When Tenure Ends: The Short-run Effects of the Elimination of Louisiana's Teacher Employment Protections on Teacher Exit and Retirement

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ABSTRACT

Most teachers have tenure protections that constrain dismissal. Some argue that tenure improves recruitment and retention by mitigating the risk of monopsony employment and substituting job security for lower salaries. Others argue that tenure reduces performance incentives making it difficult to dismiss ineffective teachers. We examine supply-side responses of teachers after the elimination of tenure before administrators could use performance to dismiss teachers. Voluntary teacher attrition increased after tenure elimination with effects concentrated in groups that are theoretically most likely to value job protections. Specifically, tenure removal increased exit of teachers with bottom decile value-added measures and retirement eligible teachers.

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I. Introduction

Most public school teachers have long-standing employment protections that shield them from arbitrary or capricious dismissal. In particular, teacher tenure policies intentionally erect barriers to dismissing teachers, typically by creating substantial obstacles to dismissal after an initial period of probationary employment (Christie & Zinth, 2011). Those who support teacher tenure argue that these laws are necessary to protect teachers from the monopsony power of school districts (Chambers, 1981), and that they improve teacher recruitment and retention by compensating for low salaries with high job security (Chermerinsky, 2014; Public Impact, 2011; Ravitch, 2015; Rothstein, 2015). Critics argue that tenure harms schools and students by reducing incentives to improve performance and making it nearly impossible to fire ineffective teachers. For example, Hanushek (2015) illustrates that, given an adequate supply of replacement teachers, school districts could improve student outcomes by regularly replacing ineffective teachers.

Prior to 2009, all states had teacher tenure protections, and none required districts to consider teacher performance before granting tenure (NCTQ, 2016). In this context, most states saw fewer than one percent of tenured teachers dismissed due to poor performance (NCES, 2012).[1] As part of larger reforms framed around flexibility and local control, by 2020, 22 states mandated that tenure could not be granted without evidence of teaching effectiveness, and 13 states allowed tenure clocks to be extended due to poor performance (Nittler and Gerber, 2020) At least seven states have updated rules to allow districts to rescind tenure protections from already-tenured teachers (NCTQ, 2016). Four states have passed laws that effectively eliminate tenure and due process rights for teachers (Nittler and Gerber, 2020; Thomsen, 2016). If

[1] Source: https://nces.ed.gov/surveys/sass/tables/sass1112_2013311_d1s_008.asp

advocates for tenure protections are correct, then many teachers may only be willing to accept difficult working conditions and relatively low pay when accompanied by the substantial job security afforded by tenure (e.g., Feinberg, 1981; Brunner & Imazeki, 2010; Rothstein, 2015). Thus, the removal of tenure without off-setting compensation could negatively affect teacher recruitment and retention (Rothstein, 2015). This may, in turn, result in the unintended consequence of inducing voluntary attrition of teachers on a scale that outpaces replacement.

There is limited empirical evidence on the relationship between tenure and teacher attrition. Kraft, Brunner, Dougherty, and Schwegmann (2020) provide a state-level analysis of the effect of various tenure reforms and related policies. Loeb, Miller, and Wyckoff (2015) and Brunner, Cowen, Strunk, and Drake (2019) estimate teacher attrition after local and state tenure reforms, respectively. However, prior evidence comes primarily from settings where tenure protections were relaxed rather than eliminated, and where reforms simultaneously added performance-based evaluation of teachers. In this study, we provide the first evidence on the effects of a state-wide elimination of permanent tenure-related job security on teacher attrition. We are also better able to separate the effect of loss of tenure from the effects of complementary policies. In most settings, the removal of tenure protections was accompanied by the immediate authority for principals to dismiss teachers based on performance measures. We use data from a unique policy context in Louisiana, where tenure was formally removed several years before job performance data were available to inform employment decisions. This allows us to better estimate teacher response to loss of job protections separate from the effects of performance on exit decisions.

Using ten years of teacher panel data, we employ a difference-in-difference model to identify whether teacher exits increased during this unique time-period when teachers could react

to the removal of tenure without a threat of dismissal due to poor performance evaluations. The difference-in-difference approach allows us to compare exit probabilities of similar teachers in similar settings, before and after the reforms to control for any exogenous changes to the composition of the workforce that might also influence exit. We then test the causal effect of tenure removal by comparing subgroups of teachers who theoretically may differ in the value they place on tenure protections. Thus, we come as close as possible to estimating the causal effects of the loss of job protections – isolated from teacher evaluation data – on short-term teacher exit.

We find that 8.3 percent of teachers exited teaching in the year of tenure removal, an 18.5 percent (1.3 percentage points) increase from the year prior to the reform, and increased exit rates were substantially more likely among groups we identify as more sensitive to the loss of tenure. As further confirmation, we find no evidence of effects on Louisiana charter school teachers, a group that was subject to teacher evaluation reforms but not tenure reforms, and no corresponding decrease in the overall demand for teachers. Estimates suggest that reform-induced attrition was equivalent to losing 1,500-1,700 teachers in the first two years after the removal of tenure protections, or 3.0 to 3.5 percent of Louisiana's teacher workforce. This exit was highly concentrated among teachers in the bottom quintile of standardized test-based performance growth. This suggests that the removal of tenure might lead to improvements in Louisiana's teacher quality if teachers are replaced by average novice teachers, as proposed by Hanushek (2015).

In what follows, we review the theoretical and empirical literature on teacher tenure and identify groups that we expect to be more or less sensitive to tenure policy changes. In Section III, we describe Louisiana's context for teacher policy reform. Section IV outlines our data,

including descriptive statistics on Louisiana teachers and their exit rates over time, and details our empirical strategies. Section V reports results from our analyses. Section VI concludes with a discussion of our results and their implications for other states considering similar reforms.

II. Teacher Tenure, Employment Risk, and the Teacher Labor Market

Literature Review

Proponents of job protections for public school teachers argue that tenure helps to attract qualified new teachers to the profession, to mitigate against the risks associated with monopsony power of school districts, and to retain current teachers (e.g., Chambers, 1981; Chermerinsky, 2014; Ravitch, 2015; Rothstein, 2015). This rationale for tenure relies on the assumption that teachers value job security as part of their full compensation package. Prior research supports the assertion that teachers consider both monetary and non-monetary benefits in their calculation of total compensation (e.g., Loeb & Page, 2000), and that teachers consider and value a variety of employment characteristics such as working conditions, school climate, and student endowments (e.g., Antos & Rosen, 1975; Kenny & Denslow, 1980; Chambers, 1981; Levinson, 1988; Horng, 2009). Most relevant for this study, teachers, like other workers, value job stability and the absence of employment risk (e.g., Goldhaber et al., 2016; Feinberg, 1981; Murnane & Olsen, 1990).

There is little research that tells us how tenure policies specifically influence the teacher workforce, but descriptive and simulation-based evidence suggests that tenure may be substitutable with other forms of compensation. Comparing states with different lengths of probationary employment before tenure, Brunner and Imazeki (2010) find that pre-tenure teacher salaries are often higher in districts where it takes longer to achieve tenure. Two prior studies utilized simulations to test the labor market impacts of diminished tenure protections (Staiger &

Rockoff, 2010; Rothstein, 2015). Both suggest that average teacher quality could improve by reducing tenure protections (either by delaying the provision of tenure or decreasing the fraction of teachers who are awarded tenure). However, Rothstein (2015) also proposes that these types of policy change might carry large costs in terms of reduced teacher retention and recruitment, unless teacher pay is increased.

There is initial evidence that tenure reforms also impact incoming teacher supply. Kraft et al. (2020) examine the effects evaluation and various tenure reforms at the state level on the supply of new teachers. They find that changes to either policy at the state level resulted in a decrease in the supply of new teachers graduating from universities in the state. This provides initial evidence that job security from tenure is valuable not only to teachers already in the labor market, but also to new entrants, and its removal reduces the supply of new teachers. Looking at the impact of reduced supply on schools, Kraft et al. (2020) found that unfilled vacancies increased at hard-to-staff schools, suggesting that the impacts of tenure removal varied across districts. However, they also found evidence that the reduction in supply was concentrated at less selective universities, indicating that average teacher quality may improve if teachers from more selective universities (proxying for quality) are less impacted by tenure removal.

