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# MATCH QUALITY, WORKER PRODUCTIVITY, AND WORKER MOBILITY: DIRECT EVIDENCE FROM TEACHERS

C. Kirabo Jackson*

*Abstract*—I investigate the importance of the match between teachers and schools for student achievement. I show that teacher effectiveness increases after a move to a different school and estimate teacher-school match effects. Match quality explains away a quarter of and has two-thirds the explanatory power of teacher quality. Match quality is negatively correlated with school switching, is unrelated to exit, and increases with experience. This paper provides the first estimates of worker-firm match quality using output data, as opposed to inferring productivity from wages or employment durations. The results suggest that workers seek high-quality matches for reasons other than higher pay.

## I. Introduction

THE productive quality of the match between workers and firms plays a central role in canonical models of worker mobility (Jovanovic, 1979; Mincer & Jovanovic, 1981; Neal, 1999; Burdett, 1978; Mortensen, 1998; Johnson, 1978). The labor market is hypothesized to efficiently allocate workers to firms through workers leaving(seeking) jobs where the productivity match between the worker and firm is low(high). Match quality is also used to explain the stylized facts that changing jobs is associated with earnings growth (Bartel & Borjas, 1981; Altonji & Shakotko, 1987; Topel & Ward, 1992) and that job separations decline with tenure and experience.[1]

Despite the importance of match effects for understanding the labor market, there is little direct evidence of their existence. Data on match-specific productivity are essentially nonexistent, forcing researchers to specify how wages relate to match-specific productivity and then to study how wages and their distribution vary with tenure and job mobility (Nagypal, 2007). This is undesirable for two reasons. First, there are numerous ways to specify wage setting, making misspecification and omitted variables bias likely. For example, taste-based discrimination can depress wages and increase job separations for certain workers at certain firms, mimicking empirical patterns consistent with productivity match effects. Second, it is difficult to distinguish workers leaving(seeking) jobs with low(high) match quality from workers leaving(seeking) jobs with low(high) pay because pay may vary across workers and firms for reasons unrelated to productivity. To avoid these problems, one must estimate match quality based on actual output. Microdata with student outcomes linked to teachers and schools provide a unique opportunity to estimate worker (teacher), firm (school), and match (a given teacher at a particular school) productivity on a measure of output (student achievement) directly.

Using a longitudinal data set of student test scores linked to teachers and schools in North Carolina, I aim to (a) determine the extent to which teacher effectiveness, as measured by ability to improve student test scores, changes depending on the schooling environment; (b) quantify the importance of the match between a teacher and a school in determining student achievement; (c) document the relationship between match quality and teacher mobility; and (d) present evidence on observable characteristics associated with high match quality.

Match quality is of interest in its own right because we have little understanding of the role of school-teacher match quality for student achievement. Studies that identify teachers associated with student test score gains show that a 1 standard deviation increase in teacher quality leads to between one-tenth and one-fifth of a standard deviation increase in students' math and reading scores (Aaronson, Barrow, & Sander, 2007; Rivkin, Hanushek, & Kain, 2005; Jackson, & Bruegmann, 2009; Kane & Staiger, 2008). However, Jackson (forthcoming) finds that high school teacher effects on test scores may be smaller in secondary school than in elementary school. Because observable teacher characteristics explain only a fraction of a teacher's value-added,[2] we have little understanding of exactly what they measure and whether a teacher who is effective at one school (say, with affluent suburban students) would be equally effective at another school (perhaps with low-income inner-city students).[3] Given the increasing use of estimated teacher value-added to identify effective teachers and policies that aim to move strong teachers from high-achieving schools into low-performing schools, it is important to understand the importance of school-specific teacher value-added—that is, the importance of match quality. Moreover, it is also important to assess the importance of the match between teacher and school itself. If match effects are economically important, policymakers should consider what kinds of teacher-to-school pairings are most productive and the effect of policies on match quality.

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[1] By match quality, I refer to the fixed time-invariant productivity associated with a particular worker-firm pairing. I am not referring to match quality that changes over time, such as that due to firm-specific human capital.

[2] There is evidence that years of experience, college selectivity, teachers' test scores, and regular licensure are associated with higher student achievement (Anthony & Goldhaber, 2007; Brewer & Ehrenberg, 1994; Brewer & Goldhaber, 2000; Clotfelter, Ladd, & Vigdor, 2006; Hanushek, 1997).

[3] Ost (2009) finds that holding total experience constant, teachers with grade-specific experience have higher value-added. This suggests that teacher value-added changes over time and may be specific to context.


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I find that teachers who switch schools are more effective after a move than before, which is suggestive of match effects. I present a variety of empirical tests showing that this cannot be explained by teachers moving to higher-achieving schools, endogenous teacher effort, or student selection. I use both a fixed-effects model and a random-effects model to estimate the importance of match quality. Across both models, a 1 standard deviation increase in match quality increases math scores by about $0.09\sigma$ and reading scores by about $0.07\sigma$—roughly two-thirds the effect of a 1 standard deviation increase in teacher quality. Match quality can account for between 10% and 40% of what is typically estimated as teacher quality, so that a sizable portion of what is typically considered a teacher effect is not portable across schools. Exploratory data analysis reveals that certain identifiable types of teachers achieve much better outcomes at certain identifiable types of schools, such that an optimal matching of teachers to schools could yield meaningfully improved outcomes overall.

Consistent with canonical models of job search, (a) among mobile teachers, match quality is higher in the new match than in the previous match; (b) general teacher quality is unrelated to switching teaching jobs, while match-specific quality is negatively correlated with switching teaching jobs; and (c) while general teacher quality is negatively correlated with exiting the profession, match-specific quality is unrelated to exiting the profession. These patterns are robust across estimation strategies and to the inclusion of both teacher and school fixed effects. The patterns suggest that match quality is an important determinant of mobility for reasons other than the level of pay because productivity and pay are largely unrelated for teachers. This is consistent with either schools providing greater nonpecuniary benefits for effective teachers or teachers caring about their effectiveness directly. As such, these findings suggest that models of worker mobility that do not account for nonpecuniary job benefits may be incomplete and underscore that using wages to infer match quality has some important limitations.

This is the first paper to validate the job search literature using direct measures of output and the first to document the relationship between match quality and worker mobility in a context where wages and productivity are unrelated, underscoring the importance of nonpecuniary benefits. In addition, this paper is the first to highlight and quantify the importance of match effects in education and to document that a sizable portion of what we call teacher quality does not transmit across schools. The findings speak to the theoretical literature on job search and the empirical literature on teacher quality and have important policy implications.[4]

The remainder of this paper is as follows. Section II outlines the empirical framework behind estimating match effects and lays out a theoretical framework based on Jovanovic (1979). Section III describes the data. Section IV provides evidence of match effects. Section V describes the estimation strategies and presents estimates of the variability of teacher, school, and match effects. Section VI tests the theoretical predictions regarding the relationship between match quality and teacher mobility I presents evidence of what kinds of teacher-school combinations are associated with higher match quality, and finally, section VII concludes.

## II. Match Quality for Teachers

The literature that decomposes wages into a worker effect and a firm effect starts with a Cobb-Douglas production function describing the output $Q_{ij}$ of worker $i$ at firm $j$ as below[5]

$$Q_{ij} = L_i^{\theta} K_j^{\varphi}. \tag{1}$$

In equation (1), $L_i$ is the human capital of worker $i$ (such as education and years of experience), $K_j$ summarizes the productive characteristics of the firm (such as technology and capital intensity), and $\theta$ and $\phi$ are parameters of the production function. One can incorporate the fact that certain worker and firm attributes are complementary with the inclusion of a match (worker-by-firm) term $M_{ij}$ (Woodcock, 2008) to yield:

$$Q_{ij} = L_i^{\theta} K_j^{\varphi} M_{ij}^{\phi}, \tag{2}$$

where worker $i$'s wage at firm $j$ is a share $\pi_{ij}$ of output, the log of worker wages is given by equation (3), which comprises four additively separable components: one due to worker productivity $\theta \ln L_i$, one due to workplace productivity $\varphi \ln K_j$, one due to the productivity match between the worker and firm $\phi \ln M_{ij}$, and the relative bargaining power of worker $i$ at firm $j$ $\ln \pi_{ij}$:

$$\ln w_{ij} = \theta \ln L_i + \varphi \ln K_j + \phi \ln M_{ij} + \ln \pi_{ij}. \tag{3}$$

If differences in bargaining power across worker-firm pairings exist, relative productivity and match-specific relative bargaining power may be confounded. For example, if discriminating firms pay equally productive female workers

[4] For example, the results suggest that teacher turnover is not unambiguously negative and could actually be welfare enhancing on average. This implies that policies discouraging teacher turnover (such as pensions tied to a particular district or pay based on seniority within a school district) reduce the likelihood that teachers find a good match and reduce student achievement ceteris paribus. In addition, the results indicate that one mechanism through which policies such as performance-based pay and accountability might improve student outcomes in equilibrium is increasing incentives to maximize match quality. That is, when schools and teachers are rewarded for their students' performance, teachers and schools have an incentive to find schools and teachers with which they are well matched, ceteris paribus. This mechanism has not been discussed in the performance pay or accountability literature to date. Finally, insofar as principals can observe match quality, the results suggest that they should have flexibility in hiring and firing decisions.

[5] See Abowd et al. (2004), Abowd, Kromarz, and Margolis (1999), and Abowd, Creecy, and Kromarz (2002).

less than their male counterparts, this would result in low $\ln \pi_{ij}$ for female workers at such firms that cannot be distinguished from low match productivity. Moreover, many models of wage determination (such as efficiency wages, deferred compensation, and in-kind benefits) predict that wages have little relation to contemporaneous productivity, making misspecification and omitted variables bias likely in equation (3). For these reasons, using wage data to infer match quality has inherent limitations. This motivates the use of teacher (worker) data linked to schools (firms) and student outcomes (a direct measure of output).

### A. *Production of Student Achievement*

Consider the following model where student achievement is a function of the entire history of school and parental inputs and a student's endowment:

$$T_{ijsa} = T_a[X_{ijs}(a), \mu_{i0}, \varepsilon_{ijsa}]. \tag{4}$$

In equation (4), $T_{ijsa}$ is student $i$'s achievement with teacher $j$ at school $s$ at age $a$, $X_{ijs}$ $(a)$ is the history of parental and school inputs up to age $a$, $\mu_{i0}$ is the student's endowment (ability), and $\varepsilon_{ijsa}$ is an idiosyncratic error (other inputs). With additive separability of inputs and where lagged achievement is a summary statistic for the full history of family, school, and student inputs, we can write equation (4) as the commonly used value-added model described by equation (5):[6]

$$T_{ijsa} = X_{ijsa}\alpha + \gamma T_{ia-1} + \eta_{ijsa}. \tag{5}$$

Explicitly incorporating teacher human capital, school technology, and the productivity of the specific teacher-school pairing as inputs into the model yields

$$T_{ijsa} = \gamma T_{ia-1} + X_{ijsa}\alpha + \theta_j + \theta_s + \theta_{sj} + \eta_{ijsa}. \tag{6}$$

Both equations (3) and (6) contain additively separable components: one due to worker (teacher) productivity $\theta_j$, one due to workplace (school) productivity $\theta_s$, and one due to the match between the worker and the firm $\theta_{js}$. However, unlike equation (3), the school, teacher, and match components in equation (6) reflect differences in actual productivity.

### B. *Identifying Match Quality Empirically*

With data on multiple teachers at multiple schools, one can estimate match (teacher-by-school) effects separately from teacher effects and school effects. Consider the ideal empirical setup where each teacher is observed at all schools and there is no correlation between potential match, teacher, and school effects.[7] Under these conditions, mean match quality for each teacher and each school is 0, so mean scores (conditional on controls for selection) of teacher $j$ at school $s$ would be a consistent estimate of match (teacher-by-school) effect $\theta_{js}$. Furthermore, the mean of the matches for teacher $j$ (across schools) would be a consistent estimate of teacher effect $\theta_j$, and the mean of matches for school $s$ (across teachers) would be a consistent estimate of school effect $\theta_s$. Identification of match effects comes from the fact that multiple teachers are observed switching across the same set of schools. To make this clear, consider two schools A and B and two teachers $p = \{1,2\}$. The difference in outcomes when teacher $p$ switches from school A to school B is $(\theta_B - \theta_A) + (\theta_{pB} - \theta_{pA})$. This reflects the difference in school effects between schools A and B, plus the difference in match effects for teacher $p$ between schools A and B. If there are no match effects, then $\theta_{1B} - \theta_{1A} = 0$, and the difference in expected outcomes associated with switching from A to B is equal to the difference in school effects only and is the same for both teachers. With no match effects, $E[\bar{Y}_{1B} - \bar{Y}_{1A}] - E[\bar{Y}_{2B} - \bar{Y}_{2A}] = 0$. However, with match effects, expected differences associated with switching schools will not be the same for both teachers, so that $E[\bar{Y}_{1B} - \bar{Y}_{1A}] - E[\bar{Y}_{2B} - \bar{Y}_{2A}] \equiv \theta_{1B} - \theta_{1A} - (\theta_{2B} - \theta_{2A}) \neq 0$.[8]

A simple example can illustrate the intuition. Suppose school A has a strong principal who, all else equal, improves the outcomes of all teachers by $\delta$. School A enrolls high-income students, and school B enrolls average students. Teacher 1 performs $\mu_1$ better with high-income students, while teacher 2 performs $\mu_2$ worse (where $|\mu_2|>|\delta|$). When teacher 1 switches from school B to A, her outcomes are $\delta + \mu_1$ better at school A than B. However, when teacher 2 switches from school B to A, her outcomes are $\delta - \mu_2$ worse. Although there is a positive school effect $\delta$ enjoyed by both teachers, the difference in outcomes associated with switching schools is not the same because the match effect for school A is positive for teacher 1 and negative for teacher 2. This differential switching response is the basis for identifying match effects.

