The Labor Market Consequences of Principal Performance Pay *

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Abstract

This paper studies whether test score-based performance pay for public-school principals affects principals' labor market decisions. I leverage the introduction of a principal performance pay system in North Carolina in July 2017. The new compensation system removed experience and education as determinants of principal salaries; now principal pay is determined by school test-score growth. I provide evidence that performance pay's financial incentives induce the sorting of principals within and exit from the principalship and the traditional public school labor force. One year after the introduction of performance pay, principals financially rewarded by the new compensation system were more likely to continue working as principals at a different school. Principals not financially rewarded, whose wages stagnate under the new performance pay system, were more likely to leave the principalship and exit or move into other positions. These labor market responses created compositional and distributional changes in the principal labor force. Performance pay induced exits from experienced principals but also principals managing persistently low-performing schools. At the same time it attracted different principals to persistently low-performing schools. My results demonstrate that principal performance pay can push potentially ineffective principals out of their positions and attract principals to harder-to-staff, low-performing schools.

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1 Introduction

There have been numerous efforts to alter educator compensation structures to attract and retain higher quality educators and induce effort by tying pay to measurable student achievement growth. For instance, the United States Department of Education's Teacher Incentive Fund provided grants to school districts to implement performance-based teacher and principal compensation systems. Standard theory in personnel economics predicts two main consequences of performance pay; employees will respond by increasing their level of effort, and more productive employees will sort into positions offering performance pay (see, e.g. Lazear, 2000). Both predictions follow from employees maximizing expected future earnings. In the context of teachers, there is some evidence to support these predictions. For example, a move to flexible pay systems by some school districts in Wisconsin increased average levels of teacher effort relative to seniority pay by attracting high value-added teachers and increasing the effort teachers exert (Biasi, 2021).

Performance-based compensation for principals is not as widespread and, therefore, not as well studied. In this paper, I study a principal performance pay system implemented in North Carolina in July 2017. This new compensation structure changed principal pay from a system based on experience and education to one where school test-score growth primarily determines principals' salaries. I examine whether North Carolina's principal performance pay system affects principals' labor market decisions and, as a result, the composition of the principal labor force and the distribution of principals across schools in North Carolina. Specifically, I ask: (1) How did performance pay's financial incentives affect principals' decisions to continue working at the same school, switch schools, or exit the principalship?; (2) If performance pay induces exits, are these from effective or ineffective principals?; (3) Does performance pay attract principals to traditionally harder-to-staff schools?; and (4) Are changes to principals' labor market decisions persistent or transient?

Understanding how changing principals' financial incentives will impact the principal labor market is crucial because principals play an important role in students' human capital development. Principals' roles as middle managers in the public school system make them responsible for schools' day-to-day operations. They play a direct role in hiring, firing, and evaluating teachers, assigning students to classrooms, assigning classrooms to teachers, determining teacher schedules, allocating school resources, and disciplining students. Prior research has established that public school principals are critical to student success, even though they do not typically directly instruct students (Horng et al., 2010; Branch et al., 2012; Coelli and Green, 2012; Corcoran et al., 2012; Grissom et al., 2015; Dhuey and Smith, 2018; Bacher-Hicks et al., 2019).

The impact of principal performance pay on students is ambiguous. Whether performance pay is beneficial or detrimental depends on how principals' labor market decisions change and how turnover-induced disruptions affect students. If, as economic theory predicts, performance pay retains and attracts high-quality principals while pushing out less effective principals, performance pay could benefit students if the positive impact of increased principal effectiveness outweighs the disruption of principal turnover. Prior work documents a decrease in school achievement following a principal's turnover on average; however, the types of labor market transitions made by principals matter (Bartanen et al., 2019). Bartanen et al. (2019) find that test performance decreases following a principal transfer but increases after a principal demotion. A potential explanation is that principal effectiveness and turnover types are correlated; low-performing principals are more likely to be demoted, while higher-performing principals are more likely to be promoted (Grissom and Bartanen, 2019). Performance pay incentives could change the relationship between principal effectiveness and labor market decisions, thus, altering turnover's impacts on students.

I use a difference-in-difference framework to assess the effects of North Carolina's principal performance pay policy on principals' labor market decisions, the composition of the NC principal labor force, and the distribution of principals across schools. I compare the trajectories of principals with those of a sample of experienced career teachers who are similar to principals but did not face significant changes to their compensation and work rules during the analysis period. My estimates are robust to different comparison groups and estimators.

My results are consistent with the predictions of standard personnel economic models of performance pay. I find evidence that principals respond to performance pay's financial incentives. One year after legislators implemented performance pay, principals with attributes rewarded by performance pay, who would see their salaries increase, are more likely to continue working as a principal—but at a different school. These school switches are driven by principals moving into persistently low-performing schools, suggesting principals are potentially moving to maximize future test-score growth in response to performance pay's incentives. However, these impacts do not persist. Two years after the introduction of performance pay, principals are no longer statistically significantly more likely to switch schools. On the contrary, principals with attributes rewarded by performance pay are less likely to stay in their current position and more likely to make other changes within the public school system (i.e., change job type) two years after performance pay's introduction.

On the other hand, principals who would face stagnant wages appear to leave the principalship and are more likely to move into different jobs in traditional public education or exit the public education labor force one year after performance pay's implementation. More experienced principals and principals from persistently low-performing schools primarily drive the exits from the principalship of these principals. Again, the impacts do not persist: I find no evidence of changes in labor market decisions for these principals in $t + 2$.

My research contributes to a growing body of knowledge regarding principal performance pay. While prior research provides correlational evidence about the impacts principal performance pay on principal labor markets (see e.g., Hamilton et al., 2012). I provide plausibly causal evidence regarding the impacts of principal performance pay on principal labor markets. In addition, to my knowledge, this is the first evaluation of North Carolina's principal performance pay system.

My paper proceeds as follows. First, I provide background on educator performance pay and the compensation of principals in North Carolina in Section 2. Next, I describe the data in Section 3, and the empirical methods used to analyze principal performance pay in Section 4. Then, I present my results regarding the impacts of principal performance pay on principals' labor market transitions, the composition of the principal labor force, and the distribution of principals to schools in Section 5. Section 6 provides additional robustness checks. Finally, Section 7 concludes with a discussion of the results and their policy implications.

2 Background

2.1 Performance Pay

In the US, educator salaries are traditionally determined using centrally planned salary schedules based on experience and education; teachers with the same experience and education level will generally earn the same salary regardless of their quality or effort (Podgursky, 2006). In recent years, with shortages of teachers and increasing public perceptions of inadequate teacher pay (Berge, 2019), many policymakers are advocating for policies that increase educator salaries while also tying compensation to measurable student achievement growth. By 2007, 25 states had implemented test score-based performance pay systems (Bond and Mumford, 2018).[1] Furthermore, the federal government has granted nearly $2.5 billion to fund performance pay systems to recipients in more than 30 states since 2006 (Pham et al., 2021).

While performance pay for school principals is less common than for teachers, there is a small literature examining the impacts of principal performance pay.Chiang et al. (2015) compared schools receiving Teacher Incentive Fund grants for performance pay arrangements to a set of control schools where principals received an unconditional bonus. They found that only 30 percent of schools provided performance bonuses that were difficult to earn and large enough to change principal behavior. Hamilton et al. (2012) studied performance-based bonuses in Pittsburgh Public Schools and found that, while retention remained relatively stable, principals who received a larger bonus were more likely to become central office administrators. In contrast, those who earned smaller bonuses were somewhat more like to become assistant principals. While this evidence is informative, endogeneity concerns make placing a causal interpretation of these results difficult. For example, principals who have characteristics rewarded by performance pay might also be more likely to move into higher-level positions even in the absence of performance pay. I contribute to this literature by providing plausibly causal evidence of the impact of principal performance pay on principal labor markets.

[1]Bond and Mumford (2018) find there were seven state-wide programs, 138 district-wide programs, and 2,925 school-specific programs implemented in at least one academic year between 1986 and 2007.

A related strand of literature studies principal labor markets more broadly. For instance, recent research has demonstrated a negative correlation between principal effectiveness and principal turnover (Grissom and Bartanen, 2019). Furthermore, others have described inequities in the distribution of principal quality (Clotfelter et al., 2007; Loeb et al., 2010; Grissom et al., 2019). For instance, Grissom et al. (2019) find that lower-quality schools tend to be led by less effective principals. Other related research examining school-principal pay has focused on measuring the impacts of compensation changes on student outcomes or understanding whether principal performance determines their compensation (Billger, 2007; Lavy, 2008). For instance, Billger (2007) found that principals pay may not increase under accountability policies. On the other hand, Lavy (2008) found that a 50% increase in principal salaries was associated with a modest improvement in academic achievement. However, these studies do not examine principal performance pay systems.

I additionally contribute to a robust literature examines the causal labor market effects of teacher salaries, including the impacts of performance pay (e.g. Adnot et al., 2017; Berlinski and Ramos, 2020; Burgess et al., 2022; Biasi, 2021; Brehm et al., 2017; Clotfelter et al., 2008; Dee and Wyckoff, 2015; Duflo et al., 2012; Elacqua et al., 2022; Goldhaber et al., 2011; Hanushek et al., 2004, 2005; Henry et al., 2011; Imberman and Lovenheim, 2014). Biasi (2021) exploited the introduction of performance pay in some school districts in Wisconsin in 2011 and found that effective teachers, in terms of value-added, were more likely to move to school districts offering performance pay than their lower-performing peers. Furthermore, switching to performance pay increased teacher effort, as measured by their value-added. Similarly, Dee and Wyckoff (2015) found that an incentive pay system in the District of Columbia Public Schools increased voluntary exits from low-performing teachers just below a threshold implying dismissal threats and increased performance of those just above. They also found that financial incentives improved the performance of high-performing teachers. Studying the same policy, Adnot et al. (2017) found that performance incentives induced exits by lower-performing teachers and had a net positive impact on student achievement. Other recent work by Berlinski and Ramos (2020) and Elacqua et al. (2022) found that merit pay in Chile induced the labor market sorting of teachers. A related strand of literature has demonstrated that teachers' labor market decisions change in response to high-stakes accountability and evaluation systems, even when there are no changes in financial incentives (e.g., Cullen et al., 2021; Brunner et al., 2019; Kraft et al., 2020; Loeb et al., 2015; Sartain and Steinberg, 2016). This paper contributes to this literature by examining principals, who play a distinctly different role in education production than teachers; therefore, my research provides new insight into the efficacy of performance pay for educators other than teachers.

2.2 Principal Pay in North Carolina

North Carolina has a largely centralized salary system for public-school educators. The state legislature sets salary schedules for public school employees annually. These state salary schedules provide a minimum compensation level for North Carolina educators, but local school districts can give additional salary supplements. Before the introduction of principal performance pay in the 2017-18 school year, years of experience in North Carolina public schools, education level, and the number of teachers in their school determined the state-mandated salaries of North Carolina principals. Principals could expect an increase in pay of roughly $1,500 for each additional year of experience, around $3,000 more per year after completing a doctorate, and roughly $1,000 more for each additional ten teachers in their school. In 2016-17, North Carolina principal salaries ranked the second-lowest in the United States, averaging $70,220 compared to the national average of $97,440 (Bureau of Labor Statistics, 2017). Earnings were particularly low for inexperienced principals in small schools, where the state-mandated minimum principal salary was as low as $52,656.

In response, North Carolina policymakers sought to increase principal pay while incentivizing effort, recruiting and retaining high-performing principals, and rewarding principals for high performance by implementing a test score-based performance pay system (General Assembly of North Carolina, 2016). The new performance-based pay system, introduced in 2017-18, eliminated experience and education as determinants of principal salaries. Now, the test-score growth status of the school(s) principals' managed over the last three years and their school's total student enrollment determine their salaries.[2] Since 2014-15, the North Carolina Department of Public Instruction has measured test-score growth status using a value-added model which classifies schools as having "not met," "met," or "exceeded" test-score growth expectations. All else equal, moving from the lowest test-score growth expectation class to the highest results in around a $15,000 salary increase in a given year. Increasing total enrollment by 300 students also increases principal salaries by approximately $3,000 per year.

This policy change caused an increase in principal salaries, with average pay increasing to $71,740 in 2017-18, a 2.2% increase relative to 2016-17 (Bureau of Labor Statistics, 2018). However, not all principals' salaries increased. The implementation of principal performance pay caused a compression of the principal salary distribution, with the lowest possible salary rising from $52,656 to $61,751 and the highest possible salary decreasing from $111,984 to $88,921. However, reducing principal salaries was an intractable policy proposition. In response, the state legislature established a "hold harmless" guarantee that a principal's salary would not fall below 2016-17 levels under the new pay structure.[3] Nevertheless, principals affected by the hold-harmless rule, whose

[2] New principals and principals who have not supervised a school for two of the last three years are placed in the lowest test-score growth category until they have three years of test score growth history. Principals managing schools that do not receive a school growth status are placed in the middle growth category.

[3] This guarantee did not include supplements from local school districts, discussed in more detail below.

salaries would stagnate in nominal terms, experienced real salary decreases and, importantly, no longer receive a yearly pay raise for each additional year of experience. While the hold harmless rule was set to expire in 2018-19, meaning principals' salaries could decrease in the second year of performance pay's implementation, the legislature extended hold harmless through the 2018-19 school year. Throughout my analyses, principals could never earn less than their 2016-17 salary.

