How School Principals Influence Student Learning*

October 2012

Elizabeth Dhuey
Centre for Industrial Relations and Human Resources
Department of Management
University of Toronto

Justin Smith
Wilfrid Laurier University
Department of Economics

Abstract

Many studies examine the importance of teachers, but few quantitative analyses exist regarding the importance of principals to student learning. We measure the effect of principals on gains in student math and reading achievement between grades three and eight in North Carolina from 1998-2009. We estimate the standard deviation of principal value added to be roughly 0.17 standard deviations in math and 0.12 in reading. Extending our base model, we find that the match between principal and school may account for a significant amount of this variation. Experience, and to some extent an advanced degree, are positively related to principal value added. We also evaluate the effect on school inputs and outcomes of replacing the current principal with another of varying quality. Replacing the principal affects few school outcomes regardless of the quality difference between incoming and outgoing principals, except that new principals without prior experience are detrimental to outcomes.

*Elizabeth Dhuey and Justin Smith gratefully acknowledge the financial support of the Social Sciences and Humanities Research Council of Canada. This paper reflects the views of the authors alone.

I. Introduction

In recent decades, relatively weak performance of students in the United States on international tests has sparked research and policy attention about methods of improving student performance. Many policy initiatives have been attempted in an effort to bolster achievement, including increasing school revenue, decreasing class size, expanding early childhood programs, and introducing vouchers and charter schools, to name a few, but not all of these initiatives have had the desired impact. Research has shown that other, less tangible factors such as teacher quality and the characteristics of a student's peers may play a much greater role. This research focuses on one of these less tangible factors: principal quality.

As the manager, principals are responsible for the overall functioning of their school. They direct and supervise the development, delivery, assessment and improvement of the education of all students in their school. Principals supervise teachers, evaluate their performance, assign them to classrooms, create teaching schedules, and make recommendations to the district about hiring or dismissal (or perform that action themselves). Principals interact directly with students by monitoring their conduct, and by disciplining problem students who, for example, are frequently truant or disruptive. They also act as a liaison between school districts and the school itself, implementing policies passed down by state or district authorities then communicating information back up regarding the success of those initiatives.

It is clear given principals' responsibilities within the school system that there are many pathways through which high quality principals might positively affect student academic achievement. One of the primary goals of this paper is therefore to measure principal quality by estimating each principal's value added to student achievement with data on North Carolina students between third and eighth grade. We first estimate a set of pure principal fixed effects from test score gains and find that the standard deviation of principal value added is approximately 0.17 in math and 0.12 in reading. Extending the model to allow a principal's effect to include a component that is fixed across schools and a component that varies across principal-school matches, we find that the standard deviations of the fixed component shrink to 0.04 in math and 0.02 in reading and that the standard deviation of the match-specific component is 0.07 in math and 0.04 in reading.

The second main goal is to use the estimated principal value added from above to determine the attributes and actions that differentiate principals of varying levels of quality. We tackle this question in two ways. First, we regress our estimated principal fixed effects on a set of principal characteristics, including experience, salary, and education. We show that experience as a principal plays a small role in increasing value added in both math and reading, and there is some evidence that having an advanced or doctorate degree increases value added in reading. Second, we assess the impacts on various school inputs and outcomes of the arrival of a new principal at the school. More specifically, using principal fixed effects generated using the strategy from above, we estimate what happens when the incoming principal has higher, lower, or similar value added compared to the outgoing principal. We also assess the impact of replacing the outgoing principal with one that has no experience as a principal in North Carolina. We find that incoming principals with no prior experience reduce math and reading scores, the fraction of students who attend school daily, and the fraction of teachers with more than 11 years of experience. These principals also increase levels of teacher turnover and percentage of teachers with zero to three years of experience. Other significant results are that replacing the current principal with one of lower value added decreases long term suspensions, and replacing the current principal with one of higher value added decreases the fraction of National Board Certified teachers in the school.

We estimate substantial variation across principals in their ability to add value to student academic achievement. This suggests that a school looking to improve student achievement could simply hire a high quality principal. It is unclear, however, what impact this would have on student achievement across all schools, since moving a high quality principal to a new school necessarily involves replacing him with an existing principal of potentially lower quality, or hiring a brand new principal, both of which could lead to a fall in achievement. Thus, the potential benefits of reallocating principals between schools are best seen as equalizing the distribution of achievement rather than a tide that lifts all boats. While our results are clear on the potential effect of hiring a good principal on achievement, they are less clear on how. We find that new principals affect non-test-score inputs and outcomes at the school level, but the results do not point to clear pathways connecting principal quality to student achievement.

II. Existing Literature on the Effect of Principals

Compared to the large, well-established teacher quality literature, there exists a relatively small quantitative literature on principals.[1] Recent evidence finds that more experienced principals improve school performance (Clark, Martorell, and Rockoff, 2009), principals that spend more time dedicated to organizational management lead schools that have higher state-assigned grades (Horng, Klasik, and Loeb, 2009), they import their policies and practices from one school to another (Cannon, Figlio, and Sass, 2012), self-assessment of principal organizational management skills predicts growth in state-assigned school grades (Grissom and

[1] See Hanushek (2006) for a review of the teacher quality literature. See Hallinger and Heck (1998) for a review of the qualitative principal literature.

Loeb, forthcoming), and principals are motivated by the opportunity to change schools (Cullen and Mazzeo, 2007).

In addition, there is a small literature on principal turnover and mobility. Beteille, Kalogrides, and Loeb (2012) find that turnover is detrimental to school performance, whereas in North Carolina, Miller (2010) finds that principal turnover does not have a large effect on school performance, but it does decrease teacher retention. Li (2011) uses a small sample of principals from North Carolina and finds that incentives created by No Child Left Behind decrease average principal quality at schools serving disadvantage students.

A small and more recent literature estimates principal value added to test score gains, using methods similar to those used to estimate teacher value added. Dhuey and Smith (2012) estimate fixed effects for principals in British Columbia, Canada, finding substantial variation across principals in terms of both math and reading value added. This research obtains a cleaner estimate of principal value added and also extends the previous analysis. Grissom, Kalogrides and Loeb (2012) compare and contrast the results obtained from variations of principal value added models to each other and to non-test-based assessments of principal quality. They find that among the models they estimate, measuring principal quality with a principal by school fixed effect is most correlated with non-test-based measures.[2] It is important to note that they do not evaluate the model we estimate in this paper or in Dhuey and Smith (2012), which disentangles principal from school fixed effects. Branch, Hanushek, and Rivkin (2012) use a principal by school fixed effect approach and find significant variation in principal quality, which varies positively with the poverty level of the school. Using aggregate data on teachers, they find that teacher exits are related to principal quality. Finally, Coelli and Green (2012) estimate the lower

[2] However, Chiang, Lipscomb, and Gill (2012) find that using school value-added data does not provide substantial information that is useful for evaluating principals.

bound of the variance of principal effects on graduation probabilities and grade 12 provincial final exam score in British Columbia, Canada. They also estimate a dynamic model of principal effects and find that that a principal's full impact does not happen immediately, but instead occurs gradually over time. To the best of our knowledge, no other research estimates separate principal and school fixed effects, which is vitally important to estimate the true effect of principals quality.

This research expands upon the work of Dhuey and Smith (2012) in several ways. In this research paper, we extend the basic model to allow for "match effects," where part of the principal effect is portable across schools, and part depends on the school where they are employed. We then relate principal characteristics, such as education levels, quality of educational institution, experience, and salary to principal quality. Finally, we calculate how new principals of differing qualities change aspects of the schooling process after entering a new school.

III. Principals in North Carolina

Many of the guidelines and procedures regarding principals in the North Carolina are governed by the North Carolina General Statutes, Chapter 115C Article 19. These laws provide rules regarding the hiring and firing of principals and lists the requirements for an individual to qualify as a school administrator, which includes passing an exam adopted by the State Board along with having a graduate degree or equivalent.

