of literature on school choice, and charter schools in particular, focus on attending an elementary or middle school and often focus on test scores or other proximal outcomes. Much less is known about the long-term effects of attending a charter high school. It is important to fill this information void for a few reasons. First, schools in general affect more than just students' test scores. Second, high schools make up a larger share of the charter sector. Third, school choice depends on freely available information for parents and students to make informed decisions about where to attend, including potential long-term benefits. We add to the empirical research on charter school effects by using a doubly-robust regression-adjusted propensity score matching approach to evaluate the impacts of charter high school attendance on 9th grade behavioral outcomes and individuals propensity to commit crime and participate in elections as young adults in North Carolina, a state with a large and growing charter school sector.

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1 Introduction

In many communities, charter schools have become a central feature of the public education ecosystem (e.g., New Orleans, District of Columbia). A rich body of evidence has emerged over the last two decades evaluating the short-term effects of charter schools on students' academic outcomes. As the sector has matured and expanded, researchers have begun to study the effects of charter schools on long-term educational attainment and labor market outcomes (e.g., Angrist et al., 2016; Davis & Heller, 2017; Sass, Zimmer, Gill, & Booker, 2016). With this maturation and growth, it has also become increasingly important to examine the short- and long- run effects of charter schools on a wider range of outcomes, including non-academic outcomes. For example, far less is known about the effect of charter schools on important behavioral and civic outcomes.

There are several reasons to look beyond academic outcomes to assess the efficacy of charter schools more broadly. First, evidence highlights the important role of non-cognitive skills in individuals' educational attainment and labor market success (Heckman, Stixrud, & Urzua, 2006; Heckman & Rubinstein, 2001). Examining the effect of charter schools on behavioral outcomes, which are reflective of both cognitive and non-cognitive skills (Heckman et al., 2006), allows us to investigate additional channels through which charter schools may affect students' later life outcomes. Second, and perhaps more fundamentally, there is a long-standing view echoed throughout the history of public education in the United States that schools play a crucial role in preparing young people to participate in civic life (Labaree, 1997; Wolfinger, & Rosenstone, 1980). In fact, preparation for citizenship was one of the central goals of Horace Mann's common schools movement in the mid- 19th century (Guttman, 1987; Labaree, 1997). Charter schools represent a new approach to the provision of public education as quasi-private entities. As such, it is important to assess whether charter schools are contributing to broader democratic

objectives for public education. As the charter market share continues to grow, it is critically important to evaluate whether charter schools effectively impart non-cognitive skills to students and prepare young people to engage productively in civic life.

To date, there are only a handful of studies on the effect of charter schools on behavioral or civic outcomes. To our knowledge, only one study has examined the effect of charter schools on attendance and discipline, and found charter schools had positive effects for students enrolled in schools that began as charter schools (Imberman, 2011).[2] Two studies that leveraged lottery-based designs to evaluate the impact of over-subscribed charter networks in New York City found large positive effects on voting behavior (Gill et al., 2018) and propensity to be incarcerated as a young adult (Dobbie & Fryer, 2015). The geographic focus on a single, large urban district limits the ability to make inferences more broadly about the effects of charter schools on civic and criminal behavior. The broader impact of charter schools on students' behavioral and civic outcomes largely remains unknown.

The present study uses the universe of charter high school students in North Carolina, a populous and diverse state, to address these gaps. We add to the nascent body of literature that goes beyond academic impacts to examine the effects of charter high schools on students short- and long- term behavioral outcomes (chronic absenteeism, suspensions, and criminal convictions), as well as long-run civic outcomes measured by propensity to participate in federal, state, and local elections. Using statewide administrative data from North Carolina, we conduct a doubly robust analysis (Imbens & Wooldridge, 2009) which combines matching with a linear probably model to estimate the impact of charter high schools on six outcomes: chronic absenteeism in 9th grade, suspensions in 9th grade, being convicted for a felony as an adult, being

[2] The distinction being between charter schools that were traditional public schools and converted to charter status.

convicted for a misdemeanor as an adult, registering to vote, and voting in a federal, state, or local election. Our sample includes cohorts that began ninth grade between 2004-05 and 2011-12 and contains approximately 9,500 charter students and 12,000 matched comparison students.

In brief, we find positive overall effects for students who switched to a charter school in 9th grade from a TPS, relative to peers who remained in the TPS sector, on all six outcomes. These students were less likely to be chronically absent, suspended, be convicted of a crime as an adult, and more likely to register and participate in elections. These results are robust to a variety of sensitivity analyses. However, we find less consistent patterns for students who remained in the charter school sector between 8th and 9th grade relative to peers who left the charter school sector in 9th grade.

2 Literature Review

In order to build a broader understanding of how charter schools affect students long-run outcomes, it is important to use a wider array of outcomes, such as behavioral and civic outcomes. We study these behavioral outcomes, including chronic absenteeism, suspensions, and criminal convictions, to provide insight into possible benefits on key non-cognitive skills that boost long-term educational and labor market prospects. The inclusion of civic outcomes, (e.g. voter registration and voting), offers critical evidence on the extent to which charter schools contribute to broader democratic goals for public education.

Education and Behavioral Outcomes

Often due to data limitations and the time horizon necessary to evaluate long-term outcomes, much of the extant literature focuses on the effect of charter schools on academic outcomes (Epple, Romano, & Zimmer, 2015). Although important, this work provides a narrow look at the broader and long-term effect of charter schools beyond students test scores. Educational institutions can develop both cognitive skills (of which test scores are often a proxy)

and non-cognitive skills and both sets of competencies have important implications for human capital development. As noted by Heckman and colleagues, social behaviors are reflective of both cognitive and non-cognitive skill, and improvements in non-cognitive abilities can positively affect a number of behaviors (including adverse and risky behaviors) and influence educational attainment and labor market outcomes (Heckman & Rubinstein, 2001; Heckman, Stixrud, & Sergio, 2006). The use of behavioral outcomes allows us to evaluate whether charter schools positively affect students through channels other than test scores that both prior evidence and theory suggest is also critical for to improve adult outcomes.

Non-academic outcomes, such as chronic absenteeism and suspensions, have also become increasingly important to study given recent shifts in federal accountability policy. The most recent reauthorization of the Elementary and Secondary Education Act (ESEA), the Every Student Succeeds Act (ESSA), requires states to include at least one non-cognitive or non-academic measure or factor in their school accountability system. There is very limited evidence on the effect of charter schools on these short-term behavioral outcomes. To our knowledge, only one paper estimates the effect charter schools have on chronic absenteeism and suspension rates using data from a large urban school district in the Southwest. Imberman (2011) found that newly opened charter schools improved students' attendance and reduced disciplinary infractions; however, public schools converted to charter schools did not have a statistically significant impact on students' behavioral outcomes.

These short-term behavioral outcomes, particularly suspensions, may also matter given the documented positive association between suspensions and a number of adverse outcomes including interactions with the juvenile and adult criminal justice systems, links that are commonly described by researchers and advocates as the "school-to-prison" pipeline (Skiba, Arredondo, & Williams, 2014). Students chronically absent from school, or those suspended and

unable to attend school, will have weaker attachment to school, fall further behind, and be at risk of dropping out. If charter schools are better able to establish consistent and effective disciplinary practices, it is possible that may lead to lower dropout rates and promote greater educational attainment from secondary school and postsecondary institutions. There are several theories from the economics of crime literature that posit and test the empirical relationship between educational attainment and adult crime. Lochner (2004) considers the relationship between education and criminal activity through a human capital framework and suggests that increases in educational attainment raises the opportunity costs of criminal activity. Empirical evidence that found increases in educational attainment reduces crime supports this theoretical framework (Machin, Marie, & Vujić, 2011; Lochner & Moretti, 2004). A related body of work found that improvements in school quality may lead to a reduction in arrests and incarceration (Billings, Deming, & Rockoff, 2013; Deming, 2011). Researchers hypothesize that access to better schools improve educational attainment, and in turn increased educational attainment reduces the propensity to commit crime. Recent evidence on oversubscribed charter high schools suggests that charter schools have positive effects on college enrollment and persistence (Angrist et al., 2016; Davis & Heller, 2017). The combination of the theory and evidence which connects educational attainment and school quality with reductions in crime and the charter lottery studies (Angrist et al., 2016; Davis & Heller, 2017) suggests that charter school attendance may also be associated with reductions in crime.

Incapacitation effects, or dynamic incapacitation effects, may also explain why charter schools could impact students' likelihood of committing a crime as an adult. Incapacitation effects are driven by the fact that being in school prevents students from having time to engage in risky behavior including criminal behavior. Researchers suggest declines in criminal behavior during school may decrease crime in later years given strong state dependence (Lochner &

Moretti, 2004) potentially through reductions in "criminal capital accumulation," (Bell, Cota, and Machin, 2018, pgs. 23). Evidence suggests that school attendance does in fact decrease contemporaneous criminal activity (Anderson, 2014; Bell, Costa, & Machin, 2018; Jacob & Lefgren, 2003). Given that some "No Excuses" charter schools are characterized by longer school days and years (Dobbie & Fryer, 2011), charter schools may decrease students' likelihood of adult criminal activity through incapacitation effects. Prior evidence also highlights that charter schools may decrease absenteeism and suspensions (Imberman, 2011), which also results in increased time in school.

