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

C. Kirabo Jackson

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ABSTRACT

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C. Kirabo Jackson
Cornell University, ILR School
Department of Labor Economics
Ives Hall East
Ithaca, NY 14853-3901
and NBER
ckj5@cornell.edu

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C. Kirabo Jackson[1]
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Northwestern University, IPR, and NBER

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 the 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). One of the key roles of the labor market is to allocate workers to firms in the most efficient manner. The hypothesized mechanism through which this efficient allocation emerges is through workers either leaving jobs where the productivity match between the worker and the 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

[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.

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 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). Distinguishing between these explanations is key to assessing whether worker turnover increases allocative efficiency. 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 teachers 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

[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)

obtained in one context can be extrapolated to others.[4] For example, we have very little evidence 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 move schools are more effective after a move than before. 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 schools that are associated with better outcomes. To assuage concerns that these patterns reflect endogenous teacher movement (a worry for all empirical papers on worker mobility) I show that teacher performance does not exhibit any decline or trending prior to a teacher moving schools.[5] To assuage the concerns that non-random assignment of students to teachers drives these patterns, I show that whether a student *will* have a new transfer teacher the following year, or a teacher who *will* leave the school 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 two-stage mixed effects model to estimate the importance of match quality. In the preferred mixed effect models, one standard deviation increase in match quality increases math and reading scores by 0.13 and 0.077 standard deviations, respectively— about the same effect as a one standard deviation increase in teacher value-added. When match quality is accounted for, the explanatory power of teacher quality falls by about 25 percent, suggesting that a non-trivial

[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 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.

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.

Consistent with canonical models of worker turnover, high match quality is strongly predictive of a teacher remaining in her current school (above and beyond observable teacher and school characteristics). This pattern is robust to the inclusion of *both teacher and school fixed effects*, suggesting that it is not driven by (a) mobile teachers having lower match quality on average or (b) schools with high turnover having low match quality on average. All the empirical patterns persist when using estimated fixed match effects or random match effects.

This is strong evidence 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 pattern of results 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.

I also find that match quality is monotonically increasing in experience for mobile teachers (both in the cross-section and based on within-teacher variation) ─ a key prediction of models of worker mobility. One policy implication of this finding is that some of the correlation between observable teacher characteristics and student outcomes may reflect match quality rather than ability *per se*. Finally, I correlate match quality with various observable teacher and school characteristics. While some observable teacher and school characteristics have predictive power, the vast majority of match quality is "unexplained".

This paper makes two important contributions to job mobility literature: First, it is the first paper to validate the extant literature using direct measures of productivity. Second, the paper documents a relationship between match quality and worker mobility in a context where wages and productivity are virtually unrelated ─ underscoring the importance of non-pecuniary benefits of employment. This paper also makes four important contributions to the education literature. First, it is the first paper to highlight and quantify the importance of match effects. Second, it documents that some of the relationship between observed teacher characteristics and student achievement may be due to match quality. Third, it documents that about a quarter of what we call a teacher effect is in fact a match quality effect *that is not portable across schools*.

Fourth, the paper provides direct evidence that teacher mobility leads to greater allocative efficiency. These findings have important implications regarding optimal teacher placement, and they highlight the importance of context for value-added measures of teacher quality.

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 suggestive evidence of match effects and provides tests for endogenous teacher mobility. Section IV describes the mixed effects approach for 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.

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

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 the fact that using wage data to infer match quality has many inherent limitations ─ motivating the use of teacher data linked to student outcomes.

With the growing availability of data linking student outcomes to individual classrooms and individual teachers, the teaching profession is one where *aspects of* worker productivity can be directly observed. One benefit of looking at a direct measure of output, as opposed to wages, is that relative bargaining power does not enter the production function and therefore cannot confound estimates of teacher (worker), school (firm) or match effects. Another benefit is that one need not rely on restrictive (and implausible) assumptions regarding how wages relate to productivity in order to infer worker productivity. Finally, one need not assume away the potential role of in-kind benefits, working conditions, and consumption value in order to infer match quality from wages. As such, analyzing match effects in the teacher labor market allows one to overcome many undesirable features of inferring match effects from wages, allows one to estimate match effects that one can be confident reflect real differences in productivity, and might provide some interesting and new insights.

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 (6) 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 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

[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. See Todd and Wolpin (2003) for a detailed discussion of these assumptions.
[7] All the results are robust to alternate ways of specifying the value-added model.

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.

