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

Andrew McEachin, Douglas Lee Lauen, Sarah Crittenden Fuller, Rachel M. Perera[1]

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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 grades and often focus on test scores or other proximal outcomes. Much less is known about the long-term effects of attending a charter school in 9th grade. It is important to fill this information void for a few reasons. First, schools in general affect more than just students' test scores. Second, secondary schools (including grades 9 to 12) 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 inverse probability weighted approach to evaluate the impacts of secondary charter 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.

[1] McEachin and Perera at the RAND Corporation; and Fuller and Lauen at the Education Policy Initiative at Carolina and the Department of Public Policy at the University of North Carolina - Chapel Hill. This project was generously funded by the Walton Family Foundation. We thank staff at the North Carolina Department of Public Instruction, University of North Carolina System Office, North Carolina Community College and the Education Policy Initiative at Carolina, for technical assistance and providing us with the data to make this research possible. Elc Estrera, Joshua Horvath, and Anna Rybinska provided excellent research assistance. We also thank the participants of the 2017 annual Association for Policy Analysis and Management meeting and 2018 Association for Education Finance and Policy meeting. Finally we thank Scott Imberman and two anonymous reviewers for their thoughtful feedback. All errors are our own.

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 two studies have examined the effect of charter schools on attendance and discipline. One found charter schools had positive effects for students enrolled in schools that began as charter schools[2] (Imberman, 2011), and the other found similar positive effects on attendance and discipline, and that this effect was largely concentrated in charter schools open for at least five years (Spees, 2019). 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 secondary students in North Carolina, a populous and diverse state, to address these gaps.[3] We add to the nascent body of literature that goes beyond academic impacts to examine the effects of secondary charter schools on students short- and long-term behavioral outcomes (chronic absenteeism, suspensions, and criminal

[2] The distinction being between charter schools that were traditional public schools and converted to charter status.
[3] Although we focus on traditional high school grades in this paper (e.g. 9th grade), we use the term secondary students instead of high school students. North Carolina's charter schools use a wide range of grade-spans, some focusing on traditional high school grades (e.g. 9 to 12) while others combine elementary and middle school with high school (e.g. k to 12 or 6 to 12).

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 inverse probability weights (IPW) with a linear probability model to estimate the impact of secondary charter schools on seven outcomes: chronic absenteeism in 9th grade, suspensions in 9th grade, being convicted of a crime as an adult, being convicted for a misdemeanor as an adult, being convicted for a felony 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 measured 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

We study students' behavioral outcomes, including chronic absenteeism, suspensions, and criminal convictions, to provide insight into possible benefits on key non-cognitive skills that boost students' 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 narrower 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). Furthermore, a number of recent studies document that not only do teachers and schools have measurable effects on students' non-tested outcomes, but their effects on these outcomes are more predictive of students' long-run success than their effect on test scores (Jackson, 2018; Jackson et al., 2020; Kraft, 2019; McEachin, Welsh, & Brewer, 2016). 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 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 two papers estimate the effect charter schools have on chronic absenteeism and suspension rates.

Imberman (2011) uses data from a large urban school district in the Southwest and 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. Spees (2019) uses North Carolina data for grades 4 to 8 and the 2006 to 2009 school years and similarly found that charter schools improved students' attendance and reduced disciplinary 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, 2013), 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; Spees, 2019), 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 (2017) 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 secondary charter 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.[4]

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

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

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.

We also merge publicly available data from the American Community Survey 5-Year Estimates to measure local economic characteristics of the counties in which charter and traditional public schools reside. In particular we incorporate unemployment rate, poverty rate, a crime index, and median income, all measured at the county level and merge this into our analytic data set.

3.1 Descriptive Statistics

Our analysis includes seven dependent variables of interest: chronically absent in 9th grade (at least 15 absences within the school year), suspended in 9th grade, convicted of any crime, convicted for a misdemeanor as an adult, convicted for a felony as an adult, registered to vote, and voted in an election.[5] Table 1 summarizes these dependent variables of interest for the full sample as well as the treatment and comparison samples weighted by their inverse probability of attending a charter school in 9th grade. To streamline our narrative, 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 5

[5] We include "any crime" as a dependent variable to provide an overall glimpse of the effect of charters on adult criminal behavior given the relatively rare occurrence of misdemeanors and felonies in our data.

percent of students were convicted of a crime, 4 percent of students were convicted of a misdemeanor and 2 percent of students were convicted of a felony (these are not mutually exclusive categories). Finally, 72 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, failing algebra 1 in middle school, and students' ELA and math achievement. 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. We also include students' achievement in grades 6 through 8 for both ELA and math. 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, 6th through 8th grade math and reading test scores standardized by grade/subject and year.
- Lagged Local Characteristic Controls: county unemployment rate, county poverty rate, county crime index, county median income, and urbanicity

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 linked to the five nearest traditional public high schools (within 15 miles) of the high school a student attended in 9th grade (e.g. this would include multiple counties if the set of nearest schools crossed county

lines). For traditional public schools, this average includes the traditional public school itself, but charter schools are always excluded from the local average. We use county-level economic indicators from the American Community Survey to ensure that treatment students are compared to TPS students in secondary 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 6th through 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. A handful of prior studies have used lottery-based instrumental variable methods to account for non-random selection into charter schools (e.g. Abdulkadiroğlu, et al., 2011; Angrist, et al., 2009; Gleason, Clark, Tuttle, & Dwoyer, 2010; Hoxby, & Murarka, 2009). In absence of lottery data, other researchers rely on a selection-on-observables approach (e.g. value-added models or matching/weighting techniques) to model selection into treatment (Bifulco, & Ladd, 2006; Imberman, 2011; McEachin, Welsh, & Brewer, 2016; Spees, & Lauen, 2019; Sass, et al., 2016; Zimmer & Buddin, 2006; Zimmer et al., 2006). Our data also lacks lottery information, so we too use a selection-on-observables approach that combines Linear Probability Models (LPM) with inverse probability weights (Abadie & Imbens, 2011; 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 secondary charter school for those that attended a secondary charter 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 the potential outcome under no treatment $Y_i(0)$ is mean independent of treatment T_i,

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

Our conditional independence assumption enters our analysis in two ways. First, we use X_i to estimate the propensity of attending a charter school and weight the observable demographics of non-charter school students toward those of charter school students. If selection into treatment is explained by X_i, then re-weighted non-charter school students represent a valid counter-factual for how charter school students would have performed in absence of treatment. Second, we estimate linear probability models controlling for the same vector of covariates X_i. Similar to the re-weighting, our LPMs assume that conditional on X_i, selection into treatment (e.g. 9th grade charter school attendance) is random.

