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

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ABSTRACT

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Match Quality, Worker Productivity, and Worker Mobility: Direct Evidence From Teachers

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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 I estimate teacher-school match effects using a mixed-effects estimator. Match quality can "explain away" a quarter of, and is as economically important as, teacher quality. Match quality is negatively correlated with turnover, 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. Because teacher wages are essentially unrelated to productivity, this is compelling evidence that workers may seek high quality matches for reasons other than higher pay.

The productive quality of the match between a worker and the firm plays a central role in canonical models of worker mobility (Jovanovic 1979, Mincer and Jovanovic 1981, Neal 1999, Burdett 1978, Mortensen 1998, Johnson 1978). The labor market is hypothesized to allocate workers to firms in the most efficient manner through workers either leaving jobs where the productivity match between the worker and firm is low or seeking jobs where match quality is high, or both. Match quality is also used to explain the stylized facts that changing jobs is associated with rapid earnings growth (Bartel and Borjas 1981, Altonji and Shakotko 1987, Topel and Ward 1992) and that job separations decline with tenure and experience.[2]

Despite the importance of match effects for understanding the labor market, there is little *direct* evidence of their existence. As pointed out in Nagypal (2007), data on match-specific quality or productivity are essentially non-existent, forcing researchers to specify a wage-setting mechanism that determines how wages relate to match-specific productivity and then study how wages and their distribution vary with tenure and job mobility. This approach is undesirable for two reasons. First, there are many different ways to specify wage setting, making mis-specification and omitted variables bias likely. For example, even if there are no productivity match effects, if some firms discriminate against females then females will have lower wages at discriminating firms. If discriminating firms also treat females employees poorly, females will be more likely to leave discriminating firms. This discrimination effect would be wrongly

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[1] I thank John Abowd, Simon Woodcock, and participants at the Cornell Junior Faculty Lunch for helpful suggestions. I thank Kara Bonneau of the North Carolina Education Research Data Center. All errors are my own.
[2] By match quality, I am referring 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.

interpreted as a match effect on mobility if one were to use wage data to infer productivity. Second, it is difficult to distinguish workers leaving jobs with low match quality from workers leaving jobs with low pay, or workers seeking better matches from workers seeking higher pay (because pay may vary across workers and firms for reasons unrelated to productivity). To avoid these problems, one must estimate match quality on actual output as opposed to wages. Micro-data with student test scores linked to teachers and schools provides 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 dataset of student test scores linked to teachers and schools in North Carolina, I aim to (1) determine the extent to which teacher effectiveness, as measured by ability to improve student test scores, changes depending on the schooling environment, (2) quantify the importance of the match between a teacher and a school in determining student achievement, (3) document the relationship between match quality and teacher mobility, and (4) present evidence on observable characteristics associated with high match quality.

In the context of teachers, 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 one standard deviation increase in teacher quality leads to between one-tenth and one-fifth of a standard deviation increase in math and reading scores (Aaronson, Barrow and Sander 2007, Rivkin, Hanushek and Kain 2005, Rockoff 2004). Also, observable teacher characteristic explain only a fraction of a teacher value-added.[3] These value-added measures have tremendous power in predicting a teacher's *future* success in the classroom (Kane and Staiger 2008), are correlated with principals' subjective evaluations of teachers (Jacob and Lefgren 2008) and teachers with high estimated value-added improve the test scores of their colleagues' students (Jackson and Bruegmann 2009). However, we have little understanding of exactly what they measure, and whether estimates obtained in one context can be extrapolated to others.[4] For example, we have very little evidence

[3] There is some evidence that years of teaching experience, selectivity of undergraduate institutions, teachers' test scores, and regular licensure are associated with higher student achievement (Anthony and Goldhaber 2007, Brewer and Ehrenberg 1994, Brewer and Goldhaber 2000, Clotfelter, Ladd and Vigdor 2006, Clotfelter, Ladd and Vigdor 2007, Hanushek 1997)

[4] A few recent papers have started to address these questions. Specifically, Jackson and Bruegmann (2009) show that about a quarter of teacher value-added can be explained by the quality of a teacher peers in the past, and Ost (2009) finds that holding total experience constant, teachers with more experience teaching the same grade have higher value-added than those who are teaching a grade for the first time (or have less years of grade-specific

that a teacher who is effective at increasing the test scores of affluent suburban kids would be effective at improving the test scores of low-income inner-city students at another school, or *vice versa*. Given the increasing use of estimated teacher value-added to identify good teachers, and policies that aim to move strong teachers from high-achieving suburban schools into low-performing inner-city schools, it is important to understand the importance of school specific teacher value-added; that is, the importance of match quality.

While it is important to assess how much of what we consider a teacher effect is, in fact, a match effect, it is also important to assess the importance of the teacher-school match itself. If match quality is an important determinant of student achievement, then policymakers should be mindful of what kinds of teacher-school pairing are most likely to be productive, and should think seriously about what kinds of policies might improve match quality.

I find that teachers who switch schools are more effective after a move than before — suggestive of match effects. This effect persists in models that include *both* teacher and school-by-year fixed effects so that they cannot be explained by teachers moving to higher-achieving schools. To assuage concerns that these patterns reflect endogenous teacher movement or student selection, I show that teacher performance does not exhibit trending prior to moving schools[5], and I show that whether a student *will* have a teacher the following year who *will* transfer in or out the following year has no predictive power for current student achievement.

Providing direct evidence of match effects, I use both a fixed effects model and a random effects model to estimate the importance of match quality. Across both models, one standard deviation increase in match quality increases math scores by between 0.0892 and 0.13 standard deviations, and reading scores by between 0.077 and 0.118 standard deviations — about the same effect as a one standard deviation increase in teacher value-added. Results suggest that match quality can account for between 10 and 25 percent of what is typically estimated as teacher quality in math and between 25 and 50 percent of what is typically estimated as teacher quality in reading. These results suggest that a sizable portion of what is typically considered to be a teacher effect is in fact a teacher-school (match) effect, that is not portable across schools. While some observable teacher and school characteristics have predictive power in explaining these sizable match effects, much of match quality is "unexplained".

experience). Both studies suggest that teacher value-added changes over time, and may be context specific.

[5] This test is a relatively powerful test of endogenous movement because unlike wages which are downward sticky, changes in teacher productivity are directly reflected in student test scores.

Consistent with canonical models of worker turnover, (a) job switching declines monotonically with experience, (b) match quality is increasing in experience (both across and within teachers), and (c) low match quality strongly predicts a teacher leaving her current school (above and beyond observable teacher and school characteristics). These patterns are all robust to the inclusion of *both teacher and school fixed effects*, and the empirical patterns persist when using estimated fixed match effects or estimated random match effects.

The mobility results suggest that match productivity is an important determinant of mobility for reasons other than the level of pay because productivity and pay are essentially unrelated in teacher labor markets. This is consistent with either (a) schools providing greater non-pecuniary benefits for effective teachers or (b) teachers caring about their effectiveness directly. As such, these findings suggest that models of worker mobility that do not account for the non-pecuniary job benefits may be incomplete, and further underscore that using wages to infer match quality has some important limitations.

This paper makes two important contributions to job mobility literature: it is the first paper to validate the extant literature using direct measures of productivity, and the first to document the relationship between match quality and worker mobility in a context where wages and productivity are virtually unrelated — underscoring the importance of non-pecuniary job benefits. This paper also makes important contributions to the education literature. It is the first to highlight and quantify the importance of match effects, and it documents that as much as 50 percent of what we call teacher quality *does not port across schools*. These findings highlight the importance of context for value-added measures of teacher quality and have important implications regarding optimal teacher placement.

The remainder of this paper is as follows. Section I outlines the theoretical justifications for the decomposition of productivity into a worker effect, a firm effect, and a match effect using wage data and student achievement, and then describes the important differences between the two. Section II describes the data. Section III provides evidence of the existence of match effects. Section IV describes the two for strategies estimating match effects and presents the variability of the teacher school and match effects. Section V documents the relationship between match quality and teacher mobility. Section VI shows what observable teacher school and workplace conditions are associated with higher match quality, and Section VII concludes.

I. Match Quality For Teachers

The literature that decomposes wages into a worker effect and a firm effect (Abowd, Creecy and Kramarz 2002, Abowd, Kramarz and Margolis 1999, Abowd, Kramarz and Lengermann, et al. 2004) starts out with a Cobb-Douglas production function describing the output Q_{ij} of worker *i* at firm *j* as below.

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

In (1), L_i is the human capital of worker *i* (such as education, years of experience, quality of schooling, etc), K_j summarizes the productive characteristics of the firm (such as technology, capital intensity, incentive structure, leadership skills, etc), and θ and φ are parameters in the production function. One could easily imagine a world where worker attributes and firm attributes are complementary so that certain pairings of workers and firms are particularly productive (un unproductive). This can easily be incorporated in the model with the inclusion of a match term to the model (Woodcock 2008). The production function can then written as below.

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

Where M_{ij} is match quality and ϕ is a parameter relating match quality to output. If worker *i*'s wage is a share π_{ij} of worker related output at firm *j*, the worker's wage can be written as $w_{ij} = (L_i^{\theta} K_j^{\varphi} M_{ij}^{\phi}) \cdot \pi_{ij}$. Therefore, the log of worker wages can be written as (3) below.

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

The log wage can be decomposed into four additively separable components; a portion attributed to worker productivity $\theta \ln L_i$, a portion attributed to workplace productivity , $\varphi \ln K_j$, a portion that is attributable to the productivity match between the worker and the firm $\phi \ln M_{ij}$, and a portion summarizing the relative bargaining power of worker *i* at firm *j* $\ln \pi_{ij}$.

Equation (3) makes clear that where there are differences in bargaining power across worker-firm pairings, $\ln \pi_{ij}$, and such differences are not randomly distributed, one may confound relative productivity with relative match specific bargaining power. The common solution has been to assume that bargaining power varies at the firm level, but does not vary at the firm-by-worker level. While this assumption is necessary to make the decomposition of wages into a firm effect and a worker effect tractable, clearly some kinds of workers have more/less bargaining power at certain firms. For example, if some firms discriminate against female/black workers this would result in low $\ln \pi_{ij}$ for female/black workers at discriminating firms. This discrimination could also lead to increased job separation among female/black workers leading one to wrongly infer a relationship between match quality and worker mobility.

