THREE ESSAYS ON PUBLIC ECONOMICS:
TEACHER TRAINING, ACCOUNTABILITY, AND PUBLIC PENSIONS

By

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To my family

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LIST OF ABBREVIATIONS

AAEE	American Association for Employment in Education
AASCU	American Association of State Colleges and Universities
ARC	Alternative Routes to Certification
BEBR	Bureau of Economic and Business Research
Calpers	California Public Employees Retirement System
Calsters	California State Teachers Retirement System
CCD	Common Core of Data
CPRE	Consortium for Policy Research in Education
CRR	Center for Retirement Research
CRRA	Constant Relative Risk Aversion
DB	Defined Benefit
DC	Defined Contribution
ERR	Expected Rate of Return
LPS	Low-Performing Schools
MSA	Metropolitan Statistical Area
NCAC	National Center for Alternative Certification
NCEI	National Center for Education Information
NCES	National Center for Educational Statistics
NCTAF	National Commission on Teaching and America's Future
NYC	New York City
NYCTF	New York City Teaching Fellows
OLS	Ordinary Least Squares
RF	Regular Fields
SASS	Schools and Staffing Survey

SF	Shortages Fields
TAAS	Texas Assessment of Academic Skills
TFA	Teach for America
TFS	Teachers Follow-up Survey
TRC	Traditional Routes to Certification

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This dissertation analyzes three different public economic policy issues. The second and third chapters investigate implications of economic policies affecting the teacher labor market, and in particular their effect on teacher shortages in United States. In the second chapter, I analyze the relative efficiency of alternative paths to teacher certification compared to traditional programs. I estimate turnover rates for novice teachers who came to teaching through alternative and traditional preparation routes. Differences in attrition rates for graduates of alternative and traditional programs appear only for novice teachers who have 1 year of experience or less. As soon as a teacher gains 2 years of experience, the effect fades out. Using estimated attrition rates in a simulation model, I estimate the upper bound of alternative programs training costs per teacher that makes the efficiency of alternative programs at least equal to that of traditional ones.

In the third chapter, I examine whether and how different types of school accountability affect the supply of teachers. I find no evidence that student-targeted accountability policies affect the likelihood that teachers will be certified to teach in their main assignment fields. School-level accountability policies do influence the teacher shortage, but only in large districts. District-level accountability policies affect teachers both in small and large districts. The effect

of accountability policies is more pronounced for high-stakes fields whose testing results are used for school evaluation. However, the impact of accountability policies varies across high-stakes fields as well: accountability policies tend to result in more certified teachers in mathematics while reducing their prevalence in English language arts.

In the fourth chapter, I analyze rates of return that public pension funds use to project their future assets. Using a theoretical model for an optimal investment strategy I discuss deviations from optimality caused by the influence of labor unions on the pension funds. Then, I empirically test whether labor unions do affect the investment strategy of public pension funds. The regression estimation results confirm that the pressure of labor unions that are interested in high rates of return may cause deviations from the optimal investment strategy. Public pension funds in states with strong unions tend to use riskier investment strategies, compatible with higher rates of return but at the cost of greater risk.

CHAPTER 1
INTRODUCTION

This dissertation focuses on three different public economic policy issues. In the next chapter I analyze the efficiency of alternative paths to teaching. For this purpose, I compare the turnover rates for novice teachers who came to teaching through alternative and traditional preparation routes. The analysis is based on a data set that pools nationally-representative teacher-level surveys from the 1999-00 and 2003-04 school years. Novice teachers who have less than 6 years of experience are assigned to 5 experience cohorts. Different assignment mechanisms for translating possibly ambiguous survey responses to cohorts are used to ensure the robustness of the results. The logistic and ordinary least squares (OLS) regressions results show that teachers who came to the profession through alternative routes are approximately 14-18% points more likely to quit their main assignment field after their first year than teachers who completed traditional certification programs. The difference in attrition rates for the participants of alternative and traditional programs is revealed only for teachers who have 1 year of experience or less. As soon as a teacher gains at least 2 years of experience, the effect fades out. After the third year, however, graduates of competitive undergraduate colleges who came to teaching through alternative routes are also approximately 11-19% more likely to quit their main assignment field than graduates of less competitive colleges. Using estimated attrition rates in a simulation model, I estimate the relative efficiency of alternative routes to certification as a solution to the teacher shortage problem. The simulation shows the level of alternative programs' training costs per teacher that equates the efficiency of alternative programs to traditional ones.

In the third chapter, I examine the effects of school accountability on the teacher labor market. Evaluating schools on the basis of student performance has been a popular education reform in the United States and abroad for over a decade. However, these policies are by no means identical in nature or potential effect: they vary in both their mechanisms and their targets for improvement. While all focus on student improvement, some of them may also increase or decrease a teachers' willingness to work as they may impose additional pressure on teachers, increase teachers' salaries, or improve or harm working conditions. Because the accountability systems treat subject fields differently, they may also affect teachers of various subjects differently. Using nationally-representative teacher-level data for the 1993-94 and 1999-00 school years, I estimate the impact of different forms of school accountability policies on the teachers labor market by comparing pre- and post-accountability teacher certification characteristics by type of schools and subject fields. I find no evidence that student-targeted policies affect the likelihood that a teacher will be certified to teach in his/her main assignment field. School-level accountability policies influence the teacher shortage, but only in large districts. District-level accountability policies affect teachers in both small and large districts. The effect of accountability policies is more pronounced for high-stake fields whose testing results are used for school evaluation. However, the impact of accountability policies varies across high-stake fields as well: an accountability policy tends to result in more certified teachers in mathematics while reducing their prevalence in English language arts.

In the forth chapter, I analyze the rates of return that public pension funds use to project their future assets. There are (at least) two points of view on this question. Some financial economists argue that the projected returns of public pension funds should be valued using rates of return on fixed income securities, while the actual practice is to use higher rates of return. I find similarities between this discussion and debates over the appropriate discount rate for evaluation of future costs caused by global warming. Using a tax-smoothing approach, I analyze deviations from optimal investment strategy and illustrate how labor unions may cause the directors of pension funds to choose riskier investment strategies. I test the hypothesis that labor unions influence investment strategies. The OLS estimation results confirm that strong labor unions appear to be associated with riskier investment strategies. Though not definitive, the results are compatible with the notion that public unions have pushed pension funds toward aggressive portfolios.

CHAPTER 2
TEACHER ATTRITION: ALTERNATIVE AND TRADITIONAL PATHWAYS TO TEACHING

Introduction

Over the last two decades, the school teachers' shortage in certain subjects has been considered a problem of high priority in the United States. The creation and quick expansion of alternative routes to certification (ARCs) was one of the numerous policies employed to address the shortage problems. The effectiveness of these programs is currently an issue that causes debate. This chapter focuses on the analysis of the attrition of novice teachers who came to the profession through ARCs. Empirical results are used to estimate the relative efficiency of these programs as compared to traditional routes to certification (TRC). Before proceeding with the analysis of teachers' attrition, it is important to understand the specific characteristics of the teacher shortages in United States.

Though a near crisis of the teachers supply in United States has often been predicted since the 1980s, a severe shortage has not yet come to pass. Between 1999 and 2001, the number of teachers in elementary and secondary public schools increased more than student enrollment. According to the American Association of State Colleges and Universities (AASCU, 2005), currently there is no deficit of teachers in United States, though there exists a misalignment of supply and demand of teachers across geographic and subject areas.

Murphy, DeArmond, and Guin (2003) estimated the late-fill ratio for the 1999-00 school year and found significant variation across the country. Nine western, southwestern, and southeastern states experienced a significantly higher late-fill rate (2.3%-5.9%) than the national average (1.5%). A nation-wide study of the demand and supply of teachers conducted by the American Association for Employment in Education (AAEE, 2004) provides estimates of the relative demand of teachers by field. It reveals a significant imbalance between the supply and demand of teachers in several particular subject areas. Fields experiencing considerable shortages include special education, physics and mathematics. There are also some shortages in other sciences, bilingual education, English as a second language, Spanish language, and technology education. Other subject fields have a relatively balanced supply and demand, and there even exists some surplus in elementary education and social sciences.

One cause of the deficit of teachers is the high attrition rate of novice teachers, which in shortage fields interacts with the insufficient production of new teachers. Only 50-60% of teachers remain in the profession 5 years after entering (AASCU, 2005). According to the National Center for Education Information (NCEI, 2005) in 2005, K-12 teachers who were 50 years of age or older constituted 42% of the total number of teachers, and about 40% of current teachers were not expected to be working 5 years later. The National Commission on Teaching and America's Future (NCTAF, 2008) reports that in 2008-09, in 19 states more than 50% of their teachers were older than 50. Hence while today the shortages problem is relatively moderate, the majority of their positions are filled with older teachers. The retirement peak will be reached during the 2010-2011 school year. In less than a decade more than 50% of all veteran teachers will leave the profession. In addition attrition rates of beginning teachers have been increasing for more than a decade. According to the National Center for Educational Statistics (NCES, 2007) during the 2004-05 school year, the annual attrition rate for public school teachers with 1-5 years of full-time experience was approximately 8%, and for teachers without full-time experience it was as high as 19%.

Another important aspect to consider is the quality of teachers. While schools might not be experiencing problems with filling positions, they still might face difficulties in finding experienced and qualified teachers. Baker and Smith (1997) found that the percentage of teachers who are not certified in field they teach has been increasing over time. Ingersoll (1997) notes that many schools report difficulties in finding qualified teachers, rather than in just filling positions. However, it is difficult to estimate the shortage in terms of quality, since there is no way to measure it directly. Quality depends on many different factors such as experience, the teacher's undergraduate college, correspondence of the teacher's main assignment field to his major in college, certification type etc.

Hence the main characteristics of the current shortage of teachers in United States are:

- considerable shortage in special education, mathematics and sciences
- high imbalance between supply and demand of teachers in western, southwestern and southeastern states
- high attrition of novice teachers
- shortages of qualified teachers

Why are these problems unable to be resolved by the market? Why is the teacher labor market unable to achieve equilibrium through a series of wage corrections? The inbalance between teachers supply and demand across fields and areas originates from a system of equal pay for all teachers of given seniority and degrees, independent of the field they work in. Moreover, there exist various entry barriers that reduce the mobility of teachers.

The supply of teachers is a direct function of the relative wage. If the reservation wage of the prospective teacher is lower than the ratio of a teacher's wage to the alternative occupation wage, than he chooses to enter the teaching profession. The opportunity costs of teaching vary across different subject fields: people specializing in mathematics and sciences are able to get higher compensation in non-educational jobs than those who specialize in the humanities. However the current system of teacher compensation creates a uniform salary schedule that does not allow for differences in opportunity costs. Proposals to implement a differential wage system with merit-based and field-based compensation face the opposition of strong professional unions. As a result, shortages of teachers occur in fields that have higher opportunity costs.

The current certification and compensation system also creates barriers for teachers' mobility across regions. Many states do not automatically confirm certificates that were issued in other states. Therefore, teachers cannot freely move into areas with shortages because they have to take additional courses and exams to get certified in the new location. Another barrier for relocation is the system by which salary rises with seniority. If teachers move to other districts, they usually lose credits for seniority and thus are paid lower salaries at the new jobs. These barriers exacerbate the problem of misalignment across regions.

Since the 1980s, various policies have been implemented to combat teacher shortages, including offering ARCs. During the last two decades, ARCs have rapidly spread across the country. The important question is whether these programs represent an effective way to increase the supply of teachers, and do they specifically address teacher shortages? There are arguments both in favor of and against alternative certification. Some features of the ARCs clearly do address current shortage issues. These programs were originally designed to lessen the problem of misalignment of teachers supply by geographical areas and subject fields.

> Most alternate route programs are created specifically to meet the demand for teachers in the areas where they are established. Seventy-one percent of providers of alternate route programs say their alternative programs serve students in a high-needs area (e.g. low socioeconomic area, high poverty level, high minority) school. An additional 27% say that they serve some students in high needs areas. Only 2% say their programs do not serve students in high needs areas (Feistrizer, 2005, p.63).

While ARCs do increase the supply of novice teachers, particularly in high-need areas, the quality of the teachers coming to the profession through these routes is a question that fosters a lot of discussion. On the one hand, according to the classification of ARC, the most common types of these routes "have been designed for the explicit purpose of attracting talented individuals who already have at least a bachelor degree in a field other than education into elementary and secondary school teaching" (Feistrizer, 2005, p.61). Ninety-eight percent of alternative programs have a bachelor's degree as an entry requirement (Feistrizer, 2005). Hence, ARCs attract highly educated individuals into teaching. Since the average competitiveness of the graduates from colleges of education is lower than that of graduates specializing in other major fields, ARCs are likely to attract more talented individuals into teaching than are TRCs. For those people who would like to change their profession in mid-career, ARCs provide an opportunity to enter teaching with relatively low entry costs. In a survey, 47% percent of the respondents who participated in alternative programs answered that they would not have become a teacher if an ARC had not been available. Only 22% answered that they would have completed a traditional program (Feistrizer, 2005).

In their turn, the opponents of the ARCs argue that they provide "under-prepared" teachers. ARC programs have reduced requirements for course work and experience prior to becoming a teacher. Ninety percent of ARCs participants work as full-time teachers before they complete the program (Feistrizer, 2005). Hence novice teachers coming to the profession through ARCs are less prepared to teach when they first time enter a class than are TRC teachers. However, this possible disparity in quality is not likely to persist as cohorts mature. Using a nonparametric investigation of experience Rivkin, Hanushek, and Kain (2005) found that experience effects are mostly concentrated in the first few years of teaching. Specifically, teachers in their first and, to a somewhat lesser extent, their second year tend to perform significantly worse in the classroom. Hence, when a teacher gains at least 2-3 years of experience, the type of preparation is not likely to matter any more and the difference in quality dissipates over time. This leads to the question regarding the length of teaching spells for the graduates of ARCs. Do they stay in teaching long enough to become as qualified as their colleagues who completed TRCs or do they leave the profession in few years? Opponents of ARCs argue that the costs of these programs exceed their benefits, since most of the teachers coming into the profession through ARC leave teaching during their first 3 years. If this is the case, students get under-prepared teachers who lack pedagogical skills and then these teachers leave the profession before they gain enough experience to become well-qualified for the job. The reasons for leaving may be different and could include being less devoted to teaching than those coming through TRCs or feeling “under-prepared” and ill-suited for the job.

Hence the relative efficiency of the ARC depends on the attrition rates of the graduates of these programs. Since the difference in preparation matters only for the novice teachers, it is important to know whether the retention rates for teachers coming through ARC are lower than for graduates of TRC. If graduates of alternative programs mostly leave teaching during their first years of working in elementary and secondary schools, then the relative efficiency of the ARC is low.

To answer this question this chapter focuses on the analysis of the attrition patterns of teachers coming to the profession through ARC and TRC. Teacher-level national representative data is used to estimate the exit rates for teachers belonging to the same experience cohort who obtained their teaching skill through TRC and ARC. The main research question of my study is whether the exit rate of ARC novice teachers from teaching in their main assignment fields is different than the corresponding exit rate of their colleagues that completed TRCs. The understanding of the attrition patterns of the participants and completers of ARCs will provide background information for the analysis of the effectiveness of these programs, which I will use at the end of this chapter to estimate relative efficiency of the ARCs.

Efficiency of Alternative Routes to Certification

As mentioned above, the shortages are linked to particular subject fields and geographical areas, not to the whole teachers' labor market. A major source of the teacher deficit is a high turnover rate of novice teachers. The market is prevented from solving these problems by the uniform salary schedule. Let us look at two segments of the market for novice teachers: the market for shortage field (SF) teachers and the market for regular field (RF) teachers. Teaching in the SF induces higher opportunity costs; hence teachers working there should be compensated with higher wages. For example, teachers working in math or sciences fields can find jobs with higher wages outside of education with greater ease than their colleagues specializing in humanities. Likewise, teachers working in schools with a high share of socially disadvantaged students might experience worse working conditions and they should be appropriately compensated. Therefore, the supply curve for SF teachers lies above the supply curve for RF teachers, since the higher wage is required in the SF to provide the same supply of teachers.

The most straight-forward way to solve the shortage problem is to increase wages for novice teachers. However, a predominant majority of teachers (70-80%) are working in the RF and the share of tenured teachers in the teachers labor force is higher than the share of novice teachers. Therefore, it is natural to assume that teacher labor unions first of all serve the interests of the RF tenured teachers. As a consequence, the current system of teachers pay favors experienced teachers. Salaries increases with years of experience more than in proportion to increases in productivity. The effect of a teacher's experience on student attainment was estimated by Hanushek, Kain, O'Brien, and Rivkin (2005). They found that only the transition from 1 to 2 years of experience has a statistically significant positive effect on the teacher's productivity. Hence, a teacher's productivity increases in the first years of teaching and then remains stable until retirement. Nevertheless, labor unions support the system of backloaded pay and are not interested in high wages for novice teachers. Therefore, the uniform wage for novice teachers is defined by the equilibrium of the RF segment of the teacher labor market.

Panel A of Figure 2-1 shows the situation of the district labor market for RF teachers. The demand is assumed to be perfectly inelastic, determined by two fixed parameters: the number of students and class size. The supply is elastic and the intersection of supply and demand determines the equilibrium number of teachers (N_{RF}) and equilibrium wage (w_{RF}). On panel B of Figure 2-1, one can see the corresponding equilibrium on the labor market for SF teachers. The SF supply curve lies above the RF supply curve, and the intersection of demand and supply occurs when the wage is equal to w_{SF}. However, because of the uniform salary schedule, the wage actually paid is the same for teachers working in SF and RF. Since the labor unions are dominated by the tenured RF teachers, the wage is fixed at w_{RF}. The equilibrium wage is too low to provide the demanded number of SF teachers and results in a shortage that is equal to $\Delta N_{SF}=N_{SF}-N_{SF}(w_{RF})$.

Theoretically, there are two possible solutions for the SF shortage problems: to increase the wage from w_{RF} to w_{SF} or to shift the supply of SF teachers to the right. Let us look at the results of the wage increase option. Due to the uniform salary schedule, it will lead to high marginal input costs of newly hired SF teachers because the salary will increase not only for SF teachers, but also for RF teachers. Assume that the wage has increased from w_{RF} to w_{SF} and now ΔN_{SF} teachers are hired in addition to $N_{SF}(w_{RF})$. Assume that the total number of teachers is N when the wage is fixed at w_{RF} level ($N=N_{RF}+N_{SF}(w_{RF})$). Then after the wage increases, the total number of teachers is $N+\Delta N_{SF}$ and new wage is w_{SF}. In this case, the marginal input cost of a newly hired SF teacher is

$$MIC\left(w_{RF} \rightarrow w_{SF}\right)=\left[w_{SF}+PV\left(w_{SF}\right)\right]+\Delta w \cdot \frac{N}{\Delta N_{SF}}, \tag{2-1}$$

where $MIC(w^*\rightarrow w_{SF})$ is the marginal input cost of the last SF teacher hired thanks to the wage increase. Δw is the difference between wages w_{SF} and w_{RF}, and $PV(w_{SF})$ is the present discounted value of future payments to a newly hired SF teacher (starting from his second year in teaching and up to his last year of work), assuming w_{SF} as the salary for all future periods. For simplicity, I assume that the present value of future payments to previously hired teachers (N) does not change. Hence, the marginal input costs can be divided into two main categories: costs related to the payments of newly hired SF teachers and costs related to the increase in pay to previously hired teachers.

Now let us look on another solution for the SF shortages: increasing the total supply of SF teachers. ARCs are one of the possible ways to increase the supply of SF teachers. These routes allow individuals in mid-career to enter the teaching profession. Thus, they attract to teaching people who would not otherwise consider this option since they already hold a bachelor's degree in another field and do not want to enter a standard certification program. Since a majority of the ARC programs are restricted to shortage fields and areas, they typically increase the supply of SF teachers. Hence, creation of ARCs shifts the SF supply function to the right and thus decreases the shortages in the SF without changing the wage. Figure 2-2 shows the new equilibrium at the SF segment of the teachers' labor market after the introduction of ARCs.

In this case the marginal input costs of the last hired SF teacher is

$$MIC_{ARC} = \left[w_{RF} + PV\left(w_{RF}\right)\right] + C_{ARC}, \tag{2-2}$$

where C_{ARC} is the average cost of training 1 ARC teacher. Again marginal costs can be divided into two categories: costs related to the salary payments of newly hired teachers and costs related to the production of newly hired teachers (C_{ARC}). Since $w_{RF} < w_{SF}$, the first component of the MIC_{ARC} is always lower than the first component of $MIC(w^*\rightarrow w_{SF})$

$$w_{RF} + PV\left(w_{RF}\right) < w_{SF} + PV\left(w_{SF}\right). \tag{2-3}$$

Hence, $MIC_{ARC} < MIC(w^* \rightarrow w_{SF})$ if ARC training costs for ΔN_{SF} are not greater than the increase in pay to previously hired teachers (N) due to wage growth Δw.

$$MIC_{ARC} < MIC\left(w^* \rightarrow w_{SF}\right) \quad \text{if} \quad C_{ARC} \leq \Delta w \cdot \frac{N}{\Delta N_{SF}}. \tag{2-4}$$

Figure 2-3 shows marginal costs of both solutions to the shortage problem. The red line represents $MIC(w^* \rightarrow w_{SF})$ and the blue line corresponds to MIC_{ARC}, assuming that $C_{ARC}=0$. The difference between the red and blue lines illustrates the upper bound for the ARC average training costs $\bar{C}_{ARC}$ that provides equal efficiency for ARC programs and the wage increase approach. Given a uniform salary constraint, the ARC is a more efficient way to decrease shortages than a wage increase provided ARC training costs per teacher do not exceed the gap between the red and blue lines.

This model is simplified and does not account for differences in attrition rates of ARC novice teachers and teachers who came to the profession through TRCs or for differences in productivity between experienced and non-experienced teachers. So I need to account for these differences and to include them into the model. Assume that the attrition rate after the first year is higher for ARC teachers than for TRC teachers. This difference affects the MIC_{ARC} in two ways. First, in order to provide ΔN_{SF} teachers the ARC has to produce $\Delta N_{SF}/R_{ARC}$ teachers, where R_{ARC} is retention rate for ARC teachers after the first year. Similarly, TRC have to produce $\Delta N_{SF}/R_{TRC}$ teachers where R_{TRC} is the retention rate for TRC teachers after the first year. Since $R_{ARC} < R_{TRC}$, ARCs have to produce more novice teachers than TRCs to provide ΔN_{SF} after the first year of teaching. Second, new teachers are less productive than experienced teachers, and hiring the ARC teachers instead of TRC teachers causes a loss in productivity, because the higher attrition of ARC teachers results in hiring more ARC novice teachers to provide ΔN_{SF}, than in case of higher wages. MIC_{ARC}^{Attr} accounts for these additional costs

$$MIC_{ARC}^{Attr} = \left[\frac{w_{RF}}{R_{ARC}} + PV\left(w_{RF}\right) \right] + \left[\frac{C_{ARC}}{R_{ARC}} + \frac{\alpha \cdot MPL}{R_{ARC}} \right], \tag{2-5}$$

where α is a coefficient of relative productivity loss induced by hiring a novice teacher ($0<\alpha<1$), and MPL is the marginal product of labor for an experienced teacher. Introducing the first-year attrition rate to the model also changes marginal costs in the case of the higher wages approach

$$MIC\left(w_{RF} \rightarrow w_{SF}\right) = \left[\frac{w_{SF}}{R_{TRC}} + PV\left(w_{SF}\right) \right] + \left[\Delta w \cdot \frac{N}{\Delta N_{SF}} + \frac{\alpha \cdot MPL}{R_{TRC}} \right]. \tag{2-6}$$

When corrections for productivity losses and different attrition rates are added to the model, the gap between the red and blue lines becomes smaller, because the share of inexperienced teachers is larger with ARCs.

Now assume that the productivity of an ARC novice teacher is lower than the productivity of a TRC novice teacher, since ARC teachers begin to teach before they complete a certification program. In this case, loss of productivity will be even higher if the shortage problem is solved by an ARC rather than by higher wages:

$$MIC_{ARC}^{Attr} = \left[\frac{w_{RF}}{R_{ARC}} + PV\left(w_{RF}\right) \right] + \left[\frac{C_{ARC}}{R_{ARC}} + \frac{\alpha \cdot \left(1+\beta\right) \cdot MPL}{R_{ARC}} \right], \tag{2-7}$$

where β is a coefficient of relative loss in the productivity induced by hiring a novice ARC teacher instead of a novice TRC teacher ($0<\beta<1$). The additional loss in productivity also decreases the gap between red and blue lines, thus making ARCs relatively less efficient. In the subsequent sections I proceed with an empirical analysis of the attrition rates of ARC and TRC teachers that allows me to estimate empirically R_{ARC} and R_{TRC}. Then, at the end of this chapter I apply empirically estimated R_{ARC} and R_{TRC} to a simple simulation model based on Equations 2-1, 2-2, 2-5, 2-6, and 2-7 to analyze the relative efficiency of ARCs as a solution to the shortage problem.

Previous Literature

It is important to remember that ARCs became an option in 1980, and until recently no statistically representative data sets describing characteristics and the length of teaching spells for graduates of these programs existed. Due to the lack of data only a few studies have addressed the question. Boe, Cook, and Sunderland (2007) analyze the attrition rates of full-time public school teachers using a nationally representative data set from the 2003-04 school year. Teachers prepared by ARCs with 1-3 years of experience exit teaching at a 12% rate, while those who came to the specialty through TRC exit at a 7% rate. For the cohort with 4-6 years of experience, the exit rate for teachers prepared by ARCs decreases to 3%, but for TRC teachers it increases to 9%. However, the authors do not consider these disparities to be significant.

Boe, Shin, and Cook (2008) explore how the intensity of different types of teacher preparation affects transferring between subject areas and exiting from teaching. Bivariate logistic regression is applied to the pooled sample of teachers with 1-5 years of experience. Extensive teacher preparation is defined as completing either 10 or more weeks of practice teaching or 5-9 weeks of practice teaching along with four common components of teacher preparation[1]. The results show that extensive pre-service preparation reduces the probability of exiting teaching but does not affect the transits between different subject fields.

Boyd, Grossman, Lankford, Wykoff, and Loeb (2006) reported the 2004-05 cumulative attrition rates of New York City (NYC) teachers by experience cohorts and by different pathways to teaching. They distinguish 6 pathways: college recommended, individual evaluation,

[1] Those components include: coursework in selecting and adapting instructional materials, coursework in educational psychology, observation of other classroom teaching, and received feedback on their teaching.

New York City Teaching Fellows (NYCTF), Teach for America (TFA), temporary license, and "other". The first two groups represent TRC, while NYCTF and TFA are two big ARC programs that are implemented at the city and at a national level respectively. The "other" category includes all teachers that do not fit into the 5 categories defined above and presumably could be considered as "other alternative routes to teaching". The turnover rate occurs to be substantially higher for the "other" category in the first year and for TFA in subsequent years. The main focus of their study is the effect of the program type on the student achievement. The results show that relatively small differences in student achievement can be attributed to preparation pathways, and these effects mainly exist only among first-year teachers. Typically, ARC teachers provide smaller gains in student achievements than TRC teachers at least during the first years of teaching. However, these differences are small in magnitude and the variation in effectiveness within the program is greater than the average difference between pathways.

Darling-Hammond, Holtzman, Gatlin, and Heilig (2005) use data for teachers from the 1996-2001 school years in the Houston Independent School District to analyze the relative efficiency of teachers by different types of preparation programs (including alternative certification group and TFA group). Teacher's participation in TFA program appear to have a positive effect on students' scores on the Texas Assessment of Academic Skills (TAAS) math test, but at the same time the negative effect of TFA on students' scores in SAT-9 and Aprenda (the test given to Spanish-speaking students) in mathematics and reading has been revealed. The effect varies across years and until 2001 it is mainly statistically insignificant. The negative effects on the student achievements were also found for the ARC teachers. In this study, experience was defined as a continuous variable, hence teachers with different types of preparation were not compared within experience cohorts. The results suggest that standard or regular certification is an important factor that increases the teachers efficiency and that negative effects of teacher's participation in the TFA program dissipates as soon as TFA recruits obtain certificates. However the benefits of this improvement in their teaching skills are relatively small since the majority of TFA participants leave teaching after their second or third year (time when they usually earn certificate).

Kane, Rockoff, and Staiger (2008) use logit regression to estimate the cumulative retention rates of NYC teachers from different preparation programs. The retention patterns for teachers coming trough TRC and for teachers coming through NYCTF program appears to be equivalent, while the retention rates for TFA teachers drop dramatically after the second year. This is not surprising since the recruits of TFA commit for only 2 years of teaching. Similarly to Boyd et al. (2006) only small impacts of the initial certification status of the teacher to student test performance is found, while large and persistent differences in teacher effectiveness are revealed within the groups of teachers who have the same level of experience and identical initial certification status.

Various studies of the effectiveness of the ARCs generally focus on the analysis of the impact of the type of preparation program on student achievements. Attrition patterns are rarely the main interest in these studies. There are, however, a number of papers analyzing teacher attrition that examine the determinants of exits from teaching and transfers between schools and districts, but do not distinguish different types of preparation programs. These empirical works mainly exploit either a discrete-choice or a duration empirical framework.

The recent studies in this research area have revealed that though teacher salaries and opportunity costs do affect transfers between districts, they have little influence on the decision to exit teaching (e.g., Imazeki, 2005; Hanushek, Kain, & Rivkin, 2004). Positive effects of the real teacher wages on the duration of teaching spells was also found by Murnane and Olson (1989), Dolton and van der Klaauw (1995), and Krieg (2004). Several studies found evidence of the impact of some teacher's characteristics (sex, age, marital status, interactions of sex and age, and interaction of sex and marriage status) on the attrition rates. For example, females are more likely to leave teaching than males (e.g., Imazeki, 2005; Stinebrickner, 1999; Murnane, & Olsen, 1989). Teacher attrition varies across subject areas and depends on the school environment. For example, teachers are more likely to leave schools with socially disadvantaged and low-achievement students (e.g., Krieg, 2004; Dolton, & van der Kalauw, 1995; Boyd et al., 2005) while elementary school teachers are less likely to quit teaching or change district than high school teachers (Murnane, & Olson, 1989; Imazeki, 2005).

My study focuses on the empirical analysis of the attrition rates of the novice teachers by preparation pathways and by experience cohorts. The advantage of using national-representative data allows me to analyze not only the effects of the big nation-wide ARC programs, like TFA or city-specific programs like NYCTF, but also to assess the average attrition rate for participants of various ARC across states that are pooled together. I assign all novice teachers to 1, 2, 3, 4 and 5 years experience cohorts. Using this approach, it is possible to compare attrition rates by the type of preparation program within the cohorts, as well as to compare attrition rates across cohorts for teachers coming through the same preparation route. Since previous studies of the effects of teacher's preparation on student achievement found evidence of quality disparity between ARC and TRC teachers only for the teachers with 1-2 years of experience, it is important to analyze the attrition of novice teachers by years of experience. Hence, instead of pooling teachers with 1-3 or 1-5 years of experience into 1 or 2 cohorts as previous works that used the same data set have done (Boe et al., 2007; Boe et. al., 2008), novice teachers are assigned to 5 separate cohorts by years of experience. I also use different criteria to assign teachers to alternative and traditional groups than Boe et al. (2007) and Boe et al. (2008). They focus analysis on the effect of the intensity of teacher preparation on teacher retention, defining three categories of preparation: extensive, some, and little. The definitions are based on the four common components of teacher preparation: coursework in selecting and adapting instructional materials, coursework in educational psychology, observation of other classroom teaching, and feedback on their own teaching. In my analysis I assign teachers to the alternative and traditional groups according to their answers about the type of certificate they hold and the type of program that they completed to earn their certificate. I also take into account the findings of the previous empirical studies of teacher attrition and control for the teachers, schools, and district characteristics that have been proven to be important determinants of a teacher's decision to leave the profession or to transfer to another school or district.

Description of Data

Data used in my study mainly comes from the Schools and Staffing Survey (SASS) and the Teachers Follow-up Survey (TFS) administered by NCES. Given the focus of my study, information on teachers' employment status for at least two consecutive years and information about types of their preparation programs was required. SASS and TFS are two interrelated surveys. TFS is administered the year after SASS, and tracks the career paths for about 10% of teachers who participated in the survey in the previous year. Thanks to this feature, it is possible to estimate turnover rates for participants of TFS. Both surveys are nationally-representative and are administered every 4-6 years in public and private schools. However, in this study I utilize data only for public schools teachers. About 40,000 public school teachers participate in SASS and respectively about 4,000 of them participate the following year in TFS. SASS includes several questionnaires: teacher, school, district, and principal and library media center. For the purposes of my study, I only used data coming from the teacher and school questionnaires which provide well-rounded information regarding the type of preparation program, educational attainment, experience, work assignments, compensation, economic and social background characteristics of teachers, and schools main attributes.

Districts size data are extracted from the Common Core of Data (CCD) also administered by NCES. This program annually collects fiscal and non-fiscal data for all public schools, public school districts and state education agencies in the United States[2]. Price indexes for Metropolitan Statistical Areas (MSAs) developed by the Bureau of Economic and Business Research (BEBR) of the University of Florida are used to adjust teachers' salaries across different regions and time periods (Dewey, 2005).

Definition of Experience Cohorts

Based on the answers of SASS participants to the questions concerning their professional preparation, two main groups of teachers are defined: "traditional" and "alternative." In 1999-00, SASS teachers were asked about the type of certificate they hold in their main assignment field. They had 5 possible options: regular or standard, probationary, provisional, temporary, and emergency. Teachers that reported they hold probationary, temporary and emergency certificates are considered neither alternative nor traditional and are dropped from the sample. Therefore, the sample includes only teachers who chose the "regular or standard" or "provisional" categories. Those teachers who chose "regular or standard" were also asked about the type of program that they had completed to earn their certificate. Hence, the definition of alternative and traditional groups is based on the responses of teachers to the survey questions. The accuracy of the definition thus depends on how big the measurement and response errors are. Ballou (1998)

[2] This information is also provided in district questionnaires of SASS, however due to relatively large number of missing observations I decided to extract these data from CCD rather than SASS since it was more complete.

argues that respondents of the 1993-94 SASS were highly inaccurate in their answers concerning certification type. First, re-interview studies conducted by the NCES found a high degree of response errors in questions dealing with certification, thus indicating the ambiguity of the questions for the teachers. Second, a significant number of respondents from 8 states, that had not implemented ARC programs by 1993, answered that they hold an alternative certificate. Third, the definition of the "provisional" certificate might have been a source of confusion since in the survey it is described as being "given to persons who are still participating in what the state calls an "alternative certification program," while in many states traditional teacher certification proceeds through two or more stages, with the "provisional" certificate given at the first stage. Hence, there are two main sources of potential response errors: teachers are unsure what type of certificate they hold and the definition of the "provisional" certificate in the survey is ambiguous.

Fortunately, the structure of the 1999-00 and 2003-04 questionnaires improved and the first source of the problems was dealt with. The major difference is that in 1999-00 and 2003-04 alternative and advanced professional certificates are included in the "regular or standard state certificate" category, and in an additional question teachers are asked how the certificate or teaching skills were obtained. The respondent has to choose between 6 options: bachelor's program, master's program, "5th year" program, alternative program, professional development, and other. I assign teachers who earned a certificate as part of a bachelor's, or master's, or "5th year program" to the traditional group. Teachers who earned a degree as part of an alternative program, or through continuing professional development, or through some "other" program are assigned to the alternative group. Thus, even if teachers are not sure about the type of certificate they hold, they clarify this issue through responses to additional questions, describing the program they have completed. Also, in 1993-94 the ARCs were only in the early stages of development. In 1999-00, they were already widely developed across states and by 2003-04 ARCs contributed a significant share of novice teachers to the education system. As the size and impact of these programs increases, misunderstanding of what is meant by "alternative program" in the survey is likely to fall compared to the 1993-94 survey. Thus, the first type of possible response error is much less likely to occur in 2003-04 and 1999-00 than in 1993-94.

The second source of the problem, an ambiguity in the definition of the "provisional" category, is more difficult to deal with. For the 2003-2004 SASS the same technique to distinguish between alternative and traditional certificates can be used since all teachers holding provisional certificates also answer an additional question regarding how they obtained their teaching skills. Hence, teachers with provisional certificates are assigned to the alternative group only if they obtained skills through alternative programs, or through continuing professional development, or through "other" program. However, in 1999-00, SASS teachers holding provisional certificates were not asked this question.

To solve this problem for 1999-00 SASS data I exclude from the sample teachers who reported that they held a provisional certificate and who were working in states where in 1999-00 a provisional certificate was the first stage certificate in the TRC programs. I also exclude teachers who reported that they held a provisional certificate and who were from the states that had no ARCs at this time period[3]. In order to check whether response errors in the 1999-00 SASS drive our results, I also run estimations for the 2003-04 sample separately. While all main results hold for 2003-04 separately, in this study I report results for the pooled sample since there

[3] These states include: Colorado, Michigan, New York, Ohio, Virginia (provisional certificate as part of standard certification) and Alaska, California, Indiana, Kansas, Montana, Nebraska, Rhode Island, and Wisconsin (no ARC in 1999-00).

are few observations for teachers belonging to an alternative group. Pooling both periods together allowed me to increase the power of estimation.

The 2003-04 SASS has the same pattern of questions concerning certification, however in this survey teachers are asked about any certificates they have, while in 1999-00 SASS they are asked whether they hold a certificate in their main assignment field. They also answer questions about additional certificates they hold. To make the definition of the certification groups consistent across years, I have constructed the same groups for 2003-04 SASS as in 1999-00 SASS by combining information on certification with information about main teaching assignments. Participants in the survey report the code for the content area their certificate allows them to teach and the code of their main assignment field. I arrange all the codes in ten major "field groups": elementary education, English and language arts, mathematics, natural sciences, foreign languages, special education, English as a second language, social sciences, specific matter subject (health education, arts and music, philosophy and religious studies), and other (vocational, technical, other). The teacher is considered certified in the main assignment field if she holds a certificate within a content area code that belongs to the same field group as her main assignment field.

I also made additional corrections to this definition:

- If the teacher holds a certificate allowing him to teach in elementary school he is considered certified if he is working only in grades 1-6.
- If the teacher holds a certificate allowing him to teach in elementary school and he has positively answered the question: "You are an elementary school teacher who teaches only one subject."
- If the teacher holds a certificate that applies to elementary grades he is considered certified if he is working only in grades 1-6.