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

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

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, we use the propensity scores as weights in an inverse-probability weighted (IPW) linear probability model to estimate the Average Treatment Effect on the Treated (ATT) (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 from model (3). 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 toward the treatment students' covariate distribution.[6]

In the second step, treatment effect estimation is carried out using a Linear Probability Model with the IPW:

$$Y_i = \beta_0 + \beta_1 T_i + \boldsymbol{X_i}\boldsymbol{\beta}_2 + \lambda_c + \epsilon_i, \qquad (4)$$

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[7], λ_c are cohort fixed effects,

[6] In Appendix A, we show the results of our main model without using 8th grade math or reading achievement in the estimation of $\hat{p}$ or as a control in our LPM. In Table A2 we show that our two groups of students are balanced on 8th grade achievement even without using it in the estimation of $\hat{p}$, and our results in A3 are similar to Table 4 when we do not include 8th grade achievement in the IPW or as a control in the LPM.

[7] 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.

and ϵ_i is an idiosyncratic error.[8] The model includes the same set of covariates used to generate the IPW in order to control for any remaining differences between the treatment and comparison groups and to increase precision. Standard errors are clustered at the secondary school level.

With the addition of the IPW to equation 4, the coefficient, β_2, is a "doubly-robust" two-step estimator, termed a regression adjusted IPW estimator. Regression adjusted weighting/matching estimators are considered better in practice than regression or weighting/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 weighting process; this estimator is also robust to misspecification of the regression function in the second step (Abadie & Imbens, 2011). However, although our two-step estimator creates two groups of students with similar observable characteristics who differ only in treatment status, it does not remove unobserved sources of bias (e.g. student motivation). As noted above, our analysis assumes that conditional on either the IPW or LPM, treatment assignment is random between the two groups of students.

4.1 Charter 8th Grade Control

Secondary charter schools present a unique context because students may have already selected into a charter school before 9th grade. Prior studies of secondary charter schools 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 secondary 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.

[8] 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.

We pursue a different approach to estimate 9th grade charter effects in this paper. We contend that potential selection issues complicate both choosing to leave a charter school between 8th and 9th grade, and choosing to enter one between 8th and 9th grade. Conditioning on students who attended a charter school in 8th grade may account for many factors associated with choosing to attend a charter school, but it does not account for factors related to leaving a charter school. This problem is particularly exacerbated when the sample contains many schools that are 6-12 or K-12 (as most charter "high schools" are in NC) because there is no application process at 9th grade; rather the default option is to continue on in the same school. That means the stayer analysis is comparing kids who defaulted to staying in charter school to students who made a decision to exit for unknown reasons. These could be students for whom the charter school wasn't a great fit, who were particularly attracted to a program at the TPS, who were "pushed out" of the charter school for behavior or performance reasons, or who moved to another location.

We opt for a strategy that uses both sets of students who attend charter schools in 9th grade. 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.[9] Similar to the approach conditioning on 8th grade charter status, we have to assume that characteristics of students and local communities account for the reasons why families and students choose to attend a charter school in 9th grade, but not in 8th grade. We further test this assumption through a variety of robustness and sensitivity analyses.

[9] In practice this is similar to the logic of evaluating the effect of attending a TPS high school in 9th grade for those who attended in a TPS in 8th grade, compared to those who left the TPS sector in 9th grade to attend a charter school.

In practice, we specify a model that both controls for 8th grade charter school status (in both the IPW model and LPM outcome model steps) 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. We fit an outcome model with indicator variables for these two types of students:

$$Y_i = \delta_0 + \delta_1 Entrant + \delta_2 Stayer + \delta_3 CH_{8^{th}} + \boldsymbol{X_i}\beta_2 + CH_{8^{th}} x \boldsymbol{X_i}\beta_3 + \lambda_c + CH_{8^{th}} x \lambda_c + \epsilon_i. \quad (5)$$

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 secondary charter school and are correlated with our outcomes of interest. 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 three alternative specifications. The first is an LPM without the IPW. In the second approach we use a combination of a matched cell fixed-effect and flexible control variables (Angrist, Pathak, & Walters, 2013; Dobbie & Fryer, 2015; 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 our main IPW method. This procedure does 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. In addition, 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).

The third and final approach uses a traditional propensity score matching method. Instead of generating our IPW from equation 3, we match students to their nearest three neighbors with replacement within a .01 caliper of the estimated propensity score.[10] After the students are matched, we estimate the same LPM in Equation weighting the comparison students by the number of times they are matched a charter school student. .

Alternative Samples and Covariates

The next set of robustness checks either alter the analytic sample or adds a new covariate to the analysis. We first restrict our comparison students who did not have a secondary charter school serving high school grades within 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 secondary charter school and those that attended a charter school 8th grade but did not have a nearby secondary charter 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 secondary charter school in 9th grade if they lived closer to one.

[10] We also estimate models that use the nearest 1 and 5 neighbors and get similar results. The results are available upon request.

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 secondary charter 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 generating the IPW and in Model (5). 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.[11]

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:

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

[11] It is important to point out the growing evidence of bias in school discipline practices (e.g. Barrett, McEachin, Mills, & Valant, Forthcoming). 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.