I.3 *How Does One Interpret Match Quality for Teachers?*

A match effect is anything that makes a teacher more productive at one school versus another (above and beyond mean differences in productivity across schools). Such effects may arise for any number of reasons. 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.4 *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.[8] 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.[9]

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 can see that they are making a difference and less happy at schools where they are not improving student outcomes.

[8] 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)

[9] 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.

The idea that teachers might take pride in "being a good teacher" is similar to that outlined in Akerlof and Kranton (2005) who emphasize that military officers believe in “service before self”, and are willing to trade off monetary rewards for the nonpecuniary gains of being a military officer. Also, workers may care about by their performance relative to their colleagues. If teachers with low match quality are more likely to be among the worst teachers at the school, they may have an incentive 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 nonpecuniary 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

I use data on all third-grade through fifth-grade students in North Carolina between 1995 to 2006 from the North Carolina Education Research Data Center.[10] The student data include demographic characteristics, standardized test scores in math and reading, and codes allowing me 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 and 29.5 percent black, and are evenly divided

[10] 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).

between boys and girls (similar to the full state sample). About 65 percent of students are the same race as their teacher, and about 50 percent are the same sex. The average class size is 23, with a standard deviation of 4. 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-year college degree or graduate degree as their highest level of education. 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 successfully matched to students 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.[11] Roughly 20 percent of teachers have advanced degrees. The variable "regular licensure" refers to whether the teacher has received a regular state license or instead is working under a provisional, temporary, emergency, or lateral entry license. About 67 percent of the teachers in the sample have regular licensure. 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 82 percent of teachers are observed in only one school. About 15 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 98.2 students on average. Matches for mobile teachers contain on average 78 student observations.

III. A Test of Endogenous Mobility and Preliminary Evidence of Match Effects

Before presenting evidence of the importance of school-teacher match quality, it is helpful to observe the evolution of teacher effectiveness before and after a move from one school to another. This allows me to implement (a) a test of endogenous teacher mobility and (b) a test

[11] 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.

that teacher effectiveness varies by her school based on patterns in the evolution of teacher effectiveness before and after a move.

To ensure that I do not confound greater effectiveness after a move (indicative of moving to higher match quality) with teachers moving to higher achievement schools, I include *both teacher and school fixed effects*. In such a framework, the model is identified based on teachers who swap schools. For example, suppose there is a high productivity school A and a low productivity school B and two teachers 1 and 2. With no match effects, if teacher 1 moves from school A to B, while teacher 2 moves from B to A, relative to their performance in school B, both teachers will perform better in school A. Taking the mean differences in outcomes between schools A and B into account, there will be no difference in teacher performance after they move. However, if there are match effects and teachers move to the school with the higher match quality then both teachers A and B will have better outcomes after a move than before (after taking the mean differences in outcomes across schools A and B into account.

While teacher and school fixed effects address many worries regarding bias, one may still worry that time varying school characteristics might affect both teacher performance in the classroom and affect teachers leaving the school. For example, if a school changes principals 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 a move. Similarly, a school may receive a strong principal (or have another sudden event that improves outcomes at the school) at the same time that it attracts new teachers so that the arrival of new teachers at a school coincides with an improvement in general effectiveness at the school.

To address these issues, instead of merely including school fixed effects, I include school-by-year fixed effects (that is, a year fixed effect for each school so that comparisons are made among teachers who teach at the same school in the same year) to control for any school specific event that may affect teacher effectiveness and could be correlated with teacher mobility. To map-out teacher effectiveness over time I estimate the following by OLS.[12]

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \sum_{\tau=-10}^{9} I_{t=\tau} \cdot \pi_{\tau} + \theta_j + \theta_{s\times y} + \eta_{ijsy} \quad . \tag{7}$$

[12] 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.

In (5) T_{ijsa} 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), θ_j is a teacher fixed effect and $\theta_{s\times y}$ is a school-by-year fixed effect. In (7) 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 mobility year (i.e. the omitted category) is the year before a teacher moves schools. Because this model includes *both* school-by-year fixed effects and teacher fixed effects, the model compares a teacher's productivity before and after a move to another school while taking into account the average quality of the school (in a particular year) she moved from and the school she moved to.

Identification of the "years before move" and "years after move" indicator variables is based on those teachers who switch schools in the sample. However, to identify the school-by-year effects, all teachers (including those who do not change schools) are used for identification. As such the "years before move" and "years after move" indicator variables map out the change in outcomes for teachers who switch schools compared to the change in outcomes for teachers who do not switch schools over time, while taking into account differences in achievement across schools that may vary from year to year.