Similar to the literature on charter schools' effects on absenteeism and suspensions, the research on the effect of charter schools on crime is thin. To our knowledge, only one paper examines the effects of charter schools on criminal outcomes using a random admissions lottery to an oversubscribed charter network in New York City (Harlem Children Zone) and finds a 4.4 percentage point decline in the likelihood that male students will report having been incarcerated about six years after an offer of admissions to the sixth grade (Dobbie & Fryer, 2015). However, Dobbie and Fryer use self-reported measures of incarceration history which may not accurately capture individual's true crime history.

Education and Civic Engagement

We also evaluate whether charter schools affect broader democratic goals for education (as measured by voter registration and voting behavior). There are a number of theories within the field of political science that suggest ways in which charter schools may impact voting behavior. For one, educational attainment may increase an individual's propensity to participate in the political process and prior evidence suggests the relationship between educational attainment and voting behavior may be causal (Milligan, Moretti, & Oreopolous, 2004; Dee, 2004; Sondheimer & Green, 2010). Again, if charter schools increase students' educational attainment—as recent

work highlights the positive effect of charter schools on college enrollment and persistence suggest—they may also increase political participation.

A separate body of work has explored the ways in which school choice, with a focus on voucher programs, may influence political participation through what is described as a "policy feedback approach" (Fleming, 2014, pgs. 56). Specifically, a policy feedback approach examines ways in which engagement with government policies and programs may influence individuals' civic behavior. Focusing on voucher schools, Fleming posits two ways market-based policies may affect civic participation. On the one hand, when a private entity provides a publicly funded market-based program, individuals may attribute positive experiences from the program to provider (a private organization) and not the funder (the government). As such, the obscured role of government may result in decreased political engagement and participation. Alternatively, market-based policies provide individuals with more agency, and agency enhancement in one policy arena maybe result in more confidence or knowledge to engagement in the political process. Fleming (2014) found evidence of increased self- reported political engagement and activism among parents who opted into a voucher program.

While theories explored in the political science literature on the effects of voucher programs on civic outcomes provide a conceptual framework to consider how charter schools may affect civic outcomes, the literature of the impacts of voucher programs on civic engagement is sparse and suffers from important limitations. Several studies have examined the effects of voucher programs on self-reported measures of political tolerance, civic engagement, and voting behavior among students and found mixed evidence of the effect of voucher programs (Mills et al., 2016; Fleming, Mitchell, & McNally, 2014; Wolf, Peterson, & West, 2001). It is important to note, however, that these studies suffer from some design weaknesses, including a reliance on self-reported civic behavior and low survey response rates. To our

knowledge, only one study to date evaluates the effect of voucher programs on actual civic outcomes, including voter registration and voting behavior, rather than relying on self-reported measures of voting behavior or intentions. Carlson, Chingos, and Campbell (2016) found no effect of New York City's voucher program on students' likelihood to register to vote or vote in an election. Further, we are only aware of one study that examines the effect of charter schools on civic outcomes. In a recent report, Gill and colleagues (2018) found that receiving an offer of admissions through a random admissions lottery to an oversubscribed charter network (Democracy Prep) is associated with a six percentage point increase in the probability that a student voted in the 2016 election. While the study's lottery design has strong internal validity, it only provides information on a single charter network from NYC, and the charter network's mission and program is centered around promoting a "life of active citizenship," (Gill et al., 2018, pg. vii). Taken together, this study along with theories and studies exploring the relationship between voucher programs and civic outcomes suggests the potential for charter schools to affect civic outcomes, although the direction of the relationship remains unclear.

In summary, theories on education and non-cognitive skills, education and crime, and education and civic outcomes suggest that it is important to assess charter schools against these outcomes. Adjacent bodies of evidence and a nascent literature on the impacts of charter schools on behavioral outcomes, criminal convictions, and civic engagement suggests that charter schools may have positive effects on these outcomes. However, only a handful of studies examine whether charter schools improve non-test score outcomes and the current literature remains thin and importantly, lacks generalizability to the broader charter sector. We examine the effects of charter high schools in North Carolina on these wider range of outcomes in both the short- and long- run to address this gap. Moreover, we address important

generalizability concerns of the current literature that is largely focused within one large urban district.

3 Data

This study uses data from three main sources. First, we use longitudinal administrative data provided by the North Carolina Department of Public Instruction (NCDPI) which includes all students who attended North Carolina public schools, including charter schools, from 2004-05 to 2015-16. These data allow us to follow individual students as long as they remain enrolled in NC public schools and contains student demographics, including gender, student ethnicity, an indicator for economic disadvantage, disability, giftedness, limited English proficiency; achievement including state standardized test scores and ACT scores; and measures of student behavior and attainment including absences, graduation, GPA, and course taking information. We also have student suspension data starting in the 2009-10 school year. At the school level these data include the percent of economically disadvantaged students, measures of short-term suspensions and within school violent acts, shares of students in each race/ethnicity category, urbanicity, and total enrollment.

We merged the student-level data from NCDPI with publicly available individual-level offender records (criminal convictions for misdemeanors and felonies) from the North Carolina Department of Public Safety (NCDPS) and publicly available population-level records on voter registration and voting from the North Carolina Board of Elections (NCBOE). Publicly available data from NCDPS provide information on criminal convictions for misdemeanors and felonies in North Carolina. The NCDPS data contain information on all criminal convictions in North

Carolina since 1972 and were obtained from their website. We matched these data to NCDPI records using first name, last name, and birthdate.[3]

We scraped the publicly available voting data from the NCBOE using the first name, last name, and birthdate of each student who appeared in the NCDPI public school data and was over the age of 18. We included all name variations in the NCDPI data in our search. The voting records include all elections at the federal, state, county, and municipal elections in North Carolina through May 2016. Due to a change in the structure of the publicly available data, we only obtained a 40% random sample of voting data for the November 2016 general election. However, since this sample is random, and therefore representative, we include it in our dataset.

From these data, we created seven cohorts of students who began 9th grade for the first time during the 2005-06 to 2011-12 school years. We only include students in the analytic sample if they appear in 8th and 9th grades in NC public schools in consecutive years. In our main analysis, we also control for students 6th and 7th grade math and reading test scores. We define treatment as attending a charter school in 9th grade. Students who subsequently leave a charter school in grades 10 through 12 are still identified as "treated" in our analysis. We observe 9,499 treatment students enrolled in a charter school in 9th grade and 709,659 potential comparison students who enrolled in traditional public schools in 9th grades. Of these, 7,981 treatment students and 624,151 potential comparison students have non-missing information on all control variables and so are eligible to be in the matched sample.

[3] We acknowledge that our definition of criminal activity is narrow and misses arrests that do not lead to convictions, as well as juvenile criminal activity (both arrests and convictions). In this manner, our measure likely undercounts individuals true criminal activity. The advantage of our data, however, is that it covers the population of adult convictions in the state of North Carolina and is not self-reported.

3.1 Descriptive Statistics

Our analysis includes six dependent variables of interest: chronically absent in 9th grade (at least 15 absences within the school year), suspended in 9th grade, convicted for a misdemeanor as an adult, convicted for a felony as an adult, registered to vote, and voted in an election. Table 1 summarizes these six dependent variables of interest for the full sample as well as the matched treatment and comparison samples. For the sake of brevity, we limit our analysis to behavioral outcomes in 9th grade (absenteeism and suspensions). In additional analyses available upon request, we examine students' measures in 10th through 12th grade. The results are qualitatively similar to those presented here. In the combined sample, roughly 16 percent of students are chronically absent and 22 percent were suspended in 9th grade. We also observe 4 percent of students were convicted of a misdemeanor and 2 percent of students were convicted of a felony. Finally, 71 percent of students registered to vote and 48 percent voted in an election.

Student level controls are measured at baseline (8th grade) except for middle school mobility and passing or failing algebra 1 in middle school. Middle school mobility is an indicator that is equal to one if a student ever switched schools while observed in grades six through eight. Algebra 1 is an indicator that is equal to one if a student first takes the course in middle school, which could be before 8th grade. Control variables are divided into three groups listed below.

- Student Background Controls: race/ethnicity, male, limited English proficient, gifted, disabled, economically disadvantaged, days absent, days absent squared, middle school mobility, old for grade, interacted economically disadvantaged with disabled, and interacted economically disadvantaged with gifted
- Student Achievement Controls: failed algebra 1 in middle school, passed algebra 1 in middle school, standardized 8th grade math test scores, and standardized 8th grade reading test scores

- Lagged Local Characteristic Controls: percent economically disadvantaged; short-term suspension rate; number of violent acts per 1000 students; percent Asian, black, Hispanic, multi-racial, American Indian, and white; per-pupil expenditures; urbanicity; and enrollment

Old for grade is an indicator that is equal to one if a student is greater than 15 years old by the 1st of September of his or her 8th grade year. Lagged local characteristics are the lagged average characteristics of the five nearest traditional public high schools (within 15 miles) of the high school a student attended in 9th grade. For traditional public schools, this average includes the traditional public school itself, but charter schools are always excluded from the local average because their student composition reflects selection in addition to local characteristics. The inclusion of lagged local characteristics ensures that treatment students are matched to comparison students in high schools with similar local characteristics.

