TEACHER TRAINING,
TEACHER QUALITY AND STUDENT ACHIEVEMENT

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Abstract

We study the effects of various types of education and training on the ability of teachers to promote student achievement. Previous studies on the subject have been hampered by inadequate measures of teacher training and difficulties addressing the non-random selection of teachers to students and of teachers to training. We address these issues by estimating models that include detailed measures of pre-service and in-service training, a rich set of time-varying covariates, and student, teacher, and school fixed effects. Our results suggest that only two of the forms of teacher training we study influence productivity. First, content-focused teacher professional development is positively associated with productivity in middle and high school math. Second, more experienced teachers appear more effective in teaching elementary math and reading and middle school math. There is no evidence that teachers’ pre-service (undergraduate) training or college entrance exam scores influence productivity.

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I. Introduction

It is generally acknowledged that promoting teacher quality is a key element in improving primary and secondary education in the United States. Indeed, one of the main goals of the No Child Left Behind law is to have a “highly qualified teacher” in every classroom. While recent research has documented the central role of teacher quality in promoting student achievement, there is no consensus on what factors enhance, or even signal, teacher quality. This has fueled debate over how best to prepare new teachers and how to improve the quality of the existing teacher labor force. To better understand the determinants of teacher quality, we consider the relationship between teacher productivity and teacher training, including formal pre-service university education, in-service professional development, and informal training acquired through on-the-job experience.

Uncertainty regarding the effects of teacher training is due in large measure to three methodological challenges in estimating the effects of training on teacher quality. First, it is difficult to isolate productivity, especially in teaching where a student’s own ability, the influences of a student’s peers, and other characteristics of schools also affect measured outcomes. The problem is exacerbated by the fact that assignment of students and teachers to classrooms is usually not random, leading to possible correlations between observed teacher attributes and unobserved student characteristics. Second, like in other occupations, there is an inherent selection problem in evaluating the effects of education and training on teacher productivity. Unobserved teacher characteristics, such as motivation or intelligence, may affect the amount and types of education and training they choose to obtain as well as subsequent performance of teachers in the classroom. Third, it is difficult to obtain data that provide much detail about the various types of training teachers receive and even more difficult to link the
training of teachers to the achievement of the students they teach. Addressing all of these issues in a single study presents significant data and estimation challenges.

In this paper we address these challenges and present new evidence on the effects of teacher university-based pre-service formal education and in-service professional development training on teacher productivity using a rich statewide administrative database from Florida. The Florida data allow us to tie student performance to the identity of their classroom teacher at all grade levels and in turn link teachers to their in-service training, their college coursework and majors, and their pre-college entrance exam scores. These data provide an opportunity to analyze the effects of both pre-service and in-service training on teacher productivity while addressing the twin selection problems associated with teacher acquisition of training and assignment of students to teachers.

Our analysis proceeds in two steps. First, we estimate student achievement models that include a substantial set of covariates that measure the time-varying characteristics of individual students, their classroom peers, and their school’s principal. We incorporate multiple levels of fixed effects that control for unmeasured time-invariant student, teacher and school characteristics. This first-stage model includes detailed information on the quantity and characteristics of education and training teachers receive after they have entered the classroom, including both formal graduate education and professional development courses sponsored by schools and school districts. We also include measures of teacher experience, which represent informal on-the-job training. This first step yields estimates of the fixed effect for each teacher, which represents the teacher’s contribution to student achievement or “value added” that does not vary over her career. In the second step we take the estimated teacher fixed effect and
regress it on characteristics of teachers’ (time-invariant) undergraduate coursework, controlling for teacher pre-college cognitive/verbal ability with college entrance exam scores.

We begin in section II by describing past literature on teacher training. Our data and methodology are discussed in sections III and IV, respectively. Our results, presented in section V, suggest that only two of the forms of teacher training influence productivity; content-focused teacher professional development is positively associated with productivity in middle and high school math and on-the-job training acquired through experience is correlated with enhanced effectiveness in teaching both math and reading in elementary and middle schools. The implications of our findings are discussed in section VI.

II. Previous Literature on the Effects of Teacher Training

In early work on teacher productivity, researchers estimated education production functions by regressing aggregate student achievement levels on measures of teacher training and various other controls using cross-sectional data (see review by Hanushek (1986)). A subsequent generation of studies used student-level two-year test-score gains and richer sets of teacher training variables to evaluate the impact of teacher training on student achievement. The state of the literature through the year 2000 has been extensively reviewed by Wayne and Youngs (2003) as well as by Rice (2003), Wilson and Floden (2003), and Wilson, et al. (2001). Rather than duplicate previous surveys we highlight new research findings over the last decade.

Recent studies of the determinants of teacher productivity can be categorized by the ways in which they deal with the potential bias associated with non-random assignment of students to teachers. Some studies rely on observed student characteristics or “student covariates” to account for student heterogeneity. One potential shortcoming of this approach is that it does not
control for unobserved characteristics like innate ability and motivation. There is evidence that better trained and more experienced teachers tend to be assigned to students of greater ability and with fewer discipline problems (e.g., Clotfelter et al. (2006), Feng (2009)). Given this positive matching between student quality and teacher training, the student-covariate studies’ lack of controls for unobserved student characteristics would tend to upwardly bias estimates of teacher value-added associated with education and training.

The recent availability of longitudinal administrative databases has brought forth a new set of studies that seek to ameliorate selection bias by controlling for time-invariant unobserved student heterogeneity via student fixed effects. Since 2000, eight studies of teacher productivity in the U.S. have employed this approach. An alternative method of avoiding selection bias is to either randomly assign teachers to students (as in the Tennessee class size experiment) or to exploit situations where there is an exogenous change in student assignments to teachers or in teachers to training. Five other recent studies exploit either experiments with random assignment, situations where there is “apparent random assignment” or “natural” experiments where assignment is based on exogenous factors.

Table 1 provides a summary of the recent research on teacher training and productivity, broken down by the methodology employed. No matter what the empirical method, nearly all of the recent studies of teacher productivity include some measure of teacher experience, which serves as a proxy for on-the-job training. Results for elementary math are about evenly split between positive and insignificant effects of teacher experience on student achievement. In contrast, all but one of the eight recent studies that separately analyze elementary reading find that student achievement is positively correlated with teacher experience. At the middle school level the subject-specific findings are essentially reversed. Studies that include middle school
consistently find positive effects of teacher experience on math achievement whereas the findings for the effects of experience on middle school reading achievement are evenly split between positive and insignificant correlations. The three studies of high school teachers yield conflicting results; Aaronson, et al. (2007) and Betts, et al. (2003) find no significant correlation between teacher experience and student achievement while Clotfelter, et al. (2007b) find strong positive effects. One difference in these studies is that Clotfelter et al. utilize course-specific end-of-course exams while the other studies rely on more general achievement exams to measure student progress.

As discussed by Rockoff (2004) and Kane, et al. (2006), the estimated effects of experience may be biased if sample attrition is not taken into account. For example, less effective teachers might be more likely to leave the profession and this can give the appearance that experience raises teacher value-added when, in reality, less effective teachers are simply exiting the sample. Alternatively, selection could work in the opposite direction; more able teachers with higher opportunity costs may be more likely to leave the profession, leading to a spurious negative correlation between teacher experience and student achievement. One method of addressing the attrition issue is to include a teacher-specific effect, to control for unmeasured teacher ability, along with the experience measures. The teacher-specific effect should purge the influence of teacher time-invariant ability on experience, yielding unbiased estimates of the marginal product of experience. While the recent student-covariate studies all include a teacher-

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1 While the inclusion of teacher effects greatly reduces the potential bias associated with teacher attrition, it does not necessarily eliminate it for two reasons. First, since multiple observations are required to compute teacher effects, elementary school teachers who leave after one year are necessarily excluded. This is not a significant problem for middle and high-school teachers, however, since they teach multiple classes within a single period (though it remains a problem for estimating the effects of experience, which can still only be done for teachers with two or more years in the classroom). Second, if there is an unobserved time-varying component of teacher productivity that is correlated with the likelihood of attrition, then this will not be fully captured by the teacher effect. For example,
specific effect, only two of the student-fixed-effects studies, Hanushek et al. (2005) and Rockoff (2004), employ teacher fixed effects in addition to student fixed effects. Both of these studies analyze only a single school district. In our work we are able to include to include both student and teacher fixed effects using data for the entire state of Florida.

In addition to experience, the other commonly measured aspect of teacher training is the attainment of graduate degrees. Nearly all of the recent student-fixed-effects and random-assignment studies include a measure of post-baccalaureate degree attainment, typically whether a teacher holds a master’s degree. Except for positive correlations between possession of a masters degree and elementary math achievement found by Betts et al. (2003), Dee (2004) and Nye, et al. (2004), recent research indicates either insignificant or in some cases even negative associations between possession of graduate degrees by a teacher and their students’ achievement in either math or reading.

In contrast to experience and possession of advanced degrees, the pre-service undergraduate training of teachers has received much less attention in the recent literature. Two studies, Aaronson, et al. (2007) and Betts et al. (2003) consider the effect of college major on later teacher productivity, but fail to find a robust relationship between undergraduate major and the impact of teachers on student achievement.\(^2\) Three studies, Kane et al. (2006), Clotfelter et al. (2006, 2007a) consider general measures of the quality of the undergraduate institution attended and find little or no relationship to teacher productivity in elementary or middle school.

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as noted by Murnane and Phillips (1981) and others, the presence of young children in the home may lower teacher productivity and also increase the likelihood of attrition. We test whether teacher-specific effects eliminate attrition bias in our empirical work below.