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 moving because their performance is poor *is not* considered endogenous mobility and is exactly the kind of mobility I aim to characterize. The worry would be if productivity before a move is endogenous to the move. 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. There is also the worry that some unobserved event leads to both a reduction in teacher effectiveness prior to a move and to teacher mobility (such as a disruptive hurricane, or a change in principal).

In both these scenarios, one would expect teacher effectiveness to be uncharacteristically low one or two years immediately prior to a move, and for there to be some systematic pattern in teacher effeteness prior to the year a teacher moves. A straightforward test of the null hypothesis that teacher performance the year (or years) directly preceding a move differs from other pre move years is the *p*-value on the hypothesis that all the coefficient on the pre-move indicator variable are equal to zero. If there were some trending in teacher effectiveness prior to a move, if some unobserved event lead to both lower (or higher) 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 compelling evidence that teacher effectiveness is exogenous to teacher mobility.

A Weak Test for The Existence of Match Effects

The second reason for observing teacher effectiveness before and after a move is to establish that teacher outcomes may in fact differ across schooling environments. Most models of worker mobility yield the prediction that workers move from jobs where their match quality is low. In such a scenario, *if workers leave jobs with low quality matches*, when a worker moves one expects that their match quality will be higher on average at the job they move to than the one they left. A straightforward test of the hypothesis that teachers value-added may change depending on the schooling environment is to see if there is any change in a teacher's value-added after she switches schools.

It is important to note that this is a weak test for the existence of match effects because there will only be a difference if all teachers (on average) move to schools with higher (or lower) match quality than their previous school. That is, if teacher mobility is unrelated to match quality then *even if there were match effects*, there would be no systematic difference in teacher effectiveness before compared to after a move. As such, finding that teachers perform systematically better (or worse) after a move is sufficient but not necessary evidence for the existence of productivity match effects.

Preliminary Findings

The "years since move" and "years until move" indicator variable coefficient estimates from equation (8) are presented in Table 2. *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. Columns 1 and 5 show the results with test score growth as the dependent variable with teacher fixed effects for math and reading, respectively. [13] Columns 2 and 6 show the results with test scores as the dependent variable, while controlling for lagged student test scores, with teacher fixed effects for math and reading, respectively. All subsequent models use test scores as the dependent variable while controlling for lagged test scores. Columns 3 and 7 show the results with teacher fixed effects and school fixed effects for math and reading, respectively. Finally columns 4 and 8 present the preferred specification with teacher fixed effects and school-by-year fixed effects for math and reading, respectively. The results are similar across all specifications, so I focus on the preferred specification with the most powerful set of controls (i.e. both teacher fixed effects and school by year fixed effects).

The results are indicative of the existence of match effects and show that teachers move from schools where the productivity of the match between them and the school is low. The point estimates show that teachers are more effective after a move than before for both subjects. Relative to the year before a move, all the post-move indicator variables have positive coefficients for all models, while the pre-move indicator variables are either negative or close to zero and positive. Roughly speaking 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. 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 for math is 0.62 and that for reading is 0.26), 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 for math is 0.0003 and that for reading is 0.007). These tests show that for both subjects while there is little evidence of endogenous teacher mobility, teacher effectiveness is significantly different after a move than

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

before.

To see more clearly how teacher effectiveness evolves over time, Figure 1 plots the estimated teacher effectiveness in math of teachers 8 years before and after a move for the different specifications (from Table 2). Figure 1 makes clear visually what the statistical tests indicate. That is, 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 (i.e. an Ashenfelter's dip) is consistent with teacher effectiveness being exogenous to teacher mobility and there being no unobserved shock that affects both mobility and teacher effectiveness.

Could dynamic student selection drive these results?

Even though the above analysis suggests that endogenous teacher mobility or endogenous teacher effectiveness does not drive the before-after patterns in the data, there is also the worry of endogenous *student* selection. In light of Rothstein (2010) finding that a student's *future* teacher has predictive power in explaining current achievement (evidence that students may sort into classrooms based on dimensions not observed by the econometrician) one may worry that student selection in unobserved dimensions could generate the observed pattern of results. Specifically, 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 outcome before a move is already *prima-facie* evidence that this is not driving the results. However, one can test this possibility directly.

To test for this possibility 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).

The coefficient on the variable denoting whether the student's teacher next year will leave the following year is 0.0007697 (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 -0.0038801 (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.

In sum, the results provide compelling evidence of match effects for both math and reading. 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. While the evidence thus far is highly suggestive of match effects, it is helpful to test for the importance of match quality directly. This is the goal of section IV.