Principals hired by a school district must be appointed by the district's board of education based on the recommendation of the district's superintendent. Vacancies for principal positions are typically posted on a state-wide system. Initial screening of candidates is done by an interview team consisting of district administrators as well as teachers, staff, and parents. The interview team recommends finalists to the superintendent (Miller 2010). The initial employment contract lasts between two to four years and is renewed for four year intervals after the first contract. Once hired, principals are evaluated on an annual basis by either the superintendent or the superintendent's designee. After three years serving as a principal, the principal is eligible for career administrator status which protects them from being dismissed without cause. In addition, prior to appointment as a principal, if the principal had career status as a teacher, the principal will retain career status as a teacher if the principal is not offered a new contract. Principal salaries are set by a statewide schedule that is a function of experience, education and school size. School districts can provide additional salary for principals, this usually accounts for approximately ten percent of total pay (Li, 2011).

These statutes designate twelve powers and duties that the principal possesses. They include: (1) grading and classifying pupils (2) making accurate reports to the Superintendent and to the local board (3) improving instruction and community spirit (4) conducting fire drills (5) disciplining students (6) protecting school property (7) reporting certain acts to law enforcement and the superintendent (8) making available school budgets and school improvement plans (9) evaluating licensed employees and developing mandatory improvement plans (10) transferring student records (11) signing driving eligibility certifications, and (12) establishing school improvement teams.

IV. Empirical Specifications

A. Estimating Principal Effects

To estimate the principal effects, we use the following value-added model of students' test scores.

$$y_{it} = \beta_0 + y_{it-1}\beta_1 + x_{it}^{'}\beta_2 + z_{s(i,t)t}^{'}\beta_3 + p_{p(i,t)t}^{'}\beta_4 + \delta_{p(i,t)} + \varphi_{s(i,t)} + \eta_t + \varepsilon_{it} \qquad (1)$$

where y_{it} is the math or reading score for student *i* at time *t*; y_{it-1} is the student's one year lagged math or reading score; $x_{it}^{'}$ is a vector of student-level demographic characteristics; $z_{s(i,t)t}^{'}$ is a vector of school-level demographic characteristics for the school that student *i* attends at time *t*; $p_{p(i,t)t}^{'}$ is a vector of principal-level time changing characteristics for student *i*'s principal at time *t*; $\delta_{p(i,t)}$, $\varphi_{s(i,t)}$, and η_t are time invariant principal, school, and year effects; ε_{it} is an idiosyncratic error term.

We treat principal effects as parameters and estimate them using ordinary least squares in a fixed effects model. Without school fixed effects in the model, all principal effects can be estimated. These estimates, however, will confound differences between principals with differences between schools. The fixed characteristics of schools may affect principal sorting and therefore should be controlled for to estimate a principal effect unaffected by sorting.

There exists one main difficulty when estimating principal effects and school effects jointly via ordinary least squares (OLS). This difficulty is that with school effects in the model, for each principal we can only identify deviations from a holdout principal in the same "connected group." A connected group is the set of principals who we can link to one another via employment at a common set of schools, and they are formed as principals move between schools over time. Identification, therefore, depends heavily on principal mobility. When a connected group consists of one principal and one school, we cannot identify that principal's effect. Fortunately, principals move frequently in our data, and of 4,415 principals, we can identify fixed effects for 4,289 (97 percent) when school effects are in the model. We parameterize the principal fixed effects so that they are deviations from the average principal in the connected group.[3]

It is possible that our estimates of the standard deviation of the principal effects are upwardly biased due to sampling error. Even in the absence of any real principal effect, we still might observe variations in the estimated effect due to random differences between samples of students. Such sampling variation is a problem particularly when a principal fixed effect is based on a small number of students. To correct for this, we first estimate the variance of the sampling error using the square of the average of the standard deviations of the principal fixed effects. We also check the robustness to other methods used in the literature.[4] Because our principal effects are estimated over very large groups of students, sampling error plays a very minor role

B. What Makes Some Principals Better At Raising Outcomes Than Others?

As a first step towards determining why some principals have higher value added than others, we relate our estimated fixed effects estimated to a small set of principal characteristics, including education, experience, and the state component of annual salary. We estimate the following specification by OLS:

[3] The previously referenced literature on principal's quality does not use this method. Mihaly et al. (2010) provides a nice example that demonstrates the impacts of estimates and their standard errors of changing the holdout observation.

[4] In particular, we examine methods used by Rothstein (2010), which is similar to the method we use, but it weights the average of the standard errors, and by Rockoff (2004), which makes assumptions about the distribution of the underlying true principal effects and estimates the variance using maximum likelihood.

$$\delta_p = \varphi_0 + Ed_p\varphi_1 + Exp_p\varphi_2 + Salary_p\varphi_3 + \zeta_p \tag{2}$$

The vector of education variables, Ed_p, contain indicators for whether the principal has a bachelors, masters, doctorate, or advanced degree from a competitive or non-competitive institution. The methodology we use to determine the competitiveness of the principal's degree is described in Section V.C. Experience (Exp_p) and salary ($Salary_p$) are each averaged over all years the principal is observed in the data, and enter the specification as cubics. As the dependent variable, we use an Empirical Bayes shrinkage estimate of the principal fixed effects, as these are most appropriate when using estimated fixed effects in subsequent regressions.[5] This regression is meant to be descriptive only, so we attach no causal interpretation to the coefficients.

We also measure the effect on various school outcomes of a change in principals. We estimate specifications of the following form:

$$O_{st} = \alpha_0 + d_{st}^{rise}\alpha_1 + d_{st}^{fall}\alpha_2 + d_{st}^{same}\alpha_3 + d_{st}^{new}\alpha_4 + O_{st-1}\alpha_5 + z_{st}^{'}\alpha_6 + \theta_s + \lambda_t + \upsilon_{it} \tag{3}$$

The independent variables of interest are a set of four dummies indicating the type of principal change a school experiences. The variable d_{st}^{rise} equals one when the school receives a new

[5] In addition to shrinking our original estimate of the standard deviation of principal effects estimated above, we also compute the standard deviation of an Empirical Bayes estimate of the principal effects. As in Jacob and Lefgren (2005), if $\delta_p \sim N(0, \sigma_\delta^2)$, then conditional on observing our "noisy" estimate of the principal effect, $\hat{\delta}_p = \delta_p + \nu_p$, a Bayesian estimate of the mean of the principal effect would shrink our existing estimates based on a signal to noise ratio:

$\delta_p^* = E[\delta_p \mid \hat{\delta}_p] = \left(\dfrac{\sigma_\delta^2}{\sigma_\delta^2 + \sigma_\nu^2}\right)\hat{\delta}_p$. If our estimate $\hat{\delta}_p$ contains mostly noise, then the estimates shrink toward zero.[5] Using our estimates of σ_δ^2 and σ_ν^2, we can construct the Empirical Bayes estimate δ_p by substituting those values into the equation above. Estimates will shrink more toward zero the higher the noise matters relative to the signal. We use the estimates from this estimator in all further analysis.

principal, and the new principal has higher value-added than the departing principal. Similarly, d_{st}^{fall} equals one when the incoming principal has lower value added than the outgoing principal, and d_{st}^{same} equals one when the incoming principal and outgoing principal have similar value added. Finally, d_{st}^{new} equals one when the incoming principal is not previously observed in the data. Differences in value added between the incoming and outgoing principal are assigned to rise, fall, or stay the same based on terciles of the difference in principal value added from the Empirical Bayes shrinkage estimate among all principal switches in year *t*.

Prior to estimating equation 3, we estimate principal value added with equation 1, using the previous five years of data up to year *t-1*. For example, if school A changes principals in 2004, we will measure the difference in value added between the incoming and outgoing principal using data from 1998 to 2003 for both principals.[6] This method excludes current and future test score data in creating the value added measure to avoid introducing simultaneity between school outcomes and the indicator variables on the right hand side. As controls in equation 3, we include a set of school level variables (z'_{st}) and school fixed effects (θ_s) to account for other factors that might be correlated with principal moves and school outcomes. In robustness checks we also estimate specifications with school-specific linear time trends to account for any changes over time in outcomes that might be specific to a school.