\(^2\) A related strand of literature compares the effectiveness of teachers who graduate from traditional university-based teacher preparation programs and those who enter teaching through an alternative route (Boyd, et al. (2006), Kane, et al. (2006), Xu, et al. (2008), Constantine, et al (2009)). These studies generally find there is little or no difference in the productivity of traditionally prepared teachers vis-à-vis alternatively certified teachers.
A fourth study, Clotfelter, et al. (2007b) does find a positive and significant relationship between the prestige of the undergraduate institution and productivity of high school teachers. Kane et al. (2006) also analyze the relationship between undergraduate grade point average (GPA) and teacher productivity in elementary and middle school. As with the other measures of undergraduate education, they find no significant relationship between GPA and subsequent teacher performance. Finally, Boyd et al. (2008) consider the relationship between characteristics of teacher preparation programs and the productivity of graduates during their first and second years of teaching in New York City schools. While graduates of some preparation programs appear to be more productive than graduates of other programs, evidence on the relationship between specific program characteristics and teacher performance is mixed. They find some evidence that rookie teachers benefit from having had carefully monitored student teaching experience and exposure to practical teaching skills, but these effects do not carry over to second-year teachers. Second-year teachers whose programs had more stringent content requirements are more productive in many instances as well. However, a number of other program features hypothesized to be beneficial yielded insignificant or even negative correlations with teacher productivity.

There are at least two shortcomings of recent estimates of the impact of undergraduate education on teacher productivity. First, most recent work has relied on relatively gross measures, like college major, which may obscure significant variation in college coursework. Even the Boyd, et al. work measures preparation program features rather than actual courses taken.3 Second, none of the recent studies that include measures of undergraduate training

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3 At least two of the older gain-score studies, Eberts and Stone (1984) and Monk (1994) do include detailed measures of courses taken.
control for the pre-college ability of future teachers. Thus, for example, a positive observed correlation between undergraduate institutional prestige and future teacher productivity could mean that institutional quality enhances the productivity of future teachers or simply that more able students are accepted into elite institutions and individual ability affects productivity as a teacher. In our work we consider the specific courses taken by teachers and control for pre-college ability with college entrance exam scores.

The only recent studies of the impact of in-service professional development (PD) on teacher productivity in the United States are Jacob and Lefgren (2004) and Garet, et al. (2008). 4 Jacob and Lefgren exploit a “natural experiment” that occurred in the Chicago public schools where low-performing elementary schools (based on students’ failing to meet national norms on a reading exam) were placed on probation and given resources to purchase additional staff development services. Employing a regression discontinuity design, they find that the additional professional development resulting from probation had no effect on student achievement in either math or reading. Garet, et. al. utilize an experimental design to study the impact of an intensive PD program for early reading teachers and subsequent follow-up with in-school coaches. While the PD intervention increased teacher knowledge and changed instruction, neither the PD program alone nor the PD program with subsequent coaching yielded improvements in student reading scores. While methodologically strong, both the Jacob and Lefgren and Garet, et al. studies consider PD in relatively narrow contexts and thus provide limited insight on the efficacy of PD in more general settings. In the present study we expand on their work by considering PD received by teachers in all schools at all grade levels and

4 Angrist and Lavy (2001) analyze the effects of teacher professional development on teacher productivity in Israel.
distinguish between training that focuses on content and that which emphasizes pedagogy.

III. Data

To study the effects of teacher training we make use of an extensive panel data set of school administrative records from Florida. The data cover all public school students throughout the state and include student-level achievement test data for both math and reading in each of grades 3-10 for the years 1999-2000 through 2004-2005. Summary statistics are available in Table 2.

Unlike other statewide databases, we can precisely match students and their teachers to specific classrooms at all grade levels. We can determine the specific classroom assignments of middle-school and high-school students, who typically rotate through classrooms during the day for different subjects. This enables us to better separate the effects of teachers from students and their peers. Having data from all K-12 grades also allows us to estimate separate models for

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5 A more detailed description of the data is provided in Sass (2006).

6 The state of Florida currently administers two sets of reading and math tests to all 3rd through 10th graders in Florida. The “Sunshine State Standards” Florida Comprehensive Achievement Test (FCAT-SSS) is a criterion-based exam designed to test for the skills that students are expected to master at each grade level. The second test is the FCAT Norm-Referenced Test (FCAT-NRT), a version of the Stanford Achievement Test used throughout the country. Version 9 of the Stanford test (the Stanford-9) was used in Florida through the 2003/2004 school year. Version 10 of the Stanford test (the Stanford-10) has been used since the 2004/05 school year. To equate the two versions of the exams we convert Stanford-10 scores into Stanford-9 equivalent scores based on the conversion tables in Harcourt (2003). The scores on the Stanford-9 are scaled so that a one-point increase in the score at one place on the scale is equivalent to a one-point increase anywhere else on the scale. The Stanford-9 is a vertically scaled exam, thus scale scores typically increase with the grade level. We use FCAT-NRT scale scores in all of the analysis. The use of vertically scaled scores to evaluate student achievement is important since a one-unit change has the same meaning for low- and high-achieving students.

7 Currently, the Texas data do not provide a way to link teachers and students to specific classrooms. For North Carolina, one can only (imperfectly) match specific teachers and students to classrooms at the elementary school level. Matching is done by identifying the person who administers each student the annual standardized test, which at the elementary school level is typically the classroom teacher.
elementary, middle and high school which affords us the opportunity to see how the impacts of teacher education and training vary across the three school types. For example, one might expect that a teacher’s content knowledge of mathematics might be more important in a high school trigonometry class than in a fourth grade class learning arithmetic.

Not only do our data directly link students and teachers to specific classrooms, they also provide information on the proportion of each student’s time spent in each class. This is potentially important for correctly matching teachers and their students at the elementary school level. While primary school students typically receive all of their academic instruction from a single teacher in a single “self-contained” classroom, this is far from universal. In Florida, five percent of elementary school students enrolled in self-contained classrooms are also enrolled in a separate math course, four percent in a separate reading course and four percent in a separate language arts course. In addition, nearly 13 percent of elementary students enrolled in self-contained elementary classes are also enrolled in some type of exceptional student education course apart from their regular classroom, either special-education or gifted courses.  

We restrict our analysis of student achievement to students who receive instruction in the relevant subject area in only one classroom. In our elementary school analysis, only students in “self-contained” classrooms are included. Elementary students spending less than one hour per day in the class are not considered as a member of the classroom peer group. At the middle and high-school levels, students who are enrolled in more than one course in the relevant subject area (mathematics and reading/language arts) are dropped, though all students enrolled in a course are included in the measurement of peer-group characteristics. To avoid atypical classroom settings

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8 Since previous studies lack data on students’ complete course enrollments, they either ignore the fact that students may receive instruction outside their primary classroom or deal with the issue in an ad-hoc fashion.
and jointly taught classes we consider only courses in which 10-40 students are enrolled and there is only one “primary instructor” of record for the class. Finally, we eliminate charter schools from the analysis since they may have differing curricular emphases and student-peer and student-teacher interactions may differ in fundamental ways from traditional public schools.

Despite limiting the sample to students who have only one class per subject, our ability to match the content taught in each classroom with the content on the state test varies by grade and subject. In elementary schools, the matching of subjects is relatively easy because students typically have only one teacher and, as indicated above, we have dropped the small percentage of students who have more than one teacher. In middle and high school, however, more students have multiple classes for the same subject, especially in reading, so that more students are dropped; and some who are not dropped may have their reading scores influenced by classes such as social studies, that involve reading but where developing reading is not the primary purpose.\(^9\) Also, even if we were able to match the test content to a specific classroom, our ability to isolate the effect of classroom factors is further constrained in the case of reading because some students may read in their leisure time. Few students do math during leisure time so this is less of a problem in that subject. For all of these reasons, we have greater confidence in our results for mathematics than for reading.

The ability to link teachers to their university coursework is another important feature of the Florida data. For relatively young teachers (those who attended a Florida public university or community college since 1995) our data include complete college transcript information, including entrance exam scores, courses taken and degrees received. Because Florida has a

\(^9\) Koedel (2009) provides some evidence that social studies teachers influence reading test scores at the high school level.
uniform course numbering system, we are able to create variables that describe each course according to its focus on teacher content knowledge, pedagogical knowledge, and classroom observation/practice in teaching.\(^{10}\) We then aggregate these measures for each teacher to capture the relevant characteristics of each teacher’s entire undergraduate training. We also know the major associated with each college degree and can thus distinguish future teachers who graduated with an education major from those who earned a degree in various non-education disciplines like mathematics and English literature.

IV. Econometric Model and Estimation Strategies

A. Measuring Teacher Productivity and Within-Career Education and Training

While the issue of measuring a teacher’s output is controversial, particularly outside the economics literature, we shall simply define the relevant product as student achievement measured by standardized tests. Consequently, we view a teacher’s productivity as their contribution to student achievement, holding other inputs constant. To empirically measure the impact of education and training on teacher productivity it is therefore necessary to first develop a model of student achievement.