To further complicate matters, there are the additional concerns that (1) many theories of wage determination predict that wages have little relation to *contemporaneous* productivity, and (2) non-pecuniary benefits such as perks, working conditions, in-kind benefits, and deferred compensation are typically not measured, but are clearly an important part of a worker's total compensation that can be used by firms to attract and retain workers. These points underscore that using wage data to infer match quality has inherent limitations ─ motivating use of teacher (worker) data linked to schools (firms) and student outcomes (direct measures of output).

I.2 *The Production of Student Achievement*

While value-added models are ubiquitous in the education literature, and estimates based on such models have great predictive power out-of-sample, it is helpful to explicitly lay out how the production technology of student achievement relates to the empirical models employed. Consider the following model of student achievement, where 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 (5) T_{ijsa} is student *i*'s achievement with teacher *j* at school *s* at age *a*, $X_{ijs}(a)$ is the history of parent and school supplied inputs up to age *a*, μ_{i0} is the student's natural endowment (ability) and ε_{ijsa} is an idiosyncratic error (other unmeasured inputs). Under the assumptions of additive separability of inputs and that lagged achievement is a summary statistic for the full history of family, school, and student inputs (including student ability), we can write (4) as (5) below.[6]

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

This value-added model in (5) is commonly used, and is the one employed in this paper. While there are several specifications used in the literature to estimate teacher value-added, as a practical matter, the predictive power of estimated teacher fixed effects are surprisingly robust across specifications (Kane and Staiger 2008).[7]

Explicitly incorporating teacher human capital, school technology, and the productivity

[6] This will be true if coefficients on inputs are geometrically declining with distance (in age), and the impact of the ability endowment is geometrically declining at the same rate as inputs (Todd and Wolpin 2003).
[7] All the results are robust to alternate ways of specifying the value-added model.

of the specific teacher-school pairing as inputs into the model yields (6) below.

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

Equation (6) is similar to equation (3) insofar as it contains three additively separable components; a portion attributed to worker (teacher) productivity θ_j, a portion attributed to workplace (school) productivity θ_s, and a portion that is attributable to the match between the worker and the firm θ_{js}. The fundamental differences between equations (6) and (3) are that these school, teacher, and match specific components reflect differences in *actual* productivity, and there is no unobserved component related to the relative bargaining power of the worker in that particular firm. As such, the use of education data where productivity is observed may validate previous studies on match quality and provide some new and helpful insights.

1.3 *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. To illustrate how this can be done, consider the ideal empirical setup where (a) each teacher is observed in all schools, and (b) there is zero correlation between *potential* match, teacher, and school effects.[8] Conditions (a) and (b), ensure that the mean match quality for each teacher is equal to zero, and that the mean match quality for each school is equal to zero so that the mean test scores (conditional on controls for student selection) of teacher *j* at school *s* would be a consistent estimate of match (teacher-by-school) effect θ_{js}. Also, the mean of all the matches for teacher *j* (across all schools) would be a consistent estimate of teacher effect θ_j, and the mean of all the matches for school *s* (across all teachers) would be a consistent estimate of school effect θ_s. Operationally, one could obtain consistent estimates of teacher, school, and match effect with the familiar fixed-effects estimator.

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 school A and B, plus the difference in match effects for teacher *p* between schools A and B. If there are

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

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. That is, with no match effects $E[\overline{Y}_{1B} - \overline{Y}_{1A}] = E[\overline{Y}_{2B} - \overline{Y}_{2A}] = \theta_B - \theta_A$. However, with match effects, expected differences associated with switching from schools will not be the same for both types of teachers so that $E[\overline{Y}_{1B} - \overline{Y}_{1A}] - E[\overline{Y}_{2B} - \overline{Y}_{2A}] \equiv \theta_{1B} - \theta_{1A} - (\theta_{2B} - \theta_{2A}) \neq 0$.

This shows that systematic performance differences associated with switching schools across teachers is how match effects can be identified empirically.[9] The intuition can be illustrated with a simple example. Suppose school A has a strong principal who, *all else equal*, improves the outcomes all teachers by δ. School A enrolls high-income students while school B enrolls average students. Teacher 1 teachers performs μ_1 better with high-income students while teacher 2 performs μ_2 *worse* (where $|\mu_2|>|\delta|$). When teacher 1 switches from school B to A, her outcomes will be unambiguously better in school A than in B by $\delta+\mu_1$. However, when teacher 2 switches from school B to A, their outcomes are *worse* by δ-μ_2. Even though there is a positive school effect δ (due to the principal) that is enjoyed by all teachers, the difference in outcomes associated with switching schools is not the same teachers 1 and 2 because the match effect for school A is positive for teacher 1 and negative for teacher 2. It is this differential switching response that is the basis for indentifying match effects in this paper.

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 estimation of match effects, the logic of the identification is most saliently illustrated in this idealized setting. In section IV, I detail my estimation approach that should uncover unbiased match effect estimates (for mobile teachers) under realistic assumptions about the distribution of teachers.

I.4 *How Does One Interpret Match Quality for Teachers?*

The discussion above makes explicit that match effects are those effects that specific to a particular teacher-school pairing and *is not* a school or teacher characteristic. While it may be difficult to disentangle a school with a large effect from a school that has many high quality matches *empirically*, these concepts are distinct. By the same logic, While it may be difficult to

[9] With the assumptions that the mean of the match effects is equal to zero in expectation for each school and for each teacher these match estimates above can be computed. Specifically, these assumptions mean that in large samples $\theta_{1B} + \theta_{1A} = 0$ and $\theta_{2B} + \theta_{2A} = 0$ and $\theta_{1B} + \theta_{2B} = 0$ and $\theta_{1A} + \theta_{2A} = 0$. With four equations and four unknown it is trivial to solve for unique values of the match effects. In the example above $\theta_{1B} = -\theta_{1A} = \theta_{2B} = -\theta_{1B} = (E[\overline{Y}_{1B} - \overline{Y}_{1A}] - E[\overline{Y}_{2B} - \overline{Y}_{2A}])/4$.

disentangle a teacher with a large effect from a teacher that has many high quality matches empirically, these concepts are also distinct. In this paper, when I refer to match quality I am referring to the productivity associated with particular teacher-school parings that is often confounded with school or teacher quality in empirical applications. In section IV, I detail the difficulties of disentangling good matches from good schools and good teachers empirically, and I use this to motivate my preferred random effects empirical strategy to do so.

A match effect is anything that makes a teacher more or less productive at one school versus another (that is not due to a characteristic of the school that affects all teachers equally). Anything that affects all teachers at a school equally would be part of a school effect (such as having high-achieving students, or strong leadership) and that 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 being particularly good at teaching certain types of students (e.g. low-income, same race, affluent, high-motivation) that attend particular schools. Alternatively, certain schools may have a teaching philosophy (e.g. emphasis on high standards) or work culture (e.g. a culture of dialogue between teachers and administrators) in which certain teachers thrive and others do not. Also, there may be differences based on the characteristics of other employees (e.g. teachers may perform better when they are surrounded by teachers who share similar approaches to the profession, or with whom they can more easily relate). In sum, match quality captures systematic complementarities between particular teachers and particular schools. While identifying these match effects is my main objective, I aim to shed some light on the reasons for these effects in section VI.

I.5 *Why Would Teachers Care About Match Quality if they Do Not Get Higher Pay?*

Teacher labor markets are an interesting context to study the relationship between match effects and worker mobility because teacher salary is, for the most part, based on a teacher's years of experience and level of education. Most theories of match quality are predicated on the notion that workers seek out high quality matches in order to increase their monetary compensation. As such, one may wonder why there would be any relationship between match quality and teacher mobility. I discuss why this is likely to be the case below.

It has been well recognized that the utility a worker derives from their job is associated with more than monetary compensation going as far back as Smith and Marshall.[10] A good recent illustration of this is the empirical fact that the unemployed are much less happy than the employed, and by more than their lower incomes would predict (Korpi 1997; Winkelmann and Winkelmann 1998; Di Tella, MacCulloch, and Oswald 2001). Also, it has also been found that one's ordinal rank in the wage hierarchy affects one's happiness conditional on ones level of pay (Brown, Gardner, and Oswald, 2006). Aside from these consumption aspects of the job, it is well accepted that workers care about working conditions, and that in-kind benefits make up a substantial part of a worker's compensation. As such, a worker's utility from her job is a function not only of her pay but also non-pecuniary benefits such as in-kind benefits, working conditions, prestige, job satisfaction, and other consumption benefits (Duncan, 1976).

With this broader view of the benefits to employment, there are two reasons why workers may have incentives to seek out high quality matches even in a context in which match quality and pay are unrelated. The first is that employers can improve the working conditions and the benefits of high productivity workers whom they want to retain *en lieu* of increasing pay. For example, principals could assign teachers to more desirably committees, offer them extra positions to supplement their income, pay for more of their training costs, or appoint them to positions of leadership. Given that teachers often spend money out of pocket to pay for classroom supplies, another way principals can, and do, effectively increase teacher pay without giving a "raise" is to pay for such supplies.[11]

The second reason workers may seek high productivity matches is that workers may derive utility directly from being a high productivity worker. Insofar as teachers have some intrinsic motivation to teach, they may be happier at schools in which they are making a difference. This idea is outlined in Akerlof and Kranton (2005) who emphasize that military

[10] As stated by Smith "Wages vary by ease vs. hardship, cleanliness, honourableness." (Smith, Adam. 1776. Wealth of Nations. Chicago: University of Chicago Press). As stated by Marshall, "every occupation involves other disadvantages besides the fatigue of the work required in it, and every occupation offers other advantages besides the receipt of money wages. The true reward which an occupation offers to labour has to be calculated by deducting the money value of all its disadvantages from that of all its advantages" (Marshall, Alfred (1890) Principles of Economics).

[11] According to the 2010 Retail Market Awareness Study released by the National School Supply and Equipment Association, public school teachers in the US spent more than $1.33 billion out of pocket on school supplies and instructional materials during the 2009-2010 academic year. The average teacher surveyed said they $936 on classroom materials during the 2007-2008 academic year.

officers believe in "service before self", and are willing to trade off monetary rewards for the non-pecuniary gains of being a military officer. Also, because workers may care about by their performance relative to colleagues, teachers may have incentives to move to schools where they have high match quality and are therefore less likely to be among the worst performing teachers.