The discussion above assumes that all teachers are observed in all schools. In reality, most teachers are observed in just a few schools. While this complicates the estimation of match effects, the logic of the identification is most saliently illustrated in this idealized setting. In section V, I detail an approach to consistently estimate match effects outside of an ideal setting.

[6] This is true when coefficients on inputs are geometrically declining with distance (in age), and the impact of the ability endowment geometrically declines at the same rate as inputs (Todd & Wolpin, 2003).

[7] Mathematically, this condition means that $\mathrm{Cov}\begin{bmatrix}\theta_s\\ \theta_j\\ \theta_{js}\end{bmatrix} = \begin{bmatrix}\sigma^2_{\theta s}I_S & 0 & 0\\ 0 & \sigma^2_{\theta j}I_J & 0\\ 0 & 0 & \sigma^2_{\theta js}I_M\end{bmatrix}$.

[8] With the assumption that the mean of the match effects is equal to 0 in expectation for each school and for each teacher $\theta_{1B} + \theta_{1A} = 0$ and $\theta_{2B} + \theta_{2A} = 0$ and $\theta_{1B} + \theta_{2B} = 0$ and $\theta_{1A} + \theta_{2A} = 0$. With four equations and four unknowns, there is a unique solution for the values of the match effects.

### C. *How Does One Interpret Match Quality for Teachers?*

While it is difficult to disentangle a school or teacher with a large effect from a school or teacher that has high-quality matches empirically, these concepts are distinct. Any factors that affect all teachers at a school equally would be part of a school effect (e.g., high-achieving students, high-quality colleagues, or strong leadership). Similarly, any factors that are specific to a teacher and affect students at all schools equally (such as general teaching ability) would be part of a teacher effect. As such, only those combinations of characteristics that vary at the teacher-by-school level are part of a match effect. Such effects arise when there is heterogeneity in the marginal effectiveness of school inputs across teachers. For example, certain teachers may be good at teaching certain types of students (perhaps same race, high motivation) who attend certain schools. Alternatively, certain schools may have a work culture in which certain teachers thrive and others do not. There may be differences due to differential responses to the characteristics of other employees (for example, experienced teachers, high-value-added teachers). In sum, match quality captures systematic complementarities between particular teachers and particular schools. I present the correlates of high and low match quality in section VII.

### D. *Theoretical Framework*

Most theories of match quality assume that workers seek out high-quality matches to increase their monetary compensation. However, teacher salaries are based primarily on years of experience and level of education. I present a framework to explain how and why match quality may be related to mobility for teachers. The theoretic framework follows Jovanovic (1979). There is imperfect information about which matches are more or less productive, and when a teacher searches for a job, she receives a random draw from a known distribution of match quality. Match quality is an experience good, so that only after a match is made is the quality of the match revealed to the employee and all employers. Employers contract with workers on an individual basis and can reward a worker with whom they match well by improving the worker's job-related utility. Unlike canonical models, I assume that employers can improve job-related utility not only by altering wages but also by altering the nonwage aspects of compensation.[9] For example, principals can offer extra positions to supplement income, pay for training costs, or appoint teachers to positions of leadership. Because teachers often spend out of pocket to pay for classroom supplies, principals can—and do—effectively increase teacher pay by paying for such supplies.[10] Even without action on the part of employers, workers may seek high-productivity matches because they derive utility directly from being productive.[11] In this framework, workers well matched with their employers are less likely to quit due to either employers increasing job-related utility or nonpecuniary job characteristics. Because information about match quality is revealed after being employed at a school, teachers who realize that they are poorly matched have an incentive to leave the job (select out of bad matches) and take a new draw from the known distribution of matches. Because having a bad match is indicative of being bad at a particular school but not at the profession as a whole, low match quality should be associated with switching schools but not with leaving teaching.

In addition to match quality, which is specific to a particular teacher-school pairing, teachers possess general teaching ability that is transferable across schools (but not necessarily across occupations). Similar to match quality, general teaching ability is an experience good that is revealed to all parties after a teaches enters the profession. Because low general teaching quality is indicative of being poor teachers at all schools, teachers with low general teaching ability should be more likely to exit the profession entirely, but no more or less likely to switch schools.

This framework generates predictions that can be taken to the data: (a) among mobile teachers, match quality should be higher in the new match than in the previous match; (b) higher general teacher quality should be negatively correlated with exiting the profession; (c) general teacher quality should be uncorrelated with switching; (d) higher match quality should be negatively associated with switching; and (e) match quality should be unrelated to exiting the profession. Consistent with these predictions, studies have found that effective teachers are less likely to exit teaching or switch schools (Jackson, 2012; Hanushek et al., 2005; Sass & Feng, 2008; Sass et al., 2012). However, these studies do not distinguish between teacher and match quality, so testing these predictions may put the documented patterns into theoretical context.

## III. Data

This paper uses data on all third-grade through fifth-grade students in North Carolina from 1995 to 2006 from the North Carolina Education Research Data Center.[12] The

[9] As far back as Adam Smith, it has been recognized that the utility a worker derives from his or her job is associated with more than monetary compensation. As Smith stated, "Wages vary by ease vs. hardship, cleanliness, honourableness" (Smith, 1776). This is illustrated by the fact that the unemployed are much less happy than the employed and by more than their lower incomes would predict (Korpi, 1997; Winkelmann & Winkelmann, 1998; Di Tella, MacCulloch, & Oswald, 2001), and that one's ordinal rank in the wage hierarchy affects one's happiness conditional on one's level of pay (Brown, Gardner, & Oswald, 2006). Aside from consumption aspects of the job, in-kind benefits compose a substantial part of a worker's compensation, and workers care about working conditions and prestige (Duncan, 1976).

[10] The average teacher surveyed for the 2010 Retail Market Awareness Study released by the National School Supply and Equipment Association said she spent $936 on classroom materials in the 2007 academic year.

[11] Insofar as teachers have some intrinsic motivation to teach and believe in service, they may be willing to trade off monetary rewards for the nonpecuniary gains of being of service (Akerlof & Kranton, 2005).

[12] These data have been used by other researchers to examine the effect of teachers on student outcomes (Clotfelter et al., 2006, 2007) and the effect of student demographics on teacher quality (Jackson, 2009).

TABLE 1.—SUMMARY STATISTICS

| Variable | Observations | Mean | Standard Deviation |
|---|---|---|---|
| Unit of observation: Student-year | | | |
| Math Scores | 1,361,473 | 0.033 | 0.984 |
| Reading Scores | 1,355,313 | 0.022 | 0.984 |
| Change in Math Score | 1,258,483 | 0.006 | 0.583 |
| Change in Reading Score | 1,250,179 | 0.001 | 0.613 |
| Black | 1,372,098 | 0.295 | 0.456 |
| White | 1,372,098 | 0.621 | 0.485 |
| Female | 1,372,098 | 0.493 | 0.500 |
| Parent Education: No HS Degree | 1,372,098 | 0.107 | 0.309 |
| Parent Education: HS Degree | 1,372,098 | 0.428 | 0.495 |
| Parent Education: Some College | 1,372,098 | 0.315 | 0.464 |
| Parent Education: College Degree | 1,372,098 | 0.143 | 0.350 |
| Same Race | 1,372,098 | 0.649 | 0.477 |
| Same Sex | 1,372,098 | 0.496 | 0.500 |
| Class Size | 1,372,098 | 23.054 | 4.053 |
| Unit of observation: Teacher-year | | | |
| Experience | 91,243 | 12.798 | 9.949 |
| Experience 0 | 92,511 | 0.063 | 0.242 |
| Experience 1 to 3 | 92,511 | 0.165 | 0.371 |
| Experience 4 to 9 | 92,511 | 0.230 | 0.421 |
| Experience 10 to 24 | 92,511 | 0.365 | 0.481 |
| Experience 25+ | 92,511 | 0.164 | 0.371 |
| Teacher Exam Score | 92,511 | −0.012 | 0.812 |
| Advanced Degree | 92,511 | 0.197 | 0.398 |
| Regular Licensure | 92,511 | 0.670 | 0.470 |
| Certified | 92,511 | 0.039 | 0.194 |

The few teachers with more than fifty years of experience are coded as having fifty years of experience.

student data include demographic characteristics, standardized test scores in math and reading, and codes allowing one to link the student test score data to information about the schools the students attended and the teachers who administered their tests. Discussions with education officials in North Carolina indicate that tests are always administered by the students' own teachers when these teachers are present. To limit the sample to teachers who I am confident are the students' actual teachers, I include only students who are administered the exam by a teacher who teaches math and reading to students in that grade. I also remove teachers who are coteaching or have a teaching aide. This process yields roughly 1.37 million student-year observations. Summary statistics for these data are presented in table 1.

The students are 62% white and 30% black and are evenly divided between boys and girls. Approximately 11% of the students' parents did not finish high school, 43% had just a high school diploma, 30% had some post–high school education, and 14% had parents with a four-year degree or higher. The average class size is 23. Reading and math scores are standardized in each grade-year cell to have a mean of 0 and unit variance.

About 92% of teachers in the sample are female, 83% are white, and 15% are black. The average teacher in the data has thirteen years of experience, and roughly 6% of the teachers have no experience.[13] Approximately 20% of teachers have advanced degrees. About 67% of the teachers in the sample have regular licensure as opposed to working under a provisional, temporary, emergency, or lateral entry license. I normalize scores on the elementary education or the early childhood education tests that all North Carolina elementary school teachers are required to take, so that these scores have a mean of 0 and unit variance for each year in the data. Teachers perform near the mean with a standard deviation of 0.81. About 4% of teachers have national board certification.

The final data set contains 27,498 teachers and 1,545 schools. The average school is observed to have 21.3 teachers, while about 80% of teachers are observed in only one school. Approximately 16% of teachers are observed in two schools, 2% in three schools, and about 1% in four or more schools. The average teacher is observed in the data for 3.26 years, and 37% are observed for one year. There are 32,922 teacher-school matches observed in the data, and each match contains 98 students and 4.2 classrooms on average. Matches for mobile teachers contain on average 78 students and 3.4 classrooms.

### A. *Who Are the Mobile Teachers?*

Because match quality is a within-teacher concept, match effects can be estimated only for mobile teachers and may not be representative of match effects for nonmobile teachers. While this does not affect the internal validity of the exercise, one may wonder how mobile teachers compare to the average teacher. To gain a sense of this, I estimate linear probability models for the likelihood that a teacher

[13] Teacher experience is based on the amount of experience credited to the teacher for the purposes of determining salary; therefore, it should reflect total teaching experience in any school district.

TABLE 2.—RELATIONSHIP BETWEEN SCHOOL SWITCHING AND TEACHER CHARACTERISTICS

| | Switch Schools in One year | | Exit Teaching in One year | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Experience: 1 to 3 | −0.002 | −0.003 | 0.015 | 0.026 |
| | [0.005] | [0.005] | [0.009]* | [0.009]*** |
| Experience: 4 to 9 | −0.006 | −0.007 | −0.023 | −0.001 |
| | [0.005] | [0.006] | [0.009]** | [0.009] |
| Experience: 10 to 24 | −0.015 | −0.012 | −0.102 | −0.068 |
| | [0.005]*** | [0.005]** | [0.009]*** | [0.009]*** |
| Experience: 25+ | −0.057 | −0.054 | −0.064 | −0.029 |
| | [0.006]*** | [0.006]*** | [0.009]*** | [0.010]*** |
| Licensure Score | −0.007 | −0.003 | −0.004 | 0.001 |
| | [0.001]*** | [0.002]** | [0.003] | [0.003] |
| Advanced Degree | 0.003 | 0.003 | 0.031 | 0.031 |
| | [0.003] | [0.003] | [0.006]*** | [0.006]*** |
| Regular License | 0.034 | 0.045 | −0.19 | −0.178 |
| | [0.005]*** | [0.005]*** | [0.008]*** | [0.008]*** |
| Year effects | Yes | Yes | Yes | Yes |
| School effects | No | Yes | No | Yes |
| Observations | 89,856 | 89,856 | 75,303 | 75,303 |
| $R^2$ | 0.17 | 0.2 | 0.03 | 0.08 |

Robust standard errors in brackets are adjusted for clustering at the teacher level.
Significant at *10%, **5%, and ***1%.

switches to another school next year, as a function of observable teacher characteristics. I estimate equation (7) by ordinary least squares (OLS):

$$Y_{jst} = T_{jt}\beta + \tau_t + \varepsilon_{jst}, \tag{7}$$

where $Y_{jst}$ is whether teacher $j$ switches from her current school $s$ at time $t$, $T_{jt}$ are time-varying teacher characteristics, $\tau_t$ is a year fixed effect, and $\varepsilon_{jst}$ is the idiosyncratic error term. I present these results in column 1 of table 2. These estimates are descriptive; I model teacher switching and exiting the profession formally in section VI.

