CALDER

NATIONAL
CENTER for ANALYSIS of LONGITUDINAL DATA in EDUCATION RESEARCH

TRACKING EVERY STUDENT'S LEARNING EVERY YEAR

Urban Institute

A program of research by the Urban Institute with Duke University, Stanford University, University of Florida, University of Missouri-Columbia, University of Texas at Dallas, and University of Washington

The Qualifications and Classroom Performance of Teachers Moving to Charter Schools

CELESTE K. CARRUTHERS

WORKING PAPER 27 • JUNE 2010 (UPDATED VERSION)

The Qualifications and Classroom Performance of Teachers Moving to Charter Schools

Celeste K. Carruthers
Carruthers@utk.edu
University of Tennessee

Contents

Acknowledgements

The author is grateful to the North Carolina Education Research Data Center for data access and technical support, to the National Center for Analysis of Longitudinal Data in Education Research (CALDER), supported through Grant R305A060018 to the Urban Institute from the Institute of Education Sciences, U.S. Department of Education for research support, and to the Lockhart Endowment and Walter-Lanzillotti Award for research and travel support. The author also thanks many individuals for helpful comments and suggestions regarding earlier drafts: David Figlio, Larry Kenny, Sarah Hamersma, Paul Sindelar, Scott Carrell, Dan Goldhaber, Matthew Kim, and participants of the 2008 American Education Finance Association meetings, Association for Public Policy and Management meetings, and Southern Economic Association meetings.

CALDER, The Urban Institute
2100 M Street N.W., Washington, D.C. 20037
202-261-5739 • www.caldercenter.org

The Qualifications and Classroom Performance of Teachers Moving to Charter Schools
Celeste K. Carruthers
CALDER Working Paper No. 27 (updated version)
June 2010

Abstract

Do charter schools draw good teachers from traditional, mainstream public schools? Using an eleven-year panel of North Carolina public school teachers, the author finds nuanced patterns of teacher quality flowing into charter schools. High rates of inexperienced and unlicensed teachers moved to charter schools, but among regularly licensed teachers changing schools, charter movers had higher licensure test scores than other moving teachers, and they were more likely to be highly experienced. I estimate measures of value added for a subset of elementary teachers and show that charter movers were less effective than other mobile teachers and colleagues within their sending schools, by 3 to 4 percent of a student-level standard deviation in achievement.

1. Introduction

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, mainstream public schools) to improve performance. Proponents of charter schools, and of school choice more generally, expect competition between traditional and choice schools to drive up the quality of education overall. Dee (1998), Hoxby (2003), and Booker et al. (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 charter programs have delivered mixed results. Observational studies of administrative data find that enrolling in a new charter school has a negative impact on student achievement growth. This penalty fades (and in some settings, reverses) as schools and students gain experience.[1] Recent studies of lottery-based admissions to urban, oversubscribed charter schools find positive impacts of charter attendance (See, e.g., Abdulkadiroglu et al. (2009), Hoxby & Murarka (2009), and Dobbie & Fryer (2009)).

Teacher quality is a profound factor in student achievement, and charters seeking to produce high achievement (or at least, meet accountability standards) will value effective teachers. The strength with which charters can recruit good teachers from mainstream schools has important policy implications. If highly qualified and effective teachers are 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.

I advance our understanding of charter teachers in two ways. First, I determine if North Car-

[1] See Bifulco & Bulkley (2008) for a review of research on charter school effectiveness.

olina's charter schools were "cream skimming" good teachers in terms of qualifications valued by mainstream systems: education, experience, and licensure. I evaluate teachers who moved to charter schools against other mobile teachers, controlling for sending and receiving school profiles. Charter movers were less experienced than other moving teachers on average, less likely to have a graduate degree, and less likely to be regularly licensed (i.e., certified). Uncertified teachers moving to charter schools, a large minority, substantially attenuated the average qualifications of all charter movers. Although these credentials currently account for nearly all variation in mainstream pay scales, they are weak signals of classroom effectiveness. My second contribution is to estimate the value added of individual teachers moving to charter schools and determine whether charter schools were drawing more or less effective teachers. Charter movers were less effective than average among colleagues in their sending schools, and less effective than other mobile teachers, by 3-4 percent of a student-level standard deviation in math and reading achievement. These results are robust to biases attributable to within-school sorting of students to teachers, and to Bayesian shrinkage estimators accounting for transient noise in value added estimates.

The subset of charter teachers who moved from the mainstream sector provide valuable insight about how well charter schools compete with mainstream schools for teaching talent. Charter teachers with no mainstream experience may be unable or unqualified to work in traditional public schools, whereas charter teachers that moved from traditional public schools are more likely to have exercised revealed preference. Jackson & Cowan (2009) show that charter school entry in North Carolina does not result in higher turnover in mainstream schools, suggesting that charter movers would likely have changed schools or left teaching regardless of charter opportunities. This underscores the importance of evaluating charter teachers against adequate counterfactuals.

Expectations about relative teacher quality in charter schools are ambiguous *a priori*. As workplaces, charters have the appeal of flexibility and autonomy, but the disadvantage of limited resources. Resource constraints stem from charter finance models that 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, its per-pupil expenses will be less than a charter school's average cost. Nevertheless, 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). Elsewhere, charter teachers tend to earn no more than their mainstream counterparts. Podgursky & Ballou (2001) and Hoxby (2002) found competitive charter teacher salaries in multi-state surveys, as well as heightened responsibilities, longer school days, and greater use of merit pay. Hoxby (2002), using a 1998 national survey of teachers, showed that charter teachers tended to log more extracurricular hours, and that charter schools did not pay premia for certification or master's degrees. Taylor (2005) also failed to find a salary boost for advanced degrees in Texas charter schools, and showed that charter teachers earned 7.5 percent less than mainstream teachers with similar experience and education.

Non-pecuniary benefits may offset lower pay in charter schools. Early advocates of the charter model stressed the professionalization and empowerment of teachers as critical tenets of charter development (see, for example, Budde (1988) and Kolderie (1990)), and modern charter schools often follow this course. In surveys, charter teachers cite collegiality, common instructional philosophies, and greater creative license as roots of job satisfaction (Malloy & Wohlstetter, 2003).

The paper proceeds as follows. Section 2 outlines pertinent features of North Carolina's charter system and describes the data. Section 3 analyzes the on-paper qualifications of teachers moving to charter schools. In section 4, I describe measures of classroom performance, evaluate charter movers' value added relative to other mobile teachers, and explore the possibility of bias from sampling error and nonrandom student sorting. Section 5 offers conclusions and open questions.

2. Charter Schools in North Carolina

2.1. Background

The North Carolina legislature authorized the state's system of charter schools in 1996. By 2007, there were nearly 28,000 students enrolled in charter schools, or about two percent of statewide enrollment. Charters are spread throughout urban, rural, and socioeconomically diverse regions of North Carolina. The state's charter legislation and oversight bear many features in common with other charter systems. North Carolina has a binding 100-school cap on the charter sector, and accordingly, a very small percent of teachers moved to charter schools in any 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. Extensive data 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. 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 application, approval, and evaluation of charter schools is closely regulated, but the schools are given wide latitude in their personnel management and daily operations. North Carolina charter schools are organized as private, nonprofit organizations. They are allotted funding from state and local boards of education on a per-pupil rate, commensurate with district per-pupil costs. Public funds cannot be used to service loans on real property or classroom facilities. 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.

Charter schools are allowed great flexibility in the recruitment, retention, and pay of their faculties. North Carolina 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. 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 teachers from outside the traditional pipeline. States vary in their treatment of teacher licensure in charter schools. Of the forty states with active charter systems in 2008, fifteen required 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)).

2.2. Data

I use data covering the universe of North Carolina public schools, students, and teachers. The data are managed by the North Carolina Education Research Data Center at Duke University.[2] I assembled a richly detailed panel describing teachers' credentials, work environments, and career paths over the years 1997 to 2007.[3] Gender, race, and school assignments were available for all teachers – including, importantly, charter teachers. I excluded teaching assistants, facilitators, and teachers simultaneously assigned to more than one school. I linked each teacher's school assignment to campus-wide statistics derived from the NCES Common Core (grades served, urbanicity of locale, school age, and quintile indicators for nonwhite student shares, proficiency rates, and total enrollment). Teacher credentials – experience, education, type of licensure, licensure test scores, and absenteeism – were uniformly available for mainstream teachers only, so I cannot characterize the qualifications of charter teachers *per se*. But the data allow me to identify teachers who moved to charter schools, affording a unique insight to the flow of labor between sectors.

3. Qualifications of Teachers Moving to Charter Schools

Table 1 describes teacher mobility patterns between charter and mainstream schools for the 5,368 teachers who were working in a charter school at some time between 1998 and 2007. The majority taught exclusively in charter schools. The results to follow focus on charter teachers who initially taught in a mainstream school before moving to the charter sector, who accounted for 34.3 percent of all charter teachers. I evaluate these charter movers against other mobile teachers, therein avoiding selection biases from omitted variables driving mobility itself.