In sum, insofar as workers (teachers) might care about non-pecuniary aspects of the job, which are likely correlated with match quality, workers (teachers) have an incentive to seek out firms (schools) with which match quality is high, *irrespective of the level of pay*. As such, workers seeking out jobs with better non-pecuniary benefits may lead to very similar patterns as workers seeking higher paying jobs as long as match quality is systematically related to non-pecuniary benefits as suggested above.

II. 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 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 attend and the teachers who administered their tests. According to state regulation, the tests must be administered by a teacher, principal, or guidance counselor. 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 being administered the exam by a teacher who teaches math and reading to students in that grade, and I remove teachers who are co-teaching 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 roughly 62 percent white 29.5 percent black, and are evenly divided between boys and girls. About 11 percent of students' parents did not finish high school, 43 percent had just a high school diploma, roughly 30 percent had some post-high school education but no four-year college degree, and roughly 14 percent of students had parents who have a four-

[12] These student-teacher linked data have been used by other researchers to look at the effect of teachers on student outcomes (Clotfelter, Ladd and Vigdor 2006, Clotfelter, Ladd and Vigdor 2007, Rothstein 2010) and the effect of student demographics on teacher quality (Jackson 2009).

year college degree or graduate degree as their highest level of education. The average class size is 23, with a standard deviation of 4. The test scores for reading and math have been standardized to have a mean of zero and unit variance, based on *all* students in that grade in that year.

About 92 percent of teachers is the sample are female, 83 percent are white, and 15 percent are black. The average teacher in the data has thirteen years of experience, and roughly 6 percent of the teachers have no experience.[13] Roughly 20 percent of teachers have advanced degrees. About 67 percent 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 zero and unit variance for each year in the data. Teachers perform near the mean, with a standard deviation of 0.81. About 4 percent of teachers have National Board Certification.

There are 27,498 teachers and 1545 schools in the final dataset. The average school is observed with 21.3 teachers while about 80 percent of teachers are observed in only one school. About 16 percent of teachers are observed in two schools, 2 percent in three schools, and about 1 percent in four or more schools. The average teacher is observed in the data for 3.26 years, and about 37 percent are observed for one year. There are 32,922 teacher-school matches observed in the data, and each match contains data from about 98 students and 4.2 classrooms, on average. Matches for mobile teachers contain on average 78 student observations and 3.4 classrooms.

II.1 *A Descriptive Analysis of Teacher School Switching in North Carolina*

To identify a match effect (a teacher-by-school effect) requires that one observe the same teacher in multiple schools. As such, the analysis is driven by the 19 percent of teachers observed in classrooms in multiple schools between 1995 and 2006. To deepen our understanding of the estimation sample, to motivate strategy, and to put the empirical findings into context, this section provides a descriptive analysis of teacher switching.

II.1a *Who are the mobile teachers?*

Because match quality is a within-teacher concept, match effects can only be estimated for mobile teachers and may not be representative of match effects for non-mobile teachers. While this does not affect the internal validity of the exercise, one may wonder how mobile

[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.

teachers compare to the average teacher. To gain a sense of this, I estimate linear probability models for both (a) the likelihood that a teacher leaves her current school next year and (b) the likelihood that a teacher switches to another school next year, as a function of observable teacher characteristics. Specifically, I estimate the equation below by Ordinary Least Squares (OLS).

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

In (7), Y_{jst} is whether teacher j leaves her current school s at time t, T_{jt} are time varying teacher characteristics, τ_t is a year fixed effect, and ε_{jst} is the idiosyncratic error term. I present these results in Table 2. Columns 1 and 2 present results for exiting her current school (for another school or out of the profession entirely), while columns 3 and 4 present results for switching to another public school in North Carolina (the relevant outcome for this analysis).

The OLS results in column 1 show that first year teachers (the reference group) and teachers with fewer than 4 years experience are the most likely to leave their current schools, while teachers with between 10 and 24 years of experience are the least likely. While teachers with more than 25 years of experience are less mobile than those with fewer than 10 years of experience, they are more likely to leave their current schools than teachers with between 10 and 24 years of experience. This pattern of results suggests that older teachers have stronger attachment to teaching (possibly due to having found a high quality match) but that very senior teachers are more mobile because they are more likely to retire.

An analysis of teacher switching (as opposed to merely leaving ones current school) confirms this conclusion. The OLS results in column 3 show that the likelihood of switching schools is monotonically decreasing in experience, which is consistent with more experienced teachers finding better matches and remaining in these schools until they retire (it is also consistent with teachers acquiring school specific human capital). These estimates imply that the sample of switching teachers are most likely to be those with fewer than 10 years of experience, and less likely to be teachers with more than 24 years of experiencing.

On average, teachers with higher licensure scores and teachers with an advanced degree are more likely to exit their current school but no more likely to switch schools. Also, teachers with a regular teaching license are 15 percentage points *less* likely to leave their current school but 4.2 percentage point *more* likely to switch schools. Because obtaining regular licensure is investing in job-specific skills (but *not* firm specific skills) this pattern is consistent with having a regular teaching license being a good proxy for attachment to the teaching profession, therefore increasing the need to find a good match. The estimates suggests that the sample of switchers are more likely to be regularly licensed teachers than the average teacher.

To assuage concerns that these relationships reflect the fact that mobile teachers differ from non-mobile teacher in their school locations, and certain schools have higher teacher turnover than others, in columns 2 and 6 I present results that also include school fixed-effects ─ comparing mobile teachers to other teachers from the same school. The coefficients on the teacher characteristics are largely unchanged.

II.1b ***How do destination schools differ from sending schools?***

Because a large part of the analysis involves comparing teacher performance in one school to their performance in another, it is instructive to describe how the schools teacher move to differ from those they leave. I present the difference in school characteristics between a teacher's school the year before and after she switches schools. Specifically, 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 I test for the statistical significance of these differences. To see if the patterns of school switching are different for different kinds of teachers, I present theses differences for all teachers, and also for sub-samples of teachers. These comparisons are presented in Table 3.

On average teachers move to schools where mean classroom reading scores are 5 percent of a standard deviation higher, school level reading test scores are 2.3 percent of standard deviation higher, and classes are 0.23 students smaller. Also, teachers move to schools where the percent minority in their class is 1.5 percentage points lower, the percentage of black students in the school is 2.5 percentage points lower, and the percentage of low income students is 3.8 percentage points lower. These teachers also experience a 7.3 percent pay increase after a move. Looking at urbanicity it does not appear that many teachers are switching out of large cities into other areas, but they are more likely to switch out of schools in mid sized cities and town into rural areas. The patterns here are consistent with the notion that teachers tend to leave low performing schools that serve low-income ethnic minority students for higher achievement schools with fewer low-income students and fewer minority students.

Columns 2 and 3 show results for white and non-white 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, and non-white teachers move to schools that have similar levels of achievement and more black students. Also, non-white teachers are more likely to switch into inner-city schools while white teachers are not. This same general pattern of switching differing by race has been found by Hanushek, Kain and Rivkin (2004) using data from Texas.

There are some minor differences in the switching patterns of teachers with high scores on their licensure exam and those with low scores. Teachers with high scores experience larger increase in student achievement, smaller decreases in the percentage of black students, and are less likely to move into a less urbanized environment after a move than low scoring teachers. Comparing the patterns of by teacher experience also reveals some interesting differences. Inexperienced teachers (fewer than 5 years) experience much larger increases in student achievement after a move (an increase of 11 percent of a standard deviation in reading scores) than do veteran teachers (more than 10 years). Also, veteran teachers move to schools where mean class sizes are 0.366 smaller while inexperienced teachers see no change after a move on average. Both groups tend to leave schools with low-income students, but teachers with fewer than five years experience see larger decreases in the percent black at their new school than experienced teachers. While inexperienced teachers appear to be switching largely out of towns, experienced teachers are switching out of mid sized cities and urban fringes.

It is clear that not all teachers are switching to or from the same schools, and that there are likely to be several schools at which we see different teachers switching in and out. This fact plays a central role in my identification strategy.

III. Evidence of the Existence of Match Effects

Canonical models of worker mobility yield the prediction that workers move from jobs where their match quality is low so that their match quality will be higher, on average, at the job they move to than the one they left. One can test this key prediction by seeing if teacher value-added improves after a move relative to before. Note: if teacher mobility is unrelated to match quality, *even if there are match effects,* there would be no systematic difference in teacher effectiveness before compared to after a move. Finding that teachers perform systematically better after a move is sufficient but not necessary evidence for the existence of productivity match effects because it would be evidence both that match effects exist, and that teachers tend to leave schools with low match quality. I test this prediction empirically by mapping-out teacher effectiveness before and after a move. To do this, I estimate the following by OLS.[14]

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \sum_{\tau=-10}^{9} I_{t=\tau} \cdot \pi_{\tau} + \theta_j + \eta_{ijsy} \quad . \tag{8}$$

In (8) 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 θ_j is a teacher fixed-effect. In (8) π_τ is the effect on student achievement of having a teacher who is τ years from leaving her current school (for example π_{-2} is the effect for a teacher who will leave her current school in 2 years and π_{+2} is the effect for a teacher who left another school two years ago). The reference year (i.e. the omitted category) is the year before a teacher switches schools. In this Difference in Difference model, the "years before move" and "years after move" variables map out changes in outcomes for teachers who switch schools relative to changes in outcomes for teachers who do not switch schools over the same time period.

As illustrated in section II, teachers tend to move to schools with higher student achievement than the schools they left. While controlling for student attributes should account for differences in achievement across schools, to ensure that any pre- and post-move differences are not driven by unmeasured achievement differences across schools, I also 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 including school fixed-effects removes mean differences in outcomes across schools, one may also worry that *time varying* school characteristics affect both teacher performance and teacher mobility. For example, if a school changes principals (or experiences some negative shock) in year *t-1*, this may cause some teachers to have poor performance in year *t-1* and cause them to leave the school in year *t*. This could lead one to wrongly infer that teachers are more effective after a move than before. To address this issue, I also estimate models that include school-by-year fixed-effects (that is, 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

[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.

any school specific events that may affect teacher effectiveness and could be correlated with teacher mobility. As such, to map-out teacher effectiveness over time while accounting for time varying differences in outcomes across schools, I estimate the following by OLS.[15]

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

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 fixed effects and teacher fixed effects, this DID model compares a teacher's outcomes before a move to her outcomes after a move while taking into account average outcomes of the schools (in a particular year) she moved to and from.