IV. Estimating the Importance of Match Effects

While the results of the previous section are highly suggestive of match effects, and suggest that any estimated match effect are likely to reflect real productivity differences, one may wonder how important such effects may be. Following Woodcock (2008) I employ two approaches to estimating match 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 (8) below and define the match effect , $\bar{e}_{js}$, as the mean value of the residual from (8) for each teacher-school pair.

$$T_{ijsy} = \gamma T_{iy-1} + X_{ijsy}\alpha + \theta_j + \theta_s + \eta_{ijsy} \quad . \tag{8}$$

This approach, while straightforward has three undesirable properties. First, because it identifies match quality based on residuals, the orthogonality condition requires that the match effects are orthogonal to the teacher and school effects. This is a restrictive assumption that loads match quality that may be correlated with the teacher effect on to the teacher effect and loads match quality that may be correlated with the school effect on to the school effect. This will lead one to understate the importance of match effects and makes it impossible to determine how much of what we estimate as a teacher effect may be a match effect. Second, the orthogonality condition normalizes the estimated match effects to be zero for each teacher and school. As a result, teachers who do not move schools are automatically given zero match quality. The third undesirable property (common to all fixed effects estimates) is that the firm, school, and match effects will be estimated with error so that the variance of the estimated effects may not accurately reflect the variance of true teacher, school, and match quality. Because these effects may be estimated with different levels of noise, comparing of the variance of one estimated effect to that of another could be very misleading.

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 (9) with teacher-by-school fixed effects (i.e. match fixed effects).

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

Note that by estimating a model with match fixed effects I do not make the random effects assumption that the teacher, school, and match effects are uncorrelated with the included covariates. Then, I take the combined error term $\theta_{js} + \eta_{ijsy}$ (which includes the match effects, the teacher effects, the schools effects, 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 covariance structure described in (10), the assumption that the random effects are uncorrelated with the covariates conditional on the estimated first stage coefficients described in (11), and *under the fixed effects identifying assumption* that the idiosyncratic error term η_{ijsy} is uncorrelated with the random effects.

$$Cov\begin{bmatrix} \theta_s \\ \theta_j \\ \theta_{js} \end{bmatrix} = \begin{bmatrix} \sigma^2_{\theta s} I_S & 0 & 0 \\ 0 & \sigma^2_{\theta j} I_J & 0 \\ 0 & 0 & \sigma^2_{\theta js} I_M \end{bmatrix}. \tag{10}$$

$$E[\theta_s \mid \hat{\gamma}, \hat{\alpha}, T, X] = E[\theta_j \mid \hat{\gamma}, \hat{\alpha}, T, X] = E[\theta_{js} \mid \hat{\gamma}, \hat{\alpha}, T, X] = 0 \tag{11}$$

This mixed effects procedure is desirable for four reasons. First, 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. Second, the estimates of the variance of the effects are the maximum likelihood estimates and will not be overstated due to estimation error. Third, this procedure does not mechanically impose the restriction that the match effect is equal to zero for teachers who are only in the data at one school, but rather apportions variation between the teacher, school, and match effects to minimize mean squared error. Fourth, since the match effects and teacher effects are estimated simultaneously, match effects, school effects and teacher effects compete for explanatory power so that one can gain some sense of how much of what we estimate to be a teacher effect can be explained by match quality.

While the random match effect are my preferred match estimates, I present results using both the orthogonal match fixed effects and the maximum likelihood random match effects and show that the main results are robust to the chosen procedure. While the results are not driven by the choice of how to estimate the match effects, it is instructive to provide some intuition for how and why these two approaches differ. I present such intuition in the following section.

IV.1 *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 (10) would be satisfied, 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 generally 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. In the worst case a teacher is observed only at one school making it difficult to attribute variation between the teacher and the match. The orthogonal match fixed effect estimator has a simple but undesirable solution to this uncertainly of automatically attributing any uncertain variation to the teacher, while the random match effects estimator uses information about the distribution of the teacher and match effects to create a "most likely scenario" (i.e. Maximum Likelihood estimate) in attributing variation between the teacher and the match.

The difference between the orthogonal match fixed effects and the maximum likelihood random match effects is best illustrated by showing how these two estimators deal with this uncertainly. Consider two scenarios: (1) a teacher is only observed in one school and has a large positive combined teacher and match effect, (2) a teacher is observed in two schools and has large positive combined teacher and match effects in both schools. I detail how each of the estimators deal with this uncertainty below to provide some intuition for how and why these estimates differ, and why the random maximum likelihood estimates are more desirable.

