Journal of Public Economics 109 (2014) 50–63

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

# Does it pay to pay teachers more? Evidence from Texas☆

Matthew D. Hendricks

*The University of Tulsa, 800 South Tucker Drive, Tulsa, OK 74104, United States*

ARTICLE INFO

*Article history:*
Received 24 September 2012
Received in revised form 4 October 2013
Accepted 4 November 2013
Available online 12 November 2013

*JEL classification:*
I21
J31
J45

*Keywords:*
Teacher pay
Teacher turnover
Student achievement

ABSTRACT

This study presents robust evidence on the relationship between teacher pay and turnover using detailed panel data from Texas. While controlling for changes in district and local labor market characteristics, I estimate an overall turnover elasticity of −1.4 and show that the effect is largest for inexperienced teachers, declines with experience, and disappears around 19 years of experience. Combining these results with what we know about the relationship between teacher value-added and experience, I show that paying teachers more improves student achievement through higher retention rates. The results also suggest that adopting a flat salary schedule may be a cheap way to improve student performance. I find no evidence that pay effects vary by the teacher's gender or subject taught.



## 1. Introduction

Important questions that continually confront education policy makers are how much should we pay teachers and how should we shape the teacher salary schedule. Conventional wisdom suggests that paying teachers more will likely improve student outcomes by attracting and retaining better teachers or by influencing current teachers' effort choices. While the potential for a relationship between teacher pay and student achievement exists, current research fails to make a strong connection. The focus of this research is to provide evidence on this relationship so that policy makers can make better decisions regarding the level of teacher pay and the shape of the pay schedule.

Prior studies of the relationship between teacher pay and student outcomes often show null or even negative effects (Hanushek, 1997, 2003). For example, in surveys of the literature prior to 1995, Hanushek (2003,1997) reports that of 119 estimates of the relationship between teacher pay and student performance only 20% are positive and statistically significant, while 7% are negative and 73% are insignificant. A more recent study by Loeb & Page (2000) might provide the best evidence of a relationship between student performance, as measured by dropout rates and college enrollment, and teacher pay. However, even their estimates are likely biased since they cannot control for all time-varying district or state characteristics that may be correlated with changes in teacher pay.

A major reason for a lack of strong evidence in this area stems from the fact that estimating the direct causal link between district salary schedules and student achievement is challenging. First, one must control for changes in district characteristics that may be correlated with changes in student achievement and changes in the salary schedule. This is difficult even when one has access to panel data, since many time-varying district characteristics are unobserved, such as parental support and comprehensive measures of student quality. Second, a change in teacher pay today is likely to influence student outcomes in current and future periods. For example, raising teacher pay may have an immediate impact on the quality of the school, but it is not clear whether that effect will show immediately in student outcomes or if the effect will appear 10 years into the future, as Loeb & Page (2000) model it. If the positive effects of pay increases do not reveal themselves until several years into the future, the task of adequately controlling for changes in district-level characteristics in the periods between a pay increase and the time that the pay increase manifests itself becomes even more difficult.

To avoid the difficulties inherent in making a direct link between teacher pay and student achievement, this study adopts an alternative approach. I link teacher pay to teacher experience, which is known to be related to student achievement. Several studies show that experienced teachers are more productive in terms of raising student achievement in a given school year (Harris and Sass 2011; Papay and Kraft, 2011; Rockoff, 2004). Uncovering the link between teacher pay and experience is the final hurdle researchers must overcome to make a connection between teacher pay and student achievement.

☆ Thanks to Dennis Epple, Jane Lincove, Chad Settle, Aaron Sojourner, Steve Steib, the anonymous referees, and the Association for Education Finance and Policy conference participants for their helpful comments. All errors are mine.

*E-mail address:* matthew-hendricks@utulsa.edu.

0047-2727/$ – see front matter © 2013 Elsevier B.V. All rights reserved.
http://dx.doi.org/10.1016/j.jpubeco.2013.11.001

To make a connection between teacher pay and experience, I focus on how pay can be used to retain more teachers which can increase average teacher experience over time. To do so, I estimate the relationship between teacher pay and teacher turnover using a large panel data set from Texas. I find that increasing teacher pay is effective in retaining teachers who would have otherwise been replaced by relatively inexperienced teachers. The implication is that increasing teacher pay raises average teacher experience in a district.

This study is not the first to investigate the relationship between teacher pay and turnover; however, no prior study makes a clean causal connection between base pay increases and teacher turnover rates. Most prior studies in this area are likely biased because they do not control for time-varying or fixed district characteristics and labor market conditions that could be correlated with teacher pay (Dolton and von der Klaauw, 1995, 1999; Murnane et al., 1989; Murnane et al., 1990; Hanushek et al., 2004; Imazeki, 2005; Rickman and Parker, 1990; Clotfelter et al., 2011; Podgursky et al., 2004). One important empirical result presented here suggests that models that do not control for all time-varying characteristics produce overestimates of the effect of teacher pay on turnover. This may occur because improvements in district working conditions tend to coincide with increases in teacher pay.

Clotfelter et al. (2008) employ a more rigorous empirical design, which controls for time-varying school, district, and labor market characteristics. However, they estimate the effect of a bonus pay program that rewards teachers for working in low-income schools. It is not clear that the results of their study are the same as we would expect to observe for a change in base salaries in a typical school district.

Overall, the current evidence of a relationship between teacher pay and turnover is too weak to inform policy regarding changes in the salary schedule. To better inform policy, we need a study that provides more robust and detailed estimates of the base pay effect. In particular, given what we know about the relationship between student performance and teacher experience, if we uncover how pay effects vary with teacher experience, we can better understand how changes in teacher pay may be related to student performance.

This study contributes to the literature by providing the most robust and detailed estimates to date regarding the relationship between teacher pay and turnover. A large and detailed panel dataset allows this work to overcome many of the difficulties encountered by previous researchers. With this data I am able to estimate teacher pay effects while flexibly controlling for changes in district characteristics and changes in local labor markets.

I find strong evidence of a negative causal relationship between teacher pay and turnover. My estimates suggest that a 1% increase in teacher pay reduces teacher turnover by 0.16 percentage points. In terms of elasticity, this suggests that a 1% increase in teacher pay reduces the turnover rate by 1.4%. Further, this pay elasticity is largest (in absolute value) for less experienced teachers and begins to decrease rapidly after around 7–8 years of experience. The effect disappears for teachers with around 19 or more years of experience. I find no evidence that pay effects vary by the teacher's subject taught or gender.

Combining these results with our knowledge of the teacher experience–productivity profile, I show that increasing teacher pay improves student performance by retaining more teachers, which increases the average experience of teachers in the district. I also show that districts may improve student performance by adopting a flat salary schedule, but this result depends on strong assumptions about teacher selection and effort that have not been tested. In terms of size, I show small effects of paying higher teacher salaries, but I argue that these estimates are likely lower bounds on the pay effect since I focus on only the retention effects of a pay increase. Increasing teacher pay is also likely to improve student performance through mechanisms not considered in this study.

This work is organized as follows: Section 2 motivates the empirical model with a discussion of the theoretical links between teacher pay, turnover, and education quality; Section 3 describes the data; Section 4 presents the regression model and results; section 5 discusses implications of the results for district policy and student achievement; Section 6 concludes.

## 2. Conceptual framework

Increasing teacher pay is likely to affect education quality through turnover reduction (or retention), which is the focus of this study. We know that there is a dynamic component of teacher quality in that, on average, teachers improve with experience (Harris and Sass 2011; Papay and Kraft, 2011; Rockoff, 2004). Thus, retaining more teachers will help schools reap the benefit of teachers learning on the job. In addition to this retention effect, increasing teacher pay is likely to improve education quality by influencing teacher effort or by differentially attracting and retaining teachers from the high end of the fixed ability distribution.

With regard to effort, efficiency wage theory suggests that increases in teacher salaries can provide an incentive for teachers to exert more effort by increasing morale or increasing the penalty associated with effort-related job termination (Shapiro and Stiglitz, 1984; Akerlof, 1982). Pay induced increases in teacher effort could presumably lead to a number of changes in teacher behavior that are conducive to student learning, including: spending more time on lecture preparation, providing more and better student feedback, spending more time with students outside of class, or taking a more active role in mentoring students.

With regard to differential selection and retention, raising teacher pay at the district level could attract a larger pool of teacher candidates that are inherently better at teaching (high ability). This is likely to occur when high quality teachers tend to have higher earnings potential in other occupations or in other teaching jobs outside of the district. Under this condition, raising teacher pay in the district could make a teaching position in the district attractive enough to lure high ability teachers away from their high paying outside opportunities. Even if the school district is not able to identify ability in the hiring process, a random hire from the distribution of applicants after a pay raise is likely to be of higher quality than a random draw from previous distributions. The theoretical mechanism is similar for differential retention. Higher pay has the potential to retain more of the most able teachers if those teachers are more responsive to salary or if they have higher attrition rates.

This study focuses on the link from teacher pay to student achievement through its potential to retain a more experienced and productive teaching staff. The point of discussing alternative pathways is to highlight the possibility that increases in teacher pay may improve student achievement through mechanisms that are unrelated to teacher experience. Therefore, it is important to bear in mind that the teacher pay effects derived in this study are likely lower bounds on the overall impact of raising teacher pay on student achievement.

The Burdett (1978) on-the-job search model motivates this work's empirical specification of the relationship between teacher pay and turnover. In the on-the-job search model, the worker's turnover behavior is a consequence of two decisions. First, workers choose whether to search for an alternative occupation given their current wage and some knowledge of the distribution of outside job opportunities. In this phase, workers choose among three options: search for alternative employment while continuing the current occupation, quit the current occupation and search while unemployed, and do not search.

Here, the optimal strategy involves dual reservation wages which depend on the wage distribution of outside opportunities (local labor market conditions) and search costs (effort and monetary costs) (Burdett, 1978). The dual reservation wages create three decision regions. If the worker's current wage is higher than the highest reservation wage, then the optimal strategy is to forgo a search. If the worker's current wage is between the two reservation wages then the optimal strategy is to search while remaining employed. Finally, if the workers wage is lower than the lowest reservation wage, then the optimal strategy is to quit (turnover) and search while unemployed.

The intuition behind this result is simple. If your current occupation pays a high wage, then it is not optimal to engage in costly search activity as it is unlikely to result in a job offer that is more attractive than your current position. If your current job is comparable to the middle or lower end of the distribution of outside opportunities, then a search strategy is optimal as it could result in a more attractive offer. In this case, one must decide to conduct the search while remaining employed or while unemployed. The typical assumption is that conducting a search while employed is more costly (in terms of effort or direct costs) than a search undertaken while unemployed. However, if one decides to terminate employment and search while unemployed, one incurs the additional cost of lost wages. The optimal search strategy is then to search while unemployed if current wages are low, since the benefit of reduced search cost exceeds the cost of forgone wages. For higher wage earners it is better to maintain employment while searching as the cost of foregone wages exceeds the benefit of lower search costs.

The second part of turnover behavior involves the decision to accept or reject an outside wage offer, given the initial decision to search. In this phase, the worker accepts an outside job offer if it exceeds his or her expected wage next period in the current state (next period wages in the incumbent employer or unemployment benefits). The worker then repeats the same decision process next period.