Principals with low levels of experience and education managing high-performing schools were the most likely to see their salaries increase under the performance pay system. For instance, consider a principal with 15 years of experience in public education and a master's degree managing a school with 40 teachers and 600 students that exceeded growth expectations over the last three years. Table 1 shows an excerpt from the state's 2016-17 principal salary schedule for schools with 33-43 teachers, where the rows represent the total years of experience for a principal and the columns the principal's education level. The salary schedule indicates that the example principal would have earned $58,320 in 2016-17. Table 2 shows the complete state principal salary schedule in 2017-18, where the rows represent school size and the columns represent school test-score growth status. According to the new salary schedule, the example principal would earn $77,806 in 2017-18, representing a 33% salary increase. The potential pay raises for inexperienced principals were so extreme that any principal with 22 years of experience or less and a master's degree (in a school with 33-43 teachers) would receive a salary increase regardless of their school's test-score growth status. Their salary under the experience-based system is lower than the lowest possible salary under performance pay.

In contrast, a principal with 40 years of experience and a doctorate managing an identical school would receive a 9% salary *decrease* (from $85,704 to $77,806). However, any principals facing a salary decrease are subject to hold-harmless and would see their salary stagnate instead of decline.

In addition to introducing test-score growth as a determinant of principal salaries, the new performance pay system changed other aspects of principal compensation in North Carolina. First, the new pay system changed how school size is measured for salary determination. In the 2016-17 school year and before, under the experience-based pay system, the number of teachers employed in the principal's school determined school size. This system was problematic because it only counted state-funded teachers. Federally funded teachers, most commonly found in Title I schools with many economically disadvantaged students, did not count toward this total. This discrepancy meant that principals of Title I schools with more federally-funded teachers were earning less than their counterparts in non-Title I schools without federally-funded teachers managing the *same number* of teachers overall, all else equal. After the introduction of performance-based pay in 2017-18, the salary schedule used the highest average daily membership (a measure of student enrollment) over the first two months of the school year to measure school size.

Second, performance pay eliminated longevity payments for principals. Longevity pay in North

Carolina is an annual lump sum paid to state government employees with more than ten years of eligible service. Longevity payments range from 1.5 to 4.5% of a principal's total salary, so eliminating longevity pay for principals represented a potentially large source of pay cuts. While longevity payments are protected by hold-harmless for principals who received them in 2016-17, principals on the margin of longevity, who would have received payments starting in 2017-18, are not protected.

In North Carolina, state and local school districts provide funding for principal salaries, adding additional complexity to the policy change. The state legislature sets a principal salary schedule that acts as a minimum salary, while school districts can supplement the state-paid salary. There is significant variation in the size of the supplement provided by school districts ranging from several districts offering no supplement at all to Wake County Public Schools offering an average supplement of $27,701 in the 2016-17 school year. The average local supplement received by school principals in the 2016-17 school year was $13,616. School districts also vary in how they provide supplements. Some districts, such as Orange County Public Schools and Wake County Public Schools, provision supplements based on fixed rules regarding school grade level or years of experience.[4] Other districts, such as Charlotte-Mecklenburg Schools, offer flexible systems that can be individualized for each principal based on comparable private-sector salaries.[5] I discuss how local salary supplements could impact my analyses in Section 4.

3 Data

I use administrative records from the North Carolina Education Research Data Center (NCERDC) regarding all public school personnel in North Carolina from the 2011-12 through 2018-19 school years. These data include an individual's position, salary, experience, demographics, education, district, and school. I use these data to construct repeated cross-sections of public school principals and teachers. I examine seven cohorts of principals and teachers who worked in a regular school during the academic years 2011-12 to 2018-19.[6]

To identify which principals are most affected by performance pay, how their salary changes, and an appropriate comparison samples of teachers, I restrict my attention to educators who worked in just one type of position during a fiscal year (e.g., principals who only worked as principals and not in any other position).[7] However, in 2012-13 and 2013-14, the NCDPI stored

[4]See `https://www.orangecountyfirst.com/Page/94` [Accessed October 2020] and `https://www.wcpss.net/cms/lib/NC01911451/Centricity/Domain/35/Principal%20and%20Assistant%20Principal.pdf` [Accessed October 2020] for information about salary supplements in Orange County and Wake County, respectively.

[5]See `https://www.cms.k12.nc.us/cmsdepartments/humanresources/Documents/2019-20%20Market%20Pay%20Plan%2012%20month%20Principal%20and%20AP.pdf` for information about Charlotte-Mecklenburg Schools' principal salary supplement.

[6]I refer to regular schools as defined in the NCES Common Core Data. I omit individuals working in alternative education schools, career and technical education schools, and special education schools.

[7]While principals who worked in other positions are still affected by performance pay, the intensity of their

their pay records in a different format than in other years (2011-12 and 2014-15 to 2018-19).[8] This means I am unable to verify that educators worked in a single type of position in 2012-13 and 2013-14. For this reason, I utilize data from 2014-15 to 2018-19 in my preferred model specifications. However, the longer panel can still provide useful information about trends in educators' labor market transitions before the principal performance pay policy. Although the impacts of performance pay will be attenuated by including principals and teachers receiving salaries from multiple positions, I use the extended data from 2011-12 to 2018-19 to glean some insight into the validity of the parallel trends assumption.

I begin by identifying a sample of teachers similar to principals: full-time career teachers with ten or more years of experience. Principals in North Carolina are hired on three-year contracts. North Carolina abolished tenure for teachers in 2013, and new teachers are employed on one-year contracts. However, administrators can offer career teachers multi-year contracts. North Carolina defines a career teacher as a teacher who has worked for five or more years at the same school. Moreover, principals tend to have more public education experience than teachers. For this reason, I restrict my analysis sample to educators with ten or more years of experience in North Carolina public education.

Additionally, I restrict my sample to educators earning more than $30,000 per year to exclude educators who are not employed full time. Table 3 compares the observable characteristics of the analysis sample of principals and experienced teachers. I report these summary statistics for 2015-16 to 2018-19. There are 6,222 principals and 58,430 teachers. On average, principals have 22 years of experience, three more years than teachers. Principals are less likely to have a master's degree and more likely to have a doctorate than teachers. Principals are also nearly twice as likely as teachers to be Black and less likely to be White than teachers. Teachers and principals have similar ages, averaging just over 47 years old.

I then track these individuals' labor market transitions into the following academic year (year t). I define four possible labor market transitions: (1) *same-school retention*, (2) *switching schools*, (3) *other employment changes*, and (4) *separations*. I define *same-school retention* as continuing to work in the same position at the same school in the next academic year. I define *switching schools* as continuing to work in the same position but at a different school than in the prior year. An individual experiences an *other employment change* in year t if they moved to a different job. This transition can include changing to a different position within the North Carolina public school system, regardless of full-time status in t. A *separation* occurs when an individual leaves the traditional North Carolina public educator labor force and exits the administrative data.

I supplement the NCERDC personnel data with data from North Carolina school report cards, the Common Core Data from the Department of Education National Center for Education Statis-

treatment is likely lower, especially if their salary for their other position was not affected by the policy change.

[8]NCDPI kept pay records at the individual-by-school level in 2012-13 and 2013-14 rather than the individual-by-position-by-pay period level.

tics, and salary schedules from the North Carolina Department of Public Instruction (NCDPI) to impute changes in the state-provided salaries of principals impacted by performance pay (those in 2016-17 or 2017-18).

The following section describes my empirical strategy for combining these data sources to uncover the causal effects of principal performance pay on principals' labor market decisions.

4 Empirical Methods

4.1 Difference-in-Differences

The key empirical challenge to identifying the causal effect of principal performance pay on principals' labor market transitions is that all North Carolina principals were impacted by performance pay simultaneously. Before and after comparisons of principals' labor market decisions will not only capture the effects of performance pay but also any secular trends in labor market transitions and the impacts of other common shocks to the principal labor force. To identify the effect of the salary schedule change, I use experienced career teachers to approximate principals' counterfactual labor market outcomes. Teachers offer a natural comparison group for principals. Although not a requirement for being a principal in North Carolina, roughly 80 percent of first-time principals from 2006 to 2010 were previously teachers (Bastian and Henry, 2015). Therefore, it is likely that experienced teachers will be similar to principals in terms of both observable and unobservable characteristics and will make similar labor market transition decisions, all else equal. Of course, no comparison group is perfect, and I address potential issues with using teachers as a comparison group below.

I use a difference-in-differences approach to compare the labor market transitions of principals and career teachers before and after principal performance pay was introduced. Using a multinomial logit model, I implement a difference-in-differences model to estimate principal performance pay's impact on four labor market transitions. Specifically, I estimate the following model specification:

$$\begin{aligned} Pr(\text{Transition}_{itds} = T) = & f(\alpha_0 + \alpha_1 \text{Principal}_{it-1ds} + \delta^T \text{Principal}_{it-1ds} \times \text{Perf Pay}_t \\ & + X_{it-1}\beta + S_{t-1s}\psi + \tau_t + D_d + \epsilon_{itds}) \end{aligned} \tag{1}$$

Where i indexes individual employees, t indexes the year, d indexes school districts, and s indexes schools. With transitions $T \in \{$Same-School Retention, Switching Schools, Other Changes, Separated$\}$. I formally define these labor market transitions in Section 3. $f()$ is the multinomial logit probability density function. $\text{Principal}_{it-1ds}$ is an indicator of being a principal in the prior year. Perf Pay_t indicates the implementation of principal performance pay in year t. I include individual-specific characteristics, X_{it-1}, and school-specific characteristics, S_{t-1s}, to control for educator and school characteristics that differ between principals and teachers as well as principal and school characteristics that correlate with performance pay's effects on principal salaries.[9] The year fixed-effects, τ_t, control for secular trends in educators' labor market decisions. Because I include year fixed-effects, I do not include an additional indicator for performance pay implementation. The school-district fixed effects, D_d, account for unobserved district characteristics correlated with performance pay and principals' labor market decisions. ϵ_{itds} are idiosyncratic errors. I estimate heteroskedasticity- and cluster-robust standard errors clustered at the district level in all models.

The average marginal effect estimate of the parameter of interest, δ^T, represents the difference-in-differences estimate of the average treatment effect of principal performance pay on the probability of labor market transition T. The average marginal effect measures how much more, or less, the principal labor market transition rates changed after introducing principal performance pay relative to teacher labor market transition rates, all else equal. Positive values of the marginal effect of δ^T indicate that performance pay increases the probability of a given labor market transition. In contrast, negative values suggest that it decreased the probability of a given labor market transition. There is a separate estimate of δ^T for each of the four labor market transitions. These are estimated simultaneously.

I also allow the treatment effect, δ, to vary over the two years following the introduction of performance pay by estimating the following specification:

$$\begin{aligned} Pr(\text{Transition}_{itds} = T) = & f(\alpha_0 + \alpha_1 \text{Principal}_{it-1ds} + \delta^T_{t+1}\text{Principal}_{it-1ds} \times \mathbb{1}(\text{Year} = 2018) \\ & + \delta^T_{t+2}\text{Principal}_{it-1ds} \times \mathbb{1}(\text{Year} = 2019) + X_{it-1}\beta + S_{t-1s}\psi \\ & + \tau_t + D_d + \epsilon_{itds}) \end{aligned} \tag{2}$$

In this specification, the $\text{Principal}_{it-1ds}$ indicator is interacted with indicators for the 2017-18 and 2018-19 school years. The average marginal effect estimates of δ_{t+1} and δ_{t+2} represent the net impact of principal performance pay on the probability of labor market transition T in 2017-18 $(t+1)$ and 2018-19 $(t+2)$, respectively.

4.2 Measuring Salary Change

For each principal exposed to the performance pay treatment, those in the 2016-17 and 2017-18 cohorts, I impute the effect of the principal performance pay on their state-provided base salary. For principals in the 2016-17 cohort, I impute the difference between what that principal would

[9]Even though these covariates are lagged, they may still be impacted by the policy change in the final cohort. In results available on request, I estimate all models without X_{it-1} or S_{t-1s} and find my results are qualitatively similar.

earn under the performance-based (2017-18) salary schedule and what that principal would have earned in a counterfactual where their salary continued to be determined by the experienced-based (2016-17) salary schedule.[10] For principals in the 2017-18 cohort, I impute the difference between what that principal would earn under the performance-based (2018-19) salary schedule and what that principal would have earned in a counterfactual where their salary continued to be determined by the prior year's performance-based (2017-18) salary schedule. In both cases, if this difference is positive, meaning the principal would earn more under the performance-based salary schedule in the following year, I say they receive a salary increase.