\(^{10}\) Courses were coded using the course descriptions in the State of Florida State Course Numbering System. The following categories are used: Education theory/foundations includes courses that cover general education theory or general issues in education. Pedagogical-instructional includes general instructional methods and theories to instruction. Pedagogical-management includes classroom management issues in general or for different groups of students. Pedagogical-content includes combinations of subject and pedagogy. Other development includes issues such as ethics, professionalism or administration. Classroom observation includes observation in the classroom. Classroom practice includes courses that require field experience. Subject content includes subject content (e.g. math). Each course was assigned a total value of one which was, in some cases, distributed over several types of training. An example may help to illustrate: SCE 4361 Introduction to Middle School Science Teaching was coded as pedagogical-content (0.3) and classroom observation (0.7). This is the based on the course description: “Introduction to the roles and responsibilities of science teachers with an emphasis on middle school students. Extensive fieldwork required.”
Boardman and Murnane (1979) and Todd and Wolpin (2003) model student achievement as a cumulative process in which the current achievement level depends on the individual’s initial endowment (e.g. innate ability) and their entire history of individual, family and schooling inputs. They then derive sets of restrictions that yield empirically tractable models of current achievement. Following their approach, we assume that the cumulative achievement function does not vary with age, is additively separable, and linear.\textsuperscript{11} Second, family inputs are assumed constant over time, and the impact of parental inputs on achievement, along with the impact of the initial individual endowment on achievement, induce a (student-specific) constant increment in achievement in each period.\textsuperscript{12} This allows the combination of these time-invariant inputs to individual achievement gains to be represented by a student-specific fixed component. Third, the marginal impacts of all prior school inputs decay geometrically with the time between the application of the input and the measurement of achievement at the same rate. Thus lagged achievement serves as a sufficient statistic for all prior schooling inputs. Given these assumptions, current achievement becomes a linear function of prior achievement, contemporaneous student/family and schooling inputs, and an individual specific constant.\textsuperscript{13}

\footnotesize{11 We partially relax the age-invariance assumption in our empirical work below by estimating separate elementary, middle and high school equations, thereby allowing the coefficients on schooling inputs to vary across grade groups.

\footnotesize{12 An observationally equivalent assumption is that the impact of home inputs is time invariant and the amount of home inputs change by a constant rate over time. If family inputs change over time (at a non-constant rate) and are correlated with variables in the achievement model, this will lead to biased estimates of the model parameters. For example, if parents compensate for an inexperienced teacher by spending more time with their child in learning activities at home, this would impart a downward bias on the estimated effect of teacher experience on student learning. There is little consistent evidence whether or not home inputs systematically vary over time with the quality of schooling inputs, however. Bonesronning (2004) finds that class size has a negative effect on parental effort in Norway, suggesting that school and home inputs are complements. In contrast, Houtenville and Conway (2008) find that parental effort is negatively correlated with school-level per pupil expenditures on instructional personnel, implying that school resources and parental effort are substitutes.

\footnotesize{13 A thorough discussion of these assumptions and the derivation of the linear education production function model can be found in Todd and Wolpin (2003), Sass (2006) and Harris and Sass (2006).}
We represent contemporaneous achievement inputs by a vector of time-varying student/family attributes, \( X_{it} \) (where the subscripts denote individuals (i) and time (t)), classroom peer characteristics, \( P_{ijmt} \) (where the subscript –i students other than individual i in classroom j at school m), time-varying teacher characteristics (eg. experience and in-service training), \( T_{kt} \) (where k indexes teachers), time-invariant teacher characteristics (eg. innate ability and pre-service education), \( \delta_k \), and non-teacher classroom-level inputs (such as books, computers, etc.), \( Z_j \). If we assume that, except for teacher quality, there is no variation in education inputs across classrooms within a school, the effect of \( Z_j \) becomes part of the school-level input vector, \( S_m \). School-level inputs can be de-composed into those that vary over time and those that are invariant over the time period of analysis. The time-varying components we measure are the administrative experience of the principal, the principal’s administrative experience squared, whether the principal is new to the school and whether the school is in its first year of operation. Time-invariant school inputs are captured by a school fixed component, \( \phi_m \). The achievement function can then be expressed as:

\[
A_{it} = \lambda A_{it-1} + \beta_1 X_{it} + \beta_2 P_{ijmt} + \beta_3 T_{kt} + \beta_4 S_m + \gamma_i + \delta_k + \phi_m + \nu_{it}
\]  

(1)

where \( \gamma_i \) represents the student-specific fixed component and \( \nu_{it} \) is a mean zero error.

We include three measures of teacher education and training in the vector of time-varying teacher characteristics, \( T_{kt} \). Experience, representing on-the-job training, is captured by a set of indicator variables representing various levels of experience; the omitted category is teachers with zero experience. This specification allows for non-linear effects of teacher experience on student achievement and avoids perfect collinearity between experience and time that would result from a continuous linear measure of teacher experience. In-service training is measured
by a vector of variables representing the number of hours spent in various types of professional
development courses. Both current-year hours of training as well as the amount of training in
each of the five prior years are measured separately to allow for delayed implementation of new
teaching strategies, human capital depreciation and possible negative impacts of
contemporaneous training on student achievement associated with absences from the classroom.
Finally, attainment of post-baccalaureate degrees is included to capture the effects of additional
formal education obtained after entering the teaching profession. The vector of coefficients on
these time-varying teacher characteristics, $\mathbf{\beta}_3$, thus represents the impact of within-career
education and training on teacher productivity. In some cases, we allow $\mathbf{\beta}_3$ to vary over time, but
have omitted the time subscript for simplicity.

Estimation of equation (2) by ordinary least squares (OLS) is problematic since the error
term is correlated with lagged achievement, rendering biased estimates of the regression
coefficients. To avoid this bias we shall focus on estimating models where $\lambda$ is assumed to
equal one and thus the dependent variable is $A_{it} - A_{it-1}$ or the student achievement gain:

$$A_{it} - A_{it-1} = \Delta A_{it} = \mathbf{\beta}_1 X_{it} + \mathbf{\beta}_2 P_{ijmt} + \mathbf{\beta}_3 T_{it} + \mathbf{\beta}_4 S_{it} + \gamma_i + \delta_k + \phi_m + \nu_{it}$$ (2)

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15 Alternatively, one could leave lagged achievement on the right hand side and employ twice and greater lags of
achievement as instruments for prior-year achievement (see, for example, Sass (2006) and Koedel and Betts (2009)).
Given that our achievement data begins in third grade and our model contains a student fixed effect, such an
approach is infeasible at the elementary school level. For middle and high school, we explored the use of
instrumental variables (IV) estimation, but the iterative procedure we employ would not converge when used with
IV regression.
This implies that the decay rate on prior inputs is zero; school inputs applied at any point in time have an immediate and permanent impact on cumulative achievement.\footnote{Thus, for example, the quality of a child's kindergarten must have the same impact on his cumulative achievement as of the end of the kindergarten year as it does on his achievement at age 18.} This is of course a strong assumption; we therefore test whether changes in the assumed value of lambda affect our results.

\textit{B. Teacher-Student Sorting}

The three-way-fixed-effects approach we employ will alleviate bias associated with sorting of students and teachers to schools and classrooms based on their time-invariant characteristics. However, as Rothstein (2009) argues, if students are dynamically assigned to teachers on the basis of prior unobserved shocks to student achievement and these shocks are serially correlated, then even models with student, teacher and school fixed effects will yield biased estimates of teacher quality. For example, if students who have an unusually good test score in one year are systematically assigned to particular teachers the following year and then fall back to their typical achievement gain, estimates of the teachers’ effects on student learning will be biased. Using data from a cohort of students in North Carolina, Rothstein conducts falsification tests that support the existence of dynamic sorting. He shows that future teachers have apparent “effects” on current achievement gains, even when controlling for student fixed effects.

Koedel and Betts (2009) argue that if dynamic sorting of student and teachers to classrooms is transitory, then the associated bias will be mitigated by observing teachers over multiple time periods. Using data from San Diego, they replicate Rothstein’s findings for a single cohort of students. However, they also show that when teachers are observed teaching
multiple classrooms of students and time-invariant characteristics are taken into account via student and teacher fixed effects, future teacher “effects” are no longer significant, thereby failing to reject the null of strict exogeneity.

For all of our analyses, we impose two sample restrictions in order to mitigate any potential bias from dynamic assignment of students to teachers. First, following Koedel and Betts, we limit our sample to teachers who taught at least two classes of students during the sample period. This restriction has the additional advantage of eliminating teachers whose estimated effects would be very imprecise due to few student observations per teacher. Second, we conduct the strict exogeneity falsification tests proposed by Rothstein by testing for apparent “effects” of both the identity of future teachers and the characteristics of future teachers on current achievement gains. We then limit our analysis sample to those districts in Florida in which the Rothstein test fails to reject the null of strict exogeneity for either teacher effects or teacher characteristics at a 95 percent confidence level.

C. Selection Into Training

Not only can the matching of students to teachers be non-random, the acquisition of training by teachers may be non-random as well. If the amount of PD a teacher obtains is

\[ \text{footnote text} \]

17 Since elementary school teachers typically teach one class per year while teachers in middle and high school teach multiple classes per year, eliminating teachers who only taught a single course during the estimation period reduces the sample by about 15 percent in elementary school, but only decreases the middle and high school samples by about one percent. Because our sample restriction eliminates elementary school teachers who start teaching in the final year of the sample or who depart after their first year of teaching, our findings should be viewed with caution when evaluating training for first-year elementary school teachers.

18 To implement Rothstein’s falsification test of future teacher effects we employ the econometric specification used by Koedel and Betts (2009), pp. 15-17. The test of future teacher characteristics is discussed in the appendix to Rothstein (2009).

19 Results of the exogeneity tests for each county in Florida are available in an Appendix available from the authors. We also eliminate some small districts where there were too few teachers to conduct the Rothstein test.
correlated with other unmeasured teacher characteristics that influence teacher productivity, our estimates of the impact of PD on student achievement could be biased.