Figure 2-4 shows the ratio of teachers certified in a main assignment field in 1999-00 (as reported by teachers themselves) and 2003-04 (constructed using content area codes of certificate and main assignment field code). The self reported data has a slightly higher ratio of correspondence of the certification area to the main assignment field, except for elementary education and special education field groups. Most probably for these two groups, the matching of content areas in certificates to main assignment fields was the most accurate, as for elementary education I applied additional corrections mentioned above. Special education is a stand alone field which could hardly be counted as any other field. For other categories I might have missed some correspondences of certification areas to main assignment fields, but in further analysis I use only teachers who are certified in their main assignment field. Hence, those teachers who might have been wrongly counted as not certified are not included in that sample.

I also tried to add data from the 1993-94 SASS and the 1994-95 TFS to my analysis, but the structure of the questions concerning certification in the 1993-04 survey differs from 1999-00 and 2003-04. Instead of two questions, teachers answered only one in which they chose from seven different certificate definitions: advanced professional certificate, regular or standard, certificate offered to persons who have completed an “alternative certification program”, provisional, probationary, temporary and emergency. Hence, there is a risk that some teachers who reported that they had a regular certificate in 1993-94 actually held the same type of certificate as some teachers who reported in 1999-00 that they had a “regular or standard certificate” earned through an alternative program and were thus assigned to the alternative group.

For my study, the 1993-94 SASS had another weak point—during that time ARCs were not widely developed. As Figure 2-5 shows, the number of teachers obtaining certification through ARCs has increased substantially since the late 1990s. Therefore, adding the 1993-94 data would not significantly increase the size of the alternative group, and since the system of

ARCs had just began to develop at that period it is not clear whether the definition for alternative programs in 1993-94 is consistent with the definition in 1999-00 and 2003-04.

Hence, the sample includes data for participants in both the SASS and TFS surveys in 1999-00 and 2003-04. I restricted data to full-time and part-time teachers holding a certificate in a main assignment field. However, the sample which I used in the analysis is even smaller due to additional restrictions based on the definition of the experience. I do not have time series data and cannot observe the duration of teachers' employment spells from the beginning to the end. I can only analyze the probability that teachers with different levels of experience leave the profession at one time point. Therefore, for the purpose of my study it is imperative to precisely define experience cohorts. Since I focus on the comparison of turnover rates of novice teachers with different level of experience, I need to unambiguously distinguish teachers with 1 year of experience from those with 2 or more years of experience. That further restricts the sample size by eliminating some observations of teachers with ambiguous experience levels.

The analysis focuses on the turnover rates of teachers with less than 6 years of experience and assigns them to 5 experience cohorts according to years of experience: 1 year cohort (includes only teachers who have 1 year of experience), 2 years, 3 years, 4 years, and 5 years respectively[4]. The SASS questionnaire includes several questions concerning teacher experience. First, respondents are asked "How many years have you worked as a full-time elementary or secondary teacher in public school?" This question is followed by "How many years have you worked as a part-time elementary or secondary teacher in public school?" Finally, both questions are asked with respect to private schools. In all these questions the teacher is supposed to report only whole years and to include the current year.

[4] I also did the same analysis with 10 experience cohorts for teachers with less than 11 years of experience. That yielded virtually the same results.

Part-time experience was considered as worthy as full-time, and experience in private schools as valuable as experience in public schools. Hence, it is natural to define total experience as the sum of full-time and part-time experience in public and private schools.

$$TE = FTE_{pub} + PTE_{pub} + FTE_{pr} + PTE_{pr}, \qquad (2\text{-}8)$$

where TE represents total experience and FTE and PTE − full-time and part-time experience respectively. However, by using this definition of total experience I risk overlooking a couple of pitfalls. First, teachers may work both full-time and part-time in the same school year. It would be preferable to count it as 1 year of total experience, rather than 2 independent years of experience. Second, teachers may have had a break in service at some point and then recently returned to teaching.

As my study focuses on the career paths of novice teachers with less than 6 years of experience, the primary interest is the comparison of turnover rates of teachers who recently entered teaching and had no breaks in their service. For example, it hardly makes sense to look for a difference in turnover rates for teachers with 1 year of experience and teachers with 2 years of experience who had a long break in the service and returned to teaching the same year as those entering the teaching profession. To avoid that sort of problem I used another piece of information concerning experience derived from the question “In what year did you begin teaching, either full-time or part-time, at the elementary or secondary level?” Using this additional information I constructed a matrix for each SASS survey, where columns show the years teachers reported as the first year of teaching, and rows show the total experience calculated using Equation 2-7.

Figure 2-6 represents a fragment of such a matrix for respondents to the 2003-04 SASS with 5 or less years of experience. Each cell shows the number of respondents who have reported their first year of teaching and TE that corresponds to this cell of the matrix. In this matrix I used a sample that includes only teachers with regular or alternative certification and for whom I have next year follow-up information (from 2004-05 TFS). It is natural to suppose that teachers whose answers lay on the diagonal of the matrix had no breaks in their service and correctly counted their years of experience. Teachers whose answers lie on the upper corner of the matrix may have had a break in their service.

However since there are relatively more respondents whose answers lie at the cells adjoining to the diagonal, one assumes that some of these respondents could have miscalculated their experience by not including the current year. For example, there is a possibility that some of the 33 respondents who reported their first year of teaching was in 2002 where TE is equal to 1 actually have not counted the current school year (2003-04). Hence, it is ambiguous to what cohort these respondents belong.

We also have some respondents in the lower corner of the matrix. While it is virtually impossible to enter teaching in the 2003-04 school year and to have 2 years of experience, 15 respondents reported such information. There are two possible explanations of these anomalies. First, as discussed above, some teachers may be working both as full-time and part-time teachers in the 2003-04 school year. In their case 1 year of experience was counted twice. This sort of mistake can be easily corrected by looking on the full-time experience of these teachers.

$$\mathrm{FTE} = \mathrm{FTE}_{\mathrm{pub}} + \mathrm{FTE}_{\mathrm{pr}}, \tag{2-9}$$

where FTE represents full-time experience of those working both in public and private schools.

The fifteen teachers mentioned above that have 1 year of FTE and 2 years of TE, most likely belong to the cohort with 1 year of experience (assuming they worked both full-time and part-time in the 2003-04 school year). For the rest of those who have 2 years of FTE, the most probable explanation is that they entered teaching in the Spring semester of the 2002-03 school year and literally their first year of teaching was 2003. Technically they have about 1.5 years of experience. Hence, it is ambiguous to what cohort these respondents belong − 1 or 2 years of experience. One should not exclude the possibility that some of these teachers actually entered teaching in the Fall semester of the 2003-04 school year, but might have counted calendar years (2003 and 2004), rather than school years (2003-04) thus causing discrepancies.

Diagonal definition. Taking all these possibilities into account, the simplest way to define experience is to use only the diagonal cells of the matrix. For example, define the 1 year experience cohort as teachers whose first year of teaching was in 2003 and who had 1 year of TE. In this case, one is secure from counting teachers with 1 year of experience as teacher with 2 years of experience etc. This definition will be referred to as "diagonal." Figure 2-7 illustrates the mechanism used in the diagonal definition for assigning teachers to the experience cohorts.

This definition discards observations lying outside the diagonal of the matrix. The main shortcoming of this approach is a sharp reduction of the sample size. While the whole sample is quite representative, there is a shortage of observations for ARC novice teachers in each experience cohort. That is why it is important to use as many observations as possible for teachers with less than 6 years of experience. The main sources for the additional observations are cells adjoining the diagonal. As discussed above, it is not always clear what cohort these observations belong to. That is why in this study 3 different definitions of the experience cohorts, including the diagonal, are used. Each definition treats the ambiguous observations on a case-by-case basis.

In these definitions, I focus mostly on the cells adjoined to the diagonal. I discard all observations lying in the lower corner of the matrix on the cells not adjoining to diagonal. It constitutes just a few observations, and most likely contains errors due to respondents' answers. In all definitions, except one, I ignore observations lying in the upper corner of the matrix which are not adjoining the diagonal because the primary interest of my study is to analyze the career paths of teachers who had no breaks in service. However, in order to increase the sample size as much as possible I exploit these observations in one definition of the experience cohorts. The definitions of experience cohorts are given more fully below:

Definition 1. This definition exploits all observations from the cells adjoining to the diagonal. The following example illustrates all the steps required to define the cohort with 2 years of experience. This cohort includes all observations lying in the corresponding cell on the diagonal of the matrix: teachers who have 2 years of TE and reported 2002 as their first year of teaching. Next, I look at the cells adjoining the diagonal cell. The lower corner includes some respondents with 2 years of TE and 2003 as their first year of teaching. For those respondents, I check their FTE and if it is equal to 1 I consider the respondent to have 1 year of experience. If the respondent has 2 years of FTE, I consider him to have 2 years of experience assuming that he entered teaching in the spring semester of the 2002-2003 school year. Figure 2-8 illustrates the logic used in definition 1 for assigning teachers to experience cohorts.

I use the same intuition for respondents who reported 3 years of TE and 2002 as their first year of teaching. They are considered to have 2 years of experience if their FTE is equal to 2 years, and they are counted in the 3 years of experience cohort if their FTE is equal to 3. Then I proceed with cells in the upper corner. If the respondents reported 1 year of TE and 2002 as their first year of teaching, and they are working as full-time or part time teachers in the 2003-04 school year, they are considered to have 2 years of experience assuming they did not include the current school year in their response. I use the same logic for all other cohorts.

Definition 2. This definition attempts to increase the sample size as much as possible and exploit all observations in the upper corner of the matrix. This cohort includes all observations lying in the corresponding cell on the diagonal of the matrix: teachers who have 2 years of TE and reported 2002 as their first year of teaching. For the cells from lower corner of the matrix that are adjoining the diagonal I use the same mechanism as in Definition 1. For all cells in the upper corner of the matrix, I assume that TE is reported correctly by respondents. Teachers with 2 years of TE whose first year of teaching was before 2002 had had a break in their service at some point, so they are considered to have 2 years of experience. Figure 2-9 illustrates the mechanism used in definition 2 for assigning teachers to experience cohorts.

The same logic applies for all cohorts except the cohort with 1 year of experience, because it is not possible to have a break between the 2002-03 and 2003-04 school years. The number of observations in each experience cohort varies across different definitions. The diagonal definition provides the smallest sample and definition 2 the largest sample. Table 2-1 shows the number of observations in each experience cohort for ARC and TRC novice teachers with less than 6 years experience. The data includes observations for teachers from 48 states (Hawaii and Alaska are excluded) and the District of Columbia. Some explanatory variables (school and district attributes) have missing observations and this slightly decreases the sample size. The total number of observations for the pooled data set consists of 5224 to 6020 observations depending on the definition of experience cohorts. Though this sample is quite representative, for the purpose of this study I compared turnover rates for teachers with different types of certification and various levels of experience. For example, I compared ARC teachers with 1 year of experience to regularly certified teachers with 1 year of experience. Looking at the distribution of teachers across years of experience I have a relatively small number of observations in each category. Hence I had to pool data from two surveys for my study. Separately, each survey does not provide enough observations.

Variables Included in the Model

Generally, teachers' attrition studies exploit several different definitions regarding teacher exits including exiting from teaching, transferring to another district, and transferring to another school. The choice of the most appropriate definition usually depends on the scope and the main research interest of the analysis. For example, for the analysis of different state and district policies, at the state level the transfers between districts and schools are of interest. At the national level they do not significantly affect the main picture. Since the primary interest of my study is the effectiveness of ARCs in solving the shortage problem at the national level, teachers are said to exit if they do not continue to teach in the same main assignment field the next year. In other words I analyze whether or not novice teachers continue to work in the main assignment field of their teaching certificate. Those who do not work in the same field the next year might represent several different types of exits including exiting from the education system, exiting from teaching to administrative work, or remaining a teacher but working in another subject field. I do not distinguish between these subcategories, since the shortage problem is an attribute of the particular subject field and I am interested in whether or not teachers who completed the ARC do in fact solve the shortage problem in the field they are certified to teach in. For example, if teachers completing the alternative program for special education usually work only a few years in the field, are they likely to change their main assignment field to elementary or vocational education? If this is the case the alternative program is not effective in solving the problem of the shortage of special education teachers.

Previous empirical studies of attrition suggest that several particular teacher characteristics should be included, such as race, gender, marital status, educational attainment, and subject field. Marital status might affect attrition because married teachers are more likely to move to another place because of spouse relocation. The effects of marital status might vary by gender since women are more likely to quit teaching due to personal or family reasons. For example, women may quit teaching for maternity leave or to care for children. To account for this effect, the interaction of gender and marital status is included in the regression.

As a measure of educational attainment a master's degree is used. Some regressions also include a dichotomous measure of college quality, which is based on an index of college quality. The index varies from 1 to 5, where 5 corresponds to the most competitive colleges and 1 to the least competitive. Different definitions for the college quality variable were explored, such as an index varying from 1 to 5, a set of four dummy variables, and different binary variable definitions. Finally, I settled on the dichotomous measure, because it provides the best fit for the model with scores 3 to 5 of the college quality index corresponding to more competitive colleges and scores 1 and 2 to less competitive colleges.

For subject fields a set of indicators shows whether a teacher works in mathematics, English language arts, natural sciences, social sciences, special education, or other subject fields[5], leaving as the base category those working in elementary education. These variables are defined according to the subject field group classifications discussed in the previous sections. I also indicate whether a teacher is a member of a professional union, since it is more difficult to fire a member of a union.

One of the main indicators of teacher quality is experience. The longer a person stays in the teaching profession the less likely he is to pursue another occupation, because he already has mastered this occupation while in another it would take him several years to become a professional. On the other hand, the longer a teacher stays in teaching the more likely he is to retire, while the annual increase in the skill of a teacher decreases with years of working. Hence,

[5] English as a Second Language is included into the "other subject fields" group since the turnover rates in this field group are closer to the "other field groups" than to the "English language arts" field group.

experience is likely to have a positive influence on a teachers' decision to stay in the profession, but this effect tends to dissipate over the years. To control for these factors, I include in the equations teachers' total experience and experience squared. Murnane and Olsen (1989) have found that individuals who start teaching before age 30 are more likely to quit teaching. I tried two different definitions of this indicator including a continuous variable showing the age they began teaching, and a dichotomous variable for whether the teacher entered the profession by age 30 or more. The first definition provided a better fit, so I chose it for the final model.

Another possible source of difference in turnover rates might be family income. A teacher who is the primary earner in the family is more likely to be sensitive to the wage and opportunity costs and may be looking for a job outside of teaching if it could provide him with a higher income. A secondary earner might be less interested in alternative occupations. Hence, I also include in some regressions a set of four dummy variables indicating what range of family income the teacher belongs to. As in case with the college quality variable, I tried different definitions of this variable and have chosen the one that provides the best fit for the model.

Since I pool together data from two different time periods (1999-00 and 2003-04), I add an indicator of school year into the regression equation in order to control for possible changes in the general economic environment and the relative prestige of the teaching profession. The teachers' individual data is supplemented by school and district characteristics. I control for the teachers' working environment by including the racial composition of students and teachers and the share of students eligible for free lunch as a proxy for the proportion of low-income students in the school. A few variables are included to describe the relative attractiveness and business activity of the area where the teacher works and the variety of job choices in a school education system in the vicinity. For these purposes, I indicate whether the school is in a rural area and include a measure of the relative size of the schools in the district: district enrollment divided by number of schools.

Several previous studies have investigated the effect of salaries and opportunity costs on teacher turnover. Imazeky (2005) found that a teacher's decision to quit the profession depends on the teacher's salary and its ratio to the average salary of teachers in the neighboring districts. Murnane and Olsen (1989) found a positive relation between the probability of quitting teaching and the average starting salary paid by business and industry to college graduates. Stinebrickner (1998) found that the length of the first teaching spell is responsive to the teacher's wage. To control for these factors I include the teacher's annual salary[6] in the regression equation, but since I am not particularly interested in the effect of wages on teacher attrition, I do not directly control for opportunity costs. However, I do account for these costs in two ways. First, I include state fixed effects into the model to catch regional differences. Second, I weight the salary of teachers by a comparable wage index. The idea behind this index is that in equilibrium the ratio of wages in all occupations between any two regions is the same and is defined by the difference in the prices and the differences in local amenities.

$$\frac{w_c^i}{w_0^i} = P_c \cdot \frac{1}{A_c}, \tag{2-10}$$

where w^i_c represents the wage in occupation i and city c, with $c=0$ denoting the reference city. The spatial cost of living index, based on the relative prices of the market-basket, is represented by P_c. A_c is a measure of the relative willingness to pay for the amenities of area c relative to the reference location 0, valued at area c prices, with higher values corresponding to preferred amenity bundles. Since in equilibrium this condition holds for all occupations, I am able to control for opportunity costs by weighting teacher salary with this comparable wage index.

[6] 2003-2004 school year salaries were deflated to the 1999 prices using CPI.

Weighted salaries are comparable across different locations. Hence, if teachers have a relatively high weighted salary they are "overcompensated" in their region compared to other occupations, and if they receive a relatively low weighted salary they are "undercompensated" and other occupations may be attractive to them. The comparable wage index is available for all MSAs. I used school zip codes to locate their latitude and longitude, and then each school was matched via geographical coordinates to the closest MSA. For those schools that are located far from any MSA, I used the average index for the state, which was found as sum of indexes for MSAs located in the corresponding state, weighted by their population.

My primary interest is in the effect of participation in an ARC on the novice teacher's decision to quit their main assignment field. I define two variables to control for certification. First, I include an indicator showing whether a teacher obtained his professional skills in the main assignment field by completing an ARC. The second certification variable controls for whether a teacher has a traditional certificate in any other field (not main assignment). It might be relatively easy and more attractive to change the main assignment field for those teachers who hold certificates in other fields. Because of the specifics of working in special education, I also include an interaction indicator of whether a teacher's main assignment field is special education and a variable showing whether the teacher holds a traditional certificate in another field.

Finally, a set of dummy variables for 5 experience cohorts of the novice teachers is included in the equation. Then, I interact cohort dummies with type of certification indicators − those are the variables of main research interest. I investigate whether teachers who came to the profession through ARCs are more likely to exit in their first years of working in the school education system compared to TRC teachers. Moreover, I am looking at the effects of ARC preparation separately for each experience cohort. For policy purposes it is important to know how quickly the effect of the program characteristics dissipates. Does experience eliminate the differences between ARC and TRC teachers, and how quickly do the differences disappear? In other words, I am interested in whether teachers who completed ARC are more likely to quit their main field only after the first year, or if they still have higher turnover in consequent years. If a statistically significant difference in turnover rates is observed only after the first year of teaching and not later, that leads to important policy implications. First, it would make it easier to predict the turnover rates of graduates of the ARC for cost-benefit analysis purposes. Second, it would imply that ARCs provide teachers of quality comparable to the graduates of TRCs. It also implies that even if ARC teachers are less qualified and less committed to the profession during their first year in the school education system, these discrepancies disappear by the second year. Summary statistics for the variables used in estimations are presented in Table A-1 of the Appendix A.

Empirical Strategy

The background for empirical estimation of teacher attrition is based on the utility model of individuals sorting across jobs. The preferences of teachers are reflected by utility functions:

$$U_{ik} = U\left(w_k, Z_k \middle| X_i\right), \tag{2-11}$$

Where w_k is the wage of the alternative job k, Z_k represents non-pecuniary job attributes (working conditions, geographical location, district and school characteristics etc.), and X_i is a scalar measure of teachers characteristics which may affect his or her preferences for w_k and Z_k. An individual chooses a job by maximizing utility subject to costs of each alternative. Hence, an individual will stay in teaching if the net utility of the job is the highest when compared to alternatives.

There are two main empirical strategies commonly used in teachers' attrition analysis. Recent studies are mostly based on a duration model framework. Duration models allow estimating the conditional probability of a teacher's exit, given he had not left during the previous years. Imazeki (2005) analyzes teacher exits and transfers to another district using the Cox proportional hazard duration model. Podgursky, Monroe, and Watson (2004) define a discrete Cox proportional hazard model to estimate the probability of a teacher leaving the state school system conditional to the years spent previously teaching. Murnane, Stringer and Willet (1988) exploit a proportional hazard model to predict the lengths of teachers' first and second spells in teaching. Stinebrickner (1998) applies two stage estimation methods. At the first stage, he estimates the joint distribution of variables with missing data, and then the results of the first stage are integrated into the duration model of the second stage. Dolton and van der Klaauw (1995) use a proportional hazard model to analyze teacher retention and turnover[7].

The structure of the data set considered does not allow for tracking the career paths of teachers for longer than 1 year. In the two time periods I pooled together, I observed only whether a teacher stays or exits the next year. This feature of the data makes it impossible to use a duration model framework because this type of model requires information about length of teaching spells beginning from the moment an individual entered the profession and for some subsequent interval of time. This is not the case in this data set, so the estimation strategy was restricted to other types of models.

Other commonly used empirical strategies to estimate teacher attrition include binary and multi-response discrete models, where the dependent variable takes two (in the case of binary models) or several (in the case of multi-response models) values, each of them representing possible outcomes. The choice of a multinomial model is preferable if some explanatory variables are likely to affect exit and transfer decisions differently. According to the results of

[7] For other examples of duration models used in teachers attrition studies see: Grissmer and Kirby (1992), Kirby, Naftel, & Berens (1999), and Stinebrickner (2002).

several previous studies some factors do in fact affect the decisions to quit teaching and to transfer to another district differently. This difference is especially pronounced for the effects of wages and opportunity costs. Imazeki (2005) and Hanushek et al. (2004) have found that transfers to other school districts are more sensitive to teachers' salaries and opportunity costs than are exit decisions. Boyd, Loeb, Lankoff, and Wykoff (2005) also found that the decision to quit the school system is more related to the level of student achievement in the school than is the decision to transfer to another school or district.

The focus of my research is on the national level; therefore I do not analyze transfers between schools and districts. My definition of a teacher exit (not teaching in the same main assignment field the next year) includes three different alternatives: continue to work as a teacher, but in a different main assignment field; still work in the school educational system, but not as a full- or part-time teacher; and exit from the elementary and secondary educational system. To account for these differences I first tried to apply a multinomial logit model to the analysis by defining four different alternative choices: stay in the same field, change field, quit teaching, and quit secondary and elementary educational system. However, I found out that I have too few observations in each category when I subdivide them by types of certificates and assign them to different experience cohorts. Table 2-2 shows the number of observations for each type of exit[8].

For some cohorts I have less than 10 exits of the 2nd and 3rd types for ARC teachers. Hence, I do not have enough observations to define a multi-response discrete model. However, since I am not particularly interested in the analysis of the effects of the salaries and opportunity costs on the teachers' decisions to quit or transfer, and the scope of my analysis is national,

[8] Table 1-2 shows number of observations for each type of exit corresponding to the sample formed by definition 1 of the experience cohorts.

rather than states, it is not necessary to distinguish between different types of exits. The main purpose of this study is to analyze the efficiency of ARCs at the federal level. For this purpose I am mainly interested in whether graduates of these programs continue to work in the field they were trained in after their first years of teaching. Thus, from the policy point of view it is not really important to distinguish between these different types of exits. Since the teachers' shortage is an attribute of the subject field, those who leave the subject field do not solve the problem of the shortage any more than do those who exit teaching.

Hence, I proceed with a binary choice model. The probability an individual exits (*y=1*) is defined as a function of the school, district and teachers' characteristics

$$P\left\{y_i = 1 \middle| X_i\right\} = F\left(x_i', \beta\right), \tag{2-12}$$

where $F\left(x_i', \beta\right)$ is a logistic distribution function

$$F\left(w\right) = \frac{\exp\left(w\right)}{1+\exp\left(w\right)}, \tag{2-13}$$

and X_i represents a vector of observable characteristics.

Results

Table A-2 of Appendix A reports the results of the logit regression estimations. The results are stable across all definitions of the experience cohorts. The coefficients have close magnitudes and similar levels of significance. To make the results more interpretable Table A-2 shows hazard ratios calculated as an exponential function of the coefficient from the regression (*exp(β)*). For individual variables it shows the relative shift in the hazard caused by a one-unit change in the variable. For example, if the hazard ratio for female teachers is 2.04, it means women are 2.04 times more likely to exit than men are. If the hazard ratio for teachers working in the special education is 0.63, it means they are by 37% less likely to exit the main assignment field than elementary school teachers are.

For the interaction of two or more variables, however, the odds ratio has to be calculated by multiplying coefficients for individual variables and their interactions with other variables. For example, for teachers who completed ARCs and belong to the 1 year experience cohort, the odds ratio can be calculated as the interaction of hazard ratios for the ARC indicator and its interaction term with the experience cohort dummy:

$$OR_{Alt\times Exp1} = \exp\left(\beta_{Alt}\right)\times\exp\left(\beta_{Alt\times Exp1}\right). \tag{2-14}$$

Hence, if the hazard ratio for ARC teachers is 0.83, and the hazard ratio for the interaction of the 1 year experience cohort dummy with ARC dummy is 3.24, then ARC teachers with 1 year of experience are 0.83×3.24=2.70 times more likely to exit than TRC teachers from the same experience cohort. The main research interest of this study is the analysis of differences in attrition rates of teachers with different types of certificates conditional on the experience level. Table 2-3 reports odds ratios for ARC teachers with less than 6 years of experience[9].

Teachers who completed ARC programs are 2.7 times more likely to exit the main assignment field immediately after the first year of teaching than are TRC teachers. This result is statistically significant at the 5% level and stable across all specifications of the experience cohorts. However, this discrepancy between teachers with different types of certificates disappears as they earn at least 2 years of experience. For 2-5 year cohorts no statistically significant difference in exit rate is revealed. The magnitudes of the odds ratios for cohorts with 2 and 3 years of experience are also stable across all specifications. For experience cohorts with 4 and 5 years of experience these magnitudes are relatively higher for the diagonal specification. The higher magnitudes for the diagonal model might be caused by the relatively small number of observations for these experience cohorts in this particular definition, which provides the

[9] I also did similar analysis with 10 cohorts (1-10 years of experience), and the result holds the same.

smallest sample for this analysis. The magnitude of the effect is also higher for the 5th year cohort for the 2nd definition of the experience cohorts compared to definition 1. The 2nd definition is the one which provides the largest sample since it might include teachers with breaks in their service. The likelihood for teachers to have breaks increases as the number of years of experience increases. Therefore, some teachers in the 5 years experience cohort in the 2nd definition might have had a break and returned to teaching 1-3 years ago. Hence, their behavior might be more similar to teachers in the cohorts 1-4. Therefore, this difference in magnitudes produces no serious doubts in the consistency of results, especially since in all specifications this effect is not statistically significant for experience cohorts with 2-5 years of experience.

Hence the results of the logit estimation suggest that the variation in the exit rate across teachers with different types of certificates exists only for novice teachers with no experience, and inclusion of indicators for experience cohorts 2-5 does not add anything to the model. To confirm these results I also test the joint significance of this set of explanatory variables. Table A-3 of the appendix presents the results of the Wald test. The hypothesis of insignificance cannot be rejected for cohorts with 2-5, 3-5, or 4-5 years of experience. Therefore, the test results also confirm the conclusion that the teacher attrition rate is different for ARC and TRC teachers only immediately after the first year of working in the school education system and not for subsequent years. After the second year their exit rates do not differ from each other.

The marginal effects of coming to the profession through ARCs rather than through TRCs on the probability of exiting the main assignment field are presented in Table 2-4 for four different social groups of teachers: white and non-white males and white and non-white females. ARC male teachers are 14-16% points more likely to change their main assignment field after the first year of teaching than male TRC teachers. The effect is quite large in magnitude, taking into account that the probability to exit after the first year for TRC teachers is about 10-12%. For ARC women the effect is even larger; they are more likely to exit by 17-18% points compared to female TRC teachers. These numbers are somewhat larger than usual estimates of teacher attrition rates. However, the common approach to the definition of attrition is not based on exit from the main assignment field, as is done in this study. Going back to the statistics presented in Table 2-2, it becomes clear that actually the number of exits from the field is comparable to the number of exits from the school education system. That is why my estimates of exit rates are relatively higher.

For other explanatory variables, the results are reasonable and consistent with previous research on the determinants of teacher attrition. Women are more likely to exit than men, but married females are less likely to leave the field—a result similar to the finding of Steinbrickner (1999). The effect of experience has a U-shaped form, as the probability of exit first decreases with experience and then rises again as the teacher approaches retirement age, with the critical point approximately at 19 years of experience. Given that average entry age is 26, the critical point corresponds to age 45. As one might expect, a high proportion of low-income students increases attrition. The same effect was found in a number of previous studies[10].

Contrary to many studies focused on the effect of opportunity costs and salaries on teacher attrition, I have detected no effect of real salaries on teachers' decisions to exit. However, most of the previous findings on this topic suggest that salary and opportunity costs mostly affect transits between school districts rather than exits from the school system[11]. Since I do not distinguish between cases when the teacher transfers to another district but continues to work in

[10] See for example Dolton and Klaauw (1995); Boyd et al. (2005).
[11] See, for example, Imazeki, 2005; Hanushek et al., 2001.

the same assignment field and a teacher who continues to work in the same field and same district, the transits might be counted as "stays" as well as "exits" in my study. Hence, it is not surprising that the effect was not confirmed by my model.

Finally, I found substantial variation in attrition rates of teachers working in different subject fields. As expected, teachers working in "Other" fields like vocational studies, technical studies, etc. are 2.08-2.27 times more likely to exit than elementary school teachers (omitted category). Surprisingly, English teachers demonstrate a higher rate of exit from the field than those specializing in mathematics and sciences, the fields experiencing the most severe shortage. However, the problem of shortage in mathematics and science primarily relates to the attraction of newly qualified teachers rather than to the retention of teachers with experience. Moreover, for mathematical and science teachers it is more difficult to change fields, since their professional preparation is specific and cannot be applied in other subjects. Specialists in English language arts may change to humanities with relative ease. These results are generally consistent with the findings of Murnane and Olson (1989). They found that high school teachers are more likely to leave teaching after a few years on the job than elementary teachers, and that English teachers have relatively shorter working spells than mathematics teachers.

My results suggest that attrition of novice teachers is quite different for graduates of different types of certification programs. Teachers that completed an ARC are 14-18% points more likely to exit after the first year in the profession than their colleagues who completed a TRC. The difference however disappears for teachers with at least 2 years of experience. These results have important implications for the policy making. Since ARC began to develop widely only since 1999, there are no available detailed statistics for the career paths of ARC teachers. This makes it difficult to analyze the efficiency of ARC programs. However, according to the results of this study, only the first year matters, after the first year ARC teachers have the same attrition patterns as TRC teachers.

Another interesting question that arises from the results concerns the reasons behind this dramatic difference in attrition rate after the first year. Do graduates of ARC exit because they are less prepared for teaching, or are individuals who decide to enter ARC different from participants in TRCs? In other words, are there attributes of the ARC that cause the higher attrition of novice teachers, or are individuals choosing these types of programs according to their personal attributes, which affect not only their program choice, but also the probability they will leave teaching or change subject field? Are ARC teachers less devoted to the profession or are they less prepared to face a classroom? Is there a self-selection of teachers by the types of programs? The next section discusses this possibility.

Analysis of Possible Self-Selection

Assuming that an individual's choice of certification programs might be based on personal qualities, which might also affect the decision to leave teaching or to change field, it is important to check whether teachers that completed ARC are, in general, similar to the graduates of TRC or if they are somehow different. For these purposes, I analyze the social and demographical characteristics of teachers for the subsamples of ARC and TRC teachers, and look for any substantial difference in characteristics between the two groups.

Since the results of logit estimation showed a substantial difference in exit rates between ARC and TRC teachers only immediately after the first year of working in the elementary and secondary education system, the most important question is whether teachers with 1 year of experience have similar characteristics in both groups. Therefore, I first address this cohort of teachers. Table A-4 of the Appendix A presents summary statistics for the observable personal characteristics of novice teachers with 1 year of experience. These characteristics include: age, race, sex, educational attainment, competitiveness of the teacher's undergraduate college, marital status, and family income and size. The next year follow-up data is not needed for this analysis because the whole SASS data set was used for the comparison of these characteristics across certification groups, thus significantly increasing the sample size. However, marital status and family characterizes are available only in the TFS, hence for these variables the sample size was relatively small.

There are no dramatic differences in average personal characteristics across certification groups; however, there are still some noticeable disparities in mean values of a few personal attributes. In particular, it seems the share of male teachers is higher among graduates of the ARCs, and also representatives of this group are more likely to have graduated from competitive undergraduate colleges, less likely to be married and on average they have lower family incomes. Simple t-tests were performed to check for statistically significant differences. I run OLS regressions for all personal characteristics with type of program indicator as an explanatory variable:

$$PC_i = \gamma_0 + \gamma_{Alt} \cdot Alt_i + \varepsilon_i, \qquad (2\text{-}15)$$

where PC_i is a personal characteristic of individual i and Alt_i is an indicator of the type of certification program. If the null hypothesis γ_{Alt}=0 cannot be rejected, then there is no reason to believe that participants of ARCs and TRCs are different from one another. However, if the hypothesis can be rejected, then there is some ground for the self-selection hypothesis. I performed t-tests for two time periods, 1999-00 and 2003-04, separately and then a difference-in-difference approach was applied for the pooled sample to check whether the differences between groups vary over the time

$$PC_i = \varphi_0 + \varphi_{Alt} \cdot Alt_i + year_i + \varphi_{Alt \cdot Year} \cdot Alt_i \cdot year_i + u_i, \qquad (2\text{-}16)$$

where $year_i$ is the indicator of the time period and $\varphi_{Alt \cdot Year}$ shows whether the disparity between groups varies across time periods.

Table A-5 of the appendix presents the results of t-tests for the teachers with 1 year of experience (the set of personal characteristics is similar to the one analyzed in Table A-4). The first four columns show γ_{Alt} and corresponding t-statistics for the 2003-04 and 1999-00 periods respectively, and the last two columns report results of the t-tests for difference-in-difference estimations $(\varphi_{Alt \cdot Year})$. The results indicate that ARC teachers are, on average, 2 years older than graduates of TRCs, they are approximately 8-12% points more likely to be male, and they more frequently represent graduates of competitive colleges. The most controversial result is observed for the indicator of a teacher holding an associate's degree. In 1999-00, ARC teachers were about 8% more likely to possess such a degree, while in 2003-04 they were 11% less likely to have it.

There is nothing surprising in age differences, since teachers entering ARCs are usually those who did not choose teaching as a specialty right after high school in contrast to the teachers who completed TRCs. The higher share of male teachers might be explained by the fact that women are more likely to choose teaching as their main specialty and to enter TRCs while men might be less likely to consider this opportunity just after the school. In the regression estimation, discussed in the previous section, I have already included controls for age and sex and the interaction of sex and marital status, so I have already accounted for possible differences between ARC and TRC categories that might be consequences of gender structure rather than the type of program. However I did not control for the competitiveness of the undergraduate college, while t-tests show that in the 2003-04 school year teachers in ARC category, in average, had attended better colleges than teachers in the traditional category. That might be caused by the development of the TFA program in the 2000s that recruits graduates from the best colleges with 2 years contracts to work in elementary and secondary schools. This disparity raises certain concerns about whether ARC and TRC novice teachers are of the same quality and whether they have the same job opportunities outside of the school system. Hence, while overall the analysis shows few major differences between the two groups of teachers, I cannot be certain there is no self-selection.

The same analysis is repeated regarding the personal characteristics of teachers with 1 to 5 years of experience (representatives of all 5 experience cohorts pooled together). While the comparison of novice teachers with 1 year of experience is of the main interest, taking into account the results of the logit estimations, this study is focused on the analysis of the turnover rates of teachers with less than 6 years of experience. The testing of possible self-selection is prolonged to teachers belonging to all 5 cohorts that are under the scope of the analysis. The summary statistics and t-tests for teachers with less than 6 years of experience are presented in Appendix A in Table A-6 and Table A-7 respectively.

The results for the pooled sample reveal even more disparities across certification groups. ARC teachers appear to be 2 years older. They are less likely to be white and the difference in the racial composition between groups increases over time. They are also less likely to have bachelor's and master's degrees, and they more frequently represent graduates of competitive colleges.

After I compared two groups of teachers with different types of certificates, I also compared personal characteristics of the ARC teachers belonging to the different experience cohorts. Since I found a higher exit rate only for ARC teachers with just 1 year of experience, then, if it is in fact the effect of personal characteristics rather than the effect of the program, it is natural to suppose that those graduates of ARC that are different from TRC teachers sort out after the first year. Exit rates for cohorts with more than 1year of experience do not vary across certification groups. Therefore relevant discrepancies in personal characteristics are not likely to be present after the sorting out occurring after the first year. In other words, if there is self-selection to the programs that causes a higher attrition after the first year of teaching, then ARC representatives of the 1 year experience cohort must be different from ARC representatives from cohort 2. Tables in Appendix A (from Table A-8 through Table A-11) illustrate t-tests performed for pair-wise comparisons of two adjoining cohorts of ARC teachers. Table A-8 shows the results of a comparison of cohorts 1 and 2. Table A-9 compares cohorts 2 and 3, Table A-10 compares cohorts 3 and 4, and Table A-11 compares cohorts 4 and 5 respectively. Assuming there is self-selection of teachers to programs, I expect to find the most significant difference between cohorts 1 and 2, which then decreases with years of experience.

However, with few exceptions, ARC teachers with 1 year of experience appear to be similar to those with 2 years of experience. In 2003-2004, first year teachers were less likely to be married than teachers with 2 years of experience, and first year teachers were also less likely to be black in 2003-04. In general, comparisons of other experience cohorts show the same picture: differences in marital status and family characteristics (which may be caused by changes in personal life, rather than by sorting out after first year); a difference in sex composition between cohorts 2 and 3, and cohorts 4 and 5 in 1999-00; and a difference in the average quality of undergraduate colleges for cohorts 3 and 4 (teachers with 3 years of experience are more likely to have graduated from a competitive college). The last finding raises concerns about the difference in the quality of teachers and their possible choice of jobs outside of the education field.

Surprisingly, the difference between ARC and TRC teachers appears to increase as experience increases. I expected the opposite effect in the case of self-selection to programs. The same tendency was also revealed comparing the experience of cohorts that graduated from ARCs. The differences between the cohorts are more pronounced for cohorts 2 and 3 and 3 and 4, rather than for the cohorts 1 and 2. These results suggest that while individuals entering different types of programs are not really different from each other, the attrition patterns for these two groups might not be the same over time. For example, white teachers with ARCs might be more likely to exit main assignment fields during their second or third year than white TRC teachers, but they might also be less likely to exit during the forth or fifth year.