These regressions estimate whether schools that change principals have different outcomes than schools that do not change principals, and whether such differences in outcomes vary with principal quality, as measured by previously determined test-based value added measures. Our school outcomes contain mainly non-test-score based variables, so in many of our regressions we are estimating whether increases or decreases in the ability of the school leader to

[6] We need to use a constant prior time period so that the principal effects are comparable across time.

produce test score gains can affect other school inputs and outcomes. As the specification is written above, the coefficients on the dummies measure the immediate impact of a change in principal during the first year. We also estimate regressions with dummies that allow the effect to occur in the two additional years following the year that the principal changed. This allows the effect of principal switches to occur more gradually over time.

The school outcome measures we use are (1) percent AYP targets met; (2) number of crimes per one hundred students; (3) number of long term suspensions; (4) percent daily attendance; (5) percent of teachers with advanced degrees in t+1; (6) teacher turnover rate between t and t+1; (7) percent licensed teachers in school in t+1; (8) percent National Board Certified teachers in school in t+1; (9) percent of classes with highly qualified teachers; (10) percent of teachers in school with 0-3 years of experience in t+1; (11) percent of teachers with 4-10 years of experience in t+1; and (12) percent of teachers with eleven or more years of experience in t+1. The teacher measures come from t+1 so that we do not pick up the spurious effect of teachers and principals moving concurrently, since we are interested in changes in these variables that occur after the principal arrives.

V. Data and Analysis Sample

A. Data Sources

The primary data comes from all public schools in North Carolina from the 1998/99-2009/10 school years. This data come from administrative records from the North Carolina Department of Public Instruction maintained and distributed by the North Carolina Education Research Data Center. This data includes a multitude of information on students, teachers, schools, districts, and classrooms in North Carolina. Most importantly for this research, it also includes information on principals. The data includes identifiers for principals, teachers, and students that permits statewide linkages over time. The data from the North Carolina Education Research Data Center has been supplemented with data from the 2000 and 2010 U.S. Censuses which was attached to the primary data via the zip code of each school.[7]

B. Regression Sample

Because we use value-added model in test scores to estimate principal quality, we restrict our focus to students that have a valid math or reading scores in year t and a valid math or reading score in year t-1. In the 1996/97 school year, the ABCs of Public Education was passed in North Carolina, This accountability legislation required all students in grade three through eight to write standardized tests in math and reading at the end of each school year. We use these standardized tests as our reading and math scores. There are 5,407,020 student-years observed between grade 4 and grade 8 between 1998/99 and 2009/10 school years that have valid test scores in both year t and t-1. We drop 9,724 observations from students who attend schools with less than 10 students. We also drop 491 observations from students who are too far ahead or behind in school for their age. Finally, we drop 8,262 observations that we cannot link to a particular school. Our final analysis sample consists of 5,388,543 student-year observations.[8]

C. Descriptive Statistics

Table 1 presents summary statistics on test scores and students demographics based on the analysis sample. The third- through eighth-grade math and reading test scores are

[7] The process for attaching the census data to school records is described in detail in a data appendix and is available from the authors upon request.
[8] Our results from the analysis are similar if we drop middle/junior high schools from the analysis.

standardized to have a mean of zero and standard deviation of one in the population. The means of these test scores are slightly positive in our analysis sample, which indicates that the students who were excluded from the sample scored marginally worse than the students who were included in the sample. About 29 percent of the sample is black, five percent is Hispanic and another five percent is classified as other race. In addition, about 24 percent of the sample is in special education and approximately 6 percent of the sample is either learning disabled in math or reading.

Table 2 shows descriptive statistics for schools, school neighborhoods, and principals in the analysis sample. There are 1,954 schools that contribute to the sample over time. Approximately 58 percent of those schools are Title 1 eligible schools and 7.4 percent are magnet schools. Forty-one percent of students in the sample are eligible for free or reduced lunch. About 22 percent of the 4,415 principals are in their first year of tenure at a school, 19 percent in the second year, 15 percent in the third year and 44 percent remain at the same school for four or more years. This shows considerable turnover of principals within schools, which helps identify the principal fixed effects. About 14 percent of the principals are in their first year of employment as a principal, and 62 percent have been a principal for 4 or more years. The average monthly salary from the state of North Carolina is roughly $6000. About 28 percent of the principals have an advanced degree and nine percent have a doctorate degree. Advanced degrees generally apply to graduate degrees that do not increase salaries, and staff are not required to report them. We also include a measure of competitiveness of the principal's undergraduate and graduate institutions. Following the literature on teacher quality, we assigned each principal's institution a competitive ranking based on information from the Barron's Profiles of American Colleges, 25th edition. 16 percent of principals went to a "competitive" school for their bachelor's degree whereas only two to four percent of principals went to a "competitive" school for the advanced or doctorate degrees.

D. Principal Mobility

Because principal value added estimates from equation 1 rely on mobility when school effects are included in the model, it is informative to examine how principals move between schools in North Carolina. Table 3 describes the mobility of principals in our sample and contains basic statistics about the number and mobility of principals in North Carolina between 1998 and 2009. Between 1560 and 1790 principals are employed each year, and between 197 and 292 of them are newly hired into the system. The main factors affecting the number of principals hired each year are retirements in the previous year and school openings or closings in a particular year. Between 74.6 and 80.2 percent of principals stay in the same school from one year to the next. 8.3 to 10.5 percent moved to a different school within North Carolina and 10.2–14.9 percent leave the sample. Of those that leave the sample, approximately 20 percent continue to work for the North Carolina Department of Instruction in another role such as a superintendent, assistant principal, teacher, or other educational support staff. The other 80 percent include principals who retired, those who moved out states, or those who left the public education sector.

The identifying assumption in our main estimation strategy for principal effects is that mobility is exogenous conditional on all the control variables that appear in our model. If principals prefer schools that have particular fixed attributes, this will not affect the causal interpretation in our model with school fixed effects or in our model without school fixed effects if we control for those fixed factors. However, the estimates of the principal effects may be biased if there are time-varying characteristics of a school or community that relate to principal mobility and student achievement.[9] Therefore, we control for a large variety of time-varying characteristics in all our analysis (see Table 1 and Table 2).

To examine this issue in detail, we empirically evaluate the determinants of principal mobility in Table 4. We include all schools in our sample and regress an indicator for a principal move between t and t+1 on school, principal, and community characteristics at time t (and in some specifications, t-1). In Columns 1-6 the dependent variable equals 1 only if the principal moves from one school to another school in the sample. In columns 7-12, the indicator equals 1 for any move, including switches to schools out of the sample or exits from the sample. We report estimates for a variety of different test score measures, including the average school level math and reading scores, the lag average school level scores, the average gain scores in math and reading, the lag average gain scores, the year-to-year difference in levels scores for each school, and the difference in school average gains between year t and t-1. None of these coefficients are statistically significant. The same exercise is repeated in Columns 7 through 12 using all principal switches. In this case, there is some evidence of principal turnover based on math test scores.[10] Appendix Table 1 reports the coefficients for the other variables in each specification. There is some evidence that the fraction in special education and number of students in a school is related to mobility in the school to school movers.[11] Title 1 eligible schools and fraction of black individuals in a community are related in the all-movers regression. However, the main

[9] For example, if schools districts choose not to renew contracts for principals who perform poorly on current or lagged test scores due to random fluctuations or one-time shocks to student performance, and if these test scores are mean-reverting, we may mistakenly attribute an improvement in scores to a new principal when in fact it is just mean reversion.
[10] All scores are measured in student-level standard deviations. Note that one student level deviation is roughly equal to two school-level standard deviations.
[11] Number of students in a school is related to mobility because principal salaries are based partly on the size of the school and therefore principals have an incentive to move to larger schools.

significant predictor of mobility in all specifications is principal tenure in a school. The more tenure in a school, the more likely the principal is to move. Therefore, we cannot be certain that mobility is unrelated to unobservable factors that change during our time period, but these results provide some evidence that very few observable factors are related to mobility.

