Charter School Effects on Charter School Students and Traditional Public School Students in North Carolina

Joshua Horvath

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Approved by:

Jane Cooley Fruehwirth

Doug Lauen

Klara Peter

Helen Tauchen

Valentin Verdier

ABSTRACT

Joshua Horvath: Charter School Effects on Charter School Students and Traditional Public School Students in North Carolina
(Under the direction of Jane Cooley Fruehwirth)

National growth of school choice has raised concerns about charter school effects on charter students and students left in traditional public schools (TPSs), particularly disadvantaged and minority students. The shift in North Carolina (NC) charters to serving higher-achieving students supports this concern. While much is known about charter school effects on charter students in primary school, much less is known about charter high school effects, and how charters affect TPS students.

My dissertation fills this gap in two ways. First, I use data covering all 9th grade public school students in NC from 2005 to 2016 to examine charter high school effects on charter student academic outcomes. I use propensity score matching and find that charters increase student English 1 and ACT scores, and decrease GPA. Charter school entrants (not in a charter in 8th grade) are more likely than TPS students to be retained in 9th grade and less likely to graduate in four years. These negative effects are significantly larger for black charter entrants than white charter entrants.

Second, I use panel data covering all public school students in NC from 1997 to 2016 to examine charter school effects on TPS student test scores in grades three through eight. Controlling for student and school fixed effects, I find no overall effect from competition, but higher-achieving charter competition has small positive effects. Lower-achieving competition has zero to small negative effects and, unlike higher-achieving competition, increases achievement gaps for some disadvantaged and minority populations.

These results suggest two things. First, the finding of no spillover effects on TPS students and some positive impacts from charter high schools suggests that the marginal expansion of charters in NC, at the least, does not hurt public school students. Second, average effects mask a considerable amount of heterogeneity. In particular, there are more negative charter high school effects on graduation for black charter entrants relative to white, and achievement gaps are increased from lower-achieving competition. This may suggest resources in charter high schools be shifted toward black charter entrants, and resources in TPSs facing lower-achieving competition be shifted to more disadvantaged and minority groups.

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CHAPTER 1

INTRODUCTION TO CHARTER SCHOOLS AND THE RELEVANT LITERATURE

1.1 Introduction

Charter schools are schools of choice that are publicly funded, tuition free, and open to any student regardless of background. Although charter schools are public schools, they operate outside the normal bounds of the traditional public school sector with significant flexibility in the use of funds, hiring practices, and curriculum choice. Since the first charter school legislation was passed in Minnesota in 1991, more than 40 states have enacted charter school legislation by 2018 (Education Commission of the States), and over 5% of public school students in the United States attended a charter school as of fall 2014 (National Center for Education Statistics).

Proponents of charter schools have argued two main claims. The first is that charter schools will have a direct and positive effect on their own student outcomes. This may be due to the increased flexibility and freedom to adjust learning environments as they see fit, or because competitive forces induce them to improve in order to attract students. The second claim is that charter schools will have a positive effect on nearby traditional public schools because charter schools introduce or add to existing competition between schools. On the other hand, opponents worry that increased flexibility and competitive forces will not drive charter schools or traditional public schools to improve. Additionally, there is concern that charter schools may negatively affect traditional public school students by draining resources and talent from traditional public schools.

Researchers have spent considerable time evaluating the effects of charter elementary and middle schools on their own students. Studies that focus on a subset of over-subscribed charter schools generally find large positive effects on students' test scores while studies utilizing fixed effect or matching methods find that charter schools are similar to traditional public schools in terms of improving student achievement.[1] In North Carolina, researchers find that in more recent years charter schools perform similarly to traditional public schools although, in earlier years, charter school effects were more negative (CREDO 2009, 2013b; Ladd et al. 2016).

While our knowledge of charter effects on test scores in elementary and middle school has grown substantially, less is known about the effects of charter high schools on longer term outcomes such as ACT scores,

[1]See section 1.3.1 for a detailed discussion of this literature.

retention, and high school graduation.[2] There are at least two reasons for the more limited literature on charter high schools and longer term outcomes. The first concerns estimation. The most credible matching estimators require baseline measures of the outcome of interest. For test scores, this is often easy because end of grade testing is required in grades three through eight. With outcomes such as retention and graduation, there is no lagged measure available. Additionally, panel data methods are not an option because many high school outcomes are not observed repeatedly for the same student over time. Second, charter high schools are simply less common than charter elementary and middle schools and so received less attention. Although test scores are important in so far as they positively predict longer term student success, it remains important to assess charter effects on longer term outcomes, such as high school graduation, which is predictive of college attendance, employment, and wages.

Additionally, much less is known concerning the spillover effects of charter schools on traditional public school students. The current literature finds mixed results, although generally, even studies that find effects, find small effects (Betts 2009; Epple et al. 2015).[3] Assessing spillover effects is important for two reasons. First, whether or not the students left in traditional public schools are hurt or helped by charter schools is important in its own right because more and more students have been exposed to potential spillovers as the charter sector has grown. Second, credible estimates of the effects of charter schools on their own students require the assumption that charter schools do not have spillover effects on traditional public schools. For example, in matching approaches to evaluate the effect of charter schools on their own students, charter students are often matched to comparison students in nearby traditional public schools. If these traditional public school students are affected by charter schools, they are no longer valid comparison students for assessing causal effects.[4] Therefore, it is important to assess whether charter schools have spillover effects on traditional public school students.

My dissertation addresses the limitations in the literature in two ways. First, using data covering all 9th grade public school students in North Carolina between 2006 and 2012, I examine charter high school effects of student outcomes including English 1 scores, ACT scores, final GPA, 9th grade retention, and four and five year high school graduation. I use a regression adjusted propensity score matching approach in which 9th grade charter school students are matched to comparison traditional public school students with similar 8th grade

[2]See Section 1.3.2 for a detailed discussion of this literature.

[3]See Section 1.3.3 for a detailed discussion of this literature.

[4]This is the first assumption of the Stable Unit Treatment Value Assumption (SUTVA) which states that whether a particular unit is treated does not affect the potential outcomes of other treated units (Imbens and Rubin 2015).

test scores and demographic characteristics. I find that charter high schools slightly increase English 1 scores, have moderate positive effects on ACT, and have small negative effects on GPA. I also allow effects to vary by whether a student was in a charter school in 8th grade and continued in a charter school in 9th grade (stayers) and whether a student was in a traditional public school in 8th grade and a charter in 9th grade (entrants). I find that charter school entrants are more likely than traditional public school students to be retained in 9th grade and less likely to graduate in four years. The negative effects on retention are driven by black students, and black charter entrants are significantly less likely to graduate than white charter entrants.

Second, using panel data covering all public school students in North Carolina from 1997 to 2016, I examine the effect of charter schools on traditional public school student math and reading test scores in grades three through eight. I control for student and school fixed unobservable characteristics and find no overall effect from charter competition on traditional public school students. However, higher-achieving charter competition has small positive effects and does not increase achievement gaps for disadvantaged or minority students. Lower-achieving charter competition has zero to small negative effects and increases achievement gaps for some disadvantaged and minority groups.

The results from my dissertation suggest three things. First, the finding of no overall competitive effect suggests that estimates of the effect of charter schools on their own students are not biased by spillovers. However, the finding of heterogeneous competitive effects based on school and student characteristics suggests that subgroup analysis of charter school effects on their own students could be biased, although the competition estimates are small which suggests the bias may be negligible.[5] Second, the finding of no competitive effects on students left in traditional public schools and some positive effects from charter high schools suggests that the marginal expansion of charter schools in North Carolina does not hurt public school students, and may even be beneficial. Third, analysis by student and school characteristics demonstrates that average effects may mask important heterogeneous effects. In particular, black charter entrants experience more negative effects from charter high schools than white charter entrants, and lower-achieving charter competition increases achievement gaps for some disadvantaged and minority groups. This may suggest that charter high schools should shift resources toward black charter entrants, and traditional public schools facing lower-achieving competition should shift resources to more disadvantaged and minority groups.

The rest of Chapter 1 proceeds as follows. Section 1.2 provides more detailed background information on

[5] The competition estimates are based on competition from charter elementary and middle schools (not high schools) so these results are only suggestive for high schools. We would have to assume that charter high schools have similar competitive effects on traditional public high schools as elementary and middle charter schools have on elementary and middle traditional public schools.

charter schools. Section 1.3 reviews the current literature concerning the effects of charter elementary and middles schools on their own students, charter high school effects on their own students, and the spillover effects of charter schools on traditional public school students. I emphasize particular gaps in the literature in order to motivate my contributions, which are explained in the remaining chapters. Chapter 2 details my empirical approach and results concerning the effects of charter high schools on charter student academic outcomes. Finally, Chapter 3 discusses estimation and effects of charter schools on traditional public school student achievement in North Carolina.

1.2 Background

Charter schools are operated by independent non-profit boards of directors and are freed from many of the regulations facing traditional public schools, but still must participate in the state accountability program. Charter schools do not have class size restrictions, curriculum requirements, and are not required to have all teachers licensed. Additionally, charter schools do not have to provide transportation and do not have to provide free and reduced price lunch to low income students. They have open enrollment policies, cannot charge tuition, and cannot be religiously affiliated. Oversubscribed schools must hold lotteries to randomly determine student entrants.

Funding policies vary by state, but in North Carolina charter schools receive state funding for each student that is equal to the per pupil allocation for average daily membership in the local education agency (LEA) in which the charter resides. Additional state funds are appropriated based on the number of students with disabilities and that are classified as limited English proficient. The LEA in which the charter student resides is required to transfer an amount equal to the per pupil local current expense appropriation fund of the LEA. In short, local and state funds follow the student so a local education agency that loses a student is also losing the funding that is attached to that student. Unlike traditional public schools, charter schools do not receive separate capital funding for school building construction or renovation.

Charter schools first opened in North Carolina in the 1997-1998 school year with the passage of the Charter School Act, and sole authority of charter school authorization was given to the State Board of Education.[6] Originally, a 100 school cap was placed on the total number of charter schools allowed in operation, but that cap was lifted in 2011. This created a situation in which the majority of growth in the charter school sector occurred from 1998 to 2002 and from 2013 to 2016. The final column of Table 1.1 shows the growth of the charter sector over time. Of all charter school openings between 1998 and 2016, 53% occurred between 1998

[6] Throughout the paper, school years will be referred to by the spring year. For example, the 1997-1998 school year will be referenced as 1998.

and 2002 and 33% between 2013 and 2016. In 1998, 34 charter schools were in operation comprising 1.7% of all public schools. By 2016, a total of 157 charter schools were in operation or about 6.1% of all public schools.

In North Carolina, traditional public schools normally follow traditional elementary (grades K-5), middle (grades 6-8), and high school (grades 9-12) grade spans. Charter schools do not follow this traditional pattern and are more likely to expand grade levels as they age. Table 1.2 shows the distribution of charter school grade spans the first and last year they are observed between 1998 and 2016. In their first year, about 36% of charter schools serve only elementary school grades and about 32% serve both elementary and middle school grades. In their last year observed, only 13% serve only elementary grades, and over 75% span some combination of elementary, middle, and high school grades. Charter schools serving some high school grade levels are included in the charter high school analysis which, in their last year observed, includes about 33% of charter schools. The competitive effect analysis focuses on effects from charter schools serving elementary or middle school grade levels which, in their last year observed, includes about 80% of charter schools.

1.3 Literature Review

1.3.1 Charter Elementary and Middle School Effects on Charter Students

Studies evaluating the effects of charter elementary and middle schools on student achievement can broadly be categorized based on the empirical strategy they employ. The main concern in estimation is that charter school students choose to enroll in charter schools, and therefore may differ in unobservable ways from students that stay in traditional public schools. The first set of studies, with the strongest internal validity, exploits random variation created by oversubscribed charter schools. Over-subscribed charter schools must hold lotteries to randomly determine student entrants so lottery winners and lottery losers theoretically have similar unobservables. However, charter school lottery information is not always available so a second group of studies controls for student selection into charter schools using student fixed effects. Fixed effect methods assume that student selection is not based on time-varying unobservables after controlling for fixed unobservable student characteristics and observable time-varying characteristics. Finally, researchers are concerned with the external validity of lottery and fixed effect studies so several studies use matching or other non-fixed effect regression methods to evaluate charter school effects. These methods often control for baseline achievement measures and assume that matched comparison students with similar baseline characteristics to treatment students are also similar in unobservables.[7]

[7] Several studies aggregate information to the school or school-grade level which leads to concerns that the self-selection problem is not controlled (Miron et al. 2002; Hoxby 2004; Bettinger 2005; Greene et al. 2006; Carlson et al. 2012). A similar problem occurs in observational studies using student level data that is not longitudinally linked (Eberts and Hollenbeck 2001; Buddin and Zimmer 2005), and in studies with linked student data that do not control for baseline test scores or student fixed effects (Lauen et al. 2015).

Lottery based strategies compare lottery winners to lottery losers to assess the effect of over-subscribed charter schools on their own students. This is termed the intent to treat effect because not all lottery winners choose to attend a charter school and some lottery losers still go to charter schools. An alternative treatment effect, and potentially one that is more policy relevant, is the treatment effect on the treated which is estimated using an instrumental variable strategy where attendance at a charter school is instrumented with the offer of admission.[8] Several studies use lottery based strategies and generally find positive effects on student math and reading test scores (Hoxby and Rockoff 2004; Hoxby and Murarka 2009; Angrist et al. 2010; Dobbie and Fryer 2011; Abdulkadirolu et al. 2011; Tuttle et al. 2013, 2015). The exception is Gleason et al. (2010) which finds no effect on student achievement, but does find some positive effects for certain student subgroups.

Although lottery based studies have strong internal validity, they are identifying the effects of over-subscribed schools which may not be representative of all charter schools, and they are identifying the effects on students in over-subscribed charter schools who entered through a lottery. In practice, some students enter over-subscribed charter schools outside the lottery because of sibling preference or other factors.[9] Additionally, many charter schools are not over-subscribed, and, even if they are, their lottery records may not be reliable. Because of these limitations, several charter school studies utilize student fixed effect strategies.

Studies utilizing student fixed effects find less favorable results than lottery studies. Several studies find negative effects on both math and reading test scores (Bifulco and Ladd 2006; Sass 2006; Booker et al. 2007; Hanushek et al. 2007; Ni and Rorrer 2012; Ladd et al. 2016) while Zimmer et al. (2003) find negative effects on math test scores and no effect on reading test scores, and Witte et al. (2007) find positive effects.[10] Several of these studies also find that charter school effects are more negative when charter schools first open or when students first enter a charter school, and then become similar to traditional public schools as the charter ages or the student has been in a charter longer. Two studies find no effect on achievement (Imberman 2011a; Nisar 2012). Finally, while most studies focus on a single state, district, or city, a handful of studies analyze several areas within the same analysis. Zimmer and Buddin (2006) find no effects in Los Angeles and small negative effects in San Diego. Zimmer et al. (2009) and Zimmer et al. (2012) find positive effects on math test scores in Texas, Denver, and Milwaukee. Otherwise they find null to negative effects on math or reading test scores in

[8]The treatment effect on the treated is more relevant because the intent to treat effect includes students that may not have actually received treatment (attended a charter school).

[9]See Zimmer and Engberg (2014) for a more detailed discussion.

[10]Although Zimmer et al. (2003) find negative effects, they conclude that charters are keeping pace with traditional public schools because the effects are so small. Additionally, they combine middle and high school into one analysis so I report results only from their elementary school analysis here.

Chicago, Denver, Milwaukee, Philadelphia, Ohio, San Diego, and Texas.

Fixed effect estimates are identified from students switching between charter and traditional public schools which raises concerns over external validity in so far as switchers are not representative of the entire charter student population. This may explain the divergent results between fixed effect and lottery based studies because each focuses on different subpopulations. Studies with the highest external validity rely on matching or regression based approaches (controlling for baseline test scores) that neither restrict the sample to over-subscribed charter schools (lottery studies) or student switchers (fixed effect studies). Results from matching and non-fixed effect regression approaches show mixed results. Several studies find positive achievement effects (Woodworth et al. 2008; Tuttle et al. 2010; CREDO 2013a; Gleason et al. 2013; Baude et al. 2014), one finds null effects (Furgeson et al. 2012), and many find negative impacts (CREDO 2009; Berends et al. 2010; CREDO 2011). Chingos and West (2015) find negative effects on math test scores and no effect on reading test scores.

Perhaps the most comprehensive examination of charter schools was conducted by CREDO. They started by evaluating charter schools in 16 states (CREDO 2009), and then updated that paper by examining charter schools in 27 states (CREDO 2013b). Although effects earlier in time were generally negative in math and reading, the updated report reveals that charter schools are improving over time until they are outperforming traditional public schools in reading and performing similarly in math. Ladd et al. (2016) uses fixed effects and also evaluates differences in effects over time in North Carolina and finds that although charter schools do not outperform traditional public schools, their effects do become less negative over time.[11]

Given the discussion above, it would be informative to know whether lottery estimates, fixed effect estimates, and matching estimates are similar when estimated on the same samples. Fortunately, there are several studies that compare charter school lottery estimates to matching estimates (Abdulkadirolu et al. 2011; Tuttle et al. 2013; Dobbie and Fryer 2013; Fortson et al. 2015), and one study that compares matching estimates to fixed effect estimates (Davis and Raymond 2012). These studies conduct within-study comparisons in which two estimation methods are performed on the same sample. For example, matching estimates and lottery estimates are compared using the same sample of lottery students. These studies generally find qualitatively similar results between methods, although sometimes the magnitude of effects differs somewhat between methods. This bolsters the use of fixed effect and matching methods in the charter school literature.

[11] Epple et al. (2015) provides a review of the literature concerning charter school effects on their own students and the spillover effects on traditional public schools.

1.3.2 Charter High School Effects on Charter Students

The literature examining charter high school effects uses several empirical strategies including random assignment based on lottery information and observational methods. Oversubscribed charter schools must hold lotteries to randomly determine student offers of admission. Because the offer is random, comparing lottery winners and losers produces estimates with high internal validity. Abdulkadirolu et al. (2011) examine the effects of Boston charter high schools using student assignment lotteries and find large positive charter high school effects on test scores. Angrist et al. (2016) estimate the effects of Boston charter high schools that held lotteries on college preparation and enrollment. They report higher pass rates on exit exams, increased SAT scores, and a shift in college enrollment from 2-year to 4-year institutions. Additionally, they find that charter schools substantially decrease the likelihood of graduating on time, but have no effect on 5-year high school graduation rates or on high school grade retention in 9th through 11th grade. Finally, Davis and Heller (2017) examine the effect of Noble Street Charter School in Chicago using randomized lottery information and find large positive effects on college enrollment and persistence.[12] These studies provide an important proof of concept: that the no-excuses model in two metro areas in the U.S. can have some positive impacts for students - with the exception of effects on graduating within 4 years in Boston. However, the external validity of these studies is weak since they are concentrated in two regions and draw from only one type of many charter school educational approaches.

Abdulkadirolu et al. (2011) addresses some generalizability concerns by using OLS on lottery and non-lottery Boston charter schools controlling for baseline test scores and demographics. They find that, among lottery schools, the observational estimates are very similar to the lottery estimates. This suggests that observational estimates are valid and can be extended to estimate impacts for non-lottery charter schools. They find that non-lottery charter school effects are about half the size of lottery charter effects, suggesting quite limited external validity of lottery estimates from the over-subscribed charters in their sample. Additionally, their validation of observational estimates against lottery estimates supports the empirical strategy I employ in Chapter 2 - at least in terms of test score outcomes. Because of concerns over external validity and the difficulty of obtaining lottery information, several studies use observational methods to identify the effects of charter high schools on student outcomes.

Several studies examining test score effects in elementary and middle school using student fixed effects, previously discussed in Section 1.3.1, also examine test scores in high school. Zimmer et al. (2003) find secondary

[12] Dobbie and Fryer (2015) examine the effects of a no-excuses middle school, Promise Academy in the Harlem Children's Zone, and find large positive effects on graduating on time, enrolling in college immediately after high school graduation, and enrolling in a 4-year institution.

charter schools do slightly better in reading and slightly worse in math, although the authors conclude charters are keeping pace with traditional public schools. Sass (2006) finds negative charter high school effects on math scores and no effect on reading with effects becoming more positive as charter schools age. Zimmer and Buddin (2006) group middle and high school together and find positive reading effects and negative math effects in San Diego and the reverse in Los Angeles. Ni and Rorrer (2012) find negative high school effects on language arts scores in the first year a charter opens, and no effect after the first year in Utah. The main concern with the fixed effect approaches are lack of generalizability because they rely on switchers. Finally, Chingos and West (2015) control for 8th grade test scores using OLS regression and find negative math effects and no effects on reading or writing in Arizona. Although these studies provide valuable information in assessing the effectiveness of charter high schools in raising test scores, they do not examine important longer term outcomes such as ACT scores, retention, or graduation.

Tuttle et al. (2015) use a matching design to examine the impacts of KIPP charter high schools on test scores and college readiness. They separately estimate effects for students entering a KIPP charter for the first time (entrants) and for those continuing from a KIPP middle school to a KIPP high school (stayers). For entrants, they report positive effects on achievement and graduation, although graduation effects are not statistically significant. KIPP high schools do not have positive effects on stayer achievement, but stayers are more likely to apply to college and more likely to take advanced courses than comparison students who did not have the option to attend a KIPP high school. Furgeson et al. (2012) use propensity score matching to form an appropriate comparison group to analyze the effects of charter high schools that were overseen by a Charter Management Organization. They find positive, statistically insignificant effects on graduation and college enrollment. Although the results concerning KIPP schools and charters under a management organization are informative, these studies lack generalizability.

Booker et al. (2011) examine charter high school effects in Florida and Chicago using probit estimation controlling for baseline student characteristics and restricting the sample to students in a charter school in 8th grade to control for selection. They find that charter schools increase graduation rates from high school and increase the probability of enrolling in college. Sass et al. (2016) extend the analysis of Booker et al. (2011) in Florida by examining effects on college persistence and wages. This study restricts the sample to students in 8th grade and uses one-to-one nearest neighbor Mahalanobis matching. They report that charter school students are more likely to graduate from high school, enroll in college, persist in college, and earn higher wages. The authors argue that restricting the sample to students who attend a charter school in 8th grade alleviates concerns over selection bias. However, 9th grade charter school students would then be compared to 9th grade traditional public school students who, in some cases, may have chosen to exit the charter sector which is simply another form of selection. It is unclear that the sample restriction would increase internal validity, and it necessarily decreases external validity.

Overall, the literature suggests that charter high schools have some positive effects on high school test scores, graduation, and college enrollment. At the very least, they are doing no worse than traditional public schools with the exception of 4-year graduation in Boston and some small negative effects on test scores in Zimmer and Buddin (2006) and Chingos and West (2015). Generalizability is the main limitation of the current literature either because samples are restricted to specific geographic regions, particular types of charter schools, or particular types of charter school students. Additionally, many high school papers focus on test scores and do not extend the analysis to more important outcomes such as high school graduation. In Chapter 2, I address the generalizability concerns and examine longer term outcomes including ACT test scores, GPA, retention, and high school graduation.

1.3.3 Charter Elementary and Middle School Effects on Traditional Public School Students

In addition to concerns over student selection into schools, competitive effect studies are also concerned over charter school location. If charter schools were randomly assigned location this would not be a concern, but it is likely that charter schools strategically locate, and location may be correlated with unobservable factors affecting traditional public school student achievement. Broadly, the literature examining the competitive effects of charter schools has developed starting with school level analyses using various empirical strategies, then moving into student and school fixed effect approaches, and finally, several recent papers have shifted to other empirical methods.

Due to data limitations, early papers on the effects of charter schools on traditional public schools conducted school level analyses. Measures of competition relied on the quantity of nearby charter schools or charter school students. Hoxby (2003) uses school level data from Michigan and Arizona, and defines treatment for a traditional public school as facing charter school enrollment of at least 6% of total public school enrollment in a district. She finds positive and significant effects on traditional public school test scores in both states. Bettinger (2005) also utilizes Michigan data but finds no effect of charter schools on traditional public schools. Ni (2009) also conducts a school level analysis in Michigan, and finds negative effects on achievement although effects are small in the short-run and become more substantial in the long-run. Hoxby, Bettinger, and Ni are concerned with non-random charter school location. Hoxby attempts to correct for this using a detrended difference-in-differences approach, Bettinger relies on an instrumental variable approach using distance from a traditional public school to a charter authorizing public university as an instrument for the number of charters within a five mile radius of a traditional public school, and Ni controls for school specific unobserved linear trends. The main concern with school level analysis is that average school test scores may be masking student movement. For example a traditional public school may be feeding a nearby charter school below average students. Then in a school level analysis, it may appear that traditional public school students improved when in fact the pool of students changed.[13]

Later papers employ student level panel data to limit bias from compositional effects. Bifulco and Ladd (2006) examine the effects of charters on nearby traditional public school students using data from North Carolina. They find no effect of charters on traditional public school student test scores using a restricted value added model of achievement where competition is measured as being near a charter or the number of nearby charters. In order to account for the endogeneity of charter school location, they use a student-school spell fixed effect which accounts for any time-invariant unobservables related to charter school location, student movement, and test score outcomes. Estimates will not be consistent if selection depends on time varying unobservables that are also related to achievement. Sass (2006) uses the same empirical strategy, but uses the enrollment share of charters as an additional measure of competition. Results from students in Florida indicate no effect on reading scores but a positive effect on math scores.

Booker et al. (2008) use a similar empirical model looking at students in Texas and find positive achievement effects. They make two notable contributions. The first is that they include both district and school level measures of competition because the district as a whole may respond to competitive pressure from charters. Second they look at heterogeneous effects based on race and traditional public school quality. Minority students and lower quality traditional public schools experience the largest positive effects. Zimmer and Buddin (2009) use California student level data and school principal survey results. A spell fixed effect regression of student achievement on varying measures of competition, including the number of nearby charters and enrollment share, finds no effects on student achievement. Survey results from traditional public school principals largely confirms this empirical finding.