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, 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. In a review of published experiments in economics journals, Oster (2017) found that 90% of the experiments were robust to using $R_{max} = 1.3\tilde{R}$, which we use as our estimated R_{max}. Oster (2017) also provides an alternative method to characterize coefficient stability and bound treatment effects.

In Equation (6), we assume that the ratio the ratio of bias due to unobserved and observed factors is 1. Instead, assuming $R_{max} = 1.3\tilde{R}$, we report (δ) the ratio of unobserved to observed bias needed to change our estimated treatment to be statistically equivalent to zero. A $\delta \geq 1$ suggests that the bias needed from unobservable factors would have to be at least as large as the bias from observable factors to cause $\tilde{\beta} = 0$. 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 $\tilde{\beta}$, as well as $\delta \geq 1$.

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. local community

characteristics based on prior school years) to create a summary measure of potential local characteristics using a simple factor analysis. Students with negative values of the factor have lagged local characteristics that lean less poor, less violent, more students of color, and more urban. Students with positive values of the factor have lagged local characteristics that lean poorer, more violent, whiter, and more rural. 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 weighted sample weights the comparison student covariate distribution toward the charter students, using a weight of $\frac{\hat{p}}{(1-\hat{p})}$where $\hat{p}$ is estimated from Equation (2). The final column for each group shows the standardized mean difference for each covariate, normalizing the mean difference by the pooled standard deviation. Before weighting, 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 weighting 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 estimation of $\hat{p}$. One downfall of weighting is that it may only create balance on variables that are included in the estimation of $\hat{p}$, 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 weighting process,

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 for the unweighted and weighted samples. As with Table 2, there are large differences between charter and TPS students in the full sample but these trend toward zero in the weighted sample. Balance on these variables that were not used in the estimation of $\hat{p}$ provides suggestive evidence that our use of the IPW 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 and the observables used in the estimation of $\hat{p}$. 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 2 percentage points less likely to be chronically absent (off a base of 10 percentage for TPS students) and 7 percentage points less likely to be suspended (off a base of 13.7 percentage for TPS students). It is possible that differential reporting practices by charter schools drive the attendance and suspension effects for both entrants and stayers. All schools in North Carolina are supposed to follow uniform guidance on reporting attendance and

discipline events. However, given that we cannot separate whether these effects are driven by differential reporting practices by charter schools and traditional public schools from organizational and instructional practices that promote better behavior, the results should be taken with caution.

Entrants were also .9 percentage points less likely to commit any crime, and .7 and .4 percentage points less likely to be convicted for a misdemeanor and felony, respectively (off bases of .03, .022, and .013). 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 2.8 percentage points more likely to register to vote and 5.4 percentage points more likely to vote than their TPS peers (off respective bases of 77 and 55 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 any crime. However, we do not find statistically significant effects on misdemeanors, felonies, or voting 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 a simple LPM without the IPW, a matched-cell fixed-effects approach, and a two-step approach that pairs PSM with three nearest neighbors and a caliper of .01 with our LPM. If our main point estimates change across these specifications, it would suggest our results are sensitive to model specification and analytic approach. The 9th grade behavior and adult voting outcomes remain largely unchanged for entrants. Across all three models, students who enter a charter school in 9th grade experience positive changes to their short- and long-run behavior. The coefficients on the crime outcomes for entrants loses statistical significance in the LPM without weighting model (although remains in the other two specifications), but are of similar magnitude to the LPM model with weighting. In short, the results for the entrants do not appear sensitive to our analytic strategy. The stayer effects are also largely consistent across specifications compared to our preferred LPM and IPW approach—for the first two specifications (LPM without weighting and matched cell fixed-effect) the stayers appear to experience a small positive effect of staying in a charter school in 9th grade on adult crime outcomes, although the results are not consistent in the LPM with IPW or matching with three nearest neighbors approaches.

In Table 6 we run our main analysis on two different samples and a model with an additional covariate in the two-step LPM with IPW technique. In the first analysis we limit the comparison set of students to those who did not have a secondary charter school with high school grades 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. Students new to the charter sector in 9th grade are less likely to be chronically absent, get suspended, be convicted of

any crime (or a misdemeanor or felony), more likely to register to vote, and vote in an election than TPS peers who do not have a charter school within 15 miles of the TPS high school they attended. Also similar to the main results, stayers are less likely to be suspended than their peers, and potentially less likely to be chronically absent and be convicted of any crime.

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 secondary 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 estimation of $\hat{p}$ 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 the LPM without weighting in Table 5. We also use the coefficients and R^2 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 $R^2_{max} = 1.3 * \tilde{R}^2$. We also assume the ratio of bias from unobservable and observable factors is one (Oster, 2017). Finally we report, δ, the ratio of bias from unobservable and observable factors needed to make our estimated treatment effects statistically equivalent to zero. If zero is not in the interval between the estimate from Table 4 and the estimated true effect in Table 7, and/or $\delta \geq 1$, the then it suggests that our analysis is not sensitive to potential omitted variable bias.

Across our 9th grade behavior, adult felony, and adult voting outcomes, 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). Further the bias from unobservable factors would have to be 1.2 to 3.0 times the size of the bias from our observable controls to make these effects statistically significantly equivalent to zero. The story is slightly less clear for the entrants effect on any crime or a misdemeanor, where the Oster Bound just includes zero but the ratio of bias from unobserved to observed factors is close to 1 (approximately .95). It is important to keep in mind that this analysis does not account for the potential benefit of the IPW in our two-step approach, and just uses the R^2 and estimated treatment effects from our simple LPM.

The bounded effects of staying in a charter school in 9th grade largely align with our main effects. Students who stayed in a charter school between 8th and 9th grade were less likely to be suspended and potentially less likely to commit a crime. The bounds suggest these students did not experience a benefit on attendance or voting.