A Test for Endogenous Teacher Mobility

In all papers on worker mobility there is the concern that worker productivity is endogenous to worker mobility. It is important to point out that teachers switching because their performance is poor *is not* considered endogenous mobility in this context and is exactly the kind of mobility I aim to characterize. The worry would be if productivity before a switch is endogenous to the switch. Specifically, the worry is that if teachers anticipate that they will leave their current job in one year, they may reduce their effort the year before a move to a new school. In such a scenario, one would observe that productivity is low before a move and 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, and for there to be some systematic pattern in teacher effeteness prior to the year a teacher moves. If there were some trending in teacher effectiveness prior to a move, if some unobserved event lead to both lower effectiveness prior to a move and the move itself, or if teachers were likely to reduce work effort in anticipation of a move, then the pre-movement year variables should have some predictive power. As such, the finding that the "years-before-move" effects have no explanatory power over a simple pre vs. post model would be evidence that teacher effectiveness is exogenous to teacher mobility and that one can interpret teacher-by-school differences in performance as being the result of match quality.

III.1 *Findings*

The "years since move" and "years until move" indicator variable coefficient estimates

[15] Equation (7) follows naturally from equation (6), where the age subscript for the student is replaced with the more general year subscript that is defined for teachers, schools, and students.

from equation (8) are presented in Table 4. *All models indicator variables for each year of teacher experience* so that any estimated effects are not driven by teachers being more experienced after a move than before. *All models* include grade fixed effects, year fixed effects, controls for student race, gender, parental education, and limited English proficiency status, indicators for the gender and racial match between the student and the teacher, teacher experience, and the class size.

The basic within-teacher results show that teachers perform better after a switch than before. Relative to the year before a move, all the post-move indicator variables have positive coefficients for both subjects, while the pre-move indicator variables are either negative or close to zero 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. The point estimates suggest that at a teacher increases test scores by about 2.5 percent and 1.4 percent of a standard deviation more at her new school than at her old school in math and reading, respectively (columns 2 and 6).[16] Columns 3 and 7 show the results that also include school fixed effects, while columns 4 and 8 show the results that also include school-by-year fixed effects. Across all these models the point estimates are largely unchanged indicating that the estimate outcome differences before and after a move are not spuriously driven by teachers moving from low achievement to high achievement schools and not driven by school-wide events that would affect both teacher mobility and teacher performance.

In the preferred models, for both subjects one cannot reject the null hypothesis that all pre-move year effects are the same (the *p*-value associated with the test of the joint significance of the pre-move year indicator variables are 0.62 and 0.26 for math and reading, respectively), while one rejects the null hypothesis that post move performance is the same as performance the year before a move at the 1 percent level (the *p*-value for joint significance of the post move indicator variables are 0.0003 and 0.007 for math and reading, respectively). For both subjects while there is little evidence of endogenous teacher mobility, teacher effectiveness is significantly different after a move than before. Moreover, these effect are not driven by differences in the types of schools teachers move from or to.

[16] 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. This is to deal with the worry that measurement error in a lagged dependent variable can lead to bias in a quasi-differenced model.

To illustrate how teacher effectiveness evolves over time, Figure 1 plots the estimated teacher effectiveness 8 years before and after a move both for models with no school effects and with school-by-year effects (from Table 4). Figure 1 makes clear visually what the statistical tests indicate. That is, irrespective of the specification, in both subjects teacher effectiveness does not exhibit any statistically significant trending or dip in years prior to a move, and teachers are more effective after a move than before. The lack of any pre-move decline in effectiveness is consistent with teacher effectiveness being exogenous to teacher mobility.

Could dynamic student selection drive these results?

Even though neither endogenous teacher mobility or effectiveness likely drive the before-after patterns in the data, one may worry about *student* selection. Sorting of students could drive the results if (a) students who are assigned to teachers who will leave the school the following year are systematically worse in unobserved dimensions than those who are not, and/or (b) students who are assigned to teachers at the teacher's new school are better in unobserved dimensions. The fact that there is no "Ashenfelter" type dip in outcomes before a move is *prima-facie* evidence that this is not driving the results. However, one can test this directly.

To test for student sorting in *unobserved* dimensions directly, one can see if (a) students in year y who will receive a teacher in year $y+1$ who will leave the school between year $y+1$ and $y+2$ have worse outcomes than those who will not, and (b) if students in year y who will have a teacher in year $y+1$ that 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 (7) but replacing the "years since/until move" variables with 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).[17] The coefficient on the variable denoting whether the student's teacher next year will leave the following year is 0.0008 (p-value = 0.879) and the coefficient on a variable denoting whether the student's teacher next year will be a new transfer from another school is -0.0039 (p-value = 0.18). Not only can one not reject the null hypothesis of no student sorting by teacher mobility status at traditional levels of significance, but the estimated coefficients are close to zero, and the signs are opposite of what would be required for student sorting to generate the patterns in Figure 1. As such, it appears that the improved outcomes observed after a teacher switches schools is not an artifact of dynamic student sorting.

[17] This is a modified version of the test presented in Rothstein (2010).

In sum, the results provide compelling evidence of the existence of match effects. Also, the fact that teachers are more effective after they move to a new school than before they move is consistent with models of worker mobility where workers tend to leave jobs with low match quality. Having established that match effects likely do exist for teachers, in section IV I estimate the match effects directly and quantify the magnitudes of teacher, school, and match effects.

IV. Estimating Match Effects and Estimating the Importance of Match Effects

As discussed in Section 1.4, 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 θ_j, the mean of all the matches for school *j* (across teachers) would be a consistent estimate of the school effect θ_s, and the mean for teacher *j* at schools *s* would be a consistent estimate of the match effect θ_{js}. Operationally, these estimates could be obtained using a fixed-effects estimator. However, because teachers are typically observed in only a few schools, the likelihood that match effects average out to zero for each teacher is unlikely. This will lead to small sample bias in the teacher, school, and match effect estimates. In this section, I describe a match fixed effects strategy and detail these shortcomings. Motivated by the limitations of the match fixed effects approach, I also propose an Empirical Baye's random match effects strategy that does not rely on large sample properties. I detail these two approaches, highlight important differences between the two, and show that the main conclusions do not hinge on how one estimates school, teacher, and match quality.

IV.1 *Orthogonal Match Fixed Effects*

The first approach is to estimate *orthogonal match fixed effects*. To do this one estimates a model with school fixed effects and teacher fixed effects and then defines match quality as the mean residual for teacher *j* at school *s*. Specifically I estimate (9) below and define the match effect , $\bar{e}_{js}$, as the mean value of the residual for each teacher-school pair.

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \theta_j + \theta_s + \eta_{ijsy} \quad . \tag{9}$$

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

This approach, while straightforward, has two undesirable properties: (1) This mechanically loads match quality that may be correlated with the teacher effect (in small samples) on to the teacher effect and loads match quality that may be correlated with the school effect (in small samples) on to the school effect. This will lead one to understate the importance of match effects, overstate the importance of teacher and school effects, and makes it impossible to determine how much of what we estimate as a teacher effect may be a match effect; (2) the firm, school, and match effects are estimated with error so that the variance of the estimated effects will not accurately reflect the variance of true teacher, school, and match quality.

For the reasons above, the variance of the estimated match fixed effects will understate the importance of match quality. However, they do have an intuitive interpretation. The orthogonal match effects have the 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.

Fixed Effect Based Estimates of the True Variance

Because the raw fixed effect estimates will be biased and are estimated with noise, I estimate a series of covariances to uncover the true variability of teacher, school, and match effects. This is done in two steps. First, I estimate an achievement model like (10) with teacher-by-school-by-year fixed effects (i.e. classroom fixed effects).

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \theta_{jsy} + \eta_{ijsy} \quad . \tag{10}$$

The classroom fixed effect term θ_{jsy} contains a piece attributed to the school, the teacher, and the match between the teacher and the school (that is $\theta_{jsy} \equiv \theta_s + \theta_j + \theta_{js} + \lambda_{jsy}$). Under the assumption that the teacher, school, and match effect are uncorrelated in the data, one can estimate the true 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, $Cov(\theta_{jsy}, \theta_{js'y+1}) \equiv \sigma^2{}_j$. 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, $Cov(\theta_{jsy}, \theta_{j'sy+1}) \equiv \sigma^2{}_s$. 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, $Cov(\theta_{jsy}, \theta_{jsy+1}) \equiv \sigma^2{}_s + \sigma^2{}_j + \sigma^2{}_{js}$. As such, one can obtain an estimate of the variance of match effects from $Cov(\theta_{jsy}, \theta_{jsy+1}) - Cov(\theta_{jsy}, \theta_{js'y+1}) - Cov(\theta_{jsy}, \theta_{j'sy+1}) \equiv \sigma^2{}_{js}$.

To illustrate how ignoring the importance of match effect could lead one to overestimate the importance of teacher effects, I also show naive estimates of the variance of teacher effects under the assumption that match effect are equal to zero. Specifically, if there were no match effects then the covariance across classrooms for the same teacher at the same school will only reflect the teacher effect and the school effect so that with the estimated school variance one would estimate that $\sigma_j^{naive} = Cov(\theta_{jsy}, \theta_{jsy+1}) - Cov(\theta_{jsy}, \theta_{j'sy+1})$. Again, because these estimates assume that the mean of match quality is zero for each teacher *in the data*, these estimates understate the importance of matches and overstate that of teachers.[18] This shortcoming motivates my use of a maximum likelihood random effects approach to estimating match effects.

IV.2 *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 an achievement model like (11) with teacher-by-school fixed effects (i.e. match fixed effects).

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \theta_{js} + \eta_{ijsy} \quad . \tag{11}$$

Note that by estimating a model with match fixed-effects I do not assume that the teacher, school, and match effects are uncorrelated with the included covariates. Then, I take the combined error term $\theta_{js} + \eta_{ijsy}$ (the match effect and the idiosyncratic error term) and estimate a random effects model to decompose the combined residual into a school effect, a teacher effect, and a teacher-by-school effect.