It is of interest to analyze whether the patterns of teacher attrition over time are different for the ARC and TRC groups. For these purposes I ran a set of OLS estimations that includes the set of explanatory variables used in the model discussed in the previous section. I also included one additional personal characteristic, its interaction with the experience dummy, its interaction with the certification type indicator, and the interaction of characteristic, experience cohort, and certification type. For example, for the analysis of effects of college quality on the attrition of teachers with one year of experience, Equation 2-10 estimates:

$$P\{y_i = 1 | X_i\} = \beta_0 + X_i'\beta + \mu_1 \cdot Alt_i + \mu_2 \cdot Exp_{ki} + \mu_3 \cdot Exp_{ki} \cdot Alt_i + \mu_4 \cdot college_i + \mu_5 \cdot Exp_{ki} + \mu_5 \cdot college_i \cdot Alt_i + \mu_6 \cdot college_i \cdot Exp_{ki} \cdot Alt_i + u_i, \tag{2-17}$$

where X_i represents the vector of observable characteristics (teacher white, teacher black, experience, experience squared, entry age, non-white students, non-white teachers, poor, number of schools, district enrollment, union, urban, special education, regular second, interaction term of special education and regular second, year). Alt_i is equal to 1 if the teacher *i* completed an ARC. Exp_{ki} indicates whether teacher *i* belongs to cohort *k*. $College_i$ is an index of the relative competitiveness of the undergraduate college attended by teacher *i*. The main interest is in coefficients μ_5 and μ_6. If μ_5 is not equal to zero, than the behavior of teachers with the same characteristics might be different for representatives of the two certification groups. Likewise, if μ_6 is different from zero, than the differences might be pronounced only for this experience cohort. Equation 2-10 was estimated for 15 different personal characteristics and for 5 different experience cohorts (each regression includes the indicator for one cohort). Due to the small sample size in this analysis, another definition of cohorts was used. The first set of regressions include the indicator of the 1 year of experience cohort, the second set includes the indicator for teachers with less than 3 years of experience, the third set for teachers with less than 4 years of experience, the fourth set for those with less than 6 years of experience, and the final set includes an indicator for teachers with 6 to 10 years of experience. Thus, this particular analysis constitutes 75 regressions and I do not report these tables in this study.

According to the results of this analysis, there is a difference in exit rates for teachers with various level of experience, however within experience cohorts there are no statistically significant differences across certification groups. This means white teachers with 1 year of experience might have a different attrition rate compared to white teachers with 2 years of experience, however white ARC teachers with 1 year of experience behave similarly to white TRC teachers from the same experience cohort. More precisely, for coefficient μ_2, μ_3, in many cases, the null hypothesis $(\mu=0)$ cannot be rejected, while coefficients μ_7 and $\mu 7$ are not statistically different from zero in the majority of regressions. However, there are a few exceptions.

The most sustainable effect was found in the family income variable. ARC teachers with high income levels are less likely to exit teaching than TRC teachers belonging to the same income category. The coefficient μ_6 is negative and is significant with a 5-10% level for all experience cohorts in the regressions where family income is used as the main personal characteristic variable. However, there is no reason to believe that this effect varies across experience cohorts for ARC teachers (μ_7 is not statistically different from zero).

Second, teachers who graduated from competitive colleges are more likely to exit their main assignment field if they completed an ARC. This effect was only revealed in 1 regression specification, however I have already found disparities in the mean values of college quality regarding ARC and TRC certified teachers in a previous analysis discussed in this section.

Finally, coefficients μ_6 and μ_7 were occasionally found to be statistically significant in 1 or 2 out of 5 specifications for a few personal characteristics. Those characteristics include bachelor degree, never married, widowed-separated-divorced, educational specialist or some graduate study. Unfortunately, I do not have enough variation in these variables for ARC teachers, as well as in the bachelor degree variable for TRC teachers. Hence, these occasional results might be driven by just a few observations.

Hence, the effects of family income and college quality raise the most concerns regarding the results presented in the previous section. Therefore, I included these two variables and their interaction with ARC indicator into the model to check whether the difference in exit rates of teachers is driven by the difference in these personal characteristics or if they are driven by some particular feature of the certification program.

Results with Controls for College Quality and Family Income

Both personal characteristics in question, college quality and family income, used in the previous analysis, are defined as an index, which varies from 1 to 5. This is the only available information for them. Both indexes are organized in descending order, where 1 corresponds to the least competitive colleges and the lowest level of income respectively. Before proceeding with the main analysis, I first tried different definitions for these variables based on the available indexes. Those included the following approaches: a set of dummy variables for each category of the index, the index itself, and a different grouping of categories. Finally, I decided to stay with the set of dummy variables for each category of income respectively, and a dichotomous indicator for the college quality. These definitions provide the best fit for the model and there are no controversial effects within the groups when categories with the opposite effects are pooled together. Hence, in my model college quality takes two values: 1 and 0. It is equal to 1 if a teacher graduated from a relatively competitive college (the original index is equal to 3, 4, or 5), and it is equal to 0 if a teacher graduated from a less competitive college (the original index is equal to 1 or 2).

First, only these variables were added to the model discussed earlier. The results are reported in Table A-12 in the Appendix A. All explanatory variables retained the same magnitudes and significance levels. For the variables of main interest, the previous results hold and ARC teachers are more likely to exit a main assignment field after the first year. After that, the difference in the exit rate across certification groups dissipates. Hence, the inclusion of additional variables does not significantly affect the model. The odds ratios and marginal effects also are very close to those found in the first specification. The analysis of the joint significance of the variables for experience cohorts presented in Table A-13 in Appendix A also confirms the previous findings (the variables describing cohorts 2 through 5 are jointly insignificant).

However, the analysis provided in the previous section shows these two characteristics could affect ARC and TRC teachers differently. Thus, the interaction terms for family income and college quality variables were added to the model. Since earlier I found a persistent effect of family income on ARC teachers' likelihood to exit the main assignment field, which does not vary across years of experience, I included only the interaction of this variable with the ARC indicator. However, for college quality, the most pronounced effects were revealed for 1 and for less than 4 years of experience cohorts. There is reason to believe that these effects are not stable and vary depending on years of experience. Hence, for college quality, the interaction terms with years of experience and ARC indicator were included in the model as well as its interactions with dummies for experience cohorts and its interactions with ARC variables. This allowed me to check not only whether the effects of college quality on the probability to exit varied across experience cohorts, but also whether these effects are the same for ARC and TRC teachers belonging to the same experience cohort.

Table A-14 of the Appendix A shows the results of the logit model with the newly added interaction terms. All explanatory variables, which are not of particular interest to this study, have almost the same magnitudes of hazard ratios and the same level of significance. Most of the family income dummies are insignificant and are not quite stable across specifications. Interestingly, ARC teachers with a high level of family income are less likely to exit than TRC teachers. The possible explanation for this effect might be that for ARC teachers with high family income, the main reason for entering teaching did not involve income, but rather having an occupation they like. On the other hand, TRC teachers, who chose this specialty when they were an undergraduate in college and do not really enjoy the profession, might feel free to quit teaching for some other occupation if they are not constrained by the fear of losing their salary.

The college quality dummy is also statistically insignificant in all specifications, except for a flag used for missing observations for this variable. I looked at these missing observations more precisely to understand what exact effect this flag variable catches. Most of the missing observations are actually caused by two reasons. The first category of missing college quality information is when a college is not included in the classification used for the construction of the index, or if the college no longer exists. The second category of missing observations includes teachers who graduated from foreign colleges. There are also other types of missing observations, like incomplete information provided by the respondent, but these cases constitute a small portion of the total number of missing observations compared to the first two categories. I tried to assign missing observations to these three categories and to run a regression with 3 flags instead of 1. The results confirm that a negative effect is driven by teachers from the first two categories and these categories are likely to represent the low quality of a college. Colleges not listed in the index are probably of a lower quality than colleges with the grade 1. Teachers that graduated from foreign colleges might experience language difficulties. Since our main focus is on the effect of the certification programs on the exits, I have pooled all the missing cases together.

While explanatory variables retain almost the same magnitudes of the coefficients and the same level of significance, one can observe that results for the variables of main interest have now changed. The ARC indicator and its interaction with college quality are now significant at a level of 10% in two specifications out of three. The hazard ratio for the interaction term is higher than 1 and for the ARC dummy it is less than 1. Therefore, ARC teachers who graduated from a low-quality college are less likely to exit a main assignment field than graduates from competitive colleges holding the same type of certificate. One can also observe that these effects are not stable across different experience cohorts. All teachers are less likely to exit after their second year independent of the type of certificate. However, the attrition of the ARC graduates from competitive colleges varies significantly across the first 3 years of experience. Particularly after their third year teachers are much more likely to quit teaching than those who graduated from less competitive colleges. These results make sense: teachers with a better undergraduate preparation are less likely to quit because they do not feel they are incompetent for the job (the factor that causes attrition during first the years of teaching), however with better education they have more chances to find a better job outside of secondary and elementary education or to move to administrative positions inside the education system (they are more likely to exit their main assignment field after their third year).

For the graduates from less competitive colleges, the results have the same pattern as before: they are more likely to exit a main assignment field after the first year, but as they gain at least 2 years of experience all differences in attrition rates between them and graduates of TRC belonging to the same cohort disappear. There is a statistically significant effect for the experience cohort 4 in the diagonal specification of experience (that provides the smallest sample). However, this result does not hold in more preferable specifications. While previous results have shown no statistically significant effects for cohorts 2 and 3, their standard errors were relatively high; with newly added variables the standard errors for these cohorts decreased dramatically, clearly indicating that no significant difference in attrition rates exists between teachers with different types of certificate belonging to the same cohort (except for effects for ARC graduates from competitive colleges that I discussed above).

Table 1-5 reports the odds ratios for ARC teachers with less than 6 years of experience for the model with new variables added[12]. The ACR graduates of a competitive college are 1.13-3.39 times more likely to exit a main assignment field just after the first year than TRC graduates of competitive colleges. The relative chances of exiting for the graduates of less competitive colleges are almost the same: they vary from 1.08 to 3.41 for teachers with 1 year of experience. For all ARC teachers the lowest relative chances of exiting are associated with the highest level

[12] I also did similar analysis with 10 cohorts (1-10 years of experience) and result holds the same.

of family income. The highest attrition probability is the attribute of the middle categories of family income which belong to the interval from 35 thousand to 100 thousands. For the graduates of competitive colleges, the probability of exiting a main assignment field is higher after the third year (0.98-4.04) than after the first year (1.13-3.39). This effect is statistically significant in all specifications.

Table 1-6 presents the marginal effects for ARC teachers. The results are calculated using mean values of family income variables. After the first year, male ARC teachers are 12-13% points more likely to exit a main assignment field than male TRC teachers. The same category females are 14-16% points more likely to exit than their colleagues who came to the profession through TRC. The exit rates both for men and women who graduated from competitive colleges are higher for the 3 year experience cohort (11-16% and 13-19% respectively). For the graduates of less competitive colleges, the exit rates are quite close to those from competitive colleges, except for the 3 year experience cohort. After the 3rd year ARC teachers from less competitive colleges are less likely to exit the main assignment field than TRC teachers. However, this effect was not statistically significant in any specification.

Then I proceed with the analysis of the joint significance of the experience cohorts indicators as I did for the previous models. Table A-15 in Appendix A shows the results of the Wald test for joint significance for all variables related to experience cohorts and ARC preparation. With college quality controls the hypothesis of joint insignificance cannot be rejected for cohorts with 3-5 years of experience tested together. The value of the F-statistics increases dramatically for the cohorts 4-5 tested without cohorts 1-3.

Generally, the new model provides the same results as the model discussed previously. With only one exception, the attrition rates of teachers with different types of certificates are not statistically different from each other for the experience cohorts 2-5, while there is substantial disparity in exit rates of teachers with only 1 year of experience. This disparity is extremely high for graduates of the less competitive colleges. While it is smaller in magnitude for the graduates of the competitive colleges, it is still strongly statistically significant. The only exception from this attrition pattern is the high exit rate of graduates from competitive colleges after the third year. While generally this result makes intuitive sense, some questions concerning that effect arise. It is not clear why it is present only for ARC teachers, as graduates from the competitive colleges might have more job opportunities outside of teaching independent of the type of the certificates they hold. One possible explanation for this difference could be that ARC teachers who had chosen another specialization in the undergraduate school might have in their possession other diplomas. Therefore, they might have higher chances to find jobs outside of the field of education and they also might have stronger motivation to look for another type of job.

However, I offer another explanation of this effect. Since the beginning of the 2000's, an ARC program TFA was rapidly developing. This program recruits the graduates of the most competitive colleges for teaching in elementary and secondary schools. Participants in this program are committed to work 2 years as school teachers. The high attrition rates after the third year for the ARC teachers who graduated from highly competitive colleges could be caused by the high exit rates of the participants of this particular nation-wide program. To check this hypothesis I run two separate logit regressions for 1999-00 and for 2003-04 subsamples. Since the program was not well developed before 2000, it could not have affected attrition patterns in 1999-00. The effect was confirmed for 2003-04, while no significant results were found for the 1999-00 sample. Thus, the estimation results probably have revealed the effect of this particular program. However, there is still one concern. According to the program terms participants sign up only for 2 years of work in the school system. Therefore, one would expect high rates of exits after their second year, rather than after the third, while our results confirm it only for the teachers with 3 years of experience. No statistically significant effect was found for the ARC teachers with 2 years of experience who graduated from competitive colleges. To understand why this could be the case I addressed recent studies focused on the analysis of this program.

Boyd et al. (2006) compare the attrition rates of novice teachers who entered teaching in the NYC through ARC and TRC. They focus on three types of ARC programs: TFA, NYCTF, and Other, which include all other teachers whose preparation does not fit other pathways. Table 2-7 shows the descriptive statistics characterizing the attrition rates that come from Boyd et al. (2006).

College recommended and Individual evaluation categories represent TRC. TFA is an alternative program recruiting graduates of the most competitive colleges to work 2 years as a teachers. NYCTF is a NYC program that targets mid-career professionals as well as recent college graduates. The other category includes teachers whose preparation program does not fit into the other five categories. Hence, TFA teacher are ARC teachers who are considered to be graduates of competitive colleges in this analysis. NYCTF graduates are also likely to belong to this category. On the contrary, the “Other” category probably includes ARC teachers who graduated from less competitive colleges.

The descriptive statistics for NYC teachers confirm our findings. ARC teachers from less competitive colleges have the highest exit rate after their first year, and the rate decreases with experience earned. ARC graduates of competitive colleges are more likely to exit after their third year (both TFA and NYCTF have the highest exit rates after their third years). Their exit rate is also high after the second and fourth years but the peak occurs after the third year. The difference in exit rates between TRC and ARC teachers is less pronounced for cohorts with 2 years of experience than for cohorts with 3 years of experience. This explains why I found a significant effect after the third year and no significant effect after the second. The differences in exit rates for "Other", presumably, the category for the ARC teachers who graduated from less competitive colleges, and TFA are also most pronounced after the third year. Hence, our findings confirm the same pattern of teachers' attrition revealed in Boyd et al. (2006).

Other studies of the TFA program confirm that the majority of its' participants do not exit after the second year. Decker, Mayer, and Glazerman (2004) analyzed the retention of program participants for several regions (Baltimore, Chicago, Los Angeles, Houston, New Orleans and Mississippi Delta). They argue that about 34% of the program alumni do not quit teaching after the second year of the program and except for them 25% continue to work in the field of education.

There are also a couple studies regarding TFA effectiveness in Houston based on data from 1996-2001. Raymond, Fletcher and Luque (2001) do not distinguish between exiting from teaching and transferring to another school. However, for both transfers and exits they found that significant proportions of TFA participants (about 40-20%) have stayed in teaching beyond 2 years, with the exception of 1998. Darling-Hammond et al. (2005) use the same data set and also report exits and transfers pooled together. Their results show that between 57% and 90% of TFA recruits leave teaching in Houston after their second year and between 72% and 100% of recruits leave after their third year. Since these studies do not distinguish between transfers and exits, it is difficult to interpret their data relative to my findings. Moreover, these studies are based on a relatively small data set (about 20 teachers in each experience cohort) for the 1996-2000 period when the TFA program was not yet well developed. Hence, I consider the statistics presented in

Boyd et al. (2006) as more representative regarding this program. They analyze about 100-300 observations per cohort, cover the 2001-2004 period, and their definition of exiting is more comparable to mine. In the next section I apply estimated attrition rates for ARC and TRC teachers to estimate the relative efficiency of ARCs in solving the teacher shortage problem.

Simulation of Efficiency of Alternative Certification Programs

Using empirical results from the previous sections, it is now possible to estimate the simple simulation model presented earlier in this chapter. This simulation uses Equations 2-1, 2-2, 2-5, 2-6, and 2-7. Assume that w_{RF}=40,000, N=1000, and N_{SF}=240. For simplicity, the retention rate after the first year is considered to be 100% both for TRC and ARC teachers. Since there is no difference in their attrition rates if they stay in teaching for at least 2 years, it is irrelevant for the comparison purposes to account for their attrition rates in consequent years. The present value of future wage payments is calculated for a 5 year period, assuming the discount rate is 5%. Also assume that the relative loss in productivity due to hiring an inexperienced teacher is 20% of the MPL (α=0.2)[13], the productivity of a novice ARC teacher is approximately 20% lower than the productivity of a novice TRC teacher (β=0.2), and the MPL is equal to the equilibrium wage.

Table 2-8 shows results of the model simulation applying different elasticities of supply (E^S). Manski (1987) estimated the elasticity of teacher supply using data from 1972. According to his estimation, the elasticity of teacher supply varies between 2.4 and 3.2. However, the elasticity of supply could have changed since 1972, because the composition of the teacher labor force, as well as economic and social characteristics of the society have changed since that. Hence, I use different elasticities varying from 2 to 10.

[13] The negative effect of lack of experience on the students' outcomes was estimated as roughly 50% of the standard deviation of teacher quality in Hanushek et al. (2005), which corresponds to a 19% efficiency loss.

The results of the simulations show that depending on the elasticity of the supply the upper bound of ARC training costs that makes ARCs competitive with TRCs varies from \$10,000 per ARC teacher ($E_{SF}^{S} = 10$) to \$99,000 ($E_{SF}^{S} = 2$). For this simulation, it was assumed that a district has 1000 teachers with less than 6 years of experience. The bigger is the district, the more efficient ARC programs would be because the second component of marginal costs for the wage growth approach $(\Delta w \cdot N)/\Delta N_{SF}$ increases as the size of district increases. The second component of ARC marginal costs (C_{ARC}) decreases as the district size increases due to economies of scale for ARC programs. The NCAC provides some data for the cost of alternative teacher certification programs across United States. Table 2-9 briefly summarizes their information. Assuming the costs estimation of the NCAC is a rough proxy for C_{ARC}, the ARC programs is an efficient way to solve shortages problems despite productivity losses and even in the case of high elasticity of supply.

This analysis is based on an important assumption, that after a few years of teaching there in no difference in the quality of teachers who came to the profession through different pathways. This assumption is based on the finding of Hanushek, Kain, O'Brien, and Rivkin (2005) that a teacher's productivity increases in the first years of teaching and then remains stable until retirement. However, it is possible that the quality of an ARC teacher will differ from the quality of a TRC teacher even after first years of teaching. Why can their quality remain different and how would it affect the results of simulation? If the assumption does not hold my estimation of the maximum training cost that allows an ARC to be efficient may be either overestimated or underestimated. The maximum effective C_{ARC} is overestimated if the quality of average ARC teacher is always below the quality of an average TRC teacher with the same level of experience. It can happen if experience gained over years of teaching does not compensate for the lack of the initial ARC teacher preparation in children psychology, methodology of teaching, and other courses that are studied more profoundly by TRC teachers. Moreover, ARC teachers may be less devoted to the profession assuming their choice of the career of teacher was not their first best choice and they would rather prefer to work in some other area, but are not able to pursue it due to personal reasons. That may reduce the productivity of ARC teachers compared to TRC teachers with the same experience. In these cases Equation 2-7 must be corrected to include all future productivity losses caused by hiring an ARC teacher instead of a TRC teacher.

The opposite situation takes place if the quality of an average ARC teacher is higher than the quality of an average TRC teacher. If ARC teachers are on average more talented or have better undergraduate educations (the descriptive statistics in Table A-4 and Table A-6 show that on average ARC teachers graduate from more competitive colleges than TRC teachers) they may become more productive than TRC teachers after they gain experience. In this case the maximum effective C_{ARC} is underestimated, and Equation 2-7 should be corrected to include all future gains in productivity caused by hiring an ARC teacher instead of a TRC teacher.

Let β_i denote the coefficient of relative loss or gain in the productivity induced by substitution of one TRC teacher by an ARC teacher with the same level of experience i. Then the marginal input costs of preparation of an ARC teacher are

$$MIC_{ARC}^{Attr} = \left[\frac{w_{RF}}{R_{ARC}} + PV\left(w_{RF}\right) \right] + \left[\frac{C_{ARC}}{R_{ARC}} + \frac{\alpha \cdot \left(1+\beta_1\right) \cdot MPL}{R_{ARC}} + PV\left(\beta_2, ..., \beta_N, MPL\right) \right], \tag{2-18}$$

where $PV(\beta_2, \ldots, \beta_N, MPL)$ shows the present value of losses or gains in productivity for an ARC teacher who works in the school education system for N years, starting from his second year in teaching and up to his last year of work. However, since ARCs began to develop rapidly only during the last decade no research is available concerning the relative productivity of ARC teachers during their whole careers. Existing studies analyze ARC teachers' efficiency in the beginning of their careers and use relatively small samples. Boyd et al. (2006) found small differences in student achievement that can be attributed to preparation pathways only among first-year teachers. Darling-Hammond et al. (2005) found that hiring a novice TFA teacher has both negative and positive effects on students' scores. Therefore, until more study is done in this area I believe that assumption that quality of experienced ARC and TRC teachers is equal is reasonable and I use it for ARC efficiency simulation in my analysis.

Conclusions

The recent development of ARCs has raised a number of questions concerning their relative efficiency compared to TRCs. I address one of these questions, whether attrition patterns are different for TRC and ARC teachers and if these differences disappear as a cohort matures. My findings confirm that teachers who come into the profession via ARCs are in fact more likely to exit their main assignment field after the first year.

The graduates of competitive colleges who entered teaching through ARC are 14-18% points more likely to exit a main assignment field after their first year than teachers who completed TRCs and also graduated from competitive colleges. Hence, there is a significant difference in attrition rates immediately after the first year between TRC and ARC teachers. However, this disparity disappears after the first year. As teachers gain at least 2 years of experience, the exit rates of ARC and TRC teachers become the same. There is only one exception for this pattern. TRC graduates of competitive colleges are 11-19% points more likely to exit after the third year of teaching than TRC teachers belonging to the same category. I found some evidence that this effect is driven by the high exit rate of the participants of the nation-wide alternative program TFA that recruits the graduates of the best colleges to work 2 years as teachers. The majority of the participants of this program do not stay in teaching longer than 3 years.

Usually, the analysis of the attrition of ARC teachers is complicated because there is a lack of data that follows the career paths of the ARC participants after they complete the program. Taking into the account my findings, it is enough to have statistics of ARC graduates attrition after the first year to predict their attrition in later years. Since the main disparities in exit rates of ARC and TRC teachers occurs just after the first year, the attrition of the ARC teachers for the subsequent years could be predicted based on the attrition pattern of TRC teachers with the same social and demographic characteristics. There is much more data available for TRC teachers that tracks their career paths over time. Hence, a researcher needs only to collect data for the ARC participants' career paths for 1 year after the completion of the program, to facilitate a cost-benefit analysis of such programs.

My findings also raise another interesting question: what is the cause of the significant difference in exit rates after the first year? Are ARC teachers less committed to the profession? Do they only view it as a temporary occupation until they find another job? Or are they less prepared to face a class than TRC teachers? TRC require more intensive in-class practice and courses on pedagogy and classroom management which help novice teachers adopt quickly to new obligations. The graduates of ARCs might be less prepared for this type of the work and might be stunned and confused during their first year of teaching. The second explanation seems reasonable, since the difference in attrition rates disappears as teachers gain more experience. However, further research is needed to answer this question more accurately. If the lack of practice in class causes a higher attrition rate of novice teachers, than emphasizing this part of preparation should be considered for the improvement of ARC programs.

Table 2-1. Number of observations in experience cohorts formed by three different approaches

Experience cohorts	1999-00		2003-04		Total
	Regular	Alternative	Regular	Alternative	
Diagonal definition					
1 year	155	29	97	34	315
2 years	146	32	95	35	308
3 years	106	32	101	40	279
4 years	67	20	77	20	184
5 years	67	13	80	31	191
Whole sample	1981	453	2126	664	5224
Definition 1					
1 year	162	31	107	36	336
2 years	164	39	129	40	372
3 years	122	38	128	50	338
4 years	80	24	117	36	257
5 years	79	18	111	36	244
Whole sample	2089	494	2390	739	5712
Definition 2					
1 year	162	31	107	36	392
2 years	183	49	166	50	508
3 years	133	40	140	49	389
4 years	89	30	121	34	292
5 years	90	18	128	47	294
Whole sample	2197	527	2527	769	6267

Table 2-2. Distribution of different types of exit by experience cohorts and types of certificates

Cohort	Stay in same field		Change field		Quit teaching		Quit school system	
	Reg.	Alt.	Reg.	Alt.	Reg.	Alt.	Reg.	Alt.
1 year (weighted)	199 (89.49)	38 (70.30)	35 (8.78)	7 (18.70)	3 (0.18)	5 (5.20)	13 (1.55)	4 (5.80)
2 years (weighted)	212 (91.30)	54 (85.90)	30 (7.20)	12 (10.50)	6 (0.50)	0 (0.20)	16 (1.00)	7 (3.40)
3 years (weighted)	160 (83.00)	56 (82.00)	40 (11.00)	9 (14.00)	8 (0.50)	4 (1.20)	23 (5.50)	7 (2.80)
4 years (weighted)	125 (83.50)	37 (83.80)	18 (12.70)	8 (14.30)	9 (2.00)	2 (0.50)	17 (1.80)	7 (1.40)
5 years (weighted)	115 (78.00)	30 (82.00)	22 (11.00)	2 (0.30)	9 (4.00)	6 (1.20)	17 (7.00)	6 (16.50)
Total (weighted)	811 (85.00)	215 (81.30)	145 (10.00)	38 (11.90)	35 (1.50)	17 (1.90)	86 (3.50)	31 (5.00)

Weighted shares of cohorts with given type of certificate in parentheses. (The weighted shares in the same row and from same certification group add up to 100%. For example, for teachers with 1 year of experience who hold regular certificates: 89.49% + 8.78% + 0.18% + 1.55% = 100%.)

Table 2-3. Odds ratios for interactions of ARC indicator with experience dummies

Experience cohort	Odds ratio		
	Diagonal	Def.1	Def.2
1 year	2.70**	2.73**	2.74**
2 years	0.86	1.11	1.02
3 years	1.49	1.33	1.25
4 years	2.41	1.07	0.83
5 years	1.37	0.85	1.31

*, **, *** − statistically significant at 10, 5, and 1% level respectively.

Table 2-4. Marginal effects for results of the logit regression estimation

Experience cohort	Diagonal	Def.1	Def.2
Male, White, “Average state”			
1 year	14%	14%	16%
2 years	-1%	1%	0%
3 years	5%	4%	3%
4 years	13%	1%	-3%
5 years	4%	-2%	4%
Female, White, “Average state”			
1 year	17%	17%	18%
2 years	-1%	1%	0%
3 years	6%	5%	3%
4 years	16%	1%	-3%
5 years	6%	-3%	5%
Male, Non-White, “Average state”			
1 year	14%	14%	15%
2 years	-1%	1%	0%
3 years	5%	4%	2%
4 years	14%	1%	-3%
5 years	5%	-2%	4%
Female, Non-White, “Average state”			
1 year	18%	17%	17%
2 years	-1%	1%	0%
3 years	6%	5%	3%
4 years	16%	1%	-3%
5 years	6%	-3%	4%

Table 2-5. Odds ratios for interactions of ARC indicator with experience dummies with new variables added

Cohort	Odds ratio Diagonal		Def.1		Def.2	
	Competitive college	Less competitive college	Competitive college	Less competitive college	Competitive college	Less competitive college
	Family income < 35 thousand					
1 year	2.63*	2.65*	2.90**	2.78*	2.70	2.44
2 years	0.99	0.68	1.37	1.07	1.32	0.59
3 years	3.13**	0.17	2.51*	0.19	2.67**	0.21
4 years	2.24	2.29	1.14	1.07	0.59	1.18
5 years	1.99	0.78	1.39	0.51	1.49	1.01
	35 thousand ≤ Family income < 50 thousand					
1 year	2.35*	2.36*	2.39**	2.29*	2.86	2.59
2 years	0.88	0.60	1.13	0.88	1.39	0.63
3 years	2.80**	0.15	2.07*	0.16	2.83**	0.22
4 years	2.00	2.04	0.94	0.88	0.62	1.25
5 years	1.78	0.69	1.14	0.42	1.58	1.07
	50 thousand ≤ Family income < 75 thousand					
1 year	3.39*	3.41*	3.12**	2.99*	3.10	2.80
2 years	1.27	0.87	1.47	1.15	1.51	0.68
3 years	4.04**	0.22	2.70*	0.21	3.07**	0.24
4 years	2.89	2.95	1.23	1.15	0.67	1.35
5 years	2.57	1.00	1.49	0.55	1.71	1.16
	75 thousand ≤ Family income < 100 thousand					
1 year	3.20*	3.22	2.87**	2.75*	2.83	2.56
2 years	1.20	0.82	1.36	1.06	1.38	0.62
3 years	3.81**	0.21	2.49*	0.19	2.80**	0.22
4 years	2.73	2.78	1.13	1.06	0.61	1.23
5 years	2.43	0.94	1.37	0.50	1.56	1.06
	Family income ≥100 thousand					
1 year	1.23*	1.24	1.13**	1.08*	1.18	1.06
2 years	0.46	0.32	0.53	0.42	0.57	0.26
3 years	1.47**	0.08	0.98*	0.08	1.16**	0.09
4 years	1.05	1.07	0.44	0.42	0.26	0.51
5 years	0.94	0.36	0.54	0.20	0.65	0.44

*, **, *** – statistically significant at 10, 5, and 1% level respectively.

Table 2-6. Marginal effects for results of the logit regression estimation with the new variables added

Experience cohort	Diagonal	Def.1	Def.2
Competitive college, Male, “Average state”, “Average income”			
1 year	13%	12%	13%
2 years	0%	1%	2%
3 years	16%	11%	12%
4 years	12%	0%	-7%
5 years	12%	3%	5%
Competitive college, Female, “Average state”, “Average income”			
1 year	16%	14%	15%
2 years	0%	2%	3%
3 years	19%	13%	14%
4 years	15%	0%	-8%
5 years	14%	4%	6%
Less competitive college, Male, “Average state”, “Average income”			
1 year	14%	12%	13%
2 years	-1%	0%	-4%
3 years	-12%	-13%	-10%
4 years	11%	-1%	2%
5 years	-3%	-10%	0%
Less competitive college, Female, “Average state”, “Average income”			
1 year	17%	15%	15%
2 years	-2%	-1%	-5%
3 years	-16%	-16%	-12%
4 years	14%	-1%	2%
5 years	-3%	-12%	0%

Table 2-7. Cumulative teacher attrition rates and exit rates by pathway for elementary, middle, and junior high school teachers in NYC, 2000-2004

Experience	College recommended	Individual evaluation	NYCTF	TFA	Temporary license	Other
	Cumulative Teacher Attrition Rates					
1 year	0.115	0.139	0.105	0.107	0.184	0.264
2 years	0.212	0.256	0.278	0.477	0.300	0.402
3 years	0.290	0.322	0.434	0.727	0.413	0.500
4 years	0.368	0.391	0.544	0.850	0.501	0.573
	Exit rates					
1 year	12%	14%	11%	11%	18%	26%
2 years	11%	14%	19%	41%	14%	19%
3 years	10%	9%	22%	48%	16%	16%
4 years	11%	10%	19%	45%	15%	15%

(Source: Boyd et al., 2006, p.23)

Table 2-8. Simulation of ARC efficiency

Variable	$E^S = 10$	$E^S = 5$	$E^S = 3$	$E^S = 2$
$w*$ (thousands)	40	40	40	40
$N*$	1000	1000	1000	1000
$N_{SF}(w_{RF})$	200	200	200	200
$N_{SF}(w_{RF})$	240	240	240	240
ΔN_{SF}	40	40	40	40
α	0.2	0.2	0.2	0.2
β	0.2	0.2	0.2	0.2
R_{ARC}	0.75	0.75	0.75	0.75
R_{TRC}	0.90	0.90	0.90	0.90
discount rate	0.05	0.05	0.05	0.05
$PV\left(w_{RF}\right)$ (thousands)	142	142	142	142
$PV\left(w_{SF}\right)$ (thousands)	145	149	152	156
w_{SF}	41	42	43	44
$MIC_{ARC}\left(C_{ARC} = 0\right)$	182	182	182	182
$MIC\left(w* \rightarrow w_{SF}\right)$	211	241	270	300
$\overline{C}_{ARC}\left(MIC_{ARC} = MIC\left(w* \rightarrow w_{SF}\right)\right)$	29	59	88	118
$MIC_{ARC}^{Attr}\left(C_{ARC} = 0\right)$	198	202	205	209
$MIC^{Attr}\left(w* \rightarrow w_{SF}\right)$	216	246	275	305
$\overline{C_{ARC}^{Attr}}\left(MIC_{ARC}^{Attr} = MIC^{Attr}\left(w* \rightarrow w_{SF}\right)\right)$	17	43	69	96
$MIC'^{Attr}_{ARC}\left(C_{ARC} = 0\right)$	215	215	215	215
$MIC'^{Attr}\left(w* \rightarrow w_{SF}\right)$	224	255	284	314
$\overline{C'^{Attr}_{ARC}}\left(MIC'^{Attr}_{ARC} = MIC'^{Attr}\left(w* \rightarrow w_{SF}\right)\right)$	10	40	69	99

Table 2-9. Cost of ARC alternative teacher certification programs

State	How much doest it cost to participate in your state's alternative certification program
Arkansas	$800 per year. Plus transportation, lodging, etc.
California	$0-$13,000
Colorado	$2,000-$7,500
Connecticut	$3,200 plus books and supplies
Delaware	About $3,000 tuition
District of Columbia	$11,000-$13,000
Florida	From 0 to $2,000+
Maryland	$600-$3,000
Michigan	$9,000-$12,000
New Hampshire	$180-$300
Oklahoma	$575
Texas	$3,000-$5,000
Wyoming	$645

(Source: http://www.teach-now.org/Table7.pdf. Last accessed September, 2009).

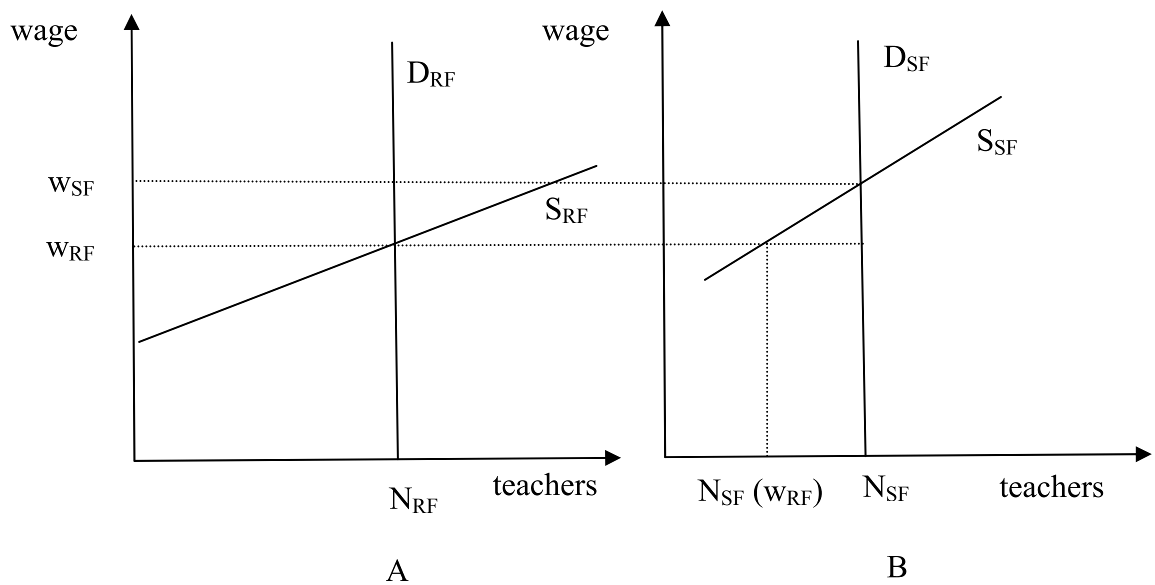

Figure 2-1. District labor market for novice teachers. A) RF equilibrium, B) SF equilibrium.

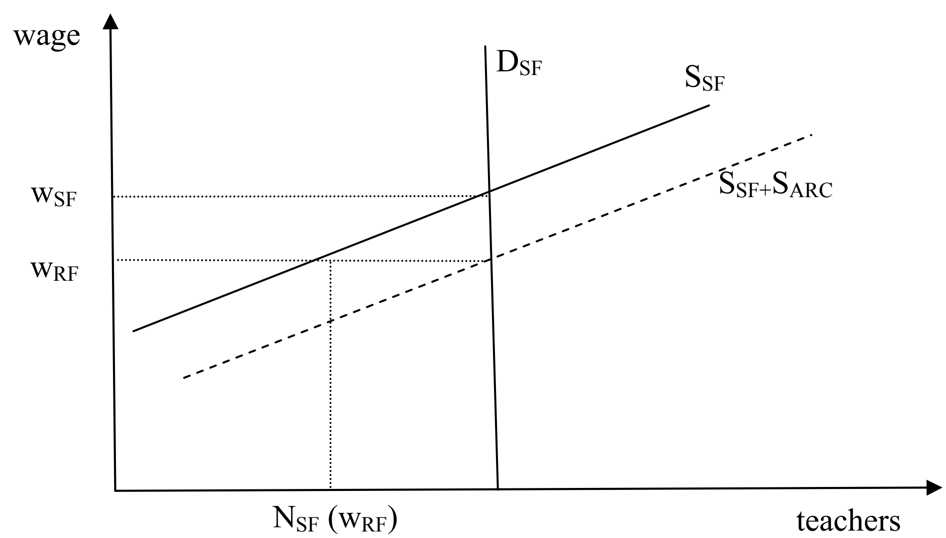

Figure 2-2. Additional supply of novice teachers in SF through ARC shifts district SF supply to the right.

