FACULTY COMPOSITION AND STUDENT ACHIEVEMENT IN CHARTER SCHOOLS

By

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To Peter, and to my family

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Abstract of Dissertation Presented to the Graduate School
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FACULTY COMPOSITION AND STUDENT ACHIEVEMENT
IN CHARTER SCHOOLS

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Charter schools are independent public schools, free from many of the regulations facing mainstream public schools. They are competitive entrants in partially deregulated education markets. Charter schools offer students and their families more choice in publicly funded education, and they offer teachers more choice in their careers. Studies examining the effectiveness of charter schools *per se* are abundant, but less is known about the qualifications and instructional effectiveness of charter teachers.

I utilize rich, unique data on North Carolina public schools, students, and teachers to develop an in-depth understanding of the state's charter teachers. First, I determine whether charter schools were drawing good teachers away from traditional public schools. I find nuanced patterns of teacher quality flowing to charter schools. Charters drew highly qualified, certified teachers, but low certification requirements attracted less qualified, uncertified teachers as well. Charters drew effective instructors from the pool of mobile teachers willing to change schools, but they did not skim the very best mainstream teachers.

Inexperienced teachers have been cited as a root cause of low student achievement in charter schools. I show that new charter schools in North Carolina had exceptionally high rates of new teachers, but that inexperienced faculties were not responsible for sub-par student achievement. Rather, students had greater math achievement in new charter schools with higher rates of new teachers, relative to charter and mainstream students in

schools with more experienced faculties. These findings suggest that inexperienced charter faculties were benign signals of development, not staffing failures.

Charter schools can exacerbate racial segregation, which tends to widen racial achievement gaps, and more than half of North Carolina's charter schools were racially imbalanced. But there may have been a silver lining, in that predominantly nonwhite charter schools were largely staffed by nonwhite teachers. I show that high rates of nonwhite teachers marginally raised the math performance of black charter students. Nonetheless, these gains were insufficient to slow the growing black-white achievement gap in charter schools.

CHAPTER 1
THE QUALIFICATIONS AND CLASSROOM PERFORMANCE OF TEACHERS MOVING TO CHARTER SCHOOLS

Do charter schools draw good teachers from traditional, mainstream public schools? Using a 1997-2007 panel of all North Carolina public school teachers, I find nuanced patterns of teacher quality flowing into charter schools. High rates of inexperienced and uncertified teachers moved to charter schools, but among certified teachers changing schools, the on-paper qualifications of charter movers were better or no different than the qualifications of teachers moving to comparable mainstream schools. I estimate measures of classroom performance for a subset of grade 3 - 5 teachers, and show that charter movers were more effective in math and reading instruction, relative to other mobile teachers. Charter movers compared less favorably, however, to non-mobile teachers and colleagues within their sending schools. The distribution of classroom performance among future charter teachers, adjusted for sampling error, was significantly lower than the distribution for exclusively mainstream teachers.

1.1 Introduction

Charter schools are independently operated public schools, free from most of the district and state regulations faced by traditional, mainstream public schools. Forty states and the District of Columbia have legislation outlining the establishment, operation, and accountability of charter schools. Charter systems are designed to provide families with more choice in their children's education, to provide teachers with more choice in their career paths, to promote innovative instruction, and to target special populations of students that may be under-served by traditional public schools. A charter program represents a new, competitive branch of publicly funded education that entrusts each campus with a degree of autonomy rarely seen in mainstream schools. Autonomy and flexible resource allocation in charters schools may draw good teachers away from the mainstream. A growing body of research has characterized the qualifications of the *stock* of charter teachers, who compare favorably to mainstream teachers in some respects (college selectivity, for instance) but not others (experience, certification). I complement and advance this literature by analyzing the qualifications and classroom performance of the *flow* of North Carolina teachers moving from mainstream to charter schools over the years 1997-2007.[1]

Charter schools, playing the role of competitive entrants in partially deregulated public education markets, are expected to spur efficiency gains by decreasing industry concentration and challenging incumbents (here, traditional public schools) to improve performance. Proponents of charter schools, and school choice more generally, expect competition between traditional and choice schools to drive up the quality of education overall. Friedman (1955, 1997) proposed vouchers as one way to stoke school competition. Dee (1998), Hoxby (2003), and most recently, Booker, Gilpatric, Gronberg, and Jansen (2008) offer empirical evidence that mainstream student performance improves in light of competition from choice schools. Long-run gains from competition will require charters to be formidable competitors, however, and the jury is still out as to whether they actually increase student learning relative to mainstream schools. The emerging consensus is that new charters have a negative impact on student achievement growth, and this penalty fades as schools and students gain experience.[2]

Teacher quality is a profound factor in student achievement,[3] and charters seeking to produce high achievement (or at least, meet accountability standards) will value effective teachers. Charter schools are heterogeneous by nature; some specialize in priming the

[1] I refer to school years by the year of their conclusion. For instance, 2007 references the 2006-2007 school year.

[2] See, e.g., Bifulco and Ladd (2006), Booker, Gilpatric, Gronberg, and Jansen (2007), Hanushek, Kain, Rivkin, and Branch (2007), and Sass (2006).

[3] Rivkin, Hanushek, and Kain (2005) and Rockoff (2004) find that a one standard deviation increase in teacher quality increases student achievement by about ten percent of a standard deviation.

gifted and college-bound, while others target students at risk of failure. Recruiting and retaining high-quality teachers will be more difficult for the latter group. The teacher mobility literature is thick with evidence of teacher preferences for high-performing and socioeconomically advantaged school environments.[4]

Charter schools may have an advantage in the teachers' labor market, regardless of their student composition. "They are free to recruit the best teachers and to raise money from foundations, corporations, and individuals" (Manuel (2007)). Charters are not generally bound by state pay scales, they can allocate budgets as they see fit, and feasibly, they can pay higher teacher salaries. One New York City charter school famously offers teacher compensation packages in excess of $125,000 (Gootman (2008)). Nationwide, charter teacher salaries are more comparable to mainstream salaries,[5] but in some states, charter teachers earn significantly less than other public school teachers with similar qualifications (Malloy and Wohlstetter (2003)). Even if charter schools cannot outbid mainstream schools on salary alone, school leaders can influence teachers' utility in non-pecuniary ways, by reducing their non-instructional duties, encouraging collegiality among faculty, manipulating class size and composition, and granting teachers more creative license and autonomy than they are afforded in mainstream schools. Early advocates of the charter model stressed the professionalization and empowerment of

[4] Findings were fairly consistent across source data and specifications: teachers were more likely to exit schools with larger proportions of black students (but to a lesser degree for black teachers) or lower student achievement. See Clotfelter, Ladd, Vigdor, and Diaz (2004), Hanushek, Kain, O'Brien, and Rivkin (2005), Falch and Strøm (2005), and Scafidi, Sjoquist, and Stinebrickner (2007).

[5] Nationally, charter teachers had an average salary of $37,000 in 2004, versus $44,500 for traditional public school teachers. The pay gap coincided with a substantial experience gap: 43.4 percent of charter teachers had three or fewer years' experience, compared to just 17.1 percent of mainstream teachers (National Center for Education Statistics (2006)). Podgursky and Ballou (2001) and Hoxby (2002) also found competitive teacher salaries in charter schools.

teachers as critical tenets of charter development (see, for example, Budde (1988) and Kolderie (1990)). High teacher satisfaction rates in charter schools typically stemmed from greater autonomy ("freedom to teach the way I want"), like-minded colleagues, and innovative teaching philosophies. Teachers who were dissatisfied in charter schools cited low pay, lack of benefits, high workload, and insufficient facilities (Malloy and Wohlstetter (2003)).

In practice, the intangible benefits of working in a charter school may be too low to offset low pay and other resource limitations. Common charter finance models allocate each school a per-pupil rate roughly equal to the surrounding district's average per-pupil cost, excluding the cost of buildings. If a district enjoys substantial economies of scale in variable cost, its per-pupil expenses will be less than a charter school's average cost. Charters with competing uses for limited resources may sacrifice some teaching talent in favor of administrative and capital improvements if doing so maximizes their objectives (student achievement, enrollment, and budget size being likely objectives). Furthermore, many states allow charters to employ a high rate of uncertified teachers. This permits charters to attract teachers from outside the traditional pipeline, but also increases the supply of low-cost, low-skilled individuals eligible to work in charter schools, including uncertified mainstream teachers nearing the expiration of temporary licenses. Recently, Wisconsin raised subject-based certification requirements for its charter teachers, prompting school leaders to argue that they could not afford to hire teachers meeting the new standard (Borsuk (2008)). Charter licensure requirements vary across states, and little is known about the qualifications of uncertified teachers in charter schools, or the impact of relaxed licensure standards on student performance in charter schools.

Much of the developing research on charter teacher quality examines the qualifications, workload, and job satisfaction of the stock of charter teachers nationwide or within particular states. Podgursky and Ballou (2001) surveyed teachers in seven states, and found that charter teachers were less likely to be certified, more likely to be inexperienced, and more likely to have merit pay than mainstream teachers. Hoxby (2002), using a 1998 national survey of teachers, showed that charter teachers had typically taken more math and science courses in college, were more likely to have graduated from a good college, and logged more extracurricular hours. Interestingly, charters paid a premium for these qualities, but not for certification or master's degrees. Taylor (2005) also failed to find a premium for advanced degrees in Texas charter schools, and showed that teachers realized a 7.5 percent pay cut upon moving to a charter school.

While a picture of teacher quality in charter schools is emerging, little is known about the flow of teaching talent between mainstream and charter schools, or the classroom performance of individual charter teachers. Here, I fully characterize the resume qualifications of all North Carolina public school teachers who moved to the charter sector between 1998 and 2007. For a subsample of elementary teachers, I characterize their classroom performance as well. North Carolina is a rare setting where passively collected administrative data include longitudinal school assignments for all charter and mainstream personnel over a period exceeding ten years. Furthermore, the data link some teachers directly to their students, allowing me to estimate measures of instructional effectiveness. By analyzing the flow of teachers from one sector to another, I determine whether charter schools were "cream skimming" good teachers from mainstream schools. If highly qualified and effective teachers were voting with their feet in favor of charter schools, their migration is a favorable signal of the decentralized model's appeal, and mainstream schools may need to emulate charter features to retain faculty. If charters were drawing less qualified and less effective teachers, whether because of low pay, poor organization, or relaxed licensure standards, the charter model is unlikely to fulfill its promise as a revolutionary vehicle for the improvement of public schools.

In this study, I evaluate the resume qualifications of North Carolina charter movers against the qualifications of teachers moving between mainstream schools, controlling for receiving school profiles. Charter movers were less experienced than other moving teachers on average, but were also more likely to have at least twenty-five years' experience. Charter schools were attracting teachers with high licensure test scores, but only among certified, regularly licensed teachers. Uncertified teachers moving to charter schools, a large minority, substantially attenuated the average qualifications of all charter movers. Resume qualifications are, at best, incomplete signals of teacher quality. For a subset of elementary grade teachers, I also evaluate their classroom performance directly, using estimated fixed effects of individual teachers on student end-of-grade math and reading exam scores. Charter movers were low in sending school distributions of classroom performance, relative to their colleagues, but compared favorably to teachers moving to similar mainstream schools. I complement these estimated mean differences in teacher quality with analyses of the variance and distribution of teacher quality, dissected from the variance in sampling error. Quality distributions for future charter teachers largely overlapped quality distributions for exclusively mainstream teachers, but were centered at significantly lower figures.

These findings neither affirm nor reject the effectiveness with which North Carolina's current charter model draws good teachers from mainstream schools. The system attracted highly qualified, certified, and effective teachers, but low licensure requirements attracted uncertified, less qualified teachers who may have had few career options in the mainstream sector.

1.2 Charter Schools in North Carolina

1.2.1 Background

North Carolina is an ideal setting to study teacher mobility into charter schools. The state's charter system is eleven years old, its schools are spread throughout urban, rural, and socioeconomically diverse regions of the state, and extensive data (described fully in section 1.2.2) have been collected over an eleven-year period for all mainstream and charter teachers in the state. These data allow me to characterize the on-paper qualifications of every teacher moving to the state's charter sector, and to estimate the classroom performance of many elementary charter movers. Charter legislation and oversight in North Carolina (described below) bear many features in common with other states' charter systems. The largest drawback of researching North Carolina's charter system in this context is its scale: the state has a 100-school cap,[6] and accordingly, a very small percent of teachers moved to charter schools in a given year. The comparison group – mainstream teachers moving to other mainstream schools – was large and varied, as were the schools they moved to, so charter and mainstream movers have common support for identification of their relative quality. The comprehensive treatment of all charter data in the state, together with the unique ability to estimate teachers' individual classroom performance over several years, makes North Carolina the best available setting for the purposes of this study.

The North Carolina legislature authorized the state's system of charter schools in 1996. There are several stated objectives of the system, including increased learning opportunities for students and new professional opportunities for teachers.[7] The state's first thirty-four charter schools opened for the 1998 school year. Table 1-1 documents the growth of North Carolina's charter system from that year up to 2007.[8] Charter students accounted for 2.0 percent of statewide enrollment by 2007, and charter teachers accounted for 2.1 percent of public school teachers. Figure 1-1 illustrates the widespread geographic range and concentration of charter enrollment in 2006. Charters were active throughout the state, though less so in the rural eastern counties. Four of the state's largest counties – Durham, Forsyth, Mecklenburg, and Wake – had the largest charter presence in terms of

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[6] The cap has been binding since 2001, although not all approved charters are active in a given year. Even with the cap, the charter system will continue to grow as schools add grades and campuses.

[7] NC Gen. Stat. 115c-238.29a (1996)

[8] Some charter schools appear to be missing from the 2007 data.

absolute enrollment. But as the second panel illustrates, charter penetration was high in several suburban and rural counties outside of the major population centers.[9]

The application, approval, and evaluation of charter schools is closely regulated, but the schools are given wide latitude in their personnel management and daily operations. Charter schools are organized as private, nonprofit organizations.[10] They are allotted funding from state and local boards of education on a per-pupil rate, commensurate with district per-pupil costs. There are restrictions, however, on how government allotments can be used for real property and classroom facilities.[11] Charters can raise additional funds by winning grants or soliciting donations, but they cannot charge tuition. Financial difficulties are common in North Carolina's charter schools. Twenty-four charters were relinquished or revoked between 1998 and 2006; of those, nine cited financial problems as a leading cause of failure.

A distinctive feature of North Carolina's charter school system is a pronounced racial segregation.[12] Just over half of the state's charter schools had racially imbalanced student populations, meaning that the percent of students who are nonwhite was twenty percentage points above or below the county-wide nonwhite representation. By comparison, just one-quarter of mainstream schools were racially imbalanced. Charter schools are required to "reasonably reflect the racial and ethnic composition of the

[9] The State Board of Education limits annual charter growth within any one district.

[10] The nonprofit requirement does not preclude for-profit education management organizations like KIPP from granting franchises in North Carolina.

[11] State funds may be used to lease property, facilities, and equipment for charter schools, but not for "any other interest in real property or mobile classroom units." Loans made to charter schools do not have the backing of any taxing authority. NC Gen. Stat. 115C-238.29H(a1) (1996)

[12] Bifulco and Ladd (2006) fully explored the segregating effects of North Carolina's charter program.

general population"[13] residing in the school district (which, for most schools, is a county district). Exception is given to schools serving a targeted population, in which case, their student body is required to resemble the racial and ethnic composition of the local target population. Many charters target gifted or at-risk students, two groups which are overrepresented by white and nonwhite students, respectively, and so the segregation of the state's charter schools is an unsurprising, albeit unforseen, consequence of the program's design.

Charter schools are held to the same accountability model as mainstream public schools, with some exceptions for charters in their first year. Each year, North Carolina schools are awarded recognition labels according to their students' performance and growth on end-of-grade and end-of-course exams.[14] In 2006, 53 percent of charter schools were given one of the lowest three recognition labels, compared to 48.1 percent of public schools statewide (Manuel (2007)). But charters were well-represented at both extremes of the performance distribution. In 2006, charters were 2.6 times as likely as mainstream schools to have performance composites (reflecting the percent of students at grade level) lower than 50,[15] *and* 2.3 times as likely to have performance composites above 90. The wide variance in racial composition and student proficiency among charter schools

[13] NC Gen. Stat. 115C-238.29F(g)(5) (1996)

[14] "School of Excellence," "School of Distinction," and "Low-Performing School" are three examples of 2006 recognition labels. A school's recognition is based on the percent of students who performed at grade level on year-end exams (the schools' performance composite), whether or not the school met state-mandated growth expectations, and whether or not students made "adequate yearly progress," a metric related to the federal No Child Left Behind Act. Schools are then charged with the coming year's growth expectations, which will in part determine the coming year's recognition. (North Carolina State Board of Education (2006, HSP-C Series))

[15] Schools with sub-50 performance composites that fail to make expected growth benchmarks are given "Low-Performing" recognition. Schools with repeat low-performing status, including charter schools, must collaborate with evaluation teams assigned by the State Board of Education to develop corrective action plans. Charters with sub-60

underscores the importance of controlling for student body composition and school-wide performance when evaluating the quality of teachers moving to charter schools. Each school's profile will affect the type of candidates willing to work there.

Charter schools are allowed great flexibility in the recruitment, retention, and pay of their faculties. The state imposes very little regulation on who can teach in a charter school. At least 75 percent of charter teachers in kindergarten through fifth grade classrooms must hold teaching certificates. This number falls to 50 percent for charter teachers of grades six through twelve.[16] Uncertified teachers are much less common in mainstream schools. Only certified teachers are eligible for tenure after four consecutive years of teaching in a mainstream public school. Tenured mainstream teachers who wish to teach in a charter school are granted one year's leave, meaning that they can return to their original school after a year, space permitting. Charters are not required to offer tenure, nor are they required to participate in the state retirement plan.

Low licensure requirements for charter faculties were put in place to attract new, nontraditional teachers from fresh sources - non-teaching vocations, Teach for America, and so forth. Recent work by Kane, Rockoff, and Staiger (2008) and Boyd et al. (2006) suggest there is little difference between the quality distributions of certified, alternatively certified, or even uncertified teachers.[17] States vary in their treatment of teacher licensure in charter schools. Of the forty states with active charter systems in 2008, fifteen required

performance composites for three consecutive years are denied 10-year renewal. (North Carolina State Board of Education (2006, EEO-U Series))

[16] Uncertified charter teachers are expected to meet the federal definition of "Highly Qualified." Although this requirement does not appear to be strictly enforced, uncertified teachers can meet the standard by majoring or passing a Praxis II exam in their subject area.

[17] We would expect this to be true within schools if administrators hire equivalently skilled candidates regardless of licensure. It may be the case, however, that low licensure standards put downward pressure on the across-school distribution of teacher quality.

all charter teachers to be certified. Others, like North Carolina, held each faculty to a minimum percentage. Only Arizona, Washington, D.C., and Texas placed no restrictions on charter teacher certification. (Center For Education Reform (2008))

1.2.2 Data

I use data covering the universe of North Carolina public schools, students, and teachers over the years 1997 to 2007. The data are maintained by the North Carolina Education Research Data Center at Duke University, in collaboration with the state Department of Public Instruction.[18] School-level variables include yearly performance composites, school size, and student demographic statistics from the NCES Common Core. For teachers and students, the Data Center processes a vast amount of detailed, passively collected administrative data. Each public school teacher and student is assigned a unique, anonymous identifer, allowing researchers to build longitudinal panels and track teachers and students across schools. I collected teachers' demographic information and school assignments from student activity reports. These reports are detailed records of every activity involving students and public school personnel, including charter school personnel. Unfortunately, this is nearly the extent of data available for charter school teachers. Nonetheless, the North Carolina data provide a rare, comprehensive picture of the flow of labor between mainstream and charter schools. For mainstream teachers, I collected additional information from personnel files: experience, education, type of licensure, and licensure test scores. I merged data from school activity reports and several personnel files to produce a longitudinal panel of public school teachers spanning the years 1997-2007. School-wide statistics (grades served, school age, and quintile indicators for student body demographics and proficiency) were merged with the teacher panel to provide a robust statistical picture of teachers' work environments and career paths.

[18] See Muschkin, Bonneau, and Dodge (2008).

Two features of North Carolina public school data are especially valuable for this study. First, I can track teachers as they change schools. This allows me to evaluate teachers' resumes at the point when they moved to a new school or opted into the charter sector. Second, many mainstream elementary students can be reliably matched to their classroom teacher. I utilize this link between the inputs and observable production of student achievement to estimate teacher fixed effects. Thus, for a subset of mobile teachers, I evaluate their classroom performance in addition to on-paper qualifications.

Table 1-2 describes teacher mobility patterns between charter and mainstream schools for the 5,346 teachers who were working in a charter school at some time between 1998 and 2007. The majority, 55.1 percent, were never observed teaching outside of the charter sector. Another 33.6 percent taught in a mainstream public school before moving to a charter. Of these, two-thirds moved directly to a charter school, without leaving the panel between schools. The remaining third taught in a mainstream school, left the panel for one or more years, and then re-entered in a charter school. I focus on direct movers, who were continuously employed over their transition to the charter sector. I compare the qualifications of direct charter movers to the qualifications of other teachers making direct moves between mainstream schools.[19] By evaluating charter movers against other mobile teachers (as opposed to public school teachers more generally), I avoid selection biases from omitted variables contributing to mobility *per se*, and I can judge what sort of talent charter schools were drawing from the pool of teachers willing to change schools. After controlling for receiving school characteristics, I determine if charter schools were recruiting more or less of each specific teacher characteristic than their mainstream

[19] Seventy-eight percent of mainstream movers did not leave the panel between schools. The results to follow in sections 1.3.1 and 1.3.2 are qualitatively similar, but with varying statistical precision, if I adopt more liberal definitions of teacher mobility. The section 1.3.3 analysis approximates the distribution of persistent teacher quality among all pre-charter teachers, whether or not they took breaks between schools.

counterparts. Results shed light on the quality and qualifications of teachers flowing into charter schools, and implicitly, the degree to which charter schools competed for good teachers.

1.3 Analytic Methods and Results

1.3.1 Qualifications of Teachers Moving to Charter Schools

Table 1-3 lists summary statistics for North Carolina's mainstream public school teachers from 1997 to 2007. Teachers were identified as school personnel with teaching assignments in school activity reports, excluding teaching assistants, facilitators, and DARE officers. I determined the highest degree attained by each teacher: 30.8 percent of teachers held a post-baccalaureate degree of some kind. A teacher's degree-granting institution was "competitive" if it was classified as such (or "competitive plus," "very competitive," etc.) by the 1995 edition of *Barron's Profiles of American Colleges*.[20] Just over three-quarters of North Carolina teachers graduated from a competitive college or university. North Carolina teachers take a variety of licensure exams, most of which are in the Praxis family. In order to include all available test information, I scaled raw licensure test scores to have a mean of zero and standard deviation of one within each test code and test year. I calculated the mean standardized licensure test score for each teacher, equal to the average of all of her unique exams records.[21] Regularly licensed teachers had completed an approved teacher education program and passed the Praxis Series of exams, or attained licensing by reciprocal or interstate agreement. The complements to regularly licensed teachers were uncertified teachers holding temporary, emergency, or

[20] The 1995 edition roughly corresponds with the graduation date of mobile teachers with six years (the median) of experience.

[21] Although exams were scaled to have mean zero, teacher test scores were positive on average (0.030). This is probably reflective of selective survival and longevity among active teachers. That is to say, teachers with higher exam scores stayed in the panel longer.

provisional licenses.[22] Teaching experience was derived from teachers' pay level code, or if that was missing, imputed empirically where reasonable. Teachers' race, gender, and school assignment were determined from school activity reports. Teachers assigned to multiple schools in any one school year were not included in the panel.[23]

Mobile teachers, summarized in the second column of Table 1-3, were earlier in their careers, on average, and less likely to have a graduate degree than teachers who were not changing schools.[24] Mobile teachers had lower licensure test scores than non-movers, by 0.015 standard deviations. Mainstream teachers moving to charter schools, summarized in the fourth column of Table 1-3, were typically less qualified than other moving teachers. Teachers transitioning to the charter system were 4.5 percentage points less likely to have graduated from a competitive college or university, and 9.9 percentage points less likely to be regularly licensed.[25] North Carolina's policy of permitting more uncertified teachers in charter schools may have had the consequence of drawing untenured mainstream teachers

[22] In compliance with the federal No Child Left Behind Act, all North Carolina teachers of core subject areas were to be "Highly Qualified" by June 30, 2006. With some exceptions, Highly Qualified teachers are fully licensed, hold at least a bachelor's degree, and demonstrate competence in each subject they teach. For mainstream elementary teachers, this effectively eliminated every alternative licensure path, including lateral entry. But North Carolina, along with nine other states and the District of Columbia, failed to make adequate progress toward staffing every class with a Highly Qualified teacher by the 2007 school year, and no state made total progress (Feller (2006)). I find virtually no change in teacher certification rates in the later years of the panel.