First consider scenario (1). If there is a teacher who is only observed in one school and has a very large teacher-by-school residual it could be for three reasons: (a) the teacher is very good, (b) there match is very good, or (c) the teacher is good and the match is good (but neither is very good). The orthogonal match fixed effect model mechanically imposes the condition that the mean of the match effect are zero for each teacher, so that the match effect is zero for all teachers with only one match. That is, the orthogonal fixed effects model automatically assumes situation (a) and attributes all of the effect to teachers. It is clear that situations (b) and (c) are possible so that by mechanically imposing situation (a) the importance of match effects will be understated and that of teachers overstated in the orthogonal match fixed effects model.

Consider now a teacher who is observed with two matches, both of which are positive and large (scenario 2). This could be because (a) the teacher has a very large positive teacher effect, (b) the teacher was very lucky and drew two very large positive matches, or (c) the teacher drew a large teacher effect and two positive match effects (but none of the draws are very large). 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. These examples make clear that unless teachers are observed in many different matches, where the mean zero assumption is likely to hold for each teacher, the importance of match effects are likely to be understated and that of teacher effects overstated in orthogonal match models.

The random match effects model differs from the orthogonal match effects model in that the estimator distributes the excess variation to both the teacher and match effects in a way that minimizes mean squared error (rather than loading 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 scenarios above.

Consider the teacher observed in one school with a very large positive combined match and teacher effect (scenario 1). This could be due to: (a) the teacher is very good, (b) there is a very large match effect, or (c) the teacher is good and there is a positive match effect. Situations (a) and (b) require one relatively rare event, while situation (c) requires two somewhat likely events to occur. The Maximum likelihood estimator uses information about the variance of the effects, chooses the most likely scenario, and attributes the variance between the match effect and the teacher effect. If the variance of the teacher effects is small, then the less likely is situation (a) and the greater weight those model will put on the match effect under situation (c). Conversely, if the variance of the match effects is small, then the less likely is situation (b) and the greater weight the model will put on the teacher effect under situation (c). This example illustrates how the models uses information about the distribution of the random effects to estimate both match effects and teacher effects, *even when teachers are observed in one school*.

Consider again the teacher who is observed with two large positive matches (scenario 2). This could be because (a) the teacher drew a very large teacher effect, (b) the teacher was very lucky and drew two very large positive match effects, or (c) the teacher drew a large teacher effect and two positive match effects. With only two matches observed, it is difficult to tell these scenarios apart. 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 to create estimates of the teacher and match effects (rather than automatically attributing the excess variation to teachers as in the orthogonal match model).

IV.1 Estimated Variability of Match Effects

In Table 3, I present the estimated standard deviations of the school effects, teacher effects, and the match effects under the orthogonal fixed effects approach and the mixed effects approach outlined above. The first column summarizes the variance of school fixed effects and teacher fixed effects under the fixed effects model with no match effects included. The units are in standard deviations of student achievement. As mentioned above, the variance of the estimated naive effects will include estimation error. In this naive model, the estimated standard deviations of the teacher and school fixed effects for math are 0.35 and 0.22, respectively. For reading, the estimated standard deviations of the teacher and school fixed effects are 0.356 and 0.247, respectively.

The second column summarizes the variability of the estimated school, teacher, and match effects based on the orthogonal match fixed effects model. As one expects, the standard deviation of the estimated school and teacher effects are largely unchanged (because the match effect are orthogonal to the teacher and school effects by construction) and the standard deviation of the estimated match effects is 0.11 for math and 0.118 for reading.[14] As discussed above, this model is likely to understate the importance of match quality. However, even in this model, if one were to compare the variability of match effects to the variability of teacher and school effects (ignoring the contribution of estimation error) one would conclude that match effects are about half as important as school effects and one-third as important as teacher effects.

The third column presents maximum likelihood estimates of the variability of the school and teacher effects that take into account any overstated variability due to estimation error. The mixed effects estimator suggests that 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 smaller than those in the first column (which decomposes the same three way error residual) underscoring the importance of taking estimation error into account. The general point has been made by others (Rockoff 2004, Kane and Staiger 2008) to motivate the use of Empirical Bayes (or shrinkage) estimates of teacher value-added.

The fourth column presents the preferred estimates of the importance of match effects. This model is the mixed effect model that takes estimation error into account and allows match

[14] 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.

effects to be correlated with teacher effects and school effects. Where match effects are included, the estimated standard deviation of school effects falls from 0.109 to 0.099 for math and from 0.0926 to 0.0655 in reading ─ suggesting that match quality can "explain away" 7 percent of school effects in math and 30 percent of school effects in reading. Where match effects are included, the estimated standard deviation of teacher effects falls from 0.19 to 0.141 for math and from 0.1107 to 0.0837 in reading ─ suggesting that match quality can "explain away" about 25 percent of teacher effects in both math and reading. In this model, the estimated standard deviation of the match effects is 0.1302 for math and 0.077 for reading. In other words, when match quality is allowed to compete for explanatory power, match effects have about 90 percent of the explanatory power of teacher effects and are more important than school effects.