This random effects estimator estimates the variances of the teacher, school, and teacher-by-school effects by Maximum Likelihood under the assumption of joint normality with the covariance structure described in (12), the assumption that the random effects are uncorrelated with the covariates conditional on the estimated first stage coefficients described in (13), and

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

under the fixed effects identifying assumption that the idiosyncratic error term η_{ijsy} is uncorrelated with the random effects.

$$Cov\begin{bmatrix}\theta_s\\ \theta_j\\ \theta_{js}\end{bmatrix}=\begin{bmatrix}\sigma^2_{\theta s}I_S & 0 & 0\\ 0 & \sigma^2_{\theta j}I_J & 0\\ 0 & 0 & \sigma^2_{\theta js}I_M\end{bmatrix}. \tag{12}$$

$$E[\theta_s \mid \hat{\gamma},\hat{\alpha},T,X]=E[\theta_j \mid \hat{\gamma},\hat{\alpha},T,X]=E[\theta_{js} \mid \hat{\gamma},\hat{\alpha},T,X]=0 \tag{13}$$

Similar to the orthogonal match fixed effects approach, teacher, school, and match effects are identified largely by those mobile teachers who are observed in different schools for which the variance on outcomes can be decomposed into a piece specific to the teacher ad another piece specific to the match between the teacher and the school. However, unlike the orthogonal match approach that 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 in all schools) are uncorrelated, but that the actual match, school, and teacher effects observed in small samples (where teachers are observed in a few schools) can be correlated. Specifically, the random effects approach apportions variation between the teacher, school, and match effects to minimize mean squared error. I describe the mechanics of this in Section IV.2.

In addition to the fact that the identifying assumptions of the random effects estimator are more plausible than the more familiar fixed effects approach, this mixed effects procedure is desirable for four reasons; (1) Because the combined residuals are obtained from a model with teacher-by-school fixed effects, the orthogonality condition is satisfied as long as the fixed effects identification assumptions are satisfied; (2) The estimates of the variance of the effects are the maximum likelihood estimates and will not be overstated due to estimation error; (3) This procedure does not mechanically impose the restriction that the mean of the match effects is equal to zero for all teachers (and all schools), but rather apportions variation between the teacher, school, and match effects to minimize mean squared error; (4) Because the match effects and teacher effects are estimated simultaneously, match effects, school effects and teacher effects compete for explanatory power so that <u>one can disentangle good schools and good teachers from those that are correlated with high quality matches (in the data)</u> and one can gauge how much of what we typically estimate to be a teacher effect can be explained by match quality.

IV.3 *Intuition for the mechanics of the orthogonal and mixed match effects estimators*

If all teachers were observed in all schools (that is, one were able to observe all the possible matches in the data), then one could use a fixed effects estimator to precisely estimate school, teacher and match effects because the orthogonality condition in (12) would be satisfied *in the data*, there would be enough variation to cleanly identify good matches from good teachers and good schools, and there would be enough observations so that all the estimates would be precise. However, in the real world this is not the case.

Generally, we 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 only observed in a small number of schools, leading to uncertainty in how much of the variation to attribute to teachers, schools or matches. The difference between the orthogonal match fixed effects and the maximum likelihood random match effects is best illustrated by showing by example how these two estimators deal with 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 <u>very</u> large positive teacher effect, (b) the teacher was very lucky and drew two <u>very</u> large positive matches, or (c) the teacher drew a large teacher effect and two positive match effects (but none of the draws are <u>very</u> 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 deal 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 more desirable.

The orthogonal match fixed effect model mechanically imposes the condition that the mean of the match effects is zero 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. This example illustrates that unless teachers are observed in many schools (where the mean zero match quality assumption may hold for each teacher), the importance of match effects *will be* understated and that of teacher effects overstated in orthogonal match fixed effects models. Moreover, the example illustrates that the estimated orthogonal teacher and match fixed-effects will be biased.

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 is its relative precision, 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 is can be illustrated by how it deals with the scenario from above.

Consider again the teacher who is observed with two large positive matches. This could be because of situations (a), (b) or (c) from above. 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 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 effect estimator uses distributional information (obtained 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 Baye's.

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 effect 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-effect estimates will be biased.

IV.4 *Estimated Variability of Match Effects*

In Table 5, I present (1) the estimated standard deviations of the raw school, teacher, and the match effects under the orthogonal fixed effects approach, (2) the covariance-based estimates of the standard deviations of teacher, school, and match effects, and (3) the maximum likelihood estimates of the standard deviations of the teacher, school, and match effects. For comparison purposes, I also present estimates of the variability of school and teacher quality under the null hypothesis of no match effects. The units are in standard deviations of student achievement.

In models that do not include match effects, the standard deviations of the raw teacher and school fixed effects for math are 0.35 and 0.22, respectively. For reading, the standard deviations of the raw teacher and school fixed effects are 0.356 and 0.247, respectively. As one expects, in models that allow for orthogonal match fixed effects, the standard deviation of the estimated school and teacher effects are virtually unchanged.[19] The standard deviations of the raw match fixed effects are 0.11 for math and 0.118 for reading. As discussed above, this understates the true importance of match quality and overstates the true importance of teacher quality. However, 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. This as a lower-bound estimate of the relative importance of match quality for mobile teachers.

The third and fourth columns present the estimates of the true variability of teacher, school, and match effects based on covariance across classrooms. In models that assume that match effect are equal to zero (i.e. covariance across classrooms for same teacher in the same school are due only to teacher and school effects) the estimated standard deviation of true teacher math quality is 0.1667, and that of schools is 0.0882. These estimates are similar to shrinkage estimates of teacher and school effect in other studies. In models that allow for match effects (i.e. covariance across classrooms for same teacher in the same school are due to teacher effects, school effects, and match effects) the estimated standard deviation of true teacher math quality is 0.1498 (about 10 percent smaller), and that of schools is unchanged. The estimated standard deviation of true match quality in math is 0.0892. These results suggest that about 10 percent 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 percent of that of teacher quality.

In reading the relative importance of match effects is even larger. In models that assume that match effect are equal to zero, the estimated standard deviation of true teacher quality in reading is 0.1095, and that of schools is 0.0504. In models that allow for match effects, the estimated standard deviation of the true teacher quality in reading is 0.0569 (almost 50 percent

[19] It is important to note that the results are similar but not identical with the inclusion of match fixed effects because in the first stage of the orthogonal match fixed effect 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.

smaller). The estimated standard deviation of match effects in reading 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 and sixth columns presents maximum likelihood estimates of the variability of the true effects. The mixed effects estimator suggests that with no match 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, providing confidence in the variability estimates, and underscoring the importance accounting for estimation error. In the mixed effect model that allows for match effects (that can be correlated with school or teacher effects) the standard deviation of true school effects falls to 0.099 for math and to 0.0655 in reading ─ suggesting that match quality can "explain away" about 7 percent of school effects in math and 30 percent of school effects in reading. Where match effects are included, the standard deviation of true teacher effects falls to 0.141 for math and to 0.0837 in reading ─ suggesting that match quality "explains away" about 25 percent of teacher effects in both subjects. The maximum likelihood estimate of the standard deviation of match effects is 0.1302 for math and 0.077 for reading. As such, match effects have about 90 percent of the explanatory power of teacher effects and are more important than school effects for both subjects.

In sum, the results in Table 5 show that *for mobile teachers* the variability in performance within teachers across schools is about on third as large as the variability in performance across teachers. Estimates of covariance across classrooms show that the variability of teacher quality that persists *across* schools is about half that of the variability that persists across classrooms *within* schools for the same teacher such that match quality is between 60 percent and 150 percent as important as teacher quality in determining student achievement. Consistent with the classroom covariance results, the maximum likelihood estimates indicate that match effects are as important as teacher effects, and that match quality can "explain away" about one quarter of the variability in what is typically estimated to be teacher quality.

Having established that match quality exists using data on actual measures of productivity, and that match quality is an important determinant of student achieving *among mobile teacher*, one may wonder if match quality predicts mobility as most theoretical models predict. This is particularly interesting in the teacher labor market context because teacher wages and match quality are unrelated, so that any relationship between match quality and teacher mobility would imply that non-pecuniary aspects of the job that are correlated with match quality are important determinants of worker mobility. I investigate these issues in the following section.

V. Does Match Quality Predict Teacher Mobility

Many models of job mobility predict that match quality should be correlated with teacher mobility. As such, it will be instructive to see if estimated match quality (based on actual productivity) predicts teacher mobility. The documented differences in performance before and after a move imply that teachers switch from bad matches to better matches, however, it does not speak to the effect of match quality on overall turnover (i.e. both switching and exit) and does not also account for other school characteristics that might predict turnover.

To test whether match quality predicts teacher mobility I merge in both the preferred estimated random match estimates (the BLUPs) and the estimated orthogonal match fixed-effects with teacher-level mobility data, and see whether teacher mobility is associated with the match quality at her current school. Specifically I estimate (14) below by OLS.

$$Leave_{jsy+1} = \alpha_1 X_{jy} + \alpha_2 X_{sy} + \overline{\theta}_{js} + \pi_j + \pi_s + \varepsilon_{jsy} \quad . \tag{14}$$

Where $Leave_{jsy+1}$ is an indicator variable equal to 1 if the teacher leaves her current school in year y (i.e. teacher j at school s at time y is not in school s at time $y+1$), X_{jy} is a set of time varying teacher level covariates, X_{sy} is a set of time varying school level covariates, θ_{js} is the estimated teacher-by-school match quality, π_s and π_j are teacher fixed effects and school fixed effects respectively, and ε_{jsy} is the idiosyncratic error term. In this model, the coefficient on match quality measures how much less (or more) likely a teacher is to leave her current school as a function of the match quality between her and the school. A negative coefficient would imply that teachers with high match quality are less likely to leave their current schools (after controlling for observable teacher and school characteristics).

For comparison proposes, I estimate models both with and without teacher and school fixed effects. Teacher effects are included to account for the possibility that teachers with high match quality may also be less mobile for other unobserved reasons, and school effects are included to account for the possibility that schools with high match quality may be more desirable for unobserved reasons. As such, (14) tests for whether *a given teacher* (who moved at least once in the data) was 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 or low mobility and high or low match quality on average*. Results of these models are presented in Table 6.

The observable teacher and school characteristics predict mobility as one would expect. Teachers are more likely to leave low-income schools with high shares of minority students and low student achievement, and teachers are less likely to leave their school if their salary is high. First year teachers are the least mobile (in terms of exit, not switching) and teachers with more than 25 years of experience are the most likely to exit.[20]

Across *all* models, higher match quality is associated with being less likely to leave. In the preferred models (that include both teacher and school fixed effects) presented in columns 5 and 6, the coefficients suggest that a one standard deviation increase in math and reading match quality reduces turnover by about 5.3 and 3.2 percentage points, respectively— an effect similar to that of a 20 percent increase in salary. Relative to a base turnover of about 25 percent, this represents about a 20 percent decrease.