[23] This affected a non-trivial percent of teacher-year observations (6.1), including teachers with roving assignments and teachers who switched schools mid-year.

[24] A mobile teacher is defined throughout as one observed in school a in year t and school $a' \neq a$ in year $t+1$.

[25] Charter movers were also more likely than mainstream movers to have missing license data (8.1 versus 4.8 percent) The main results are unaffected if I classify these individuals as unlicensed. A more problematic data quality issue is the high rate of missing licensure test scores among charter movers (11.8 versus 7.8 percent for mainstream movers). Below, I discuss where this could affect results and the sensitivity tests I used to evaluate potential biases.

nearing the expiration of their temporary licenses. I observed 1,142 teachers moving directly to charter schools - of these, 20.9 were uncertified, twice the rate of uncertified teachers moving to another mainstream school. Charter movers were less experienced as well, by 1.47 years on average, and they were 11.4 percentage points more likely to have three or fewer years of teaching experience. Interestingly, charter movers were also more likely to have *at least* twenty-five years of experience. The rate of nonwhite teachers was much higher among charter movers (26.4 percent) than among mainstream movers.

Figure 1-2 illustrates comparative kernel densities for the teaching experience of mobile teachers, by charter/mainstream destination. Clearly, charter movers were more likely to have just a few years of experience, relative to mainstream movers. They were also more likely to have around 30 years of experience. In the lower panel of Figure 1-2, limited to regularly licensed movers, the bimodality of charter teachers' experience is more pronounced. The distribution of charter movers' licensure test scores in Figure 1-3 also hints at a noisy bimodality, but more importantly, the distribution of *licensed* charter movers' licensure scores (lower panel) appears to be to the right of mainstream movers' distribution. Charter movers, particularly those with regular licensure, were somewhat more likely than their mainstream counterparts to have high licensure test scores, 0.5 - 2.0 standard deviations above the mean. The visual difference is not entirely due to noise. Wilcoxon rank-sum tests indicate that score distributions were not significantly different between charter and mainstream movers overall, but that licensed charter movers' scores were significantly greater than those of licensed mainstream movers.

The nonparametric evaluation of charter movers' qualifications, together with the parametric analyses to follow, provide a rich, descriptive picture of the value that teachers took with them when they moved to the charter sector. But this picture is incomplete without an understanding of the schools these teacher were moving between. If charter movers were highly qualified relative to other mobile teachers, but leaving low-performing schools, this would have very different policy implications than if less qualified teachers were leaving high-performing schools. I calculated the change in performance composite – the percent of students performing at grade level – between a mobile teacher's new and old school. Mainstream movers typically realized a 1.9 percentage point increase in proficiency upon changing schools, but charter movers realized a 0.2 percentage point loss. This suggests that teachers were not exploiting the charter sector to "trade up" to relatively high-performing schools. Mobile teachers moved to schools with higher rates of white students, regardless of whether they were moving to a charter or mainstream school, but this pattern was much more pronounced for charter movers. Teachers moving to charter schools realized a 9.2 percentage point increase in the rate of white students, on average, compared to 3.7 percentage points for mainstream movers.

Figures 1-2 and 1-3 offer visual depictions of the range of experience and achievement that teachers brought with them when they moved to the charter system, but comparative kernel densities do not permit the conclusion that charters were attracting more or less qualified teachers than *similar* mainstream schools. Toward that end, I conduct more parametric analyses of charter and mainstream movers by estimating Equation 1–1 via ordinary least squares for each North Carolina teacher (j) observed in year t (1997-2007), school s, and county l:

$$Q^k_{jslt} = \delta^m_{jt}\mathbf{1}(moving) + \delta^c_{jt}\mathbf{1}(tocharter) + \mathbf{X}^r_{jsl(t+1)}\theta^r + \alpha_{l(t+1)} + \varepsilon_{jslt} \tag{1–1}$$

Equation 1–1 is a reduced form expression for qualification k, where k indexes the on-paper qualifications summarized in Table 1-3: graduate degree, competitive college education, mean licensure test score, regular licensure, and three measures of experience.[26]

[26] Equation 1–1 was estimated separately for each qualification k. An alternative would have been to project teachers' mobility onto the space of their qualifications and sending school characteristics to get a sense of the factors affecting the supply of charter school teachers. I emphasize the reduced-form empirical strategy to underscore the descriptive, non-causal inference gained by examining a relatively small set of idiosyncratic labor decisions. As a robustness check, I also estimated a multinomial logit equation predicting

All mobile teachers had the indicator $\mathbf{1}(moving)$ equal to one. Teachers moving to a charter school additionally had $\mathbf{1}(tocharter)$ equal to one. Coefficients $\hat{\delta}^m_{jt}$ are the estimated difference in k between mainstream movers and non-movers. The coefficient of interest, $\hat{\delta}^c_{jt}$, is an estimate of the difference in qualification k between teachers moving to charter and mainstream schools. Controls included receiving school characteristics ($\mathbf{X}^r_{jsl(t+1)}$), dummy variables for missing school data, and receiving county-by-year effects ($\alpha_{l(t+1)}$).[27] If charter schools had higher demand for some qualifications, and were able to outbid comparable mainstream schools by manipulating employment terms and working conditions, then $\hat{\delta}^c_{jt}$ would be positive. If $\hat{\delta}^c_{jt}$ was insignificant or negative, then charters had lower demand, or were unable to realize an advantage in the teachers' labor market. Receiving school characteristics included dummy variables representing student body size and composition quintiles (the percent who were nonwhite, the percent performing at grade level, and total enrollment), the range of grades served, and a set of dummy variables controlling for missing data. These variables controlled for school environments that affected the type of candidates drawn to a particular school. County-by-year effects controlled for unobserved heterogeneity in regional variables, like non-teaching job opportunities. Robust standard errors allowed for clustering within each sending school and year.

More experienced teachers may seek graduate degrees or additional certifications to increase their pay, so I controlled for teacher experience categories (indicators for less

the likelihood of different types of school changes. Results suggested that the relative risk of moving to a charter school significantly increased for less experienced and unlicensed teachers, and significantly decreased for teachers with higher licensure test scores. These findings are in agreement with the reduced-form results to follow.

[27] Results were qualitatively similar when I controlled for sending and receiving school characteristics. Specifications with receiving school characteristics alone are preferred, as they better depict the relative flow of teacher qualifications to charter and mainstream schools of comparable size and student composition.

that three years' experience or more than twenty-five years' experience) when estimating Equation 1–1 for licensure and education variables. Since licensed and unlicensed teachers may have different incentives to consider charter schools, I limited Equation 1–1 to regularly licensed movers and produced separate "licensed mover" estimates of δ^m_{jt} and δ^c_{jt} for all qualifications except licensure itself.

Table 1-4 lists estimates of δ^c_{jt} and δ^m_{jt} for each resume qualification.[28] The first column of Table 1-4 lists coefficient estimates for δ^m_{jt}, the typical difference in qualification k between teachers moving to mainstream schools and non-moving teachers, controlling for receiving school characteristics. Estimates of δ^m_{jt} serve as the baseline to which δ^c_{jt} estimates are compared. Movers were significantly different than non-movers with respect to each qualifications, with the exception of graduate degrees. Movers were much less experienced, by 3.69 years on average, than their non-moving counterparts. They were 13.2 percentage points more likely to have three years' experience or less, and 10.2 percentage points less likely to have at least twenty-five years' experience.

The second column of Table 1-4 presents estimates of δ^c_{jt} from Equation 1–1. Column II coefficients answer the question, "were charter movers more or less qualified than teachers moving to comparable schools?" The qualifier is important, given the heterogeneity of charter school working environments. With respect to graduate education, licensure, and years of experience, charter movers were significantly less qualified. They were 2.7 percentage points less likely to hold a graduate degree, relative to mainstream

[28] Unreported coefficients for school variables and teacher experience were largely unsurprising. Relative to schools in the top quintile of grade-level performance, teachers from schools in lower quintiles were successively less likely to hold a graduate degree, a competitive college pedigree, or regular licensure. They were less experienced, and had lower licensure test scores. The same can be said for teacher qualifications in schools in the higher quintiles of percent nonwhite students, with the exception of graduate degrees. Teachers in schools with higher proportions of non-white students were more likely to hold a graduate degree. High school teachers tended to have higher qualifications, relative to middle and elementary school teachers.

movers, and 7.4 percentage points less likely to be licensed. Since mainstream movers were themselves less likely to be licensed, relative to non-movers, the coefficient estimates from the licensure equation suggests that charter movers were 8.6 percentage points less likely to be licensed than non-moving teachers. Charter movers were less experienced than mainstream movers by 1.07 years, and less experienced than non-moving teachers by 4.76 years. Despite low average years of experience, charter movers were *more* likely than other moving teachers to have at least twenty-five years' experience. This is in agreement with the bimodal pattern of experience seen in Figure 1-2.

Column I and II results were generated from analyses of all public school teachers, regardless of their licensure status. Fully licensed teachers may have had more options in the mainstream sector than unlicensed, untenured teachers. Accordingly, licensed teachers' mobility decisions better relect revealed preference. Columns III and IV list results from the subsample of licensed teachers, who accounted for 89.0 and 79.1 percent of mainstream and charter movers, respectively. Limiting the sample had little effect on results for mainstream movers; point estimates were not economically different between columns I and III. But excluding unlicensed teachers from the analysis substantially affected conclusions about the relative qualifications of charter movers. In contrast to column II, column IV results show that licensed charter movers were not statistically different than licensed mainstream movers with respect to graduate degrees or years' experience. The difference between the full and limited sample is particularly stark for licensure test scores. The coefficient for licensed charter movers in the test score equation is positive and significant.[29] Teachers moving to charter schools typically had higher test scores than

[29] The estimated difference in charter and mainstream movers' mean licensure test scores could be attributed to selection bias. Charter movers had significantly higher rates of missing licensure test data than mainstream movers (11.8 and 7.8 percent, respectively), though less so if they were licensed (5.5, 4.0). Licensure test scores proxy for each teacher's underlying knowledge. If teachers with missing test score data came from lower in the underlying distribution, then the average observed test score of all teachers

their licensed colleagues, whether or not those colleagues were changing schools. Licensed teachers moving to a charter school had an average test score 9.4 hundredths of a standard deviation higher than that of mainstream movers. By comparison, licensed mainstream movers had lower test scores than non-movers, by 2.3 percent of a standard deviation.

It is sensible that charter schools would have been able to attract teachers with higher licensure test scores. Conditional on licensure itself, test scores are not rewarded in the state's pay scale. Yet a teacher's test scores are good indicators of how well his or her students will do on their own tests. Goldhaber (2007) and Clotfelter, Ladd, and Vigdor (2007) have shown that North Carolina teachers with higher licensure test scores were associated with higher student achievement on end-of-grade math and reading exams. Charter schools, perhaps recognizing teacher test scores as good signals of teacher quality, had more success recruiting individuals with higher test scores than comparable mainstream schools.

These findings raise the possibility that teachers viewed the charter sector as a low-cost job change preceding retirement or a permanent career change. Sample attrition was high among new teachers, experienced teachers nearing retirement, and uncertified teachers,[30] and these are the same groups I observed disproportionately flowing into

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would be biased upwards from the true average of underlying knowledge. If charter movers with missing test data came from much lower in the underlying distribution than mainstream movers with missing data, the estimated difference between charter and mainstream movers' test scores would be biased in favor of the charter movers. I simulate situations like this to gage the sensitivity of charter movers' test score advantage to various counterfactual scores for teachers with missing data. The result that licensed charter movers had significantly higher test scores was robust up to a 0.5 standard deviation penalty for charter movers with missing test data. More than a 2.0 standard deviation gap was necessary to produced the result that licensed charter movers had significantly lower test scores than mainstream movers.

[30] There were 89,311 uncensored sample exits in the North Carolina teachers' panel. Of these, 28.8 percent had three or fewer years' experience, 37.7 percent had at least twenty-five, and 30.6 percent were uncertified or had missing license data.

the charter sector. Following a school change, charter movers with three or fewer years' experience stayed in the sample an average of 2.53 years (uncensored), compared to 2.89 years for mainstream movers. The difference was statistically significant, but represented just a 10% gap in duration post-move. The post-move duration of uncertified charter movers and those with more than twenty-five years' experience was not statistically different from the average post-move duration of the equivalent groups of mainstream movers. So the charter sector does not appear to have been a strong substitute for attrition among unlicensed or highly experienced teachers, but inexperienced charter movers did tend to leave North Carolina public schools somewhat faster than other teachers changing schools.

The results discussed in this section lend some support to the idea that charter schools had a realized advantage in the labor market for public school teachers; among licensed teachers changing schools, charters were better able to attract highly experienced teachers, and teachers with high licensure test scores.[31] But a large minority of mainstream teachers moving to charter schools were not fully licensed and attenuated the average qualifications of charter movers. This was likely a consequence of the state's low licensure requirements for charter schools. What remains to be seen is if the migration of uncertified teachers to the charter sector was predominantly driven by the charters' demand for low-cost labor, or by uncertified teachers' willingness to supply it.

Some resume line-items like licensure test scores are robust signals of teacher quality, and charter schools were effectively competing for licensed teachers with relatively high test scores. But were charter schools drawing teachers with histories, and not just signals,

[31] These results were not driven by the 8.5 percent of charter teachers who moved from the mainstream to charter sector, and then back to the mainstream. Excluding this group from the above analyses reinforced results, particularly with respect to licensure test scores.

of actual classroom effectiveness? In the following section, I evaluate the classroom performance of charter movers who could be reliably matched to the students they taught.

1.3.2 Classroom Performance of Teachers Moving to Charter Schools

Teachers' on-paper qualifications are readily observable to schools and econometricians, but the performance of their students is of greater value when assessing teacher quality. North Carolina students in the 3rd through 8th grades take end-of-grade (EOG) exams each spring. Each student has an exam proctor, whose name is recorded along with the student's test scores, demographic and socioeconomic information, and survey responses. For test-takers in elementary grades, exam proctors are usually classroom teachers. The Data Center matches proctor names with encrypted teacher identifiers used in other files, and then links these identifers to student test data. I utilize this valuable feature of the data to assess the classroom performance of mainstream grade 3 - 5 teachers who ultimately moved to the charter sector.[32] I estimate teacher fixed effect coefficients in EOG test score regressions, and then evaluate fixed effect estimates in the same way that I analyzed on-paper qualifications above – by parametrically comparing the fixed effects of charter movers, mainstream movers, and non-movers.

The proctor associated with each student's test score was not necessarily his or her classroom teacher. To minimize the likelihood of bad teacher-student matches, I focused on teachers with self-contained classrooms of students in grades 3 - 5. Self-contained classrooms embody the traditional structure of elementary education, where a class of students spend all or the majority of each day with one teacher. I assembled grade 3 - 5 student EOG records for more than 2.8 million student-years spanning 1997 to 2007. A teacher-student match was considered invalid if any of the following four conditions were met. In parentheses are the percent of students for which each condition was true.

[32] Charter school students are included in the EOG test data; however, more than half of all charter EOG records are missing a teacher/proctor identifier.

1. The student's proctor was unknown or not found in the assembled teacher panel.[33] (19.5%)

2. The student's proctor did not have a self-contained classroom assignment. (21.8%)

3. The grade-g student's proctor did not have a teaching assignment with students in grade g. ($< 1.0\%$)

4. The student's exam group was larger than 30 or smaller than 5. ($< 1.0\%$)

The remaining 58.3 percent of students had a proctor who was a teacher, and who led a self-contained classroom with students in the same grade as a reasonable number of EOG test-takers linked to that teacher. These limitations lend considerable validity to each allowed teacher-student match.[34] Of the 122,064 EOG test-taking classrooms with a known teacher, 71.3 percent were considered valid matches. North Carolina's end-of-grade exams are interval-scaled, meaning that a one-point increment reflects the same difference in learning anywhere on the scale of raw scores. Scores are comparable within and across grades each year, and the minimum proficient score rises for each grade. I scaled raw scores to have mean zero and standard deviation equal to one within each year, grade, and school. This calibrated the dependant variable of each education production function to account for variance in the range of raw scores over time, heteroscedasticity in raw scores across grades, and distributional shifts in student performance across schools due to Tiebout (1956) sorting.

[33] The teacher panel excluded teaching assistants and facilitators, who may have proctored exams.

[34] Restricting the sample to self-contained classrooms, while necessary to ensure valid teacher-student matches, probably resulted in a non-representative subset of classrooms. Scaled scores from self-contained classrooms were significantly lower than scores from other classrooms. The mechanism behind this gap is unclear; one possibility is that higher-quality districts and schools were more likely to adopt modern, departmentalized classroom structures. This would be problematic for the current study if the gap between performance in self-contained and departmentalized classrooms was systematically different for future charter teachers, but I did not find this to be the case.

Consider the following equation describing the standardized test score Z^k_{ijcgst} in subject k (math or reading) for student i in teacher j's classroom c, grade g, school s, and year t:

$$Z^k_{ijcgst} = \mathbf{A}_{ict}\beta_A + \bar{\mathbf{A}}_{-ict}\beta_{\bar{A}} + \mathbf{T}_{jt}\beta_T + \mathbf{X}_{st}\beta_X + \theta_j + \alpha_{lt} + \varepsilon_{ijcgst} \tag{1–2}$$

Variables in $\mathbf{A}_{ict}$ are student characteristics, including race, gender, parental education, and learning disability indicators. $\bar{\mathbf{A}}_{-ict}$ is a vector of average student characteristics in i's classroom (excluding student i), and $\mathbf{T}_{jct}$ controls for two measures of teacher inexperience. $\mathbf{X}_{slt}$ contains school-level variables, including quintile indicators for student body size and composition, grade levels, and a dummy variable equal to one when student i is in a new school. The coefficients θ_j and α_{lt} are teacher fixed effects and county-by-year effects, respectively.[35] I estimated Equation 1–2 and saved estimated teacher fixed effects, $\hat{\theta}_j$. Coefficient estimates for Equation 1–2 were unsurprising. Female students had lower math but higher reading scores than males. Nonwhite students had lower scores in both subjects, as did students without college-educated parents. Learning disabilities were strongly associated with lower scores, more so for disabilities directly related to the tested subject. Students with inexperienced teachers had lower test scores in both subjects, especially if their teacher was in her first year as opposed to her second or third. The penalty from teacher inexperience fell about 80 percent following a teacher's first year.

In this setting, teacher fixed effects are interpreted as each individual's history of classroom performance relative to expectations, given the composition of her students, intraschool sorting, and the teacher's own experience. This should be important to schools looking to hire teachers with a record of success in raising student test scores, but does not necessarily permit the interpretation of $\hat{\theta}_j$ as a transitive index of teachers' inherent quality or value added. The latter view relies on two strong assumptions: (1) $\hat{\theta}_j$

[35] With controls for teacher inexperience, estimated teacher fixed effects will account for any penalties that are common to all new teachers.

are consistent estimates of θ_j, and (2) errors, ε_{ijcgst}, are uncorrelated with θ_j. The first assumption is invalid for fixed effect estimates generally, which are inconsistent in short panels (Cameron and Trivedi (2005)). Although teacher fixed effect estimates benefit from multiple student-level signals each year, finite class size leads to considerable sampling error. Teacher fixed effect estimates are noisy, and their variance overstates the true variance in teacher quality (Rockoff (2004)). I address this in the following subsection by isolating the variance in persistent teacher value added. The second assumption is invalid if there are unmeasured student variables affecting test scores, like motivation or inherent intelligence, and if these variables systematically affect the teacher to whom a student is assigned. In that case, estimates of θ_j will be a reflection of teacher quality *and* student sorting. Positive matching, such that better students were assigned to better teachers, would bias $\hat{\theta}_j$ away from zero and overstate a teacher's effectiveness or ineffectiveness. Negative matching, which may be the case if better students were assigned to struggling teachers to ease their burden, would bias $\hat{\theta}_j$ toward zero. Clotfelter, Ladd, and Vigdor (2006) found evidence of teacher-student matching, particularly positive matching, in North Carolina schools. The bulk of student sorting was Tiebout (1956) sorting between schools, which I addressed by centering the Equation 1–2 dependant variable by year, grade, and school.

Nonetheless, Equation 1–2 fails to control for nonrandom sorting of students within schools, such as would be the case if parents were successfully lobbying school administrators to put their children in particular classrooms.[36] Two adaptations to Equation 1–2 - estimating student gains, or including lagged student test scores - would address likely pathways by which students were non-randomly matched to teachers within schools. Either method would eliminate 3rd grade teachers from the analysis here, an

[36] The existence of "teacher shopping" by parents has considerable anecdotal and analytical support. (see, e.g., Hui (2003), Crombie (2001), and Clotfelter et al. (2006))

impractical solution given the fairly small number of charter movers for which $\hat{\theta}_j$ can be estimated.[37] Furthermore, estimating student gains, even with student fixed effects, is not a fail-safe method for examining teachers' value added (Rothstein (2008)), nor will it circumvent the inherent sampling error of teacher fixed effects. It is important to emphasize that sorting biases would only have affected the analysis to the degree that charter movers were disproportionately subject to non-random within-school student sorting prior to their move. Estimated teacher fixed effects provide insight to the relative performance of teachers' classrooms, which would be of interest to potential receiving schools.

I estimated more than 28,000 teacher fixed effects for both subjects. There were 13,752 mobile teachers in the sample, 257 of which were moving to a charter school. Teacher fixed effects reflect teachers' relative performance within their schools. This limits the scope of interpretation and understates the variance in teacher quality across schools, but adequately addresses between-school Tiebout (1956) sorting.[38] Table 1-5 summarizes teacher fixed effects estimates. Teachers moving to other mainstream schools had lower fixed effects than non-movers, on average. Charter movers had even lower fixed effects than mainstream movers, by 3.7 percent of a standard deviation in math. For context, the charter-mainstream mover gap in fixed effects represented about 78.5 percent of the 0.047 standard deviation gap between male and female math performance, as estimated by

[37] As a robustness check, I estimated Equation 1–2 with lagged student achievement. This reduced the number of charter movers for whom fixed effects could be estimated, but nonetheless, the finding that charter movers had relatively high within-school math fixed effects was robust. Results for reading fixed effects, however, were statistically insignificant.

[38] Average fixed effects were 24 - 38 percent larger in absolute value when students' raw scores were calibrated to mean statewide performance rather than mean school-wide performance.

Equation 1–2. So charter schools were not drawing mainstream schools' best teachers, as measured by teacher fixed effects.

Summary statistics from Table 1-5 indicate that teachers who moved to charter schools were relatively low in the distribution of teacher quality within their sending mainstream schools. But these simple means do not control for the type of schools teachers were moving to, and charter schools may have attracted relatively high-performing teachers, compared to the flow of labor going to comparable mainstream schools. I regressed teacher fixed effect estimates against mobility indicators, receiving school characteristics, and receiving county-by-year effects:

$$\hat{\theta}_j^k = \delta_{jt}^m \mathbf{1}(moving) + \delta_{jt}^c \mathbf{1}(tocharter) + \mathbf{X}_{jsl(t+1)}^r \theta^r + \alpha_{l(t+1)} + \varepsilon_{jslt} \qquad (1\text{–}3)$$

Subjects (math and reading) are indexed by k, teachers by j, schools by s, counties by l, and years by t. Table 1-6 presents estimates of δ_{jt}^m and δ_{jt}^c. Column I lists the estimated difference in fixed effects between mainstream movers and non-movers ($\hat{\delta}_{jt}^m$ in Equation 1–3). Mobile teachers moving between mainstream schools tended to have lower math and reading fixed effects than non-moving teachers, by 1.8 and 1.4 percent of a student-level standard deviation, respectively. Column II lists the estimated difference in classroom performance between charter and mainstream movers. Charter movers' fixed effects were estimated to be 4.5 percent of a standard deviation higher in math and 4.0 percent higher in reading, relative to those of teachers moving to comparable, albeit mainstream schools. Equation 1–3 coefficients indicate a reverse of the charter-mainstream mover performance gap observed in the descriptive statistics of Table 1-5. Columns III and IV list analogous results for the subsample of regularly licensed teachers. Point estimates for charter and mainstream movers were largely unchanged by excluding unlicensed teachers.[39]

[39] The rate of regular licensure was much higher among elementary-grade teachers.