Winters (2012) restricts attention to New York City and uses student attrition to charters as the measure of competition. Different combinations of school and student fixed effects reveal mixed results when examining student achievement. The majority of previous studies are confined to one or two states. In order to assess the generalizability of results, Zimmer et al. (2009) examine competitive effects in seven states. Only Texas reveals significant positive effects. The empirical approach of this branch of the literature has two main weaknesses.

[13] Betts (2009) provides a review of the major concerns in estimating charter school competitive effects and the state of the literature.

First, the spell fixed effect method controls for selection on time-invariant factors, but not for the possibility that charter schools locate based on unobservable trends in traditional public school achievement. Second, the literature relies on an assumption that student achievement is perfectly persistent. If it is the case that there is decay in learning from year to year and lagged achievement is correlated with charter school location, then spell fixed effect estimates will be inconsistent.

Because of the limitations just mentioned, a few recent papers on this topic have shifted analysis away from the spell fixed effect approach. Imberman (2011b) examines competitive effects in a large urban district in the Southwest and employs a novel instrumental variable for being near a charter. The instruments are variations of the supply of buildings of appropriate size for a charter school. Charter schools do not generally receive funds for building schools and so rely on local buildings to rent. Results show a statistically significant drop in math and language test scores. Imberman extends the analysis to other outcomes including attendance and behavior, and finds significant positive effects in middle and high school. Mehta (2017) models competition to determine both direct and spillover effects of charter schools and finds small positive spillover effects in North Carolina.

Finally, Cremata and Raymond (2014) and Cordes (2018) allow competitive effects to vary by the achievement of charter schools relative to the district. Using a difference-in-differences strategy, Cremata and Raymond (2014) find that being near charter schools with higher achievement positively affects student achievement in nearby traditional public schools. Using a lagged value added model with school fixed effects, Cordes (2018) finds no differential effect on math test scores, but a small positive effect on reading test scores from charter schools with higher achievement. These papers, however, measure a charter school's achievement relative to the district rather than relative to the traditional public schools they define as competing with the charter school, which is arguably the more relevant comparison. Additionally, they lack generalizability because they are confined to two large urban districts.

Overall, the literature paints a very mixed picture of the spillover effects of charter schools on traditional public schools, although generally effects are null to small in magnitude. The main concerns with the current literature are reliance on the implausible assumption that what students learn over time perfectly persists, and reliance on fixed effect methods that may not adequately control for selection concerns. Additionally, more work is needed to assess heterogeneous effects based on relative school characteristics and student characteristics. In Chapter 3, I relax the implausible perfect persistence assumption, include linear school trends and lagged covariates to move beyond student and school fixed effects, and explore heterogeneity based on school and student characteristics.

Table 1.1: Traditional Public Schools, Charter Schools, and New Charter Schools by Year

Year	Traditional Public School Count	Charter School Count	Charter Schools % of All Public	New Charter School Count	Growth in Charter Schools
1995	1,952	0	0.0%	0	-
1996	1,967	0	0.0%	0	-
1997	1,989	0	0.0%	0	-
1998	2,006	34	1.7%	34	-
1999	2,028	59	2.8%	26	76.5%
2000	2,063	77	3.6%	23	39.0%
2001	2,096	90	4.1%	15	19.5%
2002	2,125	93	4.2%	8	8.9%
2003	2,145	93	4.2%	5	5.4%
2004	2,160	93	4.1%	2	2.2%
2005	2,178	97	4.3%	5	5.4%
2006	2,232	99	4.2%	2	2.1%
2007	2,288	93	3.9%	1	1.0%
2008	2,349	98	4.0%	7	7.5%
2009	2,380	97	3.9%	2	2.0%
2010	2,413	96	3.8%	0	0.0%
2011	2,424	99	3.9%	3	3.1%
2012	2,409	100	4.0%	1	1.0%
2013	2,414	108	4.3%	9	9.0%
2014	2,424	128	5.0%	22	20.4%
2015	2,431	149	5.8%	23	18.0%
2016	2,437	157	6.1%	12	8.1%

All public schools are included, and there is no restriction on school type or grade levels served. Traditional public schools include magnet schools. The growth in charter schools is defined as the number of new charter schools divided by the number of charter schools in operation the previous year.

Table 1.2: Charter School Grade Spans First Year Observed in the Data and Last Year Observed in the Data, 1998-2016

	First Year Charter Observed in Data		Last Year Charter Observed in Data	
Grade Span	Freq.	Percent	Freq.	Percent
Elementary	76	36.89	27	13.11
Middle	10	4.85	4	1.94
High	21	10.19	18	8.74
Elementary-Middle	66	32.04	87	42.23
Middle-High	23	11.17	22	10.68
All	10	4.85	48	23.3
Total	206	100	206	100

Elementary includes schools serving only grade levels in kindergarten through five. Middle includes schools serving only grade levels in six through eight. High includes schools serving only grade levels in nine through twelve. Elementary-Middle includes schools serving both Elementary and Middle grade levels. Middle-High includes schools serving both Middle and High school grade levels. All includes schools serving Elementary, Middle, and High School levels.

CHAPTER 2

CHARTER SCHOOL IMPACTS ON HIGH SCHOOL ACADEMIC OUTCOMES IN NORTH CAROLINA (CO-AUTHORS SARAH CRITTENDEN FULLER, DOUG LAUEN, AND ANDREW MCEACHIN)

2.1 Introduction

While we know quite a bit about charter school impacts on test scores for elementary and middle school grades, we know very little about charter school impacts in high school grades. This omission is notable given that about 500,000 students nationwide are enrolled in charter high schools (Snyder et al. 2016). Additionally, ACT test scores, high school graduation, and college attendance may be more predictive of later life success and so of more interest to policymakers and researchers. To date, there are only a small handful of studies of charter high school impacts on non-test score outcomes which generally report positive effects. Two of these are lottery-based studies reporting positive impacts of charters in two cities, Boston and Chicago, most of which subscribe to the so-called no excuses approach (Angrist et al. 2016; Davis and Heller 2017). The fact that these studies focus on one type of charter in two cities greatly limits the generalizability of inferences about charter high school impacts of the full range of charters or in different locales. Another study of KIPP, a charter school network that subscribes to the no excuses philosophy, finds positive high school charter effects as well (Tuttle et al. 2015). Another set of studies with superior external validity examine citywide effects in Chicago and statewide effects in Florida using observational data with probit estimation and matching analysis and generally find positive impacts (Booker et al. 2011; Sass et al. 2016). These studies, however, restrict the sample to students who were already in a charter school in 8th grade. This limits external validity with an uncertain payoff to internal validity.

Observational methods may increase external validity over lottery based studies in theory, but the current literature has limited external validity because it focuses on specific types of charter schools or specific types of students: only KIPP middle schools or only charter high school students observed in a charter school in 8th grade (i.e., charter school stayers). The present study fills this gap by examining charter schools in a populous and diverse state without restricting the analysis to students observed in charter schools in 8th grade. Further, we add to the literature that goes beyond elementary and middle school test score impacts to examine the effects of charter high schools on high school achievement and attainment outcomes. Additionally, we explore heterogeneous impacts based on characteristics of the student (ethnicity, sex, limited English proficiency status, disability status) and of the school (grade structure, age, proportion white).

Specifically, using statewide administrative data, we conduct a propensity score analysis with 3:1 nearest neighbor matching with regression adjustment estimation to estimate impacts on six outcomes: English I end of course tests, ACT scores, weighted final GPA, retention in 9th grade, four-year high school graduation, and five-year high school graduation. We include cohorts that began ninth grade between 2004-05 and 2011-2012, inclusive. Our sample contains nearly 9,500 charter students, more than 700,000 potential comparison students, and around 12,000 matched comparison students. We attain excellent balance for our preferred specification on observable 8th grade matching covariates and match about 83% of the treatment group, depending on specification. We report four treatment-on-the-treated effects: 1) the effect of attending a charter school in 9th grade, 2) the effect of attending a charter in 9th grade conditional on attending a charter school in 8th grade, 3) the effect of entering a charter high school in 9th grade for students who were not observed in a charter school in 8th grade (effect on entrants), and 4) the effect of staying in the charter school sector for those already observed in a charter in 8th grade (effect on stayers).[1]

In brief, we find weak positive overall effects on English I end of course tests, stronger positive effects on ACT scores, and small negative effects on GPA. In English 1 and ACT, the charter school effect conditional on 8th grade treatment status (charter or not) is smaller in absolute value than the effect that does not condition on baseline charter enrollment. In addition, across all three achievement outcomes, the effects are symmetric between charter entrants and charter stayers. Across the attainment outcomes, we find a different pattern. We report retention rates, four-year graduation rates, and five-year graduation rates very close to zero in all specifications. However, we find small differences between entrants and stayers, with entrants facing higher retention probabilities and lower graduation probabilities than stayers. Furthermore, the increased retention rate for charter entrants is largely driven by black students, and black charter entrants are significantly less likely to graduate in four or five years than white charter entrants. Students in older charter schools generally experience more positive effects across outcomes than students in younger charter schools. Finally, students in charter schools with above average proportions of white students are less likely to be retained and more likely to graduate than students in charter schools with below average proportions of white students.

[1]The matching estimator is estimating treatment on the treated effects because comparison students are matched and form a counterfactual for students that are observed in a charter school in 9th grade (rather than students offered admission to a charter school). However, we define a student as treated even if they leave the charter sector in subsequent years which is reminiscent of an intent to treat effect.

2.2 Data

This study uses longitudinal administrative data provided by the North Carolina Department of Public Instruction (NCDPI). This data includes all students who attended North Carolina public schools, including charter schools, from 2004-05 to 2015-16. It allows us to follow individual students longitudinally as long as they remain enrolled in NC public schools. At the student level this data set contains demographics, including gender, student ethnicity, an indicator for economic disadvantage, disability, giftedness, limited English proficiency, state standardized test scores, ACT scores, absences, graduation, GPA, and course taking information. At the school level, the data set includes percent economically disadvantaged, short terms suspension, violent acts, ethnic breakdown, per pupil expenditures, urbanicity, and total enrollment.

This study includes seven cohorts of students who began 9th grade for the first time during the 2005-06 to 2011-12 school years. Students are included in the cohort only if they appear in 8th grade and 9th grade in North Carolina public schools in consecutive years. Students are identified as treatment students if they attend a charter school anytime in 9th grade. Treatment students who subsequently exit the charter sector (sometime after 9th grade) are retained in the treatment group. The data set includes 9,496 treatment students enrolled in a charter school in 9th grade and 709,665 comparison students who enrolled in traditional public schools in 9th grade. Of these, 7,978 treatment students and 624,174 comparison students have non-missing information on all control variables and so are eligible to be in the matched sample.

2.2.1 Descriptive Statistics

There are six dependent variables of interest: English 1 end of course test scores, ACT test scores, weighted final GPA, 9th grade retention, 4-year graduation, and 5-year graduation. Table 2.1 summarizes the dependent variables of interest, provides missing counts, and summarizes by treatment and comparison groups. English 1 is normally taken in 9th grade, and we standardize the test scores within year to have a mean of zero and standard deviation of one. Standardization uses the state-wide mean and standard deviation of English 1 scores as reported in the State Testing Results (Green Book). Starting in 2012, all students in North Carolina enrolled in grade 11 for the first time are required to take the ACT which means analysis for ACT scores is conducted on a more limited set of cohorts, 2010-2012. For analysis, ACT scores are standardized to have a mean of zero and standard deviation of one using the mean and standard deviation in the analytic sample. GPA is final high school GPA. It is weighted to reflect the difficulty of a course and ranges from zero to six with an average of 3.08. Students are defined as being retained in 9th grade if they are observed in the data in the 9th grade for two consecutive years. About 9% of students are retained in the 9th grade. We construct two graduation variables for whether a student graduates within 4 or 5 years of the first year a student is observed in the 9th grade. Graduation is coded as missing if a student leaves the state, dies, leaves for a private school, or leaves for home school. About 77% of students graduate within 4 years of their first year in high school, and a slightly higher percent, 79%, graduate within 5 years.

Before matching, treatment students have statistically significantly lower retention rates and higher English 1 test scores, ACT test scores, GPA, and graduation rates. It is unclear whether these differences are causal or reflect the selection of higher performing students into charter schools. In order to explore this further, we utilize baseline information from 8th grade or earlier. Student level controls are measured at baseline except for middle school mobility and passing or failing algebra 1 in middle school. Middle school mobility is an indicator that is one if a student ever switched schools while observed in grades six through eight. Algebra 1 is counted whenever a student first takes it in middle school, which could be before 8th grade. Control variables are divided into three groups listed below.

- Demographic Controls: 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

- Achievement Controls: failed algebra 1 in middle school, passed algebra 1 in middle school, standardized 8th grade math test scores, and standardized 8th grade reading test scores[2]

- Lagged Local Characteristic Controls: economically disadvantaged, short terms suspension, violent acts, percent Asian, black, Hispanic, multi-racial, American Indian, white, per pupil expenditures, urbanicity, and enrollment

Old for grade is an indicator that is one of a student is strictly greatly than 15 years old by the 1st of September of his or her 8th grade year. Lagged local characteristics are the lagged average characteristics of the five nearest traditional public high schools within 15 miles of the high school a student is attending in 9th grade. For traditional public schools, this average includes the traditional public school itself, but charter schools are always excluded from the local average. Essentially, this requires that treatment students are matched to comparison students in high schools with similar local characteristics.

The first two columns of Table 2.2 display means of the control variables by treatment group, and suggest that students attending charter schools in 9th grade are different along several observable dimensions than traditional

[2]It is possible to use 6th or 7th grade test scores as well, but not all students observed in 8th grade are observed in 6th and 7th grade so the estimating sample would be reduced. For example, about 20% of students with non-missing information on all the control variables and 8th grade test scores are missing either 7th grade math or reading test score information.

public school students. For example treatment students are much more likely to have attended a charter school in 8th grade, are significantly more likely to be white, and have higher 8th grade math and reading test scores than comparison students. This suggests simply comparing mean outcomes is not causal, and more sophisticated econometric methods are needed.

2.3 Analysis

The main concern when estimating the effects of charter high school attendance on student outcomes is that students choose whether or not to attend a charter school, and the choice to attend may be based on unobservable student or family characteristics that are also correlated with student outcomes. For example, more motivated parents may be more likely to enroll their children in charter schools. Conversely, students performing poorly in charter schools may be more likely to exit to traditional public schools. Selection comes in two forms. First, students not enrolled in a charter school in 8th grade may select into a charter high school in 9th grade (entrants). Second, students already enrolled in a charter school in 8th grade may select out of the charter sector even though they can continue in the charter sector into 9th grade (leavers). The option to continue in the same charter school from 8th to 9th grade is prevalent in North Carolina because the majority of charter high schools are combination schools serving both middle and high school grade levels. Table 2.3 displays the frequency of different types of student moves between charter and traditional public schools between 8th and 9th grade. There are 6,663 9th grade charter students that were in a combination charter school in 8th grade or a charter school expanding to serve 9th grade (combo schools), which makes up about 65% of all 9th grade charter students. We denote students that stay within the charter sector between 8th and 9th grade "stayers" and those that enter the charter sector in 9th grade and were not in a charter in 8th grade "entrants". In terms of Table 2.3, rows 5 and 7 are stayers and rows 2 and 3 are entrants which implies that 70% of 9th grade charter students are stayers and 30% are entrants.

In order to control for student selection, we utilize a two-step approach. First, we use propensity score matching to construct a comparison group of traditional public school students that are similar in baseline characteristics to charter school students. Second, we use weighted regression on this matched sample controlling for the same set of covariates used in the first step. Together, this provides a regression adjusted propensity score matching estimator that is more robust than matching or regression alone, which is discussed in more detail shortly.

More specifically, baseline is defined 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 effect of attending a charter high school for those that attended a charter high school, or in terms of our notation

$$\Delta^{tt} = E[Y_i(1) - Y_i(0)|T_i = 1], \tag{2.1}$$

which is termed the treatment on the treated. For an individual student, both potential outcomes cannot be observed, so we require the construction of an appropriate counterfactual. Because of the selection issues already noted, we cannot simply compare the difference in means between students who do and do not attend a charter high school in 9th grade. Rather, we assume conditional independence: conditional on observable characteristics X_i, the potential outcome under no treatment $Y_i(0)$ is independent of treatment T_i,

$$Y_i(0) \perp T_i|\boldsymbol{X}_i. \tag{2.2}$$

In other words, once we match students on observable baseline characteristics, whether a student receives treatment or not is unrelated to the outcome a student would realize if not treated, and comparing the difference in means between the treated and matched comparison students is an unbiased estimate of the treatment on the treated.[3] Instead of exact matching on observable characteristics, we follow the approach of Rosenbaum and Rubin (1983) and match on the probability of treatment (propensity score) to reduce dimensionality.[4]

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

$$Prob(T_i = 1) = \frac{e^{\alpha_0 + \boldsymbol{X}_i \alpha_1 + \gamma_c}}{1 + e^{\alpha_0 + \boldsymbol{X}_i \alpha_1 + \gamma_c}}, \tag{2.3}$$

where T_i is the treatment status of student i, X_i is a vector of control variables, and γ_c is cohort fixed effects. The propensity models are estimated using logistic regression. The control vector consists of three main sets of variables: demographic controls, achievement controls, and lagged local characteristics. After propensity scores are estimated, treatment students are matched to up to 3 students with nearest propensity scores (nearest neighbors).[5]

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

[4] Because we do not exact match on covariates (we match on similar propensity scores within a pre-specified range), covariates will differ between matched treatment and comparison students. This may introduce some bias, but the estimator is consistent because we are matching on only one continuous variable - the propensity score (Imbens 2004; Abadie and Imbens 2006; Imbens and Wooldridge 2009).

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

Matching is with replacement, so the same comparison student can be matched to multiple treatment students. Matches are restricted to be within 0.01 units of the treated propensity score to ensure match quality. A student is not allowed to be matched to students in different cohorts (exact matching on cohort). Treatment students with propensity scores more than a 0.01 unit difference from any potential comparison students are unmatched and omitted from the analysis. After matching, the treatment and comparison group observable characteristics are compared to assess balance.

In the second step, treatment effect estimation is carried out on the matched sample using OLS or Logit estimation using the following regression model:

$$Y_i = \beta_0 + \beta_1 T_i + \boldsymbol{X}_i \boldsymbol{\beta}_2 + \lambda_c + \epsilon_i, \tag{2.4}$$

where Y_i is outcome for student i, T_i is an indicator for attending a charter school in 9th grade, $\boldsymbol{X}_i$ is the same vector of covariates used in the propensity score model, λ_c are cohort fixed effects, and ϵ_i is an idiosyncratic error. The model includes the same set of covariates used in the matching process in order to control for any remaining differences between the treatment and comparison groups and to increase precision. Standard errors are clustered at the high school level. OLS regression is used for continuous outcomes: standardized English 1 scores, standardized ACT scores, and weighted GPA. Logistic regression is used for dichotomous outcomes: retained in 9th grade, graduated in 4 years, and graduated in 5 years.

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

With the addition of the second estimation step in equation 2.4, the coefficient, β_2, is a two-step estimator, termed a regression adjusted propensity score matching estimator. Regression adjusted matching estimators are considered better in practice than regression or matching on its own (Imbens and Wooldridge 2009; Abadie and Imbens 2011). This is because, after matching on the propensity score, small differences remain between the covariates of the treatment and matched comparison students. Regression reduces the bias from these differences

(Imbens 2004). Additionally, this estimator is robust to misspecification of the regression function in the second step (Abadie and Imbens 2011).

2.3.1 Charter 8th Grade Control

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

While it is important to control for 8th grade charter status, either because it controls for selection or because it captures lagged effects from prior charter attendance, we pursue a different approach in this paper. We contend that a sample restricted to only students observed in 8th grade in charter schools limits external validity with uncertain benefits to internal validity. If we restrict the sample to students who attended a charter in 8th grade, students who chose to stay in a charter will be compared to students who chose to exit. The validity of this comparison rests on the assumption that selection out of a charter school is independent of any unobservable shocks. In other words, we are assuming that selection out of treatment is controlled. Conversely, if we restrict the sample to students who did not attend a charter in 8th grade, students who chose to enter a charter will be compared to those that did not. In other words, we are assuming that selection into treatment is controlled. It is unclear whether matching methods would control for selection out of treatment while not controlling for selection into treatment, so we include estimates for both sets of students.

In practice, we specify a model that both matches and controls for 8th grade charter school status (CH_8th) 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 ($Stayer$). After matching, we fit an outcome model with indicator variables for these two types of students:

$$Y_i = \delta_0 + \delta_1 Entrant + \delta_2 Stayer + \delta_3 CH\_8th + \boldsymbol{X}_i \boldsymbol{\beta}_2 + CH\_8th \times \boldsymbol{X}_i \boldsymbol{\beta}_3 + \lambda_c + CH\_8th \times \lambda_c + \epsilon_i. \quad (2.5)$$

This model implies that students in a charter school in 9th grade that were not in a charter school in 8th grade (entrants) are compared to 9th grade traditional public school students that were also not in a charter in 8th grade. Conversely, 9th grade charter students that were in a charter in 8th grade (stayers) are compared to 9th grade traditional public school students that were in a charter in 8th grade (leavers). In estimation, all covariates and cohort fixed effects are interacted with the indicator for being in a charter school in 8th grade. This implies 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).

2.3.2 Robustness Checks

The primary threat to identification is that unobservable student characteristics are correlated with a student's decision to enroll in a charter high school and are correlated with high school outcomes. Fortunately, there are several potential robustness checks available to assess the extent of this potential source of bias.

Restricted Matching Estimators

The majority of charter high school students were also in a charter school in 8th grade so we are particularly concerned that students who choose to leave their charter school between 8th and 9th grade when they could have continued (leavers) are systematically different from those that choose to stay (stayers). As a check, we restrict the potential match comparison group for students in charter schools in 9th grade to students in traditional public schools in 9th grade that did not choose to exit a charter school when they could have continued in that charter school. In terms of Table 2.3, the excluded students comprise row six, and are charter leavers. For example, students in a charter middle school that does not serve high school grades who move to a traditional public school for 9th grade are potential matches. Students in combination charter schools serving middle and high school grades who left their charter school in 9th grade are not available to match. If students that choose to exit the charter sector are systematically different from those that stay, we expect the restricted matching estimates to differ from the baseline matching estimates. Our restricted matching estimator is similar to the strategy in Tuttle et al. (2015). They compare KIPP high school students who were in a KIPP middle school to other KIPP middle school students who did not have the option to attend a KIPP high school.

Local Matching Estimators

It is also possible that charter schools locate in areas based on unobservable local or traditional public school characteristics that predict student achievement. All estimation results include lagged controls for the average characteristics of nearby schools in order to match students with comparable local environments; however, unobservable differences may still exist. We conduct local matching in which charter high school students are only allowed to match to students in one of the nearest five traditional public schools that are within 15 miles of the charter school. If the baseline comparison group is located in areas with different unobservable characteristics than the treatment group, we expect the local-matching estimates to differ from baseline. However, local matching may increase the likelihood that charter stayers are matched to charter leavers which is precisely what the restricted matching estimator in the previous section tries to avoid. Because of this, it is not clear whether divergent results from local matching are due to unobservable local characteristics or bias from matching to leavers who are systematically different from stayers.

Non-Local Matching Estimators

We also conduct non-local matching such that charter students in 9th grade can only be matched to students in traditional public schools that are not one of the five closest schools to the charter school or that are not within 15 miles of a charter school. Students in traditional public schools farther from charter schools may not have the option to attend, but still possess unobservable characteristics driving selection into charter schools and so be superior matches than local students that choose not to enter a charter high school or choose to exit their charter school. If students in local areas with charter schools that choose not to attend a charter high school are different in unobserved ways, we expect non-local matching estimates to differ from the baseline matching estimates. This is slightly different from the restricted matching estimator because the restricted estimator only restricts the potential matches for stayers (they can not be matched to leavers), while the non-local matching estimator also restricts the potential matches for entrants.

2.3.3 Heterogeneity by Student Characteristics

Often charter schools target certain student populations or specialize so it is possible that charter school effects vary by student characteristics. We explore heterogeneity based on student ethnicity, sex, limited English proficiency status, and disability status.

2.3.4 Heterogeneity by School Characteristics

We explore how treatment effects vary based on school characteristics along several dimensions. First, charter schools are more likely than traditional public schools to serve both middle and high school grade levels. About 87% of 9th grade charter school students are in combination schools compared to only 3% of traditional public school students. It is possible that these combination high schools are better for student outcomes because of the school structure. In order to explore this possibility, treatment is interacted with a dummy variable for whether a school is a combination high school or not, and the indicator is also included as a main effect. By including the combination indicator and the interaction, we are comparing charter school students in combination schools to traditional public school students in combination schools.

We also evaluate whether charter school effects are more positive the older the charter school by including an indicator for whether a school is more than 7 years old and the interaction of this indicator with treatment in the regressions. On average charter schools in North Carolina are younger than traditional public schools, so it is important to rule out the possibility that charter school effects are biased by the age of the school. In our sample, about 9% of 9th grade charter students are in schools strictly less than 8 years old, and about 7% of 9th grade traditional public school students are in school less than 8 years old. Finally, an indicator is constructed for whether a school has a higher proportion of white students than average which serves as a proxy for socio-economic status. This indicator variable and its interaction with treatment are included in regressions to assess whether charter school effects vary based on the demographic make-up of the school.