Table 2 lists summary statistics for the 1997 to 2006 panel of North Carolina's mainstream public school teachers with known following-year teaching assignments. Graduate degrees were

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

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

common: 30.6 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*.[4] 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 series. In order to include all available test information, I scaled raw licensure test scores to have a standard normal distribution within each test code and test year. I calculated each teacher's mean standardized licensure test score, equal to the average of all her unique exams records. Conditional on licensure itself, test scores were not rewarded in the state's pay scale. Yet unlike graduate degrees, a teacher's test scores are significant (albeit small) indicators of how well his students will do on their own tests (Goldhaber (2007); Clotfelter et al. (2007)). Regularly licensed teachers, who accounted for 91.3 percent of all teachers, had completed an approved teacher education and testing program, or attained licensing by reciprocal or interstate agreement. The complements to regularly licensed teachers were uncertified teachers holding temporary, emergency, or provisional licenses. Unlicensed teachers have been associated with lower student achievement (Clotfelter et al., 2007), although there tends to be much more variation in teacher quality within licensure classes than between (Boyd et al. (2006); Kane et al. (2008)). I derived years of experience from teachers' pay level codes. Although teachers had 12.7 years of experience on average, 20.7 percent had no more than three years' experience. The returns to teacher experience are initially steep, with significant student achievement gains over the first three to five years of a teacher's career. Thereafter, the im-

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

pact of teacher experience plateaus (Clotfelter et al. (2007); Murnane & Phillips (1981); Rockoff (2004)). I merged teachers' resume qualifications with annual records of teacher absences, which have been linked to lower student achievement (Clotfelter et al. (2009); Miller et al. (2007)). North Carolina teachers were absent 11.6 days in a typical school year, excluding vacation days and obvious data errors.[5]

Mobile teachers,[6] summarized in the second column of Table 2, were earlier in their careers, on average, and less likely to have graduate degrees than teachers who were not changing schools. Mobile teachers had lower licensure test scores than non-movers, by 0.011 standard deviations, and were absent an additional 1.4 days in the year prior to moving. Mainstream teachers moving to charter schools, summarized in the third column of Table 2, were typically less qualified than other moving teachers. Strikingly, charter movers were 11.0 percentage points less likely to be regularly licensed. North Carolina's policy of permitting more uncertified teachers in charter schools may have had the consequence of drawing untenured mainstream teachers nearing the expiration of their temporary licenses. I observed 1,449 teachers moving to charter schools - of these, 22.5 were uncertified, nearly twice the rate of uncertified teachers moving to another mainstream school. Ad-

[5]Vacation days, which include all mandatory holidays, were recorded inconsistently in the absenteeism data. I excluded records with negative days absent, more than 25 absences in a month, or more than 150 in a year. See Clotfelter et al. (2009) for further discussion of teacher absenteeism in North Carolina.

[6]Throughout the paper, mobile teachers are those who were next observed in a different school, with no more than a one-year gap between schools. Principal results are robust to more liberal definitions of mobility (allowing for longer gaps between schools) and to more conservative definitions (allowing for no gaps between schools).

ditionally, charter movers were significantly less experienced that mainstream movers, on average. Figure 1 illustrates comparative kernel densities for the teaching experience of mobile teachers. Charter movers were more likely to have just a few years of experience, relative to mainstream movers, but they were also more likely to have around 30 years of experience. The lower panel of Figure 1 is limited to regularly licensed movers. The experience profile of charter movers more closely tracks that of mainstream movers, but again indicates that highly experienced individuals were better-represented among teachers moving to charter schools.

Summary evidence suggests that charter movers were typically less credentialed than other mobile teachers, but this does not resolve the question of whether charter schools were attracting more or less qualified teachers than *similar* mainstream schools. Toward that end, I conduct more parametric analyses of charter and mainstream movers' credentials by estimating Equation 1 via ordinary least squares for each North Carolina teacher j observed in year t (1997-2006), school s, and county l:

$$Q^k_{jslt} = \delta^m_{jt}\mathbf{1}(moving) + \delta^c_{jt}\mathbf{1}(tocharter) + \mathbf{X}^r_{jsl(t+1)}\beta^r + \mathbf{X}^s_{jslt}\beta^s + \alpha_{l(t+1)} + \nu_{jslt} \qquad (1)$$

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. Equation 1 estimates regression-adjusted mean differences in qualification k between mainstream movers and non-movers (δ^m_{jt}), and between mainstream movers and charter movers (δ^c_{jt}). I estimate Equation 1 separately for each of the on-paper characteristics summarized in Table 2: graduate degree, competitive college education, mean licensure test score, regular licensure, three measures of experience, and absenteeism.[7] Controls

[7]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

included sending and receiving school characteristics ($\mathbf{X}^r_{jsl(t+1)}$, $\mathbf{X}^s_{jslt}$), dummy variables for missing school data, and receiving county-by-year effects ($\alpha_{l(t+1)}$). 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), urbanicity indicators, the range of grades served, and a set of dummy variables controlling for missing data. 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. If charter schools had higher demand for some qualifications, and were able to outbid mainstream schools by manipulating employment terms and working conditions, then $\hat{\delta}^c_{jt}$ would be positive. If charters had lower demand, or were unable to realize an advantage in the teachers' labor market, then $\hat{\delta}^c_{jt}$ would be insignificant or negative.

More experienced teachers may seek graduate degrees or additional certifications to increase their pay, so I controlled for teacher experience categories (indicators for less than three years' experience or more than twenty-five years' experience) when estimating Equation 1 for licensure and education variables. Since licensed and unlicensed teachers may have different incentives to

---

school teachers. I emphasize the reduced-form empirical strategy to underscore the descriptive inference gained by examining a relatively small set of idiosyncratic labor decisions. As a robustness check, I estimated a multinomial logit equation predicting the likelihood of different types of school changes, controlling for sending school characteristics. Results suggest that the relative risk of moving to a charter school significantly increased for less experienced teachers, unlicensed teachers, and teachers with higher licensure test scores. These findings are in agreement with the reduced-form results to follow.

consider charter schools, I produced separate estimates of δ_{jt}^{m} and δ_{jt}^{c} for the subsample of licensed teachers.

Table 3 lists estimates of δ_{jt}^{m} and δ_{jt}^{c} for each resume qualification. Columns I and II present results from the full sample. Column I lists coefficient estimates for δ_{jt}^{m}, the difference in qualification k between teachers moving to mainstream schools and non-moving teachers. Estimates of δ_{jt}^{m} serve as the baseline to which δ_{jt}^{c} estimates are compared. Mobile teachers had lower licensure test scores than non-movers, and were 1.3 percentage points less likely to be licensed. Movers were much less experienced, by 3.4 years on average, than their non-moving counterparts. They were 12.2 percentage points more likely to have three years' experience or less, and 6.2 percentage points less likely to have at least twenty-five years' experience. Movers were absent an additional 1.66 days, relative to non-movers.

Column II coefficients in Table 3 answer the question, "were charter movers more or less qualified than teachers moving between comparable mainstream schools?" Controlling for sending and receiving school profiles is important, given the heterogeneity of mainstream sector opportunity costs and charter school working environments. Nonetheless, results are robust to controls for receiving school controls alone, or to sending school controls alone. With respect to graduate education, competitive college provenance, licensure, and experience, charter movers were significantly less qualified. They were 2.9 percentage points less likely to hold a graduate degree, echoing findings by Hoxby (2002) and Taylor (2005). Charter movers were much less likely to be licensed than other mobile teachers, by 9.0 percentage points. Mainstream movers were themselves less likely to be licensed, relative to non-movers, so point estimates from the licensure equation suggest that charter movers were 10.3 percentage points less likely to be licensed than non-moving teachers. Charter movers were less experienced than mainstream movers by 1.40 years, and 9.1

percentage points more likely to have no more than three years' experience. There was no significant absenteeism gap between charter and mainstream movers.

Fully licensed teachers may have had more options in the mainstream sector than unlicensed, untenured teachers. Columns III and IV list results from the subsample of licensed teachers, who accounted for 88.5 and 77.5 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 narrowed or reversed the qualification gap between charter and mainstream movers, suggesting that uncertified mainstream teachers moving to charter schools attenuated the average qualifications of charter movers. The difference between the full and limited sample was particularly stark for licensure test scores and high levels of experience. Licensed teachers moving to charter schools had significantly higher licensure test scores than other moving teachers, by 6.6 percent of a standard deviation, and they were 3.1 percentage points more likely to have 25 years' experience or more.

These findings raise the possibility that teachers viewed the charter sector as a low-cost job change preceding retirement or permanent career changes. Sample attrition was high among new teachers, highly experienced teachers, and uncertified teachers, and these are the same groups I observed disproportionately flowing to the charter sector. Nonetheless, the charter sector does not appear to have been a strong substitute for or precursor to attrition for mainstream teachers. Charter movers had a typical post-move duration (uncensored) that was just 8.9% shorter than that of teachers moving to mainstream schools (2.88 years, versus 3.16).