VI. Results

A. Variation in Principal Quality

Table 5 reports standard deviations for the estimated principal value added from equation 1. Without school fixed effects the standard deviation of principal value added in math is 0.127. The standard deviation increases to 0.184 when school effects are included. Adjusting for sampling error reduces both estimates, but by very little, reflecting large numbers of students lead by each principal. There is less variation in principal value added in reading, with a standard deviation of 0.097 without school effects, and 0.136 with school effects.[12] It is vital to control for school fixed effects when calculating principal value added as it is important to disentangle the effect of an individual principal from the fixed characteristics of the school that he or she leads. If principals sort into schools based on fixed characteristics of the schools, our estimates of the principal fixed effect will be biased.

Our analysis thus far provides estimates of principal effects that are fixed across schools (and time). It is entirely plausible, however, that a principal's effectiveness varies across schools. These "match effects" might arise if a principal's effect on student achievement depends on

[12] We also ran the analysis from Table 5 using only principals that switch from school to school and the results are similar to results with the entire sample.

things like interactions with the existing teaching staff, demographic composition of the student body, location preference of the principal, or any other complementarity between the principal and school. Woodcock (2011) outlines two different methods for estimation of match effects alongside principal and school effects. The first is an orthogonal fixed effects estimator, which identifies match effects by imposing that they are orthogonal to the principal and school effects.[13] The second is a hybrid random effects estimator, which first nets out the effect of observables from the dependent variable using the usual fixed effects assumptions, then estimates the variances of the principal, school, and match effects, treating them as random effects.[14] Because identifying match effects separately from principal and school effects demands a lot of the data, and because both estimation methods have potential downsides, we present match effect estimates for descriptive purposes only, and do not use them for further analysis.[15]

The orthogonal fixed effects estimates reported Panel A of Table 6 show standard deviations of fixed principal and school effects that are virtually identical to estimates obtained from equation 1. The match effects estimate for math is 0.014 for math and 0.012 for reading. This suggests a small match component to student test score gains. The hybrid random effects results in Panel B, however, are very different. The standard deviation of the principal effect shrinks to 1/5 of its size in math, and just above 1/10 of its size in reading. Match effect standard

---

[13] The estimator proceeds by estimating the parameters in β and the experience effects by OLS using principal by school fixed effects (i.e., match fixed effects). It then take the residual plus fixed effects from that regression, computes within-match means, and regresses those on separate principal and school fixed effects. The residuals from this second regression are the match effects.

[14] The estimator first estimates the parameters by OLS using principal by school fixed effects. Then these estimated parameters are used in a restricted maximum likelihood framework to estimate the variance of the principal, school, and match effects. These estimated variances are then used to predict the fixed effects. Jackson (2010) uses a similar estimation strategy to examine teacher matches.

[15] The orthogonal fixed effects estimator mechanically forces match effects to sum to zero for each principal and school. All matches with one principal or school are therefore assigned a zero match effect. This is problematic in our data because a fair number of matches have one school. The hybrid random effects estimator assumes orthogonality between the three effects and the error term, which may not be justified.

deviations rise considerably to 0.073 in math and 0.042 in reading. Based on these estimates, much of the principal effect we observe may be related to the match between principal and school rather than an effect that is portable by principals across schools. This is important as it indicates that good policy will take into account match effects when allocating principals across schools.

Given the substantial variation across principals in their ability to influence student test score gains in both math and reading, it is natural to ask why some principals are better at generating gains than others. As a first step towards answering this question, we take the Empirical Bayes shrinkage estimates of the principal effects and regress them on a set of variables that measure principal experience, salary, and education. There is one observation per principal in these regressions. Prior to estimation, measures of experience and salary were averaged for each principal over all years they are observed in the data, then centered around the average across principals.

Results from this exercise are located in Table 7. One extra year of experience above the mean yields a relatively small 0.0035 standard deviation improvement in principal value added in math, and 0.0049 standard deviation improvement in reading. A salary increase of $1000 has no statistically significant impact on math value added, but reduces reading value added by 0.074 standard deviations. In terms of education, obtaining a doctoral degree from a non-competitive institution has a small positive (and marginally statistically significant) impact on both reading and math value added, while competitive and non-competitive advanced degrees positively influence reading value added.[16]

[16] The results for experience and education are not substantially different if salary is omitted from the analysis.

To summarize the results in this section, we find considerable variation in principal value added in both reading and math in North Carolina. While our main estimates attribute these differences across principals entirely to fixed attributes of the principal, our match effect estimates suggest that at least part of the principal's effect depends on the match with the school. In describing what makes high value added principals better able to improve student gains relative to an average principal, we have determined that experience plays a small role, and that education may also determine such differences, particularly in reading.

B. Principal Changes and School Outcomes

In this section, we analyze whether principals of varying levels of value added improve or decrease their students' outcomes along with examining how these principals change school inputs and outputs. We present results of regressions of various student and school outcomes on indicators for changes in principals where the incoming principal has either higher, lower, or the same value-added as the outgoing principal, or the incoming principal is in their first year in the data.[17] For these regressions, differences in value added are based on the average of each principal's math and reading value added. The goal is to estimate what happens in the year (or few years after) a school receives a new principal. We test whether improvements in the test score value added of the principal translate into improvements in other inputs or outcomes when that new principal arrives. Conversely, we also test whether a reduction in value added leads to a fall in these outcomes. We also test whether the test score value added of the principal is related to changes in school inputs such as the teacher complement. This analysis informs to what may

[17] While we cannot be sure that all principals observed for the first time in our data are brand new principals (since they may have worked as a principal in another state or at a private school), we interpret them as brand new principals.

happen if a school engages in a principal switch along with explores what kinds of policies are exploited by high/low quality or new principals.

Table 8 first examines what happens to school-averaged test score gains when a new principal arrives. In columns 1-2 we analyze math scores as the outcome, and correspondingly measure the difference in value added between the incoming and outgoing principal. Columns 3-4 do likewise with reading scores.[18] In Panel A, we examine the immediate impact in year *t* of a principal change. Switching to a principal with lower, similar or higher value added does not significantly change gain scores in the year the new principal arrives in columns 1 and 3. Replacing the outgoing principal with a principal not previously seen in the data decreases math scores by roughly 0.010 standard deviations and reading scores by 0.005 standard deviations. Including school-specific time trends causes the estimates for the first to lose their statistical significance and become slightly attenuated in magnitude for the results for new principals. However, a positive association is found between gaining a higher valued added principal using school specific linear trends on math score gains.

In Panel B, we extend the analysis to allow for the effects of changes in the principal to occur over three years. The coefficients thus measure the cumulative effect in years *t* to *t+2* of changing the principal at the beginning of year *t*. We continue to see improvements in test scores when a high value added principal replaces the outgoing principal in math. The cumulative effect on math scores is 0.021 standard deviations. The effect of a principal with no experience is roughly similar to the year *t* impact. Note that with the exception of small changes in magnitudes, results are robust to including school-specific time trends to the specification.

[18] Recall that changes in value added between the incoming and outgoing principal use only data in the previous five years up to year *t-1*, so there is no mechanical correlation between the outcomes and the dummy variables in the right hand side of the regression.

In addition to test scores, we also look at the effect of principal changes on a variety of school level inputs and outcomes. These are related to principal actions or policies that might have an indirect effect on future student performance, and should therefore give some perspective on how principals of varying value added affect student outcomes. We examine the school's percent of AYP (as defined by the No Child Left Behind Act) targets met, measures of student disruption such as crime and suspensions, average daily attendance, and several measures related to teachers like turnover, licensing and education, and experience. This should paint a fairly broad picture of what types of inputs and outcomes principals do and do not influence upon switching schools.

The coefficients in Panel A in Table 9 measure the immediate impact of a principal change on the outcomes listed above.[19] Replacing the current principal with one of lower value added decreases the fraction of long term suspensions. This might indicate that these principals have less of a handle on disruption in the school or it might indicate that there actually is less disruption in the school overall. Replacing the current principal with one of higher value added decreases the percentage of National Board certified teachers and increases the fraction of new teachers. Clotfelter et al (2007) and Goldhaber and Anthony (2007) both find that teachers may be less effective, where effectiveness is measures by test score gains, after receiving National Board Certification. In addition, these teachers are more expensive. In North Carolina they receive an increase of 12 percent of their base pay for certification. These facts may explain why we see a decrease of these teachers when a high value added principals enters a new school. We also find that replacing the current principal with a principal previously not seen in the data prior increases teacher turnover rate. Ronfeldt et al. (forthcoming) find that students who experience

[19] We reestimate these coefficients with school-specific linear time trends, and report the results in Appendix Table 2.

higher turnover score lower in both English language arts and math and that there exists a disruptive effect of turnover that is separate from the possible changing distribution of teacher quality within a school.