2.4 Results

2.4.1 Balance

Table 2.2 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 left panel shows the means and balance test for the entire sample before matching. The balance test is the normalized difference

$$\Delta_{tc} = 100 \times \frac{\bar{X}_t - \bar{X}_c}{\sqrt{\frac{s_t^2 + s_c^2}{2}}}. \tag{2.6}$$

The difference in treatment and comparison means ($\bar{X}_t - \bar{X}_c$) is standardized by the square-root of the average of the sample variances in the treatment and comparison groups.[6] Before matching, nearly all of the demographics and all of the pre-treatment achievement variables have a percent bias that is above the rule of thumb threshold of 10, indicating that the two groups are unbalanced. In particular, students who attend charter schools in 9th grade have higher 8th grade performance and are much more likely to have attended a charter school in 8th grade (72 percent of treatment students compared to just 1 percent of comparison students). The right panel of Table 2.2 shows the same means and balance test for the matched sample of students. An examination of this data reveals that all variables are well below the 10 percent bias threshold indicating that balance is achieved on all variables.

2.4.2 Main Results

Throughout the results section, tables are divided into achievement outcomes (test scores and GPA) and attainment outcomes (retention and graduation). Table 2.4 displays the results of the propensity score matching

[6]This is different from the t-statistic which divides each sample variance by the number of units in each sample. The t-statistic is sensitive to sample size, and we have a large sample. Therefore we may be able to detect very small differences in group means, but we are not interested in hypothesis tests concerning the difference in means. We are interested in assessing how similar the treatment and comparison samples are before and after matching.

analysis on the English 1 End of Course test score, the ACT composite score, and weighted final GPA. The first model for each outcome shows the coefficient on the 9th grade charter indicator with no control for whether the student attended a charter school in 8th grade. The second column shows the same 9th grade charter effect with an 8th grade charter attendance control included in the set of covariates. The third column for each outcome separates the treatment variable between 9th grade charter attendees who also attended a charter in 8th grade - stayers - and 9th grade charter attendees who did not attend a charter in 8th grade - entrants. 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 student entrants to students who attended a traditional public school in both 8th and 9th grade.

The left most outcome panel displays the effect of charter attendance in 9th grade on English 1 test scores. The first column shows a moderate positive impact of charter attendance of 0.11 standard deviations. Including the 8th grade charter control in the second model reduces the size of the effect by about two thirds, but the effect remains positive and statistically significant. When the effect is considered separately for charter entrants and charter stayers, the coefficients are both positive, but only the coefficient for stayers is statistically significant. Significant differences between stayers and entrants are marked with the letter a in the tables. The effect for entrants and the effect for stayers are not statistically different from one another. In the middle outcome panel, which shows the impact of 9th grade charter enrollment on ACT scores, coefficients are large and positive in all three models, although they are largest in the model that does not include a control for 8th grade charter attendance. There is no statistically significant difference between entrants and stayers for ACT scores. Turning to GPA in the third outcome panel, there is no effect in the first model, but once the control for 8th grade charter attendance is added in the second model, there is a statistically significant negative effect on GPA. When the treatment group is split between entrants and stayers, there is no statistically significant difference in the impact between treatment groups, and across all models the effect is quite small - less than a tenth of a quality point.

Table 2.5 shows the results of the propensity score analysis for 9th grade retention, high school graduation within four years, and high school graduation within five years. The left outcome panel displays the results of the three models for 9th grade retention. In the first two models, students who attended charter schools in 9th grade were retained at rates that were not statistically different from students who attended traditional public schools, and the point estimates are extremely small. However, there is a statistically significant positive effect on entrants, and this difference is statistically significantly different from stayers. The middle outcome panel shows the impact of charter school attendance in 9th grade on graduating from high school within four years. The combined indicator in the first two models shows no statistically significant impact of charter schools on graduation. However, when 9th grade charter school students are divided into entrants and stayers, there is a small but statistically significant negative effect on graduation for charter school entrants which is statistically different from stayers. The right outcome panel shows the results for graduating from high school within five years. In this panel, 9th grade charter school students do not significantly differ from students who attended traditional public schools. However, 9th grade charter school entrants and stayers have point estimates that are in different directions and are statistically significantly different from each other with charter entrants less likely to graduate within 5 years than stayers.

2.4.3 Robustness Check Results

In addition to the preferred model, we conducted analyses using three different modifications to the matched sample. The first set of models, which we term "restricted matching" models, restrict potential matches to not include students in a charter school in 8th grade that could have attended that same charter school in 9th grade, but chose to attend a traditional public school. The rationale for these models is that these "leavers" may differ in unobservable ways from stayers, while students who leave a charter school from 8th to 9th grade when they did not have the option to attend may be more similar to stayers. The second set of models, which we term "local matching" models, restricts potential matches for each charter school student to the five traditional public schools that are geographically closest (within 15 miles) to the charter school in which the treatment student is enrolled. The rationale for these models is that students who reside and attend school near one another experience a similar set of unmeasured local conditions. The third set of models, which we term "non-local matching" models, limits potential matches to students who attend traditional public schools that are not within the local matching group - that is schools that are not among the five closest to any charter high school. The rationale for these models is that students who attend schools that are not "local" to any charter high school do not have the option to select a charter high school and, therefore, may possess unmeasured characteristics that drive charter school selection but simply have no available option.

Across all outcomes presented in Table 2.6 and Table 2.7, the results for restricted and non-local matching (presented in the 2nd and 4th column of each outcome panel) produce results that are in the same direction and similar in magnitude to the results found in the preferred model (presented in the 1st column of each outcome panel), though in some cases statistical significance varies across models. The main divergence in findings comes from local matching presented in the third column of each outcome panel in Tables 2.6 and 2.7. Although ACT and GPA results are similar between the preferred estimates and local matching, there are differences in English 1, retention, and graduation. Interestingly, the differences are isolated to the stayer effects and not the entrant effects. In practice, balance in baseline covariates is not achieved with local matching so the somewhat divergent results are not surprising, and may be due to restricting stayer matches to students who chose to exit charter schools when they could have stayed. The similarity of the results across restricted and non-local matching approaches suggests that they are robust to variations in model selection. Our preferred estimates are from the unrestricted matching model because balance is strongest with this model, and because results from the more restrictive matching estimators are similar to the unrestricted results.

2.4.4 Heterogeneity by Student Characteristics Results

Table 2.8 and Table 2.9 display results by ethnicity for achievement and attainment outcomes, respectively. The first column in each outcome panel shows the marginal effects for each ethnic subgroup without controlling for 8th grade charter status. The estimates in the first row can be interpreted as the difference in the outcome variable between white charter students and white traditional public school students. The second column adds the charter 8th indicator, and the third column divides effects by entrant and stayer. The first estimate in the third column of each outcome panel can be interpreted as the difference in the dependent variable between white charter entrants and white traditional public school students not in a charter school in 8th grade.

In terms of achievement outcomes, effects do not differ by ethnicity with few exceptions. In column 8 of Table 2.8, Hispanic entrants relative to Hispanic traditional public school students not in a charter in 8th grade have statistically significantly lower ACT scores than white entrants relative to white traditional public school students not in a charter in 8th grade. Significant differences relative to white students are marked with the letter a, and effects significantly different from zero are starred. Additionally, the Hispanic entrant subgroup and other entrant subgroup have higher GPAs than white entrants. Attainment outcomes do vary by ethnicity. Black charter entrants are more likely to be retained and less likely to graduate within 4 or 5 years than white charter entrants. Other entrants and Hispanic entrants are more likely to graduate within 5 years than white entrants, and Hispanic stayers are more likely to graduate within 4 years than white stayers.

Effects by other student characteristics including sex, limited English proficiency, and disability are presented in Table 2.10 and Table 2.11 for achievement and attainment outcomes, respectively. Effects on male students are more positive than female students in terms of English 1 and ACT. However, male stayers have significantly lower GPAs than female stayers. Male stayers are also more likely to graduate on time than female stayers. In general, effects by limited English proficiency are not statistically significantly different. This is likely due to a lack of power because so few students are classified as limited English proficient. In general though, limited English proficient students experience similar or more positive effects across outcomes and are statistically significantly less likely to be retained in 9th grade. Finally, disabled charter entrants have higher English 1, ACT, and GPA than non-disabled charter entrants. This may reflect the selection of more able disabled students into charter schools. In other words, a simple disabled indicator may not adequately capture the range of learning disabilities. Effects on attainment measures do not differ significantly by disability status.

2.4.5 Heterogeneity by School Characteristics Results

Effects by school characteristics including combination schools, school age, and proportion white are included in Tables 2.12 and 2.13 for achievement and attainment outcomes, respectively. Marginal effects are presented for entrants and stayers by the school characteristics, and statistically significant differences at the 10% level between charter students in schools with the characteristic and those without are marked with the letter a. Recall that combination high schools are high schools that serve high school grades and some grade levels below high school which is very common among charter schools. Combination charter schools have negative effects on charter stayer ACT test scores and non-combination charter schools have positive effects on charter stayer ACT test scores. The difference is statistically significant. Additionally, combination charter schools relative to non-combination charter schools decrease the probability of retention and increase the probability of graduation for both charter entrants and stayers. Overall, this suggests that combination charter schools, which are more prevalent, improve student attainment more than non-combination charter schools. Older charter schools increase the likelihood charter stayers graduate within 4 years relative to younger charter schools. Across the other outcomes, effects are not statistically significantly different by school age. Finally, charter stayers in schools with above average proprotions of white students score significantly higher on tests and have higher GPAs than charter stayers in schools with below average proportions of white students. Furthermore, charter schools with higher proportions of white students decrease retention rates and increase graduation rates relative to charter schools with lower average proportions of white students.

2.5 Conclusion

The current evidence on the effectiveness of charter high schools is much smaller than elementary and middle school grades, and is limited by two main factors. First, the majority of prior high school evaluations focus on charter schools that follow the no-excuses model. Second, larger evaluations of high school charters rely on samples of students who started in a charter school in 8th grade. These two factors therefore limit what we know about the effectiveness of the vast majority of charter high schools. 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' short- and long-term outcomes.

The results of this paper build on the charter school literature in two ways. First, we used state-wide, longitudinal data to evaluate the effect of North Carolina charter high schools on students' achievement and attainment. While studies of no-excuse charter schools in a handful of urban districts report positive effects on achievement and attainment, less is known about the rest of the charter high school population. Using state-wide data for a diverse state, our work can speak to whether a broader range of charter schools also positively affect student outcomes. Similar to prior work, our results document the need to account for students 8th grade charter school status in high school evaluations. Our results also suggest a symmetry in charter school experiences for students new to charter schools in 9th grade and students who stayed in charter schools between 8th and 9th grade in terms of achievement outcomes and an asymmetry with respect to attainment outcomes. It is unclear whether this is true in prior studies which focused just on a sample of students who already attended charters in 8th grade.

Second, our paper incorporates a matching procedure which includes students who did and did not already enroll in a charter school in 8th grade. Our matching procedure includes 8th grade charter status as a predictor variable, and we report results with and without controlling for 8th grade status. 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 mixed picture on the effects of North Carolina's charter schools on students' high school outcomes. The results of our analysis show that students in charter schools, both stayers and entrants, experience small increases in ELA achievement and moderate increases in ACT achievement. Interestingly, although students are learning more, as measured by standardized test scores, we find stayers and entrants have slightly lower grades and entrants are 3 percentage points less likely to graduate in four years, on average, than their TPS peers. These results may suggest charter schools have higher standards, and students new to the higher standards (entrants) have a tougher time adjusting than students exposed to charter schools in 8th grade (stayers).

Our future work will build on these initial outcomes to include two- and four-year college attainment, voting behavior, and criminal behaviors. The results will help address an information gap in the charter high school literature, as well as help parents in North Carolina make more informed choices.

Table 2.1: Descriptive Statistics for Dependent Variables

	English 1 (Standardized)	ACT (Standardized)	GPA (Weighted)	Retained 9th Grade	Graduate 4 Years	Graduate 5 Years
Summary Statistics (Before Matching)						
Mean	0.05	0.00	3.08	0.09	0.77	0.79
Standard Deviation	0.97	1.00	0.95	0.29	0.42	0.41
Min	-4.04	-3.38	0.00	0.00	0.00	0.00
Max	3.16	3.48	6.00	1.00	1.00	1.00
Cohorts Available	2006-2012	2010-2012	2006-2012	2006-2012	2006-2012	2006-2011
Missing Count	70,169	67,667	224,223	0	33,934	28,869
Non-Missing Count	648,992	239,619	494,938	719,161	685,227	587,989
By Treatment (Before Matching)						
Treatment Mean	0.45*	0.56*	3.26	0.04*	0.87*	0.88*
Treatment Observations	8,889	4,690	5,910	9,496	9,017	7,106
Comparison Mean	0.05	-0.01	3.08	0.09	0.77	0.79
Comparison Observations	640,103	234,929	489,028	709,665	676,210	580,883
By Treatment (After Matching)						
Treatment Mean	0.48	0.58	3.26*	0.04	0.88	0.89
Treatment Observations	7,534	4,329	4,773	7,861	7,471	5,644
Comparison Mean	0.46	0.45	3.48	0.04	0.89	0.89
Comparison Observations	11,381	5,546	8,717	11,955	11,319	9,000

The sample includes pooled 9th grade cohorts from 2006-2012. Treatment students attended a charter school anytime during 9th grade. The sample is restricted to students observed in both 8th and 9th grade. Matching is based on Logit regression using up to 3 nearest neighbors within 0.01 of the treatment propensity score and includes as predictors all demographic characteristics, achievement variables, local characteristics, an indicator for charter 8th, and the interactions of charter 8th with demographic, achievement, and local characteristics. Stars indicate statistically significant differences between treatment and control groups at the 5% level.

Table 2.2: Descriptive Statistics for Treatment and Comparison Groups Before and After Matching

Variable	Before Matching Comparison Mean	Before Matching Treatment Mean	Before Matching %bias	After Matching Comparison Mean	After Matching Treatment Mean	After Matching %bias
Demographics						
Charter 8th	0.01	0.72	213.83	0.71	0.71	-0.44
Male	0.51	0.47	-6.63	0.46	0.47	1.82
Asian	0.02	0.02	-0.18	0.03	0.02	-2.97
Black	0.29	0.19	-22.66	0.20	0.19	-2.47
Hispanic	0.08	0.04	-19.45	0.04	0.04	-2.82
American Indian	0.01	0.01	-0.32	0.01	0.01	0.55
MultiRacial	0.03	0.02	-3.23	0.02	0.02	1.44
White	0.57	0.71	31.08	0.69	0.71	3.73
Limited English Proficient	0.04	0.01	-20.23	0.02	0.01	-4.54
Gifted	0.17	0.10	-20.74	0.11	0.10	-1.79
Disabled	0.10	0.09	-3.32	0.09	0.09	1.03
Econ Disadvantaged	0.45	0.16	-65.37	0.18	0.17	-2.73
Days Absent	8.13	6.40	-23.38	6.47	6.41	-1.00
Days Absent Squared	136.57	80.11	-15.66	86.09	80.45	-1.90
Middle School Mobility	0.20	0.32	27.83	0.32	0.32	1.29
Old for Grade	0.19	0.13	-17.13	0.13	0.13	0.25
Econ Disadvantaged * Disabled	0.06	0.02	-21.42	0.02	0.02	-1.28
Econ Disadvantaged * Gifted	0.03	0.01	-14.04	0.01	0.01	0.49
Achievement						
Failed Alg 1 in Middle School	0.01	0.04	15.69	0.04	0.03	-0.79
Passed Alg 1 in Middle School	0.20	0.39	42.80	0.40	0.39	-2.49
Standardized 8th Grade Math Score	0.00	0.27	27.01	0.28	0.27	-0.91
Standardized 8th Grade Reading Score	0.00	0.38	39.76	0.37	0.38	0.38
Lagged Local Characteristics						
Econ Disadvantaged Local	40.24	37.23	-23.90	38.19	37.56	-5.26
Short Term Suspensions Local	35.69	36.59	5.00	37.04	36.34	-3.98
Violent Acts Local	16.07	16.29	3.41	16.36	16.20	-2.56
Asian Pct Local	2.52	2.70	7.63	2.81	2.73	-3.71
Black Pct Local	31.41	32.54	6.31	33.40	32.49	-5.37
Hispanic Pct Local	7.35	7.69	7.54	7.97	7.64	-7.34
MultiRacial Pct Local	2.41	2.43	1.56	2.47	2.43	-3.46
American Indian Pct Local	1.29	0.52	-19.59	0.53	0.52	-0.27
White Pct Local	54.90	54.08	-4.39	52.79	54.15	7.74
PPE Local	8282.54	8191.39	-7.90	8243.90	8180.72	-6.18
Urban Local	0.26	0.24	-6.66	0.26	0.24	-3.67
Rural Local	0.32	0.27	-15.31	0.26	0.27	2.90
Suburb Local	0.12	0.09	-13.14	0.09	0.09	2.91
Town Local	0.12	0.11	-4.32	0.11	0.11	-0.04
Enrollment Local	1243.02	1348.55	21.53	1334.87	1358.17	4.60
Observations	624,174	7,978		11,955	7,861	

The sample includes pooled 9th grade cohorts from 2006-2012. Treatment students are students attending a charter school anytime during 9th grade. The sample is restricted to students observed in both 8th and 9th grade that did not repeat a grade. Standardized percent bias is calculated as 100 times the difference in sample means between the treated and comparison groups divided by the square root of the average of the sample variances of the treated and comparison groups. Standardized percent bias greater than 10 in absolute value is considered unbalanced. Matching is based on Logit regression using up to 3 nearest neighbors within 0.01 of the treatment propensity score and includes as predictors all demographic characteristics, achievement variables, local characteristics, an indicator for charter 8th, and the interactions of charter 8th with demographic, achievement, and local characteristics.

Table 2.3: Types of Student Moves Between 8th and 9th Grade

	Frequency	Percent
TPS 8th, TPS 9th	699,785	97.22
TPS 8th, CH HS 9th	1,245	0.17
TPS 8th, CH HS Combo 9th	1,777	0.25
CH Elem Combo 8th or Closing CH, TPS 9th	7,886	1.1
CH Elem Combo 8th or Closing CH, CH 9th	480	0.07
CH HS Combo 8th or CH Expanding to CH HS Combo, TPS 9th	1,994	0.28
CH HS Combo 8th or CH Expanding to CH HS Combo, CH 9th	6,663	0.93
Total	719,830	100

TPS stands for traditional public school, CH stands for charter school, and HS stands for high school. Elem Combo is defined as a school serving grades within Kindergarten to 8. HS Combo is defined as a school with a high grade at least 9 and low grade strictly below 9. The sample includes pooled 9th grade cohorts from 2006-2012. Treatment students are students attending a charter school anytime during 9th grade. The sample is restricted to students observed in both 8th and 9th grade that did not repeat a grade.

Table 2.4: High School Achievement Outcomes

	English 1 (Standardized)			ACT (Standardized)			GPA (Weighted)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Charter 9th	0.110***	0.032*		0.276***	0.114***		0.011	-0.090**	
	(0.017)	(0.016)		(0.025)	(0.024)		(0.048)	(0.044)	
Charter 9th (Entrants)			0.022			0.145***			-0.081*
			(0.023)			(0.035)			(0.044)
Charter 9th (Stayers)			0.036*			0.105***			-0.094*
			(0.019)			(0.030)			(0.052)
Charter 8th Control		Yes	Yes		Yes	Yes		Yes	Yes
Observations	28,722	18,915	18,915	15,987	9,875	9,875	21,291	13,490	13,490
R-Squared	0.673	0.697	0.697	0.699	0.721	0.722	0.552	0.582	0.582
Treated N	7639	7534	7534	4407	4329	4329	4870	4773	4773
Comparison N	21083	11381	11381	11580	5546	5546	16421	8717	8717

Charter 9th is defined as any student observed in a charter school in 9th grade. 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 OLS regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the entrant and stayer effects at the 10% level.

Table 2.5: High School Attainment Outcomes

	Retained 9th Grade			Graduate 4 Years			Graduate 5 Years		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Charter 9th	0.004	0.004		-0.005	-0.009		0.000	-0.005	
	(0.006)	(0.005)		(0.009)	(0.009)		(0.008)	(0.008)	
Charter 9th (Entrants)			0.016**			-0.037***			-0.021
			(0.007)[a]			(0.012)[a]			(0.013)[a]
Charter 9th (Stayers)			-0.000			0.003			0.002
			(0.005)			(0.009)			(0.009)
Charter 8th Control		Yes	Yes		Yes	Yes		Yes	Yes
Observations	29,879	19,816	19,816	28,314	18,790	18,790	21,466	14,644	14,644
Treated N	7967	7861	7861	7574	7471	7471	5720	5644	5644
Comparison N	21912	11955	11955	20740	11319	11319	15746	9000	9000

Charter 9th is defined as any student observed in a charter school in 9th grade. 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 Logit regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the entrant and stayer effects at the 10% level.

Table 2.6: High School Achievement Outcomes - Robustness Checks

	English 1 (Standardized)				ACT (Standardized)				GPA (Weighted)			
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Charter 9th (Entrants)	0.022	0.022	0.021	0.023	0.145***	0.145***	0.127***	0.156***	-0.081*	-0.081*	-0.084*	-0.083*
	(0.023)	(0.023)	(0.026)	(0.024)	(0.035)	(0.035)	(0.034)	(0.039)	(0.044)	(0.044)	(0.045)	(0.045)
Charter 9th (Stayers)	0.036*	0.035	-0.007	0.054**	0.105***	0.105***	0.109**	0.103***	-0.094*	-0.095*	-0.092	-0.117*
	(0.019)	(0.022)	(0.029)	(0.024)	(0.030)	(0.034)	(0.048)	(0.034)	(0.052)	(0.053)	(0.060)	(0.061)
Charter 8th Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restricted Matching		Yes				Yes				Yes		
Local Matching			Yes				Yes				Yes	
Non-Local Matching				Yes				Yes				Yes
Observations	18,915	17,841	11,206	17,281	9,875	9,255	5,851	8,971	13,490	12,642	8,214	12,130
R-Squared	0.697	0.700	0.695	0.699	0.722	0.733	0.721	0.729	0.582	0.592	0.580	0.592
Treated N	7534	7374	4631	7436	4329	4222	2696	4282	4773	4660	2853	4691
Comparison N	11381	10467	6575	9845	5546	5033	3155	4689	8717	7982	5361	7439

Charter 9th is defined as any student observed in a charter school in 9th grade. 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. For each outcome, results are displayed for various matching methods. Restricted implies that charter 8th students that exit for a TPS when they could have stayed in their 8th grade charter school for 9th grade are not available as possible matches. Local matching restricts potential matches to be from a TPS that is one of the closest 5 TPSs to a charter school and within 15 miles of a charter school. Non-local matching restricts potential matches to be from a TPS that is not one of the closest 5 TPSs to a charter school or not within 15 miles of a charter school. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and OLS regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the entrant and stayer effects at the 10% level.

Table 2.7: High School Attainment Outcomes - Robustness Checks

	Retained 9th Grade				Graduate 4 Years				Graduate 5 Years			
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Charter 9th (Entrants)	0.016**	0.016**	0.017**	0.021***	-0.037***	-0.037***	-0.037***	-0.040***	-0.021	-0.021	-0.024**	-0.021
	(0.007)^a	(0.007)^a	(0.007)	(0.007)^a	(0.012)^a	(0.012)^a	(0.011)	(0.012)^a	(0.013)^a	(0.013)^a	(0.011)	(0.013)^a
Charter 9th (Stayers)	-0.000	-0.004	0.014**	-0.001	0.003	0.004	-0.022*	0.005	0.002	0.005	-0.019	-0.001
	(0.005)	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.012)	(0.010)	(0.009)	(0.009)	(0.015)	(0.009)
Charter 8th Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restricted Matching		Yes				Yes				Yes		
Local Matching			Yes				Yes				Yes	
Non-Local Matching				Yes				Yes				Yes
Observations	19,816	18,694	11,618	18,116	18,790	17,730	11,121	17,166	14,644	13,882	8,605	13,400
Treated N	7861	7699	4788	7761	7471	7318	4579	7376	5644	5569	3365	5569
Comparison N	11955	10995	6830	10355	11319	10412	6542	9790	9000	8313	5240	7831

Charter 9th is defined as any student observed in a charter school in 9th grade. 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. For each outcome, results are displayed for various matching methods. Restricted implies that charter 8th students that exit for a TPS when they could have stayed in their 8th grade charter school for 9th grade are not available as possible matches. Local matching restricts potential matches to be from a TPS that is one of the closest 5 TPSs to a charter school and within 15 miles of a charter school. Non-local matching restricts potential matches to be from a TPS that is not one of the closest 5 TPSs to a charter school or not within 15 miles of a charter school. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and Logit regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the entrant and stayer effects at the 10% level.