4. Classroom Performance of Teachers Moving to Charter Schools

Teachers' on-paper qualifications are readily observable to schools and econometricians, but the actual achievement of their students is of greater value when assessing classroom performance.

North Carolina students in the 3rd through 8th grades take end-of-grade (EOG) exams in math and reading 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 links these identifers to student test data. I utilized this valuable feature of the data to assess the classroom performance of mainstream elementary teachers who ultimately moved to the charter sector.

I assembled grade 3 to 5 student EOG records for almost three million student-years spanning 1996 to 2007, omitting grade repeaters and test exemptions. So that teachers may be compared across time and grade levels, I scaled raw EOG scores to have mean zero and standard deviation equal to one within each year and grade.[8] The proctor associated with each student's test score was not necessarily his or her classroom teacher. To minimize the likelihood of invalid teacher-student matches, I omitted makeup tests, alternative tests, tests for severely disabled students, classrooms with less than five or more than 30 test-takers, and tests that accommodated students' need for multiple sessions, dictation, home testing, or separation from the rest of the class. Additionally, I focused on self-contained classrooms whose proctor was found in the assembled panel of teachers. 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. These limitations lend considerable validity to each allowed teacher-student match. Of the 122,255 EOG test-taking classrooms with a known teacher, 87.2 percent were considered valid matches. I cross-referenced EOG records

[8]EOG exams are interval-scaled across grades, but the range of raw scores has shifted and narrowed over time, and tends to compress in higher grades.

with course membership files for 2007, the only year for which students can be linked with certainty to their teachers. Among all student-teacher pairs in the limited sample, 92.1 percent were matched to verified course membership records, whereas a naive sample of all test records and all proctors yielded just a 73.3 percent match rate.

4.1 Classroom Performance - Main Results

Consider the following model describing student i's standardized test score Z^k_{ijt} in subject k (math or reading) in teacher j's classroom, school s, year t:

$$Z^k_{ijt} = \lambda Z^k_{it-1} + \mathbf{A}_{ijt}\beta_A + \bar{\mathbf{A}}_{-ijt}\beta_{\bar{A}} + \mathbf{T}_{jt}\beta_T + \mathbf{X}_{st}\beta_X + \theta_j + \alpha_s + \epsilon_{ijt} \tag{2}$$

Equation 2 is an educational production function that controls for once-lagged student achievement (Z^k_{it-1}) in place of prior inputs and endowed ability. The model assumes that effects of prior inputs and endowments decay uniformly and geometrically (Tood & Wolpin, 2003). These are strong assumptions, but due to the students' short time series (3 years at most), Equation 2 is the best available value added specification for the purposes of this study. Variables in $\mathbf{A}_{ijt}$ are student characteristics, including race, gender, parental education, and learning disability indicators. $\bar{\mathbf{A}}_{-ijt}$ is a vector controlling for class size and average student characteristics in i's classroom, excluding student i. The vector $\mathbf{T}_{jt}$ controls for two measures of teacher inexperience: an indicator equal to one if j is a new teacher, and another indicator equal to one if j has three or fewer years' experience. $\mathbf{X}_{st}$ contains school-level variables, including urbanicity indicators and quintile indicators for total enrollment, percent proficient, and percent nonwhite. The coefficients θ_j and α_s are teacher fixed effects and school fixed effects, respectively. The results to follow evaluate θ_j estimates under four variations of Equation 2, itemized below.

A. Without lagged achievement, without school fixed effects: assumes $\lambda = 0$ and $\alpha_s = 0$.

B. With lagged achievement, without school fixed effects: assumes $\alpha_s = 0$.

C. Without lagged achievement, with school fixed effects: assumes $\lambda = 0$.

D. With lagged achievement and school fixed effects.

Teacher fixed effects from specifications A and B do not account for unobserved, time-invariant school quality, so the tendency of higher-ability students to gravitate towards particular schools will bias teacher quality estimates. Teacher fixed effects from specifications C and D, controlling for school 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 sorting. Specifications B and D control for students' lagged-year place in their grade-cohort distribution, so teacher fixed effect estimates represent the degree to which teachers are responsible for advancing their students through the distribution, conditioning on baseline achievement. Specifications B and D necessarily limit the analysis to fourth and fifth-grade teachers, but more directly control for heterogeneous ability among students.[9]

Coefficient estimates for each specification of Equation 2 were unsurprising. Female students had lower math scores than males, but higher reading scores. 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 his first year as opposed to his second or third.

[9]Between any two measures of teacher effectiveness, pairwise correlation coefficients were positive and statistically significant.

Fixed effects were estimated for 15,887 mobile teachers, 340 of whom ultimately moved to a charter school. Table 4 summarizes the time-invariant teacher fixed effects generated by each specification of Equation 2. Moving teachers had somewhat lower value added than non-moving teachers, in agreement with recent work by Hanushek & Rivkin (2010). Relative to mainstream movers, charter movers had even lower value added, by 0.031 to 0.081 standard deviations. The gap was smaller for reading fixed effects, and for fixed effects generated by specifications controlling for students' baseline achievement.

Simple mean differences do not control for the type of schools teachers were leaving or moving to, and charter schools may have attracted relatively high-performing teachers, compared to mainstream schools with similar student populations. In parallel to the analysis of resume qualifications, I regressed teacher fixed effect estimates against mobility indicators, sending and receiving school characteristics, and receiving county-by-year effects:

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

Subjects (math and reading) are indexed by k, teachers by j, schools by s, counties by l, and years by t. Table 5 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 3), and Column II lists conditional mean differences in fixed effects between charter and mainstream movers ($\hat{\delta}_{jt}^c$).[10]

Echoing the unconditional mean differences in fixed effects, charter movers were less effective than teachers moving between mainstream schools, and mainstream movers were themselves less

[10]Due to the small number of charter movers with fixed effect estimates, and the high rate of licensure among elementary charter movers (and elementary teachers in general), I do not produce separate estimates for the subsample of regularly licensed teachers.

effective than non-mobile teachers. The exception is for math effectiveness under specification D, controlling for lagged achievement and school fixed effects. There, charter movers were not significantly different from other moving teachers. As I show in the following section, charter movers' relative fixed effects from specification A, controlling for neither lagged achievement or school fixed effects, may have been biased by systematic student sorting, so specifications including either lagged achievement or school fixed effects are preferred. Regression-adjusted mean differences are similar across preferred specifications, indicating that charter movers were less effective than teachers moving between comparable school environments, by 0.031 to 0.041 student-level standard deviations.[11]

A 0.031 to 0.041 gap between these two groups of mobile teachers has statistical and practical significance. Coefficient estimates from Equation 2 suggest that first-year teachers reduced student achievement by about 0.080 standard deviations in math and 0.050 standard deviations in reading, relative to teachers with more than three years' experience. These are very similar to returns to teacher experience estimated by Clotfelter et al. (2007). Thus, the difference between a teacher moving to the charter sector and a teacher moving elsewhere was approximately 40 to 80 percent of the gap between new and more experienced teachers. Knowing that many charter movers were themselves new or inexperienced teachers, we can conclude that North Carolina's charter schools

[11] A previous version of this paper reported that charter movers' fixed effects from a specification like C, but with student test scores normalized by school in lieu of school fixed effects, were comparably lower than the fixed effects of sending-school colleagues, yet higher than the fixed effects of other moving teachers. The latter finding was not robust, however, to the conditioning of school fixed effects on other covariates in Equation 2.

were not skimming more effective teachers from traditional, mainstream public schools.[12]

4.2 Robustness Check - Biases from Student Sorting

In this setting, teacher fixed effects are interpreted as each individual's history of classroom performance relative to expectations. 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) $\hat{\theta}_j$ are consistent estimates of θ_j, and (2) errors, ϵ_{ijt}, are uncorrelated with θ_j. The first assumption is invalid for fixed effect estimates generally, which are inconsistent in short panels (Cameron & Trivedi (2005, Ch. 21)). 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 (Kane et al. (2008); Rockoff (2004)). I return to this issue in the following section. The second assumption is invalid if there are unmeasured and dynamic student characteristics affecting test scores, like motivation or inherent intelligence, and if these variables systematically affect the teacher to whom a student is assigned, or the school a student chooses

[12]If mobile teachers tend to be more effective in their new schools (Jackson (2010)), the time-invariant fixed effects of charter movers could be biased downward, since their performance while in charter schools cannot be observed. To simulate these data limitations for all moving teachers, I excluded any mainstream mover data following a school change and replicated the analyses. Results were less precise but nonetheless suggested a 0.020 to 0.030 standard deviation gap in fixed effects between mainstream movers and non-movers (as in Table 5), and at least a 0.030 standard deviation gap in teacher fixed effects between charter and mainstream movers.

to attend. In that case, estimates of θ_j will be a reflection of teacher quality *and* student sorting. Positive matching, such that better students are observed with 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 are assigned to struggling teachers to ease their burden, would bias $\hat{\theta}_j$ toward zero. Clotfelter et al. (2006) found evidence of teacher-student matching, particularly positive matching, in North Carolina schools. The bulk of nonrandom student sorting occurred between schools, which I account for in two specifications of Equation 2 with school fixed effects.