In Panel B, we examine the cumulative effect over three years of a principal change. Coefficients are much the same as they were in Panel A, with a couple of notable exceptions. Within the three years of a principal change to one with no experience as a principal, there is a decrease in percent daily attendance along with an increase in low experienced teachers and a decrease in teachers with 11 or more years of experience. In addition, we also continue to find an increase in teacher turnover rate.

The results in Table 9 do not reveal a large number of significant effects of new principals on non-test score inputs and outcomes, but there are interesting results. The relationships between teacher characteristics and principals new to the sample suggest that having a brand new principal may be very detrimental to students over the first three years of that principal's tenure. Due to the lack of relationship between many of the student discipline and teacher measures and principal's value added measure, the only clear picture that emerges from these results is that the inputs and outcomes we examine cannot fully explain why good principals improve student value added. Therefore, it is clear from these results that further investigation is warranted into principal characteristics that are related to principal value added measures of success.

VII. Conclusion

We estimate the impact of fixed principal characteristics on performance and find that principals have a large impact on both math and reading scores. We also estimate principal match effects and find that much of the principal effect that we observe may be related to the match between principal and school. In addition experience as a principal plays a small role in principals being able to improve student gains. Finally, high/low valued added principals are able to increase/decrease math and reading test scores whereas new principals decrease math and reading test scores when entering a new school. We examine a variety of school inputs and outcomes to try to disentangle what makes a high or low value-added principal better or worse at their job. We find very little evidence using a wide variety of inputs and outcomes.

These results have important implications for policy. The main implication is that shifting principals between schools has the potential to significantly reduce achievement gaps. Policy makers can identify the most effective principals using available test score data, and allocate them between schools to potentially reduce achievement gaps. In addition, these results indicate that much more work needs to be completed to uncover what makes a good principal good.

VIII. References

Branch, Gregory F., Eric A. Hanushek, and Steven G. Rivkin. 2012. "Estimating the Effect of Leaders on Public Sector Productivity: The Case of School Principals." NBER Working Paper 17803.

Beteille, Tara, Demetra Kalogrides, and Susanna Loeb. 2012. "Stepping Stones: Principal Career Paths and School Outcomes." *Social Science Research*.

Chiang, Hanley, Stephen Lipscomb, and Brian Gill. 2012. "Is School Value-Added Indicative of Principal Quality?" Mathematica Policy Research Working Paper.

Clark, Damon, Paco Martorell, and Jonah Rockoff. 2009. “School Principals and School Performance.” Working Paper 38. Washington, DC: The Urban Institute.

Clotfelter, Charles T., Helen F. Ladd, and Jacob L. Vigdor. 2007. “Teacher Credentials and Student Achievement: Longitudinal Analysis with Student Fixed Effects.” *Economics of Education Review* 26: 673-882.

Coelli, Michael, and David Green. 2012. “Leadership Effects: School Principals and Student Outcomes.” *Economics of Education Review* 31(1): 92-109.

Cannon, Sarah, David Figlio, and Tim Sass. 2012. "Principal Quality and the Persistence of School Policies." Unpublished manuscript.

Cullen, Julie, and Michael J. Mazzeo. 2008. “Implicit Performance Awards: An Empirical Analysis of the Labor Market for Public School Administrators.” Unpublished manuscript.

Dhuey, Elizabeth, and Justin Smith. 2012. “How Important are Principals in the Production of School Achievement.” Unpublished manuscript.

Goldhaber, Dan, and Emily Anthony. 2007. “Can Teacher Quality be Effectively Assessed? National Board Certification as a Signal of Effective Teaching.” *The Review of Economics and Statistics*. 89(1): 134-150.

Grissom, Jason A., and Susanna Loeb. forthcoming. “Triangulating Principal Effectiveness: How Perspectives of Parents, Teachers, and Assistant Principals Identify the Central Importance of Managerial Skills.” *American Educational Research Journal*.

Grissom, Jason A., Demetra Kalogrides, and Susanna Loeb. 2012. “Using Student Test Scores to Measure Principal Performance.” Unpublished manuscript.

Hallinger, Philip, and Ronald H. Heck. 1998. “Exploring the Principal’s Contribution to School Effectiveness: 1980–1995.” *School Effectiveness and School Improvement* 9(2):157–91.

Hanushek, Eric A. 2006. “Teacher Quality” in *Handbook of the Economics of Education, Volume 2* Edited by Eric A. Hanushek and Finish Welch, Elsevier B.V. 1052-1078.

Horng, Eileen Lai, Daniel Klasik, and Susanna Loeb. 2009. “Principal Time-Use and School Effectiveness.” CALDER Working Paper 34. Washington, DC: The Urban Institute.

Jackson, C. Kirabo. 2010. “Match Quality, Worker Productivity, and Worker Mobility: Direct Evidence from Teachers.” NBER Working Paper 15900.

Jacob, Brian, and Lars Lefgren. 2005 “Principals as Agents: Subjective Performance Measurement in Education” National Bureau of Economic Research Working Paper No. 11463, NBER, Cambridge, MA

Li, Danielle. 2011. “School Accountability and Principal Mobility: How No Child Left Behind Affects the Allocation of School Leaders.” Unpublished manuscript.

Mihaly, Kata, Daniel McCaffrey, J. R. Lockwood, and Tim R. Sass. 2010. "Centering and Reference Groups for Estimates of Fixed Effects: Modifications to felsdvreg." *The Stata Journal* 10(1):82–103.

Miller, Ashley. 2010. “The Causes and Consequences of Principal Turnover.” Unpublished manuscript.

Rockoff, Jonah E. 2004. “The Impact of Individual Teachers on Student Achievement: Evidence from Panel Data.” *American Economic Review* 94, 247–52.

Ronfeldt, Matthew, Susanna Loeb, and James Wykoff. (forthcoming) “How Teacher Turnover Harms Student Achievement.” *American Educational Research Journal*.

Rothstein, Jesse. 2010. “Teacher Quality in Educational Production: Tracking, Decay, and Student Achievement.” *The Quarterly Journal of Economics* 125, 175-214.

Woodcock, Simon. 2011. “Match Effects.” Unpublished manuscript.

Table 1
Descriptive statistics for students in analysis sample

	Mean	Std Dev.
Math scores		
3rd Grade	0.067	0.960
4th Grade	0.038	0.986
5th Grade	0.041	0.986
6th Grade	0.054	0.983
7th Grade	0.055	0.986
8th Grade	0.052	0.984
Reading scores		
3rd Grade	0.063	0.958
4th Grade	0.029	0.987
5th Grade	0.032	0.983
6th Grade	0.047	0.975
7th Grade	0.048	0.975
8th Grade	0.046	0.973
Student demographic characteristics		
Male	0.501	0.500
Black	0.286	0.452
Hispanic	0.054	0.227
White	0.606	0.489
Other race	0.053	0.224
Special education	0.236	0.425
Learning disabled in math	0.020	0.140
Learning disabled in reading	0.041	0.199
Number of students	1,664,158	
Number of observations	5,388,543	

Notes: Test scores are standarized to have mean zero and standard deviation of one in the population of test takers within a subject, grade, and year, prior to sample exclusions. Other race includes all races except the three listed in the table.