If some teachers consistently engage in more professional development than others, due to their tastes, ability, or other time-invariant factors, teacher fixed effects will account for these differences and self-selection in PD acquisition will not lead to bias. Similarly, if the amount of PD is driven by observable factors in the achievement model, like experience, then the estimated impact of PD on student achievement will not suffer from selection bias. However, if teachers select into PD in ways that are correlated with time-varying performance, a problem analogous to the dynamic assignment of students to teachers would exist. For example, if teachers who temporarily exhibit low productivity either choose to obtain more PD or are systematically assigned to acquire more PD by administrators, but would have seen their productivity rise in the absence of any intervention, then the estimated effects of PD will be biased upward.20

The potential for bias due to time-varying self-selection into PD is limited by teacher certification requirements in Florida, however. The standard “professional certificate” in Florida is good for a period of five years. To renew her certification, a teacher must complete 120 hours of PD or earn six semester hours of college credits. The renewal application must be submitted during the last year the current certificate is valid.21 Thus certification imposes an exogenous

20 In 2000, the Florida legislature passed legislation establishing new professional development procedures. This included a provision requiring “each school principal to establish and maintain an individual professional development plan for each instructional employee assigned to the school. The individual professional development plan must . . . [be] related to specific performance data for the students to whom the teacher is assigned.” (2000 Florida Statutes, 231.600(4)(b)5). However, monitoring of district and school compliance by the Florida Department of Education did not begin until a “Professional Development System Protocol” was initiated in Spring 2003. As of 2006, Bergquist (2006) found district-level compliance with state mandated PD procedures was relatively strong, but implementation of the teacher-level requirement was in the “unacceptable” range, indicating little or no evidence the standard was being implemented. Given our sample ends in the 2004/05 school year, it seems the legislative mandate to link PD to prior performance did little to contribute to the potential endogeneity of PD acquisition in our analysis. For further details see Florida House of Representatives (2008).
21 See http://www.fldoe.org/edcert/renew.asp
minimum quantity of PD on all teachers. Further, if teachers minimize the present value of the

cost of PD by backloading PD acquisition toward the end of their certification period, then

variation in the certification period among teachers would lead to exogenous variation in the
timing of PD across teachers. We present evidence in the Appendix confirming that PD choices

are determined primarily by observable teacher characteristics and by re-certification

requirements.22

D. Computational Issues

Estimation of (2) is computationally challenging since it includes three levels of fixed
effects: individual students (γi), teachers (δk) and schools (φm). Standard fixed effects methods
eliminate one effect by demeaning the data with respect to the variable of interest (e.g. deviations
from student means). Additional effects must then be explicitly modeled through the inclusion
of dummy variable regressors. Given our data includes tens of thousands of teachers and
thousands of schools, such standard methods are infeasible.

We combine two different approaches to solve the computational problem associated
with estimating a three-level fixed effects model. First, we utilize the “spell fixed effects”
method proposed by Andrews, et al. (2006) and combine the teacher and school fixed effects into
a single effect, ηkm = δk + φm. This combined effect represents each unique teacher/school
combination or “spell.” The achievement thus becomes:

\[ \Delta A_{it} = \beta_1 X_{it} + \beta_2 P_{iitm} + \beta_3 T_{it} + \beta_4 S_{mt} + \gamma_i + \eta_{km} + \nu_{it} \]  (3)

22 There is also strong evidence from recent reports by Florida schools districts that acquisition of in-service training
is driven largely by certification requirements. For example, in 2006/07, the stated purpose for over 75 percent of
in-service hours was certification renewal and another 15 percent of in-service hours were for the purpose of
acquiring additional certifications. Only eight percent was for “professional skill building.” See Florida Department
The second approach is an extension of the iterative fixed effects estimator proposed by Arcidiacono, et al. (2005). The essence of the Arcidiacono et al. method is to estimate the fixed effect for each individual by calculating each individual’s error in each time period (ie. actual outcome minus the individual’s predicted outcome) and then compute the mean of these errors for each individual over time. With each estimate the individual fixed effects are recomputed and the process is iterated until the coefficient estimates converge.

Taking deviations from the teacher-school spell means and subtracting the de-meaned student effect from both sides, the achievement equation becomes:

\[(\Delta A_{it} - \bar{\Delta A}_{km}) - (\gamma_i - \bar{\gamma}_{km}) = \beta_1 (X_{it} - \bar{X}_{km}) + \beta_2 (P_{ijmt} - \bar{P}_{km}) + \beta_3 (T_{kt} - \bar{T}_{km}) + \beta_4 (S_{mt} - \bar{S}_{km}) + \nu_{it} \]  

where the overbar and km subscript denote the mean of the relevant variable over all students and all time periods covered by teacher k at school m. Equation (4) is estimated by ordinary least squares (OLS), using initial guesses for the individual effects. This produces estimates of \(\beta_1, \beta_2, \beta_3 \) and \(\beta_4\) which are then used to calculate predicted outcomes for each individual and in turn update the estimated individual effects. The process is iterated until the coefficient estimates converge. Standard errors are obtained by bootstrapping.

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\(^{23}\) Arcidiacono et al derive their estimator in the context of a model with only fixed effects and no other covariates. However, it is straightforward to extend their approach to models with covariates. Details of the derivation are available upon request. Arcidiacono, et. al. (2005, p.9) demonstrate in simulations that their estimator is unbiased, regardless of the number of observations per student. Their approach does introduce a small bias in a model with so-called “peer fixed effects,” but that is not relevant here. A refined procedure which yields unbiased estimates in the presence of peer fixed effects is discussed in Arcidiacono, et al. (2007). We demonstrate in the Appendix that the Arcidiacono, et al. (2005) iterative estimator produces estimates that are extremely close to the standard OLS estimates.

\(^{24}\) The standard errors from the bootstrap procedure do not account for clustering of students within a classroom or classrooms within a school. This is partly compensated for by the fact that we include classroom peer measures and teacher fixed effects (which correspond to a common average error for all students a teacher ever teaches). While recent research has produced bootstrap procedures for clustering at a single level (Cameron, Gelbach and Miller
E. Measuring the Effects of Pre-Service Education on Teacher Productivity

In order to gauge the effects of teacher ability and college preparation on future productivity we follow a two-step estimation procedure first proposed by Dickens and Ross (1984).\(^\text{25}\) In the first step we calculate the estimated teacher-school effects from the estimation of equation (4). The predicted teacher-school spell fixed effect can be expressed as the difference between the average achievement gain for all students in group \(km\) minus the product of the estimated coefficients and the group averages of the explanatory variables:

\[
\eta_{km} = \Delta A_{km} - \hat{\gamma}_{km} - \hat{\beta}_1 \bar{X}_{km} - \hat{\beta}_2 \bar{P}_{km} - \hat{\beta}_3 \bar{T}_{km} - \hat{\beta}_4 \bar{S}_{km}
\]  

(5)

These teacher-school effects can be decomposed into three time-variant components: the part of teacher effect due to the education they receive as undergraduates, the portion of the teacher effect due to pre-college ability, and the school effect. In the second step we gauge the impact of pre-service education on later teacher productivity by first de-meaning the teacher-school spell effect by school to remove the school effect component and then regressing the resulting within-school teacher effects on a vector of pre-service education variables for teacher \(k\), \(U_k\), their entrance exam scores, \(E_k\), and a random error:\(^\text{26}\)

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\(^{25}\) Alternatively, one could directly estimate the effects of teacher pre-service characteristics by entering them as explanatory variables in equation (2) and removing the teacher fixed effect, \(\delta_k\). However, given our college transcript data begin in 1995, we only have pre-service information for a fraction of teachers. Consequently, in many cases there are only one or two teachers with pre-service information in a school and the student and school effects are not well identified.

\(^{26}\) We center the teacher-school spell effects on the school mean so that the resulting teacher effects are relative to the mean teacher effect for all teachers at the school, whether or not their pre-service characteristics are known. If we did not de-mean the teacher-school effects ex-ante and instead included school indicators in equation (7), the resulting teacher effects would measure teacher productivity relative to other teachers at the same school with pre-service information.
\[ \hat{\eta}_{km} - \bar{\eta}_m = \hat{\delta}_k = \omega_1 U_k + \omega_2 E_k + \xi_{km} \]  \hspace{1cm} (6)

The estimates of the coefficient vector \( \omega_1 \) indicate the partial correlation between the characteristics of a teacher’s pre-service education and their future contribution to student achievement, holding constant their measured pre-college ability. Thus they can be interpreted as signals of future productivity. Following Dickens and Katz (1986), equation (8) is estimated by weighted least squares, with the square root of the numbers of students per teacher/school spell as weights.

V. Results

A. Effects of Experience and Professional Development Training

Initial estimates of the student achievement model, equation (5), without teacher fixed effects, are presented in Table 3. Experience enhances the productivity of both elementary and middle school teachers, but not high school teachers. Some prior research suggests that experience effects occur exclusively over the first few years on the job. Here, we find that the bulk of the experience effects are indeed in the early years, but there are still marginal effects even after 10 years of experience.

We also find contemporaneous professional development (PD) is associated with higher teacher productivity, but only in middle-school. This is unsurprising for several reasons noted in past research (Goldhaber and Anthony (2007), Harris and Sass (2009)). PD takes time away from classroom instruction and preparation time. In addition, if substitute teachers are hired so that the PD can take place during school hours, and if the substitutes are less effective or unable to maintain the continuity of instruction in the permanent teacher’s absence, then this too may
reduce measured teacher value-added. Finally, because some portion of the school year has typically passed when PD occurs, and lesson plans for later in the year may already be established, teachers may have little opportunity to immediately incorporate what they learn.

If PD boosts the human capital of teachers, we would expect to see positive effects on student achievement after the professional development training is acquired and put to use in the classroom, perhaps with some diminution over time if the knowledge depreciates or becomes less relevant. However, we only observe something close to this pattern for high school math teachers. For high school math, PD acquired in the current year has a negative effect, but the net effect on student achievement becomes positive after two years and continues to be positive three years after acquisition. Fourth and fifth-lagged PD has no significant effect on high school math achievement. For each of the other subject-grade level combinations, there is a haphazard pattern of insignificant, positive and negative effects of past PD on current student achievement gains.