Table 2.8: High School Achievement Outcomes - Student Ethnicity

	English 1 (Standardized)			ACT (Standardized)			GPA (Weighted)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Charter 9th White	0.106***	0.046**		0.291***	0.127***		0.015	-0.091	
	(0.019)	(0.021)		(0.023)	(0.027)		(0.054)	(0.059)	
Charter 9th Black	0.098***	0.060**		0.138***	0.108***		-0.029	-0.003	
	(0.024)	(0.023)		(0.039)[a]	(0.041)		(0.063)	(0.058)	
Charter 9th Hispanic	0.107**	0.047		0.234***	0.096		0.030	-0.029	
	(0.046)	(0.054)		(0.074)	(0.077)		(0.084)	(0.117)	
Charter 9th Other	0.121***	0.057		0.331***	0.150**		0.012	-0.065	
	(0.024)	(0.036)		(0.067)	(0.074)		(0.061)	(0.081)	
Charter 9th (Entrants) White			0.034			0.140***			-0.105**
			(0.026)			(0.030)			(0.047)
Charter 9th (Entrants) Black			0.035			0.117			-0.088
			(0.039)			(0.073)			(0.056)
Charter 9th (Entrants) Hispanic			0.002			0.051			0.082
			(0.069)			(0.070)[a]			(0.110)[a]
Charter 9th (Entrants) Other			0.061*			0.156			0.038
			(0.032)			(0.124)			(0.082)[a]
Charter 9th (Stayers) White			0.050*			0.124***			-0.085
			(0.026)			(0.033)			(0.070)
Charter 9th (Stayers) Black			0.073***			0.105**			0.038
			(0.028)			(0.050)			(0.070)
Charter 9th (Stayers) Hispanic			0.092			0.133			-0.140
			(0.072)			(0.128)			(0.205)
Charter 9th (Stayers) Other			0.054			0.145*			-0.161
			(0.062)			(0.084)			(0.131)
Charter 8th Control		Yes	Yes		Yes	Yes		Yes	Yes
Observations	30,020	18,653	18,653	16,641	9,723	9,723	22,374	13,572	13,572
R-Squared	0.674	0.690	0.690	0.704	0.724	0.724	0.560	0.593	0.594
Treated N	7982	7492	7492	4599	4363	4363	5134	4793	4793
Comparison N	22038	11161	11161	12042	5360	5360	17240	8779	8779

Charter 9th is defined as any student observed in a charter school in 9th grade. 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. Marginal treatment effects are calculated over ethnic groups which implies that coefficients can be interpreted as the difference between treatment and comparison students conditional on a particular ethnic group. For example, the first coefficient in column 3 is comparing white charter school students not in a charter school in 8th grade (entrants) to white traditional public school students not in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and OLS regressions, but the output is suppressed. Standard errors are clustered at the high school level. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the ethnic group relative to the white group at the 10% level.

Table 2.9: High School Attainment Outcomes - Student Ethnicity

	Retained 9th Grade			Graduate 4 Years			Graduate 5 Years		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Charter 9th White	-0.002	-0.003		-0.000	0.002		0.002	0.002	
	(0.003)	(0.004)		(0.009)	(0.010)		(0.009)	(0.010)	
Charter 9th Black	0.033**	0.027**		-0.050**	-0.026		-0.030*	-0.014	
	(0.016)^{a}	(0.013)^{a}		(0.021)^{a}	(0.019)		(0.017)^{a}	(0.018)	
Charter 9th Hispanic	-0.048***	-0.058***		0.023	0.055		0.030	0.077*	
	(0.016)^{a}	(0.022)^{a}		(0.029)	(0.040)		(0.028)	(0.041)^{a}	
Charter 9th Other	-0.004	-0.009		0.012	0.009		0.012	0.012	
	(0.007)	(0.011)		(0.017)	(0.025)		(0.019)	(0.030)	
Charter 9th (Entrants) White			0.008			-0.029**			-0.026**
			(0.006)			(0.013)			(0.013)
Charter 9th (Entrants) Black			0.053**			-0.093***			-0.064***
			(0.021)^{a}			(0.024)^{a}			(0.023)^{a}
Charter 9th (Entrants) Hispanic			-0.083***			0.008			0.074
			(0.028)^{a}			(0.040)			(0.051)^{a}
Charter 9th (Entrants) Other			-0.001			0.034			0.045
			(0.011)			(0.032)^{a}			(0.036)^{a}
Charter 9th (Stayers) White			-0.008*			0.014			0.013
			(0.004)			(0.011)			(0.012)
Charter 9th (Stayers) Black			0.014			0.009			0.014
			(0.012)			(0.024)			(0.024)
Charter 9th (Stayers) Hispanic			-0.033			0.104*			0.082
			(0.031)			(0.061)^{a}			(0.067)
Charter 9th (Stayers) Other			-0.017			-0.015			-0.017
			(0.022)			(0.033)			(0.038)
Charter 8th Control		Yes	Yes		Yes	Yes		Yes	Yes
Observations	31,306	19,634	19,634	29,724	18,601	18,601	22,896	14,703	14,703
R-Squared	0.133	0.150	0.151	0.181	0.191	0.193	0.167	0.181	0.182
Treated N	8409	7898	7898	7981	7495	7495	6127	5756	5756
Comparison N	22897	11736	11736	21743	11106	11106	16769	8947	8947

Charter 9th is defined as any student observed in a charter school in 9th grade. 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. Marginal treatment effects are calculated over ethnic groups which implies that coefficients can be interpreted as the difference between treatment and comparison students conditional on a particular ethnic group. For example, the first coefficient in column 3 is comparing white charter school students not in a charter school in 8th grade (entrants) to white traditional public school students not in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and Logit regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the ethnic group relative to the white group at the 10% level.

Table 2.10: High School Achievement Outcomes - Heterogeneity Based on Various Student Characteristics

	English 1 (Standardized)			ACT (Standardized)			GPA (Weighted)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Charter 9th (Entrants) & Not Demo	-0.004	0.020	0.012	0.104***	0.145***	0.126***	-0.093**	-0.077*	-0.099**
	(0.022)	(0.023)	(0.023)	(0.034)	(0.034)	(0.036)	(0.045)	(0.044)	(0.048)
Charter 9th (Entrants) & Demo	0.054*	0.134	0.118**	0.192***	0.076	0.319***	-0.054	-0.003	0.139**
	(0.031)[a]	(0.106)	(0.049)[a]	(0.046)[a]	(0.128)	(0.060)[a]	(0.049)	(0.180)	(0.064)[a]
Charter 9th (Stayers) & Not Demo	0.011	0.034*	0.032*	0.091***	0.103***	0.109***	-0.064	-0.094*	-0.097*
	(0.021)	(0.020)	(0.019)	(0.033)	(0.030)	(0.031)	(0.045)	(0.052)	(0.055)
Charter 9th (Stayers) & Demo	0.064***	0.204	0.073*	0.120***	0.298**	0.058	-0.129**	-0.117	-0.064
	(0.023)[a]	(0.127)	(0.043)	(0.035)	(0.123)	(0.069)	(0.066)[a]	(0.172)	(0.071)
Charter 8th Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo = Male	Yes			Yes			Yes		
Demo = Limited English Proficient		Yes			Yes			Yes	
Demo = Disability			Yes			Yes			Yes
Observations	18,915	18,915	18,915	9,875	9,875	9,875	13,490	13,490	13,490
R-Squared	0.697	0.697	0.697	0.722	0.722	0.722	0.582	0.582	0.583
Treated N	7534	7534	7534	4329	4329	4329	4773	4773	4773
Comparison N	11381	11381	11381	5546	5546	5546	8717	8717	8717

Charter 9th is defined as any student observed in a charter school in 9th grade. 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. Marginal treatment effects are calculated over various student demographic characteristics that vary by column (male, limited English proficient, and disability) which implies that coefficients can be interpreted as the difference between treatment and comparison students conditional on a particular characteristic. For example, the first coefficient in column 1 is comparing female charter school students not in a charter school in 8th grade (entrants) to female traditional public school students not in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and OLS regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the demo group relative to the not demo group at the 10% level.

Table 2.11: High School Attainment Outcomes - Heterogeneity Based on Various Student Characteristics

	Retained 9th Grade			Graduate 4 Years			Graduate 5 Years		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Charter 9th (Entrants) & Not Demo	0.010	0.017**	0.013**	-0.031**	-0.039***	-0.037***	-0.015	-0.022*	-0.022*
	(0.007)	(0.007)	(0.007)	(0.013)	(0.012)	(0.012)	(0.014)	(0.013)	(0.012)
Charter 9th (Entrants) & Demo	0.022**	-0.065**	0.037	-0.045***	0.048	-0.038	-0.027*	0.078	-0.010
	(0.010)	(0.030)[a]	(0.024)	(0.017)	(0.063)	(0.029)	(0.015)	(0.068)	(0.029)
Charter 9th (Stayers) & Not Demo	-0.003	0.002	0.000	-0.011	0.002	-0.001	-0.013	0.001	0.003
	(0.005)	(0.005)	(0.004)	(0.008)	(0.009)	(0.009)	(0.010)	(0.008)	(0.008)
Charter 9th (Stayers) & Demo	0.003	-0.194***	-0.005	0.018	0.068	0.044	0.018	0.075	-0.012
	(0.007)	(0.053)[a]	(0.020)	(0.013)[a]	(0.082)	(0.039)	(0.011)[a]	(0.104)	(0.032)
Charter 8th Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demo = Male	Yes			Yes			Yes		
Demo = Limited English Proficient		Yes			Yes			Yes	
Demo = Disability			Yes			Yes			Yes
Observations	19,685	19,685	19,685	18,790	18,790	18,790	14,644	14,644	14,644
Treated N	7802	7802	7802	7471	7471	7471	5644	5644	5644
Comparison N	11883	11883	11883	11319	11319	11319	9000	9000	9000

Charter 9th is defined as any student observed in a charter school in 9th grade. 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. Marginal treatment effects are calculated over various student demographic characteristics that vary by column (male, limited English proficient, and disability) which implies that coefficients can be interpreted as the difference between treatment and comparison students conditional on a particular characteristic. For example, the first coefficient in column 1 is comparing female charter school students not in a charter school in 8th grade (entrants) to female traditional public school students not in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and Logit regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the demo group relative to the not demo group at the 10% level.

Table 2.12: High School Achievement Outcomes - Heterogeneity Based on Various School Characteristics

	English 1 (Standardized)			ACT (Standardized)			GPA (Weighted)		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Charter 9th (Entrants) & Not Char	0.065**	0.035	0.036	0.203***	0.150*	0.329***	0.028	-0.142*	-0.086
	(0.028)	(0.045)	(0.076)	(0.070)	(0.078)	(0.106)	(0.063)	(0.082)	(0.089)
Charter 9th (Entrants) & Char	0.024	0.032	0.031	0.018	0.155***	0.139***	-0.145	-0.039	-0.074
	(0.042)	(0.024)	(0.029)	(0.075)	(0.040)	(0.047)	(0.093)	(0.049)	(0.063)
Charter 9th (Stayers) & Not Char	-0.023	0.030	0.040	0.141	0.045	0.014	-0.032	-0.286	-0.148*
	(0.088)	(0.038)	(0.030)	(0.097)	(0.074)	(0.069)	(0.082)	(0.193)	(0.080)
Charter 9th (Stayers) & Char	-0.026	0.070**	0.074**	-0.175**	0.112**	0.125**	0.022	0.005	-0.023
	(0.051)	(0.027)	(0.033)	(0.072)^{a}	(0.049)	(0.049)^{a}	(0.098)	(0.051)	(0.082)
Charter 8th Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Char = Combo	Yes			Yes			Yes		
Char = Old		Yes			Yes			Yes	
Char = High White			Yes			Yes			Yes
Observations	11,880	16,419	16,419	6,742	8,737	8,737	8,228	11,657	11,657
R-Squared	0.753	0.693	0.693	0.770	0.720	0.721	0.660	0.581	0.578
Treated N	6970	7426	7426	4302	4336	4336	4378	4681	4681
Comparison N	4910	8993	8993	2440	4401	4401	3850	6976	6976

Charter 9th is defined as any student observed in a charter school in 9th grade. 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. Combo indicates a school serves grade levels outside 9 through 12. Old indicates that a school is strictly more than 7 years old. High White indicates that a school serves a higher proportion of white students than the average public school in North Carolina. Marginal treatment effects are calculated over various school characteristics that vary by column (Combo, Old, High White) which implies that coefficients can be interpreted as the difference between treatment and comparison students conditional on a particular characteristic. For example, the first coefficient in column 1 is comparing non-combo 9th grade charter school students not in a charter school in 8th grade (entrants) to non-combo 9th grade traditional public school students not in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and OLS regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the char group relative to the not char group at the 10% level.

Table 2.13: High School Attainment Outcomes - Heterogeneity Based on Various School Characteristics

	Retained 9th Grade			Graduate 4 Years			Graduate 5 Years		
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Charter 9th (Entrants) & Not Char	0.006	0.023**	0.071*	-0.032	-0.076***	-0.100***	-0.028	-0.035**	-0.046
	(0.010)	(0.011)	(0.036)	(0.026)	(0.016)	(0.032)	(0.033)	(0.015)	(0.038)
Charter 9th (Entrants) & Char	-0.030	0.020**	0.014**	0.102***	-0.034***	-0.039***	0.108***	-0.026*	-0.028*
	(0.025)	(0.010)	(0.007)[a]	(0.032)[a]	(0.013)	(0.015)[a]	(0.035)[a]	(0.015)	(0.015)
Charter 9th (Stayers) & Not Char	-0.001	0.007	0.039**	0.002	-0.033*	-0.023	0.022	-0.032	-0.016
	(0.011)	(0.009)	(0.017)	(0.010)	(0.019)	(0.024)	(0.014)	(0.022)	(0.021)
Charter 9th (Stayers) & Char	-0.038**	-0.005	-0.015**	0.142***	0.014	0.015	0.109***	0.012	0.013
	(0.017)[a]	(0.009)	(0.007)[a]	(0.040)[a]	(0.012)[a]	(0.012)	(0.036)	(0.010)	(0.011)
Charter 8th Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Char = Combo	Yes			Yes			Yes		
Char = Old		Yes			Yes			Yes	
Char = High White			Yes			Yes			Yes
Observations	12,563	17,087	17,193	11,978	16,313	16,313	9,230	12,458	12,458
Treated N	7247	7694	7751	6888	7369	7369	5074	5510	5510
Comparison N	5316	9393	9442	5090	8944	8944	4156	6948	6948

Charter 9th is defined as any student observed in a charter school in 9th grade. 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. Combo indicates a school serves grade levels outside 9 through 12. Old indicates that a school is strictly more than 7 years old. High White indicates that a school serves a higher proportion of white students than the average public school in North Carolina. Marginal treatment effects are calculated over various school characteristics that vary by column (Combo, Old, High White) which implies that coefficients can be interpreted as the difference between treatment and comparison students conditional on a particular characteristic. For example, the first coefficient in column 1 is comparing non-combo 9th grade charter school students not in a charter school in 8th grade (entrants) to non-combo 9th grade traditional public school students not in a charter school in 8th grade. Demographic control variables, achievement control variables, and local characteristics are included as covariates in matching and Logit regressions, but the output is suppressed. Standard errors are clustered at the high school level. The letter a indicates a statistically significant difference between the char group relative to the not char group at the 10% level.

CHAPTER 3

THE EFFECTS OF CHARTER SCHOOLS ON TRADITIONAL PUBLIC SCHOOL STUDENTS IN NORTH CAROLINA

3.1 Introduction

Charter schools are privately run public schools with open enrollment policies, and have significant flexibility in the use of funding, hiring practices, and curriculum choice compared to traditional public schools. The charter school model is based on the principle that schools should be held accountable for student success while having the freedom to innovate and adjust learning environments. National growth in the charter sector, since its inception in 1992, to over seven percent of all public schools in 2016 has raised concerns over the students left behind, particularly disadvantaged students. Proponents of charter schools often argue that increased competition will induce traditional public schools to increase productivity while opponents worry that charters may drain traditional public school resources and talent. Implicit in these arguments is that effects on traditional public school students may depend on the relative achievement of charter and traditional public schools, which has largely been ignored in the literature. In North Carolina, between 1997 and 2005 (period 1), charter schools enrolled lower-achieving students relative to nearby traditional public schools, but between 2006 and 2016 (period 2) enrolled more white and higher-achieving students, which supports concerns over disadvantaged students left behind. In order to understand the effects of this new generation of charter schools, this paper evaluates whether competitive effects differ by the relative achievement of charter and traditional public schools, and whether disadvantaged students are differentially affected.

Competitive pressure is an often cited mechanism through which charter schools can affect traditional public school students, and it stems from two sources. First, students lost to charter schools decrease traditional public school district revenue because state and local per pupil allotments are transferred from districts to charters. Lost revenue may result in staffing reductions, program cuts, or school shutdown which implies a financial incentive for traditional public schools that are not overcrowded to keep students. Overcrowded schools might not face the same financial incentive, and possibly benefit from the opening of charter schools that save districts the cost of a new school.

Second, charter schools may skim higher-performing students or teachers from traditional public schools.

This might increase difficulty in attaining desired achievement levels or meeting accountability standards which implies an academic incentive for traditional public schools to retain talented teachers and students. This incentive may be stronger for schools facing competition from charter schools with relatively higher average lagged student test scores that may recruit high-quality teachers and students from traditional public schools. In response to these competitive pressures, traditional public schools can possibly re-allocate resources, cut inefficient teachers or programs, induce staff to increase productivity, or improve school-family connections.[1] In turn, these steps might have positive effects on student achievement in traditional public schools.

Even absent a response to competitive pressure, traditional public school students might be affected by charter schools through a re-allocation of students, teachers, and funding. If higher or lower-achieving students sort into charter schools, there may be negative or positive peer effects on students in traditional public schools. Similarly, if higher or lower-performing teachers sort into charter schools, there may be negative or positive teacher effects on students in traditional public schools.[2] These effects may be exacerbated by higher-achieving charters if they have recruited higher-performing students and teachers from traditional public schools. Finally, the financial effect is ambiguous depending on the overcrowdedness of the district. Because of these different mechanisms, the overall competitive effect is an empirical question.

There are two primary challenges when estimating the effects of charter schools on students in traditional public schools. First, competitive effects may be confounded by student selection. For example, in a school level analysis, if higher-achieving students select into charter schools, average traditional public school achievement goes down regardless of any competitive effects.[3] Second, charter school location may be based on unobservable traditional public school characteristics that are also related to student achievement. For example, charter schools may locate near high-quality traditional public schools. Even if there is no competitive effect, a failure to account for endogenous locational decisions creates upwardly biased estimates because traditional public school quality is positively correlated with proximity to a charter school and student achievement.

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[1]Ericson et al. (2001) survey district administrators in a non-random sample of 49 districts across 5 states in 1999 and find, in response to charters, some districts form specialty schools, implement gifted or at-risk programs, after school programs, specialized curriculum, or parental involvement activities. Administrators report tracking students that leave for charters and comparing student achievement with charters. Zimmer and Buddin (2009) survey traditional public school principals in California in 2002 and find that 20% of principals who indicate students in their local attendance area attend a charter change at least one of the following: teacher compensation, teacher hiring/firing/discipline policies, curriculum, instructional practices, or professional development.

[2]Carruthers (2012) and Jackson (2012) find that teachers who move from traditional public to charter schools in North Carolina are less effective in terms of value added and have weaker credentials.

[3]Researchers are primarily concerned with the effect of charter schools on students in traditional public schools (TPS), but the effect on average TPS achievement is not without policy relevance. For example, state accountability standards are not adjusted based on the competitive environment. Since 2014 in North Carolina, schools have been given a performance grade that is based on school achievement (80%) and student growth (20%). Since the grade is largely based on achievement, a loss of high-achieving students to charters has a negative effect on the performance grade even if the TPS remains just as efficient in terms of growth.

Empirically, I model student test score gains as a function of competition and control for student-school spell fixed effects in order to account for the most likely contributors to the endogeneity of measures of competition.[4] Spell fixed effects account for student selection based on time-invariant student or family characteristics which is preferable to studies utilizing alternative strategies relying on school-level data, which may suffer from student selection bias (Holmes et al. 2003; Hoxby 2003; Bettinger 2005; Ni 2009). Spell fixed effects also account for the non-random location of charter schools based on time-invariant characteristics.

I contribute to the existing literature in several ways. First, I assess whether effects vary based on the relative achievement of charter and traditional public schools. Cremata and Raymond (2014) and Cordes (2018) allow effects to vary based on charter school achievement, but do not measure achievement relative to competing traditional public schools. Relative achievement is central because the degree of competitive pressure and allocative effects depend on the relationship between a traditional public school and competing charter schools. Additionally, both papers focus on large urban centers which may limit external validity. Second, I contribute to studies of charter schools in North Carolina by considering the new generation of charters which differ substantially from those that have been studied, given the shift toward higher-achieving students (Holmes et al. 2003; Bifulco and Ladd 2006; Mehta 2017; Jinnai 2014; Gao and Semykina 2017).

Third, I implement two strategies to test whether spell fixed effects alleviate endogeneity concerns. The first strategy includes school-specific linear trends in estimation to allow for the possibility that charter schools locate based on underlying trends in achievement. This adds to the literature that employs spell fixed effect models but does not control for trends (Sass 2006; Bifulco and Ladd 2006; Booker et al. 2008; Zimmer et al. 2009; Zimmer and Buddin 2009; Winters 2012; Jinnai 2014). The second strategy exploits the timing of the charter school applications and uses lagged school achievement and other lagged school characteristics to capture relevant shocks to traditional public schools at the time of application. The timing and arduous nature of the application process make it likely that charter schools locate based on persistent unobservable characteristics, but locating based on time-varying shocks is possible. These two strategies are particularly useful because instrumental variables that are robust to time-varying unobservable confounders are difficult to obtain, and my evidence suggests that panel models may be sufficient.[5] Fourth, I bound competitive estimates by estimating effects over a range of persistence parameter values between zero and one. This relaxes the implausible assumption often

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[4]A student-school spell fixed effect is defined as the set of observations for a student while at a particular school. Spell fixed effects are an alternative to controlling separately for school and student fixed effects. For example, demeaning with respect to a spell fixed effect is an alternative to explicitly including hundreds of school dummy variables in a regression and demeaning with respect to student fixed effects.

[5]Several papers utilize instrumental variable estimation in order to account for selection on time-varying unobservables (Bettinger 2005; Imberman 2011b; Gao and Semykina 2017).

maintained in the literature that what a student learned in the previous year is fully retained in the current year.[6][7] If this assumption fails and lagged achievement is correlated with competition, estimates will be biased. I also utilize dynamic panel models that estimate the persistence parameter as a comparison to my main results.

Results show that charter schools have no economically significant effect on math or reading test scores. However, charter schools with higher average lagged student achievement than traditional public schools have small positive effects on traditional public school students while lower-achieving charter competition has zero to small negative effects. This indicates that differences in relative charter and traditional public school achievement across settings may partially explain why the literature has found divergent results. Higher-achieving competition differentially benefits Hispanic and economically disadvantaged students in math relative to their counterparts, which reduces achievement gaps. Lower-achieving competition differentially hurts Hispanic, economically disadvantaged, limited English proficient, and disabled students in reading relative to their counterparts, which augments achievement gaps. This suggests that, contrary to the concerns of charter school critics, charter competition and even higher-achieving charter competition does not negatively affect traditional public school students. Estimates are insensitive to the inclusion of school trends, lagged school achievement and other lagged school characteristics, which suggests that spell fixed effects may be adequate to control for endogeneity concerns. Estimates are sensitive to the persistence assumption with effects weakening in magnitude with lower persistence; however, the sign and significance of effects is almost always preserved between high and low persistence values. Additionally, dynamic panel estimation generally yields persistence values greater than 0.5, and competitive effect estimates largely reflect the results from my main estimation strategy.

The rest of the paper proceeds as follows. Section 3.2 describes the construction of the competition measures and summarizes the data. Section 3.3 discusses the empirical model, endogeneity concerns, and robustness checks. Section 3.4 provides the main empirical results and results of the robustness checks. Section 3.5 concludes.

3.2 Data

The North Carolina Department of Public Instruction (NCDPI) collects administrative data on all students in North Carolina public schools, and this information is made available through the North Carolina Education Research Data Center (NCERDC). Individual students are linked across years and linked to teachers and schools. The data contains student demographic information including sex, race, limited English proficiency, economically disadvantaged status, disability status, and academically gifted status. In addition, North Carolina

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[6]Zimmer and Buddin (2009) assume the opposite extreme - that there is no persistence in test scores.

[7]Booker et al. (2008) acknowledge these concerns.

requires end of grade exams in grades 3 through 8 for all students in math and reading. Test scores are reported in developmental scale scores and are designed to measure a student's growth in math and reading comprehension as he or she progresses through school. In order to account for differential scales across years and grades, and to ease interpretation, math and reading test scores are standardized by grade-year.[8] School level data files from NCDPI contain latitudes and longitudes for charter and traditional public schools in North Carolina. I link each traditional public school to every charter school and calculate distances between each pair to create distance based measures of competition.[9]

3.2.1 Time Periods

I consider two time periods: period 1 (1997-2005) and period 2 (2006-2016). The cut-off year is chosen as 2006 because the majority of previous studies in North Carolina are restricted to time periods before 2006. Additionally, charter schools in period 1 differ substantially in size and student composition from those in period 2. This relates to competitive mechanisms such as the degree of competitive pressure a traditional public school faces and allocative effects resulting from non-random teacher and student sorting between schools. Therefore, I extend the literature examining North Carolina charter schools by analyzing this new generation of charter schools in period 2.

3.2.2 Measures of Competition

Attrition Measures

One of the main challenges facing an empirical analysis of competitive effects is identifying reliable measures of which traditional public schools are competing with charter schools. Perhaps the strongest measure is the attrition rate to charter schools. This measure of competition is less common in the literature, but is used by Cremata and Raymond (2014) and Winters (2012). I define the traditional public school attrition rate as the number of students that non-structurally switch from the traditional public school to any charter school before the begining of the current year divided by the traditional public school's prior enrollment. The analysis is restricted to schools serving grades 3 through 8 so attrition rates are restricted to student moves between those grades. I define non-structural switchers as students that switch schools when they could have stayed an additional year at their prior school.[10] In order to account for potential non-linearities in competitive effects, the main attrition

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[8] I conduct standardization by subtracting the average score for a particular grade-year and dividing by the standard deviation of scores in that grade-year. Standardized test scores within grade-year have a mean of zero and standard deviation of one. Charter school students are included in the standardization.

[9] See Appendix A for more details on the geocoding process.

[10] Some students move residence which implies a traditional public school may be counted as competing with a charter school many miles away. In order to limit this problem, attrition rates are only calculated based on students that switch schools within 20 miles of one another. This restriction excludes 5% of non-structural student moves from traditional public schools to charter schools.

competition measures are dummy variables for attrition rates strictly greater than 0 and less than or equal to 1%, and attrition rates strictly greater than 1%.[11] This also allows effects to vary based on the degree of competition. I define school-year observations with zero percent attrition as not competing, and this is the omitted category in the analysis. Table 3.1 defines the main set of competition measures used throughout this paper.