Two prior studies examine the effects of tenure policy changes on the current teacher workforce (Loeb et al., 2015; Brunner et al., 2019). Loeb et al. (2015) study a local tenure reform in New York City public schools that gave principals discretion to deny tenure or extend a teacher's tenure clock based on performance. They report a substantial drop in tenure approvals when tenure was linked to performance from 94 percent prior to reform to 56 percent after, and non-approval was strongly associated with lower teacher value-added on standardized tests and lower performance ratings from principals. In most cases where tenure was not approved,

teachers were not dismissed but the tenure clock was extended. Despite the opportunity to gain tenure through performance improvement, the study finds that teachers whose tenure was extended had voluntary exit rates 66% higher than tenured teachers. They conclude that extending the tenure clock for poor performance induced voluntary exit of low-performers and improved teacher quality on average. As the authors note, they cannot determine if teachers voluntarily exited the system because they received a signal that they were ineffective, or if they did so because of the increased risk associated with the failure to receive tenure.

The second study by Brunner et al. (2019) examines the effects of a set of statewide reforms in Michigan that included weakened tenure protections, extended tenure clocks, performance-based assessment, and limit on collective bargaining rights. The authors estimate the unique effect of this broad reform on teacher attrition by comparing changes in exit rates for teachers (who were affected by the tenure policy) and other school staff (who were not affected). Overall, they find no increases in teacher exit except for a small effect among less-experienced teachers in hard-to-staff schools. They conclude that staffing effects are quite small but might be problematically concentrated in high need schools. Due to data limitations, the effects on teacher quality in Michigan were not explored.

Our study is designed to provide clearer evidence to help inform policymakers and researchers about the unique effects of the removal of job security on teacher attrition. We build on this prior work by focusing on a setting where tenure was effectively removed and not just delayed. Further, by focusing on a period where teacher performance measures were not yet implemented, this is the first analysis that attempts to tease out the unique effect of lost job security from the effects of performance-based evaluation.

Theoretical Framework

Below, we outline a theoretical framework through which to consider tenure (or similar protections) as a valued component of an employee's total compensation package. Building on and extending earlier work on teacher compensation (e.g., Murnane & Olsen, 1990; Brunner & Imazeki, 2010; Rothstein, 2015), we formalize how the loss of tenure and the replacement of a traditional tenure system with a performance-based system may be viewed through the framework of utility maximization under risk aversion. If tenure protections are a critical element of total compensation, the loss of such protections might substantially diminish the attractiveness of public school teaching, relative to other employment options. We use this framework to identify groups of teachers whom theory predicts will place different values on tenure protections. In our empirical analysis, we use comparisons of these groups to substantiate a causal effect of the removal of tenure protections on teacher exit.

As in prior literature on teacher employment (e.g., Loeb & Page, 2000), we assume that a utility-maximizing employee derives both monetary and non-monetary benefits from her position where utility (u) in time t, can be expressed as:

$$u_t = f\{C_t\} \tag{1}$$

where C is a compensation package that includes both wages and other utility-bearing job characteristics, including working conditions, school characteristics, and student endowments (e.g., Antos & Rosen, 1975; Kenny & Denslow, 1980; Chambers, 1981; Levinson, 1988; Horng, 2009). At the end of each year, a teacher compares her current position to her next best option, and will return the following fall if:

$$u(C_{t+1}^{teach}) > u(C_{t+1}^{alt}) \tag{2}$$

that is, if the utility derived from wage and non-wage benefits of the current position will exceed the utility of the next-best alternative.

Absent tenure protection, a teacher faces a risk of dismissal (r) from her position due to factors within her control (effort) and beyond her control (luck). This introduces uncertainty into her assessment of her future compensation, which can be formally modelled as utility under conditions of risk and risk-aversion (Arrow, 1965). The possibility of not being invited back to her job in the fall influences a risk-averse teacher to view her future teaching compensation as an expected value with associated risk, rather than a certain income.[2]

In the case of a positive probability of dismissal (r>0) in the following year:

$$Expected\, u_{t+1} = (1 - r) \cdot u(C_{t+1}) - u(\pi) \tag{3}$$

where π is the teacher's risk premium, representing the amount the teacher would be willing to pay to eliminate the risk of dismissal. The presence of a positive risk premium places a monetary value on job security and reduces the expected value of next year's teaching position. Assuming the next-best alternative has been offered with certainty, the inequality relevant to a teacher's decision to exit teaching is:

$$(1 - r) \cdot u(C_{t+1}) - u(\pi) > u(C_{t+1}^{alt}) \tag{4}$$

By subtracting equation (4) from equation (2), we get the economic value of job security as:

$$r \cdot u(C_{t+1}) + u(\pi) \tag{5}$$

Generically, *tenure* is a policy that provides a teacher full job security at n years. If the effect of tenure on a teacher's compensation is to reduce the risk of dismissal very close to $r=0$, a teacher with tenure enjoys the more valuable risk-free compensation bundle in eq. (2). For a

[2] Applying a similar approach to teacher incentive pay, Neal (2011) illustrates that the risk-inducing influence of factors beyond the teacher's control (such as student endowments, reduction in school resources, or even bad weather on testing days) can significantly reduce the value to teachers of what appear to be large potential performance bonuses. Murnane and Olsen (1989) take a slightly different approach by adding risk as a third component of utility derived from teaching in addition to wages and job characteristics.

teacher who has achieved tenure (*t≥n)*, its value is equal to the utility gained by living without the risk of dismissal in eq (5). Empirical evidence suggests that the risk of dismissal with tenure is almost zero in most states (NCES, 2012). A not-yet-tenured teacher's value of *tenure* is reduced by the probability of dismissal prior to year *n*, as well as discounting due to the time-value of money:

$$u_{t+1}(tenure|t < n) = \left[(1-r)^{n-t} \cdot u\left(C_{t+1}^{teach}\right) - u(\pi)\right] \cdot (1-\rho)^{-(n-t)} \quad (6)$$

where ρ is the teacher's discount rate.

The important implication for our empirical analysis is that the theory identifies three variables that influence how much a teacher values tenure: 1) the risk of dismissal without protections; 2) the present value of guaranteed future employment; and 3) an alternative source of guaranteed income. From these, we derive three comparative analyses to test the causal effects of tenure removal. First, we compare teachers with a high probability of dismissal, based on recent student test performance, to teachers with a low-probability of performance-based dismissal. Second, we compare teachers who had just achieved tenure (and therefore value it at its full non-discounted rate) to teachers for whom tenure was still a discounted, future benefit. Third, we compare teachers who can retire with full benefits (a risk-free alternative to teaching) to those who lack a guaranteed alternative. Finally, we conduct several falsification tests, including by replicating analyses with Louisiana charter school teachers, a group that should not place any value on state tenure protections, because the prior state tenure law only applied to teachers in traditional public schools.

III. Teacher Tenure and Policy Reform in Louisiana

Before describing these comparison groups in detail, it is necessary to understand the context of Louisiana's policy change. As noted above, several states enacted simultaneous

changes that initiated performance-based evaluation for teachers *and* removed employment protections for those who underperform. Similarly, the Louisiana legislature enacted multiple teacher policy reforms from 2010 to 2015. The teacher evaluation system, known as Compass, was announced and developed in 2010.[3] Evaluations for all teachers statewide were conducted for the first time in 2012-13, but due to the transition to a new state testing regime, results were not available to school districts and principals until the fall of the 2013-14 school year, and results were not high-stakes for teacher employment until 2015-16.[4]

In July 2012, the state effectively removed lifetime tenure and made all teachers, tenured or untenured, potentially subject to dismissal based on the COMPASS assessment data. While the state continues to officially grant tenure, initial approval is contingent on evaluations, and tenure can be removed if a teacher does not achieve the highest rating on Compass for five out of six consecutive years.[5] The common definition of teacher tenure as a near guarantee that you cannot be fired no longer applies for *any* Louisiana public school teacher.[6] Thus, we discuss the tenure reform in Louisiana as the removal of tenure protections for all teachers.

While other states simultaneously implemented tenure reforms and the use of teacher evaluation data in employment decisions, the implementation timeline in Louisiana provides a

[3] Under Compass, all teachers receive an effectiveness score that consists of equally-weighted quantitative and qualitative measures. For more information on the Compass evaluation system, see http://www.louisianabelieves.com/teaching/compass.

[4] The state's original intent was to provide Compass data to principals in fall 2013. Louisiana also transitioned to the Common Core standards that year, and the first Common Core-based testing occurred in spring 2014. The state chose to delay Compass data until the second year of the new testing regime and provided data for informational purposes only until the 2015-2016 year. Summer 2016 is the first year that non-voluntary teachers exits would occur based on Compass results.