Column 1 shows that the likelihood of switching schools is monotonically decreasing in experience, consistent with more experienced teachers finding better matches and remaining in these schools. On average, teachers with higher licensure scores and teachers with an advanced degree are no more likely to switch schools, while those with a regular teaching license are 4.2 percentage points more likely to switch schools. Because these relationships could reflect the fact that mobile teachers differ from nonmobile teachers at their school locations and certain schools have higher teacher turnover than others, the estimates in column 2 include school fixed effects. The patterns are similar. In sum, the sample of switchers is more likely to have fewer than 10 years of experience, less likely to have more than 24 years of experience, and more likely to be regularly licensed teachers than the average teacher.

### B. *How Do Destination Schools Differ from Sending Schools?*

Because the analysis involves comparing teachers' performance at one school to their performance at another, it is instructive to describe differences in school characteristics between a teacher's school the year before and after she switches schools. For each characteristic $X_{ij}$ for teachers who switch schools in year $t$, I present $\Delta X_{ij} = X_{ij't-1} - X_{ijt}$, and test for the statistical significance of these differences. I present these differences for all teachers and for subsamples of teachers in table 3. On average, teachers move to schools where mean reading test scores are $0.023\sigma$ higher and classes are 0.23 students smaller. Teachers move to schools where the percentages of black and low-income students in the school are 2.5 and 3.8 percentage points lower, respectively. Teachers also experience a 7.3% pay increase after a move. Teachers do not tend to switch out of large cities but do switch out of midsized cities and towns into rural areas. These patterns are consistent with teachers leaving low-performing schools for higher-achievement schools with fewer low-income students and fewer minority students.

Columns 2 and 3 show results for white and nonwhite teachers, respectively. While all teachers experience a pay increase and go to schools with fewer low-income students after a move, white teachers go to schools with higher-achieving students and more white students, while nonwhite teachers move to schools that have similar levels of achievement and more black students. In addition, similar to findings obtained by Hanushek, Kain, and Rivkin (2004) using Texas data, nonwhite teachers are more likely to switch to inner-city schools while white teachers are not. Teachers with high scores experience larger increases in student achievement and smaller decreases in the percentage of black students, and they are less likely to move to a less urbanized environment after a move than low-scoring teachers. Inexperienced teachers (fewer than five years) gain larger increases in student achievement after a move than do veteran teachers (more than ten years). All groups tend to leave schools with low-income students, but inexperienced teachers see larger decreases in the percentage of black students at their new school than experienced

TABLE 3.—COMPARING SENDING AND RECEIVING SCHOOLS BY TEACHER TYPE

| | Difference in Characteristics between Receiving and Sending Schools | | | | | | |
|---|---|---|---|---|---|---|---|
| | All | White | Nonwhite | High Score | Low Score | More than five years Experience | Less than Ten years Experience |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Reading scores (class) | −0.054 | −0.06 | −0.028 | −0.076 | −0.036 | −0.111 | −0.025 |
| | [0.009]*** | [0.010]*** | [0.023] | [0.018]*** | [0.018]* | [0.024]*** | [0.013]** |
| Reading scores (school) | −0.023 | −0.03 | 0.013 | −0.034 | −0.005 | −0.061 | 0.001 |
| | [0.005]*** | [0.005]*** | [0.013] | [0.010]*** | [0.010] | [0.014]*** | [0.006] |
| Mean class size | 0.236 | 0.184 | 0.503 | 0.133 | 0.356 | −0.008 | 0.366 |
| | [0.052]*** | [0.055]*** | [0.141]*** | [0.106] | [0.106]*** | [0.137] | [0.072]*** |
| % nonwhite teachers | 0.015 | 0.021 | −0.015 | 0.019 | −0.001 | 0.028 | 0.003 |
| | [0.004]*** | [0.005]*** | [0.010] | [0.009]** | [0.009] | [0.013]** | [0.006] |
| Log of salary | −0.073 | −0.073 | −0.068 | −0.075 | −0.07 | −0.072 | −0.052 |
| | [0.002]*** | [0.002]*** | [0.006]*** | [0.003]*** | [0.004]*** | [0.005]*** | [0.002]*** |
| Percent free lunch | 0.038 | 0.039 | 0.034 | 0.038 | 0.039 | 0.046 | 0.035 |
| | [0.003]*** | [0.003]*** | [0.010]*** | [0.006]*** | [0.007]*** | [0.009]*** | [0.004]*** |
| Percent black | 0.025 | 0.029 | 0.003 | 0.028 | 0.011 | 0.036 | 0.016 |
| | [0.003]*** | [0.003]*** | [0.007] | [0.005]*** | [0.005]** | [0.009]*** | [0.003]*** |
| Log of enrollment | −0.013 | −0.016 | −0.001 | −0.01 | −0.008 | −0.019 | −0.001 |
| | [0.006]** | [0.006]** | [0.014] | [0.012] | [0.011] | [0.016] | [0.007] |
| Large city | −0.002 | 0.001 | −0.012 | −0.002 | −0.008 | 0.002 | 0.001 |
| | [0.003] | [0.003] | [0.007]* | [0.005] | [0.004]* | [0.007] | [0.003] |
| Midsized city | 0.015 | 0.02 | −0.011 | 0.022 | −0.002 | 0.006 | 0.01 |
| | [0.005]*** | [0.005]*** | [0.012] | [0.011]** | [0.010] | [0.016] | [0.006]* |
| Urban fringe | 0.003 | 0.001 | 0.019 | −0.003 | 0.025 | −0.011 | 0.015 |
| | [0.006] | [0.006] | [0.012] | [0.012] | [0.011]** | [0.017] | [0.007]** |
| Town | 0.02 | 0.02 | 0.014 | 0.02 | 0.022 | 0.031 | 0.01 |
| | [0.004]*** | [0.005]*** | [0.010] | [0.008]** | [0.009]** | [0.014]** | [0.005]** |
| Rural area | −0.036 | −0.041 | −0.01 | −0.037 | −0.036 | −0.026 | −0.035 |
| | [0.006]*** | [0.007]*** | [0.013] | [0.013]*** | [0.012]*** | [0.019] | [0.008]*** |

Each coefficient represents a separate regression of each covariate on a "post switch" indicator variable. Robust standard errors in brackets are adjusted for clustering at the teacher level. Significant at *10%, **5%, and ***1%.

teachers. While inexperienced teachers appear to be switching largely out of towns, experienced teachers are switching out of midsized cities and urban fringes. In sum, not all teachers are switching to or from the same schools, so several schools have different teachers switching in and out. This fact plays a central role in my identification strategy.

## IV. Preliminary Evidence of the Existence of Match Effects

The first empirical prediction from the theoretic framework is that match quality should be higher in a new match than in a previous match, and thus teachers should be relatively more effective after than before switching schools. I can test this first prediction empirically by mapping out teacher effectiveness before and after a move. To do this, I estimate equation (8) by OLS,[14]

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \sum_{\tau=-10}^{9} I_{t=\tau} \cdot \pi_\tau + \theta_j + \eta_{ijsy}, \qquad (8)$$

where $T_{ijsy}$ is student $i$'s achievement with teacher $j$ at school $s$ in year y, $X_{ijsy}$ is a vector of control variables (student race, gender, parental education, limited English proficiency, the gender and racial match between the student and the teacher, class size, and teacher experience), and $\theta_j$ is a teacher fixed effect. Because one cannot simultaneously estimate teacher, year, and experience effects,[15] as commonly done, I include indicator variables for experience bins (0, 1 – 3, 4 – 9, 10 – 24, 25 and up). In equation (8), $\pi_\tau$ is the effect of having a teacher who is $\tau$ years from leaving her current school (for example, $\pi_{-2}$ is the effect for a teacher who will leave her current school in two years, and $\pi_{+2}$ is the effect for a teacher who left another school two years ago). The reference year is the year before a teacher switches schools. In this difference-in-difference (DID) model, the "years before/after move" variables map out changes in outcomes for teachers who switch schools relative to changes for teachers who do not switch schools over the same time period.[16]

To ensure that any pre- and postmove differences are not driven by unmeasured achievement differences across

[14] Equation (8) follows naturally from equation (7), where the age subscript for the student is replaced with the more general year subscript that is defined for teachers, schools, and students.

[15] This is pointed out in Rockoff (2004) and Papay and Kraft (2010).

[16] Teacher switching is defined within the sample. This definition of switching captures 72% of all switching that takes place in the data. This ensures that the before-and-after comparisons are truly a within-teacher concept. As such, a teacher with only premove data or a teacher with only postmove data will not be included in the mobility analysis variables. Similarly, if a teacher switches from third grade at one school to second grade at another school and then starts teaching third grade in the new school two years later, she or he will enter the data in all the premove years and then again in the third postmove year.

schools, I can estimate equation (8), including school fixed effects. In such models, none of the effects can be driven by level differences across schools that affect all teachers equally (as this is absorbed by the school effect). As such, any within-teacher differences in performance associated with a move across schools must be due to a differential response to schools across teachers (i.e., match effects). While the inclusion of school fixed effects removes mean differences across schools, one may also worry that time-varying school characteristics affect both teacher performance and teacher mobility. For example, if a school experiences some negative shock in year $t - 1$, this may cause some teachers to have poor performance in year $t - 1$ and to leave the school in year $t$. To address this issue, I estimate models that include a year fixed effect for each school, so that comparisons are made among teachers at the same school in the same year, to control for any school-specific events that may affect both teacher effectiveness and teacher mobility. To map out teacher effectiveness over time while accounting for time-varying differences in outcomes across schools, I estimate equation (9) by OLS:

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \sum_{\tau=-10}^{9} I_{t=\tau} \cdot \pi_\tau + \theta_j + \theta_{s\times y} + \eta_{ijsy}. \tag{9}$$

All variables are defined as before, and $\theta_{s\times y}$ is a school-by-year fixed effect. Because this model includes both school-by-year and teacher fixed effects, this DID model compares a teacher's outcomes before and after a move, while accounting for average outcomes of the schools (in a particular year) to and from which she moved.

### A. *A Test for Endogenous Teacher Mobility*

In all papers on worker mobility, there is the concern that productivity is endogenous to worker mobility. Note that the case of teachers switching because their performance is poor is not endogenous productivity and is exactly the kind of mobility I aim to characterize. The concern would be whether productivity is endogenous to the switch. Specifically, the concern is that when teachers anticipate that they will leave their current job in one year, they may reduce their effort the year before the move to a new school. In such a scenario, one would observe that productivity is low before a move and thus wrongly infer that a teacher was leaving a low-productivity match. In this scenario, one would expect teacher effectiveness to be uncharacteristically low one or two years immediately prior to a switch. By the same argument, if teachers aim to impress those at their new school, they may exert more effect temporarily right after a move and seem more productive at their new school. In this scenario, one would expect teacher effectiveness to be uncharacteristically high one or two years immediately after a switch; one would also expect some systematic pattern in teacher effectiveness after the first year a teacher moves. Under these scenarios, there is some systematic pattern in teacher effectiveness prior to or after a move. As such, the finding that individual years before or after a move variables have no explanatory power over a simple pre- versus postmodel would suggest that effectiveness is exogenous to mobility.

### B. *Findings*

The "years until/since move" indicator variable coefficient estimates from equations (8) and (9) are presented in table 4 and visually in figure 1. The basic within-teacher results show that teachers perform better after a switch than before. Relative to the year before a move, all the postmove indicator variables have positive coefficients for both subjects, while the premove indicator variables are either negative or close to 0 and positive. This is indicative of the existence of match effects and shows that teachers move from schools where the productivity of the match between them and the school is low.[17] The point estimates suggest that a teacher increases test scores by about 2.5% and by 1.4% of a standard deviation more at her new school than at her old school in math and reading, respectively (columns 2 and 6).[18] Results that also include school fixed effects or school-by-year fixed effects are very similar, indicating that the estimated outcome differences before and after a move are not spuriously driven by teachers moving from low- to high-achievement schools or by schoolwide events that would affect both teacher mobility and teacher performance.

For both subjects, there is little evidence of endogenous teacher mobility even though teacher effectiveness is significantly different after a move than before. In the preferred models, for both subjects, one cannot reject the null hypothesis that all premove year effects are the same and that all postmove years are the same at the 20% level, while one rejects the hypothesis that premove performance and postmove performance are equal at the 1% level. This is consistent with the relatively uniform effectiveness before a move, the one-time upward shift in effectiveness after a move, and the relatively uniform effectiveness after a move, as depicted in figure 1.

### C. *Could Dynamic Student Selection Drive These Results?*

Readers may worry that sorting of students could drive the results if (a) students who are assigned to teachers who

[17] Results are similar in models that include smaller bin sizes and also models that control for teacher experience parametrically with a second-order polynomial. Moreover, if these estimates merely reflected an experience effect, then they would not be relatively larger in the same year of a move than all other years. There would be a greater concern for bias due to an experience effect if the model were simply a before-and-after model.

[18] Columns 1 and 5 show the results with test score growth as the dependent variable with teacher fixed effects for math and reading, respectively. The results are largely unchanged.