To assuage worries that these patterns are driven by the particulars of the maximum likelihood random effect estimates, columns 7 and 8 show the preferred mobility regressions replacing the random match effects with the estimated orthogonal fixed match effects. The coefficients on the match fixed effects for both subjects are negative and statistically significant at the one percent level— indicating that irrespective of how one estimates match effects, teachers are more likely to stay in schools with which match productivity is high.[21]

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. This relationship is robust to including controls for school characteristics, teacher characteristics, and time invariant teacher fixed effects and school fixed effects. This is consistent with recent findings that those teachers who leave inner city schools are those who were the least effective at that school (E. A. Hanushek, J. F. Kain, et al. 2005, Sass and Feng 2008, Jackson and Cowan 2010). Importantly, these relationships persist conditional on teacher salary so that match quality affects teacher mobility for reasons unrelated to pay. This suggests that non-pecuniary job aspects (such as working conditions, job satisfaction, or in-kind benefits) that are correlated with match effects

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[20] These results are consistent with studies on the determinants of teacher mobility (Hanushek, Kain and Rivkin 2004, Lankford, Loeb and Wyckoff 2002, Jackson 2009).

[21] The point estimates on the fixed effects are smaller than those of the random effects because the fixed effects are estimated with error. In contrast, the random match effects are "shrunken" to reflect estimation error so that the coefficients do not suffer from attenuation bias and are therefore larger than those on the match fixed effects.

exert an important effect on teacher mobility decisions.

VI. The Correlates of Match Quality

The previous sections suggest that productivity match effects exist, are qualitatively important, and are predictive of teacher mobility. In an attempt to gain a deeper understanding of these match effects I do two things. First, I regress the random match effects on observable teacher and school characteristics to get a sense of what kinds of schools and what kinds of teachers are associated with high match quality. This allows me to test one of the central predictions of most models of worker mobility, i.e. that match quality is increasing in experience. Then I take advantage of a workplace conditions survey (conducted in 2002, 2004 and 2006) to see if average teacher responses at the school level are correlated with average match quality at the school. These data are sub-optimal in that I can link teachers survey responses to individual schools but not teachers. However, this allows for more detailed information on school conditions than is typically available. With multiple years of survey data, I am also able to see if *changes* in mean survey responses about workplace conditions are correlated with *changes* in average match quality within schools over time— removing the effect of any potentially confounding unobserved, time-invariant school characteristics that are related to both match quality and workplace conditions.

Table 7 presents the observable correlates of match quality. Columns 1 and 6 indicate 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 effect so that this is not due to more experienced teachers moving to better matches over time*. One important pattern to note is that unlike the relationship between experience and student achievement which is increasing at low levels, is flat between 10 and 20 years, and then declines after 20 years of experience, the effect of experience on match quality is monotonically increasing. In conjunction with the fact that school switching decreases monotonically with experience (section II) this is consistent with the notion that as teachers gain more years of experience they are more likely to have settled into a school with a high productivity match.

To assess whether this relationship between years of experience and match quality reflects a composition effect (i.e. teachers with good matches being more likely to remain in the profession and have more years of experience) or teachers moving to schools with higher match quality over time, I estimate this relationship with the inclusion of teacher fixed-effects. This within-teacher estimate documents the relationship between match quality and experience *among those mobile teachers who switch schools over time*. I present the estimated coefficients in Figure 2 (all estimates are relative to first year teachers and are significant at the 5 percent level). Consistent with the pre and post comparisons depicted in Figure 1, Figure 2 shows that the positive relationship between match quality and experience is due *in part* to teachers moving from schools with lower quality matches to schools with higher quality matches. However, even within teachers, match quality in increasing in experiencing — compelling evidence of the validity of canonical models of job search involving match quality.

Columns 2 and 7 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. For both subjects, possessing an advanced degree is associated with lower match quality, and white teachers have higher match quality in math than other teachers. 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 due to the fact that such teachers have higher match quality. This pattern is consistent with a world in which teachers with fewer teaching options have lower match quality on average as they have less scope for finding a high quality match.

Columns 5 and 10 include both school and teacher characteristics. The results indicate that relative to schools in large towns, average match quality is lower in both math and reading in small towns, large 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), and the percentage of white students at the school is associated with higher match quality in reading. It is worth pointing out that while several relationships between observable teacher and school characteristics and match quality are statistically significant, these covariates can only explain about two percent of the variation in match quality.

In Table 8 I look for the relationship between teacher responses to the workplace conditions survey and match quality. Because this survey is designed to preserve anonymity, the data cannot be linked to individual teachers. They can however be linked to schools. The survey asked teachers questions about leadership quality, time allocation, school standard, opportunities for professional development, and the management style of the school.[22] Respondents were asked to state how much they agreed with particular statements on a 5 point scale in 2002 and a 6 point scale in 2004 and 2006 (where higher values indicate agreement). To allow for comparability across survey years, I normalized all responses to be mean zero and unit variance for each survey. I then take the mean of these responses to each question at the school level to see if these workplace survey responses correlate with match quality at the school.

Columns 3 and 7 present the cross-sectional relationship between mean responses and match quality in math and reading, respectively. Surprisingly, for both math and reading, scores on "Principal is a strong and supportive leader" are associated with lower match quality. Also common across both subjects, "teachers are held to high standards" and "there is an atmosphere of mutual respect at school" are associated with higher match quality. Additionally, for reading, responses to a few questions are statistically significant but follow no consistent pattern. One problem with interpreting this cross-sectional relationship is that other unobserved school factors may explain both match quality and teacher responses to the surveys. For example, schools that have poor match quality may also be those to which particularly strong principals are assigned, making it appear that strong principals cause lower match quality. One way to deal with this concern is to include school fixed-effects to see if *changes* in survey responses within a school over time are correlated with *changes* in match quality within a school over time.

In columns 4 and 8, I present results that include school fixed effects and survey year fixed effects. These results are much more consistent across subjects and none of the results have the unexpected sign. The results indicate that for both subjects agreement with "Teachers have time to collaborate with colleagues", "Teachers are held to high standards", and "New teachers have effective mentors" are statistically significantly associated with higher match quality. However, the combined R-squared of the survey responses and the fixed effects is only 0.08 for reading and 0.09 for math ─ suggesting that school characteristics may account for a relatively small share of match quality. This is unsurprising because match quality is, by definition, an interaction between teachers and schools rather than a school or teacher specific attribute.

Given that researchers have found that teacher effectiveness might depend on the match between the teacher and her students (Dee 2004, 2005; Ouazad 2008) and may depend on the

[22] While there were 38 questions, many of them were asking essentially the same thing. As such, I removed largely redundant questions resulting in 11 questions.

characteristics of her colleagues (Jackson and Bruegmann 2009) I also estimate models that interact observable teacher characteristics with observable characteristics of the students and interact observable teacher characteristics with observable characteristics of other teachers (not shown). Such models yield nothing systematic and explain little of the variation in match quality.

VII. Conclusions

I document that teachers perform better in the classroom after a move to another school than before the move. I present a test of endogenous teacher mobility and I find that teacher effectiveness is likely orthogonal to teacher mobility. Also, I test for student selection and find that students are not differentially selected into classrooms for teachers who will switch school or those who have switched schools. Both these tests lend credibility to the findings.

I then provide the first estimates of match effects using direct measures of worker productivity (as opposed to inferring them from wages) and find that match quality is as important in determining student achievement as teacher quality. The results indicate that the inclusion of match effect reduces the explanatory power of teacher effects by between 10 and 50 percent— indicating that part of what we typically interpret as a teacher quality effect may in fact be a match quality effect that is not portable across schooling environments. Supporting canonical models of worker mobility, teachers at schools with which match quality is high are less likely to exit their schools than those with low match quality (even though there is no relationship between productivity and wages for teachers). I also find that match quality increases in experience, and school *switching* decreases monotonically with experience — patterns consistent with workers switching jobs until they find a high quality match.

These results are important for the literature on worker mobility because they validate previous theoretical and empirical work on worker mobility using wages to infer match quality. Also, I find that match quality predicts mobility in a context where there is no relationship between wages and productivity — suggesting that the reduced turnover at jobs with high match quality is not merely due to worker responses to high wages and that workers may value high productivity matches for reasons other than monetary compensation (such as unmeasured in-kind benefits, work conditions, satisfaction, or social validation associated with performing well).

While I am unable to identify observable teacher or school characteristics that explain a substantial portion of the match effects, the results do provide compelling evidence of match effects, and show that they are predictive of worker mobility. This has important policy implications. The fact that a teacher who performs well in one school may not be as effective in another means that policy-makers should be cautious about identifying strong teachers in one school and moving them to another. Also, the fact that match quality can explain away a sizable portion of the variability of what we typically consider teacher quality means that policy simulations based on teacher quality estimates that do not account for match quality could be quite inaccurate, and that one must consider the working environment in which teachers operate in hiring and firing decisions. From a macroeconomic standpoint, the fact that match quality may be an important determinant of student achievement suggests that student achievement can be increased by achieving the optimal allocation of teachers to schools. Fortunately, the results indicate that teachers tend to leave schools at which they are poorly matched, so that teacher turnover (which is generally considered a bad thing) may in fact move us closer to that optimal allocation, and could have some benefits *all else equal*.

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Tables and Figures

Table 1: *Summary Statistics*

Variable	Observations	Mean	Standard Deviation
Unit of Observation: Student-Year			
Math Scores	1361473	0.033	0.984
Reading Scores	1355313	0.022	0.984
Change in Math Score	1258483	0.006	0.583
Change in Reading Score	1250179	0.001	0.613
Black	1372098	0.295	0.456
White	1372098	0.621	0.485
Female	1372098	0.493	0.500
Parent Ed.: No HS Degree	1372098	0.107	0.309
Parent Ed.: HS Degree	1372098	0.428	0.495
Parent Ed.: Some College	1372098	0.315	0.464
Parent Ed.: College Degree	1372098	0.143	0.350
Same Race	1372098	0.649	0.477
Same Sex	1372098	0.496	0.500
Class Size	1372098	23.054	4.053
Unit of Observation: Teacher-Year			
Experience	91243	12.798	9.949
Experience 0	92511	0.063	0.242
Experience 1 to 3	92511	0.165	0.371
Experience 4 to 9	92511	0.230	0.421
Experience 10 to 24	92511	0.365	0.481
Experience 25+	92511	0.164	0.371
Teacher Exam Score	92511	-0.012	0.812
Advanced Degree	92511	0.197	0.398
Regular Licensure	92511	0.670	0.470
Certified	92511	0.039	0.194

Notes: The few teachers with more than 50 years of experience are coded as having 50 years of experience.