Distance Measures

In addition to attrition measures of competition, I construct several distance based measures that count the number of charter schools within a certain radius of a traditional public school. Distance based measures of competition must strike a balance; a larger distance band may include schools that are not competing in the treatment group, and a small band may exclude some schools that do compete. Overall, 75% of non-structural student moves from traditional public to charter schools are less than 10 miles apart which implies schools farther than 10 miles from a charter are unlikely to face a significant degree of competition. Based on this information, I construct distance measures of competition using a maximum 10 mile radius.

There is an additional complication because students switching to charters in urban areas travel smaller distances than in rural areas. The first row of Table 3.2 shows that in urban areas the median distance between schools for students making non-structural moves from a traditional public school to a charter school is 4.25 miles while in rural areas it is 7.81. In order to account for this, I add the restriction that competing traditional public schools be one of the closest 10 traditional public schools to a charter. In practice, this allows the distance band defining which schools compete to be flexible based on the density of schools in an area.[12] The fifth row of Table 3.2 shows the average distance of charter schools to the 10th nearest traditional public school by urbanicity. The table shows that, on average, the 10th closest traditional public school to a charter in urban areas is 4.4 miles, and in rural areas is 11.7 miles. With this in mind, I define distance based competition as an indicator that is one if a traditional public school is within 10 miles of a charter school and is one of the 10 nearest traditional public schools to a charter.[13] Several papers employ distance based measures of competition, but do not allow distance bands to vary based on urbanicity (Bifulco and Ladd 2006; Booker et al. 2008; Sass

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[11] This cutoff point is chosen to allow effects to vary based on the degree of competition while leaving enough observations in the high attrition category to detect effects. Among traditional public schools facing attrition, 75% have attrition rates below 1%, and 25% have attrition rates above 1%.

[12] Certain charter schools in urban areas are nearby a large number of traditional public schools. For example, one charter school in an urban area in period 2 is within 5 miles of 69 traditional public schools. Allowing all of these schools into the treatment group would give a large amount of weight to the effects of this one charter school.

[13] There is an added complexity because traditional public schools (TPSs) open and close. Then a TPS could move in and out of treatment based on the closing and opening of other TPSs that are closer to the charter. In order to account for this, all restricted measures of competition have the added restriction that if a TPS is ever one of the nearest 10 TPSs to a charter within a period, it is always counted as one of the nearest 10 in that period.

2006).

Attrition vs Distance Measures

I choose attrition measures of competition as the main competition measures because they are more likely to correctly identify which schools are competing, and because they do not rely on arbitrary distance bands and other restrictions as the distance measures do. However, traditional public schools not facing attrition, but nearby charter schools, may still be affected if the threat of competition is enough to induce a competitive response. Additionally, the impact of charter schools on traditional public school attrition may be large upon charter entry and then taper in later years. This may result in attrition measures not considering a traditional public school as competing even though it is still nearby a charter school. For these reasons, I show results using distance based measures as a check. Additionally, specifications using attrition based measures of competition also control for traditional public schools within 10 miles of a charter but not facing attrition as a check for whether distance measures capture effects that attrition measures do not.

Relative Achievement Measures

One purpose of this paper is to assess whether competitive effects depend on the relative achievement of traditional public and charter schools. In order to test this hypothesis, I create variables for both attrition and distance competition measures to indicate if competing charter schools have higher average achievement than a traditional public school. In the case of attrition, I define a traditional public school as competing with higher-achieving charter schools if the average lagged standardized test scores of students in charter schools to which the traditional public school students have attrited is greater than the average lagged standardized test scores of students in the traditional public school.[14] The charter school average is weighted by the number of attriters so that charter schools competing more heavily with a traditional public school are given more weight. I construct the relative achievement measures separately for math and reading. When math test scores are the dependent variable of interest, I use the relative math achievement measures to define relative achievement. When reading test scores are the dependent variable of interest, I use the relative reading achievement measures to define relative achievement. I use a similar process for distance based measures except the charter school average is taken over charter schools within a certain distance and is weighted by charter school membership.

[14] I use the lagged test scores of current students instead of the lagged school average because new charter schools do not have a lagged school average. A portion of the identifying variation in the analysis comes from entering charter schools so it is useful to have a measure of relative achievement that is available for traditional public schools competing with entering charter schools. This implies that, for new charter schools, the measure of achievement is based solely on student test scores while at a different school.

3.2.3 Summary Statistics

Summary statistics are computed for period 1 and period 2 separately and are displayed in Panel A and B of Table 3.3, respectively. Math and reading test scores are only available for grades 3 through 8, so the sample is restricted to students in those grade levels. Columns 1 and 4 include traditional public schools that do not face attrition to charter schools, columns 2 and 5 include traditional public schools that face attrition to charter schools, and columns 3 and 6 include charter schools. Significant differences between groups within each period are starred at the $p < .05$ level and are always relative to the attrition group (within panel comparisons). Significant differences between the same group across time periods are lettered at the $p < .05$ level in Panel A (between panel comparisons). The top half of the table weights statistics by school membership and the bottom half does not.

In period 1 and period 2, schools not facing attrition do not statistically significantly differ in terms of math and reading test scores compared to schools facing attrition. In both periods, schools facing attrition have higher proportions of black, Hispanic, and gifted students, but lower proportions of white students than schools not facing attrition. Additionally, traditional public schools facing attrition have higher enrollment and are more likely to be urban middle schools than traditional public schools not facing attrition. This indicates that schools facing attrition and those not differ along several observable dimensions possibly correlated with achievement, which implies they should be controlled in the analysis.

The characteristics of charter schools relative to traditional public schools facing attrition in each period are more striking. Period 1 charters have lower math and reading test scores than competing traditional public schools, but period 2 charters have higher math and reading scores. In terms of ethnicity, period 1 charters relative to competing traditional public schools are relatively balanced although Hispanic students are under-represented in charters. Period 2 charters are significantly more likely to be white and less likely to be Hispanic than competing traditional public schools. In both periods, charters have lower proportions of limited English proficient students, and period 2 charters have lower proportions of disabled students than competing traditional public schools.[15] Overall, charter schools have lower enrollment than competing traditional public schools and are more likely to offer grade levels spanning combinations of elementary, middle, and high school grades.

So far I have made comparisons within Panel A and Panel B of Table 3.3, but it is also useful to compare charter schools across time periods (columns 3 and 6). Significant differences between time periods for charter schools are marked with the letter, a, in Panel A. Period 1 charters have significantly lower test scores in math

[15]There is some concern about charter school reporting of academically gifted and free lunch eligible students so these measures should not be compared between traditional public and charter schools.

and reading than period 2 charters. They also have lower proportions of Hispanic and limited English proficient students. Urbanicity does not differ between periods, but charters in period 1 are significantly smaller than those in period 2.[16]

In order to further explore how the relative achievement of charter schools has shifted over time, Table 3.4 gives the proportion of traditional public schools that are competing with lower or higher-achieving charters for both math and reading by period. The first row shows that 32% of period 1 traditional public schools facing attrition have lower average lagged student math scores than charter schools to which the traditional public school students attrite. By period 2, 55% have lower achievement than competing charters. A similar pattern emerges in reading with 49% of traditional public schools facing competition from higher-achieving charter schools in period 1, and 66% in period 2. This indicates that, in both math and reading, traditional public schools are more likely to face higher-achieving competition in period 2 than in period 1. If competitive effects depend on relative achievement, this shift may result in differential effects between periods.

3.3 Empirical Strategy

3.3.1 Empirical Model

I use a value added model of student test scores in math and reading to estimate the effects of charter schools on traditional public school student achievement. Current traditional public school student test scores are modeled as a function of lagged test scores which serve as a proxy for unobserved historical school and family inputs for a student.[17] The lagged value added model of achievement is given by

$$q_{ijt} = \beta_0 + \lambda q_{ij,t-1} + \boldsymbol{c}_{jt}\boldsymbol{\beta}_1 + \boldsymbol{w}_{ijt}\boldsymbol{\beta}_2 + \boldsymbol{x}_{j,t-1}\boldsymbol{\beta}_3 + \mu_i + \gamma_j + \theta_t + \phi_j t + \epsilon_{ijt}, \tag{3.1}$$

where q_{ijt} is either standardized math or reading test score for student i in school j at time t. The vector $\boldsymbol{c}_{jt}$ includes measures of competition, $\boldsymbol{w}_{ijt}$ is a vector of contemporaneous student and school-level control variables, $\boldsymbol{x}_{j,t-1}$ is a vector of lagged school-level control variables, μ_i is student unobserved heterogeneity, γ_j is school unobserved heterogeneity, θ_t is year-grade unobserved shocks, $\phi_j t$ is a school specific linear time trend, and ϵ_{ijt} is an idiosyncratic error. I assume that measures of competition $\boldsymbol{c}_{jt}$ may be correlated with both student and school heterogeneity.

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[16]This is because charter schools that opened in period 1 grew significantly in terms of enrollment by period 2, and because the initial enrollment of opening charter schools is much higher in period 2 than in period 1.

[17]See Todd and Wolpin (2003) for a detailed derivation. In order for lagged test scores to serve as an appropriate proxy, it is assumed that the education production function does not change over time, that the effect of inputs declines as the distance in time from current achievement increases, and that the rate of decline is uniform across inputs.

Equation 3.1 can not be consistently estimated using fixed effect or first difference estimation to remove student heterogeneity without further assumptions. For example, fixed effect estimation, depending on the approach, implicitly or explicitly time demeans all variables with respect to each cross-section. The new time demeaned error ($\epsilon_{ijt} - \bar{\epsilon}_i$) is correlated with time demeaned lagged achievement ($q_{ij,t-1} - \bar{q}_i$) because $\bar{\epsilon}_i$ is a function of $\epsilon_{ij,t-1}$ which is correlated with $q_{ij,t-1}$ by construction. Additionally, $\bar{q}_i$ is a function of q_{ijt} for all t and so is correlated with both ϵ_{ijt} and $\bar{\epsilon}_i$. In order to avoid this problem, I assume that persistence in achievement is one ($\lambda = 1$) and write a gains-score model as

$$\Delta q_{ijt} = \beta_0 + \boldsymbol{c}_{jt}\boldsymbol{\beta}_1 + \boldsymbol{w}_{ijt}\boldsymbol{\beta}_2 + \boldsymbol{x}_{j,t-1}\boldsymbol{\beta}_3 + \mu_i + \gamma_j + \theta_t + \phi_j t + \epsilon_{ijt}. \tag{3.2}$$

Without lagged achievement on the right hand side, standard fixed effect or first difference estimation can be applied to remove student heterogeneity. Controlling for student and school fixed effects addresses many factors that may bias estimates of the effect of competition on student test scores. The primary endogeneity concern comes from the non-random location of charter schools that may be correlated with unobserved traditional public school characteristics that are also correlated with achievement. I include school fixed effects to control for any fixed unobservable school characteristics correlated with charter school location. This removes bias from fixed unobservables such as charter location decisions made based on relatively constant factors such as urbanicity or local school quality. Of secondary concern is that student school choice decisions may be based on unobservable student characteristics that create a correlation between proximity to a charter school and these unobservables. Student fixed effects control for any constant factors such as student ability or family characteristics that may be correlated with competition.

I include time-varying control variables to account for factors at the student and school level that may be correlated with student achievement and competition. The contemporaneous control variables are an indicator for being within 5 miles of a closing traditional public school, being within 5 miles of an opening traditional public school, whether the school has increased its grade span, and whether a school has decreased its grade span. I include these controls because charter school location may be correlated with restructuring in local school districts, and shifting resources, teachers, and students may affect student achievement. Additional contemporaneous control variables include an indicator for cohort student moves between schools, non-cohort moves, and grade repetition. Charter school location may be correlated with student turnover in a local area and switching schools may be associated with a drop in test scores.[18] A cohort move is defined as a student switching schools

[18] Contemporaneous control variables may be viewed as mediators depending on the extent to which charter schools cause them.

when at least 15% of his or her cohort in the previous school also made the same move. All other student moves between schools are considered non-cohort. The one period lagged control variables are school enrollment, proportions of disabled, limited English proficient, gifted, white, black, and Hispanic students which are meant to control for school characteristics that are possibly correlated with charter school location and achievement. A complete list of control variables with definitions is presented in Table 3.5.

Estimation of equation 3.2 is problematic because student effects, school effects, and linear trends enter the model separately. One possibility is to time demean the model with respect to student effects and explicitly include school dummy variables and school dummy variables interacted with a linear time trend. This approach is computationally infeasible because it requires the inclusion of thousands of covariates. Instead, I define student-school spell effects as $\psi_{ij} = \mu_i + \lambda_j$. In the dummy variable context, a spell fixed effect could be eliminated using the interaction of school and student dummy variables. First differencing or time demeaning with respect to student-school spells greatly simplifies estimation. The use of spell fixed effects provides a consistent estimator under strict exogeneity but is less efficient than including student and school effects separately.[19] Because of the large sample size, loss of precision is less of a concern, and in practice I do not have a problem detecting effects. In order to additionally control for linear school trends, I first difference equation 3.2 with respect to spell effects. This eliminates student and school heterogeneity, and because the first difference is with respect to the student-school spell effect, linear trends reduce to school effects ($\phi_j t - \phi_j(t-1) = \phi_j$).

$$\Delta\Delta q_{ijt} = \Delta \boldsymbol{c}_{jt}\boldsymbol{\beta}_1 + \Delta \boldsymbol{w}_{ijt}\boldsymbol{\beta}_2 + \Delta \boldsymbol{x}_{j,t-1}\boldsymbol{\beta}_3 + \Delta\theta_t + \phi_j + \Delta\epsilon_{ijt}. \tag{3.3}$$

Consistent estimation of equation 3.3 using pooled ordinary least squares (POLS) requires that $\boldsymbol{c}_{jt}$ is uncorrelated with $\epsilon_{ij,t-1}$, ϵ_{ijt}, and $\epsilon_{ij,t+1}$. I estimate whether results are robust to the inclusion of school trends, but I report the main set of results using POLS on equation 3.3 omitting ϕ_j, and explicitly including the first differenced grade-year dummy variables to eliminate $\Delta\theta_t$. In order to estimate equation 3.3 with trends, I estimate a school fixed effect regression to remove ϕ_j. I am interested in how effects differ between periods so in practice all control variables are interacted with a dummy variable for period 2, and school time trends are allowed to vary by period. Finally, standard errors are clustered at the school level to account for correlation in error terms

For example, other school openings and closings, changing grade spans, or turnover could, to a certain degree, be caused by charter schools and so these variables could be mediators. I am more concerned that charter schools choose to locate in areas with higher turnover, closings, etc and so view these variables as confounds. I have purposely excluded school inputs as contemporaneous covariates because these may respond to competition. For example, I exclude peer composition because charter schools may cause a shift in peer composition and so peer characteristics mediate the charter school effect.

[19] See Abowd et al. (1999) for a more detailed discussion.

within schools.

Equation 3.3 makes it clear that only students with 2 consecutive non-missing test score gains will be included in the estimation sample. Additionally, because first differences are taken with respect to student-school spells, only two consecutive gains in the same school are valid. Between 1997 and 2016 about 4 million observations meet this criteria. The majority of traditional public schools in North Carolina follow traditional grade spans with elementary (K through 5) and middle (6 through 8). A student in 6th grade in a middle school still has a test score gain between 5th and 6th grade even if 5th grade was at a different school because I construct test score gains without considering which school a student attends.[20] This implies that the majority of students in the estimation sample are in 5th grade, 7th grade, and 8th grade (about 95%) because these students potentially have two test score gains while in the same school. Some 4th and 6th graders are also included in the estimation sample because of grade repetition or because some traditional public schools serve non-traditional grade spans with both elementary and middle grade levels.

Equation 3.3 also makes clear that identification of competitive effects relies on variation in $\boldsymbol{c}_{jt}$ within a student-school spell. Attrition and distance measures of competition may vary based on the opening, closing, expanding, or moving of charter schools. For attrition, identification relies on students in schools facing attrition in one period and not in another. About 30% of students with two valid test score gains in the estimation sample experience variation between no attrition, 0-1% attrition, or $>$1% attrition. Identification does not come from students moving from an area without a charter to an area with a charter because first differences are with respect to a student while at a particular school. Identification of the effect of higher-achieving competition relative to lower-achieving competition comes from changes in relative achievement within a student-school over time even if the level of attrition does not vary. In other words, school level attrition can be constant and the difference in competitive effects between higher and lower-achieving competition is still identified as long as relative achievement changes over time.

3.3.2 Persistence

The perfect persistence assumption is unlikely to hold in student achievement because what a student learned in the previous year is unlikely to be fully retained in the current year. If competition is correlated with lagged achievement and λ is not one, the estimator for $\boldsymbol{\beta}_1$ will be inconsistent because a portion of lagged achievement is effectively left in the error term.[21] In order to account for this, I utilize persistence parameters in increments

[20] This is why controls are included in the model for student moves between schools. If student test score gains were constructed only using observations for a student while at a particular school, there would be no need to control for student moves.

[21] If persistence is not one, imposing perfect persistence in a gain-score model leaves $(\lambda - 1)$ proportion of lagged achievement in the error term:$q_{ijt} - q_{ij,t-1} = \beta_0 + \boldsymbol{c}_{jt}\boldsymbol{\beta}_1 + \boldsymbol{w}_{ijt}\boldsymbol{\beta}_2 + \boldsymbol{x}_{j,t-1}\boldsymbol{\beta}_3 + \mu_i + \gamma_j + \theta_t + \phi_j t + [(\lambda - 1)q_{ij,t-1} + \epsilon_{ijt}]$.

of 0.01 from 0 to 1 and provide lower and upper bounded competition estimates based on these results. I report main results assuming persistence of one, and secondary results assuming alternative persistence values.

An alternative strategy is to leave lagged achievement on the right hand side and estimate equation 3.1 using an Arellano-Bond approach. In order to implement this strategy, the equation includes school dummy variables and is first differenced with respect to student fixed effects. By construction, first differenced lagged achievement is correlated with the first differenced error term. In order to account for this endogeneity, further lags of achievement are used as instruments for first differenced lagged achievement. The main concern with this approach is that the second lag of achievement, which is commonly used as an instrument, is likely to be endogenous because of measurement error in test scores and autocorrelation in the error term. To avoid this problem, the third or later lag of achievement can be used as the instrument, but this is a heavy data requirement and the third lag may be a weak instrument. Another alternative is to use alternative tests as instruments. For example, twice lagged reading test scores can be used as an instrument for first differenced lagged math test scores. This approach requires the assumption that shocks to math and reading test scores are uncorrelated which seems unlikely to hold in practice. See Andrabi et al. (2011) for a more detailed discussion.

Although I do not report dynamic panel estimates as my main results because of sensitivity in estimation and concern over identifying assumptions, I do report these estimates as a robustness check. Including school dummy variables and first differencing with respect to student fixed effects using the dynamic panel approach is computationally infeasible because of the size of my data. Instead, I first difference with respect to student-school spell fixed effects. This implies that the persistence parameter is identified from students with 4 consecutive test scores while in a particular school.[22] I use thrice lagged math test scores, reading test scores, and school level demographics as instruments.

3.3.3 Robustness Checks

The primary threat to identification after controlling for fixed unobservable student and school level confounders is based on unobservable trends or time-varying shocks correlated with competition and student achievement.

Trends

First, I check robustness to the inclusion of linear school trends. Including non-linear trends in estimation is computationally demanding; instead, I allow linear trends to vary between periods. In estimation, this requires

[22] I also estimate models with school dummy variables first differencing with respect to student fixed effects on period 1 and period 2 separately and find results that are fairly similar. It was not possible to run these models on both periods at the same time because of memory constraints.

that I separately define two school fixed effects for the same school: one for period 1 and the other for period 2. If competition measures are biased by correlation with time-varying unobservable traditional public school characteristics, we expect competitive effect estimates to be sensitive to the addition of school trends.

Time-Varying Unobservables

Unobserved shocks correlated with charter school location decisions are less of a concern because of the charter school approval process. Considering the timing and length of the application process, it seems conservative to assume that the decision to apply likely responds to shocks that occurred two or more years before the planned opening year but not one year before.[23] For this to be a threat to my identification strategy, shocks two or more years prior have to be correlated with current achievement and current measures of competition. Fortunately, given the first difference estimation strategy, I can include school average student test score gains lagged three periods in the model to capture lagged shocks. More specifically, write equation 3.2 with spell fixed effects, but without school trends as

$$\Delta q_{ijt} = \beta_0 + \boldsymbol{c}_{jt}\boldsymbol{\beta}_1 + \boldsymbol{w}_{ijt}\boldsymbol{\beta}_2 + \boldsymbol{x}_{j,t-1}\boldsymbol{\beta}_3 + \psi_{ij} + \theta_t + [\zeta\bar{\epsilon}_{j,t-3} + \tilde{\epsilon}_{ijt}], \tag{3.4}$$

where the error term is divided into an average lagged school component representing relevant shocks at the time of application and an idiosyncratic student component ($\epsilon_{ijt} = \zeta\bar{\epsilon}_{j,t-3} + \tilde{\epsilon}_{ijt}$). Furthermore, aggregate equation 3.2, with spell fixed effects but without school trends, to the school level and lag three periods.

$$\Delta \bar{q}_{j,t-3} = \beta_0 + \boldsymbol{c}_{j,t-3}\boldsymbol{\beta}_1 + \bar{\boldsymbol{w}}_{j,t-3}\boldsymbol{\beta}_2 + \boldsymbol{x}_{j,t-4}\boldsymbol{\beta}_3 + \bar{\psi}_j + \theta_{t-3} + \bar{\epsilon}_{j,t-3}. \tag{3.5}$$

Then we can solve for $\bar{\epsilon}_{j,t-3}$ in equation 3.5 and plug into equation 3.4. In practice, this implies including thrice lagged average student test score gains, thrice lagged school control variables, and thrice lagged competition measures in estimation. The $\bar{\psi}_j$ and θ_{t-3} are subsumed by spell fixed effects and time effects in equation 3.4. If shocks three periods ago are correlated with current charter school location and current achievement, the addition of these lagged covariates may affect the competition estimates. Sensitivity of the estimate for $\boldsymbol{\beta}_1$ to the addition of thrice lagged covariates may suggest that shocks near the time of application are relevant to test scores at time t and correlated with current competition measures, which implies a biased competitive effect

[23] In North Carolina, the charter school application process is arduous and lengthy. Deadlines have varied across time, but in general applications are due at least one year before a school opens. For a specific example, charter schools planning to open in the 2014-2015 school year had an application deadline of March 1, 2013. Additionally, letters of intent were due January 4, 2013 and a letter of intent is required for the application to be considered.

estimate.

Distance and Private School Controls

In addition to separately running specifications with distance based measures of competition, I also estimate a specification that combines distance and attrition measures. There is some concern that traditional public schools not facing attrition but nearby charter schools should be treated differently from schools not facing attrition and not nearby charter schools. In order to account for this, I add an indicator to the main specification using attrition measures of competition that is one for traditional public schools not facing attrition but within 10 miles of any charter school. This separately identifies the effect of traditional public schools nearby a charter that are not facing attrition from traditional public schools facing attrition. Estimated effects are then relative to the omitted category of schools that are not facing attrition and are not within 10 miles of any charter school.

Charter school entry, exit, and expansion may be correlated with the presence of private schools. It is also possible that charter schools affect the opening and closing of private schools. I obtain private school information from the National Center for Education Statistics Private School Universe Survey from 1996 to 2014. This survey is conducted every other year. It does not contain exact private school location information in every year, but does always contain mailing city and zipcode. I convert mailing city and zipcodes to latitudes and longitudes and compute distances between private schools and traditional public schools to construct indicators for whether a traditional public school is within 5 miles of an opening or closing private school. These measures are much cruder than my competition measures because the addresses are not physical addresses, addresses are not at the street level, and the survey suffers from non-response. However, this is the only private school data available, and I check the sensitivity of competition estimates to the inclusion of these private school control variables.

Fixed Effect Estimation

First differences (FD) and fixed effect (FE) estimation methods are able to remove student and school heterogeneity combined into a student-school spell effect. First differences is the main estimation strategy used in this paper; however, it is useful to compare FD estimates to FE estimates for the main set of results as a test of strict exogeneity (Wooldridge 2010). If strict exogeneity is violated such that competition is correlated with ϵ_{ijt} from any time period, FD and FE estimators generally have different probability limits. Thus, large differences between FD and FE estimates may suggest a violation of this assumption.

Distance Based Competition Measures

So far I have generally focused my discussion on the endogeneity of competition measures based on where charter schools locate which is appropriate for distance based measures of competition. Attrition, however, is determined both by proximity to a charter and the decision of students to move to that charter. This adds an additional concern for attrition based measures of competition because attrition rates may be correlated with unobserved time-varying school characteristics. I compare results using distance and attrition based measures of competition as a check of this potential source of bias.

Relative Achievement Instrumental Variable

Considering heterogeneity in competitive effects introduces another layer of challenge because relative achievement measures of competition are a function of lagged student test scores, which introduces additional endogeneity concerns. More specifically, the relative achievement measure for attrition is defined as

$$H_{jt}(\bar{q}_{j,t-1}) = 1[\bar{q}_{j,t-1} < \bar{CH}_{j,t-1}], \tag{3.6}$$

which is an indicator that is one if the average lagged test scores of students in a traditional public school are lower than the average lagged test scores of students in competing charter schools. The average lagged achievement of students in traditional public school j at time t is defined as

$$\bar{q}_{j,t-1} = \frac{1}{N_{jt}} \sum_{i \in j,t} q_{i,t-1},$$

where $q_{i,t-1}$ is the lagged standardized math or reading test score of student i in school j at time t whether or not the student attended school j at time $t-1$.