School fixed effects will not control for nonrandom sorting of students within schools, however, such as would be the case if parents were successfully lobbying school administrators to put their children in particular classrooms. The existence of "teacher shopping" by parents has considerable anecdotal support,[13] and there are abundant practical reasons why school leaders might not want to randomly assign students to classrooms. If future charter teachers were systematically assigned lower-performing classes, more so than future mainstream movers, teacher fixed effects would be biased against charter movers. Rothstein (2010), using a subset of the North Carolina data employed here, shows that common value added methodologies falsely ascribe significant value to a students' *future* teachers. Much attention has been devoted to resolving this issue, and I apply some of the lessons learned to the evaluation of charter movers. Koedel & Betts (2009), for instance, show that teacher effect estimates derived from multiple years of data are less subject to sorting biases than single-year classroom effects. Kane & Staiger (2008) exploit an experiment where students were randomly assigned to teachers to test for the magnitude of pre-experiment bias under different achievement function specifications. One conclusion was that controls for a

[13] See, e.g., Hui (2003) and Crombie (2001).

student's lagged achievement and observable characteristics of peers within his classroom were sufficient to drive the bias from nonrandom sorting to zero. Here, Equation 2 was estimated for a multi-cohort panel, generating teacher fixed effect estimates covering at most eleven school years. Peer characteristics – at the classroom and school level – were included in all specifications, and students' prior achievement was included in preferred specifications.

In spite of these controls, student sorting will continue to affect North Carolina teacher quality estimates if classroom assignments are determined by unobserved student characteristics that fail to be orthogonal to Equation 2 controls. Sorting biases would only have affected the analysis to the degree that charter movers were *more* affected by non-random, within-school student assignment than other mobile teachers. I investigate this possibility directly, by substituting following-year teacher indicators for current teacher indicators in the four specifications of Equation 2 listed above, and then comparing the false "effects" of movers and non-movers, and of charter and mainstream movers. Mainstream movers had significantly lower false effects than non-movers, but by just 0.005 to 0.009 standard deviations. Charter movers' false effects were significantly lower than those of mainstream movers in specification A of the math educational production function. This suggests that contemporaneous teacher fixed effects from specification A – with the most restrictive assumptions about learning decay and unobserved school quality – may be biased in such a way that affects comparisons of charter and mainstream movers. Controls for school fixed effects and/or lagged achievement were not sufficient to drive the joint significance of false effects to zero, but they were sufficient to eliminate any significant, spurious difference between mainstream movers and charter movers.

4.3 Robustness check - Biases from Sampling Error

Sampling error from finite panel length and class size cause the variance of teacher fixed effects to overstate the variance of true value added. Furthermore, if sampling error disproportionately affects certain groups of teachers who are more likely to transition to charter schools (new teachers, for instance), then comparing fixed effects may put charter movers at a disadvantage. Following several recent studies, I partitioned the variance in persistent teacher quality from that of sampling error in students' residual achievement and constructed estimates of teacher effectiveness that account for likely sampling error.[14]

Consider Equation 2, omitting teacher fixed effects (θ_j).

$$Z_{ijt}^k = \lambda Z_{it-1}^k + \mathbf{A}_{ijt}\beta_A + \bar{\mathbf{A}}_{-ijt}\beta_{\bar{A}} + \mathbf{T}_{jt}\beta_T + \alpha_s + e_{ijt} \tag{4}$$

Residuals are $e_{ijt} = \theta_j + \eta_{jt} + \varepsilon_{ijt}$, where θ_j is the persistent effectiveness with which teacher j can boost students' place in their cohort distribution, η_{jt} represents non-persistent classroom shocks, and ε_{ijt} represents non-persistent student shocks. I estimated Equation 4 under the same four alternative assumptions about decay (λ) and school fixed effects (α_s) outlined in section 4.1, and then decomposed residual variance into the variance of each component: $\hat{\sigma}_\theta^2$, $\hat{\sigma}_\eta^2$, and $\hat{\sigma}_\varepsilon^2$. Of particular interest is $\hat{\sigma}_\theta$, the standard deviation of persistent teacher quality. Table 7 lists total and teacher-induced residual variance under each specification of the educational production function. A one-standard-deviation increase in persistent teacher quality was expected to increase student achievement by 0.139 to 0.202 student-level standard deviations for math, and 0.074 to 0.145 standard deviations for reading. For context, note that the unconditional black-white achievement gap in this sample is about 0.723 standard deviations for math. Thus, a one-standard-deviation increase in within-school teacher quality, controlling for lagged achievement and school fixed effects under

[14]See, e.g., Carrell & West (2008); Hanushek & Rivkin (2010); Kane et al. (2008); and Kane & Staiger (2008).

specification D, was predicted to increase math achievement by 0.139 standard deviations, or 19.2 percent of the black-white achievement gap.

Residual variance components, along with the number of students taught by each teacher, were used to approximate persistent value added. Following Kane & Staiger (2008), I constructed teacher j's value added by scaling her classes' weighted mean residual towards zero according to an estimate of her signal-to-total variance ratio. This Bayesian shrinkage estimator disproportionately attenuated the value added of less experienced teachers, who are expected to have been more affected by sampling error. Computational details are provided in the Appendix.

Figure 2 illustrates the effect of this scaling and affords a visual comparison of future charter teachers' value added versus the value added of their exclusively mainstream counterparts. Panel I plots comparative kernel densities of teachers' math fixed effects ($\hat{\theta}_j$), controlling for students' lagged achievement but not school fixed effects (specification B). Panel II plots densities of teachers' mathematics value added estimates from Bayesian shrinkage estimators ($\hat{\theta}_j^{Bayes}$), again controlling for lagged achievement but not school fixed effects.[15] Two salient conclusions emerge. First, Bayesian scaling significantly shrinks the perceived distribution of teacher effectiveness. This is consistent with evidence from Table 7 suggesting that persistent teacher quality accounts for just 30.5 percent of math achievement unexplained by background, peers, and baseline achievement. And second, the distribution of future charter teachers' value added is significantly left of the distribution of other mainstream teachers' value added, regardless of Bayesian scaling. Wilcoxon rank-sum tests reject the hypothesis that future charter teachers and exclusively mainstream teach-

[15]Figures for reading value added, and for specifications with school fixed effects, were qualitatively equivalent to Figure 2.

ers were drawn from the same distribution of value added. Figure 2 provides further evidence that teachers flowing to the charter sector typically had low classroom performance relative to other mainstream teachers, but also demonstrates considerable overlap and variation in teacher quality across groups.

To gage the difference in charter and mainstream movers' persistent value added, I estimated Equation 3, substituting $\hat{\theta}_j^{Bayes}$ for teacher fixed effect estimates. As in the main analysis of teacher fixed effects, controlling for sending and receiving school profiles suggests that charter schools were drawing teachers with lower value added, relative to other mobile teachers. Coefficient estimates for the regression-adjusted difference in mainstream movers' persistent value added, relative to that of non-movers ($\hat{\delta}_{jt}^m$), and for the difference between mainstream and charter movers ($\hat{\delta}_{jt}^c$) are listed in Table 8. Mainstream movers possessed significantly lower value added than non-moving teachers, reaffirming the conclusion that schools typically lose their less effective teachers. Relative to mainstream movers, charter movers' persistent value added was even lower, by 0.026 to 0.064 (scaled) standard deviations.

5. Conclusions, Implications, and Caveats

A founding purpose of North Carolina's charter legislation was to "create new professional opportunities for teachers."[16] Implicit in these opportunities is a new dimension of upstream competition in public education. Autonomous charter schools are well-positioned to exploit any inefficiencies in monopsonistic markets for public teachers. Mainstream teachers in North Carolina, as in other systems, are paid according to rigid salary schedules that climb steadily with experience and graduate degrees, despite compelling evidence that the returns to experience are largely

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

exhausted after the first few years of a teacher's career, and that the returns to graduate degrees are insignificant. Charter administrators are free to hire, compensate, and fire according to merit and robust signals of teacher quality. Charters also have the flexibility to structure work environments that are more appealing to teachers, whereas many elements of mainstream employment (especially curricula) are centrally managed. But do charter schools have the resources to exploit these inefficiencies and skim good teachers from the mainstream? Tighter budgets and institutional inexperience may limit the appeal of working in a charter school.