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 suspensions relative to their peers who left the charter school sector, and potentially small effects on adult criminal behavior. However relative to charter school leavers, students who stayed in a charter school between 8th and 9th grade do not experience consistent changes to their 9th grade attendance or adult voting 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 new to a charter school in 9th grade experience consistent benefits from attending a secondary charter school. Black students (both entrants and stayers) experience large positive effects on 9th grade suspensions, and adult criminal outcomes for stayers (although with similar magnitudes for entrants). These students, however, do not experience a positive effect

from charters on voting or absenteeism. Finally, economically disadvantaged students experience similar positive benefits from attending a NC secondary charter school as either a stayer or entrant.

In Table 9 we present unique effects of NC secondary charter 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 suspension in 9th grade, with just economically disadvantaged white students experiencing large effect chronic absenteeism. Black students who are also economically disadvantaged have a particularly large reduction in the likelihood of being suspended in 9th grade, are substantially less likely to be convicted of any crime or a misdemeanor, and although not statistically significant, more likely to register to vote and vote in an election. However, for the long-term outcomes, it appears that the original effect for economically disadvantaged students on voting was driven by white economically disadvantaged students.

In the final analysis, we generate a summary measure of the types of the average local 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 county unemployment rate, county poverty rate, county crime index, county median income, urbancity, 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.[12] We used the full sample of

[12] 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.

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 white students, and rural districts.

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 in the first two tertiles experience positive effects across the short-run behavior and long-run adult criminal activity outcomes. These communities are more likely to be urban, have higher share of minority students, but also have better economic prospects and lower shares of violence than the third tertile. Interestingly, however, students in the first tertile have a mixed benefit of attending a charter school on voting outcomes (with just entrants experiencing a positive effect on voting, but not registering). However, entrants in the second and third tertile experience positive effects on registering to vote and voting. These results align with the Tables 8 and 9 which show black students are less likely to experience a positive effect of entering or staying in a charter school on voting. Finally students in the third tertile, both stayers and entrants, have large positive effects of attending a charter school in 9th grade on the likelihood of being suspended.

6 Conclusion

The current evidence on the effectiveness of charter schools, and high school grades 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 secondary charter 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 an inverse probability weighted linear probability model which includes students who did and did not already enroll in a charter school in 8th grade. 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. We also find suggestive evidence of a reduction in the propensity to commit a crime as an adult for students who stayed in a charter school between 8th and 9th grade, although this result is more sensitive to model specification. However, there are groups of students who remained in the charter sector in 9th grade who experience beneficial outcomes, including Black, 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				Weighted Sample			
	All Students		Charter Students		TPS Students		Charter Students		TPS Students	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Chronically Absent	0.156	0.363	0.087	0.282	0.173	0.378	0.087	0.282	0.092	0.289
Suspended in 9th grade^	0.212	0.409	0.054	0.225	0.221	0.415	0.054	0.225	0.121	0.326
Commit Crime	0.051	0.220	0.018	0.134	0.057	0.232	0.018	0.134	0.021	0.144
Commit Misdemeanor	0.038	0.192	0.014	0.118	0.043	0.203	0.014	0.118	0.017	0.129
Commit Felony	0.022	0.147	0.007	0.083	0.025	0.155	0.007	0.083	0.007	0.083
Registered to Vote	0.715	0.451	0.792	0.406	0.698	0.459	0.792	0.406	0.793	0.405
Vote in Election	0.477	0.499	0.583	0.493	0.469	0.499	0.583	0.493	0.571	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 weighted sample uses an Inverse Probability Weighting scheme to reshape the comparison students to match the observable characteristics of the treated students. The IPW is generated from a logistic regression using 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					IPW 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	0.73	0.45	0.01	0.12	1.54	0.73	0.45	0.75	0.43	-0.04
8th Grade Days Absent	6.49	6.43	8.33	8.84	-0.17	6.49	6.43	6.38	6.29	0.01
Male	0.48	0.50	0.51	0.50	-0.05	0.48	0.50	0.47	0.50	0.01
Asian	0.02	0.14	0.02	0.15	-0.01	0.02	0.14	0.02	0.15	-0.01
White	0.71	0.46	0.56	0.50	0.22	0.71	0.46	0.71	0.46	0.00
Black	0.20	0.40	0.29	0.45	-0.15	0.20	0.40	0.19	0.40	0.01
Hispanic	0.03	0.18	0.09	0.28	-0.15	0.03	0.18	0.04	0.20	-0.02
Other	0.06	0.23	0.07	0.25	-0.02	0.06	0.23	0.06	0.24	0.00
8th Grade Reading Ach.	0.37	0.91	0.00	0.99	0.27	0.37	0.91	0.36	0.90	0.01
8th Grade Math Ach.	0.26	0.95	0.00	1.00	0.18	0.26	0.95	0.24	0.93	0.01
7th Grade Reading Ach.	0.35	0.90	0.01	0.99	0.25	0.35	0.90	0.35	0.91	0.00
7th Grade Math Ach.	0.32	0.94	0.02	1.00	0.22	0.32	0.94	0.30	0.93	0.01
6th Grade Reading Ach.	0.35	0.90	0.01	0.99	0.25	0.35	0.90	0.35	0.90	0.01
6th Grade Math Ach.	0.31	0.94	0.02	1.00	0.21	0.31	0.94	0.26	0.93	0.04
Economic Disadvantage	0.17	0.37	0.46	0.50	-0.47	0.17	0.37	0.16	0.37	0.01
Gifted	0.10	0.29	0.16	0.37	-0.15	0.10	0.29	0.09	0.29	0.01
Special Education	0.10	0.31	0.13	0.34	-0.06	0.10	0.31	0.09	0.29	0.03
8th Grade Algebra	0.78	0.96	0.41	0.80	0.30	0.78	0.96	0.83	0.97	-0.04
8th Grade ELL	0.01	0.11	0.05	0.22	-0.16	0.01	0.11	0.02	0.13	-0.03
8th Grade Old for Grade	0.14	0.34	0.21	0.41	-0.13	0.14	0.34	0.13	0.33	0.02
Lagged Local Characteristics										
County Unemployment	7.76	2.08	7.74	1.99	0.01	7.76	2.08	8.09	2.09	-0.11
County Poverty Rate	9.68	2.85	11.00	3.90	-0.28	9.68	2.85	9.81	3.23	-0.03
County Crime Index Rate	3808	1410	4326	1616	-0.24	3808	1410	3490	1422	0.16
County Median Income	50862	9495	45975	8892	0.38	50862	9495	51038	9646	-0.01
Percent Urban	0.25	0.35	0.26	0.36	-0.03	0.25	0.35	0.23	0.35	0.04
Percent Rural	0.28	0.35	0.33	0.35	-0.09	0.28	0.35	0.25	0.34	0.07
Percent Suburban	0.10	0.20	0.12	0.21	-0.07	0.10	0.20	0.08	0.17	0.06
Percent Town	0.11	0.24	0.13	0.23	-0.05	0.11	0.24	0.12	0.23	-0.02
Observations	7981		624151			7981		624151		