The first two columns of Table 2 display means of the control variables by treatment group, and show that students attending charter schools in 9th grade are different along several observable dimensions than traditional public school students. For example, treatment students are much more likely to have attended a charter school in 8th grade, are significantly more likely to be white, and have higher 8th grade math and reading test scores than comparison students. In the next section we describe our selection-on-observable methods used to handle non-random assignment of students to charter and traditional public schools.

4 Analysis

Students choose schools for a variety of reasons, and many of these are not observed by the researchers. In order to estimate the causal effect of schools on students' outcomes, observational studies of school choice have to account for these sources of bias. In order to control for student selection, we utilize a two-step approach and test this approach against three alternative

specifications. First, we use propensity score matching to construct a comparison group of traditional public school students that are similar in baseline characteristics to charter school students. Baseline is defined as 8th grade for time varying characteristics (e.g. achievement and economic disadvantage) and time-invariant characteristics (e.g. gender). Second, we estimate linear probabilities models controlling for covariates used in the matching model, as well as students 6th and 7th grade achievement. We do not include 6th and 7th grade achievement in the matching process so that we can assess whether there is balance along this dimension between treatment and control students. Comparison students are weighted by the number of times they are matched to treated students. In this "doubly-robust" approach only one of the two methods (PSM first step or LPM in the second step) needs to be correctly specified to estimate a causal effect of NC charter schools on students' behavioral and civic outcomes (Imbens & Wooldridge, 2009; Wooldridge, 2010).

To complete this analysis, we define baseline as 8th grade, and treatment T_i is defined as students that attend a charter school at any point in 9th grade. Let $Y_i(1)$ denote the potential outcome of student *i* had he or she attended a charter in 9th grade ($T_i = 1$) , and $Y_i(0)$ denote the potential outcome of student *i* had he or she attended a traditional public school in 9th grade ($T_i = 0$). We are interested in the average treatment effect on the treated, or the effect of attending a charter high school for those that attended a charter high school in 9th grade:

$$\Delta^{tt} = E[Y_i(1) - Y_i(0)|T_i = 1] \quad (1)$$

For an individual student, both potential outcomes cannot be observed, so we require the construction of an appropriate counterfactual. Our analysis assumes conditional independence: conditional on observable characteristics X_i, the potential outcome under no treatment $Y_i(0)$ is mean independent of treatment T_i,

$$Y_i(0) \perp T_i|X_i \quad (2)$$

Once we match students on observable baseline characteristics, whether a student receives treatment or not is unrelated to the outcome a student would realize if not treated, and comparing the difference in means between the treated and matched comparison students is an unbiased estimate of the treatment on the treated.[4] There are a variety of ways to implement matching estimators. We present a straightforward use of nearest neighbor matching with a caliper and show alternative approaches as specifications checks.

In the first step, the probability of attending a charter school in 9th grade is modeled as

$$Prob(T_i = 1) = \frac{e^{\alpha_0 + \boldsymbol{X}_i \alpha_1 + \gamma_c}}{1 + e^{\alpha_0 + \boldsymbol{X}_i \alpha_1 + \gamma_c}}, \quad (3)$$

where T_i is the treatment status of student i, X_i is a vector of control variables, and γ_c is a dummy variable for each cohort. The propensity models are estimated using logistic regression. The control vector consists of student-level control variables and lagged local characteristics. After propensity scores are estimated, treatment students are matched to at most 3 students with nearest propensity scores (nearest neighbors).[5] Matching is with replacement and restricted to be within 0.01 units of the treated propensity score. After matching, the treatment and comparison group are compared on observable characteristics, both those used in the matching step and those not used in the matching step but correlated with treatment and the outcomes of interest, to assess balance on observables. Although our matching process creates two groups of students with similar observable characteristics who differ only in treatment status, it does not remove

[4] It is not necessary to assume independence from $Y_i(1)$ because we are interested in the treatment effect on the treated (Imbens, 2004).

[5] Matching to the three nearest neighbors rather than one nearest neighbor may introduce a small amount of additional bias, but the larger comparison group increases efficiency. In practice, we find highly similar results regardless of the number of matches - one, three, or five.

unobserved sources of bias. As noted above, our analysis assumes that conditional on the matching process, treatment assignment is random between the two groups of students.

In the second step, treatment effect estimation is carried out on the matched sample using a Linear Probability Model:

$$Y_i = \beta_0 + \beta_1 T_i + \boldsymbol{X}_i \boldsymbol{\beta}_2 + \lambda_c + \epsilon_i, \quad (4)$$

where Y_i is outcome for student i, T_i is an indicator for attending a charter school in 9th grade, X_i is the same vector of covariates used in the propensity score model with the addition of quadratics of students' 6th and 7th grade math and reading test scores[6], λ_c are cohort fixed effects, and ϵ_i is an idiosyncratic error.[7] The model includes the same set of covariates used in the matching process in order to control for any remaining differences between the treatment and comparison groups and to increase precision. Standard errors are clustered at the high school level.

The regression is weighted because in the matching process some comparison students are matched to multiple treatment students and some treatment students match to less than three nearest neighbors. Matched treatment students always receive a weight of one. Comparison students are re-weighted so that the sum of comparison weights is equal to the number of matched treatment students. For example, if no comparison student was matched more than once and each treatment student was matched to exactly three comparison students, each matched comparison student would be weighted one-third, and the matched comparison weights would

[6] Few students are missing 6th and 7th grade test scores either due to entering the NC public school system in 8th grade or censoring (e.g. our first cohort will not have 6th or 7th grade test scores. We use the lead of students prior achievement to handle missing prior test scores. For example, if a student is missing the 6th grade math test but has a 7th grade math score, we use the student's 7th grade score in place of her 6th grade score. We also include binary indicators for missing 6th and 7th grade math and reading test scores. Students must have 8th grade math and reading test scores to be included in the analysis.

[7] We also estimate our results using logistic regressions and estimating the average partial effects, and the results are qualitatively similar and available upon request (Wooldridge, 2010). We prefer the linear probability models as it is easier to incorporate fixed-effects and interpret the coefficients.

sum to the total number of matched treatment students. If one of the comparison students was matched to two treatment students, that student's weight would be adjusted to two-thirds.

With the addition of the second estimation step in equation 4, the coefficient, β_2, is a two-step estimator, termed a regression adjusted propensity score matching estimator. Regression adjusted matching estimators are considered better in practice than regression or matching on its own (Imbens & Wooldridge, 2009; Abadie & Imbens, 2011; Wooldridge, 2010). The final regression adjustment reduces bias from small differences in observables leftover after the matching process; this estimator is also robust to misspecification of the regression function in the second step (Abadie & Imbens, 2011).

4.1 Charter 8th Grade Control

Propensity score matching provides strong protection against selection bias in so far as selection into or out of charter schools is uncorrelated with unobservable characteristics conditional on observable baseline characteristics. Charter high schools present a unique context because students may have already selected into a charter school before high school. Some prior studies restrict analysis to students that attended a charter school in 8th grade with the idea that unobservable characteristics predicting selection into a middle school charter also predict selection into a high school charter (Sass et al., 2016; Booker et al., 2011). They argue that this restriction limits selection bias at the cost of some external validity.

We pursue a different approach to estimate 9th grade charter effects in this paper. We contend that a sample restricted to only students observed in 8th grade in charter schools limits external validity with uncertain benefits to internal validity. If we restrict the sample to students who attended a charter in 8th grade, students who chose to stay in a charter will be compared to students who chose to exit. Our approach builds on this logic while expanding the external validity of our estimates. The prior studies that condition their analysis on attending a charter

school in 8th grade assume that selection out of a charter school is independent of any unobservable shocks. We argue that if one is comfortable with conditional independence assumptions in our analysis, then a second comparison is equally valid, comparing students who switched into a charter school in 9th grade to those that never attended a charter school in 8th or 9th grade

In practice, we specify a model that both matches and controls for 8th grade charter school status and separately identifies the effect of charter attendance for 9th grade charter students that did not attend a charter in 8th grade (*Entrant*) and those that are continuing in a charter from 8th grade (*Stayer*). Students who never attended a charter school in 8th or 9th grade (*TPS students*) serve as the counterfactual for entrants, and students who switched out of a charter school to a TPS in 9th grade (*leavers)* serve as the counterfactual for stayers. Our model separately identifies both comparisons. After matching, we fit an outcome model with indicator variables for these two types of students:

$$Y_i = \delta_0 + \delta_1 Entrant + \delta_2 Stayer + \delta_3 CH\_8th + \boldsymbol{X}_i\boldsymbol{\beta}_2 + CH\_8th \times \boldsymbol{X}_i\boldsymbol{\beta}_3 + \lambda_c + CH\_8th \times \lambda_c + \epsilon_i \quad (5)$$

Model (5) also interacts all covariates and cohort fixed effects with the indicator for being in a charter school in 8th grade. This means that δ_2 can be interpreted as if we had restricted the sample to students in a charter school in 8th grade and can be compared to charter school impacts from prior studies that utilize this restriction (Booker et al., 2011; Sass et al., 2016).

