Essays on the Teacher Labor Market

by

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Abstract

This thesis presents three empirical essays on the teacher labor market. Chapter one exploits the exogenous variation in teacher pay arising from state-mandated pay increases to identify the causal effect of teacher pay on teacher qualifications. Results suggest that, while state-mandated increases do raise teacher pay, they lead in the short run to a reduction in teacher quality as measured by the selectivity of a teacher's undergraduate institution and the probability that math and science teachers majored in these fields. This result appears to be due to the fact that, in the wake of an across-the-board pay hike, newly hired teachers are of lower quality than incumbents.

Chapter two estimates the impact of state-mandated pay raises on the likelihood of a teacher exiting the state public school system. To explore the effects on the quality of the teacher workforce, the analysis also investigates whether the responsiveness of the exit decision to the pay raise varies with the subject matter expertise of the teacher, as measured by the type of degree held. The findings suggest that general pay raises tend to increase the retention of experienced teachers, particularly at the secondary school level. However, the strength of the retention effect varies with the subject matter expertise of the teacher and the union status of the district. In nonunion districts, the retention effects are stronger for experienced teachers with academic degrees than for those with education degrees. The opposite relationship holds in union districts.

Chapter three uses a conditional logit model to investigate the determinants of a new teacher's choice of state in which to begin teaching, as a function of salary, student characteristics, and geographic proximity to the college state. The findings indicate that geographic proximity and proportion minority enrollment dominate the location decision. The overall salary level does not appear to influence the probability of a teacher locating in a state.

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Contents

Chapter One

The Short-Run Effect of State-Mandated Salary Increases on Teacher Qualifications: Do States Get What They Pay For?

1.1. Introduction

As calls for improvements to the educational system resound in public policy discussions, a substantial body of research has been devoted to investigating the effects of teacher salary on student achievement. Comprehensive reviews of the literature led Hanushek (1986, 1997) to conclude that there is no "consistent relationship" between teacher salaries or other school resources and student performance. As noted by Hanushek, Kain, and Rivkin (1999), the question of how to interpret this lack of evidence of a systematic relationship remains open. Some researchers argue that data limitations and methodological difficulties plague many studies and thus preclude an accurate estimation of the underlying causal relationship (see, for example, Loeb and Page 2000). Other researchers suggest that the weak relationship emerging from the existing body of literature accurately reflects a weak—perhaps non-existent—relationship between teacher salaries and student outcomes (see, for example, Ballou 1996).

Conventional wisdom suggests that any effect of teacher salaries on student achievement occurs via the effect of salaries on teacher quality. In light of the difficulties associated with estimating the direct relationship between teacher salaries and student outcomes, this paper examines the causal relationship between teacher salaries and teacher qualifications. Determining the effect of teacher salaries on measurable teacher qualifications constitutes a critical step towards achieving the broader goal of estimating

the relationship between teacher salaries and student outcomes. To the extent that teacher qualifications correlate highly with teacher quality, estimates of the effect of salary on qualifications clarify a key mechanism by which salaries potentially affect student outcomes.

The direct relationship between teacher salaries and teacher qualifications has a strong bearing on current policies. Improving qualifications has often been the focus of public school policies; the federal "No Child Left Behind" Act, for example, requires "highly qualified" teachers in public school classrooms. Policy proposals often suggest manipulating salaries as a means of attracting and retaining qualified teachers. However, the efficacy of such proposals depends on the precise relationship between teacher salaries and qualifications.

Recent papers investigating the relationship between teacher salaries and teacher qualifications or teacher quality include work by Figlio (2002) and Hanushek, Kain, and Rivkin (1999). Figlio finds that, for nonunion school districts, there is a positive relationship between a district unilaterally raising its teaching salaries relative to other districts in the same county and the probability of that district hiring "well-qualified" teachers. Hanushek, Kain, and Rivkin estimate the relationship between starting teacher salaries and student achievement, arguing that student achievement itself provides an indirect measure of teacher quality. They find evidence of a positive relationship between teacher salaries and student achievement but argue that specification checks cast doubt on a causal interpretation.

The aforementioned papers improve upon earlier cross-sectional analyses by using panel data and employing fixed effects to control for time invariant factors, but

changes over time in unobserved factors may still bias the estimates. In particular, teacher quality and teacher salaries are likely to be jointly determined by unobserved district-level teacher supply and demand conditions that vary over time, both across and within districts. If so, even estimates using fixed-effects models may not provide a reliable guide to the direct impact of a rise in teacher salaries on the quality of teachers hired.

The analysis in this paper illuminates the relationship between teacher salaries and teacher quality by using an instrumental variables strategy to estimate the impact of teacher salaries on teacher qualifications. Specifically, I identify the effect of the average starting salary offered by a district on the average qualifications of teachers employed in that district using the potentially exogenous variation in district salaries induced by state-mandated salary increases. The next section provides background for thinking about the effects of teacher pay raises on teacher qualifications. An explanation of the empirical strategy employed in this analysis follows. I then describe the data used and the salary mandates studied. The remaining sections present results and conclusions.

1.2. Background

The variation used in this paper to identify the effects of salaries on qualifications comes from state mandates intended to raise teacher salaries throughout the state. In the long run, such widespread pay raises for teachers will likely increase the number of college graduates choosing to teach.[1] The overall increase in pay potentially attracts both

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[1] See, for example, Dolton (1990) and Manski (1987).

low-quality and high-quality individuals. If districts hire better-qualified teachers from the enlarged applicant pool, then the qualifications of teachers may rise on average.[2]

The short run effects, however, may differ substantially from the long run effects. Previous researchers have suggested that school districts may face upward-sloping supply curves due to the limited mobility of teachers.[3] Some school districts are geographically separated; in at least some regions, salary differentials may not be large enough to cover the moving costs associated with switching districts. The psychic costs of moving may be substantial for many teachers. Boyd, Lankford, Loeb, and Wyckoff (2003) find evidence that teachers prefer to teach in areas very close to their hometowns and conclude that the geographical scope of teacher labor markets is small. Married teachers that are secondary earners in their household are further limited in their mobility, as they are in part constrained by their spouses' jobs. The possibility of an upward-sloping labor supply curve seems particularly plausible when viewing labor supply at the state level, as these factors potentially limit movement across states even more than movement across districts.[4] Such an upward-sloping supply curve implies that raising salaries may lead to higher employment. The effect of higher salaries on qualifications will then depend, in part, on the qualifications of newly hired teachers relative to the qualifications of existing teachers.

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[2] Ballou (1996) finds evidence that districts do not necessarily hire the best-qualified applicants. Ballou and Podgursky (1995) argue that perverse effects of across-the-board salary increases on retention rates and vacancy rates imply that the quality of the teacher workforce will not improve much—and may even decline—following such general pay raises.

[3] See Merrifield (1999) for a review of the literature.

[4] The source of variation in salaries used in this analysis occurs at the state level. As reported below, I find evidence of upward-sloping short-run supply curves.

There is some reason to believe that teachers hired immediately following the salary raises may be less qualified than incumbent teachers. As discussed below, the data used in this analysis shows some evidence of a secular decline in teacher qualifications, with teachers who graduated more recently being less qualified than teachers from earlier cohorts. Consequently, hiring more recent college graduates in response to a salary increase may decrease overall teacher qualifications in the short run.[5]

Pay raises may also affect qualifications via the effects on the retention of existing teachers. While previous work indicates that raising salaries tends to increase retention rates overall, Murnane and Olsen (1990) find evidence that this effect is weaker for "the most academically able" teachers.[6] If less-qualified teachers are retained at greater rates than better-qualified teachers are, this retention effect may actually contribute to an overall decline in teacher qualifications in the short run.

The California Class Size Reduction program demonstrates the possibility of declining teacher qualifications in the short run. Encouraged in part by evidence of positive achievement effects of smaller class sizes from the Tennessee STAR program, the California legislature devoted billions of dollars to reduce class sizes in kindergarten through the third grade. The employment of teachers in the early grades increased dramatically as districts staffed the smaller classes. Evaluations of the California

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[5] More generally, the effect depends on the characteristics and qualifications of the marginal individual induced by the pay raise to enter (or return to) teaching. Consider, for example, women who may enter teaching from outside of the labor force. These women may be well-qualified individuals who are highly productive in home production. Alternatively, they may be less-qualified individuals with relatively low earnings potential. In future work I will address this empirical question of the qualifications of the marginal entrant into teaching.

[6] Other studies investigating the relationship between salaries and teacher attrition include Theobald and Gritz (1996), Murnane et al. (1991), Murnane and Olsen (1989), and Baugh and Stone (1982).

program indicate that the program has led in the short run to a substantial decline in the average qualifications of teachers.[7] While the potential positive employment effects of state salary mandates are not expected to be nearly as sharp as the employment effects associated with the class size reduction program, the California program serves as an example of the negative effects on qualifications that may result from policies that lead to rapid hiring.

This analysis uses data from the two most recent rounds of the Schools and Staffing Survey to provide evidence on the short-run effects of teacher salary increases on average teacher qualifications. In future work, I intend to use different data sources to examine the long-run effects of pay raises on qualifications.

1.3. Empirical Strategy

This analysis seeks to identify the relationship between the teacher salary offered by a district *d* in state *s* at time *t* and the average qualifications of teachers working in district *d* in state *s* at time *t*. A simple form of this key relationship may be expressed as follows:

$$Q_{dst} = X_{dst}'\beta_1 + \beta_2 Ln(W)_{dst} + \delta_S + \gamma_t + \varepsilon_{dst} \quad (1)$$

where Q_{dst} is an average qualification, X_{dst} is a vector of district characteristics affecting the supply of and demand for teachers, $Ln(W)_{dst}$, is the log of the teacher salary offered by the district, β_1 and β_2 are parameters to be estimated, δ_S are state fixed effects, γ_t are

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[7] See, for example, Bohrnstedt and Stecher (2002).

year dummies, and ε is a stochastic error term. When properly identified, the parameter β_2 may be interpreted as the causal effect of teacher salary on the average qualifications of teachers employed in a given district.

Estimating this equation using the common regression method of Ordinary Least Squares (OLS) may lead to a biased estimate of the parameter of interest, β_2. The problem stems from the likely possibility that a district's salary and the qualifications of a district's teachers are simultaneously determined. That is, in addition to changes in salary causing changes in teacher qualifications, existing teacher qualifications may influence the salaries offered by districts. If better-qualified teachers self-select into a district for non-salary reasons, over time these better-qualified teachers may demand higher wages and ultimately drive up salaries. In this scenario, OLS estimation would indicate a positive relationship between teacher salaries and teacher qualifications—but this relationship would not be causal. The positive relationship results from a more mechanical finding that districts pay more for better-qualified teachers.

Compensating differentials may also cloud the interpretation of β_2. Districts may increase their salaries as a way of compensating teachers for working conditions generally deemed unfavorable. If the regression equation does not control fully for these working conditions then the effect of the unfavorable conditions will load on to the salary variable, confounding the estimated relationship between salaries and qualifications.

I seek to identify β_2 using potentially exogenous variation in district salaries. In particular, I instrument for district salaries using state-mandated salary increases. Assuming district compliance, such mandates are likely highly correlated with increases in district salaries. Since state legislatures rather than district school boards set the

mandates, the resulting salary increases are not necessarily determined by the qualifications of teachers in particular districts. Districts under state salary mandates arguably increase salaries not because of changing teacher qualifications or unobservable reasons that may be related to the qualifications of its existing teachers, but rather because the state orders the salary increase.

To implement this approach, I employ a two-stage least squares (2SLS) estimation strategy. The salary mandates studied occurred between 1993 and 1999. I construct the instrument as a time-varying dummy variable: in 1993, the variable equals zero for all districts; in 1999, the variable equals one if the district is located in a state that mandates a teacher salary increase during the period studied and zero otherwise. Since the mandate varies at the state level, I use a fixed effects specification to capture time-invariant characteristics of the states. This strategy attempts to identify the average causal effect of salary increases by essentially comparing the average qualifications of teachers in districts "forced" to increase their teacher salaries to the average qualifications of teachers in districts not forced to increase their teacher salaries. I estimate the following 2SLS model:

$$Ln(W)_{dst} = X_{dst}'\alpha_1 + \alpha_2 Law_{st} + \delta_s + \gamma_t + \nu_{dst} \quad (2)$$

$$Q_{dst} = X_{dst}'\beta_1 + \beta_2 Ln(W)_{dst} + \delta_s + \gamma_t + \varepsilon_{dst} \quad (3)$$

where Law_{st} is the salary mandate dummy variable used in the second stage as an instrument for district salaries. Teacher qualifications analyzed include three measures that previous analyses suggests may be positively related to student achievement: selectivity of the undergraduate institution attended by the teacher, the average SAT

score of the undergraduate institution attended, and an indicator of whether math and science teachers majored in math or science. [8]

1.4. Data and Descriptive Statistics

The school district data used in this study come primarily from the Schools and Staffing Survey (SASS). Designed to be representative at the state as well as the national level for public schools, the SASS obtains detailed information from school districts, school administrators, and teachers regarding school characteristics and policies. Data on salaries, enrollment, fraction minority students and other district characteristics are from the district record. The teacher component of the SASS features a variety of variables relating to teacher qualifications including major for each degree and undergraduate institution attended. I link the undergraduate institution information to data from college guides and surveys on the selectivity and average SAT score of the undergraduate institution.[9] For each of the qualifications analyzed, I obtain the average qualification of a given district by aggregating individual data from the teacher record to the district level using sampling weights.

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[8] Ehrenberg and Brewer (1994) find a positive relationship between student test scores and the selectivity of a teacher's undergraduate institution. Ferguson (1991) and Ehrenberg and Brewer(1995) find a positive relationship between student test scores and teacher verbal aptitude scores. Goldhaber and Brewer (1996) find a positive relationship between student math/science test scores and teachers having a major in math/science; they do not find a statistically significant relationship between test scores and teachers' majors for English or history. In this analysis, a math teacher is considered to have a major in math if she attained a bachelor's, master's, advanced graduate, educational specialist, doctorate, or professional degree with a major in mathematics, statistics, physics, or engineering. A science teacher is considered to have a major in science if she attained a bachelor's, master's, advanced graduate, educational specialist, doctorate, or professional degree with a major in biology/life science, chemistry, geology/earth science, another natural science, physics, or engineering.

[9] The selectivity data come from the 1990 Lovejoy's guide. The reported selectivity ranking ranges from 1 to 5, with 5 denoting the most selective institution. For readability, I rescale the ranking so that the index ranges from 20 to 100, with 100 denoting the most selective institution. The average SAT data come from a 1983 survey conducted by the Higher Education Research Institute.

Data regarding state-mandated salary increases derives primarily from the "Legislative Update" feature of *Education Week*. These updates include summaries of education-related bills enacted by state legislatures. I searched the updates and compiled bills related to teacher pay for the years 1993 through 1999. In an effort to improve the accuracy of the data, I read state education statutes to ascertain additional details regarding the salary mandates. Finally, I supplemented the salary information with reports produced by the Southern Regional Education Board.

To assess the impact of changes in teacher salaries, I utilize repeated observations of the districts. My main sample consists of districts sampled in both the 1993-94 and 1999-2000 rounds of the SASS.[10] I run the main analysis on 2,214 districts with matching teacher records. Due to the sampling procedure using in the SASS, large school districts are overrepresented in my sample. The overrepresentation of large school districts, which employ larger numbers of teachers, means that the estimates are likely to capture the effect of salary increases on the qualification of the average teacher rather than the (unweighted) average district.

1.4.1. Description of salary mandates

As summarized in Table 1.1, nineteen states mandated salary increases between 1993 and 1999. The legislative reports indicate that these mandates were generally funded. States differed in the number of increases passed during the time period studied as well as in the nature of the increases. Most states implemented more than one increase

[10] There are four rounds of the SASS: 1987-88, 1990-91, 1993-94, and 1999-2000. The 1990-91 round does not contain data on the teacher's undergraduate institution and thus cannot be use to obtain the teacher qualifications analyzed here. I use data from the 1987-88 round to perform falsification checks.

during the period, but did not legislate pay raises in every year. In the majority of cases, mandates took the form of across-the-board percentage increases; Georgia, for example, funded several 6 percent pay raises for teachers. In some instances, the pay raises depended on the education and experience level of the teacher or on the wealth of the district. Arkansas, for example, increased minimum salaries for various education-experience levels, while one of the mandates passed in Louisiana provided raises ranging from $750 to $1,200 depending on the relative wealth of the school district.[11]

Reasons for the mandates varied as well. Some legislatures described the mandates as cost-of-living increases. In other cases, such as Georgia, the governor urged the mandates as part of a drive to align state salaries with the national average salary. One of the mandates passed in Tennessee occurred in response to a court order to address funding inequities and equalize teacher pay across large and small districts. The political climate likely played a role as well, perhaps explaining why in New Mexico, for example, a proposal to increase teacher salaries for the 1997-98 school year failed before the passage of a nine percent pay raise for the 1998-99 school year. Given the varied factors influencing the passing of the mandates, the assumption that, overall, these mandates were not systematically related to underlying state trends in teacher qualifications seems plausible. Falsification tests of this identifying assumption are implemented below.

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[11] The present analysis investigates the average effect of all salary mandates by using a single dummy that turns on in 1999 if the district in which the state is located passed at least one increase between 1993 and 1999. The next phase of my research will further parameterize the law dummy to explore the potentially different impacts of the various types of mandates and to exploit differences in the frequency and timing of the pay raises.

1.4.2. Descriptive statistics

Table 1.2 presents a comparison of summary statistics for districts in states with and without salary mandates. The two groups of districts are broadly similar on most characteristics. The largest difference occurs with unionization; districts in states without mandates have higher rates of unionization than districts in states with mandates. These states with increases were located primarily in the South, which tend to be nonunion. In the present analysis, I include dummies for the unionization status of the districts; in the next stage of this work, I will attempt to control for differences in unionization more fully by interacting the unionization status with the instrument and exploring whether the effects of salary vary with unionization.

1.5. Effects of State Mandates on District Salaries

The first-stage estimates presented in Table 1.3 indicate that state-mandated salary increases did, in fact, lead to increases in district salaries. On average, districts located in states that mandated pay raises for teachers increased their salaries by about 7 percent relative to districts located in states without such salary mandates.[12] Columns 2 through 4 present estimates of the average effects of a state mandate on the log salaries offered at three education-experience levels: bachelor's degree with zero years of teaching experience, master's degree with zero years of experience, and master's degree with twenty years of experience, respectively. As the estimates are similar across education-experience levels, the remainder of the analysis will use the average of the

[12] All salaries are converted to 1999 dollars using the CPI-U.

salaries offered to beginning teachers with a bachelor's degree or a master's degree as the district salary.

1.5.1. Effect of state mandates on state funding

As a check of whether these mandates were funded as stated in the legislative reports, I estimate the relationship between the law dummy and state funds given to districts for elementary and secondary education. I calculate the funding variable, "transfer," as the sum of state governmental expenditures to independent school districts and to general-purpose governments for elementary and secondary education.[13] The results reported in Table 1.4 indicate that state transfers for elementary and secondary education increased in states with salary mandates relative to states without mandates, as would be expected if these mandates were funded. On average, the salary mandates increased the level of transfers by about 13 percent and the level of transfers per pupil by about 14 percent. The sizes of these estimates exceed the average impact of the mandates on salaries reported in Table 1.3. As reported below, I find evidence that the number of teachers employed increases following the pay raises. Districts may have used the state funds to hire more teachers in addition to paying existing teachers more.

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[13] State intergovernmental expenditures to general purpose governments are included to capture funding to dependent school districts, which are classified as agencies of another government, such as a county, municipality, or township. These regressions exclude Hawaii, which has only one, statewide school district and thus has no intergovernmental transfers to districts. The expenditures are measured in 1999 dollars.

1.5.2. Falsification test

To be valid instruments for district salaries, the state salary mandates must be uncorrelated with confounding factors affecting teacher salary levels. One such confounding factor is preexisting trends. To check for preexisting trends, I regress log district teacher salaries on the law dummy for a period prior to the 1993 to 1999 period studied. If, for example, the apparent increase in salaries offered by districts in states with mandates between 1993 and 1999 resulted primarily from mean reversion, then the estimate of the law dummy would be negative in the pre-period. Alternatively, a positive coefficient would suggest that districts in states with and without mandates had been raising salaries even prior to the law adoptions, in which case the laws probably did not cause salaries to rise but rather codified current practice.

Table 1.5 presents the results of this falsification test. In order to be able to compare salary changes over the various time periods, I restrict the sample to districts repeated in all four rounds of the SASS.[14] As with the sample used in the main analysis, states that mandated a salary increase between 1993 and 1999 (i.e., during the treatment period) saw significant relative increases in teacher salaries between 1993 and 1999. The same relationship does not hold for earlier periods, however. Salaries paid by a district between 1987 and 1990 or during the longer time period of 1987 to 1993 are unrelated to whether a state subsequently mandated an increase between 1993 and 1999. The coefficients on the law dummy for the pre-periods are negative, suggesting a slight

[14] Union status and fraction of students approved for free or reduced price lunch are not available in the 1987-88 and 1990-91 rounds of the SASS.

degree of mean reversion, but the coefficients are small in magnitude and are not statistically significant.

1.6. Effects of Salary on Employment

Instrumental variable estimates reported in Table 1.6 suggest that teacher pay raises increase the number of teachers hired. I find that a 10 percent salary increase leads, on average, to a 5.4 percent increase in the number of full-time equivalent teachers employed. The increase in the number of teachers employed translates into a reduction in the student-teacher ratio. A 10 percent salary increase is associated with an average decrease of 0.8 students per full-time equivalent teacher in the district. Comparing the IV estimates reported in columns (2) and (4) to the OLS estimates reported in columns (1) and (3) suggests a downward bias in OLS estimates of the effect of teacher salaries on the number of teachers employed. The positive employment effects are consistent with the possibility raised earlier that short-run labor supply curves are upward-sloping due to limits on teacher mobility.[15] At the same time, districts generally have a fixed number of teacher slots to fill. Hence, even a highly elastic supply response to a salary increase will lead to a limited quantity of new hiring.