[5] Based on data from 2015, districts varied widely in the proportion of teachers in the highest-ranked group (highly effective) from 0% to over 90%. The median rate was 27%. Estimations based on Koedel and Betts (2010) suggest that at the median rate, a teacher has less than 10% probability of maintaining "tenured" status. These data were not known to teachers during the time of our study.

[6] This legislation also prohibits teachers rated as ineffective from receiving pay raises, although compensation and retirement benefits cannot be decreased due to poor performance. In addition, teachers who lose "tenure" status are no longer eligible to receive compensated sabbatical or medical leaves.

unique opportunity to isolate the effects of tenure reform as a loss of job security. If we assume that teachers primarily exit in the summer, exits due to *anticipation* of high-stakes performance evaluation would have begun as early as summer 2010. Because administrators did not receive actual performance data for several years, any voluntary exits influenced by evaluation data could not have begun until summer 2014, and dismissal due to evaluation data was permitted beginning in summer 2016. Meanwhile, the tenure reform took effect immediately in summer 2012. We should observe teachers' pure response to the loss of tenure protections and the associated expectations of employment risk in 2012 to 2015. Our study focuses on *voluntary* exit in these periods before *involuntary* exits resulting from principal responses to Compass evaluation scores could have begun. We examine exit trends from summer 2006 through summer 2015 to estimate the effect of this decrease in job security on the teacher workforce in Louisiana.

IV. Data and Empirical Strategy

Data

We use a ten-year panel of teacher- and school-level data, spanning the 2005-06 through 2014-15 school years. Our full analytic dataset consists of 485,231 teacher–year observations from 81,342 teachers and 1,793 schools. Data were provided by the Louisiana Department of Education (LDOE) and include elements from the state's personnel, student performance, and student enrollment systems. Teacher-level variables include teacher demographics, teacher experience[7], teaching certificates, college degrees, whether or not a teacher graduated from a

[7] The data do not include an explicit measure of experience, so we generate this measure using a combination of reported salary information and district hire dates. In most cases we rely on salary data to determine teacher experience level; teacher salaries are determined by teaching experience and education levels and are codified in the teacher salary schedules that were available for each district. For teachers for whom we could not generate salary-schedule-based experience levels, we used district hire dates. We exclude 1.3% of teachers for whom we could not determine either experience measure.

Louisiana college, whether or not a teacher entered the profession through an alternative pathway, and school assignments. We define teacher exit as departure from public school employment anywhere in the state. If a teacher does not appear in the data the following year, she is coded as exiting during the summer. Thus, with data through the 2015-16 school year, we observe summer exits from 2006 through 2015.[8]

Our analytic strategy requires us to identify comparison groups of teachers who are at high- and low-risk of dismissal due to job performance. As noted, these data were not produced until 2014, and are never included in administrative teacher data, so we constructed a similar growth-based performance measure. Classroom roster data allowed us to match students to teachers by class and subject. We merged these data with state standardized test scores and other student demographic information to estimate teacher effectiveness measures for math, English, social studies, and science. Effectiveness measures were estimated using a two-step value-added modeling approach described in Appendix A. The data allow us to generate estimates of teacher effectiveness for teachers of the four tested subjects in grades 4-8. Our estimates proxy for information teachers might intuitively know from self-assessment prior to 2013 and for actual Compass data provided in fall 2013 and fall 2014.

We also constructed several school-level measures to reflect school conditions that change over time and might influence a teacher's propensity to exit, including aggregate student demographic and educational needs (race/ethnicity, English proficiency, special education status, and free/reduced price lunch eligibility). Finally, we include state-reported school performance scores (SPS), which aggregate student proficiency rates on state standardized tests and are used

[8] Some teachers temporarily exit for one or more years before returning to a teaching position. We count all teachers who exit in all years as permanent exits from teaching. This potential over-counting of exiting teachers applies consistently to pre- and post-tenure reform years. We observe 11 percent of exiting teachers returning to teaching. Results including these teachers upon return are presented in Appendix B and are consistent with the main results.

to determine the school's accountability status.[9]

Empirical Strategies

Descriptive Year Fixed Effects Model

We begin with a descriptive estimation of aggregate changes in the probability of teacher exit after tenure reform. Next, we test whether these effects can be causally attributed to the tenure policy change by applying the theory outlined above in comparative analysis. We first estimate the probability that *any* teacher exits each summer from 2006 to 2015. During this time, there were multiple changes that could affect teacher exit, so we estimate teacher exit as a function of individual year indicators, with the final year before the passage of the first policy reform (2009) as our reference year. We estimate:

$$EXIT_{ijt} = \alpha_0 + \boldsymbol{\delta_t YEAR_t} + X_{it} + Z_{jt} + \varphi_j + \mu_j + \varepsilon_{it} \quad (7)$$

where $EXIT_{ijt}$ is equal to one if a teacher *i* in school *j* exits teaching in summer of year *t*. $EXIT_{ijt}$ is equal to zero if the teacher returns to any public school teaching position in Louisiana.[10,11] We estimate eq (7) and all other models as linear probability models.[12] $YEAR_t$ indicators allow us to observe teacher exit for four pre-policy reform years (summers of 2006 to 2009), two years after the announcement of the *evaluation reform* but prior to any implementation (2010 and 2011), two years *post-tenure reform* implementation *only* (2012 and 2013), and two years when some

[9] SPS calculations change across the time period under study, so SPS scores in this study are normalized within year and school level. Details on the calculations for each year are reported at https://www.louisianabelieves.com/resources/library/performance-scores.

[10] Throughout our analysis, we identify a teacher as an *exiter* in the summer if she did not return to teaching the following fall (e.g. a teacher "exits" in 2012 if she does not return to teaching in 2012-13). Thus, we identify summer 2012 as the first year of post-tenure reform exit. All years are identified by the summer after the academic year in our tables (for example, exit following the 2010-11 school year will be labeled 2011). Our statewide dataset allows us to observe teachers in any Louisiana TPS or charter school. We cannot observe whether teachers who exit are employed in private or out-of-state schools.

[11] Between 1-2% of teachers exit teaching to enter an administrative position in Louisiana public schools. Because we are focused on the supply of teachers, we code moves to administration as exits from teaching. Results are robust to alternative coding that counts these teachers as non-exiters (available on request).

[12] Results from logit models (not shown) are similar.

evaluation data were available but not high stakes (2014 and 2015). Significant and positive year effects ($\boldsymbol{\delta_t}$) in 2012 and 2013, relative to prior years, might indicate an increase in teacher exit specifically with the loss of job security. Year effects in 2014 and 2015, relative to pre-2012 years, might indicate a combined effects of tenure reform and access to data that could inform voluntary exit or future dismissal.

Because school conditions common to multiple teachers influence exit decisions, we include time-varying school characteristics (Z_{jt}), time-invariant school fixed effects (φ_j), and time-varying teacher-level controls (X_{it}). Thus, full specification of eq (7) estimates changes in probabilities of exit for similar teachers within the same school. We decompose estimation error into μ_j, which is unexplained variance correlated at the school level, and ε_i, which is random error, and adjust for μ_j with clustered standard errors within schools.[13]

Difference-in Differences Model

The patterns shown in the descriptive framework cannot identify if the relationship between teacher exit and tenure reform is causal, as the coefficients could be influenced by omitted factors. Ideally, we would be able to compare similar teachers affected by the tenure policy change to a group of teachers who were not. This is not feasible given the policy design, which simultaneously applied the reform to all traditional public school teachers in the state. Instead, we identify comparison groups of otherwise similar teachers whom we expect to be differentially affected by the loss of tenure. If post-policy exit responses are stronger for these groups relative to groups of otherwise similar teachers, it raises confidence that the response

[13] Traditional school-level clustering may be insufficient in the case of analyses using small Ns of clusters (in our case, schools). Although we doubt that our robust clustering strategy suffers from too few schools, we also estimated more conservative wild bootstrapped standard errors for our base specification. All reported results are robust to wild-bootstrapping, available upon request from the authors. See Cameron & Miller (2015) and Webb (2013) for an explanation of the procedure.

predicted in our descriptive model is due to the tenure policy change rather than to other policy or contextual factors.