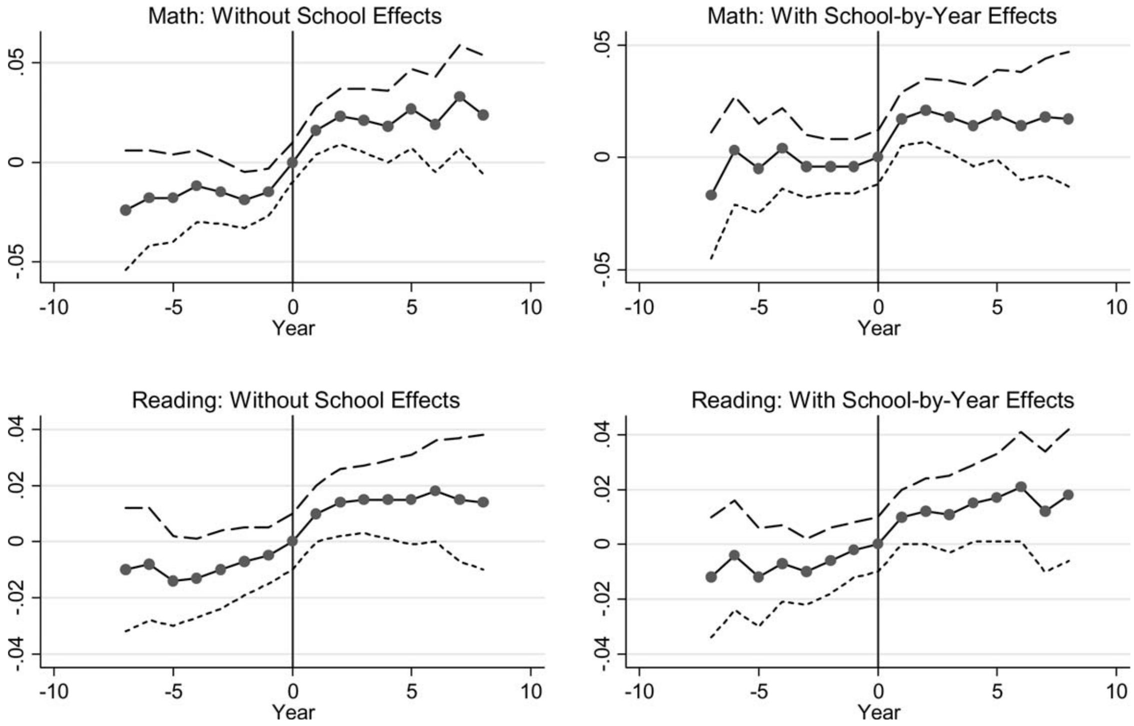
TABLE 4.—TEACHER EFFECTIVENESS BEFORE AND AFTER A MOVE

| | Math | | | | Reading | | | |
|---|---|---|---|---|---|---|---|---|
| | Growth | Score | Score | Score | Growth | Score | Score | Score |
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| 10 years before move | −0.059 | −0.049 | −0.048 | −0.013 | −0.069 | −0.053 | −0.059 | −0.042 |
| | [0.027]** | [0.025]* | [0.025]** | [0.023] | [0.027]** | [0.026]** | [0.026]** | [0.024]* |
| 9 years before move | −0.046 | −0.041 | −0.045 | 0.001 | −0.046 | −0.033 | −0.035 | −0.034 |
| | [0.019]** | [0.018]** | [0.018]** | [0.017] | [0.015]*** | [0.014]** | [0.014]** | [0.014]** |
| 8 years before move | −0.011 | −0.008 | −0.017 | −0.011 | −0.01 | 0.001 | −0.006 | −0.013 |
| | [0.016] | [0.015] | [0.015] | [0.014] | [0.012] | [0.011] | [0.011] | [0.011] |
| 7 years before move | −0.008 | −0.002 | −0.009 | 0.009 | −0.011 | 0.002 | −0.002 | −0.005 |
| | [0.013] | [0.012] | [0.012] | [0.012] | [0.010] | [0.010] | [0.010] | [0.010] |
| 6 years before move | −0.005 | −0.002 | −0.01 | 0.001 | −0.016 | −0.004 | −0.012 | −0.013 |
| | [0.012] | [0.011] | [0.011] | [0.010] | [0.009]* | [0.008] | [0.008] | [0.009] |
| 5 years before move | 0.002 | 0.004 | −0.002 | 0.01 | −0.014 | −0.003 | −0.009 | −0.008 |
| | [0.010] | [0.009] | [0.009] | [0.009] | [0.008]* | [0.007] | [0.007] | [0.007] |
| 4 years before move | −0.003 | 0.001 | −0.006 | 0.002 | −0.008 | 0.001 | −0.007 | −0.011 |
| | [0.009] | [0.008] | [0.008] | [0.007] | [0.007] | [0.007] | [0.007] | [0.006]* |
| 3 years before move | −0.007 | −0.003 | −0.007 | 0.002 | −0.005 | 0.003 | −0.002 | −0.007 |
| | [0.007] | [0.007] | [0.007] | [0.006] | [0.006] | [0.006] | [0.006] | [0.006] |
| 2 years before move | −0.002 | 0.001 | 0.001 | 0.002 | −0.001 | 0.005 | 0.002 | −0.003 |
| | [0.006] | [0.006] | [0.006] | [0.006] | [0.006] | [0.005] | [0.005] | [0.005] |
| Year of move (0) | 0.013 | 0.016 | 0.009 | 0.006 | 0.006 | 0.01 | 0.001 | −0.001 |
| | [0.006]** | [0.005]*** | [0.006] | [0.006] | [0.005] | [0.005]** | [0.005] | [0.005] |
| 1 year after move | 0.028 | 0.032 | 0.026 | 0.023 | 0.017 | 0.02 | 0.011 | 0.009 |
| | [0.007]*** | [0.006]*** | [0.006]*** | [0.006]*** | [0.005]*** | [0.005]*** | [0.005]** | [0.005]* |
| 2 years after move | 0.034 | 0.039 | 0.033 | 0.027 | 0.02 | 0.024 | 0.014 | 0.011 |
| | [0.007]*** | [0.007]*** | [0.007]*** | [0.007]*** | [0.006]*** | [0.006]*** | [0.006]** | [0.006]* |
| 3 years after move | 0.031 | 0.037 | 0.03 | 0.024 | 0.02 | 0.025 | 0.015 | 0.01 |
| | [0.008]*** | [0.008]*** | [0.008]*** | [0.008]*** | [0.007]*** | [0.006]*** | [0.007]** | [0.007] |
| 4 years after move | 0.028 | 0.034 | 0.028 | 0.02 | 0.022 | 0.025 | 0.015 | 0.014 |
| | [0.010]*** | [0.009]*** | [0.009]*** | [0.009]** | [0.008]*** | [0.007]*** | [0.007]** | [0.007]* |
| 5 years after move | 0.037 | 0.043 | 0.038 | 0.025 | 0.022 | 0.025 | 0.016 | 0.016 |
| | [0.011]*** | [0.010]*** | [0.011]*** | [0.010]** | [0.009]** | [0.008]*** | [0.008]* | [0.008]* |
| 6 years after move | 0.027 | 0.035 | 0.029 | 0.02 | 0.024 | 0.028 | 0.019 | 0.02 |
| | [0.013]** | [0.012]*** | [0.012]** | [0.012]* | [0.010]** | [0.009]*** | [0.010]** | [0.010]** |
| 7 years after move | 0.043 | 0.049 | 0.044 | 0.024 | 0.022 | 0.025 | 0.016 | 0.011 |
| | [0.014]*** | [0.013]*** | [0.014]*** | [0.013]* | [0.011]* | [0.011]** | [0.011] | [0.011] |
| 8 years after move | 0.033 | 0.04 | 0.034 | 0.023 | 0.02 | 0.024 | 0.014 | 0.017 |
| | [0.016]** | [0.015]*** | [0.015]** | [0.015] | [0.013] | [0.012]** | [0.012] | [0.012] |
| 9 years after move | 0.061 | 0.069 | 0.062 | 0.04 | 0.052 | 0.056 | 0.047 | 0.052 |
| | [0.020]*** | [0.018]*** | [0.019]*** | [0.018]** | [0.016]*** | [0.015]*** | [0.015]*** | [0.015]** |
| Lagged scores | - | 0.762 | 0.762 | 0.765 | - | 0.732 | 0.731 | 0.732 |
| | - | [0.002]*** | [0.002]*** | [0.002]*** | - | [0.002]*** | [0.002]*** | [0.002]*** |
| Teacher effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| School effects | No | No | Yes | Yes | No | No | Yes | Yes |
| School × Year Effects | No | No | Yes | Yes | No | No | Yes | Yes |
| Observations | 1,249,122 | 1,249,122 | 1,249,122 | 1,249,122 | 1,241,150 | 1,241,150 | 1,241,150 | 1,241,150 |
| Prob pre = 0 | 0.20 | 0.24 | 0.31 | 0.62 | 0.02 | 0.04 | 0.04 | 0.26 |
| Prob post = 0 | <0.01 | <0.01 | <0.01 | <0.01 | <0.01 | <0.01 | <0.01 | <0.01 |

Robust standard errors in brackets clustered at the teacher level. Significant at *10%, **5%, and ***1%. All models include grade and year fixed effects and controls for student race, gender, parental education, and limited English proficient status. Models also include an indicator for the gender and racial match between the student and the teacher, teacher experience, and the class size.

will leave the school the following year are systematically worse in unobserved dimensions than those who are not, or (b) students who are assigned to teachers at the teacher's new school are better in unobserved dimensions. The fact that there are no pre- or postmove trends in outcomes is prima facie evidence that this is not driving the results. However, to test for student sorting in unobserved dimensions directly, one can determine whether (a) students in year $y$ who will receive a teacher in year $y+1$ who will leave the school between years $y+1$ and $y+2$ have worse outcomes than those who will not, and (b) students in year $y$ who will have a teacher in year $y+1$ who transferred from another school between years $y$ and $y+1$ have better outcomes than those who do not. To do this, I estimate a model similar to equation (9), but add indicators for the mobility status of a student's future teacher (the year $y+1$ mobility status of a student's time $y+1$ teacher). The coefficient on the variable denoting whether the student's teacher next year will leave the following year is 0.001 ($p$-value $=$ 0.74), and the coefficient on a variable denoting whether the student's teacher next year will be a new transfer from another school is 0.002 ($p$-value $= 0.26$)—both close to 0. As such, it appears that the improved outcomes observed after a teacher switches schools are not an artifact of dynamic student sorting and likely reflect teachers' schools with which they are poorly matched.

FIGURE 1.—CHANGE IN TEACHER MATH VALUE-ADDED BEFORE AND AFTER A MOVE AND THE 95% CONFIDENCE INTERVAL




## V. Estimating Match Effects and Estimating the Importance of Match Effects

As discussed in section IIC, in the ideal empirical setup, after accounting for possible student selection, the mean of all the matches for teacher $j$ (across schools) would be a consistent estimate of the teacher effect $\theta_j$, the mean of all the matches for school $j$ (across teachers) would be a consistent estimate of the school effect $\theta_s$, and the mean for teacher $j$ at schools $s$ would be a consistent estimate of the match effect $\theta_{js}$. However, because teachers are typically observed at only a few schools, it is unlikely that match effects average out to 0 for each teacher. This will lead to small sample bias in the teacher, school, and match effect estimates. In this section, I present a fixed-effects strategy and detail this small sample bias. I then present an empirical Bayes random-effects strategy that does not rely on large sample properties.

### A. *Orthogonal Match Fixed Effects*

The first approach is to estimate orthogonal match fixed effects. To do this, I estimate equation (10), a model with school fixed effects and teacher fixed effects, and define the match effect for each teacher-school pair, $\bar{e}_{js}$, as the mean value of the residual for teacher $j$ at school $s$:

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \theta_j + \theta_s + \eta_{ijsy}. \tag{10}$$

Because match effects are computed from residuals, the estimated match effects are orthogonal to teacher and school effects, the mean match quality for each teacher is equal to 0, and the mean match quality for each school is equal to 0 by construction.

This approach, while straightforward, has the undesirable feature that it mechanically loads match quality that is correlated with the teacher or school effects (in small samples) on to the teacher or school effects, respectively. This makes it impossible to determine how much of what we estimate as a teacher effect may in fact be a match effect, and it overstates the importance of teacher and school effects while understating the importance of match effects. However, while their magnitude may be understated, orthogonal match effects do have the intuitive interpretation of being the observed within-teacher variation in performance that can be attributed to working in different school environments for mobile teachers. I detail how to uncover estimates of the variance of true teacher, school, and match quality in a fixed-effects framework below.