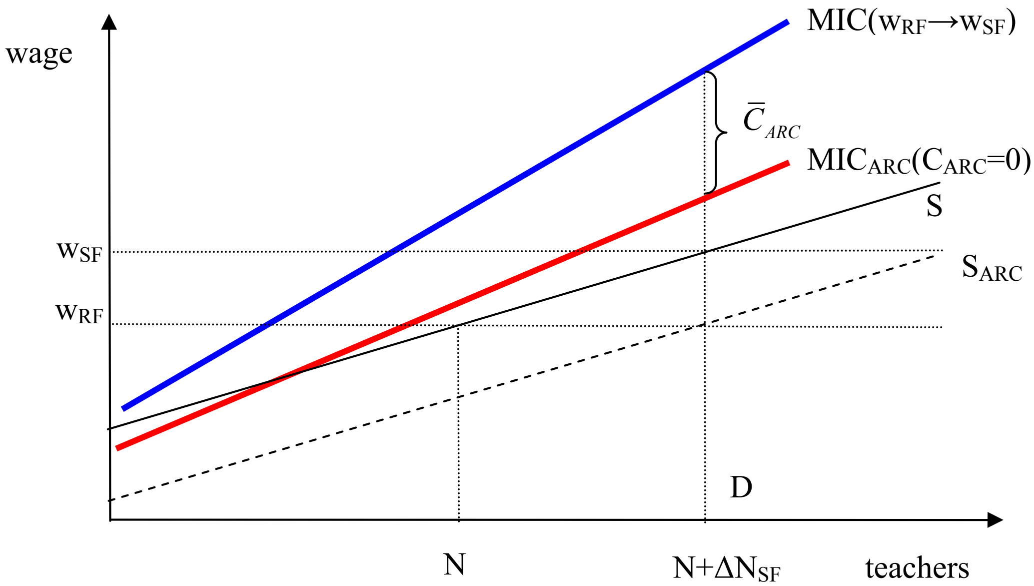

Figure 2-3. District's labor market for novice teachers.

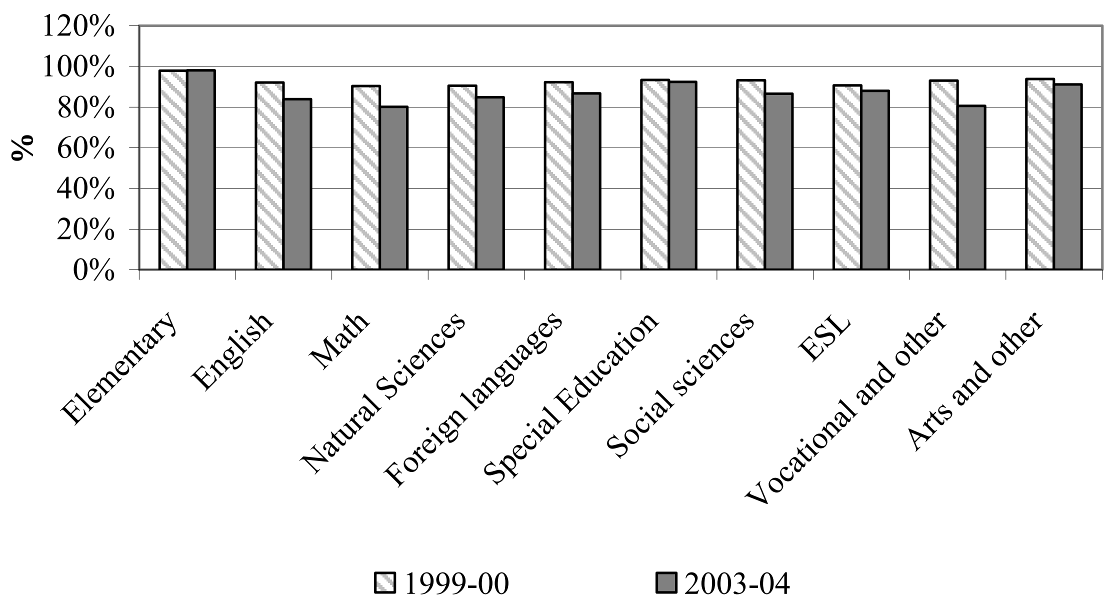

Figure 2-4. Share of teachers certified in main assignment field in 1999-00 and 2003-04 by field groups (the full SASS samples of public school teachers for the 1999-00 and 2003-04 school years are used).

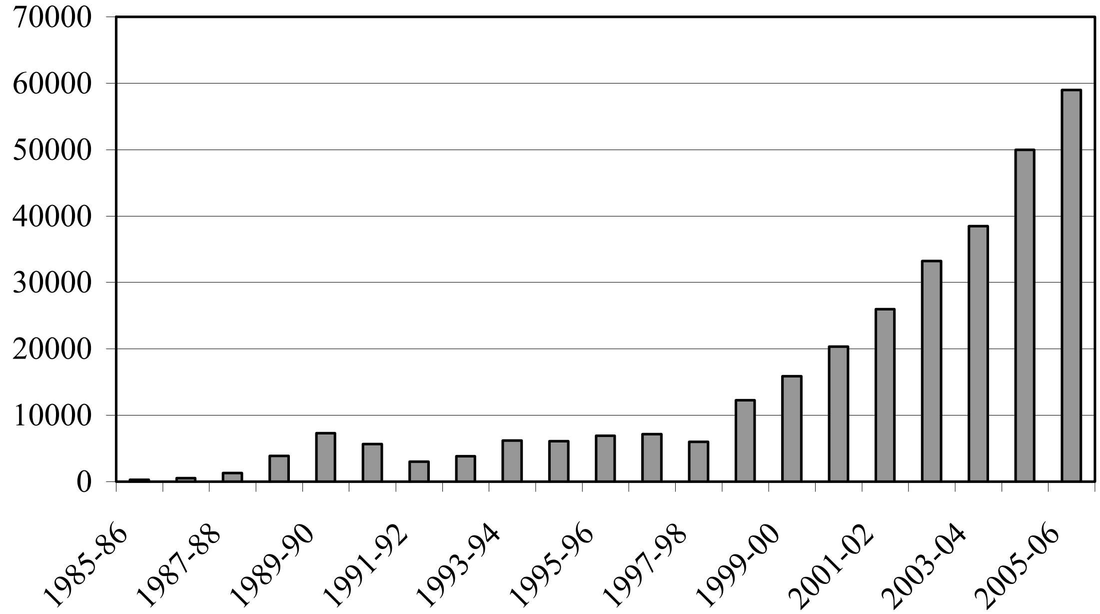

Figure 2-5. Number of individuals issued certificates through ARC by year (Source: http://www.teach-now.org/intro.cfm. Last accessed September, 2009).

TE	Year first time teaching (2003-04 SASS)					
	2004	2003	2002	2001	2000	1999
1	1	131	33	3	1	0
2	0	15	131	38	4	0
3	0	0	8	143	41	2
4	0	0	1	13	98	31
5	0	0	0	2	15	111

Figure 2-6**.** Experience matrix. TE shows years of teachers experience. For example, 33 teachers in the 2003-04 SASS reported that they have 1 year of experience and that they began teaching in 2002.

TE	Year first time teaching (2003-04 SASS)					
	2004	2003	2002	2001	2000	1999
1		1				
2			2			
3				3		
4					4	
5						5

Figure 2-7. Diagonal definition of experience cohorts.

Total experience	Year first time teaching (2003-04 SASS)					
	2004	2003	2002	2001	2000	1999
1		1	FTE=1			
2		FTE=1 / FTE=2	2	FTE=2		
3			FTE=2 / FTE=3	3	FTE=3	
4				FTE=3 / FTE=4	4	FTE=4
5					FTE=4 / FTE=5	5

Figure 2-8. Definition 1. The diagram shows that teachers whose answers to 2003-04 SASS belong to the cells adjoining to diagonal cells are redistributed to the closest diagonal cells according to their answers on the questions regarding full-time and part-time experience.

Total experience	Year first time teaching (2003-04 SASS)					
	2004	2003	2002	2001	2000	1999
1		1	FTE=1			
2		FTE=1 FTE=2	2	2	2	2
3			FTE=2 FTE=3	3	3	3
4				FTE=3 FTE=4	4	4
5					FTE=4 FTE=5	5

Figure 2-9. Definition 2. The diagram shows that teachers whose answers to 2003-04 SASS belong to the cells adjoining to diagonal cells or lay in the upper-right corner of the matrix are redistributed to the diagonal cells according to teachers' answers on the questions regarding full-time and part-time experience. All teachers who reported that have the same experience as in the diagonal cell in the same row of the matrix and who also reported they began teaching earlier than teachers whose answers belong to diagonal cell at this row, are assumed to have the same experience as teachers whose answers belong to the diagonal cell.

CHAPTER 3
THE IMPACT OF SCHOOL ACCOUNTABILITY ON TEACHER QUALITY

Introduction

For over a decade, the main education reform in the United States has been focused on efforts to improve the efficiency of school level education by imposing assessment-based accountability systems. These systems are based on the ranks or grades assigned to schools according to testing results of their students. The goal of the reform is to induce competition between schools and create incentives for them to increase efficiency. A majority of states developed some form of an accountability system in the period 1993 to 2000. These systems vary significantly across states; they utilize different mechanisms and have different targets. By 1999, almost every state had implemented state-mandated tests, as well as some form of assistance for low-performing schools (LPSs). Some states also introduced various systems of sanctions and rewards for teachers, schools, and districts based on a schools' performance.

While all the policies are focused primarily on student improvement, some may also affect, in both positive and negative ways, the supply of and demand for teachers. School rankings and systems of assistance and punishment for LPSs and school districts may encourage administrators to apply more effort to hire and attain qualified teachers, thus affecting the demand for teachers. On the other hand, these policies may change the work environment of teachers, their salaries, and pose additional requirements. They may limit a teachers' class organizing flexibility thus affecting their willingness to work, or their supply. These possible, and controversial, effects raise a number of questions concerning the impact of accountability systems on the teacher labor market. Do teachers avoid the pressure of accountability and flee from LPSs to high-performing schools (HPSs) or to other occupations outside of education? Are teachers working in various subject fields affected differently? Do districts and schools apply more effort? Have they been able to fill positions in LPSs with qualified teachers? What effects prevail?

In order to estimate the direct and indirect effects of school accountability on teachers, one has to understand the mechanisms of accountability policies. Do they affect all teachers or only particular subgroups? For example, some policies like sanctions and assistance for schools, which are assigned according to school performance, are more likely to affect teachers working in LPSs. Other policies mostly affect teachers working in particular subject fields. Almost all states have introduced testing in reading, writing and mathematics, while some of them also have tests in other fields. Certainly, teachers working in high-stakes fields whose testing results are used for school evaluation are subject to higher pressure from accountability policies than those teaching in low-stakes fields.

The determinants of teacher supply also vary across subject fields, which may cause different responses from teachers working in different fields to accountability policies. The private sector offers more job opportunities and higher wages to those who have degrees in mathematics or science. Consequently, the shortage of qualified teachers in these fields is more critical than in language arts, social sciences, and other fields. Hence, the decision to work as a teacher is driven by different factors in high-shortage and low-shortage fields. It is likely that teachers working in high-shortage fields are less sensitive to school and district policies than teachers working in low-shortage fields, since they chose teaching to alternative occupations with higher salaries.

Taking into account the variety of accountability policies and possible differences regarding their impact on teacher supply and demand, it is important to analyze policies separately and to distinguish their effects on teachers working in different types of schools and different subject fields. This chapter focuses on the analysis of the impact of different accountability policies on teacher quality, more particularly I examine the likelihood of a teacher to be certified in his or her main assignment field; the indicator reflects whether a teacher is well prepared to perform his of her teaching assignments.

Previous Literature

A number of recent studies analyzed the effect of accountability reforms on student outcomes, while only a few have addressed the potential impact of the accountability systems on the teacher labor market. Interview and survey research confirms that teachers do in fact feel the pressure of accountability (Barksdale-Ladd, & Karen, 2000). Teachers in states with stronger accountability policies feel they have less influence on setting performance standards. They also worry about the security of their job because of the performance of their students (Loeb, & Estrada, 2003). They view the high-stakes tests as an invasion of their classrooms (Luna, & Turner, 2001).

Three studies have addressed the impact of accountability on the teachers' career decisions and turnover rates. Clotfelter, Ladd, Vigdor, and Diaz (2004) and Boyd, Hamilton, Loeb, and Diaz (2008) utilize panel data sets for North Carolina and New York respectively, while Loeb and Estrada (2003) use a national-representative data set. The main issues addressed in these three papers include comparing of turnover rates between testing and non-testing grades in pre- and post-accountability periods, and estimating of the likelihood teachers leave LPSs compared to other schools. The findings are somewhat controversial. Clotfelter et al. (2004) found that in the post-accountability period teachers are more likely to transfer from LPSs to other schools or from testing grades to other grades. Boyd et al. (2008) and Loeb and Estrada (2003), on the contrary, found evidence of a positive effect of accountability for LPSs and high-stakes grades.

Boyd et al. (2008) analyze the response of teachers to the implementation of state-mandated testing in New York State using information on public school teachers from 1994 through 2002. The authors estimate the likelihood of teachers to quit the fourth grade (statewide assessment grade) using a logit model. They track turnover rates through the eight-year period and focus attention on 2 years, 1994-95 and 2001-2002, before and after the introduction of an accountability system (the accountability system in New York State placed pressure on schools and districts during 1997-98). They find that the probability of a teacher leaving was lower in testing grades and lower for teachers working in LPSs. This reduction in turnover was mostly due to a reduced number of transfers between grades rather than to a reduction of decisions to quit the teaching profession. This result is mostly driven by teachers in suburban schools. The reduction of the turnover rate was greater for novice teachers than for experienced teachers; however, more experienced teachers were more likely to leave the fourth grade in HPSs than in LPSs. The second question addressed by Boyd et al. (2008) is whether the characteristics of teachers entering fourth grade in different types of schools changed after the introduction of the accountability system. The authors found that teachers entering the testing grade in the post-accountability period were less likely to be novice and more likely to be from competitive undergraduate colleges. The analysis of the overall turnover of teachers across all grades showed no statistically significant results.

Clotfelter et al. (2004) explore the effect of accountability systems on the ability of LPSs to attract and retain quality teachers in North Carolina. They estimate the likelihood teachers quit LPSs using a discrete-time duration model with a sample of elementary school teachers who taught in the period 1994-2000. The North Carolina school accountability program called ABC (where A is for accountability, B for basic skills, and C for local control) was implemented in

1996-97, and the analysis focuses on the probability teachers quit LPSs in the post-accountability period. Results show that after the introduction of the accountability system the probability to leave the school increased for a typical teacher from 16-17% to 17-19%. For novice teachers, the effect is even more pronounced, increasing from 31-34% to 36-39%. The authors also address the question whether accountability has influenced the quality of teachers. As a measure of teacher quality, they use an indicator of whether the vacancy was filled by a teacher who transferred from within the district, which means that the teacher most likely has teaching experience. The results of a probit model show no statistically significant effect of the accountability system on the percentage of within-district transfers. Finally, the authors found that the percentage of teachers from non-competitive undergraduate colleges and the percentage of teachers without experience dropped before reform; however, after reform it increased. These results are not statistically significant, but the authors argue that the effect was not revealed only because the time period after reform was too brief to translate the changes in flows into significant changes in stocks.

Susanna Loeb and Felicia Estrada (2003) use two-period national-representative panel data to compare teachers' answers to survey questions in the pre-accountability school year (1993-94) and the post-accountability school year (1999-2000). In the post-accountability period some states had strong accountability systems while other states had weak or no accountability systems. Since accountability systems are not uniform across states, in order to make a cross-state analysis one needs a universal measure of accountability systems defined in the same terms for all states. The authors use an Accountability Index by States, constructed in Carnoy and Loeb (2003), which measures the relative strength of the state accountability systems and takes values from 0 to 5. In the absence of detailed administrative data, which tracks the career paths of teachers for all states during this period, their study focuses on teachers' responses to the survey questions: "If you could go back to your college days and start over again, would you become a teacher or not?" and "How long do you plan to remain in teaching?". The authors compared the answers to these questions in the 1999-00 and 1993-94 school years for states with a different strength of accountability in 1999-00.

Their results showed no statistically significant differences in the teachers' answers to the first question (desire to enter the teacher profession) for high- and low-accountability states. However, the results indicate that several categories of teachers are more likely to express a desire to choose the same profession in the post-accountability period compared to the pre-accountability period. These categories include teachers with less than 5 years experience, those working in urban and rural schools, teachers with less than 5 years experience working in schools with more than half of the students from low-income households, and in schools where a majority of the students are of black or Hispanic origin. Hence, these findings support the hypothesis that accountability systems have a positive effect on the teacher supply in LPSs. The authors found no evidence of a relationship between accountability and the desire to quit the teacher profession. Finally, the authors also looked at the characteristics of new entrants to teaching. They found no statistically significant relationships between the competitiveness of a teachers' undergraduate college and the measure of the accountability system.

Summarizing the findings of the previous studies, Loeb and Estrada (2003) and Boyd et al. (2005) found no evidence of change in overall teacher turnover; however, they found some positive accountability effects on teacher turnover and quality in LPSs, which with accountability are likely to have more experienced teachers and a smaller probability that teachers leave. On the contrary, Clotfelter et al. (2004) found that the probability of teachers leaving LPSs has increased due to the introduction of an accountability system. The advantage of studies based on single state data is the possibility to track the career path of teachers in detail before and after the accountability reform in the state. However, looking at a single state makes it impossible to compare different accountability policies and to analyze them separately. Both studies (Boyd et al., 2008; Clotfelter et al., 2004) are based on the analysis of the overall impact of complicated accountability systems consisting of various mechanisms. The single policies constituting these accountability systems may have opposite effects on the teacher labor market and counteract each other. Another possible pitfall of studies based on the analysis of a single state are other factors which might have affected the supply of and demand for teachers over time.

Cross-state analysis allows a comparison of different accountability systems. However, due to the lack of detailed administrative data, the Loeb and Estrada (2003) study focuses not on actual teacher turnover or quality, but on the teachers' answers concerning choosing the same profession and their plans to remain in teaching. These responses may be subjective. Moreover, the survey does not take into account those teachers who have already quit teaching due to accountability pressure. Also, the measure of accountability strength used aggregates all state accountability policies together in one index, while some of these policies may have opposite effects on teachers and counteract each other thus leading to a relatively small net effect of the whole system.

My study makes two main contributions to the area of study. Like Loeb and Estrada (2004), it utilizes a national-representative data set and compares different accountability environments. However, in order to catch the effect of different policies separately, it utilizes three dummy variables that measure the strength of different types of accountability policies instead of one aggregated index. In the absence of data for teacher turnover rates right after the implementation of an accountability system in each state, I analyze whether teacher quality has changed in the post-accountability period and whether these changes are more pronounced for states with strong accountability systems. As an indicator of teacher quality, I use the likelihood of a teacher being certified in his or her main assignment field.

Previous studies mainly focus on the analysis of the difference of accountability effects on teachers working in testing grades and non-testing grades and teachers working in LPSs and HPSs. While this study also compares the effects for different types of schools, it also takes into account potential differences of the impact of accountability across subject fields. For this purpose, 4 subject fields are used for the analysis: a high-stakes and low-shortage field (English language arts), a high-stakes and high-shortage field (mathematics), a low-stakes and low-shortage field (social sciences), and a low-stakes and high-shortage field (science). Hence, this paper adds to the existing research by analyzing the effects of different accountability policies separately and comparing the effects across different subject fields.

Types of Accountability Policies

Like Carnoy and Loeb (2003), this study utilizes a database developed by the Consortium for Policy Research in Education (CPRE) to define the different types of accountability policies which were implemented in 1993 through 1999. This dataset provides a description of the state accountability systems which were in effect in 1999. These descriptions are not uniform and, while they follow the same pattern of questions, they are not directly comparable but vary both in terminology and in level of scrutiny. As a result, a variety of policies arise, each different from the other in some details. These various policies might be arranged in groups according to the main targets to which they are applied. Thus, I define three main groups: student, school, and district-targeted policies.

Student-targeted. These policies directly affect students. Examples of such policies are testing and a system of awards and requirements for students. By 1999, testing had been widely introduced in United States. Almost all states adopted testing in reading, mathematics and writing by this time, and many states also had testing in other fields. The testing grades and form of the tests significantly varied across states. While not affecting teachers directly, testing may add additional pressure to teachers working in the high-stakes fields, especially when student test scores are used for school evaluation and the assignment of assistance or punishment to a school or district. It implies a greater responsibility and may lead to less flexibility for teachers in arranging curriculum, teaching programs, and their class. They may have to concentrate their efforts on training students to the test and apply more of their time to "difficult-to-teach" students.

The different requirements and awards for students like minimal test scores, attendance rates, summer remedial programs, stipends, and certificates increase the pressure on students and this may affect teachers. However, it is not likely to have a strong impact on teachers. Some states require students to pass the exam to graduate (exit test), which also primarily affects students, though it still might impose additional pressure on teachers.

Overall, the student-targeted policies do not affect teachers directly. However, they might reduce the teacher supply in high-stake fields by contributing to additional pressure on them and thus decreasing the attractiveness of the job. The effect is not likely to be strong, and is likely to be more pronounced in the states which also have school-targeted or district-targeted policies based on the testing results of students. While student-targeted policies are applied to all types of schools, they may affect teachers working in LPSs in a more negative way, since it is difficult to improve scores of low performing students.

School-targeted. These policies include the assignments of school rankings, different types of sanctions and assistance for LPSs, and some form of awards for HPSs. School ranking based on the test scores of students and their dynamics over the year is also one of the most widely used policies. It provides public information about school performance, and may attach a "stigma" label to a school, which may have a negative psychological effect on teachers. The systems of ranking and the approaches used to evaluate student scores and school progress over time also vary significantly across states.

In some states, the performance of particular subgroups of students (for example minority students or economically disadvantaged students) plays an important role in the ranking criteria. Rankings by themselves do not significantly affect teachers. However, in many states, the ranking is used to impose sanctions on schools which do not satisfy state standards. These sanctions can take a harsh form, like closing a school, withholding school funds, or allowing vouchers for students in LPSs to transfer to better schools. These sorts of policies are likely to reduce teachers' willingness to work in LPSs, since they may worry about the security of their job. In order to avoid this uncertainty, they might try harder to transfer to another school or to find a job outside of the school system.

Another type of sanction for LPSs has a more administrative character and may positively affect the demand for teachers. Policies like school reconstitution, the reassignment of school administration, or intervention into the management process may force the administration to try harder to fill vacancies and to retain qualified teachers. Assistance to LPSs usually takes the form of technical support, the organization of workshops, assistance in planning, and additional financing. While it does not affect teachers directly, it may improve working conditions in LPSs and thus positively affect the supply of teachers. Finally, some states have recognition or even financial rewards for teachers working in the best schools. This might affect the supply of teachers by increasing the attractiveness of working in a HPS compared to other schools. Hence, some school-targeted policies are working in different directions. Most of them are likely to reduce teachers supply in LPSs. However, administrative sanctions may boost the demand for good teachers. Hence, the overall effect of a strong school-targeted accountability system is ambiguous. However, most likely, the negative effect for LPSs will prevail.

District-targeted. These policies are less common than student- and school-targeted policies. Only a few states had some district level accountability policies, except district ranking or accreditation, in the 1999-00 school year. Mostly, these policies are similar to those of school-targeted accountability, but they are applied at the district level, rather than the school level. They include the ranking or accreditation of school districts and administrative sanctions or assistance for low-performing districts. These policies are mostly focused on improving the district management. Administrative sanctions, like the suspension of the school board, state takeover, limiting the authority of or firing the superintendent, or public hearings may force district authorities to put forth more effort to hire qualified teachers.

District-targeted policies are likely to boost teacher demand and may increase the quality of teachers, especially in LPSs. Some accountability policies may have no effect on teachers at all, while others may affect teacher shortages in different ways. All accountability policies are more likely to be pronounced for high-stake fields (mathematics and English language arts) when compared to low-stake fields, because performance in high-stake fields affect school evaluation, and therefore the assignment of awards, sanctions and assistance.

Columns 2 through 15 of Table B-1 in Appendix B illustrate the types of accountability policies discussed above that were implemented in each state in 1999-00. Construction of this table was based on information from a data base developed by the CPRE. Columns 16 through19 show three dummy variables which illustrate the strength, by state, of student, school, and district-targeted accountability policies. These dummies take the value of one if this type of accountability is relatively strong in the given state.

Student-targeted policies are considered strong if a state not only tests, but also has a relatively strong system of requirements for student promotion to the next grade or graduation. School-targeted policies are considered strong if there is a system of state-wide school evaluation and a relatively strong system of sanctions and assistance at the school level. District-targeted accountability policy is considered strong if there exists a district evaluation system and a system of administrative sanctions and assistance is well developed at the district level.

This study focuses on the effects of these three types of policies and, for comparison reasons, they are supplemented by an alternative aggregate measure of state accountability system. The aggregated index, constructed by Carnoy and Loeb (2003), is shown in the last column of Table B-1 in Appendix B. According to the analysis of the mechanisms of different policies presented in this section, one can try to predict the possible effect of each policy on the teacher labor market.

Student-targeted policies are not likely to affect the teacher labor market. However, when school- or district-targeted policies are based on the results of student testing, teachers working in a high-stakes field may experience additional pressure which may reduce the supply of teachers in these fields. Consequently, this effect is likely to be revealed by an analysis of school- and district-targeted policies, rather than student-targeted policies.

School-targeted policies may have both positive and negative impacts on the teacher labor market. However, the negative effect of accountability pressure is likely to prevail in LPSs.

District-targeted policies are focused on the improvement of district management, thus positively affecting teacher demand. All types of accountability policies are likely to be more pronounced for high-stake fields than for low-stake fields. The responsiveness of teachers to the accountability policies is likely to be higher in fields in which teacher shortage is not severe (i.e., English language arts and social sciences), rather than in high-shortage fields (i.e., mathematics and natural sciences), assuming teachers in high-shortage fields are selected from those who are more dedicated to teaching, having demonstrated low sensibility to higher pay in alternative occupations.

Empirical Strategy

One of the possible measures of teacher quality is certification. I define the dependent variable as the probability that a teacher has a regular or alternative certificate in his or her main assignment field. The teacher quality indicator takes a value of 1 if a teacher is certified and 0 if not. Most states introduced state-wide accountability policies in the period from 1993 to 1999. In order to reveal the effects of the accountability policies, I use the pooled data set which combines the 1993-94 and 1999-00 school years. A logistic probability model is used to estimate the probability of a teacher's being certified in a main assignment field. The regression equation includes year and state dummies, the subject field, and accountability policy dummies and their interactions with an LPS indicator. Since there is no available dataset providing information on school performance, it is not possible to create a precise indicator of LPSs. However, there is a correlation between the percent of economically disadvantaged students in the school and the likelihood the school performs badly. That is why I use the share of students eligible for the free lunch program as a proxy for school performance. Explanatory variables include the teacher's characteristics (sex, race, experience, age when the teacher first began full- or part-time teaching), school characteristics (ratio of free lunch eligible students, ratio of minority students, ratio of minority teachers, urban city dummy), and district characteristics (enrollment, contract with teachers' union). The model is the following

$$
\begin{aligned}
&Pr(y_i = 1 \mid x_i) = F(x'_{it}\beta) = \frac{exp(x'_{it}\beta)}{1+exp(x'_{it}\beta)}, \\
&x'_{it}\beta = \beta_0 + \sum_{j=1}^{m}\beta_j \cdot x_{jit} + \gamma_1 \cdot year_t + \gamma_2 \cdot AI_i \cdot year_t + \gamma_3 \cdot Field_{it} + \gamma_4 \cdot LPS_{it} + \gamma_5 \cdot year_t \cdot LPS_{it} + \\
&\gamma_6 \cdot Field_{it} \cdot LPS_{it} + \gamma_7 \cdot Field_{it} \cdot year_t + \gamma_8 \cdot Field_{it} \cdot year_t \cdot LPS_{it} + \gamma_9 \cdot Field_{it} \cdot AI_i \cdot year_t + \\
&\gamma_{10} \cdot AI_i \cdot year_t \cdot LPS_{it} + \gamma_{11} \cdot Field_{it} \cdot AI_i \cdot year_t \cdot LPS_{it} + \sum_{k=2}^{K}\delta_k \cdot state_k + u_{jit},
\end{aligned}
\qquad (3\text{-}1)
$$

where x_{jit} are the school, district, and teacher's characteristics; $year_t$ is a dummy variable equal to 1 for 1999-00 school year; AI_t is an accountability policy dummy or index of accountability; $Field_{it}$ is a dummy indicating the subject field in two-way comparisons; LPS_{it} is defined as the share of K-12 students eligible for free lunch; and $state_k$ is dummy for the state k. The coefficients of interest are γ_2, γ_9, γ_{10}, and γ_{11}.

I define 4 subject fields (English language arts, mathematics, social sciences, and sciences) and run the logit regressions for each of these fields separately. For these regressions that use observations only for one field, I do not include a $Field_{it}$ dummy and its interactions with other variables in the regression equation. I also run regressions for each possible pair of fields. All analyses are weighted and clustered by states. Since the model includes a large set of dummy variables, I also estimate a linear probability model with the same set of explanatory variables. Results of linear probability estimations are not reported in this study since they were consistent with the logistic probability model results.

Description of Data

The teachers, schools and district characteristics come from SASS for the 1993-94 and the 1999-00 school years. The survey consists of teacher, school, district and principal questionnaires for public and private schools. In this study, only the public school data is used.

Table 3-1 provides statistics on the certification ratios by subject field and time period. In both periods, the ratio of teachers' certification was highest in English and lowest in social sciences. Since 1993-94, the proportion of certified teachers has decreased in all 4 fields; however, the most dramatic change occurred in the high-shortage fields (mathematics and sciences). Figure 3-1 illustrates the difference in certification ratio by fields and years. Table B-2 of Appendix B shows summary statistics for variables by fields and years.

Results

The results discussed in this section include the logistic regressions, estimated for each subject field group separately, and "paired" regression estimations, where the sample for each regression includes observations representing two different groups. The full set of results is included in Table B-3 through Table B-22 of Appendix B. This section only addresses estimation results for variables pertaining to the main focus of my study.

Table 3-2 presents the estimated effects of accountability policies on the likelihood of teacher certification in 4 different subject field groups. The detailed results for these estimations are reported in Appendix B (Table B-3 through Table B-6). In general, the signs and coefficients of all explanatory variables are reasonable. Male teachers and experienced teachers are more likely to be certified, while teachers working in schools with high proportions of economically disadvantaged students, or high ratios of minority teachers, are less likely to be certified. The proportion of certified teachers is generally higher if a district has a contract with a union. Each regression includes two variables estimating the effect of the particular accountability policy on the likelihood of teacher certification. The variables include a dummy indicating that in the 1999-00 school year the state had implemented this type of policy, and the interaction of the policy dummy with the share of students eligible for a free lunch program which is used as a proxy for LPSs.

The first two rows in the Table 3-2 show results for student-targeted policies. As expected, they have no statistically significant impact on the teacher labor market. The only exception is a positive effect on social sciences teachers, which does not vary across different types of schools. Social sciences is a low-stake field, and teachers working in it are not subject to pressure from testing. However, a strong student-targeted accountability system, where students face test requirements for promotion to the next grade may make students more responsible thus creating a more comfortable environment for teachers.

The last two columns show the results for aggregated measures of the whole accountability system of the state. None of them are statistically significant. As noted earlier, the aggregate index is based on the strength of the whole system and thus treats all policies in the same way, but some of them may have quite opposite effects on teacher supply and demand. As a result, they may counteract each other. That may explain the absence of statistically significant results for the aggregated index of accountability. Contrary to expectations, school-targeted policies do not affect teacher quality. This result is somewhat unexpected and will be discussed later in this section.

The only consistently statistically significant effects of accountability can be observed for district-targeted policies. The results suggest that strong accountability at the district level negatively affects the high-stake-low-shortage subject field (English language arts), but this negative impact is mitigated for LPSs. Such impacts perfectly fit the hypothesis that teachers working in high-stake fields feel the pressure of accountability which reduces their supply. However, in the presence of strong district accountability policies, the authorities may apply more effort to hire and attain qualified teachers in LPSs thus diminishing the negative effect of accountability pressure. It also explains the positive effect of district level polices on the social sciences teachers. They do not suffer from the pressure of accountability as much as do high-stake fields teachers, and at the same time they also experience the positive effect caused by improvement of the district management.

The most striking result is that district accountability policies affect only low-shortage fields. The positive impact on science teachers is significant only at the 10% significance level and is not confirmed by the linear probability model. Since mathematics is a high-stake field, it is more likely to be affected by district policies than social sciences. However, the labor market is not the same for high-shortage and low-shortage fields. The efforts of district administrators to fill vacancies with qualified teachers are likely to be more efficient in the case of low-shortage subject fields because there are more potential candidates for these jobs in the labor market. Moreover, the pressure of accountability on teachers also varies across subject fields. First of all, there is evidence that it is easier to improve student scores in mathematics compared to reading.

Previous research on the effect of accountability on student outcomes has found that the gain in student scores in the post-accountability periods is usually larger in mathematics compared to reading (e.g., Rouse, 1998; Hanushek, & Raymond, 2004; Figlio, & Rouse, 2005; Reback, 2006). The possible explanation of this effect is that it is easier for teachers to affect mathematics scores because it is almost an entirely school-based subject, while reading skills require more at-home learning and develop over longer periods of time. Therefore, mathematics teachers are likely to be less pressed by testing standards than reading teachers. Second, high-shortage field teachers may be less sensitive to the accountability pressure because their supply is mostly driven by other labor market factors, like wages in alternative occupations and the availability of jobs outside of the school education system. Therefore, the difference in the effects of accountability policies on teachers working in low-shortage and high-shortage fields can be explained by labor market factors and by specificities of the learning process in these fields.

Table 3-3 shows the effect of district-targeted accountability practices on the likelihood of a teacher to be certified, conditional on the type of school. The likelihood of certification is calculated using logit estimation results (Table B-5 of the Appendix B). Schools where the proportion of free lunch eligible students is at or below the 25th percentile of the whole distribution are considered high-performing, while schools where the proportion of free lunch eligible students is at or above the 75th percentile are viewed as low-performing.

The results indicate that due to the pressure of accountability the quality of teachers diminishes in the English language arts fields. The ratio of certified teachers decreases by 7.5% points in high performing schools and by 4.4% points in LPSs. On the contrary, the effect of accountability is positive for social sciences, a low-stake, low-shortage field, and it increases the teacher certification ratio by 3.04% points in HPSs and by 4.4% points in LPSs. Hence, the policy was more effective for LPSs in low-shortage subject fields. For high-shortage fields, no statistically significant results were found, hence we can make no conclusions about the effects of accountability policies in these fields. There was a weak significant positive effect for the sciences field group in the logistic probability model (Table 3-2), however it was not confirmed by linear model estimations and the level of significance was only 10%.

It is also interesting to look at the comparative analysis of different field groups together. The "paired" regression estimations for all types of accountability policies are presented in Appendix B (Table B-7 through Table B-12). Since only district-targeted policies demonstrate statistically significant effects on the teachers labor market, I will only discuss "paired" regression estimation results for this type of accountability policy. The estimates regarding the variables of main interest are presented in Table 3-4. They include a dummy for district-targeted policy, an interaction term for the policy dummy and the subject field dummy (not base category), an interaction term for the policy dummy and the LPS indicator, and an interaction of the policy dummy, a field dummy, and the LPSs indicator.

Similar to the previous findings, the results show that district-targeted accountability policies mostly affect English language arts teachers (high-stake, low-shortage field). The effect was negative when compared to low-stake fields and to the high-stake high-shortage field. The negative influence of accountability policies is less pronounced for LPSs. The intuition behind these effects is the same as discussed before; English language arts teachers are exposed to more accountability pressure than teachers of mathematics and teachers working in low-stake fields. This negative influence is decreased thanks to more efficient work of district authorities, who, under the accountability system, are forced to operate more efficiently, especially with respect to LPSs. Generally, all results, except the lack of evidence for school-targeted accountability effects, are consistent with the hypotheses. Student-targeted accountability does not affect teachers. The aggregated measure of accountability system strength is not statistically significant in all regression estimations, possible due to a counteraction of positive and negative effects of some policies. Teachers working in high-stake, low-shortage fields are more affected by accountability than those working in high-shortage, low-stake field.

Surprisingly, the estimates of the impact of school level accountability do not support the hypothesis that school-targeted policies should negatively affect teachers working in LPSs. This result is somewhat unexpected because at least some school-targeted policies, like sanctions and awards assigned according to school performance, should have affected the working environment in different types of schools in different ways. Under these policies, teachers working in LPSs are likely to feel insecure about their jobs and be less flexible in their classrooms, so it is natural to assume that qualified teachers would be willing to transfer to other schools to avoid additional pressure. However, we observe no such effect. One possible explanation is that in small school districts, teachers may not be able to transfer schools without changing their place of residence. Therefore, the effect of school-targeted policies may be strong only in large school districts, where teachers can switch from one school to another without moving to another residence. In order to check whether school-targeted policies do in fact affect teacher quality in large districts, I include controls for the district size in the same set of regressions. The results are presented in the next section.

Results with Controls for District Size

I ran the same regressions for each subject field group separately, but I controlled for the possible differences in the impact of accountability policies in small and large districts. For this purpose, I include in each regression equation two additional variables: an interaction term of a policy dummy and district enrollment, and an interaction term of a policy dummy, a LPS indicator and district enrollment. The detailed results of the regressions estimated for each field group are presented in the Appendix B (Table B-13 through Table B-16). The estimates regarding variables of main interest are summarized in Table 3-5. As in previous findings, the effects of student-targeted accountability and aggregated policies are, in general, mostly statistically insignificant. The only statistically significant result confirmed by both logistic and linear probability models is a positive effect of the aggregated index of accountability on science teachers' certification in large districts.

School-targeted policies still show no statistically significant results for English language arts or mathematics fields. A policy dummy and an interaction of the policy variable with an LPS indicator remain insignificant for all fields. However, school level accountability has a positive effect, in large districts, on the likelihood that social sciences teacher are certified. This effect is smaller for teachers working in LPSs. It corresponds to the hypothesis that due to the negative effect of accountability, qualified teachers tend to transfer from LPS to better performing schools. Also, in large districts, school-targeted policies increase the likelihood that science teachers will be certified if they work in LPSs. Since the sciences represent a high-shortage field, the labor market of teachers may be larger in large districts. Therefore, while it may be not possible to hire qualified teachers for LPSs in small districts even with the additional effort by district authorities, it may be easier to do so in large districts where there is a relatively larger labor market for teachers.