Class performance is one dimension, along with licensure test scores, where charter schools may have been able to exercise a competitive advantage in the teachers' labor market. North Carolina's public education pay scale does not have provisions for merit pay, with the notable exception of $1,500 bonuses for teachers in schools meeting "exemplary" growth standards. Charters had the allocative freedom to recruit and reward high-performing teachers, budget permitting. I find that charter schools were not drawing relatively high-performing teachers, when measured against their sending school colleagues. But teachers who moved to the charter sector were more effective than teachers moving to comparable schools, in terms of size, student proficiency, and racial composition. These findings were not reconciled by charter movers' lower propensity to "trade up" to a school with higher student proficiency. Controlling for sending and receiving school characteristics (answering the question, "were charter movers more effective than teachers *leaving* and *moving to* comparable schools?") decreased point estimates and statistical significance, but left intact the conclusion that charter movers had higher classroom performance than mainstream movers.

1.3.3 Variation in Classroom Performance

Teacher fixed effects are strong predictors of student achievement, but collectively, they yield a poor approximation of the overall distribution of teacher quality. Sampling error from finite panel length and class size cause the variance of teacher fixed effects to overstate the variance of true value added. If sampling error disproportionately affects certain groups of teachers (new teachers, for instance), then the variance and transitivity of teacher quality distributions would be distorted. This subsection evaluates the quality of teachers from a second-moment perspective and explicitly accounts for the inflating effects of sampling error. I compute the variance in persistent teacher quality across all teachers, then separately for future charter teachers and exclusively mainstream teachers. Results complement the mean differences in classroom performance discussed in the previous subsection. First, I estimated Equation 1–2, omitting teacher fixed effects (θ_j).

$$Z^k_{ijcgst} = \mathbf{A}_{ict}\beta_A + \bar{\mathbf{A}}_{-ict}\beta_{\bar{A}} + \mathbf{T}_{jt}\beta_T + \alpha_{lt} + e_{ijcgst} \tag{1–4}$$

Suppressing notation for grade and school, the errors are $e_{ijct} = \theta_j + \varepsilon_{ijct}$. There are two components to each student residual, e_{ijct}: teacher j's persistent value-added (θ_j), and non-persistent noise (ε_{ijct}) encompassing sampling error and transient shocks to average classroom performance. The average student residual for each class can be expressed like so:

$$\hat{\bar{e}}_{jct} = \theta_j + \frac{1}{N_{jct}} \sum_{i=1}^{N_{jct}} \varepsilon_{ijct},$$

where N_{jct} is class size in year t. If θ_j and $\bar{\varepsilon}_{jct}$ are independent, the variance of $\hat{\bar{e}}_{jct}$ across teachers can be decomposed into the the variance of persistent value added and the variance of non-persistent error: $\mathbb{E}[\hat{\bar{e}}^2_{jct}] = \sigma^2_\theta + \sigma^2_\varepsilon$, where σ^2_θ is the variance of persistent teacher quality within schools, and σ^2_ε is the variance of error within schools. Consider two average residuals from two different classrooms taught by the same teacher: $\hat{\bar{e}}_{jct}$ and $\hat{\bar{e}}_{jc't'}$, where $c \neq c'$ and $t \neq t'$. If θ_j and ε_{jct} are uncorrelated, and if the measurement errors, ε_{jct} and $\varepsilon_{jc't'}$, are uncorrelated as well, then

$$\mathbb{E}[\hat{\bar{e}}_{jct}\hat{\bar{e}}_{jc't'}] = \sigma^2_\theta.$$

The assumption that θ_j and ε_{jct} are uncorrelated is non-trivial – in fact, it is one of the assumptions that must be met in order to interpret estimated teacher fixed effects as part of a transitive index of teacher quality. Positive matching of better students with better teachers will increase estimates of σ^2_θ. Additionally, omitting teacher fixed effects in Equation 1–4 may bias other coefficients if they are correlated with θ_j; this, in turn, will bias estimated residuals, $\hat{\bar{e}}_{ijct}$. Calibrating the dependant variable by school in Equation 1–4 limits biases from between-school sorting, but within school teacher-student matching patterns may nonetheless affect σ^2_θ estimates. So long as the correlation between θ_j and ε_{jct} is not systematically different for subsamples of interest, the calculated variance of persistent residuals within groups can be compared. Following Carrell and West (2008), I estimate σ^2_θ by computing the average covariance of all classroom residual pairs between teacher j's class c in year t and $c' \neq c$ in year $t' \neq t$:

$$\hat{\sigma}^2_\theta = \left[\sum_{j=1}^{J} \sum_{c=1}^{C_j} \hat{\bar{e}}_{jct} \hat{\bar{e}}_{jc't'} \right] / N \tag{1–5}$$

J is the number of teachers, C_j is the number of classes taught by teacher j, and N is the number of pairs.

Table 1-7 presents estimates of total and signal standard deviations.[40] All standard errors (in parentheses below each standard deviation estimate) were estimated by bootstrap, with an equal number of charter participants and non-participants selected in each sampling. The third and sixth columns of Table 1-7 list the standard deviation of teacher fixed effects, by group. As expected, estimates of the variation in persistent teacher residuals (signal) were much smaller than the variation in teacher fixed effects (SD(FE)). The latter suggests that a one-standard-deviation increase in teacher quality improved student math performance by 0.237 standard deviations, three times the bonus from having an experienced teacher rather than a new one. But judging by the preferred measure of dispersion, a one-standard-deviation increase in teacher quality would yield a still-substantial 0.179 standard deviation increase in student math performance, closer to the difference between having a college-educated parent versus a parent with "some college." Estimates of signal variance were somewhat smaller for reading; a one-standard deviation increase in persistent teacher quality was predicted to increase

[40] A modification to this procedure yielded notably different results. First, I estimated σ^2_θ using student, rather than classroom-averaged residuals. I computed the average covariance of all residual pairs between student i in teacher j's classroom c and $i' \neq i$ in teacher j's classroom $c' \neq c$. Estimates of signal variances were remarkably similar under this alternative method. Estimates of total variance in individual student residuals were 2 - 3 times the size of those shown in Table 1-7.

student achievement by 0.137 standard deviations. Signal variation was 48 to 61 percent of total variation, suggesting that differences in teacher quality accounted for much, but not all, of the variation in class performance within schools. Table 1-7 also lists signal standard deviations separately for future charter teachers and teachers who were never observed in a charter school. The variance of math performance was wider for future charter teachers, but the variance of reading performance was narrower. Both statistics were within two standard errors of the corresponding signal estimate for exclusively mainstream teachers.

Following Kane et al. (2008), I constructed a simple Bayesian shrinkage estimator to account for sampling error in class residuals attributed to teacher quality. I used estimates of signal and noise variance listed in Table 1-7, along with the number of classes observed for each teacher (C_j) to scale average class residuals ($\bar{e}_j$):

$$\tilde{\theta}_j = \bar{e}_j \left(\frac{C_j}{C_j + \hat{\sigma}_\varepsilon^2 / \hat{\sigma}_\theta^2} \right) \tag{1–6}$$

Equation 1–6 shrinks each teacher's average residual towards zero according to the terms in parentheses. Residuals for teachers with more classes and groups with larger signal-to-noise ratios were scaled by less, since their residuals were expected to be less affected by sampling error.

In section 1.3.1, I showed that a high rate of charter movers were uncertified and under-qualified. This would not be problematic for charter schools if certified and uncertified teachers were part of the same underlying distribution of teaching quality, so long as charters did not draw heavily from the lower end. In that case, low licensure requirements may be one way to retain effective teachers in public education. In agreement with Kane et al. (2008), I find little observable difference in teacher quality distributions between licensure groups, but a fairly wide (albeit scaled) variance within each group. For both subjects, Wilcoxon rank-sum tests failed to reject the hypotheses that teachers with regular and temporary licenses were drawn from the same distribution of persistent teacher quality. Figure 1-4 plots comparative densities of teacher quality by charter participation. The persistent quality distribution for future charter teachers was significantly lower than that for exclusively mainstream teachers, especially for math. Figure 1-4 provides further evidence that teachers flowing into the charter sector typically had lower relative class performance within their schools than exclusively mainstream teachers.

1.4 Conclusions

A founding purpose of North Carolina's charter legislation was to "create new professional opportunities for teachers."[41] I examine the value that mainstream teachers brought with them when they took advantage of these opportunities and moved to charter schools. In terms of resume line-items like experience, education, licensure, and licensure test scores, I find mixed evidence that charters were hiring good teachers away from mainstream schools. Teachers moving to charter schools were more likely to be inexperienced, but they were also more likely to have at least twenty-five years of experience. Among regularly licensed teachers, the licensure test scores of charter movers were substantially better than those of mainstream movers. But a large minority of teachers without regular licensure attenuated the average qualifications of charter movers. Low licensure standards for charter faculties had the consequence of drawing into the charter system a high rate of uncertified teachers, some of which may have been nearing the expiration of temporary licenses and running low on career options in mainstream schools. Alternatively or temporarily licensed teachers are not necessarily bad teachers (see Goldhaber (2007) and Kane et al. (2008)), although in North Carolina they are associated with lower student achievement (Clotfelter et al. (2007)). I find no significant difference in persistent teacher quality between broad licensure types, but a wide variance of quality within each type. Ideally, charter schools would recruit uncertified teachers

[41] NC Gen. Stat. 115c-238.29a(4) (1996)

who were high in the quality distribution but unable or unwilling to attain traditional certification. This would help to retain effective teachers in public schools. Some states allow charter teachers to meet a compromised certification standard (much like the federal standard for Highly Qualified uncertified teachers), contingent on their education, teaching experience, and performance on alternative certification exams. To the extent that these signals are correlated with underlying teacher quality, compromised certification standards would induce charter schools to attract relatively high-quality uncertified teachers from mainstream schools.

For a limited sample of grade 3 - 5 teachers, I estimate measures of teacher quality using classroom performance on standardized end-of-grade exams. Were charter schools cream skimming more effective teachers? Charters were not skimming above-average teachers from the stock of school faculties, although according to the more parametric classroom performance analyses of section 1.3.2, charters may have been drawing higher-ranked teachers from the flow of teachers changing schools. This is not to say that the teachers who moved to charter schools held a common deficiency in class performance. I show that the distribution of future charter teacher quality, even when its persistent component was formally dissected, was wide and largely overlapped the quality distribution of teachers who never taught in a charter school.

Table 1-1. North Carolina charter schools, students, and teachers, 1998-2007

Year	Schools	(%)	Students	(%)	Teachers	(%)
1998	34	(1.7)	4,642	(0.4)	330	(0.5)
1999	57	(2.7)	8,555	(0.7)	601	(0.8)
2000	76	(3.6)	12,691	(1.0)	862	(1.1)
2001	90	(4.1)	15,523	(1.2)	1,086	(1.4)
2002	93	(4.2)	18,235	(1.4)	1,292	(1.6)
2003	93	(4.2)	20,420	(1.5)	1,390	(1.7)
2004	93	(4.1)	21,955	(1.6)	1,509	(1.8)
2005	97	(4.3)	25,248	(1.8)	1,669	(1.9)
2006	97	(4.2)	27,441	(1.9)	1,789	(2.0)
2007	92	(3.9)	27,700	(2.0)	1,894	(2.1)

Notes: Each count of charter schools, students, and teachers represents the indicated percent (%) of all public schools, students, or teachers in the sample. Teacher and school counts were tabulated from the teachers' panel, described fully in section 1.2.2. Student counts were tabulated from NCES Common Core data.

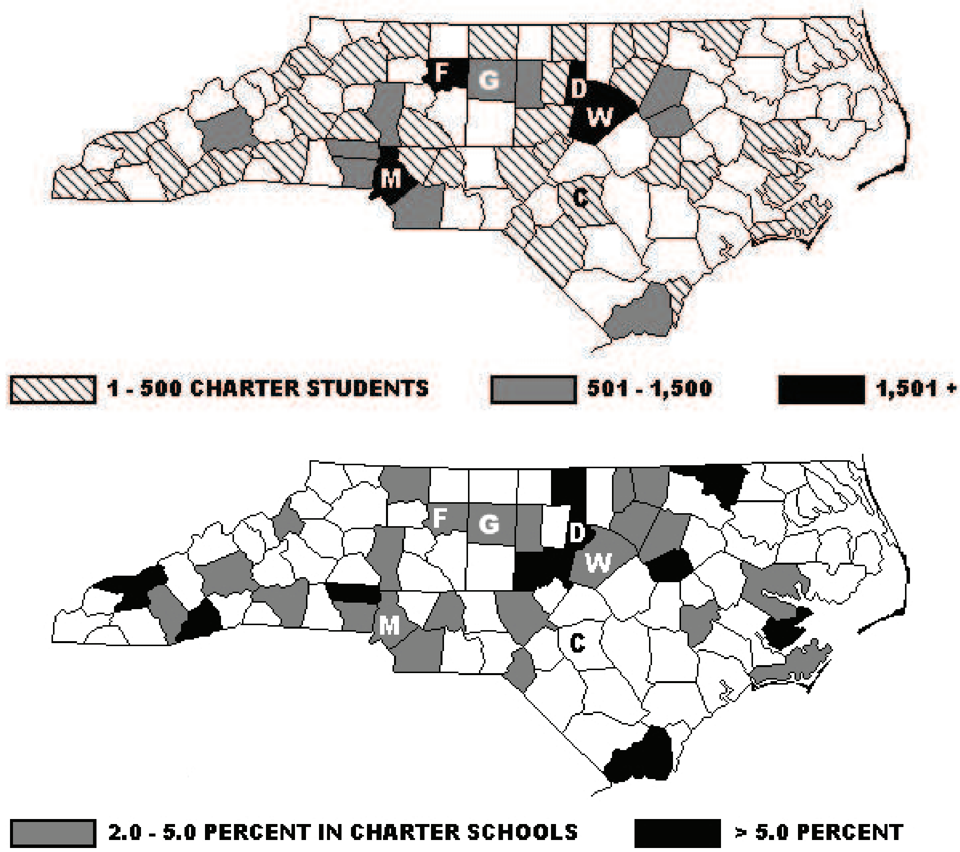


Figure 1-1. 2006 Charter enrollment and penetration. Major population centers in Cumberland (C), Durham (D), Forsyth (F), Guilford (G), Mecklenburg (M), and Wake (W) counties.

Table 1-2. In-sample mobility patterns of charter teachers

Teacher Mobility pattern	Percent
Started and ended in the charter system (right censored)	21.5
Started and ended in the charter system (uncensored)	33.6
Mainstream to charter	25.1
Mainstream to charter to mainstream	8.5
Charter to mainstream	10.7
Other patterns	<1.0

Notes: n = 5,346 teachers. The first two mobility patterns apply to teachers who taught exclusively in charter schools. Right censored charter teachers entered the sample in the charter system and were still teaching there in 2007, the last year of the panel. Uncensored teaching spells ended before 2007. The following four mobility patterns apply to teachers who taught in charter and mainstream schools. The percent of all charter participants who followed each pattern is indicated at right.

Table 1-3. North Carolina public school teachers: Summary statistics

Teacher qualification	All teachers	All movers	Mainstream movers	Charter movers
Holds graduate degree (%)	30.8	27.5	27.5	25.2
	(46.2)	(44.6)	(44.7)	(43.4)
Attended competitive college (%)	76.2	75.0	75.1	70.6
	(42.6)	(43.3)	(43.3)	(45.6)
Mean licensure test score	0.030	0.015	0.015	0.029
	(0.857)	(0.839)	(0.838)	(0.905)
Regularly licensed (%)	89.5	88.8	89.0	79.1
	(30.7)	(31.6)	(31.3)	(40.7)
Teaching experience (years)	11.93	8.89	8.92	7.45
	(9.92)	(8.72)	(8.71)	(8.97)
Experience $\leq$ 3 years (%)	26.1	36.3	36.1	47.5
	(43.9)	(48.1)	(48.0)	(50.0)
Experience $\geq$ 25 years (%)	14.5	7.5	7.5	8.9
	(35.2)	(26.3)	(26.3)	(28.5)
Nonwhite (%)	16.9	18.1	17.9	26.4
	(37.4)	(38.5)	(38.3)	(44.1)
Female (%)	79.8	79.8	79.8	79.2
	(40.1)	(40.2)	(40.2)	(40.6)
n (teacher-years)	886,343	58,629	57,487	1,142

Notes: Standard deviations appear in parentheses below each mean. Data for moving teachers are evaluated in the year immediately preceding a school change.

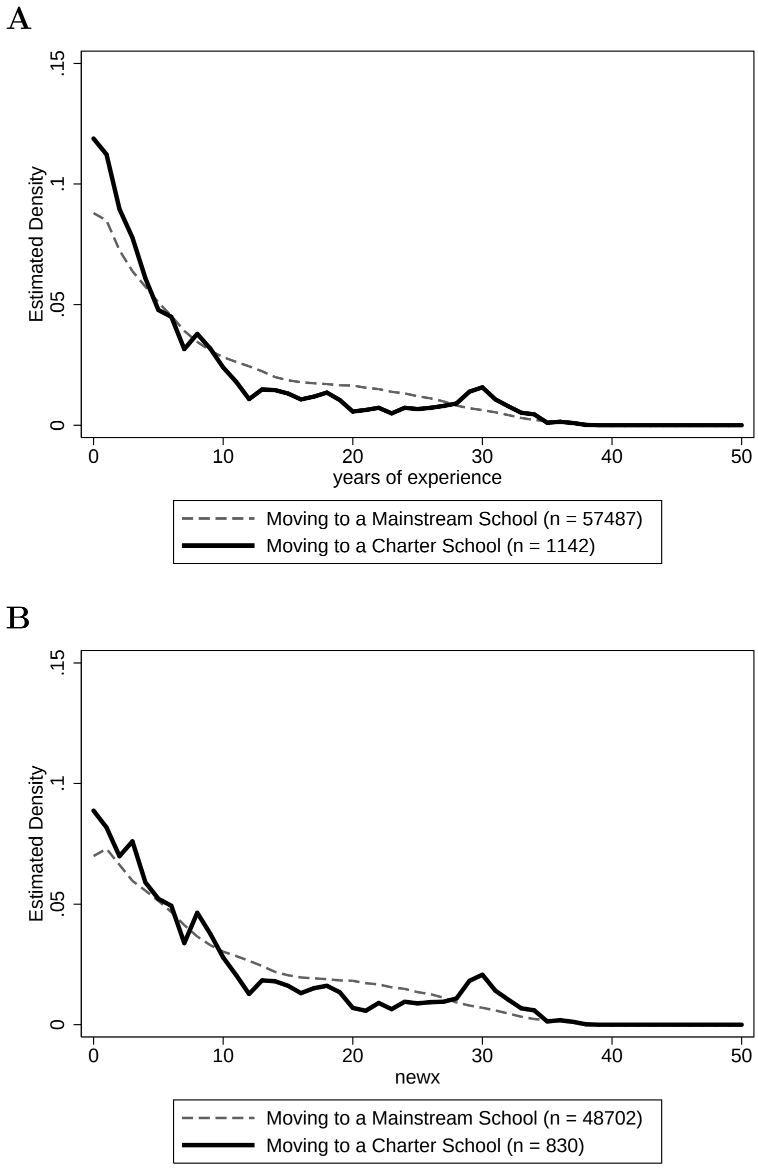
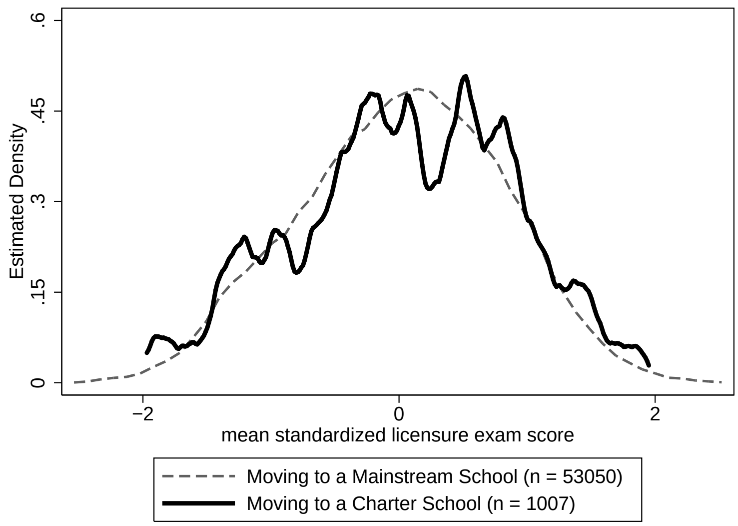


Figure 1-2. Density estimates: Years' experience of all mobile teachers (A) and of licensed mobile teachers (B). Densities were estimated using Epanechnikov kernel functions and halfwidths of 0.50 years.

A



B

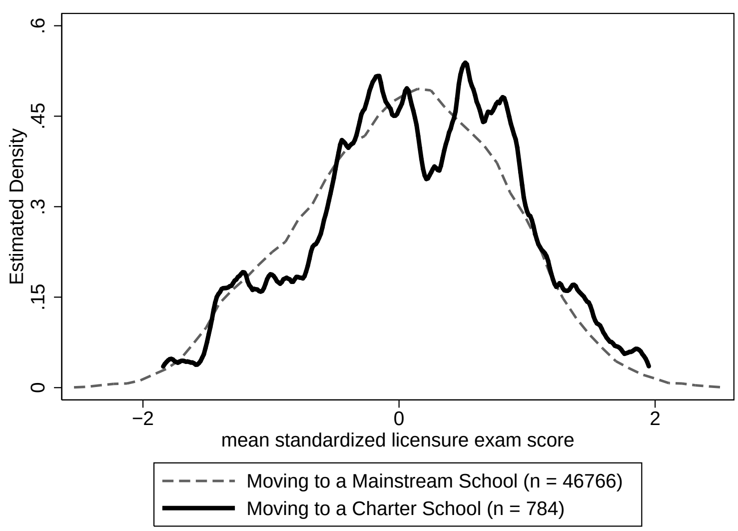


Figure 1-3. Density estimates: Mean licensure test scores of all mobile teachers (A) and of licensed mobile teachers (B). Densities were estimated using Epanechnikov kernel functions and halfwidths of 0.05 standard deviations.

Table 1-4. Regression results: Qualifications of teachers changing schools, by licensure and charter/mainstream destination

	I	II	III	IV
Sample	All	All	Licensed	Licensed
Destination	Mainstream	Charter	Mainstream	Charter
(Equation 1–1 coefficient)	($\hat{\delta}^m_{jt}$)	($\hat{\delta}^c_{jt}$)	($\hat{\delta}^m_{jt}$)	($\hat{\delta}^c_{jt}$)
Graduate degree	0.003	-0.027	0.007	-0.028
	(1.41)	(2.03)	(3.06)	(1.70)
Competitive college	-0.011	-0.016	-0.006	0.003
	(5.63)	(1.20)	(2.88)	(0.21)
Licensed	-0.012	-0.074		
	(8.53)	(6.15)		
Mean licensure test score	-0.027	0.019	-0.023	0.094
	(7.00)	(0.68)	(5.77)	(3.33)
Experience (years)	-3.69	-1.07	-3.66	-0.44
	(81.75)	(3.52)	(76.29)	(1.21)
Experience $\leq$ 3 years	0.132	0.070	0.120	0.049
	(58.29)	(4.58)	(51.97)	(2.90)
Experience $\geq$ 25 years	-0.102	0.070	-0.100	0.059
	(65.22)	(5.86)	(60.91)	(4.37)

Notes: n = 886,343 teacher-years. Column I lists the estimated difference in qualification k between teachers moving to mainstream schools and non-movers (δ^m_{jt} in Equation 1–1). Cells in column II list the estimated difference in k between charter and mainstream movers (δ^c_{jt}). Columns III and IV present these same coefficients for the subsample of regularly licensed teachers. Control variables included receiving school characteristics (student racial composition, performance composite, school size, school age, grade ranges served), a set of dummy variables for missing data, and county-by-year effects. The absolute values of t-statistics are reported in parentheses below each coefficient. Robust standard errors were clustered within each school and year.

Table 1-5. North Carolina public school teacher fixed effects: Summary statistics

Fixed effect estimates	All teachers	All movers	Mainstream movers	Charter movers
Math	-0.016	-0.036	-0.035	-0.072
	(0.258)	(0.262)	(0.263)	(0.253)
Reading	-0.013	-0.029	-0.028	-0.057
	(0.225)	(0.227)	(0.227)	(0.242)
n (teacher-years)	167,244	13,752	13,495	257

Notes: Cells represent average fixed effect estimates, by subject and mobility status. Standard deviations appear in parentheses below each mean. Data for moving teachers were evaluated in the year immediately preceding a school change.