Table 2
Descriptive statistics for schools and principals in analysis sample

	Mean	Std Dev.
School characteristics		
Fraction male	0.515	0.048
Fraction white	0.572	0.281
Fraction black	0.305	0.255
Fraction other race	0.056	0.082
Fraction hispanic	0.067	0.080
Fraction in special education	0.229	0.120
Fraction learning disabled in math	0.025	0.020
Fraction learning disabled in reading	0.050	0.029
Percent eligible for free/reduced lunch	0.408	0.216
Title 1 eligible school	0.581	0.492
Number of full time teachers	37.444	14.947
Magnet school	0.074	0.261
Number of students in school	552.324	241.571
Pupil teacher ratio	14.743	3.757
Neighbourhood census characteristics		
Fraction male	0.487	0.018
Fraction white	0.707	0.206
Fraction black	0.227	0.187
Fraction other race	0.052	0.072
Fraction hispanic	0.057	0.044
Fraction under 18 years old	0.242	0.033
Fraction over 65 years old	0.129	0.040
Fraction married	0.510	0.112
Fraction renters	0.310	0.121
Number of schools	1,954	
Principal characteristics		
1 year of tenure	0.218	0.413
2 years of tenure	0.192	0.394
3 years of tenure	0.148	0.355
4 years or more of tenure	0.442	0.497
1 year of experience	0.136	0.343
2 years of experience	0.128	0.334
3 years of experience	0.112	0.316
4 years or more of experience	0.623	0.485
Monthly state salary	5992.939	888.664
Advanced degree	0.279	0.449
Doctorate degree	0.090	0.287
Competitive bachelor school	0.164	0.371
Competitive masters school	0.126	0.332
Competitive advanced degree school	0.036	0.186
Competitive doctorate school	0.024	0.155
Number of principals	4,415	

Notes: Figures are based on 20,188 school-year observations on schools over time. All statistics in the table are averages across 20,188 observations. Monthly state salary is based on the NC salary schedule, and is reported in constant year 2009 dollars. Years of tenure is amount of time a principal is observed in a particular school at time t, while years of experience is number of years observed in the data at time t. The definition of "competitive" degrees is described in Section V.C.

Table 3
Mobility of Principals

Year	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
Total Employed	1,560	1,583	1,617	1,634	1,649	1,674	1,699	1,706	1,736	1,750	1,758	1,790
New Hires	203	211	209	197	196	208	230	252	276	292	248	222
Mobility												
Same school next year	0.802	0.791	0.800	0.796	0.802	0.784	0.779	0.760	0.746	0.771	0.783	
Different school next year	0.083	0.102	0.094	0.103	0.090	0.098	0.097	0.100	0.105	0.101	0.094	
Out of sample next year	0.115	0.107	0.106	0.102	0.107	0.118	0.124	0.141	0.149	0.127	0.123	

Notes: Summary statistics in this table are based on a population of 20,188 observations on 4415 principals in 1954 schools. Note that 32 observations from this population were dropped because they represent principals who work in two different schools in the same year. Those 32 observations are included in subsequent analyses. New hires are principals who were not in the sample in t-1. Principals who are "out of sample" in year t+1 are no longer observed as a principal in the data.

Table 4
Regression of principal turnover on school test scores

	School to school switch						All switches					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Average school level math scores	-0.013	-0.017					-0.107**	-0.094**				
	(0.024)	(0.029)					(0.039)	(0.044)				
Average school level reading scores	0.007	0.020					-0.024	-0.014				
	(0.029)	(0.034)					(0.046)	(0.053)				
Lag average school level math scores		0.002						0.006				
		(0.028)						(0.044)				
Lag average school level reading scores		0.016						-0.018				
		(0.032)						(0.051)				
Average gain math scores			0.009	-0.009					-0.073**	-0.074**		
			(0.021)	(0.023)					(0.033)	(0.036)		
Average gain reading scores			-0.002	-0.004					0.006	-0.037		
			(0.029)	(0.032)					(0.044)	(0.047)		
Lag average gain math scores				0.022						-0.001		
				(0.024)						(0.035)		
Lag average gain reading scores				0.003						-0.072		
				(0.034)						(0.049)		
Difference in levels between year t and t-1 math					-0.011						-0.044	
					(0.023)						(0.036)	
Difference in levels between year t and t-1 reading					0.002						0.003	
					(0.027)						(0.042)	
Difference in school average gains between year t and t-1 math						-0.015						-0.035
						(0.015)						(0.024)
Difference in school average gains between year t and t-1 reading						-0.003						0.017
						(0.020)						(0.030)
Number of Observations	18268	16293	18268	16293	16293	16293	18268	16293	18268	16293	16293	16293

Notes: ***p<0.01; **p<0.05; *p<0.1. Dependent variable is an indicator for either a school to school switch, or any switch out of a school between t and t+1. All regressions include year and school fixed effects. Average school level scores are the means of the levels of all student scores in all grades in a school in a particular year. Average school gain scores average the student level gains between t-1 and t for all grades 4 - 8 in a school in a year. Lag average level and gain scores are the school scores from t-1. Difference in levels is the school's year t score minus its year t-1 score in a particular subject. All scores are measured in student-level standard deviations, and note that one student level standard deviation is roughly equal to two school-level standard deviations. Standard errors are clustered by school.

Table 5
Student level estimates of principal fixed effects

	Math		Reading	
	(1)	(2)	(3)	(4)
Standard deviation	0.127	0.184	0.097	0.136
Adjusted standard deviation	0.124	0.172	0.092	0.116
10th percentile	-0.142	-0.208	-0.102	-0.136
25th percentile	-0.069	-0.083	-0.046	-0.052
50th percentile	0.002	0.000	0.004	0.001
75th percentile	0.079	0.080	0.056	0.057
90th percentile	0.149	0.215	0.110	0.152
(75th percentile - median)	0.077	0.081	0.052	0.056
Fixed effects:				
School	no	yes	no	yes

Notes: Statistics in this table are derived from the estimated principal fixed effects. Columns 1 and 3 use the full sample of 4415 principals, whereas columns 2 and 4 include only the 4289 principal effects that are separately identified from school effects.. All regressions include grade fixed effects, year fixed effects, demographic, school, and census control variables. In addition, principal tenure is also included as a control for principal characteristics.

Table 6
Estimates of match effects

	Math (1)	Reading (2)
Panel A: Orthogonal fixed effects estimates		
Standard deviation for principal	0.185	0.135
Standard deviation for school	0.197	0.145
Standard deviation for match	0.014	0.012
Panel B: Hybrid random effect estimates		
Standard deviation for principal	0.039	0.018
Standard deviation for school	0.120	0.095
Standard deviation for match	0.073	0.042

Notes: Statistics in this table are based on the same sample and control variables used in Table 5. There are 4,415 principals, 1,954 schools, and 5,783 matches. Standard deviations on Panel A are based on taking the sample standard deviation of the estimated principal, school, or match effects. Standard deviations in Panel B are Restricted Maximum Likelihood (REML) estimates. In fixed effects models, the principal, school, and match effects are centered within connected groups.

Table 7
Relationship between principal quality and fixed principal character

	Math	Reading
	(2)	(4)
Experience	0.0035**	0.0049**
	(0.0018)	(0.0018)
Experience^2	-0.0003	0.0010*
	(0.0008)	(0.0006)
Experience^3	0.0000	-0.0003*
	(0.0001)	(0.0002)
Salary	0.0292	-0.0740**
	(0.0523)	(0.0362)
Salary^2	0.1211	-0.4114
	(0.3943)	(0.2553)
Salary^3	-2.4418	3.7750**
	(2.6032)	(1.8076)
Competitive bachelor school	0.0038	0.0052
	(0.0069)	(0.0041)
Competitive masters school	-0.0038	0.0013
	(0.0075)	(0.0045)
Competitive doctorate school	-0.0120	-0.0040
	(0.0161)	(0.0097)
Non-competitive doctorate school	0.0165*	0.0078*
	(0.0092)	(0.0060)
Competitive advanced degree school	0.0095	0.0175**
	(0.0116)	(0.0080)
Non-competitive advanced degree school	-0.0004	0.0069*
	(0.0062)	(0.0039)
Number of Observations	4289	4289

Notes: ***p<0.01; **p<0.05; *p<0.1. Dependent variable is the Empirical Bayes fixed effect for each principal. Prior to estimation, experience and salary are averaged over all years the principal is observed. Experience and salary are centered around the average across all principals. Education variables are indicator variables equal to one if the principal ever had the particular degree. Standard errors are robust to heteroskedasticity.