4.2 Specification and Robustness Checks

The primary threat to identification is that unobservable student characteristics may be correlated with a student's decision to enroll in a charter high school and are correlated with high school outcomes. While we cannot rule out unobserved bias in our analyses, we estimate a number of specification and robustness checks to strengthen the validity of our analysis.

Alternative Matching Estimators

In our analysis we also present results from four alternative specifications. The first two are small changes to the original analysis using either 1 or 5 nearest neighbors (within a .01 caliper) and the same second step LPM. This tests whether our main results are sensitive to the number of comparison students used in the first step. The third approach uses the logistic regression from the main two-step analysis. However, instead of using the estimated propensity scores to match treatment students to comparison students (as we do in our main model), we use the propensity scores as weights in an inverse-probability weighted (IPW) linear probability model (Abadie & Imbens, 2011; Imbens & Wooldridge, 2009; Wooldridge, 2010). In this analysis treated students receive a weight of 1 and comparison students receive a weight of $\frac{\hat{p}}{1-\hat{p}}$ where $\hat{p}$ is the estimated probability of attending a charter school in 9th grade. Instead of matching treatment students to select few comparison students, this IPW analysis uses the full sample of comparison students in the LPM. Comparison students are weighted to match the treatment students' covariate distribution.

The fourth approach uses a combination of an exact match fixed-effect and flexible control variables (Angrist, Pathak, & Walters; Dobbie & Fryer; Waddington & Berends, 2018). In this approach we create a matched cell fixed-effect for students 8th grade school, gender, race/ethnicity, and economic disadvantage. We then use this matched cell fixed-effect in our LPM above, controlling for the same covariates used in the main matching process and students' 6th and 7th grade achievement. This analysis makes two key changes. First, we do not estimate the probability of attending a charter school in 9th grade. Instead, it exactly matches students on key 8th grade baseline characteristics, including 8th grade school. Second, the method only identifies treatment effects for matched cells with variation in treatment status (i.e., students

exactly matched on race, gender, economic disadvantage, and 8th grade school but with variation in charter and TPS attendance in 9th grade).

Alternative Samples and Covariates

The next set of robustness checks either alter the analytic sample or add a new covariate to the analysis. We first restrict our comparison students who did not have a charter high school with 15 miles of the TPS they attended in 8th grade. The comparison group is then made up of two types of students: those that attended a TPS in 8th grade and did not have a nearby charter high school and those that attended a charter school 8th grade but did not have a nearby charter high school. This approach is similar to Tuttle et al. (2015) and Sass, Zimmer, Gill, and Booker (2016), and assumes that the comparison students would have attended a charter high school if they lived closer to one.

The second analysis restricts our sample to students (both treatment and comparison) who did not attend the same school in 8th and 9th grade. Many of the charter high schools in NC include both middle and high school grades. In this sensitivity analysis both types of students changed schools between 8th and 9th grade and had the potential to move to a charter school.

The final robustness check controls for students' 8th grade suspensions in the matching process and the LPM. Because we only have suspension data for the 2010-11 and 2011-12 cohorts, we do not include 8th grade suspensions as a control variable in our main analysis. However, baseline behavior is likely a strong predictor of behavior in 9th grade and is a proxy for behaviors related to crime as an adult.[8]

[8] It is important to point out the growing evidence of bias in school discipline practices (e.g. Barrett, McEachin, Mills, & Valant, 2019). For this reason measures of behavior as a control variable or a dependent variable are not measured without error, and represent both students' actual behaviors and potential biases from discrepant practices at the school and district level.

Treatment Effect Bounding.

We bound our main results using a method extended by Oster (2017) based on the work by Altonji, Elder, and Taber (2005). Her bounding method uses both coefficient stability and changes in R^2 to estimate the true effect. Coefficient stability refers to changes in treatment effect estimates when observable controls are added to the model. Estimates that are stable to the inclusion of covariates likely suffer less from omitted variable bias than estimates sensitive to the inclusion of covariates. As argued by Oster (2017), it is also important to consider the change in R^2 due to the inclusion of covariates. If we assume that the ratio of bias due to observed and unobserved factors is 1, Oster provides a formula to estimate the true treatment effect:

$$\beta^* = \tilde{\beta} - \left[\beta^o - \tilde{\beta}\right]\frac{R_{max}-\tilde{R}}{\tilde{R}-R^o}, \qquad (6)$$

where β^* is the true effect, $\tilde{\beta}$is the treatment effect with a full set of control variables, $\beta^o$is the treatment effect from a short regression with just the treatment effect variables and no other covariates, and $\tilde{R}$ and $R^o$are the R^2 from the respective models. R_{max} is the $R^2$from hypothetical model which includes all observed and unobserved factors. Oster suggests three measures of R_{max}: $2 * \tilde{R} - R^o$, $1.3 * \tilde{R}$, and 1. The latter is the most conservative estimate and assumes 100% of the variation in the outcome would be explained by all observable and unobservable factors (e.g. no measurement error). We consider our results robust to potential omitted variable bias if zero is not include in the interval between $[\beta^*, \tilde{\beta}]$ for positive estimates of $\tilde{\beta}$ and $[\tilde{\beta}, \beta^*]$ for negative estimates of $\tilde{\beta}$.

4.3 Heterogeneity by Student Characteristics

We build on our main effects by exploring heterogeneity based on student characteristics in three main analyses. First, we run separate two-step models by students' race/ethnicity, economic disadvantage, and gender. Second, we run separate models for the two-way interaction between

white/black and economically disadvantaged/non-economically disadvantaged. Third, we attempt to measure whether NC charter school effectiveness varies by students' potential peer exposure counterfactual. The intuition is to measure the school characteristics to which students who attended a charter in 9th grade would have experience had they gone to a local traditional public school instead. To do this, we use the lagged local characteristics (e.g. school characteristics based on prior cohorts of students) to create a summary measure of potential peer characteristics using a simple factor analysis. Students with negative values of the factor have lagged local characteristics that lean less poor, less violent, whiter, and less urban. Students with positive values of the factor have lagged local characteristics that lean poorer, more violent, less white, and more urban. We then group this factor into tertiles and run our main two-step model separately by tertile.

5 Results

5.1 Balance

Table 2 shows descriptive statistics for demographics, pre-treatment achievement, and lagged local characteristics for students who attended 9th grade at a charter school and those who attended 9th grade at a traditional public school. The full sample includes all charter and TPS students, regardless of their propensity to attend a charter school in 9th grade. The matched sample conditions on the nearest neighbor 3:1 matching within a caliper of .01. The final column for each group shows the standardized mean difference for each covariate, normalizing the mean difference by the pooled standard deviation. Before matching, nearly all of the demographics and all of the pre-treatment achievement variables have large standardized differences, especially prior achievement and economic disadvantage. However, after matching the two groups have very similar observable characteristics, and the mean standardized difference is close to zero.

We also present evidence of balance on variables that were not used in the matching process. One downfall of propensity score matching is that it may only create balance on variables that are included in the matching process, leaving large differences between the two groups on other dimensions. In Table 3 we present the mean and standard deviation of variables not included in the matching process including students' 6th and 7th grade math and reading achievement, an indicator for having switched schools in middle school, 8th grade suspension (cohorts 2010-11 and 2011-12 only), and parental education (cohorts 2005-6 and 2007-8 only) for charter and TPS students both unmatched and matched. As with Table 2, there are large differences between charter and TPS students in the full sample but these trend toward zero in the matched sample. Balance on these variables that were not used in the matching process provides suggestive evidence that our matching analysis created two groups that are similar in expectation except for treatment status, conditional on observable characteristics.

5.2 Main Results

Throughout the results section, we present separate estimates for entrants and stayers and all models control for 8th grade charter status, the observables used in the matching process, and students' 6th and 7th grade math and reading achievement. The coefficient for stayers compares charter sector stayers to students who attended a charter in 8th and then a traditional public school in 9th. The coefficient for entrants compares charter school entrants to students who attended a traditional public school in both 8th and 9th grade. In main results tables we also present a simple F-test comparing the entrants' coefficient to stayers', as well as the mean outcome for the respective counterfactuals: mean outcomes for TPS students to compare to entrants, and mean outcomes for leavers to compare to stayers.

We present our main results in Table 4. Across all outcomes, charter schools have statistically significant effects on students who enter a charter school in 9th grade (entrants) in the direction of

improved student behavior and civic outcomes relative to students who attended a TPS in 8th and 9th grade. For example, entrants were 1.3 percentage points less likely to be chronically absent (off a base of 10 percentage for TPS students) and 6.6 percentage points less likely to be suspended (off a base of 13.6 percentage for TPS students). They were also .5 and .9 percentage points less likely to be convicted of a felony and misdemeanor (off respective bases of 1.4 and 2.4 percent, respectively). The effects on crime are small in magnitude, but they represent large changes relative to the small probability of students being convicted of crimes as young adults. Finally, entrants are 5 percentage points more likely to vote and 2 percentage points more likely to register to vote than their TPS peers (off respective bases of 55 and 77 percent). In short, students who switched to a charter school in 9th grade from a TPS in 8th grade experience positive effects in terms of behavioral and civic outcomes.