### B. *Fixed Effects-Based Estimates of the True Variance*

Because the raw fixed effects estimates are estimated with noise, the variance of the estimated effects will not accurately reflect the variance of true teacher, school, and match quality. As such, I estimate a series of covariances across classrooms to remove the contribution of idiosyncratic year-specific shocks (such as a dog barking on the day of the test) and random sampling variation to uncover the true variability of teacher, school, and match effects. This is done in two steps. First, I estimate an achievement model like equation (11) with teacher-by-school-by-year fixed effects (that is, classroom fixed effects):

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \theta_{jsy} + \eta_{ijsy}. \quad (11)$$

The classroom fixed-effect term $\theta_{jsy}$ contains a piece attributed to the school, the teacher, the match, and idiosyncratic classroom-specific shocks, so that $\theta_{jsy} \equiv \theta_s + \theta_j + \theta_{js} + \lambda_{jsy}$. Under the assumption that the idiosyncratic shocks, the teacher, the school, and the match effect are all uncorrelated in the data, one can estimate the variance of the teacher, school, and match effects with a series of covariances. Specifically, the covariance of classroom effects one year apart within teachers and across schools is the variance of the persistent teacher component common across schooling environments, $\mathrm{Cov}(\theta_{jsy}, \theta_{js'y+1}) \equiv \sigma_j^2$. Similarly, the covariance of classroom effects one year apart across teachers and within schools is the variance of the persistent school component common to all teachers, $\mathrm{Cov}(\theta_{jsy}, \theta_{j'sy+1}) \equiv \sigma_s^2$. Finally, the covariance of classroom effects one year apart within teachers and within schools is the variance of the persistent teacher component, the common school component, and the component specific to the match between teachers and schools, $\mathrm{Cov}(\theta_{jsy}, \theta_{jsy+1}) \equiv \sigma_s^2 + \sigma_j^2 + \sigma_{js}^2$. As such, one can obtain an estimate of the variance of match effects from $\mathrm{Cov}(\theta_{jsy}, \theta_{jsy+1}) - \mathrm{Cov}(\theta_{jsy}, \theta_{js'y+1}) - \mathrm{Cov}(\theta_{jsy}, \theta_{j'sy+1}) \equiv \sigma_{js}^2$. For comparison purposes, I present naive estimates of the variance of teacher effects under the assumption of no match effects.[19] Because these estimates assume that match quality and teacher quality are uncorrelated in the data, these estimates understate the importance of matches and may overstate that of teachers.[20] This motivates my use of a maximum likelihood approach.

### C. *Maximum Likelihood Random Match Effects*

The second approach is to estimate random match effects. For this, one estimates teacher, school, and teacher-by-school effects simultaneously using a mixed-effects estimator. This is done in two steps. First, I estimate a model like equation (12) with teacher-by-school fixed effects $\mu_{js}$:

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \mu_{js} + \eta_{ijsy}. \quad (12)$$

Then I take the combined error term $\mu_{js} + \eta_{ijsy}$ (the teacher-by-school effect and the idiosyncratic error term) and estimate a random-effects model to decompose the combined residual into a school effect, a teacher effect, a teacher-by-school effect, and a transitory classroom effect $\lambda_{jsy}$. Accounting for teacher-school-year (classroom) effects is important insofar as one does not observe teachers for many years at a particular school. In the extreme, if one observes a teacher for one year at a school, it is impossible to disentangle match effects from idiosyncratic classroom-specific shocks unless one accounts for this source of variability.

The random-effects estimator estimates the variances of the teacher, school, match, and classroom effects by maximum likelihood under the assumptions of joint normality with the covariance structure described in equation (13), that the random effects are uncorrelated with the covariates conditional on the estimated first-stage coefficients described in equation (14), and the fixed effects identifying assumption that the error term $\eta_{ijsy}$ are uncorrelated with the random effects:

$$\mathrm{Cov}\begin{bmatrix} \theta_s \\ \theta_j \\ \theta_{js} \\ \lambda_{jsy} \end{bmatrix} = \begin{bmatrix} \sigma^2_{\theta s} I_S & 0 & 0 & 0 \\ 0 & \sigma^2_{\theta j} I_J & 0 & 0 \\ 0 & 0 & \sigma^2_{\theta js} I_M & 0 \\ 0 & 0 & 0 & \sigma^2_{\lambda jsy} I_C \end{bmatrix}. \quad (13)$$

$$\begin{aligned} E[\theta_s|\hat{\gamma}, \hat{\alpha}, T, X] &= E[\theta_j|\hat{\gamma}, \hat{\alpha}, T, X] = E[\theta_{js}|\hat{\gamma}, \hat{\alpha}, T, X] \\ &= E[\theta_{jst}|\hat{\gamma}, \hat{\alpha}, T, X] = 0. \end{aligned} \quad (14)$$

Similar to the orthogonal match fixed-effects approach, teacher, school, and match effects are identified largely by mobile teachers. However, unlike the orthogonal match approach, which assumes that the actual match, school, and teacher effects observed in the data are uncorrelated, the random match effects approach assumes that the potential match, school, and teacher effects (that one would observe if all teachers were observed at all schools) are uncorrelated but that the actual match, school, and teacher effects observed in small samples (where teachers are observed at few schools) can be correlated. This model apportions variation between teacher, school, match, and classroom effects to minimize mean squared error. I describe the mechanics below.

### D. *Intuition for the Mechanics of the Orthogonal and Mixed Match Effects Estimators*

If all teachers were observed at all schools, then one could use a fixed-effects estimator to precisely estimate school, teacher, and match effects because the orthogonality condition in equation (13) would be satisfied in the data, and there would be enough variation to cleanly identify good matches from good teachers and good schools. However, this is not the case in the real world. Generally we

[19] Specifically, if there were no match effects, then the covariance across classrooms for the same teacher at the same school will reflect only the teacher effect and the school effect, so that $\sigma_j^{naive} = \mathrm{Cov}(\theta_{jsy}, \theta_{jsy+1}) - \mathrm{Cov}(\theta_{jsy}, \theta_{j'sy+1})$.

[20] For example, if a teacher has two good matches, then this covariance approach will attribute covariance across schools to the persistent teacher effect, when in fact the matches are positively correlated in the sample. With a large number of matches per teacher, this would not pose a problem. However, in small samples, this leads to biased estimates.

observe several teachers at the same school so that estimation of school effects is not problematic. However, most teachers are not observed in most schools, and many teachers are observed in only a small number of schools, leading to uncertainty regarding how much of the variation to attribute to teachers or matches. The difference between the orthogonal match fixed effects and the maximum likelihood random match effects is best illustrated by showing an example of how these two estimators handle this uncertainty.

Consider a teacher who is observed with two matches, both of which are positive and large. This could be because (a) the teacher has a very large positive teacher effect, (b) the teacher was very lucky and drew two very large positive matches, or (c) the teacher drew a large teacher effect and two positive match effects (but none of the draws are very large). With only two observed matches for this teacher, there is no way to know for certain which state of the world generated the observed data. I detail how each of the estimators deals with uncertainty associated with this scenario to provide intuition for how and why these estimates differ and why the random maximum likelihood estimates of match effects are desirable.

The orthogonal match fixed-effect model mechanically imposes the condition that the mean of the match effects is 0 for each teacher. This precludes the possibility of two positive matches ruling out situations b and c. That is, the orthogonal fixed-effects model assumes situation a and attributes the average of the match effects to teachers. Unless teachers are observed in many schools (so that the mean 0 match quality assumption holds for each teacher), estimated effects will be biased, the importance of match effects will be understated, and the importance of teachers will be overstated in orthogonal match fixed effects models.

The random match effects model differs from the orthogonal match effects model in that the estimator distributes the excess variation to both the teacher and match effects in a way that minimizes mean squared error (rather than loading it all on the teacher). The larger(smaller) is the estimated variance of the teacher effects relative to the variance of match effects and the greater(less) its relative precision is, the more(less) of the excess variation is attributed to the teacher effect. More generally, excess variability is distributed among the effects in proportion to their estimated variance and the precision with which those variances are estimated. The intuition for this can be illustrated by how it is applied to the above scenario.

Consider again the teacher observed with two large positive matches. This could be because of situations a, b, or c. With only two observed matches for this teacher, there is no way to know for certain which state of the world generated the observed data. However, if the variance of teacher effects is large relative to the variance of match effects, then it is more likely that this person drew a very large teacher effect than two very large match effects, and therefore the model will attribute more of the excess variation to the teacher effect. Conversely, if the variance of match effects is large relative to the variance of teacher effects, then it is more likely that this person drew two very large match effects rather than a very large teacher effect, and therefore the model will attribute more of the excess variation to the match effects. This example illustrates how the mixed-effects estimator uses distributional information (from the mobile teachers) to create the best linear unbiased predictions (BLUPS) of the teacher and match effects (rather than mechanically attributing the excess variation to teachers, as in the orthogonal match model). The resulting random effect estimates are empirical Bayes.

Both models identify match effects from the variability in teacher performance across schools (for mobile teachers), but they differ in how they resolve uncertainty about the sources of variability in the data. The orthogonal match fixed effects are the estimates obtained when one ascribes any uncertain variation to the teacher, while the random-effects estimates are what one obtains when the model apportions some of the variability to the match and some to the teacher in a way that is most consistent with the distributional assumptions of the model. In principle, both models should yield consistent estimates in large samples; however, in small samples (as is the case in the real world), the fixed-effects estimates will be biased. While these two methods differ in their approach, the main conclusion of the paper does hinge on how one estimates school, teacher, and match effects.

### E. *Estimated Variability of Match Effects*

In table 5, I present the standard deviations of the raw fixed effects and the covariance-based and maximum likelihood–based estimates of the standard deviations of the teacher, school, and match effects. The units are in standard deviations of student achievement.

The standard deviations of the raw school, teacher, and match fixed effects for math are 0.22, 0.35, and 0.11, respectively. For reading, the standard deviations of the raw school, teacher, and match fixed effects are 0.247, 0.356, and 0.118, respectively.[21] While these variances are inflated due to estimation error and idiosyncratic classroom-level shocks, if one were to take the estimates at face value, one would conclude that match quality is about half as important as school quality and one-third as important as teacher quality.

The third and fourth columns present the estimates of the true variability of teacher, school, and match effects based on covariance across classrooms to remove estimation error and idiosyncratic classroom-level errors. In models that assume that match effects are equal to 0, the estimated stan-

[21] It is important to note that the results are similar but not identical to the inclusion of match fixed effects because in the first stage of the orthogonal match fixed-effects estimator, match fixed effects are included. Where orthogonal match effects are not estimated, only teacher and school fixed effects are included in the first stage.

TABLE 5.—ESTIMATED VARIABILITY OF EFFECTS

| | Raw Fixed Effects | | Covariance Estimates[a] | | Random Effects | | |
|---|---|---|---|---|---|---|---|
| Math | | | | | | | |
| SD of School Effects | 0.2285 | 0.2285 | 0.0882 | 0.0882 | 0.106 | 0.099 | 0.098 |
| SD of teacher effects | 0.3503 | 0.3506 | 0.1667[b] | 0.1498 | 0.19 | 0.141 | 0.142 |
| SD of match effects | - | 0.1121 | - | 0.0892 | - | 0.1302 | 0.0953 |
| SD of classroom effects | - | - | - | - | - | - | 0.141 |
| SD of residuals | 0.5023 | 0.50704 | - | - | 0.50895 | 0.5076 | 0.4942 |
| Reading | | | | | | | |
| SD of school effects | 0.2475 | 0.2472 | 0.0504 | 0.0504 | 0.0926 | 0.06547 | 0.0648 |
| SD of teacher effects | 0.3563 | 0.3564 | 0.1095[b] | 0.05695 | 0.1107 | 0.08377 | 0.08384 |
| SD of match effects | - | 0.1182 | - | 0.08785 | - | 0.0777 | 0.05967 |
| SD of classroom effects | - | - | - | - | - | - | 0.09304 |
| SD of residuals | 0.5481 | 0.5467 | - | - | 0.61125 | 0.5553 | 0.5499 |

The fixed effects and covariance estimates are computed under the assumption that teacher and match effects are not correlated in the sample. Alternately, the random-effects model allows correlations between estimated school, teacher, and match effects in small samples.

[a]The variance of the school effect is computed as the covariance between the classroom effects across different teachers from the same school. The variance of the teacher effect is computed as the covariance between the classroom effects across schools from the same teacher. Finally, the variance of the match effects is computed as the covariance between the classroom effect within the same teachers at the same school minus the estimated variance of the school and teacher effects.

[b]The naive variance of the teacher effect is computed as the covariance between the classroom effects within schools for the same teacher minus the estimated school variance.

dard deviation of teacher math quality is 0.1667 and that of schools is 0.0882. These estimates are very similar to shrinkage estimates of teacher and school effects in other studies and are consistent with approximately 40% of estimated effectiveness being persistent (Staiger & Rockoff, 2010; Goldhaber & Hansen, 2010). The similarity to other studies should assuage concerns that the variability of teacher quality among mobile teachers (for whom these correlations are computed) is different from that for all teachers.

For math, in models that allow for orthogonal match effects, the estimated standard deviation of teacher math quality is 0.1498 and that of schools is unchanged. The estimated standard deviation of match quality in math is 0.0892. These results suggest that approximately 10% of what we typically call a teacher effect in math is actually a match effect and that the explanatory power of match quality in math is about 60% of that of teacher quality. For reading, in models that assume match effects are equal to 0, the estimated standard deviation of teacher quality in reading is 0.1095 and that of schools is 0.0504. In models that allow orthogonal match effects, the estimated standard deviation of teacher quality in reading falls to 0.0569. The estimated standard deviation of reading match effects is 0.0878, suggesting that about half of what we typically call a teacher effect in reading is actually a match effect, and the explanatory power of match quality in reading is greater than that of teacher quality.