Table 8

Effect of new principals over time on math and reading score gains

	Math		Reading	
	(1)	(2)	(3)	(4)
Panel A: One year				
Lower value added	0.006	-0.003	0.006	-0.004
	(0.016)	(0.021)	(0.012)	(0.015)
Similar value added	-0.006	-0.007	0.006	0.004
	(0.013)	(0.016)	(0.009)	(0.011)
Higher value added	0.019	0.030*	0.005	0.02
	(0.016)	(0.018)	(0.014)	(0.014)
First observation in data	-0.010***	-0.007	-0.005**	-0.002
	(0.004)	(0.004)	(0.002)	(0.003)
Panel B: Three years				
Lower value added	-0.001	-0.029	-0.005	-0.021
	(0.011)	(0.022)	(0.009)	(0.017)
Similar value added	-0.001	-0.011	0.004	-0.003
	(0.010)	(0.017)	(0.006)	(0.011)
Higher value added	0.021*	0.031*	0.008	0.022
	(0.012)	(0.018)	(0.010)	(0.015)
First observation in data	-0.010***	-0.016***	-0.006***	-0.008**
	(0.003)	(0.005)	(0.002)	(0.004)
Number of observations	9936	9936	9936	9936
School specific linear trends?	no	yes	no	yes

Notes: ***p<0.01; **p<0.05; *p<0.1. In Panel A, the reported coefficients are based on indicators that are equal to one in the year of the principal switch. In Panel B, indicators are equal to one in the year of the switch and subsequent two years following a principal change. If a school changes principals twice within two years, indicators are recoded to reflect the most current principal change. Regressions include school and year effects. Standard errors are clustered at the school level.

Table 9
Effect of new principals on school level outcomes

	% AYP target met	Crime	Long term suspensions	% Daily attendance	% Teachers w/ adv degrees	Teacher turnover rate	% Licensed teachers	% National Board Certified teachers	% Classes w/ highly qualified teachers	% Teachers 0-3 yrs experience	% Teachers 4-10 yrs experience	% Teachers 11+ yrs experience
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: One year												
Lower value added	0.000	0.004	-0.082**	0.000	0.005	0.014	0.007	0.004	-0.007	0.008	-0.003	-0.005
	(0.013)	(0.068)	(0.035)	(0.001)	(0.006)	(0.008)	(0.005)	(0.004)	(0.005)	(0.008)	(0.008)	(0.006)
Similar value added	-0.008	0.073	0.052	0.001	0.016***	0.007	-0.003	0.003	0.004	-0.008	0.012*	-0.003
	(0.013)	(0.077)	(0.095)	(0.001)	(0.005)	(0.009)	(0.006)	(0.004)	(0.006)	(0.007)	(0.007)	(0.007)
Higher value added	0.015	-0.102	-0.019	-0.002	-0.006	0.003	-0.009	-0.007*	0.007	0.014*	-0.006	-0.008
	(0.014)	(0.103)	(0.185)	(0.001)	(0.006)	(0.008)	(0.006)	(0.004)	(0.008)	(0.007)	(0.007)	(0.007)
First observation in data	-0.003	0.032	-0.019	0.000	0.001	0.010***	-0.002	-0.001	-0.001	0.002	-0.003	0.001
	(0.003)	(0.022)	(0.044)	(0.000)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Panel B: Three years												
Lower value added	-0.005	0.020	-0.061	0.000	0.001	0.008	-0.002	0.002	-0.001	0.007	0.008	-0.015*
	(0.009)	(0.057)	(0.049)	(0.001)	(0.006)	(0.007)	(0.005)	(0.006)	(0.009)	(0.009)	(0.008)	(0.008)
Similar value added	0.011	0.027	0.073	0.001	0.007	0.004	0.000	-0.001	0.012	0.003	0.002	-0.005
	(0.010)	(0.061)	(0.191)	(0.001)	(0.006)	(0.008)	(0.005)	(0.004)	(0.008)	(0.007)	(0.007)	(0.007)
Higher value added	0.013	-0.070	0.049	-0.001	-0.007	0.000	-0.007	-0.007*	0.003	0.011	-0.004	-0.007
	(0.009)	(0.075)	(0.118)	(0.001)	(0.006)	(0.008)	(0.006)	(0.004)	(0.009)	(0.008)	(0.008)	(0.007)
First observation in data	-0.004	0.018	-0.012	-0.001***	-0.001	0.007***	-0.002	-0.001	0.000	0.007***	-0.004	-0.004*
	(0.002)	(0.017)	(0.031)	(0.000)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Mean value of dep. variable	0.931	0.419	0.240	0.951	0.258	0.169	0.934	0.119	0.949	0.228	0.289	0.483
Number of Observations	8545	8545	6717	9936	9936	9936	9936	8545	8545	9936	9936	9936
Years Included	2003-09	2003-09	2003-04, 2006-09	2003-09	2003-09	2003-09	2003-09	2003-09	2003-09	2003-09	2003-09	2003-09

Notes: ***p<0.01; **p<0.05; *p<0.1. In Panel A, the reported coefficients are based on indicators that are equal to one in the year of the principal switch. In Panel B, indicators are equal to one in the year of the switch and subsequent two years following a principal change. If a school changes principals twice within two years, indicators are recoded to reflect the most current principal change. All regressions use the average of math and reading value added for each principal. Regressions include school and year effects. Standard errors are clustered at the school level.

Appendix Table 1

Regression of principal turnover on school and neighbourhood characteristics, Table 3 continued

	School to school switch						All switches					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Fraction male at school	0.029	0.038	0.03	0.034	0.033	0.033	0.086	0.141	0.104	0.158	0.162	0.164
	(0.071)	(0.079)	(0.071)	(0.078)	(0.078)	(0.078)	(0.119)	(0.127)	(0.119)	(0.127)	(0.127)	(0.127)
Fraction black at school	0.034	0.034	0.042	0.018	0.014	0.018	0.004	-0.009	0.159*	0.125	0.113	0.13
	(0.061)	(0.071)	(0.058)	(0.067)	(0.068)	(0.067)	(0.091)	(0.105)	(0.084)	(0.097)	(0.098)	(0.097)
Fraction other race at school	0.063	0.039	0.063	0.026	0.028	0.027	0.041	0.061	0.072	0.096	0.088	0.087
	(0.131)	(0.145)	(0.131)	(0.145)	(0.145)	(0.144)	(0.205)	(0.227)	(0.203)	(0.226)	(0.226)	(0.226)
Fraction hispanic at school	0.018	0.058	0.019	0.04	0.039	0.041	-0.022	0.024	0.075	0.119	0.098	0.104
	(0.091)	(0.102)	(0.089)	(0.100)	(0.100)	(0.100)	(0.138)	(0.153)	(0.136)	(0.150)	(0.150)	(0.150)
Fraction in special education at school	0.061*	0.061*	0.060*	0.064*	0.063*	0.064*	-0.003	0.003	-0.024	-0.016	-0.014	-0.014
	(0.032)	(0.035)	(0.032)	(0.035)	(0.035)	(0.035)	(0.055)	(0.058)	(0.055)	(0.058)	(0.058)	(0.058)
Fraction learning disabled in math at school	-0.208	-0.084	-0.201	-0.077	-0.076	-0.077	-0.107	-0.100	-0.054	-0.078	-0.071	-0.065
	(0.185)	(0.206)	(0.184)	(0.206)	(0.206)	(0.206)	(0.297)	(0.330)	(0.297)	(0.329)	(0.331)	(0.331)
Fraction learning disabled in reading at school	0.053	-0.003	0.048	-0.025	-0.023	-0.024	0.307	0.355	0.337	0.41	0.374	0.375
	(0.148)	(0.165)	(0.147)	(0.164)	(0.164)	(0.164)	(0.245)	(0.272)	(0.248)	(0.275)	(0.274)	(0.274)
Percent eligible for free/reduced lunch at school	-0.027	-0.038	-0.027	-0.041	-0.041	-0.041	-0.043	-0.047	-0.036	-0.041	-0.038	-0.041
	(0.024)	(0.026)	(0.024)	(0.026)	(0.026)	(0.026)	(0.036)	(0.038)	(0.035)	(0.038)	(0.038)	(0.038)
Title 1 eligible school	-0.014	-0.016	-0.014	-0.017	-0.016	-0.016	-0.032**	-0.031*	-0.030*	-0.029*	-0.029*	-0.030*
	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.010)	(0.015)	(0.017)	(0.016)	(0.017)	(0.017)	(0.017)
Number of full time teachers	0.001	0.001	0.001	0.001	0.001	0.001	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Magnet school	-0.01	-0.007	-0.01	-0.006	-0.005	-0.005	0.016	0.038*	0.011	0.033	0.032	0.032
	(0.015)	(0.017)	(0.015)	(0.017)	(0.017)	(0.017)	(0.022)	(0.023)	(0.022)	(0.023)	(0.023)	(0.023)
Number of students in school	0.000*	0.000*	0.000*	0.000*	0.000*	0.000*	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Pupil teacher ratio	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.003*	-0.003	-0.003*	-0.003	-0.003	-0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
2 years of tenure	0.042***	0.046***	0.042***	0.046***	0.046***	0.046***	0.101***	0.106***	0.101***	0.106***	0.106***	0.106***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.010)
3 years of tenure	0.075***	0.086***	0.075***	0.086***	0.086***	0.086***	0.163***	0.180***	0.163***	0.180***	0.179***	0.179***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)
4 years or more of tenure	0.093***	0.105***	0.092***	0.105***	0.105***	0.105***	0.278***	0.299***	0.276***	0.298***	0.298***	0.297***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)
Fraction male in community	0.402	0.206	0.398	0.205	0.216	0.208	0.056	-0.537	0.054	-0.564	-0.577	-0.619