Table 1.7 presents estimates of the effect of teacher salaries on the overall experience of teachers and on their tenure with a particular school. The overall effect on experience is negative, as would be expected if the salary raises result in an influx of new teachers into the profession, but the estimate is imprecisely estimated and not

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[15] Papers finding evidence of districts possessing some degree of monopsony power include Merrifield (1999), Luizer and Thornton (1986), and Landon and Baird (1971).

significantly different from zero. The imprecision of the estimate may reflect offsetting effects on average experience; in addition to increasing the number of beginning teachers, pay raises may also increase the retention of experienced teachers, thereby driving up average experience levels. While the overall effect on the average experience of teachers in a district may not change, the IV estimates reported in column (4) suggests that salary hikes decrease the average number of years that a teacher has taught at her current school. This decrease in tenure with current school may result from both an increase in the number of beginning teachers and transfers of teachers across schools.

1.7. Effects of Salary on Teacher Qualifications

This section investigates the relationship between the average starting teacher salary offered by a district and the average qualifications of teachers employed in that district. Tables 1.8, 1.9, and 1.10 report estimates of the effect of teacher salaries on the selectivity of the undergraduate institution attended, the average SAT of the undergraduate institution attended, and the fraction of math and science teachers who majored in math or science, respectively. The first two columns of each table present OLS and IV estimates for the qualification averaged over all sampled teachers in the district. The next two columns present estimates for the qualification averaged over newly hired teachers in the district, defined as teachers who began teaching within three years of the survey. The last two columns present estimates averaged over incumbent teachers, who began teaching four or more years before the survey.

In all cases, the comparison of OLS and IV estimates suggests an upward bias in the OLS estimates. Whereas the IV estimates generally indicate a negative effect of

salary increases on average teacher qualifications, the OLS estimates of the relationship are consistently positive and, except for the fraction of math and science teachers who majored in their teaching field, are statistically significant. The endogeneity bias discussed in an earlier section may explain the upward bias. The positive OLS coefficients on log salary suggests that higher salaries are associated with better-qualified teachers, but do not necessarily imply a causal effect of salary on qualifications since salaries and qualifications may simultaneously influence one another. The OLS estimates may be interpreted as indicating that better-qualified teachers tend to receive higher salaries from districts, but the IV estimates suggest that—in the short run—higher salaries do not *cause* an improvement in teacher qualifications.

1.7.1. Effect on selectivity of undergraduate institution

IV estimates reported in Table 1.8 suggest that increasing teacher salaries leads to a short-run decrease in the average selectivity of the undergraduate institution attended by teachers in the district. The magnitude of the estimate is small—a 10 percent salary raise leads to a decrease of 1.2 units of the selectivity index, compared to a standard deviation of about 9 units over all teachers—but the estimate is statistically significant. This negative effect appears to be nearly twice as strong among newly hired teachers as among incumbent teachers.

A secular decline in this teacher qualification over time may contribute to this negative result. Figure 1.1 graphs the average selectivity of undergraduate institution attended for cohorts of teachers based on the year the teacher attained her bachelor's degree. The regression line shows the predicted values from a regression of selectivity

on year attained bachelor's degree. There appears to be a slight negative slope to the estimates, suggesting that average selectivity has decreased slightly over time. In the short run, the pool of prospective teachers from which districts can hire following a salary increase may come from less selective undergraduate institutions, driving down the average among newly hired teachers. If the salary raise increases the retention of better-qualified incumbent teachers, then one would not expect to see as large of a decline in qualifications among incumbent teachers.

1.7.2. Effect on average SAT of undergraduate institution

The results presented in Table 1.9 for the average SAT of the undergraduate institution attended indicate even larger differential effects for newly hired versus incumbent teachers. While the effect of salaries on this SAT measure averaged over all teachers in a district is positive but statistically insignificant, the effect averaged over newly hired teachers is negative and statistically significant. Increasing salaries by 10 percent appears in the short run to decrease the average SAT of colleges attended by newly hired teachers by 20 points, which is almost a third of a standard deviation. However, among incumbent teachers with more than four years of teaching experience, a salary increase seems to exert a positive, albeit smaller, effect, with a 10 percent salary increase leading to an average increase of about 10 points.

As with the selectivity measure, there is some evidence of a secular decline in average SAT. Figure 1.2, constructed analogously to Figure 1.1, suggests that the recent cohorts of teachers come from colleges with slightly lower average SAT scores than older cohorts of teachers. This secular decline likely affects the average qualifications of

new hires to some degree, but the decline may not be sufficient to explain the sizable difference in college quality between newly hired and incumbent teachers as measured by average SAT.

1.7.3. Effect on fraction of math and science teachers with a major in the teaching field

Raising salaries also appears to decrease the fraction of math and science teachers in a district who majored in math or science (Table 1.10). Unlike the college selectivity results, however, this effect appears to be stronger among incumbent teachers than newly hired teachers. This effect may stem from differential retention of teachers with weaker qualifications. Previous work has suggested that teachers with science degrees have more outside opportunities and are less likely than teachers with other degrees to remain in teaching (see, for example, Murnane et. al. (1991)). If the retention effect of a salary increase is largest among teachers who majored in subjects other than math and science, the negative effect on incumbent teachers will result.

1.7.4. Falsification test

Simultaneous determination of the state mandates and the average district qualifications investigated would undermine my identification strategy. If the decision of state legislatures to mandate salary increases is influenced by the qualifications of teachers in the state, then the endogeneity problem that potentially exists at the district level will persist even when variation in salaries comes from state mandates. The results reported in Table 1.11 do not show evidence of a systematic relationship between average

district qualifications prior to 1993 and the mandating of pay raises between 1993 and 1999. The estimates are small in magnitude and are not statistically significant.

1.8. Alternative Identification Strategy

The main identification strategy used in this analysis contrasts changes in teaching salaries and average qualifications between states that did and states that did not mandate salary increases. While the falsification tests alleviate concerns that the salary laws were adopted in states experiencing differential preexisting salary trends, one would ideally like to identify the effects of salary by contrasting changes across districts *within* states that mandated salary increases. One strategy for doing so involves interacting the state-level law dummy with a base-year characteristic of the district and using the interaction as the instrument for salary.

To implement such a strategy, I interact the law dummy with the base year salary of the district and estimate the following 2SLS model:

$$\text{Ln}(W)_{dst} = X_{dst}'\alpha_1 + \alpha_2 \text{Ln}(W_0)_{ds} + \alpha_3(\text{Law}*\text{Ln}(W_0))_{dst} + \lambda_{st} + \nu_{dst} \quad (4)$$

$$Q_{dst} = X_{dst}'\beta_1 + \beta_2 \text{Ln}(W_0)_{ds} + \beta_3 \text{Ln}(W)_{dst} + \lambda_{st} + \varepsilon_{dst} \quad (5)$$

where W_0 is the salary offered by the district in 1993 and the interaction of the law dummy with the base-year district salary, $Law*Ln(W_0)_{dst}$, instruments for log salary in the second stage.

With the inclusion of state-year effects, λ_{st}, the identification comes from contrasting changes across districts within in a state generated by the differential salary effects of the mandate. A main effect of the base year salary is now included, but the

main effect of the law dummy is absorbed by the state-year dummies. The first-stage results reported in Table 1.12 indicate that the interaction of the law dummy with the base-year salary of a district has a statistically significant negative effect on the average log starting salary offered by a district. Columns (1) and (2) correspond to specifications including state fixed effects and year effects and are presented for comparison. As reported in the discussion of the main analysis, the coefficient on the law dummy in column (1) indicates that districts in states that mandate pay raises increase their salaries relative to districts in states that do not mandate pay raises. The results in column (2) produced from including the interaction term and base-year salary indicate that, while district in states with mandated raises increase their salaries on average, the magnitude of the increase relates inversely to the base-year salary: the lower the salary in 1993, the larger the increase in salary between 1993 and 1999.[16] This inverse relationship persists with the inclusion of state-year effects in column (3). The estimates suggest that the mandates were "more binding"—that is, generated larger percentage increases—in lower wage districts.[17]

Table 1.13 presents the effects of teacher salary on selectivity, average SAT, and fraction of math and science teachers who majored in their teaching field averaged over all teachers in a district. The estimates become far less precise after the imposition of the

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[16] The interaction between the law dummy and the base-year salary has been demeaned to make the main effect of the law comparable between columns (1) and (2).

[17] This differential impact of state mandates on salary increases for districts within state would be particularly strong in states where the mandates took the form of increasing minimum salaries rather than raising salaries across-the-board for all teachers; all districts are subject to across-the-board increases, but minimum increases would not be binding for districts already above the legislated minimum. In future work, I will examine different types of mandates separately to exploit this potential difference in the mechanism by which salaries affect qualifications.

stringent restriction of using only within-state variation, suggesting that there is insufficient power for the within-state analysis. Furthermore, the use of the interactions may exacerbate any measurement error that is present.[18] Nonetheless, the signs of the coefficients on log salary are negative and do not refute the suggestion from the main analysis that salary increases lead to decreases in average qualifications in the short run. The negative effect on the proportion of math and science teachers with a major in their teaching subject appears to be rather robust, as the estimates remain statistically significant even in the presence of state-year dummies.

1.9. Conclusions

This paper attempts to assess the causal effect of teacher pay on teacher qualifications, as measured by the selectivity and average SAT of a teacher's undergraduate institution and the likelihood that math and science teachers majored in their teaching field. In particular, I estimate the relationship between teacher salaries offered by a district and the average qualifications possessed by teachers employed in that district. I instrument for district-level changes in salaries using state-mandated salary increases in an attempt to identify the effect of salaries from a plausibly exogenous source of variation.

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[18] Since I do not have the full universe of districts, the salaries are drawn from probability samples and are, consequently, random variables. As a result, a district with a "low draw" from the 1993 salary distribution will likely have a higher draw in 1999, indicating greater observed growth over the 1993 to 1999 period even in the absence of a salary mandate. To eliminate this concern that the observed salary growth may be due to measurement error rather than the causal effect of state mandates, in future work I will instrument for the initial salary, possibly by using a salary estimate from an alternative data source.

The results suggest that, in the short run, salary increases tend to decrease the average qualifications of teachers employed in a given district. On average, teachers hired after the salary increases graduated from lower quality undergraduate institutions than incumbent teachers. Pay raises also appear to negatively impact subject matter expertise in math and science—the fraction of math and science teachers with academic majors in those fields tends to decline after a salary increase.

Negative short-run effects do not preclude positive long-run effects of salaries on teacher qualifications. Higher teacher salaries may ultimately induce better-qualified individuals to enter into and remain in the teaching profession. Yet, potential long-run benefits notwithstanding, the negative short-run consequences of salaries on teacher qualifications raise doubts about the efficacy of using general pay raises to improve teacher quality and student achievement. District hiring practices may be partly responsible for the negative effects on average qualifications. The results of this analysis bear out the worst-case scenario proposed by Ballou and Podgursky (1995) and Ballou (1996), in which across-the-board pay raises lead to a decline in teacher quality due, in part, to the failure of districts to hire the best-qualified individuals from the pool of applicants. Inefficiencies in the hiring process may become increasingly detrimental in the presence of a secular decline in average qualifications, as the qualifications of a teacher drawn at random declines over time. I also note that the results of this analysis are in the spirit of earlier findings by Angrist and Guryan (2003), who find that state-mandated licensing exams are associated with pay raises but no commensurate increase in teacher qualifications; testing may impose barriers to entry into teaching without

improving quality. Licensing requirements appear to be another aspect of hiring practices that potentially hinder efforts to raise the quality of the teacher workforce.

Future work will clarify the causal channels affecting the link between teacher salaries and teacher qualifications. The present analysis used the average effect of state salary mandates to identify the impact of salaries on qualifications. Most of the mandates took the form of across-the-board percentage pay raises for all teachers throughout the state. Other forms of salary mandates that differentially affect districts or teachers may have different consequences. In the next phase of this research, I will explore separately the increases in minimum pay levels and other types of salary mandates.

Combining salary increases with incentives to improve hiring processes and otherwise target well-qualified individuals could potentially raise the average qualifications of teachers hired and retained. In the absence of such additional policies, however, the evidence presented here suggests that general pay raises will fail in the short run to produce the desired improvements in teacher quality.

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Table 1.1. Summary of State Salary Mandates

State	Percentage Increase [a]	Level Increase [a]	Increase in Minimum [a]	Example of a Salary Mandate
Alabama	Yes	No	Yes	8.5% pay raise for public education employees
Arkansas	No	No	Yes	Minimum salary of $23,000 for teachers with a master's degree and no experience
Delaware	Yes	No	No	3% pay raise for teachers
Georgia	Yes	No	Yes	6% pay raise for teachers
Idaho	Yes	No	No	2% pay raise for teachers and nonprofessional school employees
Iowa	No	No	Yes	Minimum salary of $23,000 for beginning teachers
Kentucky	Yes	No	No	2.3% salary increase for certified teaching and administrative staff members
Louisiana	Yes	Yes	No	Raises range from $750 to $1,200 depending on a school district's relative wealth
Mississippi	Yes	Yes	No	3% pay raise for teachers
Missouri	No	No	Yes	Minimum teacher salary of $18,000
New Mexico	Yes	No	Yes	9% pay raise for teachers
North Carolina	Yes	No	Yes	Changes to the state minimum schedule result in teacher raises ranging from 4.4% to 11%
South Carolina	Yes	No	No	4.7% pay raise for teachers
Tennessee	Yes	No	No	3% pay raise for teachers
Texas	No	Yes	Yes	$3,000 pay raise for teachers
Utah	Yes	No	No	Average pay raise of about 4% for teachers
Virginia	Yes	No	No	2.3% pay raise for public school employees
West Virginia	No	Yes	No	$756 pay raise for teachers
Wisconsin	Yes	No	No	2.1% pay raise for teachers and other school personnel

[a] Most states mandated more than one salary increase between 1993 and 1999. These columns indicate whether any of the mandates took the form of a percentage increase in salary (e.g., a 6% pay raise), a level increase in salary (e.g., raises of $756), or an increase in minimum salaries (e.g., increasing the minimum salary paid to beginning teachers to $23,000).

Table 1.2. Summary Statistics

Variable	Year	Districts in States Without Salary Mandates N=1295 Mean (Standard Deviation)	Districts in States With Salary Mandates N = 919 Mean (Standard Deviation)
Average Starting Salary [a]	1993	27656 (4443)	24649 (2784)
	1999	27717 (4602)	26254 (2782)
Selectivity	1993	55.8 (8.64)	52.2 (9.40)
	1999	56.1 (9.01)	51.3 (9.78)
Average SAT	1993	919 (62.5)	872 (70.1)
	1999	918 (63.1)	868 (72.6)
Fraction Math/Science Teachers with Major in Teaching Field	1993	.466 (.407)	.415 (.398)
	1999	.512 (.429)	.384 (.405)
Number of Full-Time Equivalent Teachers	1993	331 (1462)	417 (820)
	1999	379 (1751)	483 (968)
Enrollment	1993	5950 (27590)	6929 (13503)
	1999	6201 (28622)	7150 (13917)
Fraction Minority Students	1993	.194 (.250)	.254 (.279)
	1999	.208 (.266)	.289 (.299)
Fraction of Students Approved for Free/Reduced Price Lunch	1993	.397 (.309)	.507 (.289)
	1999	.315 (.231)	.435 (.223)
Union—Meet and Confer [b]	1993	.102 (.303)	.098 (.297)
	1999	.070 (.255)	.089 (.284)
Union—Collective Bargaining [c]	1993	.833 (.373)	.263 (.440)
	1999	.847 (.360)	.247 (.431)

[a] Salaries are converted to 1999 dollars using the CPI-U.
[b] Union—Meet and Confer is a dummy variable that equals one if the district has a meet and confer agreement with a teacher's union and zero otherwise.
[c] Union—Collective Bargaining is a dummy variable that equals one if the district has a collective bargaining agreement with a teacher's union and zero otherwise.

Table 1.3. First-Stage Estimates with State and Year Fixed Effects

	Dependent Variable			
	Ln(Average Starting Salary)	Ln(Salary for B.A., 0 Experience)	Ln(Salary for M.A., 0 Experience)	Ln(Salary for M.A., 20 Yrs Experience)
	(1)	(2)	(3)	(4)
Law	0.070	0.069	0.072	0.058
	(0.014)	(0.013)	(0.014)	(0.011)
Ln(Enrollment)	0.021	0.019	0.023	0.034
	(0.003)	(0.003)	(0.003)	(0.005)
Fraction Minority	0.065	0.058	0.071	0.070
	(0.010)	(0.009)	(0.011)	(0.016)
Fraction Lunch	-0.032	-0.029	-0.034	-0.059
	(0.008)	(0.008)	(0.009)	(0.014)
State Unemp. Rate	-0.016	-0.014	-0.017	-0.016
	(0.005)	(0.005)	(0.005)	(0.005)
Union—Meet	0.008	0.009	0.007	0.033
	(0.011)	(0.010)	(0.012)	(0.019)
Union—Coll Bargain	-0.014	-0.013	-0.015	0.026
	(0.009)	(0.009)	(0.009)	(0.015)
Observations	4382	4382	4382	4382
R-squared	0.76	0.77	0.74	0.79

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.4. Effect of Law on State Funding

	Dependent Variable	
	Ln(Transfer)	Ln(Per Pupil Transfer)
	(1)	(2)
Law	0.133	0.140
	(0.063)	(0.063)
Ln(Enrollment)	0.001	
	(0.002)	
Fraction Minority	0.024	0.025
	(0.019)	(0.019)
Fraction Lunch	-0.049	-0.053
	(0.042)	(0.042)
State Unemployment Rate	0.079	0.104
	(0.084)	(0.084)
Union—Meet and Confer	-0.001	-0.003
	(0.006)	(0.005)
Union—Collective Bargaining	0.001	0.003
	(0.005)	(0.006)
Observations	4380	4380
R-squared	0.96	0.92

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.5. Falsification Test: Effect of Law on Salary in Pre-Period

	Dependent Variable: Ln(Salary)		
	Time Period		
	1987-1990	1987-1993	1993-1999
	(1)	(2)	(3)
Law	-0.010	-0.016	0.063
	(0.011)	(0.017)	(0.013)
Ln(Enrollment)	0.016	0.017	0.015
	(0.003)	(0.003)	(0.003)
Fraction Minority	0.040	0.042	0.045
	(0.011)	(0.012)	(0.011)
State Unemployment Rate	0.007	0.006	-0.013
	(0.003)	(0.004)	(0.006)
Observations	2178	2172	2164
R-squared	0.76	0.71	0.74

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.6. Effect of Salary on Employment

	Dependent Variable			
	Ln(FTE Teachers)		Student/Teacher Ratio	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Ln(Salary)	0.120	0.538	2.866	-8.066
	(0.068)	(0.167)	(1.000)	(2.745)
Ln(Enrollment)	0.894	0.886		
	(0.009)	(0.009)		
Fraction Minority	0.060	0.033	-0.190	0.671
	(0.023)	(0.025)	(0.345)	(0.365)
Fraction Lunch	0.055	0.068	-1.397	-1.868
	(0.020)	(0.021)	(0.348)	(0.391)
State Unemployment Rate	-0.006	-0.003	0.177	0.082
	(0.005)	(0.004)	(0.120)	(0.103)
Union—Meet and Confer	0.043	0.039	-0.021	0.171
	(0.013)	(0.014)	(0.196)	(0.262)
Union—Coll. Bargaining	0.021	0.027	1.089	1.150
	(0.017)	(0.018)	(0.291)	(0.320)
Observations	4382	4382	4382	4382
R-squared	0.98	0.98	0.32	0.28

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.7. Effect of Salary on Experience and Teacher Tenure with Current School

	Dependent Variable			
	Years Teaching Experience		Years with Current School	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Ln(Salary)	-1.168	-4.241	-1.069	-8.068
	(1.459)	(4.246)	(1.255)	(4.403)
Ln(Enrollment)	0.474	0.539	-0.086	0.060
	(0.123)	(0.156)	(0.116)	(0.145)
Fraction Minority	-0.662	-0.463	-1.662	-1.211
	(0.568)	(0.647)	(0.560)	(0.543)
Fraction Lunch	-0.238	-0.331	0.152	-0.060
	(0.525)	(0.503)	(0.485)	(0.460)
State Unemployment Rate	0.458	0.434	0.415	0.362
	(0.184)	(0.193)	(0.171)	(0.176)
Union—Meet and Confer	-0.406	-0.378	-0.067	-0.003
	(0.374)	(0.386)	(0.374)	(0.416)
Union—Coll. Bargaining	0.220	0.179	0.519	0.425
	(0.415)	(0.422)	(0.378)	(0.386)
Observations	4382	4382	4382	4382
R-squared	0.11	0.11	0.15	0.13

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.8. Effect of Salary on Selectivity of Undergraduate Institution

	Dependent Variable: Selectivity of Undergraduate Institution					
	Average over all teachers		Average over new hires		Average over incumbents	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Salary)	4.837	-12.333	5.671	-21.442	5.367	-11.499
	(1.963)	(5.159)	(3.888)	(8.303)	(1.901)	(5.787)
Ln(Enrollment)	0.004	0.360	-0.052	0.568	0.159	0.508
	(0.148)	(0.177)	(0.273)	(0.333)	(0.189)	(0.232)
Fraction Minority	-1.999	-0.870	-0.104	1.480	-2.224	-1.140
	(1.065)	(1.134)	(1.407)	(1.427)	(1.138)	(1.202)
Fraction Lunch	-1.115	-1.644	-2.681	-3.078	-1.047	-1.583
	(0.625)	(0.599)	(1.092)	(1.059)	(0.718)	(0.696)
State Unemployment Rate	-0.138	-0.263	0.089	-0.059	-0.320	-0.435
	(0.183)	(0.171)	(0.324)	(0.297)	(0.205)	(0.200)
Union—Meet and Confer	0.054	0.214	1.406	1.657	-0.542	-0.387
	(0.686)	(0.695)	(0.966)	(1.028)	(0.811)	(0.812)
Union—Coll. Bargaining	-0.146	-0.382	1.021	0.482	-0.316	-0.554
	(0.636)	(0.640)	(1.164)	(1.110)	(0.689)	(0.707)
Observations	4370	4370	3008	3008	4345	4345
R-squared	0.36	0.35	0.23	0.20	0.34	0.32