To measure the effect of removing tenure protections, we exploit the idea that, in theory, not all teachers valued tenure equally. The theory outlined above suggests that three factors influence exit: 1) the risk of dismissal without protections; 2) the present value of guaranteed future employment; and 3) the value of an alternative source of guaranteed income. First, we consider teachers who vary in their risk of future dismissal due to poor performance. Here we compare teachers who, based our internal estimates of performance, fall into the top or bottom quintiles statewide. Teachers in the bottom quintile of effectiveness would face a substantially higher risk of dismissal absent tenure protections and therefore should place a higher value on tenure. While all teachers might find teaching less attractive without job protections, low-performing teachers will see the greatest value in immediate exit as doing so would enable them to build human capital and therefore increase their compensation in another sector or profession. Thus, we expect teachers with low effectiveness to be more responsive to the removal of tenure (through an increased probability of exit) than teachers with high effectiveness. To establish these comparison groups, we estimated a teacher fixed-effect in an estimation of student test scores, controlling for prior performance and student demographics (see Appendix A). We then divided teachers in quintiles based on their estimated effect on student test-score growth. Because these estimates are less precise across time in the middle of the distribution (Koedel and Betts, 2007), we compare top-quintile teachers (those with the biggest average gains in student performance) to bottom-quintile teachers (those with the lowest gains), while continuing to control for teacher and school characteristics, as in eq (7).

Second, we compare otherwise similar teachers who differ in the present value of future

guaranteed employment. Before the policy change, new teachers entered the profession with the expectation of tenure benefits in the future, while already-tenured teachers have certain benefits of tenure. Due to discounting of future benefits, already-tenured teachers unambiguously value tenure more than not-yet-tenured teachers do, as long as there is a positive risk premium attached to a lack of job security and a positive discount rate. Therefore, we should expect a greater response to the policy change among tenured than pre-tenure teachers. To make this comparison across teachers who are otherwise very similar, we compared teachers in their final pre-tenure years (2^{nd} and 3^{rd} year teachers) to teachers in their first post-tenure years (4^{th} and 5^{th} year teachers) in the year of reform. We expect these two groups to be similar in their response to other factors, but we expect fourth- and fifth-year already-tenured teachers to have a greater response to the loss of tenure than second- and third-year not-yet-tenured teachers. This analysis is somewhat different than the others we present in that we are evaluating the effect of tenure reform on a cohort of teachers that we follow throughout the panel rather than a yearly cross-section of teachers that belong to specific groups. This is because teachers that enter the profession after 2011 are aware of the evaluation policy reform and after 2012 are aware of the tenure reform, yet these teachers would appear in the analysis for years 2014 and 2015.

Third, we compare otherwise similar teachers who vary in the value of their next-best job alternative. While we cannot observe outside job offers, in the Louisiana context, all retirement-eligible teachers face a certain alternative income through the state's teacher pension plan. We operationalize this comparison group by examining attrition for teachers who are already eligible for full retirement benefits as a certain alternative to teaching relative to those who have substantial experience but are not yet eligible for retirement. Louisiana teachers in the period studied here were eligible for full retirement benefits after 25 years of teaching. We compare the

effects of tenure removal between otherwise similar teachers who are highly experienced but not-yet-eligible for retirement (10-19 years of teaching) and those who are fully eligible for retirement (25 or more years of teaching). Teachers with 20-24 years of experience were eligible for smaller, partial retirement benefits, so we repeat this analysis comparing those who are fully eligible to those who are partially eligible, and again comparing those who are partially eligible to those who are not yet eligible. We expect these very experienced teachers to have similar job market alternatives and a similar response to other policy changes, but we expect a greater response to the removal of tenure for the retirement-eligible groups who can exit with certain income.

Teachers might exit their positions for numerous reasons unrelated to state policy changes. For example, during the period studied, employment opportunities in other sectors were growing after the Great Recession and post-Katrina revival in the region. We use a difference-in-differences (DID) model to separate the effect of tenure removal on teacher mobility from other hard-to-observe factors that might have simultaneously influenced teacher exit. Specifically, this inference is made possible by comparing the change in the mean exit rate of teachers who should have been more affected by the reform to that of an otherwise comparable group of teachers who should have been less affected (Bloom 1999; Duflo, 2001; Shadish, Cook & Campbell, 2002). Our DID analysis estimates the following linear probability model:

$$EXIT_{ijt} = \alpha_0 + \delta_t YEAR_t + \gamma_0 RESPONDER_{it} + \beta_t YEAR_t * RESPONDER_{it} + X_{it} + Z_{jt} + \varphi_j + \mu_j + \varepsilon_{it} \quad (8)$$

where $RESPONDER_{it}$ is a dichotomous variable that identifies teacher *i* at time *t* as member of a group that we expect to be *more* responsive to the loss of tenure relative to a non-responder. We expect that both groups were similarly responsive to other unobserved factors that influence exit.

Table 1 describes our analytic subgroups, identifying responders and comparison teachers.

As in equation (7), $YEAR_t$ is a vector of year indicators. In the model, α_0 is the mean probability of exit for non-responders during the reference year 2009, and $\alpha_0 + \gamma_0$ is the baseline mean probability of exit for responders in that year. Thus, γ_0 measures average differences between the groups. β_t is a vector of our variables of interest for causal analysis, measuring differential deviations from the reference year for responders relative to non-responders. In this case, we are testing for a differential change in exit rates after tenure reform for teachers we expect to be more affected by the loss of tenure compared to teachers we expect to be less affected. The estimates also control for time-varying teacher characteristics (X_{ij}), time-varying school characteristics (Z_j), school fixed effects (φ_j), and decomposed school and random errors are estimated as described above.

Falsification Analyses

It is possible that omitted factors might influence any results from our main comparative analyses, causing our "responder" groups to have differential exit rates in response to the removal of tenure protections. To address these concerns, we run three falsification tests. First, we investigate whether information about teacher quality is influencing exits, rather than tenure reform. We do this by replicating eq (8) comparing teachers for whom we could estimate a growth-based performance measure (VAM) and teachers for whom we could not estimate a VAM. The Compass system was designed to provide a state-level VAM for teachers, but this is only feasible for teachers in tested grades and subjects with students with a pre-test score. For teachers who do not meet these requirements, a school-based growth measure would be substituted for VAMs. These measures could include other locally administered tests, student portfolio reviews, or other measures and would be designed by teachers with administrator

approval. Thus, the stakes attached to high-stakes evaluation were likely higher for teachers eligible for VAMs who could not control the data on which their evaluations were based. If teachers are responding to evaluation data, rather than the removal of tenure, we would expect a greater effect from teachers with VAMs than those without.

Second, we test the effects of the tenure reform on a group of teachers who were unaffected by the policy change. Louisiana charter school teachers are at-will employees of independent school operators. These teachers were never protected by the original tenure policy, and therefore, were unaffected by tenure reform. Charter teachers were subject to the requirement for Compass evaluation and the implementation of Common Core standards. These policy conditions generate a plausible comparison group of teachers who were fully affected by teacher evaluation policy but unaffected by tenure policy, so we estimate eq (7) for charter school teachers and expect no increase in exit in 2012 and 2013 for this group.

A third possible omitted factor could be an exogenous reduction in the size of the teacher workforce. We conduct a final falsification test to identify whether teacher exits were affected by exogenous changes in the demand for teachers, which could be triggered by reductions in student enrollment. Here we replace the dependent variable in equation (7) with time-varying measures of the number of students (total school enrollment). If the demand for teachers decreased concurrent with teacher policy changes, we would see a significant decline in the student population in those years.

V. Results

Descriptive Analysis

Table 2 reports descriptive statistics for all traditional public school teachers in Louisiana and then broken down into the six teacher subgroups that form the core of our causal analysis.

We report average characteristics in the pre-reform years of our panel (from summer 2006 to summer 2011, in Panel A) and in the post-reform years (from 2012 through 2015, in Panel B). In the pre-reform years, 6.9 percent of all TPS teachers exited, increasing to 8.5 percent in the years after tenure was removed.

Raw changes in exit rates for our three expected responder groups (least effective teachers, 4th and 5th year teachers, and retirement eligible teachers) are all greater than the changes in exit rates for comparison groups. The difference in the differences of the unadjusted exit rates shown in Table 1 suggest that least-effective teachers had a 1.1 percentage point greater change in exit rates between the two time periods than highly effective teachers, 4th and 5th year teachers had a 0.7 percentage point greater change in exit rates relative to 2nd and 3rd year teachers, and the same difference for retirement eligible versus ineligible teachers was 4.7 percentage points. The remaining summary statistics provided in Table 2 suggest that the comparison and responder groups vary on many covariates but these differences, although statistically significant, are typically small in magnitude. We now turn to our descriptive and DID analyses to substantiate these results.