Even where PD does seem to show consistent positive effects, they appear modest in size relative to experience. Given the average number of hours of professional development coursework per year is approximately 50, the difference between having no professional development versus the average professional development is at most about \((0.005 \times 50 = 0.25)\) scale score points or about one-fifth the difference between a rookie middle school math teacher and one with a year of experience. Also, we estimate the cost of 50 hours of PD is $3,175, yielding an effect-to-cost ratio of 0.0019.\(^{27}\) In comparison, Harris (2009) finds the comparable

\[^{27}\text{Effects on test scores in education research are often reported by dividing by the standard deviation of the test score level. The 0.25 scale score points translates to 0.006 standard deviations. The average hourly wage (plus fringe benefits) of teachers in 2007 was $37 (Harris, 2009). Thus, the opportunity cost of average annual PD is 50*37=$1,850. There are other costs such as PD development, the time and preparation of PD instructors, and facilities. According to the Florida House of Representatives (2008), these direct costs were approximately $1,150.}\]
The ratio for class size reduction is roughly 0.026-0.086 and even this ratio is considered small relative to alternative education programs such as peer tutoring. Nor would a ratio of this size pass a cost-benefit test (Harris, 2009; Krueger, 2003). Thus, the effects of PD seem small by any measure.

Adding teacher fixed effects to the model controls for time-invariant teacher characteristics, thereby ameliorating bias due to self-selection of teachers into training based on time invariant traits like motivation or intelligence. However, the inclusion of teacher fixed effects means that any training effects are identified by within-teacher variation in training, which is generally smaller than the variance in training across teachers. The inclusion of teacher effects alters the results in some important ways. As shown in Table 4, the positive effects of early-career experience (1-2 years) in elementary and middle school found in the model without teacher effects (Table 3) remain largely unchanged; however, the precision is substantially reduced for experience effects above 5 years in elementary math and middle school reading. Overall, the results indicate that experience effects in elementary and middle school are quantitatively substantial, ranging from 1.1 to 2.4 scale score points for the first 1-2 years of experience to as much as 2.3-3.7 scale score points for 15-24 years of experience. This translates to 0.04 to 0.09 of a standard deviation in achievement gains or 0.03 to 0.06 of a standard deviation in the achievement level for the first couple of years of experience and as much as 0.16 of a standard deviation in achievement gains for a teacher with 15-24 years of experience (relative to a first-year teacher). In high school, the estimated effects of teacher experience are per teacher in 2002/03 or about $1,325 in 2007 dollars, yielding $3,175 in total costs per teacher. The costs from Harris (2009) for class size are expressed in thousands. So, the comparable ratio for PD is 0.006/3.175=0.0019.
reduced relative to those from the model excluding teacher fixed effects. More experienced high
school teachers are generally less productive than when they were rookie high school instructors.

Consistent with previous research, the contemporaneous effects of PD are now
consistently insignificant or negative. The results for prior PD are qualitatively similar to the
model without teacher effects, though some of the anomalous negative effects of prior PD
become insignificant. The only situations where prior PD has positive effects on current student
achievement gains are middle and high school math.

In Table 5 we control for formal education in the form of attainment of advanced degrees
and differentiate between content-oriented PD and PD designed to address other components of a
teacher’s human capital, such as classroom management techniques and pedagogy. Since our
model includes teacher fixed effects, post-baccalaureate degrees earned prior to the period of
analysis wash out when we demean the data. Thus our approach measures the impact of changes
in the possession of an advanced degree (for a given teacher) during the period of study.28 Our
results indicate that obtaining an advanced degree during one’s teaching career is positively
correlated with teacher productivity only in the case of middle school math. For middle school
reading the effect is actually negative and for all others the effects are insignificant. This may be
because graduate degrees include a combination of pedagogy and content and our other evidence
suggests that only the latter has a positive influence on teacher productivity.

28 The estimated coefficient on the advanced-degree variable measures the average productivity differential between
the time before and the time after receipt of the degree. Before the degree is received some knowledge may have
already been acquired through coursework already completed, thus biasing the estimated effect toward zero. However,
work toward an advanced degree may take away from time available for class preparation and other
teaching-related activities, which would tend to lower productivity before receipt of the degree and upwardly bias
the estimated impact of the degree.
Other explanations for the graduate degree results arise from issues of methodology. Our use of teacher fixed effects approach imposes the implicit assumption that the receipt of the graduate degree reflects a sudden infusion of new preparation. In reality, the receipt of the degree is the culmination of several years of graduate courses whose influence may already be reflected in the teacher effects, especially for those teachers who take graduate courses over many years before receiving a graduate degree. Another possibility is that teachers load up on courses in the academic year preceding the receipt of the degree and therefore have less time to devote to their students. We found evidence above of such a contemporaneous decline in productivity when we considered the effects of other forms of professional development.

We find that the positive effects of total PD on the productivity of middle and high school math teachers, discussed above, are attributable mainly to content-oriented training. For middle school math we find positive effects for PD acquired up to five years in the past. The coefficients on once and twice-lagged PD are also positive for high school math, but only twice lagged PD is statistically significant for high school math teachers (in addition to contemporaneous PD). The lagged effects of other non-content in-service hours on middle school math are less frequently positive and significant and more frequently negative and significant. On the reading side we continue to find null or negative coefficients of all forms of PD.

In Table 6 we investigate whether the marginal effects of PD and advanced degrees differ by the experience of the teacher. If on-the-job training acquired through experience is a substitute for PD and formal education, then the marginal effects of PD and advanced degrees should decline as a teacher acquires experience. The results provide at best modest support for this hypothesis, however. For content oriented PD acquired by math teachers within the past two
years (which is the maximum number of relevant lags for teachers in the lowest experience category, 0-2 years of experience), we find positive and significant effects of PD in six instances. Three arise for teachers with 0-2 years (contemporaneous and twice-lagged PD for high school math, once-lagged PD for middle school math). Two significant occurrences are for teachers with 3-9 years of experience (once lagged elementary math, twice lagged middle school math) and once for veteran teachers with 10 or more years of experience (high school math). There is no clear pattern to the magnitude of the PD coefficients with respect to experience.

B. Alternative Specifications and Alternative Samples

The student-and-school fixed effect model results presented in Table 3 and the student-and-teacher/school spell fixed effect model results in Table 4 represent just two possible specifications of the student achievement model. Other commonly estimated specifications vary in their treatment of student heterogeneity and the assumed degree of persistence in prior schooling inputs. In the Appendix to this paper we present estimates of models that employ observable student characteristics rather than student fixed effects to control for time-invariant student characteristics. We also estimate student and teacher/school spell models where the persistence of lagged schooling inputs ($\lambda$) is systematically varied from 1.0 to 0.2. The effects of PD on student achievement are quite robust to changes in the assumed level of persistence. In contrast, the positive and inter-temporally consistent effects of PD for high school math teachers no longer hold up when student fixed effects are omitted.

In addition to exploring the robustness of our results with respect to alternative controls for student heterogeneity and varying degrees of persistence in past schooling inputs, we estimate a variety of alternative specifications that account for possible ceiling effects in the test instrument, that allow for variation in the number of lags of prior PD included in the model, and
that check for collinearity between acquisition of PD and formal education through the attainment of advanced degrees. Details of these alternative specifications and the resulting estimates are provided in the Appendix. In each case, the basic findings presented in Tables 3 and 4 are found to be robust.\(^{29}\)

We also consider the effects of altering the estimation sample on our results. The results presented in Tables 3 and 4 are based on separate samples of elementary, middle and high school students. In addition to highlighting the differential effects of teacher training across grade groups, this approach maximizes the number of districts we can include in our sample because for many districts, evidence of dynamic student-teacher matching is found at some grade groups within a district and not others. The advantage of pooling across grade groups is an increase in the number of observations per student and thus a potential improvement in the identification of student and teacher effects. In the Appendix we present estimates of the achievement model, pooled across all grades with grade-group interactions to allow for differential slopes. Similar to the results from grade-group subsamples, we find that contemporaneous PD has no significant effects on either math or reading achievement and prior PD has virtually no positive effects on reading achievement. Lagged PD for math teachers continues to have some positive effects on current-year achievement gains.

**C. Pre-Service Training Effects**

In this section we use the estimated teacher effects as the dependent variable in order to analyze the effects of pre-service training on the time-invariant component of a teacher’s productivity. Table 7 displays the results of estimating equation (7), measuring a teacher’s pre-

\(^{29}\) Consistent with our findings, Koedel and Betts (2008) show that ceiling effects are generally not a significant source of bias in achievement models.
service formal education, $U_k$, by the number of credits earned in various types of education and subject-relevant non-education courses. Pre-college ability is taken into account by including the SAT-equivalent entrance exam scores of future teachers.\(^{30}\) This allows us to distinguish between the human capital effects of college coursework and the self-selection of individuals into particular types of courses. Unfortunately, our sample size drops significantly because our data only contain information on a teacher’s college coursework if she attended a public university in Florida in 1995 or later. In addition, even for the subsample of future teachers with transcript information, there are a substantial number of missing observations on the college entrance exam. In the Appendix we present results for a larger sample that does not control for college entrance exam scores.