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.9. Effect of Salary on Average SAT of Undergraduate Institution

	Dependent Variable: Average SAT of Undergraduate Institution					
	Average over all teachers		Average over new hires		Average over incumbents	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Salary)	51.864	25.311	69.204	-200.667	60.468	98.702
	(16.098)	(34.723)	(31.753)	(56.451)	(14.233)	(44.146)
Ln(Enrollment)	2.599	3.148	1.221	7.275	2.931	2.139
	(1.094)	(1.284)	(1.674)	(2.507)	(1.247)	(1.529)
Fraction Minority	-35.305	-33.611	-13.624	2.239	-40.439	-42.889
	(6.760)	(7.143)	(10.946)	(11.142)	(7.086)	(7.584)
Fraction Lunch	-22.582	-23.407	-32.767	-37.286	-21.660	-20.443
	(5.120)	(4.870)	(8.401)	(7.584)	(4.777)	(4.686)
State Unemployment Rate	-1.327	-1.514	-0.564	-2.101	-1.728	-1.469
	(1.131)	(1.076)	(2.161)	(1.857)	(1.195)	(1.243)
Union—Meet and Confer	-3.196	-2.939	-5.037	-2.618	-1.580	-1.937
	(4.227)	(4.341)	(6.763)	(8.358)	(4.220)	(4.327)
Union—Coll. Bargaining	-3.797	-4.148	5.713	0.487	-4.413	-3.882
	(4.099)	(4.021)	(7.337)	(6.927)	(4.283)	(4.395)
Observations	4374	4374	2996	2996	4346	4346
R-squared	0.51	0.51	0.33	0.28	0.48	0.48

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.10. Effect of Salary on Fraction of Math and Science Teachers who Majored in Their Field

	Dependent Variable: Fraction with Major in Main Field (Math/Science)					
	Average over all teachers		Average over new hires		Average over incumbents	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Salary)	0.026	-1.079	-0.006	-0.736	0.062	-0.977
	(0.152)	(0.416)	(0.240)	(0.798)	(0.156)	(0.433)
Ln(Enrollment)	-0.013	0.010	0.010	0.022	-0.019	0.003
	(0.011)	(0.014)	(0.018)	(0.023)	(0.012)	(0.016)
Fraction Minority	0.015	0.082	0.078	0.122	0.013	0.078
	(0.048)	(0.054)	(0.086)	(0.093)	(0.050)	(0.056)
Fraction Lunch	-0.107	-0.142	0.034	0.018	-0.122	-0.155
	(0.047)	(0.052)	(0.094)	(0.092)	(0.047)	(0.053)
State Unemployment Rate	-0.008	-0.014	-0.015	-0.021	-0.001	-0.007
	(0.013)	(0.015)	(0.020)	(0.022)	(0.015)	(0.016)
Union—Meet and Confer	0.057	0.062	0.039	0.046	0.051	0.054
	(0.031)	(0.033)	(0.070)	(0.073)	(0.035)	(0.035)
Union—Coll. Bargaining	0.041	0.024	-0.023	-0.019	0.043	0.024
	(0.032)	(0.032)	(0.091)	(0.096)	(0.036)	(0.036)
Observations	3114	3114	1133	1133	2926	2926
R-squared	0.06	0.02	0.08	0.06	0.06	0.03

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.11. Falsification Test: Effect of Law on Average Qualifications in Pre-Period

	Dependent Variable		
	Selectivity	Average SAT	Major in Main
	(1)	(2)	(3)
Law	0.003	0.834	-0.029
	(0.426)	(3.564)	(0.035)
Ln(Enrollment)	-0.002	4.011	0.004
	(0.249)	(1.692)	(0.012)
Fraction Minority	-3.736	-58.735	0.024
	(1.025)	(10.095)	(0.060)
State Unemployment Rate	-0.006	-1.042	0.004
	(0.099)	(0.718)	(0.009)
Observations	2172	2172	1545
R-squared	0.43	0.55	0.07

Regressions include state fixed effects, year effects, and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.12. First Stage Estimates with Various Fixed Effects

	Dependent Variable: Ln(Salary)		
		Instruments	
	Law	Law, Law*93LnSal	Law*93LnSal
	(1)	(2)	(3)
Law	0.070	0.069	
	(0.014)	(0.012)	
Law*1993Ln(Salary)		-0.290	-0.261
		(0.039)	(0.039)
1993 Ln(Salary)		0.881	0.877
		(0.026)	(0.026)
Ln(Enrollment)	0.021	0.003	0.003
	(0.003)	(0.001)	(0.001)
Fraction Minority	0.065	0.012	0.012
	(0.010)	(0.005)	(0.005)
Fraction Lunch	-0.032	-0.006	-0.007
	(0.008)	(0.003)	(0.003)
State Unemployment Rate	-0.016	-0.016	0.000
	(0.005)	(0.005)	(0.000)
Union—Meet and Confer	0.008	0.005	0.003
	(0.011)	(0.003)	(0.003)
Union—Collective Bargaining	-0.014	0.005	0.002
	(0.009)	(0.004)	(0.004)
State Fixed Effects?	Yes	Yes	No
Year Effects?	Yes	Yes	No
State*Year Effects?	No	No	Yes
Observations	4382	4382	4382
R-squared	0.76	0.91	0.93

Regressions include metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 1.13. IV Estimates of Effect of Salary on Teacher Qualifications with State-Year Dummies

	Dependent Variable		
	Selectivity	Average SAT	Major in Teaching Field
	(1)	(2)	(3)
Ln(Salary)	-6.393	-11.855	-2.554
	(25.590)	(146.839)	(1.124)
Ln(Enrollment)	13.674	55.196	2.214
	(21.793)	(122.758)	(0.873)
Fraction Minority	-0.070	2.690	-0.011
	(0.155)	(1.178)	(0.012)
Fraction Lunch	-2.235	-35.021	0.035
	(1.113)	(6.971)	(0.050)
State Unemployment Rate	-1.068	-23.526	-0.118
	(0.694)	(5.236)	(0.048)
Union—Meet and Confer	0.128	-2.783	0.045
	(0.714)	(4.358)	(0.033)
Union—Coll. Bargaining	0.002	-3.312	0.040
	(0.677)	(4.279)	(0.033)
Observations	4370	4374	3114
R-squared	0.37	0.51	0.03

Regressions include state-year dummies and metropolitan status dummies.
Standard errors adjusted for correlation within state-year are reported in parentheses.

Figure 1.1. Average Selectivity of Undergraduate Institution by Year Attained Bachelor's Degree

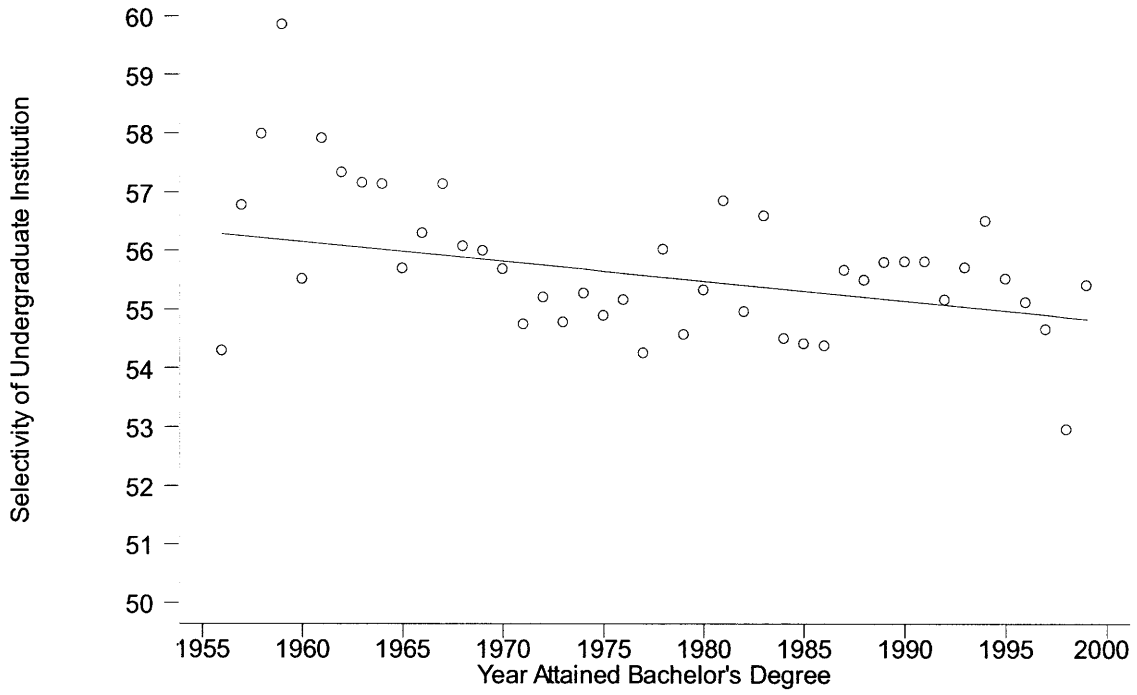
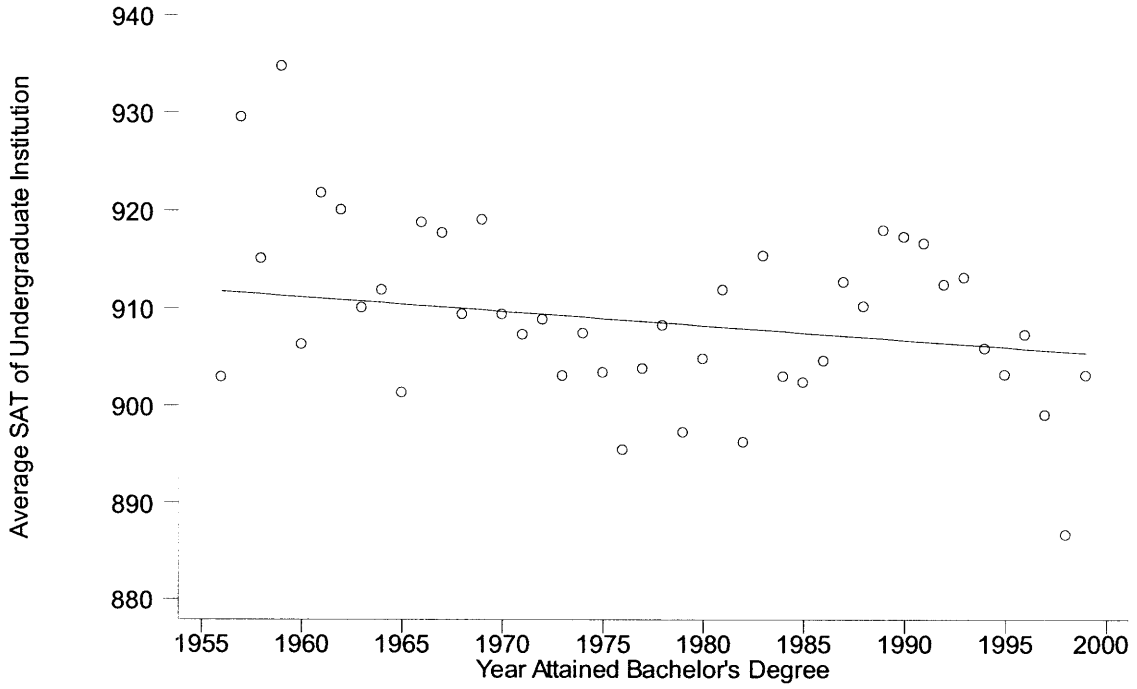


Figure 1.2. Average SAT of Undergraduate Institution by Year Attained Bachelor's Degree



Chapter Two

The Effect of State–Mandated Salary Increases on Teacher Exit

2.1. Introduction

Calls for widespread pay raises as a means of improving the quality of the teacher workforce have been made and debated for decades. As fears of overall teacher shortages wax and wane with enrollment projections, one of the persistent motivations for such calls continues to be a desire to improve student achievement by stemming the exit of highly qualified teachers. The need to recruit and retain well-qualified teachers with training in technical fields often garners special attention amid concerns over student performance in mathematics and science and potential shortages in these areas. Some stakeholders in the debate argue for higher teacher pay as a means of increasing the attractiveness of the profession, thereby inducing highly qualified individuals to remain in teaching. Others doubt the efficacy of such general pay increases and advocate instead for more targeted raises or other strategies that feature differentiated compensation.

One of the few generally agreed points in discussions about improving the quality of the teacher workforce concerns subject matter expertise. In order to teacher well, the argument goes, teachers need to know well what they teach. In mandating highly qualified teachers as part of the 2001 No Child Left Behind Act, the federal government included demonstrated subject matter competence in its definition of highly qualified: newly hired elementary school teachers must pass a test covering subject matter knowledge and teaching skills in areas of the basic elementary school curriculum, while newly hired middle or high school teachers must demonstrate "a high level of

competency" in each academic subject they teach by either passing an academic subject test or completing an academic major, coursework equivalent to an undergraduate academic major, a graduate degree, or another advanced credential. Key players in the education arena have endorsed requiring teachers to major in an academic discipline—as opposed to education—as a step towards ensuring that teachers possess adequate content knowledge. In issuing their recommendations for improving teacher preparation and quality, the American Federation of Teachers (2003) recently urged colleges and universities to require an academic major for prospective teachers. A coalition of business leaders, educators, and governors similarly endorsed an academic major requirement for teachers in their recommendations (Koppich 2001), calling for an end to the education major.

The importance of expertise in the subject area taught may be heightened at the secondary school level, where students learn advanced concepts in departmentalized courses. Furthermore, higher attrition of teachers with academic degrees disproportionately affects secondary schools due to the larger proportion of teachers with academic degrees. For example, in the sample used in this analysis, secondary teachers are evenly split across degree type—49 percent have an academic degree and 51 percent have an education degree—whereas the majority of elementary teachers (76 percent) have an education degree.

To date, little empirical evidence exists on the efficacy of general pay raises as a means of improving teacher quality. Much has been written about the general relationship between teacher compensation and teacher recruitment or retention, but few studies investigate the consequences of widespread salary increases. Figlio (2002), for

example, finds evidence suggesting that unilaterally raising salaries relative to other districts in the same county can improve the quality of teachers a nonunion school district hires, as measured by college selectivity and subject matter expertise; but, he notes that the finding does not address the question of whether pay raises spread across a large number of schools would improve teacher quality. The author of a recent report issued by the Education Commission of the States concludes from his review of the literature that existing research provides no guidance on the effects of across-the-board salary increases (Allen 2005).

This paper aims to provide evidence on how widespread salary increases affect teacher retention by exploiting variations in salary produced by state-mandated pay raises. In particular, I use data from the 1993-94 and 1999-2000 Schools and Staffing Surveys and the corresponding Teacher Follow-up Surveys to estimate the impact of state-mandated pay raises on the probability of a teacher exiting the state public school system. In addition to exploring the overall relationship between widespread pay raises and the likelihood of teacher exit, these rich datasets permit an investigation of whether the sensitivity of the exit decision to the pay raise varies with teacher qualifications such as years of teaching experience and subject matter expertise, as measured by the type of degree held.

The paper proceeds as follows. The next section provides background for thinking about the effects of pay raises on the teacher exit decision. I then describe the data and the sample studied. After explaining the empirical strategy, I present the results of analyses estimating the impact of mandated pay raises on teacher exit decisions. The final section concludes.

2.2. Background

As originally argued by Kershaw and McKean (1962), the failure of the public school salary schedule to adjust for differences in opportunity costs may explain the relatively high rates of attrition among teachers with specific subject matter expertise, technical skills, or other widely valued qualifications. The vast majority of public school districts pay teachers according to salary scales that depend solely on the teacher's educational attainment, as measured by highest degree earned or number of graduate credits accumulated, and years of experience. Whereas a second year teacher with a bachelor's degree in, say, physics, likely would command a much higher salary outside of the teaching profession than a second year teacher with a bachelor's degree in elementary education, both individuals would receive the same salary as teachers in a given public school district. The lure of higher salaries outside of teaching may eventually entice the teacher with the physics degree to leave the profession. More generally, teachers possessing subject matter expertise in lucrative fields or any other qualification rewarded more highly outside of teaching may face relatively high opportunity costs and choose to leave the profession. Such an opportunity cost argument may help explain the finding of Henke *et al* (2001) that beginning teachers who majored in engineering, mathematics, or natural science are more likely to exit the profession than teachers who majored in education.[19]

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[19] Similarly, several studies investigating variation in exit rates by subject taught find higher rates of attrition among secondary teachers who teach specific subjects than among elementary teachers, with attrition rates being highest among science teachers (see, for example, Grissmer and Kirby (1992) and Murnane *et al* (1991)).

An analysis of SAT scores for prospective teachers offers evidence consistent with the notion that teachers with degrees in math and science or other academic fields may have better outside opportunities than teachers with degrees in education. Gitomer *et al* (1999) analyze data on individuals who took the Praxis II tests used in many states as a requirement for teacher licensure. Specifically, they examine the SAT scores of all Praxis II test takers in 1994-1997 for whom SAT scores were available. For any given area of licensure, they find individuals with academic majors tend to have higher SAT scores on average than individuals who majored in education. Furthermore, when comparing scores across licensure areas, individuals with math and science majors have the highest mean scores. For example, math majors seeking licensure in math have a mean SAT score of 1152. The mean score for this group exceeds both the mean SAT score of education majors seeking math licensure (1105) as well as the mean score of social studies majors seeking licensure in social studies (1088). Since the data include all Praxis II test takers, regardless of whether they ultimately begin teaching, these findings must be viewed as at best suggestive of the relationship between major and test score among individuals who actually enter teaching. Still, to the extent that higher SAT scores indicate higher academic ability and academic ability is valued by all employers, these general patterns support the notion that teachers with academic degrees—especially those who major in math or science—have greater outside opportunities.

While raising teacher salaries potentially decreases the likelihood of exit by making teaching relatively more attractive, the effect on the quality composition of the teacher workforce depends partly on whether and how the sensitivity of the exit decision to salary varies across teacher qualifications. If a pay raise reduces the likelihood of exit

of better-qualified and less-qualified teachers by similar magnitudes, then the salary change increases retention without affecting quality; schools may experience some benefit from not having to incur the costs of hiring as many new teachers as before the raise, but the raise does not increase the proportion of highly-qualified teachers. As hypothesized by Murnane and Olsen (1990), the effect of a salary increase on the likelihood of exit may depend on the teacher's opportunity cost, but whether a higher opportunity cost increases or decreases the retention effect is unclear *a priori.* If less-qualified teachers have very low exit probabilities in the absence of salary changes—perhaps due to limited outside opportunities—a pay raise may not produce a sizable effect on the retention of these teachers; however, the increase in the relative attractiveness of teaching may lead some highly-qualified teachers who would have otherwise exited to remain in the profession. A pay raise would positively affect the quality composition in this scenario, as better-qualified teachers are more sensitive to the salary increase. Alternatively, a negative effect on the quality composition could arise if less-qualified teachers respond more strongly than better-qualified teachers. Pay raises of the magnitude implemented in practice may not be sufficient to entice highly-qualified teachers to continue forgoing lucrative outside options, but may be large enough to induce less-qualified teachers with lower opportunity costs to remain in teaching. Which scenario will arise in the presence of a widespread pay raise is an empirical question.

A relatively small number of studies have explored this question of whether and how the influence of salary on the likelihood of exiting teaching varies with teacher qualifications. The 1990 Murnane and Olsen study of the effect of salary and opportunity costs on the length of stay in teaching for North Carolina teachers examines the

interaction between salary and scores on a standardized test. They find a stronger influence of salary among lower-scoring teachers; in particular, the impact of salary on length of stay in teaching is approximately 30 percent less for teachers who scored in the top quartile than for teachers with lower scores. In contrast, Podgursky, Monroe, and Watson (2004) find in their analysis of the exit decision of Missouri teachers that the interaction between salary and a teacher's ACT score is statistically insignificant. The few papers that specifically investigate the effects of widespread pay raises on the qualifications of the teacher workforce make predictions primarily based on simulations. Ballou and Podgursky (1995) simulate the effects of a 20 percent teacher pay raise under various scenarios and conclude that an across-the-board pay raise produces at best a moderate improvement in the average SAT score of the teacher workforce and at worst a modest decline in the average. Stinebrickner (2001) simulates the impact of two different wage increases: one in which all teachers received a 25 percent pay raise and one in which the average wage increases by 25 percent, but the raise received by a given teacher depends linearly on the teacher's SAT score. His simulations suggest that both policies increase the amount of time individuals certified as teachers spend teaching by both increasing the proportion choosing to teach and decreasing the likelihood of exit among teachers. Both policies also increased the labor supply of individuals with high SAT scores relative to lower-scoring individuals, although the increase in the proportion of high scorers is higher under the policy that bases the pay raise on SAT score. Manksi (1987) similarly finds that a substantial pay raise could generate an increase in the average SAT score of the teacher workforce—a 40 percent across the board pay raise would raise the average SAT score in his data from 950 to 972—but that combining a pay

raise with a minimum SAT requirement could achieve a large increase in the average SAT of the workforce with a smaller percentage raise. Such simulations can yield useful insights, but do not provide strong evidence regarding how widespread pay raises affect teacher quality in practice.

This paper seeks to provide new empirical evidence on the how the retention effects of general pay raises impact the quality of the teacher workforce. Instead of simulating the effects of policy changes, I exploit actual state-mandated pay raises passed during the 1990s. Utilizing a national dataset that identifies the status of a teacher one year after the initial survey year enables me to estimate the average effect of salary mandates on the likelihood of exit and to explore whether the effect varies with the subject matter expertise of the teacher.