Table 3 provides results from our descriptive analysis of all teachers and causal analysis of teacher subgroups.[14] All tabled coefficients are based on linear probability models and include both school fixed-effects and standard errors clustered at the school level. Although our data begin in 2006, not all analyses can be conducted for all pre-policy years. Specifically, teacher effectiveness measures could not be calculated until 2009, and the early career cohort comparison is only feasible beginning in 2010 and continuing to 2015. The exact years included in each analysis can be identified based on non-missing coefficients in Table 3. In all

[14] Tables 3-5 includes only the coefficients on implementing time and interactions. Results with full coefficients including all control variables are provided in Appendix C.

specifications, the omitted reference year is 2009 – the final year before the passage of teacher evaluation reform. We are most interested in estimated effects in 2012 and 2013, during which time tenure reform was implemented without teacher evaluation data. We might also see changes in exit due to the passage of the evaluation reform (2010 and 2011) or due to the availability of non-high-stakes evaluation results (2014 and 2015).

Panel A (Column 1) of Table 3 provides results estimating eq (7) for the probability of teacher exit for all TPS teachers across all years of our panel. Ceteris paribus, estimated teacher exit probabilities were very similar before reform (2009) and after the passage of the evaluation reform, with no increase in 2010 and a small (but significant) increase of half a percentage point in 2011. In 2012, teacher tenure was removed, and estimated exit probabilities increased significantly and more substantially by 2.1 percentage points in 2012 and remained higher by 2.9 percentage points in 2013 (both relative to 2009). In the final period when evaluation data were available, exit probabilities remained at around 2.2 to 2.4 points above 2009 levels, but did not increase relative to the period of tenure reform only.

These results suggest that tenure reforms were associated with an increase in the probability of teacher exit, controlling for teacher characteristics and school settings. Post-tenure teacher exit rates increased and remained at a fairly stable elevated level when non-binding teacher evaluation results were made available. This timing suggests that the tenure reform was more influential than the evaluation data, but we cannot yet determine if tenure reform *caused* an increase in teacher exits. To do so, we turn to results from our comparative analyses.

Difference-in-Differences Models

Panel B (Columns 2-5) of Table 3 provides estimated year effects from eq (8) for our three teacher subgroups groups. For each specification, the coefficients of interest are the

interaction between the responder indicator and year indicators. These coefficients indicate a differential effect in post-reform years for the groups we expect to be more responsive to tenure reforms. A visual comparison of estimated exit probabilities for each responder type and its comparison group is provided in Figure 1.

Differential Responses Based on Teacher Effectiveness

The estimated effects of tenure reform on teachers with low and high VAMs are provided in Table 3, Column 2 and illustrated in Figure 1, Panel A. We identify low VAMs as those in the bottom quintile statewide, and high VAMs as those in the top quintile. Assuming that our internally-estimated VAM appropriately proxies for teachers' assessment of their own effectiveness, our findings are consistent with the theory and previous empirical evidence that less-effective teachers (those at greater risk of future dismissal under high-stakes evaluation) are more likely to voluntarily exit (e.g., Goldhaber, Gross & Player, 2010; Feng & Sass, 2017). Teachers with low VAMS were, on average, approximately 2 percentage points more likely to exit than teachers with high VAMs across all years of the analysis. While there was no statistically significant change in this gap in the initial post-evaluation reform years of 2010 and 2011, the gap between the two groups increased to 3.8 percentage points ($p<0.05$) in 2012 and to 4.5 percentage points ($p<0.05$) in 2013, the years in which the tenure reforms were enacted and before high-stakes evaluation outcomes were available. Then, in 2014, when evaluation data were available, the difference between these groups was again statistically similar to pre-reform levels. During this time period, the coefficients on year indicators indicate the there was no significant effect of tenure reform on teachers with high VAMs. In summary, we observe that the increase in exit probabilities in the period of tenure reform was concentrated among teachers with low estimated effectiveness, and therefore a higher probability of future dismissal,

compared to no estimated effect on teachers with high estimated effectiveness. The timing of these effects is important. Teachers responding to evaluation could have exited when the evaluation reform was passed or waited until evaluation data were provided. Instead, we see the differential increase in exits only immediately following the removal of tenure. This provides initial evidence that tenure reform, and not the evaluation policy, was the trigger for the observed increase in exit rates throughout the state.

Differential Response Rate for Tenured vs. Untenured Early-Career Teachers

The estimated exit rates for early-career teachers with and without tenure over time are illustrated in Figure 1, Panel B, based on coefficients from the DID analysis that are presented in Table 3, Column 3. Here we compare recently tenured teachers who lost these protections with similar teachers who had not yet gained tenure. Our findings are consistent with the theory that newly tenured teachers exited at a higher rate post-reform because they valued the loss of tenure significantly more than non-tenured teachers valued the loss of future tenure. We cannot rule out an alternative hypothesis that non-tenured teachers, who have less experience, might also have more uncertainty about whether their performance will improve over time or how their principals might respond to greater autonomy in dismissal decisions. While the pre-reform period is shortened due to data restrictions in this group, prior to tenure reform, we estimate that untenured teachers are more likely to exit than recently tenured teachers by 2 to 4 percentage points. Similar to the first comparison, we find no statistically significant effect of tenure reform on untenured teachers, but significantly larger effects on recently tenured teachers by 4.0 percentage points in 2012, and 3.0 percentage points in 2013. This effectively closed the gap in exit rates for the two groups temporarily. Figure 1 illustrates that the gap returned by 2014, which again suggests that teachers were responding to the unique occurrence of the removal of tenure and not

anticipation of high-stakes evaluation.

Differential Response Rates for non-Eligible and Retirement-Eligible Late-Career Teachers

We next compare teachers with a certain income upon exit (25 or more years of experience) and teachers with significant experience, but not enough to access certain income through retirement (10-19 years of experience). Estimated coefficients are displayed in Table 3, Column 4, and illustrated in Figure 1, Panel C. As expected, retirement-eligible teachers are substantially more likely to exit in pre-reform years by 8 to10 percentage points. We see a small increase in exit rates with the initial passage of evaluation reform. This effect is less than 1 percentage point among non-eligible teachers ($p<0.05$ in 2011 only) and 1to 2 percent points higher ($p<0.01$) among retirement-eligible teachers. Exit rates among ineligible teachers increase again with the removal of tenure in 2012 by 1.4 to 1.9 percentage points ($p<0.01$). This suggests that there was small response to tenure removal among all experienced teachers. However, the estimated gap between ineligible and retirement-eligible teachers grew substantially to 5.0 percentage points 2012 and 6.0 percentage points in 2013. In 2013 and 2014, the gap was still large, but it started to shrink closer to pre-reform levels. This evidence also suggests that the increase in teacher exit in 2012 was due to tenure reform. As expected, we see that teachers with certain alternative retirement income were more responsive than those without this alternative. The differential effect we observe at the time of evaluation reforms and the arrival of evaluation data is substantially smaller than the differential effect we observe in the years immediately following the removal of tenure.

Because Louisiana offers some teachers access to partial retirement benefits, we replicated this analysis comparing teachers eligible for full retirement benefits to those eligible for partial benefits (Table 3, Column 5) and comparing teachers with partial retirement benefits

to those with none (Table 3, Column 6). These results confirm the core analysis from Column 4. As expected based on their opportunities for alternative income, we find the largest effect of tenure reform among teachers eligible for full retirement benefits, but even teachers eligible for partial benefits were more likely to exit after tenure reform relative to those with no eligibility.

Varying Impacts of Effectiveness on Teachers with Different Levels of Experience

Because we find that both late career and low VAM teachers are more likely to exit, we also examine whether or not the tenure reform appeared to have differential effects on the least and most effective teachers at different points of their careers. Table 4 replicates the results from Table 3, Column 2 for teachers at various experience levels. The responder has VAMs in the bottom quintile statewide; the comparison group has VAMs in the top quintile. We find that there are no differential rates of attrition based on effectiveness for teachers in the early years of their career (0-3 years and 4-9 years) or for retirement eligible teachers (25 or more years of experience). Low VAM mid-career teachers (10-19 years of experience) and teachers who are eligible for partial retirement (20-24 years) display that largest changes in differential exit rates after tenure reform based on VAMs. These results are unexpected given that mid-career teachers and those approaching full retirement benefits may have the most to gain by a few additional years of employment, despite the risk of future dismissal. However, their experience may also give them better alternative job opportunities in education administration or support organizations.