The average lagged student achievement of competing charter schools is defined as

$$\bar{CH}_{j,t-1} = \frac{N_1 \bar{q}_{1,t-1} + ... + N_{M_{jt}} \bar{q}_{M_{jt},t-1}}{N_1 + ... + N_{M_{jt}}}.$$

The $\bar{q}_{m,t-1}$ are average lagged student test scores for charter school m competing with traditional public school j (constructed in the same way as the traditional public school average). Charter schools are indexed by m and run $1, ..., M_{jt}$. The number of competitors M_{jt} can vary for traditional public school j at time t. For charter competitors, the N_m is not charter school size but rather is the number of students that attrited from TPS j to charter school m. Charter students are not in the estimation sample, but are used to construct average charter school test scores for this competition measure.

Essentially, specifications that account for relative achievement include dummy variables for each level of attrition (0-1% or $>$1%), and interact these dummy variables with a dummy variable for facing higher-achieving competition ($H_{jt}(\bar{q}_{j,t-1})$). Consider the main estimating equation 3.3 but ignore school effects ϕ_j.[24] In practice, results are insensitive to the inclusion of trends. Competition measures are first differenced, and I have argued that attrition is uncorrelated with $\epsilon_{ij,t-1}$, ϵ_{ijt}, and $\epsilon_{ij,t+1}$, which implies that first differenced attrition is uncorrelated with $\Delta\epsilon_{ijt}$ from the estimating equation 3.3. However, the interactions with relative achievement measures are correlated with the first differenced error through $H_{jt}(\bar{q}_{j,t-1})$ because relative achievement for school j is a function of $q_{i,t-1}$ for students i in school j at time t which is correlated with $\epsilon_{ij,t-1}$.

Whether or not the size of the correlation between relative achievement measures and the error is significant depends on the dependence in ϵ_{ijt} and how many observations we are averaging over for school j at time t. If there are many students and errors are independent, the school average of achievement converges to a deterministic value that is uncorrelated with $\epsilon_{ij,t-1}$. If schools on average are small, the error may be strongly correlated with the average of lagged student achievement. Finally, if the errors are strongly correlated within schools even a large number of students does not get rid of the correlation between $\bar{q}_{j,t-1}$ and $\epsilon_{ij,t-1}$. On average, schools in the estimation sample have around 600 students so the main concern is the possibility that individual errors are strongly correlated within school.

In order to test for this potential source of bias, I construct instruments that are uncorrelated with the error term in equation 3.3 ($\phi_j + \Delta\epsilon_{ijt}$), but correlated with the first difference of relative achievement measures of competition interacted with attrition dummy variables. The instruments are constructed using the lagged test scores of students in school j at time $t-2$. Student test scores lagged three periods are unlikely to be correlated with the first differenced error unless there is significant serial correlation in the errors. More specifically the instruments are interactions of attrition dummy variables with

$$H_{jt}(\bar{q}_{j,t-3}) = 1[\bar{q}_{j,t-3} < \bar{CH}_{j,t-1}], \tag{3.7}$$

where

$$\bar{q}_{j,t-3} = \frac{1}{N_{j,t-2}} \sum_{i \in j,t-2} q_{i,t-3}.$$

The only difference between the instrumental variable and the competition measure is that the instrument is based on a comparison of charter school achievement relative to the traditional public school lagged student test

[24] School fixed effects implicitly time demean all variables and the error term with respect to school averages. The IV strategy relies on further lags of $\bar{q}_{jt-1}$ so including school fixed effects will introduce a correlation between the demeaned instrument and the demeaned error in the first stage.

scores from two periods ago rather than lagged test scores from the current period.[25] Instruments enter in levels in the first differenced estimating equation 3.3.

3.3.4 Heterogeneity by School Type

Competitive effects may vary based on the relative characteristics of charter and traditional public schools. I have already defined the main relative achievement measures of competition, but I define an additional relative achievement measure as an indicator that is one if competing charter schools have higher average lagged student test scores than the district in which the traditional public school resides. This tests whether results are sensitive to how broadly relative achievement is defined. I also define an indicator for whether charter schools competing with a traditional public school have higher average lagged student test scores than other charter schools. This measure does not depend on the relative achievement of traditional public schools, and tests whether this is an important consideration. Finally, I also explore heterogeneity in treatment effects by defining indicators for whether a traditional public school has lower proportions of white students, disabled students, or limited English proficient students than competing charter schools.

3.3.5 Heterogeneity by Student Type

Effects may also vary based on the characteristics of individual students. For example, when faced with higher-achieving competition, traditional public schools may focus more on moving lower-achieving students into proficiency. On the other hand, the loss of higher-achieving students may induce traditional public schools to shift effort and resources to these types of students in order to prevent further attrition. From the allocative side, lower or higher-achieving charters may recruit certain types of teachers and students from traditional public schools resulting in differential effects on disadvantaged students left behind. In order to explore these possibilities, I interact competition measures with student ethnicity (black, Hispanic), limited English proficiency, academically gifted status, disability status, and economically disadvantaged status. Considering the shift in the charter school sector to serving more high-achieving white students, it is important to consider the spillover effects on students traditionally more at-risk of academic failure left in traditional public schools.

3.3.6 School Level Effects

So far the analysis has been concerned with identifying the overall effect of charter schools on traditional public school student test scores and how effects vary by school and student characteristics. This does not

[25] Charter school lagged student test scores may include the test scores of students in traditional public schools the period before switching to a charter. Then charter school average lagged student achievement could be correlated with shocks at the traditional public school in $t-1$. Except for charter schools that just opened, the majority of student lags will not be from a traditional public school, but from the charter school itself, which limits this concern. However, in the first year of operation charter school student lagged test scores are solely based on traditional public schools. A later robustness check removes traditional public schools competing with charter schools in their first year of operation from the analysis and indicates that this is not a concern.

explain, however, the mechanisms through which charter schools may be affecting traditional public schools. While it is difficult to identify if traditional public schools are actually responding to competition because of data limitations, it is possible to explore several allocative mechanisms. Following the general set-up of the student level models, I model the change in several school level covariates as a function of school level demographic control variables and competition measures using school fixed effect estimation. The dependent variables of interest include school membership, number of teachers, class size, proportion white, proportion black, proportion Hispanic, proportion gifted, proportion disabled, proportion limited English proficient, and proportion economically disadvanted. I examine whether lower or higher-achieving charter competition differentially affects these school level outcomes and whether any changes in these school level variables may explain effects on student test scores.

3.4 Results

3.4.1 Main Results

Table 3.6 presents the main results using attrition measures of competition. All specifications assume a persistence parameter of one. Columns 1 through 3 present results for math test scores and columns 4 through 6 for reading. This table addresses whether effects depend on the relative achievement of charter and traditional public schools and whether effects differ by period. Column 1 divides traditional public schools into those facing 0-1% attrition (low), those facing attrition >1% (high), and those facing no attrition which is the excluded category. Effects in column 1 are statistically insignificant and close to zero indicating no effect of competition on math test scores.

Column 2 interacts the attrition measures from column 1 with indicators for whether the lagged student test scores of the traditional public school are lower on average than the lagged student test scores of charter schools to which the traditional public school students are attriting. Results indicate that students in traditional public schools competing with lower-achieving charter schools relative to students in schools facing no attrition lose -0.007 and -0.016 standard deviations in math test scores for 0-1% and >1% attrition levels, respectively. Alternatively, students in traditional public schools facing 0-1% attrition from higher-achieving charter schools gain a statistically significant 0.027 standard deviations in math test scores relative to those facing lower-achieving competition, and those facing >1% attrition gain 0.043 standard deviations. The effect of higher-achieving competition relative to students in schools experiencing no attrition is 0.020 standard deviations for 0-1% attrition, and 0.027 standard deviations for >1% attrition.

Column 3 additionally interacts the competition variables from column 2 with period 2 indicators to assess whether effects vary by period. The first four rows are the effects in period 1 and indicate that lower-achieving charter competition has small, statistically insignificant negative effects while higher-achieving charter competition has positive effects around 0.025 standard deviations relative to students in schools facing no attrition. The effect of lower-achieving competition in period 2 relative to students in schools facing no attrition, is the sum of the coefficients in rows 1 and 2 and the coefficients in rows 5 and 6. This indicates there are slightly more negative effects from >1% attrition from lower-achieving charters in period 2, around -0.026 standard deviations, than in period 1. Finally, the effects from higher-achieving competition relative to no competition in period 2 are the sum of all odd rows for attrition 0-1% and even rows for >1% attrition, and indicate small positive effects from higher-achieving competition of 0.016 and 0.025 standard deviations for 0-1% and >1% attrition, respectively. Overall, this suggests effects are similar between periods for math.

Column 4 of Table 3.6 displays results for reading test scores and indicates that students in traditional public schools facing 0-1% attrition gain 0.004 standard deviations, and those facing more than 1% attrition gain 0.009 standard deviations relative to students in schools facing no attrition to charter schools. Although these effects are statistically significant, effect sizes smaller than 0.01 standard deviations are not economically meaningful. Column 5 shows that the small positive effects are driven by competition with higher-achieving charter schools. Additionally, column 6 indicates that in both period 1 and period 2 higher-achieving competition has positive effects in reading, and lower achieving competition has zero to small negative effects. The main difference in reading is that effects from lower-achieving competition are slightly more negative in period 1 than in period 2. Relative achievement effects in reading are slightly smaller in magnitude than math but reflect a similar pattern. Overall, the main results suggest that higher-achieving charter competition has small positive effects in math and reading, but overall effects are close to zero in math and reading. Because effects between periods are similar in both math and reading, future tables will omit the period interactions and focus on relative achievement.

3.4.2 Persistence Results

The main set of results in Table 3.6 use a persistence parameter of one. Figure 3.1 and 3.2 demonstrate the effect of different persistence parameters on competitive effect estimates for math and reading test scores. Estimates are displayed for the main relative achievement competition groups: attrition 0-1% lower-achieving charter competition, attrition 0-1% higher-achieving charter competition, attrition >1% lower-achieving charter competition, and attrition >1% higher-achieving charter competition. The x-axis is persistence values ranging from 0 to 1 in increments of 0.01. The graph demonstrates that there is a strong linear relationship between assumed persistence parameters and competition estimates with the premium to higher-achieving competition increasing in magnitude with larger persistence values. However, the sign and significance of effects is generally preserved regardless of the persistence value.

Figure 3.1 and 3.2 also display the results from the dynamic panel estimators. The vertical line is the persistence parameter value from this estimation procedure and the triangles represent the competitive effect estimates where the solid black triangle corresponds to the sold black line and so on. Ideally, the dynamic panel estimates (solid and dashed triangles) would exactly coincide with the main results (solid and dashed lines), but they are very similar with the exception of the effect of lower-achieving charter competition on reading test scores for the 0-1% attrition group. Overall, the dynamic panel results are similar to my main results and persistence is estimated to be greater than 0.5 which lends some support to assuming perfect persistence. Finally, Table 3.7 displays the coefficient estimates using my main estimation strategy assuming persistence of one in columns 1 and 4, and persistence of zero in columns 2 and 5. The dynamic panel results are displayed in columns 3 and 6.

3.4.3 Robustness Check Results

Trend Results

I test for sensitivity to the inclusion of linear school trends because charter schools may locate based on underlying trends in traditional public school achievement. Results for math and reading in column 2 of Table 3.8 and Table 3.9 with the inclusion of school trends are very similar to the main results displayed in column 1 of Table 3.8 and Table 3.9. This suggests that competition measures are not significantly biased by unobservable trends in school achievement.

Time-Varying Unobservable Results

As discussed in Section 3.3.3, charter schools may respond to local shocks around the time of application. If this is the case, estimates might be affected by the addition of thrice lagged school average student test score gains, thrice lagged school characteristics, and thrice lagged competition measures as additional regressors. Column 3 of Table 3.8 and Table 3.9 shows results for math and reading test scores with the inclusion of these additional covariates and demonstrate that this is not a concern because estimates are largely unchanged.

Distance and Private School Control Results

Charter schools may affect or respond to the opening or closing of private schools in an area, and private school openings and closings may also affect traditional public school student achievement. Column 5 of Table 3.8 and Table 3.9 presents results including private school controls and are very similar to column 1 without private school controls. Interestingly, the private school coefficients are statistically significant and indicate that closing private schools have very small negative effects on traditional public school student math and reading test scores, and that opening private schools have small positive effects on reading test scores.

Additionally, traditional public schools within 10 miles of a charter school that are not experiencing attrition may still experience competitive effects. Column 4 of Table 3.8 and Table 3.9 presents results including an indicator for students in schools within 10 miles of any charter but not facing attrition. The addition of this indicator does not affect competition estimates, and is statistically insignificant and close to zero. This suggests the attrition measures of competition are not missing competitive effects by excluding schools that are nearby charter schools but not experiencing attrition.

Fixed Effect Estimation Results

Differences between fixed effect and first difference estimates may suggest a violation of strict exogeneity. Results using spell fixed effects without controlling for school trends are presented in Table 3.10 and can be compared to Table 3.6. Estimates are almost identical between methods and alleviate concern about violations of strict exogeneity.

Distance Based Competition Measure Results

Distance based measures of competition serve as a useful check of bias in attrition measures due to student schooling decisions based on local or school level shocks. Results using distance based measures are presented in Table 3.11. As with attrition, there is a positive gap between effects from higher-achieving and lower-achieving competition. Effect sizes are generally larger in magnitude using the distance based measures, but largely reflect results using attrition which indicates that this is not a major concern.

Relative Achievement Instrumental Variable Results

As discussed in Section 3.3.3, relative achievement competition measures are possibly correlated with the first differenced error term because I define relative achievement based on lagged student test scores which are correlated with lagged error terms by construction. In order to test for the extent of this bias, instrumental variables are constructed for relative achievement based on average lagged student test scores using twice lagged average lagged student test scores, which are plausibly exogenous. Theoretically, when math is the outcome, I could use two instruments constructed from twice lagged average lagged student test scores: one interacted with attrition 0-1% and the other interacted with attrition $>1\%$. However, I can also construct instruments based on reading test scores. The specifications in Table 3.12 include all four instruments based on math and reading interacted with the two attrition levels. This over-identifies the model and allows for tests of over-identifying restrictions.

Before analyzing the coefficients, note that first stage F tests of the excluded instruments indicate that the instruments are strongly correlated with the endogenous variables. The Hansen J P-value is the p-value associated with a test of the joint null hypothesis that the instruments are valid. The null is marginally not rejected in the case of math (p=0.10) and not rejected in the case of reading (p=0.53). This does raise some suspicion about the validity of the instruments in specifications with math as the dependent variable. However, the large amount of data used in estimation and that this is not a strong rejection may alleviate concerns. Additionally, the instruments may be identifying different local average treatment effects which would also cause a rejection of the null.

The coefficient estimates in column 1 and 2 for math and reading largely reflect the premium associated with higher-achieving competition. Estimates for attrition between 0-1% are slightly larger in magnitude when compared to the main results. The largest difference comes from a sizable increase in the magnitude of effects for attrition greater than 1%. In math, students in schools facing attrition $>1\%$ from lower-achieving charters see a drop in gains of -0.055 standard deviations (compared to OLS of -0.007); among the IV results, this is the largest deviation from the main results. Those facing higher-achieving charters see gains in math of 0.055 standard deviations (compared to OLS of 0.026), relative to schools with no attrition. In reading, the IV results for attrition greater than 1% are also larger in magnitude than the main results, but to a lesser degree.

Although results are not displayed, I run specifications using variations of the instruments such as only including instruments constructed from math scores in math regressions and using further lags of achievement to construct the instruments. Results generally reflect those presented in Table 3.12. Overall, IV results suggest that, if anything, bias in the OLS model is toward zero so that the main OLS estimates are understating the true effect. Instrumental variable results are not reported as the main results because I can not include linear trends in the IV models and because the Hansen test of over-identifying restrictions is marginally passed in the math specifications. Additionally, depending on the construction of the instruments, estimates are not always statistically significantly different from zero in the reading IV specifications, although they are always greater in magnitude than the OLS estimates.

3.4.4 Heterogeneity by School Type Results

Although I have shown effects depend on relative school achievement, it is possible that broader definitions of relative achievement or relative school demographics are also important. Table 3.13 presents results for various measures of relative charter and traditional public school achievement and demographics. Attrition group dummy variables are interacted with indicators for whether competing charter schools have higher average lagged student test scores than the twice lagged average lagged student test scores of the traditional public school. Essentially this uses the instrumental variable for relative achievement developed in a prior section as the measure of competition. The relative achievement premium is reduced in math and eliminated in reading which suggests that further lags of achievement are less relevant than current lags in constructing relevant achievement measures. The next set of interactions use an indicator if competing charter school average lagged student achievement is greater than the average lagged student achievement of the district in which the traditional public school resides. For low levels of attrition, a relative achievement premium exists but is weakened relative to the main results. This suggests that measuring achievement relative to the traditional public is more relevant than measuring relative to the district.

Next, attrition levels are interacted with a dummy variable indicating whether a charter school has higher average lagged student test scores than the average of all charter schools. This removes any relation to traditional public schools and results show a small premium in math for the 0-1% attrition level, but all other groups have no effect. Finally, attrition level dummy variables are interacted with indicators if competing charter schools have higher proportion of white, disabled, or limited English proficient students than the traditional public school. None of these interactions are statistically significant. Overall, these results suggest that heterogeneous treatment effects are driven by differences in relative achievement, and that, although I find small effects using different definitions of relative achievement, the largest effects are found using relative achievement based on lagged student test scores which I use for the main set of results throughout this paper.

3.4.5 Heterogeneity by Student Type Results

As discussed in section 3.3.5, traditional public schools may respond to charter competition in ways that differentially affect different types of students. Similarly, allocative effects from shifting students, teachers, and resources may differentially affect different types of students. Table 3.14 presents results that interact relative achievement measures of competition with student demographic characteristics. Rows 4-8 display the interaction coefficients, but note that the variable definitions change with the column headings. For example, columns 1 and 2 interact competition measures with an indicator if a student is black. Columns 3 and 4 interact competition measures with an indicator if a student is Hispanic. In order to avoid the use of a triple interaction term, the relative achievement interactions are replaced with group indicator variables for each combination of attrition level and relative achievement: attrition 0-1% lower-achieving charter competition, attrition 0-1% higher-achieving charter competition, attrition >1% lower-achieving charter competition, and attrition >1% higher-achieving charter competition. These group indicators are then interacted with student demographics.

For math test scores, the interaction terms indicate that in general groups are very similar. The exceptions are higher gains for Hispanic and economically disadvantaged students relative to non-Hispanic and non-economically disadvantaged students in schools facing 0-1% attrition from higher-achieving charter schools (columns 3 and 11). This suggest, at the least, that higher-achieving competition does not differentially hurt disadvantaged students left behind. In reading, Hispanic, limited English proficient, disabled, non-gifted, and economically disadvantaged students experience more negative effects than their counterparts from lower achieving competition. This suggests that achievement gaps for disadvantaged students are augmented in schools facing lower-achieving charter competition. Additionally, the interaction terms range from -0.028 to -0.046 standard deviations for Hispanic, limited English proficient, and economically disadvantaged students which are not inconsequential. This indicates that higher-achieving competition does not negatively affect disadvantaged students and may even be beneficial, while lower-achieving competition negatively affects disadvantaged students more than their counterparts, although the effects are small.

3.4.6 School Level Effects Results

Results from school level regressions exploring the mechanisms through which charter schools affect traditional public schools are displayed in Table 3.15. Coefficient estimates are displayed for low and high attrition schools interacted with higher-achieving charter competition. In order to ease interpretation, the bottom half of the table converts the competitive effect estimates to approximate percent changes for each attrition level relative achievement group relative to schools facing no attrition. Stars on percent changes indicate significant differences between that group and schools facing no attrition.

The effects on student membership in column 1 for the attrition 0-1% lower-achieve and higher-achieve groups are statistically significant and small suggesting a decrease of about 0.4% in membership. School facing >1% attrition from lower-achieving charter competition experience declines in membership of about 1.8% and schools facing >1% attrition from higher-achieving charter competition experience declines in membership of about 2.8%. Across groups, there is no effect on number of teachers which suggests that class sizes should go down, which is the case in column 3. The decrease in class size is largest in magnitude for schools facing >1% attrition from higher-achieving charter competition which may partially explain the more positive effects from higher-achieving competition on student test scores, but the difference in class size effects relative to lower-achieving competition is very small.

Effects on school ethnicity proportions, gifted, disabled, limited English proficient, and economically disadvantaged proportions are displayed in the remaining columns. In general, effects on these outcomes are small. Higher-achieving charter competition slightly decreases the proportion of white students and increases the proportion of economically disadvantaged students while lower-achieving charter competition decreases the proportion of Hispanic students and increases the proportion of gifted students. If higher proportions of gifted and smaller proportions of economically disadvantaged students suggest a stronger peer group, this would lead us to suspect that lower-achieving charter competition should have more positive effects on student test scores than higher-achieving competition. This is at odds with the results I find, which suggests that these allocative effects may not explain the more positive effects on test scores from higher-achieving competition. This leaves the possibility that teacher quality and responses to competition may be the relevant mechanisms.

3.4.7 Effects in Context

Studies in states other than North Carolina following a similar empirical strategy to that employed in this paper find mixed results. Sass (2006) finds a positive effect on math test scores and no effect on reading in Florida. Zimmer et al. (2009) analyze effects across eight geographic locations and generally find no significant effects apart from a small positive effect in Texas. In North Carolina, previous results have also been mixed. Bifulco and Ladd (2006) find no effect on student test scores, although effects are generally positive, while Holmes et al. (2003) find a small positive effect using a school level analysis. Jinnai (2014) finds a small positive effect and shows that defining competition at the grade level rather than the school level increases effect sizes. Mehta (2017) finds a small positive spillover effect when estimating a model of charter and traditional public school competition that models charter school entry, school inputs, and student school choices.

All the studies mentioned that are conducted in North Carolina are estimated using data between 1996 and 2005 and use distance based measures of competition. Using distance based measures of competition in period 1 (1997-2005), I confirm the general finding of small positive effects in math and reading. The attrition measures I employ do not confirm this finding, but the specifications I use also include a control for traditional public schools facing no attrition within 10 miles of a charter school, and the coefficient is statically insignificant and close to zero. This suggests that schools facing attrition that are not within 10 miles of any charter are responsible for the slightly divergent findings.

Comparing effect sizes to other studies is complicated by differences in competition measures and differences in the construction of outcome measures. Perhaps most directly comparable is Cremata and Raymond (2014) which use standardized test score gains as the outcome and attrition measures of competition interacted with charter achievement as the treatment. They define high-achieving competition as an indicator that is one if the average achievement of charter competitors of a traditional public school is above the district average. In Washington DC, where charter enrollment was over 40% of public school enrollment by 2012, they find high-achieving competition improves math scores by 0.04-0.08 (when significant) and reading test scores by 0.06 to 0.15 relative to low-achieving competition. Cordes (2018) also allows effects to vary by charter school achievement, and defines high-achieving competition in New York City as an indicator if the average proficiency of competing charters is above the 75th percentile of the city in the prior year. She finds no premium to high-achieving competition in math, but in reading finds a premium of 0.017 standard deviations. I generally find higher-achieving competition increases test score gains by 0.01-0.03 standard deviations which falls between the effect sizes in these two studies.

Finally, it is useful to compare effect sizes to other policy interventions. Of particular, importance are the estimated effects of charter schools on charter school students as these studies often rely on the assumption of no spillover effects on traditional public schools. The CREDO National Charter School Study includes the analysis of charter schools in 27 states and generally finds aggregate effect sizes between -0.03 and 0.03 standard deviations (CREDO 2013b). Finally, Ladd et al. (2016) find effects between 0 and -0.03 standard deviations in North Carolina using a student fixed effects approach.

3.5 Conclusion

This paper examines the effects of charter schools on traditional public school students in North Carolina and finds that, on average, charter schools in North Carolina have no economically significant effect on math or reading test scores. Also, I examine whether effects differ by the relative achievement of traditional public and charter schools, and find higher-achieving competition has small positive effects, while lower-achieving competition has zero to very small negative effects. Interactions with student characteristics suggest that more disadvantaged students left behind in traditional public schools facing lower-achieving charter competition generally experience the same or more negative effects in reading than their counterparts. Additionally, the cases of more positive effects on math test scores for disadvantaged students relative to their counterparts occur in schools facing higher-achieving competition. This suggests that the shift in charter schools to serving higher-achieving students has, at the very least, not hurt disadvantaged students left behind and may even be beneficial.

Charter schools may locate based on trends in traditional public school achievement, or based on relevant shocks near the time of charter school application. Linear school trends and lagged school controls are strategies to address these concerns, and results are insensitive to their inclusion. This suggests that controlling for time-invariant student and school heterogeneity may be adequate to control for endogeneity concerns. Results are somewhat sensitive to the persistence assumption, but effects are generally the same sign and significance regardless of assuming perfect or zero persistence in student test scores.

The findings of this paper have implications for research on the competitive effects of charter schools on traditional public school students. Although the effect sizes I find are small, the literature, when effects are significant, generally finds small positive or negative effects. Differential effects based on the relative achievement levels of competitors may explain differences in competitive effects across previous competition studies, and even competition studies based on the same state if characteristics of the charter school sector change over time within a state. This suggests that future work examining the effects of charter schools on traditional public school students should consider heterogeneous treatment effects based on the relative characteristics of traditional public and charter schools.

The findings of this study also have implications for research on the competitive effects of charter schools on charter school students. Studies of charter school productivity often employ local matching techniques or use lottery information to compare students lotteried in and those lotteried out of an oversubscribed charter school. The methods assume that charter schools have no spillover effects on traditional public schools which has been shown here to depend on the relative achievement of competitors. This may be especially relevant for lottery based studies where oversubscribed charter schools are possibly in high demand because of their high achievement.