In sum, the results in Table 3 suggest that about one quarter of what one would typically estimate as a teacher fixed effect is in fact a match effect that can change depending on the teacher's school. The results also indicate that match effects are quantitatively important in determining student achievement ─ suggesting that policymakers and researchers should aim to understand what types of situations are conducive to creating high match quality. These results suggest that teacher value-added estimates obtained in one school may only be weak predictors of effectiveness in a different schooling environment. To test this implication directly one would need random assignment of teachers to schools ─ a policy that has yet to be tried.

Having established that match quality exists using data on actual measures of productivity, one may wonder if they predict 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 aspect 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

The previous two sections show that teachers tend to be more effective after they move than before, and that match effects have much explanatory power. Given that many models of job mobility predict that match quality should be correlated with teacher mobility it will be instructive to see if estimated match quality (based on actual productivity) has any predictive power in explaining teacher mobility. To test for whether match quality predicts teacher mobility

I merge in both the preferred random match estimates and the estimated 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 (12) below by OLS.

$$Leave_{jsy+1} = \alpha_1 X_{jy} + \alpha_2 X_{sy} + \bar{\theta}_{js} + \pi_j + \pi_s + \varepsilon_{jsy} \quad . \tag{12}$$

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 given school as a function of the match quality between her and the school. A negative coefficient on match quality would imply that teachers with high match quality are less likely to leave their current schools (after controlling for observable teacher and school characteristics).

Teacher fixed 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 fixed effects are included to account for the possibility that schools with high match quality may be more desirable for unobserved reasons. As such, identification in (13) 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 4.

To put the match results in context, and to see what types of schools experience greater teacher attrition in general, columns 1 and 2 show results that do not include school or teacher fixed effects. The results of these intermediate models (columns 1 and 2) indicate that in the cross-section, schools with 10 percentage points more black students experience about 6 percentage points higher turnover, and schools where mean reading scores are one standard deviation lower experience between 7.5 and 6.9 percentage points higher turnover. These results are consistent with studies on the determinants of teacher mobility (Hanushek, Kain and Rivkin 2004, Lankford, Loeb and Wyckoff 2002, Jackson 2009). Relative to rookie teachers, those with 1 to 3 years and 4 to 9 years of experience are about 3.1 percentage points more likely to leave, while teachers with 10 to 24 years and more than 25 years of experience are about 1.4 and 10 percentage points more likely to leave, respectively. Also, teachers with high license scores and advanced degrees (attributes likely associated with better outside options) are more likely to leave, while teachers with regular licensure (a signal of attachment to teaching) are less likely to leave their current school. As one would expect, teachers are more likely to stay in schools with high salaries so that a 20 percent higher salary is associated with being about 5 percentage points less likely to leave the school (this salary effect persists across all models).

Consistent with match quality predicting mobility, in 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 coefficient on the random math match effects is -0.456 and that for the random reading match effect is -0.397 (both significant at the 5 percent level). These models 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 any worries that the mobility 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. As one can see the coefficients on the match effects for both subjects are negative and statistically significant at the one percent level— indicating that however one estimates match effects, teachers are more likely to stay in schools with which match productivity is high. It is worth noting that the point estimates on the fixed effects are smaller than those of the random effects. This is because the fixed effects are estimated with error and are not adjusted to reflect this fact so that the coefficients on these fixed effects are attenuated toward zero. In contrast, the random match effects are "shrunken" to reflect the estimation error so that the coefficients do not suffer from attenuation bias and are therefore larger than those on the match fixed effects.

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, this relationship is conditional on teacher salary so that it shows 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 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 unique in that I can link teachers survey responses to individual schools (but not teachers). This allows for more detailed information on school conditions than is typically available, and may provide some guidance on what kinds of school environments are associated with high match quality. 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 5 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. 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. 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.

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 6 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.[15] Respondents were

[15] While there were 38 questions, many of them were asking essentially the same thing. As such, I removed largely

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. It is worth noting that 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 not surprising given that match quality is, by definition, an interaction between teachers and schools rather than a school specific or a 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 characteristics of her colleagues (Jackson and Bruegmann 2009) I also estimate models that

redundant questions resulting in 11 questions.