The story is less clear, however, for students who stayed in a charter school in 9th grade relative to students who attended a charter school in 8th grade but left for a TPS in 9th grade. Stayers did experience a decrease in the propensity to be chronically absent and suspended in 9th grade relative to peers who left the charter school sector in 9th grade. They were also marginally less likely to be convicted of a misdemeanor. However, we do not find statistically significant effects on the other three outcomes for stayers. An initial positive shock to students' outcomes from NC charter schools could explain why students new to the sector have different short and long-run outcomes than students who remain in the charter school sector.

5.3 Robustness Check Results

The ability of our main analysis to estimate the causal effect of charter schools on students' outcomes rests on a number of assumptions, especially conditional independence. While a sufficient condition for matching does not exist, we present a variety of specification and robustness analyses to check the internal validity of our main analysis.

In Table 5 we replicate our main results using the two-step approach with one and five nearest neighbors, an IPW linear probability model, and a matched-cell fixed-effects approach. If our main point estimates change across these specifications, it would suggest our results are sensitive to model specification and analytic approach. However, across most of our outcomes and specifications, the direction and statistical significance of the coefficients remain the same. Two exceptions are chronic absenteeism in nearest neighbor matching with one neighbor and committing a felony with the matched cell fixed-effect approach. In both cases the coefficient is in the same direction and roughly the same magnitude of the main results, while the change in statistical significance is driven by reduced precision in these two methods

In Table 6 we run our main analysis on two different samples and a model with an additional covariate in the two-step matching technique. In the first analysis we limit the comparison set of students to those who did not have a charter high school within 15 miles of the TPS high school they attended in 9th grade. The logic of this analysis is that these students potentially would have otherwise attended a charter school if they lived closer to one. Again, the results for entrants are very similar to our main results except for chronic absenteeism and committing a felony as an adult. Students new to the charter sector in 9th grade are less likely to get suspended, be convicted of a misdemeanor, more likely vote in an election, and register to vote than TPS peers who do not have a charter school within 15 miles of the TPS high school they attended. However, entrants are no more or less likely to be chronically absent or be convicted of a felony than these peers. Also similar to the main results, stayers do not have statistically significantly different results than their leaver peers, except for suspensions in 9th grade and committing a misdemeanor as a young adult.

The second analysis in Table 6 limits both treatment and comparison students to those that changed schools between 8th and 9th grade. Given that most of the HS charter schools in NC

include middle and high school grades, students who leave a combination charter school may do so for unobservable reasons. In this analysis we are comparing students who are new to a charter school in 9th grade who also changed schools between 8th and 9th grade to students who remained in the TPS sector but also changed schools between 8th and 9th grade. Similarly, we are comparing students who remained in the charter school sector between 8th and 9th grade but changed charter schools to students who left the charter school sector between 8th and 9th grade and obviously changed schools. The results across outcomes in this analysis mirrors the main results for entrants and are slightly more beneficial for stayers than the main results. In the final analysis in Table 6 we include 8th grade suspensions as a control variable in both the matching model and the main LPM, only for the 2010-11 and 2011-12 cohorts. Students behavior in 8th grade is strongly predictive of their behavior in 9th grade, and our single best proxy for the likelihood of committing a crime as an adult. If the results are not sensitive to the inclusion of this control variable, it adds strength to the internal validity of our main conditional independence assumptions. Again, these results are similar across outcomes in magnitude and significance to our main results in Table 4.

The final robustness check uses the methods extended by Oster (2017) to bound treatment effects in an observational setting. The results in Table 7 present a number of useful statistics in separate panels for entrants and stayers. For each outcome and treatment type, we present in the first row the main effects from Table 4 as well as the R^2. We also use the coefficients and R2 from this model as our measure of $\tilde{\beta}$and $\tilde{R}^2$. Next for each outcome and treatment effect type, we estimate the true effect using three different values of R^2_{max}. We also assume the ratio of bias from observable and unobservable factors is one (Oster, 2017). If zero is not in the interval

between the estimate from Table 4 and the estimated true effect in Table 7, then it suggests that our analysis is not sensitive to potential omitted variable bias.

Across all outcomes for entrants, Table 7 shows that zero is not within the interval of the estimated main effect from Table 4 and the estimated true effect using the bounding method of Oster (2017). In fact, across all outcomes our estimated effect from Table 4 is smaller than the estimated true effect across all values of R^2_{max}. For example, in Table 4 entrants were 1.3 percentage points less likely than TPS students to be chronically absent in 9th grade. Our true effect is estimated to be between 1.7 and 9 percentage points.

However as with the main results in Table 4 the story is less clear for the stayers. Zero is not in the interval between the estimated main effect and our estimate true effect for chronic absenteeism and suspensions in 9th grade. However, for some values of R^2_{max} zero is within the interval for the other four outcomes. This pattern is consistent with our lack of statistical significance in our main effects and our other robustness checks.

In summary, our main results suggest that students who are new to charter schools in 9th grade experience positive behavioral and civic outcome changes relative to students who remain in the TPS sector. These results also hold up to a variety of sensitivity and robustness checks. Students who stay in a charter school between 8th and 9th grade also experience similar effects on 9th grade chronic absenteeism and suspensions relative to their peers who left the charter school sector, however they do not experience statistically significant effects across the four long-term outcomes. In the next subsection we examine how these main effects potentially vary across student subgroups.

5.4 Heterogeneity by Student Characteristics Results

In the next series of analyses, we push on the main results to assess whether they vary across subgroups of students. While the main effects are estimands of interest to a wide variety of

audiences, it is important to understand which groups of students are potentially driving the positive effects for entrants, and whether there are groups of stayers that experience positive or negative effects.

In Table 8 we present separate analyses by students' race/ethnicity, economic disadvantage (for both economically disadvantaged and non-economically disadvantaged students), and gender (for both male and female students). In each case, we re-run the two-step main effects analysis separately by subgroup. The results show interesting patterns for a few subgroups. First, although less than 10 percent of the student population, the share of Hispanic students in NC has risen steadily over the past decade. These students experience the largest benefits of attending a charter school in NC, for both entrants and stayers. Further with the exception of registering to vote, male students (both entrants and stayers) experience consistent benefits from attending NC charter high schools as either an entrant or stayer. Finally, economically disadvantaged students experience similar positive benefits from attending a NC charter high school as either a stayer or entrant. With the exception of suspensions in 9th grade, NC charter schools do not have a consistent effect for black students as either stayers or entrants.

In Table 9 we present unique effects of NC charter high schools for the two-way interactions between black/white and economic disadvantage. Across all four groupings and both types of treatment, students experience large effects in the short-term behavioral outcomes of chronic absenteeism and suspension in 9th grade. However, for the long-term outcomes, it appears that the original effect for economically disadvantaged students was driven by white economically disadvantaged students, especially for entrants. The coefficients for black students, regardless of economic disadvantage, and white students who stayed in charters between 8th and 9th grade, regardless of economic disadvantage, indicates that these groups of students are not receiving systematic long-run behavioral or civic benefits from attending charter high schools.

In the final analysis, we generate a summary measure of the types of peers students have the potential to be exposed to using the lagged local characteristics. These characteristics capture the average characteristics of TPS schools within 15 miles of the actual high school they attended, measured from the prior school year. Charter schools' effectiveness may vary depending on the types of other experiences to which students could be exposed. To generate this summary measure, we used the lagged local share of economically disadvantaged students; short term suspension rate; number of within school violent acts per 1000 students; percent white, black, and Hispanic; and percent urban, rural, suburban, and town. To condense this down to a single measure we used a simple factor analysis and used the first factor.[9] We used the full sample of students to estimate this factor, and broke the full sample of students into three equal size groups (or tertiles) along the distribution of the factor. With the tertiles in hand, we repeated the main two-step analysis separately by tertile. Table 10 presents the mean values for the local characteristics used in the factor analysis across each tertile pooled across the matched sample. As you move across the factor groupings (from negative to positive, or lower to higher tertiles) the potential peer group increases in poverty rate, violence in schools, share of minority students, and urbanicity.

We report the results of the two-step analysis separately by tertiles of potential peers in Table 11. A number of interesting patterns emerge. Entrants and stayers in the highest tertile group, students who would have been exposed to potentially more challenging environments if they did not attend a charter school, experience beneficial effects of attending a charter high school for the short-term behavioral outcomes and crime as an adult. This is an important outcome because students in this tertile would otherwise have been exposed to higher shares of short-term

[9] The first factor explained over 50 percent of the variation and had an eigenvalue over 3. The second factor explained less than 15 percent of the variation and had an eigenvalue less than 1.3.

suspensions and within school violence than the other two tertiles. For these students, charter schools have the potential to offset higher propensities for maladaptive behavioral outcomes in high school. However, they did not receive a benefit for civic outcomes. Further we see the main voting effect is largely driven by students in the lowest tertile of the potential peers factor.