Applying the on-the-job search model to teachers suggests that teacher turnover is related to current and (expected) future pay and working conditions and local labor market characteristics. Current teacher pay influences turnover decisions directly if it is so low that some teachers decide to quit and conduct a search while unemployed. Increasing current pay may also reduce turnover indirectly by decreasing the proportion of teachers choosing to conduct an on-the-job search. Alternatively, current pay may influence the teacher's effort devoted to the search in that lower pay serves as motivation for a more intensive search. Also, depending on the timing of information, teachers may initially make their search decision based on current pay and update that decision as they receive more information to project their actual teaching salary next period. In any case, increasing current teacher salaries is likely to reduce teacher turnover.

Future teacher pay enters the model when teachers who are conducting a search must decide whether or not to accept an outside offer. The reservation wage increases with the teacher's salary next period or, depending on the timing of an offer, the expected value of future wages. It is not clear whether teachers conducting a search typically receive offers before or after they have knowledge of their future pay in the current teaching position. Therefore, offers may be accepted or rejected based on actual future pay or an expectation formed by the teacher given information at the time of the offer. Whether or not teachers know their future pay or form an expectation, future pay measures are likely to be negatively related to turnover.

Local labor market conditions enter the model by influencing the teachers' knowledge of the distribution of outside offers. As local labor markets thrive, teachers are more inclined to conduct a search (holding teacher pay and working conditions constant). Similarly, as outside school districts increase their salary schedules or improve their working conditions, teachers in the incumbent district are likely to attempt to transfer.

Overall, the model suggests that holding district characteristics and outside opportunities constant, increases in current and future teacher pay will reduce teacher turnover rates. Better pay and working conditions in outside opportunities, including other school districts, likely increases teacher turnover. Translating these results to an empirical model is straightforward in the case of teacher pay, since it is observable in the data. However, measures of expected future pay are not. To measure expected future pay in the empirics, I use future pay on the current salary schedule and measures of actual future pay given that the teacher remains in the school district, since this is likely the information teachers use to form their expectation.

I control for district characteristics and outside opportunities using a large number of fixed-effects. District-by-year fixed-effects account for district level changes in student quality, parental support, and outside opportunities. I also include district-by-experience fixed-effects, which control for (time invariant) personnel practices within the district that vary by experience, such as assigning less experienced teachers to lower performing schools or undesirable classrooms. Finally, I include experience-by-year fixed-effects, which control for statewide changes in outside opportunities for teachers with a given experience level. Implicit in these fixed-effects are controls for average differences in turnover rates by year, district, and experience. In addition to fixed-effects, I control for a teacher's outside opportunities in other school districts with teacher pay measures from surrounding school districts.

The model also suggests that prior teacher pay does not influence turnover decisions. Given this result, I use these irrelevant pay measures as proxies (or placebos) to test for the presence of unobservables that may cause bias in the teacher pay estimates.

## 3. Data

This study employs administrative data from the Texas Education Agency (TEA) which cover the years 1996–2012. The data is a teacher-level longitudinal file containing over 5 million teacher-by-year observations that identify teacher experience, degree, full-time equivalence (FTE), base pay, district affiliation, subject taught, and gender.

From this data, I construct teacher turnover rates for teachers with a bachelor's degree that vary by district, year, and experience. To create this turnover measure, I adopt the following definition of the turnover rate for teachers with experience $e$ in district $j$ during year $t$ ($\text{turnover}_{jte}$): $\text{turnover}_{jte}$ is the percent of full-time equivalent teachers employed by district $j$ in year $t$ with experience $e$ that are no longer employed by district $j$ in year $(t+1)$. This turnover definition does not distinguish between different types of turnover, such as teachers leaving the district for a different district or leaving the profession completely or temporarily taking a leave of absence. This definition of turnover simply measures the percentage of teachers who leave a district for any reason.

Naturally, this definition of turnover does not allow one to observe turnover rates in 2012, so the sample used in the analysis is restricted to years 1996–2011. Also, in order to observe a turnover rate in a district-by-year-by-experience cell, there must be at least one teacher observation within the cell. I restrict the analysis to districts that have at least one full-time teacher in each of the 336 year-by-experience cells defined by the years 1996–2011 (16 years) and experience levels 0–20 (21 experience levels). There are 165 districts in the sample that employ at least one full-time teacher in each of these 336 experience-by-year cells, and therefore satisfy this restriction. The sample that I analyze thus contains 55,440 district-by-year-by-experience observations of turnover rates.

Using the individual teacher pay data, I also reconstruct each district's salary schedule for teachers holding a bachelor's degree over the years 1996–2011. To do so, I collapse teacher base salary to 55,440 district-by-year-by-experience observations of the median base salary of full-time teachers employed within the cell.[1]

Table 1 gives an overview of the data by providing within experience averages of turnover rates, base pay, and the number of full-time equivalent teachers across the 165 districts and 16 years. The table reiterates some well documented characteristics of the teacher labor force. First, turnover rates are highest among less experienced teachers and

[1] In some districts there is variation in base teacher pay within a district-year-experience cell. However variation within a cell is small. Most within cell standard deviations are less than $1000. This variation is likely a result of additional base pay awarded to teachers with additional college credits, teachers with coaching responsibilities, or teachers with other additional duties.

**Table 1**
Descriptive statistics.

| Experience | (1) Turnover (%) | (2) Base pay | (3) Teachers (FTE) |
|---|---|---|---|
| 0 | 20.9 | 32,277.5 | 70.7 |
| 1 | 18.9 | 32,706.5 | 72.1 |
| 2 | 18.6 | 33,148.2 | 68.2 |
| 3 | 16.6 | 33,591.6 | 62.1 |
| 4 | 15.6 | 34,126.7 | 55.6 |
| 5 | 14.8 | 34,724.4 | 49.5 |
| 6 | 13.6 | 35,356.4 | 44.1 |
| 7 | 13.0 | 36,014.9 | 39.5 |
| 8 | 11.9 | 36,728.6 | 35.7 |
| 9 | 11.7 | 37,463.5 | 32.5 |
| 10 | 10.5 | 38,266.8 | 30.0 |
| 11 | 9.9 | 39,070.0 | 27.8 |
| 12 | 9.6 | 39,847.7 | 25.7 |
| 13 | 8.8 | 40,611.6 | 23.9 |
| 14 | 8.5 | 41,368.1 | 22.4 |
| 15 | 8.2 | 42,128.0 | 21.0 |
| 16 | 7.9 | 42,871.1 | 19.9 |
| 17 | 7.9 | 43,591.6 | 18.8 |
| 18 | 7.3 | 44,315.9 | 17.8 |
| 19 | 8.2 | 45,010.8 | 16.7 |
| 20 | 8.4 | 45,685.7 | 15.8 |
| **7.1** | **14.0** | **36,488.3** | **769.5** |

N = 2640 district-by-year observations (165 districts across 16 years: 1996–2011). The table reports mean turnover rates, base pay, and number of teachers across districts and years and by experience level. The sample is restricted to teachers who hold a bachelor's degree. Column 3 reports the average number of full-time equivalent teachers (FTE). The bold numbers at the bottom of the table report weighted averages of experience, turnover rates, and base pay, with the number of full-time teachers in an experience cell (column 3) used as weights. The bold number at the bottom of column 3 reports the sum of the number of teachers across experience cells.

progressively decline until about 18 years of experience. After 18 years of experience, the turnover rates begin to increase, which is likely a result of retirement decisions. Second, teacher salary increases with each year of experience gained. This is a well-known characteristic of the single salary pay schedule employed by nearly all public school districts. Finally, the distribution of teacher experience is skewed toward lower experience levels (positively skewed). The average district in the sample employs about 770 teachers with a bachelor's degree and between 0 and 20 years of experience (column 3, last row). Of these 770 teachers, about 70% have fewer than 10 years of experience. The average teacher in the sample has 7.1 years of experience, has a 14% probability of leaving the district next year, and earns a base salary of about $36,500 (bold numbers in the last row).

Figs. 1 and 2 show part of the salary schedules (experience = 0, 5, 10, and 20) for the two largest school districts in the sample, Houston and Dallas, and the two smallest districts in the sample, Llano and Brownsboro. The graphs give a sense of the within-district variation in pay schedules for the 165 districts in the sample. Each of the graphs illustrates the type of base pay variation that I use to identify the effect of teacher pay on turnover. In the empirical analysis I exploit differential variation in teacher pay within a district and across experience cells. For example between 1996 and 1997, Dallas Independent School District (ISD) increased the base pay it awards to teachers with 5 years of experience. Over the same period, Dallas ISD decreased pay for teachers with 10 and 20 years of experience and held starting pay constant. Differences in experience-specific base pay changes like this are evident in nearly every time period and district shown in Figs. 1 and 2.

I exploit this variation to identify the effect of base pay on turnover in a difference-in-difference-in-difference-in-difference (4-diff) regression framework. For example, if base pay has a negative causal effect on turnover then one would expect that since Dallas ISD increased pay for teachers with 5 years of experience and at the same time it reduced pay for teachers with 10 years of experience, then the turnover rate for teachers with 5 years of experience in Dallas ISD should decrease over this period relative to the turnover rate for teachers with 10 years of experience. That alone is a difference-in-difference (diff-in-diff) design since it compares the difference in turnover rates across experience groups before and after a relative pay change. My identification strategy compares this diff-in-diff to the same diff-in-diff in other districts that did not impose the same relative change in teacher pay, hence the 4-diff design.

In this context, the diff-in-diff estimator is a powerful tool with which one can estimate the effect of teacher pay on turnover because it is not subject to bias from time-varying district-specific factors (district-by-year effects) nor is it subject to bias from factors that are experience specific and do not vary over time (district-by-experience effects). The major advantage of the 4-diff design over the diff-in-diff