In terms of resume line-items, I find mixed evidence that North Carolina charters were hiring less qualified teachers away from mainstream schools. Teachers moving to charter schools were less experienced on average, less likely to be regularly licensed, and less likely to have graduate degrees. But charter schools looking to boost student achievement and minimize payroll may have placed little value on credentials like these, with stronger connections to mainstream pay scales than student achievement. High licensure test scores and low rates of absenteeism are robust but uncompensated signals of mainstream student achievement, and strategic charter schools may have targeted teachers with credentials like these. Charter movers had the same absenteeism rate as other moving teachers, and among regularly licensed teachers, the licensure test scores of charter movers were 0.066 standard deviations higher than those of mainstream movers. In terms of student achievement, however, the licensure test score gap between mainstream and charter movers is small. Clotfelter et al. (2007) showed that a one-standard deviation increase in mean licensure test scores resulted in a 0.011 standard deviation increase in students' math achievement levels and a 0.003 standard deviation increase in reading levels. A 0.066 standard deviation test score advantage over mainstream movers, then, translates roughly to a 0.001 standard deviation bonus to math achievement and an infinitesimal bonus to reading achievement.

Relaxed licensure standards, intended to reduce barriers to teaching, are one piece of the "charter experiment" in North Carolina and elsewhere. But this policy had the consequence of drawing a high rate of uncertified mainstream teachers into the charter sector, some of whom may have been nearing the end of their probationary periods or lacking mainstream teaching opportunities. Alternatively or temporarily licensed teachers are not necessarily bad teachers (see, e.g., Boyd et al. (2006) and Kane et al. (2008)), although they have been associated with lower student achievement (Clotfelter et al., 2007). A migration of uncertified teachers into charter schools would not be problematic if charter schools were adept at judging teacher quality, regardless of licensure status. The effectiveness of uncertified teachers in charter schools remains an important and unanswered question.

The classroom performance of fully licensed teachers moving to charter schools may be indicative of charters' success in identifying and recruiting effective teachers. I estimated teacher fixed effects for a subset of grade 3 to 5 teachers, and found that charter schools were drawing below-average teachers from school faculties. The discrepancy was not limited to mean differences; the quality distribution for future charter teachers was significantly left of the quality distribution for teachers who never taught in a charter school, and charter movers were less effective than mainstream movers by a statistically and economically meaningful 3.1 to 4.1 percent of a student-level standard deviation in achievement.

Taken together, these findings demonstrate nuanced patterns of teacher quality flowing to charter schools. North Carolina's charter schools were drawing well-credentialed, licensed teachers. But whether because of resource constraints, limited scale, or unobserved costs of working in the charter sector, charter schools were not drawing unambiguously superior teachers from the mainstream sector as a whole, nor were they skimming above-average teachers from mainstream faculties.

Three important caveats and open questions must be emphasized alongside these results. First, charter teachers' value added *while in charter schools* remains an important but unexplored topic, largely because of data limitations. Second, although North Carolina's charter infrastructure resembles that of many other systems in terms of finance and regulation, results may not generalize to other settings. The relative quality of charter movers under different charter regimes, and particularly among successful urban charter programs, is another avenue for fruitful research. And finally, rather than being less effective classroom leaders, it may be the case that charter movers were less devoted to standardized testing, and tended to direct their energies toward discipline, character-building, or other non-tested virtues. This possibility is difficult to explore with administrative data, but presents an opportunity for observational or qualitative research. Nonetheless, North Carolina's end-of-grade exams reflect a set of appropriate cognitive standards, and the relative ability of future charter teachers to boost student achievement has important implications for education stakeholders. Regarding this dimension of teacher quality, North Carolina charter teachers compared unfavorably to other teachers while they were teaching in mainstream schools.

Appendix: Construction of Bayesian Shrinkage Estimators

Residuals from Equation 4 are $e_{ijt} = \theta_j + \eta_{jt} + \varepsilon_{ijt}$, where θ_j is teacher j's persistent contribution, and η_{jt} and ε_{ijt} are non-persistent classroom-level and student-level shocks, respectively. An example of η_{jt} would be the shared effect of a dog barking outside the classroom on test day, and ε_{ijt} could be driven by student i having a uniquely bad morning prior to taking his end-of-grade exam. The average student residual for each class can be expressed like so:

$$\hat{\bar{e}}_{jt} = \theta_j + \eta_{jt} + \frac{1}{N_{jt}} \sum_{i=1}^{N_{jt}} \varepsilon_{ijt},$$

where N_{jt} is class size in year t. If θ_j, η_{jt}, and $\bar{\varepsilon}_{jt}$ are independent, the variance of $\hat{\bar{e}}_{jt}$ across teachers can be decomposed into the the variance of persistent value added and the variance of non-persistent error: $\mathbb{E}[\hat{\bar{e}}^2_{jct}] = \sigma^2_\theta + \sigma^2_\eta + \sigma^2_\varepsilon$, where σ^2_θ is the variance of persistent teacher quality within schools, σ^2_η is the variance of classroom-by-year residuals not attributable to teachers, and σ^2_ε is the variance of student-by-year residuals not attributed to classroom effects or teacher effects. Consider two average residuals from two different classes taught by the same teacher: $\hat{\bar{e}}_{jt}$ and $\hat{\bar{e}}_{jt'}$, where $t \neq t'$. If the three residual components are uncorrelated contemporaneously, and if non-persistent shocks are uncorrelated intertemporally, then

$$\mathbb{E}[\hat{\bar{e}}_{jt}\hat{\bar{e}}_{jt'}] = \sigma^2_\theta.$$

The assumption that θ_j, η_{jt}, and ε_{jt} 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 unbiased measures of teacher quality. Positive matching of better students with better teachers, for instance, will increase estimates of σ^2_θ. Additionally, omitting teacher fixed effects in Equation 4 may bias other coefficients if they are correlated with θ_j; this, in turn, will bias estimated residuals, $\hat{\bar{e}}_{ijct}$. Controlling for school fixed effects in Equation 4 limits biases from between-school sorting, but within-school assignment patterns may nonetheless affect σ_θ^2 estimates. Following Carrell & West (2008), I estimate σ_θ^2 by computing the pairwise covariance of classroom-averaged residuals between teacher j's class in year t and all j classes in year $t' \neq t$:

$$\hat{\sigma}_\theta^2 = \left[\sum_{j=1}^{J}\sum_{t=1}^{T_j} \hat{\bar{e}}_{jt}\hat{\bar{e}}_{jt'}\right]/N$$

where J is the number of teachers in the sample, T_j is the number of classes taught by teacher j, and N is the number of same-teacher pairs.

The remaining steps follow Kane & Staiger (2008). The variance of student-by-year residuals was approximated by $\hat{\sigma}_\varepsilon^2 = var(e_{ijt} - \bar{e}_{jt})$, the variance of deviations from class means. The variance of class-by-year residuals was taken to be the gap between the total variance of errors and the sum of teacher-induced and student-by-year residual variance: $\hat{\sigma}_\eta^2 = var(e_{ijt}) - (\hat{\sigma}_\theta^2 + \hat{\sigma}_\varepsilon^2)$. For each teacher j, I computed $\tilde{e}_j = \sum_t w_{jt}\bar{e}_{jt}$, a weighted average of her classroom-averaged residuals. Weights are as follows:

$$w_{jt} = \left(\frac{1}{\hat{\sigma}_\eta^2 + \frac{\hat{\sigma}_\varepsilon^2}{N_{jt}}}\right) * \left[\sum_{s=1}^{T_j} \frac{1}{\hat{\sigma}_\eta^2 + \frac{\hat{\sigma}_\varepsilon^2}{N_{js}}}\right]^{-1}$$

Note that weights favor classes with more students. As class size grows, sampling error is expected to diminish. Class size *per se* was included as a control variable in all educational production function regressions, so losses from attending larger classes (estimated to be 0.002 to 0.005 standard deviations) will not be reflected in teachers' value added estimates.

The empirical Bayes estimator of each teacher's value added was computed by scaling $\tilde{e}_j$ toward zero by the approximated signal-to-total variance ratio in residual classroom performance:

$$\hat{\theta}_j^{Bayes} = \tilde{e}_j * \left(\frac{\hat{\sigma}_\theta^2}{var(\tilde{e}_j)}\right)$$

$$var(\tilde{e}_j) = \hat{\sigma}_\theta^2 + \left(\sum_{s=1}^{T_j} \frac{1}{\hat{\sigma}_\eta^2 + \frac{\hat{\sigma}_\varepsilon^2}{n_{js}}}\right)^{-1}$$

Note that the only components of $\hat{\theta}_j^{Bayes}$ that are unique to teacher j are T_j, the number of classes she taught in the panel, and N_{jt}, the number of students in a particular class. The scaling factor multiplying $\tilde{e}_j$ is increasing in N_{jt} and T_j, so $\tilde{e}_j$ will be scaled by less for teachers with more students or more experience.

*References

Abdulkadiroglu, A., Angrist, J., Dynarski, S., Kane, T., & Pathak, P. (2009). Accountability and flexibility in public schools: Evidence from boston's charters and pilots. Working Paper No. 15549, National Bureau of Economic Research, Cambride, MA.

Bifulco, R., & Bulkley, K. (2008). Charter schools. In H. F. Ladd, & E. Fiske (Eds.) *Handbook of Research in Education Finance and Policy*, (pp. 425–446). New York, NY: Routledge.