Table 2: *Relationship Between School Switching and Teacher Characteristics*

	1	2	3	4
	Exit School in 1 year		Switch Schools in 1 year	
Experience: 1 to 3	0.015	0.026	-0.002	-0.003
	[0.009]+	[0.009]**	[0.005]	[0.005]
Experience: 4 to 9	-0.023	-0.001	-0.006	-0.007
	[0.009]*	[0.009]	[0.005]	[0.006]
Experience: 10 to 24	-0.102	-0.068	-0.015	-0.012
	[0.009]**	[0.009]**	[0.005]**	[0.005]*
Experience: 25+	-0.064	-0.029	-0.057	-0.054
	[0.009]**	[0.010]**	[0.006]**	[0.006]**
Licensure Score	-0.004	0.001	-0.007	-0.003
	[0.003]	[0.003]	[0.001]**	[0.002]*
Advanced Degree	0.031	0.031	0.003	0.003
	[0.006]**	[0.006]**	[0.003]	[0.003]
Regular License	-0.19	-0.178	0.034	0.045
	[0.008]**	[0.008]**	[0.005]**	[0.005]**
Year FX	YES	YES	YES	YES
School FX	NO	YES	NO	YES
Observations	75303	75303	89856	89856
R-squared	0.03	0.08	0.17	0.2

Robust standard errors in brackets are adjusted for clustering at the teacher level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 3: *Comparing sending and Receiving Schools (By teacher type)*

	1	2	3	4	5	6	7
	Difference in characteristics between receiving and sending school						
	All	White	non-white	Hi Score	Low score	>5 years exp	<10 years
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]**
% Non-white 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 Enrolment	-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
	[0.003]	[0.003]	[0.007]+	[0.005]	[0.004]+	[0.007]	[0.003]
Mid-sized 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	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 an "post switch" indicator variable.
Robust standard errors in brackets are adjusted for clustering at the teacher level.
+ significant at 10%; * significant at 5%; ** significant at 1%.

Table 4: *Teacher Effectiveness Before and After a Move*

	1	2	3	4	5	6	7	8
	Math				Reading			
	Growth	Score	Score	Score	Growth	Score	Score	Score
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	-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	-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	-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	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	1249122	1249122	1249122	1249122	1241150	1241150	1241150	1241150
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%; * significant at 5%; ** significant at 1%

All models include grade and year fixed effects and controls for student race, gender, parental education, and LEP status. Models also include an indicator for the gender and racial match between the student and the teacher, teacher experience, and the class size.

Table 5: *Estimated Variability of Effects*

Math						
	Raw Fixed Effects		Covariance Estimates [a]		Random Effects	
Std. Dev. of School Effects	0.2285	0.2285	0.0882	0.0882	0.106	0.099
Std. Dev. of Teacher Effects	0.3503	0.3506	0.1667[b]	0.1498	0.19	0.141
Std. Dev. of Match Effects	-	0.1121	-	0.0892	-	0.1302
Std. Dev. of residuals	0.5023	0.50704			0.50895	0.5076

Reading						
Std. Dev. of School Effects	0.2475	0.2472	0.0504	0.0504	0.0926	0.06547
Std. Dev. of Teacher Effects	0.3563	0.3564	0.1095[b]	0.05695	0.1107	0.08377
Std. Dev. of Match Effects	-	0.1182	-	0.08785	-	0.0777
Std. Dev. of residual	0.5481	0.5467			0.61125	0.5553

Notes: 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 for 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 effect across different teachers from the same school. The variance of the teacher effect is computed as the covariance between the classroom effect 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.

Table 6: *Match Quality and Teacher Mobility*

	1	2	3	4	5	6	7	8
	Dependent Variable: Leave Current School The following Year							
	OLS	OLS	Teacher Effects	Teacher Effects	Teacher and School Effects	Teacher and School Effects	Teacher and School Effects	Teacher and School Effects
Random Match Effect Reading	-	-0.845	-	-0.555	-	-0.397	-	-
	-	[0.051]**	-	[0.171]**	-	[0.170]*	-	-
Random Match Effect Math	-0.454	-	-0.425	-	-0.456	-	-	-
	[0.026]**	-	[0.082]**	-	[0.078]**	-	-	-
Fixed Match Effect Reading	-	-	-	-	-	-	-	-0.102
	-	-	-	-	-	-	-	[0.032]**
Fixed Match Effect Math	-	-	-	-	-	-	-0.154	-
	-	-	-	-	-	-	[0.032]**	-
Log(Salary)	-0.255	-0.251	-0.327	-0.327	-0.295	-0.296	-0.295	-0.296
	[0.019]**	[0.019]**	[0.035]**	[0.035]**	[0.035]**	[0.035]**	[0.035]**	[0.035]**
% Free lunch at School	-0.012	-0.009	0.066	0.065	0.033	0.032	0.033	0.033
	[0.014]	[0.014]	[0.022]**	[0.023]**	[0.023]	[0.023]	[0.023]	[0.023]
% Black Students at School	0.062	0.062	0.285	0.283	0.216	0.212	0.215	0.211
	[0.011]**	[0.011]**	[0.043]**	[0.043]**	[0.074]**	[0.075]**	[0.075]**	[0.075]**
School: Log(enrollment)	-0.003	-0.004	-0.005	-0.008	0.074	0.071	0.074	0.072
	[0.005]	[0.005]	[0.016]	[0.016]	[0.023]**	[0.023]**	[0.023]**	[0.023]**
School: Mean Reading (Z) Scores	-0.074	-0.069	-0.094	-0.092	-0.068	-0.068	-0.069	-0.068
	[0.007]**	[0.007]**	[0.016]**	[0.016]**	[0.017]**	[0.017]**	[0.017]**	[0.017]**
Teacher Experience: 1-3	0.032	0.031	0.201	0.2	0.226	0.226	0.226	0.226
	[0.009]**	[0.009]**	[0.013]**	[0.013]**	[0.012]**	[0.012]**	[0.012]**	[0.012]**
Teacher Experience: 4-9	0.038	0.039	0.244	0.243	0.289	0.289	0.289	0.289
	[0.010]**	[0.010]**	[0.017]**	[0.017]**	[0.017]**	[0.017]**	[0.017]**	[0.017]**
Teacher Experience: 10-24	0.014	0.017	0.211	0.21	0.272	0.271	0.271	0.271
	[0.012]	[0.012]	[0.022]**	[0.022]**	[0.022]**	[0.022]**	[0.022]**	[0.022]**
Teacher Experience: 25+	0.098	0.102	0.186	0.185	0.229	0.228	0.228	0.228
	[0.014]**	[0.014]**	[0.027]**	[0.027]**	[0.027]**	[0.027]**	[0.027]**	[0.027]**
Licensure Score	0.008	0.007	0.009	0.011	0.007	0.009	0.009	0.009
	[0.003]**	[0.003]**	[0.038]	[0.038]	[0.039]	[0.039]	[0.039]	[0.039]
Advanced Degree	0.049	0.048	-0.016	-0.015	-0.017	-0.016	-0.017	-0.016
	[0.006]**	[0.006]**	[0.020]	[0.020]	[0.020]	[0.020]	[0.020]	[0.020]
Regular Licensure	-0.158	-0.161	0.067	0.066	0.077	0.075	0.077	0.076
	[0.008]**	[0.008]**	[0.019]**	[0.019]**	[0.019]**	[0.019]**	[0.019]**	[0.019]**
Observations	74152	74006	74152	74006	74152	74006	74152	74006

Robust standard errors in brackets. All models include year fixed effects and cluster standard errors at the teacher level.
+ significant at 10%; * significant at 5%; ** significant at 1%

Table 7: *The Correlates of Match Quality*

	1	2	3	4	5	6	7	8	9	10
	Math Match Random Effect					Reading Match Random Effect				
Teacher: 1-3 years exp.	0.007		0.005		0.005	0.002		0.002		0.002
	[0.001]**		[0.001]**		[0.001]**	[0.001]**		[0.001]**		[0.001]**
Teacher: 4-10 years exp.	0.009		0.007		0.007	0.006		0.005		0.005
	[0.001]**		[0.001]**		[0.001]**	[0.001]**		[0.001]**		[0.001]**
Teacher: 10-25 years exp.	0.012		0.01		0.01	0.011		0.01		0.01
	[0.001]**		[0.001]**		[0.002]**	[0.001]**		[0.001]**		[0.001]**
Teacher: 25+ years exp.	0.019		0.018		0.018	0.016		0.015		0.015
	[0.002]**		[0.002]**		[0.002]**	[0.001]**		[0.001]**		[0.001]**
Teacher: Certified		0.013	0.012		0.012		0.006	0.005		0.005
		[0.002]**	[0.002]**		[0.003]**		[0.001]**	[0.001]**		[0.001]**
Teacher: Regular license		0.009	0.007		0.007		0.006	0.003		0.003
		[0.001]**	[0.001]**		[0.001]**		[0.000]**	[0.000]**		[0.000]**
Teacher: License score		0.004	0.004		0.004		0.001	0.001		0.001
		[0.001]**	[0.001]**		[0.001]**		[0.000]+	[0.000]**		[0.000]*
Teacher: Advanced degree		-0.002	-0.004		-0.004		-0.001	-0.003		-0.003
		[0.002]	[0.002]*		[0.002]*		[0.001]	[0.001]**		[0.001]**
Teacher: White		0.015	0.015		0.015		0.004	0.004		0.003
		[0.005]**	[0.005]**		[0.005]**		[0.003]	[0.003]		[0.003]
Teacher: Black		0.001	0		0		-0.001	-0.002		-0.002
		[0.005]	[0.005]		[0.006]		[0.003]	[0.003]		[0.003]
School: Small Town				-0.009	-0.009				-0.009	-0.009
				[0.004]*	[0.004]*				[0.002]**	[0.002]**
School: Large or Midsized city				-0.007	-0.007				-0.008	-0.008
				[0.004]+	[0.004]				[0.002]**	[0.002]**
School: Rural				-0.009	-0.007				-0.009	-0.008
				[0.004]*	[0.004]+				[0.002]**	[0.002]**
School: %White				0.012	0				0.007	0.002
				[0.002]**	[0.002]				[0.001]**	[0.001]*
School: %Free lunch				0.001	0.002				-0.001	0
				[0.002]	[0.002]				[0.001]	[0.001]
School: Enroll				0.004	0.004				0.001	0.001
				[0.001]**	[0.001]**				[0.001]*	[0.001]*
Observations	74676	74676	74676	74665	74665	74528	74528	74528	74517	74517
R-squared	0	0.01	0.02	0	0.02	0.02	0.01	0.02	0	0.02

Robust standard errors in brackets clustered at the teacher level.