6 Conclusion

The current evidence on the effectiveness of charter schools, and charter high schools in particular, is focused largely on examining the impact of charter schools on academic outcomes. Given the maturation and continued growth of the charter sector, it has become increasingly important to assess whether charter schools have positive effects on students through channels other than test scores including imparting important non-cognitive skills. Moreover, as the share of students educated in charter schools continues to grow, it is important to examine whether charter schools are contributing to or detracting from broader democratic goals for public education (as measured through civic engagement). While there are strong theoretical underpinnings connecting education to behavioral and civic outcomes and adjacent bodies of evidence that suggest the likelihood of charter schools influencing these outcomes, the evidence base on the effect of charter schools on these types of outcomes is remarkably thin and importantly lacks generalizability. As enrollment in charter schools has and continues to increase, it is important for students, parents, policymakers, and educators to have a detailed understanding of effectiveness of a growing sector of schools on students' behavioral and civic skills.

The results of this paper build on the charter school literature in two ways. First, we used state-wide, longitudinal data to evaluate the effect of North Carolina charter high schools on students' 9th grade behavioral outcomes and students' long-term behavioral and civic outcomes

as measured by interactions with the criminal justice system and participation in elections. While the extant literature on charter schools' impact on behavioral and civic outcomes reports positive effects, these studies have limited external validity as they rely on data from two large urban districts and suffer from other issues related to generalizability. All in all, far less is known about the impacts of charter schools more broadly on these outcomes. Using state-wide data for a diverse state, our work can speak to whether a broader range of charter schools positively affect non-academic outcomes for students.

Second, our paper incorporates a matching procedure which includes students who did and did not already enroll in a charter school in 8th grade. Our matching procedure includes 8th grade charter status as a predictor variable, and controls for 8th grade status in our LPMs. We also separately estimate the effect of switching into and staying in charter schools in 9th grade. Both margins are important for consumers, educators, and policymakers as they represent potentially different educational decisions, as well as extend our external validity to more students.

The current results paint a consistently beneficial picture for students who switch to a charter school in 9th grade from the TPS sector. These students are less likely to be chronically absent and suspended in 9th grade and are less likely to commit crimes and more likely to register and vote as an adult. In many cases, these effects are driven by students from less resourced backgrounds or who would otherwise be exposed to more difficult educational environments. However, on average we do not find statistically significant effects on long-term outcomes of NC charter high schools for students who remain in the charter sector between 8th and 9th grade compared to students. However, there are groups of students who remained in the charter sector in 9th grade who experience beneficial outcomes, including Hispanic students, economically

disadvantaged students, and students who would otherwise be exposed to more difficult educational environments.

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Table 1: Descriptive Statistics for Dependent Variables

	Combined Sample		Full Sample				Matched Sample			
	All Students		Charter Students		TPS Students		Charter Students		TPS Students	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Chronically Absent	0.158	0.365	0.087	0.282	0.173	0.378	0.081	0.272	0.097	0.295
Suspended in 9th grade^	0.212	0.409	0.054	0.225	0.221	0.415	0.054	0.226	0.122	0.327
Commit Misdemeanor	0.041	0.198	0.014	0.118	0.043	0.204	0.012	0.110	0.020	0.138
Commit Felony	0.023	0.149	0.007	0.083	0.025	0.156	0.006	0.077	0.008	0.090
Registered to Vote	0.712	0.453	0.792	0.406	0.698	0.459	0.795	0.404	0.792	0.406
Vote in Election	0.479	0.500	0.583	0.493	0.468	0.499	0.581	0.493	0.569	0.495

Notes: ^ Available for the 2010-11 and 2011-12 Cohorts. The combined sample includes all 9th grade cohorts from 2006-2012 who are observed in both 8th and 9th grade. The full sample splits the combined sample into students attending a charter in 9th grade (treated students) and students attending a traditional public school in 9th grade (comparison students). The matched sample includes treated students who are matched to up to three comparison students. Matching is based on Logit regression using up to 3 nearest neighbors within 0.01 of the treatment propensity score and includes as predictors all demographic characteristics, achievement variables, local characteristics, an indicator for charter 8th, and the interactions of charter 8th with demographic, achievement, and local characteristics.

Table 2: Descriptive Statistics for Treatment and Comparison Groups

	Full Sample					Matched Sample				
	Charter Students		TPS Students		Std.	Charter Students		TPS Students		Std.
	Mean	SD	Mean	SD	Difference	Mean	SD	Mean	SD	Difference
Student-level Characteristcs										
8th Grade Charter	0.725	0.446	0.014	0.117	1.540	0.712	0.453	0.714	0.452	-0.003
8th Grade Days Absent	6.490	6.430	8.340	8.870	-0.169	6.410	6.280	6.470	6.630	-0.007
Male	0.479	0.500	0.513	0.500	-0.048	0.473	0.499	0.470	0.499	0.004
Asian	0.021	0.142	0.022	0.146	-0.006	0.022	0.145	0.025	0.157	-0.017
White	0.706	0.456	0.557	0.497	0.220	0.710	0.454	0.691	0.462	0.030
Black	0.201	0.401	0.291	0.454	-0.148	0.194	0.395	0.204	0.403	-0.019
Hispanic	0.035	0.183	0.086	0.280	-0.153	0.037	0.188	0.043	0.202	-0.023
Other	0.058	0.234	0.066	0.247	-0.022	0.060	0.237	0.062	0.242	-0.008
8th Grade Reading Ach.	0.368	0.907	-0.002	0.995	0.275	0.377	0.908	0.374	0.957	0.002
8th Grade Math Ach.	0.255	0.950	0.000	0.999	0.185	0.267	0.950	0.271	0.993	-0.003
Economic Disadvantage	0.168	0.374	0.461	0.498	-0.470	0.167	0.373	0.178	0.382	-0.020
Gifted	0.096	0.294	0.164	0.370	-0.144	0.103	0.304	0.111	0.314	-0.017
Special Education	0.104	0.305	0.131	0.337	-0.059	0.091	0.287	0.088	0.283	0.007
8th Grade Algebra	0.778	0.956	0.408	0.797	0.297	0.817	0.965	0.838	0.967	-0.015
8th Grade ELL	0.012	0.108	0.050	0.218	-0.158	0.012	0.107	0.016	0.125	-0.026
8th Grade Old for Grade	0.135	0.342	0.207	0.405	-0.135	0.128	0.334	0.128	0.334	-0.001
Lagged Local Characteristics										
Percent Econ. Disadvantaged	0.372	0.119	0.402	0.130	-0.170	0.375	0.119	0.382	0.125	-0.039
Short Term Suspensions	0.371	0.168	0.359	0.193	0.046	0.363	0.166	0.370	0.183	-0.028
Within School Violent Acts	0.165	0.061	0.161	0.067	0.041	0.162	0.060	0.164	0.065	-0.020
Percent Asian	0.027	0.022	0.025	0.023	0.054	0.027	0.023	0.028	0.024	-0.028
Percent Black	0.334	0.170	0.315	0.188	0.075	0.325	0.168	0.334	0.168	-0.041
Percent Hispanic	0.076	0.044	0.073	0.044	0.048	0.076	0.044	0.080	0.045	-0.052
Percet Multi-Race	0.024	0.011	0.024	0.013	0.010	0.024	0.011	0.025	0.011	-0.024
Percent American Indian	0.005	0.010	0.013	0.055	-0.140	0.005	0.010	0.005	0.021	-0.007
Percent White	0.534	0.173	0.549	0.203	-0.059	0.542	0.172	0.527	0.180	0.058
Per Pupil Expenditures	8180	1028	8274	1262	-0.058	8181	1040	8244	997	-0.044
Percent Urban	0.249	0.348	0.263	0.356	-0.029	0.242	0.344	0.255	0.361	-0.026
Percent Rural	0.282	0.347	0.329	0.348	-0.094	0.266	0.344	0.254	0.338	0.024
Percent Suburban	0.099	0.199	0.120	0.208	-0.074	0.095	0.199	0.090	0.186	0.018
Percent Town	0.111	0.239	0.126	0.227	-0.045	0.114	0.239	0.114	0.226	-0.002
Enrollment	1356	520	1241	454	0.167	1358	518	1335	497	0.032
Observations	624151		7981			11910		7981		

Notes: The sample includes pooled 9th grade cohorts from 2006-2012 and treatment is defined as any student attending a charter school anytime during 9th grade. We restrict the full sample to students observed in both 8th and 9th grade. "Std. Difference" is the standardized mean difference between the comparison and treatment groups, using the pooled standard deviation. Matching is described in more detail in the paper. We use a logistic regression to estimate the propensity of attending a charter school in 9th grade, and match 9th grade charter school students to up to 3 nearest comparison students within a caliper of .01. The matching model includes as predictors all demographic characteristics, achievement variables, local characteristics, an indicator for charter 8th, and the interactions of charter 8th with demographic, achievement, and local characteristics.