Booker, K., Gilpatric, S. M., Gronberg, T. J., & Jansen, D. W. (2008). The effects of charter schools on traditional public school students in texas: Are students who stay behind left behind? *Journal of Urban Economics*, *64*(1), 123–145.

Boyd, D., Grossman, P., Lankford, H., Loeb, S., & Wyckoff, J. (2006). How changes in entry requirements alter the teacher workforce and affect student achievement. *Education Finance and Policy*, *1*(2), 176–216.

Budde, R. (1988). *Education by Charter: Restructuring School Districts*. Andover, MA: Regional Laboratory for Educational Improvement of the Northeast & Islands.

Cameron, A. C., & Trivedi, P. K. (2005). *Microeconometrics: Methods and Applications*. New York, NY: Cambridge University.

Carrell, S. E., & West, J. E. (2008). Does professor quality matter? evidence from random assignment. Working Paper No. 14081, National Bureau of Economic Research, Cambride, MA.

Center For Education Reform (2008). *Charter School Laws Across the States*. Washington, DC: Center For Education Reform, 10 ed.

Clotfelter, C. T., Ladd, H. F., & Vigdor, J. L. (2006). Teacher-student matching and the assessment of teacher effectiveness. *Journal of Human Resources*, *41*(4), 778–820.

Clotfelter, C. T., Ladd, H. F., & Vigdor, J. L. (2007). Teacher credentials and student achievement: Longitudinal analysis with student fixed effetcs. *Economics of Education Review*, *26*(6), 673–682.

Clotfelter, C. T., Ladd, H. F., & Vigdor, J. L. (2009). Are teacher absences worth worrying about in the united states? *Education Finance and Policy*, *4*(2), 115–149.

Crombie, N. (2001). Parents go teacher-shopping. *The Oregonian*. 27 May, Sec. D, p D01.

Dee, T. S. (1998). Competition and the quality of public schools. *Economics of Education Review*, *17*(4), 419–427.

Dobbie, W., & Fryer, R. (2009). Are high quality schools enough to close the achivement gap? evidence from a social experiment in harlem. Working Paper No. 15473, National Bureau of Economic Research, Cambride, MA.

Goldhaber, D. (2007). Everybody's doing it, but what does teacher testing tell us about teacher effectiveness? *Journal of Human Resources*, *42*(4), 765–794.

Gootman, E. (2008). At charter school, higher teacher pay. *New York Times*. 7 Mar., Sec. A, p A01.

Hanushek, E. A., & Rivkin, S. G. (2010). Constrained job matching: Does teacher job search harm disadvantaged urban schools? Working Paper No. 15816, National Bureau of Economic Research, Cambride, MA.

Hoxby, C. M. (2002). Would school choice change the teaching profession? *Journal of Human Resources*, *37*(4), 846–891.

Hoxby, C. M. (2003). School choice and school productivity: Could school choice be a tide that lifts all boats? In *The Economics of School Choice*, (pp. 287–341). University of Chicago.

Hoxby, C. M., & Murarka, S. (2009). Charter schools in new york city: Who enrolls and how they affect their students' achievement. Working Paper No. 14852, National Bureau of Economic Research, Cambride, MA.

Hui, T. K. (2003). It's teacher shopping season. *The News & Observer*. 15 Jul., Sec. B, p B01.

Jackson, C. K. (2010). Match quality, worker producitivty, and worker mobility: Direct evidence from teachers. Working Paper No. 15990, National Bureau of Economic Research, Cambride, MA.

Jackson, C. K., & Cowan, J. (2009). School competition and teacher quality: Evidence from charter entry in north carolina. Working Paper, Cornell University.

Kane, T. J., Rockoff, J. E., & Staiger, D. O. (2008). What does certification tell us about teacher effectiveness? evidence from new york city. *Economics of Education Review*, *27*(6), 615–631.

Kane, T. J., & Staiger, D. O. (2008). Estimating teacher impacts on student achievement; an experimental evaluation. Working Paper No. 14607, National Bureau of Economic Research, Cambride, MA.

Koedel, C., & Betts, J. (2009). Does student sorting invalidate value-added models of teacher effectiveness? an extended analysis of the rothstein critique. Working Paper 0902, University of Missouri-Columbia.

Kolderie, T. (1990). *Beyond Choice to New Public Schools: Withdrawing the Exclusive Franchise in Public Education*. Washington, DC: Progressive Policy Institute.

Malloy, C. L., & Wohlstetter, P. (2003). Working conditions in charter schools: What's the appeal for teachers? *Education and Urban Society*, *35*(2), 219–241.

Miller, R. T., Murnane, R. J., & Willet, J. B. (2007). Do teacher absences impact student achievement? longitudinal evidence from one urban school district. Working Paper No. 13356, National Bureau of Economic Research, Cambride, MA.

Murnane, R., & Phillips, B. (1981). Learning by doing, vintage, and selection: Three pieces of the puzzle relating to teaching experience and teaching performance. *Economics of Education Review*, *1*(4), 453–465.

Muschkin, C., Bonneau, K., & Dodge, K. (2008). North carolina education research data center (grant #200300138) annual report to the spencer foundation. Last retrieved June 21, 2010, from http://www.childandfamilypolicy.duke.edu/pdfs/projects/NCERDC_SpencerReport2009_final.pdf.

Podgursky, M., & Ballou, D. (2001). *Personnel Policy and Charter Schools*. Washington, DC: Thomas B. Fordham Foundation.

Rockoff, J. E. (2004). The impact of individual teachers on student achievement: Evidence from panel data. *American Economic Review*, *94*(2), 247–252.

Rothstein, J. (2010). Teacher quality in educational production: Tracking, decay, and student achievement. *Quarterly Journal of Economics*, *125*.

Taylor, L. L. (2005). Teacher cream skimming: Do charter schools attract the most effective teachers? Cited with permission. Last retrieved Jul. 9, 2009, from http://econweb.tamu.edu/workshops/PERC%20Applied%20Microeconomics/Lori%20L.%20Taylor.pdf.

Tood, P. E., & Wolpin, K. I. (2003). On the specification and estimation of the production function for cognitive achievement. *The Economic Journal*, *113*(485), F3–33.

Table 1: In-sample mobility patterns of charter teachers

Teacher Mobility pattern	Percent
Started and ended in the charter system (right censored)	21.3
Started and ended in the charter system (uncensored)	32.9
Mainstream to charter	25.5
Mainstream to charter to mainstream	8.8
Charter to mainstream	10.9
Other patterns	<1.0

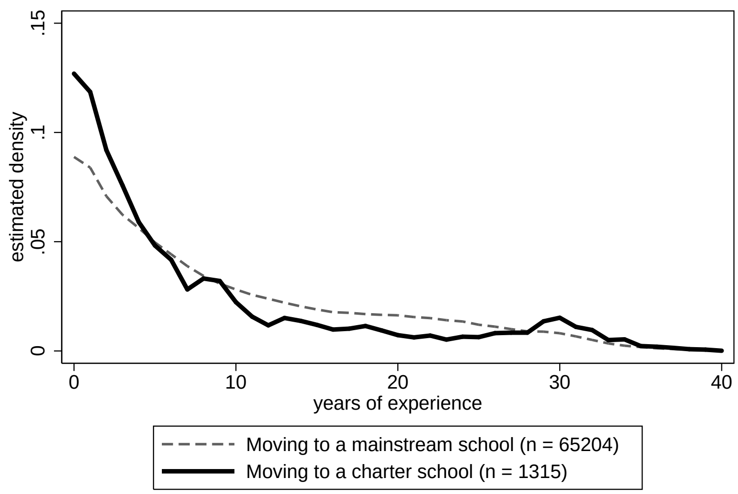
Notes: $n = 5{,}368$ teachers. The first two mobility patterns apply to teachers who taught exclusively in charter schools. Right censored charter teachers entered the sample in the charter system and were still teaching there in 2007, the last year of the panel. Uncensored teaching spells ended before 2007. The last 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 2: North Carolina public school teachers: Summary statistics

Teacher qualification	I All teachers	II Mainstream movers	III Charter movers
Holds graduate degree (%)	30.6	28.1***	24.4***
	(46.1)	(44.9)	(43.0)
Attended competitive college (%)	76.3	74.9***	68.7***
	(42.5)	(43.4)	(46.4)
Mean licensure test score ($\sim N(0,1)$)	0.022	0.011***	0.005
	(0.859)	(0.837)	(0.926)
Regularly licensed (%)	91.3	88.5***	77.5***
	(28.2)	(31.9)	(41.8)
Teaching experience (years)	12.7	9.4***	8.0***
	(9.6)	(8.9)	(9.4)
Experience $\leq$ 3 years (%)	20.7	32.5***	42.3***
	(40.5)	(46.8)	(49.4)
Experience $\geq$ 25 years (%)	14.2	8.2***	9.6
	(34.9)	(27.5)	(29.5)
Days absent	11.6	13.0***	13.2
	(9.9)	(11.2)	(11.7)
Black (%)	14.3	15.7***	25.0***
	(35.0)	(36.4)	(43.3)
Hispanic (%)	0.8	1.3***	1.4
	(8.9)	(11.1)	(11.7)
Other, non-white (%)	1.2	1.2	2.0***
	(10.7)	(10.7)	(14.0)
White (%)	83.7	81.9***	71.6
	(36.9)	(38.5)	(45.1)
Female (%)	80.1	79.2***	78.7
	(39.9)	(40.6)	(41.0)
n (teacher-years)	704,134	68,812	1,449

Notes: The table lists summary statistics for all 1997-2006 North Carolina mainstream school teachers with known school assignments in the following year. Standard deviations are in parentheses below each mean. Data for moving teachers reference the year immediately preceding a school change. Mainstream movers (Column II) are mainstream teachers who were next observed in a different mainstream school, and asterisks in Column II indicate a significant difference between mainstream movers and non-mobile teachers. Charter movers (Column III) were next observed in a charter school, and asterisks in Column III indicate significant differences between mainstream and charter movers. *** indicates 1% significance, ** indicates 5%, and * indicates 10%.