A potential issue with this definition of salary change regards endogeneity of salary changes for the 2017-18 cohort, who are making labor market transition decisions after the initial introduction of principal performance pay. A principal's salary in 2018-19 will be determined, in part, by the test-score growth of their school in 2017-18, one-year post-performance pay. If performance pay's incentives affect principal effort and labor markets in the year following its introduction, then the policy change may affect a principal's salary in 2018-19. I prefer to use the definition above of salary change because it directly measures the financial incentives principals face each year.[11]

As an example of the salary change measure, consider an average principal in terms of experience, education, school size, and test score growth in the 2016-17 cohort. Tables 1 and 2 show the 2016-17 and 2017-18 salary schedules respectively. I present summary statistics for the 2016-17 cohort of principals affected by the first year of performance pay in Table 4. This Table includes all full-time principals working at a single school earning more than $30,000 in 2016-17. An average principal in 2016-17 has 22 years of experience, a master's degree, managed 39 teachers and 588 students, and met growth expectations. If in 2017-18, the 2016-17 salary schedule determined their salary, this principal would earn a base salary of $62,352[12], while under the 2017-18 salary schedule, they would earn $71,322. So, this principal would have experienced a salary increase of $8,970. 72.1 percent of principals in 2016-17 would receive a salary increase. Those principals whose salary would rise would see an average increase of $7,032.

Table 4 also highlights the differences in principal characteristics by the salary effects of principal performance pay. Section 2.2 discusses how experience, education, school test-score growth,

[10]In the performance-based salary schedule of 2017-18, the past three years of school test score growth partly determine principal salaries. I use school test score growth in the prior year to simplify this imputation. To test the accuracy of the imputation, I predict principals' actual salaries in 2017-18 using principals' imputed 2017-18 salary in an OLS regression. I present my estimates from this regression Appendix Table A1. I find that a one-dollar increase in imputed salary is associated with a statistically significant 1.03-dollar increase in actual salary. Furthermore, I fail to reject the null hypothesis that the coefficient on imputed salary equals one at any conventional confidence level.

[11]I test the robustness of my results to models using an alternative definition of salary change using the difference between what that principal would earn under the performance-based (2017-18) salary schedule and what that principal would have earned in a counterfactual where their salary continued to be determined by the experienced-based (2016-17) salary schedule. In this alternative definition, I additionally define the salary changes for principals in the 2017-18 cohort, who make decisions two years post-policy change, using school characteristics from the year just before treatment, 2016-17. I present these results in Appendix Table A2.

[12]To compute this value, I add one additional year of experience to the 2016-17 average

and school size determine principals' salary changes, Table 4 demonstrates this in practice. Principals whose salaries will not increase under performance pay have ten more years of experience on average and are five percentage points (50%) more likely to have a doctorate. They are also 16 percentage points (100%) more likely to manage a school that was not meeting test-score growth expectations and 15 percentage points (50%) less likely to manage a school exceeding expectations. They managed schools with 100 fewer students and five fewer teachers than principals whose salaries would have increased. Since principal characteristics correlate with their school's characteristics, other school and principal characteristics are associated with performance pay's impact on salaries. For instance, principals whose salary would not increase are, on average, eight years younger, ten percentage points more likely to be female, eight percentage points more likely to be Black, and eight percentage points more likely to be White.

Figure 1 plots hypothetical pay in 2017-18 under the performance-based, 2017-18 pay schedule versus the hypothetical, counterfactual salary in 2017-18 under the experienced-based, 2016-17 pay schedule. Points above the dashed 45-degree line represent principals who experience a pay increase under the 2017-18 pay schedule. This figure accounts for the hold-harmless rule that a principal will not earn less in 2017-18 than their salary in 2016-17. Thus, any points below the 45-degree line represent principals affected by hold harmless, and their salary on the y-axis represents their earnings in 2016-17 under the experienced-based salary system rather than their performance pay salary in 2017-18. This figure shows that salary changes occurred across the principal pay distribution. It suggests significant variation in the salary changes induced by the switch to a performance-based salary schedule, likely translating into considerable heterogeneity in the effect of principal performance pay.

I test for heterogenous effects by financial incentives by estimating variations of the following model specification:

$$\begin{aligned} Pr(Transition_{itds} = T) =& f(\alpha_0 + \alpha_1 \text{Principal}_{it-1ds} + \delta_1^T \text{Principal}_{it-1ds} \times \text{Perf Pay}_t \times \text{No Salary Inc}_t \\ & + \delta_2^T \text{Principal}_{it-1ds} \times \text{Perf Pay}_t \times \text{Salary Inc}_t + X_{it-1}\beta + S_{t-1s}\psi \\ & + \tau_t + D_d + \epsilon_{itds}) \end{aligned} \quad (3)$$

In Equation 3, I add interactions of the $\text{Principal}_{it-1ds} \times \text{Perf Pay}_t$ term with an indicator for salary increases and an indicator for no salary increase. The indicator variable Salary Inc_t equals one if a principal would experience a salary increase while No Salary Inc_t equals one if a principal would not experience a salary increase. I define what it means to experience a salary increase in Section 4.2. The average marginal effects of δ_1^T and δ_2^T represent the impacts of performance pay on the probability of labor market transition T for principals' whose salary would not increase and for would increase, respectively. Since any labor market transitions will occur before the beginning of the year t school year, δ_1^T and δ_2^T measure principals' responses to a potential salary change, assuming they remain at the same school, rather than their responses to their actually salary changing. I replace the indicators for salary change with principal and school characteristics to estimate treatment effect heterogeneity by those characteristics in other analogous specifications.

4.3 Identifying Assumptions

Several identifying assumptions must be satisfied for my difference-in-differences estimates to have a plausibly causal interpretation. First, in the absence of the policy change, the trends in average labor market transitions over time would be the same for principals and teachers, conditional on the included covariates: the conditional parallel trends assumption. I test the plausibility of the conditional parallel trends assumption in two ways using an extended, less detailed panel of principal and teacher labor market transitions spanning 2012-13 to 2018-19.[13] I will note from the outset that this panel does not exclude educators who are working in multiple positions. This means that principals may earn income from other sources unaffected by performance pay, or teachers may work part-time as principals. Both of these factors introduce noise and may attenuate any effect estimates. For these reasons, I prefer to use the more detailed panel spanning 2014-15 to 2018-19 in my primary analyses. While this extended panel is not the ideal data for performing tests of parallel trends, it can provide some suggestive evidence of the assumption's plausibility.

First, Figure 2, Panels A through D, plot sample average labor market transition rates for principals and teachers by year. The solid vertical line between 2017 and 2018 represents the implementation of principal performance pay in July of 2017. The dashed vertical line between 2016 and 2017 represents when legislators began discussions about changing the structure of principal salaries in October of 2016.[14] In Figure 2A, teachers have higher same-school retention rates than principals throughout, but the trends in same-school retention match quite well in the pre-policy change period for teachers and principals. The comparable figures for each of the other labor market transitions suggest that the parallel trends assumption plausibly holds for each transition, except for switching schools in 2013-14. In 2013-14, principals' switching schools rate visibly increased relative to teachers.

Second, I formalize this pre-trends analysis using an event study. I estimate the following multinomial logit model:

[13] I describe the extended panel in more detail in 3.

[14] See `https://www.ncleg.gov/documentsites/committees/BCCI-6680/October%2024/1.%20Oct_24_Joint_SBA_Pay_agenda.pdf` [accessed February 2020] for the agenda of the first meeting in which legislatures discuss change school principal pay.

$$
\begin{aligned}
Pr(\text{Transition}_{itds} = T) = & f(\alpha_0 + \alpha_1 \text{Principal}_{it-1ds} + \sum_{t=-5}^{2} \delta_t \text{Principal}_{it-1ds} \times \mathbb{1}(\text{Year} = t) \\
& + X_{it-1}\beta + S_{t-1s}\psi + \tau_t + D_d + \epsilon_{itds})
\end{aligned}
\tag{4}
$$

Equation 4 parallels Equation 1, but allows for the treatment effect to vary over time through the inclusion of the δ_t parameters. The year $t-2$, or 2015-16, is omitted and serves as the reference year. Omitting $t-2$ allows for some anticipation of performance pay in the year before its passage. The marginal effect estimates of δ_j, where $j \in t$, represent the change in principals' average labor market transition rate relative to teachers between year j and year $t-2$ (two years before performance pay's introduction). Under the assumptions listed above, the post-policy δ_ts, where $t \geq 0$, represent an estimate of the interval-specific average treatment on the treated. In the pre-policy years, the δ_ts, where $t < 0$ provide a test of differential pre-policy trends. A statistically significant δ_t in the pre-policy years provides some evidence that parallel trends may not be plausible.

Figure 3 shows the marginal effect estimates of the δ_ts for each labor market transition. Except for one significant pre-trend for switching schools in 2013-14 ($t-4$), I find no evidence of differential pre-policy trends between principals and teachers. This result suggests that the estimates of switching schools effects may not be as robust as the other transitions. However, this differential trend occurs well before performance pay's implementation, which tempers concerns about parallel trend violations.

While this event study analysis finds generally insignificant pre-trends, it uses data where effects may be attenuated. There could be significant pre-trends that are not detectable using these data. However, this is the best available data providing historical labor market transition rates for educators in North Carolina, and this analysis provides suggestive evidence of the plausibility of parallel trends.

With this caveat in mind, I provide additional robustness checks to evaluate the potential impact of differential trends on my estimates by using other estimation strategies that provide more plausible comparison groups and an alternative comparison group. First, I use a propensity score weighting technique for difference-in-differences proposed in (Stuart et al., 2014) to ensure that teachers more similar to principals carry more weight in the difference-in-differences analysis. Second, I estimate models comparing principals to teachers within the same school using separate linear probability models with school fixed effects. Finally, I use assistant principals as an alternative comparison group in a difference-in-differences analysis. I present the results of the analyses mentioned above in Section 6.

The second assumption is the absence of contemporaneous differential shocks. During my primary analysis period, 2015-16 to 2018-19, teacher pay policies in North Carolina were relatively stable. In 2013, the state eliminated multi-year contracts for teachers, but state legislators reinstated them in 2016. Additionally, performance-based bonuses of $1,500 to $2,000 were available to teachers from 2010 to 2014 through the Race to the Top program (Lauren and Kozlowski, 2014). Policymakers reintroduced performance incentives for teachers in grades and courses subject to end-of-grade testing for the 2017-18 school year.[15] These $2,000 bonuses are available to teachers who are in the top 25% of the state or district in test-score growth. These teacher performance pay policies may impact teachers' labor market transitions. However, these performance bonuses are relatively small compared to the treatment experienced by principals. If teachers respond to performance incentives similarly to principals, my estimate of the principal performance pay treatment effects would be biased toward zero.

The third assumption is the stable unit treatment value assumption (SUTVA). If the change in principal pay structure impacts teachers' labor supply decisions, this may violate SUTVA. To understand how this may affect my analyses, I will consider an example regarding same-school retention for simplicity. Principals pushed out by principal performance pay must be replaced. If their replacement is a teacher, then same-school retention rates for principals and teachers will decrease following the policy change. This spillover will bias my estimates of retention effects toward zero.

A similar issue will arise if teachers respond to their principals' labor supply decisions, regardless of the principal salary structure. A teacher may decide to leave teaching or move to a new school, potentially following their principal if their principal leaves their position. Bartanen et al. (2019) document a decrease in teacher retention rates of between one and three percentage points following the exit of principals from schools in Tennessee and Missouri. Again, this will decrease the same-school retention rates of both principals and teachers and bias my same-school retention effect estimates toward zero, attenuating my effect estimates.

Next, multinomial logit models rely on the assumption of independence of irrelevant alternatives (IIA) to model choices. In this context, IIA implies that an individual's odds of choosing one transition over another, say same-school retention over separation, do not depend on the presence, or absence, of other alternatives. A violation of the IIA assumption might occur if two alternatives are close substitutes. For instance, if principals use other changes as transitions to retirement, then the availability of complete separation from the labor force might affect the odds of choosing retention over other changes. I perform the Small-Hsiao test of the IIA assumption, and I fail to reject the null hypothesis of independence. I also estimate separate linear probability models that do not assume IIA.

Finally, an additional source of bias might come from school districts using their local salary supplements to circumvent the state's performance-based pay structure (Hui, 2018). However, even in the worst-case scenario, where all districts avoid performance pay, my difference-in-differences

[15] See the North Carolina Department of Education's funding allotment policy manual for more details: `https://files.nc.gov/dpi/documents/fbs/allotments/general/2017-18policymanual.pdf` [accessed August 2020]

estimate of the performance pay treatment effect would equal zero, representing the policy's actual impact in that hypothetical. I provide some evidence about how prevalent school district circumvention might be by examining how principal salary supplements changed relative to teachers'. Each year the North Carolina Association of County Commissioners publishes average educator salary supplements for each school district. I use salary supplement data from the 2014-15 to 2018-19 (three years before and two years after the policy change) and perform a difference-in-differences analysis of the changes in salary supplements of principals relative to teachers. I present average salary supplements for principals and teachers before and after performance pay in Appendix Table A3. Before the policy change, the average salary supplement for principals and teachers was $12,971.33 and $3,917.66, respectively. Following principal performance pay, average principal and teacher supplements rose by $1,090.67 and $540.83, respectively. These changes imply a difference-in-differences of $549.83. So, average principal salary supplements increased by $549.83 more than teacher supplements over the same period. This estimate is not statistically significantly different from zero, but this analysis does not have much power because there are relatively few school districts. This result provides evidence that the circumvention of performance pay by school districts was not detectable on average. However, it is possible that school district circumvention is changing the distribution of principal salary supplements, but not the average. This type of change could happen, for instance, if school districts are redistributing principal salary supplements after the policy change.