Future work will examine other mechanisms through which charter schools affect traditional public school students. For example, changes in teacher composition may partially explain the observed effects on traditional public school students. As higher-achieving charter schools have grown in North Carolina, it is also important to understand which types of charter schools are approved by the State Board of Education, where these charter schools locate, and whether they are accessible to students from all backgrounds. Future research will use information collected from individual charter school applications to address these questions.

Table 3.1: Competition Measure Definitions

Competition Measure	Definition
Attrition Based Measures	
Attrition 0%	Indicates if a TPS has 0% of prior year enrollment non-structurally switch to any charter in the current year (base)
Attrition 0-1%	Indicates if a TPS has 0-1% of prior year enrollment non-structurally switch to any charter in the current year
Attrition >1%	Indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year
Higher-Achieving Charter	Indicates if the average lagged test scores of students in charter schools from which a TPS faces attrition are higher than the average lagged test scores of the TPS students
Distance Based Measures	
Within 10 Miles 0 Charter	Indicates if a TPS is not within 10 miles of any charter and not one of the closest 10 TPSs to any charter (base)
Within 10 Miles 1 Charter	Indicates if a TPS is within 10 miles of one charter and one of the closest 10 TPSs to that charter
Within 10 Miles 2 or More Charters	Indicates if a TPS is within 10 miles of 2 or more charters and one of the closest 10 TPSs to those charters
Higher-Achieving Charter	Indicates if the average lagged test scores of students in charter schools within 10 miles of the TPS are higher than the average lagged test scores of the TPS students

TPS refers to traditional public school, and CH refers to charter school. All competition measures have the added restriction that schools can compete only if they serve at least one grade level that is the same. I define non-structural switchers as students that switch schools when they could have stayed an additional year at their prior school. Attrition based measures of competition have the added restriction that non-structural moves are only counted toward attrition if the distance between schools is less than 20 miles. All distance competition measures have the added restriction that a traditional public school be one of the closest 10 traditional public schools to a charter school. Distances between schools are straight line distances.

Table 3.2: Summary of Distances between Traditional Public and Charter Schools by Urbanicity

	Urban	Suburban	Town	Rural
Non-Structural Student Moves From TPSs to Charters:				
Median Distance Between Schools	4.25	5.66	3.29	7.81
75th Percentile Distance Between Schools	6.80	9.25	7.65	12.21
Number of Switchers	14,719	6,904	4,828	13,333
Average Distance of Charter Schools to the:				
5th Closest TPS	2.7	4.4	7.2	7.9
10th Closest TPS	4.4	6.8	13.3	11.7
15th Closest TPS	6.0	8.9	17.3	15.3

TPS refers to traditional public school. Pooled for years 1998-2016. Distances are in miles. Student moves are restricted to grade levels between 3 and 12. Average distance between schools only considers schools that overlap in at least one grade level. Non-structural switchers are defined as students that switch schools when they could have stayed an additional year at their prior school.

Table 3.3: Charter and Traditional Public School Characteristics by Attrition Status and Period

	Panel A: Period 1 (1997-2005)			Panel B: Period 2 (2006-2016)		
	(1) TPS No Attrition	(2) TPS Attrition	(3) Charter	(4) TPS No Attrition	(5) TPS Attrition	(6) Charter
Math Std	-0.00^a	0.01^a	-0.19*^{a}	-0.01	-0.01	0.09
Read Std	-0.01	0.01^a	-0.06^a	-0.01	-0.02	0.21*
Black	0.28*^{a}	0.35^a	0.36^a	0.24*	0.31	0.28
Hispanic	0.05*^{a}	0.06^a	0.02*^{a}	0.12*	0.14	0.06*
White	0.63*^{a}	0.54^a	0.58	0.56*	0.47	0.60*
Free Lunch	0.36^a	0.35^a	0.12*^{a}	0.45*	0.46	0.26*
Lim Eng Prof	0.03*^{a}	0.03^a	0.00*^{a}	0.06	0.06	0.02*
Disability	0.14^a	0.14^a	0.14	0.15*	0.14	0.13*
Gifted	0.12*^{a}	0.15	0.06*^{a}	0.14*	0.16	0.02*
Enrollment	507.61*^{a}	627.43^a	210.05*^{a}	497.73*	612.30	432.72*
Urban	0.21*^{a}	0.39^a	0.41	0.20*	0.35	0.41
Rural	0.45*^{a}	0.32^a	0.28	0.50*	0.36	0.29
Suburban	0.18^a	0.18	0.13	0.15*	0.18	0.14
Town	0.16*	0.11	0.19*	0.15*	0.11	0.15
Elementary	0.61^a	0.60	0.20*^{a}	0.63	0.61	0.12*
Middle	0.19*	0.28	0.09*^{a}	0.20*	0.29	0.04*
Elem-Middle	0.13*^{a}	0.10^a	0.48*	0.10*	0.07	0.53*
Middle-High	0.05*	0.02	0.11*	0.05*	0.02	0.06*
Elem-Mid-High	0.02*	0.01	0.13*^{a}	0.02*	0.01	0.26*
Observations	10188	3488	565	13766	6941	1125

TPS refers to traditional public school. TPS Attrition includes TPSs that are facing any amount of attrition to charter schools that are no more than 20 miles away from the TPSs. All groups are restricted to schools serving a grade level between 3 and 8. Statistics are weighted by school membership in grades 3 through 8 for the top panel and unweighted for the bottom panel. Standard errors are clustered at the school level. Significant differences relative to TPS Attrition within each period are starred, *, for $p < 0.05$. Significant differences between periods for each group are lettered, ^{a}, for $p < 0.05$ in Panel A.

Table 3.4: Count and Proportion of Traditional Public Schools Experiencing Attrition to Higher or Lower-Achieving Charter Schools for Period 1 and Period 2 in Math and Reading Conditional on Schools Experiencing Any Attrition

	Lower-Achieving Charter Competition	Higher-Achieving Charter Competition	Total
Math			
Period 1	2,316 (68%)	1,103 (32%)	3,419 (100%)
Period 2 Full	3,067 (45%)	3,734 (55%)	6,801 (100%)
Period 2 Split	937 (39%)	1,454 (61%)	2,391 (100%)
Reading			
Period 1	2,047 (60%)	1,372 (40%)	3,419 (100%)
Period 2 Full	2,320 (34%)	4,481 (66%)	6,801 (100%)
Period 2 Split	712 (30%)	1,679 (70%)	2,391 (100%)

Sample is restricted to traditional public schools (TPSs) facing attrition. Higher-achieving charter competition includes TPSs facing attrition and the lagged test scores of the TPS students are lower on average than that of the lagged test scores of students in charter schools from which the TPS is facing attrition. Full includes TPSs facing attrition from charter schools operating in period 2 that opened in period 1 or period 2. Split isolates TPSs facing attrition from charter schools operating in period 2 that opened in period 2 and does not count charter schools that opened in period 1 in defining competition. Period 1 ranges from 1997-2005, and period 2 from 2006-2016.

Table 3.5: Control Variable Definitions

Name	Definition
<u>Student Level</u>	
Cohort Switcher*	Indicates Student Moves with >15% of Cohort to a New School
Non-Cohort Switcher*	Indicates a Non-Cohort Switcher Move
Grade Repeater*	Indicates a Student Repeats a Grade
<u>School Level</u>	
Within 5 Miles Closed*	Indicates a School is Within 5 Miles of a Closing TPS
Within 5 Miles Open*	Indicates a School is Within 5 Miles of an Opening TPS
Grade Decrease*	Indicates a School Decreases Grade Span
Grade Increase*	Indicates a School Increases Grade Span
Enrollment	Number of Students Attending a School
Disabled	Proportion of Disabled Students in a School
Limited English Proficient	Proportion of Limited English Proficient Students in a School
Gifted	Proportion of Gifted Students in a School
White	Proportion of White Students in a School
Black	Proportion of Black Students in a School
Hispanic	Proportion of Hispanic Students in a School

*Indicates covariates that are included contemporaneously with the dependent variable. All other covariates are lagged one period in regressions.

Table 3.6: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement and Period for Years 1997-2016 without School Trends

VARIABLES	(1) Math	(2) Math	(3) Math	(4) Read	(5) Read	(6) Read
Attrition 0-1%	0.003	-0.007***	-0.007	0.004**	-0.002	-0.008**
	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)
Attrition >1%	0.005	-0.016**	-0.004	0.009**	-0.001	-0.005
	(0.005)	(0.007)	(0.010)	(0.004)	(0.006)	(0.008)
Attrition 0-1% * Higher-Achieve CH		0.027***	0.032***		0.011***	0.017***
		(0.004)	(0.007)		(0.003)	(0.005)
Attrition >1% * Higher-Achieve CH		0.043***	0.030*		0.017**	0.021**
		(0.009)	(0.016)		(0.007)	(0.010)
Attrition 0-1% * Period2			-0.000			0.011**
			(0.006)			(0.005)
Attrition >1% * Period2			-0.022*			0.005
			(0.013)			(0.011)
Attrition 0-1% * Higher-Achieve CH * Period2			-0.007			-0.009
			(0.008)			(0.006)
Attrition >1% * Higher-Achieve CH * Period2			0.021			-0.006
			(0.018)			(0.013)
Observations	4,016,929	4,016,929	4,016,929	3,994,994	3,994,994	3,994,994

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Period2 is an indicator for years between 2006 and 2016. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. I assume persistence is one. Period 1 ranges from 1997-2005, and period 2 from 2006-2016. Standard errors are clustered at the school level.

Table 3.7: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016 without School Trends: Various Persistence Strategies

VARIABLES	(1) Math	(2) Math	(3) Math	(4) Read	(5) Read	(6) Read
Attrition 0-1%	-0.007***	-0.002	-0.007***	-0.002	0.000	-0.003
	(0.003)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)
Attrition >1%	-0.016**	-0.003	-0.016***	-0.001	0.002	0.010**
	(0.007)	(0.004)	(0.002)	(0.006)	(0.003)	(0.005)
Attrition 0-1% * Higher-Achieve CH	0.027***	0.008***	0.021***	0.011***	0.004*	0.011***
	(0.004)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
Attrition >1% * Higher-Achieve CH	0.043***	0.011**	0.039***	0.017**	0.005	0.000
	(0.009)	(0.005)	(0.004)	(0.007)	(0.004)	(0.006)
Persistence			0.662			0.875
			(0.036)			(0.042)
High Persistence	Yes			Yes		
Low Persistence		Yes			Yes	
Arellano-Bond			Yes			Yes
Observations	4,016,929	4,016,929	2,331,311	3,994,994	3,994,994	2,318,503

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. High persistence assumes that persistence in test scores is 1, and low assumes 0 persistence. Standard errors are clustered at the school level except for Arellano-Bond estimation which are clustered at the student level. I conduct Arellano-Bond estimation by first differencing a lagged value added model and instrumenting for lagged test score differences using thrice lagged student math and reading test scores and thrice lagged school characteristics. The first difference is taken with respect to student-school spell fixed effects.

Table 3.8: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016: Math Robustness Checks

VARIABLES	(1) Math	(2) Math	(3) Math	(4) Math	(5) Math
Attrition 0-1%	-0.007***	-0.007***	-0.006*	-0.008**	-0.007**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Attrition >1%	-0.016**	-0.018***	-0.017**	-0.017**	-0.016**
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
Attrition 0-1% * Higher-Achieve CH	0.027***	0.026***	0.025***	0.027***	0.027***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Attrition >1% * Higher-Achieve CH	0.043***	0.043***	0.045***	0.043***	0.043***
	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)
Attrition 0% Within 10 CH				-0.002	
				(0.004)	
Within 5 Closed Private					-0.009***
					(0.003)
Within 5 New Private					0.000
					(0.003)
Trends		Yes			
Time-Varying Unobservables			Yes		
Distance Control				Yes	
Private School Controls					Yes
Observations	4,016,929	4,016,929	3,545,591	4,016,834	4,016,834

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. Models with school trends are estimated using school fixed effect estimation. I assume persistence is one. Standard errors are clustered at the school level. Trends includes a linear trend for each school. Time-Varying Unobservables includes thrice lagged average school achievement, controls, and competition measures. Distance Control includes a dummy variable that is one if a school is within 10 miles of any charter, but not experiencing attrition. Private School Controls includes indicators that are one if a TPS is within 5 miles of a new or closing private school.

Table 3.9: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016: Reading Robustness Checks

VARIABLES	(1) Read	(2) Read	(3) Read	(4) Read	(5) Read
Attrition 0-1%	-0.002	-0.002	-0.000	-0.003	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Attrition >1%	-0.001	-0.002	0.004	-0.002	-0.001
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
Attrition 0-1% * Higher-Achieve CH	0.011***	0.012***	0.010***	0.011***	0.012***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Attrition >1% * Higher-Achieve CH	0.017**	0.016**	0.012	0.017**	0.017**
	(0.007)	(0.007)	(0.008)	(0.007)	(0.007)
Attrition 0% Within 10 CH				-0.002	
				(0.003)	
Within 5 Closed Private					-0.005*
					(0.002)
Within 5 New Private					0.006***
					(0.003)
Trends		Yes			
Time-Varying Unobservables			Yes		
Distance Control				Yes	
Private School Controls					Yes
Observations	3,994,994	3,994,994	3,166,660	3,994,899	3,994,899

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. Models with school trends are estimated using school fixed effect estimation. I assume persistence is one. Standard errors are clustered at the school level. Trends includes a linear trend for each school. Time-Varying Unobservables includes thrice lagged average school achievement, controls, and competition measures. Distance Control includes a dummy variable that is one if a school is within 10 miles of any charter, but not experiencing attrition. Private School Controls includes indicators that are one if a TPS is within 5 miles of a new or closing private school.

Table 3.10: Fixed Effect Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016 without School Trends

VARIABLES	(1) Math	(2) Math	(3) Read	(4) Read
Attrition 0-1%	0.002	-0.009***	0.004**	-0.001
	(0.002)	(0.003)	(0.002)	(0.002)
Attrition >1%	0.004	-0.018***	0.010***	-0.000
	(0.005)	(0.007)	(0.003)	(0.005)
Attrition 0-1% * Higher-Achieve CH		0.029***		0.010***
		(0.004)		(0.003)
Attrition >1% * Higher-Achieve CH		0.043***		0.017***
		(0.009)		(0.006)
Observations	8,735,199	8,734,975	8,694,404	8,694,180

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are in levels. Spell fixed effects are included to control for student and school heterogeneity, and models are estimated using fixed effect estimation. I assume persistence is one. Standard errors are clustered at the school level.

Table 3.11: First Differenced Regressions of Standardized Student Test Score Gains on Distance Competition Measures Interacted with Relative Achievement for Years 1997-2016 without School Trends

VARIABLES	(1) Math	(2) Math	(3) Read	(4) Read
Within 10 1 CH	0.002	-0.018**	0.005	-0.010*
	(0.006)	(0.008)	(0.005)	(0.005)
Within 10 2 or More CH	0.019*	-0.006	0.015**	-0.001
	(0.010)	(0.011)	(0.007)	(0.007)
Within 10 1 CH * Higher-Achieve CH		0.044***		0.027***
		(0.006)		(0.004)
Within 10 2 or More CH * Higher-Achieve CH		0.057***		0.031***
		(0.011)		(0.007)
Observations	4,015,639	3,969,834	3,993,715	3,948,171

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Within 10 1 CH indicates if a TPS is within 10 miles of one charter school and one of the closest 10 TPSs to that charter. Within 10 2 or More CH indicates if a TPS is within 10 miles of more than one charter school and one of the closest 10 TPSs to those charter schools. Within 10 miles of no charter school is the omitted category. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools within 10 miles. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. I assume persistence is one. Standard errors are clustered at the school level.

Table 3.12: First Differenced Instrumental Variable Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016 without School Trends

VARIABLES	(1) Math	(2) Read
Attrition 0-1%	-0.012*	-0.003
	(0.006)	(0.005)
Attrition >1%	-0.055***	-0.017
	(0.020)	(0.017)
Attrition 0-1% * Higher-Achieve CH	0.041***	0.016*
	(0.016)	(0.008)
Attrition >1% * Higher-Achieve CH	0.11***	0.045*
	(0.032)	(0.021)
First Stage F (0-1%)	171	169
First Stage F (>1%)	77	72
Hansen J P-Value	0.10	0.53
Observations	3,545,591	3,525,152

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using instrumental variables. I assume persistence is one. Standard errors are clustered at the school level. Terms interacted with Higher-Achieve CH are endogenous. There are four instrumental variables including relative achievement constructed from thrice lagged math scores interacted with 0-1% and >1% attrition, and relative achievement measures constructed from thrice lagged reading scores interacted with 0-1% and >1% attrition. The inclusion of instruments based on both math and reading overidentifies the model and allows for a test of overidentifying restrictions which is reported as Hansen J p-value. The null is that the instruments are valid. F-tests of the four excluded instruments are also reported for the two first stages (one for each endogenous regressor).

Table 3.13: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement and Demographic Measures for Years 1997-2016 without School Trends: Heterogeneity by School Type

VARIABLES	(1) Math	(2) Math	(3) Math	(4) Math	(5) Math	(6) Math	(7) Read	(8) Read	(9) Read	(10) Read	(11) Read	(12) Read
Attrition 0-1%	-0.002	-0.002	-0.001	-0.000	0.001	0.003	0.004	0.001	0.003	0.004*	0.004*	0.004*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Attrition >1%	-0.002	0.005	0.004	0.012	0.005	0.004	0.011**	0.011**	0.011**	0.009*	0.008*	0.008**
	(0.008)	(0.006)	(0.007)	(0.008)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.004)
Attrition 0-1% * Higher-Achieve CH 2Lag	0.013***						0.001					
	(0.004)						(0.003)					
Attrition >1% * Higher-Achieve CH 2Lag	0.017*						0.001					
	(0.009)						(0.007)					
Attrition 0-1% * Higher-Achieve CH District		0.012***						0.007**				
		(0.004)						(0.003)				
Attrition >1% * Higher-Achieve CH District		0.002						-0.003				
		(0.009)						(0.007)				
Attrition 0-1% * Higher-Achieve CH-CH			0.008**						0.003			
			(0.004)						(0.003)			
Attrition >1% * Higher-Achieve CH-CH			0.003						-0.004			
			(0.009)						(0.007)			
Attrition 0-1% * Higher-White CH				0.006						-0.001		
				(0.004)						(0.003)		
Attrition >1% * Higher-White CH				-0.009						-0.001		
				(0.009)						(0.007)		
Attrition 0-1% * Higher-Disabled CH					0.003						-0.000	
					(0.004)						(0.003)	
Attrition >1% * Higher-Disabled CH					0.001						0.002	
					(0.009)						(0.006)	
Attrition 0-1% * Higher-LEP CH						0.001						0.000
						(0.005)						(0.004)
Attrition >1% * Higher-LEP CH						0.013						0.006
						(0.014)						(0.010)
Observations	3,876,324	4,016,929	4,016,929	4,016,929	4,016,929	4,016,929	3,854,874	3,994,994	3,994,994	3,994,994	3,994,994	3,994,994

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized TPS student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has 0-1% of prior year enrollment non-structurally switch to any CH in the current year. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH 2Lag indicates if the second lag of average lagged scores of students in a TPS are lower than the average lagged scores of students in competing CHs. Higher-Achieve CH District indicates if the lagged scores of students in competing CHs are greater than the average lagged scores of the district in which the TPS is located. Higher-Achieve CH-CH indicates if the CHs competing with a TPS have higher average lagged scores than that of the average of all CHs. Higher-White CH indicates if the TPS has a lower proportion of white students compared to competing CHs. Higher-Disabled CH and Higher-LEP CH (Limited English Proficient) are defined similarly. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. I assume persistence is one. Standard errors are clustered at the school level.

Table 3.14: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Relative Achievement Competition Measures Interacted with Student Demographics for Years 1997-2016 without School Trends: Heterogeneity by Student Type

	Black		Hispanic		Lim Eng Proficient		Disabled		Gifted		Econ Disadv	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Math	Read	Math	Read	Math	Read	Math	Read	Math	Read	Math	Read
Attrition 0-1% Lower-Achieve CH	-0.006**	-0.002	-0.007**	-0.002	-0.007***	-0.002	-0.008***	-0.001	-0.009***	-0.004	-0.007*	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Attrition 0-1% Higher-Achieve CH	0.014***	0.009***	0.014***	0.009***	0.019***	0.010***	0.019***	0.009***	0.021***	0.010***	0.012***	0.011***
	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)
Attrition >1% Lower-Achieve CH	-0.021***	0.001	-0.022***	-0.000	-0.016**	-0.000	-0.016**	-0.002	-0.017**	-0.003	-0.034***	0.017*
	(0.008)	(0.007)	(0.008)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.011)	(0.010)
Attrition >1% Higher-Achieve CH	0.021***	0.011**	0.021***	0.013**	0.026***	0.016***	0.025***	0.014***	0.027***	0.016***	0.025***	0.017***
	(0.008)	(0.005)	(0.008)	(0.005)	(0.007)	(0.004)	(0.007)	(0.005)	(0.007)	(0.005)	(0.009)	(0.006)
Attrition 0-1 Lower-Achieve CH * Demo	-0.006	-0.002	-0.008	0.004	-0.012	-0.005	0.003	-0.011**	0.005	0.007*	-0.002	-0.004
	(0.004)	(0.004)	(0.006)	(0.006)	(0.008)	(0.009)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Attrition 0-1% Higher-Achieve CH * Demo	0.007	0.003	0.017**	-0.006	0.003	-0.010	0.000	0.002	-0.007	-0.001	0.010**	-0.001
	(0.005)	(0.004)	(0.007)	(0.005)	(0.008)	(0.007)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Attrition >1% Lower-Achieve CH * Demo	0.000	-0.003	0.006	-0.028*	-0.007	-0.046*	-0.007	0.004	0.004	0.008	0.014	-0.033***
	(0.010)	(0.009)	(0.015)	(0.015)	(0.018)	(0.024)	(0.011)	(0.014)	(0.010)	(0.010)	(0.010)	(0.012)
Attrition >1% Higher-Achieve CH * Demo	0.009	0.010	0.002	-0.011	-0.016	-0.019	0.004	0.008	-0.012	-0.010	-0.001	-0.006
	(0.009)	(0.007)	(0.011)	(0.009)	(0.013)	(0.012)	(0.009)	(0.009)	(0.010)	(0.008)	(0.008)	(0.007)
Observations	3,405,467	3,389,246	2,722,869	2,708,509	4,013,095	3,990,724	4,013,095	3,990,724	4,013,095	3,990,724	2,486,153	2,471,538

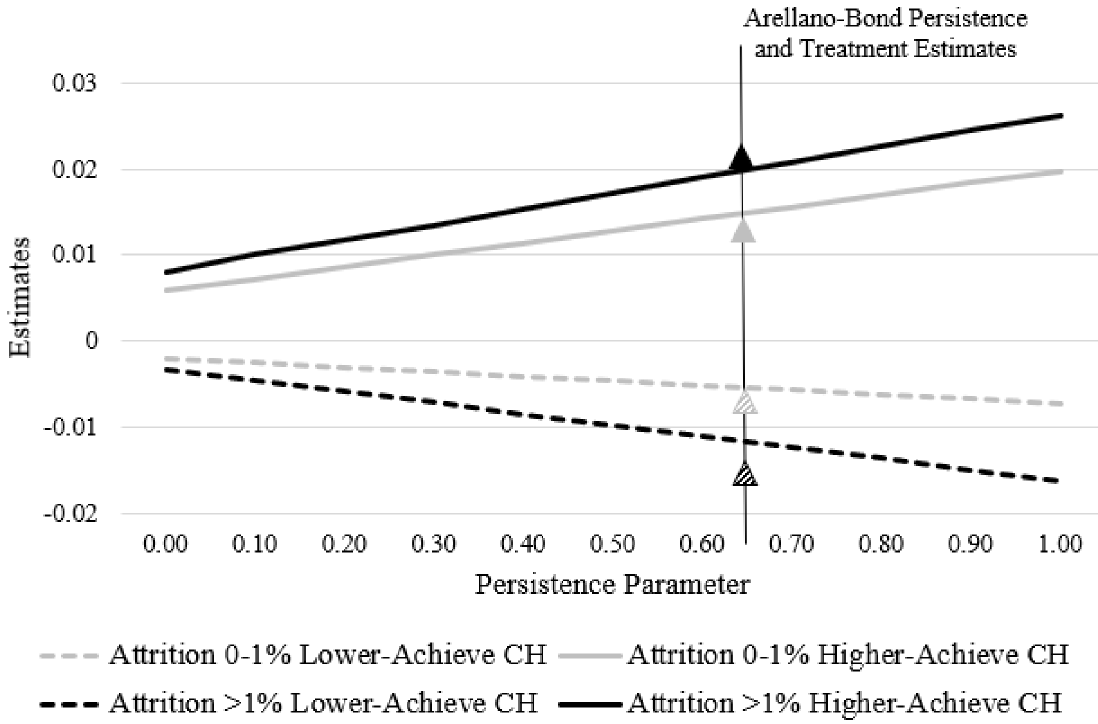
TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Instead of interacting Higher-Achieve CH with attrition dummies, I create separate group indicators for each combination of attrition (0-1% or >1%) and achievement (higher or lower). I interact these group indicators with indicators for whether a student is black, Hispanic, limited English proficient, disabled, gifted, or economically disadvantaged. The interaction terms change based on the specific demographic characteristic based on the column heading. Economically disadvantaged information at the student level is only available in period 2. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. I assume persistence is one. Standard errors are clustered at the school level.