Table 1-6. Regression results: Classroom performance of teachers changing schools

	I	II	III	IV
Sample	All	All	Licensed	Licensed
Destination	Mainstream	Charter	Mainstream	Charter
(Equation 1–3 coefficient)	($\hat{\delta}_{jt}^{m}$)	($\hat{\delta}_{jt}^{c}$)	($\hat{\delta}_{jt}^{m}$)	($\hat{\delta}_{jt}^{c}$)
Math	-0.018	0.045	-0.017	0.044
	(7.20)	(2.64)	(6.76)	(2.35)
Reading	-0.014	0.040	-0.014	0.044
	(6.79)	(2.41)	(6.36)	(2.43)

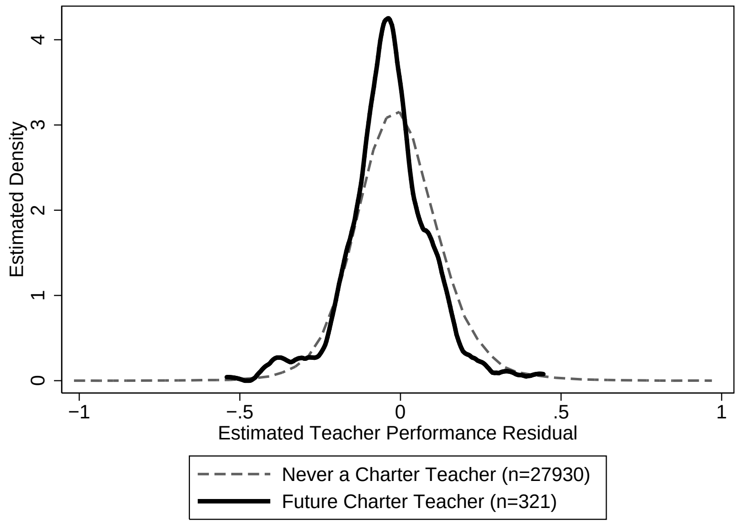
Notes: n = 167,244 teacher-years. Column I lists the estimated difference in fixed effects between mainstream movers and non-movers (δ_{jt}^{m} in Equation 1–2). Column II lists the estimated difference in fixed effects between charter and mainstream movers (δ_{jt}^{c}). Columns III and IV present these same coefficients for the subsample of regularly licensed teachers. Unreported control variables include receiving school characteristics (student racial composition, performance composite, school size, school age, grade ranges served), a set of dummy variables for missing data, and county-by-year effects. The absolute values of t-statistics are reported in parentheses below each coefficient. Robust standard errors were clustered within each school and year.

Table 1-7. Variation in teacher quality

Subject Measure of variance	Math Total	Math Signal	Math SD(FE)	Reading Total	Reading Signal	Reading SD(FE)
All teachers	0.291	0.179	0.237	0.263	0.137	0.205
	(0.009)	(0.017)	(0.013)	(0.008)	(0.015)	0.012
Never a charter teacher	0.294	0.156	0.236	0.260	0.136	0.205
	(0.011)	(0.019)	(0.018)	(0.008)	(0.016)	(0.011)
Future charter teacher	0.307	0.183	0.255	0.276	0.132	0.232
	(0.009)	(0.015)	(0.008)	(0.008)	(0.014)	(0.009)

Notes: n = 167,244 teacher-years. Student math and reading scores were regressed against student characteristics, teacher experience indicators, peer characteristics, and school-by-year effects (Equation 1–4). "Total" is the standard deviation of student residuals from Equation 1–4 estimates. "Signal," calculated by Equation 1–5, is the standard deviation of teachers' persistent value-added, by group. "SD(FE)" is the standard deviation of teacher fixed effects, estimated by Equation 1–2. Standard errors, in parentheses below each standard deviation estimate, were estimated by bootstrap with an equal number of charter participants and non-participants selected in each sampling.

A



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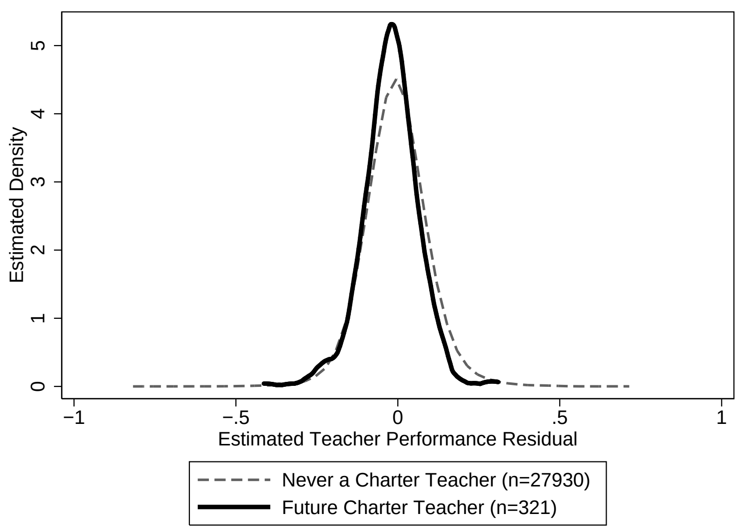

Figure 1-4. Density estimates: Persistent teacher quality in math (A) and reading (B) for future charter teachers and exclusively mainstream teachers. Densities were estimated using Epanechnikov kernel functions and halfwidths of 0.025 scaled standard deviations.

CHAPTER 2
NEW TEACHERS AND STUDENT ACHIEVEMENT IN CHARTER SCHOOLS

Charter schools, particularly new ones, are staffed by high rates of new teachers, and this may help to explain why their students struggle to meet expectations. Inexperienced charter faculties are widely acknowledged, but to date, no study has linked teacher experience to student performance in charter schools. I examine whether inexperienced faculties affected student achievement in North Carolina charters, using a twelve-year panel of student test data. The share of new teachers in the state's charter faculties was quite high in new schools, but fell sharply as the schools aged. Consistent with earlier research, I find significant returns to charter school age, but this maturation could not be attributed to declining rates of new teachers. Rather, charter students benefited from higher rates of new teachers in recently-new schools, more so in math than reading. These findings suggest that inexperienced faculties were not a root cause of low student achievement in new charter schools and may have been symptomatic of development.

2.1 Introduction

Charter schools are publicly funded, independent alternatives to traditional, mainstream public schools. Some charter systems are reinventing chronically low-performing urban school districts,[1] while others represent more tentative experiments in school choice and decentralized school management. A series of studies by the National Center for Education Statistics (NCES) reported that charter students were scoring lower than mainstream students on math and reading exams (National Center for Education Statistics (2004) and Braun, Jenkins, and Grigg (2006)). These studies used cross-sectional snapshots of fourth-grade students in 2003, and despite extensive controls for observable student characteristics, findings may have been influenced by selection bias. Enrolling

[1] See, for instance, charter systems covering Chicago, New Orleans, New York City, and Washington, D.C.

in a charter school is a choice; if students and their families made this choice based on unobservable factors (like inherent ability), cross-sectional analyses will fail to evaluate charter students' performance against adequate counterfactuals.[2]

Longitudinal microdata, which track individual student test scores over time, can circumvent selection bias by controlling for inherent ability with student fixed effects or lagged test scores. Several economists have used longitudinal data covering large, statewide school systems to produce estimates of the causal effect of charter enrollment on student performance. This thread of education research seeks to understand how a student's level of achievement changes as a result of enrolling in a charter school. Doing so refocuses attention from static charter/mainstream comparisons to the dynamic performance of students who opted into independent schools. Furthermore, longitudinal data permit us to see how charter schools–many of which have recently opened–improve over time.[3] Hanushek et al. (2007) showed that Texas students incurred significant penalties in new charter schools, but in schools older than two years, student gains were not statistically different from those of mainstream students. Sass (2006) found similarly optimistic results for Florida's charter schools, which improved to par with mainstream schools by their fifth year of operation. Both studies controlled for heterogeneity in student ability with lagged achievement levels. An alternative strategy uses student fixed effects to parameterize heterogeneous, unobserved ability. This methodology was employed by Bifulco and Ladd (2006) and Booker et al. (2007) to examine the effectiveness of charter schools in North Carolina and Texas, respectively. Bifulco and Ladd (2006) showed that despite evidence of improvement over time, the effect of attending a North

[2] Also, families may be drawn to charter schools for reasons beyond the scope of test performance: greater discipline, emphasis on community service, lower dropout rates, etc.

[3] Bifulco and Bulkley (2008) provide a thorough review of the research on charter effectiveness.

Carolina charter school was persistently negative in schools of all ages. Booker et al. (2007) decomposed the effect of charter enrollment in Texas by school age *and* student tenure. Students who moved to a charter school in Texas experienced an initial shock but recovered within three years. Charter schools are promising vehicles for competition in public education systems, but lengthy maturation periods weaken their appeal (Carnoy, Jacobson, Mishel, and Rothstein (2006)). Why do new charters struggle, and what might hasten their improvement?

Faculty development may be a substantial start-up cost in charter schools. The relative inexperience of charter faculties is widely acknowledged.[4] Large shares of young, inexperienced teachers are viewed as a sign of weakness and impracticality in the charter model (Rimer (2003)). Studies have repeatedly shown that new teachers are less effective at producing student achievement. The returns to experience are steep and highly non-linear, with statistically significant gains coming in the first three to five years of a teacher's career.[5] If charters are unable to attract and retain experienced teachers, and if new teachers face the same difficulties in charter schools as they do in traditional public schools, then it should be no surprise that charter students do not perform as well as mainstream students. The conclusion that new teachers contribute to the struggles of just-opened charters would be an easy one to make, despite a paucity of quantitative evidence linking faculty inexperience to student performance in charter schools. Hanushek et al. (2007) found that inexperienced faculties may have contributed to the estimated penalty from enrolling in a new Texas charter school, although their students benefitted from smaller class sizes.

[4] See, e.g., Hoxby (2002), Malloy and Wohlstetter (2003), and Podgursky and Ballou (2001).

[5] A selection of studies reaching the conclusion that the returns to experience are steepest over a teacher's first few years includes Clotfelter et al. (2007), Murnane and Phillips (1981), and Rockoff (2004).

Why would charter schools, under intense media and state scrutiny, risk their goodwill by hiring high rates of new teachers? Charter budgets are notoriously strained. In most charter systems, a school's revenue is proportionate to the number of students enrolled. Various taxing authorities pay charter schools a per-pupil rate roughly equal to the surrounding district's per-pupil cost, excluding capital expenses. This ensures that public funds follow the students who opt out of traditional public schools–a common feature of other school choice plans–but puts charters at a financial disadvantage. The district's variable cost must cover all of a school's expenses, including facility rental.[6] Furthermore, a school with at most a few hundred students will not enjoy the economies of scale afforded to a district with many thousands of students. New teachers can be paid less than experienced teachers, particularly if they are uncertified and have few opportunities in mainstream schools. Charter administrators and stakeholders may find that they can maximize their objectives (budgets, and therefore, student enrollment being likely objectives) with less experienced faculties. And it may be the case that new teachers are not so damaging to student performance in charter schools.

A charter school is a wholly different environment for students and teachers, and we should not be quick to assume that new charter teachers will negatively affect student performance to the same degree observed for new teachers in mainstream schools. There is some experimental evidence of non-traditional novice teachers outperforming their traditional counterparts (Glazerman, Mayer, and Decker (2006)). Also, in a postsecondary setting, students benefitted from having less experienced, less educated math and science teachers, although they failed to retain those benefits in subsequent follow-on courses (Carrell and West (2008)). So nontraditional new teachers may be more effective than the

[6] Policies vary, but in many states, charters cannot apply public funds toward payments on real property. Like any school, charters are able to raise additional funds through donations or grants (or fees, as in Chicago public schools), but they cannot charge tuition.

research on teacher experience (none of which focuses on charter teachers) would indicate, or at least, they may be better at "teaching to the test."

Hiring young and inexperienced teachers could be an efficient way for charter school leaders to allocate their limited resources. In doing so, they can direct more of their budget toward recruiting and overhead expenses. But this alternative personnel strategy could have large social costs in the form of lower student achievement. If so, charter schools with highly inexperienced faculties would not be fulfilling their roles as viable options for public school students. There is no evidence, however, that students perform better or worse in charter schools that rely heavily on new teachers.

In this paper, I link faculty profiles of North Carolina's charter and mainstream schools to a twelve-year panel of student math and reading test scores. This is the first quantitative effort to date that fully explores relationships between faculty inexperience and student performance in charter schools. I document high rates of new teachers and teacher turnover in charter schools, relative to mainstream schools of the same age. As charters aged, however, the rate of new teachers declined steeply, and on average, converged to the mainstream level by the sixth or seventh year of operation. I incorporate faculty statistics–most importantly, the share of new teachers–into common models of charters' value added. Consistent with earlier research, I find that charters improved with age, but this maturation was not due to declining rates of new teachers. During their first five to six years of operation, charters with higher rates of new teachers produced higher student math and reading levels, relative to schools with more experienced faculties. These findings suggest that young charter schools benefitted from–or at least, were not harmed by–rates of faculty churn that signal trouble in mainstream schools. What is less clear is the mechanism behind the empirical benefits attributed to inexperienced charter faculties. Were North Carolina's charter schools hiring more effective, albeit less experienced, teachers, or did low-cost faculties allow charters to redirect resources in ways that yielded greater student achievement? In either case, high rates of new teachers in charter schools were not a root cause of low student achievement, and may have been important signs of development.

2.2 North Carolina Charter Schools, Students, and Teachers

2.2.1 North Carolina Charter Schools

In June of 1996, the North Carolina General Assembly passed the Charter Schools Educational Opportunity Act, with the intention of expanding learning opportunities for students, creating professional opportunities for teachers, and engendering creativity in public education.[7] The extent of North Carolina's charter system is limited by a 100-school cap, which has been binding since the 2002 school year.[8],[9] North Carolina's first thirty-four charter schools opened for the 1998 school year, enrolling 4,642 students. By 2007, there were more than 27,000 charter students, accounting for two percent of all public enrollment in the state, and more than seven percent in some districts.

North Carolina is an ideal setting to study the effectiveness of charter schools serving as complementary alternatives to existing, fairly stable mainstream public schools.[10] The state's charter system is ten years old, serves a diverse range of students and locales, and bears many features in common with other state charter systems, particularly regarding school finance and faculty requirements. Each charter school receives a per-pupil transfer

[7] See the State Board of Education's Policy Manual for institutional details not covered in this section (North Carolina State Board of Education (2006)).

[8] I refer to school years by the year of their traditional spring conclusion. For instance, 2002 references the 2001-2002 school year.

[9] Although the cap is binding, new charters have opened each year since 2002 to replace schools that closed. Also, statewide charter enrollment continues to grow as existing schools add grades and campuses.

[10] Charter schools assume this role in many states, but in some troubled urban school districts, part of their purpose is to reform chronically underperforming, costly school systems. The results discussed here will be more generalizable to the complementary type of charter system.

from district, state, and federal governments, equivalent to the per-pupil cost incurred by nearby public schools, but excluding facility expenses. Within each school, at least seventy-five percent of elementary-grade teachers are expected to hold full teaching licenses, as are at least fifty percent of grade six through twelve teachers. This is a considerably relaxed licensure standard relative to North Carolina's mainstream schools, but similar to charter licensure requirements in other states (Center for Education Reform, 2008).[11] Rich, longitudinal data (described in detail in section 2.2.2) on charter and mainstream student performance has been collected for six grades and twelve years, covering the entire history of the state's charter system. North Carolina has contributed largely to the national debate on charter schools, thanks in part to studies by Bifulco and Ladd (2006, 2007), who found strong evidence of sub-par student achievement gains and racial segregation in the state's charter schools. I revisit their findings on student achievement in this paper, with the benefit of five additional years of data, and for the first time, I analyze the effect of faculty experience on charter student achievement.

North Carolina's charter system is at a critical juncture. Little has changed since 1996, but the 100-school cap, charter school funding, and the evaluation of individual schools remain polarizing issues in state and local politics. One research group has recommended the state maintain the 100-school cap (Manuel (2007)), citing poor progress in existing charters. Another group, the Blue Ribbon Charter School Commission, advised doing away with the cap but cracking down on low-performing schools by revoking the charter of any school performing below median standards without signs of improvement[12] (Owens (2007)). This paper will inform state and national debates on the effectiveness of

[11] There is no required minimum for the percent of licensed teachers in traditional public schools. In practice, about 90 percent of mainstream public school teachers are fully licensed.

[12] This policy would curiously impose a measure of central tendency as a minimum standard of quality.

the charter model, as well as discussions on the role of inexperienced teachers in charter schools.

2.2.2 North Carolina Public School Data

I use data on the universe of North Carolina public schools, students, and teachers over the years 1996-2007. The data are maintained by the North Carolina Education Research Data Center (NCERDC) at Duke University, in collaboration with the state Department of Public Instruction.[13] These data have been utilized in a variety of education research projects, including Bifulco and Ladd's (2006, 2007) evaluations of North Carolina's charter schools.

My primary measures of student performance are end-of-grade (EOG) test score records for students in grades three through eight. EOG tests cover the state's Standard Course of Study in math and reading.[14] Raw scores are intended to be interval-scaled across and within grades, meaning that a one-unit increase represents the same knowledge gain at all points in the range of scores, which varies over time. I standardized raw scores to have mean zero and standard deviation one within each grade and year. Standardized EOG scores, then, represent students' place in the distribution of scores for their cohort, and changes in student test scores over time represent movement within that distribution.[15] Each student's test score is linked to an anonymous identifier that

[13] See Muschkin et al. (2008).

[14] Standardized test scores are incomplete measures of student learning, and fail to asses non-academic merits that parents value (discipline, for instance). This could be problematic for the current analysis if charter schools differentially focus on non-academic merits, and if parents who send their children to charter schools place less emphasis on test scores. Nonetheless, EOG scores and gains are a large part of North Carolina's school accountability methodology, and charter schools are held to the same accountability standards as their mainstream counterparts.

[15] The results to follow are similar–but attenuated–if I standardize by year, therein preserving the vertical scale across grades. Education inputs affect students' place in their cohort distributions more so than their place in the grade 3 - 8 distribution, so point

allows me to track his or her progress across grades. Additionally, each score is linked to a broad set of student characteristics–gender, race/ethnicity, exceptionalities, parental education, and assorted survey questions.

I assembled a panel of students in grades three through eight by cohort, starting with the cohort in grade three in 1996, up through the cohort in grade three in 2005. I excluded students who repeated a grade or had gaps in their time series. Changing schools is disruptive and tends to negatively affect a student's test score growth (Hanushek, Kain, and Rivkin (2004)), so it is necessary to control for student mobility in typical student achievement production functions. I identified five mutually exclusive types of school changes: movement across counties, movement within a county, structural moves (usually indicating the transition from elementary school to middle school),[16] movement from a mainstream school to a charter school, and vice versa.

The data allow me to track more than 1.7 million unique students, of whom 31,634 attended a charter school at some time. For computational convenience, I randomly sampled complete testing histories for twenty percent of exclusively mainstream students, those who were never observed in a charter school. Table 2-1 categorizes charter students by their mobility, or lack thereof, between charter and mainstream schools. Nearly forty-eight percent of charter students were observed in a mainstream school before moving to a charter school–of those, 28.2 percent transitioned back to the mainstream after one or more years. Just less than sixteen percent of charter students originated in the charter sector and then moved to a mainstream school. In models of charter value

estimates tend to be larger in absolute value when EOG scores are normalized by grade and year rather than year alone. Nonetheless, the conclusion that inexperienced charter faculties were associated with higher student achievement was robust to either distribution of achievement levels, as well as changes in those levels.

[16] Following the literature, I defined a structural move as any school change where at least thirty percent of the mover's school-grade cohort made the same move (Hanushek et al. (2007), Bifulco and Ladd (2006), Sass (2006), and Booker et al. (2007)).

added with student fixed effects, the counterfactual to a student's achievement in a charter school is her own achievement in a mainstream school. More than a third of charter students–35.6 percent–were exclusively observed in charter schools, so they would not contribute to estimates of charter effectiveness in these models. An alternative model described in section 2.3 controls for inherent student ability with lagged test scores rather than student fixed effects, therein permitting any student with at least two years of test records to contribute to results.

Most EOG records indicate students' exam proctors, who may have been classroom teachers. This is one of the many valuable features of the North Carolina data, because it allows researchers to link individual teachers to their students. Unfortunately, charter teachers are not well-represented in the EOG files, so I cannot confidently link individual charter teachers to their students. Charter teachers are, however, comprehensively listed in school activity reports, allowing me to link faculty profiles to student test records. Until now, no study has exploited this unique window on the typically opaque data wall surrounding charter faculties.

2.2.3 North Carolina Charter Teachers

Each school-related activity involving direct student contact with a public school employee is recorded in school activity reports. Activities include regular class meetings as well as non-academic activities like lunch and recess. I identified teachers as individuals who predominantly held teaching assignments in the activity reports, and I excluded teaching assistants, facilitators, and DARE officers. I dropped teachers with multiple school assignments in a given year, as there was no clear way to distinguish mid-year moves from roving, multi-school assignments. New teachers satisfied two empirical conditions: (1) they were not observed with a teaching assignment in a prior year, and (2) they were entering their "first year of employment in education," according to a variable in the school activity reports.[17] The data also indicate teachers' gender and race/ethnicity. I created faculty profiles for every charter and mainstream school by aggregating experience, gender, and race/ethnicity variables to the school level for each year. These profiles were then linked to student EOG test records. I calculated student-teacher ratios and dropped students in schools where the ratio appeared to be less than 5 or more than 25.[18]

Table 2-2 summarizes characteristics of the schools and faculties with EOG test-takers, by charter designation, over the years 1996-2007. Charter schools enrolled fewer students on average, and they had slightly smaller student-teacher ratios (15.0 versus 18.0). But 13.6 percent of charter faculties were new teachers, more than twice the rate in mainstream faculties. Charters had lower shares of female teachers, by eleven percentage points, and significantly higher rates of black, non-Hispanic teachers.

Clearly, charter schools relied on new teachers to a much greater degree than mainstream schools. But simple averages mask dynamic trends in faculty experience. Figure 2-1 plots the rate of first-year teachers in school faculties, by age of school and charter designation. On average, nearly thirty percent of teachers in new charter schools were in their first year of teaching. This is about three times the rate of new teachers in just-opened mainstream schools. But the rate of new charter teachers shrank dramatically as their schools aged, reaching par with mainstream schools by the sixth to eighth year of operation.

New teachers are at a high risk of leaving the profession. Staffing a school with a lot of new teachers may be a low-cost personnel strategy, but a precursor to high turnover.

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[17] There were a small number of new teachers who satisfied the first but not the second condition, because their previous assignment had been as a teaching assistant. I classified these individuals as new teachers.

[18] This affected about two percent of the full sample (and an even smaller percent of charter students). Results are qualitatively unchanged if these schools and students were included.

I calculated forward-looking teacher turnover rates for each school, equal to the percent of teachers at school x in year t that were not at school x in year $t+1$. Of these, about half moved to another school and half left the sample. Looking back to Table 2-2, note that teacher turnover was high in North Carolina–19 percent, on average, meaning that about one in five teachers in a particular school were not there the following year. But turnover in charter schools was even higher, 33.6 percent, meaning one in three charter teachers did not return. But again, the simple average is misleading, because teacher turnover fell sharply as charter schools aged. Figure 2-2 illustrates turnover in charter and mainstream schools in their first through ninth year. Much like the rate of new teachers, the typical rate of teacher turnover in charter schools was very high initially (almost forty percent) but declined sharply as the schools aged. By the seventh year of operation, average turnover in charter schools was statistically indistinguishable from turnover in mainstream schools.

The evaluation of charter school effectiveness has repeatedly pointed to a pattern of difficult early years followed by gradual improvement. Booker et al. (2007) attributed part of the estimated penalty to the fact that new charter schools necessarily enroll high rates of school-changers, who tend to make lower achievement gains upon moving to a new school. Additional volatility may come from high rates of inexperienced and exiting teachers. Can we attribute the maturation of charter schools to falling rates of new teachers? Section 2.3 suggests that new teachers were not responsible for low student achievement in North Carolina's charter schools.