5 Results

5.1 Performance Pay and Principals' Labor Market Decisions

Table 5, Panel A, presents average marginal effect estimates from the multinomial logit model of Equation 1. This model is estimated with same-school retention as the baseline category and includes all covariates and school district fixed effects.[16] I present only the average marginal effect estimates of the difference-in-differences parameters. Appendix Table A6 contains the average marginal effect estimates for all covariates. Estimates in each cell of Table 5, Panel A, come from a single multinomial logit model.[17] The columns in Table 5 each represent one labor market transition, and the average marginal effect estimates measure how performance pay changed the probability of a given transition for principals relative to the change in teachers.

The main parameters of interest are the average marginal effects of principal and performance pay interaction. These parameters represent the difference-in-differences estimate of the average

[16] I present average marginal effect estimates from analogous, more parsimonious models with and without controls, but no school district fixed effects, in Appendix Tables A4 and A5. The results are qualitatively similar to my preferred specification.

[17] I present estimates from analogously specified linear probability models, to allow for violations of the IIA assumption in Appendix Table A7. The marginal effect estimates are qualitatively similar.

treatment effect of principal performance pay on each labor market transition. The average treatment effect in this model specification can be interpreted as either assuming a constant performance pay treatment effect for all principals or the weighted average of each principal's treatment effect. The average effect of principal performance pay is ambiguous: there is significant heterogeneity in how principals are affected by the policy change. The aggregate impact measured in Panel A represents the impacts of receiving a salary increase or not as well as the impacts of incentive changes. I find that overall, principals' same school retention rates declined by 2.6 percentage points following the introduction of principal performance pay. In comparison, the probability of switching schools and other employment changes increased by 1 and 1.5 percentage points, respectively.

To understand how the aggregate impacts of performance pay change over time, Table 5, Panel B, estimates the model in Equation 2. Table 5, Panel B, provides evidence that the effect of principal performance changed over time. I find that the significant decreases in same school retention and increases in school switching occur in the first year of performance pay's implementation, and I fail to reject the null hypotheses that performance pay had no impact on same-school retention and school switching in year $t+2$. I also fail to reject the null hypothesis that performance pay did not affect separations in either year. On the other hand, I find that performance pay's impact on other changes persists two years after introduction, with a 1.4 percentage point increase in $t+1$ and a 1.6 percentage point increase in the probability of other changes in $t+2$. Overall, these results suggest a significant impact of principal performance pay on principals' labor market decisions.

5.2 Heterogeneity by Financial Incentives

The above results provide evidence of a significant impact of principal performance pay on average across North Carolina principals and how this aggregate impact changes over time. While this is relevant to policymakers at the state level who want to understand the macro-level implications of principal performance pay, these results smooth over the differential impacts principals experience by changes in their financial incentives. In this analysis, I examine changes in principals' expected salaries year $t-1$ to year t. Since any educator labor market transition occurs before school year t begins, and before receiving any pay, I am estimating principals' responses to potential salary changes, rather than actual salary changes. To understand how these performance pay-induced salary changes affect principals' labor market transitions, in Table 5, Panel C, I allow the impact of performance pay to differ for principals whose salary would increase under performance pay and for those whose salary would stagnate by estimating Equation 3.

The intended effect of principal performance pay is to retain effective principals and induce exits from ineffective principals. Policymakers achieve these effects through changes in principal salaries: effective principals receive higher compensation while less effective principals do not.

Table 5, Panel C, shows that principals whose salary would not increase under performance pay are more likely to leave the principalship and move into other positions or exit North Carolina public education in $t + 1$. A marginally significant decrease in the probability of remaining a principal but switching schools appears to drive these exits from the principalship. Additionally, principals whose salaries would not increase are less likely to stay at their current school, but this estimate is not statistically significant. The estimates in $t+2$ provide no evidence that statistically significant changes in these principals' labor market transitions persist.

Principals whose salary would increase due to performance pay are 2.7 percentage points less likely to stay in the same position at the same school in $t + 1$ and are 2.3 percentage points more likely to switch to a different school. In contrast, in $t + 2$, they were 1.9 percentage points more likely to make other employment changes. Notably, I find no evidence that principals financially rewarded by performance pay are more or less likely to separate in $t + 1$ or $t + 2$ or switch schools in $t + 2$.

Together, these results suggest that principals respond to the financial incentives of performance pay and that their responses vary over time. Principals not financially rewarded by performance pay, whose salaries would stagnate, are more likely to leave the principalship. However, the impact is transient: there is no evidence of any performance pay effects for these principals in $t + 2$. On the other hand, principals financially rewarded by performance pay, whose salaries would increase, are more likely to continue working as principals at a different school in $t + 1$. These principals are more likely to make other changes and leave the principalship in $t + 2$.

These results raise two important questions for evaluating the impact of principal performance pay. First, why would principals who will earn more money under performance pay at their current school decide to switch schools? Second, how is the composition of the principal labor force altered by the labor market changes induced by performance pay?

5.3 Distribution of Principals to Schools

I've provided evidence suggesting that principals who would potentially see their salaries increase if they stay at their current school are more likely to switch to a different school following the introduction of principal performance pay. One potential explanation is that principals are switching to schools where they expect to have the best potential to improve students' future test scores. This behavior assumes principals respond to financial incentives to maximize their expected future earnings. I provide a test of this hypothesis in Table 6, where I estimate how principal performance pay impacted the probability that a principal switched to a recurring low-performing school.

Table 6 presents average marginal effect estimates from a logit model where the dependent variable is an indicator of switching to a low-performing school. The NCDPI classifies a school as recurring low-performing if it earned a school performance grade of "D" or "F" for two or three of the last three years. NCDPI releases a list of recurring low-performing schools each year. I define recurring low-performing schools using the list from the 2014-15 school year, the first year this data is available. The logit model is specified according to Equation 3.

On the one hand, principal performance pay might affect principals' propensity to switch to underperforming schools since principals are now financially incentivized to raise student test scores, and there is more room for improvement in schools that are recurring low-performing. On the other hand, challenges addressing systemic problems may disincentivize switches to low-performing schools, since principals face potential salary decreases if their school under-performs. I find suggestive evidence of the former hypothesis; principals appear to be incentivized to switch to low-performing schools. Principals whose salaries would increase under performance pay – who are more likely to switch schools overall – are more likely to switch to a low-performing school following the introduction of principal performance pay. This estimate is significant at the 10 percent level. This result supports the hypothesis that principals are switching schools in response to performance pay's financial incentives. However, regardless of why principals are switching schools, performance pay appears to incentivize principals to move into recurring low-performing schools.

5.4 Composition of the Principal Labor Force

The evidence presented shows that the switch to performance pay pushes principals who would not receive a pay increase out of the principalship. How does this change the composition of the principal labor force? The new performance pay system benefits principals who have worked in schools that met or exceeded their test score growth targets and removes rewards for extended service and higher education. So, the principals adversely affected by the policy change will likely be from lower-performing schools and have more experience. I examine the performance-pay treatment effects heterogeneity by principal experience and schools' recurring low-performance status to test these hypotheses.

First, consider Table 7, Panel A, which examines how performance pay's effect varies by principal experience. I group principals into years of experience terciles. The first, second, and third terciles span 10 to 18, 19 to 23, and 24 to 48 years of experience, respectively. I estimate a model that parallels Equation 3, and present marginal effect estimates. The top three rows of Panel A show the impacts of performance pay one-year post-implementation. These estimates suggest that principals in the second and third terciles of the experience distribution drive all of the significant labor market impacts of performance pay. In $t+1$, principals in the second experience tercile are 4.7 percentage points less likely to stay at the same school, 3.2 percentage points more likely to switch schools, and 2.1 percentage points more likely to make other changes following the implementation of performance pay. This result suggests that some middle experience principals induced to leave their current role by performance pay continue to work as principals in other schools. In contrast, others leave the principalship for other positions. A potential explanation is that some principals in this middle experience group receive a pay increase while others do not.

Also, in $t+1$, Table 7, Panel A, suggests that highly experienced principals, in the top experience tercile, are more likely to exit the principalship as a result of performance pay, potentially due to their higher probability of stagnating wages. The most experienced principals experience a 4.0 percentage point decrease in same-school retention rates, a 2.5 percentage point increase in the probability of other changes, and a 1.9 percentage point increase in separation rates.

In line with the aggregate estimates in Table 5, Panel B, there is little evidence of systematic changes in principals labor market decisions in $t+2$ Table 7, Panel A. It appears that essentially all of the increases in the rate of other employment changes in $t+2$ in Table 5, Panel B, are driven by principals in the third experience tercile. I find no other evidence of statistically significant impacts of performance pay in $t+2$ in Table 7, Panel A. Overall, this panel suggests that performance pay induced exits from more experienced principals.

Next, Table 7, Panel B, shows how principal performance pay's effects vary by school performance. I find that, in $t+1$, principals in persistently low-performing schools were more likely to leave their current position and move to other public education jobs or exit North Carolina public education entirely. In $t+2$, these principals in persistently low-performing schools were still more likely to move to other positions in public education and less likely to switch schools. These results suggest that performance pay induced exits from principals in the lowest-performing schools.

On the other hand, performance pay incentivized principals in higher-performing schools to continue working as principals in different schools for one and two years post-performance pay. However, in $t+2$, these principals were more likely to make other employment changes.

Together, these results imply that the switch to performance pay had consequences on the composition of the stock of existing principals. In the year immediately following implementation, principals with more experience but from low-performing schools were more likely to leave the principalship. This result implies that stock of continuing principals will be less experienced but have a better record of improving student test scores than before principal performance pay.

Furthermore, exits from persistently low-performing schools provide openings for new principals. I have demonstrated that principals with characteristics rewarded by performance pay are more likely to switch to low-performing schools, filling these new vacancies. If these principals are higher-quality, as policymakers desire, then the net impact would be that performance pay attracted higher-quality principals to low-performing schools.

6 Additional Robustness Checks

Using teachers as the control group for principals in a difference-in-differences analysis has potential issues. While principals and teachers are similar in various observed characteristics, as demonstrated in Table 3, they are likely different in unobservable ways that may correlate with labor market transitions. In Section 4.3, I show that an event study suggests that pre-policy trends are similar between principals and teachers, except for a significant increase in principals' switching school rates relative to teachers in 2013-14. However, this event study analysis is likely underpowered due to data limitations before 2014-15. In this section, I examine the robustness of my results to different estimation strategies that provide more plausible comparisons and an alternative comparison group.

6.1 Propensity Score Matching

First, I implement propensity score matching following the method of inverse propensity score weighting proposed in Stuart et al. (2014). I begin by defining four groups, G based on treatment and time status:

$$G = \begin{cases} 1 \text{ if Principal=1 and Post=0} \\ 2 \text{ if Principal=1 and Post=1} \\ 3 \text{ if Principal=0 and Post=0} \\ 4 \text{ if Principal=0 and Post=1} \end{cases} \tag{5}$$

Where Principal equals zero for teachers and Post equals zero before performance pay's implementation. I then estimate propensity scores equal to the probability of belonging to each group k, defined as $e_k(X_{it-1}, S_{t-1s}, D_d)$, conditional on individual and school covariates (X_{it-1} and S_{t-1s}) as well as district fixed effects (D_d) using a multinomial logit model. I use the same covariates as my preferred specification in Equation 1.

Next, I create weights such that each of the four groups is weighted to be similar to Group 1, which are principals before performance pay. I define the weight for individual i as:

$$w_i = \frac{e_1(X_{it-1}, S_{t-1s}, D_d)}{e_g(X_{it-1}, S_{t-1s}, D_d)} \tag{6}$$

Where $e_1(X_{it-1}, S_{t-1s}, D_d)$ represents the probability that i belongs to Group 1 and $e_g(X_{it-1}, S_{t-1s}, D_d)$ represents i's propensity score for the group they are actually in. Notice that individuals in Group 1 receive a weight of 1, while members of other groups have weights proportional to their probability of being in Group 1. I then use these weights in the estimation of my preferred difference-in-differences specification.

I present estimates from the propensity score weighted models in Table 8. This table replicates the main difference-in-differences results in Table 5. The propensity score-weighted estimates are similar but less precise than their unweighted counterparts. This aligns with expectations given that propensity score weighting reduces the effective sample size. The only noticeable difference is in Panel B, where separations in 2018 are now significantly higher at the five percent level, but this change does not impact the interpretation of the results. While the propensity score matching results are generally less precise, they are qualitatively quite similar to the unmatched comparisons.

6.2 Assistant Principal Comparison

Second, I use assistant principals as an alternative comparison group in a difference-in-differences analysis. As a comparison group, assistant principals benefit from being even more similar to principals than experienced teachers: assistant principals have made costly career decisions that place them on a trajectory to become a principal. While 80% of first-time North Carolina principals were formerly teachers, 94% were formerly assistant principals (Bastian and Henry, 2015). Furthermore, assistant principals have been paid based on an experience-based salary system nearly identical to principals before the introduction of principal performance pay, which continued through 2018-19, the last year of my analysis. Ostensibly, assistant principals appear to be quite a good comparison group for principals. However, there are issues particular to assistant principals that reduce their attractiveness as a comparison group. First, since assistant principals are potentially next in line to become principals once a position is vacant, assistant principals' labor market decisions would likely be impacted by principal performance pay if it affects principals' labor market decisions.