2.3. Empirical Strategy

This paper uses a difference-in-differences strategy to identify the effect of state-mandated pay raises on the exit decisions of teachers. Nineteen state legislatures mandated pay raises for public school teachers between 1993 and 1999, thereby compelling all districts within the state to raise their salaries by a specified amount or percentage. I estimate the reduced-form effect of the pay raises on the probability of a teacher exiting the state school system as the change over time in the average difference in probability of exit between teachers located in states that mandated pay raises and teachers located in states that did not pass such mandates. Stated in a regression framework, I estimate the following model.

$$Exit_{ijst} = X_{ijst}'\beta_1 + \beta_2 Law_{st} + \delta_s + \gamma_t + \varepsilon_{ijst} \quad (1)$$

The dependent variable $Exit_{ijst}$ equals one if teacher i, who taught in public school j in state s at time t, exited the state public school system at time $t+1$ and equals zero otherwise. Law_{st} is a time-varying dummy variable: in 1993, the variable equals zero for all states; in 1999, the variable equals one if the state mandated a teacher salary increase during the period studied and zero otherwise. The coefficient on Law_{st}, β_2, is the key parameter of interest; the estimate of β_2 represents the effect of the salary mandate, capturing the difference over time between exit probabilities of teachers in states that passed mandates and teachers in states that did not. X_{ijst} is a vector of teacher, school, and state characteristics that potentially affect the demand for teachers and the likelihood of teachers exiting the school system. The state fixed effects, δ_s, control for time-invariant characteristics of the states, and the year effect, γ_t, captures time shocks common across all states. Since the dependent variable is dichotomous, I estimate equation (1) using a probit model.

In addition to estimating the model over all teachers, I also compute estimates separately for subgroups of teachers to allow for differences that may lead to variation in sensitivity to the salary mandate across groups. To explore differences based on subject matter expertise, I first split teachers into two categories defined by whether they have a degree in an academic discipline or a degree in education. Many teachers in the sample have more than one undergraduate major field of study or earned a degree beyond the bachelor's degree. I place a teacher in the education degree category if all of her majors fall into the education fields, which include general education (e.g., elementary education), subject area education (e.g., science education), special education (e.g., speech/language impaired), or other education (e.g., curriculum and instruction). If any

of the major fields of study for her bachelor's, master's, doctorate, or professional degrees occurred in a discipline outside of the education fields, I define the teacher as having an academic degree. I further categorize teachers with academic degrees to distinguish between those with degrees in mathematics or science and those with academic degrees in other fields. As experience has been found to be another important dimension of teacher quality (see e.g., Rivkin, Kain, and Hanushek 2005), I also report results separately by teacher experience level. New teachers are defined as those teachers who began teaching within three years of the Schools and Staffing Survey. Experienced teachers began teaching four or more years before the survey.

2.4. Data and Descriptive Statistics

The Schools and Staffing Survey (SASS) combined with the corresponding Teacher Follow-up Survey (TFS) provide detailed data on teacher attrition and individual characteristics for a nationally representative sample of teachers. Each round of the SASS collects information from districts, schools, administrators, and teachers. Conducted in the school year following the SASS, the TFS surveys a subset of the SASS teachers to obtain follow-up information. This analysis uses two rounds of these surveys: the 1993-94 SASS with the 1994-95 TFS and the 1999-2000 SASS with the 2000-01 TFS. School and district characteristics including enrollment, fraction minority students, fraction of students approved for free or reduced-price lunch, union status, and urbanicity come from the SASS school and district records. The SASS teacher record contains a variety of teacher characteristics including major field for each degree earned, years of teaching experience, age, sex, and race/ethnicity. From the TFS record, I ascertain details

about the employment status of the teacher one year after the SASS school year. The data include not only information on whether the SASS teacher remained in the teaching profession, but also details about the nature of any teaching transitions that may have occurred and the employment status of the individual if she is no longer teaching. The TFS also reports the respondent's marital status and number of dependent children under the age of five. In addition to the SASS and TFS data, I use state unemployment rates from the Bureau of Labor Statistics as a control for prevailing labor market conditions.

The analysis sample consists of teachers included in the TFS who worked full-time in regular public elementary and secondary schools at the time of the corresponding SASS and who have complete school and district records. Forty-five teachers who reported not having a bachelor's degree were dropped from the analysis. The final sample contains 5,658 teachers.

Data on the pay raises mandated by nineteen states between 1993 and 1999 come primarily from "Legislative Update," an *Education Week* feature that includes summaries of education-related bills enacted by state legislatures. I searched the updates and compiled bills related to teacher pay for the years studied. In an effort to improve the accuracy of the data, I read state education statues to ascertain additional details regarding the salary mandates. I also supplemented the salary information with reports produced by the Southern Regional Education Board. Many of the pay raises took the form of across-the-board percentage increases; in a few instances, the pay raises

depended on the education and experience level of the teacher or on the wealth of the district.[20]

Table 2.1 reports summary statistics for a variety of teacher and district characteristics thought to affect the teacher exit decision. The table presents statistics separately for teachers in states that did and states that did not pass salary mandates. Most of the characteristics are broadly similar for the two groups of teachers. Including these characteristics as covariates in the analysis helps to control for systematic differences in observable characteristics between the groups.

Variation in exit rates by field and experience

Consistent with previous literature (e.g., Henke et al. 2001), the exit rates reported in Table 2.2 suggest that teachers in the sample with academic degrees tended to exit at higher rates than teachers with education degrees. Among teachers with academic degrees, teachers who majored in math or science exited at a higher rate than teachers who majored in other fields. The table also illustrates differences by experience level, with new teachers exiting at higher rates than experienced teachers.

Variation in salaries by field and experience

Table 2.3 presents the results of regressing log base salary on years of experience, dummies for education and degree type, and other characteristics. The estimates reflect the salient features of the typical public school salary schedule, which rewards teaching

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[20] For more details on the mandates and the varied factors influencing their passage, see chapter one of this thesis.

experience and educational attainment—salary increases by about 3 percent with each year of experience and jumps by nearly 9 percent for teachers who earn a master's degree or higher. However, the estimates also provide evidence of a wage premium for teachers with math or science degrees. In the full sample, teachers with math or science degrees earn, on average, about 2 percent more than teachers with education degrees. The premium appears to be higher among secondary school teachers—at the secondary level, teachers with math or science degrees earn 3.7 percent more than those with education degrees. There is also some evidence of a relatively small premium for teachers with academic degrees in fields other than math and science relative to teachers with education degrees, but the estimates are imprecise.

Thus, although salaries as specified in district salary schedules typically vary only with educational attainment and years of teaching experience, analyzing the actual base salaries paid to individual teachers suggests teachers with math and science degrees earn slightly higher salaries than teachers with education degrees. Previous research similarly found evidence of differences in earnings by field. Horn and McGuire (1984) compare official salary schedules to actual salaries observed among secondary school teachers and find that, while experience and educational attainment dominate as determinants of salary, teaching field and other teacher attributes also contribute to observed earnings. Using the 1990-91 SASS, Chambers (1996) similarly finds that teachers who majored in mathematics earned about 2 percent more than teachers who majored in general elementary education.

2.5. Effect of State Mandates on Teacher Salaries

The estimates presented in Table 2.4 suggest that the state-mandated pay raises had the intended effect of increasing teacher salaries.[21] On average, teachers in states that mandated salary increases earned nearly 7 percent more in base salary than teachers located in states that did not mandate pay raises (column 1). This result holds for both the full sample and the subsample of secondary school teachers.

Comparing the estimates across subgroups of teachers reveals that the average effect of the mandate on pay varied with teacher experience level and degree type. Among teachers with academic degrees as well as secondary school teachers as a whole, the passage of a mandate tended to raise salaries more for experienced teachers than for new teachers. Teachers with academic degrees tended to receive larger salary increases in response to the mandates than teachers with education degrees, particularly at the secondary level. To further explore differences by degree type, columns 10 and 11 of Table 2.4 present estimates by the field category of the academic degree—math and science or an academic degree in a field other than math or science.[22] The estimates suggest the mandate had the largest effect on the salaries of secondary teachers with math/science degrees—for this group, salaries increased by about 11 percent in mandate states relative to non-mandate states.

While the majority of the results reported in Table 2.4 are statistically significant, several of the coefficients are imprecisely estimated. Specifically, estimates for new

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[21] These results were obtained by ordinary least squares estimation of a model analogous to equation (1) with log base teacher salary as the dependent variable.

[22] The small samples sizes and limited power preclude obtaining meaningful estimates by experience level within degree field.

secondary teachers with education degrees, teachers in the full sample with math/science degrees, and secondary school teachers with academic degrees in fields other than math or science are not statistically significant. The positive signs and magnitudes of these coefficients suggest that the mandates raised salaries for these subgroups of teachers, but the standard errors are relatively large. As the interpretation of the effect of the mandate on the likelihood of teacher exit depends on the first-stage relationship between the passage of a mandate and teacher salaries, the next section will present results only for those subgroups for which a significant first stage exists.

2.6. Effect of State Mandates on Probability of Exit

For the full sample of teachers, there is some evidence that the passage of a state salary mandate increases teacher retention, but the results are not consistent across groups of teachers (Table 2.5). Among experienced teachers with education degrees, the mandates appear to reduce the likelihood of exit by 2.3 percentage points. The estimates for teachers with academic degrees are also negative, but are smaller in magnitude and statistically insignificant.

The results for new teachers are somewhat puzzling. Although the first stage estimates from Table 2.4 indicate that state salary mandates led to a nearly 10 percent increase in pay for this group, the marginally significant estimates in Table 2.5 (column 8) suggest that the mandates are associated with a relatively large rise in the probability of teachers exiting the state school system. The results may reflect endogeneity bias. If states passed mandates in response to high exit rates among new teachers, the estimates

might reflect a relatively high exit rate of these teachers in mandate states rather than a causal effect of salary mandates. I explore this possibility using a falsification test below.

The results estimated in the subsample of secondary school teachers provide stronger evidence of a retention effect of widespread pay raises. Among these teachers, the marginal effect of the mandate is to decrease the probability of exit by about 3 percentage points. As with the full sample, the responsiveness of secondary teachers to the mandates varies across subgroups. The effects appear to be stronger among experienced teachers than new teachers. Considering the effects by degree type, the results suggest a significant decrease in the likelihood of exit for teachers with academic degrees—the mandates appear to lower the probability of an experienced secondary school teacher with an academic degree exiting the state public school system by 3.4 percentage points. A similar finding emerges for secondary school teachers with academic degrees specifically in math or science (column 10), although the estimated effect is only marginally significant. The smaller magnitude of the marginal effect suggests a weaker responsiveness among secondary school teachers with education degrees than those with academic degrees, though the estimates for education-degree holders are imprecise.

Interpreting the results in terms of responsiveness to *salary* requires considering the reduced-form estimates of the effects of the mandates on the probability of exit (Table 2.5) in light of the first-stage estimates of the effects of the mandates on salary presented earlier (Table 2.4). Among experienced secondary school teachers, salaries for those with academic degrees increased by 9.5 percent on average compared to a 5.8 percent increase for those with education degrees. The finding of a stronger retention effect of

the mandates on experienced secondary school teachers holding academic degrees may reflect this larger salary increase rather than a greater sensitivity of their exit behavior to salaries. However, the elasticities implied by the first-stage and reduced-form estimates indicate that experienced secondary teachers with academic degrees are, in fact, highly sensitive to salary, with an elasticity with respect to salary of –4.4. The estimates for experienced secondary teachers with education degrees are imprecise, but it is worth noting that the implied elasticity for this group is –3.7. Table 2.4 also indicates that teachers with academic degrees specifically in math or science experienced the largest salary increase in response to the mandates. The implied elasticity of –3.0 for math/science degree-holders suggests that this group of teachers is sensitive to salary in deciding whether or not to exit, but to a lesser extent than teachers with other types of degrees.

Interaction Between Salary Mandate and Union Status

Previous work suggests that unionization affects the structure of teacher compensation. Holmes (1979), for example, finds that differences in earnings across groups of teachers tend to decline as union activity increases. Zwerling and Thomason (1995) present evidence that collective bargaining and increased union density significantly raise the salaries of teachers at the high end of the salary scale but have little or no effect on salaries at the low end, evidence which they interpret as supportive of the hypothesis that collective bargaining tends to reorient the teacher salary structure to favor the median teacher rather than the marginal teacher. Given previous evidence of union effects on teacher pay, it is plausible that the effect of salary mandates on salaries and

ultimately on teacher exit differs between union and nonunion school districts. I explore this possibility by adding to the main specifications an interaction between the law dummy and an indicator for district union status.[23] Tables 2.6 and 2.7 present the results.

The first-stage estimates presented in Table 2.6 suggest that unions tend to depress the effect of the mandates on salaries. In nonunion districts, the mandates are associated with higher salaries, raising salaries by 7.6 percent on average in the full sample of teachers and 9.3 percent on average among secondary school teachers. The negative coefficients on most of the interaction terms indicate that the increase in salary associated with mandates is smaller for teachers in union districts than for those in nonunion districts. Particularly among secondary teachers, the negative interaction effects tend to offset the main effects of the law dummy, implying that the mandates have effectively zero—or, in some instances, a slightly negative—effect on teacher salaries in union districts.

Given the first-stage results, the reduced-form estimates shown in Table 2.7 present a puzzling picture of the influence of unions on the effect of the salary mandates on teacher exit. The results for nonunion school districts, particularly at the secondary school level, suggest that the exit behavior of experienced teachers with academic degrees is sensitive to the salary mandate but the exit behavior of experienced teachers with education degrees is not. For experienced secondary school teachers with academic degrees in nonunion school districts, the mandates appear to produce a nearly 10 percent

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[23] The union dummy equals one if the district has either a collective bargaining or a meet-and-confer agreement with a teachers' union and equals zero if the district has no agreement. Among districts in the sample that have any union agreement (71% of the sample), the vast majority (91%) have a collective bargaining agreement.

increase in pay and lead to a 4 percentage point decrease in the probability of exit. Yet, in spite of a similar pay increase, the marginal effect of the mandates on experienced secondary school teachers with education degrees in nonunion districts is a statistically insignificant .006. In union districts, on the other hand, experienced teachers with education degrees exhibit the greatest reduction in the likelihood of exit. Among experienced teachers in union districts, first-stage estimates suggest larger salary gains for teachers with academic degrees than for those with education degrees, yet the retention effects of the mandate appear to be smaller for teachers with academic degrees.

While I do not have a compelling explanation for these results, I note that they are broadly consistent with previous findings by Hoxby (1996) and Figlio (2002). Hoxby finds that unions have a positive effect on teacher salaries and other school inputs yet have a negative effect on student achievement. To the extent that teacher subject matter expertise and college selectivity affect student achievement, Figlio's results may help explain this relationship—he finds a significant positive relationship between a higher salaries and the probability of hiring teachers with these qualifications for nonunion districts but not for union districts. The results presented here suggest a relationship on the retention side similar to that found by Figlio on the recruitment side. Among nonunion school districts, higher salaries appear to be associated with greater retention of teachers with subject matter expertise, as measured by possessing a degree in an academic field. This relationship weakens in union districts, where stronger retention effects occur for teachers with education degrees.

2.7. Pre-Period Falsification Test

As a check on the validity of interpreting the results as the effects of the salary mandates, I compare the main results to results for the pre-period. In particular, I estimate the model using data from the 1987 SASS with the corresponding 1988 TFS and the 1993 SASS with the corresponding 1994 TFS.[24] Mandate status is defined as before: a state is considered a mandate state if its legislature mandated at least one teacher salary increase between 1993 and 1999. A finding of a similar effect of the mandates during the 1987 to 1993 period would suggest that the retention effects found in the main analysis are spurious and did not result from the salary mandates. Finding zero effect of the mandates in the pre-period would support the notion that the mandates drove the retention effects. Since the 1987-88 round of the SASS did not collect data on district union status, the test does not include specifications with the law-union interaction.

Overall, the results reported in Table 2.8 are consistent with mandates having a real retention effect on teachers. The estimated marginal effects in the pre-period tend to be relatively small in magnitude and are not statistically significant. However, the results do not completely allay concerns that a systematic relationship between teacher exit prior to 1993 and the passage of salary mandates between 1993 and 1999 may exist for particular subgroups. The large positive marginal effect estimated in the main analysis for new teachers in the full sample with education degrees raised the possibility that mandates may have passed in states where exit rates for teachers tended to be high. Though the corresponding estimate for the pre-period is not statistically significant, the

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[24] Union status and fraction of students approved for free or reduced-price lunch are not available in the 1987-88 round of the SASS.

magnitude of the positive marginal effect suggests that the possible endogeneity cannot be ruled out conclusively. The sizable negative marginal effects found for new teachers with academic degrees also raise concern that the passage of mandates may have been related to existing teacher exit patterns. Nevertheless, the smaller, statistically insignificant estimates for teachers overall as well as for the experienced teachers for whom the main analysis indicated significant retention effects offer support for attributing the effects to the salary mandates.

Potential Composition Bias

Another threat to the validity of the results arises from the possibility of composition bias. This paper has analyzed the effect of state salary mandates on the likelihood of teachers exiting the state school system. Mandates may also impact the probability of teachers choosing to enter the state school system. If the characteristics of individuals who choose to teach in the state change in response to the salary mandate, the observed variation in exit probabilities with the passage of mandates may reflect this change in the composition of the teacher workforce rather than changes in the exit decisions of individual teachers. The third chapter of this thesis investigates the influence of salary mandates and other state characteristics on the choice of teaching state, allowing any potential effects to depend on several teacher attributes. As the results indicate that the passage of a mandate does not significantly affect the likelihood of a teacher beginning her career in a particular state, that analysis suggests that composition effects do not drive the exit probability results found here.

2.8. Conclusions

This paper provides empirical evidence on the retention effects of widespread teacher pay raises. Exploiting actual salary mandates allows an analysis of the ways in which an often-proposed strategy for increasing teacher retention affects teacher attrition in practice. I estimate the effects of the salary mandates by comparing the changes over time in exit probabilities of teachers who worked in states that mandated pay raises to the exit probabilities of teachers located in states that did not legislate pay raises.

Overall, the evidence suggests that widespread pay raises increase the retention of experienced teachers. These retention effects tend to be stronger at the secondary school level. On average across all secondary school teachers, the state salary mandates studied appear to have raised teacher pay by about 7 percent and reduced the probability of a teacher exiting the state school system by about 3 percentage points.

The strength of the retention effect of the pay raises varies with the subject matter expertise of the teacher, as measured by the type of degree held. Furthermore, the pattern of the variation across subject matter expertise differs with the union status of the teacher's district. In nonunion districts, the retention effects are stronger for experienced teachers with academic degrees than for those with education degrees. Significant effects also emerge for teachers with academic degrees specifically in the fields of mathematics or science. In union districts, however, salary mandates appear to exert the strongest impact on the exit decisions of experienced teachers with education degrees.

This variation in the responsiveness of teacher exit decisions to the salary mandates suggests that widespread pay raises are a blunt tool for retaining high quality teachers. General teacher pay raises will likely induce some teachers to remain in the

profession, but the effect will depend on the interaction of teacher qualifications and district characteristics, including experience level, subject matter expertise, and union status. Though further research is needed to understand the determinants of the union-nonunion differences and to explore the effects of pay raises on new teachers, this paper provides support for the notion that injecting the pay system with the flexibility to account for differences across teachers in sensitivity to salary could represent a critical component of effective strategies for retaining high quality teachers.

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Table 2.1. Summary Statistics for Selected Variables

Variable	Year	Teachers in States Without Salary Mandates N = 3238 Mean (Standard Deviation)	Teachers in States With Salary Mandates N = 2420 Mean (Standard Deviation)
Teacher Characteristics			
Exited the State Public School System	1993	.061 (.239)	.080 (.271)
	1999	.080 (.272)	.088 (.283)
Base Salary [a]	1993	42,625 (12,614)	32,197 (6,871)
	1999	40,653 (12,172)	34,723 (8,152)
Academic Degree in Math/Science	1993	.081 (.273)	.070 (.255)
	1999	.097 (.295)	.065 (.247)
Academic Degree in Other Fields	1993	.294 (.456)	.199 (.399)
	1999	.325 (.468)	.235 (.424)
Years of Experience	1993	16.0 (9.28)	13.2 (8.70)
	1999	14.3 (10.4)	14.7 (10.0)
District Characteristics			
Enrollment	1993	4,509 (24,425)	5,603 (13,054)
	1999	5,209 (28,345)	6,186 (14,214)
Fraction Minority Students	1993	.176 (.238)	.249 (.259)
	1999	.175 (.245)	.251 (.285)
Fraction of Students Approved for Free or Reduced-Price Lunch	1993	.361 (.307)	.494 (.279)
	1999	.297 (.230)	.415 (.205)
Union—Collective Bargaining [b]	1993	.873 (.333)	.207 (.406)
	1999	.863 (.344)	.238 (.426)
Union—Meet and Confer [c]	1993	.057 (.231)	.102 (.303)
	1999	.045 (.208)	.059 (.236)

[a] Salaries are converted to 1999 dollars using the CPI-U.

[b] Union—Collective Bargaining is a dummy variable that equals one if the district has a collective bargaining agreement with a teacher's union and zero otherwise.

[c] Union—Meet and Confer is a dummy variable that equals one if the district has a meet-and-confer agreement with a teacher's union and zero otherwise.

Table 2.2. Teacher Exit Rates

	Full Sample		Secondary Teachers	
	N	% Exit	N	% Exit
All Teachers	5658	7.6	3265	7.9
Experience Level				
New	1868	10.8	1055	11.1
Experienced	3790	6.9	2210	7.3
Degree Type				
Academic	2196	8.2	1618	9.4
Math/Science	554	9.6	501	10.3
Other Academic	1642	7.8	1117	9.0
Education	3462	7.3	1647	6.6
Degree Type by Experience Level				
Academic, New	830	11.8	585	14.4
Academic, Experienced	1366	7.1	1033	8.1
Education, New	1038	10.0	470	6.7
Education, Experienced	2424	6.9	1177	6.6

Notes: N is an unweighted count of the number of teachers in each group. The reported percentages are the weighted percentages of teachers in each group who exit the state public school system within one year of the SASS round in which they are surveyed.