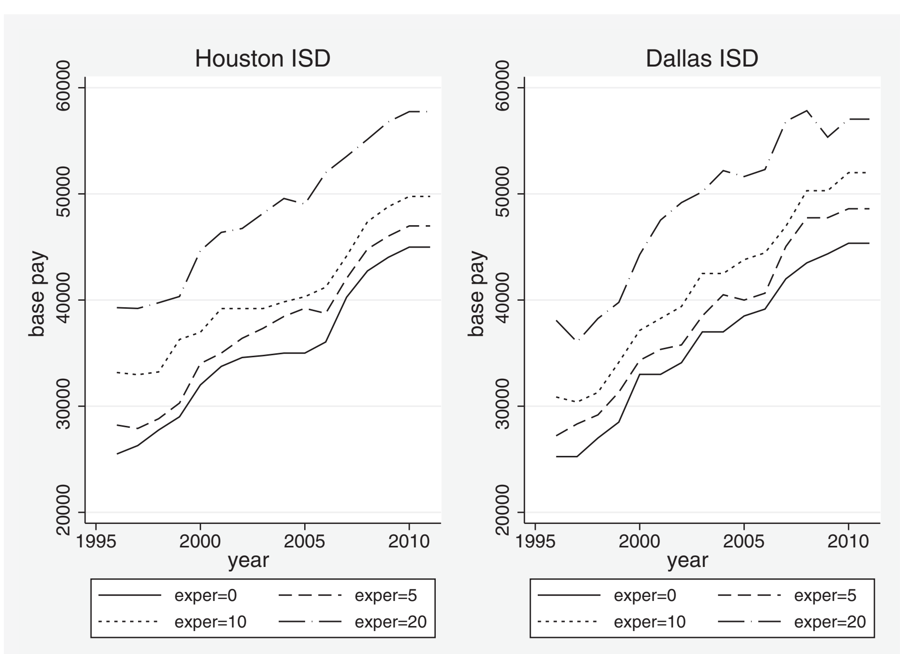


**Fig. 1.** Salary schedules 1996–2011: largest districts.

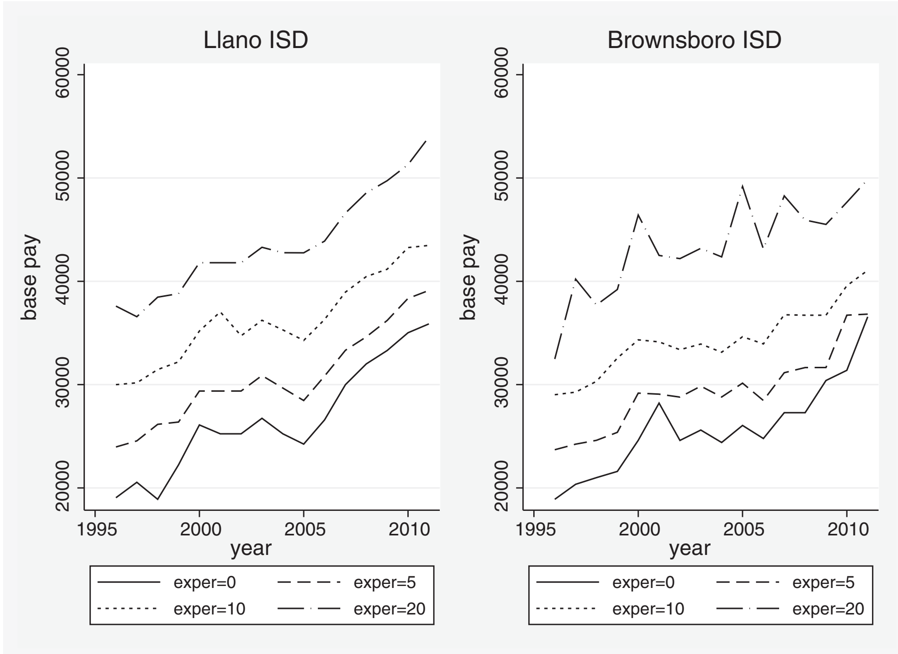


**Fig. 2.** Salary schedules 1996–2011: smallest districts.

design, however, is that the 4-diff design, in addition to being robust to these factors, is robust to experience-specific and time-varying factors that are common across districts (experience-by-year effects). For example, if a state were to change its teacher tenure policy from 3 years to 4 years of service required for eligibility, this change would likely bias a diff-in-diff estimator, while the 4-diff design would remain robust.

Fig. 3 operationalizes this 4-diff design for changes over the years 1996–1997 and for experience levels 4 and 1. The diff-in-diff in turnover rates for teachers with 4 years of experience and 1 year of experience is reported on the vertical axis. On the horizontal axis is the diff-in-diff in base pay for teachers with 4 years and 1 year of experience. The points on the graph correspond to these values for the 165 districts in the sample. Before examining the results, first note that there is substantial variation across districts in terms of their relative changes in base pay across experience groups between 1996 and 1997. Some districts increased pay for teachers with 4 years of experience relative to newer teachers by as much as \$1000. Most districts seem to have made their salary schedules flatter during this period by increasing pay for teachers

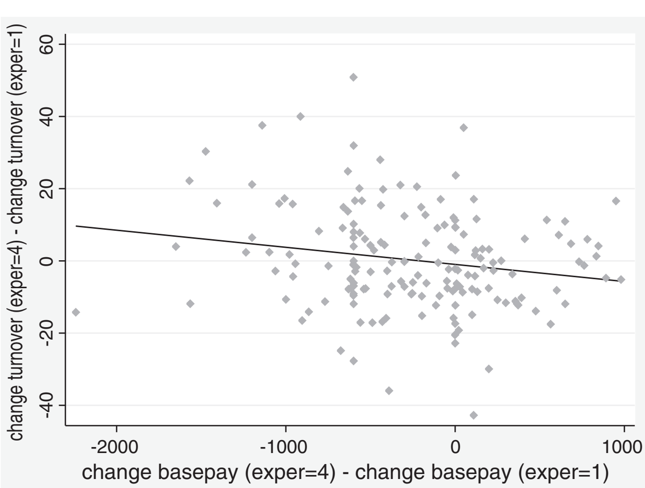


**Fig. 3.** 4-diff 1996–1997.

with 1 year of experience relative to teachers with 4 years of experience. One district closed the gap by more than \$2000.

The regression line in Fig. 3, the slope of which is a 4-diff estimator, provides this study's first evidence of a negative causal relationship between base pay and teacher turnover. The negative slope, which is significant at the 5% level, suggests that when districts increase pay for one experience group relative to another, the result is a decrease in that group's relative turnover rate. Of course, this evidence comes from only 2 years and 2 experience groups. In the formal analysis that follows, I employ a more comprehensive regression-based 4-diff method that uses all 16 years and 21 experience groups and incorporates additional controls.

## 4. Regression specification and estimates

The model I intend to estimate is the following:

$$T_{jte} = \beta' \mathbf{x}_{jte} + \theta_{jt} + \alpha_{et} + \gamma_{je} + \varepsilon_{jte} \tag{1}$$

The dependent variable ($T_{jte}$) represents a district $j$ by year $t$ by experience $e$ turnover rate. The vector $\mathbf{x}_{jte}$ represents observed covariates that vary by district and year and experience level, including current and future district base salary, and current and future base teacher salary in other surrounding districts. In some specifications, I include prior base salaries to test for potential bias in the teacher pay estimates. I discuss the specifics of this test below.

The parameters $\theta_{jt}$, $\alpha_{et}$, and $\gamma_{je}$ are, respectively, district-by-year, experience-by-year, and district-by-experience fixed-effects. The district-by-year effect accounts for time-varying district characteristics that have a common effect across all experience levels, such as changes in average student quality or changes in the local labor market that affect teachers of all experience levels. The experience-by-year effect accounts for time varying and experience-specific factors that are common across all districts, such as changes in state tenure laws or changes in state-level labor market opportunities available to less experienced teachers. The district-by-experience effect captures experience-specific factors that vary across districts but are fixed over time. These include fixed-differences across districts in terms of personnel policies and local labor market opportunities. The error term ($\varepsilon_{jte}$) contains unobserved factors that vary by district, year, and experience.

I estimate the parameter vector $\beta$ by ordinary least squares (OLS). All standard errors are clustered at the district level, which produces larger estimates of the variance of the OLS estimators relative to clustering by experience.

The teacher pay coefficients in $\beta$ are assumed to represent linear approximations of the causal effect of teacher pay on teacher turnover. OLS estimation of Eq. 1 provides a consistent estimator for this effect assuming that unobservables that vary by district, year, and experience are not correlated with teacher pay. That is, the teacher pay estimators in Eq. 1 are only subject to bias if there are unobserved changes at the district level that are experience-specific and those changes are correlated with within-district experience-specific changes in teacher pay. As an example, this type of correlation would arise if school districts adopt a policy of assigning less experienced teachers to less desirable classrooms and at the same time those districts reduce pay for less experienced teachers.

The strategy used to identify teacher pay effects in this study, as specified in Eq. 1, can be interpreted as a 4-diff estimator. It is a 4-diff estimator because the pay effect is identified by comparing two diff-in-diff estimates. The first diff-in-diff is the change in turnover rates for two experience groups in a district that imposed experience-specific and differential changes to its pay schedule (diff-in-diff). This diff-in-diff is compared to the same diff-in-diff in another district that did not impose the same experience-specific and differential changes to its pay schedule. The simplest example of this strategy is the comparison of changes in turnover rates for novice and veteran teachers in a district that only increased pay for veteran teachers to the same changes in turnover rates in another district that did not change its pay schedule.

I begin the analysis by presenting baseline estimates of Eq. 1 (my preferred specification) and results from alternative specifications. Later, I test for any remaining sources of bias using a placebo (or proxy) test. Finally, I explore whether the marginal effect of teacher pay on turnover varies by the teacher's experience, subject taught, or gender.

The baseline results suggest that a 1% increase in a teacher's current pay ($\text{basepay}_{jte}$) and the teacher's future pay in the current contract ($\text{basepay}_{jt,e+1}$) would reduce teacher turnover by 0.16 percentage points (Table 2, column 5). A 1% increase in the teacher's current salary, while holding future salary fixed, is associated with 0.1 percentage point reduction in teacher turnover, while the same effect for future salary is cut to 0.06 percentage points (Table 2, column 5). The baseline estimates also suggest that teacher pay in the nearest other district in the sample[2] ($\text{basepay}_{ote}$) does not have a statistically significant effect on turnover.

Aside from the baseline results in column 5, Table 2 shows how the estimates vary with specifications that include fewer fixed-effects (columns 1–4). These results suggest that models that fail to control for time-varying local labor market and district characteristics will likely overestimate the effect of teacher pay. In particular, if one compares the traditional fixed-effects model in column 1 to a more robust specification in column 4, which controls for time varying district and local labor market characteristics through district-by-year fixed-effects, one can see that the combined teacher pay effects decrease from 0.24 percentage points to 0.22.

Controlling for experience-by-year fixed-effects, which may account for differences across time in experience specific labor market conditions across the state or changes in state policies, also appears to be important. If one compares the results in column 1 to column 2, one can see that adding experience-by-year fixed-effects again reduces the teacher pay effect estimates.

The addition of district-by-experience fixed-effects, which controls for time-invariant labor market conditions or time-invariant district personnel policies appear to be less important if one is concerned about overestimating teacher pay effects. After controlling for experience-by-district fixed-effects (comparing column 1 to column 3), the combined teacher pay effects increase. Overall, if one accepts the baseline results in column 5 as the true teacher pay effects, then these results suggest that a typical fixed-effects model,

**Table 2**
Turnover and teacher pay, baseline estimates.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $\text{basepay}_{jte}$ | −11.51*** | −10.19*** | −14.77*** | −10.45*** | −9.95** |
| | (2.976) | (3.047) | (3.242) | (3.144) | (3.922) |
| $\text{basepay}_{jt,e+1}$ | −12.85*** | −12.92*** | −10.44*** | −11.27*** | −6.134* |
| | (3.324) | (3.474) | (3.407) | (3.249) | (3.160) |
| $\text{basepay}_{ote}$ | 1.766 | 3.385 | 1.639 | −0.736 | 2.077 |
| | (2.127) | (2.507) | (2.486) | (1.954) | (2.612) |
| Combined effect | −24.36*** | −23.11*** | −25.21*** | −21.72*** | −16.08*** |
| R-squared | 0.238 | 0.244 | 0.290 | 0.300 | 0.358 |
| District FEs | Yes | Yes | Yes | Yes | Yes |
| Year FEs | Yes | Yes | Yes | Yes | Yes |
| Experience FEs | Yes | Yes | Yes | Yes | Yes |
| Exper. by year FEs | No | Yes | No | No | Yes |
| Dist. by Exper. FEs | No | No | Yes | No | Yes |
| Dist. by year FEs | No | No | No | Yes | Yes |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, reported in parentheses, are clustered at the district level. Teacher turnover rate for teachers with a bachelor's degree and experience $e$ in district $j$ during year $t$ is the dependent variable. $\text{basepay}_{jte}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$,$\text{basepay}_{jt,e+1}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e+1$ years of experience in year $t$, $\text{basepay}_{ote}$ is the natural log of teacher contract salary in the district nearest to district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$. All models include 52,800 district-by-year experience observations: 165 districts, 16 years (1996–2011), and 20 experience levels (0–19). The combined effect is the sum of the coefficients on $\text{basepay}_{jte}$ and $\text{basepay}_{jt,e+1}$.