The fifth, sixth, and seventh columns present maximum likelihood estimates. The mixed-effects estimator suggests that with no match or classroom effects, the standard deviations of teacher quality for math and reading are 0.19 and 0.11, respectively, and that the standard deviations of school quality for math and reading are 0.106 and 0.0926, respectively. These estimates are similar to the covariance-based estimates, supporting the variability estimates and underscoring the importance of accounting for estimation error. In the mixed-effects model that allows for match effects only (that can be correlated with school or teacher effects), the standard deviation of school effects falls to 0.099 for math and 0.0655 for reading; this indicates that match quality can explain away about 7% and 30% of school effects in math and reading, respectively. Where match effects are included, the standard deviation of teacher effects falls to 0.141 for math and 0.0837 for reading, which suggests that match quality explains away approximately 25% of teacher effects in both subjects. Because one may confound match effects with classroom effects in small samples, the estimated standard deviations of match effects of 0.13 for math and 0.077 for reading are likely inflated. In the maximum likelihood model that accounts for classroom-level effects, the standard deviation of match effects is 0.0953 for math and 0.0597 for reading. As such, match effects have about two-thirds of the explanatory power of teacher effects and are economically important.

One may worry that these calculations may overstate the relative importance of match effects because the variation in teacher quality is measured within schools. If there is substantial sorting of teachers across schools, the variability in teacher quality within schools may understate the importance of teachers. To assess the degree to which this might be true, I compare the standard deviation of teacher effects in models that do and do not include school effects. Starting with the naive model, the standard deviation of estimated raw teacher fixed effects without the inclusion of school or match fixed effects is 0.321 and 0.327 for math and reading, respectively. This is almost identical to the variability within schools. Similarly, in the maximum likelihood model that does not include school effects, the estimated standard deviations are 0.211 and 0.1294 for math and reading, respectively. Again, this is almost identical to the estimated variability within schools (without match effects). Furthermore, this is consistent with Rivkin et al. (2005) and Aaronson et al. (2007), who show that the within-school variation in value-added is as large as between-school variation, suggesting the estimated variability of teacher quality based on within-school comparisons is not understated, so the relative importance of match effects is likely not overstated.

## VI. Does Match Quality Predict Teacher Mobility?

The remaining empirical predictions from the theoretical framework presented in section IID were that (a) match-specific quality should be negatively associated with switching, (b) match-specific quality should be largely unrelated to exiting the profession, (c) general teacher quality should be negatively correlated with exiting the profession, and (d) general teacher quality should be unrelated to switching within teaching. To test these empirical predictions, I merge both the preferred estimated random estimates (the BLUPs) and the estimated orthogonal fixed effects with teacher-level mobility data and determine whether teacher mobility (switching schools or exiting the data) is associated with teacher quality (occupation-specific ability) and match quality (firm-specific ability) at her current school. Specifically, I estimate equation (15) by logistic regression:

$$Pr(Y_{jsy+1}) = \frac{1}{1 + e^{-(\alpha_1 X_{jy} + \alpha_2 X_{sy} + \pi_1 \overline{\theta}_j + \pi_2 \overline{\theta}_{js})}}. \tag{15}$$

$Y_{jsy+1}$ is an indicator variable equal to 1 if the teacher switches from her current school in year $y$ (teacher $j$ at school $s$ at time $y$ is not at school $s$ at time $y + 1$, but is at another school teaching any grade in the NC public school system in year $y + 1$) or exits the North Carolina public school system entirely; $X_{jy}$ is a set of time-varying teacher-level covariates; $X_{sy}$ is a set of time-varying school-level covariates; and $\theta_j$ and $\theta_{js}$ are standardized estimated teacher and match effects.

To allow for comparisons conditional on the teacher identity or school identity and assuage concerns that the relationships are due to high-mobility teachers or schools having bad matches on average, I also estimate conditional logistic regressions that condition on either the teacher or the school. Finally, because allowing for both unobserved teacher- and school-level heterogeneity is not tractable in a nonlinear model, I estimate a linear probability model with both teacher and school fixed effects. Specifically, I estimate equation (16) by OLS:

$$Switch_{jsy+1} = \alpha_1 X_{jy} + \alpha_2 X_{sy} + \pi_3 \overline{\theta}_{js} + \pi_j + \pi_s + \varepsilon_{jsy}, \tag{16}$$

where $\pi_s$ and $\pi_j$ are teacher and school fixed effects, respectively, and $\varepsilon_{jsy}$ is the idiosyncratic error term. This model tests whether a given mobile teacher is more or less likely to remain in her current school where the estimated match quality is higher, taking into account that certain schools may have high(low) mobility and high(low) match quality on average. The results of these models are presented in table 6. For the logistic regressions, I present the odds ratios (less than 1 means less likely to exit or switch and more than 1 means more likely to exit or switch) and the $p$-value associated with the null hypothesis that the estimates odds ratio is equal to 1 (no change in likelihood). For the linear probability models, I present the marginal effects and compute the implied odds ratio based on the mean of the dependent variable for comparison. I present results only for math, although they are similar for reading.

The results in columns 1 and 2 are consistent with the theory. That is, increasing teacher quality (a transferable skill within teaching) by $1\sigma$ decreases the likelihood of exiting the teaching profession by 12% but is unrelated to switching schools. This is consistent with teachers leaving the profession for other professions with which they may have a better match.[22] Furthermore, while increasing match quality (a school-specific skill) by $1\sigma$ decreases the likelihood of exiting the profession by only 6%, it decreases the likelihood of switching by 33%. While the previous literature has not made the distinction between teacher effectiveness due to school-specific versus general skills, these results show that effectiveness due to school-specific skills is associated with staying at the school, while effectiveness due to general teaching ability is associated with staying in the profession.

Columns 3 and 4 include school and teacher characteristics, which have little impact on the relationships between match and teacher quality and mobility. The observable teacher and school characteristics predict mobility, as one would expect; higher salaries are associated with both decreased switching and decreased exiting (with stronger effects on exiting), the percentage of black students at the school is associated with increased switching and decreased exiting (with larger effects on switching), and higher math achievement at the school is associated with less switching and exiting. While exiting the profession increases monotonically with experience, switching schools decreases monotonically with experience. This is consistent with teachers being more likely to retire as they age and with teachers settling in on a good match as they age. Higher licensure scores and having an advanced degree (indicative of general cognitive skills that may be transferable to other occupations and schools) are associated with increased exiting and switching. Having regular licensure (which indicates attachment to teaching and is a marketable trait for other schools) is associated with 53% less exiting but 67% more switching.

These general relationships persist in models that condition on the individual teacher (columns 5 and 6) or those that condition on the school (columns 7 and 8). The linear models that condition on both teacher and school (columns 9 and 10) show that increasing match quality by $1\sigma$ has no statistically significant effect on exiting the profession but is associated with being 2.7 percentage points ($p$-value =

[22] These finding are consistent with Loeb, Kalogrides, and Béteille (2011), who find that more effective schools are able to attract and hire more effective teachers from other schools when vacancies arise.

TABLE 6.—MATCH QUALITY AND TEACHER MOBILITY

| | Exit Teaching | Switch Schools | Exit Teaching | Switch Schools | Exit Teaching | Switch Schools | Exit Teaching | Switch Schools | Exit Teaching | Switch Schools | Exit Teaching | Switch Schools |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Logistic | | | | Conditional Logit | | | | OLS | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Match effect math BLUP | 0.942*** | 0.669*** | 0.942*** | 0.681*** | 0.935*** | 0.851*** | 0.946*** | 0.725*** | −0.0090 | −0.027*** | | |
| | (0.000) | 0.000 | (0.000) | 0.000 | (0.091) | (0.000) | (0.002) | 0.000 | (0.075) | (0.000) | | |
| Teacher effect math BLUP | 0.880*** | 1.102* | 0.906*** | 1.004 | | | 0.895*** | 1.001 | | | | |
| | 0.000 | (0.09) | (0.000) | (0.14) | | | (0.000) | (0.285) | | | | |
| School effect math BLUP | 0.964*** | 0.884*** | 1.032*** | 0.947*** | 0.9360 | 0.873*** | | | | | | |
| | (0.000) | 0.000 | (0.008) | (0.013) | (0.269) | (0.003) | | | | | | |
| Match effect math FE | | | | | | | | | | | −0.0013 | −0.0104*** |
| | | | | | | | | | | | (0.582) | (0.000) |
| Log(Salary) | | | 0.3135 | 0.6139 | 0.2639 | 0.6557 | 0.3003 | 0.6518 | −0.2820 | −0.0124 | −0.2790 | −0.0147 |
| | | | (0.000) | (0.000) | (0.000) | (0.120) | 0.000 | (0.011) | (0.000) | (0.432) | (0.000) | (0.355) |
| % Free lunch | | | 1.0763 | 0.9347 | 1.1241 | 1.9079 | 1.1549 | 1.1052 | 0.0270 | 0.0079 | 0.0268 | 0.0076 |
| | | | (0.245) | (0.591) | (0.300) | (0.001) | (0.110) | (0.681) | (0.220) | (0.437) | (0.224) | (0.457) |
| % black | | | 1.1445 | 2.2524 | 1.8908 | 3.0526 | 1.3152 | 11.7870 | 0.1140 | 0.1150 | 0.1090 | 0.1150 |
| | | | (0.012) | 0.000 | (0.012) | (0.000) | (0.261) | (0.000) | (0.102) | (0.002) | (0.121) | (0.002) |
| Log(enrollment) | | | 0.9911 | 0.9472 | 1.1806 | 0.9593 | 1.1309 | 5.1500 | 0.0435 | 0.0282 | 0.0423 | 0.0277 |
| | | | (0.729) | (0.319) | (0.093) | (0.644) | (0.133) | (0.000) | (0.045) | (0.009) | (0.050) | (0.010) |
| Mean math scores at school | | | 0.7922 | 0.7827 | 0.8057 | 0.7161 | 0.8049 | 0.9191 | −0.0425 | −0.0062 | −0.0439 | −0.0070 |
| | | | (0.000) | (0.001) | (0.011) | (0.006) | (0.000) | (0.604) | (0.003) | (0.342) | (0.002) | (0.282) |
| 1–3 years' experience | | | 1.1688 | 0.9892 | 3.9196 | 0.8033 | 1.2105 | 1.0260 | 0.2290 | −0.0026 | 0.2280 | −0.0027 |
| | | | (0.000) | (0.887) | 0.000 | (0.020) | (0.000) | (0.743) | (0.000) | (0.692) | (0.000) | (0.680) |
| 4–9 years' experience | | | 1.2461 | 0.9065 | 5.4739 | 0.7945 | 1.3284 | 0.9818 | 0.2900 | −0.0009 | 0.2890 | −0.0010 |
| | | | (0.000) | (0.244) | 0.000 | (0.086) | (0.000) | (0.838) | (0.000) | (0.916) | (0.000) | (0.903) |
| 10–25 years' experience | | | 1.1468 | 0.7182 | 3.7886 | 0.9195 | 1.2700 | 0.8187 | 0.2600 | 0.0120 | 0.2590 | 0.0117 |
| | | | (0.011) | (0.000) | 0.000 | (0.673) | (0.000) | (0.044) | (0.000) | (0.241) | (0.000) | (0.255) |
| 25 or more years' experience | | | 1.8626 | 0.4471 | 3.1582 | 0.9926 | 2.1085 | 0.5066 | 0.2170 | 0.0115 | 0.2170 | 0.0110 |
| | | | (0.000) | (0.000) | (0.000) | (0.978) | 0.000 | (0.000) | (0.000) | (0.337) | (0.000) | (0.358) |
| Licensure score | | | 1.0278 | 1.0514 | 0.9707 | 1.7437 | 1.0152 | 1.0324 | −0.0098 | 0.0167 | −0.0104 | 0.0178 |
| | | | (0.028) | (0.035) | (0.891) | (0.184) | (0.249) | (0.217) | (0.789) | (0.326) | (0.778) | (0.301) |
| Advanced degree | | | 1.2423 | 1.1264 | 0.8816 | 1.3840 | 1.2386 | 1.1411 | −0.0244 | 0.0072 | −0.0216 | 0.0075 |
| | | | (0.000) | (0.030) | (0.287) | (0.142) | 0.000 | (0.023) | (0.206) | (0.382) | (0.262) | (0.366) |
| Regular licensure | | | 0.4644 | 1.6753 | 2.2255 | 0.6676 | 0.4700 | 1.7986 | 0.0828 | −0.0061 | 0.0815 | −0.0066 |
| | | | (0.000) | (0.000) | (0.000) | (0.127) | (0.000) | (0.000) | (0.000) | (0.416) | (0.000) | (0.379) |
| Odds ratio for match | 0.942 | 0.670 | 0.942 | 0.682 | 0.935 | 0.851 | 0.947 | 0.725 | 0.971 | 0.346 | 0.996 | 0.746 |
| Teacher effects | N | N | N | N | Y | Y | N | N | Y | Y | Y | Y |
| School effects | N | N | N | N | N | N | Y | Y | Y | Y | Y | Y |
| Year effects | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Observations | 74,676 | 74,676 | 74,154 | 74,154 | 44,215 | 14,142 | 74,032 | 59,326 | 74,154 | 74,154 | 74,008 | 74,008 |

Robust p-value in parentheses. Asterisks added to indicate statistical significance for match school and teacher effect variables only. Significant at *10%, **5%, and ***1%.

0.000) less likely to switch schools (an implied 66% reduction). This is what one should observe when the estimated match effects capture a school-specific factor that is not transferable across schools but is valuable in schools. To assuage concerns that these patterns are driven by the particulars of the random-effects estimates, columns 11 and 12 show the preferred mobility regressions replacing the random match effects with the estimated orthogonal fixed match effects. The results, though less precise, are similar.