2.3 Analytic Methods and Results

I begin with a basic model of the cumulative production of student achievement, explicitly accounting for charter enrollment and student mobility:

$$Z^k_{iGT} = \mathbf{C}_{iGT}\alpha^k_G + \mathbf{A}_{iGT}\beta^k_G + \mathbf{F}_{ST}\zeta^k_G + (\mathbf{C}_{iGT} * \mathbf{f}_{ST})\tau^k_G + \mathbf{M}_{iGT}\theta^k_G + \gamma^k_{iG} \qquad (2\text{–}1)$$

$$+\sum_{\substack{h=1,\\ r=1}}^{\substack{G-1,\\ T-1}} \lambda_{hr}^{k}\left(\mathbf{C}_{ihr}\alpha_{g}^{k}+\mathbf{A}_{ihr}\beta_{g}^{k}+\mathbf{M}_{ihr}\theta_{g}^{k}+\mathbf{F}_{sr}\zeta_{g}^{k}+\gamma_{ih}^{k}\right)+\varepsilon_{iGT}^{k}.$$

Z_{iGT}^{k} is student i's contemporaneous test score in subject k (math or reading), grade G, school S, and year T. Inputs in the production of student achievement include:

- Charter attendance indicators ($\mathbf{C}_{iGT}$).
- Individual student characteristics ($\mathbf{A}_{iGT}$) - gender, race/ethnicity, parental education, exceptionality classification (academically gifted and/or learning disabled), and subsidized lunch status.
- Faculty characteristics ($\mathbf{F}_{ST}$) - the percent of teachers who were new to public education, and the percent of teachers who were black, Hispanic, or other non-Caucasian ethnicities. Some of these are allowed to have a distinct effect in charter schools, via $\mathbf{C}_{iGT} * \mathbf{f}_{ST}$.
- Student mobility indicators ($\mathbf{M}_{iGT}$).
- Individual student fixed effects (γ_{iG}).

In the model described by Equation 2–1, the effects of inputs from prior grades and years decay according to λ_{gt}^{k}. Also, the effects of inputs are allowed to vary across grades–for instance, it may be the case that making an intra-district, non-structural move is more disruptive in higher grades. Similarly, a student may have an inherently good match with the fourth-grade content, making γ_{i4} larger than her other fixed effects. Unfortunately, this flexibility renders the model inestimable. The number of parameters equals the number of observations, and it is the task of the econometrician to impose reasonable restrictions that permit identifying variation in the variables of interest. First, I assume that student fixed effects were constant across grades: $\gamma_{iG} = \gamma_i \forall g$. Similarly, I assume that the effects of inputs abbreviated by $\mathbf{C}_{igt}$, $\mathbf{A}_{igt}$, $\mathbf{M}_{igt}$, and $\mathbf{F}_{igt}$ were constant across grades. I restrict the effect of prior inputs to be zero for the history of grade-years: $\lambda_{gt} = 0 \forall g < G, t < T$. These restrictions yield Equation 2–2:

$$Z_{iGT}^{k} = \mathbf{C}_{iGT}\beta_{C}^{k} + \mathbf{F}_{ST}\beta_{F}^{k} + (\mathbf{C}_{iGT} * \mathbf{f}_{ST})\beta_{f}^{k} + \mathbf{M}_{iGT}\beta_{M}^{k} + \gamma_{i}^{k} + \varepsilon_{iGT} \tag{2–2}$$

Equation 2–2 is a model of student achievement where a student's inherent propensity to produce achievement is represented by an unobserved, time-invariant fixed effect (γ_i^k). Students' gender, race/ethnicity, parental income, and subsidized lunch status were largely time-invariant, so $\mathbf{A}_{iGT}$ can be omitted since its variables are highly collinear with γ_i^k. I also estimate a model with lagged achievement, instead of student fixed effects:

$$Z_{iGT}^k = \lambda^k Z_{iG-1T-1}^k + \mathbf{C}_{iGT}\beta_C^k + \mathbf{F}_{ST}\beta_F^k + (\mathbf{C}_{iGT} * \mathbf{f}_{ST})\beta_f^k + \mathbf{M}_{iGT}\beta_M^k + \gamma_C^k + \varepsilon_{iGT} \qquad (2\text{–}3)$$

Note the addition of county effects (γ_C) in 2–3. Student fixed effects, in conjunction with cross-county move indicators, negate the need to control for county effects in Equation 2–2. But in the absence of student fixed effects, γ_C indicators are necessary to control for differences in typical student performance across counties. Equations 2–2 and 2–3 require at least two continuous years of testing data to identify the relative effect of charter enrollment; accordingly, I limit the sample to students with at least one prior year of math and reading test scores. To date, studies on the quality of charter schools have typically used either a fixed effects or lagged achievement methodology, but not both. I present results from Equations 2–2 and 2–3 side-by-side, and show that the effectiveness of charter schools appears quite different across models. The difference can be reconciled by (1) the source of identifying variation each model relies on, and (2) the assumptions that led to their derivation from Equation 2–1.

Equation 2–3 has conceptual and operational advantages over Equation 2–2. The fixed effects specification represented by Equation 2–2 makes the strong assumption that prior years' inputs do not decay, whereas Equation 2–3 is more flexible, allowing λ^k to be estimated rather than assumed. Equation 2–3, which controls for initial proficiency, is also less subject to mean reversion than Equation 2–2. Mean reversion, in this setting, is problematic when students with below-average initial scores tend to make above-average advancements within their cohort distribution. North Carolina's EOG test score gains are greatly affected by mean reversion, particularly in math. Figure 2-3 plots local polynomial estimates of the nonparametric relationship between gains and initial scores against kernel densities of initial scores. Gains tended to be higher if a student's initial score was low, and lower if his initial score was high. If students enrolled in charter schools following an atypically low (high) test score, mean reversion would bias the estimated impact of charter enrollment on students' place in the distribution up (down).

Additionally, the gains model with lagged achievement offers a wider scope for inference, albeit a less causal one. Fixed effect models evaluate the effectiveness of a charter education by measuring a student's charter performance against her mainstream performance. A student's mainstream achievement is an intuitive counterfactual for her performance in charter schools. But this identification strategy hinges on sector-switchers, who may be systematically different than students who spent fewer than two years in mainstream schools. Equation 2–3 is under-parameterized by comparison, and identifying variation in charter enrollment is predominantly cross-sectional, rather than longitudinal. Accordingly, inference regarding the effect of charter schools or faculty characteristics should be attributed to cross-sectional variation in those inputs. Coefficient estimates from Equation 2–3 will reflect global relationships between inputs and student achievement, whereas estimates from Equation 2–2 with fixed effects will show how student achievement reacted to changes in each input.

The advantages of the lagged achievement model represented by Equation 2–3 come at considerable cost to the rigorous treatment of selection. With $Z^k_{iG-1T-1}$ in the list of inputs, student fixed effects (γ_i) and time-invariant student characteristics like gender, race/ethnicity, parental education, and exceptionalities (variables represented by $\mathbf{A}_{iGT}$ in Equation 2–1) would be endogenous. The fixed effects model, however, will control for any differential selection into charter schools stemming from students' inherent rate of achievement growth. Both models deal with heterogeneous ability in a way that negates the need to control for observable student characteristics in $\mathbf{A}_{iGT}$. Before moving on, I will summarize those characteristics for charter and mainstream students, and highlight their similarities and differences.

Table 2-3 lists summary statistics for individual student characteristics contained in $\mathbf{A}_{iGT}$, by charter/mainstream enrollment.[19] Charter students had lower math scores and gains than mainstream students, but higher reading scores. Charter students were slightly more likely to be white, although summary statistics for race and ethnicity fail to demonstrate the extent of racial polarization in North Carolina's charter schools. Charter parents were more educated on average. Among charter students, 32.7 percent had a parent with a four-year college degree, compared to just 21.6 percent of mainstream students. Charter parents were also more likely to hold postgraduate degrees. The likelihood of various learning disabilities was similar for students in both sectors, although mainstream students were three times as likely to be academically gifted. Charter students were also less likely to have free or reduced-price lunch, by 16.9 percentage points. Judging by these summary statistics, a typical charter student came from a more advantaged background than his mainstream peers, was relatively strong in reading, but relatively weak in math.

I estimate Equation 2–2 using the "within" fixed effects estimator, and I estimate Equation 2–3 using ordinary least squares (OLS). In both models, I allow for robust standard errors to be clustered by student identifers. Selected coefficient estimates from Equations 2–2 and 2–3 are presented in Tables 2-4 and 2-5, respectively.

Turning first to the fixed effects specifications without faculty variables (Table 2-4, columns I and III), the effect of a charter education appears to be negative for math and reading. Charter students saw a drop of 0.169 standard deviations (sd) in math and a

[19] Table 2-3 summary statistics are limited to students with at least one prior year of test data, the same sample for which Equations 2–2 and 2–3 are estimated. Sample means are qualitatively similar for broader groups of EOG test-takers.

drop of 0.085 sd in reading.[20] These estimates are economically significant, representing 22 percent of the black-white gap in math (where black students scored 0.779 sd sd lower on average), and about 12 percent of the black-white gap in reading (where black students scored 0.735 sd lower). In agreement with the literature, I find changing schools to be disruptive, much more so for math than reading. Moving to a charter school from the mainstream reduced math scores by 0.013 sd, but this penalty was no worse than the effect of moving to another mainstream school. Students who moved from a charter school to the mainstream, however, realized a significantly larger 0.039 sd penalty to math levels.

Columns II and IV of Table 2-4 list coefficient estimates when faculty characteristics are included in the fixed effects model. Adding faculty characteristics changes the estimated effectiveness of charter schools, but not significantly. Confidence intervals overlap for column II and IV point estimates. Of particular interest for this paper is the effect of new teachers on student achievement. A ten percentage-point increase in the rate of new teachers at a student's school decreased his achievement gains by 0.017 sd in math and 0.013 sd in reading. Given this, and the prevalence of new teachers in charter schools, we might conclude that charter schools would have been more effective, but for their reliance on new teachers. But this inference assumes that new teachers have the same effect in both sectors, an assumption I relax later on.

Adding faculty characteristics to the fixed effects specification does not change the conclusion that North Carolina were ineffective at raising student achievement, but switching to the lagged achievement specification does. Coefficient estimates for the lagged achievement model of test gains are listed for both subjects in Table 2-5. The

[20] The column I specifications in Table 2-4 is similar to Bifulco and Ladd (2006) preferred fixed effects model in their study of North Carolina charter schools, although their dependent variable was the change standardized level scores rather than the level itself. They found that charter enrollment reduced student gains by 0.160 sd in math and 0.095 in reading.

effect of charter enrollment on math performance was small and negative in column I, but small and positive when faculty characteristics were added in column II. Enrolling in a charter school increased reading achievement in both specifications, by as much as 0.027 sd. These results stand in contrast to the fixed effects estimates, where charter schools had a negative effect on achievement in both subjects. Recall that the lagged achievement estimates are based on largely cross-sectional, pooled variation in charter enrollment. Controlling for once-lagged achievement, charter students achieved somewhat higher reading levels than mainstream students, and ambiguously different math levels. The stark differences between the two models would be reconciled if charter students were "cream skimmed" from the pool of public school students, and if their lagged achievement failed to control for an inherent, unobserved propensity to produce higher test scores. In that case, controlling for student fixed effects would show that charter students were not meeting their potential. Fixed effect models may be better suited to assess sharp changes in education inputs that approximate quasi-experimental treatments, like the effect of moving to a charter school. The lagged achievement model is more appropriate for my purpose here, which is to determine whether charter students performed better or worse in schools with less experienced faculties. This specification allows data from all students to contribute to the estimated relationship between faculty experience and student outcomes, whether or not those students had experience in mainstream schools, and it permits a broader scope of inference.

Despite different modes of inference, estimated effects of faculty characteristics on student achievement were similar in fixed effect and lagged achievement models. A ten percentage-point increase in the rate of new teachers significantly decreased math and reading gains in both models, although the estimated penalty to reading gains attributed to new teachers grew significantly from -0.013 sd in the fixed effects model to -0.020 sd in the lagged achievement model. For the remainder of this section, I use the lagged achievement model to decompose the relationship between new teachers and student achievement in charter schools.

Column I of Tables 2-6 (math) and 2-7 (reading) replicates the columns II and IV specifications from Table 2-5, respectively, but breaks the single dummy variable for charter enrollment into ten indicators representing first through tenth-year charters. Results shed light on dynamic trends in charter effectiveness. The math performance of students in first-year charters was 0.081 sd lower than that of mainstream students. Students in fifth-year and older charters had math scores that were statistically indistinguishable or greater than mainstream student scores scores. The pattern of improvement was more dramatic for reading achievement. First-year charter schools had a negative impact on reading achievement, second- and third-year charters were on par with mainstream schools, and beginning with fourth-year charters, reading scores were significantly higher in charter schools, by as much as 0.042 sd (about 5.7 percent of the black-white gap in reading).

What role did faculty development and experience play in the improvement of new charter schools? Recall that rates of new teachers and teacher turnover were very high for new charter schools, but fell quite sharply over the years where improvement in student test scores was greatest. Column II specifications in Tables 2-6 and 2-7 add the interaction, "New (%) x charter" to the list of inputs. The coefficient for "New teachers(%)" suggests that for every 10-percentage-point increase in the rate of new teachers, mainstream students realized about a 0.024 sd drop in their math scores and a 0.022 sd drop in reading scores. Coefficients on "New (%) x charter" measure how the effect of new teachers was different in charter schools, relative to the baseline effect of new teachers in mainstream schools. If the share of new teachers had a similar or magnified effect on charter students' test scores (that is, if the coefficient on the interaction is statistically insignificant or negative), then faculty development and retention would explain much of sub-par achievement in new charter schools, and the subsequent improvement in older charters. But coefficients on the interaction were *positive* and statistically significant for both subjects, meaning that inexperienced faculties were less damaging to student achievement in charter schools than they were in mainstream schools. In fact, inexperienced faculties appeared to benefit charter students–the magnitude of the interaction coefficient is 0.026 sd for math, which more than offsets the 0.024 baseline penalty. So according to point estimates alone, the marginal effect of higher rates of new teachers was 0.002 sd for charter students, a small but positive boost.[21] Estimates of the impact of "year t charter" indicators in column II are interpreted as the effect of charter enrollment, outside of what was due to new teachers. Estimated penalties from attending younger charter schools increased between columns I and II, and benefits from older charters decreased. This is further evidence that high rates of new teachers were less harmful, or even beneficial, to student achievement in charter schools.

Column III specifications in Tables 2-6 and 2-7 interact charter age with the percent of faculty who were new to understand the relationship between new teachers and student performance in charter schools of various ages.[22] Coefficients for the charter age indicators represent the relationship between charter enrollment (by age of school) and student test scores, aside from what was statistically attributed to new teachers. Coefficients on charter enrollment interacted with the percent of teachers who were new represent the additional penalty (or reduction in that penalty) from a charter school having a ten percentage-point increase in the percent of new teachers, relative to the baseline effect of new teachers on mainstream student performance estimated.

[21] This boost may not be statistically significant, however. Future drafts will bootstrap standard errors for marginal effects involving two coefficient estimates.

[22] These specifications implicitly assume that the effect of inexperienced faculties was constant for mainstream schools of all ages. Robustness checks showed this to be valid. Interactions between (mainstream) school age and the rate of new teachers were statistically insignificant, and the estimated effects of new teachers in charter schools of various ages changed very little.

As in column II of each table, the baseline effect of more inexperienced faculties was negative; mainstream student performance decreased 0.024 sd in math and 0.022 sd in reading per each ten percentage-point increase in new teachers. But higher rates of new teachers in first through sixth-year charter schools actually increased math levels, relative to mainstream and charter students in schools with more experienced faculties. Not only were the coefficients on "New (%) x year t charter" positive, they were greater in absolute value than the baseline experience penalty to mainstream students, by 0.017 sd in first-year charters (-0.024 + 0.041), and as much as 0.030 sd in fifth-year charters (-0.024 + 0.054). Students in older charters, however, derived no benefit from faculty inexperience, with the notable exception of math performance in tenth-year charters.[23]

I illustrate the marginal effect of new teachers on charter student achievement in Figure 2-4, using the point estimates listed in column III of Tables 2-6 and 2-7. The solid line in each panel represents the baseline penalty to mainstream student test scores from a ten percentage-point increase in new teachers, estimated to be 0.022 sd for math and 0.024 sd for reading. Scattered points are the sum of baseline effects and the relative effect of the interaction, "New (%) x year t charter." In the top panel of Figure 2-4, five marginal effects are greater than zero, meaning that in charter schools of particular vintages, less experienced faculties increased math achievement, relative to more experienced charter and mainstream faculties. A similar, but attenuated, pattern was found for reading test scores. These results imply that fresh, inexperienced faculties were not responsible for low achievement in recently-new charter schools. But students in older charters were typically better off with more experienced faculties.

[23] Tenth-year charter schools were the oldest charters in the data, and would have opened in 1998, the first school year following authorization. Of the 34 charter schools that opened in 1998, 31 had grade 3 - 8 EOG test-takers, and 20 were still operating in 2007. Just less than 1,900 students took EOG exams in tenth-year charters, a large enough figure to consider the math bonus attributed to new teachers in these schools seriously, rather than rule it out as spurious.

The conclusion that less experienced faculties benefitted charter students is at odds with a long history of research on teacher experience and student achievement. Note that the results outlined above estimate the relationship between *faculty* experience and individual student outcomes in North Carolina's public schools, so it may still be the case that charter students would have performed better under the direct instruction of more experienced teachers. Nonetheless, finding that less experienced faculties were associated with higher student achievement has compelling policy implications. What mechanism caused higher math and reading performance to align with higher rates of new teachers? Consider two plausible explanations. The first is a direct link between new charter teachers and student performance. More successful charters may have recruited inexperienced but effective teachers, perhaps from non-traditional sources. As faculty members gained experience and turnover settled, the benefits of maintaining high rates of new faculty would have fallen. Alternatively, there may have been an indirect benefit of inexperienced faculties on student outcomes. Development-stage charters with smaller payrolls could have directed more funds to overhead expenses that benefitted students–by improving facilities or reducing class sizes, for instance. Column IV specifications in Tables 2-6 and 2-7 control for student-teacher ratios in addition to the list of inputs in column III. As with the other school-level inputs, I allow student-teacher ratios to have a distinct impact in the charter sector. More students per teacher slightly *increased* student achievement in mainstream schools, by 0.003 sd in math and 0.005 sd in reading. This small but counter-intuitive relationship is not unheard of in non-experimental settings.[24] Larger classes in charter schools offset (or more than offset, in the case of math) baseline effects. More importantly, controlling for class size had little to no effect on the estimated benefits that charter students realized from attending schools with less experienced

[24] See Rice and Schwartz (2008) for a review of research on the effectiveness of class size reductions.

faculties. So if new charter teachers indirectly benefitted student achievement, they did not appear to do so through smaller class sizes.

2.4 Conclusions

This paper explored the effectiveness of North Carolina charter schools in their first through tenth years, and for the first time, examined how inexperienced faculties contributed to student math and reading performance. I presented estimates from two derivations of the student achievement production function described by Equation 2–1: levels with student fixed effects (Equation 2–2) and levels with lagged achievement (Equation 2–3). Some consistent patterns emerged across models. Enrolling in a first-year charter negatively affected students' test score levels. Students who moved from a mainstream school to a charter school of any age saw lower gains immediately following their move. Higher rates of new teachers decreased mainstream students' math and reading levels. These conclusions were reached with varying statistical and economic significance in both models, despite different treatments of heterogeneous, unobserved student ability. The estimated impact of attending a charter school on student achievement was considerably more sanguine in the lagged achievement model, highlighting the importance of statistical assumptions and identifying variation in policy-relevant analyses.

I documented a clear pattern of improvement in charter student achievement following charters' first year of operation, when the rate of new teachers in charter faculties was falling sharply. But the weak performance of students in new charter schools could not be attributed to inexperienced faculties. Prior to charter schools' sixth year, higher shares of new teachers were associated with slightly higher student achievement, particularly for math. This relationship was ambiguous beyond year six, suggesting that in older, more established charters, the effect of faculty experience was no different than it was in traditional, mainstream public schools.

How might new teachers have been responsible for higher student achievement? Perhaps a new charter teacher was no more effective than a new mainstream teacher, but young, inexperienced charter faculties allowed school leaders to shift resources in ways that increased student achievement. I find no evidence that charters did this through smaller class sizes, although I cannot rule out other investments: improved facilities, recruitment, training, and so forth. If these indirect mechanisms were largely responsible for my findings, charter finance models could be improved by covering more overhead expenses, and allowing schools to recruit and retain more experienced teachers. Or perhaps new charter teachers were directly responsible for higher student achievement, because new charter schools were recruiting more effective teachers. In this case, mainstream schools could benefit from emulating charters' workplace conditions and faculty requirements. Regardless, my findings suggest that new teachers in recently-opened charter schools were symptoms of development, not staffing failures. There is ample concern for older charter schools that persistently relied on new teachers, however, as the gains from inexperienced faculties were small and short-lived.

Table 2-1. Student transitions in and out of North Carolina charter schools

Type of transition(s)	Percent of students observed in charter schools
only observed in charter schools	35.6
moved from mainstream to charter	34.3
moved from charter to mainstream	15.5
moved from mainstream to charter to mainstream	13.5
other patterns with at least two transitions	1.1

Notes: n = 31,634 total individuals observed in charter schools.

Table 2-2. North Carolina public school faculties: Summary statistics for schools with grade 3 - 8 EOG test-takers.

Type of school	Mainstream	Charter	
Statistic	Mean	Mean	Difference
number of students	536.5	236.5	300.0*
	(237.7)	(183.9)	
number of teachers	31.6	15.8	15.8*
	(14.0)	(10.7)	
student-teacher ratio	18.0	15.0	3.0*
	(15.9)	(8.0)	
percent new teachers	6.4	13.6	- 7.2
	(6.5)	(16.2)	
teacher turnover (percent leaving)	19.0	33.6	- 14.6*
	(14.5)	(25.6)	
percent female teachers	88.9	77.9	11.0*
	(11.2)	(16.8)	
percent white teachers	83.0	67.8	15.2*
	(19.0)	(34.8)	
percent black, non-Hispanic teachers	15.0	27.5	- 12.5*
	(17.6)	(33.7)	
percent Hispanic teachers	0.7	1.7	- 1.0*
	(2.0)	(5.1)	
percent other, non-Hispanic race/ethnicity	1.3	3.0	- 1.7*
	(5.9)	(10.3)	
n (school-years)	18,948	701	

Notes: Standard deviations are in parentheses below each mean. * indicates a significant difference at 95% confidence or more.

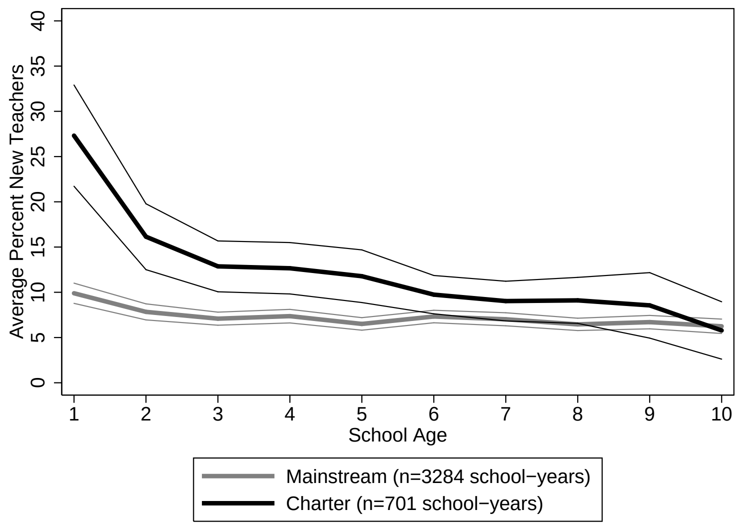


Figure 2-1. Average rate of new teachers, by age of school and charter/mainstream designation. Thinner lines represent confidence intervals.