**Table 3**
Alternative specification and sample restrictions.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $\text{basepay}_{jte}$ | −16.64*** | −12.66** | −10.04** | −8.917* | −12.34*** |
| | (5.182) | (5.400) | (4.671) | (5.102) | (4.643) |
| $\text{basepay}_{jt,e+1}$ | −7.070** | −5.639 | −4.581 | −11.52*** | −6.588* |
| | (3.362) | (3.571) | (3.895) | (3.036) | (3.739) |
| $\text{basepay}_{j,t+1,e+1}$ | 11.04** | 4.423 | 1.332 | 6.102 | |
| | (5.228) | (4.488) | (4.658) | (4.858) | |
| $\text{basepay}_{ote}$ | 9.119** | 7.216* | 6.029 | 6.717* | 3.247 |
| | (3.796) | (3.656) | (4.113) | (4.003) | (3.086) |
| $\text{basepay}_{o,t+1,e+1}$ | −11.03*** | −5.119 | −3.747 | −6.196 | |
| | (4.046) | (4.132) | (4.918) | (4.785) | |
| Combined effect | −23.71*** | −18.30*** | −14.62*** | −20.44*** | −18.92*** |
| Cell size restriction | None | min ≥3 | min ≥5 | min ≥7 | None |
| Year restriction | None | None | None | None | 1996–06 |
| Observations | 49,500 | 26,100 | 18,000 | 14,400 | 36,300 |
| Number of districts | 165 | 87 | 60 | 48 | 165 |
| R-squared | 0.360 | 0.501 | 0.585 | 0.630 | 0.377 |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, reported in parentheses, are clustered at the district level. Teacher turnover rate for teachers with a bachelor's degree and experience $e$ in district $j$ during year $t$ is the dependent variable. $\text{basepay}_{jte}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$. $\text{basepay}_{ote}$ is the natural log of teacher contract salary in the district nearest to district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$. The combined effect is the sum of the coefficients on $\text{basepay}_{jte}$ and $\text{basepay}_{jt,e+1}$. All models include district-by-year FEs, experience-by-year FEs, and experience-by-district FEs. Estimates in columns 2–4 are estimated on samples of districts that meet the indicated minimum number of teachers employed within a district-year-experience cell. The results in column 5 are from a restricted sample that includes only the sample years prior to Texas' statewide performance pay initiatives: 1996–2006.

[2] Distance between district $j$ and the nearest outside district in the sample was calculated using latitude and longitude coordinates for each district. Latitude and longitude coordinates are available in the Common Core of Data maintained by the National Center for Education Statistics. Each of the 165 districts in the sample has a neighboring district in the sample that is within 82 miles. The average distance between a district and its nearest neighbor in the sample is 14 miles (standard deviation is 14.5 miles).

such as that in column 1, is biased and produces overestimates of the teacher pay effect by nearly 50%.

An alternative explanation of the decreased effect size after introducing district-by-year, experience-by-year, and district-by-experience fixed effects involves differential impacts of measurement error. It is well known that measurement error in an explanatory variable tends to bias regression coefficients toward zero. In this case, noise in the district salary schedule measures (median pay within a district-year-experience cell) likely biases all of the models in Table 2 toward zero. In addition, it is a widely held view that including more fixed effects in a regression model exacerbates this attenuation bias (Griliches and Hausman, 1986). Given this result, differential attenuation bias offers a potential alternative explanation for the decrease in pay effects in column 5 relative to column 1.

An obvious way to test this explanation is to reduce or eliminate measurement error in the teacher pay variables and reestimate the baseline models. If measurement error explains the difference in estimates in columns 5 and 1, then as measurement error in teacher pay is reduced we should observe two trends: both model estimates should move away from zero (become negative numbers with larger absolute values) and the estimates from the two models should converge.

The estimates in Appendix A Table 8 provide a practical application of this test. The table reports the results of the models in columns 5 and 1 with successive restrictions on the sample used in the estimation. Row 1 redisplays the baseline combined effects from Table 2 columns 1 and 5. Each successive row adds a restriction on the minimum number of teachers employed by a district within an experience-year cell. For example, in row 2 the sample is restricted to the 60 school districts that employed at least 5 teachers in each experience-year cell over the period 1996–2011. In row 3, the sample is restricted to the 40 districts that employed at least 10 teachers in each experience-year cell. The benefit of these restrictions is that they likely successively reduce error in measures of the teacher pay schedule.

Visual evidence of this is present in Figs. 1 and 2. Notice that the larger district salary schedules show relatively smooth transitions over time when compared to the smaller districts in the sample, which tend to oscillate around a trend. This oscillation is likely noise in the teacher pay measures, and this error appears to decrease as the sample is restricted to larger districts.

The results in Appendix A Table 8 provide no support for the measurement error explanation of the gap between the estimates in columns 1 and 5 of Table 2. As measurement error is reduced (looking down rows 1 and 2 of Appendix A Table 8) the combined effect estimates neither move away from zero nor converge. If anything, the results suggest the opposite trends. This evidence lends support for the initial interpretation of the gap. The addition of district-by-year, experience-by-year, and district-by-experience fixed-effects control for important unobservables that would otherwise bias traditional fixed-effects estimates by at least 50%.

Table 3 presents results from alternative specifications and restrictions on the sample used in the analysis. In columns 1–4, I add the teacher's actual pay next period if she remains in the district ($\text{basepay}_{j,t+1,e+1}$) and future pay in the outside district ($\text{basepay}_{o,t+1,e+1}$) to the baseline model. Theoretically, this is an important addition to the empirical model because in a world of perfect information one would expect teachers to base their turnover decisions entirely on next period salaries. In this case, cells on the current salary schedule (base pay at time $t$) would not enter the turnover decision. Each of the forward pay measures is intended to better measure the information with which teachers form their expectation of future teacher pay and make turnover decisions.

The estimates in column 1 are produced using the full sample of 165 districts. The estimates in columns 2–4 impose progressively stronger restrictions on the sample of districts used in the estimation. For example, in column 2, the sample is restricted to the 87 districts that employ at least 3 teachers in each experience-year cell. In column 3, the sample is restricted to the 60 districts that employ at least 5 teachers in each experience-year cell.

The results in columns 1–4 of Table 3 suggest that the baseline model is not sensitive to the addition of future salary measures in the current or outside districts. In column 1 the future pay measures are statistically significant and have an unexpected sign. However, when the sample is restricted to larger districts, which reduces noise in the base pay measures, the future pay measures become statistically indistinguishable from zero. This suggests that the statistical significance of the future pay measures in the full-sample may not be a real effect but perhaps instability caused by collinearity. While the future pay measures are not significant in these alternative samples, the pay measures used in the baseline model are stable and statistically significant across sample restrictions.

These results are consistent with an environment in which teachers make turnover decisions with imperfect information regarding their future teaching salary. With perfect information, turnover decisions would be based entirely on actual teaching salary next period given that he or she remains in the district: $\text{basepay}_{j,t+1,e+1}$. However, the data tells a different story. Teacher turnover appears unrelated to future teaching salary while it is negatively and strongly related to pay on the current salary schedule: $\text{basepay}_{jte}$ and $\text{basepay}_{jt,e+1}$. The data therefore suggests that teachers make turnover decisions with scarce information regarding the future salary schedule.

This result is not surprising after a closer examination of how teachers are paid in Texas. First, Texas teacher salary schedules are not collectively bargained.[3] Rather, the salary schedule is determined primarily by the district superintendent and locally elected school board, and it must adhere to a statewide minimum salary schedule.[4] Second, in a typical district, next year's salary schedule is not approved by the board until late June, which is just prior to the July 1st start of the next fiscal year.[5] Given that complete information regarding the future salary schedule arrives after the school year has completed, and only 2 months prior to the start of the new school year, it is unsurprising that the empirical results show that teachers conduct and conclude their job search relying primarily on information contained in the current salary schedule.

The alternative specification in column 5 of Table 3 is the baseline model estimated on the subsample of years before the 2006–07 school year. Starting in the 2006–07 school year, several districts in the sample began receiving state funding for performance pay programs as part of the Governor's Educator Excellence Grant (GEEG) program, the Texas Educator Excellence Grant (TEEG) program, and the District Awards for Teacher Excellence (DATE) program. Although this study focuses on base teacher salary, there is concern that districts participating in the performance pay programs may alter base salaries in ways that may impact the baseline model estimates. The motivation behind the specification in column 5 of Table 3 is to avoid confounding the baseline estimates with the emergence of performance pay programs by restricting the sample to the years prior to GEEG, TEEG, and DATE. The results show a larger combined effect for the years prior to the state-sponsored performance pay programs: a 0.19 percentage point decrease in turnover for a 1% increase in the salary schedule versus a 0.16 percentage point effect in the baseline model. This result suggests that the emergence of performance pay programs in 2007 did not significantly alter the impact of base pay on teacher turnover. If anything, performance pay programs appear to dampen the base pay effect.

In addition to the alternative specifications presented in Table 3, I estimated a number of other alternative specifications that are not shown here, but are available on request. I tested whether alternative outside

[3] In Texas, collective bargaining is prohibited by public school teachers: Texas government code 617.002.

[4] See Texas education code 21.402 for the legal framework governing the minimum salary schedule.

[5] Texas education code 44.0011.