In sum, consistent with classic models of match quality and mobility, teachers (workers) are less likely to leave their current school when match quality is high and no more likely to leave the profession. In contrast, teachers with high general teaching ability are more likely to remain in the profession and no more likely to switch schools. The patterns of exiting and switching are consistent with prior findings that teachers who exit from inner-city schools are the least effective, indicating that such patterns are due to teacher-school and teacher-profession match quality and are consistent with standard models of job search. Importantly, these relationships persist conditional on teacher salary, so match quality affects teacher mobility for reasons unrelated to pay. This suggests that nonpecuniary job aspects (such as working conditions, job satisfaction, or in-kind benefits) that are correlated with match effects exert a significant impact on employee mobility decisions.

### A. *Correlates of Match Quality*

While match quality is a teacher-school concept, if teachers leave bad matches and are more likely to have outside options if they are more desirable to other schools, then one might expect to see certain patterns in the data. One might expect that (a) teachers with more years of experience (who have been able to shop for a good match) will on average have better matches, (b) characteristics that make a teacher more employable should be associated with better matches (more able to be employed at a school with high match quality), and (c) larger schools and schools in more densely populated areas should have better matches (because larger markets and schools allow for greater specialization, increasing the likelihood of a good match).

To test for such patterns, I regress the standard normalized match random effects on the observable teacher and school characteristics included in equation (8).[23] Table 7 presents the observable correlates of match quality. Columns 1 and 2 show that teachers with more years of experience have higher match quality in both math and reading. Note that teacher experience is already accounted for when estimating match effects. To assess whether this relationship reflects a composition effect or teachers moving to schools with higher match quality over time, I estimate this relationship with the inclusion of teacher fixed effects (columns 3 and 4). This within-teacher estimate documents the relationship between match quality and experience among mobile teachers who switch schools over time. While the within-teacher relationship is smaller in magnitude, there is a clear positive monotonic relationship between experience and match quality within teachers. This is consistent with the pre- and post-comparisons depicted in figure 1 and the mobility patterns documented in table 6 and is indicative of teachers moving from schools with lower-quality matches and remaining in schools with higher-quality matches.

Columns 1 and 2 show that certified teachers, teachers with regular licensure, and teachers with higher scores on their license exams have better matches in both math and reading. Furthermore, white teachers have higher match quality in math than other teachers do. These results imply that at least part of the reason more experienced teachers, teachers who have a regular license, certified teachers, and white teachers may be associated with better student outcomes is the fact that such teachers have traits that are desirable to employers, and as a result, they are better able to search for higher match quality (as opposed to these traits being productive per se). Relative to schools in large cities, average match quality is lower in both math and reading in small towns, midsized cities, and rural areas. Match quality is positively associated with school size for both math and reading (possibly due to greater scope for classroom specialization). These patterns are consistent with the job search model, such that match quality is higher in geographic areas with thicker markets and schools with greater scope for specialization. Columns 5 through 8 show the same models with the raw fixed effects. While patterns are less pronounced and less precise (as one would expect), the patterns are largely similar.

### B. *Do Certain Kinds of Teachers Perform Better at Certain Kinds of Schools?*

To better understand these match effects, it is helpful to assess what types of teacher-school combinations are associated with better or worse outcomes. In principle, one could run a value-added model with both teacher and school fixed effects while including each teacher variable interacted with each school variable. Because this would result in a regression with hundreds of variables, this approach is impractical. I therefore employ a factor analytical approach that aggregates all the variables into a few manageable factors that categorize teachers and schools into types (table A1). I then interact the teacher and school factors to determine whether certain teacher types have better outcomes with certain school types.[24]

To create factors for teachers, I included teachers' value-added in math and reading, their certification status, whether they were fully licensed, their average score on

[23] These are simple regressions of the form $\overline{\theta}_{js} = \rho_1 X_j + \rho_2 X_s + \varepsilon_{jsy}$.

[24] This approach has been used by economists to study teaching practices associated with student outcomes (Lavy, 2011) and the relationship between teacher traits and student outcomes (Rockoff et al., 2011).

TABLE 7.—THE CORRELATES OF MATCH QUALITY

| | Standardized BLUPS | | | | Standardized Raw Fixed Effects | | | |
|---|---|---|---|---|---|---|---|---|
| | Math | Reading | Math | Reading | Math | Reading | Math | Reading |
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |
| Teacher: 1–3 years experience | 0.059 | 0.022 | 0.043 | 0.015 | 0.031 | 0.025 | 0.052 | 0.052 |
| | [0.012]*** | [0.011]** | [0.012]*** | [0.010] | [0.017]* | [0.017] | [0.036] | [0.031]* |
| Teacher: 4–10 years experience | 0.094 | 0.108 | 0.061 | 0.037 | 0.035 | 0.028 | 0.056 | 0.077 |
| | [0.017]*** | [0.016]*** | [0.020]*** | [0.018]† | [0.016]† | [0.018] | [0.044] | [0.039]† |
| Teacher: 10–25 years experience | 0.142 | 0.247 | 0.081 | 0.047 | 0.028 | 0.03 | 0.065 | 0.122 |
| | [0.019]*** | [0.018]*** | [0.024]*** | [0.022]† | [0.016]* | [0.018]* | [0.028]† | [0.046]*** |
| Teacher: 25 or more years experience | 0.254 | 0.388 | 0.088 | 0.049 | 0.048 | 0.031 | 0.084 | 0.132 |
| | [0.023]*** | [0.023]*** | [0.028]*** | [0.024]** | [0.017]*** | [0.018]* | [0.050]* | [0.049]*** |
| Teacher: White | 0.201 | 0.077 | - | - | −0.066 | −0.057 | - | - |
| | [0.073]*** | [0.081] | - | - | [0.038]+ | [0.035] | - | - |
| Teacher: Black | −0.004 | −0.048 | - | - | −0.059 | −0.055 | - | - |
| | [0.076] | [0.085] | - | - | [0.039] | [0.036] | - | - |
| Teacher: Certified | 0.172 | 0.135 | 0.019 | 0.014 | 0.021 | 0.025 | 0.003 | 0.048 |
| | [0.033]*** | [0.032]*** | [0.021] | [0.018] | [0.022] | [0.018] | [0.034] | [0.037] |
| Teacher: Regular license | 0.071 | 0.077 | −0.002 | −0.007 | −0.015 | −0.024 | −0.026 | −0.013 |
| | [0.010]*** | [0.010]*** | [0.009] | [0.007] | [0.009]* | [0.008]*** | [0.014]* | [0.014] |
| Teacher: License score | 0.051 | 0.024 | −0.019 | 0.039 | 0 | −0.005 | 0.004 | −0.056 |
| | [0.010]*** | [0.011]** | [0.028] | [0.026] | [0.005] | [0.005] | [0.070] | [0.092] |
| Teacher: Advanced degree | −0.048 | −0.057 | 0.014 | 0.002 | −0.005 | −0.007 | 0.008 | 0.022 |
| | [0.022]** | [0.022]** | [0.020] | [0.018] | [0.012] | [0.012] | [0.043] | [0.044] |
| School: Small town | −0.092 | −0.192 | −0.152 | −0.111 | −0.027 | −0.01 | −0.252 | −0.082 |
| | [0.054]*† | [0.042]*** | [0.087]* | [0.102] | [0.033] | [0.029] | [0.193] | [0.176] |
| School: Midsized city | −0.052 | −0.157 | −0.067 | −0.066 | −0.042 | −0.012 | −0.24 | −0.101 |
| | [0.052] | [0.039]*** | [0.083] | [0.098] | [0.031] | [0.027] | [0.187] | [0.172] |
| School: Rural | −0.063 | −0.171 | −0.098 | −0.103 | −0.032 | −0.004 | −0.215 | −0.071 |
| | [0.053] | [0.040]*** | [0.082] | [0.096] | [0.032] | [0.027] | [0.186] | [0.172] |
| School: % White | −0.024 | 0.052 | −0.167 | −0.013 | −0.021 | −0.02 | −0.205 | −0.052 |
| | [0.031] | [0.030]* | [0.080]† | [0.071] | [0.019] | [0.018] | [0.125] | [0.121] |
| School: % Free lunch | 0.019 | −0.011 | 0.001 | −0.015 | −0.024 | −0.012 | −0.079 | −0.071 |
| | [0.031] | [0.030] | [0.029] | [0.025] | [0.022] | [0.021] | [0.051] | [0.046] |
| School: Enroll | 0.049 | 0.025 | 0.087 | 0.049 | 0.003 | 0.008 | 0.009 | 0.054 |
| | [0.014]*** | [0.014]* | [0.033]*** | [0.029]* | [0.009] | [0.009] | [0.051] | [0.050] |
| Observations | 88,944 | 88,768 | 88,944 | 88,768 | 88,944 | 88,768 | 88,944 | 88,768 |
| $R^2$ | 0.02 | 0.02 | 0.82 | 0.86 | 0 | 0 | 0.2 | 0.21 |
| Teacher effects | No | No | Yes | Yes | No | No | Yes | Yes |

Omitted categories are “large city” and “zero years of experience.” Robust standard errors in brackets Significant at *10%, **5%, and ***1%.

licensure exams, the 75th percentile of the SAT distribution of their college, years of teaching experience, and possession of an advanced degree. These variables are loaded onto three factors: effective teachers, teachers with high cognitive ability, and teachers with strong paper credentials. To create factors for school characteristics, I used student demographics, student achievement levels, enrollment, and urbanicity variables. The school variables are loaded onto three factors: high-achieving suburban schools; midachievement rural, white schools; and low-achieving inner-city schools. I also take advantage of a teacher workplace conditions survey that can be linked to schools but not to teachers. The survey contains 23 questions common across all survey years. Teacher are asked to state their level of agreement with statements such as, “Teachers have time to collaborate with colleagues,” and, “Teachers are held to high standards.” These survey responses are loaded onto four factors: strong leadership and high standards, abundant resources, teachers have time, and emphasis on professional development.

To identify which teacher-school combinations are associated with better or worse student outcomes, I estimate a value-added model like equation (17) by OLS, where $Z_j$ and $Z_s$ are teacher and school types (factors), respectively:

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \delta(Z_j \cdot Z_s) + \theta_j + \theta_s + \eta_{ijsy}. \tag{17}$$

The results are presented in table 8. Because teacher peer characteristics are also an important part of the school environment, I use the mean of the teacher types as a school-level characteristic.

Table 8 reveals a few patterns that are robust across subjects. Effective teachers (high-value-added teachers) perform relatively better at schools with highly credentialed teachers, better at schools with an emphasis on professional development, and relatively worse at high-achievement suburban schools. In contrast, teachers with strong credentials (experienced, licensed, and have a master’s degree) perform relatively worse at schools with more cognitive-type teachers (teachers from selective colleges with high scores on their exams), better at inner-city schools, and worse at schools with an emphasis on professional develop-

TABLE 8.—MATCH QUALITY AND TEACHER SCHOOL COMBINATIONS

| | Math | Reading |
|---|---|---|
| Effective × Mean(Effective at school) | −0.00905 | −0.0129 |
| | [0.0296] | [0.0324] |
| Effective × Mean(Cognitive at school) | −0.142*** | −0.0383 |
| | [0.0407] | [0.0446] |
| Effective × Mean(Credentialed at school) | 0.0898*** | 0.0607** |
| | [0.0259] | [0.0283] |
| Cognitive × Mean(Effective at school) | −0.00593 | −0.00553 |
| | [0.0116] | [0.0126] |
| Cognitive × Mean(Cognitive at school) | −0.0472*** | 0.0297 |
| | [0.0169] | [0.0184] |
| Cognitive × Mean(Credentialed at school) | −0.0137 | 0.0042 |
| | [0.0115] | [0.0126] |
| Credentialed × Mean(Effective at school) | −0.0306** | −0.000829 |
| | [0.0134] | [0.0147] |
| Credentialed × Mean(Cognitive at school) | −0.0417** | −0.0476** |
| | [0.0184] | [0.0201] |
| Credentialed × Mean(Credentialed at school) | −0.0133 | 0.00546 |
| | [0.0124] | [0.0136] |
| Effective × Suburban, High Achieving, White School | −0.0658*** | −0.0486*** |
| | [0.0116] | [0.0126] |
| Effective × Rural, Low income, White School | −0.0289** | −0.00669 |
| | [0.0145] | [0.0158] |
| Effective × Urban, Low Achieving, Poor, Black School | −0.0870*** | −0.0191 |
| | [0.0140] | [0.0154] |
| Cognitive × Suburban, High Achieving, White School | 0.00638 | 0.0083 |
| | [0.00479] | [0.00523] |
| Cognitive × Rural, Low income, White School | 0.00911 | −0.00234 |
| | [0.00687] | [0.00751] |
| Cognitive × Urban, Low Achieving, Poor, Black School | 0.00196 | 0.00252 |
| | [0.00663] | [0.00724] |
| Credentialed × Suburban, High Achieving, White School | −0.00175 | −0.0021 |
| | [0.00551] | [0.00602] |
| Credentialed × Rural, Low income, White School | 0.00427 | 0.0127 |
| | [0.00789] | [0.00862] |
| Credentialed × Urban, Low Achieving, Poor, Black School | 0.0216*** | 0.0198** |
| | [0.00753] | [0.00823] |
| Effective × Strong Leadership | −0.00462 | −0.00196 |
| | [0.0109] | [0.0119] |
| Effective × Well Resourced | −0.0264** | −0.0105 |
| | [0.0120] | [0.0131] |
| Effective × Time | 0.00874 | 0.012 |
| | [0.0129] | [0.0141] |
| Effective × Professional Development | 0.0796*** | 0.0354** |
| | [0.0154] | [0.0169] |
| Cognitive × Strong Leadership | −0.0101** | −0.00933* |
| | [0.00464] | [0.00508] |
| Cognitive × Well Resourced | 0.00441 | 0.00402 |
| | [0.00567] | [0.00620] |
| Cognitive × Time | −0.0045 | 0.00205 |
| | [0.00606] | [0.00662] |
| Cognitive × Professional Development | 0.00889 | 0.0113 |
| | [0.00663] | [0.00726] |
| Credentials × Strong Leadership | −0.0178*** | −0.00671 |
| | [0.00539] | [0.00590] |
| Credentials × Well Resourced | −0.00388 | −0.00121 |
| | [0.00680] | [0.00743] |
| Credentials × Time | −0.00551 | −0.00556 |
| | [0.00702] | [0.00767] |
| Credentials × Professional Development | −0.0253*** | −0.0206** |
| | [0.00755] | [0.00826] |
| Observations | 1,133,980 | 1,127,033 |
| Pr[teacher type interactions] = 0 | 5.36E-08 | 0.0315 |
| Pr[school characteristic interactions] =0 | 6.73E-09 | 0.0416 |
| Pr[school survey type interactions] = 0 | 0 | 0.00162 |

Robust standard errors in brackets. Significant at *10%, **5%, and ***1%. All models include teacher and school fixed effects and control for lagged student achievement.

ment. Finally, high-cognitive teachers perform worse at schools with strong leadership and schools with an emphasis on professional development. Because researchers have found racial match effects (Dee, 2004, 2005), I test for such effects at the school level (see table A2). Once teacher and school effects are included, there is evidence that white teachers perform relatively better at rural schools with high shares of white students, but little evidence that black teachers perform any better or worse at schools with larger shares of white or black teachers or students.