Falsification Tests

Results for our three falsification tests are displayed in Table 5. All three results support our conclusion that there was a direct and unique response to tenure reform, beyond a broad response to teacher policy reforms in Louisiana. First, we examine whether teachers in subjects

and grades where VAMs can be calculated were more responsive. If the evaluation policy is the cause of teachers exits, we would expect a greater response among teachers for whom VAMs can be estimated, so these teachers are the responder group and teachers without VAMs are the comparison group. We find that teachers with VAMs are always less likely to exit than teachers without VAMs, and that the gap between the two groups is statistically similar across all years of reform (Table 4, Column 1). This is illustrated in Figure 1, Panel D.

Next, we check for a concurrent exit increase among charter school teachers, who were subject to the full evaluation policy reform but not the tenure reform. While teacher exit is more volatile in charter schools overall, we find no statistically significant increases in teacher exit in 2012 or 2013. Instead, there is a small increase of 1.9 percentage points with the arrival of evaluation data in 2014 ($p<0.10$) that increases to 4.1 percentage points in 2015 ($p<0.01$). This would be expected if charter teachers are affected by the evaluation reform and the availability of evaluation outcomes, as they were in 2010 and 2014. However, we find no evidence that teachers unaffected by tenure reform had an exit response caused by other factors in the policy context in 2012.

Finally, we test for a coincidental reduction in the demand for teachers by estimating equation (7) with student enrollment as the dependent variable. If student enrollment dropped at the same time as tenure reform, we should see a significant, negative coefficient for 2012, relative to the omitted pre-reform year. Instead, we an insignificant but positive coefficient for 2012, and significant and positive coefficients in 2013, 2014, and 2015. This suggests that the demand for teachers was stable or increasing through the post-tenure reform period, and a reduction in the demand for teachers is not a plausible alternative explanation for the increase in teacher exits.

VI. Discussion and Policy Implications

There has been substantial debate about the value of teacher job protections. Those who support teacher tenure argue that tenure is an important part of teacher compensation, augmenting teachers' salaries with enhanced job stability. However, opponents of teacher employment protections have advocated for reform or removal of state tenure laws because they believe that such laws protect ineffective teachers, causing them to remain in the profession when they would otherwise exit and make way for individuals who are more adept at teaching.

While the effect of tenure protections on the teacher workforce is an empirical question, it has been difficult to examine given the widespread and long-term existence of stable tenure protections. Using Louisiana's rapid and comprehensive removal of tenure protections as a case study, we provide the first empirical evidence about the effects of removing teacher job security separately from teacher evaluations on teacher exit. We find that the removal of tenure led to an increase in teacher exit immediately after implementation. Moreover, we find predictably larger jumps in exit rates for teachers who have more to lose in terms of overall compensation. These findings hold for three specific teacher populations with differential benefits from tenure: those that receive the greatest protection from tenure; teachers who lost tenure protections compared to similar teachers who never had tenure; and teachers who had alternative options such as defined income retirement eligibility compared to similar teachers who did not. However, and importantly, our results show that teachers with the lowest VAMs – those who are least effective at improving student achievement on standardized tests – are more likely to exit once tenure protections are removed. The reform had no significant impact on the most effective teachers. Of course, standardized test scores and teachers' abilities to improve them are not the only, or even the most important, measure of teacher effectiveness. Nonetheless our results show that, at least

according to this measure of teacher quality, the removal of tenure protections leads to greater attrition of low-performing teachers.

The primary limitation to this work is that it is difficult to separate the effect of the removal of tenure from the implementation of a teacher evaluation system. Here, we attempt to isolate the effect of the removal of job security by focusing on a period before teacher evaluations were available for use. However, this empirical issue may be less relevant for policy. All states that have diminished or removed tenure protections have done so in combination with the implementation of teacher evaluation systems that have high stakes for teacher employment. Even if we are unable to fully identify the causal effects of the removal of job security alone, our results are generalizable to policy contexts in other states that include both reductions in job security and enhanced teacher evaluation.

Another potential source of bias stems from the timing of Louisiana's tenure reform at the tail end of the Great Recession. It is likely that some retirement-eligible teachers postponed retirement during the Great Recession for a variety of fiscal reasons. If this occurred, the overall exits and the retirement rate in particular might have increased around the time of tenure reform as the economy improved. If this is the case, then our overall descriptive and DiD sub-analyses that compare retirement-eligible with partially-eligible and ineligible teachers may overstate the impact of tenure reform on retirement-eligible teachers' attrition. However, this history should not affect our early-career teacher analyses, as there is no reason to think that 4th-year teachers would be differentially affected by the Great Recession relative to 3rd-year teachers. Our other results should also be unaffected by this concern.

There are important implications of these results for other states considering a similar removal of teacher tenure. First, we show that the removal of tenure protections will result in

substantial teacher attrition. There may be some concern about these teacher exits given that the literature shows that teacher turnover can be harmful to teachers who remain in their schools and to students (e.g., Guin, 2004; Ronfeldt, Loeb & Wyckoff, 2013; Ost, 2014; Haunshek, Rivkin & Schiman, 2016). However, teacher turnover may not always be a bad thing if it causes the profession to lose less effective employees who theoretically face the greatest increased risk for dismissal, assuming a sufficient supply of teachers who can replace those who exit. Our results from Louisiana support Loeb et al.'s (2015) findings from New York City's tenure reform; less-effective teachers are more likely to voluntarily exit the system when tenure protections are removed. Thus, policymakers will want to be prepared for teacher exits that result from the removal of employment protections by working to recruit higher-quality teachers to replace those who leave.

Of course, being prepared to hire new teachers may be challenging given Kraft et al.'s (2020) findings that teacher tenure and evaluation reforms diminish incoming teacher supply (although potentially also predominantly of lower-performing teachers). Moreover, most states are facing teacher shortages, particularly in schools and districts located in urban and remote areas and with the lowest-performing students and for secondary math and science teachers as well as teachers of students with disabilities (e.g., Cowan et al., 2016; Sutcher, Darling-Hammond, & Carver-Thomas, 2016;). Given these contextual challenges, state policymakers will need to consider how to replace exiting teachers with more effective teachers, especially in these "hard-to-staff" positions, schools, and districts.

Our results also imply costs to the public education system that should be considered. NCTAF (2007) estimates that the cost to recruit, hire, and train a new teacher is between $4,366 to $17,872 per new teacher, depending on the geographic context. Even if these are over-

estimates of the true replacement costs for teachers, state and district policymakers will need to be prepared to absorb additional costs fairly quickly after the passage of a similar tenure reforms. In addition, by pushing teachers to retire earlier, states lose pension contributions and must pay out more benefits. Such shocks to pension systems can cause pension funding shortfalls, dramatically impacting state coffers and districts' future abilities to compensate teachers (Backes et al., 2015). However, in the short-run, districts might see lower payroll costs as higher-paid, more experienced teachers retire earlier.

None of this is to say that states should not consider removing tenure protections for teachers, especially if they institute mechanisms by which to identify the most effective employees and then work to retain them, but it is likely necessary to plan ahead to cushion the effects of increased exit in the short-run. To that end, it is important for policymakers to view these protections as part of what teachers value about their jobs. Our results suggest that when tenure is removed, some teachers may view its loss as worthy of exiting the profession. State and local education agencies might need to provide alternative compensation – either in the form of salary or other working conditions – to induce current teachers who they wish to retain (such as experienced teachers, particularly in shortage areas) to remain in public schools and to facilitate recruitment. It is hard to know the monetary value teachers place on tenure, although Brunner and Imazeki (2010) provide estimates of the value of additional years of probationary employment. Our results suggest that states can target compensating salary or other incentives towards those teachers most at risk of exit who they may want to retain (e.g., very senior teachers) and to new entrants they most want to recruit.