Table 3.15: School Fixed Effect Regressions of Various School Outcomes on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016 without School Trends

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	Member	Teacher Count	Pupil/ Teacher Ratio	White %	Black %	Hispanic %	Gifted %	Disabled %	LEP %	Econ Disadvanted %
Coefficient Estimates										
Attrition 0-1%	-2.53*	-0.02	-0.08*	0.11	-0.04	-0.04	0.01	0.03	-0.06	0.17
	(1.35)	(0.10)	(0.04)	(0.07)	(0.06)	(0.04)	(0.09)	(0.06)	(0.04)	(0.24)
Attrition >1%	-10.01***	-0.17	-0.18**	0.17	-0.03	-0.20**	0.46**	0.18	-0.12	0.11
	(2.29)	(0.15)	(0.09)	(0.20)	(0.20)	(0.10)	(0.18)	(0.15)	(0.09)	(0.51)
Attrition 0-1% * Higher-Achieve CH	-0.62	-0.00	-0.05	-0.32***	0.14	0.12*	0.05	0.08	0.12*	-0.11
	(1.89)	(0.13)	(0.05)	(0.10)	(0.09)	(0.06)	(0.12)	(0.08)	(0.06)	(0.34)
Attrition >1% * Higher-Achieve CH	-4.02	-0.08	-0.13	-0.51**	0.37	0.33**	-0.66***	-0.03	0.13	0.72
	(2.89)	(0.20)	(0.10)	(0.23)	(0.24)	(0.13)	(0.23)	(0.18)	(0.13)	(0.62)
Approximate Percentage Change										
Attrition 0-1% & Lower-Achieve CH	-0.3%*	-0.0%	-0.5%*	0.2%	-0.1%	-0.4%	0.0%	0.0%	-1.3%	0.5%
Attrition >1% & Lower-Achieve CH	-1.8%***	-0.4%	-1.3%**	0.4%	-0.1%	-2.0%**	3.2%**	1.2%	-2.3%	0.2%
Attrition 0-1% & Higher-Achieve CH	-0.4%**	-0.0%	-0.08%***	-0.5%***	0.3%	0.6%	0.5%	0.8%*	0.9%	0.1%
Attrition >1% & Higher-Achieve CH	-2.8%***	-0.7%	-2.2%***	-1.0%**	0.7%**	1.0%	-2.0%	0.9%	0.0%	1.4%*
Observations	31,953	27,554	27,534	31,981	31,981	31,981	31,971	31,971	31,971	28,205
Number of Schools	1996	1989	1989	1996	1996	1996	1996	1996	1996	1989

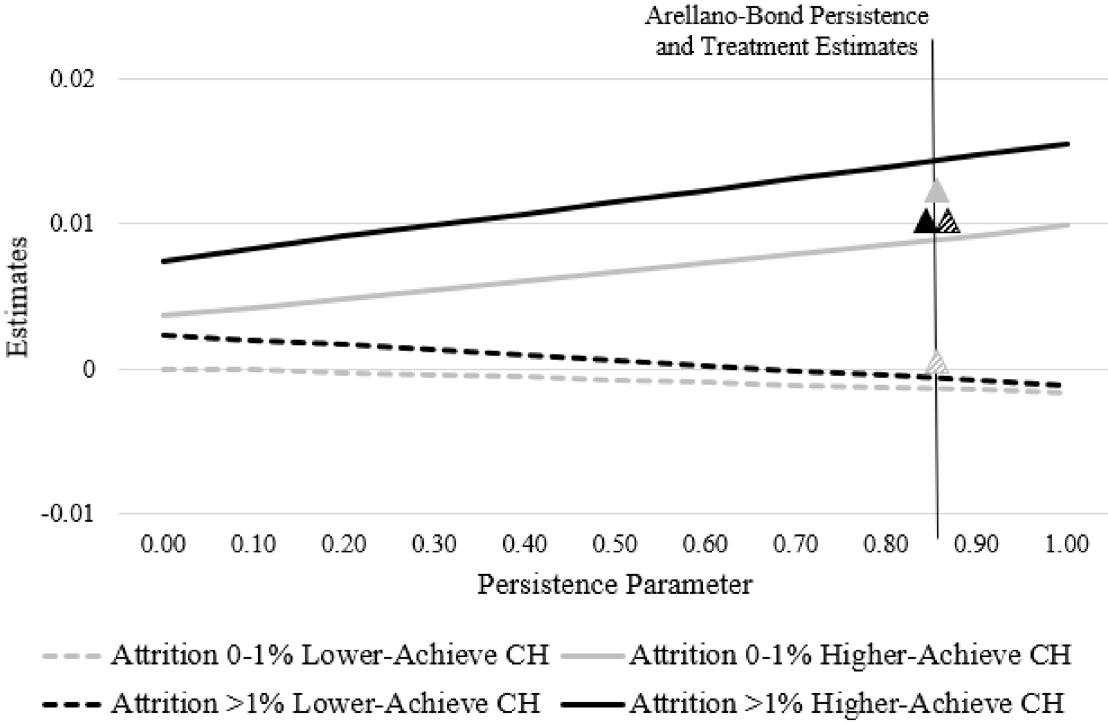
TPS refers to traditional public school, and CH refers to charter school. Following the student level models, all school level model dependent variables are in gains. I use fixed effect estimation and cluster standard errors at the school level. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Competition measures are interacted with a dummy variable indicating if a school is in its third or later year of strictly positive attrition because allocative effects may be largest in the initial years of competition. These interaction terms are omitted. Therefore, the coefficient estimates can be interpreted as the effect of attrition in the first two years of exposure to attrition relative to schools not facing attrition. Approximate percentage changes are calculated as the coefficient estimate for the attrition group divided by the average level of the dependent variable for the attrition group between 1997 and 2016.

Figure 3.1: Relative Achievement Regression Coefficients by Persistence Parameter Values for Math, 1997-2016



TPS refers to traditional public school, and CH refers to charter school. This table reports the marginal effects of charter competition on traditional public school students for the groups: attrition 0-1% lower-achieving charter competition, attrition 0-1% higher-achieving charter competition, attrition $>1\%$ lower-achieving charter competition, and attrition $>1\%$ higher-achieving charter competition. Different estimates are reported for different persistent parameters ranging from 0 to 1 in increments of 0.10. The dependent variable is standardized math test score gains ($q_t - \lambda q_{t-1}$) where the gain varies based on the persistence parameter λ. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition $>1\%$ indicates if a TPS has $>1\%$ of prior year enrollment non-structurally switch to any charter in the current year. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS.

Figure 3.2: Relative Achievement Regression Coefficients by Persistence Parameter Values for Reading, 1997-2016



TPS refers to traditional public school, and CH refers to charter school. This table reports the marginal effects of charter competition on traditional public school students for the groups: attrition 0-1% lower-achieving charter competition, attrition 0-1% higher-achieving charter competition, attrition >1% lower-achieving charter competition, and attrition >1% higher-achieving charter competition. Different estimates are reported for different persistent parameters ranging from 0 to 1 in increments of 0.10. The dependent variable is standardized math test score gains ($q_t - \lambda q_{t-1}$) where the gain varies based on the persistence parameter λ. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS.

APPENDIX A

APPENDIX FOR THE EFFECTS OF CHARTER SCHOOLS ON TRADITIONAL PUBLIC SCHOOL STUDENTS IN NORTH CAROLINA

A.1 Geocoding

School level data files from NCDPI contain latitudes and longitudes for charter and traditional public schools in North Carolina. School latitudes are available starting in 2001 and addresses in 1999. Charter schools start entering in 1998 so I impute addresses from 1999 to 1998. This means that any schools open in 1998 that close in 1999 will not have location information. School universe files are not available for 2016 at the time of this study, so I obtain 2016 address information from the Educational Directory and Demographical Information Exchange (EDDIE) which is maintained by the North Carolina Department of Public Instruction. I then geocode all school addresses that do not have a corresponding latitude and longitude using http://www.gpsvisualizer.com/geocoder/. This website accesses Bing Maps for the actual conversion of addresses to latitude and longitude.

I link each traditional public school to every charter school and calculate distances between each pair to create distance based measures of competition. The STATA command I use is geodist which computes geodetic distances between two pairs of latitudes and longitudes. The geodetic distance is the length of the shortest curve between two points along the surface of a mathematical model of the Earth. There is concern that some school addresses in 1999 and 2000 may be mailing and not physical addresses. If an address contains any post office box information, I assume it is a mailing address and impute 2001 location information to 1999 and 2000. In general, school mailing addresses and physical location are likely to coincide and even if mailing addresses are used, they are likely close in proximity to the actual school.

Because some school-year location information is geocoded by the author and some is already available in the school universe files, there is slight variation in latitudes and longitudes across time for some schools without an address change. Even within the school universe files there is slight variation in latitudes and longitudes, and there are instances when a latitude and longitude for a school will drastically change for one year without an address change. To partially account for this, if a school address does not change over time, I impute the most recent latitude and longitude. I use the most recent assuming that accuracy in geocoding methods has improved over time. Finally, I impute surrounding school-year location information for any large jumps in latitude and longitude within a school. After these revisions, 6.2% of public schools moved more than 0.5 miles sometime between 1998 and 2016; among charter schools the proportion is 15.8%.

A.2 Split Competition Measures

For each measure of competition (such as the number of charters within 10 miles of a traditional public school) two sets of variables are defined based on whether or not period 1 charter schools are taken into account in period 2. The main set of results are reported using *full* measures of competition that include charter schools that opened in period 1 when defining competition in period 2. Specifications are also run using *split* measures where competition is defined separately in each period so effects in period 1 can be interpreted as the effects of charter schools that entered sometime in period 1, and effects in period 2 can be interpreted as the effects of charter schools that entered sometime in period 2 ignoring charter schools that entered in period 1.

The main set of results presented in Table 3.6 use full competition while Table A.1 presents results using split competition measures. This may provide a more direct comparison because then the effects of opening, closing, and expanding charter schools within each period are being compared. By construction any estimates for period 1 are left unchanged when compared to Table 3.6, but any coefficients on variables interacted with period 2 will likely change. Overall, results do not change very much compared to Table 3.6 in terms of sign and significance.

Table A.1: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016 without School Trends: Split Competition Measures

VARIABLES	(1) Math	(2) Math	(3) Read	(4) Read
Attrition 0-1%	0.005*	-0.006	0.003	-0.002
	(0.003)	(0.004)	(0.003)	(0.003)
Attrition >1%	0.003	-0.013	0.010**	-0.004
	(0.006)	(0.008)	(0.005)	(0.007)
Attrition 0-1% * Higher-Achieve CH		0.028***		0.011**
		(0.006)		(0.004)
Attrition >1% * Higher-Achieve CH		0.031***		0.024***
		(0.011)		(0.008)
Observations	4,016,929	4,016,929	3,994,994	3,994,994

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. I assume persistence is one. Standard errors are clustered at the school level.

A.3 Relative Achievement Defined by School Achievement Lags

The primary definition of relative charter and traditional public school achievement used in this paper relies of the lagged test scores of students attending a school at time t. Using lagged test scores allows charter schools that are new to have a measure of achievement in their first year. The average lagged achievement of a new charter school does not exist, so if I were to use lagged school achievement, identifying variation from the opening of new charter schools would be lost. The test scores of students at the end of period t can not be used to construct relative achievement measures because these test scores are outcomes of the competitive process.

The concern with using lagged student test scores is that, in their first year of operation, new charter school achievement will be defined solely based on student test scores while in a different school. Additionally, students and families may not be accurate in assessing the expected quality of a new charter school which may result in noisy measures of relative achievement for the first year charters open. On the other hand, using lagged school level achievement would remove a significant amount of variation in competition measures due to the opening of new charter schools. This may be especially important for distance based measures of competition.

Table A.2 presents results using school lags of achievement rather than the lagged test scores of students in a school at time t to construct relative achievement. Differences between these two measures may suggest that student's expectations of charter school quality before a charter opens is noisy or that the variation from the opening of new charter schools is important to the estimates. Results for both attrition and distance measures of competition reflect those found using student lags which suggest these are not a concern.

Table A.2: First Differenced Regressions of Standardized Student Test Score Gains on Attrition and Distance Competition Measures Interacted with Relative Achievement (Defined by Lagged School Achievement) for Years 1997-2016 without School Trends

VARIABLES	(1) Math	(2) Read	(3) Math	(4) Read
Attrition 0-1%	-0.005*	0.000		
	(0.003)	(0.003)		
Attrition >1%	-0.011	0.011		
	(0.007)	(0.007)		
Attrition 0-1% * Higher-Achieve CH	0.024***	0.010***		
	(0.004)	(0.003)		
Attrition >1% * Higher-Achieve CH	0.034***	0.004		
	(0.010)	(0.008)		
Within 10 1 CH			-0.013	-0.001
			(0.010)	(0.008)
Within 10 2 or More CH			-0.002	0.011
			(0.014)	(0.010)
Within 10 1 CH * Higher-Achieve CH			0.032***	0.019***
			(0.008)	(0.005)
Within 10 2 or More CH * Higher-Achieve CH			0.048***	0.021***
			(0.010)	(0.007)
Observations	3,785,903	3,765,061	3,794,057	3,773,207

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if average lagged TPS achievement is lower than the average lagged achievement of charter schools from which the TPS is facing attrition. Within 10 1 CH indicates if a TPS is within 10 miles of one charter school and one of the closest 10 TPSs to that charter. Within 10 2 or More CH indicates if a TPS is within 10 miles of more than one charter school and one of the closest 10 TPSs to those charter schools. Within 10 miles of no charter is the omitted category. Higher-Achieve CH indicators are one if average lagged TPS achievement is lower than the average lagged achievement of charter schools within 10 miles. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. I assume persistence is one. Standard errors are clustered at the school level.

A.4 Varying Attrition Cut-Off Levels

Table A.3 demonstrates that the choice of attrition cut-offs to define low and high attrition groups does not substantially affect results.

Table A.3: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016 without School Trends: Various Attrition Cut-Offs

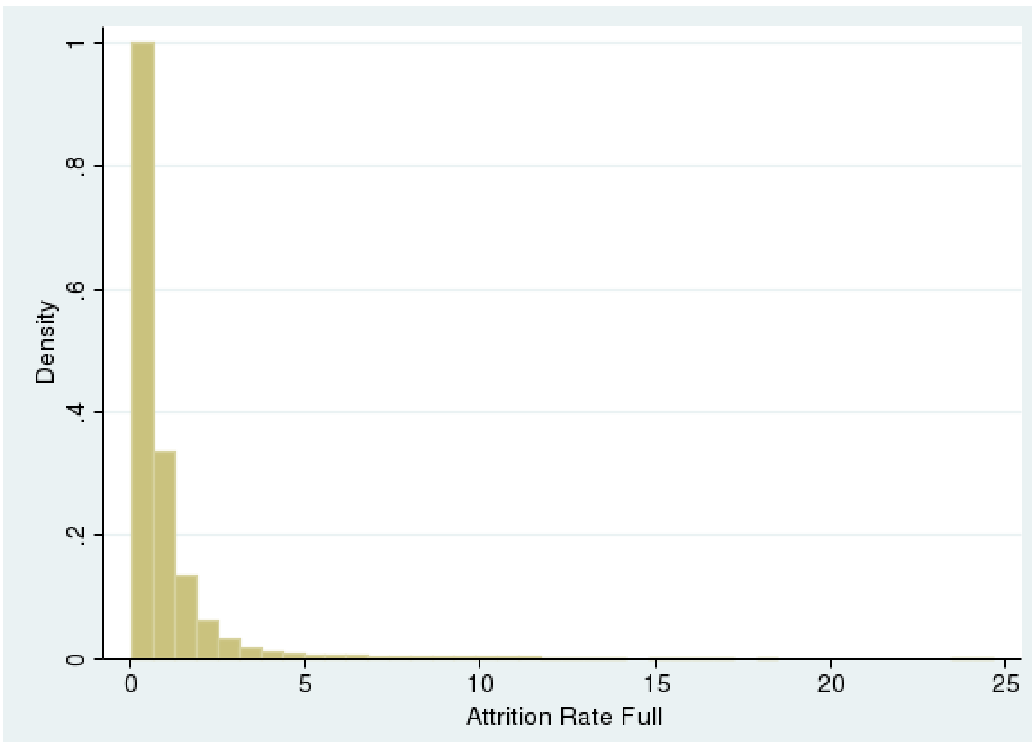
VARIABLES	(1) Math	(2) Math	(3) Math	(4) Read	(5) Read	(6) Read
Attrition 0-1%	-0.007***			-0.002		
	(0.003)			(0.003)		
Attrition >1%	-0.016**			-0.001		
	(0.007)			(0.006)		
Attrition 0-1% * Higher-Achieve CH	0.027***			0.011***		
	(0.004)			(0.003)		
Attrition >1% * Higher-Achieve CH	0.043***			0.017**		
	(0.009)			(0.007)		
Attrition 0-0.75%		-0.007***			-0.002	
		(0.003)			(0.003)	
Attrition >0.75%		-0.012**			0.003	
		(0.005)			(0.005)	
Attrition 0-0.75% * Higher-Achieve CH		0.027***			0.012*	
		(0.004)			(0.003)	
Attrition >0.75% * Higher-Achieve CH		0.038***			0.012**	
		(0.007)			(0.006)	
Attrition 0-0.5%			-0.008***			-0.003
			(0.003)			(0.003)
Attrition >0.5%			-0.009*			0.003
			(0.005)			(0.004)
Attrition 0-0.5% * Higher-Achieve CH			0.026***			0.011***
			(0.004)			(0.004)
Attrition >0.5% * Higher-Achieve CH			0.035***			0.013***
			(0.006)			(0.004)
Observations	4,016,834	4,016,834	4,016,834	3,994,899	3,994,899	3,994,899

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Period2 is an indicator for years between 2006 and 2016. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. I assume persistence is one. Standard errors are clustered at the school level.

A.5 Remove Attrition Outliers

Figure A.1 displays the density of school attrition percents conditional on schools that are facing any attrition. In the regression results, I have removed all students in schools with greater than 5% attrition from the analysis sample. Less than 1% of schools facing attrition have attrition greater than 5%.

Figure A.1: School Level Histogram of Percent Attrition Conditional on Any Attrition



Mean:0.86; Std:1.23 Count:11,879; Min:0.036; Max:24.7

Table A.4: First Differenced Regressions of Standardized Student Test Score Gains on Attrition Competition Measures Interacted with Relative Achievement for Years 1997-2016 without School Trends: No Outliers

VARIABLES	(1) Math	(2) Math	(3) Read	(4) Read
Attrition 0-1%	0.003	-0.007***	0.004*	-0.002
	(0.002)	(0.003)	(0.002)	(0.003)
Attrition >1%	0.005	-0.017**	0.008**	-0.001
	(0.005)	(0.007)	(0.004)	(0.006)
Attrition 0-1% * Higher-Achieve CH		0.027***		0.011***
		(0.004)		(0.003)
Attrition >1% * Higher-Achieve CH		0.042***		0.016**
		(0.010)		(0.007)
Observations	4,005,520	4,005,425	3,983,667	3,983,572

TPS refers to traditional public school, and CH refers to charter school. Dependent variables are standardized traditional public school student test score gains in math or reading. All regressions include grade-year dummy variables, controls for student switching, grade repetition, and school level covariates. Attrition 0-1% indicates if a TPS has strictly greater than 0% but less than or equal to 1% of prior year enrollment non-structurally switch to any CH in the current year. Attrition >1% indicates if a TPS has >1% of prior year enrollment non-structurally switch to any charter in the current year. Attrition equal to zero is the omitted category. Non-structural switchers are students that switch schools when they could have stayed an additional year at their prior school. Higher-Achieve CH indicators are one if the average lagged test scores of students in a TPS are lower than the average lagged test scores of students in charter schools from which the TPS is facing attrition. Dependent and independent variables are first differenced with respect to student-school spells to remove student and school heterogeneity, and the resulting equation is estimated using OLS. I assume persistence is one. Standard errors are clustered at the school level.

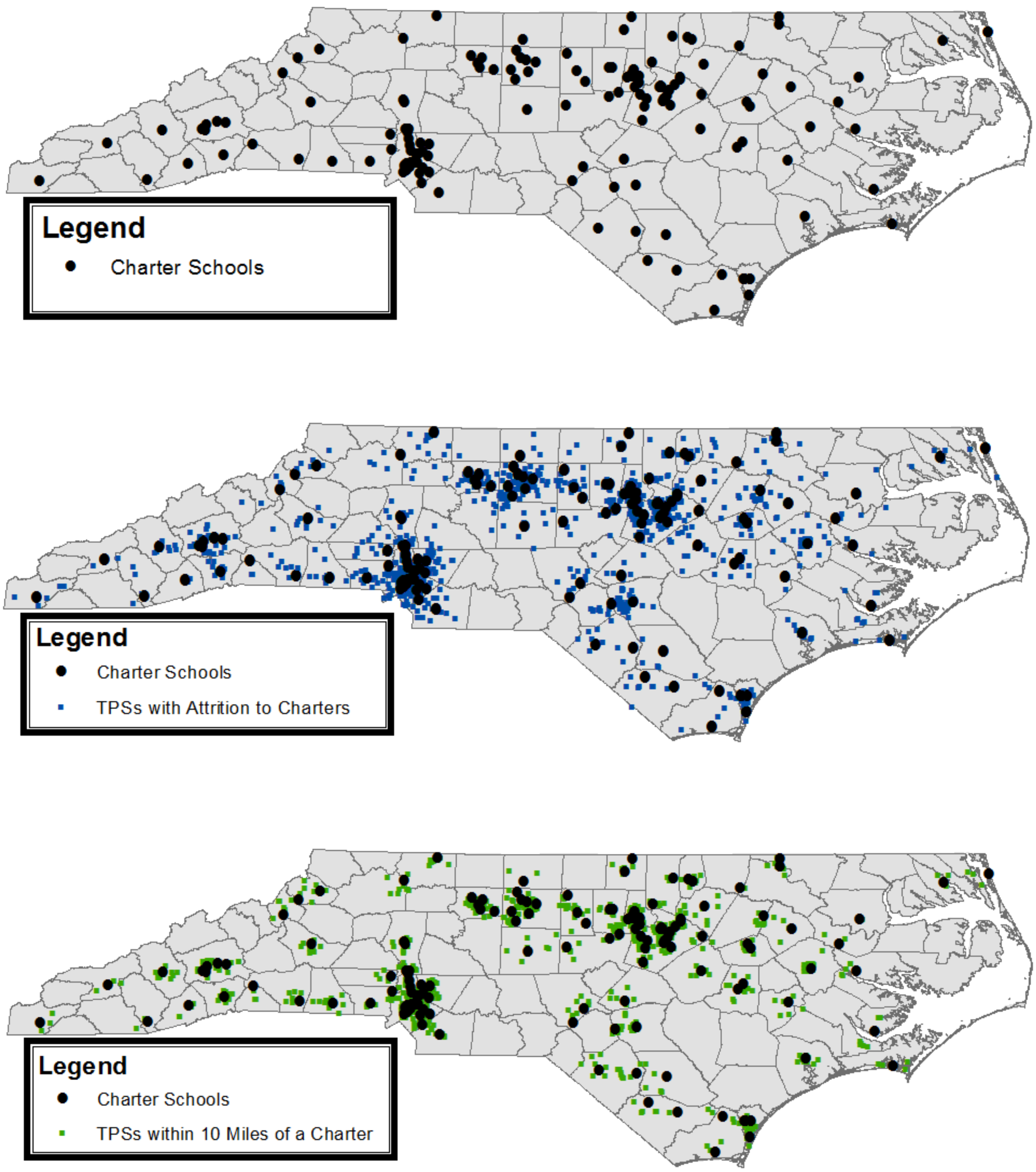
A.6 Charter School Grade-Spans

Table A.5: Count and Proportion of Charter Schools By Grade-Span When First and Last Observed in the Data

	First Year Charter Observed in Data		Last Year Charter Observed in Data	
Grade Span	Freq.	Percent	Freq.	Percent
PK-1	1	0.49	0	0.00
PK-5	1	0.49	0	0.00
PK-8	1	0.49	2	0.97
K-0	1	0.49	0	0.00
K-1	3	1.46	2	0.97
K-2	11	5.34	2	0.97
K-3	14	6.8	3	1.46
K-4	13	6.31	7	3.4
K-5	29	14.08	12	5.83
K-6	28	13.59	15	7.28
K-7	5	2.43	12	5.83
K-8	21	10.19	52	25.24
K-9	1	0.49	6	2.91
K-10	1	0.49	4	1.94
K-11	0	0.00	3	1.46
K-12	5	2.43	31	15.05
1-3	1	0.49	0	0.00
1-5	0	0.00	1	0.49
1-6	1	0.49	0	0.00
1-7	1	0.49	0	0.00
1-8	1	0.49	0	0.00
1-11	1	0.49	0	0.00
1-12	1	0.49	1	0.49
2-8	1	0.49	1	0.49
3-8	0	0.00	1	0.49
4-4	1	0.49	0	0.00
4-8	2	0.97	1	0.49
4-9	1	0.49	1	0.49
5-5	1	0.49	0	0.00
5-6	2	0.97	1	0.49
5-7	1	0.49	1	0.49
5-8	2	0.97	1	0.49
5-12	0	0.00	2	0.97
6-6	2	0.97	0	0.00
6-7	1	0.49	0	0.00
6-8	7	3.4	3	1.46
6-9	2	0.97	0	0.00
6-10	3	1.46	4	1.94
6-11	0	0.00	1	0.49
6-12	11	5.34	12	5.83
7-8	0	0.00	1	0.49
7-10	1	0.49	0	0.00
7-12	3	1.46	1	0.49
8-9	1	0.49	1	0.49
8-11	0	0.00	1	0.49
8-12	2	0.97	2	0.97
9-9	3	1.46	0	0.00
9-10	6	2.91	2	0.97
9-11	2	0.97	1	0.49
9-12	10	4.85	15	7.28
Total	206	100	206	100

A.7 Charter and Traditional Public School Location in North Carolina

Figure A.2: Charter and Traditional Public School Location in North Carolina in 2016: Schools Serving a Grade in 3 through 8



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