Table 3-6 shows the effect of school-targeted accountability on the likelihood that a teacher is certified, conditional on the type of school and size of district. The discrepancy between low-performing and HPSs is greater for social sciences teachers in large districts compared to small districts. In general, teachers are more likely to be certified in large districts (probably due to a larger local teacher labor market); however, teachers working in LPSs in large districts are less likely to be certified compared to teachers in HPSs. We do not observe such a difference in small districts. This result confirms the hypothesis that certified teachers tend to flee from LPSs when there is a choice of better performing schools in the vicinity. Due to the accountability policies in LPSs, the probability that a teacher is certified is lower for those sciences teachers working in small districts compared to teachers in large districts.

The third section of Table 3-5 presents the results regarding district-targeted policies. The results suggest that due to district level accountability, the likelihood low-shortage field teachers are certified is higher in large districts. This difference also can be explained by a larger local labor market of teachers in large districts. The most striking result is that district accountability has opposite effects on English and mathematics teachers. While English teachers, on average, are negatively affected by district accountability, there is a positive impact on mathematics teachers' certification ratio. Following the same logic as in the previous section, both the sensitivity of these two groups of teachers to the pressure of accountability and their labor markets may differ. Since it is easier for a teacher, in terms of quantitative scores, to increase student test scores in mathematics, math teachers do not feel the accountability pressure as much as English language arts teachers. At the same time, the district authorities apply more effort to hire and retain teachers in both high-stake fields. Therefore, since the administration operates more efficiently under a strong district level accountability system, we can observe an overall positive effect on mathematics teachers. However, district efforts are not enough to fully offset the negative impact of accountability pressure on English arts teachers, which leads to a negative net effect for them.

In the large districts, the negative impact of policy on English teachers is decreased thanks to a larger labor market of teachers. However, teachers working in low performing schools are more exposed to the pressure of accountability and tend to transfer to better schools. Therefore, in large districts, the positive effect of district-targeted policies is less for teachers working in LPSs. On the contrary, since in large districts mathematics teachers have more job opportunities outside of the education system, the positive effect of district-targeted accountability policies on the mathematics teachers is less in large districts than in small districts. Also, in the case of mathematics teachers who are less sensitive to accountability pressure, district-targeted policies are more efficient in LPSs.

Table 3-7 shows the effect of district-targeted accountability on the likelihood that a teacher is certified, conditional to the type of school and size of the district. In HPSs in large districts, the negative impact of accountability on the quality of English language arts teachers fall from 9.6% points to 5.6% points. However, there is no noticeable difference between large and small districts in the certification ratio of LPS teachers. In large and small districts, accountability positively affects the likelihood that a teacher in mathematics is certified. However, in large districts, the effect diminishes from 6.7% points to 3.4% points. For LPSs, the drop in the certification ratio is less dramatic in large districts compared to small ones; it decreases only by 0.5% points.

To understand better the effects of school and district-targeted accountability I ran "paired" regression estimations for each type of accountability policy with controls for district size. The detailed results are presented in the Appendix B (Table B-17 through Table B-22). This section focuses only on the results for the school and district-targeted policies, because the effects of the student policies and aggregated index are, in general, statistically insignificant. The estimates for school-level accountability effects are summarized in Table 3-8.

In all subject fields school-level accountability in general decreases the probability that a teacher will be certified. Because of a larger local teacher market, the negative effect of the school-targeted policies is lower in large districts when compared to small ones. However the difference between the certification ratio of small and large districts is lower for high-shortage fields than for low-shortage fields. These results can be explained by the fact that while large districts have a bigger local market of teachers, there are more alternative occupations outside of the education system. Hence, high-shortage teachers have, in general, more opportunities to quit teaching and escape accountability pressure. In large districts, social science teachers working in LPSs are less likely to be certified compared to teachers in other schools. These results confirm the hypothesis that qualified teachers are less willing to work in LPSs and tend to transfer to better schools. Table 3-9 presents the results of district-targeted policies for pairs of fields. These results are consistent with previous findings that suggest district accountability reduces the certification ratio of English teachers and boosts the certification ratio of mathematics teachers. This difference is less pronounced in large districts and in LPSs.

Conclusions and Directions for Further Work

The goal of school accountability reform is to improve student outcomes. However, the mechanisms and nature of accountability policies affect not only students but also teachers. This study analyses the impact of accountability on teacher quality. In contrast with previous research, the effects of different policies are measured separately and not only for high-stake fields, but also for low-stake fields.

The results suggest that school and district-targeted accountability do affect teachers in all subject field groups, while accountability at the student level affects teachers only when it is linked to school or district level policies. The aggregated measure of the strength of the whole accountability system reveals no statistically significant effects of accountability on the teacher labor market. Apparently that aggregated measure is not the best way to estimate the impact of accountability on teacher certification. It includes all types of accountability policies, some of which do not affect teachers and some that may affect them in opposite directions.

School-targeted policies affect the teacher labor market only in large districts and mostly in low-shortage fields. This effect decreases the quality of teachers in LPSs. In an effort to avoid the pressure of accountability, qualified teachers prefer to work in HPSs where the threat of school sanctions is lower. This effect takes place only in large districts because in small districts teachers have limited choices of schools and cannot easily transfer to another school.

The effects of district-targeted policies vary significantly across different subject fields. In general, they affect high-stake fields; however, the nature of the effect is quite different. While for English language arts teachers the negative effects of accountability pressure prevail, on the contrary, the quality of mathematics teachers increases in states with strong district-targeted accountability policies. These opposite effects suggest a relatively higher sensitivity of low-shortage field teachers to the pressure of accountability. Perhaps they may be explained by two factors. First, it is more difficult to improve student performance on tests in language arts than in mathematics. Second, the high-shortage field teacher supply might be less sensitive to accountability policies than the low-shortage field. Therefore, the effect of accountability in low-shortage fields is driven primarily by the negative impact of accountability pressure, which is lessened in LPSs and in large districts by the positive effect of district management improvements. However, in high-shortage fields the negative effect of "accountability pressure" is weak, and overall the effect is positive due to district efforts to find qualified teachers.

Another source of difference in teacher responses to district and school level accountability arises from the difference of the labor markets for low-shortage and high-shortage fields. Teachers with degrees in high-shortage fields have more alternative job opportunities outside of the school system compared to teachers working in low-shortage fields. This difference is especially apparent in large districts. Therefore, while the school and district level policies are relatively more effective in large districts for teachers working in low-shortage fields, they are less effective for those working in high-shortage fields.

Further research should more fully define the accountability policy variables for the 1993-94 school year. The database used in this study contains information only for the 1999-00 school year accountability systems. The assumption for the 1993-94 school year is that accountability policies had not been implemented. However, that is not true for all types of accountability policies. Some states implemented testing or some form of accreditation and LPS assistance before the 1993-94 school year. Hence, it would be useful to more precisely define the policies variables. It is also of interest to try other definitions of teacher quality and to analyze the same models with other dependent variables. These possible measures of shortage may be include the correspondence of a teacher's bachelor's degree to his assignments in school, the competitiveness of a teacher's undergraduate college, a teacher's decision to quit teaching or to move to another school. Another interesting direction for further research is to analyze the effect of school-targeted accountability using more disaggregated accountability policy variables, such as sanctions, awards and assistance. These policies may have positive and negative effects on teacher willingness to work in LPSs, and it might be useful to look at their effects separately. Since school-level policies affect teacher quality only in large districts, this analysis should focus only on the large districts.

One of the most interesting findings of this study is the different effects of the school and district-targeted policies on English language arts teachers and mathematics teachers. To better understand the nature of teacher responses to the policy, and to analyze whether the reason for the different reaction is in fact explained by the difference in the teacher labor market for low-shortage and high-shortage fields, it would be interesting to analyze teacher movements from one school to another or to an occupation outside of the school system in large districts, conditional on type of school and subject field.

Table 3-1. Teachers' certification ratios by year and field

Field	1993	1999	Change
English language arts	94.9%	90.5%	-4.4%
Mathematics	93.6%	87.7%	-5.9%
Social sciences	91.3%	86.9%	-4.5%
Sciences	93.7%	87.4%	-6.3%

Table 3-2. Logit results for regressions estimated separately for each field group

Variable	English language arts	Mathematics	Social Sciences	Sciences
	odds ratio	odds ratio	odds ratio	odds ratio
Student policy	0.86	0.64	2.25*	1.48
Student policy * LPS	1.86	2.75	0.63	0.91
School policy	0.76	0.53	0.88	0.59
School policy * LPS	1.68	0.80	0.67	1.73
District policy	0.25***	3.00	2.18***	0.53*
District policy * LPS	5.10***	0.64	3.00	1.27
Accountability Index	1.12	0.95	1.03	0.87
Accountability Index * LPS	0.75	0.71	0.94	1.14
Number of observations	8747	6547	5593	5794
State fixes effects	yes	yes	yes	yes
Weights	yes	yes	yes	yes

*, **, *** – statistically significant at 10, 5, and 1% level respectively.

Table 3-3. The effect of district-targeted accountability on the likelihood of teacher certification

Field	HPS	LPS
English language arts	-7.50%	-4.40%
Mathematics	4.93%	6.04%
Social Sciences	3.04%	4.40%
Sciences	-4.54%	-4.51%

Table 3-4. Logit results of regressions estimated for pairs of field groups (effect of district-targeted policies)

Variable	English lang. arts (base) vs. Mathematics	English lang. arts (base) vs. Social Sc.	English lang. arts (base) vs Sciences	Math (base) vs. Social Sc.	Math (base) vs. Sciences	Social Sc. (base) vs. Sciences
	odds ratio	odds ratio	odds ratio	odds ratio	odds ratio	odds ratio
District	0.43*	0.37*	0.24***	2.14	2.72	1.65**
District * Field	4.53***	3.67*	2.29**	0.51	0.76	0.61
District * LPS	3.56***	3.94**	3.94***	0.61	0.60	2.03
District * LPS * Field	0.30	1.40	0.61	2.14	5.31	0.47

*, **, *** – statistically significant at 10, 5, and 1% level respectively.

Table 3-5. Logit results for regressions with controls for district size estimated for each field group separately

Variable	English language arts	Mathematics	Social Sciences	Sciences
	odds ratio	odds ratio	odds ratio	odds ratio
Student policy	0.90	0.63	1.99	1.28
Student policy * LPS	1.48	1.46	0.66	0.75
Student policy * bigD	1.28	2.23	1.45	1.69**
Student policy * bigD *LPS	0.87	0.76	0.72	1.03
School policy	0.55	0.52	0.67	0.63
School policy * LPS	1.49	0.44	1.08	1.00
School policy * bigD	3.24	1.71	1.96**	0.91
School policy *bigD *LPS	0.37	0.67	0.35***	1.76***
District policy	0.20***	6.55***	1.49	0.43
District policy * LPS	9.39***	0.12*	1.79	1.52
District policy * bigD	22.37***	0.01***	2.76*	7.72
District policy *bigD *LPS	0.01***	4.49***	0.01	0.28
Accountability Index	1.00	0.89	1.00	0.79
Accountability Index * LPS	0.97	0.89	0.95	1.22
Accountability Index * bigD	1.16*	1.01	1.06	1.20***
Accountability Index *bigD *LPS	0.78***	0.95	0.96	0.79**
Number of observations	8747	6547	5593	5794
State fixes effects	yes	yes	yes	yes
Weights	yes	yes	yes	yes

*, **, *** – statistically significant at 10, 5, and 1% level respectively.

Table 3-6. The effect of school-targeted accountability on the likelihood of teacher certification conditional on the size of the district

Field	Small district		Large district	
	HPS	LPS	HPS	LPS
English language arts	-2.6%	-2.6%	-1.7%	-1.9%
Mathematics	-7.3%	-11.7%	-6.2%	-10.9%
Social sciences	-2.0%	-2.0%	-1.5%	-1.8%
Sciences	-2.6%	-3.2%	-2.7%	-3.1%

Table 3-7. The effect of district-targeted accountability on the likelihood of teacher certification conditional on the size of the district in percentage points

Field	Small district		Large district	
	HPS	LPS	HPS	LPS
English	-9.6	-3.6	-5.6	-3.5
Math	6.7	5.6	3.4	5.1
SS	2.4	3.2	4.5	5.0
Sciences	-5.7	-5.4	-3.3	-3.3

Table 3-8. Logit results for regressions estimated for pairs of field groups for school-targeted policies, conditional on district size

Variable	English vs. Math	English vs. Social Sc.	English vs. Sciences	Math (base) vs. Soc. Sc.	Math (base) vs. Sciences	Social Sc. vs. Sciences
School policy	0.51***	0.53**	0.59	0.59	0.61	0.88
School policy * Field	1.07	1.42	1.02	1.22	0.91	0.67
School policy * LPS	1.26	1.95	1.42	0.66	0.53	0.59
Sch. policy *LPS * Field	0.45	0.31	0.62	0.80	1.54	1.99
Sch. policy *bigD	3.08*	3.43*	3.24**	1.68	1.77	1.78***
Sch. policy *bigD * LPS	0.37	0.28	0.36	0.64	0.65	0.47***
Sch. policy * bigD * Field	0.69*	0.56	0.25***	0.99	0.43***	0.42***
Sch. policy *bigD*LPS* Field	1.44	1.58	5.71***	0.83	3.28***	3.89***
Number of observations	15379	14340	14571	12140	12341	11417
State fixes effects	yes	yes	yes	yes	yes	yes
Weights	yes	yes	yes	yes	yes	yes

*, **, *** – statistically significant at 10, 5, and 1% level respectively.

Table 3-9. Logit results for regressions estimated for pairs of field groups for district-targeted policies, conditional on district size

Variable	English vs. Math	English vs. Social Sc.	English vs. Sciences	Math vs. Social Sc.	Math vs. Sciences	Social Sc. vs. Sciences
District policy	0.41**	0.34**	0.20***	6.42***	4.48***	0.97
District policy * Field	10.18***	2.01	1.95	0.20*	0.21*	1.06
District policy * LPS	4.10***	5.10***	6.69***	0.09**	0.14**	3.09
District policy *LPS * Field	0.07***	2.25	0.55	42.52***	8.58*	0.27
District policy *bigD	8.00***	5.51	11.28***	0.01***	0.01***	2.99
District policy *bigD * LPS	0.03**	0.03	0.01***	7.05***	4.80***	0.01*
District policy * bigD * Field	0.01***	3.51	5.64	8.81***	2.91***	0.01*
District policy *bigD*LPS* Field	5.04***	0.01**	0.70	0.01***	0.01**	4.54***
Number of observations	15379	14340	14571	12140	12341	11417
State fixes effects	yes	yes	yes	yes	yes	Yes
Weights	yes	yes	yes	yes	yes	yes

*, **, *** – statistically significant at 10, 5, and 1% level respectively.

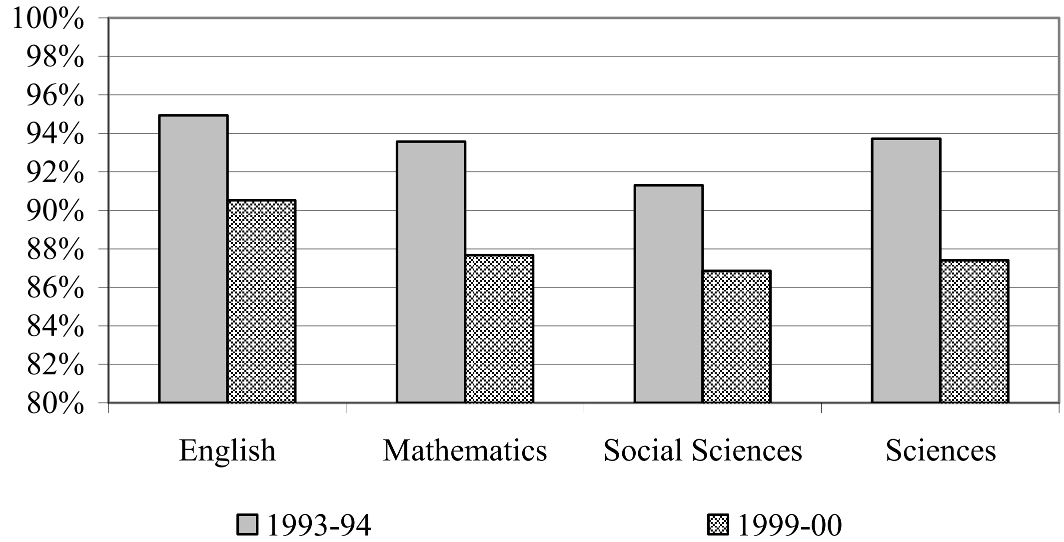

Figure 3-1. Teachers' certification ratio by fields in the 1993-94 and 1999-00 school years.

CHAPTER 4
INVESTMENT STRATEGIES OF PUBLIC PENSION FUNDS

Introduction

During the late 1990s and into the 2000s, many public pension funds shifted toward more aggressive investment strategies. Though some of them simply increased their holdings of stocks relative to bonds, others emulated the Swensen model, sometimes known as the Yale model, which became popular because of the consistent high returns obtained by David Swensen, who took charge of investing Yale's endowment in 1985 and recently left. The idea was that funds that have limited need for liquidity should put substantial shares of their portfolios in illiquid assets, such as real estate and hedge funds, thereby gaining higher expected returns with little increase in risk.

The timing of this shift by public pension funds was unfortunate, causing them to be heavily invested in illiquid assets just as the financial crisis of 2008-09 froze their markets, making them impossible to value. Many public pension funds suffered enormous losses. According to Bonafede, Foresti, and Browning (2009) 93% of 59 state retirement systems that reported their actuarial data for 2008 were underfunded. The average assets-to-liabilities ratio has decreased from 94% in 2007 to 77% in 2008. Many big state retirement systems experienced a decline in assets of about 20-30%. The State Board of Administration that manages many of Florida's public investments had a $62-billion loss in assets which constitutes about a third of their value (Freedberg, & Humburg, 2008). The sharp decrease in the assets was caused by higher-risk investments which had been projected to provide high profits, but generated big losses instead. The Colorado Public Employees Retirement Association that had $41.4 billion in assets in December 2007 lost more than $10 billion last year and now can meet only 51.8% of its obligations (Hoover, 2009). Its 26% loss in assets is a result of decisions made in the 1990s to lower contribution rates and to project an 8.5% return on investments. New York State's pension fund lost 26.3% of its assets' value (Hakim, 2009). The California Public Employees Retirement System (Calpers) assets value fell 23.4% last year and assets of the California State Teachers Retirement System (Calsters) shrank by 27% (Garrahan, 2009).

Standard actuarial practices coupled with legal funding requirements will force public pension funds—unless there is a remarkable market recovery—to siphon off much larger shares of state and local revenues just when those revenues have been driven down by the recession and by falling property values. In addition, public employees are being required to accept pay cuts, increased pension contributions, and involuntary part time. The impact of the losses by public pension funds on state and local governments would have been far worse had it not been for aggressive monetary and fiscal policies at the federal level. The Federal Reserve, by doubling its balance sheet, re-established liquidity in frozen markets and created arbitrary values for trouble assets. The Congress authorized a stimulus package of some $800 billion, including $58 billion to state and local governments. Without such federal support layoffs by state and local jurisdictions would have been greater and the feedback into the national recession larger. Even with the federal assistance, state and local governments are likely to experience budget difficulties for several years, in substantial part because of the need to rebuild their pension assets.

The federal government was able to provide an effective stimulus package without escalating long-term interest rates, which would have made the federal debt unsustainable and thus killed the stimulus, only because China and Japan were willing to continue to hold and indeed even to absorb more U.S. Treasury securities. No one can be certain that they will do the same again if in a few years there is another crisis. Indeed, China's central bank has called for a global reserve currency to replace the dollar, and French President Nicolas Sarkozy has stated the costs of the dollar as a reserve currency may exceed its benefits. If the next financial crisis is also a dollar crisis, instead of one in which the dollar benefits as a safe haven, the ability of the federal government to rescue state and local governments from stress caused by their pension funds will be severely limited.

For this reason, it has become an urgent policy issue to understand why pension funds invest aggressively, putting local jurisdictions at greater risk of experiencing serious financial difficulties. Logically, the more fundamental question is what is the optimal position for public pension funds to take on? I do not, however, address that question directly in this chapter. If pension funds use rate of return that is higher than risk-free rate the ethical problem arises as current taxpayers who are consumers of product of labor of public employees may shift risks of the pension fund investments to future taxpayers. Intergenerational equity requires that each generation of taxpayers pays for the public services it consumes contemporaneously. Therefore, one school of thought contends that since public pensions are a very certain obligation—historically they have always been paid—the appropriate rate of return to project for pension portfolios is that on the safest asset, U.S. Treasury securities (Clark, Craig, & Ahmet, 2008; Waring, 2008). However, currently this is not the case. While the nominal market rate for fixed income securities is no greater than 5%, public pension plan actuaries use nominal rates in the neighborhood of 8% (Clark et al., 2008).

However, the question of intergenerational equity may be viewed from another perspective. New generations usually consume more than the previous ones. Hence if tax payments of the current taxpayers cover all future benefit payments to current public employees and the risks are not shifted to future taxpayers it may make the current generation relatively worse off in the context of intergenerational equity and cause larger tax distortions, thus reducing welfare. Therefore, another school of thought places optimal public pension investing into the context of tax smoothing or, more generally, tax-and-expenditure smoothing. According to this view, if future taxpayers could determine current investment strategy, they would balance the expected rate of return (ERR) against the risk of distortions caused by higher taxes to meet required pension obligations. Yet another view says that though current beneficiaries share no risk, future beneficiaries who are currently public employees do share the risk of lower pay or higher contributions should investment returns sour before they retire. An appropriate investment strategy, by this view, should include their utility among its concerns.

Actually the same debates are recently going regarding appropriate discount rates that should be used to evaluate future losses caused by global warming. Stern (2006) has used a very low discount rate of 1.4% to estimate the present value of damages of global warming. Nordhaus (2006) criticized this approach to discounting arguing that too low a discount rate would lead to average annual consumption losses over the indefinite future. Taking into account that Stern projects that future generations will be richer than the current generation (per capita consumption would grow at 1.3% per year and would increase from $7,800 today to $94,000 in 2200), a cut in consumption today in favor of future generations looks contradictory to the concept of intergenerational equity. Instead of the 0.1% rate of social time preference used by Stern, Nordhaus suggests a 3% rate that is consistent with a logarithmic utility function, market interest rates and rates of private savings and investment. Other economists have supported Nordhaus' critiques of the low discount rate. Mendelson (2008) claims that "low discount rates are equitable only in the sense that they make every generation worse off" (p.52). He notes that since market assets earn a higher rate of return than if investment dollars are spent on abatement of greenhouse gas emissions the opportunity cost of losing future earnings is high (p.52). Weyant (2008) also supports using the market rate of return rather than a low rate based on the intergenerational equity concept: "The essential problem I have with *Review's* use of a purely ethically based rate of time preference is that it leads to a rate of return on capital that is inconsistent with the currently observed rate of return on capital" (p.90).

Hence the use of a rate of return that corresponds to risk-free assets for evaluation of public pension funds future earnings would lead to cuts in current consumption, high opportunity costs, and higher tax distortions. Therefore, the optimal rate of return in the general case would always be higher than the risk-free rate (except for special cases such as infinite risk-aversion of taxpayers). In a perfect world it would not be difficult to define the optimal rate of return. According to theory individuals make optimal choices and in a first-best context each individual just chooses the optimal investment strategy to provide appropriate savings for retirement. However, as Barr and Diamond (2008) note, in reality there are too many deviations from a simple theoretical world. Therefore due to limitations and market imperfections, such as imperfect information, incomplete markets and progressive taxation, the policy should be formulated in a second-best context.

Under the tax-smoothing approach it is not surprising that the overwhelming majority of pension funds does not invest entirely or even mostly in Treasury securities or base their projected returns only on such securities. As an empirical matter, in the United States at least, there is little to be learned from trying to distinguish funds that follow such a strategy from those that do not, since virtually none do. That being the case, I will explore pension returns in the context of tax smoothing approach. A number of previous studies have modeled the optimal investment strategies of public pension funds using a tax distortion approach. Epple and Schipper

(1981) provided such a model as an explanation that underfunding of public pension funds may be optimal. D'Arcy, Dulebohn, and Oh (1999) presented a model to show that underfunding of public pension funds is optimal when the growth in pension costs over time is below the growth in the tax base. Recently Lucas and Zeldes (2009) provided a model of optimal investment strategy for public pension funds deriving the optimal condition for the share of investments in stocks that minimizes tax distortions.

I use Lucas and Zeldes (2009) theoretical framework to analyze why pension funds may be using overestimated rates of return to discount their liabilities. Following this framework I consider the effect of a pension board's directing actuaries to use a projected rate of return that is too high in the sense that it does not accurately reflect the actual tradeoff between risk and return available from the financial markets and or, alternatively, the true cost of tax distortions. The model itself has the very straightforward implication that underestimating risk aversion or underestimating the risk associated with a given ERR, which of course leads to using a projected rate of return that is too high, results in excessive hiring of public employees. (It would also lead to setting their pension/current salary ratios too high, but I do not dwell on that.) The point is that in addition to the macroeconomic risks associated with projected rates of return that are set too high, there is also misallocation of public resources.

In the empirical portion of this question, I address the issues of whether union strength is associated with a high or a low ERR. I proceed with the assumption that many projected rates of return are too high, since there is a considerable variability in the projected rates of return used by public pension funds. My hypothesis is that union strength leads to a higher projected rate of return, since unions will gain from the resulting underestimation of the true cost of hiring more employees or giving existing ones larger benefits. It is also possible that strong unions would oppose higher projected rates of return, and the riskier portfolios associated with them, because they wish to avoid the risk of larger contributions or reduced benefits if markets crash. I do not think that is the case: in most unions older workers, who face less risk of either higher contributions or reduced pensions, are in charge, at least according to conventional wisdom. But the issue is an empirical one. I nest the two hypotheses, allowing the data to reject either one or neither.

What I find is that, as I expected, greater union strength is associated with public pension funds that allocate their portfolios toward riskier assets. My results are merely indicative, rather than definitive, for various reasons. First, I am not able to use projected rates of return directly but must construct proxies for them using portfolio allocations. Second, because of data limitations, I use ordinary least squares with many covariates rather than better ways of achieving identification, such as difference in difference, regression discontinuity, or instrumental variables. Third, one of my observation years extremely over-represents pension funds in a single state (Pennsylvania). Given the urgency of the issue, however, even a preliminary result associating risky strategies by public pension funds with union strength is an important finding.

Theoretical Background

Under the tax-smoothing approach Lucas and Zeldes (2009) suggest an optimal pension fund asset allocation model that minimizes the welfare cost of distortion taxes for two periods. The model is based on a trade-off between the higher average return on equities which lowers average taxes and the greater risk of equities which increases expected tax distortions. The optimal share of stocks in the portfolio of the pension fund is found by minimizing tax distortion in two periods. Lucas and Zeldes (2009) formulate the following model:

$$
\begin{aligned}
&\min\left(E\left[\left(T_1+\frac{w}{2}\cdot T_1^2\right)+\beta\left(T_2+\frac{w}{2}\cdot T_2^2\right)\right]\right), \text{ s.t.}\\
&T_1=C_1+\theta_1,\\
&T_2=C_2+\theta_2,\\
&C_2=L_1(1+\gamma)-X\left[1+r_f+\lambda\left(r_s-r_f\right)\right],
\end{aligned}
\tag{4-1}
$$

where T_i is total taxes paid in period i and is equal to the sum of pension contributions C_i and other taxes θ_i, β is a subjective discount rate, and w is a curvature parameter (the marginal tax rate is assumed to be proportional to total tax collections). Contributions in the second period, C_2, must be equal to the underfunding that occurs in the second period. L_i represents liabilities in period i, future liabilities are stochastic with expected growth rate $E(\gamma)$, and standard deviation of growth $\sigma(\gamma)$. X shows invested funds in the first period and is the sum of pension assets in the first period, A_1, and contributions in the first period, C_1, minus net payments in the first period, B_1. The constant risk-free rate is given by r_f, stock returns are stochastic with an expected return $E(r_s)$ and standard deviation $\sigma(r_s)$.

The solution of the model 4-1 shows the optimal fraction of pension assets invested in stocks λ:

$$
\lambda=\frac{\frac{\left(E(r_s)-r_f\right)}{w}+E\left(\theta_2\left(r_s-r_f\right)\right)+E\left(L_1(1+\gamma)\left(r_s-r_f\right)\right)-X\left(1+r_f\right)\left(E(r_s)-r_f\right)}{X\left(E\left(1+r_s\right)^2-\left(1+r_f\right)^2\right)}. \tag{4-2}
$$

In the general case it is optimal to hold some stocks (λ>0)[14]. So it is not surprising that on average the ERR used by pension funds exceeds the risk-free rate. However, there exists considerable variation in ERR across public pension finds. For example, data collected for 2006 for local pension funds by the Center for Retirement Research (CRR) at Boston College shows that ERR varies from 6% to 8.8% (the sample consists of data for 87 local pension funds). If

[14] The share of stocks is zero only if there is strong negative correlation between other tax distortions and equity returns, or, if there is no correlation between other tax distortions and equity returns, if risk aversion is approaching infinity, future tax liabilities are zero and the plan is fully funded (Lucas, & Zeldes, 2009).

public pension funds use the optimal ERR why does it vary across funds? It looks like some public pension funds may use an ERR that exceeds the optimal ERR and therefore causes larger tax distortions. Further in this chapter I analyze why that may happen.

The share of stocks is negatively related to the volatility of stock returns $E(1+r_s)^2$, which can be expressed as a function of the standard deviation of the stock returns:

$$E\left(1+r_s\right)^2=\sigma\left(r_s\right)^2+\left(1+E\left(r_s\right)\right)^2. \tag{4-3}$$

Let express the optimal share of stocks as a function of standard deviation of the stock returns:

$$\begin{aligned}
&\lambda=\frac{Y}{\sigma\left(r_s\right)^2+Z},\\
&Y=\frac{\left(E\left(r_s\right)-r_f\right)}{w\cdot X}+\frac{E\left(\theta_2\left(r_s-r_f\right)\right)+E\left(L_1\left(1+\gamma\right)\left(r_s-r_f\right)\right)}{X}-\left(1+r_f\right)\left(E(r_s)-r_f\right),\\
&Z=2\left(E\left(r_s\right)-r_f\right)+\left(\left(E\left(r_s\right)\right)^2-r_f^{\,2}\right).
\end{aligned} \tag{4-4}$$

Assume the general case with $\lambda>0$. Then the share of stocks decreases as standard deviation of the stock returns increases:

$$\frac{\partial\lambda}{\partial\sigma\left(r_s\right)}=-\frac{2\cdot\sigma\left(r_s\right)\cdot Y}{\left(\left(\sigma\left(r_s\right)\right)^2+Z\right)^2}\leq 0. \tag{4-5}$$

Therefore, the optimal λ depends on the relationship between the risk and the rate of return ($\sigma(r_s)$). However, in the case of a lack of transparency, the taxpayers in the first period might not be aware of the true relationship between return and risk. In this case, if the pension fund uses a false $\sigma(r_s)$, the chosen share of stocks would not be optimal.

Assume $\sigma(r_s)$ is a quadratic function of r_s and some exogenous positive parameter *a:*

$$\sigma\left(r_s\right)=\frac{r_s^{\,2}}{a}. \tag{4-6}$$

The higher is *a* the higher is the optimal share of stocks λ*:*

$$\frac{\partial\lambda}{\partial a}=\frac{\partial\lambda}{\partial\sigma\left(r_s\right)}\cdot\frac{\partial\sigma\left(r_s\right)}{\partial a}=\frac{2\cdot\sigma\left(r_s\right)\cdot Y\cdot r_s^{\,2}}{a^2\cdot\left(\sigma\left(r_s\right)^2+Z\right)^2}\geq 0. \tag{4-7}$$

However, due to a lack of transparency, taxpayers do not know the exact form of the risk function. Assume the pension fund wishes to invest more in stocks than it is optimal. In this case it would pretend that the relationship between risk and stocks return is $\sigma\left(r_s, a_2\right)$, while the true relationship is $\sigma\left(r_s, a_1\right)$, where $a_2 > a_1$. That would lead to a bigger fraction of stocks in the pension fund portfolio than is optimal and consequently to higher tax distortions and welfare loss.

Why might pension funds tend to underestimate risk? Assume a state or local government spends its budget on two main items: payments to public employees and financing another public good. The government income has only one source—tax payments *(T)*. The government chooses how to distribute its budget optimally: how many employees to hire *(n)* and how much to spend on other public good (*Y*). Assume that the other public good is the numeraire good and its price is equal 1. The price of a public employee consists of two items—wage and ERR on the pension fund investments. Hence, the government faces the budget constraint

$$n \cdot p\left(w, r\right) + Y = T, \tag{4-8}$$

where $p(w,r)$ is the price of a public employee, that depends on the wage (w) and the ERR on investments in pension funds (r). The higher is share of stocks λ the higher is ERR and less investments per public employee is required in the first period. Therefore price of a public employee increases in w and decreases in r:

$$\frac{\partial p\left(w, r\right)}{\partial w} > 0, \frac{\partial p\left(w, r\right)}{\partial r} < 0. \tag{4-9}$$

The optimal number of employees *(n*)* is determined by the tangency of the budget constraint and the indifference curve that occurs at point *A* in Figure 4-1. So, the optimal number of public employees is n_A, and the optimal level of other public good production is Y_A. However, if instead of using the true relationship between risk and rate of return $\sigma(r_s, a_1)$ a false relationship $\sigma(r_s, a_2)$ is used (a_2>a_1), then the price of a public employee decreases and the budget constraint rotates to the right since it becomes possible to hire more public employees with the same tax base. Therefore, in the case of an overestimated ERR, the tangency point would be at point *B* on Figure 4-1, and it would provide a higher level of utility; more public employees will be hired (n_B) and lower levels of other public goods (Y_B) will be produced due to the substitution effect, assumed here to outweigh the income effect.

Hence, with a low ERR employers would have to invest more in the pension fund to provide enough funds to finance the future retirement benefit payments. That could be achieved either through increasing taxes or through decreasing the number of public employees or their wages. Both of these options could harm current public employees. On the other hand, retirement benefits for the majority of public pension funds are guaranteed by state and local governments, so in the case of insufficient funding of retirement benefits, public employees still get their benefits in full. Therefore, labor unions representing public employees seek a high rate of return. When public pension funds are dominated by labor unions, they may tend to underestimate risk. As a result, investments in stocks will be higher than optimal and ERR will be overestimated. The next sections provide an empirical test of this hypothesis. Using a proxy measure of the union power, I estimate its effect on the ERR. If there is a positive correlation between labor union power and the ERR used by pension funds, then it is likely that public pension funds use an overestimated ERR because of pressure coming from labor unions.

Data Description

The data for the hypothesis testing comes from the United States Census of State and Local Government Employee-Retirement Systems 2007 Survey. The survey contains information on the receipts and payments to the system including employee contributions and the structure of holdings and investments. Unfortunately, there is no information about the ERR, which is required for testing the hypothesis. Therefore, I constructed this variable using data on the holdings and investments structure of public pension funds and historical estimates of the long-term rates of return for different asset classes.

Usually 4 different classes of assets are distinguished: money market or cash equivalents, bonds, stocks or equities, and real property. The risk and rate of return are positively correlated, and as the rate of return increases the risk grows. A summary of historical returns by class of asset is provided in Table 4-1 (Ibbotson, 1997). The historical statistics for government bonds and corporate stocks performance is also summarized in Table 4-2 (Siegel, 2007). Apparently, stock market returns, long-term bonds, and short-term bonds in Siegel (2007) correspond to the DJI stocks, bonds, and T-Bills in Ibbotson (1997) respectively. To construct the ERR for the retirement systems participating in the Census survey, I assign different types of holdings and investments of the pension fund to the corresponding asset class. Table 4-4 illustrates the assignments of assets to different categories. The ERR for the retirement systems is calculated using the following formula:

$$\begin{aligned} ERR_i = \alpha_{ic} \cdot ERR_c + \left(\sum_b \alpha_{ib}\right) \cdot ERR_b + \left(\sum_s \alpha_{is}\right) \cdot ERR_s + \\ \alpha_{ip} \cdot ERR_p + \alpha_{iv} \cdot ERR_v + \alpha_{im} \cdot ERR_m, \end{aligned} \tag{4-10}$$

where α_{ij} shows share of asset j in the total holdings and investments of the retirement system i. The classes of assets are denoted by the following indexes: c − cash, short-term investments, b − bonds, s − big and medium cup stocks, p − real property, v − small cap stocks, and m − mortgage. ERR for mortgage is a little bit complicated issue because usually a mortgage pool is divided into tranches with different rates of return and different risks. However, pension funds are restricted from investing in highly risky assets, hence they are likely to hold mortgages with a risk level not exceeding high cap corporate stocks. The ERR for mortgages was calculated as the average of the ERR for bonds and high and medium cap stocks. The data for the labor union power, politics and other state characteristics comes from the Statistical Abstract of the United States and Department of Labor statistics.

Since I am constructing the ERR using historical estimates, there is a risk that the results of my estimations are driven by the particular design of the ERR construction. To confirm my results, I also run the same model using other definitions of the dependent variable that measures the relative risk of the pension fund portfolio. These two measures include ERR and average risk of the portfolio that are calculated using Wilshire's Asset Class Assumptions (Bonafede et al., 2009). Table 4-3 illustrates the rate of return and risk by different classes of assets that are assumed in this report and Table 4-5 shows the assignments of assets to different categories according to Wilshire's classification. The average risk measure is constructed using the same logic as in Equation 4-18, but with expected risk estimates instead of ERR.

Empirical Strategy

In the empirical section of this chapter, I analyze the influence of labor unions on the ERR. Therefore, the empirical model for hypothesis testing is

$$ERR_i = \beta_0 + \beta_c \cdot Contr_i + \beta_u \cdot Union_i + \beta_p \cdot Politics_i + \sum_j \beta_j \cdot a_{ij} + e_i, \qquad (4\text{-}11)$$

where $Contr_i$ is the share of the employee contribution in total contributions of the retirement system i, $Union_i$ − the measure of the labor union power in the state where the retirement system i is located, $Politic_i$ − is a measure of the politic preferences of the state's population and state government, and a_{ij} − a set of other state characteristics that might affect the investment strategy of the retirement system. The variable of main interest is *Union*. In the case when the dependent variable is relative risk based on Wilshire's Asset Class Assumptions, rather than ERR, the equation remains the same. The hypothesis is tested by the value of the coefficient β_{Union}. If pension funds located in the states with relatively high union power have on average a higher ERR than funds located in states where unions are relatively weak, then is it is likely that the ERR is overestimated because of the pressure of labor unions.