Table 2.3. Salary Regressions with Dummies for Degree Type

	Dependent Variable: Ln (Salary)		
	No Degree Dummies	Academic Degree Dummy	Academic Field Dummies
	(1)	(2)	(3)
		Full Sample	
Years of Experience	0.030	0.031	0.031
	(0.002)	(0.002)	(0.002)
Master's Degree or Higher	0.088	0.087	0.087
	(0.007)	(0.007)	(0.007)
Academic Degree		0.009	
		(0.007)	
Math/Science Degree			0.022
			(0.013)
Academic but not Math/Sci Deg			0.005
			(0.008)
N		5658	
		Secondary Teachers	
Years of Experience	0.030	0.030	0.030
	(0.002)	(0.002)	(0.002)
Master's Degree or Higher	0.103	0.099	0.099
	(0.009)	(0.009)	(0.009)
Academic Degree		0.022	
		(0.009)	
Math/Science Degree			0.037
			(0.013)
Academic but not Math/Sci Deg			0.016
			(0.010)
N		3265	

Notes: The omitted degree category is education degree. Additional controls include years of experience squared; a quadratic in age; indicators for black, male, not married, and having dependent children under the age of five; log district enrollment; district fraction minority students; district fraction of students eligible for free or reduced-price lunch; an indicator for a district having a collective bargaining or meet-and-confer agreement with a union; metropolitan status dummies; state unemployment rate; state fixed effects; and a year effect. Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 2.4. First-stage Estimates: Effect of Law on Salary

	Dependent Variable: Ln (Salary)										
	All Teachers			Teachers with Academic Degrees			Teachers with Education Degrees			Teachers with Academic Degrees (by field)	
	All	New	Exper.	All	New	Exper.	All	New	Exper.	Math/Sci	Not Math/Sci
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
								Full Sample			
Law	0.066	0.073	0.063	0.077	0.054	0.097	0.069	0.095	0.062	0.079	0.057
	(0.014)	(0.021)	(0.015)	(0.025)	(0.021)	(0.030)	(0.013)	(0.028)	(0.012)	(0.050)	(0.022)
N	5658	1868	3790	2196	830	1366	3462	1038	2424	554	1642
								Secondary Teachers			
Law	0.067	0.044	0.072	0.079	0.045	0.095	0.060	0.032	0.058	0.109	0.051
	(0.021)	(0.022)	(0.022)	(0.027)	(0.026)	(0.035)	(0.025)	(0.033)	(0.025)	(0.046)	(0.033)
N	3265	1055	2210	1618	585	1033	1647	470	1177	501	1117

Notes: Additional controls include quadratics in years of experience and age; indicators for having a master's or higher degree, black, male, not married, and having dependent children under the age of five; log district enrollment; district fraction minority students; district fraction of students eligible for free or reduced-price lunch; an indicator for a district having a collective bargaining or meet-and-confer agreement with a union; metropolitan status dummies; state unemployment rate; state fixed effects; and a year effect. Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 2.5. Reduced-form Estimates: Effect of Law on Probability of Exit

	Dependent Variable: Probability of Exit										
	All Teachers			Teachers with Academic Degrees			Teachers with Education Degrees			Teachers with Academic Degrees (by field)	
	All	New	Exper.	All	New	Exper.	All	New	Exper.	Math/Sci	Not Math/Sci
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Full Sample					
Law	-0.123	0.234	-0.212	-0.134	-0.050	-0.190	-0.118	0.573	-0.233		-0.112
Probit Coef.	(0.098)	(0.176)	(0.119)	(0.130)	(0.212)	(0.167)	(0.117)	(0.266)	(0.130)		(0.138)
Law	-0.014	0.040	-0.022	-0.015	-0.007	-0.016	-0.013	0.087	-0.023		-0.012
Marginal Eff.	(0.011)	(0.033)	(0.011)	(0.013)	(0.030)	(0.013)	(0.013)	(0.048)	(0.012)		(0.013)
N	5658	1868	3790	2194	824	1364	3462	1019	2424		1640
						Secondary Teachers					
Law	-0.281	0.063	-0.384	-0.326	-0.072	-0.433	-0.169		-0.187	-0.496	
Probit Coef.	(0.114)	(0.189)	(0.112)	(0.160)	(0.265)	(0.203)	(0.135)		(0.168)	(0.335)	
Law	-0.029	0.010	-0.032	-0.036	-0.011	-0.034	-0.014		-0.014	-0.034	
Marginal Eff.	(0.010)	(0.030)	(0.007)	(0.015)	(0.041)	(0.013)	(0.010)		(0.011)	(0.020)	
N	3263	1048	2208	1617	582	1027	1646		1176	474	

Notes: Estimates are presented only for groups for whom the first-stage estimates are significant (see Table 4). Marginal effects are calculated for a discrete change in the dummy variable from 0 to 1. Additional controls include quadratics in years of experience and age; indicators for having a master's or higher degree, black, male, not married, and having dependent children under the age of five; log district enrollment; district fraction minority students; district fraction of students eligible for free or reduced-price lunch; an indicator for a district having a collective bargaining or meet-and-confer agreement with a union; metropolitan status dummies; state unemployment rate; state fixed effects; and a year effect. Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 2.6. First-stage Estimates: Effect of Law on Salary with Law X Union Interaction

	Dependent Variable: Ln (Salary)										
	All Teachers			Teachers with Academic Degrees			Teachers with Education Degrees			Teachers with Academic Degrees (by field)	
	All	New	Exper.	All	New	Exper.	All	New	Exper.	Math/Sci	Not Math/Sci
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Full Sample					
Law	0.076	0.085	0.073	0.080	0.079	0.092	0.083	0.097	0.075	0.095	0.057
	(0.015)	(0.020)	(0.016)	(0.027)	(0.022)	(0.031)	(0.014)	(0.029)	(0.013)	(0.055)	(0.024)
Union	0.036	0.012	0.051	0.051	0.057	0.077	0.034	0.003	0.048	0.104	0.003
	(0.023)	(0.019)	(0.024)	(0.045)	(0.027)	(0.055)	(0.022)	(0.026)	(0.022)	(0.047)	(0.051)
LawXUnion	-0.042	-0.046	-0.042	-0.010	-0.097	0.021	-0.056	-0.006	-0.058	-0.074	0.001
	(0.029)	(0.022)	(0.034)	(0.037)	(0.029)	(0.052)	(0.032)	(0.031)	(0.035)	(0.059)	(0.034)
N	5658	1868	3790	2196	830	1366	3462	1038	2424	554	1642
						Secondary Teachers					
Law	0.093	0.054	0.101	0.085	0.063	0.099	0.098	0.035	0.098	0.143	0.045
	(0.019)	(0.022)	(0.020)	(0.029)	(0.026)	(0.034)	(0.024)	(0.033)	(0.025)	(0.041)	(0.034)
Union	0.050	0.032	0.071	0.039	0.076	0.075	0.059	0.024	0.070	0.142	-0.020
	(0.026)	(0.029)	(0.029)	(0.047)	(0.038)	(0.056)	(0.025)	(0.027)	(0.030)	(0.059)	(0.059)
LawXUnion	-0.098	-0.041	-0.110	-0.022	-0.076	-0.016	-0.147	-0.015	-0.163	-0.131	0.027
	(0.034)	(0.028)	(0.042)	(0.041)	(0.035)	(0.051)	(0.049)	(0.031)	(0.063)	(0.044)	(0.052)
N	3265	1055	2210	1618	585	1033	1647	470	1177	501	1117

Notes: Additional controls include quadratics in years of experience and age; indicators for having a master's or higher degree, black, male, not married, and having dependent children under the age of five; log district enrollment; district fraction minority students; district fraction of students eligible for free or reduced-price lunch; state unemployment rate; state fixed effects; and a year effect. Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 2.7. Reduced-form Estimates: Effect of Law on Probability of Exit with Union Interaction

	Dependent Variable: Probability of Exit										
	All Teachers			Teachers with Academic Degrees			Teachers with Education Degrees			Teachers with Academic Degrees (by field)	
	All	New	Exper.	All	New	Exper.	All	New	Exper.	Math/Sci	Not Math/Sci
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Full Sample					
Law	-0.062	0.180	-0.130	-0.146	-0.039	-0.270	-0.039	0.509	-0.130	-0.771	-0.085
Probit Coef.	(0.122)	(0.189)	(0.147)	(0.146)	(0.218)	(0.180)	(0.141)	(0.299)	(0.155)	(0.357)	(0.144)
LawXUnion	-0.251	0.186	-0.384	0.047	-0.048	0.325	-0.355	0.222	-0.541	0.259	-0.104
Probit Coef.	(0.181)	(0.263)	(0.192)	(0.249)	(0.440)	(0.321)	(0.202)	(0.428)	(0.217)	(0.518)	(0.293)
Law	-0.008	0.030	-0.014	-0.016	-0.006	-0.022	-0.005	0.075	-0.014	-0.047	-0.009
Marginal Eff.	(0.014)	(0.034)	(0.015)	(0.015)	(0.031)	(0.012)	(0.016)	(0.053)	(0.015)	(0.016)	(0.014)
LawXUnion	-0.028	0.044	-0.033	0.001	-0.008	0.025	-0.036	0.103	-0.041	0.004	-0.014
Marginal Eff.	(0.018)	(0.053)	(0.016)	(0.030)	(0.064)	(0.034)	(0.018)	(0.103)	(0.017)	(0.040)	(0.029)
N	5658	1856	3790	2194	824	1364	3462	1019	2424	533	1640
						Secondary Teachers					
Law	-0.134	0.142	-0.261	-0.327	0.011	-0.548	0.062		0.072	-0.573	
Probit Coef.	(0.156)	(0.229)	(0.142)	(0.200)	(0.286)	(0.205)	(0.143)		(0.161)	(0.391)	
LawXUnion	-0.534	-0.288	-0.486	0.005	-0.365	0.455	-0.997		-1.276	0.304	
Probit Coef.	(0.246)	(0.441)	(0.221)	(0.326)	(0.505)	(0.375)	(0.318)		(0.337)	(0.575)	
Law	-0.015	0.023	-0.023	-0.036	0.002	-0.041	0.006		0.006	-0.038	
Marginal Eff.	(0.016)	(0.038)	(0.011)	(0.018)	(0.047)	(0.011)	(0.014)		(0.014)	(0.020)	
LawXUnion	-0.047	-0.040	-0.035	-0.009	-0.048	0.020	-0.061		-0.057	0.025	
Marginal Eff.	(0.018)	(0.060)	(0.014)	(0.038)	(0.064)	(0.042)	(0.013)		(0.013)	(0.054)	
N	3263	1048	2208	1617	582	1027	1646		1176	474	

Notes: Estimates are presented only for groups for whom the first-stage estimates are significant (see Table 6). Marginal effects are calculated for discrete changes in the dummy variables. Additional controls include quadratics in years of experience and age; indicators for having a master's or higher degree, black, male, not married, and having dependent children under the age of five; log district enrollment; district fraction minority students; district fraction of students eligible for free or reduced-price lunch; state unemployment rate; state fixed effects; and a year effect. Standard errors adjusted for correlation within state-year are reported in parentheses.

Table 2.8. Falsification Test: Effect of Law on Probability of Exit in Pre-Period

	Dependent Variable: Probability of Exit										
	All Teachers			Teachers with Academic Degrees			Teachers with Education Degrees			Teachers with Academic Degrees (by field)	
	All	New	Exper.	All	New	Exper.	All	New	Exper.	Math/Sci	Not Math/Sci
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Full Sample					
Law	0.083	-0.034	0.110	0.197	-0.331	0.372	0.040	0.197	0.020	0.161	0.335
Probit Coef.	(0.076)	(0.183)	(0.100)	(0.161)	(0.264)	(0.201)	(0.098)	(0.191)	(0.123)	(0.329)	(0.210)
Law	0.009	-0.005	0.011	0.021	-0.048	0.037	0.004	0.024	0.002	0.011	0.039
Marginal Eff.	(0.009)	(0.026)	(0.010)	(0.020)	(0.035)	(0.025)	(0.010)	(0.026)	(0.011)	(0.024)	(0.029)
N	6055	1922	4130	1813	631	1159	4240	1272	2965	437	1358
						Secondary Teachers					
Law	0.168	-0.074	0.218	0.033	-0.472	0.191	0.208	0.013	0.147	0.190	0.281
Probit Coef.	(0.113)	(0.267)	(0.120)	(0.180)	(0.360)	(0.230)	(0.151)	(0.342)	(0.171)	(0.385)	(0.233)
Law	0.018	-0.012	0.020	0.003	-0.078	0.013	0.020	0.001	0.012	0.013	0.026
Marginal Eff.	(0.013)	(0.041)	(0.013)	(0.015)	(0.052)	(0.018)	(0.016)	(0.032)	(0.015)	(0.029)	(0.025)
N	3205	1008	2188	1318	451	837	1884	535	1335	388	907

Notes: Marginal effects are calculated for a discrete change in the variable from 0 to 1. Additional controls include quadratics in years of experience and age; indicators for having a master's or higher degree, black, male, not married, and having dependent children under the age of five; log district enrollment; district fraction minority students; district fraction of students eligible for free or reduced-price lunch; state unemployment rate; state fixed effects; and a year effect. Standard errors adjusted for correlation within state-year are reported in parentheses.

Chapter Three

How do state characteristics impact the teacher location decision? Evidence from a conditional logit model

3.1. Introduction

The difficulties school systems face in recruiting well-qualified teachers has been the subject of much policy discussion for decades. States often devote resources to recruiting across state lines in an effort to attract teachers, especially recent college graduates. Articles from newspapers in various parts of the country report anecdotes of out-of-state recruitment efforts as states attempt to lure teachers away from one another. At one University of Nebraska-Lincoln teacher recruiting event, for example, districts from outside the state outnumbered Nebraska districts (O'Connor 2000). A state-funded teacher recruitment program in California known as CalTeach extended its search for teachers into thirty states using advertising slogans such as "Left coast. Right job." and "The opportunities in California for K-12 teachers are as golden as our sunset" (Mitchell 2002; Okoben 2001). Worried about the potential loss of teachers, school leaders in other states such as Colorado have adjusted their own recruitment efforts in response to competition from other states (Mitchell 2002).

In addition to advertising throughout the country, some states try to lure teachers by offering financial incentives. Widespread pay raises persistently arise as a proposed strategy for attracting teachers from other states and stemming the flowing of teachers out of state. In 2000, for example, the Oklahoma state legislature mandated a $3,000 increase in teacher salaries. Regarding the increase, the state superintended remarked, "Out-of-state recruiters are stealing our teachers. We knew we had to do something,"

suggesting that the salary raise was intended to curb the exit of teachers from the state (Blair 2000).

Although policymakers appear to believe that they can affect flows of teachers across state lines via salary or other policy instruments, there has been little empirical research into what influences the choice of the state in which to begin one's teaching career. Using data on beginning public school teachers from the Schools and Staffing Survey, this paper estimates a conditional logit model to investigate the determinants of this location decision. In particular, I seek to estimate the marginal effects of state characteristics on the likelihood that a teacher who recently graduated from college in state *j* chooses to teach in state *k*. The key state characteristics of interest include salary, student characteristics, and geographic proximity to the college state. I also explore whether the influence of these characteristics on the location decision varies with teacher quality, as measured by the selectivity of the teacher's undergraduate institution and type of degree held.

The paper proceeds as follows. The next section provides background on potential determinants of the teacher location decision. I then describe the econometric model used to examine the choice of teaching state. After describing the data, I present the results of the conditional logit analysis. The final section concludes.

3.2. Background

Numerous studies have inferred the influence of various student and school characteristics from the pattern of movements of teachers across schools or out of the teaching profession. Early work by Greenberg and McCall (1974) investigates the

movement of teachers within the San Diego school system. As salary schedules generally do not vary across schools within a given school system, they hypothesize that movement across schools should, in part, reflect teacher preferences over nonpecuniary characteristics of schools, particularly student characteristics. They find that teachers tend to move toward schools with lower minority enrollment percentages, higher achievement scores, and lower percentages of families on welfare. Studies examining movements of teachers across districts within states suggest similar teacher preferences. In recent work examining the influence of various characteristics on teacher transitions in Georgia, Scafidi, Sjoquist, and Stinebrickner (forthcoming) argue that the strong tendency of teachers to leave schools with high minority populations largely drives the observed tendencies of teachers to leave schools with lower income or lower test scores. Regarding the influence of salary, Hanushek, Kain, and Rivkin (2004) find evidence from their analysis of Texas public schools suggesting that salary influences teacher mobility, with higher salaries being associated with lower rates of transferring either to other schools or out of the Texas public school system. However, they note that the likelihood of a teacher exiting a school correlates more strongly with student characteristics than with salary.

While most previous research has not investigated the spatial geography of teacher labor markets, recent work by Boyd *et al* (2005) explores distance as a factor affecting the location decisions of teachers. Their study of first-time public school teachers in New York state indicates that teachers prefer jobs in close geographic proximity to their hometowns. However, their work does not assess the importance of

distance relative to other key determinants, as they do not include student characteristics or teacher salaries in their analysis.

Understanding the role that these factors play in the location decisions of well-qualified teachers can help inform debates over how to improve teacher quality. As with teachers overall, evidence suggests that better qualified teachers prefer lower minority, lower poverty, and higher performing schools. In exploring the distribution of teachers across schools in New York state, Lankford, Loeb, and Wyckoff (2002) find a negative correlation between the percentages of minority, low-income, and low-performing students in a school and the percentage of well-qualified teachers within a school, where measures of qualifications include competitiveness of the college attended by the teacher and the passage of a test of general knowledge. In related work by Boyd *et al* (2002), the authors find that the initial job placement of better-qualified teachers in lower performing schools accounts for more of this sorting than does the pattern of exits and transfers of more qualified teachers to preferred schools. Regarding the influence of distance, Boyd *et al* (2005) find that teachers with higher SAT scores are slightly more likely to select jobs farther from their hometowns.

Evidence of the influence of salaries on the location decisions of well-qualified teachers is mixed. Ballou and Podgursky (1997) explore the relationship between changes in state teacher salaries relative to the earnings of other college-educated workers and changes in teacher quality without controlling for student or other state characteristics. Their main analysis yields no evidence of a relationship between average teacher qualifications and whether the state experienced high, moderate, or low growth in relative teacher salaries. However, one of the analysis checks they perform indicates that

the proportion of teachers from selective colleges increased significantly in high-growth states and decreased significantly in low-growth states. At the district level, unilateral pay raises may be effective in attracting well-qualified teachers. Figlio (2002) finds evidence that nonunion school districts that raise their salaries relative to other districts in the same county increase their likelihood of hiring teachers who graduated from selective colleges or who majored in their teaching field.

This paper seeks to contribute to the literature by assessing the influence of these key factors as determinants of initial teacher location decisions across states. Much of the previous research uses state-specific datasets that preclude an investigation of teacher movements across state lines. Using a national dataset, I estimate the influence of salary, distance, and student characteristics on the probability that a teacher who recently graduated from college locates in a particular state.

3.3. Econometric Model

To investigate the initial teacher location decision, this paper utilizes the conditional logit model developed by McFadden (1973), also known as McFadden's choice model. In deciding where to begin her teaching career, each recently graduated teacher considers the characteristics of all the states and chooses the state that maximizes her utility.[25] The conditional logit model[26] has often been used for analyzing such

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[25] Though a new teacher may not be able to choose a particular school or district in which to begin her career, the model assumes that sufficient teaching opportunities exist to allow new teachers to choose their initial teaching state.

[26] Terminology varies in the literature. Typically, the term "multinomial logit model" refers to models in which the choice of alternatives is expressed as a function only of the attributes of the individual making the choice, and the term "conditional logit model" refers to models in which the choice is expressed as a

location decisions, as it allows for the modeling of the choice among alternatives as a function of the characteristics of the alternatives.[27]

McFadden derives the conditional logit model from random utility models, beginning with the assumption that individuals rationally make choices that maximize their utility subject to expenditure constraints. The presence of imperfect information and imperfect optimization leads to the assumption that utility has a random component. Suppose that for an individual i from place of origin j facing K choices, the perceived utility of a particular choice k is $U_{ijk} = Z_{ijk}'\beta + \varepsilon_{ijk}$, where X_{ijk} is a vector of choice-specific characteristics and individual attributes. Given the rationality assumption, an individual selecting choice k implies that U_{ijk} is the maximum among the K utilities. Thus, the probability that choice k is made is $Prob(U_{ijk} > U_{ijm})$ for all $m \neq k$. As shown by McFadden (1973), under the assumption that ε_{ijk} are independently and identically distributed with an extreme value distribution, the probability of individual i from place j choosing option k, P_{ijk}, is given by the following.

$$P_{ijk} = \exp[Z'_{ijk}\beta] / \sum_{k=1}^{K} \exp[Z'_{ijk}\beta]$$

Estimates of the parameter vector β, which is constant across choices, can then be obtained using maximum likelihood methods.

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function of the characteristics of the alternatives. Some researchers, however, use the term multinomial logit model even for models that are functions of alternative-specific characteristics.

[27] Polsky *et al* (2002) conduct a similar analysis for doctors, using a conditional logit model to assess the initial practice location choices of recent medical graduates. In addition to the previously described Boyd *et al* (2005) study of the location decisions of New York teachers, recent applications of the conditional logit model include studies of college choice (Long 2004b) and the location choice of welfare recipients (O'Keefe 2004).