**Table 4**
Turnover and teacher pay, placebo/proxy test.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $basepay_{jte}$ | −9.651** | −10.66** | −10.04 | −8.912 | −8.454** |
| | (4.449) | (4.269) | (6.151) | (6.437) | (4.050) |
| $basepay_{j,t,e+1}$ | −6.360** | −9.222** | −5.338 | −8.700* | −7.133** |
| | (3.037) | (4.614) | (3.340) | (4.640) | (3.062) |
| $basepay_{ote}$ | 2.543 | 3.848 | 4.723 | 4.677 | 6.375** |
| | (2.724) | (2.866) | (2.958) | (2.987) | (2.619) |
| $basepay_{jt,e\text{-}1}$ | 1.029 | | | −0.489 | −0.143 |
| | (3.038) | | | (3.252) | (2.727) |
| $basepay_{j,t\text{-}1,e}$ | | 6.095 | | 5.073 | 2.756 |
| | | (4.246) | | (5.150) | (3.319) |
| $basepay_{j,t\text{-}1,e\text{-}1}$ | | | 1.599 | −0.397 | 2.391 |
| | | | (4.812) | (5.100) | (3.447) |
| Combined effect | −16.01*** | −19.88*** | −15.37** | −17.61** | −15.59*** |
| F test on placebos | | | | 0.77 | 0.54 |
| Observations | 50,160 | 49,500 | 47,025 | 47,025 | 47,025 |
| R-squared | 0.333 | 0.361 | 0.335 | 0.335 | 0.462 |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, reported in parentheses, are clustered at the district level. Teacher turnover rate for teachers with a bachelor's degree and experience $e$ in district titj during year $t$ is the dependent variable. $basepay_{jte}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$. $basepay_{ote}$ is the natural log of teacher contract salary in the district nearest to district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$. The combined effect is the sum of the coefficients on $basepay_{jte}$ and $basepay_{jt,e+1}$. The row labeled F test on placebos reports the p-value from the joint test that all placebos ($basepay_{jt,e\text{-}1}$, $basepay_{j,t\text{-}1,e}$, and $basepay_{j,t\text{-}1,e\text{-}1}$) are insignificant. All models include district-by-year FEs, experience-by-year FEs, and experience-by-district FEs. The specification in column 5 is weighted by the number of teachers employed by the district in each year.

district identifiers mattered by using the average pay in districts within a 100-mile radius as the outside pay measure. These models produced insignificant results and less precise estimators of the outside pay effects. I estimated models that use the mode rather than the median of teacher pay within an experience-year cell to measure the salary schedule. These models produced nearly identical results when one imposes additional restrictions to the sample. When the number of teachers in a district-year-experience cell is small, the nonexistence of a mode is troublesome, but when the sample is restricted to large districts, the results using the mode and median largely coincide. Appendix A Table 10 shows that the baseline results are not overly sensitive to either cell-size weights (number of teachers in a district-year-experience cell) or district-year weights (number of teachers employed by the district in the given year). Finally, Appendix A Table 11 shows that the pay effects persist when the sample is restricted to only tenured teachers (teachers with three or more years of experience).

The on-the-job search model presented above suggests that prior cells on a salary schedule do not causally affect current teacher turnover decisions. As such, prior pay cells cannot be related to current teacher turnover rates unless they are partially correlated with remaining time, experience, and district varying unobservables. If one assumes that prior cells on the salary schedule have a partial correlation with unobservables that is equal to the partial correlation between $basepay_{jte}$ and unobservables and $basepay_{jt,e+1}$ and unobservables, then the coefficients on prior salary cells provide a test for remaining endogeneity in the baseline estimates (Hendricks, 2013). This test has been labeled a placebo test and is a special case of the proxy-based bounding procedure outlined in Hendricks (2013). If the prior cells on the salary schedule (the placebos) have statistically significant impacts when added to the baseline model, then that suggests that the baseline model is subject to remaining bias.

Adding prior pay measures to the model may also provide an additional control for changes in teacher composition within a district-year-experience cell.[6] If prior pay is low or high, it will affect prior turnover rates and potentially the composition of teachers who remain in the current year in that they may differ systematically in their current-year turnover propensity. As such, prior pay measures may influence turnover rates in the current period. Therefore, adding them to the model also provides a means to test and control for prior changes in the teacher pool.

[6] I thank an anonymous referee for this insight.

In Table 4, I add three prior pay measures to the baseline model: $basepay_{jt,e\text{-}1}$, $basepay_{j,t\text{-}1,e}$, and $basepay_{j,t\text{-}1,e\text{-}1}$. When individually added to the baseline model (Table 4, columns 1–3), each is statistically insignificant at conventional levels. When all three placebos are added to the baseline model, they are jointly and individually insignificant (Table 4, column 4) and remain so when the model is estimated with district weights (Table 4, column 5). These results suggest that the baseline estimates are not subject to bias from arbitrary omitted variables that are correlated with both current and prior teacher pay. Also, there is no evidence that the baseline model is biased by prior retention of a teacher pool that is more or less likely to turnover in the current period.

A few other potential sources of bias in the teacher pay estimates are worth mentioning. First, district pay schedules may be determined in part by anticipated experience-specific teacher turnover. For example, district officials may anticipate increased turnover among novice teachers in the future and respond by raising novice teacher pay. This type of reverse causality is not a major threat to the validity of the results presented here, because it likely biases the results toward finding a smaller relationship between teacher pay and turnover. Future salaries may be particularly prone to this bias since district officials may set future salaries in part based on current turnover rates. This could explain the null effect of future salaries in the alternative specifications reported in Table 3.

In states where teacher salaries are collectively bargained, there is a potentially more serious threat to validity on the other side of teacher pay negotiations. Larger groups of teachers within an experience-year cell are likely more influential in salary negotiations. If these teachers are also more committed to the district and use their political influence to raise their own salaries, this could introduce a spurious negative relationship between teacher pay and turnover. However, as mentioned earlier, teacher salaries are not collectively bargained in Texas, and teachers typically do not play any role in the adoption of a new salary schedule. As a result, this type of endogenous salary determination is not likely to exist in Texas. Nevertheless, I test for this in Appendix A Table 12 by adding a covariate measuring the fraction of teachers employed in an experience-year cell relative to total employment in the district-year. The addition of this measure of political influence does little to change the baseline results, and the variable itself is statistically insignificant.

A final concern is that the median salary (within an experience cell) may not accurately measure the district salary schedule and may instead measure changes in teacher composition. For example, the median within-experience teacher pay in a district may increase even if the district did not change its salary schedule. This can occur if most of the new teachers in the cell receive supplemental base pay for additional college credits, coaching, or other additional duties. These teachers may also be more attached to the district and less likely to turnover. This could cause a spurious correlation between pay and turnover by making it appear as though higher pay reduces turnover when in fact the negative observed relationship is instead a result of a change in teacher composition.

The estimates in Appendix A Table 8 provide the opportunity to test for and mitigate this source of bias. Again, the table reestimates the baseline model with successive restrictions on the number of teachers within each experience-year. These restrictions reduce the sample size but increase the accuracy of measures of the district salary schedule. The estimates from the restricted samples confirm the main results already presented, although the point estimates are reduced slightly. It is not clear whether this reduction stems from the fact that pay effects are smaller in larger districts or from the elimination of bias caused by changes in teacher composition. In either case, the results suggest that bias caused by changes in teacher composition is too small to alter this study's main findings.

Overall, the best estimates of the average causal effect of teacher pay on turnover is –0.16 percentage points for a 1% increase in each cell on the salary schedule. At the mean (unweighted) turnover rate of 11.9, this estimate translates to an elasticity of –1.4. However, this is an average pay effect, which assumes that the impact of a change in teacher pay is homogeneous across teacher experience, subject taught, and gender. This may not be the case if teachers differ along these attributes in terms of outside opportunities, teacher–school match quality, or household production responsibilities. In the next two tables I reestimate the baseline model but allow the pay effects to vary across these teacher characteristics.

The estimates in Table 5 modify the baseline model in two ways to allow base pay to have marginal effects that vary with teacher experience. In models 1 and 2 of Table 5, base pay is interacted with experience bins to allow for differing marginal effects by teacher experience without imposing a functional form on the effects. Model 1 presents results when 6 experience bins are used, and model 2 uses 10 experience bins. In both models, $basepay_{jte}$, $basepay_{jt,e+1}$ and $basepay_{ote}$ are interacted with experience bin dummies. The combined effects reported in the table represent the sum of the coefficients on $basepay_{jte}$ and $basepay_{jt,e+1}$ within each experience bin. Model 3 allows the marginal effects of base pay to vary with experience by interacting $basepay_{jte}$, $basepay_{jt,e+1}$, and $basepay_{ote}$ with experience and experience squared. The combined effects in model 3 represent the sum of the coefficients on $basepay_{jte}$ and $basepay_{jt,e+1}$ in the level form and each interaction (basepay*exper and basepay*exper$^2$). Model 3 represents a more restrictive model in that it fits a functional form to the marginal effects, while models 1 and 2 estimate heterogeneous effects more flexibly but with potentially less precision.

The results in Table 5 suggest that a 1% increase in $basepay_{jte}$ and $basepay_{j,t,e+1}$ reduces turnover among teachers with 0–4 years of experience by about 0.24 percentage points (Table 5, model 1 and model 2). The pay effect declines rapidly with experience thereafter. The effect among teachers with 6–7 years of experience is about 0.16 percentage points for a 1% increase in pay (Table 5, model 2). For teachers with 8–11 years of experience the pay effect persists and is about 0.09 percentage points for a 1% increase in pay (Table 5, model 1 and model 2). Among teachers with 12 or more years of experience, the pay effects are statistically indistinguishable from zero, although the coefficient estimates are still negative and converging to zero as experience increases.

Overall the results in models 1 and 2 suggest large pay effects on turnover for relatively new teachers. The pay effects dissipate with additional years of experience and disappear around 19 years of experience. The more restrictive but more precise estimates in model 3 largely confirm this result. Model 3 suggests that the pay effect for new teachers is about 0.25 percentage points for a 1% increase in pay, and the effect decreases by 0.013 percentage points for each additional year of experience. These results suggest that pay increases have no impact on turnover rates for teachers with more than 19 or 20 years of experience.

Since there are fewer teachers at higher experience levels, one may suspect that these results could be driven by larger measurement error in base pay for teachers with more experience. However, the results in Appendix A Table 9 suggest that is not the case. As the sample is restricted to larger districts, which again reduces noise in the base pay measures, the results are largely unchanged.

**Table 5**
Turnover and teacher pay, heterogeneity by experience.

| Experience | Base pay comb. effect | $basepay_{ote}$ |
|---|---|---|
| *Model 1: 6 experience bins* | | |
| 0–2 | −23.64*** | −0.91 |
| 3–5 | −23.82*** | 7.88** |
| 6–8 | −14.51*** | 5.64 |
| 9–11 | −9.23* | −1.24 |
| 12–15 | −8.02 | 0.56 |
| 16–19 | −4.05 | 2.74 |
| *Model 2: 10 experience bins* | | |
| 0–1 | −28.32*** | 3.99 |
| 2–3 | −19.79*** | −3.80 |
| 4–5 | −24.04*** | 8.22* |
| 6–7 | −16.43*** | 4.87 |
| 8–9 | −9.16* | 3.35 |
| 10–11 | −10.18* | −3.55 |
| 12–13 | −9.42 | 1.43 |
| 14–15 | −7.25 | −1.44 |
| 16–17 | −8.92 | −4.16 |
| 18–19 | 0.80 | 8.95 |
| | Combined effect | Combined effect |
| *Model 3: fitting a flexible functional form* | | |
| basepay | −25.50*** | −26.41*** |
| | (3.76) | (5.39) |
| basepay*exper | 1.30*** | 1.73 |
| | (0.31) | (1.38)) |
| basepay*exper$^2$ | | −0.024 |
| | | (0.068) |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are clustered at the district level. Number of observations is 52,800 in all models. Teacher turnover rate for teachers with a bachelor's degree and experience *e* in district *j* during year *t* is the dependent variable. All models include district-by-year FEs, experience-by-year FEs, and experience-by-district FEs. Models 1 and 2 are the baseline model with $basepay_{jte}$, $basepay_{jt,e+1}$ and $basepay_{ote}$ interacted with experience bin dummies. The combined effect is the sum of the coefficients on $basepay_{jte}$ and $basepay_{jt,e+1}$ for each experience bin interaction. The 1st column of model 3 is the baseline model but adds the following interactions: $basepay_{jte}$*experience, $basepay_{jt,e+1}$*experience, and $basepay_{ote}$*experience. The 2nd column of model 3 adds interactions with experience squared. The combined effects in model 3 are the sums the coefficients on $basepay_{jte}$ and $basepay_{jt,e+1}$ for the levels and the interactions with experience and experience squared.