Results

I use two proxies for labor union power: a dummy variable for a right to work law and the percentage of union membership in the state. I ran regressions including each of these two variables separately and also a regression with both of these variables included. Those are the variables of main interest. The hypothesis of the labor union effect on the ERR is supported if the union membership and right to work laws are statistically significant with positive and negative signs respectively. Union membership is likely to be higher in states where labor unions are relatively strong. Likewise, if a state has a right to work law, then unions are likely to be relatively weak in that state. As explanatory variables, I include two pension fund characteristics and several state characteristics that could affect the future tax income of the state or that characterize which political party is dominant. Summary statistics for variables used in the analysis are shown in Table 4-6.

I used data for the latest two available years (2006 and 2007) because the 2007 data has one strange feature: about 50% of all observations come from Pennsylvania state and local pension funds. To deal with this strange irregularity of the data, I also run the same model for the 2006 data set that has about 20% of its observations coming from Pennsylvania. I also ran regressions for both time periods without Pennsylvania and weighted observations by the total number of fund members to make sure that the results are not driven by observations coming from a single state. I do not report results for the regressions runs for the sample that excludes

Pennsylvania because when the weights are used, these results are similar to the regression results for the whole sample.

Table 4-7 shows the results of the OLS regressions estimations for the ERR constructed using historical data for the rates of return. As a proxy for labor union power in this regression I include the right to work variable. For both years, the variable measuring labor union power is statistically significant and has the expected sign: negative. The effect of a right to work law is relatively large: it decreases ERR by approximately 24 percentage points of the standard deviation. Other explanatory variables also show the expected results. The increasing impact of the total number of fund members can be explained by the risk-pooling effect.

Table 4-8 repeats the same analysis but with another measure of labor union power—state union membership. Again for both years, the variable of main interest is statistically significant and has the expected negative sign. The union membership variable is significant at 5% level, and its effect is more pronounced than the effect of right to work laws. It increases ERR by approximately 39 percentage points of the standard deviation. Other variables also have the expected signs. The positive effects of the total number of fund members and state population reflect the risk-pooling effect. Table 4-9 shows results for the OLS regression with both union membership and right to work law variables included. In this setting union membership is not statistically significant, however the right to work law has a higher magnitude in this regression compared to the results presented in Table 4-7. The lack of statistical significance of the union membership variable might be explained by the high negative correlation between union membership and right to work variables.

Then, I run the same sets of regressions with alternative definitions of the dependent variable. As these alternative definitions I use ERR and average portfolio risk that are defined using Wilshire's Asset Class Assumptions. Tables 4-10, 4-11, and 4-12 show the results of these estimations for ERR and Tables 4-13, 4-14, and 4-15 for average risk. Both measures of labor union power retain the same signs and the same pattern of statistical significance in all regressions, with only one exception: the union membership is not statistically significant in estimations applied to 2007 data. In three out of six regressions I found a negative effect of the state net migration on the ERR and average risk for the 2007 data set. The states with high net migration might expect higher future tax revenues and thus might be less concerned by current underfunding of the public pension funds. However, this result is not stable across different specifications.

Hence, empirical results support the hypothesis of labor union influence on pension funds which encourages funds to use an overestimated ERR and make risky investments. However, the model is very rough since the data set does not provide the ERR. I had to construct the ERR according to the pension fund's asset structure. Therefore, the dependent variable in my study is not precisely defined. Also my measures of labor union power are not perfect. However, my results show that the union effect is quite possible and provides motivation for further research in this area.

Conclusions

In this chapter, I first reviewed current debates over the discount rate that should be used for valuation of public pension funds' future earnings on investments. Some financial economists argue that future earnings of public pension funds should be valued using rates of return on fixed income securities, while the actuarial pension practice is to use market rates of return. This discussion is similar to the debate over the appropriate discount rates for evaluation of future costs caused by global warming. Participants of these debates provide solid arguments in favor of using the market rate instead of the risk-free rate. Most importantly, additional contributions into pension funds that would be required in the case of low discount rates would cause high opportunity cost because otherwise these assets could have been invested with a higher rate of return.

Then, using a tax-smoothing model I illustrate how lack of transparency regarding the real relationship between rate of return and risk may cause deviations from optimality and why labor unions might want pension funds to use an overestimated rate of return. I look at the possible effect of the influence of the labor unions on the pension funds' investment strategies. Finally, I test the hypothesis of union influence empirically. The OLS estimation results confirm that the pressure of labor unions that desire high rates of return may cause deviations from optimal investment strategy.

Table 4-1. 1926-1996 average annual rates of return

Asset class	Nominal rate of return (%)	Real rate of return (%)
Small Cap Stocks	12.5	9.4
Real Estate	11.1	8.0
DJI	10.0	6.9
Bonds	5.2	2.1
T-Bills	3.7	0.6
Inflation	3.1	—

(Source: Ibbotson (1997), p. 25)

Table 4-2. 1926-2001 average annual rates of return

Asset class	Nominal rate of return (%)	Real rate of return (%)
Stock market	10.02	6.90
Long-term government bonds	5.20	2.20
Long-term government bonds	3.90	0.70

(Source: Siegel (2007), p.13., p.15.)

Table 4-3. Wilshire asset class assumptions

Asset class	Rate of return (%)	Risk (%)
U.S. Equity	8.50	16.0
Non-U.S. Equity	8.50	17.0
Private Equity	11.55	26.0
Real Estate	7.00	15.0
U.S. Bonds	4.00	5.0
Non-U.S. Bonds	3.75	10.0

(Source: Bonafede et al. (2009), p.12.)

Table 4-4. Assignment of survey items for investments and holdings to general asset classes

Variable description	Cash, Short-term government bonds	Bonds	Big and medium cap	Real Property	Small cap	Mortgage
Expected rate of return	0.6	2.1	6.9	8.0	9.4	4.5
Cash, short-term investments	X					
Federal securities		X				
State and Local Gov. securities		X				
Corporate bonds (bonds and mortgage-backed securities issued by FHLB, FHLMC, FNMA, Farm credit banks, and SLMA)		X				
Other corporate bonds (include debentures, convertible bonds, and railroad equipment certificates)		X				
Other securities (shares in mutual funds, conditional sales contracts, direct loans, loans to members, etc.)			X			
Investments trusts (shares in funds administered by private agencies, governmental investment accounts)			X			
Foreign and international securities (include corporate stocks and corporate equities)			X			
Mortgages						X
Real property				X		
Corporate stocks (include common and preferred stocks, warrants)			X			
Other Investments (include venture capital, partnerships, real estate investment trusts, and leveraged buyouts)					X	

Table 4-5. Assignment of survey items for investments and holdings to general asset classes using Wilshire asset class assumptions

Variable description	U.S. Bonds	U.S. Equity	Non-U.S. Equity	Real Estate	Private. Equity	Mortgage
ERR (%)	4.00	8.50	8.50	7.00	11.55	6.25
Risk (%)	5.00	16.00	17.00	15.00	26.00	9.50
Federal securities	X					
State and Local Gov. securities	X					
Corporate bonds (bonds and mortgage-backed securities issued by FHLB, FHLMC, FNMA, Farm credit banks, and SLMA)	X					
Other corporate bonds (include debentures, convertible bonds, and railroad equipment certificates)	X					
Other securities (shares in mutual funds, conditional sales contracts, direct loans, loans to members, etc.)		X				
Investments trusts (shares in funds administered by private agencies, governmental investment accounts)		X				
Foreign and international securities (include corporate stocks and corporate equities)			X			
Mortgages						X
Real property				X		
Corporate stocks (include common and preferred stocks, warrants)		X				
Other Investments (include venture capital, partnerships, real estate investment trusts, and leveraged buyouts)					X	

Table 4-6. Summary statistics

Variable	2007 Mean	2007 Std. Dev.	2006 Mean	2006 Std. Dev.
ERR based on historical data	5.82	0.69	5.75	0.68
ERR based on Wilshire asset class assumptions	7.39	0.73	7.34	0.70
Average risk based on Wilshire asset class assumptions	13.85	1.82	13.99	1.78
Employee contribution dummy (0 if contribution=0)	0.992	0.087	0.916	0.278
Log of total members	12.183	1.460	6.215	2.270
Right to work law	0.372	0.483	0.575	0.495
Union membership (%)	13.48	6.44	13.44	6.48
Share of population with bachelor degree	0.278	0.046	0.280	0.035
Share of population living in metropolitan areas	0.822	0.140	0.892	0.083
Birth to death ratio	1.790	0.455	1.409	0.341
Share of net migration in total population of state	0.023	0.045	0.062	0.052
Low House: Republicans to Democrats	1.289	1.038	1.027	1.441
Log of population	15.000	1.100	14.60	1.100
Number of observations	1720		782	

Table 4-7. OLS regressions results for ERR based on the historical data and right to work law as a measure of labor union power

Variable	2007 β	2007 std. dev.	2006 β	2006 std. dev.
Employee contribution dummy (0 if contribution=0)	0.18	0.24	-0.04	0.33
Log of total members	0.06*	0.03	0.08***	0.03
Right to work law	-0.46***	0.14	-0.49***	0.14
Share of population with bachelor degree	0.18	1.64	1.58	1.69
Share of population living in metropolitan areas	-0.03	0.54	-0.33	0.54
Birth to death ratio	0.11	0.14	-0.05	0.16
Share of net migration in total population of state	-1.26	1.10	-0.17	1.33
Low House: Republicans to Democrats	0.09	0.05	0.06	0.06
Log of population	0.06	0.06	0.05	0.05
Constant	3.89	1.07	4.21	0.99
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table 4-8. OLS regressions results for ERR based on the historical data and state union membership as a measure of labor union power

Variable	2007		2006	
	β	std. dev.	β	std. dev.
Employee contribution dummy (0 if contribution=0)	0.15	0.25	-0.09	0.35
Log of total members	0.06	0.03	0.07**	0.03
State union membership	0.02**	0.01	0.03**	0.01
Share of population with bachelor degree	0.77	1.61	2.16	1.63
Share of population living in metropolitan areas	0.00	0.60	-0.28	0.62
Birth to death ratio	0.10	0.13	-0.06	0.15
Share of net migration in total population of state	-1.36	1.05	-0.33	1.36
Low House: Republicans to Democrats	0.08	0.06	0.06	0.06
Log of population	0.11*	0.06	0.10*	0.06
Constant	2.57	1.07	2.68	1.04
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table 4-9. OLS regressions results for ERR based on the historical data and right to work law and state union membership as a measure of labor union power

Variable	2007		2006	
	β	std. dev.	β	std. dev.
Employee contribution dummy (0 if contribution=0)	0.20	0.24	-0.03	0.33
Log of total members	0.06*	0.03	0.08***	0.03
State union membership	-0.01	0.02	-0.01	0.01
Right to work law	-0.55***	0.20	-0.59***	0.18
Share of population with bachelor degree	0.26	1.62	1.66	1.66
Share of population living in metropolitan areas	0.04	0.55	-0.25	0.55
Birth to death ratio	0.10	0.13	-0.06	0.16
Share of net migration in total population of state	-1.75	1.11	-0.72	1.44
Low House: Republicans to Democrats	0.08	0.05	0.05	0.05
Log of population	0.05	0.06	0.04	0.06
Constant	4.13	1.22	3.95	1.12
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table 4-10. OLS regressions results for ERR based on Wilshire asset class assumptions and right to work law as a measure of labor union power

Variable	2007		2006	
	β	std. dev.	β	std. dev.
Employee contribution dummy (0 if contribution=0)	0.23	0.28	0.00	0.40
Log of total members	0.07**	0.03	0.08***	0.03
Right to work law	-0.44***	0.15	-0.49***	0.15
Share of population with bachelor degree	0.97	1.69	2.02	1.72
Share of population living in metropolitan areas	-0.34	0.56	-0.50	0.54
Birth to death ratio	0.15	0.14	-0.06	0.13
Share of net migration in total population of state	-1.55	1.06	0.03	1.24
Low House: Republicans to Democrats	0.09	0.05	0.05	0.05
Log of population	0.07	0.06	0.06	0.05
Constant	5.11	1.13	5.48	1.01
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table 4-11. OLS regressions results for ERR based on Wilshire asset class assumptions and state union membership as a measure of labor union power

Variable	2007		2006	
	β	std. dev.	β	std. dev.
Employee contribution dummy (0 if contribution=0)	0.20	0.29	-0.04	0.41
Log of total members	0.06*	0.04	0.08***	0.03
State union membership	0.02	0.01	0.02**	0.01
Share of population with bachelor degree	1.66	1.69	2.64	1.69
Share of population living in metropolitan areas	-0.25	0.62	-0.44	0.62
Birth to death ratio	0.13	0.14	-0.07	0.12
Share of net migration in total population of state	-2.02*	1.07	-0.27	1.33
Low House: Republicans to Democrats	0.08	0.06	0.05	0.06
Log of population	0.12**	0.06	0.11*	0.06
Constant	3.85	1.09	3.98	1.05
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table 4-12. OLS regressions results for ERR based on Wilshire asset class assumptions and right to work law and state union membership as a measure of labor union power

Variable	2007		2006	
	β	std. dev.	β	std. dev.
Employee contribution dummy (0 if contribution=0)	0.26	0.28	0.02	0.39
Log of total members	0.07**	0.03	0.09***	0.03
State union membership	-0.02	0.02	-0.01	0.01
Right to work law	-0.6***	0.21	-0.61***	0.18
Share of population with bachelor degree	1.11	1.67	2.12	1.69
Share of population living in metropolitan areas	-0.21	0.57	-0.40	0.54
Birth to death ratio	0.13	0.13	-0.07	0.13
Share of net migration in total population of state	-2.45**	1.06	-0.68	1.39
Low House: Republicans to Democrats	0.08	0.05	0.05	0.05
Log of population	0.06	0.06	0.04	0.06
Constant	5.56	1.29	5.08	1.16
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table 4-13. OLS regressions results for average risk based on Wilshire asset class assumptions and right to work law as a measure of labor union power

Variable	2007		2006	
	β	std. dev.	β	std. dev.
Employee contribution dummy (0 if contribution=0)	0.23	0.28	0.00	0.40
Log of total members	0.07**	0.03	0.08***	0.03
Right to work law	-0.44***	0.15	-0.49***	0.15
Share of population with bachelor degree	0.97	1.69	2.02	1.72
Share of population living in metropolitan areas	-0.34	0.56	-0.50	0.54
Birth to death ratio	0.15	0.14	-0.06	0.13
Share of net migration in total population of state	-1.55	1.06	0.03	1.24
Low House: Republicans to Democrats	0.09	0.05	0.05	0.05
Log of population	0.07	0.06	0.06	0.05
Constant	5.11	1.13	5.48	1.01
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table 4-14. OLS regressions results for average risk based on Wilshire asset class assumptions and state union membership as a measure of labor union power

Variable	2007		2006	
	β	std. dev.	β	std. dev.
Employee contribution dummy (0 if contribution=0)	0.20	0.29	-0.04	0.41
Log of total members	0.06*	0.04	0.08***	0.03
State union membership	0.02	0.01	0.02**	0.01
Share of population with bachelor degree	1.66	1.69	2.64	1.69
Share of population living in metropolitan areas	-0.25	0.62	-0.44	0.62
Birth to death ratio	0.13	0.14	-0.07	0.12
Share of net migration in total population of state	-2.02*	1.07	-0.27	1.33
Low House: Republicans to Democrats	0.08	0.06	0.05	0.06
Log of population	0.12**	0.06	0.11*	0.06
Constant	3.85	1.09	3.98	1.05
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table 4-15. OLS regressions results for average risk based on Wilshire asset class assumptions and right to work law and state union membership as a measure of labor union power

Variable	2007		2006	
	β	std. dev.	β	std. dev.
Employee contribution dummy (0 if contribution=0)	0.26	0.28	0.02	0.39
Log of total members	0.07**	0.03	0.09***	0.03
State union membership	-0.02	0.02	-0.01	0.01
Right to work law	-0.6***	0.21	-0.61***	0.18
Share of population with bachelor degree	1.11	1.67	2.12	1.69
Share of population living in metropolitan areas	-0.21	0.57	-0.40	0.54
Birth to death ratio	0.13	0.13	-0.07	0.13
Share of net migration in total population of state	-2.45**	1.06	-0.68	1.39
Low House: Republicans to Democrats	0.08	0.05	0.05	0.05
Log of population	0.06	0.06	0.04	0.06
Constant	5.56	1.29	5.08	1.16
Number of observations	1720		782	
Weights	yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

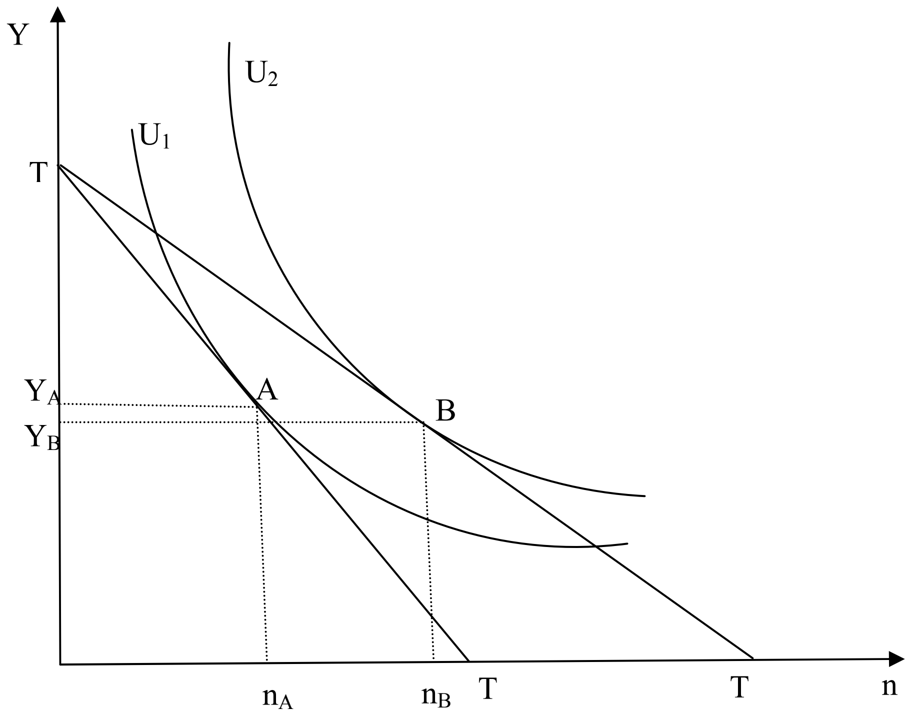

Figure 4-1. Overestimation of ERR leads to hiring more public employees than optimal.

CHAPTER 5
CONCLUSION

My dissertation investigated three different economic policy issues. I analyzed the effects of two recent educational reforms in US and its effects on the teachers labor market. I showed that attrition rates for teachers who came to the profession through alternative routes are different from attrition rates of teachers who came to the profession through traditional pathways only for novice teachers with less than 2 years of experience. I estimated the relative efficiency of alternative programs and using simulation model have illustrated how to calculate the upper bound of alternative programs training costs.

Then I examined the effect of the school accountability on the supply and demand of teachers. I constructed the set of variables describing different types if accountability policies. I distinguished three main types: school-targeted accountability, school-targeted and district-targeted. I revealed no results of student-targeted policies on the teachers. School-targeted policies do affect teachers but only in the large districts and its effects vary across subject fields and across types of schools. District-targeted policies do affect teachers supply in a positive way.

In the fourth chapter I analyzed possible deviations from optimal investment strategy for public pension funds due to influence of labor unions and illustrated how lack of transparency shifts the results from optimality. Then I tested the hypothesis that because of lack of transparency labor unions encourage pension funds to make too risky investments. I found some evidence that labor unions do in fact affect pension funds investment strategies and the more powerful are labor unions in the state the more risky are the investments of the state and local public pension funds.

APPENDIX A
DETAILED RESULTS FOR TEACHER ATTRITION ESTIMATION

Table A-1. Summary statistics

		Diagonal		Definition 1		Definition 2	
Variable	Definition of variable	Mean	Std.	Mean	Std.	Mean	Std.
School's characteristics							
Non-white students	Ratio of minority students	0.358	0.342	0.364	0.344	0.365	0.342
Non-white teachers	Ratio of minority teachers	0.143	0.216	0.145	0.216	0.145	0.216
Poor	Ratio of students eligible for free lunch	0.381	0.279	0.383	0.279	0.385	0.280
Flag poor	=1 if missing observation for poor	0.012	0.110	0.012	0.107	0.011	0.105
Urban	=1 if school in big- or mid-size city	0.784	0.412	0.790	0.408	0.787	0.409
District's characteristics							
Average size of the school in the district	District enrollment/Number of schools in the district	584.4	264.7	587.7	264.2	589.2	268.0
Teacher's characteristics							
Exit	=1 if exit MA field next year	0.180	0.384	0.182	0.386	0.181	0.385
Year	=1 if 2003-04 school year	0.512	0.500	0.523	0.500	0.524	0.499
Married	=1 if married	0.732	0.443	0.725	0.447	0.723	0.447
Master	=1 if has master's degree	0.501	0.500	0.494	0.500	0.488	0.500
Master flag	=1 if missing observation for master	0.007	0.086	0.007	0.083	0.007	0.084
Female	=1 if teacher is female	0.767	0.423	0.765	0.424	0.769	0.421
Non-white	=1 if teacher is non-white	0.095	0.293	0.100	0.300	0.098	0.297
Experience	Total experience (years)	16.9	10.0	16.1	10.1	15.6	10.0
Experience squared		385.5	371.0	361.1	368.7	343.7	365.0
Entry age	At what age first time entered teaching	26.03	6.60	26.13	6.60	26.17	6.62
Union	=1 if teacher is member of union	0.806	0.395	0.805	0.396	0.802	0.398
Special education	=1 if special education is MA field	0.114	0.317	0.115	0.320	0.118	0.322
English	=1 if MA field is English language arts	0.099	0.299	0.101	0.302	0.099	0.298
Mathematics	=1 if MA field is mathematics	0.073	0.260	0.071	0.257	0.069	0.254
Sciences	=1 if MA field is natural sciences	0.065	0.247	0.064	0.245	0.062	0.242
Social Sciences	=1 if MA field is social sciences	0.058	0.234	0.060	0.237	0.061	0.239
Other fields	=1 if MA field is elementary education	0.321	0.467	0.320	0.466	0.326	0.469
Regular second	=1 if additional traditional certificate	0.292	0.455	0.284	0.451	0.285	0.452
Salary	Real salary ($10'000)	0.212	0.409	0.216	0.411	0.213	0.410
Alternative	=1 if alternative certificate in MA field	0.032	0.177	0.031	0.174	0.030	0.169
Exp1	=1 if experience is 1 year	0.036	0.186	0.040	0.196	0.044	0.206
Exp2	=1 if experience is 2 years	0.032	0.175	0.036	0.187	0.039	0.193
Exp3	=1 if experience is 3 years	0.032	0.176	0.042	0.200	0.041	0.199

Table A-1. Continued

Variable	Definition of variable	Diagonal Mean	Diagonal Std.	Definition 1 Mean	Definition 1 Std.	Definition 2 Mean	Definition 2 Std.
Exp4	=1 if experience is 4 yeas	0.034	0.181	0.038	0.192	0.042	0.201
Exp5	=1 if experience is 5 years	0.659	0.474	0.661	0.474	0.655	0.476
College	=1 if highly competitive college	0.049	0.217	0.047	0.212	0.055	0.228
Flag college	=1 if missing observations for college	4.200	1.147	4.148	1.139	4.124	1.134
Income35	=1 if family income is less than $35'000	0.060	0.237	0.064	0.244	0.066	0.247
Income50	=1 if $35'000<family income <$50'000	0.175	0.380	0.181	0.385	0.183	0.387
Income100	=1 if $75'000<family income <$100'000	0.249	0.432	0.246	0.431	0.242	0.428
Income100plus	=1 if family income > $100'000	0.136	0.343	0.133	0.340	0.135	0.342
Sample's characteristics							
Total number of observations		5,224	100%	5,712	100%	6,020	100%
Alternative no exit		565	17%	630	18%	667	18%
Alternative with exit		552	4%	603	4%	629	4%
Traditional no exit		2,050	65%	2,286	64%	2,425	64%
Traditional with exit		2,057	14%	2,193	14%	2,299	14%

Table A-2. Results of logit regression estimation

Coefficient	Diagonal Definition β	Std. error	Hazard ratio	Definition 1 β	Std. error	Hazard ratio	Definition 2 β	Std. error	Hazard ratio
Alt. certificate in MA	-0.18	0.16	0.83	-0.15	0.15	0.86	-0.13	0.15	0.87
Alt. cert. X Exp.=1	1.18**	0.53	3.24**	1.16**	0.51	3.18**	1.14**	0.50	3.13**
Alt. cert. X Exp.=2	0.03	0.56	1.03	0.26	0.51	1.29	0.15	0.47	1.17
Alt. cert. X Exp.=3	0.58	0.48	1.79	0.43	0.47	1.54	0.36	0.45	1.43
Alt. cert. X Exp.=4	1.06	0.67	2.89	0.22	0.62	1.24	-0.05	0.61	0.95
Alt. cert. X Exp.=5	0.49	0.98	1.64	-0.01	0.92	0.99	0.40	0.74	1.50
Exp.=1 year	-0.42	0.40	0.65	-0.45	0.36	0.64	-0.50	0.35	0.61
Exp.=2 years	-0.69*	0.38	0.5*	-0.72**	0.34	0.49**	-0.57*	0.32	0.57*
Exp.=3 years	-0.18	0.35	0.83	-0.09	0.31	0.91	-0.41	0.30	0.67
Exp.=4 years	-0.1	0.40	0.9	0.08	0.37	1.09	0.30	0.34	1.34
Exp.=5 years	0.21	0.45	1.24	0.44	0.36	1.55	-0.00	0.37	0.99
Reg. cert. in other field	-0.04	0.15	0.96	0.02	0.14	1.03	0.13	0.14	1.14
Reg. cert. in other field X MA field is sp. ed.	0.37	0.37	1.45	0.44	0.36	1.55	0.27	0.35	1.31
Special Education	-0.49*	0.26	0.61*	-0.61***	0.26	0.54***	-0.51**	0.24	0.60**
English	0.88***	0.20	2.4***	0.89***	0.19	2.44***	0.90***	0.18	2.47***
Math	0.39	0.24	1.48	0.43*	0.24	1.53*	0.40*	0.23	1.49*
Sciences	-0.08	0.30	0.92	-0.02	0.28	0.98	-0.08	0.27	0.92
SocSc	0.57**	0.27	1.77**	0.60***	0.25	1.82***	0.53**	0.25	1.7**
Other	0.82***	0.17	2.27***	0.80***	0.16	2.22***	0.73***	0.16	2.08***
Year	-0.19*	0.12	0.82*	-0.16	0.11	0.85	-0.15	0.11	0.86
Married	0.33	0.27	1.39	0.20	0.25	1.22	0.00	0.25	1.00
Married female	-0.46	0.29	0.63	-0.38	0.28	0.68	-0.10	0.28	0.91
Master	0.15	0.14	1.16	0.15	0.13	1.16	0.23*	0.13	1.26*
Master flag	0.78	0.60	2.17	0.78	0.60	2.19	0.76	0.60	2.13
Female	0.69***	0.27	1.99***	0.62***	0.26	1.86***	0.34	0.25	1.40
Non-white	0.09	0.20	1.098	0.001	0.19	1.001	-0.06	0.19	0.95
Experience	-0.07*	0.04	0.94*	-0.07*	0.03	0.94*	-0.08**	0.03	0.93**
Experience sq.	0.01***	0.00	1.01***	0.01***	0.00	1.01***	0.01***	0.00	1.01***
Age first time teaching	-0.00	0.01	1.00	0.00	0.01	1.00	0.001	0.01	1.00
Union member	-0.17	0.16	0.84	-0.19	0.15	0.83	-0.18	0.14	0.84
Real salary	0.01	0.07	1.01	-0.00	0.07	0.99	-0.01	0.07	0.99
Minority students	0.08	0.29	1.09	0.00	0.28	1.002	0.02	0.27	1.02
Minority teachers	0.18	0.40	1.20	0.24	0.39	1.27	0.28	0.36	1.32
Free lunch eligible	0.72***	0.31	2.06***	0.66**	0.29	1.94**	0.63**	0.28	1.88**
Flag for free lunch	0.29	0.46	1.33	0.26	0.45	1.3	0.27	0.44	1.31
Av. school size	-0.00	0.00	0.99	-0.00	0.00	0.99	-0.00	0.00	0.99
Urban	0.07	0.16	1.07	0.06	0.15	1.06	0.03	0.15	1.03
Constant	-1.81***	0.70	0.16***	-1.73	0.65	0.18	-1.58	0.64	0.21
State fixed effects	yes			yes			yes		
# of observations	5224			5712			6020		

*, **, *** – statistically significant at 10, 5, and 1% level respectively.

Table A-3. Wald test for joint significance

Hypothesis	F-statistic Diagonal	Def. 1	Def.2
Alt ×Exp. 2=0 Alt ×Exp. 3=0 Alt ×Exp. 4=0 Alt ×Exp. 5=0	P>F=0.42	P>F=0.89	P>F=0.92
Alt ×Exp. 3=0 Alt ×Exp. 4=0 Alt ×Exp. 5=0	P>F=0.28	P>F=0.81	P>F=0.83
Alt ×Exp. 4=0 Alt ×Exp. 5=0	P>F=0.26	P>F=0.94	P>F=0.86

Table A-4. Summary statistics for teachers with 1 year of experience (weighted)

Personal characteristic	2003-04				1999-00			
	Alternative		Traditional		Alternative		Traditional	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Age	30.47	0.73	28.27	0.60	31.18	0.63	29.15	0.42
Teacher white	0.87	0.03	0.91	0.02	0.86	0.03	0.92	0.01
Teacher black	0.11	0.03	0.07	0.02	0.07	0.02	0.06	0.01
Entry age	29.47	0.73	27.27	0.60	30.17	0.63	28.14	0.42
Female	0.69	0.04	0.81	0.02	0.69	0.04	0.77	0.02
Bachelor degree	0.99	0.00	1.00	0.00	0.98	0.01	1.00	0.00
Master degree	0.14	0.02	0.15	0.02	0.12	0.02	0.16	0.02
Associate's degree	0.08	0.02	0.18	0.03	0.14	0.03	0.07	0.01
College quality (index 1-5)	3.01	0.11	2.74	0.05	2.82	0.06	2.77	0.04
Education specialist or adv. grad. study	0.02	0.01	0.01	0.01	0.04	0.02	0.03	0.01
Married	0.33	0.07	0.42	0.06	0.36	0.09	0.51	0.06
Widowed, divorced, separated^	0.02	0.02	0.03	0.02	0.09	0.05	0.06	0.02
Never married ^	0.65	0.07	0.55	0.06	0.55	0.10	0.43	0.06
Family income (category: 1-5) ^	2.57	0.20	2.59	0.15	2.08	0.18	2.27	0.13
Family size ^	2.18	0.19	2.42	0.14	n/a	n/a	n/a	n/a
Kids under of age of 5^	0.22	0.07	0.32	0.06	0.07	0.05	0.20	0.06
Number of obs. (SASS/TFS)	359/89		909/188		326/59		597/123	

^ – data and sample weights from TFS

Table A-5. t-tests for difference in mean values of personal characteristics of ARC and TRC teachers with 1 year of experience (weighted)

Personal characteristic	2003-04		1999-00		D-in-D	
	β	t-st.	β	t-st.	β	t-st.
Age	2.20**	2.34	2.03***	2.67	0.17	0.14
Teacher white	-0.04	-1.33	-0.06	-1.55	0.02	0.32
Teacher black	0.04	1.49	0.02	0.64	0.03	0.74
Entry age	2.20***	2.33	2.04***	2.69	0.16	0.13
Female	-0.12***	2.62	-0.08*	1.80	-0.05	0.71
Bachelor degree	-0.01**	-2.21	-0.02***	-2.74	0.01	1.61
Master degree	-0.01	-0.41	-0.05	-1.46	0.03	0.69
Associate's degree	-0.11***	-3.40	0.08**	2.28	-0.18***	-3.99
College quality (index 1-5)	0.26**	2.09	0.05	0.64	0.21	1.47
Education specialist or advanced graduate study	0.00	0.18	0.01	0.40	-0.01	-0.29
Married ^	-0.09	-0.96	-0.15	-1.36	0.06	0.44
Widowed, divorced, separated^	-0.01	-0.32	0.03	0.60	-0.04	-0.68
Never married ^	0.10	1.03	0.12	1.00	-0.02	-0.14
Family income (category: 1-5) ^	-0.14	-0.61	-0.27	-1.31	0.12	0.39
Family size ^	-0.24	-1.03	n/a	n/a	n/a	n/a
Kids under of age of 5^	-0.10	-1.07	-0.13*	-1.71	0.03	0.24
Number of obs. (SASS/TFS)	956/205		1235/247		1235/452	

^ – data and sample weights from TFS; ***,**,* - statistically significant at 1,5,10% respectively

Table A-6. Summary statistics for ARC and TRC teachers with less than 6 years of experience (weighted)

Personal characteristic	2003-04				1999-00			
	Alternative		Traditional		Alternative		Traditional	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Age	32.64	0.34	30.80	0.22	33.51	0.33	31.20	0.17
Teacher white	0.83	0.02	0.90	0.01	0.86	0.01	0.89	0.01
Teacher black	0.14	0.02	0.07	0.01	0.09	0.01	0.07	0.01
Entry age	29.67	0.34	27.50	0.22	30.58	0.32	28.19	0.17
Female	0.74	0.02	0.79	0.01	0.69	0.02	0.77	0.01
Bachelor degree	0.98	0.00	1.00	0.00	0.98	0.00	1.00	0.00
Master degree	0.23	0.02	0.28	0.01	0.20	0.02	0.22	0.01
Associate's degree	0.12	0.01	0.14	0.01	0.11	0.01	0.08	0.01
College quality (index 1-5)	2.92	0.04	2.78	0.02	2.89	0.03	2.78	0.02
Education specialist or adv.grad.study	0.02	0.00	0.02	0.00	0.02	0.01	0.02	0.00
Married ^	0.51	0.04	0.49	0.03	0.65	0.06	0.63	0.03
Widowed, divorced, separated^	0.10	0.03	0.06	0.02	0.04	0.01	0.07	0.02
Never married ^	0.39	0.04	0.45	0.03	0.32	0.05	0.31	0.03
Family income (category: 1-5) ^	2.88	0.10	2.81	0.07	2.45	0.09	2.46	0.06
Family size ^	2.56	0.12	2.40	0.08	n/a	n/a	n/a	n/a
Kids under of age of 5^	0.40	0.06	0.40	0.04	0.28	0.08	0.30	0.04
Number of obs. (SASS/TFS)	1921/335		3997/652		1604/250		4921/721	

^ – data and sample weights from TFS

Table A-7. t-tests for difference in mean values of personal characteristics of ARC and TRC teachers with less than 6 years of experience (weighted)

Personal characteristic	2003-04		1999-00		D-in-D	
	β	t-st.	β	t-st.	β	t-st.
Age	1.84***	4.57	2.31***	6.23	-0.47	-0.86
Teacher white	-0.07***	-4.16	-0.03*	-1.71	-0.05*	-1.89
Teacher black	0.07***	4.04	0.02*	1.73	0.05**	2.06
Entry age	2.17***	5.43	2.39***	6.64	-0.23	-0.42
Female	-0.05***	-2.70	-0.08***	-4.07	0.03	1.02
Bachelor degree	-0.02***	-4.20	-0.02***	-4.66	0.00	-0.56
Master degree	-0.05***	-2.39	-0.02	-1.06	-0.03	-1.10
Associate's degree	-0.02	-1.24	0.03**	2.03	-0.05**	-2.26
College quality (index 1-5)	0.14***	2.94	0.11***	3.03	0.03	0.48
Education specialist or advanced graduate study	0.00	0.23	0.00	-0.33	0.00	0.40
Married ^	0.02	0.38	0.02	0.03	-0.002	-0.02
Widowed, divorced, separated^	0.04	1.23	0.04	1.23	0.07*	1.80
Never married ^	-0.06	-1.14	-0.06	-1.14	-0.07	-0.85
Family income (category: 1-5) ^	0.08	0.62	-0.04	-0.32	0.12	0.66
Family size ^	0.16	1.13	n/a	n/a	n/a	n/a
Kids under of age of 5^	0.00	-0.05	0.00	-0.05	0.01	0.13
Number of obs. (SASS/TFS)	5918/987		6525/971		12443/1958	

^ – data and sample weights from TFS; ***,**,* - statistically significant at 1,5,10% respectively

Table A-8. t-tests for difference in mean values of personal characteristics of alternatively certified teachers with 1 and 2 years of experience

Personal characteristic	2003-04		1999-00		D-in-D	
	β	t-st.	β	t-st.	β	t-st.
Age	-1.52	-1.44	-0.66	-0.78	-0.86	-0.64
Teacher white	0.01	0.24	0.03	0.64	-0.02	-0.35
Teacher black	0.02	0.48	-0.02	-0.43	0.03	0.65
Entry age	-0.54	-0.51	0.32	0.39	-0.87	-0.64
Female	0.05	0.94	0.08	1.53	-0.03	-0.37
Bachelor degree	0.01	0.97	-0.01	-1.40	0.02*	1.70
Master degree	-0.05	-1.11	-0.06	-1.44	0.01	0.23
Associate's degree	-0.10**	-2.26	0.03	0.84	-0.13**	-2.22
College quality (index 1-5)	-0.07	-0.54	-0.04	-0.42	-0.04	-0.23
Education specialist or advanced graduate study	0.01	0.46	0.03	1.40	-0.03	-1.05
Married ^	-0.05	-0.47	-0.31**	-2.31	0.26	1.56
Widowed, divorced, separated^	-0.12**	-2.25	0.04	0.61	-0.16**	-1.96
Never married ^	0.17	1.63	0.27**	1.98	-0.10	-0.60
Family income (category: 1-5) ^	-0.14	-0.55	-0.39	-1.42	0.24	0.64
Family size ^	-0.07	-0.27	n/a	n/a	n/a	n/a
Kids under of age of 5^	-0.06	-0.53	-0.27	-1.44	0.21	1.00
Number of obs. (SASS/TFS)	775/157		677/134		1452/291	