In the teacher location choice model used in this analysis, $Z_{ijk}'\beta$ is defined as follows.

$$Z_{ijk}'\beta = \beta_1 X_{jk1} + \beta_2(X_{jk2}W_{i2}) + \beta_3 X_{jk3} + \beta_4(X_{jk4}W_{i4}) + \ldots + \varepsilon_{ijk}$$

This model views the state in which the teacher attended college as the state of origin.[28] The K state choices include remaining in the college state j or choosing to teach in another state. The variables X_{jk} are characteristics of the K potential destination states that may influence the probability of a recently graduated teacher locating in a given state; the j subscript reflects the fact that some of the characteristics of the state choices—namely distance and nonmigration, as defined below—depend on the college state. The variables W_i are teacher attributes. Any terms that do not vary across the choices cancel out of the probability expression. Consequently, to allow for variation in the importance of the state variables across teachers, I follow the common practice in the literature and include interactions between the state characteristics and teacher attributes, $X_{jk}W_i$.[29]

As noted earlier, the previous literature suggests that salary, student characteristics, and distance from place of origin factor into the location decision. To assess the influence of salary in the choice of state, I include the average beginning salary offered in the state, as obtained by averaging the district salaries paid to beginning teachers with a bachelor's degree. Variables describing student characteristics include

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[28] Ideally, I would like to investigate mobility over a specified period time after obtaining the undergraduate degree. The Schools and Staffing Survey, however, includes data on only the current teaching state and the state from which the teacher graduated; other changes in location cannot be tracked in this dataset.

[29] Note that the main effects of the teacher attributes drop out of the model. Scott, Coomes, and Izyumov (2005) emphasize the importance of including such interactions in their study of the initial location choice of employment-based immigrants. Other recent examples of allowing for the influence of individual attributes in conditional logit models by interacting individual attributes with choice characteristics include Boyd et al. (2005) and Long (2004a).

the average fraction of district students that are minority and the average fraction of district students that are eligible for free or reduced price lunch. As a crude measure of student achievement, I use the average high school graduation rate of twelfth graders.[30] As a measure of distance, I include a quadratic in the distance between the college state and the teaching state being considered.[31] While the distance measures control for the variable costs associated with moving, I also follow common practice and include a nonmigration indicator variable that captures the fixed costs associated with moving away from the state of origin. The nonmigration dummy equals one if the destination state under consideration is the same state as the college state (that is, $k = j$) and equals zero otherwise. Other covariates include the following variables that potentially affect the supply of and demand for teachers or reflect general labor market conditions in the state: state enrollment, the fraction of schools located in urban settings (large or mid-size cities), the fraction of districts that have collective bargaining agreements with teacher unions, and the state unemployment rate. Finally, the model includes state fixed effects to control for unobserved characteristics of the states that do not vary over time.

The importance of state characteristics in the location decision may vary with the teacher's own attributes. Of particular interest for policymakers seeking to improve

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[30] The school record component of the Schools and Staffing Survey reports the percentage of twelfth graders who graduated the previous school year. Lack of comparability or availability across states precludes the use of more common indicators of academic achievement in this analysis. The National Assessment of Educational Progress, often called the "nation's report card," provides an achievement measure that could potentially be compared across states; however, national samples are not designed to be representative at the state level, and some states choose not to participate in state assessments. Similarly, annual dropout rates may be calculated for some states from the Common Core of Data, but not all states report their dropout data. Measures such as average SAT scores cannot be compared directly across states due to variation in the proportion and composition of students taking such tests.

[31] I used the Great Circle Distance Formula to calculate the distance between the geographic centers of the fifty states and Washington, DC. The variables included in the model measure distance in thousands of miles.

teacher quality is the question of whether the influence of state characteristics such as average salary changes with the qualifications of the teacher. To allow for such variation, I estimate specifications that include interactions between the state characteristics and the following teacher attributes: selectivity of undergraduate institution attended, sex, age, and race/ethnicity.

As noted in the introduction, some policymakers appear to believe that raising the overall level of salaries in a state may affect the flow of teachers into and out of the state. To explore this proposition, I also estimate a model in which I replace the salary variable with a time-varying indicator variable, *Law*, that turns on in 1999 if the legislature of the state mandated at least one teacher pay raise between 1993 and 1999. Results from the previous chapters of this thesis indicate that the passage of such mandates led to real increases in teacher salaries. The coefficient on *Law* in this model provides a reduced form estimate of the impact of a widespread teacher pay raise on the likelihood of a teacher choosing to begin her teaching career in a state.

Chapter two of this thesis found that the retention effects of salary varies with the subject matter expertise of the teacher, as measured by the type of degree held. To explore whether teachers with different types of degrees also vary in their responsiveness to salary and other state characteristics when deciding where to begin teaching, I analyze results separately for teachers with academic degrees and teachers with education degrees.[32] As concerns about subject matter expertise tend to be particularly strong at the

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[32] Some teachers have more than one major field of study. As explained in chapter two of this thesis, I place a teacher in the education degree category if all of her majors fall into the education fields, which include general education (e.g., elementary education), subject area education (e.g., science education), special education (e.g., speech/language impaired), or other education (e.g., curriculum and instruction). If

secondary level where teachers must instruct students in relatively advanced concepts in departmentalized courses, I also repeat the analyses for a subsample of secondary school teachers.

3.4. Data and Descriptive Statistics

The sample of recent college graduates who entered teaching used in this analysis comes from the 1993-94 and 1999-2000 rounds of the Schools and Staffing Survey (SASS). In addition to providing demographic information, the teacher component of the SASS reports the year the individual began teaching as well as key education data, including the undergraduate institution attended by the teacher, her major, and the year in which she earned her degree. To focus on the initial location decisions of recent college graduates, I identify for the analysis sample full-time public school teachers in regular elementary and secondary schools who both earned their bachelor's degree and began teaching within three years of the survey. I also link the undergraduate institution information to data from a college guide on the selectivity of the institutions.[33] The main sample consists of 6,996 teachers with nonmissing data, each of whom faces a choice of fifty-one locations, including the fifty states and the District of Columbia.[34] State

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any of her major fields of study occurred in a discipline outside of the education fields, I define the teacher as having an academic degree.

[33] The selectivity data come from the 1990 Lovejoy's guide. The selectivity ranking ranges from 1 to 5, with 5 denoting the most selective institution.

[34] To estimate the conditional logit model, the data must be arranged such that each individual i has J records, where each of the J records represents one of the alternatives available to the individual. The dependent variable is a choice indicator variable that equals one for the alternative chosen by individual i and zero for the alternatives not chosen. Given this data structure, the number of observations in the analysis exceeds the number of individuals in the sample. Since the sample used here consists of 6,996 teachers who each choose from one of 51 alternatives, the analysis contains $6{,}996 \times 51 = 356{,}796$ observations.

characteristics concerning salary, enrollment, fraction minority, fraction eligible for free or reduced-price lunch, high school graduation rates, urbanicity, and collective bargaining are derived as weighted averages of data from the SASS district and school records. Data on state unemployment rates come from the Bureau of Labor Statistics.

The specification involving mandated pay raises utilizes data obtained primarily by compiling information for the years 1993 through 1999 from legislative summaries reported in the periodical *Education Week* and from searches of state statutes. During this time period, nineteen states passed legislation requiring teacher pay raises. On average, the salary paid to beginning teachers with bachelor's degrees increased by about 7 percent in districts located in states that mandated pay raises relative to districts in states without such mandates.[35]

Table 3.1 reports summary statistics describing the teacher sample and key state characteristics by survey year. Each survey year, about 19 percent of beginning teachers who recently graduated from college chose to teach in a state other than the state in which they attended college. Nearly 10 percent of the new teachers attended highly selective undergraduate institutions, defined as a college or university ranked as a 4 or 5 on a five-point selectivity index. Regarding state characteristics, average state unemployment declined and enrollment increased over the time period studied, while means for other variables remained fairly stable.

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[35] See chapter one of this thesis for the demonstration of this result and for additional information on the mandates and the varied factors influencing their passage.

3.5. Results

Table 3.2 presents the results of estimating the conditional logit model of the initial teacher location decision for the full sample of beginning teachers. Column (1) displays estimates for the specification that includes only state characteristics with no interactions with individual attributes. Column (2) adds interactions with teacher demographic variables, namely dummies for male, black, Hispanic, and age greater than or equal to thirty. Finally, column (3) adds interactions with indicators for graduating from a highly selective undergraduate institution and for holding a master's or higher degree. Following Kaushal (2005), the table reports the marginal effects evaluated at the mean probability of choosing a state. Table A.1 in the appendix reports the corresponding coefficients from the conditional logit model.

The estimates of the marginal effects of state characteristics on the probability of a beginning teacher locating in a given state are generally similar across the specifications. As seen by comparing estimates in columns (1) through (3), neither the magnitude nor the statistical significance of the estimates changes substantially with the addition of interactions with teacher attributes. The discussion below focuses on the estimates presented in column (3) for the specification that includes the full set of interactions. Taking into account all of the interactions, the main effects reported in column (3) represent the estimated influence of each state characteristic on the likelihood of a state being chosen by a typical female, non-Black, non-Hispanic, teacher under the age of thirty who recently graduated from a non-highly-selective college and who does not hold a master's or higher degree.

Both the geographic proximity of the teaching state to the college state and the racial composition of the state's students appear to weigh heavily in the location decision. As evidenced by the magnitude and significance of the estimates for nonmigration and distance, recent college graduates demonstrate a strong propensity to begin teaching in or near the state in which they earned their bachelor's degree. Attending college in a given state increases a beginning teacher's likelihood of starting her teaching career in that state by approximately 6 percentage points, all else equal. Increasing the distance between the college state and the state under consideration by 1,000 miles decreases the likelihood of a new teacher choosing to locate in the state by more than 5 percentage points. Distance appears to be slightly less of a deterrent for teachers from more selective institutions, as indicated by the significant positive interaction effect, but these teachers still exhibit a strong preference for teaching near their college states. Consistent with the previous literature examining movements across schools and districts, the findings also suggest that teachers prefer locations with relatively low proportions of minority students.

There is limited evidence suggesting that several other state characteristics affect teachers' initial location choices. The signs and magnitudes of the marginal effects indicate that the typical beginning teacher tends to prefer states with larger enrollments, lower proportions of students eligible for free or reduced-price lunch, higher graduations rates, higher fractions of urban districts, and higher fractions of districts with collective bargaining agreements. However, the estimates for these characteristics have relatively large standard errors and are not statistically significant. The negative signs on most of the estimates for unemployment suggests that teachers prefer states with lower unemployment rates, but these marginal effects are smaller in magnitude than those of

other characteristics and are imprecisely estimated. Although only a few statistically significant interaction effects emerge, there is some suggestion that the influence of these characteristics varies with the attributes of the teachers. For example, the estimates suggest black teachers strongly prefer urban areas and are more willing than other teachers to locate in lower income areas, as proxied by student eligibility for free or reduced-price lunch.

Notably, there is no evidence of average salaries significantly influencing the location choice of the typical teacher. Table 3.3 highlights the estimated effects of salary on teacher choice using two different salary measures: the first three columns repeat the estimates from Table 3.2 using average state salary and the last three columns reports results from estimating the models using a salary mandate dummy.[36] As argued in chapter one of this thesis, the passage of a statewide salary mandate during the time period analyzed provides a source of variation in teacher salaries that is plausibly exogenous with respect to teacher qualifications. Regardless of which measure is used, the main effect of the salary is both very small in magnitude and statistically insignificant. The marginal effect of the interaction of average salary with the selectivity dummy suggests a $10,000 increase in a state's average salary (1999 dollars) is associated with a 1.5 percentage point decrease in the probability of choosing a state for teachers from highly selective institutions relative to teachers from less selective colleges; however, this interaction effect becomes negligible when using the salary

[36] Table A.2 in the appendix reports the corresponding coefficients from the conditional logit model for the specifications that use the law dummy.

mandate dummy. Overall, the estimates do not support the notion that higher overall levels of teacher pay lure new teachers into a state.

Academic vs. Education Degrees

Tables 3.4 and 3.5 report the results of estimating the full model separately for teachers with academic degrees and teachers with education degrees. The similarity of the estimates of the effects of nonmigration and distance (Table 3.4) indicates that the propensity to choose states in close proximity to the college state does not vary with degree type. As found for the full sample of teachers, neither group demonstrates a significant responsiveness to average state salary in choosing their initial teaching state. The negative interaction effect of average salary with the indicator for attending a highly selective college appears to be stronger among teachers with academic degrees than those with education degrees; however, as seen in Table 3.5, the effect does not persist in the model that uses state salary mandates as the source of variation in salary.

While the effects of geographic proximity and salary appear to be broadly similar across teachers with academic degrees and those with education degrees, the evidence suggests some differences in the influence of state characteristics across the groups. In particular, the marginal effects of state enrollment and fraction minority enrollment appear to affect the location choice of teachers with academic degrees more strongly than that of teachers with education degrees. The main effects of enrollment indicate that increasing state enrollment by one million students increases the likelihood of a teacher with an academic degree choosing the state by a statistically significant 3.1 percentage points, compared to an insignificant .009 increase for teachers with education degrees.

Minority student enrollment appears to have a negative effect on the likelihood of either group choosing a state, but the negative effect is stronger for the teachers with academic degrees. Patterns for other state characteristics are less clear. Differences emerge for a few of the interactions of individual attributes with state characteristics—for example, relatively large positive effects of the average graduation rate and fraction of school districts that are urban occur for male teachers with academic degrees in contrast to negligible effects of these characteristics for male teachers with education degrees—but many of the estimates are imprecise.

Secondary School Teachers

As seen in Tables 3.6 and 3.7, the main findings for the full sample of teachers hold for the subsample of secondary school teachers as well. Secondary school teachers exhibit a strong preference for beginning their teaching careers in or near the state in which they attended college, although some subgroups of teachers such as those from highly selective undergraduate institutions are less deterred by distance than others. Regardless of whether one uses the average state salary or the salary mandate dummy, the estimates yield no evidence of a significant relationship between state beginning teacher salary levels and the probability of a teacher choosing a particular state. The estimates indicate a strong negative association between a state's fraction minority students and the likelihood of a secondary teacher choosing the state. Estimates for other characteristics tend to be imprecise, but the signs of the marginal effects suggest that teachers prefer states with larger enrollments and higher fractions of districts with collective bargaining agreements. The effects of graduation rates and urbanicity appear

to vary with teacher characteristics, and there is no evidence of a substantial impact on unemployment.

The patterns across secondary school teachers with academic degrees versus education degrees are similar to those found for the full sample of teachers (Tables 3.8 and 3.9). The marginal effects of the nonmigration dummy and distance are large and of similar magnitude for both types of teachers. The effects of salary tend to be small and statistically insignificant. Also as found for the full sample, stronger enrollment effects emerge for teachers with academic degrees than those with education degrees.

3.6. Conclusions

Evidence from estimating a conditional logit model of the initial location choice of beginning teachers strongly indicates that geographic proximity to the college state and minority enrollment dominate other measured state characteristics in the teacher location decision. Recent college graduates exhibit a strong preference for teaching in the state from which they graduated. For those new teachers who do migrate to another state, distance plays a significant role in the choice of state, with the probability of locating in the state decreasing as the distance from the college state increases. Teachers also prefer states with lower proportions of minority students. Although the strength of preferences over different characteristics sometimes varies with individual attributes, geographic proximity and minority enrollment appear to weigh heavily in the location choice of teachers, regardless of the selectivity of their undergraduate institution, their subject matter expertise, the level of school taught, or other teacher characteristics.

While previous research indicates a significant effect of salary on the choice of district or school within a state, the results of this analysis suggest that the effect of salary does not hold at the state level. The estimates yielded no evidence of an effect of a state's average beginning teacher salary on the location decision. Legislating a widespread pay raise does not appear to significantly affect a beginning teacher's willingness to locate in a particular state.

The findings suggest that state policymakers face considerable difficulties in attempting to recruit qualified teachers from other states. The propensity for recent college graduates to begin teaching in the same state in which they earned their bachelor's degrees casts doubt on the efficacy of devoting resources to recruiting out of state. This study does not examine the effects of other recruitment strategies, such as offering bonuses to particular teachers. However, the importance of factors over which policymakers have no control—such as geographic proximity to the college state and the proportion of minority enrollment—raises doubts about the success of widespread pay raises and other general out-of-state recruitment strategies in attracting highly qualified recent college graduates from other states.

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Table 3.1. Summary Statistics

	1993 N = 3478	1999 N = 3518
	Mean (Standard Deviation)	Mean (Standard Deviation)
Teacher Characteristics		
Teach Outside of College State	0.188 (0.391)	0.193 (0.395)
Attended Highly Selective Undergraduate Institution	0.094 (0.292)	0.094 (0.292)
Academic Degree	0.313 (0.464)	0.364 (0.481)
Master's Degree or Higher	0.068 (0.253)	0.083 (0.275)
Male	0.316 (0.465)	0.330 (0.470)
Black	0.037 (0.188)	0.063 (0.243)
Hispanic	0.062 (0.240)	0.063 (0.244)
Age	29.3 (6.51)	27.8 (6.34)
State Characteristics		
Starting Salary	25,105 (3,502)	25,568 (3,243)
Enrollment	829,454 (931,972)	896,434 (1,030,764)
Fraction Minority Students	0.216 (0.209)	0.242 (0.206)
Fraction of Students Eligible for Free or Reduced-Price Lunch	0.458 (0.137)	0.406 (0.129)
12^{th} Grade Graduation Rate	0.913 (0.062)	0.897 (0.066)
Fraction Urban	0.221 (0.153)	0.223 (0.140)
Fraction of Districts with Collective Bargaining	0.586 (0.405)	0.597 (0.407)
Unemployment Rate	6.37 (1.51)	4.16 (1.05)

Notes: With the exception of enrollment and the unemployment rate, state characteristics are calculated as weighted averages across all public schools or districts in the Schools and Staffing Survey. State enrollment is calculated as the weighted sum of district enrollments. The state unemployment rate comes from the Bureau of Labor Statistics. Salaries are converted to 1999 dollars using the CPI-U.

Table 3.2. Marginal Effects of State Characteristics on the Choice of Initial Teaching State

	No Interactions	Interactions with Demographic Attributes	Interactions with All Attributes
	(1)	(2)	(3)
Salary (per $10,000)	0.0001	-0.001	0.002
	(0.007)	(0.007)	(0.007)
Salary X Male		0.001	0.001
		(0.004)	(0.004)
Salary X Black		0.004	0.003
		(0.011)	(0.011)
Salary X Hispanic		0.005	0.005
		(0.011)	(0.011)
Salary X Age 30+		0.004	0.002
		(0.005)	(0.004)
Salary X High Select			-0.015
			(0.006)
Salary X Master's			0.005
			(0.005)
Nonmigration	0.062	0.060	0.060
	(0.003)	(0.003)	(0.003)
Nonmig X Male		0.002	0.001
		(0.003)	(0.003)
Nonmig X Black		0.003	0.003
		(0.006)	(0.006)
Nonmig X Hispanic		0.014	0.014
		(0.009)	(0.009)
Nonmig X Age 30+		0.003	0.004
		(0.004)	(0.003)
Nonmig X High Select			-0.005
			(0.004)
Nonmig X Master's			-0.003
			(0.005)
Distance (per 1,000 miles)	-0.051	-0.051	-0.055
	(0.005)	(0.005)	(0.005)
Dist X Male		0.004	0.004
		(0.003)	(0.003)
Dist X Black		-0.005	-0.004
		(0.006)	(0.006)
Dist X Hispanic		0.004	0.004
		(0.009)	(0.008)
Dist X Age 30+		-0.005	-0.003
		(0.004)	(0.004)
Dist X High Select			0.015
			(0.003)
Dist X Master's			0.001
			(0.004)
Enrollment	0.021	0.020	0.019
(per 1,000,000 students)	(0.012)	(0.012)	(0.012)
Enroll X Male		0.001	0.001
		(0.001)	(0.001)
Enroll X Black		0.002	0.002
		(0.003)	(0.003)

(Table 3.2. continued)

Enroll X Hispanic		0.003	0.003
		(0.002)	(0.002)
Enroll X Age 30+		-0.002	-0.002
		(0.001)	(0.001)
Enroll X High Select			0.004
			(0.001)
Enroll X Master's			-0.0001
			(0.002)
Fraction Minority	-0.076	-0.082	-0.087
	(0.039)	(0.039)	(0.040)
Minority X Male		-0.004	-0.003
		(0.010)	(0.010)
Minority X Black		0.030	0.031
		(0.019)	(0.019)
Minority X Hispanic		0.002	0.005
		(0.025)	(0.025)
Minority X Age 30+		0.007	0.008
		(0.010)	(0.010)
Minority X High Select			0.010
			(0.015)
Minority X Master's			0.004
			(0.015)
Fraction Lunch	-0.018	-0.028	-0.021
	(0.016)	(0.016)	(0.017)
Lunch X Male		0.012	0.010
		(0.010)	(0.010)
Lunch X Black		0.049	0.049
		(0.026)	(0.026)
Lunch X Hispanic		0.021	0.019
		(0.028)	(0.027)
Lunch X Age 30+		0.006	0.006
		(0.016)	(0.016)
Lunch X High Select			-0.015
			(0.015)
Lunch X Master's			-0.044
			(0.021)
Graduation Rate	0.025	0.016	0.019
	(0.032)	(0.035)	(0.037)
Grad Rate X Male		0.029	0.028
		(0.018)	(0.018)
Grad Rate X Black		-0.057	-0.059
		(0.036)	(0.036)
Grad Rate X Hispanic		0.015	0.012
		(0.040)	(0.039)
Grad Rate X Age 30+		0.009	0.008
		(0.026)	(0.026)
Grad Rate X High Select			-0.021
			(0.028)
Grad Rate X Master's			-0.002
			(0.028)

(Table 3.2. continued)