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Figure 1: Estimated exit probabilities by responder types and comparison groups

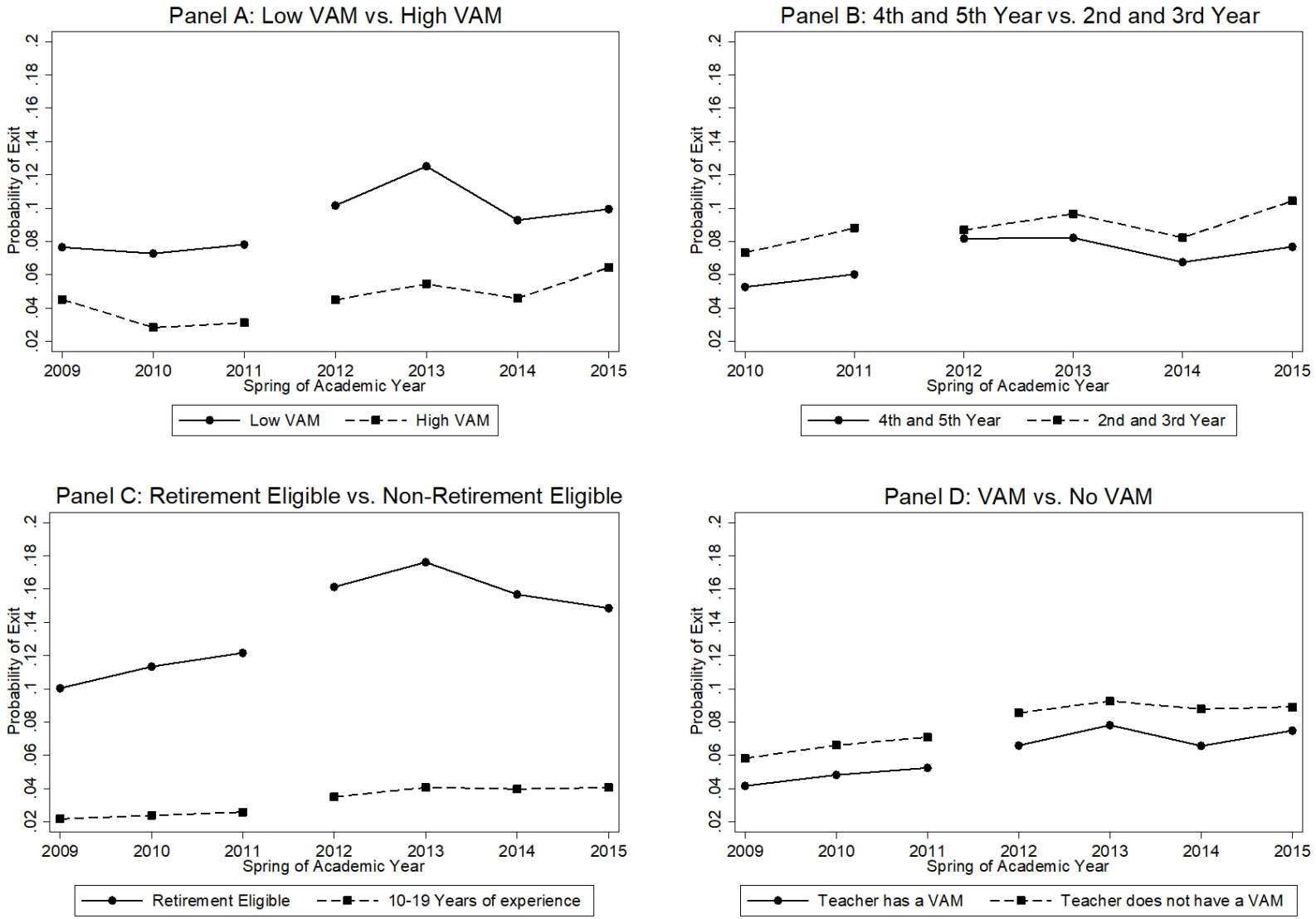

Table 1: Analytic Groups

Analytic Group	Responder	Comparison
Effectiveness	Bottom quintile of teacher effectiveness	Top quintile of teacher effectiveness
Early Career	2009 and 2008 cohort of teachers (4th and 5th year teacher cohorts in year of tenure reform)	2011 and 2010 cohort of teachers (2nd and 3rd year teacher cohorts in year of tenure reform)
Later Career (Full vs. Non)	Retirement eligible teachers (25+ years of experience)	Non-retirement eligible teachers with higher levels of experience (10-19 years of experience)
Later Career (Full vs. Partial)	Retirement eligible teachers (25+ years of experience)	Partial eligible teachers with higher levels of experience (20-14 years of experience)
Later Career (Partial vs. Non)	Partial eligible teachers with higher levels of experience (20-14 years of experience)	Non-retirement eligible teachers with higher levels of experience (10-19 years of experience)

Table 2: Summary Statistics by Teacher Group, 2006-2015

Panel A: 2006-2011 (Pre-Tenure Reform)

		By Effectiveness		By Tenure Eligibility		By Retirement Eligibility	
	TPS	Comparison Top Quintile	Responder Bottom Quintile	Comparison 2-3 years Not tenured	Responder 4-5 years Tenured	Comparison 10-19 years Not eligible	Responder >24 years Eligible
Teacher exit	0.069	0.036	**0.079**	0.083	0.058	0.028	**0.118**
Teacher characteristics							
Years of experience	15.0	13.6	**12.9**	1.3	**1.6**	14.3	**32.7**
Female	0.826	0.952	**0.832**	0.779	**0.791**	0.837	0.834
Black	0.213	0.201	0.224	0.205	0.211	0.166	**0.284**
Other minority race	0.018	0.013	**0.022**	0.070	**0.081**	0.013	**0.005**
Bachelors degree only	0.681	0.750	0.732	0.809	0.815	0.710	**0.469**
SPED certification	0.247	0.102	**0.147**	0.192	0.193	0.253	**0.286**
STEM certification	0.130	0.059	**0.177**	0.178	0.176	0.140	**0.085**
Alternative Preparation	0.007	0.012	**0.020**	0.070	**0.055**	0.000	0.000
In-state college graduate	0.929	0.941	**0.914**	0.721	0.734	0.983	0.992
School characteristics							
Percent black	45.0	42.9	**47.0**	53.9	54.3	40.3	**46.7**
Percent other minority	5.3	6.9	**5.5**	6.0	5.9	5.2	5.5
Percent free/reduced price lunch	63.1	67.8	68.6	71.0	70.6	60.9	**62.6**
Percent limited English proficient	1.6	2.1	**1.5**	1.7	1.7	1.5	**1.8**
Percent special education	14.0	13.4	**12.8**	13.4	13.5	14.1	**14.5**
Percent gifted	3.5	3.5	**4.2**	3.2	3.1	3.7	3.8
School performance z-score	0.062	0.393	**-0.139**	-0.158	-0.160	0.181	**0.157**
Number of observations	279,305	5,690	6,077	18,071	17,896	71,227	77,689

Panel B: 2012-2015 (Post-Tenure Reform)

		By Effectiveness		By Tenure Eligibility		By Retirement Eligibility	
	TPS	Comparison Top Quintile	Responder Bottom Quintile	Comparison 2-3 years Not tenured	Responder 4-5 years Tenured	Comparison 10-19 years Not eligible	Responder >24 years Eligible
Teacher exit	0.085	0.051	**0.105**	0.095	**0.077**	0.038	**0.175**
Teacher characteristics							
Years of experience	14.6	13.6	**12.7**	2.5	**4.9**	14.3	**33.8**
Female	0.825	0.957	**0.844**	0.781	**0.805**	0.835	0.838
Black	0.194	0.170	**0.233**	0.192	0.201	0.191	**0.227**
Other minority race	0.028	0.018	0.025	0.037	**0.079**	0.023	**0.013**
Bachelors degree only	0.672	0.720	0.703	0.741	**0.698**	0.683	**0.510**
SPED certification	0.250	0.120	**0.153**	0.194	**0.233**	0.261	**0.312**
STEM certification	0.153	0.063	**0.249**	0.193	0.192	0.145	**0.109**
Alternative Preparation	0.010	0.009	**0.017**	0.043	**0.019**	0.000	0.000
In-state college graduate	0.903	0.934	**0.903**	0.752	**0.816**	0.966	**0.985**
School characteristics							
Percent black	42.7	37.9	**46.7**	48.0	47.9	39.5	**42.4**
Percent other minority	8.6	10.1	**7.3**	8.0	8.1	8.7	9.1
Percent free/reduced price lunch	65.7	65.9	**70.1**	68.5	68.3	64.2	63.9
Percent limited English proficient	2.2	2.9	**1.8**	2.0	2.1	2.1	**2.4**
Percent special education	11.9	11.3	11.4	11.8	11.9	11.8	12.6
Percent gifted	3.7	3.7	**4.3**	3.3	3.3	3.8	**4.2**
School performance z-score	0.147	0.470	**-0.149**	0.016	0.023	0.214	**0.273**
Number of observations	173,282	10,473	10,566	19,595	18,322	49,809	37,193

Source: Author calculations from Louisiana administrative data. Data available for teacher effectiveness beginning in 2008-09. Prior to 2012, Louisiana teachers were eligible for tenure in their 4th year of teaching. Louisiana teachers are eligible for partial retirement benefits in their 20th year a public school teacher experience, and for full retirement benefits in their 25th year. Responder group means in bold represent significant differences from their respective comparison group at the $p<.05$ level.