^ – data and sample weights from TFS; ***,**,* - statistically significant at 1,5,10% respectively

Table A-9. t-tests for difference in mean values of personal characteristics of alternatively certified teachers with 2 and 3 years of experience

Personal characteristic	2003-04		1999-00		D-in-D	
	β	t-st.	β	t-st.	β	t-st.
Age	-1.02	-0.94	-1.25	-1.30	0.24	0.16
Teacher white	0.08	1.49	-0.02	-0.30	0.09	1.27
Teacher black	-0.09*	-1.85	-0.04	-0.93	-0.05	-0.76
Entry age	0.00	0.00	-0.24	-0.25	0.24	0.16
Female	0.05	1.09	-0.13***	-2.34	0.18***	2.49
Bachelor degree	0.00	0.49	0.00	0.22	0.00	0.28
Master degree	-0.06	-1.13	0.01	0.28	-0.07	-1.04
Associate's degree	0.05	1.13	-0.03	-0.61	0.08	1.25
College quality (index 1-5)	0.14	1.24	-0.12	-1.14	0.25*	1.68
Education specialist or advanced graduate study	0.00	0.20	-0.02**	-2.21	0.02	1.63
Married ^	-0.07	-0.67	0.02	0.16	-0.10	-0.52
Widowed, divorced, separated^	0.10*	1.76	0.05	1.54	0.05	0.80
Never married ^	-0.03	-0.26	-0.07	-0.48	0.04	0.24
Family income (category: 1-5) ^	-0.14	-0.53	-0.20	-0.75	0.07	0.18
Family size ^	-0.22	-0.78	n/a	n/a	n/a	n/a
Kids under of age of 5^	-0.21	-1.35	0.07	0.32	-0.28	-1.08
Number of obs. (SASS/TFS)	780/146		673/133		1453/276	

^ – data and sample weights from TFS; ***,**,* - statistically significant at 1,5,10% respectively

Table A-10. t-tests for difference in mean values of personal characteristics of alternatively certified teachers with 3 and 4 years of experience

Personal characteristic	2003-04		1999-00		D-in-D	
	β	t-st.	β	t-st.	β	t-st.
Age	-0.24	-0.21	-1.99*	-1.83	1.75	1.12
Teacher white	-0.04	-0.64	-0.06	-1.34	0.02	0.27
Teacher black	0.04	0.69	0.05	1.18	-0.01	-0.14
Entry age	0.74	0.66	-0.99	-0.91	1.73	1.11
Female	-0.04	-0.79	0.08	1.35	-0.12	-1.53
Bachelor degree	0.01	0.53	0.01	0.68	0.00	0.04
Master degree	-0.01	-0.21	-0.08	-1.56	0.07	0.83
Associate's degree	-0.01	-0.15	0.02	0.46	-0.03	-0.43
College quality (index 1-5)	0.28**	1.96	0.05	0.42	0.24	1.30
Education specialist or advanced graduate study	-0.02*	-1.81	0.01	0.86	-0.03*	-1.91
Married ^	-0.30**	-2.28	-0.12	-0.78	-0.17	-0.85
Widowed, divorced, separated^	-0.06	-0.64	-0.01	-0.63	-0.05	-0.54
Never married ^	0.36***	3.48	0.13	0.84	0.23	1.20
Family income (category: 1-5) ^	-0.10	-0.27	0.10	0.34	-0.19	-0.43
Family size ^	-0.92**	-2.22	n/a	n/a	n/a	n/a
Kids under of age of 5^	0.06	0.24	-0.16	-0.79	0.22	0.69
Number of obs. (SASS/TFS)	735/122		651/95		1386/217	

^ – data and sample weights from TFS; ***,**,* - statistically significant at 1,5,10% respectively

Table A-11. t-tests for difference in mean values of personal characteristics of alternatively certified teachers with 4 and 5 years of experience

Personal characteristic	2003-04		1999-00		D-in-D	
	β	t-st.	β	t-st.	β	t-st.
Age	-1.28	-1.20	-1.94*	-1.78	0.66	0.44
Teacher white	0.01	0.27	0.01	0.26	0.01	0.08
Teacher black	-0.01	-0.26	0.00	0.05	-0.02	-0.24
Entry age	-0.28	-0.27	-0.94	-0.86	0.66	0.43
Female	-0.01	-0.28	-0.11**	-2.00	0.10	1.28
Bachelor degree	0.01	0.52	0.02	1.18	-0.01	-0.22
Master degree	-0.05	-0.82	-0.06	-0.97	0.01	0.10
Associate's degree	0.00	0.09	0.05	1.33	-0.04	-0.75
College quality (index 1-5)	-0.24	-1.78	0.02	0.22	-0.26	-1.53
Education specialist or advanced graduate study	0.00	0.19	0.01*	1.80	-0.01	-0.59
Married ^	0.06	0.41	0.11	0.50	-0.05	-0.19
Widowed, divorced, separated^	-0.09	-0.67	0.01	0.89	-0.10	-0.76
Never married ^	0.02	0.21	-0.13	-0.56	0.15	0.60
Family income (category: 1-5) ^	-0.54	-1.36	0.09	0.22	-0.63	-1.09
Family size ^	0.75*	1.72	n/a	n/a	n/a	n/a
Kids under of age of 5^	-0.18	-0.77	0.38**	2.07	-0.56	-1.88
Number of obs. (SASS/TFS)	782/107				600/58	

^ – data and sample weights from TFS; ***,**,* - statistically significant at 1,5,10% respectively

Table A-12. Results of logit regression estimation

Variable	Diagonal Definition β	Std. error	Hazard ratio	Definition 1 β	Std. error	Hazard ratio	Definition 2 β	Std. error	Hazard ratio
Alt. certificate in MA	-0.15	0.15	0.86	-0.19	0.16	0.82	-0.16	0.15	0.85
Alt. cert. X Exp.=1	1.16**	0.51	3.18**	1.15**	0.52	3.16**	1.13**	0.50	3.08**
Alt. cert. X Exp.=2	0.26	0.51	1.29	0.31	0.53	1.37	0.22	0.47	1.24
Alt. cert. X Exp.=3	0.43	0.47	1.54	0.48	0.48	1.61	0.43	0.46	1.53
Alt. cert. X Exp.=4	0.22	0.62	1.24	0.22	0.62	1.25	-0.06	0.61	0.94
Alt. cert. X Exp.=5	-0.01	0.92	0.99	0.09	0.95	1.10	0.48	0.76	1.61
Exp.=1 year	-0.45	0.36	0.64	-0.54	0.37	0.58	-0.57	0.36	0.57
Exp.=2 years	-0.72**	0.34	0.49**	-0.81**	0.35	0.45**	-0.66**	0.32	0.52**
Exp.=3 years	-0.09	0.31	0.91	-0.16	0.32	0.86	-0.46	0.31	0.63
Exp.=4 years	0.08	0.37	1.09	0.04	0.37	1.04	0.26	0.34	1.30
Exp.=5 years	0.44	0.36	1.55	0.39	0.36	1.47	-0.06	0.37	0.94
College	-0.00	0.07	0.99	0.12	0.13	1.12	0.10	0.13	1.10
Flag college	-0.66	0.55	0.51	0.56*	0.27	1.75*	0.46*	0.24	1.58*
Income35	-0.79*	0.44	0.46*	0.28*	0.21	1.33*	0.35*	0.21	1.42*
Income50	0.07	0.53	1.07	0.08	0.16	1.08	0.11	0.16	1.12
Income100	-0.87	0.60	0.42	-0.23	0.16	0.79	-0.16	0.16	0.85
Income100plus	-1.94***	0.71	0.14***	0.14	0.21	1.15	0.16	0.20	1.17
Reg. cert. in other field	0.02	0.14	1.03	0.03	0.14	1.03	0.13	0.14	1.14
Reg. cert. in other field X MA field is sp. ed.	0.44	0.36	1.55	0.42	0.36	1.53	0.26	0.35	1.30
Special Education	-0.61***	0.26	0.54***	-0.58**	0.26	0.56**	-0.49**	0.24	0.61**
English	0.89***	0.19	2.44***	0.90***	0.19	2.45***	0.91***	0.19	2.49***
Math	0.43*	0.24	1.53*	0.43*	0.24	1.54*	0.41*	0.23	1.51*
Sciences	-0.02	0.28	0.98	-0.04	0.28	0.96	-0.09	0.27	0.91
SocSc	0.60***	0.25	1.82***	0.58**	0.25	1.78**	0.52**	0.25	1.68**
Other	0.80***	0.16	2.22***	0.78***	0.16	2.18***	0.73***	0.16	2.07***
Year dummy	-0.16	0.11	0.85	-0.18	0.13	0.83	-0.17	0.12	0.85
Married	0.20	0.25	1.22	0.26	0.26	1.29	0.06	0.26	1.06
Married female	-0.38	0.28	0.68	-0.38	0.28	0.68	-0.09	0.28	0.91
Master	0.15	0.13	1.16	0.17**	0.13	1.19**	0.25**	0.13	1.29**
Master flag	0.78	0.60	2.19	0.32	0.61	1.37	0.37	0.60	1.44
Female	0.62***	0.26	1.86***	0.59	0.26	1.81	0.31	0.25	1.36
Non-white	0.00	0.19	1.00	-0.00	0.19	0.99	-0.06	0.19	0.94
Experience	-0.07*	0.04	0.94*	-0.07**	0.04	0.94**	-0.07**	0.03	0.93**
Experience sq.	0.01***	0.00	1.01***	0.01***	0.00	1.01***	0.01***	0.00	1.01***
Age first time teaching	0.00	0.01	1.00	0.00	0.01	1.00	0.00	0.01	1.00
Union member	-0.19	0.15	0.83	-0.18	0.15	0.84	-0.16	0.14	0.85
Real salary	-0.99	0.67	0.37	0.00	0.07	1.00	-0.01	0.06	0.99
Minority students	0.00	0.28	1.00	-0.02	0.29	0.98	0.02	0.28	1.02
Minority teachers	0.24	0.39	1.27	0.25	0.40	1.29	0.26	0.37	1.30
Free lunch eligible	0.66**	0.29	1.94**	0.69***	0.29	2***	0.66***	0.28	1.94***

Table A-12. Continued

Variable	Diagonal Definition β	Std. error	Hazard ratio	Definition 1 β	Std. error	Hazard ratio	Definition 2 β	Std. error	Hazard ratio
Flag for free lunch	0.26	0.45	1.30	0.27	0.44	1.31	0.28	0.43	1.33
Av. school size	-0.00	0.00	0.99	-0.00	0.00	0.99	-0.00	0.00	0.99
Urban	0.06	0.15	1.06	0.04	0.15	1.05	0.03	0.15	1.03
Constant	-1.74	0.66	0.18	-1.85	0.68	0.16	-1.74	0.66	0.18
State fixed effects	yes			yes			yes		
# of observations	5224			5712			6020		

*, **, *** − statistically significant at 10, 5, and 1% level respectively

Table A-13. Wald test for joint significance

Hypothesis	F-statistic Diagonal	Def. 1	Def.2
Alt ×Exp. 2=0 Alt ×Exp. 3=0 Alt ×Exp. 4=0 Alt ×Exp. 5=0	P>F=0.40	P>F=0.85	P>F=0.85
Alt ×Exp. 3=0 Alt ×Exp. 4=0 Alt ×Exp. 5=0	P>F=0.25	P>F=0.78	P>F=0.74
Alt ×Exp. 4=0 Alt ×Exp. 5=0	P>F=0.26	P>F=0.93	P>F=0.81

Table A-14. Results of logit regression estimation

Variable	Diagonal Definition β	Std. error	Hazard ratio	Definition 1 β	Std. error	Hazard ratio	Definition 2 β	Std. error	Hazard ratio
Alt. certificate in MA	-0.43	0.32	0.65	-0.45	0.31	0.64	-0.33	0.30	0.72
Alt. cert. X Exp.=1	1.66*	0.92	5.25*	1.55*	0.87	4.69*	1.36	0.83	3.89
Alt. cert. X Exp.=2	0.29	1.06	1.34	0.59	0.94	1.81	-0.06	0.86	0.94
Alt. cert. X Exp.=3	-1.08	0.96	0.34	-1.11	0.91	0.33	-1.11	0.80	0.33
Alt. cert. X Exp.=4	1.51	1.02	4.53	0.59	0.91	1.81	0.63	0.96	1.88
Alt. cert. X Exp.=5	0.43	1.10	1.54	-0.15	1.06	0.86	0.47	0.96	1.61
Exp.=1 year	-0.42	0.58	0.66	-0.41	0.54	0.66	-0.43	0.52	0.65
Exp.=2 years	-1.48***	0.48	0.23***	-0.95**	0.48	0.39**	-0.58	0.42	0.56
Exp.=3 years	0.09	0.49	1.10	0.07	0.43	1.07	-0.30	0.42	0.74
Exp.=4 years	-0.25	0.60	0.78	0.03	0.52	1.04	0.38	0.49	1.46
Exp.=5 years	0.08	0.58	1.08	0.48	0.49	1.62	-0.51	0.50	0.6
College	0.04	0.16	1.04	0.00	0.16	1.00	-0.01	0.16	0.99
Flag college	0.68***	0.28	1.98***	0.64***	0.27	1.9***	0.49**	0.24	1.63**
College × Exp1	-0.12	0.55	0.89	-0.15	0.53	0.86	-0.15	0.51	0.86
College × Exp2	0.93*	0.50	2.53*	0.22	0.51	1.25	-0.09	0.45	0.91
College × Exp3	-0.55	0.53	0.58	-0.33	0.48	0.72	-0.24	0.47	0.79
College × Exp4	0.18	0.69	1.20	0.00	0.65	1.00	-0.18	0.59	0.83
College × Exp5	0.10	0.73	1.10	-0.12	0.60	0.88	0.64	0.62	1.89
Alt × College	0.63*	0.33	1.88*	0.68**	0.32	1.97**	0.52	0.32	1.68
Alt × College × Exp1	-0.64	1.11	0.53	-0.63	1.08	0.53	-0.42	1.04	0.66
Alt × College × Exp2	-0.25	1.24	0.78	-0.43	1.13	0.65	0.28	1.04	1.33
Alt × College × Exp3	2.28**	1.11	9.76**	1.88*	1.07	6.55*	2.04**	0.97	7.69**
Alt × College × Exp4	-0.65	1.33	0.52	-0.62	1.24	0.54	-1.21	1.25	0.30
Alt × College × Exp5	0.31	1.57	1.37	0.32	1.49	1.38	-0.13	1.29	0.88
Income35	0.22	0.25	1.25	0.3	0.23	1.36	0.39*	0.23	1.47*
Income50	0.06	0.19	1.07	0.13	0.18	1.13	0.12	0.18	1.13
Income100	-0.18	0.18	0.84	-0.22	0.18	0.80	-0.14	0.18	0.87
Income100plus	0.41*	0.24	1.51*	0.33	0.23	1.39	0.33	0.22	1.39
Alt × Income35	-0.25	0.50	0.78	-0.07	0.44	0.93	-0.14	0.45	0.87
Alt × Income50	-0.37	0.42	0.69	-0.27	0.39	0.77	-0.08	0.38	0.92
Alt × Income100	-0.06	0.37	0.94	-0.08	0.36	0.92	-0.09	0.36	0.91
Alt × Income100plus	-1.01**	0.45	0.36**	-1.01***	0.43	0.36***	-0.97**	0.42	0.38**
Reg. cert. in other field	-0.02	0.15	0.98	0.04	0.14	1.04	0.13	0.14	1.14
Reg. cert. in other field X MA field is sp. ed.	0.35	0.38	1.42	0.42	0.37	1.52	0.24	0.35	1.28
Special Education	-0.47*	0.27	0.63*	-0.59**	0.26	0.55**	-0.48*	0.25	0.62*
English	0.86***	0.20	2.37***	0.90***	0.19	2.45***	0.91***	0.19	2.47***
Math	0.43*	0.24	1.54*	0.47**	0.24	1.6**	0.44*	0.23	1.55*
Sciences	-0.12	0.30	0.89	-0.04	0.28	0.96	-0.10	0.27	0.90
SocSc	0.58**	0.27	1.79**	0.61***	0.25	1.84***	0.55**	0.25	1.73**
Other	0.82***	0.17	2.27***	0.79***	0.16	2.20***	0.72***	0.16	2.06***

Table A-14. Continued

Variable	Diagonal Definition β	Std. error	Hazard ratio	Definition 1 β	Std. error	Hazard ratio	Definition 2 β	Std. error	Hazard ratio
Year	-0.25*	0.13	0.78*	-0.19	0.13	0.83	-0.18	0.12	0.84
Married	0.33	0.27	1.39	0.25	0.27	1.28	0.08	0.26	1.08
Married female	-0.47	0.30	0.62	-0.38	0.29	0.69	-0.11	0.28	0.90
Master	0.16	0.14	1.18	0.18	0.13	1.19	0.26**	0.13	1.30**
Master flag	0.49	0.63	1.63	0.48	0.61	1.61	0.53	0.60	1.70
Female	0.68***	0.27	1.98***	0.60**	0.26	1.82**	0.33	0.26	1.39
Non-white	0.11	0.20	1.12	0.00	0.19	1.00	-0.03	0.19	0.97
Experience	-0.07*	0.04	0.94*	-0.07*	0.04	0.94*	-0.07**	0.03	0.93**
Experience sq.	0.01***	0.00	1.01***	0.01***	0.00	1.01***	0.01***	0.00	1.01***
Age first time teaching	-0.00	0.01	0.99	0.00	0.01	1.00	0.00	0.01	1.00
Union member	-0.16	0.16	0.85	-0.17	0.15	0.84	-0.16	0.14	0.85
Real salary	0.00	0.07	1.00	-0.00	0.07	0.99	-0.01	0.07	0.99
Minority students	0.04	0.30	1.04	-0.03	0.29	0.97	0.02	0.28	1.02
Minority teachers	0.21	0.40	1.23	0.24	0.40	1.28	0.25	0.37	1.29
Free lunch eligible	0.77***	0.31	2.16***	0.71***	0.29	2.03***	0.66***	0.28	1.93***
Flag for free lunch	0.30	0.45	1.36	0.29	0.44	1.34	0.30	0.44	1.34
Av. school size	-0.00	0.00	0.99	-0.00	0.00	0.99	-0.00	0.00	0.99
Urban	0.06	0.16	1.06	0.05	0.15	1.05	0.02	0.15	1.02
Constant	-1.85	0.73	0.16	-1.86	0.68	0.16	-1.73	0.66	0.18
State fixed effects	yes			yes			yes		
# of observations	5224			5712			6020		

*, **, *** – statistically significant at 10, 5, and 1% level respectively.

Table A-15. Wald test for joint significance

Hypothesis	F-statistic Diagonal	Def. 1	Def.2
Alt ×Exp2=0 Alt ×Exp3=0 Alt ×Exp4=0 Alt ×Exp5=0 College ×Alt ×Exp2=0 College ×Alt ×Exp3=0 College ×Alt ×Exp4=0 College ×Alt ×Exp5=0	P>F=0.07	P>F=0.17	P>F=0.03
Alt ×Exp3=0 Alt ×Exp4=0 Alt ×Exp5=0 College ×Alt ×Exp3=0 College ×Alt ×Exp4=0 College ×Alt ×Exp5=0	P>F=0.03	P>F=0.11	P>F=0.01
Alt ×Exp4=0 Alt ×Exp5=0 College ×Alt ×Exp4=0 College ×Alt ×Exp5=0	P>F=0.45	P>F=0.61	P>F=0.17

APPENDIX B
DETAILED RESULTS FOR ESTIMATION OF THE EFFECT OF SCHOOL ACCOUNTABILITY

Table B-1. Accountability policy

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
State	Test R,W,M	Test other	Student account.	Exit test	School evaluation	Subgroup	LPS Sanctions	Sch. adm. sanctions	HPS Award	LPS assistance	LPS fin. assistance	District evaluation	District sanctions	District assistance	Student	School	District	Carnoy-Loeb
AL	1	1	0	1	1	0	0	0	0	1	1	0	0	0	1	1	0	4
AK	1	0	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0	1
AZ	1	0	1	0	0	0	0	0	0	1	0	0	0	0	1	0	0	2
AR	1	0	0	0	0	0	0	0	0	1	1	0	1	0	0	0	0	1
CA	1	1	0	0	1	1	1	1	1	0	1	0	0	0	0	1	0	4
CO	1	1	1	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1
CT	1	1	1	0	0	0	0	0	0	0	0	0	0	0	1	0	0	1
DE	1	1	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0	1
FL	1	0	1	1	1	1	1	1	1	1	1	0	0	0	1	1	0	5
GA	1	1	0	1	0	0	0	0	1	1	1	0	0	0	1	0	0	2
HI	1	0	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0	1
ID	1	1	0	0	1	0	1	0	0	1	1	0	0	0	0	1	0	1
IL	1	1	1	0	1	0	0	0	0	1	1	0	0	0	1	0	0	2.5
IN	1	0	0	1	1	0	0	0	1	1	1	1	0	0	1	1	0	3
IA	0	0	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0	0
KS	1	1	0	0	1	0	0	0	0	1	1	0	0	0	0	1	0	1
KY	1	1	0	0	0	0	0	0	1	1	1	0	0	0	0	1	0	4
LA	1	1	1	1	1	0	1	1	1	1	1	0	0	0	1	1	0	3
ME	1	1	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1
MD	1	1	0	1	1	0	0	1	1	1	1	0	0	0	1	1	0	4
MA	1	1	0	0	0	0	0	1	0	1	1	0	0	0	0	0	0	2
MI	1	1	1	0	1	0	0	0	0	1	0	0	0	0	0	0	0	1
MN	1	0	0	1	0	0	0	0	0	1	0	0	0	0	1	0	0	2
MS	1	0	0	1	0	0	0	0	0	0	0	1	1	1	1	0	1	3
MO	1	1	0	0	0	0	0	1	1	1	0	1	1	0	0	0	0	1.5
MT	1	1	0	0	1	0	0	0	0	1	1	0	0	0	0	0	0	1

Table B-1. Continued.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
State	Test R,W,M	Test other	Student account.	Exit test	School evaluation	Subgroup	LPS Sanctions	Sch. adm. sanctions	HPS Award	LPS assistance	LPS fin. assistance	District evaluation	District sanctions	District assistance	Student	School	District	Carnoy-Loeb
NE	0	0	0	0	0	0	0	0	0	1	0	0	0	0	0	0	0	0
NC	1	1	1	0	1	0	0	1	1	1	0	1	1	0	0	1	0	5
ND	1	1	0	0	1	0	1	0	0	1	1	0	0	0	0	1	0	1
OH	1	1	1	1	0	0	0	1	0	0	1	1	1	1	1	0	1	3
OK	1	1	1	0	1	0	0	0	0	1	1	0	0	0	0	1	0	1
OR	1	1	1	0	0	0	0	0	0	1	1	0	0	0	0	0	0	2.5
PA	1	0	1	0	0	0	0	0	1	0	0	0	0	0	0	1	0	1
RI	1	0	0	0	0	1	0	0	0	1	0	0	0	0	0	1	0	1
SC	1	1	1	1	0	1	0	0	0	0	0	1	1	1	1	0	1	3
SD	1	1	0	0	1	0	0	0	0	1	1	0	0	0	0	0	0	1
TN	1	1	0	1	0	0	0	0	0	1	1	0	0	0	1	0	0	1.5
TX	1	1	0	1	1	1	1	1	1	1	0	1	0	0	1	1	0	5
UT	1	0	0	0	1	0	0	0	0	1	0	0	0	0	0	0	0	1
VT	1	0	1	0	0	0	0	0	0	1	0	0	0	0	0	0	0	1
VA	1	1	1	1	1	0	0	0	0	0	0	0	0	0	1	0	0	2
WA	1	1	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0	1
WV	1	1	1	0	1	0	1	0	0	1	1	1	1	1	0	1	1	3.5
WI	1	1	0	0	0	0	0	0	0	1	1	0	0	0	0	0	0	2
WY	1	0	0	0	0	0	0	0	0	1	1	1	1	0	0	0	0	1
Total	48	34	18	16	20	5	8	12	14	46	30	11	9	6	19	18	5	

Table B-2. Summary statistics by fields and year

	English language arts				Mathematics				Social Sciences				Sciences			
	1993-94		1999-00		1993-94		1999-00		1993-94		1999-00		1993-94		1999-00	
Variable	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std
Certification	0.95	0.0058	0.91	0.011	0.94	0.01	0.88	0.013	0.91	0.010	0.87	0.014	0.94	0.01	0.87	0.016
LPS_field	0.32	0.0181	0.37	0.016	0.289	0.017	0.32	0.016	0.285	0.016	0.32	0.02	0.28	0.01	0.33	0.014
white teacher	0.92	0.0134	0.87	0.015	0.91	0.01	0.89	0.014	0.92	0.014	0.90	0.01	0.93	0.01	0.89	0.016
black teacher	0.07	0.013	0.10	0.016	0.07	0.01	0.08	0.015	0.07	0.013	0.08	0.01	0.05	0.01	0.07	0.014
Sex	0.19	0.0142	0.17	0.018	0.44	0.02	0.39	0.024	0.63	0.022	0.61	0.02	0.54	0.03	0.47	0.020
Entry age	25.6	0.2	25.4	0.3	25.2	0.3	25.0	0.3	25.7	0.3	25.6	0.2	25.8	0.3	26.4	0.3
Experience	15.8	0.5	15.3	0.4	16.0	0.5	14.6	0.4	16.5	0.6	15.2	0.4	15.3	0.5	13.5	0.4
Union agreement	0.69	0.089	0.68	0.097	0.70	0.09	0.70	0.086	0.70	0.088	0.71	0.08	0.71	0.08	0.72	0.081
Urban	0.55	0.024	0.75	0.028	0.58	0.03	0.76	0.032	0.54	0.031	0.74	0.03	0.55	0.03	0.74	0.032
Free lunch	0.32	0.018	0.37	0.016	0.29	0.02	0.32	0.016	0.29	0.016	0.32	0.02	0.28	0.01	0.33	0.014
Minority students	0.29	0.035	0.36	0.037	0.28	0.03	0.32	0.030	0.28	0.037	0.32	0.03	0.27	0.03	0.32	0.033
Minority teachers	0.12	0.016	0.17	0.020	0.12	0.02	0.16	0.014	0.12	0.016	0.14	0.02	0.11	0.01	0.15	0.016
District enrollment	0.256	0.543	0.569	2.091	0.269	0.592	0.597	2.384	0.264	0.623	0.599	2.351	0.264	0.661	0.451	11900
bigD * LPS	0.115	0.312	0.311	1.319	0.114	0.347	0.313	1.436	0.104	0.331	0.281	1.132	0.101	0.297	0.237	7518
Student * year	0.000	0.000	0.510	0.106	0.000	0.000	0.500	0.101	0.000	0.000	0.500	0.100	0.000	0.000	0.480	0.100
Student * LPS	0.000	0.000	0.200	0.045	0.000	0.000	0.160	0.037	0.000	0.000	0.170	0.040	0.000	0.000	0.150	0.036
Student * bigD	0.000	0.000	0.199	0.617	0.000	0.000	0.199	0.679	0.000	0.000	0.195	0.628	0.000	0.000	0.156	5030
Student * bigD * LPS	0.000	0.000	0.102	0.337	0.000	0.000	0.092	0.375	0.000	0.000	0.092	0.364	0.000	0.000	0.064	2157
School * year	0.000	0.000	0.480	0.108	0.000	0.000	0.470	0.102	0.000	0.000	0.450	0.100	0.000	0.000	0.440	0.102
School * LPS	0.000	0.000	0.190	0.046	0.000	0.000	0.170	0.038	0.000	0.000	0.160	0.040	0.000	0.000	0.160	0.038

Table B-2. Continued

Variable	English language arts				Mathematics				Social Sciences				Sciences			
	1993-94		1999-00		1993-94		1999-00		1993-94		1999-00		1993-94		1999-00	
	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std	Mean	Std
School * bigD	0.000	0.000	0.212	0.758	0.000	0.000	0.208	0.849	0.000	0.000	0.221	0.961	0.000	0.000	0.223	0.959
School * bigD * LPS	0.000	0.000	0.101	0.376	0.000	0.000	0.096	0.405	0.000	0.000	0.101	0.490	0.000	0.000	0.105	0.485
District * year	0.000	0.000	0.110	0.055	0.000	0.000	0.110	0.054	0.000	0.000	0.100	0.050	0.000	0.000	0.100	0.051
District * LPS	0.000	0.000	0.040	0.019	0.000	0.000	0.030	0.013	0.000	0.000	0.030	0.020	0.000	0.000	0.030	0.014
District * bigD	0.000	0.000	0.014	0.080	0.000	0.000	0.010	0.052	0.000	0.000	0.009	0.040	0.000	0.000	0.009	0.047
District * bigD * LPS	0.000	0.000	0.008	0.046	0.000	0.000	0.004	0.020	0.000	0.000	0.003	0.016	0.000	0.000	0.003	0.015
Index * year	0.000	0.000	3.010	0.356	0.000	0.000	2.970	0.326	0.000	0.000	2.900	0.330	0.000	0.000	2.800	0.331
Index * LPS	0.000	0.000	1.170	0.170	0.000	0.000	1.005	0.142	0.000	0.000	0.960	0.140	0.000	0.000	0.940	0.135
Index * bigD	0.000	0.000	2.309	10.726	0.000	0.000	2.438	12.187	0.000	0.000	2.484	11.946	0.000	0.000	1.675	5.984
Index * bigD * LPS	0	0	1.287	6.695	0.000	0.000	1.299	7.266	0.000	0.000	1.161	5.620	0.000	0.000	0.886	3.769
Number of observations	8747				6547				5593				5794			
Population size	471404.76				313613.9				252977.01				272132.38			

Table B-3. Logit results for student-targeted accountability policy

Variable	English odds ratio	t-st.	Mathematics odds ratio	t-st.	Social sciences odds ratio	t-st.	Sciences odds ratio	t-st.
student * year	0.86	-0.31	0.64	-0.81	2.25*	1.87	1.48	1.22
student * LPS	1.86	0.81	2.75	1.31	0.63	-0.59	0.91	-0.19
year	0.81	-0.58	1.00	0.01	0.62**	-2.07	0.39***	-3.50
LPS * year	0.39*	-1.72	0.24**	-1.96	0.60	-0.70	1.55	0.96
white teacher	0.97	-0.05	1.11	0.52	0.49**	-2.30	0.68**	-2.25
black teacher	0.48	-1.22	0.83	-0.57	0.22***	-3.58	0.42**	-2.20
Sex	1.36**	2.16	1.68*	2.91	1.88***	3.19	1.35	1.15
Entry age	1.00	-0.27	0.99	-1.33	1.00	0.30	1.00	-0.02
Experience	1.06***	4.65	1.04***	3.28	1.13***	6.94	1.06***	2.96
Union agreement	0.85	-0.85	1.48	1.24	1.55*	1.90	1.55*	1.95
Urban	1.16	0.71	1.09	0.47	1.21	1.00	0.91	-0.36
Free lunch	0.48**	-2.27	0.94	-0.09	1.15	0.28	0.37***	-3.14
Minority students	0.58	-1.27	0.80	-0.45	1.48	0.69	1.52	0.97
Minority teachers	1.90	0.98	0.59	-1.12	0.42*	-1.84	0.79	-0.59
District enrollment	0.91	-0.74	0.96	-0.57	1.04	0.91	0.83***	-3.18
Constant	15.80	4.08	11.94	5.11	2.27	1.53	11.13	3.29
Number of obs.	8747		6547		5593		5794	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-4. Logit results for school-targeted accountability policy

Variable	English odds ratio	t-st.	Mathematics odds ratio	t-st.	Social sciences odds ratio	t-st.	Sciences odds ratio	t-st.
school * year	0.76	-0.57	0.53	-1.04	0.88	-0.30	0.59	-1.53
school * LPS	1.68	0.68	0.80	-0.25	0.67	-0.54	1.73	0.99
year	0.85	-0.48	1.04	0.09	0.93	-0.26	0.60**	-2.16
LPS * year	0.44	-1.57	0.50	-0.78	0.74	-0.44	1.21	0.41
white teacher	0.97	-0.06	1.17	0.87	0.49***	-2.33	0.68**	-2.14
black teacher	0.48	-1.26	0.90	-0.32	0.22***	-3.46	0.43**	-2.19
Sex	1.35**	2.10	1.63***	2.72	1.88***	3.15	1.35	1.14
Entry age	1.00	-0.38	0.99	-1.27	1.00	0.36	1.00	0.00
Experience	1.06***	4.68	1.04***	3.39	1.13***	7.01	1.06***	2.95
Union agreement	0.85	-0.83	1.48	1.27	1.58*	1.95	1.54**	1.98
Urban	1.16	0.73	1.08	0.42	1.20	0.91	0.92	-0.33
Free lunch	0.47***	-2.44	0.85	-0.24	1.02	0.05	0.36***	-3.26
Minority students	0.57	-1.31	0.80	-0.44	1.48	0.68	1.49	0.88
Minority teachers	1.99	1.11	0.66	-0.90	0.40**	-2.26	0.79	-0.61
District enrollment	0.91	-0.76	0.94	-0.95	1.03	0.59	0.83***	-3.07
Constant	17.29	3.78	14.44	4.88	2.89	2.00	13.60	3.42
Number of obs.	8747		6547		5593		5794	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-5. Logit results for district-targeted accountability policy

Variable	English		Mathematics		Social sciences		Sciences	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
District*year	0.25***	-3.62	3.00	1.46	2.18***	3.10	0.53*	-1.82
District* LPS	5.10***	2.72	0.64	-0.52	3.00	0.80	1.27	0.36
year	0.89	-0.40	0.70	-0.84	0.81	-0.81	0.49***	-3.01
LPS * year	0.45	-1.57	0.40	-1.11	0.55	-1.04	1.48	0.95
white teacher	1.01	0.02	1.13	0.64	0.47***	-2.35	0.68**	-2.17
black teacher	0.50	-1.14	0.85	-0.45	0.21***	-3.63	0.43**	-2.15
Sex	1.36**	2.12	1.67***	2.77	1.90***	3.13	1.36	1.18
Entry age	1.00	-0.40	0.99	-1.26	1.00	0.27	1.00	-0.01
Experience	1.06***	4.75	1.04***	3.35	1.13***	7.04	1.06***	2.99
Union agreement	0.85	-0.84	1.51	1.28	1.58**	1.96	1.55**	1.97
Urban	1.17	0.84	1.08	0.42	1.20	0.89	0.91	-0.36
Free lunch	0.48	-2.31	0.94	-0.09	1.04	0.08	0.37	-3.23
Minority students	0.56	-1.38	0.81	-0.42	1.49	0.72	1.54	0.97
Minority teachers	1.99	1.07	0.64	-0.95	0.40**	-2.24	0.77	-0.63
District enrollment	0.91	-0.74	0.95	-0.74	1.04	0.95	0.83***	-2.94
Constant	15.64	3.72	11.94	4.72	2.97	2.01	11.94	3.47
Number of observations	8747		6547		5593		5794	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-6. Logit results for aggregated accountability policy index

Variable	English		Mathematics		Social sciences		Sciences	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Index * year	1.12	0.69	0.95	-0.22	1.03	0.28	0.87	-1.15
Index * LPS	0.75	-0.98	0.71	-1.03	0.94	-0.27	1.14	0.80
year	0.54	-1.23	0.91	-0.16	0.80	-0.61	0.70	-1.05
LPS * year	1.42	0.41	1.17	0.13	0.68	-0.41	1.06	0.10
white teacher	1.01	0.02	1.15	0.78	0.48**	-2.31	0.69**	-2.14
black teacher	0.49	-1.21	0.82	-0.60	0.22***	-3.44	0.43**	-2.21
Sex	1.38**	2.18	1.67***	2.82	1.88***	3.18	1.36	1.17
Entry age	1.00	-0.41	0.99	-1.28	1.00	0.32	1.00	0.00
Experience	1.06***	4.69	1.04***	3.30	1.13***	6.95	1.06***	3.01
Union agreement	0.87	-0.72	1.46	1.23	1.58*	1.93	1.54*	1.95
Urban	1.14	0.66	1.09	0.45	1.20	0.91	0.92	-0.33
Free lunch	0.47***	-2.50	0.91	-0.15	1.06	0.12	0.36***	-3.27
Minority students	0.58	-1.32	0.82	-0.43	1.49	0.71	1.54	0.94
Minority teachers	1.99	1.06	0.64	-0.96	0.40**	-2.15	0.78	-0.63
District enrollment	0.91	-0.73	0.97	-0.52	1.04	0.77	0.83***	-2.89
Constant	17.29	3.74	14.30	5.06	2.69	1.92	13.74	3.22
Number of obs.	8747		6547		5593		5794	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-7. Logit results for sample consisting of English language arts (base field) and mathematics teachers

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	0.86	-0.35	0.67	-0.92	0.43*	-1.85	1.08	0.51
Policy * Math	0.76	-0.66	0.91	-0.20	4.53***	3.39	0.92	-0.68
Policy * LPS	1.80	0.87	1.46	0.54	3.56***	2.33	0.74	-1.07
Policy * LPS * Math	1.38	0.38	0.58	-0.58	0.30	-1.43	0.96	-0.12
Math	0.53***	-2.39	0.52***	-2.46	0.52***	-2.39	0.52***	-2.41
year	0.79	-0.68	0.85	-0.53	0.81	-0.71	0.57	-1.24
Math * year	1.27	0.64	1.17	0.34	0.93	-0.17	1.40	0.62
LPS * year	0.44	-1.65	0.55	-1.24	0.52	-1.34	1.62	0.59
LPS * Math	2.08	1.26	2.18	1.36	2.10	1.29	2.12	1.30
LPS * Math * year	0.52	-0.78	0.77	-0.27	0.70	-0.44	0.68	-0.27
white teacher	1.08	0.33	1.12	0.43	1.09	0.37	1.12	0.43
black teacher	0.64	-1.38	0.66	-1.27	0.64	-1.34	0.63	-1.44
Sex	1.54***	3.45	1.52***	3.34	1.54***	3.37	1.55***	3.44
Entry age	0.99	-1.20	0.99	-1.30	0.99	-1.30	0.99	-1.26
Experience	1.05***	4.93	1.05***	4.99	1.05***	5.05	1.05***	5.00
Union agreement	1.08	0.68	1.09	0.73	1.11	0.82	1.11	0.79
Urban	1.12	0.70	1.11	0.66	1.12	0.72	1.11	0.63
Free lunch	0.48***	-2.46	0.44***	-2.82	0.47***	-2.53	0.45***	-2.70
Minority students	0.68	-1.04	0.67	-1.05	0.68	-1.06	0.68	-1.08
Minority teachers	1.09	0.19	1.19	0.38	1.17	0.34	1.17	0.34
District enrollment	0.94	-0.78	0.92	-0.91	0.93	-0.83	0.94	-0.76
Constant	17.64	7.58	20.49	7.64	17.99	7.45	20.49	7.16
Number of obs.	15379		15379		15379		15379	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-8. Logit results for sample consisting of English language arts (base field) and social sciences teachers