Fraction Urban	0.013	0.011	0.010
	(0.019)	(0.019)	(0.019)
Urban X Male		0.014	0.013
		(0.010)	(0.010)
Urban X Black		0.045	0.047
		(0.016)	(0.016)
Urban X Hispanic		-0.013	-0.015
		(0.025)	(0.025)
Urban X Age 30+		-0.017	-0.016
		(0.010)	(0.011)
Urban X High Select			0.015
			(0.013)
Urban X Master's			-0.001
			(0.013)
Fraction Coll. Bargaining	0.035	0.032	0.032
	(0.029)	(0.029)	(0.030)
Coll Barg X Male		0.003	0.003
		(0.004)	(0.004)
Coll Barg X Black		0.010	0.011
		(0.010)	(0.010)
Coll Barg X Hispanic		0.017	0.018
		(0.009)	(0.010)
Coll Barg X Age 30+		-0.002	-0.002
		(0.004)	(0.004)
Coll Barg X High Select			0.007
			(0.005)
Coll Barg X Master's			-0.003
			(0.006)
Unemployment Rate	-0.002	-0.002	-0.002
	(0.001)	(0.001)	(0.001)
Unemp X Male		-0.001	-0.001
		(0.001)	(0.001)
		0.22	0.18
Unemp X Black		-0.002	-0.003
		(0.002)	(0.002)
Unemp X Hispanic		0.005	0.005
		(0.002)	(0.002)
Unemp X Age 30+		0.002	0.001
		(0.001)	(0.001)
Unemp X High Select			-0.003
			(0.002)
Unemp X Master's			0.0004
			(0.002)
Number of Individuals	6996	6996	6996

Notes: The table reports the marginal effects of state characteristics on the probability of a beginning teacher i who graduated from college in state j choosing to teach in state k. State characteristics are measured for each potential destination state, including the college state. The values of distance and nonmigration for destination state k depend on college state j (see text for variable definitions). The marginal effects are computed around the mean probability of choosing a state. The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. All specifications include state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table 3.3. Marginal Effects of Average Salary/Salary Mandate (Law) on the Choice of Initial Teaching State

	Salary Variable: Average Starting Salary			Salary Variable: Law Dummy		
	No Interactions	Interactions with Demographic Attributes	Interactions with All Attributes	No Interactions	Interactions with Demographic Attributes	Interactions with All Attributes
	(1)	(2)	(3)	(4)	(5)	(6)
Salary/Law	0.0001	-0.001	0.002	-0.001	-0.002	-0.001
	(0.007)	(0.007)	(0.007)	(0.003)	(0.003)	(0.003)
X Male		0.001	0.001		-0.003	-0.004
		(0.004)	(0.004)		(0.002)	(0.002)
X Black		0.004	0.003		0.010	0.011
		(0.011)	(0.011)		(0.009)	(0.009)
X Hispanic		0.005	0.005		-0.006	-0.006
		(0.011)	(0.011)		(0.007)	(0.007)
X Age 30+		0.004	0.002		0.006	0.005
		(0.005)	(0.004)		(0.005)	(0.005)
X High Select			-0.015			-0.0004
			(0.006)			(0.005)
X Master's			0.005			-0.009
			(0.005)			(0.005)
Number of Individuals	6996	6996	6996	6970	6970	6970

Notes: The table reports the marginal effects of state salary variables on the probability of a beginning teacher *i* who graduated from college in state *j* choosing to teach in state *k*. The salary variables are measured for each potential destination state, including the college state. The marginal effects are computed around the mean probability of choosing a state. The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. As salary mandate data is not available for the District of Columbia, teachers who graduated from or currently teach in DC have been omitted from specifications that include the law dummy, and each teacher in these specifications faces a choice of fifty states. Additional controls include a nonmigration dummy, a quadratic in distance, enrollment, fraction minority students, fraction of students eligible for free or reduced-price lunch, twelfth-grade graduate rate, fraction urban, fraction of districts with collective bargaining, unemployment rate, the interaction of each of these state characteristics with each teacher attribute, and state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table 3.4. Marginal Effects of State Characteristics on Choice of Initial Teaching State by Teacher Degree Type

	All Teachers	Teachers with Academic Degrees	Teachers with Education Degrees
	(1)	(2)	(3)
Salary (per $10,000)	0.002	-0.003	-0.0003
	(0.007)	(0.011)	(0.008)
Salary X Male	0.001	0.005	0.001
	(0.004)	(0.006)	(0.006)
Salary X Black	0.003	-0.002	0.009
	(0.011)	(0.019)	(0.013)
Salary X Hispanic	0.005	-0.006	0.010
	(0.011)	(0.014)	(0.013)
Salary X Age 30+	0.002	0.004	0.001
	(0.004)	(0.007)	(0.006)
Salary X High Select	-0.015	-0.018	-0.005
	(0.006)	(0.007)	(0.010)
Salary X Master's	0.005	0.009	0.008
	(0.005)	(0.008)	(0.008)
Nonmigration	0.060	0.059	0.060
	(0.003)	(0.004)	(0.003)
Nonmig X Male	0.001	0.009	-0.003
	(0.003)	(0.005)	(0.004)
Nonmig X Black	0.003	0.003	0.005
	(0.006)	(0.011)	(0.007)
Nonmig X Hispanic	0.014	0.023	0.010
	(0.009)	(0.012)	(0.011)
Nonmig X Age 30+	0.004	-0.001	0.005
	(0.003)	(0.006)	(0.004)
Nonmig X High Select	-0.005	-0.012	0.004
	(0.004)	(0.006)	(0.006)
Nonmig X Master's	-0.003	-0.005	0.001
	(0.005)	(0.006)	(0.007)
Distance (per 1,000 miles)	-0.055	-0.053	-0.056
	(0.005)	(0.005)	(0.007)
Dist X Male	0.004	0.009	-0.0002
	(0.003)	(0.004)	(0.004)
Dist X Black	-0.004	-0.003	-0.007
	(0.006)	(0.010)	(0.006)
Dist X Hispanic	0.004	0.013	-0.003
	(0.008)	(0.011)	(0.010)
Dist X Age 30+	-0.003	-0.007	-0.001
	(0.004)	(0.006)	(0.005)
Dist X High Select	0.015	0.011	0.014
	(0.003)	(0.005)	(0.005)
Dist X Master's	0.001	0.002	-0.002
	(0.004)	(0.005)	(0.006)
Enrollment	0.019	0.031	0.009
(per 1,000,000 students)	(0.012)	(0.016)	(0.013)
Enroll X Male	0.001	0.002	0.0001
	(0.001)	(0.002)	(0.001)
Enroll X Black	0.002	0.004	-0.002
	(0.003)	(0.004)	(0.005)

(Table 3.4. continued)

Enroll X Hispanic	0.003	0.007	0.001
	(0.002)	(0.003)	(0.002)
Enroll X Age 30+	-0.002	-0.004	-0.001
	(0.001)	(0.002)	(0.002)
Enroll X High Select	0.004	0.003	0.003
	(0.001)	(0.002)	(0.002)
Enroll X Master's	-0.0001	-0.002	0.003
	(0.002)	(0.003)	(0.002)
Fraction Minority	-0.087	-0.140	-0.064
	(0.040)	(0.048)	(0.045)
Minority X Male	-0.003	-0.005	-0.001
	(0.010)	(0.014)	(0.012)
Minority X Black	0.031	0.035	0.026
	(0.019)	(0.026)	(0.031)
Minority X Hispanic	0.005	-0.018	0.017
	(0.025)	(0.038)	(0.035)
Minority X Age 30+	0.008	0.029	-0.002
	(0.010)	(0.014)	(0.012)
Minority X High Select	0.010	0.012	0.013
	(0.015)	(0.019)	(0.036)
Minority X Master's	0.004	0.018	-0.023
	(0.015)	(0.020)	(0.028)
Fraction Lunch	-0.021	-0.010	-0.024
	(0.017)	(0.023)	(0.020)
Lunch X Male	0.010	0.027	-0.001
	(0.010)	(0.017)	(0.014)
Lunch X Black	0.049	0.056	0.044
	(0.026)	(0.040)	(0.034)
Lunch X Hispanic	0.019	0.033	0.006
	(0.027)	(0.038)	(0.038)
Lunch X Age 30+	0.006	-0.015	0.016
	(0.016)	(0.026)	(0.018)
Lunch X High Select	-0.015	-0.024	-0.009
	(0.015)	(0.023)	(0.029)
Lunch X Master's	-0.044	-0.090	0.033
	(0.021)	(0.028)	(0.036)
Graduation Rate	0.019	-0.036	0.046
	(0.037)	(0.044)	(0.042)
Grad Rate X Male	0.028	0.092	-0.015
	(0.018)	(0.028)	(0.024)
Grad Rate X Black	-0.059	-0.053	-0.048
	(0.036)	(0.049)	(0.056)
Grad Rate X Hispanic	0.012	0.063	-0.027
	(0.039)	(0.070)	(0.050)
Grad Rate X Age 30+	0.008	-0.003	0.007
	(0.026)	(0.040)	(0.031)
Grad Rate X High Select	-0.021	0.001	-0.064
	(0.028)	(0.030)	(0.041)
Grad Rate X Master's	-0.002	0.019	-0.060
	(0.028)	(0.040)	(0.051)

(Table 3.4. continued)

Fraction Urban	0.010	0.023	-0.001
	(0.019)	(0.024)	(0.023)
Urban X Male	0.013	0.037	-0.003
	(0.010)	(0.014)	(0.012)
Urban X Black	0.047	0.020	0.080
	(0.016)	(0.020)	(0.022)
Urban X Hispanic	-0.015	0.027	-0.037
	(0.025)	(0.036)	(0.031)
Urban X Age 30+	-0.016	-0.032	-0.010
	(0.011)	(0.016)	(0.013)
Urban X High Select	0.015	0.010	0.017
	(0.013)	(0.014)	(0.023)
Urban X Master's	-0.001	0.0002	-0.004
	(0.013)	(0.017)	(0.024)
Fraction Coll. Bargaining	0.032	0.038	0.027
	(0.030)	(0.037)	(0.033)
Coll Barg X Male	0.003	0.002	0.004
	(0.004)	(0.006)	(0.004)
Coll Barg X Black	0.011	0.015	0.006
	(0.010)	(0.011)	(0.011)
Coll Barg X Hispanic	0.018	0.026	0.010
	(0.010)	(0.012)	(0.013)
Coll Barg X Age 30+	-0.002	-0.001	-0.003
	(0.004)	(0.006)	(0.005)
Coll Barg X High Select	0.007	0.007	0.006
	(0.005)	(0.005)	(0.008)
Coll Barg X Master's	-0.003	-0.007	0.003
	(0.006)	(0.007)	(0.008)
Unemployment Rate	-0.002	-0.001	-0.002
	(0.001)	(0.002)	(0.002)
Unemp X Male	-0.001	-0.002	-0.0005
	(0.001)	(0.001)	(0.001)
Unemp X Black	-0.003	-0.002	-0.004
	(0.002)	(0.002)	(0.003)
Unemp X Hispanic	0.005	0.003	0.005
	(0.002)	(0.003)	(0.002)
Unemp X Age 30+	0.001	0.001	0.001
	(0.001)	(0.002)	(0.001)
Unemp X High Select	-0.003	-0.002	-0.003
	(0.002)	(0.002)	(0.003)
Unemp X Master's	0.0004	-0.001	0.002
	(0.002)	(0.003)	(0.003)
Number of Individuals	6996	2371	4625

Notes: The table reports the marginal effects of state characteristics on the probability of a beginning teacher i who graduated from college in state j choosing to teach in state k. State characteristics are measured for each potential destination state, including the college state. The values of distance and nonmigration for destination state k depend on college state j (see text for variable definitions). The marginal effects are computed around the mean probability of choosing a state. The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. All specifications include state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table 3.5. Marginal Effects of Average Salary/Salary Mandate (Law) on the Choice of Initial Teaching State by Teacher Degree Type

	Salary Variable: Average Starting Salary			Salary Variable: Law Dummy		
	All Teachers	Teachers with Academic Degrees	Teachers with Education Degrees	All Teachers	Teachers with Academic Degrees	Teachers with Education Degrees
	(1)	(2)	(3)	(4)	(5)	(6)
Salary/Law	0.002	-0.003	-0.0003	-0.001	-0.004	0.001
	(0.007)	(0.011)	(0.008)	(0.003)	(0.004)	(0.003)
X Male	0.001	0.005	0.001	-0.004	-0.001	-0.007
	(0.004)	(0.006)	(0.006)	(0.002)	(0.003)	(0.004)
X Black	0.003	-0.002	0.009	0.011	0.015	0.006
	(0.011)	(0.019)	(0.013)	(0.009)	(0.014)	(0.011)
X Hispanic	0.005	-0.006	0.010	-0.006	0.011	-0.014
	(0.011)	(0.014)	(0.013)	(0.007)	(0.018)	(0.008)
X Age 30+	0.002	0.004	0.001	0.005	0.006	0.005
	(0.004)	(0.007)	(0.006)	(0.005)	(0.006)	(0.005)
X High Select	-0.015	-0.018	-0.005	-0.0004	0.002	-0.003
	(0.006)	(0.007)	(0.010)	(0.005)	(0.006)	(0.007)
X Master's	0.005	0.009	0.008	-0.009	-0.009	-0.007
	(0.005)	(0.008)	(0.008)	(0.005)	(0.007)	(0.009)
Number of Individuals	6996	2371	4625	6970	2358	4612

Notes: The table reports the marginal effects of state salary variables on the probability of a beginning teacher *i* who graduated from college in state *j* choosing to teach in state *k*. The salary variables are measured for each potential destination state, including the college state. The marginal effects are computed around the mean probability of choosing a state. The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. As salary mandate data is not available for the District of Columbia, teachers who graduated from or currently teach in DC have been omitted from specifications that include the law dummy, and each teacher in these specifications faces a choice of fifty states. Additional controls include a nonmigration dummy, a quadratic in distance, enrollment, fraction minority students, fraction of students eligible for free or reduced-price lunch, twelfth-grade graduate rate, fraction urban, fraction of districts with collective bargaining, unemployment rate, the interaction of each of these state characteristics with each teacher attribute, and state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table 3.6. Marginal Effects of State Characteristics on the Choice of Initial Teaching State for Secondary School Teachers

	No Interactions	Interactions with DemographicAttributes	Interactions with All Attributes
	(1)	(2)	(3)
Salary (per $10,000)	-0.004	-0.008	-0.007
	(0.009)	(0.009)	(0.009)
Salary X Male		0.004	0.003
		(0.005)	(0.005)
Salary X Black		0.0001	-0.001
		(0.013)	(0.013)
Salary X Hispanic		0.016	0.013
		(0.014)	(0.015)
Salary X Age 30+		0.005	0.004
		(0.005)	(0.005)
Salary X High Select			-0.008
			(0.007)
Salary X Master's			0.011
			(0.006)
Nonmigration	0.059	0.055	0.054
	(0.003)	(0.003)	(0.004)
Nonmig X Male		0.005	0.005
		(0.004)	(0.003)
Nonmig X Black		0.008	0.008
		(0.008)	(0.007)
Nonmig X Hispanic		0.025	0.024
		(0.013)	(0.013)
Nonmig X Age 30+		0.004	0.004
		(0.004)	(0.004)
Nonmig X High Select			-0.001
			(0.005)
Nonmig X Master's			-0.004
			(0.006)
Distance (per 1,000 miles)	-0.049	-0.053	-0.058
	(0.006)	(0.006)	(0.007)
Dist X Male		0.008	0.008
		(0.003)	(0.003)
Dist X Black		0.003	0.004
		(0.006)	(0.006)
Dist X Hispanic		0.010	0.010
		(0.010)	(0.010)
Dist X Age 30+		-0.002	0.001
		(0.004)	(0.004)
Dist X High Select			0.018
			(0.005)
Dist X Master's			0.001
			(0.006)
Enrollment	0.027	0.025	0.023
(per 1,000,000 students)	(0.016)	(0.016)	(0.015)
Enroll X Male		0.001	0.001
		(0.001)	(0.001)
Enroll X Black		0.004	0.004
		(0.003)	(0.003)

(Table 3.6. continued)

Enroll X Hispanic		0.003	0.003
		(0.003)	(0.003)
Enroll X Age 30+		-0.002	-0.002
		(0.002)	(0.002)
Enroll X High Select			0.004
			(0.002)
Enroll X Master's			-0.002
			(0.003)
Fraction Minority	-0.092	-0.106	-0.112
	(0.046)	(0.046)	(0.046)
Minority X Male		0.014	0.016
		(0.014)	(0.014)
Minority X Black		0.049	0.048
		(0.027)	(0.028)
Minority X Hispanic		0.018	0.025
		(0.030)	(0.031)
Minority X Age 30+		0.007	0.007
		(0.015)	(0.015)
Minority X High Select			0.003
			(0.020)
Minority X Master's			0.022
			(0.025)
Fraction Lunch	-0.011	-0.015	-0.007
	(0.019)	(0.022)	(0.022)
Lunch X Male		0.001	-0.001
		(0.016)	(0.016)
Lunch X Black		0.033	0.034
		(0.035)	(0.035)
Lunch X Hispanic		0.013	0.008
		(0.040)	(0.041)
Lunch X Age 30+		0.006	0.006
		(0.019)	(0.019)
Lunch X High Select			-0.028
			(0.021)
Lunch X Master's			-0.040
			(0.030)
Graduation Rate	-0.001	-0.029	-0.034
	(0.038)	(0.039)	(0.040)
Grad Rate X Male		0.065	0.064
		(0.020)	(0.020)
Grad Rate X Black		-0.037	-0.041
		(0.046)	(0.045)
Grad Rate X Hispanic		0.031	0.017
		(0.051)	(0.051)
Grad Rate X Age 30+		0.001	0.002
		(0.034)	(0.033)
Grad Rate X High Select			-0.017
			(0.030)
Grad Rate X Master's			0.026
			(0.038)

(Table 3.6. continued)

Fraction Urban	0.013	0.007	0.008
	(0.023)	(0.022)	(0.023)
Urban X Male		0.022	0.024
		(0.013)	(0.014)
Urban X Black		0.009	0.005
		(0.022)	(0.021)
Urban X Hispanic		-0.010	-0.012
		(0.035)	(0.036)
Urban X Age 30+		-0.011	-0.014
		(0.013)	(0.014)
Urban X High Select			-0.023
			(0.019)
Urban X Master's			0.020
			(0.018)
Fraction Collective Bargaining	0.035	0.030	0.030
	(0.039)	(0.038)	(0.039)
Coll Barg X Male		0.006	0.006
		(0.005)	(0.005)
Coll Barg X Black		0.016	0.015
		(0.011)	(0.011)
Coll Barg X Hispanic		0.019	0.021
		(0.012)	(0.012)
Coll Barg X Age 30+		-0.0002	-0.001
		(0.006)	(0.006)
Coll Barg X High Select			0.002
			(0.006)
Coll Barg X Master's			0.001
			(0.007)
Unemployment Rate	-0.002	-0.002	-0.0004
	(0.002)	(0.002)	(0.002)
Unemp X Male		-0.002	-0.002
		(0.001)	(0.001)
Unemp X Black		-0.004	-0.005
		(0.003)	(0.003)
Unemp X Hispanic		0.002	0.001
		(0.003)	(0.003)
Unemp X Age 30+		0.001	0.0002
		(0.001)	(0.001)
Unemp X High Select			-0.005
			(0.002)
Unemp X Master's			-0.0001
			(0.003)
Number of Individuals	4427	4427	4427

Notes: The table reports the marginal effects of state characteristics on the probability of a beginning teacher *i* who graduated from college in state *j* choosing to teach in state *k*. State characteristics are measured for each potential destination state, including the college state. The values of distance and nonmigration for destination state *k* depend on college state *j* (see text for variable definitions). The marginal effects are computed around the mean probability of choosing a state. The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. All specifications include state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table 3.7. Marginal Effects of Average Salary/Salary Mandate (Law) on the Choice of Initial Teaching State for Secondary School Teachers

	Salary Variable: Average Starting Salary			Salary Variable: Law Dummy		
	No Interactions	Interactions with Demographic Attributes	Interactions with All Attributes	No Interactions	Interactions with Demographic Attributes	Interactions with All Attributes
	(1)	(2)	(3)	(4)	(5)	(6)
Salary/Law	-0.004	-0.008	-0.007	-0.004	-0.004	-0.003
	(0.009)	(0.009)	(0.009)	0.26	0.33	0.43
X Male		0.004	0.003		-0.0005	-0.001
		(0.005)	(0.005)		(0.003)	(0.003)
X Black		0.0001	-0.001		0.016	0.018
		(0.013)	(0.013)		(0.012)	(0.012)
X Hispanic		0.016	0.013		-0.009	-0.009
		(0.014)	(0.015)		(0.009)	(0.010)
X Age 30+		0.005	0.004		0.0002	0.0002
		(0.005)	(0.005)		(0.005)	(0.005)
X High Select			-0.008			0.002
			(0.007)			(0.006)
X Master's			0.011			-0.005
			(0.006)			(0.006)
Number of Individuals	4427	4427	4427	4424	4424	4424

Notes: The table reports the marginal effects of state salary variables on the probability of a beginning teacher *i* who graduated from college in state *j* choosing to teach in state *k*. The salary variables are measured for each potential destination state, including the college state. The marginal effects are computed around the mean probability of choosing a state. The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. As salary mandate data is not available for the District of Columbia, teachers who graduated from or currently teach in DC have been omitted from specifications that include the law dummy, and each teacher in these specifications faces a choice of fifty states. Additional controls include a nonmigration dummy, a quadratic in distance, enrollment, fraction minority students, fraction of students eligible for free or reduced-price lunch, twelfth-grade graduate rate, fraction urban, fraction of districts with collective bargaining, unemployment rate, the interaction of each of these state characteristics with each teacher attribute, and state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table 3.8. Marginal Effects of State Characteristics on Choice of Initial Teaching State by Degree Type for Secondary School Teachers