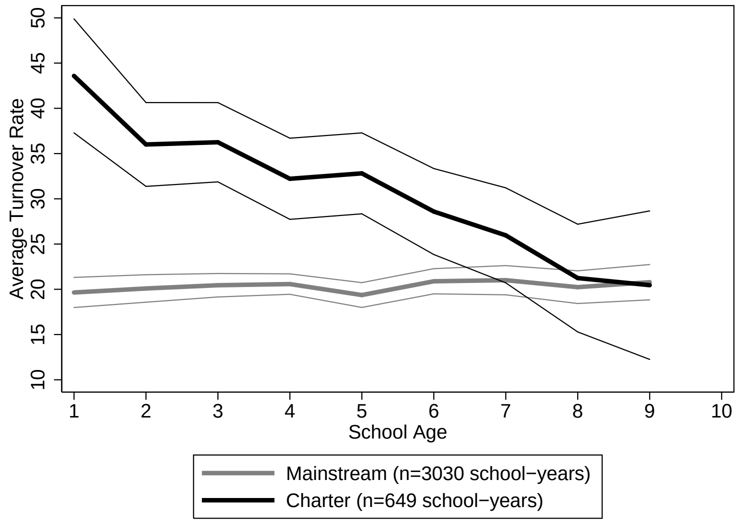
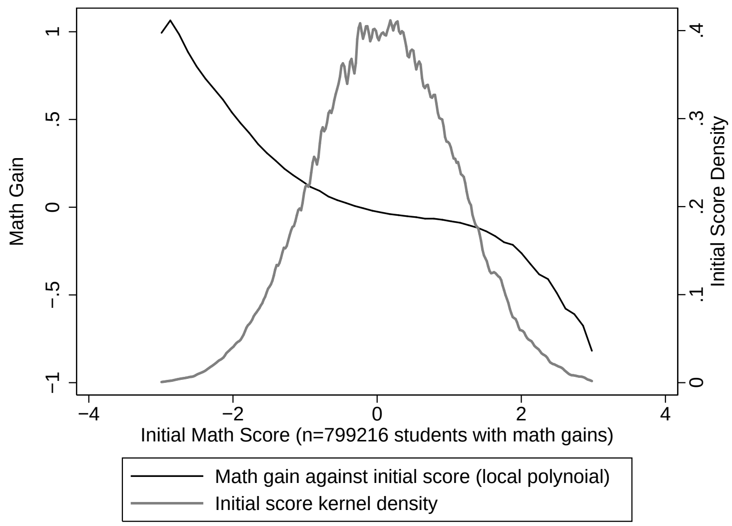


Figure 2-2. Average faculty turnover rates, by age of school and charter/mainstream designation. Thinner lines represent confidence intervals

A



B

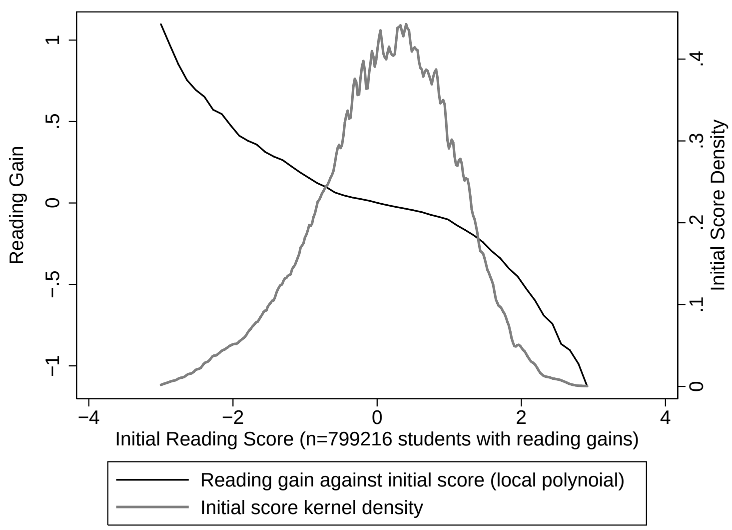


Figure 2-3. Mean reversion in math (A) and reading (B) EOG test scores. Test gains were estimated as first-degree local polynomial functions of initial scores, with 0.50 standard deviation halfwidths. Initial score densities were estimated using Epanechnikov kernel functions and 0.50 standard deviation halfwidths.

Table 2-3. North Carolina public school students: Summary statistics for grade 3 - 8 EOG test-takers

Type of school	Mainstream	Charter	
Statistic	Mean	Mean	Difference
Math score	0.110	0.032	0.078*
	(0.967)	(0.988)	
Reading score	0.105	0.142	-0.036*
	(0.951)	(0.966)	
Female	0.509	0.513	-0.005*
	(0.500)	(0.500)	
White, non-Hispanic	0.618	0.633	-0.015*
	(0.486)	(0.482)	
Black, non-Hispanic	0.283	0.292	-0.008*
	(0.451)	(0.454)	
Hispanic	0.046	0.022	0.023*
	(0.209)	(0.148)	
other, non-Hispanic	0.053	0.053	0.001
	(0.224)	(0.223)	
Missing race/ethnicity	0.001	0.002	0.000*
	(0.035)	(0.042)	
High school or less	0.431	0.233	0.198*
	(0.495)	(0.423)	
Less than 4 years of college	0.196	0.192	0.004*
	(0.397)	(0.394)	
Four-year college graduate	0.218	0.337	-0.119*
	(0.413)	(0.473)	
Postgraduate degree	0.049	0.075	-0.025*
	(0.217)	(0.263)	
Missing parental education	0.106	0.163	-0.057*
	(0.307)	(0.369)	
Learning disabled - math	0.019	0.023	-0.004*
	(0.137)	(0.150)	
Learning disabled - reading	0.041	0.041	0.000
	(0.198)	(0.198)	
Learning disabled - writing	0.042	0.044	-0.001
	(0.202)	(0.205)	
Learning disabled - language	0.007	0.009	-0.002*
	(0.086)	(0.095)	
Other learning disability	0.002	0.002	-0.001*
	(0.040)	(0.048)	
n (student-years)	741,736	57,480	

Notes: Standard deviations are in parentheses below each mean. * denotes significant differences at the 5% confidence level.

Table 2-3. Continued

Type of school Statistic	Mainstream Mean	Charter Mean	Difference
Academically gifted	0.174	0.054	0.120*
	(0.379)	(0.227)	
Subsidized lunch	0.332	0.168	0.164*
	(0.471)	(0.374)	
Missing subsidized lunch status	0.163	0.182	-0.019*
	(0.370)	(0.386)	
n (student-years)	741,736	57,480	

Notes: Standard deviations are in parentheses below each mean. * denotes significant differences at the 5% confidence level.

Table 2-4. Fixed effect estimates: The effect of charter enrollment on test score gains

	I	II	III	IV
Subject	Math	Math	Reading	Reading
Charter	-0.169	-0.156	-0.085	-0.081
	(31.20)	(28.56)	(15.93)	(14.98)
Moved to charter	-0.013	-4.86E-3	-2.71E-3	1.79E-3
	(2.58)	(0.96)	(0.51)	(0.33)
Moved from charter	-0.039	-0.041	3.53E-3	2.49E-3
	(7.34)	(7.58)	(0.61)	(0.43)
Structural change	4.59E-3	7.43E-3	2.23E-3	1.77E-3
	(3.67)	(5.49)	(1.64)	(1.20)
School change	-0.015	-0.014	-8.79E-3	-8.51E-3
	(7.72)	(7.13)	(4.11)	(3.96)
County change	-0.016	-0.017	-5.37E-3	-5.44E-3
	(4.69)	(4.87)	(1.41)	(1.42)
New teachers (%)		-0.017		-0.013
		(16.86)		(12.60)
Nonwhite teachers (%)		-0.011		-2.60E-4
		(16.85)		(0.38)
Female teachers (%)		-1.72E-3		-2.05E-3
		(2.84)		(3.36)

Notes: n = 799,216 student-years. Absolute values of t-statistics are in parentheses below each coefficient. Robust standard errors allow for clustering by student identifiers.

Table 2-5. Lagged achievement estimates: The effect of charter enrollment on test score gains

Subject	I Math	II Math	III Reading	IV Reading
Charter	-0.00677	5.45E-3	0.015	0.027
	(2.92)	(2.34)	(6.00)	(10.75)
Moved to charter	-0.171	-0.153	-0.104	-0.087
	(32.78)	(29.29)	(18.60)	(15.57)
Moved from charter	0.115	0.121	0.062	0.068
	19.02	20.01	(9.68)	(10.64)
Structural change	5.90E-4	5.67E-3	1.72E-3	5.06E-3
	(0.35)	(3.29)	(0.95)	(2.72)
School change	-0.057	-0.051	-0.057	-0.051
	(26.96)	(23.67)	(24.61)	(21.80)
County change	-0.052	-0.051	-0.043	-0.042
	(14.71)	(14.42)	(11.19)	(10.91)
New teachers (%)		-0.020		-0.020
		(21.01)		(19.40)
Nonwhite teachers (%)		-0.021		-0.021
		(52.14)		(47.81)
Female teachers (%)		-2.25E-3		-4.07E-3
		(4.66)		(7.93)

Notes: n = 799,216 student-years. Absolute values of t-statistics are in parentheses below each coefficient. Robust standard errors allow for clustering by student identifiers.

Table 2-6. The effect of charter enrollment on math achievement gains, disaggregated by school age

Specification	I	II	III	IV
Year 1 charter	-0.081	-0.139	-0.184	-0.119
	(6.17)	(9.88)	(8.78)	(4.94)
Year 2 charter	-2.22E-3	-0.038	-0.019	0.041
	(0.26)	(4.16)	(1.66)	(2.67)
Year 3 charter	-3.78E-3	-0.031	-0.048	0.013
	(0.52)	(4.04)	(4.48)	(0.86)
Year 4 charter	-0.019	-0.045	-0.057	4.65E-3
	(2.85)	(6.43)	(5.52)	(0.29)
Year 5 charter	0.028	2.41E-3	-0.029	0.031
	(4.50)	(0.36)	(3.24)	(2.22)
Year 6 charter	-2.23E-3	-0.025	-0.017	0.042
	(0.36)	(3.83)	(1.79)	(2.81)
Year 7 charter	0.027	7.57E-3	0.036	0.095
	(4.30)	(1.16)	(3.96)	(6.59)
Year 8 charter	-0.012	-0.032	-0.011	0.048
	(1.65)	(4.36)	(1.05)	(3.25)
Year 9 charter	-7.27E-3	-0.024	7.00E-4	0.060
	(0.88)	(2.80)	(0.07)	(4.02)
Year 10 charter	0.016	4.80E-4	-0.017	0.039
	(1.28)	(0.04)	(0.95)	(1.89)
New teachers (%)	-0.018	-0.024	-0.024	-0.024
	(17.04)	(20.57)	(20.58)	(20.00)
New x charter		0.026		
		(10.72)		
New x year 1 charter			0.041	0.040
			(7.06)	(6.87)
New x year 2 charter			0.014	0.014
			(3.07)	(2.97)
New x year 3 charter			0.040	0.038
			(6.13)	(5.93)
New x year 4 charter			0.036	0.033
			(5.19)	(4.80)
New x year 5 charter			0.054	0.052
			(9.16)	(8.81)
New x year 6 charter			0.018	0.017
			(2.45)	(2.28)

Notes: n = 799,216 student-years. Coefficients were estimated by Equation 2–3. Absolute values of t-statistics are in parentheses below each coefficient. Robust standard errors allow for clustering by student identifiers.

Table 2-6. Continued

Specification	III	IV
New (%) x year 7 charter	-9.39E-3	-0.011
	(1.10)	(1.30)
New x year 8 charter	9.70E-4	-3.90E-4
	(0.10)	(0.04)
New x year 9 charter	-0.013	-0.016
	(1.32)	(1.62)
New x year 10 charter	0.049	0.045
	(2.37)	(2.17)
Student-teacher ratio		3.18E-3
		(9.30)
Student-ratio x charter		-3.66E-3
		(5.33)

Notes: n = 799,216 student-years. Coefficients were estimated by Equation 2–3. Absolute values of t-statistics are in parentheses below each coefficient. Robust standard errors allow for clustering by student identifiers.

Table 2-7. The effect of charter enrollment on reading achievement gains, disaggregated by school age

Specification	I	II	III	IV
Year 1 charter	-0.033	-0.059	-0.081	-0.016
	(2.46)	(4.08)	(3.90)	(0.67)
Year 2 charter	0.014	-0.00249	0.023	0.087
	(1.56)	(0.26)	(1.96)	(5.28)
Year 3 charter	-1.34E-3	-0.014	-0.030	0.033
	(0.17)	(1.68)	(2.54)	(1.97)
Year 4 charter	0.026	0.014	-0.005	0.056
	(3.51)	(1.75)	(0.46)	(3.17)
Year 5 charter	0.024	0.012	-0.012	0.050
	(3.36)	(1.62)	(1.22)	(3.21)
Year 6 charter	0.033	0.023	0.022	0.080
	(4.95)	(3.25)	(2.14)	(4.85)
Year 7 charter	0.039	0.030	0.061	0.120
	(5.66)	(4.22)	(6.06)	(7.45)
Year 8 charter	0.033	0.024	0.038	0.098
	(4.40)	(3.12)	(3.52)	(6.19)
Year 9 charter	0.022	0.014	0.011	0.072
	(2.38)	(1.55)	(0.91)	(4.37)
Year 10 charter	0.042	0.035	0.047	0.103
	(3.08)	(2.55)	(2.30)	(4.33)
New teachers (%)	-0.019	-0.022	-0.022	-0.021
	(16.67)	(16.96)	(16.92)	(16.20)
New x charter		0.012		
		(4.49)		
New x year 1 charter			0.020	0.019
			(3.40)	(3.21)
New x year 2 charter			-3.65E-3	-5.30E-3
			(0.74)	(1.06)
New x year 3 charter			0.025	0.023
			(3.26)	(3.03)
New x year 4 charter			0.028	0.026
			(3.51)	(3.21)
New x year 5 charter			0.034	0.030
			(4.84)	(4.35)
New x year 6 charter			0.013	0.014
			(1.66)	(1.72)

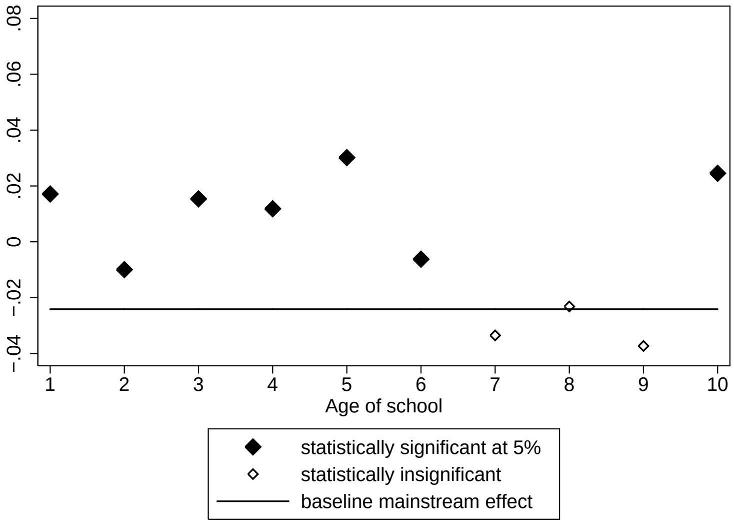
Notes: n = 799,216 student-years. Coefficients were estimated by Equation 2–3. Absolute values of t-statistics are in parentheses below each coefficient. Robust standard errors allow for clustering by student identifiers.

Table 2-7. Continued

Specification	III	IV
New (%) x year 7 charter	-0.027	-0.028
	(2.75)	(2.90)
New x year 8 charter	-5.10E-3	-8.70E-3
	(0.50)	(0.86)
New x year 9 charter	0.018	0.014
	(1.63)	(1.30)
New x year 10 charter	-6.62E-3	-0.010
	(0.29)	(0.45)
Student-teacher ratio		4.72E-3
		(12.75)
Student-ratio x charter		-3.69E-3
		(4.90)

Notes: n = 799,216 student-years. Coefficients were estimated by Equation 2–3. Absolute values of t-statistics are in parentheses below each coefficient. Robust standard errors allow for clustering by student identifiers.

A



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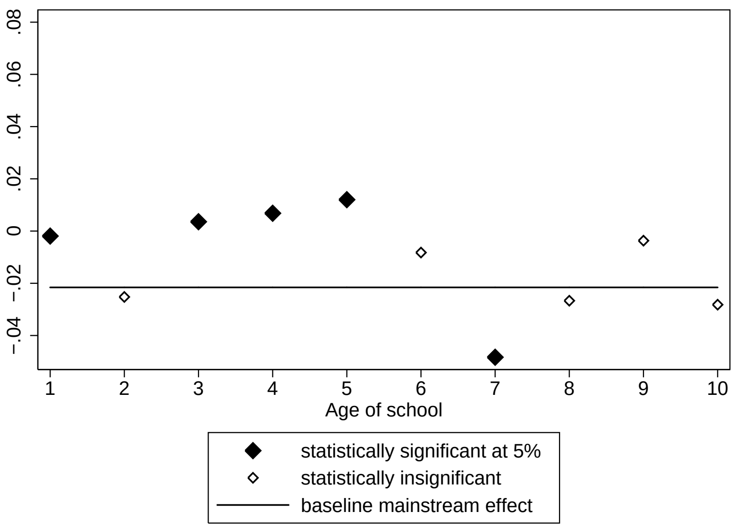

Figure 2-4. Marginal effects of a ten-percentage-point increase in new teachers on charter students' math (A) and reading (B) levels, by age of school

CHAPTER 3
A SILVER LINING? TEACHERS, STUDENT, AND RACIAL IMBALANCE IN CHARTER SCHOOLS

An unintended consequence of decentralized charter school systems is that they can exacerbate racial segregation across schools. Racial isolation in charter schools has been shown to reduce the achievement gains of black students and widen the black-white achievement gap. But there may be a silver lining: North Carolina charters with predominantly nonwhite student populations were staffed by relatively high rates of nonwhite teachers, who may have been more effective instructors of nonwhite students. I show that disproportionately nonwhite faculties marginally raised the math performance of black students in charter schools. But these gains were insufficient to slow the growing gap between students in predominantly white and predominantly nonwhite charter schools.

3.1 Introduction

There is a negative and persistent correlation between racial segregation and the academic performance of disadvantaged, minority students. Studies dating back to the Coleman Report (Coleman et al. (1966)) have shown that student achievement is significantly lower in schools with higher rates of nonwhite students. This correlation is driven in part by causal peer exposure effects, and in part by the way families and teachers sort across neighborhoods and schools. Family preferences for school and neighborhood quality lead to self-segregation along those dimensions (Bayer, Ferreira, and McMillan (2007)), and consequently, heterogeneous demographic profiles in neighborhood schools. A variety of studies have tried to circumvent the effect of sorting to understand the causal effect of segregation on student outcomes. Guryan (2004) used intertemporal and geographic variance in the timing of court-ordered desegregation plans to show that large gains in white exposure indices (the fraction of students who were white in a typical black student's school) yielded modest reductions in black dropout rates. Cutler and Glaeser (1997) relied on aggregate city-level data (presumably less affected by sorting than school- or student-level data) to show that more segregated cities had significantly larger gaps in black-white dropout rates. Card and Rothstein (2007) extended this considerably, showing that the black-white gap in SAT scores was positively correlated with cities' degree of segregation, and that neighborhood segregation appeared to be more of a factor than within-school segregation.

Despite integration efforts across the country (court-ordered and voluntary), racial, ethnic, and economic segregation persists in many public school districts. The socioeconomic topology of urban and suburban areas and the wide variance of economic prosperity within demographic groups hinder even the most ambitious school assignment mechanisms from maintaining racial or economic balance across schools.[1] Some have hoped that school choice would ameliorate segregation patterns by giving disadvantaged, urban families the option to send their children to more affluent, outlying schools (see, e.g., Rabinovitz (1997) and Schneider, Teske, and Marschall (2000)). But in practice, many elements of school choice–including charter schools, private school vouchers, and open enrollment–have increased racial isolation indices.[2]

Racial imbalance in student populations has been shown to widen the black-white test score gap in at least one setting with school choice: North Carolina's charter schools. Bifulco and Ladd (2007) explored the effects of racial imbalance in North Carolina charter schools on student test outcomes and the black-white achievement gap. Black students who made a segregating move to a charter school tended to see lower math gains as a

[1] By way of example, consider the integration efforts of Wake County, North Carolina, and San Francisco, California. Despite busing some students for over an hour each day, Wake County's plan for "economic integration" falls far short of ensuring that no more than 40 percent of each school's students have subsidized lunch (Silberman (2006)). In San Francisco's public school system, economic integration efforts may have actually intensified racial and ethnic segregation (Glater and Finder (2007)).

[2] See, for example, Bifulco and Ladd (2007), Frankenberg, Lee, and Orfield (2003), Institute on Race and Poverty (2008), and Levin (1998). Hoxby (2003) noted that negative peer effects arising from school choice and student sorting could be more than offset by productivity gains in deregulated education markets.

result. Furthermore, white *and* black students appeared to prefer charters with more white students. Where racially imbalanced and balanced charter schools were available, black mainstream students were more likely to move to a balanced charter than a predominantly black charter, whereas white students were more likely to move to a predominantly white charter.

Nonetheless, there may be an intrinsic–if not completely offsetting–benefit to charter schools' polarized student compositions. Racially imbalanced student populations in choice schools are generally coincident with racially imbalanced faculties. I show that this was the case for the first ten years of North Carolina's charter system–charter schools were much more likely than mainstream schools to have disproportionately high rates of nonwhite teachers. Research from non-choice and experimental settings have shown that assignment to a demographically similar teacher can be beneficial. So even if black student achievement was sub-par in largely black schools, students may have derived some benefit from exposure to more black teachers. Ehrenberg et al. (1995), using the 1988 and 1990 waves of the National Educational Longitudinal Study (NELS), showed that assignment to an own-race or own-gender teacher typically had no effect on subject test gains, but did yield higher subjective evaluations by teachers. A wealth of studies in education, sociologic, and multi-disciplinary literatures provide evidence that students benefit from being paired with a teacher of the same race–however, the magnitude of this benefit and the causal mechanisms responsible for it are unclear (Ferguson (1998)).

The most compelling evidence on student-teacher matching by race comes from Dee (2004, 2005). In the earlier study, Dee exploited the random assignment of students to classrooms in Tennessee's Project STAR experiment to show that assignment to an own-race teacher significantly increased math and reading scores by 3 - 5 percentage points (10 - 18 percent of a standard deviation). Dee (2005) revisited the NELS data, employed a matched-pairs methodology to control for student heterogeneity, and examined how two different subject teachers evaluated the same student at the same time. Results suggested that own-race and own-gender teachers were less likely to identify a student as disruptive or inattentive.

The literature on student-teacher racial pairing has yet to examine the gain or loss from demographically similar charter teachers, likely because of data availability and endogeneous student and teacher sorting into these schools. But if students and teachers are sorting into charter schools on the basis of race, effectively re-segregating a fraction of public schools, policy stakeholders should develop a close analysis of the benefits and costs therein. I document considerable racial imbalance in charter faculties (which appears to have been more severe in schools with very high shares of nonwhite students), and determine whether students benefitted–empirically, at least–from attending a charter school with more teachers who shared their race or gender. I incorporate faculty racial profiles into common models of charter effectiveness, estimate an average effect of charter enrollment on achievement gains for four race/gender categories, and examine how that effect varied with changes in faculty race and gender profiles. I find that black students had marginally greater math performance in charters with disproportionate rates of nonwhite teachers, relative to black students in more representative schools. But this effect was small compared to the effect of racial imbalance in charter student populations, which widened the performance gap between largely white and largely nonwhite schools.

3.2 Racial Imbalance in North Carolina Charter Schools

3.2.1 Data

I employ data on North Carolina public schools and students over the years 1996 - 2007.[3] Students' end-of-grade (EOG) exam scores serve as measures of student achievement. EOG exams are administered statewide to public school students in grades three through eight. I assembled a panel of student test scores by cohort, starting with

[3] The data are maintained by the North Carolina Education Research Data Center at Duke University. See Muschkin et al. (2008).

the 1996 grade three cohort,[4] up through the 2005 grade 3 cohort. Each student in the sample has at least three consecutive years of math and reading test data. Necessarily, this sample construction will exclude students who were only observed in grades three and four. I dropped students who repeated a grade or had gaps in their time series. I sampled complete testing histories for 20 percent of exclusively mainstream students–those who were never observed in a charter school–to ease computational burdens. Raw EOG math and reading scores are designed by accountability administrators to be vertically scaled across grades each year. The range of raw scores has varied over time, so I standardized level scores to have a zero mean and standard deviation equal to one by year, therein preserving the vertical scale across grades and allowing for longitudinal comparison. The change in a student's standardized score from one year to the next reflects advancing knowledge.

I constructed campus-by-year faculty profiles from aggregated school activity reports. Activity reports document each school-related activity where personnel have direct contact with students–classroom instruction, lunch, field trips, etc. I identified teachers to be individuals with teaching assignments listed in the activity reports, excluding teaching assistants, facilitators, and DARE officers. The reports have very little information about teacher qualifications, but they do list teachers' race/ethnicity, gender, and status as a first-year teacher. I used these data to construct faculty profiles for charter and mainstream schools over years 1996 - 2007. I aggregated race, gender, and experience indicators to the school level, and calculated the percent of teachers who were white, nonwhite (black, Hispanic, or other non-Caucasion), female, and new to public education. These profiles were then linked to student test records. I limited the analysis to black and white public school students. Longitudinal microdata are not available for private school students. Hispanic and other non-black, non-Caucasian students accounted for

[4] Meaning, students who were in grade three in the 1995-1996 school year.

10.5 percent of mainstream students and 7.5 percent of charter students, a non-trivial and growing share of public school students in North Carolina, but one that is nonetheless too small to examine separately.