Table 3: Balance on Baseline Characteristics not Used in Matching

	Full Sample					Full Sample				
	Charter Students		TPS Students		Std.	Charter Students		TPS Students		Std.
	Mean	SD	Mean	SD	Difference	Mean	SD	Mean	SD	Difference
7th Grade Reading Ach.	0.348	0.903	0.011	0.993	0.251	0.365	0.898	0.361	0.939	0.003
7th Grade Math Ach.	0.317	0.942	0.016	0.998	0.219	0.338	0.936	0.308	0.970	0.022
6th Grade Reading Ach.	0.352	0.904	0.011	0.993	0.254	0.365	0.898	0.346	0.938	0.015
6th Grade Math Ach.	0.309	0.939	0.016	0.997	0.213	0.322	0.929	0.293	0.964	0.022
8th Grade Mobile Student	0.351	0.477	0.205	0.404	0.234	0.323	0.467	0.317	0.465	0.008
8th Grade Suspended^	0.084	0.278	0.239	0.427	-0.304	0.081	0.272	0.106	0.307	-0.061
Parent Ed No HS#	0.030	0.171	0.075	0.264	-0.143	0.032	0.176	0.031	0.172	0.006
Parent Ed HS	0.268	0.443	0.388	0.487	-0.184	0.247	0.431	0.251	0.434	-0.008
Parent Ed HS+	0.077	0.266	0.089	0.284	-0.031	0.077	0.267	0.079	0.270	-0.004
Parent Ed Trade School	0.023	0.149	0.021	0.144	0.008	0.023	0.151	0.026	0.160	-0.014
Parent Ed Jr. College	0.145	0.352	0.120	0.325	0.051	0.144	0.352	0.125	0.331	0.039
Parent Ed 4yr	0.364	0.481	0.249	0.432	0.177	0.381	0.486	0.375	0.484	0.009
Parent Ed Graduate School	0.095	0.293	0.057	0.232	0.100	0.095	0.293	0.112	0.316	-0.041

Notes: ^=2010-11 and 2011-12 Cohorts only, #=Parent Education data only available for 2005-6 and 2006-7 cohorts, #=Parent Education data only available for 2005-6 and 2006-7 cohorts. The sample includes pooled 9th grade cohorts from 2006-2012 and treatment is defined as any student attending a charter school anytime during 9th grade. We restrict the full sample to students observed in both 8^{th} and 0^{th} grade that did not repeat a grade. "Std. Difference" is the standardized mean difference between the comparison and treatment groups, using the pooled standard deviation. Matching is described in more detail in the paper. We use a logistic regression to estimate the propensity of attending a charter school in 9^{th} grade, and match 9^{th} grade charter school students to up to 3 nearest comparison students within a caliper of .01. The matching model includes as predictors all demographic characteristics, achievement variables, local characteristics, an indicator for charter 8th, and the interactions of charter 8th with demographic, achievement, and local characteristics.

Table 4: Main Short and Long-term Behavioral Outcomes

	Chronic Absenteeism	Suspended in 9th grade	Commit Felony	Commit Misdemeanor	Voted in Election	Registered to Vote
Entrant	-0.013*	-0.066***	-0.005*	-0.009**	0.050***	0.021*
	(0.007)	(0.012)	(0.002)	(0.003)	(0.012)	(0.010)
Stayer	-0.015*	-0.062***	-0.001	-0.005+	-0.001	-0.007
	(0.007)	(0.013)	(0.001)	(0.003)	(0.011)	(0.010)
R-squared	0.172	0.122	0.034	0.043	0.066	0.050
Stayer-Entrant	-0.002	0.004	0.004	0.004	-0.051***	-0.028*
SE(S-E)	(0.009)	(0.015)	0.002	0.004	(0.015)	(0.013)
# of Treated Student	7862	4949	7859	7859	7862	7862
# of Control Student	11910	6625	11905	11905	11910	11910
mean (Y) TPS	0.101	0.136	0.014	0.024	0.548	0.772
mean (Y) Leaver	0.095	0.117	0.006	0.018	0.577	0.800

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Notes: Students observed in a charter school in 9th grade can come from a traditional public school in 8th grade (Charter 9th Entrants) or a charter school in 8th grade (Charter 9th Stayers). Including the control for being in a charter school in 8th grade implies that Charter 9th Entrants are being compared to 9th grade traditional public school students that were not in a charter school in 8th grade. Conversely, Charter 9th Stayers are being compared to 9th grade traditional public school students that were in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and LPM regressions, but the output is suppressed. Standard errors are clustered at the high school level.

Table 5: Model Specification Check on the Main Results

	Chronic Absenteeism	Suspended in 9th grade	Commit Felony	Commit Misdemeanor	Voted in Election	Registered to Vote
Nearest Neighbor (N=1)						
Entrant	-0.013	-0.060***	-0.005+	-0.010*	0.052***	0.032**
	(0.009)	(0.013)	(0.003)	(0.004)	(0.015)	(0.012)
Stayer	-0.022*	-0.067***	-0.001	-0.007+	-0.01	-0.020+
	(0.009)	(0.014)	(0.002)	(0.003)	(0.013)	(0.012)
Nearest Neighbor (N=5)						
Entrant	-0.017**	-0.066***	-0.004*	-0.009**	0.047***	0.026**
	(0.006)	(0.011)	(0.002)	(0.003)	(0.011)	(0.008)
Stayer	-0.017*	-0.064***	-0.001	-0.007*	-0.005	-0.009
	(0.007)	(0.013)	(0.001)	(0.003)	(0.010)	(0.010)
Inverse Probability Weighted LPM						
Entrant	-0.017**	-0.066***	-0.004**	-0.009***	0.050***	0.028***
	(0.006)	(0.010)	(0.001)	(0.002)	(0.011)	(0.007)
Stayer	-0.014*	-0.067***	-0.001	-0.005*	-0.001	-0.006
	(0.007)	(0.013)	(0.001)	(0.003)	(0.010)	(0.009)
Matched Cell Fixed-Effects LPM						
Entrant	-0.022**	-0.070***	-0.003	-0.007*	0.052***	0.028**
	(0.008)	(0.018)	(0.002)	(0.003)	(0.015)	(0.009)
Stayer	-0.014+	-0.083***	-0.004+	-0.010***	0.003	0.001
	(0.008)	(0.015)	(0.002)	(0.003)	(0.009)	(0.009)

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Notes: Students observed in a charter school in 9th grade can come from a traditional public school in 8th grade (Charter 9th Entrants) or a charter school in 8th grade (Charter 9th Stayers). Including the control for being in a charter school in 8th grade implies that Charter 9th Entrants are being compared to 9th grade traditional public school students that were not in a charter school in 8th grade. Conversely, Charter 9th Stayers are being compared to 9th grade traditional public school students that were in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and LPM regressions, but the output is suppressed. The same covariates in the two-step approach are used to generate the IPW weights. In these regressions, students in a charter school are given a weight of 1, and students in traditional public schools are given a weight of p/(1-p). The matched cell fixed-effect groups students by 8th grade school, gender, race/ethnicity, and economic disadvantage. Standard errors are clustered at the high school level.

Table 6: Main Result Robustness Checks

Not within 15 Miles						
	Chronic Absenteeism	Suspended in 9th grade	Commit Felony	Commit Misdemeanor	Voted in Election	Registered to Vote
Entrant	0.003	-0.095***	-0.004	-0.014**	0.041*	0.033**
	(0.007)	(0.015)	(0.003)	(0.004)	(0.018)	(0.011)
Stayer	-0.003	-0.074***	-0.002	-0.006*	0.005	0.018
	(0.011)	(0.018)	(0.002)	(0.003)	(0.018)	(0.019)
Change Schools						
	Chronic Absenteeism	Suspended in 9th grade	Commit Felony	Commit Misdemeanor	Voted in Election	Registered to Vote
Entrant	-0.016*	-0.073***	-0.004*	-0.008*	0.044***	0.024**
	(0.006)	(0.012)	(0.002)	(0.003)	(0.012)	(0.008)
Stayer	-0.007	-0.053*	-0.005	-0.009+	-0.054*	-0.034*
	(0.016)	(0.021)	(0.004)	(0.004)	(0.025)	(0.017)
Control for 8th Grade Suspensions						
	Chronic Absenteeism	Suspended in 9th grade	Commit Felony	Commit Misdemeanor	Voted in Election	Registered to Vote
Entrant	-0.024**	-0.068***	-0.004+	-0.009**	0.076***	0.02
	(0.009)	(0.015)	(0.002)	(0.003)	(0.019)	(0.015)
Stayer	-0.026**	-0.046***	-0.001	-0.009**	0.003	-0.016
	(0.010)	(0.013)	(0.001)	(0.003)	(0.018)	(0.015)

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Notes: Students observed in a charter school in 9th grade can come from a traditional public school in 8th grade (Charter 9th Entrants) or a charter school in 8th grade (Charter 9th Stayers). Including the control for being in a charter school in 8th grade implies that Charter 9th Entrants are being compared to 9th grade traditional public school students that were not in a charter school in 8th grade. Conversely, Charter 9th Stayers are being compared to 9th grade traditional public school students that were in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and LPM regressions, but the output is suppressed. The first panel restricts TPS students to those that do not live within 15 miles of a charter high school based on the location of their 8th grade school. The second panel restricts the analysis to both charter and TPS students that switched schools between 8th and 9th grade. The third panel includes 8th grade suspension data in the two-step matching procedure (suspension data is only available for the 2010-11 and 2011-12 cohorts). Standard errors are clustered at the high school level.