	All Teachers (1)	Teachers with Academic Degrees (2)	Teachers with Education Degrees (3)
Salary (per $10,000)	-0.007	-0.009	-0.006
	(0.009)	(0.012)	(0.011)
Salary X Male	0.003	0.012	-0.003
	(0.005)	(0.007)	(0.007)
Salary X Black	-0.001	-0.006	0.003
	(0.013)	(0.019)	(0.017)
Salary X Hispanic	0.013	0.006	0.018
	(0.015)	(0.017)	(0.024)
Salary X Age 30+	0.004	0.004	0.003
	(0.005)	(0.008)	(0.008)
Salary X High Select	-0.008	-0.014	0.007
	(0.007)	(0.008)	(0.014)
Salary X Master's	0.011	0.011	0.023
	(0.006)	(0.008)	(0.012)
Nonmigration	0.054	0.054	0.055
	(0.004)	(0.004)	(0.004)
Nonmig X Male	0.005	0.012	-0.001
	(0.003)	(0.006)	(0.005)
Nonmig X Black	0.008	0.013	0.006
	(0.007)	(0.012)	(0.011)
Nonmig X Hispanic	0.024	0.030	0.026
	(0.013)	(0.019)	(0.017)
Nonmig X Age 30+	0.004	0.005	0.004
	(0.004)	(0.006)	(0.005)
Nonmig X High Select	-0.001	-0.010	0.014
	(0.005)	(0.007)	(0.011)
Nonmig X Master's	-0.004	-0.003	-0.009
	(0.006)	(0.008)	(0.008)
Distance (per 1,000 miles)	-0.058	-0.060	-0.058
	(0.007)	(0.007)	(0.006)
Dist X Male	0.008	0.014	0.002
	(0.003)	(0.005)	(0.005)
Dist X Black	0.004	0.002	0.003
	(0.006)	(0.012)	(0.008)
Dist X Hispanic	0.010	0.016	0.003
	(0.010)	(0.013)	(0.013)
Dist X Age 30+	0.001	-0.001	0.002
	(0.004)	(0.006)	(0.006)
Dist X High Select	0.018	0.012	0.026
	(0.005)	(0.006)	(0.007)
Dist X Master's	0.001	0.004	-0.014
	(0.006)	(0.007)	(0.009)
Enrollment	0.023	0.038	0.006
(per 1,000,000 students)	(0.015)	(0.020)	(0.017)
Enroll X Male	0.001	0.002	-0.0001
	(0.001)	(0.002)	(0.002)
Enroll X Black	0.004	0.005	0.001
	(0.003)	(0.004)	(0.005)

(Table 3.8. continued)

Enroll X Hispanic	0.003	0.009	-0.002
	(0.003)	(0.004)	(0.003)
Enroll X Age 30+	-0.002	-0.004	-0.001
	(0.002)	(0.002)	(0.003)
Enroll X High Select	0.004	0.004	0.003
	(0.002)	(0.003)	(0.002)
Enroll X Master's	-0.002	-0.002	-0.006
	(0.003)	(0.003)	(0.004)
Fraction Minority	-0.112	-0.146	-0.084
	(0.046)	(0.055)	(0.056)
Minority X Male	0.016	-0.005	0.029
	(0.014)	(0.016)	(0.019)
Minority X Black	0.048	0.051	0.054
	(0.028)	(0.032)	(0.051)
Minority X Hispanic	0.025	-0.044	0.084
	(0.031)	(0.044)	(0.055)
Minority X Age 30+	0.007	0.026	-0.004
	(0.015)	(0.017)	(0.018)
Minority X High Select	0.003	0.002	-0.008
	(0.020)	(0.022)	(0.074)
Minority X Master's	0.022	0.009	0.028
	(0.025)	(0.025)	(0.029)
Fraction Lunch	-0.007	-0.007	-0.009
	(0.022)	(0.025)	(0.029)
Lunch X Male	-0.001	0.023	-0.017
	(0.016)	(0.019)	(0.023)
Lunch X Black	0.034	0.009	0.059
	(0.035)	(0.054)	(0.056)
Lunch X Hispanic	0.008	0.032	-0.014
	(0.041)	(0.046)	(0.062)
Lunch X Age 30+	0.006	-0.027	0.027
	(0.019)	(0.026)	(0.027)
Lunch X High Select	-0.028	-0.027	-0.031
	(0.021)	(0.027)	(0.056)
Lunch X Master's	-0.040	-0.059	0.006
	(0.030)	(0.032)	(0.046)
Graduation Rate	-0.034	-0.051	-0.010
	(0.040)	(0.047)	(0.049)
Grad Rate X Male	0.064	0.127	0.003
	(0.020)	(0.031)	(0.032)
Grad Rate X Black	-0.041	-0.052	-0.008
	(0.045)	(0.059)	(0.081)
Grad Rate X Hispanic	0.017	0.071	-0.032
	(0.051)	(0.074)	(0.072)
Grad Rate X Age 30+	0.002	-0.023	0.009
	(0.033)	(0.037)	(0.050)
Grad Rate X High Select	-0.017	-0.014	-0.040
	(0.030)	(0.033)	(0.056)
Grad Rate X Master's	0.026	0.027	0.028
	(0.038)	(0.045)	(0.065)

(Table 3.8. continued)

Fraction Urban	0.008	0.026	-0.017
	(0.023)	(0.028)	(0.029)
Urban X Male	0.024	0.029	0.020
	(0.014)	(0.019)	(0.018)
Urban X Black	0.005	-0.020	0.053
	(0.021)	(0.024)	(0.034)
Urban X Hispanic	-0.012	0.010	-0.010
	(0.036)	(0.052)	(0.043)
Urban X Age 30+	-0.014	-0.029	-0.004
	(0.014)	(0.022)	(0.017)
Urban X High Select	-0.023	-0.021	-0.017
	(0.019)	(0.023)	(0.042)
Urban X Master's	0.020	0.026	0.009
	(0.018)	(0.023)	(0.030)
Fraction Coll. Bargaining	0.030	0.044	0.014
	(0.039)	(0.041)	(0.049)
Coll Barg X Male	0.006	-0.001	0.013
	(0.005)	(0.007)	(0.006)
Coll Barg X Black	0.015	0.017	0.015
	(0.011)	(0.012)	(0.016)
Coll Barg X Hispanic	0.021	0.022	0.018
	(0.012)	(0.017)	(0.017)
Coll Barg X Age 30+	-0.001	-0.002	0.001
	(0.006)	(0.007)	(0.008)
Coll Barg X High Select	0.002	0.002	0.001
	(0.006)	(0.006)	(0.012)
Coll Barg X Master's	0.001	-0.003	0.007
	(0.007)	(0.008)	(0.011)
Unemployment Rate	-0.0004	-0.001	-0.0001
	(0.002)	(0.002)	(0.002)
Unemp X Male	-0.002	-0.003	-0.001
	(0.001)	(0.002)	(0.002)
Unemp X Black	-0.005	-0.002	-0.008
	(0.003)	(0.003)	(0.004)
Unemp X Hispanic	0.001	-0.0001	0.001
	(0.003)	(0.005)	(0.004)
Unemp X Age 30+	0.0002	0.001	-0.0004
	(0.001)	(0.002)	(0.002)
Unemp X High Select	-0.005	-0.002	-0.008
	(0.002)	(0.002)	(0.006)
Unemp X Master's	-0.0001	-0.0001	0.002
	(0.003)	(0.003)	(0.003)
Number of Individuals	4427	1927	2500

Notes: The table reports the marginal effects of state characteristics on the probability of a beginning teacher *i* who graduated from college in state *j* choosing to teach in state *k*. State characteristics are measured for each potential destination state, including the college state. The values of distance and nonmigration for destination state *k* depend on college state *j* (see text for variable definitions). The marginal effects are computed around the mean probability of choosing a state. The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. All specifications include state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table 3.9. Marginal Effects of Average Salary/Salary Mandate (Law) on the Choice of Initial Teaching State by Teacher Degree Type for Secondary School Teachers

	Salary Variable: Average Starting Salary			Salary Variable: Law Dummy		
	All Teachers	Teachers with Academic Degrees	Teachers with Education Degrees	All Teachers	Teachers with Academic Degrees	Teachers with Education Degrees
	(1)	(2)	(3)	(4)	(5)	(6)
Salary/Law	-0.007	-0.009	-0.006	-0.003	-0.003	-0.003
	(0.009)	(0.012)	(0.011)	(0.004)	(0.005)	(0.005)
X Male	0.003	0.012	-0.003	-0.001	0.0004	-0.003
	(0.005)	(0.007)	(0.007)	(0.003)	(0.004)	(0.005)
X Black	-0.001	-0.006	0.003	0.018	0.018	0.016
	(0.013)	(0.019)	(0.017)	(0.012)	(0.017)	(0.013)
X Hispanic	0.013	0.006	0.018	-0.009	0.004	-0.023
	(0.015)	(0.017)	(0.024)	(0.010)	(0.019)	(0.010)
X Age 30+	0.004	0.004	0.003	0.0002	0.001	-0.0001
	(0.005)	(0.008)	(0.008)	(0.005)	(0.006)	(0.005)
X High Select	-0.008	-0.014	0.007	0.002	0.005	-0.006
	(0.007)	(0.008)	(0.014)	(0.006)	(0.006)	(0.012)
X Master's	0.011	0.011	0.023	-0.005	-0.009	0.004
	(0.006)	(0.008)	(0.012)	(0.006)	(0.008)	(0.010)
Number of Individuals	4427	1927	2500	4424	1925	2499

Notes: The table reports the marginal effects of state salary variables on the probability of a beginning teacher *i* who graduated from college in state *j* choosing to teach in state *k*. The salary variables are measured for each potential destination state, including the college state. The marginal effects are computed around the mean probability of choosing a state. The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. As salary mandate data is not available for the District of Columbia, teachers who graduated from or currently teach in DC have been omitted from specifications that include the law dummy, and each teacher in these specifications faces a choice of fifty states. Additional controls include a nonmigration dummy, a quadratic in distance, enrollment, fraction minority students, fraction of students eligible for free or reduced-price lunch, twelfth-grade graduate rate, fraction urban, fraction of districts with collective bargaining, unemployment rate, the interaction of each of these state characteristics with each teacher attribute, and state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table A.1. Conditional Logit Model Estimates of the Choice of Initial Teaching State with Average Salary

	No Interactions	Interactions with Demographic Attributes	Interactions with All Attributes
	(1)	(2)	(3)
Salary (per $10,000)	0.006	-0.077	0.086
	(0.374)	(0.378)	(0.366)
Salary X Male		0.054	0.039
		(0.208)	(0.211)
Salary X Black		0.211	0.176
		(0.561)	(0.555)
Salary X Hispanic		0.250	0.245
		(0.577)	(0.562)
Salary X Age 30+		0.193	0.087
		(0.244)	(0.234)
Salary X High Select			-0.797
			(0.326)
Salary X Master's			0.281
			(0.284)
Nonmigration	3.217	3.117	3.100
	(0.139)	(0.140)	(0.144)
Nonmig X Male		0.079	0.068
		(0.145)	(0.141)
Nonmig X Black		0.158	0.169
		(0.320)	(0.312)
Nonmig X Hispanic		0.733	0.723
		(0.452)	(0.455)
Nonmig X Age 30+		0.171	0.183
		(0.184)	(0.181)
Nonmig X High Select			-0.249
			(0.214)
Nonmig X Master's			-0.166
			(0.255)
Distance (per 1,000 miles)	-3.689	-3.762	-4.099
	(0.302)	(0.313)	(0.315)
Dist X Male		0.281	0.297
		(0.256)	(0.250)
Dist X Black		0.044	0.100
		(0.756)	(0.752)
Dist X Hispanic		1.557	1.591
		(0.784)	(0.779)
Dist X Age 30+		-0.343	-0.163
		(0.350)	(0.349)
Dist X High Select			1.431
			(0.327)
Dist X Master's			0.132
			(0.414)
Distance^2 (per 1,000^2 miles^2)	0.463	0.483	0.548
	(0.113)	(0.119)	(0.114)
Dist^2 X Male		-0.034	-0.038
		(0.060)	(0.060)
Dist^2 X Black		-0.128	-0.140
		(0.215)	(0.216)

(Table A.1. continued)

Dist^2 X Hispanic		-0.580	-0.604
		(0.243)	(0.255)
Dist^2 X Age 30+		0.042	0.007
		(0.084)	(0.085)
Dist^2 X High Select			-0.295
			(0.078)
Dist^2 X Master's			-0.030
			(0.094)
Enrollment	1.068	1.055	1.014
(per 1,000,000 students)	(0.640)	(0.643)	(0.626)
Enroll X Male		0.059	0.066
		(0.058)	(0.059)
Enroll X Black		0.108	0.125
		(0.148)	(0.150)
Enroll X Hispanic		0.146	0.161
		(0.105)	(0.101)
Enroll X Age 30+		-0.130	-0.110
		(0.069)	(0.066)
Enroll X High Select			0.215
			(0.076)
Enroll X Master's			-0.005
			(0.105)
Fraction Minority	-3.953	-4.263	-4.550
	(2.034)	(2.033)	(2.065)
Minority X Male		-0.206	-0.142
		(0.513)	(0.518)
Minority X Black		1.540	1.604
		(0.984)	(1.012)
Minority X Hispanic		0.106	0.246
		(1.295)	(1.314)
Minority X Age 30+		0.382	0.423
		(0.534)	(0.529)
Minority X High Select			0.506
			(0.799)
Minority X Master's			0.188
			(0.757)
Fraction Lunch	-0.924	-1.448	-1.084
	(0.809)	(0.854)	(0.903)
Lunch X Male		0.623	0.528
		(0.543)	(0.540)
Lunch X Black		2.524	2.545
		(1.350)	(1.346)
Lunch X Hispanic		1.071	0.973
		(1.434)	(1.423)
Lunch X Age 30+		0.312	0.335
		(0.835)	(0.815)
Lunch X High Select			-0.805
			(0.801)
Lunch X Master's			-2.263
			(1.111)

(Table A.1. continued)

Graduation Rate	1.305	0.812	1.000
	(1.682)	(1.835)	(1.938)
Grad Rate X Male		1.501	1.472
		(0.935)	(0.949)
Grad Rate X Black		-2.987	-3.045
		(1.890)	(1.871)
Grad Rate X Hispanic		0.786	0.643
		(2.072)	(2.039)
Grad Rate X Age 30+		0.469	0.412
		(1.334)	(1.334)
Grad Rate X High Select			-1.104
			(1.466)
Grad Rate X Master's			-0.087
			(1.443)
Fraction Urban	0.681	0.571	0.514
	(0.992)	(0.982)	(1.003)
Urban X Male		0.720	0.670
		(0.516)	(0.508)
Urban X Black		2.363	2.436
		(0.827)	(0.827)
Urban X Hispanic		-0.667	-0.772
		(1.316)	(1.288)
Urban X Age 30+		-0.893	-0.834
		(0.538)	(0.548)
Urban X High Select			0.755
			(0.689)
Urban X Master's			-0.063
			(0.683)
Fraction Collective Bargaining	1.817	1.687	1.645
	(1.523)	(1.508)	(1.571)
Coll Barg X Male		0.151	0.147
		(0.194)	(0.198)
Coll Barg X Black		0.545	0.553
		(0.501)	(0.505)
Coll Barg X Hispanic		0.885	0.937
		(0.486)	(0.502)
Coll Barg X Age 30+		-0.101	-0.094
		(0.230)	(0.227)
Coll Barg X High Select			0.347
			(0.248)
Coll Barg X Master's			-0.165
			(0.289)
Unemployment Rate	-0.121	-0.126	-0.091
	(0.069)	(0.076)	(0.077)
Unemp X Male		-0.053	-0.059
		(0.043)	(0.044)
Unemp X Black		-0.128	-0.159
		(0.116)	(0.120)
Unemp X Hispanic		0.241	0.240
		(0.091)	(0.092)
Unemp X Age 30+		0.080	0.067
		(0.057)	(0.058)

(Table A.1. continued)

Unemp X High Select			-0.138
			(0.079)
Unemp X Master's			0.020
			(0.089)
Number of Individuals	6996	6996	6996

Notes: State characteristics are measured for each potential destination state, including the college state. The values of distance and nonmigration for destination state k depend on college state j (see text for variable definitions). The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. All specifications include state fixed effects. Standard errors are adjusted for correlation within college state by survey year.

Table A.2. Conditional Logit Model Estimates of the Choice of Initial Teaching State with Salary Mandate Dummy Variable (Law)

	No Interactions	Interactions with Demographic Attributes	Interactions with All Attributes
	(1)	(2)	(3)
Law	-0.078	-0.085	-0.033
	(0.142)	(0.159)	(0.163)
Law X Male		-0.182	-0.194
		(0.119)	(0.122)
Law X Black		0.530	0.598
		(0.466)	(0.465)
Law X Hispanic		-0.319	-0.320
		(0.353)	(0.370)
Law X Age 30+		0.297	0.278
		(0.246)	(0.242)
Law X High Select			-0.022
			(0.271)
Law X Master's			-0.478
			(0.285)
Nonmigration	3.199	3.111	3.099
	(0.140)	(0.140)	(0.144)
Nonmig X Male		0.073	0.066
		(0.145)	(0.142)
Nonmig X Black		0.169	0.180
		(0.331)	(0.327)
Nonmig X Hispanic		0.739	0.712
		(0.464)	(0.468)
Nonmig X Age 30+		0.169	0.171
		(0.175)	(0.173)
Nonmig X High Select			-0.277
			(0.200)
Nonmig X Master's			-0.184
			(0.256)
Distance (per 1,000 miles)	-3.727	-3.783	-4.095
	(0.307)	(0.316)	(0.320)
Dist X Male		0.274	0.288
		(0.264)	(0.261)
Dist X Black		-0.068	0.0002
		(0.746)	(0.749)
Dist X Hispanic		1.469	1.483
		(0.779)	(0.774)
Dist X Age 30+		-0.330	-0.176
		(0.337)	(0.339)
Dist X High Select			1.444
			(0.334)
Dist X Master's			0.051
			(0.422)
Dist2 X Master's			-0.026
			(0.096)
Distance^2 (per 1,000^2 miles^2)	0.470	0.486	0.539
	(0.115)	(0.118)	(0.115)
Dist^2 X Male		-0.032	-0.036
		(0.061)	(0.062)

(Table A.2. continued)

$Dist^2$ X Black		-0.050	-0.059
		(0.200)	(0.203)
$Dist^2$ X Hispanic		-0.554	-0.569
		(0.234)	(0.244)
$Dist^2$ X Age 30+		0.043	0.016
		(0.080)	(0.080)
$Dist^2$ X High Select			-0.268
			(0.078)
Enrollment	1.032	0.967	0.902
(per 1,000,000 students)	(0.637)	(0.636)	(0.614)
Enroll X Male		0.056	0.061
		(0.058)	(0.060)
Enroll X Black		0.274	0.304
		(0.125)	(0.126)
Enroll X Hispanic		0.134	0.156
		(0.107)	(0.105)
Enroll X Age 30+		-0.112	-0.087
		(0.071)	(0.067)
Enroll X High Select			0.272
			(0.082)
Enroll X Master's			-0.070
			(0.105)
Fraction Minority	-4.180	-4.437	-4.607
	(2.125)	(2.132)	(2.173)
Minority X Male		-0.199	-0.152
		(0.497)	(0.495)
Minority X Black		1.104	1.059
		(1.075)	(1.127)
Minority X Hispanic		0.458	0.559
		(1.300)	(1.339)
Minority X Age 30+		0.566	0.504
		(0.542)	(0.539)
Minority X High Select			-0.809
			(0.772)
Minority X Master's			0.612
			(0.739)
Fraction Lunch	-1.110	-1.513	-1.207
	(0.873)	(0.936)	(0.986)
Lunch X Male		0.487	0.426
		(0.553)	(0.553)
Lunch X Black		2.779	2.874
		(1.718)	(1.740)
Lunch X Hispanic		1.290	1.198
		(1.435)	(1.423)
Lunch X Age 30+		0.267	0.315
		(0.838)	(0.819)
Lunch X High Select			-0.762
			(0.787)
Lunch X Master's			-2.321
			(1.066)

(Table A.2. continued)

Graduation Rate	1.027	0.626	0.745
	(1.830)	(1.986)	(2.083)
Grad Rate X Male		1.233	1.185
		(0.964)	(0.963)
Grad Rate X Black		-3.161	-3.103
		(2.032)	(2.043)
Grad Rate X Hispanic		0.900	0.805
		(2.157)	(2.105)
Grad Rate X Age 30+		0.555	0.542
		(1.346)	(1.346)
Grad Rate X High Select			-1.374
			(1.414)
Grad Rate X Master's			-0.513
			(1.433)
Fraction Urban	0.470	0.462	0.499
	(0.977)	(1.008)	(1.034)
Urban X Male		0.615	0.606
		(0.594)	(0.603)
Urban X Black		0.126	0.092
		(1.285)	(1.293)
Urban X Hispanic		-0.346	-0.613
		(1.624)	(1.606)
		0.83	0.70
Urban X Age 30+		-0.923	-0.997
		(0.637)	(0.662)
Urban X High Select			-0.467
			(0.785)
Urban X Master's			0.822
			(0.848)
Fraction Collective Bargaining	1.568	1.412	1.452
	(1.612)	(1.607)	(1.672)
Coll Barg X Male		0.103	0.095
		(0.190)	(0.192)
Coll Barg X Black		0.754	0.745
		(0.441)	(0.445)
Coll Barg X Hispanic		0.993	1.023
		(0.540)	(0.554)
Coll Barg X Age 30+		0.096	0.054
		(0.266)	(0.265)
Coll Barg X High Select			-0.147
			(0.274)
Coll Barg X Master's			-0.231
			(0.317)
Unemployment Rate	-0.117	-0.125	-0.087
	(0.068)	(0.075)	(0.077)
Unemp X Male		-0.045	-0.052
		(0.042)	(0.042)
Unemp X Black		-0.206	-0.245
		(0.114)	(0.119)
Unemp X Hispanic		0.263	0.258
		(0.092)	(0.093)

(Table A.2. continued)

Unemp X Age 30+		0.082	0.064
		(0.053)	(0.054)
Unemp X High Select			-0.182
			(0.074)
Unemp X Master's			0.059
			(0.087)
Number of Individuals	6970	6970	6970

Notes: State characteristics are measured for each potential destination state, including the college state. The values of distance and nonmigration for destination state k depend on college state j (see text for variable definitions). The main effects of the teacher attributes drop out of the probability expression for the conditional logit model. As salary mandate data is not available for the District of Columbia, teachers who graduated from or currently teach in DC have been omitted from specifications that include the law dummy, and each teacher in these specifications faces a choice of fifty states. All specifications include state fixed effects. Standard errors are adjusted for correlation within college state by survey year.