3.2.2 Descriptive Figures and Statistics

North Carolina is a good setting to study racial imbalance in charter schools. The state's charter system was authorized in 1996, and the first schools opened for the 1998 school year. The legislation included what turned out to be a hollow requirement to maintain racial and ethnic balance in charter schools. Charter campuses must "reasonably reflect the racial and ethnic composition of the general population."[5] An exception is made for schools serving targeted populations–college-bound students, for example, or students at risk of failure. In that case, student populations must reflect the racial and ethnic profile of the local target groups. This exception has driven a striking pattern of racial imbalance in North Carolina charter schools, described in Tables 3-1 through 3-4 and illustrated in Figures 3-1, 3-2, and 3-3.

Tables 3-1 through 3-4 present comparative summary statistics for charter and mainstream students, by race and gender. Black charter students had significantly lower math and reading level scores than black mainstream students, and significantly lower gains in math. White charter students had lower math level scores and gains than their white mainstream counterparts, but higher reading levels alongside lower reading gains. Males typically performed better in math, and females performed better in reading. The black-white achievement gap was about 0.098 standard deviations wider in the charter sector for females, and 0.115 standard deviations wider for males.

Tables 3-1 through 3-4 also identify the likelihood of being enrolled in a racially imbalanced school. Throughout the paper, I define a racially imbalanced student population to be one whose share of nonwhite students was twenty percentage points

[5] NC Gen. Stat. 115C-238.29F(g)(5) (1996)

above or below the surrounding county share.[6] Schools with nonwhite student imbalances were at least twenty percentage points above the county mean, and schools with white student imbalances were at least twenty percentage points below. I define nonwhite and white imbalanced faculties similarly. The twenty percent cutoff is arbitrary, but consistent with many court-mandated integration plans and Bifulco and Ladd (2007). Forty-eight percent of black charter students were enrolled in a school with a racially imbalanced (i.e., disproportionately large) nonwhite student population, compared to about 29 percent of black mainstream students. Black charter students were also more likely to be enrolled in a disproportionately *white* school. White charter students were 3.5 times more likely than white mainstream students to be in a white-imbalanced school, and were rarely found in a nonwhite-imbalanced school. Racially imbalanced faculties were more common in charter schools, particularly the schools black students attended. Charter students of both races were more likely to be in a school with an imbalanced student population *and* and an imbalanced faculty, though this was much more prevalent for black charter students. Forty percent of black charter students were in doubly imbalanced schools like this, more than three times the rate of black mainstream students.[7] These sample means suggest a lack of student diversity North Carolina's charter schools, and a lack of faculty diversity in charters with relatively large shares of black students. Figures 3-1, 3-2, and 3-3 confirm and elaborate these patterns.

[6] County-level demographic shares are benchmarks, against which the demographic composition of particular schools are evaluated. A finer measure of local demographics (by zip code, for instance) may better reflect the pool of students a school could reasonably draw from, but county statistics are more relevant for policy purposes. For the vast majority of students in North Carolina, school districts are contiguous with county boundaries.

[7] The correlation between nonwhite student and faculty imbalances among black charter students will complicate efforts to estimate their separate effects. I address this in regressions to follow by distinguishing between "moderate" and "severe" nonwhite imbalances in school faculties.

Figure 3-1 plots kernel density estimates of the campus-level percent of students who were nonwhite, by charter/mainstream designation and weighted by enrollment. The distribution for charter schools was starkly bimodal; charters were much more likely than mainstream schools to have fewer than 20 percent or more than 90 percent nonwhite students. Faculty composition may be a factor in how students and their families sort into charter schools. Figure 3-2 plots weighted kernel density estimates of the campus-level percent of teachers who were nonwhite, by charter/mainstream designation. Charters were somewhat less likely than mainstream schools to have very low rates of nonwhite teachers, but more likely to have very high rates, in the range of 60 - 100 percent. Figure 3-3 illustrates the nonparametric relationship between student and faculty compositions in charter and mainstream schools. The share of nonwhite teachers in schools tended to increase with the share of nonwhite students, but the relationship was much steeper for charters where at least 60 percent of students were nonwhite.

3.3 Analytic Methods and Results

North Carolina's charter schools were polarized in student racial compositions, and to a lesser extent in faculty racial compositions. If parents had strong preferences for teachers who shared their race, then unrepresentative faculties would have driven much of the student segregation observed in these schools. Bifulco and Ladd (2007) showed that the segregating effects of the state's charter schools increased racial isolation indices and widened the black-white achievement gap. However, there is evidence from other settings where students performed better, or were perceived better, by teachers of like race. We might expect to see significant gains result from student-faculty racial dynamics in the charter sector, where student and teacher sorting is not driven by centralized assignment. It seems reasonable that students who enrolled in a charter school with a disproportionate share of own-race teachers are students whose parents believed they would benefit from the match. That is to say, if there is a significant gain (to some students) from racially imbalanced faculties, we should see evidence of this in charter schools, particularly in a charter system where a large share of schools were racially imbalanced, like North Carolina. Some students may have realized significantly greater achievement with demographically similar charter faculties; this would in part justify demand-driven racial sorting across charter schools, in spite of decades of desegregation efforts. The analyses to follow test whether differences in charter student achievement were attributable to larger shares of demographically similar teachers.

3.3.1 Methods

I estimate the following model describing subject k (math or reading) scores for student i in grade G, year T, school s, and county C:

$$Z_{iGT}^{k} = \lambda^{k} Z_{iG-1T-1}^{k} + C_{iT}\beta_{C}^{k} + \mathbf{F}_{sT}\beta_{F}^{k} + (C_{iT} * \mathbf{f}_{sT})\beta_{f}^{k} + \mathbf{M}_{iT}\beta_{M}^{k} + \alpha_{G}^{k} + \alpha_{CT}^{k} + \varepsilon_{iGT}^{k} \quad (3\text{–}1)$$

Inputs include an indicator for enrollment in a charter school (C_{iT}), faculty characteristics ($\mathbf{F}_{sT}$, some of which are allowed to have a distinct impact in charter schools via $C_{iT} * \mathbf{f}_{sT}$), and student mobility indicators ($\mathbf{M}_{iT}$). Observable endowments such as parental education and income, as well as unobservable endowments like students' inherent ability, are absorbed in the lagged level score, $Z_{iG-1T-1}$. The coefficient α_{CT}^{k} represents county-year fixed effects, and α_{G}^{k} represents common grade-level effects. The rate of proportional depreciation in the effect of prior year inputs is given by λ^{k}, and is assumed to be constant across grades and time. I estimate Equation 3–1 by ordinary least squares (OLS). Robust standard errors allow for clustering by student identifiers. Clustering will modify standard errors for any student-specific correlation in residuals, but will not parameterize that correlation like fixed effects. I also estimate how these inputs affect a student's *gain* in standardized math and reading scores:

$$\Delta Z_{iGT}^{k} = (1-\lambda^{k}) Z_{iG-1T-1}^{k} + C_{iT}\beta_{C}^{k} + \mathbf{F}_{sT}\beta_{F}^{k} + (C_{iT} * \mathbf{f}_{sT})\beta_{f}^{k} + \mathbf{M}_{iT}\beta_{M}^{k} + \alpha_{G}^{k} + \alpha_{CT}^{k} + \varepsilon_{iGT}^{k} \quad (3\text{–}2)$$

Following Hanushek et al. (2007) and Sass (2006), I instrument for $Z_{iG-1T-1}$ in Equation 3–2 using twice lagged level scores, $Z_{iG-2T-2}$. As in Equation 3–1, I allow for robust standard errors, clustered by student identifiers. I estimate Equations 3–1 and 3–2 separately for black females, black males, white females, and white males. The levels specification requires at least two years of continuous test data, whereas the gains specification requires three. To ensure that results from both models reflect the achievement of the same set of students, I limit the levels analysis to students with at least two prior years of test data. Consequently, the results to follow describe the effect of charter enrollment and racial compositions on the achievement of grade five through eight students.

The vector $\mathbf{F}_{sT}$ contains school-wide faculty statistics at time T: the percent of teachers who were new, the percent who shared i's gender, and four indicators of racial imbalance in student and faculty compositions. Changing schools tends to have a negative effect on student gains (Hanushek et al. (2004)), particularly if a student is moving to a charter school from the mainstream sector (see, e.g., Bifulco and Ladd (2006), Booker et al. (2007), Hanushek et al. (2007), and Sass (2006)). Accordingly, $\mathbf{M}_{iT}$ is a vector of mutually exclusive mobility indicators for students who moved within their county, across counties, to a charter school, from a charter school, and for students making structural school changes. Following the literature, I defined a structural change as any move shared by at least thirty percent of a student's cohort.[8]

Interactions between charter enrollment and faculty race/gender profiles are of particular interest here. If students benefitted from enrolling in a charter school where a higher proportion of the faculty shared their race and/or gender, then I would expect $\hat{\beta}_f^k > 0$ for those interactions. Identifying variation in $C_{iT} * \mathbf{f}_{sT}$ comes from (mostly

[8] In North Carolina, grade six is the typical starting grade for middle school. So $\hat{\alpha}_6^k$ will reflect the average gain (or loss) resulting from a move to grade six in year T, which is a structural change for most students. The structural change indicator in $\mathbf{M}_{iT}$ will control for any other school changes that involved at least thirty percent of one's cohort–reassignment, moving to a middle school with a starting grade other than six, etc.

cross-sectional) changes in faculty race/gender profiles across the spectrum of charter schools. Coefficient estimates from levels and gains equations will help us determine if, for instance, black male charter students made larger gains in schools with higher shares of black teachers, controlling for their initial level of achievement. The nature of this inference is largely descriptive, because teachers and students sort endogenously into charter schools. Nonetheless, results will be an informative glimpse at the relationship between student achievement and student-faculty racial profiles in charter schools. Later on, I investigate how endogenous sorting may have affected my findings.

These models draw from several recent studies on the effectiveness of charter schools. Bifulco and Ladd (2006) identified the relative effectiveness of charter enrollment for North Carolina students with experience in charter and mainstream schools, and showed that these sector-switchers performed significantly worse in the former. Booker et al. (2007) broke down the effect of a charter education by school age and student tenure, demonstrating significant returns to both. Both studies used student fixed effects to parameterize heterogeneity in unobserved ability. By contrast, Sass (2006) and Hanushek et al. (2007) used lagged achievement models similar to the levels and gains specifications employed here to estimate the average effectiveness of charter schools by school age. In all of these studies, the mover year experience for a student transitioning from a mainstream school to a charter school was significantly negative, in terms of test performance. Students moving in the other direction, however, made up for much of this loss in their first mainstream year.

3.3.2 Results

Tables 3-5 through 3-8 present coefficient estimates from the levels and gains equations, which were estimated separately for black females (Table 3-5), black males (Table 3-6), white females (Table 3-7), and white males (Table 3-8). I proceed through these results by first outlining some common themes across race/gender cells (regarding the effect of moving to or from a charter school and the effect of more male teachers in charter schools). Then, I discuss how the test performance of each race/gender group was affected by various types of racial imbalance.

Consistent with the literature on the effectiveness of charter schools, I find that moving to a charter school resulted in significant penalties to math and reading achievement. These penalties were large compared to the effect of other types of (unreported) school changes. The major cost of stratifying the sample in this way is loss of precision, but degrees of freedom appear to be large enough to draw inference, even in the smallest race/gender cells. The primary benefit of stratification is that I can allow coefficients to vary by race and gender. For instance, white students incurred a larger average penalty upon moving to a charter school than black students, although confidence intervals overlapped. Female charter students of both races saw significantly larger levels and gains in both subjects, relative to mainstream females, whereas the effect of charter enrollment on males' test outcomes was either negative or insignificant. A charter education was predicted to increase the math level scores of black females by 0.221 standard deviations (sd), relative to to black mainstream females. This accounts for about 30 percent of the black-white gap in math levels.

North Carolina's charter schools had higher rates of male teachers: 22 percent, versus 11 percent in mainstream schools. Males are historically under-represented in public teaching, and Dee (2007) showed that male students in the NELS survey performed significantly better when assigned to a male teacher. I find that in North Carolina, there was generally a negative correlation between student performance and the fraction of faculties that were male, except for white male students in charter schools. Coefficient estimates for "% male teachers" in Table 3-8 report the effect of a ten percentage-point increase in male faculty on mainstream test outcomes for white males. The effect was negative and significant for math and reading outcomes (by as much as 0.015 sd for reading levels), meaning that mainstream white males performed worse in schools with more male teachers. Coefficients on the interaction, "charter x % male teachers" represent the effect of a ten percentage-point increase in *charter* male faculty, *relative to* the baseline coefficient for "% male teachers." These interaction estimates were positive and significant in Table 3-8, and larger in absolute value than baseline effects. So the marginal effect of more male *charter* teachers was positive: higher rates of male teachers in charter schools were associated with greater math and reading performance for white male charter. For black males and females of both races, the effect of higher male representation in school faculties tended to be negative, but less so in charter schools.

Tables 3-5 through 3-8 include estimates of the impact of student and faculty racial imbalance in charter and mainstream schools. Schools were identified as having imbalanced nonwhite (white) student populations if the share of nonwhite students was twenty percentage points above (below) the county-wide share. Racial imbalance in faculties were similarly defined, but for black students, I included two degrees of nonwhite faculty imbalance. In moderately imbalanced nonwhite faculties, the share of nonwhite teachers was 20 - 40 percentage points above the county mean, and in severely imbalanced nonwhite faculties, the share was more than 40 percentage points above the county mean. Baseline imbalance coefficients represent the estimated effect of a particular racial imbalance on student achievement, relative to student achievement in balanced schools. Interaction coefficients show how the effect of that imbalance was *different* in charter schools. In Table 3-5, for example, coefficient estimates for "nonwhite student imbalance" show that black females tended to see 0.014 sd lower math levels and 0.016 sd lower reading levels in schools with disproportionately large shares of nonwhite students, relative to their counterparts in balanced schools (the omitted category). But the penalty attributed to imbalanced rates of nonwhite students was even larger in charter schools, by 0.038 - 0.043 sd. So in total, a black female in a largely nonwhite charter school was expected to score 0.054 - 0.057 sd lower than a black female in a racially balanced mainstream school. Black females in schools with disproportionately large white student populations realized significantly higher achievement levels: 0.031 sd in math and 0.019 sd in reading, and the effect was not significantly different in charter schools.

The last four rows of Table 3-5 describe how racial imbalance among black females' teachers affected test performance. Black females saw significantly lower math levels and gains in schools with moderate or severe nonwhite faculty imbalances. If there were offsetting effects of nonwhite faculty in charter schools on math levels, they were not statistically significant at conventional levels. Math gains, however, increased by 0.40 sd in schools with severe nonwhite faculty imbalances, relative to black female charter students with balanced faculties. Relative to all students, charter or otherwise, enrolling in a charter school with a severely imbalanced and largely nonwhite faculty increased black females' math gains by 0.017 sd (-.023 + 0.040). This accounts for more than 40 percent of the black-white gap in math gains for mainstream females.[9] But black females benefitted much more by enrolling in charters with *white*-imbalanced faculties in all measures of achievement except reading gains. In Table 3-6, we see that black males also benefitted from white-imbalanced charter faculties, by as much as 0.199 sd in math levels (about 28 percent of the black-white gap among mainstream males). Black charter students were about six percentage points more likely than black mainstream students to be in a school with an imbalanced share of white teachers, relative to the surrounding country. There were more than 350 black students in charters like this (with more than 1100 student-years of testing data), and of these, 81.2 percent were observed in mainstream schools before moving to the charter system. So I can observe the type of schools they opted out of. Even if the charter system as a whole was racially polarized, nontrivial numbers of minority students may have been taking advantage

[9] From Tables 3-1 and 3-3, average math gains were 0.286 sd for black females, and 0.327 sd for white females. The 0.017 sd bonus from severely imbalanced faculties represents 41.5 percent of the 0.041 sd gap.

of the opportunity to move to largely white charter schools from largely nonwhite mainstream schools. One-quarter of of black students in white-imbalanced charter schools had previously attended nonwhite-imbalanced mainstream schools (in terms of student or faculty composition). But this was true for 41.2 percent of black charter students generally, who were also less likely to have moved from a white-imbalanced mainstream schools.

In Table 3-6 (reporting results for black males), coefficient estimates for "charter x moderate nonwhite faculty imbalance" show that there was no statistically significant bonus from moderately imbalanced faculties. But severely imbalanced nonwhite charter faculties increased the math levels and gains of black males by 0.047 and 0.043 sd, respectively, relative to black males in charters with balanced faculties. These effects were larger in absolute value than the baseline effects of severely nonwhite-imbalanced faculties (-0.027 sd and a statistically insignificant -0.013 sd, respectively). Judging by coefficient estimates alone, the marginal effect of severely imbalanced shares of nonwhite charter teachers was small but positive for black males' math performance, increasing math levels by an estimated 0.020 sd. This is an economically modest boost, representing just 2.8 percent of the black-white gap in math levels for males. Also, note that among black males in charter schools, nonwhite faculty imbalances were highly coincident with nonwhite student imbalances. So any benefit from nonwhite faculty would likely have been paired with negative peer effects arising from large shares of nonwhite students, which are estimated to have decreased black males' math levels by 0.017 sd.

Tables 3-7 and 3-8 report results for white females and white males, respectively. White females had significantly higher math levels and gains in schools with white-imbalanced student populations. The bonus from this own-race imbalance was just 0.016 sd for math levels and 0.009 sd for math gains, but in charter schools it was considerably larger, by 0.052 and 0.050 sd, respectively. So a disproportionate share of white peers was associated with a total increase in charter females' math levels by 0.068 sd, and an increase in their math gains by 0.059 sd. White males also had higher math performance in white-imbalanced charter schools, but significantly *lower* reading gains. The effect of teachers' racial imbalances on white students' relative achievement across sectors was more ambiguous. White faculty imbalances were associated with higher math levels and gains for males and females, but this effect was either not significantly different in charter schools, or it significantly offset the baseline effect, by 0.032 sd in the case of females' math gains.

The results outlined above suggest that racial imbalance in charter student populations increased performance gaps between white-imbalanced and nonwhite-imbalanced schools, particularly with respect to math achievement. Black students achieved significantly lower math and reading levels in schools with disproportionate rates of nonwhite students, and for black females, this penalty was even larger in charter schools with nonwhite student imbalances. White students realized greater math levels and gains in charters with imbalanced rates of white students, relative to whites in racially balanced mainstream and charter schools. Together, these patterns suggest that while largely white charter schools were pulling away, largely black charters were falling behind. I investigated whether there was a silver lining to this normatively undesirable pattern, in that black charter students may have derived some benefit from predominantly nonwhite *faculties*. Black students did in fact have somewhat higher math achievement in charters with high rates of nonwhite teachers, relative to their counterparts in schools with racially balanced faculties. In section 3.3.3, I discuss how this premium may have been affected by the way students sorted into racially imbalanced schools.

3.3.3 Student Fixed Effects Analysis

The gain from own-race teachers would be under (over) stated if students with inherently lower (higher) growth rates differentially sorted into schools with more own-race teachers. For instance, the results outlined above may be under-optimistic with respect to disproportionately nonwhite faculties if high-growth students of either race were more likely to sort into largely white charter schools. Lagged achievement would insufficiently control for heterogeneous student ability if this were the case. I investigate this possibility by estimating student fixed effects for mainstream test gains, and then analyzing the gap in averaged fixed effects between balanced and nonwhite-imbalanced schools, by charter/mainstream sectors. I proxy charter students' fixed effects using the fixed effects estimated for *future* charter students before they moved from mainstream to charter schools. If mainstream students who moved to charter schools staffed largely with nonwhite teachers had lower average fixed effects, this would be circumstantial evidence of the type of differential sorting that would have biased my earlier findings against nonwhite faculties. Student fixed effect estimates were generated in a gains equation much like Equation 3–2:

$$\Delta Z^k_{iGT} = \mathbf{M}_{iT}\beta^k_M + \mathbf{F}_{sT}\beta^k_F + \alpha_{GT} + \alpha^k_i + \varepsilon^k_{iGT} \qquad (3\text{–}3)$$

As in Equation 3–2, Equation 3–3 includes controls for student mobility ($\mathbf{M}_{iT}$) and faculty characteristics ($\mathbf{F}_{sT}$). Students' inherent growth is represented by a fixed effect (α^k_i), rather than lagged achievement. Note that in order for future charter students to have valid fixed effects estimates, they must have had at least four continuous years of testing data (five years is the maximum length of a student's time series), including at least three years in mainstream schools. I generated fixed effects estimates for 10,160 future charter students, representing 24.9 percent of charter students from section 3.3.2. The vast majority of these students (94 percent) ultimately sorted into balanced or nonwhite-imbalanced charter schools.

Tables 3-9 and 3-10 summarize average student fixed effects on math and reading exams, respectively, for students in schools with racially balanced faculties ($\bar{\bar{\alpha}}^k_{bal}$), and average fixed effect for students with moderately or severely nonwhite-imbalanced faculties ($\bar{\bar{\alpha}}^k_{mod}, \bar{\bar{\alpha}}^k_{sev}$). The third column of Table 3-9 lists the charter-mainstream difference in average math fixed effects for each faculty composition category. Among all students in severely racially imbalanced schools, charter students had 0.026 sd lower math fixed effects. Turning to the third column of Table 3-10, among all students in schools with racially balanced faculties, charter students had 0.031 sd lower reading fixed effects than mainstream students. Otherwise, charter students were not statistically different from mainstream students in similarly staffed schools. The fourth row of Tables 3-9 and 3-10 lists the balanced-moderate imbalance difference in average fixed effects ($\bar{\hat{\alpha}}^k_{bal} - \bar{\hat{\alpha}}^k_{mod}$), for each public education sector. Students in balanced schools outperformed students in moderately imbalanced schools in math, but not reading. The fifth row of each table lists the balanced-severe imbalance difference ($\bar{\hat{\alpha}}^k_{bal} - \bar{\hat{\alpha}}^k_{sev}$), by sector. Again, students in balanced schools had higher math fixed effects, by as much as 0.049 sd in the charter sector, but significantly lower reading fixed effects. So students who sorted into balanced charter schools tended to have higher inherent math gains, but lower reading gains, than students who sorted into imbalanced charters. But the same could be said for mainstream students; mainstream students' math fixed effects were higher in schools with racially balanced faculties, but reading fixed effects were higher in schools with nonwhite-imbalanced faculties.

Charter students' elective sorting would not have biased results against nonwhite charter faculties unless the balanced-imbalanced gap was wider in the charter sector than in the mainstream sector. That is, the estimated benefit to black charter student achievement that I attributed to largely nonwhite faculties would have understated the true benefit if students who sorted into these schools had lower relative growth rates than mainstream students with nonwhite-imbalanced faculties. The intersections of difference rows and columns in Tables 3-9 and 3-10 show the cross-sector difference in balanced-imbalanced gaps, calculated according to the following expression:

$$[\bar{\hat{\alpha}}^k_{bal} - \bar{\hat{\alpha}}^k_j]^{charter} - [\bar{\hat{\alpha}}^k_{bal} - \bar{\hat{\alpha}}^k_j]^{mainstream} \qquad j = mod, sev \tag{3–4}$$

The math achievement gap between balanced and severely nonwhite-imbalanced schools was 0.020 sd wider in the charter sector, and this difference was marginally significant (with 93.7% confidence). Interestingly, the gap in reading achievement between balanced and both types of nonwhite-imbalanced schools was significantly narrower in charter schools. With respect to reading gains, charter students with nonwhite-imbalanced faculties outperformed mainstream students with nonwhite-imbalanced faculties. So my estimates of the effectiveness of nonwhite-imbalanced charter faculties in raising reading performance (which were typically insignificant in Tables 3-5 through 3-8) may have *overstated* the true effect.

This robustness check was limited by the fact that 75 percent of charter students had insufficient experience in mainstream schools to assess their inherent growth prior to enrolling in a charter school. Nonetheless, the analysis provides evidence that the correlation between disproportionate rates of nonwhite teachers and higher math performance among black males may have been under-estimated.

3.4 Conclusions

I document a starkly bimodal distribution in the representation of nonwhite students in North Carolina's charter schools. Faculty racial compositions were also somewhat polarized; white teachers were concentrated in charters with large shares of white students, and nonwhite teachers were over-represented in schools with large shares of nonwhite students. This presented an opportunity to test whether black charter students benefitted from disproportionate shares of nonwhite teachers, and whether white charter students benefitted from disproportionate shares of white teachers. Research has shown that assignment to an own-race teacher can improve student outcomes. I extend this thread of the literature into the charter sector, to better understand student-faculty racial dynamics in charter schools, and to evaluate whether those dynamics offset or reinforce the effects of increased racial isolation in charter schools. Consistent with earlier research, I find that racial polarization patterns in charter student populations widened the gap between math performance in disproportionately white versus disproportionately nonwhite schools. Black females realized lower math levels in mainstream schools with high rates of nonwhite students, and even lower levels in charter schools with high nonwhite representations, while white students realized higher math levels and gains in white-imbalanced charter schools. There was a silver lining, however. Black students were negatively affected by high rates of nonwhite peers, but they realized small bonuses to math achievement from locally disproportionate rates of nonwhite teachers, relative to their peers in schools with more representative faculties. But this gain from nonwhite faculties was too small to advise black students against attending racially balanced mainstream schools or charters with imbalanced shares of *white* faculty.