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. The Inverse Probability Weighting (IPW) 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 the charter students receive a weight of 1 and the comparison students receive a weight of $\frac{\hat{p}}{1-\hat{p}}$. The logistic regression 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 the Estimation of the Inverse Probability Weight

	Full Sample					Weighted Sample				
	Charter Students		TPS Students		Std. Difference	Charter Students		TPS Students		Std. Difference
	Mean	SD	Mean	SD		Mean	SD	Mean	SD	
8th Grade Mobile Student	0.351	0.477	0.204	0.403	0.235	0.351	0.477	0.293	0.455	0.088
8th Grade Suspended^	0.084	0.278	0.239	0.426	-0.304	0.084	0.278	0.117	0.321	-0.077
Parent Ed No HS#	0.030	0.171	0.075	0.264	-0.143	0.030	0.171	0.027	0.162	0.013
Parent Ed HS	0.268	0.443	0.388	0.487	-0.184	0.268	0.443	0.210	0.408	0.095
Parent Ed HS+	0.077	0.266	0.089	0.284	-0.031	0.077	0.266	0.079	0.269	-0.006
Parent Ed Trade School	0.023	0.149	0.021	0.144	0.008	0.023	0.149	0.030	0.171	-0.032
Parent Ed Jr. College	0.145	0.352	0.120	0.325	0.051	0.145	0.352	0.122	0.327	0.048
Parent Ed 4yr	0.364	0.481	0.249	0.432	0.177	0.364	0.481	0.425	0.494	-0.089
Parent Ed Graduate School	0.095	0.293	0.057	0.232	0.100	0.095	0.293	0.107	0.309	-0.030

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 8th and 0th 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. The Inverse Probability Weighting (IPW) 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 the charter students receive a weight of 1 and the comparison students receive a weight of $\frac{\hat{p}}{1-\hat{p}}$. The logistic regression 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 Crime	Commit Misdemeanor	Commit Felony	Registered to Vote	Voted in Election
Entrant	-0.020***	-0.070***	-0.009**	-0.007**	-0.004**	0.028***	0.054***
	(0.006)	(0.010)	(0.003)	(0.003)	(0.002)	(0.007)	(0.013)
Stayer	-0.014+	-0.064***	-0.005+	-0.004	-0.001	-0.008	-0.009
	(0.008)	(0.012)	(0.003)	(0.003)	(0.001)	(0.010)	(0.012)
R-squared	0.161	0.120	0.049	0.038	0.031	0.051	0.060
Stayer-Entrant	0.006	0.006	0.004	0.003	0.003+	-0.036***	-0.063***
SE(S-E)	0.008	0.014	0.004	0.004	0.002	0.011	0.017
# of Treated Student	7625	5036	7625	7622	7622	7625	7625
# of Control Student	569050	280122	569050	568834	568834	569050	569050
mean (Y) TPS	0.103	0.137	0.030	0.022	0.013	0.770	0.547
mean (Y) Leaver	0.088	0.117	0.018	0.015	0.005	0.800	0.579

+ 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. Treated students (e.g. Entrants and Stayers) receive a weight of 1, and comparison students receive a weight of $\frac{\hat{p}}{1-\hat{p}}$ .. Standard errors are clustered at the secondary school level.

Table 5: Model Specification Check on the Main Results

	Chronic Absenteeism	Suspended in 9th grade	Commit Crime	Commit Misdemeanor	Commit Felony	Registered to Vote	Voted in Election
Linear Probability Model without Weighting							
Entrant	-0.020**	-0.066***	-0.007	-0.005	-0.004	0.032**	0.060***
	(0.007)	(0.017)	(0.004)	(0.003)	(0.003)	(0.010)	(0.016)
Stayer	-0.013+	-0.078***	-0.010**	-0.006*	-0.005*	0.002	-0.004
	(0.008)	(0.014)	(0.004)	(0.003)	(0.002)	(0.008)	(0.011)
Matched Cell Fixed-Effects LPM							
Entrant	-0.032***	-0.072***	-0.006+	-0.005+	-0.002	0.022*	0.048***
	(0.007)	(0.019)	(0.004)	(0.003)	(0.002)	(0.009)	(0.012)
Stayer	0.000	-0.070***	-0.008*	-0.006+	-0.004	-0.001	-0.004
	(0.009)	(0.014)	(0.004)	(0.003)	(0.002)	(0.011)	(0.013)
Nearest Neighbor PSM (n=3)							
Entrant	-0.022***	-0.080***	-0.011**	-0.008*	-0.005*	0.031***	0.060***
	(0.007)	(0.012)	(0.004)	(0.003)	(0.002)	(0.008)	(0.013)
Stayer	-0.020**	-0.070***	-0.003	-0.002	-0.001	0.000	-0.006
	(0.008)	(0.013)	(0.003)	(0.003)	(0.001)	(0.010)	(0.013)

+ 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 all three models. In the first panel, we estimate the same LPM as Table 4 but without the IPW. In the second panel In the second panel, we add matched cell fixed-effects that groups students by 8th grade school, gender, race/ethnicity, and economic disadvantage to our LPM. In the final panel, instead of using a IPW, we match students (with replacement) to their three nearest neighbors within .01 of their estimated probability, and estimate our LPM on this matched sample. Standard errors are clustered at the secondary school level.