While these correlations should be interpreted with caution, the estimated effects are sufficiently large that there could be nontrivial gains to optimally matching teachers to schools. For example, teachers who are 1 SD above the mean in effectiveness would improve test scores by $0.05\sigma$ more at schools that score $1\sigma$ higher on their emphasis on professional development. In addition, teachers who are $1\sigma$ above the mean in effectiveness would improve test scores by $0.03\sigma$ less at a school that is more suburban, affluent, and highachieving.

## VII. Conclusion

I document that teachers perform better in the classroom after a move to another school than before the move. I present a variety of empirical tests showing that this cannot be explained by teachers moving to higher-achieving schools, endogenous teacher effort, or student selection. I then provide the first direct estimates of match effects using measures of worker output (as opposed to inferring them from wages) and find that match quality is an important determinant of student achievement. The variability of match effects is about two-thirds as large as teacher effects and about one-quarter of what we typically interpret as a teacher quality effect is a match quality effect that is not portable across schools. Although there is no direct relationship between productivity and wages for teachers, various empirical patterns are consistent with the canonical job search models. Specifically, teachers with high school-specific quality are less likely to switch from such schools but no more likely to exit the profession, while teachers with high general teaching quality are no more likely to switch schools but less likely to exit the profession. In addition, match quality increases and school switching decreases monotonically with experience, consistent with workers' switching jobs until they find a productive match.

These findings validate previous theoretical and empirical work on worker mobility using wages to infer match quality. Furthermore, that match quality predicts mobility in a context where there is no relationship between wages and productivity suggests that workers may value high-productivity matches for reasons other than monetary compensation. Both findings are important contributions to the literature on worker mobility. They are also important for the education literature and have important policy implications. Certain kinds of teacher-school combinations are associated with better outcomes, such that a teacher placement policy maximizing match quality could lead to meaningfully improved student outcomes. The findings also indicate that policymakers should be cautious about identifying strong teachers in one school and moving them to another. Moreover, because match and teacher quality are often confounded, policy simulations based on teacher quality estimates that do not account for match quality could be inaccurate. Fortunately, the results indicate that teachers tend to leave schools at which they are poorly matched, so that teacher turnover (which is generally considered negative) may in fact move us closer to an optimal allocation of teachers to schools.

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## TABLE APPENDIX

TABLE A1.—TEACHER AND SCHOOL FACTORS

| | Factor | | | | |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | Uniqueness |
| Math estimated value-added | 0.5551 | 0.0554 | 0.0545 | 0.0012 | 0.6858 |
| Reading estimated value-added | 0.5534 | 0.0087 | 0.0413 | 0.0033 | 0.692 |
| Fully certified | 0.0184 | 0.1316 | 0.1161 | 0.0809 | 0.9623 |
| Average score on licensure exams | 0.0326 | 0.5485 | −0.0439 | 0.0326 | 0.6951 |
| Years of experience | 0.0512 | −0.1082 | 0.5006 | 0.0417 | 0.7334 |
| Advanced degree | −0.01 | 0.0841 | 0.3209 | 0.162 | 0.8636 |
| 75th percentile of the SAT distribution at college | 0.0219 | 0.5296 | −0.0416 | −0.005 | 0.7173 |
| Fully licensed | 0.0566 | −0.0251 | 0.3782 | −0.0712 | 0.8481 |

Factor 1: High value-added teachers (effective) ($\sigma = 0.81$)
Factor 2: High test score and selective college (cognitive) ($\sigma = 0.42$)
Factor 3: Experienced, fully licensed, and fully certified (credentials) ($\sigma = 0.40$)

| | Factor | | | | | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | Uniqueness |
| Percent white students at school | 0.3993 | 0.7873 | −0.3828 | 0.091 | 0.031 | 0.065 |
| Percent black students at schools | −0.3556 | −0.7592 | 0.4077 | −0.1415 | 0.0519 | 0.1083 |
| Percent on free lunch | −0.5794 | −0.5389 | 0.0909 | −0.4711 | −0.1633 | 0.1169 |
| Mean math scores | 0.8808 | 0.3504 | −0.0099 | 0.2295 | −0.1021 | 0.0382 |
| Mean reading scores | 0.8954 | 0.3493 | 0.0101 | 0.2327 | 0.0147 | 0.0218 |
| Mean parental education | 0.7023 | 0.1071 | 0.3153 | 0.459 | 0.177 | 0.1538 |
| City | 0.045 | −0.1869 | 0.6289 | 0.0288 | 0.0157 | 0.5665 |
| Rural | −0.0331 | 0.1102 | −0.5789 | −0.1118 | 0.008 | 0.639 |
| Total enrollment | 0.194 | 0.0682 | 0.0967 | 0.4311 | −0.0129 | 0.7623 |

Factor 1: Suburban, high achieving, high parental education, affluent, white students ($\sigma = 0.95$)
Factor 2: Rural, medium achieving, affluent, white students ($\sigma = 0.93$)
Factor 3: Urban, low achieving, low income, nonwhite students ($\sigma = 0.51$)

TABLE A1.—(CONTINUED)

| Question | Factor 1 | Factor 2 | Factor 3 | Factor 4 | Factor 5 | Uniqueness |
|---|---|---|---|---|---|---|
| Teachers have reasonable student loads. | 0.1417 | 0.1877 | 0.5683 | 0.0783 | 0.031 | 0.6081 |
| Teachers are protected from duties that interfere with teaching. | 0.3488 | 0.2315 | 0.6701 | 0.1308 | 0.0462 | 0.3511 |
| Teachers have time to collaborate with colleagues. | 0.1729 | 0.1968 | 0.5757 | −0.3025 | 0.0383 | 0.4851 |
| Time is provided for professional development. | 0.2776 | 0.216 | 0.3985 | −0.6199 | 0.0056 | 0.3299 |
| Leadership tries to address concerns about time. | 0.7442 | 0.2 | 0.4263 | 0.1789 | 0.0217 | 0.1641 |
| Teachers have quiet space to work individually. | 0.1314 | 0.566 | 0.2647 | 0.0778 | 0.0011 | 0.5456 |
| Teachers have sufficient office supplies. | 0.2736 | 0.5754 | 0.2058 | 0.129 | 0.1198 | 0.5169 |
| Classrooms/labs have current technology. | 0.1901 | 0.6579 | 0.1088 | −0.2193 | 0.041 | 0.4565 |
| Teachers have reliable communication technology. | 0.1782 | 0.7121 | 0.154 | 0.0954 | 0.014 | 0.4222 |
| School environment is clean and safe. | 0.4246 | 0.5251 | 0.1522 | −0.0895 | 0.0508 | 0.3937 |
| Leadership tries to address concerns about facilities. | 0.7551 | 0.3876 | 0.2202 | 0.1697 | 0.0018 | 0.1415 |
| Principal is a strong, supportive leader. | 0.8701 | 0.1612 | 0.1464 | −0.119 | 0.1012 | 0.1577 |
| Leaders shield teachers from disruptions. | 0.7358 | 0.2583 | 0.334 | −0.0795 | 0.0556 | 0.2371 |
| Administrators give priority to supporting teachers. | 0.8625 | 0.1861 | 0.2034 | −0.1245 | 0.0397 | 0.1519 |
| Teachers are held to high standards. | 0.6411 | 0.2602 | 0.1144 | −0.2135 | 0.1496 | 0.3669 |
| New teachers have effective mentors. | 0.58 | 0.2504 | 0.1252 | −0.2617 | 0.026 | 0.466 |
| Leaders try to address concerns about leadership. | 0.8828 | 0.1963 | 0.2033 | 0.1829 | 0.0313 | 0.0915 |
| Teachers are centrally involved in decision making. | 0.7446 | 0.2004 | 0.2649 | 0.2089 | 0.2898 | 0.1955 |
| Teachers are recognized as educational experts. | 0.7277 | 0.1983 | 0.2658 | 0.1623 | 0.3226 | 0.2216 |
| Parents have many avenues to express concerns. | 0.6153 | 0.211 | 0.0706 | 0.1903 | 0.0343 | 0.4655 |
| There is an atmosphere of mutual respect at school. | 0.8205 | 0.2229 | 0.1818 | 0.1369 | 0.0913 | 0.202 |
| Resources are available for professional development. | 0.3436 | 0.3042 | 0.226 | 0.5956 | 0.0541 | 0.378 |
| Leadership tries to provide quality professional development. | 0.6664 | 0.2258 | 0.1394 | 0.4731 | 0.0282 | 0.2469 |

Factor 1: Strong leadership, good school culture, high standards ($\sigma = 0.97$)
Factor 2: Well resourced ($\sigma = 0.84$)
Factor 3: Teachers have time ($\sigma = 0.80$)
Factor 4: Emphasis placed on professional development ($\sigma = 0.81$)

Factors that explain at least 1% of the covariance are included in the analysis and described.

TABLE A2.—RELATIONSHIP BETWEEN TEACHER ETHNICITY AND MATCH QUALITY

| | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| | Math | Reading | Math | Reading |
| Lagged achievement | 0.805*** | 0.781*** | 0.798*** | 0.773*** |
| | [0.00124] | [0.00142] | [0.00124] | [0.00143] |
| White teacher | 0.0575 | 0.0201 | | |
| | [0.0719] | [0.0487] | | |
| Black teacher | −0.101*** | −0.0404** | | |
| | [0.0251] | [0.0164] | | |
| White teacher × Percent White teachers at school | −0.0677 | −0.0198 | −0.0147 | 0.0238 |
| | [0.0900] | [0.0604] | [0.0234] | [0.0204] |
| Black teacher × Percent Black teachers at school | 0.237** | 0.126** | −0.00135 | −0.00402 |
| | [0.0964] | [0.0641] | [0.0609] | [0.0540] |
| Black × Suburban, High Achieving, Affluent, White School | −0.00873 | −0.0112 | 0.0251 | 0.00193 |
| | [0.0177] | [0.0112] | [0.0377] | [0.0195] |
| Black × Rural, Medium Achieving, Low income, White School | 0.0252 | 0.0265 | 0.0333 | 0.0429 |
| | [0.0275] | [0.0170] | [0.0390] | [0.0300] |
| Black × Urban, Low Achieving, Low Income, Black School | −0.000445 | 0.0207 | 0.00657 | 0.0403 |
| | [0.0243] | [0.0145] | [0.0459] | [0.0278] |
| White × Suburban, High Achieving, Affluent, White School | −0.00464 | −0.00602 | 0.0141 | −0.00278 |
| | [0.0170] | [0.0108] | [0.0363] | [0.0191] |
| White × Rural, Medium Achieving, Low Income, White School | 0.0199 | 0.0268 | 0.0619* | 0.0488* |
| | [0.0266] | [0.0164] | [0.0361] | [0.0274] |
| White × Urban, Low Achieving, Low Income, Black School | −0.00669 | 0.0141 | 0.00993 | 0.0376 |
| | [0.0232] | [0.0138] | [0.0441] | [0.0264] |
| Grade and year effects | Yes | Yes | Yes | Yes |
| School effects | Yes | Yes | Yes | Yes |
| Teacher effects | No | No | Yes | Yes |
| Observations | 1,322,810 | 1,314,602 | 1,322,810 | 1,314,602 |
| Pr(Ethnicity interactions are all=0) | <0.000 | <0.000 | 0.469 | 0.438 |

Robust standard errors in brackets. Significant at *10%, **5%, and ***1%.