**Table 6**
Turnover and teacher pay, heterogeneity by subject.

| | Math | Science | Self-cont. | English | Other |
|---|---|---|---|---|---|
| Comb. effect | −12.73** | −17.87*** | −14.48*** | −21.20*** | −17.18*** |
| Turnover (%) | 14.5 | 14.6 | 11.5 | 13.5 | 13.5 |
| Elasticities | −0.88 | −1.23 | −1.26 | −1.57 | −1.28 |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are clustered at the district level. Results produced from a single regression. N = 131,885 district by year by experience by subject observations. Teacher turnover rates by experience *e*, district *j*, year *t*, and subject *s* is the dependent variable. The combined effect is the sum of the coefficients on $basepay_{jte}$ and $basepay_{jt,e+1}$. All models include district-by-year FEs, experience-by-year FEs, experience-by-district FEs, subject by year FEs, subject by district FEs, and subject by experience FEs. Regression also includes base pay in nearest outside district interacted with subject.

**Table 7**
Turnover and teacher pay, heterogeneity by gender.

| | (1) | (2) |
|---|---|---|
| $basepay_{jte}$*women | −13.77*** | −8.689* |
| | (3.925) | (5.038) |
| $basepay_{jte}$*men | −10.69 | −4.507 |
| | (6.824) | (7.714) |
| $basepay_{jt,e+1}$*women | −4.317 | −6.989 |
| | (3.670) | (4.605) |
| $basepay_{jt,e+1}$*men | −9.241 | −12.77 |
| | (6.916) | (9.500) |
| $basepay_{ote}$*women | 2.504 | 7.007 |
| | (3.477) | (4.164) |
| $basepay_{ote}$*men | 6.846 | 7.631 |
| | (4.144) | (5.721) |
| Observations | 94,688 | 19,840 |
| R-squared | 0.188 | 0.355 |
| Combined effect men | −19.93*** | −17.28*** |
| Combined effect women | −18.09*** | −15.68*** |
| F-test: combined effects equal | 0.59 | 0.64 |
| F-test: outside dist. effects equal | 0.20 | 0.88 |
| Combined effect elasticity men | −1.40 | −1.57 |
| Combined effect elasticity women | −1.52 | −1.50 |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are clustered at the district level. Teacher turnover rates by experience, district, year, and gender is the dependent variable. The combined effect is the sum of the coefficients on $basepay_{jte}$ and $basepay_{jt,e+1}$ within gender. All models include district-by-year FEs, experience-by-year FEs, experience-by-district FEs, gender-by-year FEs, gender-by-district FEs, and gender-by-experience FEs. The table reports p-values from F-tests of gender equality. The elasticities were calculated using the mean turnover rates in each sample by gender.

In terms of elasticities, the pattern of diminishing marginal effects of base pay is similar. Elasticities are largest (in absolute value) for less experienced teachers, around −1.3 to −1.4, and they increase slightly in the first few years. After 7–8 years of experience, elasticities decline rapidly and converge to zero at around 19–20 years of experience. These results suggest that not only are pay effects larger in absolute terms for less experienced teachers, but they are also larger in relative terms.[7]

With regard to the impact of outside district pay, only teachers with 3–5 years of experience show a statistically significant response. In model 1, the coefficient on outside pay for teachers with 3–5 years of experience is 7.88, which is significant at the 5% level. Also, in model 2, the coefficient on outside pay for teachers with 4–5 years of experience is 8.22 and is significant at the 10% level. All other outside pay coefficients are insignificant in both models. The results imply that a 1% increase in pay in the nearest outside district would increase turnover in the incumbent district by about 0.08 percentage points among teachers with 3–5 years of experience. This result is interesting in that it suggests that teachers who have recently gained tenured status[8] are more apt to transfer to a higher paying outside district than are other teachers. This may imply that some teachers stay in their current district so that they may gain tenure status, and once that status is gained, they look to switch to higher-paying districts.

Why are less experienced teachers more responsive to changes in pay? Prior research offers a few explanations. The quality of the teacher–school match may be higher for more experienced teachers (Jackson, 2010). Therefore, more experienced teachers may be reluctant to break a quality match regardless of the pay they receive. Pay for more experienced teachers may be high relative to the distribution of outside offers so that few experienced teachers are likely to conduct a search. Raising their pay will therefore do little to alter their search behavior and turnover propensity. Experienced teachers may be less inclined to search based on a longer history of unsuccessful search outcomes. Alternatively, more experienced teachers may have remained in teaching longer because they enjoy the nonpecuniary aspects of the job and would remain committed regardless of pay. Finally, because of the back-loaded nature of teacher retirement plans, the cost of turnover in terms of lost retirement benefits is higher for more experienced teachers (Costrell and Podgursky, 2010; Costrell and Podgursky, 2009).

Table 6 presents estimates of the baseline model but allows the base pay effects to vary by the teacher's subject taught. The sample used in the regression contains 131,885 district-by-year-by-experience-by-subject observations. This is a subsample of what was used to estimate the baseline model, which includes 52,800 district-by-year-by-experience observations. The sample used in Table 6 is restricted to the 26,377 district-by-year-by-experience observations for which there is at least one teacher employed in each of the 5 subject categories (so that the turnover rate is observed). Therefore, the sample contains 26,377 district-by-year-by-experience observations across 5 subject categories. In Table 6, I present the combined effects of $basepay_{jte}$ and $basepay_{jt,e+1}$ interacted with a subject dummy. Also, the model includes base pay in the nearest outside district interacted with subject dummies, and district-by-year FEs, experience-by-year FEs, experience-by-district FEs, subject by year FEs, subject by district FEs, and subject by experience FEs.

The results suggest fairly uniform effects of base pay changes across subjects. Teachers in all subjects respond to base pay increases, and the effects are about the same as we observe on average. A 1% increase in pay results in about a 0.16 percentage point decrease in turnover. None of the effects across subject are statistically distinguishable from the others at conventional significance levels. Also, although the average turnover rates vary across subjects (last row of Table 6), the elasticity of the pay effect is relatively stable at around −1.3. The estimated elasticity for math teachers is lower, but again the effects are not statistically different across subjects.

Table 7 presents estimates of the baseline model but allows the base pay effects to vary by gender. The sample used in column 1 contains 94,688 district-by-year-by-experience-by-gender observations and is restricted to the district-by-year-by-experience observations for which there is at least one teacher employed from each gender. The sample used in column 2 is more restrictive in that it is limited to the 31 districts that employ both male and female teachers in all experience-year cells (years 1996–2011 and experience 0–20). The table reports interactions between base pay (in the current and outside district) and gender dummies and combined effects by gender. Also, the model includes district-by-year, experience-by-year,

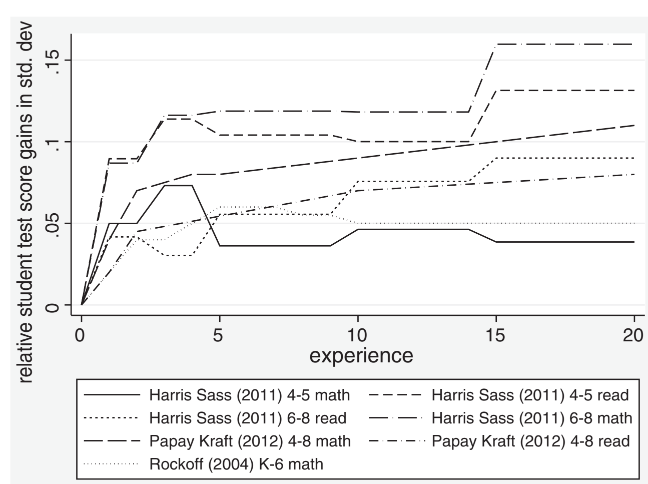


**Fig. 4.** Productivity–experience profiles in the literature.

[7] This result is contrary to what was reported in Clotfelter 2008, but this may be a result of their differing research design. They suggest that the larger relative effects they find for more experienced teachers may be driven by the fact that the within district bonuses may be effective in reducing within district turnover of more experienced teachers but less effective in reducing the incentive to leave teaching altogether, which is a relatively popular choice for less experienced teachers.

[8] In Texas, teachers earn tenure after their third year of teaching: Texas Education Code 21.102.

experience-by-district, gender-by-year, gender-by-district, and gender-by-experience fixed-effects.

The point estimates in Table 7 suggest that men are slightly more responsive to pay increases relative to women. For a 1% increase in the salary schedule male turnover rates decrease by 0.17 to 0.19 percentage points versus 0.16 to 0.18 percentage points for women. However, this difference is not statistically significant. Also, given that male turnover rates are higher in each sample (14.22 vs. 11.92 in column 1 and 10.98 vs. 10.42 in column 2), the relative impacts of pay across gender appear to be uniform. Using these mean turnover rates, male and female turnover elasticities are comparable around −1.5 in each sample. Overall, these results suggest that while men are slightly more likely to turnover in a given period, they are not measurably more responsive to changes in pay.

## 5. Implications for student achievement and policy

In terms of policy, the results presented here suggest that subject specific base pay changes are not likely to perform better in terms of reducing turnover rates relative to broader base pay changes that apply to teachers of all subject areas. However, the results imply that base pay increases that are targeted to less experienced teachers would be more cost effective in reducing turnover than general pay increases. Also, a flat salary schedule in which teachers make the same salary regardless of experience would perform better in terms of retaining teachers than the typical salary schedule which awards pay increases for each year of experience. Below, I discuss the implications for student achievement growth if districts were to shift their salary schedule through base pay increases, adopt a flatter schedule, or do both.

Before I can link teacher pay to student achievement, I first provide a brief summary of what we know about the relationship between teacher experience and student achievement. There is a large amount of literature and consensus on this topic. The evidence suggests that teachers improve with experience, in terms of their value-added to student performance on standardized exams. Fig. 4 shows the relationship between teacher experience and student achievement gains on standardized exams reported in three studies: (Harris and Sass 2011; Papay & Kraft 2011, Rockoff, 2004). For each profile, Fig. 4 indicates the grade levels and subject area for which the profile was estimated.

The vertical axis reports the difference in average student gains on exams for a teacher with experience reported on the horizontal axis relative to what those students would be expected to gain had they been assigned to a first-year teacher. The differences are reported in terms of standard deviations. Each study reveals a similar experience–productivity profile. Teacher performance improves dramatically in the first 4 years of teaching and then levels off in subsequent years. The average of these profiles is illustrated in Fig. 5. The average profile suggests that a student assigned to an experienced teacher (4+ years of experience) gains, on average, 0.08 standard deviations more over the school year than she would gain had she been assigned to a new teacher.