With the lone exception of courses on classroom management taken by future high school reading teachers, we find no evidence that the quantity of any particular type of college coursework is associated with greater productivity.\(^{31}\) This finding is corroborated by an analysis of college majors, presented in the Appendix. There we find that education majors are no more or less productive than teachers whose initial bachelor’s degree was in another discipline. We also find that teacher college entrance exam scores are not associated with teacher productivity.\(^{32}\)

Overall, while our results are inconsistent across grades and subjects, it appears that colleges of education might improve the performance of their graduates, and schools might

\(^{30}\) To maximize the amount of college entrance exam information available we include data from the state university system, the community college system and a database on applicants for a state merit-based scholarship known as “Bright Futures.” ACT scores as well as community college placement exam scores were converted to SAT-equivalent scores using concordance tables.

\(^{31}\) It is important to keep in mind that we are only analyzing math and reading teachers. It is quite possible that in other areas, such as science in secondary schools, that content knowledge acquired through undergraduate coursework could be an important determinant of teacher productivity.

\(^{32}\) The only previous study to include entrance exam scores, Ferguson and Ladd (1996), utilizes school-level average composite scores on the American College Test (ACT), rather than the scores of individual teachers.
improve the productivity of existing teachers, if they were to place somewhat greater emphasis on content knowledge, including that which is pedagogically oriented. This conclusion is suggested by both the apparently positive effects of content-oriented courses in teacher preparation programs and the effects of content-oriented in-service professional development in middle and high school math.

VI. Summary and Conclusions

Our study differs in several important ways from other recent contributions to the rapidly expanding literature on teacher training. First, ours is the first multi-district study to simultaneously control for unobserved student, teacher and school heterogeneity through the use of multiple levels of fixed effects. Coupled with tests with for dynamic student-teacher sorting, we argue this approach significantly attenuates selection bias that may have affected much of the previous literature on teacher training. Second, while most recent research has focused on teacher experience and attainment of advanced degrees, ours is the first analysis to concurrently estimate the impacts of experience, post-baccalaureate degrees, in-service professional development and pre-service undergraduate education on the productivity of teachers, as well as the interactions among them. Further, we are able to measure the various forms of training at a finer level than in most prior studies, including distinguishing specific types of coursework at the undergraduate level and differentiating between different kinds of professional development training received while teaching. Finally, ours is the first study to distinguish between the quality of undergraduate training and the innate ability of future teachers by including individual-specific college entrance exam scores.
Our findings corroborate some of the evidence presented in prior research, but also uncover some important new insights. Like other recent work, we find generally positive, but somewhat mixed, evidence on the effects of experience and little or no evidence of the efficacy of advanced degrees for teachers. We find that the first few years of experience substantially increase the productivity of elementary and middle school teachers but have little impact on the effectiveness of teachers at the high school level. Only in the case of middle school math do we find that obtaining an advanced degree enhances the ability of a teacher to promote student achievement. For all other grade/subject combinations the correlation between advanced degrees and student achievement is negative or insignificant.

The results vary considerably by grade level and subject. While this may reflect real differences in training impacts, it may also be partly due to the varying degree to which the actual content of courses match the content of the exams, as well as varying external influences on exam scores. We are arguably best able to measure a teacher’s impact on student learning in middle school math because: (a) math appears less prone than reading to the influence of non-school activities that students participate in; (b) course content is more closely linked to test content in middle school than in high school, where math courses become more diverse and specialized and English/language arts courses including subjects like literature; and (c) middle school teachers have many more student observations per teacher than do elementary teachers (reducing the “noise” in teacher productivity estimates). If we were to focus only on middle school math, the results would be more positive with regard to the efficacy of teacher experience, professional development, advanced degrees, and undergraduate coursework. While very few previous studies have included middle school, some of those that have (Betts, et al (2003),
Rivkin et al. (2005) find a relatively stronger effects of experience on teacher productivity in middle school, suggesting the same pattern.

Our weak and inconsistent findings for the effect of a general system of professional development on teacher productivity are consistent with the studies of accountability-based professional development and PD for early-reading teachers by Jacob and Lefgren (2004) and by Garet, et al. (2008), respectively. Both those studies analyzed PD applied to elementary school teachers and found no significant impacts of PD on student achievement. The results in the present study are somewhat more positive, particularly in grades beyond elementary school and for PD that focuses on content.

We find almost no evidence that specific undergraduate coursework in education affects an individual’s later productivity as a teacher. Similarly, only in the case of high school reading teachers do teachers whose first undergraduate degree was in education significantly outperform non-education majors. When pre-college ability is taken into account by college entrance exam scores, even this differential becomes insignificant. Our results are consistent with findings from the only other recent studies to include data on college majors, Aaronson, et al. (2007) and Betts, et al. (2003), as well as studies comparing teachers who completed traditional teacher preparation programs with “alternatively certified” teachers (Boyd, et al. (2006), Kane, et al. (2006), Xu, et al. (2008), Constantine, et al (2009)).

Although much work remains to fully understand the ways in which training affects the ability of teachers to promote student learning, our analysis, in combination with other recent research, does offer some tentative suggestions for shaping future policy. First, our finding (and that of others) that experience greatly enhances the productivity of elementary and middle school teachers early in their careers indicates that policies designed to promote retention of young
teachers can yield significant benefits over and above avoiding the cost of hiring new teachers. Second, our finding (consistent with prior research), that advanced degrees are uncorrelated with the productivity of elementary school teachers suggests that current salary schedules, which are based in part on educational attainment, may not be an efficient way to compensate teachers in primary school. Third, our evidence that only content-oriented professional development coursework taken by middle and high-school math teachers appears effective suggests that relatively more resources ought to be put into content-focused training for teachers in the upper grades and that changes are warranted in PD at the elementary level and in pedagogical in-service training generally. Finally, given we find scant evidence that the amount of undergraduate coursework in education effects future productivity and our work and that of others does not find education majors are significantly more productive as teachers than non-education majors, it seems worthwhile to rethink the structure of traditional preparation programs and continue experimentation with so-called “alternative certification” programs that facilitate the entry of non-education majors into teaching.
References


### Table 1
**Results of Recent Studies of the Effects of Teacher Training on Student Achievement in the United States, By Teacher Training Type**

<table>
<thead>
<tr>
<th>Method/Studies</th>
<th>Undergraduate Studies</th>
<th>Graduate Degrees</th>
<th>In-Service Training</th>
<th>Experience</th>
</tr>
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<tr>
<td><strong>Gain Score with Student Covariates</strong></td>
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<tr>
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<td>Major (MH 0)</td>
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<td>(MH 0)</td>
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<td>Hill et al. (2005)</td>
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<td>Kane, et al. (2006)</td>
<td>GPA (ME/MM 0, RE/RM 0) College Selectivity (ME/MM 0, RE/RM 0)</td>
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<td><strong>Panel Data with Student Fixed Effects</strong></td>
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<td>Major (All Mix)</td>
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<td>(ME 0, RE 0, MM +, RM 0, MH 0, RH 0)</td>
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</table>

Each cell starts by listing the specific variable under consideration, except for the last two columns where in-service training and experience are defined the same way across studies. Effects on student achievement are given in parentheses. The first letter indicates the subject area: M = math, R = reading, C=combined. The second letter indicates the grade level: E = elementary, M = middle school, and H = high school. This is followed by information regarding the effects of the specified variable on student achievement scores in the previously specified subject and grade in the preferred specifications: ++ = positive and significant in nearly all preferred specifications; + = often positive and significant; 0 = insignificant; - = often negative and significant; -- = negative and significant in nearly all preferred specifications; and Mix = mix of positive/significant and negative/significant.
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<th>High School (Grades 9-10)</th>
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<td>4.7607**</td>
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<td>-0.0003</td>
<td>0.0055**</td>
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<td>(2.23)</td>
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<td>New Principal at School</td>
<td>-0.5340**</td>
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<td>0.8040***</td>
<td>-0.6227***</td>
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<td>(0.16)</td>
<td>(1.63)</td>
<td>(1.82)</td>
<td>(4.59)</td>
<td>(3.07)</td>
</tr>
</tbody>
</table>

| Student Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Teacher Fixed Effects | No  | No  | No  | No  | No  | No  |
| School Fixed Effects  | Yes | Yes | Yes | Yes | Yes | Yes |

Number of Students 216,903 295,930 223,793 240,327 192,633 161,961
Number of Observations 439,786 683,064 501,640 487,485 436,052 325,796

Models include grade-by-year and repeater-by-grade dummies. Absolute values of bootstrapped t-statistics, based on 50 repetitions, appear in parentheses. * indicates statistical significance at the .10 level and ** indicates significance at the .05 level and *** indicates significance at the .01 level in a two-tailed test.
### Table 4
Iterated OLS Estimates of the Effects of Teacher Experience and In-Service Training on Student Math and Reading Achievement in Florida, 1999/2000-2004/2005