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. Given the importance of match effects, this finding indicates that further research is needed into the kinds of teacher school pairings that would be most productive.

VII. Conclusions

Using a data set that allows one to match teachers to student test scores which can then be linked to personnel records, I document that teachers tend to 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 about 25 percent— suggesting that part of what we typically interpret as a teacher quality effect may in fact be a match quality effect that is not be portable across schooling environments. Supporting canonical models of worker mobility, I find that teachers at schools with which match quality is high are less likely to leave their schools than those with low match quality (even though there is no relationship between productivity and wages for teachers) and match quality is increasing in experience.

These results are important for a few reasons. First, they validate previous theoretical and empirical work on worker mobility that use wages to infer match quality. Second, I find that match quality predicts teacher 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). The findings also indicate that part of the observed association between certain observable teacher and school characteristics and student achievement may reflect match quality as opposed to these characteristics actually being more productive. For example, more experienced teachers are more likely to have moved schools and to have settled on a school with high match quality. In such a scenario more experienced teachers will be associated with better student outcomes not because experience increases productivity, but because they have settled in schools in which match quality is higher. This has direct implications for policies that aim to keep experienced teachers in poorly performing schools.

Even though the results provide compelling evidence of match effects, and show that they are highly predictive for worker mobility, I am unable to identify any observable teacher or school characteristics that can explain a substantial portion of the match effects. Given that these match effects are found to be as important as teacher effects (which researchers have also been unable to explain with observables) further research is needed on what they are, what teacher-school pairings are likely to be the most productive, and on what policies and practices may be particularly conducive to ensuring teachers are as productive as possible. From a macroeconomic standpoint, the fact that match quality may be an important determinant of student achievement suggests that average student achievement could be increased by achieving the optimal match between teachers and schools. Fortunately, the results indicate that teachers tend to leave schools at which they are poorly matched, so that teacher turnover may in fact move us closer to that optimal allocation, and could have some benefits.

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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
Peer Experience 0	85490	0.064	0.164
Peer Experience 1 to 3	85490	0.166	0.255
Peer Experience 4 to 9	85490	0.230	0.289
Peer Experience 10 to 24	85490	0.364	0.334
Peer Experience 25+	85490	0.164	0.256
Peer Teacher Exam Score	85490	-0.009	0.578
Peer Advanced Degree	85490	0.198	0.274
Peer Regular Licensure	85490	0.676	0.426
Peer Certification	85490	0.039	0.140

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

Table 2: *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 fixed effects, year fixed effects and controls for student race, gender, parental education, and limited English proficiency. Models also include an indicator for the gender and racial match between the student and the teacher, teacher experience, and the class size.

Table 3: *Standard Deviations of the Teacher, School, and Match Effects*

	Math			
	Fixed Effects	Fixed Effects	Random Effects	Random Effects
Std. Dev. of School Effects	0.2285	0.2285	0.106	0.099
Std. Dev. of Teacher Effects	0.3503	0.3506	0.19	0.141
Std. Dev. of Match Effects	-	0.1121	-	0.1302
Std. Dev. of residuals	0.5023	0.50704	0.50895	0.5076

	Reading			
Std. Dev. of School Effects	0.2475	0.2472	0.0926	0.06547
Std. Dev. of Teacher Effects	0.3563	0.3564	0.1107	0.08377
Std. Dev. of Match Effects	-	0.1182	-	0.0777
Std. Dev. of residual	0.5481	0.5467	0.61125	0.5553

Notes:

Table 4: *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 5: *The Correlates of Match Quality*

	1	2	3	4	5	6	7	8	9	10
	Math Match Effect					Reading Match 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 Mid sized 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: %Freelunch				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 6: *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%