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	0.91	-0.18	0.76	-0.58	0.37*	-1.66	1.14	0.86
Policy * Soc. Sc.	2.53	1.55	1.26	0.41	3.67*	1.93	0.90	-0.50
Policy * LPS	1.80	0.78	1.73	0.73	3.94**	2.15	0.77	-0.89
Policy * LPS* Soc. Sc.	0.31	-1.31	0.30	-1.51	1.40	0.31	1.15	0.41
Soc. Sc.	0.30***	-4.88	0.30***	-5.01	0.30***	-4.92	0.30***	-4.89
year	0.82	-0.54	0.88	-0.40	0.89	-0.39	0.53	-1.38
Soc. Sc. * year	0.63	-1.20	0.84	-0.40	0.81	-0.72	1.30	0.44
LPS * year	0.39*	-1.72	0.43	-1.57	0.46	-1.52	1.22	0.23
LPS * Soc. Sc.	2.94***	2.63	2.92***	2.70	2.92***	2.65	2.89***	2.63
LPS * Soc. Sc. * year	2.39	1.07	2.75	1.51	1.55	0.73	0.95	-0.05
white teacher	0.79	-0.53	0.79	-0.53	0.80	-0.51	0.81	-0.48
black teacher	0.38*	-1.92	0.38*	-1.93	0.38*	-1.95	0.39*	-1.89
Sex	1.55***	3.32	1.55***	3.28	1.57***	3.32	1.57***	3.42
Entry age	1.00	-0.14	1.00	-0.09	1.00	-0.19	1.00	-0.17
Experience	1.08***	7.76	1.08***	7.80	1.08***	7.72	1.08***	7.76
Union agreement	1.06	0.39	1.08	0.47	1.08	0.50	1.08	0.50
Urban	1.15	0.92	1.15	0.94	1.15	0.97	1.14	0.88
Free lunch	0.41***	-2.93	0.39**	-3.23	0.41***	-2.98	0.40***	-3.06
Minority students	0.81	-0.89	0.79	-0.95	0.80	-0.95	0.82	-0.83
Minority teachers	1.11	0.25	1.12	0.27	1.09	0.23	1.09	0.23
District enrollment	0.99	-0.25	0.98	-0.29	0.99	-0.24	0.99	-0.19
Constant	12.06	4.76	13.87	4.80	13.33	4.81	13.20	4.52
Number of obs.	14340		14340		14340		14340	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-9. Logit results for sample consisting of English language arts (base field) and sciences teachers

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	0.95	-0.11	0.82	-0.44	0.24***	-3.77	1.12	0.76
Policy * Sciences	1.28	0.72	0.64	-1.30	2.29**	2.02	0.78***	-2.36
Policy * LPS	1.77	0.80	1.52	0.59	3.94***	2.37	0.76	-1.01
Policy * LPS* Sciences	0.60	-0.74	1.12	0.15	0.61	-0.76	1.48	1.63
Sciences	0.75	-1.36	0.74	-1.43	0.76	-1.27	0.75	-1.39
year	0.80	-0.65	0.84	-0.55	0.92	-0.29	0.57	-1.32
Sciences * year	0.51**	-1.99	0.71	-0.92	0.51**	-2.24	1.19	0.37
LPS * year	0.39*	-1.82	0.45	-1.60	0.44*	-1.66	1.31	0.34
LPS * Sciences	0.99	-0.03	1.00	0.00	0.96	-0.09	0.99	-0.03
LPS * Sciences * year	3.90*	1.85	2.89	1.49	3.35**	1.99	0.90	-0.10
white teacher	0.84	-0.49	0.84	-0.51	0.86	-0.45	0.86	-0.45
black teacher	0.47**	-2.05	0.46**	-2.15	0.47**	-2.04	0.47**	-2.09
Sex	1.36*	1.89	1.36*	1.86	1.36*	1.87	1.36*	1.87
Entry age	1.00	-0.33	1.00	-0.42	1.00	-0.45	1.00	-0.45
Experience	1.06***	4.44	1.06***	4.39	1.06***	4.50	1.06***	4.46
Union agreement	1.09	0.57	1.08	0.56	1.09	0.61	1.09	0.62
Urban	1.04	0.25	1.06	0.31	1.05	0.32	1.04	0.25
Free lunch	0.45***	-2.56	0.43***	-2.88	0.46***	-2.60	0.44***	-2.81
Minority students	0.84	-0.60	0.83	-0.66	0.84	-0.65	0.85	-0.59
Minority teachers	1.34	0.58	1.39	0.70	1.38	0.64	1.36	0.63
District enrollment	0.88	-1.49	0.88	-1.50	0.88	-1.44	0.88	-1.47
Constant	15.64	5.36	18.36	5.01	16.12	5.12	18.54	4.81
Number of obs.	14571		14571		14571		14571	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-10. Logit results for sample consisting of mathematics (base field) and sciences teachers

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	0.72	-0.74	0.66	-0.88	2.14	1.01	0.97	-0.15
Policy * Sciences	1.73	1.58	0.72	-0.83	0.51	-0.98	0.87	-1.03
Policy * LPS	2.39	1.17	0.79	-0.26	0.61	-0.60	0.75	-0.91
Policy * LPS* Sciences	0.44	-1.02	1.95	0.77	2.14	0.75	1.49	1.57
Sciences	1.32	0.96	1.31	0.93	1.32	0.94	1.34	0.97
year	0.98	-0.04	0.99	-0.01	0.77	-0.67	0.89	-0.23
Sciences * year	0.41***	-2.54	0.64	-0.99	0.58	-1.22	0.82	-0.38
LPS * year	0.23*	-1.94	0.45	-0.87	0.39	-1.17	0.96	-0.03
LPS * Sciences	0.51	-1.00	0.51	-1.01	0.51	-1.01	0.50	-1.00
LPS * Sciences * year	6.96***	2.56	3.29	1.13	4.22	1.60	1.31	0.23
white teacher	0.94	-0.40	0.97	-0.23	0.95	-0.35	0.96	-0.30
black teacher	0.66	-1.53	0.67	-1.42	0.66	-1.49	0.64	-1.65
Sex	1.52***	2.40	1.52**	2.32	1.52***	2.36	1.54***	2.38
Entry age	0.99	-0.86	0.99	-0.84	0.99	-0.82	0.99	-0.83
Experience	1.05***	4.06	1.05***	4.09	1.05***	4.13	1.05***	4.11
Union agreement	1.52***	2.51	1.54***	2.58	1.55***	2.59	1.52***	2.56
Urban	1.01	0.05	1.01	0.05	1.00	0.03	1.01	0.04
Free lunch	0.84	-0.31	0.78	-0.43	0.81	-0.35	0.80	-0.38
Minority students	1.09	0.27	1.08	0.24	1.12	0.32	1.12	0.32
Minority teachers	0.68	-1.25	0.73	-1.09	0.71	-1.12	0.70	-1.14
District enrollment	0.90*	-1.92	0.89**	-2.05	0.89*	-1.92	0.90*	-1.89
Constant	9.30	5.15	11.59	5.12	9.78	4.99	11.47	4.89
Number of obs.	12341		12341		12341		12341	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-11. Logit results for sample consisting of mathematics (base field) and social sciences teachers

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	0.64	-0.83	0.64	-0.80	2.72	1.14	1.03	0.14
Policy * Soc. Sc.	3.46***	2.25	1.34	0.49	0.76	-0.30	0.97	-0.12
Policy * LPS	2.64	1.10	0.91	-0.09	0.60	-0.55	0.73	-0.99
Policy *LPS*S.Sc.	0.23	-1.43	0.55	-0.54	5.31	1.16	1.23	0.56
Soc. Sc.	0.54**	-2.17	0.54**	-2.19	0.54**	-2.16	0.54**	-2.17
year	1.04	0.08	1.01	0.02	0.75	-0.68	0.76	-0.48
Soc. Sc. * year	0.54	-1.23	0.83	-0.34	0.97	-0.06	1.02	0.03
LPS * year	0.23*	-1.90	0.43	-0.91	0.39	-1.16	1.08	0.07
LPS * Soc. Sc.	1.57	0.94	1.51	0.91	1.49	0.86	1.51	0.88
LPS * Soc.Sc.* year	3.63	1.38	2.66	0.91	1.82	0.69	1.01	0.01
white teacher	0.92	-0.41	0.93	-0.35	0.91	-0.46	0.92	-0.42
black teacher	0.54*	-1.87	0.55*	-1.80	0.54*	-1.90	0.52**	-2.05
Sex	1.68***	3.32	1.67***	3.20	1.68***	3.22	1.68***	3.34
Entry age	0.99	-0.66	0.99	-0.54	0.99	-0.61	0.99	-0.57
Experience	1.07***	6.41	1.07***	6.53	1.07***	6.47	1.07***	6.36
Union agreement	1.46*	1.85	1.51**	2.00	1.52**	2.00	1.51*	1.97
Urban	1.11	0.85	1.11	0.76	1.09	0.76	1.09	0.71
Free lunch	0.74	-0.52	0.69	-0.62	0.73	-0.53	0.70	-0.60
Minority students	1.15	0.39	1.14	0.35	1.16	0.42	1.16	0.42
Minority teachers	0.47**	-2.24	0.49**	-2.29	0.48**	-2.31	0.48**	-2.23
District enrollment	0.99	-0.11	0.98	-0.52	0.99	-0.22	1.01	0.05
Constant	6.62	3.79	7.92	4.05	7.46	3.89	7.61	4.18
Number of obs.	12140		12140		12140		12140	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-12. Logit results for sample consisting of social sciences teachers (base field) and sciences teachers

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	2.56***	2.45	1.08	0.20	1.65**	2.06	1.02	0.11
Policy * Sciences	0.45	-1.42	0.48	-1.34	0.61	-0.86	0.88	-0.60
Policy * LPS	0.47	-0.97	0.46	-1.07	2.03	0.71	0.94	-0.21
Policy * LPS * Sc.	2.41	1.04	3.46	1.50	0.47	-0.65	1.22	0.65
Sciences	2.44***	4.45	2.44***	4.46	2.46***	4.50	2.46***	4.48
year	0.53***	-2.71	0.79	-0.93	0.77	-1.12	0.77	-0.70
Sciences * year	0.80	-0.58	0.79	-0.64	0.59*	-1.77	0.81	-0.38
LPS * year	0.88	-0.19	1.06	0.11	0.68	-0.77	0.83	-0.22
LPS * Sciences.	0.33***	-2.45	0.34***	-2.37	0.33***	-2.39	0.33***	-2.40
LPS * Sc.* year	1.90	0.92	1.51	0.63	2.69*	1.92	1.49	0.41
white teacher	0.68***	-2.58	0.68***	-2.58	0.68***	-2.54	0.68***	-2.55
black teacher	0.38***	-3.01	0.39***	-3.10	0.38***	-3.01	0.39***	-3.04
Sex	1.54***	3.71	1.55***	3.57	1.55***	3.62	1.55***	3.67
Entry age	1.00	0.07	1.00	0.17	1.00	0.10	1.00	0.12
Experience	1.08***	6.65	1.08***	6.59	1.08***	6.61	1.08***	6.62
Union agreement	1.54***	2.64	1.57***	2.79	1.57***	2.81	1.55***	2.72
Urban	1.07	0.52	1.07	0.52	1.06	0.45	1.06	0.50
Free lunch	1.01	0.04	0.93	-0.20	0.97	-0.08	0.96	-0.12
Minority students	1.58	1.23	1.57	1.16	1.60	1.24	1.60	1.21
Minority teachers	0.56**	-2.21	0.54***	-2.38	0.54***	-2.41	0.54***	-2.39
District enrollment	0.94**	-2.13	0.93**	-2.15	0.94**	-1.91	0.94*	-1.80
Constant	2.94	2.20	3.63	2.58	3.39	2.57	3.42	2.42
Number of obs.	11417		11417		11417		11417	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-13. Logit results for student-targeted accountability policy with controls for district size

Variable	English		Mathematics		Social Sciences		Sciences	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Student * year	0.90	-0.20	0.63	-0.83	1.99	1.50	1.28	0.76
Student * LPS	1.48	0.57	1.46	0.55	0.66	-0.47	0.75	-0.47
Student * bigD	1.28	0.26	2.23	1.21	1.45	0.95	1.69**	2.21
Student *bigD *LPS	0.87	-0.12	0.76	-0.36	0.72	-0.54	1.03	0.07
year	0.73	-0.93	0.87	-0.40	0.61**	-2.01	0.39***	-3.75
LPS * year	0.50	-1.52	0.35	-1.57	0.62	-0.63	1.58	0.97
white teacher	1.00	0.00	1.05	0.28	0.48**	-2.32	0.71*	-1.91
black teacher	0.47	-1.22	0.72	-0.99	0.21***	-3.68	0.44**	-2.11
Sex	1.40***	2.34	1.68***	3.08	1.90*	3.21	1.35	1.14
Entry age	1.00	-0.33	0.99	-1.35	1.00	0.36	1.00	0.02
Experience	1.06***	4.65	1.04***	3.32	1.13***	6.90	1.06***	3.01
Union agreement	0.84	-0.86	1.42	1.10	1.55*	1.92	1.55**	1.97
Urban	1.14	0.65	1.08	0.43	1.21	0.99	0.89	-0.47
Free lunch	0.63	-1.41	1.25	0.34	1.25	0.45	0.36***	-2.79
Minority students	0.54	-1.41	0.68	-0.82	1.43	0.64	1.46	0.89
Minority teachers	1.82	0.95	0.57	-1.31	0.42*	-1.92	0.74	-0.74
District enrollment	1.34	1.32	1.17	1.55	1.09	0.91	0.75**	-2.29
bigD * LPS	0.58***	-2.62	0.72***	-4.00	0.91	-0.74	0.91	0.48
Constant	14.30	4.02	12.81	5.29	2.23	1.54	11.59	3.33
Number of observations	8747		6547		5593		5794	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-14. Logit results for school-targeted accountability policy with controls for district size

Variable	English		Mathematics		Social Sciences		Sciences	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
School * year	0.55	-1.61	0.52	-1.10	0.67	-1.04	0.63	-1.19
School * LPS	1.49	0.73	0.44	-0.88	1.08	0.10	1.00	0.00
School * bigD	3.24	1.57	1.00	0.99	1.00**	2.19	1.00	-0.56
School *bigD *LPS	0.37	-1.19	1.00	-0.59	1.00***	-2.99	1.00***	3.35
year	0.80	-0.70	0.89	-0.33	0.93	-0.24	0.60**	-2.11
LPS * year	0.56	-1.26	0.87	-0.17	0.73	-0.44	1.26	0.49
white teacher	1.00	0.01	1.12	0.66	0.48***	-2.36	0.65***	-2.59
black teacher	0.48	-1.23	0.83	-0.55	0.22***	-3.47	0.42**	-2.27
Sex	1.41***	2.35	1.69*	3.00	1.86*	3.09	1.34	1.10
Entry age	1.00	-0.51	0.98	-1.44	1.01	0.47	1.00	0.05
Experience	1.06***	4.69	1.04***	3.32	1.12***	6.96	1.06***	2.94
Union agreement	0.86	-0.81	1.45	1.24	1.59*	1.91	1.53**	1.96
Urban	1.16	0.74	1.10	0.51	1.21	0.96	0.93	-0.30
Free lunch	0.61	-1.50	1.14	0.18	1.06	0.13	0.36***	-3.26
Minority students	0.56	-1.34	0.69	-0.77	1.43	0.63	1.54	0.96
Minority teachers	1.90	1.07	0.70	-0.73	0.39**	-2.22	0.81	-0.54
District enrollment	1.03	0.18	1.11	1.44	1.02	0.24	0.73**	-1.97
bigD * LPS	0.72	-1.47	0.71***	-4.59	1.01	0.13	1.01	0.34
Constant	17.10	3.84	15.54	4.97	2.87	2.01	15.02	3.40
Number of observations	8747		6547		5593		5794	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-15. Logit results for district-targeted accountability policy with controls for district size

Variable	English odds ratio	t-st.	Mathematics odds ratio	t-st.	Social Sciences odds ratio	t-st.	Sciences odds ratio	t-st.
District * year	0.20***	-3.81	6.55***	3.09	1.49	1.12	0.43	-1.62
District * LPS	9.39***	4.55	0.12*	-1.88	1.79	0.75	1.52	0.37
District * bigD	22.37***	3.56	0.01***	-4.97	2.76*	1.80	7.72	0.48
District *bigD *LPS	0.01***	-3.68	4.49***	3.44	0.01	-1.56	0.28	-0.21
year	0.83	-0.67	0.64	-1.14	0.79	-0.85	0.50***	-3.05
LPS * year	0.52	-1.39	0.52	-0.82	0.58	-0.92	1.40	0.83
white teacher	1.01	0.01	1.11	0.56	0.47***	-2.38	0.68**	-2.21
black teacher	0.47	-1.33	0.84	-0.49	0.20***	-3.84	0.42**	-2.22
Sex	1.42***	2.42	1.70***	3.04	1.92***	3.14	1.35	1.17
Entry age	1.00	-0.51	0.99	-1.35	1.00	0.32	1.00	0.01
Experience	1.06***	4.73	1.04***	3.35	1.13***	7.08	1.06***	2.98
Union agreement	0.84	-0.87	1.48	1.22	1.60**	1.98	1.55**	1.99
Urban	1.17	0.82	1.11	0.54	1.20	0.90	0.90	-0.40
Free lunch	0.62	-1.46	1.15	0.21	1.13	0.25	0.35***	-3.01
Minority students	0.53	-1.49	0.68	-0.82	1.34	0.52	1.54	0.95
Minority teachers	2.10	1.12	0.71	-0.72	0.43**	-2.00	0.79	-0.56
District enrollment	1.36	1.31	1.24	1.55	1.11	1.06	0.76**	-2.00
bigD * LPS	0.57**	-2.24	0.68***	-3.51	0.90	-0.83	0.90	-0.52
Constant	14.01	3.73	11.70	4.67	2.89	2.02	12.30	3.44
Number of obs.	8747		6547		5593		5794	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-16. Logit results for aggregated accountability policy index with controls for district size

Variable	English odds ratio	t-st.	Mathematics odds ratio	t-st.	Social Sciences odds ratio	t-st.	Sciences odds ratio	t-st.
Index * year	1.00	-0.04	0.89	-0.52	1.00	0.01	0.79	-1.61
Index * LPS	0.97	-0.12	0.89	-0.36	0.95	-0.20	1.22	1.18
Index * bigD	1.16*	1.73	1.01	0.04	1.06	0.87	1.20***	2.38
Index *bigD *LPS	0.78***	-2.60	0.95	-0.53	0.96	-0.62	0.79***	-1.99
year	0.61	-1.01	1.03	0.04	0.83	-0.55	0.78	-0.70
LPS * year	1.03	0.03	0.82	-0.17	0.66	-0.44	1.03	0.04
white teacher	1.01	0.01	1.11	0.55	0.46***	-2.40	0.69**	-2.11
black teacher	0.46	-1.29	0.80	-0.66	0.21***	-3.39	0.44*	-1.99
Sex	1.46***	2.48	1.70***	3.06	1.88***	3.18	1.36	1.16
Entry age	1.00	-0.29	0.99	-1.36	1.00	0.29	1.00	0.10
Experience	1.06***	4.73	1.04***	3.33	1.13***	6.94	1.06***	3.01
Union agreement	0.85	-0.80	1.43	1.15	1.58*	1.95	1.54*	1.96
Urban	1.15	0.71	1.09	0.44	1.23	1.02	0.96	-0.17
Free lunch	0.42***	-2.59	0.94	-0.09	1.08	0.18	0.27***	-3.22
Minority students	0.56	-1.32	0.70	-0.77	1.46	0.65	1.57	0.98
Minority teachers	1.82	0.93	0.66	-0.90	0.43*	-1.88	0.77	-0.62
District enrollment	0.84	-0.44	1.18	0.57	0.87	-0.40	0.41***	-2.47
bigD * LPS	1.36	0.71	0.92	-0.20	1.08	0.23	1.01	1.59
Constant	16.78	3.90	14.44	4.65	2.94	2.09	17.46	3.23
Number of obs.	8747		6547		5593		5794	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-17. Logit results for sample consisting of English language arts (base field) and mathematics teachers with controls for district size

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	0.88	-0.28	0.51***	-2.56	0.41**	-2.11	0.99	-0.10
Policy*Math	0.74	-0.62	1.07	0.17	10.18***	7.36	0.93	-0.58
Policy * LPS	1.43	0.62	1.26	0.49	4.10***	3.18	0.90	-0.39
Policy*LPS * Math	0.99	-0.01	0.45	-0.89	0.07***	-2.69	1.01	0.04
Policy*bigD	1.42	0.38	3.08*	1.67	8.00***	2.59	1.11	1.43
Policy *bigD * LPS	0.77	-0.25	0.37	-1.30	0.03**	-2.17	0.85**	-2.10
Policy * bigD * Math	1.50	0.56	0.69*	-1.75	0.01***	-12.10	0.94**	-2.07
Policy*bigD*LPS* Math	1.02	0.03	1.44	1.26	5.04***	5.10	1.07**	2.03
Math	0.53***	-2.38	0.52***	-2.46	0.53***	-2.39	0.52***	-2.42
year	0.70	-1.10	0.78	-0.88	0.76	-1.02	0.64	-1.04
Math * year	1.23	0.59	1.14	0.29	0.91	-0.22	1.43	0.65
LPS * year	0.57	-1.25	0.74	-0.66	0.61	-1.08	1.22	0.26
LPS * Math	2.05	1.23	2.20	1.35	2.08	1.26	2.12	1.28
LPS * Math * year	0.57	-0.71	0.88	-0.14	0.76	-0.35	0.59	-0.36
white teacher	1.08	0.30	1.12	0.43	1.09	0.36	1.07	0.30
black teacher	0.60	-1.54	0.64	-1.38	0.63	-1.49	0.60	-1.60
Sex	1.57***	3.71	1.58***	3.84	1.58***	3.88	1.60***	4.04
Entry age	0.99	-1.23	0.99	-1.38	0.99	-1.38	0.99	-1.21
Experience	1.05***	4.92	1.05***	4.94	1.05***	5.04	1.05***	5.00
Union agreement	1.06	0.51	1.08	0.71	1.08	0.68	1.08	0.66
Urban	1.09	0.63	1.12	0.70	1.12	0.78	1.11	0.68
Free lunch	0.62	-1.62	0.56**	-2.02	0.59*	-1.77	0.45***	-2.45
Minority students	0.61	-1.41	0.62	-1.35	0.60	-1.48	0.62	-1.37
Minority teachers	1.07	0.14	1.19	0.38	1.27	0.49	1.17	0.34
District enrollment	1.24	1.55	1.07	1.08	1.29	1.63	0.98	-0.07
bigD * LPS	0.66***	-4.65	0.72***	-3.97	0.63***	-3.59	1.09	0.27
Const.	17.46	7.58	20.91	7.70	16.61	7.47	20.49	7.16
Number of obs.	15379		15379		15379		15379	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-18. Logit results for sample consisting of English language arts (base field) and social sciences teachers with controls for district size

Variable	Student odds ratio	Student t-st.	School odds ratio	School t-st.	District odds ratio	District t-st.	Aggregated index odds ratio	Aggregated index t-st.
Policy * year	0.83	-0.40	0.53**	-2.10	0.34**	-2.08	1.03	0.25
Policy * SS	2.86*	1.84	1.42	0.70	2.01	1.04	0.95	-0.25
Policy * LPS	1.82	1.02	1.95	1.35	5.10***	3.07	0.94	-0.24
Policy * LPS * SS	0.28	-1.46	0.31	-1.47	2.25	1.01	1.02	0.05
Policy * bigD	1.71	0.57	3.43*	1.74	5.51	1.23	1.14	1.65
Policy * bigD * LPS	0.54	-0.59	0.28	-1.57	0.03	-1.29	0.82**	-2.11
Policy * bigD * SS	0.61	-0.78	0.50	-1.03	3.51	1.55	0.93	-1.61
Policy * bigD * LPS * SS	1.95	1.20	1.58	0.77	0.01**	-2.01	1.10**	2.03
SS	0.30***	-4.86	0.30***	-4.94	0.30***	-4.86	0.30***	-4.91
year	0.76	-0.79	0.84	-0.57	0.84	-0.58	0.59	-1.20
SS. * year	0.65	-1.16	0.87	-0.36	0.83	-0.68	1.20	0.33
LPS * year	0.48	-1.60	0.52	-1.35	0.52	-1.36	0.95	-0.07
LPS * SS	2.94***	2.60	2.94***	2.69	2.92***	2.62	2.92***	2.63
LPS * SS * year	2.08	0.98	2.51	1.49	1.48	0.67	1.15	0.13
white teacher	0.79	-0.52	0.79	-0.53	0.79	-0.57	0.80	-0.50
black teacher	0.37*	-1.95	0.37**	-1.97	0.36**	-2.18	0.37*	-1.91
Sex	1.57***	3.50	1.57***	3.40	1.58***	3.49	1.58***	3.72
Entry age	1.00	-0.18	1.00	0.01	1.00	-0.21	1.00	-0.08
Experience	1.08***	7.81	1.08***	7.85	1.08***	7.74	1.08***	7.78
Union agreement	1.06	0.34	1.08	0.48	1.08	0.45	1.08	0.47
Urban	1.14	0.89	1.15	0.92	1.15	0.94	1.16	0.97
Free lunch	0.49	-2.41	0.45	-2.85	0.49	-2.44	0.38	-2.86
Minority students	0.76	-1.25	0.79	-1.10	0.74	-1.37	0.78	-1.08
Minority teachers	1.12	0.29	1.05	0.13	1.17	0.42	1.11	0.23
District enrollment	1.22	1.88	1.07	0.69	1.24	1.88	0.88	-0.42
bigD * LPS	0.73	-3.36	0.83	-1.47	0.72	-2.98	1.24	0.67
Constant	12.43	4.77	14.59	4.90	13.07	4.95	13.20	4.55
Number of obs.	14340		14340		14340		14340	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively.

Table B-19. Logit results for sample consisting of English language arts (base field) and sciences teachers with controls for district size

Variable	Student odds ratio	Student t-st.	School odds ratio	School t-st.	District odds ratio	District t-st.	Aggregated index odds ratio	Aggregated index t-st.
Policy * year	0.90	-0.23	0.59	-1.57	0.20***	-4.64	0.96	-0.28
Policy * Sc.	1.31	0.72	1.02	0.06	1.95	1.06	0.85	-1.51
Policy * LPS	1.57	0.71	1.42	0.70	6.69***	3.74	0.96	-0.18
Policy * LPS * Sc.	0.48	-1.20	0.62	-0.75	0.55	-0.47	1.27	1.00
Policy * bigD	1.59	0.52	3.24**	2.14	11.28***	2.53	1.23***	3.63
Policy * bigD * LPS	0.70	-0.33	0.36	-1.57	0.01***	-2.36	0.75***	-5.48
Policy * bigD * Sc.	0.93	-0.10	0.25***	-2.89	5.64	0.38	0.90*	-1.68
Policy * bigD * LPS * Sc.	1.70	0.51	5.71***	3.32	0.70	-0.05	1.00*	1.72
Sc.	0.76	-1.32	0.76	-1.37	0.76	-1.27	0.75	-1.38
year	0.76	-0.89	0.81	-0.69	0.90	-0.42	0.66	-0.99
Sc. * year	0.51**	-2.02	0.72	-0.90	0.51**	-2.22	1.05	0.12
LPS * year	0.44*	-1.86	0.53	-1.40	0.47*	-1.66	1.02	0.03
LPS * Sc.	0.95	-0.10	0.96	-0.08	0.94	-0.13	1.03	0.06
LPS * Sc. * year	3.90*	1.90	2.80	1.46	3.32*	1.98	1.07	0.07
white teacher	0.88	-0.39	0.84	-0.52	0.85	-0.49	0.86	-0.45
black teacher	0.48**	-1.98	0.45**	-2.17	0.46**	-2.22	0.45**	-2.14
Sex	1.38*	1.86	1.38*	1.83	1.38*	1.91	1.39*	1.90
Entry age	1.00	-0.37	1.00	-0.49	1.00	-0.55	1.00	-0.34
Experience	1.06***	4.43	1.06***	4.35	1.06***	4.46	1.06***	4.45
Union agreement	1.09	0.57	1.09	0.58	1.09	0.60	1.09	0.59
Urban	1.02	0.13	1.06	0.33	1.05	0.30	1.06	0.35
Free lunch	0.52**	-2.14	0.50***	-2.34	0.51**	-2.18	0.35***	-3.26
Minority students	0.82	-0.74	0.85	-0.55	0.82	-0.70	0.85	-0.55
Minority teachers	1.25	0.46	1.36	0.70	1.40	0.66	1.30	0.53
District enrollment	0.98	-0.41	0.89	-1.41	0.89	-0.01	0.99***	-4.16
bigD * LPS	0.99	-1.08	0.99	-0.92	0.99	-1.01	1.01***	3.86
Constant	16.61	5.34	20.49	4.98	15.80	5.19	17.12	4.65
Number of obs.	14571		14571		14571		14571	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table B-20. Logit results for sample consisting of mathematics (base field) and sciences teachers with controls for district size

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	0.66	-1.00	0.61	-1.11	4.48***	2.70	0.87	-0.85
Policy * Sc.	1.80	1.63	0.91	-0.23	0.21*	-1.92	0.93	-0.52
Policy * LPS	1.48	0.60	0.53	-0.75	0.14**	-2.02	0.94	-0.19
Policy * LPS * Sc.	0.49	-1.07	1.54	0.48	8.58*	1.73	1.25	0.85
Policy * bigD	2.31	1.49	1.77	1.56	0.01***	-4.95	1.13*	1.75
Policy * bigD * LPS	0.73	-0.45	0.65	-0.88	4.80***	4.03	0.83**	-2.07
Policy * bigD * Sc.	0.66	-0.92	0.43***	-2.35	2.91***	2.52	0.97	-0.74
Policy * bigD * LPS * Sc.	1.59	0.61	3.28***	2.54	0.01**	-2.15	1.05	0.91
Sc.	1.35	1.01	1.32	0.96	1.32	0.94	1.35	0.99
year	0.91	-0.28	0.91	-0.26	0.73	-0.84	1.05	0.09
Sc. * year	0.42***	-2.49	0.67	-0.90	0.59	-1.16	0.73	-0.60
LPS * year	0.30*	-1.74	0.64	-0.51	0.44	-1.04	0.68	-0.33
LPS * Sc.	0.49	-1.04	0.49	-1.04	0.50	-1.02	0.51	-0.97
LPS * Sc. * year	6.55***	2.55	2.75	0.98	4.01	1.54	1.79	0.49
white teacher	0.95	-0.37	0.92	-0.55	0.94	-0.39	0.92	-0.53
black teacher	0.62*	-1.73	0.64*	-1.66	0.66	-1.48	0.63*	-1.72
Sex	1.52***	2.41	1.52***	2.32	1.54***	2.41	1.54***	2.44
Entry age	0.99	-0.89	0.99	-0.96	0.99	-0.88	0.99	-0.83
Experience	1.05***	4.12	1.05***	3.98	1.05***	4.08	1.05***	4.09
Union agreement	1.49***	2.34	1.52***	2.58	1.55***	2.60	1.52***	2.53
Urban	0.99	-0.09	1.01	0.08	1.01	0.05	1.03	0.21
Free lunch	0.97	-0.04	0.91	-0.15	0.90	-0.19	0.68	-0.63
Minority students	1.03	0.09	1.04	0.13	1.05	0.17	1.06	0.17
Minority teachers	0.63*	-1.76	0.76	-0.96	0.73	-1.03	0.73	-1.11
District enrollment	0.97	-0.69	0.95	-0.66	0.99	-0.01	0.64**	-2.17
bigD * LPS	0.86	-1.35	0.82*	-1.86	0.84	-1.33	1.68	1.53
Constant	9.78	5.44	12.55	5.31	9.68	4.93	12.94	5.06
Number of obs.	12341		12341		12341		12341	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table B-21. Logit results for sample consisting of mathematics (base field) and social sciences teachers with controls for district size

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	0.58	-1.03	0.59	-0.98	6.42***	2.69	0.95	-0.26
Policy * SS	3.90***	2.42	1.22	0.34	0.20*	-1.88	1.02	0.09
Policy * LPS	1.70	0.68	0.66	-0.44	0.09**	-2.06	0.88	-0.43
Policy * LPS * SS	0.29	-1.17	0.80	-0.20	42.52***	2.76	1.04	0.13
Policy * bigD	2.50	1.22	1.68	0.84	0.01***	-5.95	1.05	0.83
Policy * bigD * LPS	0.62	-0.56	0.64	-0.65	7.05***	3.90	0.92	-1.15
Policy * bigD * SS	0.44	-1.27	0.99	-0.02	8.81***	2.47	0.99	-0.43
Policy * bigD * LPS * SS	1.71	0.72	0.83	-0.31	0.01***	-3.27	1.03	1.33
SS	0.54**	-2.17	0.54**	-2.20	0.53**	-2.17	0.54**	-2.19
year	0.92	-0.19	0.90	-0.26	0.69	-0.91	0.85	-0.30
SS * year	0.57	-1.19	0.88	-0.25	1.01	0.01	0.94	-0.09
LPS * year	0.32*	-1.71	0.63	-0.54	0.48	-0.95	0.79	-0.21
LPS * SS	1.60	0.98	1.54	0.94	1.52	0.89	1.51	0.89
LPS * SS * year	2.94	1.27	2.16	0.78	1.63	0.59	1.35	0.26
white teacher	0.90	-0.55	0.90	-0.51	0.90	-0.54	0.88	-0.72
black teacher	0.49***	-2.37	0.52**	-2.06	0.52**	-2.09	0.50**	-2.27
Sex	1.70***	3.44	1.68***	3.31	1.70***	3.34	1.70***	3.52
Entry age	0.99	-0.63	0.99	-0.50	0.99	-0.60	0.99	-0.63
Experience	1.07***	6.34	1.07***	6.48	1.07***	6.47	1.07***	6.34
Union agreement	1.43*	1.76	1.49**	2.00	1.51**	1.97	1.49*	1.94
Urban	1.11	0.82	1.11	0.83	1.11	0.85	1.11	0.83
Free lunch	0.90	-0.19	0.81	-0.34	0.84	-0.30	0.71	-0.56
Minority students	1.04	0.11	1.05	0.15	1.02	0.06	1.06	0.17
Minority teachers	0.47***	-2.42	0.50**	-2.03	0.53*	-1.95	0.51**	-2.04
District enrollment	1.16***	2.73	1.11**	2.28	1.19***	2.51	0.96	-0.21
bigD * LPS	0.77***	-5.30	0.78***	-3.96	0.75***	-5.12	1.10	0.33
Constant	6.69	3.88	7.92	4.04	7.17	3.87	8.08	4.21
Number of obs.	12140		12140		12140		12140	
State fixes effects	yes		yes		yes		yes	
Weights	yes		yes		yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

Table B-22. Logit results for sample consisting of social sciences (base field) and sciences teachers with controls for district size

Variable	Student		School		District		Aggregated index	
	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.	odds ratio	t-st.
Policy * year	2.42*	1.99	0.88	-0.31	0.97	-0.08	0.95	-0.34
Policy * Sc.	0.42	-1.42	0.67	-0.72	1.06	0.09	0.90	-0.52
Policy * LPS	0.45	-0.92	0.59	-0.7	3.09	1.47	0.95	-0.19
Policy * LPS * Sc.	2.10	0.84	1.99	0.79	0.27	-1.18	1.24	0.74
Policy * bigD	1.18	0.48	1.78***	4.14	2.95	1.42	1.14***	3.51
Policy * bigD * LPS	0.96	-0.07	0.47***	-4.43	0.01*	-1.92	0.87**	-2.02
Policy * bigD * Sc.	1.35	0.67	0.42***	-4.37	0.01*	-1.69	0.97	-1.05
Policy * bigD * LPS * Sc.	1.15	0.13	3.89***	6.51	4.54***	2.47	1.03	0.64
Sc. Dummy	2.45***	4.4	2.43***	4.43	2.45***	4.48	2.46***	4.29
Year dummy	0.54***	-2.61	0.79	-0.87	0.78	-1.08	0.81	-0.58
Sc. * year	0.79	-0.61	0.78	-0.65	0.59*	-1.77	0.79	-0.41
LPS * year	0.88	-0.19	1.07	0.13	0.67	-0.79	0.90	-0.13
LPS * Sc.	0.32***	-2.49	0.34***	-2.37	0.33***	-2.39	0.34***	-2.34
LPS * Sc. * year	1.95	0.95	1.48	0.62	2.69*	1.92	1.38	0.34
white teacher	0.70***	-2.35	0.66***	-2.75	0.68***	-2.54	0.66***	-2.67
black teacher	0.39***	-2.95	0.37***	-3.24	0.38***	-3.06	0.38***	-3.11
Sex	1.56***	3.69	1.53***	3.44	1.56***	3.56	1.54***	3.55
Entry age	1.00	0.13	1.00	0.24	1.00	0.13	1.00	0.14
Experience	1.09***	6.67	1.09***	6.64	1.09***	6.66	1.09***	6.67
Union agreement	1.54***	2.64	1.56***	2.79	1.57***	2.84	1.57***	2.86
Urban	1.06	0.43	1.06	0.47	1.06	0.44	1.10	0.75
Free lunch	1.04	0.12	0.92	-0.24	0.96	-0.12	0.82	-0.59
Minority students	1.56	1.22	1.59	1.21	1.56	1.19	1.60	1.18
Minority teachers	0.52***	-2.67	0.55***	-2.51	0.55***	-2.35	0.57**	-2.04
District enrollment	0.91	-1.07	0.90	-1.00	0.91	-1.02	0.58***	-3.58
bigD * LPS	1.04	0.28	1.03	0.24	1.05	0.38	1.72**	2.07
Constant	2.95	2.15	3.75	2.59	3.41	2.53	4.08	2.81
Number of obs.	11417		11417		11417		11417	
State fixes effects	yes		yes		Yes		yes	
Weights	yes		yes		Yes		yes	

*, **, *** - statistically significant at 10, 5, 1 percent level respectively

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