I



II

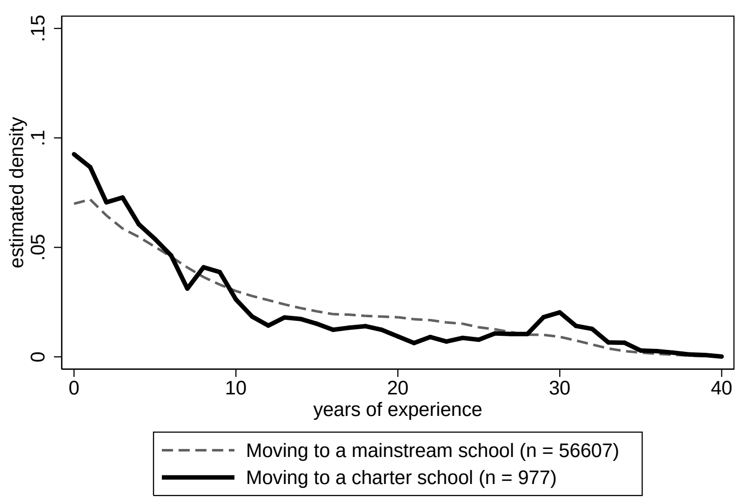


Figure 1: Density estimates - years' experience of all mobile teachers (I) and of licensed mobile teachers (II). Densities were estimated using Epanechnikov kernel functions and halfwidths of 0.50 years.

Table 3: Regression results - qualifications of teachers changing schools, by mainstream/charter destination

	I	II	III	IV
Receiving school type	Mainstream	Charter	Mainstream	Charter
Sample	all	all	licensed	licensed
(Equation 1 coefficient)	($\hat{\delta}^m_{jt}$)	($\hat{\delta}^c_{jt}$)	($\hat{\delta}^m_{jt}$)	($\hat{\delta}^c_{jt}$)
Holds graduate degree	0.008	-0.029	0.012	-0.025
	(4.30)	(2.37)	(6.09)	(1.66)
Attended competitive college	-0.005	-0.036	0.001	-0.010
	(2.97)	(2.90)	(0.52)	(0.72)
Mean licensure test score ($\sim N(0,1)$)	-0.019	-0.031	-0.015	0.066
	(5.29)	(1.19)	(3.98)	(2.52)
Regularly licensed	-0.013	-0.090		
	(10.41)	(8.15)		
Teaching experience (years)	-3.40	-1.40	-3.33	-0.61
	(79.65)	(4.95)	(73.51)	(1.82)
Experience $\leq$ 3 years	0.122	0.091	0.111	0.059
	(59.42)	(6.60)	(52.90)	(3.81)
Experience $\geq$ 25 years	-0.062	0.010	-0.065	0.031
	(50.29)	(1.14)	(46.49)	(2.77)
Days absent	1.66	0.25	1.75	0.40
	(34.11)	(0.74)	(33.22)	(0.99)

Notes: n = 704,134 teachers (627,278 of whom were regularly licensed) with known school assignments in the following year. Column I lists the regression-adjusted mean difference in each qualification between teachers moving to traditional, mainstream public schools and non-movers (δ^m_{jt} in Equation 1). Column II lists the regression-adjusted mean difference in each qualification between charter and mainstream movers (δ^c_{jt}). Columns III and IV report δ^m_{jt} and δ^c_{jt} estimates when the analysis is limited to regularly licensed teachers. Control variables included receiving and sending school characteristics (quintile indicators for percent nonwhite, performance composite, and total enrollment; locale indicators; grade ranges served), a set of dummy variables for school missing data, and receiving 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.

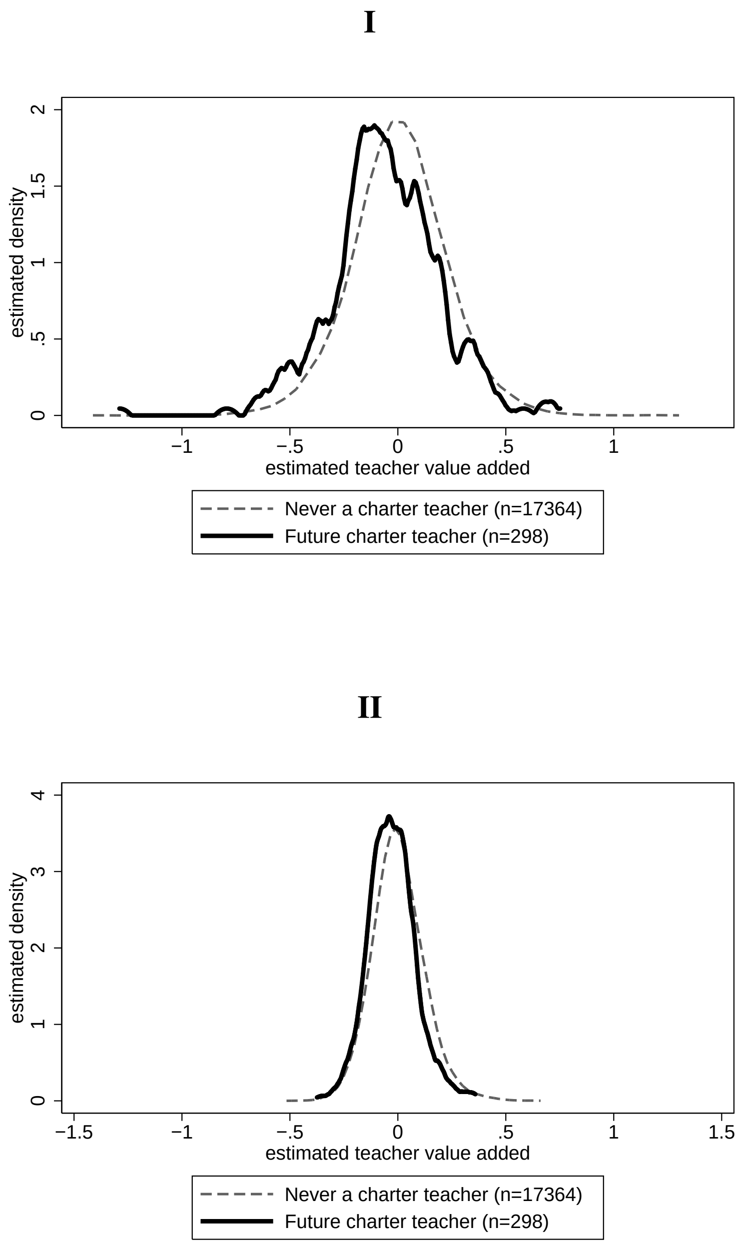


Figure 2: Figures illustrate kernel density estimates of future charter teachers' math value added (solid line), relative to the same for exclusively mainstream teachers (dashed line). Panel I: distribution of teacher fixed effects, controlling for lagged student achievement. Panel II: distribution of persistent value added (via Bayesian shrinkage estimators), controlling for lagged achievement. Densities were estimated using Epanechnikov kernel functions and halfwidths of 0.025 standard deviations.