It is important to note that these studies estimate within-teacher productivity profiles, in that they show the expected productivity gains of an individual teacher over her career. This is important because the productivity growth illustrated in Figs. 4 and 5 is not a result of correlation between fixed teacher quality and experience, since their models include teacher fixed-effects. Therefore the profile is not simply the result of attrition by less able teachers, but rather average productivity gains by the typical teacher.

One concern for bias in these profiles, however, is that the sample is not a random draw of all teachers. These profiles are identified using teachers that have more than one productivity (value added) observation, and as such the sample is restricted to teachers who continue to teach for more than 1 year. In the context of this study, we want to know the productivity profile for all teachers, which could be different

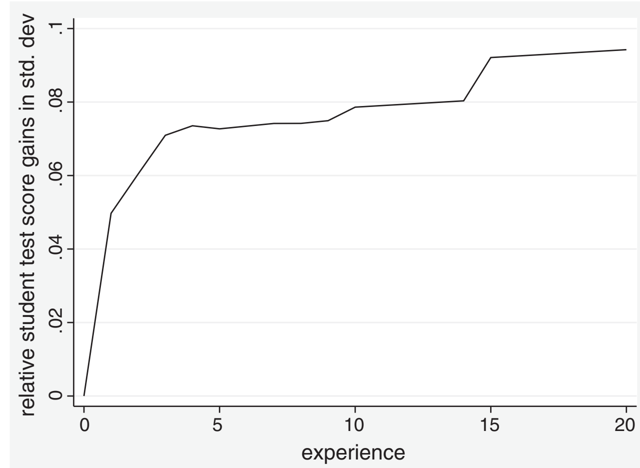


**Fig. 5.** Mean productivity–experience profile.

than the profile for teachers who remain in the district for at least 2 years, shown in Figs. 4 and 5.

Papay & Kraft (2011) test this possible source of bias by comparing initial growth rates of teachers who remain in the district to teachers who eventually leave. They find no evidence that teachers who stay have differing growth trajectories than teachers who leave. This suggests that if we were to retain these teachers through higher pay, they would improve in productivity over their career in a manner similar to what is illustrated in Figs. 4 and 5. By combining this average profile with the experience-specific marginal effects of base pay on turnover provided in Table 5 (model 3 in column 1), I am able to simulate the impact of alternative salary schedules on student performance.

To give the simulation traction, I assume that the distribution of teachers in Table 1 represents the steady-state distribution of a representative district over the sample period. As such, I assume that the original turnover rates (absent a policy change) for each experience cell are given in Table 1. I also assume that the district size remains constant for all time periods, so that the district employs 770 teachers in each period. When teachers leave the district, I assume that they are replaced with random draws from the distribution of new hires illustrated in Fig. 6. This distribution follows from the turnover rates and teacher distribution shown in Table 1 and the steady-state assumption.

Fig. 6 shows the distribution of experience among teachers who leave the district in a given period and the new teachers who replace them. From the distributions shown one can see that when a teacher leaves the district, the average teacher hired to replace her is likely to

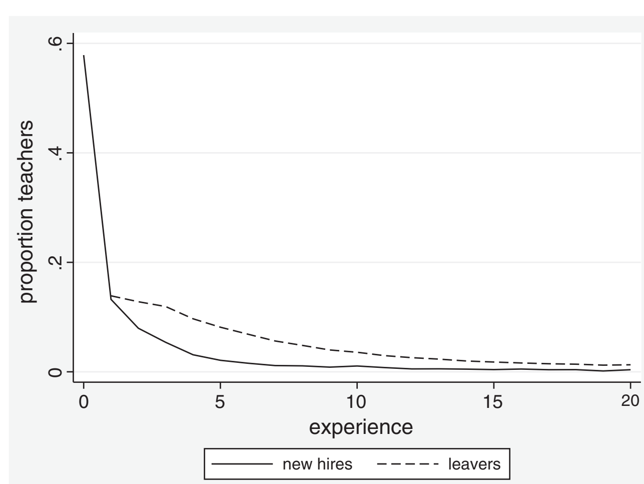


**Fig. 6.** Distributions of teachers who leave and new hires.

be less experienced. In fact, the distributions suggest that the average teacher who leaves the district has 6.2 years of experience and is replaced by a teacher with, on average, 1.8 years of experience. In terms of productivity, this suggests that each time a teacher is lost to turnover, on average, each student that will be taught by her replacement will gain 0.015 standard deviations less in achievement over the school year relative to what he or she would have gained had the incumbent teacher been retained.

Given this benefit of retention, I examine four pay policies that may influence retention and student achievement: maintaining the original salary schedule given in Table 1, awarding a 5% salary increase so that each cell on the schedule is increased by 5%, adopting a flat salary schedule that has the same total payroll next period as the original schedule, and combining the 5% pay increase with a flat schedule. The total payroll in the policy that combines the flat schedule and 5% increase is the same as the schedule with the 5% pay increase. Fig. 7 shows each of the four salary schedules examined in the simulation.

Using the estimated marginal effects of pay on turnover rates shown in Table 5, model 3, I simulate the effects of these salary schedules on student achievement growth. Throughout the simulation, I assume that the distribution of new hires does not change with the salary schedule. By doing so, I focus on the retention effects of a salary schedule change and ignore any selection effects. A salary increase (an upward shift in the salary schedule) likely improves the distribution of new district hires by attracting more experienced teachers to the district and potentially attracting teachers with a higher fixed ability. Since I ignore these potential benefits in the simulation, these results can be viewed as lower bounds. I leave a thorough investigation of these potential selection effects to future research.

Fig. 8 illustrates the effect of each policy on average teacher value-added over a 30 year period. If the district maintains its original salary schedule, then the distribution of teachers in the district does not change and teacher productivity remains constant. With this policy, relative teacher value-added is 0.067, which implies that the average teacher in the district adds 0.067 standard deviations more to student achievement than a first-year teacher. Paying teachers more does have a payoff, although the effects are small. A 5% increase in the salary schedule improves average teacher value-added by 0.0005 standard deviations within 3 years and 0.0007 within 10 years. This additional productivity is roughly equivalent to what the average teacher gains annually in value-added between her fifth and seventh year of experience.

Adopting a flatter salary schedule also improves teacher productivity slightly, and by extension student performance. By combining a 5% salary increase with a flat schedule, the district further increases average teacher value-added.

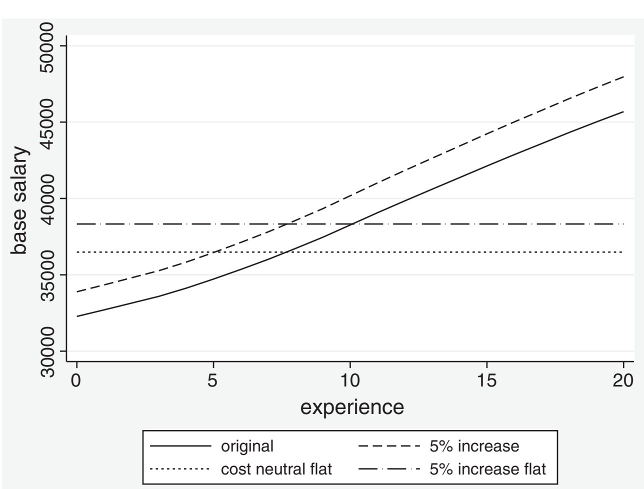


**Fig. 7.** Salary schedules.

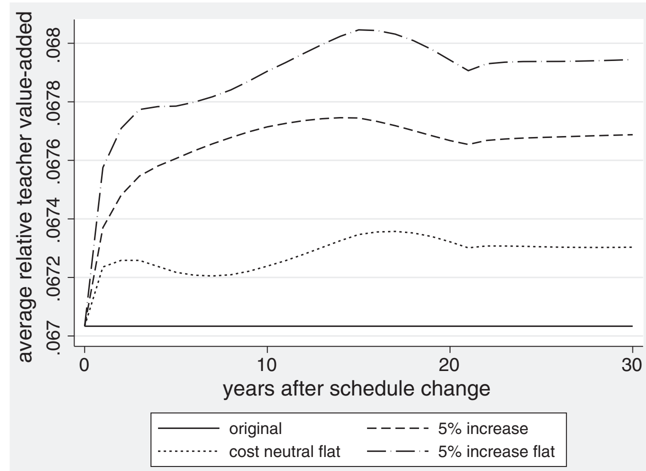


**Fig. 8.** Policy effects: teacher value added.

This simulation suggests that there is a payoff in terms of retention effects if the district pays teachers more. There also may be a payoff for adopting a flat salary schedule, although there is a risk that the flat schedule would have a negative impact on the distribution of new district hires, in that they would be less experienced on average. There is also a risk that a flat schedule would reduce teacher morale and effort, particularly among veteran teachers. If this occurs, the positive retention effects of adopting a flat schedule could be erased.

The overall impact of increasing the salary schedule, although shown to be positive here, is likely to be larger than the simulation suggests. This simulation captures only retention effects of a pay increase. In addition to retaining more experienced teachers, a pay increase is likely to improve the distribution of new teacher hires (a selection effect), improve teacher morale and, by extension, potentially increase teacher productivity. Also, raising teacher pay reduces teacher turnover, which has been shown to have positive effects on student achievement beyond its impact on teacher experience (Ronfeldt et al., 2013).

## 6. Conclusion

This study shows five major results. First, it provides robust evidence that an increase in base teacher pay reduces teacher turnover. The overall elasticity is about −1.4. Second, the pay effect is largest for less experienced teachers, decreases with experience, and disappears once a teacher reaches about 19 years of experience. Third, the teacher pay effect does not vary substantially across the teacher's subject taught or gender. Fourth, by increasing teacher pay, districts will likely see an improvement in student performance because the average experience of teachers will increase. Finally, a flat salary schedule performs well in terms of retaining a more productive distribution of teachers. If a flat schedule does not alter the distribution of new teacher hires and does not influence the effort choices of veteran teachers, then adopting a flat schedule may be a cheap way to improve student achievement.

This study focuses on the retention effects of teacher pay policies. To better understand the overall impact of teacher pay on student achievement, researchers should attempt to estimate the effect of teacher pay on teacher selection and effort. It is likely that higher pay will also improve student performance through these mechanisms, but evidence on this is scarce. A better understanding of these mechanisms will also help us predict the overall effects of adopting a flat salary schedule which, based on retention effects alone, appears to be an effective policy.

## Appendix A

**Table 8**
Sensitivity of baseline results to sample restrictions.

| | (1) | (2) | | |
|---|---|---|---|---|
| Sample restriction | Combined effect | Combined effect | Obs. | Districts |
| Full sample | −24.36*** | −16.08*** | 52,800 | 165 |
| min cell size 5 | −23.84*** | −12.53*** | 19,200 | 60 |
| min cell size 10 | −22.35*** | −13.95*** | 12,800 | 40 |
| min cell size 15 | −22.10*** | −11.58** | 8640 | 27 |
| min cell size 20 | −23.70*** | −11.08*** | 5120 | 16 |
| min cell size 25 | −24.94*** | −12.08*** | 4160 | 13 |
| min cell size 30 | −27.43** | −9.691** | 2880 | 9 |
| min cell size 35 | −20.28** | −10.99** | 1920 | 6 |
| min cell size 40 | −20.34** | −8.286 | 1600 | 5 |
| Fixed effects | dist, year, exper | dist-by-year, exper-by-year, dist-by-exper | | |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Teacher turnover rate for teachers with a bachelor's degree and experience $e$ in district $j$ during year $t$ is the dependent variable. Min cell size refers to the minimum number of teachers employed in an experience-year cell for a district. For example, the sample in the row denoted "min cell size 5" is restricted to the 60 school districts that employed at least 5 teachers in each experience-year cell over the period 1996–2011. The combined effect is the sum of the coefficients on $\text{basepay}_{jte}$ and $\text{basepay}_{jt,e+1}$. Each combined effect estimate is from a separate regression. The combined effects reported in column (1) include only district, year, and experience fixed-effects, while the combined effects reported in column (2) include district-by-year, experience-by-year, and district-by-experience fixed effects.