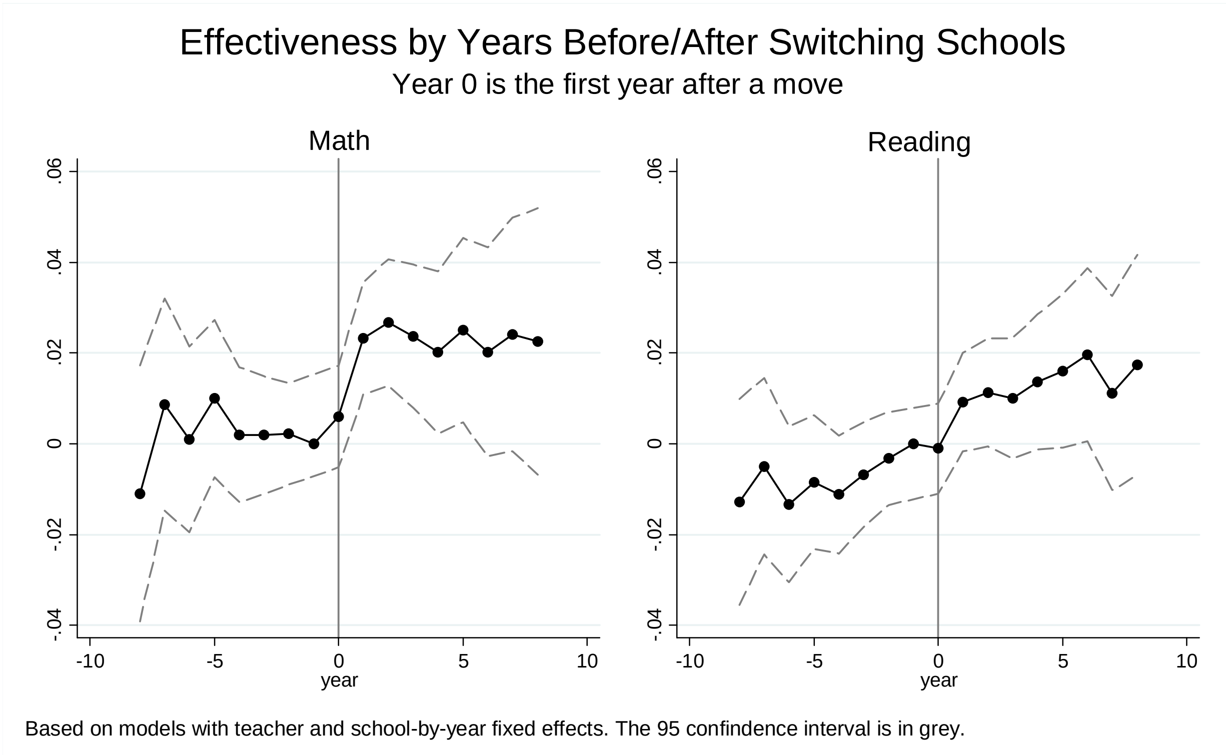

Figure 1: *Change in Teacher Math Value-added Before and After a Move*

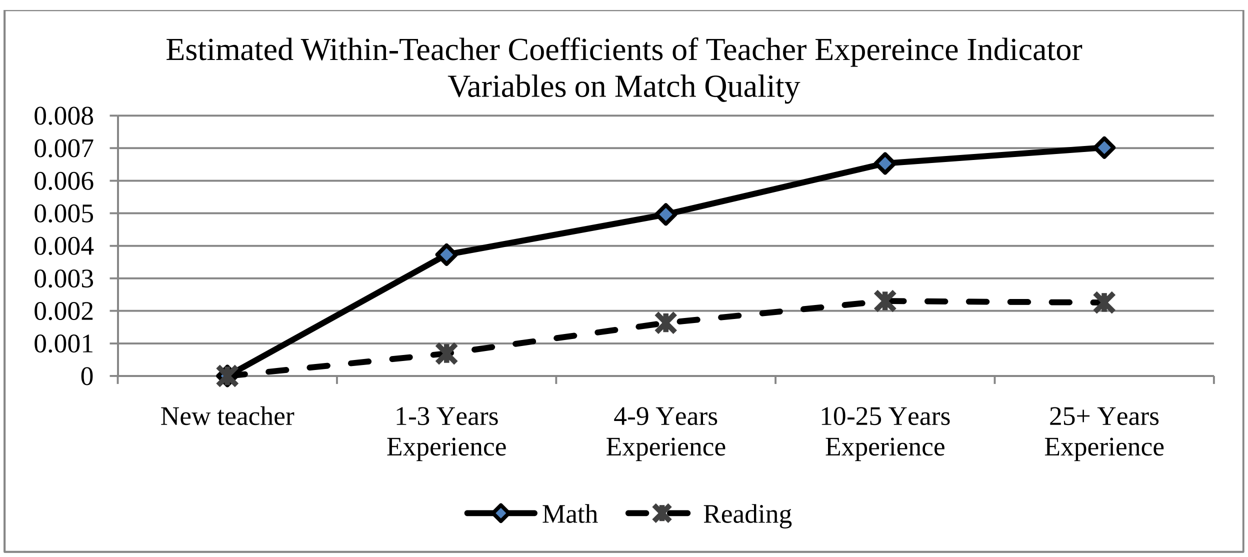

Figure 2: *The Within-Teacher Relationship Between Experience and Match Quality.*