<table>
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<tr>
<th></th>
<th>Math</th>
<th>Reading</th>
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</thead>
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<td>Elementary (Grades 4-5)</td>
<td>Middle (Grades 6-8)</td>
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<tr>
<td>1-2 Years of Experience</td>
<td>1.2845* (1.91)</td>
<td>2.0165*** (5.85)</td>
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<td>3-4 Years of Experience</td>
<td>1.8808*** (2.63)</td>
<td>2.6978*** (6.23)</td>
</tr>
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<tr>
<td>5-9 Years of Experience</td>
<td>0.9314 (1.10)</td>
<td>2.7568*** (5.27)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10-14 Years of Experience</td>
<td>1.1908 (1.14)</td>
<td>2.7441*** (4.20)</td>
</tr>
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<td>15-24 Years of Experience</td>
<td>0.9918 (0.91)</td>
<td>3.7083*** (4.44)</td>
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<tr>
<td>25+ Years of Experience</td>
<td>0.0498 (0.04)</td>
<td>4.2256*** (4.21)</td>
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<tr>
<td>Total In-service Hours,</td>
<td>-0.0035* (1.74)</td>
<td>0.0013 (0.79)</td>
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<tr>
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<td>0.0006 (0.24)</td>
<td>0.0051** (2.87)</td>
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<tr>
<td>Total In-service Hours,</td>
<td>-0.0048* (1.93)</td>
<td>-0.0011 (0.64)</td>
</tr>
<tr>
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</tr>
<tr>
<td></td>
<td>0.0007 (0.64)</td>
<td>0.0016 (1.10)</td>
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<tr>
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</tr>
<tr>
<td>Total In-service Hours,</td>
<td>-0.0001 (0.03)</td>
<td>0.0044** (2.46)</td>
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<tr>
<td></td>
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<td></td>
</tr>
<tr>
<td></td>
<td>-0.0018 (0.54)</td>
<td>0.0043** (2.44)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Schools</td>
<td>-0.4953 (1.14)</td>
<td>-1.0714*** (3.84)</td>
</tr>
<tr>
<td>Attended</td>
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<tr>
<td>Structural Move</td>
<td>0.1352 (0.08)</td>
<td>-0.8415 (3.50)</td>
</tr>
<tr>
<td></td>
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<td></td>
</tr>
<tr>
<td>Non-Structural Move</td>
<td>0.9764*** (3.09)</td>
<td>-0.2561 (1.21)</td>
</tr>
<tr>
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<td></td>
</tr>
<tr>
<td>Fraction of Peers - Female</td>
<td>-0.4995 (0.38)</td>
<td>0.5011 (1.23)</td>
</tr>
<tr>
<td></td>
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<td></td>
</tr>
<tr>
<td>Fraction of Peers - Black</td>
<td>4.4849*** (2.96)</td>
<td>6.7867*** (11.96)</td>
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</tr>
<tr>
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<tr>
<td>---------------------------</td>
<td>-------</td>
<td>-------</td>
</tr>
<tr>
<td>Fraction of Peers -</td>
<td>0.3427</td>
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<td>Changed Schools</td>
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<td>(0.55)</td>
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<td>Fraction of Peers -</td>
<td>0.3842</td>
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<td>Structural Move</td>
<td>(0.14)</td>
<td>(3.48)</td>
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<td>Peer Mean Age in Sept.</td>
<td>-0.2132***</td>
<td>0.2111***</td>
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<tr>
<td>(months)</td>
<td>(3.05)</td>
<td>(9.13)</td>
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<tr>
<td>Class Size</td>
<td>-0.1356***</td>
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<td>(3.96)</td>
<td>(3.75)</td>
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<td>New School</td>
<td>-4.0335</td>
<td>-1.4962</td>
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<td>(0.96)</td>
<td>(0.59)</td>
</tr>
<tr>
<td>Principal’s Administrative</td>
<td>-0.0727</td>
<td>0.0289</td>
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<tr>
<td>Experience</td>
<td>(0.55)</td>
<td>(0.40)</td>
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<tr>
<td>Principal’s Administrative</td>
<td>0.0004</td>
<td>-0.0011</td>
</tr>
<tr>
<td>Experience²</td>
<td>(0.09)</td>
<td>(0.58)</td>
</tr>
<tr>
<td>New Principal at School</td>
<td>-0.7713***</td>
<td>-0.0603</td>
</tr>
<tr>
<td></td>
<td>(2.66)</td>
<td>(0.38)</td>
</tr>
</tbody>
</table>

| Student Fixed Effects     | Yes   | Yes   | Yes   | Yes   | Yes   | Yes   |
| Teacher Fixed Effects     | Yes   | Yes   | Yes   | Yes   | Yes   | Yes   |
| School Fixed Effects      | Yes   | Yes   | Yes   | Yes   | Yes   | Yes   |

| Number of Students        | 216,903| 295,930| 223,793| 240,327| 192,633| 161,961|
| Number of Observations    | 439,786| 683,064| 501,640| 487,485| 436,052| 325,796|

Models include grade-by-year and repeater-by-grade dummies. Absolute values of bootstrapped t-statistics, based on 50 repetitions, appear in parentheses. * indicates statistical significance at the .10 level and ** indicates significance at the .05 level and *** indicates significance at the .01 level in a two-tailed test.
Table 5  
Iterated OLS Estimates of the Effects of Teacher Experience, Content and Non-Content In-Service Training and Advanced Degrees on Student Math and Reading Achievement in Florida, 1999/2000-2004/2005

<table>
<thead>
<tr>
<th>Experience</th>
<th>Math</th>
<th>Reading</th>
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<tbody>
<tr>
<td></td>
<td>Elementary (Grades 3-5)</td>
<td>Middle (Grades 6-8)</td>
</tr>
<tr>
<td>1-2 Years of Experience</td>
<td>1.2766**</td>
<td>2.0403***</td>
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<tr>
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<td>(1.83)</td>
<td>(7.80)</td>
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<td>3-4 Years of Experience</td>
<td>1.8661**</td>
<td>2.7132***</td>
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<tr>
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<td>(2.34)</td>
<td>(6.58)</td>
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<tr>
<td>5-9 Years of Experience</td>
<td>0.9255</td>
<td>2.7718***</td>
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<tr>
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<td>(0.93)</td>
<td>(6.87)</td>
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<tr>
<td>10-14 Years of Experience</td>
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<td>2.7773***</td>
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<tr>
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<td>(0.87)</td>
<td>(4.32)</td>
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<td>15-24 Years of Experience</td>
<td>0.9828</td>
<td>3.7720***</td>
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<tr>
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<td>(0.68)</td>
<td>(5.09)</td>
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<td>25+ Years of Experience</td>
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<td>4.3061***</td>
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<tr>
<td>Content In-service Hours</td>
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<td>(0.74)</td>
<td>(0.69)</td>
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<tr>
<td>Content In-service Hours-1</td>
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<td>0.0071**</td>
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<td>(2.26)</td>
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<td>0.0067*</td>
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<td>(2.63)</td>
<td>(1.83)</td>
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<td>Content In-service Hours-3</td>
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<td>0.0031</td>
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<td>(0.19)</td>
<td>(0.89)</td>
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<td>(0.01)</td>
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<tr>
<td>Content In-service Hours-5</td>
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<td>0.0044*</td>
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<td>(0.97)</td>
<td>(1.66)</td>
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<tr>
<td>Other In-service Hours</td>
<td>-0.0036</td>
<td>0.0011</td>
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<td>(1.13)</td>
<td>(0.59)</td>
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<td>0.0045**</td>
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<td>(1.94)</td>
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<td>-0.0011</td>
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<td>Other In-service Hours&lt;sub&gt;t-5&lt;/sub&gt;</td>
<td>Advanced Degree</td>
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<td>-0.0003 (0.09)</td>
<td>-0.3783 (0.74)</td>
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<td>0.0044** (2.15)</td>
<td>0.7778** (2.18)</td>
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<td>0.0006 (0.25)</td>
<td>-0.3193 (0.50)</td>
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<td>0.0008 (0.27)</td>
<td>-0.3016 (0.54)</td>
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<td>0.0061* (1.92)</td>
<td>-1.2715* (1.93)</td>
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<td>0.0026 (0.86)</td>
<td>-0.6830 (0.77)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
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</thead>
<tbody>
<tr>
<td>Student Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Teacher Fixed Effects</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>School Fixed Effects</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
</tbody>
</table>

| Number of Students   | 216,893                  | 295,824                       | 223,631                     | 240,317                  | 192,595                       | 161,880                       |
| Number of Observations| 439,766                | 682,705                       | 501,284                     | 487,465                  | 435,908                       | 325,630                       |

Models include the following time varying student/class/school characteristics: number of schools attended by the student in the current year, “structural” move by student, “non-structural move” by student, indicator of a student repeating a grade, class size, fraction of classroom peers who are female, fraction of classroom peers who are black, average age (in months) of classroom peers, fraction of classroom peers who changed schools, fraction of classroom peers who made a “structural move,” indicator for a new school, indicator for a new principal at a school, principal’s years of administrative experience. All models also include grade-by-year and repeater-by-grade dummies. Absolute values of bootstrapped t-statistics, based on 50 repetitions, appear in parentheses. * indicates statistical significance at the .10 level and ** indicates significance at the .05 level and *** indicates significance at the .01 level in a two-tailed test.
### Table 6
Iterated OLS Estimates of the Effects of Teacher Experience, Content and Non-Content In-Service Training and Advanced Degrees by Experience Level on Student Math and Reading Achievement in Florida, 1999/2000-2004/2005