Table 6: Main Result Robustness Checks

	Chronic Absenteeism	Suspended in 9th grade	Commit Crime	Commit Misdemeanor	Commit Felony	Registered to Vote	Voted in Election
Not within 15 Miles							
Entrant	-0.016+	-0.110***	-0.014**	-0.011**	-0.006*	0.051***	0.078***
	(0.009)	(0.014)	(0.005)	(0.004)	(0.002)	(0.010)	(0.016)
Stayer	0.013	-0.062**	-0.006	-0.002	-0.001	-0.021	0.002
	(0.011)	(0.020)	(0.004)	(0.003)	(0.002)	(0.019)	(0.025)
Changed Schools							
Entrant	-0.020***	-0.070***	-0.009**	-0.007**	-0.004**	0.028***	0.055***
	(0.006)	(0.010)	(0.003)	(0.003)	(0.002)	(0.007)	(0.013)
Stayer	-0.006	-0.078***	-0.015**	-0.010**	-0.009*	-0.009	-0.016
	(0.016)	(0.017)	(0.005)	(0.003)	(0.004)	(0.015)	(0.019)
Control for 8th Grade Suspensions							
Entrant	-0.027***	-0.075***	-0.008***	-0.008***	-0.003*	0.034**	0.080***
	(0.006)	(0.014)	(0.003)	(0.002)	(0.001)	(0.011)	(0.017)
Stayer	-0.022+	-0.040**	-0.010*	-0.008*	-0.001	-0.018	-0.008
	(0.012)	(0.013)	(0.004)	(0.004)	(0.001)	(0.013)	(0.017)

+ 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 the logistic regression to estimate the IPW and LPM, but the output is suppressed. The first panel restricts TPS students to those that do not live within 15 miles of a secondary charter school with high school grades 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 secondary school level.

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

Entrant Effect

	Chronic Absenteeism	δ	Suspensions	δ	Ever Crime	δ	Ever Misdemeanor.	δ	Ever Felony	δ	Ever Register	δ	Ever Vote	δ
Main effect (Stayer)	-0.020		-0.066		-0.007		-0.005		-0.004		0.032		0.060	
Oster Bound	-0.003	1.190	-0.042	2.720	0.000	0.950	0.000	0.921	-0.001	1.190	0.016	2.000	0.040	2.950

Stayer Effect

	Chronic Absenteeism	δ	Suspensions	δ	Ever Crime	δ	Ever Misdemeanor.	δ	Ever Felony	δ	Ever Register	δ	Ever Vote	δ
Main effect (Stayer)	-0.013		-0.078		-0.010		-0.006		-0.005		0.002		-0.004	
Oster Bound	0.012	0.552	-0.055	2.820	-0.002	1.280	-0.001	1.100	-0.002	1.400	-0.006	0.228	-0.008	-0.920

Note: The main effects are the LPM results in Table 5 without the IPW. The short regression controls for just 8th grade charter status to fully identify the Entrant and Stayer effects. The full model includes all of the demographic control variables, achievement control variables, and local characteristics. δ is the size of the ratio of the effect of unobservable to observables needed to change the treatment effect to zero.

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

		Chronic Absenteeism	Suspended in 9th grade	Commit Crime	Commit Misdemeanor	Commit Felony	Registered to Vote	Voted in Election
White Students	Entrants	-0.017**	-0.059***	-0.007**	-0.005+	-0.005***	0.023*	0.057**
	Stayers	-0.011	-0.049***	-0.003	-0.001	0.000	-0.009	-0.013
Black Students	Entrants	-0.009	-0.110***	-0.011	-0.010	-0.002	0.005	0.011
	Stayers	-0.003	-0.141***	-0.016*	-0.013**	-0.005	0.018	0.024
Hispanic Students	Entrants	-0.106***	-0.128***	-0.005	-0.008	0.000	0.136***	0.143***
	Stayers	-0.007	-0.147**	-0.069**	-0.064*	-0.008	-0.034	-0.010
Other Students	Entrants	-0.016	-0.058***	-0.017***	-0.014***	-0.005**	0.055**	0.084**
	Stayers	-0.067+	-0.022	-0.029+	-0.007	-0.014	0.041	0.023
Economically Disadvantaged	Entrants	-0.044***	-0.137***	-0.012	-0.013	0.001	0.031**	0.063***
	Stayers	-0.026	-0.121**	-0.020+	-0.022*	0.000	0.027	-0.019
Non-Economically Disadvantaged	Entrants	-0.012*	-0.051***	-0.007**	-0.005+	-0.005***	0.027***	0.051**
	Stayers	-0.009	-0.053***	-0.005	-0.003	-0.001	-0.013	-0.011
Male Students	Entrants	-0.036***	-0.086***	-0.013*	-0.011*	-0.007*	0.022**	0.059***
	Stayers	-0.021*	-0.075***	-0.009+	-0.007	-0.002	-0.023	-0.012
Female Students	Entrants	-0.006	-0.062***	-0.005	-0.003	-0.002	0.036***	0.053***
	Stayers	-0.005	-0.065***	-0.002	-0.002	0.000	0.001	-0.008

+ 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 IPW generating logistic regression 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. Treated students (e.g. Entrants and Stayers) receive a weight of 1, and comparison students receive a weight of $\frac{\hat{p}}{1-\hat{p}}$ .. Standard errors are clustered at the secondary school level.