Second, when principal performance was implemented, assistant principals received a significant unconditional salary increase. Beginning in 2017-18, the first year of principal performance pay, all assistant principals across North Carolina received a more than $9,000 pay raise, representing a significant increase in income relative to the prior year. For example, an assistant principal with 15 years of experience in 2016-17 would have earned $45,010 over a ten-month contract. The same assistant principal would have earned $54,170 in the next year, 2017-18, a 20% increase in pay. A similar salary increase was implemented in 2018-19. This represents a significant contemporaneous differential shock, threatening the internal validity of difference-in-differences estimates using assistant principals as a control group.

Regardless, using assistant principals as the comparison group can be informative. Many principals also received a significant salary increase due to performance pay. The magnitude of the average principal pay increase was similar to that of assistant principals. However, other financial incentives for principals changed concurrently with their salary increases. While assistant principals only received an increase in income, principals faced a substantial change in the structure of their financial incentives. One would expect an unconditional pay raise to decrease assistant principal turnover, increase retention and decrease other employment changes and exits. Comparing the differences in labor market decisions of principals and assistant principals nets out the effect of salary increases and leaves the impact of the incentive changes faced by principals.

Table 9 presents estimates of Equations 1 to 3 using assistant principals as the comparison group. The table replicates Table 5. The estimates in Panel A indicate that principals experienced a significant decrease in the probability of same-school retention and a commensurate increase in the probability of other employment changes relative to assistant principals. Panel B suggests that this impact persisted for two years post-performance pay. These estimates reflect both the impact of the assistant principal salary increase and the introduction of performance pay. Suppose the assistant principal pay raise increased assistant principal retention and decreased the attractiveness of other changes. In that case, the effects estimated in Panel A and B are likely driven by changes in the assistant principals' labor market decisions. Panel C further supports this hypothesis and shows that only principals who did not receive a salary increase under performance had significant increases in other employment changes and decreases in retention in $t + 1$.

On the other hand, principals who received a salary increase did not experience significant changes in labor market transitions relative to assistant principals in $t + 1$. This result is not surprising. Principals who received a salary increase in $t + 1$ saw their salaries rise by roughly $7,000, comparable to the pay raise experienced by assistant principals. Notably, in $t + 1$, there is no detectable change in the probability of switching schools relative to assistant principals for principals whose salaries would increase. However, the point estimate for the impact on the probability of switching schools for salary increase principals in $t+1$ is quite similar to the estimate in Table 5. However, the standard errors are larger, and the estimate is less precise. Thus, we cannot rule out that there is no impact of performance pay on the probability of switching schools when we use assistant principals as the comparison group.

In $t + 2$, principals who received a salary increase appear to drive the significant increase in other changes and decreased retention during the period. Again, this makes sense. While some principals' salaries increased in $t + 2$, the magnitude of the principal pay raise was generally more modest relative to assistant principals. This continues to suggest that gains in the retention of assistant principals due to their pay increases are driving the retention and other change effects present in these models.

6.3 Within School Comparisons

Third, I estimate models comparing principals and teachers within the same school using separate LPMs with school fixed effects for each labor market transition. I use linear probability models in this estimation strategy to avoid the incidental parameters problem that arises from including fixed effects that increase with sample size in nonlinear models. Within-school models limit the difference-in-differences comparisons to teachers and principals within the same school over time. This strategy controls for any fixed, unobserved school characteristics that correlate with being a principal and labor market transitions (e.g., work environment) and restricts the comparison group for each principal to only teachers at their school, who may be more similar in unobservable characteristics. On the other hand, within-school comparisons may exacerbate the impact of SUTVA violations caused by teachers' responses to their principal's labor market decisions because the model only makes comparisons of teachers with their own principal.

To implement the within-school estimation strategy, I estimate separate LPMs for each labor market transition specified identically to Equations 1 through 3 except for including school fixed effects instead of district fixed effects. Table 10 presents estimates from these linear probability models, and replicates Table 5. Importantly, now, each column represents a separate regression model. The estimates from the within-school models are qualitatively similar to those in Table 5, with some notable differences. In Panel B, the decrease in retention in $t+2$ is no longer statistically significant, though the point estimate is quite similar to the multinomial logit marginal effect. In Panel C, some point estimates are larger than in Table 5, though the difference in coefficient estimates may not be statistically significant across the models. Overall, the within-school linear probability model estimates are qualitatively similar to Table 5.

7 Conclusion

Principals are a critical input in education. Therefore, policies that change principals' labor market decisions can have important implications for students. This paper examines the labor market effects of a principal performance pay policy in North Carolina. I find that principals who would face stagnant or falling wages in the year following the policy change are more likely to leave the principalship and move into another position in public education. At the same time, principals who would see their salaries increase are more likely to continue working as a principal but switch to a different school. This suggests that school principals respond to the new financial incentives of performance pay.

When implementing principal performance pay, policymakers in North Carolina sought to increase principal pay while incentivizing effort, recruiting and retaining high-performing principals, and rewarding principals for high-performance (General Assembly of North Carolina, 2016). A concern for policymakers and the public was that performance-pay-induced exits would be from highly experienced principals and that students would suffer (Hui, 2017). I find highly experienced principals are more likely to exit the principalship under performance pay, but the principals that left were potentially less effective. Performance pay induces exits principals managing persistently low-performing schools.

Another potential consequence of principal performance pay is principal churn: performance pay might incentivize principals to switch schools to maximize future earnings by finding schools where test-score gains may be easier. I find that principals who are financially rewarded by performance pay are more likely to switch schools. These switches tend to be to recurring low-performing schools, consistent with the hypothesis that principals move to schools where they expect to increase test scores in the future. If performance pay rewards higher-quality principals, as desired by policymakers, then this suggests performance pay attracts higher-quality principals to traditionally harder-to-staff, persistently low-performing schools.

Economic models of performance pay predict it will incentivize workers to exert more effort and result in more effective workers sorting into positions with performance pay. In this paper, I provide evidence of sorting following the implementation of performance pay. Performance pay pushed out principals from low-performing schools and attracted different principals to lower-performing schools. However, further research is needed to estimate its impacts on principals' effort. The present study also does not address the efficacy of performance pay in attracting higher-quality new principals to the principal labor force. However, these component is essential to evaluate whether principal performance pay was a successful policy and warrant future research.

My results add to a growing literature examining the impacts of performance pay for educators. Performance pay for teachers has become more common, and research examining its impacts on student achievement and the teacher labor force has flourished. Principal performance pay is less common and less studied. My findings have important implications for policymakers. Since North Carolina's implementation represents the first large-scale principal performance pay program in the United States, policymakers may look to North Carolina as a model. My results suggest that principal performance pay affects the composition and distribution of principals by pushing experienced but potentially ineffective principals out of their positions and attracting different principals to traditionally harder-to-staff, low-performing schools.

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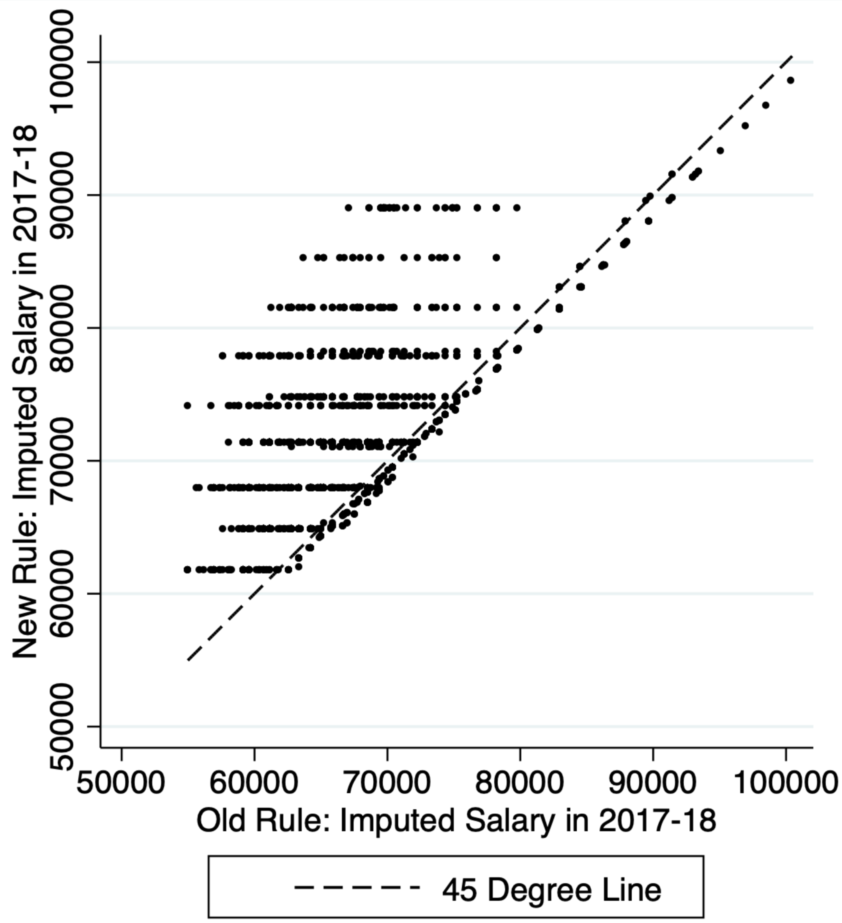
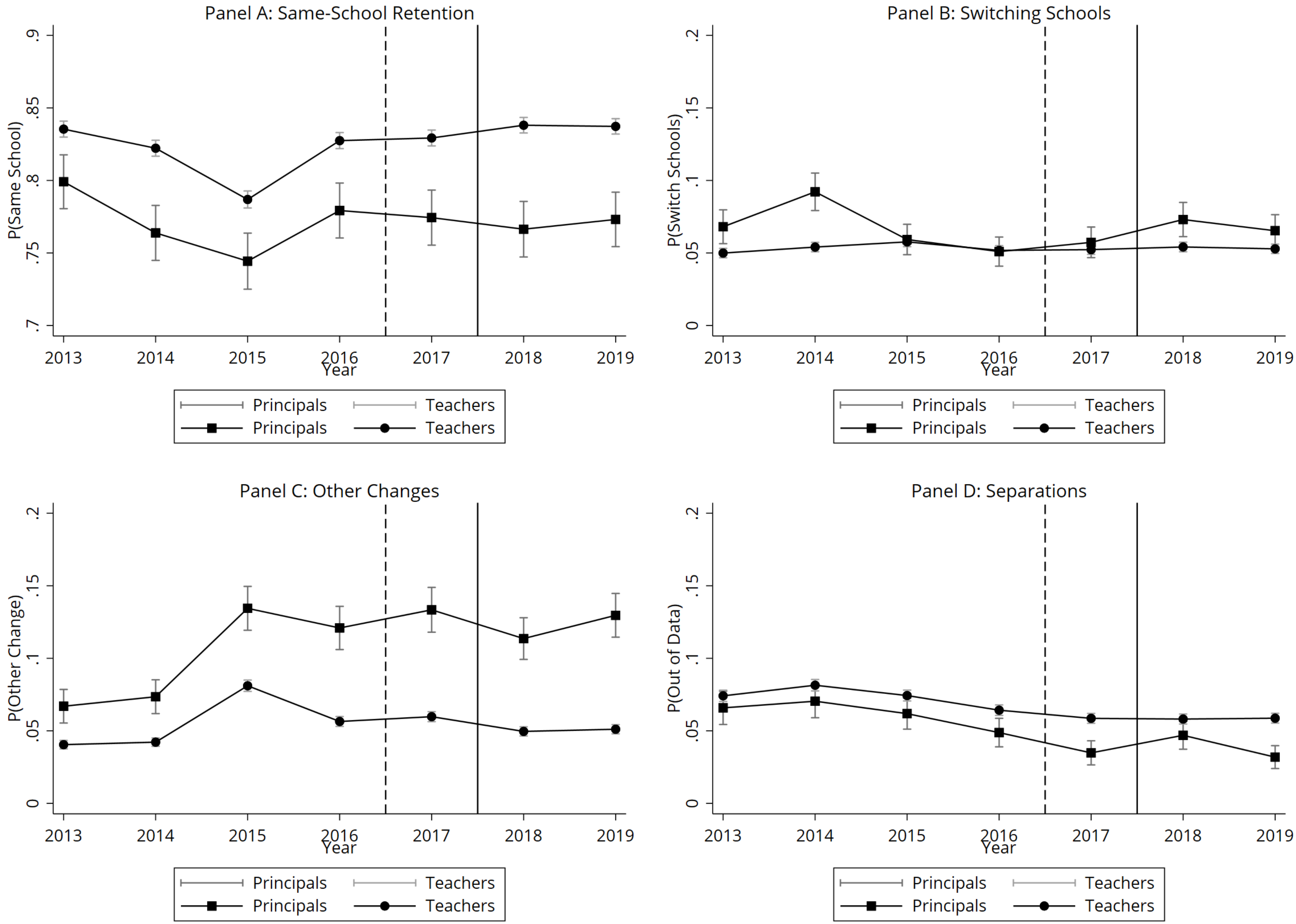
Figure 1: Imputed Pay in 2017-18 Under 2017-18 Schedule vs Imputed Pay in 2017-18 Under 2016-17 Schedule (Accounting for Hold Harmless)