**Table 9**
Sensitivity of experience heterogeneity results to sample restrictions.

| Experience | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| 0–2 | −23.64*** | −26.47*** | −31.41*** | −36.44*** | −28.48*** |
| 3–5 | −23.82*** | −24.13*** | −26.62*** | −26.53*** | −25.23*** |
| 6–8 | −14.51*** | −11.42** | −18.12*** | −20.85*** | −17.33** |
| 9–11 | −9.228* | −12.56*** | −11.78** | −11.90** | −6.749 |
| 12–15 | −8.022 | −8.123* | −2.542 | −4.157 | −0.300 |
| 16–19 | −4.052 | −9.410* | −7.769 | −6.598 | −1.394 |
| Cell size restriction | None | min ≥3 | min ≥7 | min ≥11 | min ≥15 |
| Observations | 52,800 | 27,840 | 15,360 | 11,520 | 8640 |
| Num. of districts | 165 | 87 | 48 | 36 | 27 |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Teacher turnover rate for teachers with a bachelor's degree and experience $e$ in district $j$ during year $t$ is the dependent variable. All models include district-by-year FEs, experience-by-year FEs, and experience-by-district FEs. Estimates are from the baseline model with $\text{basepay}_{jte}$, $\text{basepay}_{jt,e+1}$ and $\text{basepay}_{ote}$ interacted with experience bin dummies. The results shown are the sum of the coefficients on $\text{basepay}_{jte}$ and $\text{basepay}_{jt,e+1}$ for each experience bin interaction. Each column represents a stronger sample restriction on the minimum number of teachers employed within a district-year-experience cell.

**Table 10**
Sensitivity of baseline results to weights.

| | (1) | (2) | (3) |
|---|---|---|---|
| $\text{basepay}_{jte}$ | −9.949** | −5.887** | −5.889** |
| | (3.922) | (2.664) | (2.589) |
| $\text{basepay}_{jt,e+1}$ | −6.134* | −6.688*** | −7.351*** |
| | (3.160) | (2.222) | (2.507) |
| $\text{basepay}_{ote}$ | 2.077 | 3.085 | −0.165 |
| | (2.612) | (2.342) | (2.155) |
| Combined effect | −16.08*** | −12.57*** | −13.24*** |
| R-squared | 0.358 | 0.484 | 0.574 |
| Weights | None | District-year | Cell-size |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, reported in parentheses, are clustered at the district level. Teacher turnover rate for teachers with a bachelor's degree and experience $e$ in district $j$ during year $t$ is the dependent variable. $\text{basepay}_{jte}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$, $\text{basepay}_{jt,e+1}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e+1$ years of experience in year $t$, $\text{basepay}_{ote}$ is the natural log of teacher contract salary in the district nearest to district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$. All models include 52,800 district-by-year-by-experience observations: 165 districts, 16 years (1996–2011), and 20 experience levels (0–19). The combined effect is the sum of the coefficients on $\text{basepay}_{jte}$ and $\text{basepay}_{jt,e+1}$. All models include district-by-year FEs, experience-by-year FEs, and experience-by-district FEs.

**Table 11**
Baseline results when sample is restricted to tenured teachers only.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $\text{basepay}_{jte}$ | −8.354** | −9.981** | −6.322 | −2.652 | −2.245 |
| | (4.151) | (4.402) | (3.947) | (3.108) | (4.045) |
| $\text{basepay}_{jt,e+1}$ | −5.285 | −2.292 | −2.016 | −9.278*** | −9.638** |
| | (3.334) | (4.142) | (3.969) | (3.291) | (3.663) |
| $\text{basepay}_{ote}$ | 2.920 | 6.006** | 3.385 | 3.009 | 3.940 |
| | (2.968) | (2.931) | (2.965) | (3.256) | (3.919) |
| Cell size restriction | None | min ≥3 | min ≥5 | min ≥7 | min ≥11 |
| Experience restriction | exper ≥3 | exper ≥3 | exper ≥3 | exper ≥3 | exper ≥3 |
| Observations | 44,880 | 23,664 | 16,320 | 13,056 | 9792 |
| Num. of districts | 165 | 87 | 60 | 48 | 36 |
| Combined effect | −13.64*** | −12.27*** | −8.338** | −11.93*** | −11.88*** |
| R-squared | 0.291 | 0.418 | 0.510 | 0.559 | 0.583 |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, reported in parentheses, are clustered at the district level. Teacher turnover rate for teachers with a bachelor's degree and experience $e$ in district $j$ during year $t$ is the dependent variable. $\text{basepay}_{jte}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$,$\text{basepay}_{jt,e+1}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e+1$ years of experience in year $t$, $\text{basepay}_{ote}$ is the natural log of teacher contract salary in the district nearest to district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$. The combined effect is the sum of the coefficients on $\text{basepay}_{jte}$ and $\text{basepay}_{jt,e+1}$. All models include district-by-year FEs, experience-by-year FEs, and experience-by-district FEs.

**Table 12**
Sensitivity of baseline results to measure of political influence.

| | (1) | (2) |
|---|---|---|
| $\text{basepay}_{jte}$ | −9.949** | −9.904** |
| | (3.922) | (3.934) |
| $\text{basepay}_{jt,e+1}$ | −6.134* | −6.026* |
| | (3.160) | (3.150) |
| $\text{basepay}_{ote}$ | 2.077 | 2.109 |
| | (2.612) | (2.602) |
| Share of teachers in cell | | −2.295 |
| | | (3.264) |
| Observations | 52,800 | 52,800 |
| R-squared | 0.358 | 0.358 |
| Combined effect | −16.08*** | −15.93*** |

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors, reported in parentheses, are clustered at the district level. Teacher turnover rate for teachers with a bachelor's degree and experience $e$ in district $j$ during year $t$ is the dependent variable. $\text{basepay}_{jte}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$,$\text{basepay}_{jt,e+1}$ is the natural log of teacher contract salary in district $j$ for a teacher with a bachelor's degree and $e+1$ years of experience in year $t$, $\text{basepay}_{ote}$ is the natural log of teacher contract salary in the district nearest to district $j$ for a teacher with a bachelor's degree and $e$ years of experience in year $t$. The combined effect is the sum of the coefficients on $\text{basepay}_{jte}$ and $\text{basepay}_{jt,e+1}$. All models include district-by-year FEs, experience-by-year FEs, and experience-by-district FEs.

## References


Akerlof, George A., 1982. Labor contracts as partial gift exchange. Q. J. Econ. 97 (4), 543–569.

Burdett, Kenneth, 1978. A theory of employee job search and quit rates. Am. Econ. Rev. 68 (1), 212–220.

Clotfelter, Charles, Glennie, Elizabeth, Ladd, Helen, Vigdor, Jacob, 2008. Would higher salaries keep teachers in high-poverty schools? Evidence from a policy intervention in North Carolina. J. Public Econ. 92 (5–6), 1352–1370.

Clotfelter, Charles, T., Ladd, Helen F., Vigdor, Jacob L., 2011. Teacher mobility, school segregation, and pay-based policies to level the playing field. Educ. Financ. Policy 6 (3), 399–438.

Costrell, Robert M., Podgursky, Michael, 2009. Peaks, cliffs, and valleys: the peculiar incentives in teacher retirement systems and their consequences for school staffing. Educ. Financ. Policy 4 (2), 175–211.

Costrell, Robert M., Podgursky, Michael, 2010. Distribution of benefits in teacher retirement systems and their implications for mobility. Educ. Financ. Policy 5 (4), 519–557.

Dolton, Peter, von der Klaauw, Wilbert, 1995. Leaving teaching in the UK: a duration analysis. Econ. J. 105 (429), 431–444.

Dolton, Peter, von der Klaauw, Wilbert, 1999. The turnover of teachers: a competing risks explanation. Rev. Econ. Stat. 81 (3), 543–550.

Griliches, Zvi, Hausman, Jerry A., 1986. Errors in variables in panel data. J. Econ. 31 (1), 93–118.

Hanushek, E.A., 1997. Assessing the effects of school resources on student performance: an update. Edu. Eval. Policy Anal. 19 (2), 141–161.

Hanushek, Eric A., 2003. The failure of input-based schooling policies. Econ. J. 113 (485), F64–F98.
Hanushek, Eric A., Kain, John F., Rivkin, Steven G., 2004. Why public schools lose teachers. J. Hum. Resour. 39 (2), 326–354.
Harris, Douglas N., Sass, Tim R., 2011. Teacher training, teacher quality and student achievement. J. Public Econ. 95 (7), 798–812.
Hendricks, Matthew D., 2013. Identification with imperfect proxies. Working Paper.
Imazeki, Jennifer, 2005. Teacher salaries and teacher attrition. Econ. Educ. Rev. 24 (4), 431–449.
Jackson, C. Kirabo, 2010. Match quality, worker productivity, and worker mobility: direct evidence from teachers. National Bureau of Economic Research Working Paper, 15990.
Loeb, Susanna, Page, Marianne E., 2000. Examining the link between teacher wages and student outcomes: the importance of alternative labor market opportunities and non-pecuniary variation. Rev. Econ. Stat. 82 (3), 393–408.
Murnane, Richard J., Randall, J., Olsen, 1989. The effect of salaries and opportunity costs on duration in teaching: evidence from Michigan. Rev. Econ. Stat. 71 (2), 347–352.
Murnane, Richard J., Randall, J., Olsen, 1990. The effects of salaries and opportunity costs on length of stay in teaching: evidence from North Carolina. J. Hum. Resour. 25 (1), 106–124.
Papay, J.P., Kraft, M.A., 2011. Productivity returns to experience in the teacher labor market: methodological challenges and new evidence on long-term career growth. Working Paper.
Podgursky, Michael, Monroe, Ryan, Watson, Donald, 2004. The academic quality of public school teachers: an analysis of entry and exit behavior. Econ. Educ. Rev. 23 (5), 507–518.
Rickman, Bill D., Parker, Carl D., 1990. Alternative wages and teacher mobility: a human capital approach. Econ. Educ. Rev. 9 (1), 73–79.
Rockoff, Jonah E., 2004. The impact of individual teachers on student achievement: evidence from panel Data. Am. Econ. Rev. 94 (2), 247–252.
Ronfeldt, Matthew, Loeb, Susanna, Wyckoff, James, 2013. How teacher turnover harms student achievement. Am. Educ. Res. J. 50 (1), 4–36.
Shapiro, Carl, Stiglitz, Joseph E., 1984. Equilibrium Unemployment as a Worker Discipline Device. Am. Econ. Rev. 74 (3), 433–444.