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

		Chronic Absenteeism	Suspended in 9th grade	Commit Crime	Commit Misdemeanor	Commit Felony	Registered to Vote	Voted in Election
Black & Economically Disadvantaged	Entrants	-0.016	-0.157***	-0.016	-0.016+	0.000	-0.017	0.003
	Stayers	0.008	-0.227***	-0.034*	-0.034*	-0.003	0.059	0.061
Black & Non-Economically Disadvantaged	Entrants	0.001	-0.048*	-0.002	-0.001	-0.002	0.031	0.018
	Stayers	-0.003	-0.094**	-0.005	-0.004	-0.005	-0.003	0.002
White & Economically Disadvantaged	Entrants	-0.053***	-0.102***	-0.009	-0.013	0.004	0.047*	0.109***
	Stayers	0.007	-0.052	0.007	0.003	0.005	0.377***	0.224**
White & Non-Economically Disadvantaged	Entrants	-0.012+	-0.052***	-0.006**	-0.004	-0.006***	0.021+	0.051*
	Stayers	-0.011	-0.049***	-0.003	-0.002	0.000	-0.015	-0.013

+ 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 IPW generating logistic regression 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. Treated students (e.g. Entrants and Stayers) receive a weight of 1, and comparison students receive a weight of $\frac{\hat{p}}{1-\hat{p}}$ .. Standard errors are clustered at the secondray 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
County Unemployment Rate	6.720	216462	7.730	212674	8.830	211755
County Poverty Rate	8.150	216462	10.700	212674	14.300	211755
County Crime Index	4489	216462	4260	212674	4229	211755
County Median Income	54734	216462	45254	212674	38204	211755
Percent Urban	0.575	216462	0.250	212674	0.027	211755
Percent Rural	0.075	216462	0.366	212674	0.451	211755
Percent Suburban	0.169	216462	0.156	212674	0.043	211755
Percent Town	0.016	216462	0.067	212674	0.269	211755
Percent Economically Disadvantaged	34.700	216462	39.700	212674	45.800	211755
Short Term Suspensions	31.800	216462	34.800	212674	41.400	211755
Within School Violent Acts	16.200	216462	16.700	212674	15.400	211755
Percent Black	34.700	216462	31.100	212674	29.300	211755
Percent Hispanic	8.530	216462	7.790	212674	6.610	211755
Percent White	49.700	216462	55.300	212674	57.500	211755

Note: These characteristics represent the mean characteristics of TPS schools within 15 miles of the actual secondary school students attended in 9th grade, measured from the prior school year.

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

		Chronic Absenteeism	Suspended in 9th grade	Commit Crime	Commit Misdemeanor	Commit Felony	Registered to Vote	Voted in Election
Lowest Tertile	Entrant	-0.022**	-0.034**	-0.008**	-0.007**	-0.003*	0.015	0.040**
	Stayer	-0.006	-0.039***	-0.005	-0.003	-0.002	-0.016	-0.009
Middle Tertile	Entrant	-0.025**	-0.095***	-0.013***	-0.008*	-0.007***	0.040***	0.062***
	Stayer	-0.002	-0.091***	-0.010+	-0.009+	0.000	-0.029	-0.008
Highest Tertile	Entrant	-0.01	-0.119***	-0.004	-0.003	-0.002	0.026*	0.064**
	Stayer	-0.031	-0.084**	0.003	0.003	0.002	0.011	0.014

+ 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 IPW generating logistic regression 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. Treated students (e.g. Entrants and Stayers) receive a weight of 1, and comparison students receive a weight of $\frac{1}{(1-p)}$. Standard errors are clustered at the secondary school level. The main effects are reported separately by tertile of the potential peer exposure factor. The characteristics of the tertiles are found in Table 10. Standard errors are clustered at the secondary school level.

Appendix A

Table A. 1: Descriptive Statistics for Treatment and Comparison Groups

	Full Sample					IPW Sample				
	Charter Students		TPS Students		Std. Diff.	Charter Students		TPS Students		Std. Diff.
	Mean	SD	Mean	SD		Mean	SD	Mean	SD	
Student-level Characteristics										
8th Grade Charter	0.73	0.45	0.01	0.12	1.54	0.72	0.45	0.72	0.45	0.00
8th Grade Days Absent	6.49	6.43	8.33	8.84	-0.17	6.32	6.09	6.47	6.37	-0.02
Male	0.48	0.50	0.51	0.50	-0.05	0.47	0.50	0.47	0.50	0.00
Asian	0.02	0.14	0.02	0.15	-0.01	0.02	0.15	0.03	0.16	-0.02
White	0.71	0.46	0.56	0.50	0.22	0.72	0.45	0.71	0.46	0.01
Black	0.20	0.40	0.29	0.45	-0.15	0.19	0.39	0.19	0.39	0.00
Hispanic	0.03	0.18	0.09	0.28	-0.15	0.04	0.19	0.04	0.20	-0.02
Other	0.06	0.23	0.07	0.25	-0.02	0.06	0.24	0.06	0.24	-0.01
7th Grade Reading Ach.	0.35	0.90	0.01	0.99	0.25	0.36	0.90	0.37	0.91	-0.01
7th Grade Math Ach.	0.32	0.94	0.02	1.00	0.22	0.33	0.94	0.32	0.98	0.01
6th Grade Reading Ach.	0.35	0.90	0.01	0.99	0.25	0.37	0.90	0.37	0.92	0.00
6th Grade Math Ach.	0.31	0.94	0.02	1.00	0.21	0.32	0.93	0.30	0.97	0.02
Economic Disadvantage	0.17	0.37	0.46	0.50	-0.47	0.16	0.37	0.17	0.38	-0.02
Gifted	0.10	0.29	0.16	0.37	-0.15	0.10	0.31	0.11	0.31	-0.02
Special Education	0.10	0.31	0.13	0.34	-0.06	0.10	0.29	0.09	0.29	0.00
8th Grade Algebra	0.78	0.96	0.41	0.80	0.30	0.91	0.98	0.89	0.98	0.01
8th Grade ELL	0.01	0.11	0.05	0.22	-0.16	0.01	0.11	0.01	0.12	-0.02
8th Grade Old for Grade	0.14	0.34	0.21	0.41	-0.13	0.12	0.33	0.12	0.33	0.00
Lagged Local Characteristics										
County Unemployment	7.76	2.08	7.74	1.99	0.01	8.04	2.13	8.10	2.11	-0.02
County Poverty Rate	9.68	2.85	11.00	3.90	-0.28	9.82	2.96	9.87	3.32	-0.01
County Crime Index Rate	3808	1410	4326	1616	-0.24	3635	1243	3617	1350	0.01
County Median Income	50862	9495	45975	8892	0.38	50920	10001	50845	9522	0.01
Percent Urban	0.25	0.35	0.26	0.36	-0.03	0.22	0.33	0.23	0.35	-0.01
Percent Rural	0.28	0.35	0.33	0.35	-0.09	0.24	0.34	0.24	0.33	0.01
Percent Suburban	0.10	0.20	0.12	0.21	-0.07	0.09	0.20	0.09	0.19	0.01
Percent Town	0.11	0.24	0.13	0.23	-0.05	0.11	0.24	0.12	0.23	-0.02