+ significant at 10%; * significant at 5%; ** significant at 1%

Omitted categories are "large town" and "zero years of experience".

Table 8: *Match Quality and Teacher Workplace Survey Responses*

	1	2	3	4	5	6	7	8
	Reading				Math			
	School Effect	School Effect	Match Effect	Match Effect	School Effect	School Effect	Match Effect	Match Effect
Teachers have reasonable student loads.	-0.005	-0.005	-0.001	-0.001	-0.015	-0.015	-0.003	-0.002
	[0.003]+	[0.003]+	[0.001]	[0.001]	[0.004]**	[0.004]**	[0.001]**	[0.002]
Teachers are protected from duties that interfere with teaching.	0.002	0.002	-0.001	-0.001	0.008	0.008	0.001	-0.002
	[0.005]	[0.005]	[0.001]	[0.001]	[0.007]	[0.007]	[0.002]	[0.002]
Teachers have time to collaborate with colleagues.	-0.013	-0.013	0.001	0.002	-0.007	-0.007	0.004	0.005
	[0.003]**	[0.003]**	[0.001]	[0.001]*	[0.004]+	[0.004]+	[0.001]**	[0.002]**
Principal is a strong, supportive leader.	-0.021	-0.021	-0.002	-0.001	-0.026	-0.026	-0.003	0.002
	[0.004]**	[0.004]**	[0.001]+	[0.001]	[0.006]**	[0.006]**	[0.002]+	[0.002]
Leaders shield teachers from disruptions.	0.017	0.017	0.001	0	0.03	0.03	0.002	0.002
	[0.005]**	[0.005]**	[0.001]	[0.001]	[0.007]**	[0.007]**	[0.002]	[0.002]
Teachers are held to high standards.	0.026	0.026	0.005	0.003	0.041	0.041	0.009	0.004
	[0.006]**	[0.006]**	[0.001]**	[0.001]**	[0.010]**	[0.010]**	[0.002]**	[0.002]*
New teachers have effective mentors.	-0.001	-0.001	0.001	0.002	-0.013	-0.013	-0.001	0.004
	[0.004]	[0.004]	[0.001]	[0.001]*	[0.006]*	[0.006]*	[0.002]	[0.002]*
Teachers are centrally involved in decision-making.	-0.012	-0.012	0	0.001	-0.025	-0.025	-0.002	-0.002
	[0.005]**	[0.005]**	[0.001]	[0.001]	[0.007]**	[0.007]**	[0.002]	[0.002]
Parents have many avenues to express concerns.	0.022	0.022	0	-0.001	0.028	0.028	0.002	-0.001
	[0.004]**	[0.004]**	[0.001]	[0.001]	[0.006]**	[0.006]**	[0.002]	[0.002]
There is an atmosphere of mutual respect at school.	0.026	0.026	0.002	0	0.03	0.03	0.004	-0.001
	[0.005]**	[0.005]**	[0.001]*	[0.001]	[0.008]**	[0.008]**	[0.002]*	[0.002]
Leadership tries to provide quality professional development.	-0.016	-0.016	-0.001	0.001	-0.025	-0.025	-0.003	0.002
	[0.004]**	[0.004]**	[0.001]	[0.001]	[0.005]**	[0.005]**	[0.002]	[0.002]
Observations	22692	22692	22320	22320	22692	22692	22374	22374
School and Year Fixed Effects Included?	NO	NO	NO	YES	NO	NO	NO	YES
R-squared	0.13	0.13	0.01	0.08	0.1	0.1	0	0.09

Robust standard errors in brackets clustered at the school level.
+ significant at 10%; * significant at 5%; ** significant at 1%

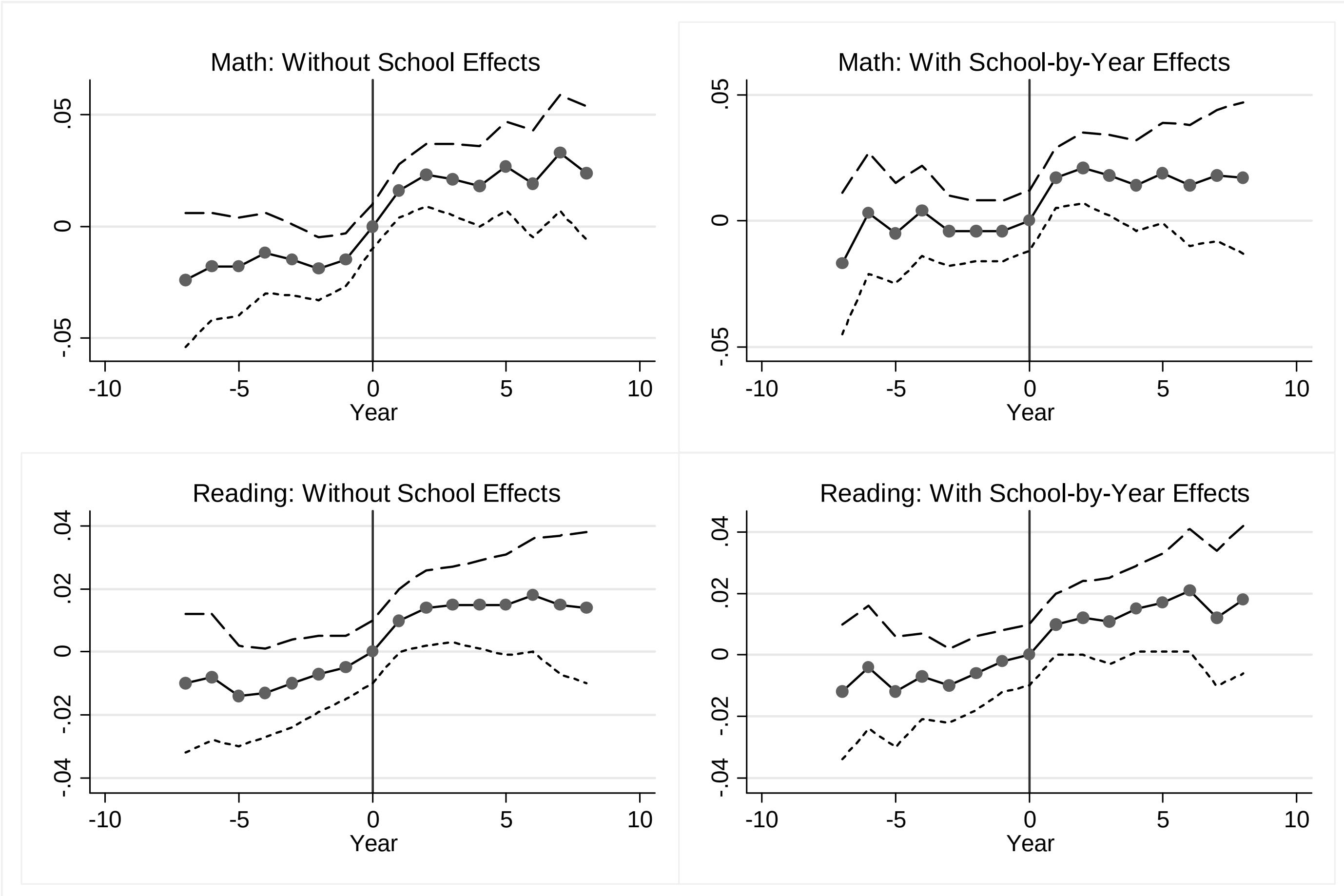

Figure 1: *Change in Teacher Math Value-added Before and After a Move and the 95 Percent Confidence Interval*

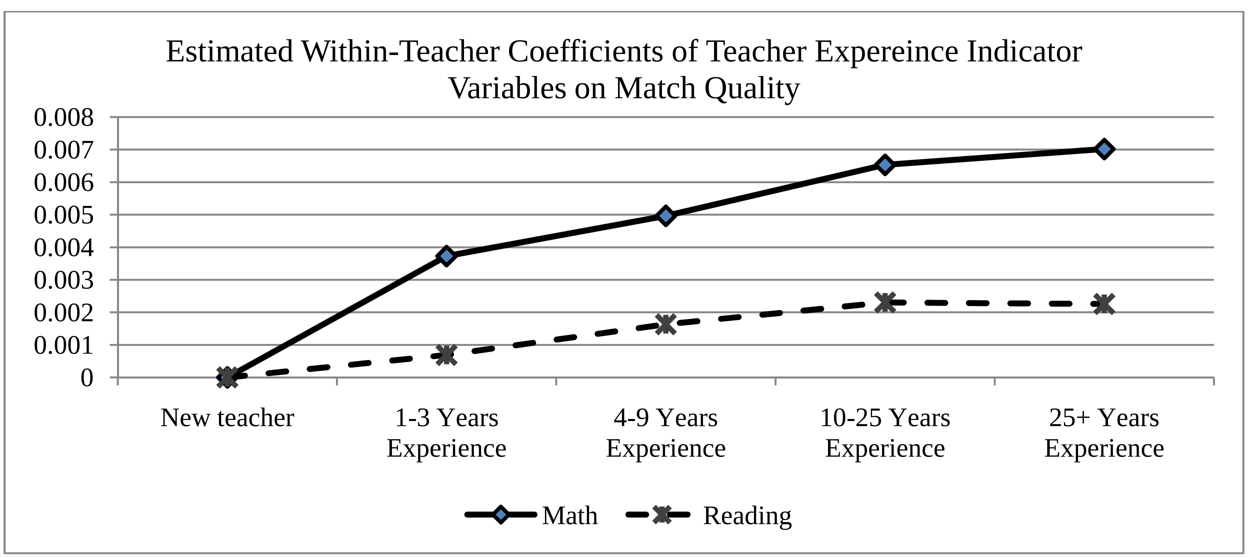

Figure 2: *The Within-Teacher Relationship Between Experience and Match Quality.*