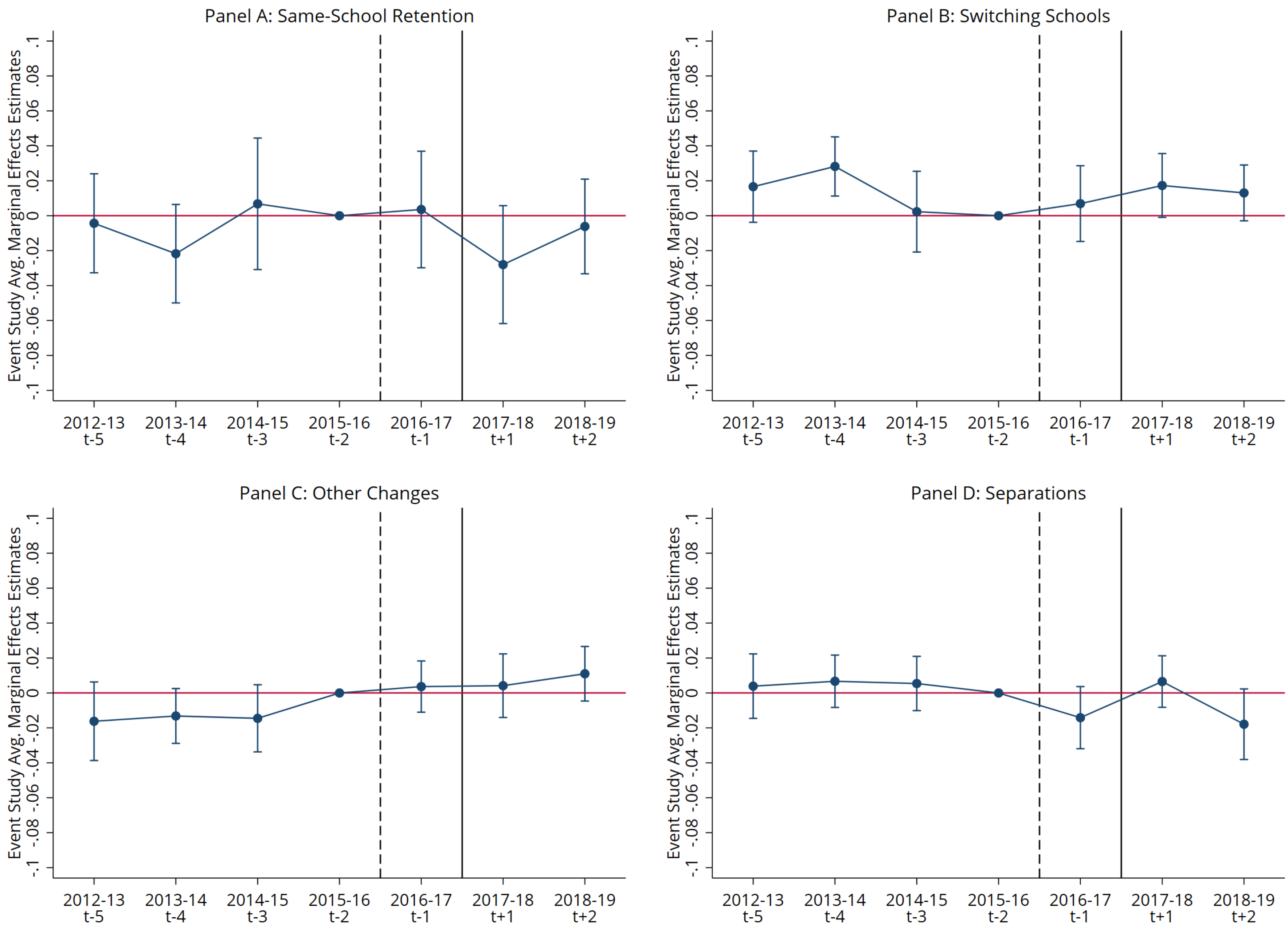
Note: Salary is imputed using the 2016-17 and 2017-18 principal pay schedules published by the North Carolina Department of Public Instruction and principal characteristics from NCERDC personnel pay files.

Figure 2: Trends in Labor Market Transitions from 2012-13 to 2018-19



Note: Data are from the NCERDC. The dashed vertical lines represent when legislators began to discuss changes to principal pay policy. The solid vertical lines represent when principal performance pay was introduced.

Figure 3: Labor Market Transition Event Study from 2012-13 to 2018-19



Note: Data are from the NCERDC. The dashed vertical lines represent when legislators began to discuss changes to principal pay policy. The solid vertical lines represent when principal performance pay was introduced.

Table 1: Excerpt From the 2016-17 Salary Schedule (School Size: 33-43 Teachers)

	Yearly Salary		
Years of Experience	Masters	Sixth Yr Adv	Doctorate
0-18	$58,320	$59,832	$61,356
19	$59,088	$60,600	$62,124
20	$59,904	$61,416	$62,940
21	$60,696	$62,208	$63,732
22	$61,512	$63,024	$64,548
23	$62,352	$63,864	$65,388
24	$63,192	$64,704	$66,228
25	$64,104	$65,616	$67,140
26	$64,980	$66,492	$68,016
27	$65,880	$67,392	$68,916
28	$66,780	$68,292	$69,816
29	$67,728	$69,240	$70,764
30	$68,712	$70,224	$71,748
31	$69,696	$71,208	$72,732
32	$70,572	$72,084	$73,608
33	$71,976	$73,488	$75,012
34	$73,416	$74,928	$76,452
35	$74,880	$76,392	$77,916
36	$76,380	$77,892	$79,416
37	$77,904	$79,416	$80,940
38	$79,464	$80,976	$82,500
39	$81,048	$82,560	$84,084
40	$82,668	$84,180	$85,704
41	$84,324	$85,836	$87,360
42+	$86,016	$87,528	$89,052

Table 2: 2017-18 North Carolina Principal Salary Schedule (Complete)

	Yearly Salary		
Enrollment	Base	Growth Met	Growth Exceeded
0 to 400	$61,751	$67,926	$74,101
401 to 700	$64,839	$71,322	$77,806
701 to 1,000	$67,926	$74,719	$81,511
1,001 to 1,300	$71,014	$78,115	$85,216
1,300+	$74,101	$81,511	$88,921

Note: Principal salary schedules are published by the North Carolina Department of Public Instruction here `https://www.dpi.nc.gov/districts-schools/district-operations/financial-and-business-services/compensation-`
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Table 3: Summary Statistics by Occupation

	(1) All	(2) Principals	(3) Teachers
Years of Experience	19.653	22.190	19.383
	(7.032)	(6.564)	(7.027)
Total Enrollment	781.975	586.868	802.839
	(490.199)	(377.846)	(496.208)
Number of Teachers	50.077	38.877	51.265
	(26.815)	(21.469)	(27.050)
Master's Degree	0.953	0.774	0.972
	(0.211)	(0.419)	(0.164)
Sixth Yr Adv Degree	0.024	0.117	0.014
	(0.152)	(0.321)	(0.116)
Doctorate	0.023	0.109	0.014
	(0.151)	(0.312)	(0.118)
Age	47.429	47.597	47.411
	(8.647)	(7.415)	(8.768)
Female	0.799	0.608	0.820
	(0.401)	(0.488)	(0.385)
Asian	0.008	0.001	0.008
	(0.087)	(0.034)	(0.091)
Black or African American	0.144	0.224	0.135
	(0.351)	(0.417)	(0.342)
Hispanic	0.017	0.004	0.018
	(0.128)	(0.066)	(0.133)
Native American	0.009	0.011	0.009
	(0.095)	(0.104)	(0.094)
Other Race	0.002	0.002	0.002
	(0.041)	(0.040)	(0.041)
White	0.821	0.758	0.828
	(0.383)	(0.428)	(0.377)
Retirement Elig Early	0.308	0.337	0.306
	(0.462)	(0.473)	(0.461)
Retirement Elig Full	0.142	0.151	0.141
	(0.349)	(0.358)	(0.348)
Growth Expectation Not Met	0.247	0.226	0.249
	(0.431)	(0.419)	(0.432)
Growth Expectation Met	0.415	0.447	0.411
	(0.493)	(0.497)	(0.492)
Growth Expectation Exceeded	0.308	0.273	0.312
	(0.462)	(0.446)	(0.463)
No Test-Score Growth	0.030	0.053	0.028
	(0.172)	(0.224)	(0.165)
Elementary School	0.485	0.567	0.476
	(0.500)	(0.496)	(0.499)
Middle School	0.214	0.195	0.216
	(0.410)	(0.397)	(0.411)
High School	0.282	0.205	0.290
	(0.450)	(0.403)	(0.454)
Other Grades School	0.020	0.033	0.019
	(0.140)	(0.179)	(0.135)
New School	0.005	0.008	0.005
	(0.071)	(0.089)	(0.068)
Observations	64652	6222	58430

Note: Data are from the NCERDC from the years 2014-15 to 2018-19. Standard deviations are in parentheses.

Table 4: Summary Statistics by Salary Increase: Principals in the 2016-17 Cohort

	(1) All	(2) No Salary Inc	(3) Salary Inc
Proportion Salary Increase	0.721	0.000	1.000
	(0.449)	(0.000)	(0.000)
Change in Salary	4770.427	-1064.699	7032.183
	(5547.227)	(459.672)	(4929.356)
Years of Experience	22.227	28.868	19.653
	(6.459)	(5.579)	(4.703)
Total Enrollment	588.046	509.288	618.573
	(385.304)	(341.633)	(396.912)
Number of Teachers	38.964	35.317	40.377
	(21.598)	(19.823)	(22.095)
Master's Degree	0.778	0.765	0.783
	(0.416)	(0.425)	(0.412)
Sixth Yr Adv Degree	0.108	0.082	0.119
	(0.311)	(0.275)	(0.323)
Doctorate	0.114	0.153	0.098
	(0.317)	(0.360)	(0.298)
Age	47.606	53.737	45.229
	(7.326)	(6.126)	(6.303)
Female	0.610	0.674	0.586
	(0.488)	(0.469)	(0.493)
Asian	0.002	0.007	0.000
	(0.044)	(0.083)	(0.000)
Black or African American	0.225	0.285	0.202
	(0.418)	(0.452)	(0.401)
Hispanic	0.004	0.002	0.005
	(0.067)	(0.048)	(0.073)
Native American	0.014	0.011	0.015
	(0.118)	(0.106)	(0.122)
Other Race	0.002	0.000	0.003
	(0.044)	(0.000)	(0.051)
White	0.753	0.694	0.775
	(0.432)	(0.461)	(0.418)
Retirement Elig Early	0.340	0.735	0.187
	(0.474)	(0.442)	(0.390)
Retirement Elig Full	0.142	0.406	0.040
	(0.349)	(0.492)	(0.196)
Growth Expectations Not Met	0.210	0.338	0.161
	(0.408)	(0.474)	(0.368)
Growth Expectations Met	0.473	0.450	0.481
	(0.499)	(0.498)	(0.500)
Growth Expectations Exceeded	0.265	0.148	0.310
	(0.441)	(0.356)	(0.463)
No Test-Score Growth	0.052	0.064	0.048
	(0.223)	(0.245)	(0.213)
Elementary School	0.571	0.600	0.559
	(0.495)	(0.490)	(0.497)
Middle School	0.188	0.183	0.190
	(0.391)	(0.387)	(0.393)
High School	0.202	0.158	0.219
	(0.402)	(0.365)	(0.414)
Other Grades School	0.039	0.059	0.031
	(0.193)	(0.237)	(0.173)
New School	0.011	0.009	0.012
	(0.104)	(0.095)	(0.107)
Observations	1568	438	1130

Note: Data are from the NCERDC. The sample includes all full-time principals who worked in a single school during the 2016-17 school year. Change in salary equals how much a principal would earn in the 2017-18 school year under performance pay relative to what they would have earned if the previous, experience-based salary schedule was still in place in 2017-18. Standard errors are in parentheses.