Table 4: Teacher fixed effect estimates - summary statistics

	I All teachers	II Mainstream movers	III Charter movers
Teachers' math fixed effects			
A. Without lagged achievement, without school fixed effects	0.021	-0.008***	-0.089***
	(0.281)	(0.289)	(0.289)
	[7.10***]		
B. With lagged achievement, without school fixed effects	0.013	-0.009***	-0.047***
	(0.218)	(0.226)	(0.245)
	[8.40***]		
C. Without lagged achievement, with school fixed effects	0.029	-0.007***	-0.072***
	(0.289)	(0.294)	(0.304)
	[5.62***]		
D. With lagged achievement and school fixed effects	0.018	-0.005***	-0.048***
	(0.253)	(0.251)	(0.267)
	[7.37***]		
Teachers' reading fixed effects			
A. Without lagged achievement, without school fixed effects	0.018	-0.009***	-0.065***
	(0.233)	(0.241)	(0.255)
	[4.09***]		
B. With lagged achievement, without school fixed effects	0.010	-0.004***	-0.042***
	(0.157)	(0.167)	(0.174)
	[3.05***]		
C. Without lagged achievement, with school fixed effects	0.025	-0.006***	-0.059***
	(0.237)	(0.241)	(0.283)
	[3.12***]		
D. With lagged achievement and school fixed effects	0.014	-0.004***	-0.035**
	(0.194)	(0.195)	(0.206)
	[2.71***]		
n (teacher-years)	143,365	15,887	340
n (lagged achievement specifications)	97,591	11,757	249

Notes: Teacher fixed effects were estimated by regressing student achievement against current teacher indicators and other inputs in the educational production function, Equation 2. Specifications A through D reference four alternate assumptions about lagged achievement and school fixed effects. Cells list mean teacher fixed effects, by subject, specification, and mobility status. Standard deviations are in parentheses below each mean, and F-statistics from Wald tests of the joint significance of teacher fixed effects are in brackets below each standard deviation. Data for moving teachers reference the year immediately preceding a school change. Mainstream movers (Column II) are mainstream teachers who were next observed in a different mainstream school, and asterisks in Column II indicate a significant difference between mainstream movers and non-mobile teachers. Charter movers (Column III) were next observed in a charter school, and asterisks in Column III indicate significant differences between mainstream and charter movers. *** indicates 1% significance, ** indicates 5%, and * indicates 10%.

Table 5: Regression results - math and reading fixed effects of teachers changing schools, by mainstream/charter destination

	I Mainstream movers	II Charter movers
Teachers' math fixed effects		
A. Without lagged achievement, without school fixed effects	-0.032	-0.060
	(12.40)	(3.80)
B. With lagged achievement, without school fixed effects	-0.022	-0.035
	(9.45)	(2.19)
C. Without lagged achievement, with school fixed effects	-0.033	-0.041
	(12.33)	(2.31)
D. With lagged achievement, without school fixed effects	-0.023	-0.023
	(8.68)	(1.27)
Teachers' reading fixed effects		
A. Without lagged achievement and school fixed effects	-0.028	-0.036
	(13.50)	(2.60)
B. With lagged achievement, without school fixed effects	-0.013	-0.031
	(7.82)	(2.59)
C. Without lagged achievement, with school fixed effects	-0.029	-0.040
	(13.07)	(2.44)
D. With lagged achievement and school fixed effects	-0.018	-0.031
	(8.47)	(2.24)

Notes: $n = 143{,}365$ teachers with known following-year assignments (97,591 in lagged achievement specifications), of whom 15,887 were changing schools and 340 were moving to a charter school (11,757 and 249, respectively, in lagged achievement specifications). Specifications A through D reference four alternate assumptions about lagged achievement and school fixed effects in the educational production function, Equation 2. Column I lists regression-adjusted mean differences in teacher fixed effects between movers and non-movers (δ^m_{jt} in Equation 3) and Column II lists regression-adjusted mean differences in teacher fixed effects between charter and non-charter movers (δ^c_{jt}). Control variables included receiving and sending school characteristics (quintile indicators for percent nonwhite, performance composite, and total enrollment; locale indicators; grade ranges served), a set of dummy variables for school missing data, and receiving 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 6: Future teachers' false "effects" - summary statistics

	I All teachers	II Mainstream movers	III Charter movers
Teachers' false math fixed effects			
A. Without lagged achievement, without school fixed effects	0.009	0.006	-0.041***
	(0.256)	(0.266)	(0.290)
	[3.22***]		
B. With lagged achievement, without school fixed effects	-0.001	-0.001	0.019
	(0.194)	(0.203)	(0.256)
	[2.99***]		
C. Without lagged achievement, with school fixed effects	0.006	-0.001***	-0.016
	(0.243)	(0.254)	(0.287)
	[2.26***]		
D. With lagged achievement and school fixed effects	0.001	0.002	0.015
	(0.190)	(0.199)	(0.245)
	[1.76***]		
Teachers' false reading fixed effects			
A. Without lagged achievement, without school fixed effects	0.009	0.001***	-0.012
	(0.242)	(0.256)	(0.285)
	[2.53***]		
B. With lagged achievement, without school fixed effects	0.001	-0.004**	0.012
	(0.178)	(0.193)	(0.241)
	[1.71***]		
C. Without lagged achievement, with school fixed effects	0.008	-0.001***	0.011
	(0.230)	(0.244)	(0.276)
	[1.85***]		
D. With lagged achievement and school fixed effects	0.002	0.001	0.004
	(0.187)	(0.201)	(0.229)
	[1.32***]		
n (teacher-years)	94,216	11,316	226
n (lagged achievement specifications)	51,349	6,348	126

Notes: Future teacher "effects" were estimated by regressing student achievement against leading teacher indicators and other inputs in the educational production function, Equation 2. Specifications A through D reference four alternate assumptions about lagged achievement and school fixed effects in the educational production function, Equation 2. Column I lists average false fixed effects for all teachers. Standard deviations are in parentheses below each mean, and F-statistics from Wald tests of the joint significance of false teacher fixed effects are in brackets below each standard deviation. Column II lists mean false fixed effects for teachers moving to mainstream schools, with standard deviations in parentheses below each mean, and asterisks indicating statistically significant differences, relative to non-movers. Column III lists mean false fixed effects for teachers moving to charter schools, with standard deviations in parentheses below each mean and asterisks indicating statistically significant differences, relative to mainstream movers. *** indicates 1% significance, ** indicates 5%, and * indicates 10%. Data for moving teachers were evaluated in the year immediately preceding a school change.

Table 7: Variation in classroom residuals

	I standard deviation of average classroom residuals (total)	II standard deviation of persistent teacher-induced residuals (signal)
Variation in math residuals		
A. Without lagged achievement, without school fixed effects	0.793	0.202
	(0.003)	(0.009)
B. With lagged achievement, without school fixed effects	0.537	0.164
	(0.003)	(0.007)
C. Without lagged achievement, with school fixed effects	0.783	0.163
	(0.003)	(0.008)
D. With lagged achievement and school fixed effects	0.530	0.139
	(0.003)	(0.008)
Variation in reading residuals		
A. Without lagged achievement, without school fixed effects	0.811	0.145
	(0.003)	(0.008)
B. With lagged achievement, without school fixed effects	0.579	0.092
	(0.002)	(0.006)
C. Without lagged achievement, with school fixed effects	0.804	0.109
	(0.003)	(0.007)
D. With lagged achievement and school fixed effects	0.576	0.074
	(0.002)	(0.005)

Notes: n = 143,365 teacher-years (97,591 in lagged achievement specifications). Column I lists the standard deviation of classroom-averaged residuals from Equation 4. Column II lists the standard deviation of persistent teacher-induced components of classroom residuals, computed as the pairwise covariance of same-teacher classroom residuals. Bootstrapped standard error estimates are in parentheses below each standard deviation.

Table 8: Regression results - persistent value added of teachers changing schools, by mainstream/charter destination

	I Mainstream movers	II Charter movers
Teachers' persistent value added in math		
A. Without lagged achievement, without school fixed effects	-0.029	-0.064
	(11.49)	(4.02)
B. With lagged achievement, without school fixed effects	-0.020	-0.041
	(8.96)	(2.53)
C. Without lagged achievement, with school fixed effects	-0.028	-0.029
	(12.48)	(1.97)
D. With lagged achievement and school fixed effects	-0.019	-0.027
	(8.95)	(1.73)
Teachers' persistent value added in reading		
A. Without lagged achievement, without school fixed effects	-0.023	-0.038
	(11.62)	(2.76)
B. With lagged achievement, without school fixed effects	-0.011	-0.034
	(6.31)	(2.89)
C. Without lagged achievement, with school fixed effects	-0.022	-0.026
	(11.96)	(2.01)
D. With lagged achievement and school fixed effects	-0.010	-0.035
	(6.54)	(2.99)

Notes: n = 143,365 teachers with known following-year assignments (97,591 in lagged achievement specifications), of whom 15,887 were changing schools and 340 were moving to a charter school (11,757 and 249, respectively, in lagged achievement specifications). Specifications A through D reference four alternate assumptions about lagged achievement and school fixed effects in the educational production function, Equation 4. A teacher's persistent value added is her weighted mean classroom-level residual, scaled by a Bayesian factor that accounts for sampling error. See the appendix for computation details. Column I lists regression-adjusted mean differences in persistent value added between movers and non-movers (δ^m_{jt} in Equation 3) and Column II lists regression-adjusted mean differences in persistent value added between charter and non-charter movers (δ^c_{jt}). Control variables included receiving and sending school characteristics (quintile indicators for percent nonwhite, performance composite, and total enrollment; locale indicators; grade ranges served), a set of dummy variables for school missing data, and receiving 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.

CALDER