<table>
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<th>Experience Level</th>
<th>Math</th>
<th>Reading</th>
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</thead>
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<tr>
<td></td>
<td>Elementary (Grades 4-5)</td>
<td>Middle (Grades 6-8)</td>
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<tr>
<td>1-2 Years of Experience</td>
<td>1.2270 (1.60)</td>
<td>2.0224*** (7.13)</td>
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<tr>
<td>3-4 Years of Experience</td>
<td>2.2359** (2.39)</td>
<td>2.8369*** (4.90)</td>
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<tr>
<td>5-9 Years of Experience</td>
<td>1.3827 (1.18)</td>
<td>2.9599*** (5.19)</td>
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<tr>
<td>10-14 Years of Experience</td>
<td>2.7718* (1.72)</td>
<td>2.7248*** (3.36)</td>
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<tr>
<td>15-24 Years of Experience</td>
<td>2.6418 (1.55)</td>
<td>3.6660*** (4.29)</td>
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<tr>
<td>25+ Years of Experience</td>
<td>1.7307 (0.99)</td>
<td>4.1164*** (4.06)</td>
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<tr>
<td>Content In-service Hours, $x_0$</td>
<td>-0.0073 (1.16)</td>
<td>0.0078 (1.53)</td>
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<td>Content In-service Hours, $x_3$</td>
<td>-0.0041 (0.71)</td>
<td>-0.0001 (0.02)</td>
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<td>Content In-service Hours, $x_{10}$</td>
<td>-0.0012 (0.20)</td>
<td>-0.0011 (0.22)</td>
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<tr>
<td>Content In-service Hours, $x_{0}$</td>
<td>-0.0014** (1.17)</td>
<td>0.0095* (1.78)</td>
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<tr>
<td>Content In-service Hours, $x_3$</td>
<td>0.0111** (2.09)</td>
<td>0.0056 (1.31)</td>
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<tr>
<td>Content In-service Hours, $x_{10}$</td>
<td>-0.0037 (0.69)</td>
<td>0.0054 (0.98)</td>
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<td>Content In-service Hours, $x_{0}$</td>
<td>-0.0062 (0.72)</td>
<td>-0.0070 (1.01)</td>
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<td>Content In-service Hours, $x_3$</td>
<td>-0.0107** (2.06)</td>
<td>0.0161*** (3.16)</td>
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<td>Content In-service Hours, $x_{10}$</td>
<td>-0.0143** (2.29)</td>
<td>0.0028 (0.45)</td>
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<td>Content In-service Hours, $x_{3}$</td>
<td>0.0005 (0.06)</td>
<td>-0.0021 (0.47)</td>
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<td>Content In-service Hours_3</td>
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<tr>
<td>---------------------------</td>
<td>---------</td>
<td>--------</td>
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<tr>
<td>× 10+ Yrs. Experience</td>
<td>(0.64)</td>
<td>(1.29)</td>
</tr>
<tr>
<td>Content In-service Hours_4</td>
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<tr>
<td>× 3-9 Yrs. Experience</td>
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<td>(1.39)</td>
</tr>
<tr>
<td>Content In-service Hours_4</td>
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<td>× 10+ Yrs. Experience</td>
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<td>(0.73)</td>
</tr>
<tr>
<td>Content In-service Hours_5</td>
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<td>× 3-9 Yrs. Experience</td>
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<td>(0.74)</td>
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<tr>
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</tr>
<tr>
<td>× 10+ Yrs. Experience</td>
<td>(0.90)</td>
<td>(2.01)</td>
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<tr>
<td>Other In-service Hours_1</td>
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</tr>
<tr>
<td>× 0-2 Yrs. Experience</td>
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<td>(0.18)</td>
</tr>
<tr>
<td>Other In-service Hours_1</td>
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<td>0.0012</td>
</tr>
<tr>
<td>× 3-9 Yrs. Experience</td>
<td>(1.74)</td>
<td>(0.32)</td>
</tr>
<tr>
<td>Other In-service Hours_1</td>
<td>-0.0040</td>
<td>-0.0010</td>
</tr>
<tr>
<td>× 10+ Yrs. Experience</td>
<td>(0.97)</td>
<td>(0.32)</td>
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<tr>
<td>Other In-service Hours_1,1</td>
<td>0.0057</td>
<td>0.0034</td>
</tr>
<tr>
<td>× 0-2 Yrs. Experience</td>
<td>(1.08)</td>
<td>(1.12)</td>
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<tr>
<td>Other In-service Hours_1,1</td>
<td>-0.0028</td>
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<tr>
<td>× 3-9 Yrs. Experience</td>
<td>(0.69)</td>
<td>(1.56)</td>
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<td>Other In-service Hours_1,1</td>
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<td>× 10+ Yrs. Experience</td>
<td>(0.16)</td>
<td>(0.54)</td>
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<td>-0.0010</td>
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<td>(0.24)</td>
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<tr>
<td>Other In-service Hours_2</td>
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<tr>
<td>× 3-9 Yrs. Experience</td>
<td>(0.50)</td>
<td>(0.12)</td>
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<tr>
<td>Other In-service Hours_2</td>
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<td>-0.0125***</td>
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<td>(2.21)</td>
<td>(3.73)</td>
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<tr>
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<td>-0.0005</td>
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<td>× 3-9 Yrs. Experience</td>
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<td>(0.18)</td>
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<tr>
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<td>-0.0013</td>
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<td>× 10+ Yrs. Experience</td>
<td>(1.22)</td>
<td>(0.41)</td>
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<tr>
<td>Other In-service Hours_4</td>
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<td>0.0054*</td>
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<td>× 3-9 Yrs. Experience</td>
<td>(0.27)</td>
<td>(1.78)</td>
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<td>Other In-service Hours_4</td>
<td>0.0003</td>
<td>0.0088***</td>
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<td>× 10+ Yrs. Experience</td>
<td>(0.05)</td>
<td>(2.91)</td>
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<tr>
<td>Other In-service Hours_5</td>
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<td>-0.0024</td>
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<td>× 3-9 Yrs. Experience</td>
<td>(1.99)</td>
<td>(0.66)</td>
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<tr>
<td>Other In-service Hours_5</td>
<td>-0.0066</td>
<td>0.0098***</td>
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<td>× 10+ Yrs. Experience</td>
<td>(1.08)</td>
<td>(2.79)</td>
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<tr>
<td>Advanced Degree</td>
<td>0.2477</td>
<td>1.4208***</td>
</tr>
<tr>
<td>× 0-2 Yrs. Experience</td>
<td>(0.26)</td>
<td>(2.67)</td>
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<td>Advanced Degree</td>
<td>Advanced Degree</td>
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<tr>
<td>-------------------------------</td>
<td>-----------------</td>
<td>-----------------</td>
</tr>
<tr>
<td></td>
<td>-0.6382</td>
<td>-0.0236</td>
</tr>
<tr>
<td>$× 3\text{-}9\text{ Yrs. Experience}$</td>
<td>(0.87)</td>
<td>(0.05)</td>
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<td>-0.4495</td>
<td>1.0034**</td>
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<tr>
<td>$× 10\text{+ Yrs. Experience}$</td>
<td>(0.69)</td>
<td>(2.41)</td>
</tr>
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</table>

| Number of Clusters               | 216,893         | 295,824         | 223,631         | 240,317         | 192,595         | 161,880         |
| Number of Observations          | 439,766         | 682,705         | 501,284         | 487,465         | 435,908         | 325,630         |

Models include the following time varying student/class/school characteristics: number of schools attended by the student in the current year, “structural” move by student, “non-structural move” by student, indicator of a student repeating a grade, class size, fraction of classroom peers who are female, fraction of classroom peers who are black, average age (in months) of classroom peers, fraction of classroom peers who changed schools, fraction of classroom peers who made a “structural move,” indicator for a new school, indicator for a new principal at a school, principal’s years of administrative experience. All models also include grade-by-year and repeater-by-grade dummies. Absolute values of bootstrapped t-statistics, based on 50 repetitions, appear in parentheses. * indicates statistical significance at the .10 level and ** indicates significance at the .05 level and *** indicates significance at the .01 level in a two-tailed test.
Table 7

<table>
<thead>
<tr>
<th></th>
<th>Math</th>
<th>Reading</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Elementary (Grades 3-5)</td>
<td>Middle (Grades 6-8)</td>
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<tr>
<td>Gen. Educ. Theory Credits</td>
<td>-0.0869 (0.08)</td>
<td>-0.8196 (0.77)</td>
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<td>Pedagogical - Instructional Credits</td>
<td>-1.1218 (1.13)</td>
<td>0.6119 (0.61)</td>
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<td>Pedagogical - Management Credits</td>
<td>2.4152 (0.21)</td>
<td>2.8635 (0.37)</td>
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<td>Pedagogical - Content Credits</td>
<td>1.0890 (1.29)</td>
<td>0.9704 (1.39)</td>
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<tr>
<td>Professional Development Credits</td>
<td>0.7413 (0.84)</td>
<td>-0.0286 (0.03)</td>
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<td>Classroom Observation Credits</td>
<td>-3.8180 (0.22)</td>
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<tr>
<td>Classroom Practice Credits</td>
<td>-0.4917 (0.49)</td>
<td>1.0370 (1.00)</td>
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<tr>
<td>Subject Content Credits</td>
<td>-1.7832 (0.64)</td>
<td>0.0184 (0.01)</td>
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<tr>
<td>Mathematics Credits</td>
<td>-0.3690** (2.32)</td>
<td>-0.0277 (0.11)</td>
</tr>
<tr>
<td>Statistics Credits</td>
<td>-0.7864 (0.34)</td>
<td>5.0413** (2.52)</td>
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<tr>
<td>English Literature Credits</td>
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<td></td>
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<tr>
<td>Math Education Credits</td>
<td>1.9152 (1.51)</td>
<td>-0.2972 (0.38)</td>
</tr>
<tr>
<td>Language Arts Educ. Credits</td>
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</tr>
<tr>
<td>SAT Total Score</td>
<td>-0.0012 (0.30)</td>
<td>-0.0020 (0.40)</td>
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<tr>
<td>R-squared</td>
<td>0.0140</td>
<td>0.0176</td>
</tr>
<tr>
<td>Number of Observations</td>
<td>1,125</td>
<td>616</td>
</tr>
</tbody>
</table>

R-squared 0.0140 0.0176 0.0281 0.0100 0.0215 0.0455
Number of Observations 1,125 616 325 1,307 419 368
The dependent variable is the (within-school) teacher fixed effect estimated from a model of student achievement using all Florida public school students in the relevant grades. Observations are weighted by the square root of the number of students per teacher. Absolute values of t-statistics appear in parentheses. * indicates statistical significance at the .10 level and ** indicates significance at the .05 level and *** indicates significance at the .01 level in a two-tailed test.