Table 5: Performance Pay and Labor Market Transitions: Baseline Multinomial Logit Model

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separation
	Panel A: Constant Treatment Effect			
Principal x Post	-0.0259**	0.0101*	0.0154**	0.0004
	(0.0117)	(0.0059)	(0.0066)	(0.0066)
	Panel B: Constant Treatment Effect by Year			
Principal x 2018 ($t+1$)	-0.0361**	0.0130*	0.0140*	0.0091
	(0.0145)	(0.0076)	(0.0073)	(0.0075)
Principal x 2019 ($t+2$)	-0.0144	0.0073	0.0169**	-0.0099
	(0.0147)	(0.0085)	(0.0077)	(0.0098)
	Panel C: Treatment Effect by Salary Change and Year			
Year $t+1$:				
Principal x 2018 x No Salary Inc	-0.0304	-0.0243*	0.0342***	0.0205**
	(0.0217)	(0.0133)	(0.0092)	(0.0100)
Principal x 2018 x Salary Inc	-0.0267*	0.0231***	0.0051	-0.0015
	(0.0158)	(0.0081)	(0.0082)	(0.0108)
Year $t+2$:				
Principal x 2019 x No Salary Inc	-0.0099	0.0051	0.0098	-0.0050
	(0.0204)	(0.0146)	(0.0120)	(0.0145)
Principal x 2019 x Salary Inc	-0.0176	0.0096	0.0193**	-0.0114
	(0.0173)	(0.0102)	(0.0076)	(0.0122)
Covariates?	Yes	Yes	Yes	Yes
District FEs?	Yes	Yes	Yes	Yes
Observations	64652	64652	64652	64652
Mean of Dep. Var.	0.8171	0.0689	0.0542	0.0598

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. The dependent variable is a categorical variable indicating a labor market transition $T \in \{$Same School Retained, Switch Schools, Other Changes, Separated$\}$. Each panel represents a single multinomial logit model. Average marginal effect estimates from a multinomial logit model where the baseline category is Retained. Includes school district fixed effects. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Performance Pay and Labor Market Transitions: Switching to Low-Performing Schools

	(1) Switch to Low-Performing
Year $t+1$**:**	
Principal x 2018 x No Salary Inc	-0.0071
	(0.0063)
Principal x 2018 x Salary Inc	0.0056*
	(0.0031)
Year $t+2$**:**	
Principal x 2019 x No Salary Inc	0.0009
	(0.0054)
Principal x 2019 x Salary Inc	-0.0051
	(0.0057)
Covariates?	Yes
District FEs?	Yes
Observations	64652
Mean of Dep. Var.	0.0129

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. The dependent variable is an indicator of switching to a low-performing school. Average marginal effect estimates from a logit model. Includes school district fixed effects. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Performance Pay and Labor Market Transitions: Heterogeneity by Principal and School Characteristics

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separation
	Panel A: Experience by Year			
Year $t+1$:				
Principal x 2018 x Exp 1st Tercile	0.0159	0.0105	-0.0073	-0.0190
	(0.0250)	(0.0125)	(0.0108)	(0.0188)
Principal x 2018 x Exp 2nd Tercile	-0.0469**	0.0324***	0.0210**	-0.0066
	(0.0237)	(0.0096)	(0.0095)	(0.0190)
Principal x 2018 x Exp 3rd Tercile	-0.0396**	-0.0053	0.0255***	0.0194**
	(0.0182)	(0.0117)	(0.0084)	(0.0086)
Year $t+2$:				
Principal x 2019 x Exp 1st Tercile	-0.0181	0.0047	0.0122	0.0012
	(0.0218)	(0.0125)	(0.0101)	(0.0175)
Principal x 2019 x Exp 2nd Tercile	-0.0137	0.0104	0.0090	-0.0057
	(0.0224)	(0.0111)	(0.0089)	(0.0184)
Principal x 2019 x Exp 3rd Tercile	-0.0179	0.0064	0.0259**	-0.0144
	(0.0186)	(0.0111)	(0.0102)	(0.0120)
	Panel B: Persistently Low Performing Schools			
Year $t+1$:				
Principal x 2018 x Low Perf.	-0.0580***	0.0018	0.0183*	0.0380***
	(0.0203)	(0.0119)	(0.0099)	(0.0106)
Principal x 2018 x Not Low Perf.	-0.0292*	0.0173**	0.0127	-0.0007
	(0.0155)	(0.0078)	(0.0084)	(0.0086)
Year $t+2$:				
Principal x 2019 x Low Perf.	0.0308	-0.0413*	0.0194*	-0.0089
	(0.0333)	(0.0221)	(0.0110)	(0.0183)
Principal x 2019 x Not Low Perf.	-0.0258*	0.0201**	0.0160*	-0.0103
	(0.0151)	(0.0089)	(0.0083)	(0.0108)
Covariates?	Yes	Yes	Yes	Yes
District FEs?	Yes	Yes	Yes	Yes
Observations	64652	64652	64652	64652
Mean of Dep. Var.	0.8171	0.0689	0.0542	0.0598

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. The dependent variable is a categorical variable indicating a labor market transition $T \in$ {Same School Retained, Switch Schools, Other Changes, Separated}. Average marginal effect estimates from a multinomial logit model where the baseline category is Retained. Includes school district fixed effects. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Performance Pay and Labor Market Transitions: Propensity Score Matching

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separation
	Panel A: Constant Treatment Effect			
Principal x Post	-0.0339**	0.0122	0.0163	0.0054
	(0.0151)	(0.0086)	(0.0111)	(0.0075)
	Panel B: Constant Treatment Effect by Year			
Principal x 2018 ($t+1$)	-0.0422**	0.0135	0.0117	0.0170**
	(0.0187)	(0.0098)	(0.0129)	(0.0084)
Principal x 2019 ($t+2$)	-0.0252	0.0107	0.0222*	-0.0077
	(0.0183)	(0.0121)	(0.0130)	(0.0108)
	Panel C: Treatment Effect by Salary Change and Year			
Year $t+1$:				
Principal x 2018 x No Salary Inc	-0.0476*	-0.0212	0.0433***	0.0255**
	(0.0245)	(0.0168)	(0.0150)	(0.0103)
Principal x 2018 x Salary Inc	-0.0286	0.0218**	-0.0024	0.0092
	(0.0206)	(0.0102)	(0.0145)	(0.0117)
Year $t+2$:				
Principal x 2019 x No Salary Inc	-0.0311	0.0195	0.0139	-0.0023
	(0.0259)	(0.0182)	(0.0187)	(0.0139)
Principal x 2019 x Salary Inc	-0.0230	0.0093	0.0244*	-0.0107
	(0.0195)	(0.0134)	(0.0129)	(0.0133)
Covariates?	Yes	Yes	Yes	Yes
District FEs?	Yes	Yes	Yes	Yes
Observations	64652	64652	64652	64652
Mean of Dep. Var.	0.8171	0.0689	0.0542	0.0598

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. The dependent variable is a categorical variable indicating a labor market transition $T \in$ {Same School Retained, Switch Schools, Other Changes, Separated}. Each panel represents a single multinomial logit model. Average marginal effect estimates from a multinomial logit model where the baseline category is Retained. Includes school district fixed effects. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. Estimates are weighted using propensity scores described in Section 6 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Performance Pay and Labor Market Transitions: Assistant Principal Comparison

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separation
	Panel A: Constant Treatment Effect			
Principal x Post	-0.0547**	0.0096	0.0475**	-0.0024
	(0.0231)	(0.0123)	(0.0189)	(0.0068)
	Panel B: Constant Treatment Effect by Year			
Principal x 2018 ($t+1$)	-0.0588**	0.0081	0.0445**	0.0063
	(0.0260)	(0.0146)	(0.0203)	(0.0081)
Principal x 2019 ($t+2$)	-0.0504**	0.0111	0.0509**	-0.0117
	(0.0248)	(0.0138)	(0.0214)	(0.0089)
	Panel C: Treatment Effect by Salary Change and Year			
Year $t+1$:				
Principal x 2018 x No Salary Inc	-0.0846**	-0.0285	0.1020***	0.0110
	(0.0329)	(0.0216)	(0.0268)	(0.0102)
Principal x 2018 x Salary Inc	-0.0371	0.0173	0.0188	0.0011
	(0.0259)	(0.0148)	(0.0202)	(0.0090)
Year $t+2$:				
Principal x 2019 x No Salary Inc	-0.0517	0.0182	0.0462	-0.0128
	(0.0318)	(0.0222)	(0.0289)	(0.0120)
Principal x 2019 x Salary Inc	-0.0477*	0.0088	0.0496**	-0.0106
	(0.0265)	(0.0147)	(0.0214)	(0.0099)
Covariates?	Yes	Yes	Yes	Yes
District FEs?	Yes	Yes	Yes	Yes
Observations	11099	11099	11099	11099
Mean of Dep. Var.	0.7215	0.0925	0.1460	0.0399

Note: Data are from the NCERDC. The sample includes principals and assistant principals from 2014-15 to 2018-19. The dependent variable is a categorical variable indicating a labor market transition $T \in$ {Same School Retained, Switch Schools, Other Changes, Separated}. Each panel represents a single multinomial logit model. Average marginal effect estimates from a multinomial logit model where the baseline category is Retained. Includes school district fixed effects. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. Estimates are weighted using propensity scores described in Section 6 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Performance Pay and Labor Market Transitions: Within School Comparisons

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separation
	Panel A: Constant Treatment Effect			
Principal x Post	-0.0331**	0.0122*	0.0197*	0.0012
	(0.0133)	(0.0072)	(0.0108)	(0.0044)
	Panel B: Constant Treatment Effect by Year			
Principal x 2018 ($t+1$)	-0.0427***	0.0152*	0.0211**	0.0064
	(0.0134)	(0.0090)	(0.0097)	(0.0064)
Principal x 2019 ($t+2$)	-0.0236*	0.0092	0.0184*	-0.0040
	(0.0129)	(0.0089)	(0.0095)	(0.0058)
	Panel C: Treatment Effect by Salary Change and Year			
Year $t+1$:				
Principal x 2018 x No Salary Inc	-0.0503**	-0.0215*	0.0638***	0.0080
	(0.0232)	(0.0116)	(0.0182)	(0.0142)
Principal x 2018 x Salary Inc	-0.0401***	0.0296***	0.0046	0.0059
	(0.0149)	(0.0107)	(0.0104)	(0.0061)
Year $t+2$:				
Principal x 2019 x No Salary Inc	-0.0204	0.0142	0.0119	-0.0057
	(0.0221)	(0.0147)	(0.0155)	(0.0114)
Principal x 2019 x Salary Inc	-0.0260*	0.0082	0.0212**	-0.0034
	(0.0144)	(0.0100)	(0.0106)	(0.0060)
Covariates?	Yes	Yes	Yes	Yes
School FEs?	Yes	Yes	Yes	Yes
Observations	64652	64652	64652	64652
Mean of Dep. Var.	0.8171	0.0689	0.0542	0.0598

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. Each column represents a separate linear probability model. The dependent variables are indicators for the labor market transitions $T \in \{\text{Same-School Retained, Switch Schools, Other Changes, Separated}\}$. Coefficient estimates are from linear probability models. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A Appendix

Table A1: Testing the Validity of Imputed Salary in 2017-18

	(1) Actual Salary 2018
Imputed Salary 2018	1.038^{***}
	(0.188)
Observations	1215

Note: Data are from the NCERDC. The sample include full-time principals who worked in a single school during the 2016-17 school year who continue to work in the same school in 2017-18. The independent variable is a principal's actual salary in 2017-18. The dependent variable is a principal's imputed salary in 2017-18. This imputed salary is calculated using principals' characteristics in 2016-17. The coefficient estimate is from an OLS regression. The standard error estimates are clustered at the school district level.

Table A2: Baseline Multinomial Logit Model with Alternative Salary Change Definition

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separation
Year $t+1$**:**				
Principal x 2018 x No Salary Inc	-0.0303	-0.0244*	0.0344***	0.0202**
	(0.0218)	(0.0133)	(0.0092)	(0.0101)
Principal x 2018 x Salary Inc	-0.0269*	0.0231***	0.0050	-0.0013
	(0.0158)	(0.0081)	(0.0082)	(0.0108)
Year $t+2$**:**				
Principal x 2019 x No Salary Inc	0.0083	0.0037	0.0209**	-0.0330**
	(0.0237)	(0.0136)	(0.0104)	(0.0143)
Principal x 2019 x Salary Inc	-0.0336**	0.0093	0.0148*	0.0095
	(0.0168)	(0.0107)	(0.0080)	(0.0106)
Covariates?	Yes	Yes	Yes	Yes
District FEs?	Yes	Yes	Yes	Yes
Observations	64652	64652	64652	64652

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. The dependent variable is a categorical variable indicating a labor market transition $T \in$ {Same School Retained, Switch Schools, Other Changes, Separated}. Each panel represents a single multinomial logit model. Average marginal effect estimates from a multinomial logit model where the baseline category is Retained. Includes school district fixed effects. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Local School District Salary Supplements: Difference-in-Differences

	Pre-Performance Pay	Post-Performance Pay	Difference (Post-Pre)
Average Teacher Supplement	\$3917.66	\$4458.50	\$540.83*
	(147.72)	(121.5)	(211.11)
Average Principal Supplement	\$12,971.33	\$14,062.00	\$1,090.67
	(328.98)	(263)	(467.77)
			Diff-in-Diff
Difference (Principal-Teacher)	\$9,053.67***	\$9603.50***	\$549.83
	(360.63)	(289.71)	(513.21)

Note: School-district level data on average salary supplements for principals and teachers in North Carolina from the 2014-15 to 2018-19 school years come from the North Carolina Association of County Commissioners. Pre-performance pay includes the 2014-15 through 2016-17 school years. Post-performance pay includes the 2017-18 and 2018-19 school years. Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Multinomial Logit Model: No Controls