Racial segregation (or re-segregation) is an increasingly common outcome of public school choice plans. The best available evidence suggests that segregation attributable to school choice increases the achievement gaps between white and black students, or between high-SES and low-SES students (see, e.g., Bifulco and Ladd (2007) and Hastings et al. (2008)), and my findings reiterate this point. Charter schools have considerable flexibility to recruit nontraditional teachers. North Carolina's charter system, like many state systems, has relaxed licensure and pay standards for charter faculties. This flexibility may have allowed the state's charter schools to recruit more males and nonwhite teachers, two under-represented groups in traditional, mainstream public schools. I show that while some black students benefitted from an influx of nonwhite teachers in charter schools, their gains were insufficient to slow the growing gap between students in predominantly white versus predominantly nonwhite schools.

Table 3-1. Black female public school students: Achievement and racial imbalance summary statistics

	Charter	Mainstream	Difference
Math level score	-0.143	-0.008	-0.135*
	(0.805)	(0.748)	
Math gain	0.254	0.286	-0.032*
	(0.470)	(0.446)	
Reading level score	0.030	0.069	-0.039*
	(0.786)	(0.749)	
Reading gain	0.294	0.301	-0.007
	(0.511)	(0.499)	
Attends school with nonwhite student imbalance	0.468	0.281	0.187*
	(0.499)	(0.449)	
Attends school with white student imbalance	0.070	0.048	0.022*
	(0.255)	(0.213)	
Attends school with nonwhite faculty imbalance	0.452	0.180	0.272*
	(0.498)	(0.384)	
Attends school with white faculty imbalance	0.091	0.025	0.066*
	(0.288)	(0.156)	
Attends school with nonwhite student and faculty imbalances	0.395	0.130	0.265*
	(0.489)	(0.336)	
Attends school with white student and faculty imbalances	0.024	0.011	0.013*
	(0.153)	(0.105)	
n (student years)	6,835	83,473	

Notes: Standard deviations are in parentheses below each mean. * denotes a statistically significant difference at 95% confidence. For computational convenience, I sampled 20% of students who were never observed in a charter school. n includes all sampled students (including 100% of all students who were ever observed in a charter school) with at least three years of math and reading test data, by race/gender.

Table 3-2. Black male public school students: Achievement and racial imbalance summary statistics

	Charter	Mainstream	Difference
Math level score	-0.203	-0.070	-0.133*
	(0.776)	(0.774)	
Math gain	0.247	0.271	-0.025*
	(0.486)	(0.468)	
Reading level score	-0.146	-0.080	-0.066*
	(0.803)	(0.798)	
Reading gain	0.298	0.301	-0.003
	(0.556)	(0.546)	
Attends school with nonwhite student imbalance	0.496	0.291	0.205*
	(0.500)	(0.454)	
Attends school with white student imbalance	0.067	0.050	0.017*
	(0.250)	(0.219)	
Attends school with nonwhite faculty imbalance	0.486	0.189	0.297*
	(0.500)	(0.391)	
Attends school with white faculty imbalance	0.084	0.025	0.058*
	(0.277)	(0.157)	
Attends school with nonwhite student and faculty imbalances	0.428	0.137	0.291*
	(0.495)	(0.344)	
Attends school with white student and faculty imbalances	0.021	0.012	0.009*
	(0.143)	(0.108)	
n (student years)	6,017	73,096	

Notes: Standard deviations are in parentheses below each mean. * denotes a statistically significant difference at 95% confidence. For computational convenience, I sampled 20% of students who were never observed in a charter school. n includes all sampled students (including 100% of all students who were ever observed in a charter school) with at least three years of math and reading test data, by race/gender.

Table 3-3. White male public school students: Achievement and racial imbalance summary statistics

	Charter	Mainstream	Difference
Math level score	0.574	0.611	-0.037*
	(0.857)	(0.843)	
Math gain	0.284	0.327	-0.043*
	(0.461)	(0.437)	
Reading level score	0.710	0.651	0.059*
	(0.751)	(0.759)	
Reading gain	0.275	0.296	-0.022*
	(0.479)	(0.474)	
Attends school with nonwhite student imbalance	0.016	0.051	-0.035*
	(0.126)	(0.220)	
Attends school with white student imbalance	0.448	0.121	0.327*
	(0.497)	(0.326)	
Attends school with nonwhite faculty imbalance	0.018	0.022	-0.004*
	(0.134)	(0.146)	
Attends school with white faculty imbalance	0.122	0.024	0.098*
	(0.328)	(0.154)	
Attends school with nonwhite student and	0.006	0.009	-0.003*
faculty imbalances	(0.078)	(0.094)	
Attends school with white student and	0.116	0.018	0.098*
faculty imbalances	(0.320)	(0.133)	
n (student years)	14,303	172,325	

Notes: Standard deviations are in parentheses below each mean. * denotes a statistically significant difference at 95% confidence. For computational convenience, I sampled 20% of students who were never observed in a charter school. n includes all sampled students (including 100% of all students who were ever observed in a charter school) with at least three years of math and reading test data, by race/gender.

Table 3-4. White female public school students: Achievement and racial imbalance summary statistics

	Charter	Mainstream	Difference
Math level score	0.623	0.641	-0.017*
	(0.877)	(0.877)	
Math gain	0.283	0.323	-0.040*
	(0.472)	(0.456)	
Reading level score	0.607	0.560	0.047*
	(0.782)	(0.796)	
Reading gain	0.272	0.298	-0.026*
	(0.509)	(0.502)	
Attends school with nonwhite student imbalance	0.022	0.054	-0.032*
	(0.146)	(0.225)	
Attends school with white student imbalance	0.419	0.123	0.296*
	(0.493)	(0.329)	
Attends school with nonwhite faculty imbalance	0.022	0.024	-0.002
	(0.147)	(0.153)	
Attends school with white faculty imbalance	0.112	0.025	0.087*
	(0.316)	(0.158)	
Attends school with nonwhite student and	0.007	0.009	-0.003*
faculty imbalances	(0.082)	(0.097)	
Attends school with white student and	0.106	0.019	0.086*
faculty imbalances	(0.307)	(0.138)	
n (student years)	13,585	171,626	

Notes: Standard deviations are in parentheses below each mean. * denotes a statistically significant difference at 95% confidence. For computational convenience, I sampled 20% of students who were never observed in a charter school. n includes all sampled students (including 100% of all students who were ever observed in a charter school) with at least three years of math and reading test data, by race/gender.

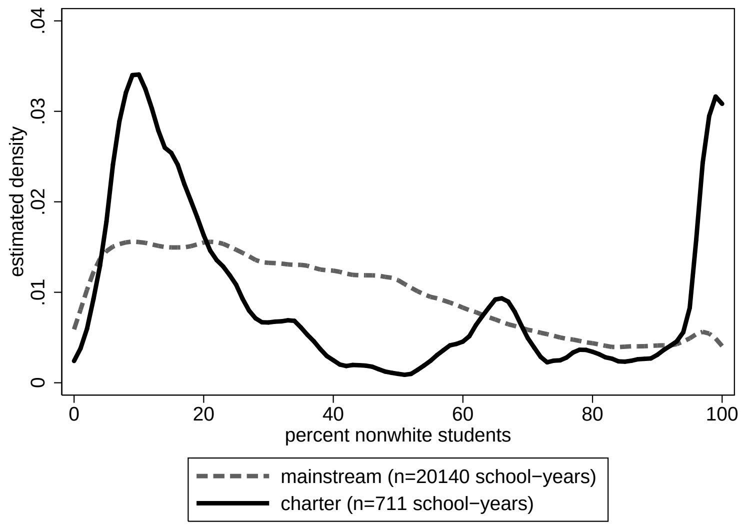

Figure 3-1. Density estimates: Percent of schools' students who were nonwhite, by charter/mainstream designation

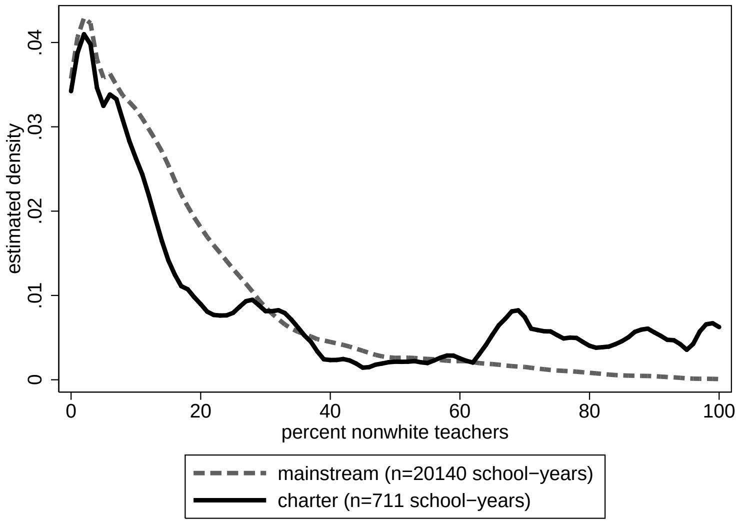

Figure 3-2. Percent of schools' teachers who were nonwhite, by charter/mainstream designation

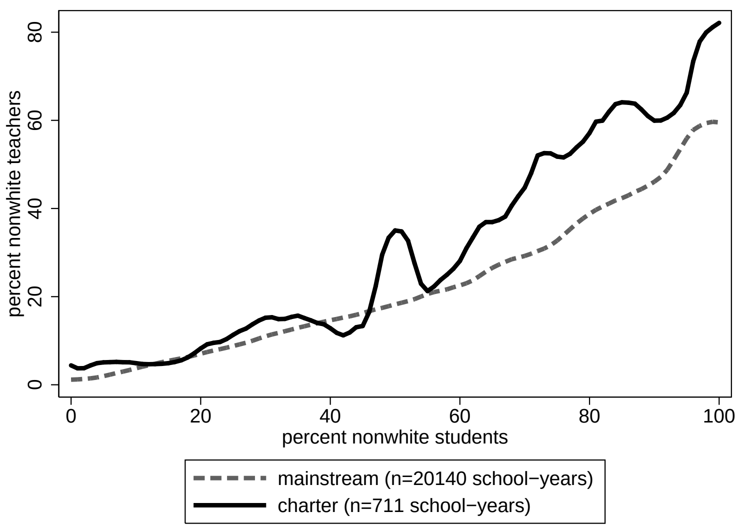

Figure 3-3. Local polynomial (degree zero, mean smoothing) estimates: Nonparametric relationship between nonwhite teachers and nonwhite students in schools.

Table 3-5. The effects of charter enrollment and racial imbalance on black female achievement

Econometric model Subject	Levels Math	Levels Reading	Gains Math	Gains Reading
Enrolled in charter	0.222	0.109	0.185	0.034
	(6.42)	(3.06)	(5.34)	(0.93)
Moved to charter	-0.116	-0.055	-0.124	-0.050
	(9.81)	(4.31)	(10.04)	(3.61)
Moved from charter	0.047	0.020	0.093	0.055
	(4.14)	(1.72)	(7.91)	(4.31)
Female teachers (%)	0.027	0.019	0.025	0.016
	(13.23)	(8.82)	(12.01)	(7.42)
Charter x % female teachers	-0.029	-0.011	-0.023	-2.4E-3
	(6.72)	(2.36)	(5.32)	(0.52)
Nonwhite student imbal.	-0.014	-0.016	-1.8E-3	2.3E-3
	(3.50)	(3.71)	(0.46)	(0.54)
Charter x nonwhite student imbal.	-0.043	-0.038	-0.017	3.5E-4
	(2.64)	(2.20)	(1.04)	(0.02)
White student imbal.	0.031	0.019	0.019	4.3E-3
	(4.30)	(2.62)	(2.75)	(0.61)
Charter x white student imbal.	-0.021	0.013	-0.027	0.011
	(0.95)	(0.53)	(1.25)	(0.46)
Moderate nonwhite faculty imbal.	-0.020	-0.011	-0.017	-3.8E-3
	(3.98)	(1.91)	(3.29)	(0.68)
Charter x moderate nonwhite faculty imbal.	0.012	0.017	0.020	0.020
	(0.60)	(0.77)	(0.93)	(0.85)
Severe nonwhite faculty imbal.	-0.031	-7.1E-3	-0.023	1.2E-4
	(3.83)	(0.79)	(2.78)	(0.01)
Charter x severe nonwhite faculty imbal.	0.035	-5.5E-3	0.040	8.9E-3
	(1.87)	(0.28)	(2.17)	(0.44)
White faculty imbal.	-0.012	-2.4E-3	-0.008	4.6E-3
	(1.18)	(0.23)	(0.80)	(0.41)
Charter x white faculty imbal.	0.185	0.103	0.128	0.049
	(7.45)	(3.97)	(5.37)	(1.84)

Notes: n = 90,308 student-years. Coefficients estimates for Equation 3–1 (Levels) and Equation 3–2 (Gains), limited to black females. Continuous faculty characteristics were scaled so that estimated coefficients are the effect of a 10 percentage-point increase. Absolute values of t-statistics are in parentheses below each coefficient. Cluster-robust standard errors allow for student-level correlation in residuals. In schools with a "moderate nonwhite faculty imbalance," the percent of faculty who were nonwhite was 20 - 40 percentage points above the county mean. A "severe nonwhite faculty imbalance" indicates the share was more than 40 percentage points above the county mean.

Table 3-6. The effects of charter enrollment and racial imbalance on black male achievement

Econometric model Subject	Levels Math	Levels Reading	Gains Math	Gains Reading
Enrolled in charter	-0.044	0.022	-0.025	0.025
	(3.22)	(1.38)	(1.83)	(1.57)
Moved to charter	-0.102	-0.059	-0.112	-0.068
	(7.76)	(4.14)	(8.09)	(4.35)
Moved from charter	0.032	0.038	0.080	0.083
	(2.55)	(2.84)	(6.14)	(5.62)
Male teachers (%)	-0.024	-0.021	-0.021	-0.017
	(10.71)	(8.44)	(9.42)	(6.88)
Charter x % male teachers	0.014	8.9E-3	0.010	5.6E-3
	(3.07)	(1.72)	(2.10)	(1.05)
Nonwhite student imbal.	-0.017	-0.013	-7.4E-3	4.3E-3
	(3.96)	(2.63)	(1.76)	(0.89)
Charter x nonwhite student imbal.	-0.027	-0.024	6.1E-3	0.027
	(1.47)	(1.16)	(0.34)	(1.30)
White student imbal.	0.015	0.015	4.0E-3	6.5E-3
	(1.86)	(1.81)	(0.54)	(0.80)
Charter x white student imbal.	-0.013	-0.065	-0.011	-0.068
	(0.51)	(2.35)	(0.44)	(2.52)
Moderate nonwhite faculty imbal.	-0.034	-0.026	-0.028	-0.020
	(5.85)	(4.12)	(4.89)	(3.02)
Charter x moderate nonwhite faculty imbal.	0.029	0.018	0.023	0.018
	(1.30)	(0.68)	(1.00)	(0.66)
Severe nonwhite faculty imbal.	-0.027	-0.013	-0.013	4.1E-4
	(3.03)	(1.35)	(1.47)	(0.04)
Charter x severe nonwhite faculty imbal.	0.047	-0.023	0.043	-0.031
	(2.27)	(0.98)	(2.03)	(1.33)
White faculty imbal.	5.3E-3	-1.1E-3	8.0E-3	1.5E-3
	(0.48)	(0.09)	(0.74)	(0.12)
Charter x white faculty imbal.	0.199	0.156	0.133	0.102
	(6.41)	(4.88)	(4.43)	(3.26)

Notes: n = 79,113 student-years. Coefficients estimates for Equation 3–1 (Levels) and Equation 3–2 (Gains), limited to black males. Continuous faculty characteristics were scaled so that estimated coefficients are the effect of a 10 percentage-point increase. Absolute values of t-statistics are in parentheses below each coefficient. Cluster-robust standard errors allow for student-level correlation in residuals. In schools with a "moderate nonwhite faculty imbalance," the percent of faculty who were nonwhite was 20 - 40 percentage points above the county mean. A "severe nonwhite faculty imbalance" indicates the share was more than 40 percentage points above the county mean.

Table 3-7. The effects of charter enrollment and racial imbalance on white female achievement

Econometric model Subject	Levels Math	Levels Reading	Gains Math	Gains Reading
Enrolled in charter	0.075	0.190	0.053	0.117
	(2.99)	(7.13)	(2.14)	(4.59)
Moved to charter	-0.135	-0.082	-0.149	-0.091
	(14.82)	(8.69)	(15.96)	(8.96)
Moved from charter	0.134	0.076	0.173	0.106
	(11.46)	(6.79)	(14.72)	(8.66)
Female teachers (%)	0.016	0.015	0.015	0.015
	(11.96)	(10.76)	(11.26)	(10.07)
Charter x % female teachers	-0.015	-0.023	-0.009	-0.013
	(4.70)	(6.93)	(2.99)	(4.07)
Nonwhite student imbal.	-0.020	-0.011	-0.009	0.002
	(4.31)	(2.37)	(2.26)	(0.34)
Charter x nonwhite student imbal.	-0.043	0.025	-0.033	0.037
	(1.42)	(0.76)	(1.04)	(1.09)
White student imbal.	0.016	-0.001	0.009	-0.004
	(4.57)	(0.37)	(2.73)	(1.30)
Charter x white student imbal.	0.052	0.007	0.050	0.010
	(6.51)	(0.89)	(6.61)	(1.26)
Nonwhite faculty imbal.	-0.049	-0.032	-0.028	-0.003
	(6.94)	(4.14)	(4.01)	(0.37)
Charter x nonwhite faculty imbal.	0.027	-0.063	0.038	-0.059
	(0.91)	(1.97)	(1.25)	(1.72)
White faculty imbal.	0.032	-0.001	0.031	0.001
	(4.08)	(0.15)	(3.97)	(0.16)
Charter x white faculty imbal.	-0.023	0.014	-0.032	-0.001
	(1.55)	(0.94)	(2.15)	(0.04)

Notes: n = 186,628 student-years. Coefficients estimates for Equation 3–1 (Levels) and Equation 3–2 (Gains), limited to white females. Continuous faculty characteristics were scaled so that estimated coefficients are the effect of a 10 percentage-point increase. Absolute values of t-statistics are in parentheses below each coefficient. Cluster-robust standard errors allow for student-level correlation in residuals. In schools with a "moderate nonwhite faculty imbalance," the percent of faculty who were nonwhite was 20 - 40 percentage points above the county mean. A "severe nonwhite faculty imbalance" indicates the share was more than 40 percentage points above the county mean.

Table 3-8. The effects of charter enrollment and racial imbalance on white male achievement

Econometric model Subject	Levels Math	Levels Reading	Gains Math	Gains Reading
Enrolled in charter	-0.075	-0.047	-0.038	-0.017
	(8.33)	(4.83)	(4.39)	(1.77)
Moved to charter	-0.137	-0.066	-0.151	-0.077
	(14.53)	(6.78)	(15.56)	(7.19)
Moved from charter	0.141	0.099	0.177	0.134
	(11.90)	(8.43)	(14.67)	(10.53)
Male teachers (%)	-0.014	-0.015	-0.012	-0.014
	(9.59)	(10.03)	(8.74)	(8.83)
Charter x % male teachers	0.021	0.024	0.014	0.016
	(6.21)	(6.80)	(4.23)	(4.56)
Nonwhite student imbal.	-0.017	-0.002	-0.008	0.008
	(3.68)	(0.43)	(1.77)	(1.75)
Charter x nonwhite student imbal.	-0.011	-0.005	-0.005	-0.003
	(0.39)	(0.18)	(0.18)	(0.08)
White student imbal.	0.012	0.004	0.006	-0.001
	(3.23)	(1.04)	(1.84)	(0.17)
Charter x white student imbal.	0.044	-0.015	0.037	-0.018
	(5.17)	(1.77)	(4.58)	(2.18)
Nonwhite faculty imbal.	-0.053	-0.029	-0.034	-0.003
	(7.15)	(3.58)	(4.79)	(0.41)
Charter x nonwhite faculty imbal.	-0.020	-0.014	0.005	0.016
	(0.74)	(0.48)	(0.18)	(0.56)
White faculty imbal.	0.044	0.010	0.044	0.015
	(5.47)	(1.15)	(5.40)	(1.67)
Charter x white faculty imbal.	-0.003	0.019	-0.010	0.001
	(0.17)	(1.17)	(0.68)	(0.08)

Notes: n = 185,211 student-years. Coefficients estimates for Equation 3–1 (Levels) and Equation 3–2 (Gains), limited to white males. Continuous faculty characteristics were scaled so that estimated coefficients are the effect of a 10 percentage-point increase. Absolute values of t-statistics are in parentheses below each coefficient. Cluster-robust standard errors allow for student-level correlation in residuals. In schools with a "moderate nonwhite faculty imbalance," the percent of faculty who were nonwhite was 20 - 40 percentage points above the county mean. A "severe nonwhite faculty imbalance" indicates the share was more than 40 percentage points above the county mean.

Table 3-9. Average student fixed effects on math exams, by charter/mainstream designation and racial imbalance

Type of school	Charter	Mainstream	Difference
Racially balanced faulty	9.1E-3	0.014	-5.2E-3
	(0.384)	(0.219)	(0.004)
Moderate nonwhite-imbalanced faculty	0.014	-0.019	0.033
	(0.383)	(0.229)	(0.017)
Severe nonwhite-imbalanced faculty	-0.040	-0.014	-0.026
	(0.337)	(0.231)	(0.010)
Difference	-5.3E-3	0.033	-0.038
(balanced - moderate)	(0.017)	(0.001)	(0.017)
	[0.31]	[23.70]	[2.22]
Difference	0.049	0.029	0.020
(balanced - severe)	(0.011)	(0.003)	(0.011)
	[4.60]	[11.01]	[1.86]
n (student-years)	10,160	500,520	

Notes: Student fixed effects were estimated for math and reading gains, described by Equation 2–2. The table gives the average student math fixed effects for three types of schools in each sector: schools with racially balanced faculties, schools with moderate nonwhite-imbalanced faculties (where the percent of nonwhite teachers was 20 - 40 percentage points above the county average), and schools with severe nonwhite-imbalanced faculties (where the percent of nonwhite teachers was more than 40 percentage points above the county average). Charter student fixed effects were proxied by the fixed effects estimated for *future* charter students, who were observed in mainstream schools before moving to the charter sector. Standard deviations are in parentheses below each mean, standard errors are in parentheses below each *difference* in means, and t-statistics are in brackets below each difference in means.

Table 3-10. Average student fixed effects on reading exams, by charter/mainstream designation and racial imbalance

Type of school	Charter	Mainstream	Difference
Racially balanced faulty	-0.034	-3.4E-3	-0.031
	(0.459)	(0.231)	(0.005)
Moderate nonwhite-imbalanced faculty	0.016	4.9E-3	0.012
	(0.463)	(0.239)	(0.020)
Severe nonwhite-imbalanced faculty	0.031	0.015	0.015
	(0.435)	(0.258)	(0.013)
Difference (balanced - moderate)	-0.051	-8.3E-3	-0.042
	(0.014)	(0.003)	(0.015)
	[3.52]	[3.07]	[2.89]
Difference (balanced - severe)	-0.065	-0.019	-0.046
	(0.014)	(0.003)	(0.014)
	[4.76]	[6.46]	[3.31]
n (student-years)	10,160	500,520	

Notes: Student fixed effects were estimated for math and reading gains, described by Equation 2–2. The table gives the average student reading fixed effects for three types of schools in each sector: schools with racially balanced faculties, schools with moderate nonwhite-imbalanced faculties (where the percent of nonwhite teachers was 20 - 40 percentage points above the county average), and schools with severe nonwhite-imbalanced faculties (where the percent of nonwhite teachers was more than 40 percentage points above the county average). Charter student fixed effects were proxied by the fixed effects estimated for *future* charter students, who were observed in mainstream schools before moving to the charter sector. Standard deviations are in parentheses below each mean, standard errors are in parentheses below each *difference* in means, and t-statistics are in brackets below each difference in means.

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BIOGRAPHICAL SKETCH

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