Table 7: Main Treatment Effect Bounding Using Oster (2017)

Entrant Effect												
	Chronic Absenteeism	R2 or R2_max	Suspended	R2 or R2_max	Ever Misdemeanor	R2 or R2_max	Commit Misdemeanor	R2 or R2_max	Voted in Election	R2 or R2_max	Registered to Vote	R2 or R2_max
Main effect	-0.013	0.172	-0.066	0.122	-0.009	0.043	-0.005	0.034	0.021	0.050	0.050	0.066
2*R2_tilde-R_not	-0.027	0.344	-0.112	0.230	-0.017	0.085	-0.012	0.067	0.043	0.100	0.077	0.132
1.3*R2_tilde	-0.017	0.224	-0.081	0.159	-0.011	0.056	-0.007	0.044	0.027	0.065	0.058	0.086
R2_max=1	-0.090	1	-0.590	1	-0.306	1	-0.281	1	0.690	1	0.608	1
Stayer Effect												
	Chronic Absenteeism	R2 or R2_max	Suspended	R2 or R2_max	Ever Misdemeanor	R2 or R2_max	Commit Misdemeanor	R2 or R2_max	Voted in Election	R2 or R2_max	Registered to Vote	R2 or R2_max
Main effect	-0.015	0.172	-0.062	0.122	-0.005	0.043	-0.001	0.034	-0.007	0.050	-0.001	0.066
2xR2_tilde-R_not	-0.007	0.344	-0.058	0.230	0.001	0.085	0.006	0.067	-0.032	0.100	-0.005	0.132
1.3xR2_tilde	-0.013	0.224	-0.061	0.159	-0.004	0.056	0.001	0.044	-0.012	0.065	-0.002	0.086
R2_max=1	-0.031	1	-0.067	1	-0.014	1	-0.010	1	0.026	1	0.004	1

Table 8: Short- and Long-Term Behavioral Outcomes by Student Demographics

		Chronic Absenteeism	Suspended in 9th grade	Felony Conviction	Misdemeanor Conviction	Voted in Election	Registered to Vote
White Students	Entrants	-0.010	-0.062***	-0.007***	-0.009**	0.051**	0.025*
	Stayers	-0.015	-0.045**	-0.001	-0.003	-0.019	-0.017+
Black Students	Entrants	0.012	-0.095***	0.004	-0.008	0.012	0.020
	Stayers	-0.038*	-0.183***	-0.006	-0.026**	0.028	0.023
Hispanic Students	Entrants	-0.129***	-0.065**	0.000	-0.017+	0.128**	0.138**
	Stayers	-0.072*	-0.175***	-0.011	-0.053**	0.024	0.059
Other Students	Entrants	-0.001	-0.048*	-0.011*	-0.009+	0.103***	0.096**
	Stayers	-0.077**	-0.033	0.002	-0.01	0.033	-0.015
Economically Disadvantaged	Entrants	-0.043**	-0.111***	-0.005	-0.021**	0.042*	0.013
	Stayers	-0.041+	-0.147***	-0.003	-0.031**	0.048+	0.043
Non-Economically Disadvantaged	Entrants	-0.005	-0.046***	-0.006***	-0.004	0.038*	0.024*
	Stayers	-0.004	-0.046***	-0.001	-0.004	-0.006	-0.012
Male Students	Entrants	-0.032***	-0.090***	-0.007+	-0.012*	0.058**	0.013
	Stayers	-0.028***	-0.065***	-0.003	-0.008+	-0.024	-0.019
Female Students	Entrants	-0.008	-0.051***	-0.002	-0.007*	0.050**	0.045***
	Stayers	0.000	-0.061***	0.001	-0.004	-0.017	-0.015

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Notes: Each student subgroup represents a unique analysis (e.g. we re-run the matching and LPM models separately by subgroup). Students observed in a charter school in 9th grade can come from a traditional public school in 8th grade (Charter 9th Entrants) or a charter school in 8th grade (Charter 9th Stayers). Including the control for being in a charter school in 8th grade in the third column for each outcome implies that Charter 9th Entrants are being compared to 9th grade traditional public school students that were not in a charter school in 8th grade. Conversely, Charter 9th Stayers are being compared to 9th grade traditional public school students that were in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and LPM regressions, but the output is suppressed. Standard errors are clustered at the high school level.

Table 9: Short- and Long-Term Behavioral Outcomes for Race by Economic Disadvantage

		Chronic Absenteeism	Suspended in 9th grade	Commit Felony	Commit Misdemeanor	Voted in Election	Registered to Vote
Black & Economically Disadvantaged	Entrants	-0.053	-0.094	0.015	-0.036	-0.007	0.017
	Stayers	-0.094**	-0.252***	-0.032	-0.100***	-0.012	-0.005
Black & Non-Economically Disadvantaged	Entrants	-0.038	-0.169***	-0.037*	-0.019	0.074	0.082*
	Stayers	-0.047*	-0.105*	-0.023	-0.005	0.047	-0.029
White & Economically Disadvantaged	Entrants	-0.104*	-0.179**	-0.007	-0.049*	0.138*	-0.002
	Stayers	0.017	-0.02	0.000	-0.015	0.051	0.073
White & Non-Economically Disadvantaged	Entrants	-0.003	-0.047**	-0.005	0.000	0.076***	0.030+
	Stayers	-0.017+	-0.050*	0.000	0.001	-0.001	-0.023

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Notes: Each student subgroup represents a unique analysis (e.g. we re-run the matching and LPM models separately by subgroup). Students observed in a charter school in 9th grade can come from a traditional public school in 8th grade (Charter 9th Entrants) or a charter school in 8th grade (Charter 9th Stayers). Including the control for being in a charter school in 8th grade in the third column for each outcome implies that Charter 9th Entrants are being compared to 9th grade traditional public school students that were not in a charter school in 8th grade. Conversely, Charter 9th Stayers are being compared to 9th grade traditional public school students that were in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and LPM regressions, but the output is suppressed. Standard errors are clustered at the high school level.

Table 10: Factor Analysis Grouping of Students by Lagged Local Characteristics

	Lowest Tertile		Middle Tertile		Highest Tertile	
	Mean	N	Mean	N	Mean	N
Percent Economically Disadvantaged	0.323	5805	0.365	7109	0.441	6858
Short Term Suspensions	0.256	5805	0.350	7109	0.461	6858
Within School Violent Acts	0.143	5805	0.162	7109	0.186	6858
Percent Black	0.134	5805	0.317	7109	0.508	6858
Percent White	0.746	5805	0.536	7109	0.354	6858
Percent Hispanic	0.065	5805	0.087	7109	0.070	6858
Percent Urban	0.054	5805	0.297	7109	0.501	6858
Percent Rural	0.333	5805	0.237	7109	0.230	6858
Percent Suburban	0.092	5805	0.141	7109	0.050	6858
Percent Town	0.171	5805	0.107	7109	0.058	6858

Table 11: Short- and Long-Term Behavioral Outcomes by Students' Local Educational Options

		Chronic Absenteeism	Suspended in 9th grade	Commit Felony	Commit Misdemeanor	Voted in Election	Registered to Vote
Lowest Tertile	Entrants	-0.005	-0.091***	-0.016***	-0.005+	0.105***	0.054***
	Stayers	0.008	-0.057***	-0.003	-0.002	0.058**	0.025+
Middle Tertile	Entrants	-0.017	-0.043*	0.000	0.001	0.048*	0.022
	Stayers	-0.009	-0.031*	-0.003	0.000	-0.038*	-0.022
Highest Tertile	Entrants	-0.014	-0.078***	-0.008	-0.013**	0.002	-0.002
	Stayers	-0.023	-0.098***	-0.013**	-0.008*	-0.016	0.000

+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Notes: Each student subgroup represents a unique analysis (e.g. we re-run the matching and LPM models separately by subgroup). Students observed in a charter school in 9th grade can come from a traditional public school in 8th grade (Charter 9th Entrants) or a charter school in 8th grade (Charter 9th Stayers). Including the control for being in a charter school in 8th grade in the third column for each outcome implies that Charter 9th Entrants are being compared to 9th grade traditional public school students that were not in a charter school in 8th grade. Conversely, Charter 9th Stayers are being compared to 9th grade traditional public school students that were in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and LPM regressions, but the output is suppressed. The main effects are reported separately by tertile of the potential peer exposure factor. The lowest tertile represents schools with less school violence, lower poverty rates, lower shares of minority students, and less urban environments. Standard errors are clustered at the high school level.