	(0.573)	(0.627)	(0.573)	(0.627)	(0.627)	(0.626)	(0.968)	(1.106)	(0.976)	(1.112)	(1.113)	(1.111)
Fraction black in community	-0.111	-0.189	-0.118	-0.194	-0.19	-0.193	-0.766***	-0.753**	-0.816***	-0.788**	-0.799**	-0.809**
	(0.151)	(0.191)	(0.151)	(0.191)	(0.191)	(0.191)	(0.262)	(0.312)	(0.264)	(0.314)	(0.315)	(0.315)
Fraction other race in community	-0.048	-0.038	-0.058	-0.059	-0.058	-0.057	-0.955**	-1.308**	-0.972**	-1.284**	-1.313**	-1.322**
	(0.287)	(0.350)	(0.286)	(0.349)	(0.350)	(0.349)	(0.454)	(0.543)	(0.453)	(0.542)	(0.543)	(0.542)
Fraction hispanic in community	-0.271	-0.025	-0.267	-0.02	-0.022	-0.024	0.085	0.594	0.068	0.551	0.592	0.602
	(0.405)	(0.506)	(0.405)	(0.507)	(0.507)	(0.506)	(0.652)	(0.801)	(0.653)	(0.802)	(0.801)	(0.800)
Fraction under 18 years old in community	0.234	-0.03	0.217	-0.004	0.001	-0.003	1.034	0.683	0.806	0.453	0.432	0.413
	(0.478)	(0.584)	(0.478)	(0.581)	(0.581)	(0.581)	(0.806)	(0.954)	(0.808)	(0.952)	(0.951)	(0.951)
Fraction over 65 years old in community	0.375	0.359	0.364	0.35	0.358	0.359	0.332	-0.194	0.361	-0.136	-0.241	-0.247
	(0.433)	(0.545)	(0.435)	(0.546)	(0.545)	(0.544)	(0.719)	(0.890)	(0.721)	(0.894)	(0.893)	(0.892)
Fraction married in community	-0.105	-0.147	-0.102	-0.145	-0.145	-0.145	-0.213	-0.163	-0.197	-0.139	-0.139	-0.141
	(0.115)	(0.136)	(0.115)	(0.136)	(0.136)	(0.136)	(0.183)	(0.215)	(0.184)	(0.215)	(0.216)	(0.216)
Fraction renters in community	0.108	0.067	0.113	0.075	0.075	0.075	0.418	0.242	0.476	0.299	0.283	0.284
	(0.190)	(0.224)	(0.190)	(0.223)	(0.223)	(0.223)	(0.340)	(0.399)	(0.340)	(0.398)	(0.398)	(0.398)
Number of Observations	18268	16293	18268	16293	16293	16293	18268	16293	18268	16293	16293	16293

Notes: ***p<0.01; **p<0.05; *p<0.1. Dependent variable is an indicator for either a school to school switch, or any switch out of a school between t and t+1. All regressions include year and school fixed effects. Average school level scores average the levels of all student scores in all grades in a school in a particular year. Columns correspond to regressions in Table 4. Standard errors are clustered by school.

Appendix Table 2
Effect of new principals on school level outcomes with linear time trends

	% AYP target met	Crime	Long term suspensions	% Daily attendance	% Teachers w/ adv degrees	Teacher turnover rate	% Licensed teachers	% National Board Certified teachers	% Classes w/ highly qualified teachers	% Teachers 0-3 yrs experience	% Teachers 4-10 yrs experience	% Teachers 11+ yrs experience
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: One year												
Lower value added	-0.010	0.020	-0.068	-0.001	0.005	0.015	0.010	-0.001	0.006	0.005	0.003	-0.008
	(0.017)	(0.067)	(0.055)	(0.001)	(0.006)	(0.010)	(0.006)	(0.004)	(0.006)	(0.009)	(0.008)	(0.006)
Similar value added	-0.018	0.045	-0.098	0.000	0.015**	0.003	-0.009	0.006	0.002	-0.005	0.008	-0.003
	(0.020)	(0.086)	(0.182)	(0.001)	(0.006)	(0.011)	(0.006)	(0.004)	(0.008)	(0.008)	(0.007)	(0.008)
Higher value added	0.017	-0.011	-0.311	-0.001	-0.007	0.006	-0.010*	-0.006	-0.002	0.010	-0.001	-0.008
	(0.025)	(0.098)	(0.397)	(0.001)	(0.006)	(0.011)	(0.006)	(0.005)	(0.009)	(0.008)	(0.007)	(0.008)
First observation in data	-0.003	0.031	0.023	0.000	0.001	0.009***	0.000	-0.001	0.002	0.000	-0.002	0.002
	(0.004)	(0.022)	(0.028)	(0.000)	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Panel B: Three year												
Lower value added	-0.006	-0.030	0.037	-0.001	0.003	0.007	0.009	0.000	0.007	-0.002	0.006	-0.003
	(0.018)	(0.087)	(0.093)	(0.001)	(0.006)	(0.011)	(0.007)	(0.004)	(0.009)	(0.008)	(0.007)	(0.007)
Similar value added	0.005	0.038	0.124	0.001	0.006	0.005	0.000	0.001	-0.004	-0.005	0.006	-0.001
	(0.021)	(0.102)	(0.125)	(0.001)	(0.006)	(0.013)	(0.007)	(0.004)	(0.010)	(0.007)	(0.008)	(0.008)
Higher value added	0.035	-0.072	-0.221	-0.001	-0.008	0.007	-0.002	-0.001	0.003	0.006	-0.002	-0.003
	(0.021)	(0.115)	(0.357)	(0.001)	(0.007)	(0.013)	(0.008)	(0.004)	(0.009)	(0.009)	(0.007)	(0.007)
First observation in data	-0.009*	0.023	0.054	-0.001**	-0.001	0.010***	0.003	-0.002	0.006**	0.004	-0.002	-0.002
	(0.005)	(0.029)	(0.036)	(0.000)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
Mean value of dep. variable	0.931	0.419	0.240	0.951	0.258	0.169	0.934	0.119	0.949	0.228	0.289	0.483
Number of Observations	8545	8545	6717	9936	9936	9936	9936	8477	8545	9936	9936	9936

Notes: ***p<0.01; **p<0.05; *p<0.1. In Panel A, the reported coefficients are based on indicators that are equal to one in the year of the principal switch. In Panel B, indicators are equal to one in the year of the switch and subsequent two years following a principal change. If a school changes principals twice within two years, indicators are recoded to reflect the most current principal change. All regressions use the average of math and reading value added for each principal. Regressions include school effects and school-specific linear time trends. Standard errors are clustered at the school level.