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. Diff." is the standardized mean difference between the comparison and treatment groups, using the pooled standard deviation. The Inverse Probability Weighting (IPW) 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 the charter students receive a weight of 1 and the comparison students receive a weight of $\frac{1}{(1-p)}$. The logistic regression includes as predictors all demographic characteristics, achievement variables from 6th and 7th grade only, local characteristics, an indicator for charter 8th, and the interactions of charter 8th with demographic, achievement, and local characteristics.

Table A. 2: Balance on Baseline Characteristics not Used in the Estimation of the Inverse Probability Weight

	Full Sample					Weighted Sample				
	Charter Students		TPS Students		Std. Diff.	Charter Students		TPS Students		Std. Diff.
	Mean	SD	Mean	SD		Mean	SD	Mean	SD	
8th Grade Reading Ach.	0.37	0.91	0.00	0.99	0.27	0.39	0.90	0.37	0.93	0.02
8th Grade Math Ach.	0.26	0.95	0.00	1.00	0.18	0.28	0.94	0.27	0.99	0.01
8th Grade Mobile Student	0.35	0.48	0.20	0.40	0.24	0.29	0.46	0.29	0.45	0.00
8th Grade Suspended^	0.08	0.28	0.24	0.43	-0.30	0.08	0.27	0.12	0.32	-0.08
Parent Ed No HS#	0.03	0.17	0.08	0.26	-0.14	0.03	0.18	0.02	0.15	0.04
Parent Ed HS	0.27	0.44	0.39	0.49	-0.18	0.17	0.38	0.20	0.40	-0.06
Parent Ed HS+	0.08	0.27	0.09	0.28	-0.03	0.04	0.19	0.07	0.25	-0.10
Parent Ed Trade School	0.02	0.15	0.02	0.14	0.01	0.02	0.15	0.02	0.14	0.01
Parent Ed Jr. College	0.15	0.35	0.12	0.33	0.05	0.14	0.35	0.15	0.36	-0.02
Parent Ed 4yr	0.36	0.48	0.25	0.43	0.18	0.47	0.50	0.40	0.49	0.11
Parent Ed Graduate School	0.09	0.29	0.06	0.23	0.10	0.12	0.33	0.14	0.35	-0.03

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 8th and 0th grade that did not repeat a grade. "Std. Diff." is the standardized mean difference between the comparison and treatment groups, using the pooled standard deviation. The Inverse Probability Weighting (IPW) 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 the charter students receive a weight of 1 and the comparison students receive a weight of $\frac{1}{(1-p)}$. The logistic regression includes as predictors all demographic characteristics, achievement variables from 6th and 7th grade only, local characteristics, an indicator for charter 8th, and the interactions of charter 8th with demographic, achievement, and local characteristics.

Table A. 3: Main Short and Long-term Behavioral Outcomes

	Chronic Absenteeism	Suspended in 9th grade	Commit Crime	Commit Misdemeanor	Commit Felony	Registered to Vote	Voted in Election
Entrant	-0.020***	-0.067***	-0.009*	-0.006+	-0.005**	0.032***	0.061***
	(0.006)	(0.010)	(0.004)	(0.003)	(0.002)	(0.008)	(0.014)
Stayer	-0.015+	-0.067***	-0.006*	-0.005	-0.002	-0.003	-0.005
	(0.008)	(0.012)	(0.003)	(0.003)	(0.001)	(0.010)	(0.014)
R-squared	0.171	0.117	0.043	0.036	0.024	0.052	0.061
Stayer-Entrant	0.005	-0.001	0.003	0.001	0.004+	-0.035**	-0.066***
SE(S-E)	0.009	0.014	0.005	0.004	0.002	0.012	0.019
# of Treated Student	6464	4533	6464	6461	6461	6464	6464
# of Control Student	469948	263488	469948	469769	469769	469948	469948
mean (Y) TPS	0.098	0.136	0.028	0.020	0.011	0.776	0.546
mean (Y) Leaver	0.085	0.121	0.018	0.015	0.005	0.804	0.583

+ 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 from 6th and 7th grade only, and local characteristics are included as covariates in matching and LPM regressions, but the output is suppressed. Treated students (e.g. Entrants and Stayers) receive a weight of 1, and comparison students receive a weight of $\frac{1}{(1-p)}$. Standard errors are clustered at the secondary school level.