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separations
Principal	-0.0146	0.0102	0.0315***	-0.0271***
	(0.0146)	(0.0076)	(0.0055)	(0.0063)
Principal x Post	-0.0229*	0.0103*	0.0150**	-0.0024
	(0.0121)	(0.0058)	(0.0068)	(0.0072)
2017	-0.0001	0.0056	0.0002	-0.0056
	(0.0070)	(0.0040)	(0.0050)	(0.0036)
2018	0.0104	-0.0006	-0.0043	-0.0055
	(0.0068)	(0.0037)	(0.0044)	(0.0038)
2019	0.0214***	-0.0007	-0.0114**	-0.0092***
	(0.0080)	(0.0038)	(0.0045)	(0.0035)
Covariates?	No	No	No	No
District FEs?	No	No	No	No
Observations	64652	64652	64652	64652

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. The dependent variable is a categorical variable indicating a labor market transition $T \in \{$Same-School Retained, Switch Schools, Other Changes, Separated$\}$. Average marginal effect estimates from a multinomial probit model where the baseline category is Retained. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Multinomial Logit Model: All Covariates, No School District Fixed Effects

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separations
Principal	-0.0088	0.0063	0.0263***	-0.0239***
	(0.0151)	(0.0080)	(0.0061)	(0.0060)
Principal x Post	-0.0256**	0.0101*	0.0154**	0.0001
	(0.0117)	(0.0060)	(0.0067)	(0.0067)
2017	-0.0019	0.0054	0.0004	-0.0039
	(0.0068)	(0.0040)	(0.0049)	(0.0034)
2018	0.0082	-0.0006	-0.0042	-0.0034
	(0.0070)	(0.0040)	(0.0044)	(0.0035)
2019	0.0186**	-0.0010	-0.0113**	-0.0062*
	(0.0082)	(0.0040)	(0.0045)	(0.0033)
Age	-0.0029	0.0032**	0.0021	-0.0025
	(0.0022)	(0.0014)	(0.0016)	(0.0015)
Age Sq	0.0000	-0.0000**	-0.0000*	0.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Experience	0.0142***	-0.0039***	-0.0014*	-0.0088***
	(0.0017)	(0.0010)	(0.0009)	(0.0009)
Experience Sq	-0.0003***	0.0001**	0.0000*	0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Female	-0.0073*	-0.0035	0.0054**	0.0054**
	(0.0042)	(0.0026)	(0.0027)	(0.0024)
Asian	-0.0037	0.0166*	-0.0028	-0.0101
	(0.0184)	(0.0099)	(0.0108)	(0.0121)
Black	-0.0409***	0.0236***	0.0195***	-0.0022
	(0.0065)	(0.0035)	(0.0041)	(0.0029)
Hispanic	-0.0196*	0.0146*	0.0058	-0.0007
	(0.0109)	(0.0087)	(0.0085)	(0.0053)
Native American	0.0333***	0.0069	-0.0154	-0.0248**
	(0.0102)	(0.0136)	(0.0114)	(0.0124)
Other Race	0.0352	-0.0389	-0.0161	0.0199
	(0.0395)	(0.0290)	(0.0254)	(0.0218)
Sixth Yr Adv Degree	-0.0402***	0.0135**	0.0193***	0.0074
	(0.0100)	(0.0062)	(0.0055)	(0.0064)
Doctorate	-0.0223**	0.0100*	0.0121**	0.0002
	(0.0102)	(0.0056)	(0.0047)	(0.0064)
Ret Elig Early	-0.0300***	-0.0092**	-0.0043	0.0435***
	(0.0061)	(0.0040)	(0.0037)	(0.0045)
Ret Elig Full	-0.0539***	-0.0130**	0.0055	0.0614***
	(0.0074)	(0.0053)	(0.0048)	(0.0036)
Growth Not Met	-0.0167***	0.0072***	0.0041*	0.0055**
	(0.0045)	(0.0024)	(0.0024)	(0.0022)
Growth Exceeded	0.0162***	-0.0073**	-0.0077***	-0.0012
	(0.0044)	(0.0033)	(0.0024)	(0.0020)
No Growth Info	-0.0248*	0.0227**	0.0051	-0.0029
	(0.0137)	(0.0102)	(0.0075)	(0.0067)
Middle School	-0.0056	0.0022	0.0029	0.0004
	(0.0061)	(0.0044)	(0.0037)	(0.0024)
High School	0.0255***	-0.0237***	-0.0003	-0.0014
	(0.0057)	(0.0043)	(0.0040)	(0.0024)
Other Grades School	0.0109	-0.0315***	0.0139	0.0067
	(0.0169)	(0.0109)	(0.0090)	(0.0071)
New School	0.0027	0.0135	-0.0176	0.0014
	(0.0207)	(0.0085)	(0.0115)	(0.0163)
Covariates	Yes	Yes	Yes	Yes
District FEs?	No	No	No	No
Observations	64652	64652	64652	64652

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. The dependent variable is a categorical variable indicating a labor market transition $T \in \{$Same-School Retained, Switch Schools, Other Changes, Separated$\}$. Average marginal effect estimates from a multinomial logit model where the baseline category is Retained. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Multinomial Logit Model: All Covariates, With School District Fixed Effects

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separations
Principal	-0.0063	0.0061	0.0244***	-0.0242***
	(0.0153)	(0.0080)	(0.0064)	(0.0062)
Principal x Post	-0.0259**	0.0101*	0.0154**	0.0004
	(0.0117)	(0.0059)	(0.0066)	(0.0066)
2017	-0.0022	0.0049	0.0008	-0.0035
	(0.0069)	(0.0041)	(0.0049)	(0.0034)
2018	0.0075	-0.0010	-0.0037	-0.0028
	(0.0070)	(0.0039)	(0.0044)	(0.0036)
2019	0.0173**	-0.0012	-0.0106**	-0.0055
	(0.0085)	(0.0040)	(0.0046)	(0.0034)
Age	-0.0030	0.0034**	0.0022	-0.0026*
	(0.0023)	(0.0014)	(0.0015)	(0.0015)
Age Sq	0.0000	-0.0000**	-0.0000*	0.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Experience	0.0140***	-0.0040***	-0.0013	-0.0088***
	(0.0017)	(0.0011)	(0.0009)	(0.0010)
Experience Sq	-0.0003***	0.0001**	0.0000	0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Female	-0.0080*	-0.0036	0.0060**	0.0056**
	(0.0042)	(0.0025)	(0.0026)	(0.0024)
Asian	-0.0004	0.0096	0.0014	-0.0106
	(0.0164)	(0.0092)	(0.0105)	(0.0123)
Black	-0.0386***	0.0190***	0.0224***	-0.0028
	(0.0045)	(0.0033)	(0.0030)	(0.0030)
Hispanic	-0.0218**	0.0150*	0.0070	-0.0002
	(0.0103)	(0.0080)	(0.0079)	(0.0050)
Native American	0.0340**	-0.0077	-0.0101	-0.0161
	(0.0138)	(0.0130)	(0.0137)	(0.0118)
Other Races	0.0402	-0.0451	-0.0158	0.0207
	(0.0402)	(0.0293)	(0.0253)	(0.0218)
Sixth Yr Adv Degree	-0.0391***	0.0150**	0.0172***	0.0069
	(0.0096)	(0.0061)	(0.0051)	(0.0065)
Doctorate	-0.0240**	0.0114**	0.0114**	0.0012
	(0.0102)	(0.0054)	(0.0048)	(0.0065)
Ret Elig Early	-0.0290***	-0.0092**	-0.0049	0.0431***
	(0.0059)	(0.0038)	(0.0038)	(0.0042)
Ret Elig Full	-0.0531***	-0.0134**	0.0052	0.0613***
	(0.0074)	(0.0053)	(0.0048)	(0.0037)
Growth Not Met	-0.0133***	0.0035	0.0048**	0.0050**
	(0.0044)	(0.0024)	(0.0024)	(0.0023)
Growth Exceeded	0.0142***	-0.0058*	-0.0059**	-0.0025
	(0.0042)	(0.0033)	(0.0027)	(0.0022)
No Growth Info	-0.0275**	0.0245**	0.0059	-0.0030
	(0.0139)	(0.0107)	(0.0066)	(0.0068)
Middle School	-0.0054	0.0025	0.0018	0.0011
	(0.0062)	(0.0044)	(0.0036)	(0.0025)
High School	0.0230***	-0.0230***	0.0007	-0.0007
	(0.0053)	(0.0038)	(0.0039)	(0.0026)
Other Grades School	0.0156	-0.0311***	0.0073	0.0083
	(0.0157)	(0.0108)	(0.0075)	(0.0077)
New School	0.0115	0.0015	-0.0138	0.0008
	(0.0224)	(0.0091)	(0.0118)	(0.0189)
Covariates	Yes	Yes	Yes	Yes
District FEs?	Yes	Yes	Yes	Yes
Observations	64652	64652	64652	64652

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. The dependent variable is a categorical variable indicating a labor market transition $T \in \{$Same-School Retained, Switch Schools, Other Changes, Separated$\}$. Average marginal effect estimates from a multinomial logit model where the baseline category is Retained. Standard error clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Linear Probability Models: All Covariates, With School District Fixed Effects

	(1) Same-School Retention	(2) Switch Schools	(3) Other Changes	(4) Separations
Principal	-0.0177	0.0054	0.0324***	-0.0201***
	(0.0166)	(0.0086)	(0.0091)	(0.0047)
Principal x Post	-0.0323**	0.0110	0.0199*	0.0015
	(0.0129)	(0.0070)	(0.0106)	(0.0044)
2017	-0.0022	0.0049	0.0008	-0.0035
	(0.0068)	(0.0041)	(0.0048)	(0.0035)
2018	0.0071	-0.0010	-0.0031	-0.0030
	(0.0070)	(0.0039)	(0.0042)	(0.0037)
2019	0.0171**	-0.0013	-0.0101**	-0.0057*
	(0.0084)	(0.0040)	(0.0044)	(0.0034)
Age	0.0070**	0.0041***	0.0018	-0.0129***
	(0.0028)	(0.0014)	(0.0013)	(0.0022)
Age Sq	-0.0001***	-0.0000***	-0.0000**	0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Experience	0.0131***	-0.0041***	-0.0009	-0.0082***
	(0.0018)	(0.0009)	(0.0008)	(0.0013)
Experience Sq	-0.0003***	0.0001***	0.0000	0.0002***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Female	-0.0067	-0.0034	0.0062**	0.0040*
	(0.0041)	(0.0025)	(0.0026)	(0.0023)
Asian	-0.0020	0.0101	0.0021	-0.0102
	(0.0162)	(0.0105)	(0.0088)	(0.0116)
Black or African American	-0.0442***	0.0215***	0.0254***	-0.0027
	(0.0062)	(0.0041)	(0.0044)	(0.0030)
Hispanic	-0.0218**	0.0154	0.0069	-0.0004
	(0.0109)	(0.0093)	(0.0078)	(0.0052)
Native American	0.0252	-0.0100	-0.0085	-0.0067
	(0.0157)	(0.0137)	(0.0111)	(0.0069)
Other Race	0.0175	-0.0353**	-0.0116	0.0294
	(0.0369)	(0.0177)	(0.0180)	(0.0292)
Sixth Yr Adv Degree	-0.0488***	0.0156**	0.0253***	0.0079
	(0.0118)	(0.0074)	(0.0081)	(0.0074)
Doctorate	-0.0271**	0.0118**	0.0158**	-0.0005
	(0.0122)	(0.0058)	(0.0067)	(0.0076)
Ret Elig Early	-0.0044	-0.0069**	-0.0033	0.0145***
	(0.0057)	(0.0034)	(0.0034)	(0.0041)
Ret Elig Full	-0.0870***	-0.0106**	0.0030	0.0945***
	(0.0085)	(0.0042)	(0.0041)	(0.0070)
Growth Not Met	-0.0143***	0.0043	0.0049**	0.0051**
	(0.0047)	(0.0026)	(0.0025)	(0.0024)
Growth Exceeded	0.0132***	-0.0053*	-0.0054**	-0.0025
	(0.0040)	(0.0031)	(0.0026)	(0.0021)
No Growth Info	-0.0291*	0.0265**	0.0068	-0.0042
	(0.0160)	(0.0129)	(0.0078)	(0.0068)
Middle School	-0.0061	0.0026	0.0020	0.0014
	(0.0066)	(0.0049)	(0.0036)	(0.0024)
High School	0.0209***	-0.0214***	0.0010	-0.0005
	(0.0053)	(0.0040)	(0.0037)	(0.0026)
Other Grades School	0.0148	-0.0312***	0.0088	0.0076
	(0.0175)	(0.0109)	(0.0094)	(0.0081)
New School	0.0110	0.0018	-0.0138	0.0010
	(0.0224)	(0.0118)	(0.0101)	(0.0152)
Covariates	Yes	Yes	Yes	Yes
District FEs?	Yes	Yes	Yes	Yes
Observations	64652	64652	64652	64652

Note: Data are from the NCERDC. The sample includes principals and teachers from 2014-15 to 2018-19. Each column represents a separate linear probability model. The dependent variables are indicators for the labor market transitions $T \in \{\text{Same-School Retained, Switch Schools, Other Changes, Separated}\}$. Coefficient estimates are from linear probability models. Standard errors clustered at the school district level and robust to heteroskedasticity are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$