Table 3: Model Estimates of the Probability of Teacher Exit

	Panel A: All Teachers	Panel B: Main Comparison Groups		Panel C: Retirement Sub-groups		
	(1)	(2)	(3)	(4)	(5)	(6)
	All Teachers	**Low** vs. High VAM	**Tenured** vs. Untenured	**Full** vs. 10-19	**Full** vs. Partial	**Partial** vs. 10-19
2006	0.017***			0.008***	0.007	0.009***
	(0.002)			(0.002)	(0.005)	(0.002)
2007	0.003			0.003	0.000	0.004*
	(0.002)			(0.002)	(0.005)	(0.002)
2008	0.006***			0.003	-0.001	0.004*
	(0.002)			(0.002)	(0.004)	(0.002)
2010 (Evaluation)	0.003*	-0.011	-0.029***	0.003	0.010**	0.002
	(0.002)	(0.008)	(0.008)	(0.002)	(0.005)	(0.002)
2011	0.009***	-0.008	-0.014*	0.005**	0.015***	0.005**
	(0.002)	(0.009)	(0.008)	(0.002)	(0.005)	(0.002)
2012 (Tenure)	0.025***	0.007	-0.013	0.014***	0.031***	0.014***
	(0.002)	(0.009)	(0.010)	(0.003)	(0.005)	(0.002)
2013	0.032***	0.011	-0.002	0.018***	0.052***	0.020***
	(0.002)	(0.009)	(0.009)	(0.003)	(0.006)	(0.003)
2014	0.025***	0.004	-0.012	0.017***	0.029***	0.018***
	(0.002)	(0.010)	(0.012)	(0.003)	(0.005)	(0.003)
2015	0.028***	0.018*	0.013	0.017***	0.025***	0.019***
	(0.002)	(0.010)	(0.011)	(0.003)	(0.005)	(0.003)
Responder		0.027**	-0.039***	0.073***	0.058***	0.016***
		(0.012)	(0.008)	(0.003)	(0.004)	(0.003)
Responder*2006				-0.013***	-0.014**	-0.000
				(0.004)	(0.006)	(0.005)
Responder*2007				-0.017***	-0.014**	-0.002
				(0.004)	(0.006)	(0.005)
Responder*2008				-0.002	0.002	-0.005
				(0.004)	(0.006)	(0.005)
Responder*2010		0.007	0.007	0.011**	0.003	0.007
		(0.014)	(0.011)	(0.004)	(0.006)	(0.005)
Responder*2011		0.014	0.015	0.027***	0.018***	0.008
		(0.013)	(0.010)	(0.005)	(0.006)	(0.005)
Responder*2012		0.028**	0.038***	0.051***	0.036***	0.015***
		(0.014)	(0.012)	(0.005)	(0.007)	(0.006)
Responder*2013		0.035**	0.027**	0.060***	0.029***	0.031***
		(0.014)	(0.012)	(0.005)	(0.007)	(0.006)
Responder*2014		0.020	0.025*	0.043***	0.033***	0.011**
		(0.014)	(0.013)	(0.005)	(0.006)	(0.005)
Responder*2015		0.011	0.013	0.034***	0.028***	0.006
		(0.015)	(0.013)	(0.005)	(0.007)	(0.005)
Constant	0.137***	0.183***	0.064	0.074***	0.109***	0.036***
	(0.010)	(0.037)	(0.045)	(0.014)	(0.027)	(0.011)
N	503,479	32,806	29,223	231,388	159,046	149,186
N Schools	1,651	1,179	1,421	1,641	1,632	1,615
N Teachers	84,955	16,034	17,072	46,038	29,555	30,432

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Names in bold in the table headers are the responder groups. Regressions include school fixed effects and controls for time-varying teachers and school characteristics (see full results in Appendix C1). Standard errors are clustered at the school level.

Table 4: Teacher Attrition and the Effect of Tenure Reform 2009-2015 – Teacher Effectiveness by Experience Group

	0-3 Years	4-9 Years	10-19 Years	20-24 Years	25+ Years
	(1)	(2)	(3)	(4)	(5)
2010	-0.020	-0.025	0.004	0.004	0.027
	(0.033)	(0.015)	(0.010)	(0.018)	(0.032)
2011	0.003	-0.022	-0.001	-0.026	0.075**
	(0.033)	(0.017)	(0.011)	(0.017)	(0.035)
2012	0.033	-0.002	-0.001	-0.001	0.113***
	(0.035)	(0.017)	(0.012)	(0.020)	(0.040)
2013	-0.006	-0.006	0.011	0.024	0.120***
	(0.036)	(0.018)	(0.013)	(0.023)	(0.039)
2014	0.014	-0.001	-0.001	0.013	0.116***
	(0.037)	(0.018)	(0.012)	(0.024)	(0.041)
2015	0.060	0.008	0.011	-0.004	0.131***
	(0.037)	(0.019)	(0.014)	(0.026)	(0.042)
Responder	0.053	0.034	0.007	-0.011	0.041
	(0.043)	(0.026)	(0.013)	(0.051)	(0.055)
Responder*2010	0.001	-0.010	0.007	0.035	0.036
	(0.047)	(0.027)	(0.015)	(0.055)	(0.055)
Responder*2011	-0.024	-0.001	0.019	0.066	0.033
	(0.046)	(0.027)	(0.015)	(0.050)	(0.058)
Responder*2012	-0.032	-0.008	0.026*	0.100*	0.042
	(0.048)	(0.028)	(0.015)	(0.058)	(0.062)
Responder*2013	0.016	0.012	0.032*	0.107*	0.082
	(0.050)	(0.029)	(0.016)	(0.057)	(0.062)
Responder*2014	-0.022	0.003	0.036**	0.056	0.042
	(0.048)	(0.030)	(0.015)	(0.057)	(0.062)
Responder*2015	-0.065	0.004	0.035*	0.110*	-0.004
	(0.048)	(0.030)	(0.018)	(0.061)	(0.063)
Constant	0.119	0.237***	0.140**	1.517	-0.657***
	(0.112)	(0.082)	(0.067)	(1.359)	(0.238)
N	6,256	8,576	9,977	3,095	4,902
N Schools	1,012	1,072	1,058	801	890
N Teachers	4,463	5,080	5,155	1,782	2,492

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Names in bold in the table headers are the responder groups. Regressions include school fixed effects and controls for time-varying teachers and school characteristics (see full results in Appendix C2). Standard errors are clustered at the school level.

Table 5: Falsification Tests

	VAM vs. no VAM	Charter Schools	Student Enrollment
2006			47.905***
			(4.257)
2007		-0.041	12.373***
		(0.027)	(4.135)
2008		-0.004	6.818***
		(0.011)	(2.397)
2010	0.006***	-0.008	-0.614
	(0.002)	(0.011)	(2.501)
2011	0.012***	0.017	2.372
	(0.002)	(0.013)	(3.633)
2012	0.028***	0.012	11.191**
	(0.003)	(0.012)	(4.717)
2013	0.034***	0.012	19.646***
	(0.002)	(0.012)	(5.614)
2014	0.029***	0.018	25.257***
	(0.002)	(0.012)	(6.533)
2015	0.030***	0.047***	27.447***
	(0.002)	(0.013)	(7.362)
Responder	-0.003		
	(0.004)		
Responder*2010	-0.005		
	(0.004)		
Responder*2011	-0.004		
	(0.004)		
Responder*2012	-0.005		
	(0.005)		
Responder*2013	0.001		
	(0.005)		
Responder*2014	-0.007		
	(0.005)		
Responder*2015	0.000		
	(0.005)		
Constant	0.114***	0.261**	656.195***
	(0.012)	(0.115)	(40.790)
N	351,048	23,260	495,064
N Schools	1,574	132	1,553
N Teachers	72,819	8,451	84,082

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Names in bold in the table headers are the responder groups. Regressions include school fixed effects and controls for time-varying teachers and school characteristics (see full results in Appendix C3). Standard errors are clustered at the school level.

Appendix A: Teacher Value-Added Model

For a given teacher j, student i, classroom c and school year t, we estimate a standard value-added model:

$$A_{it} = \mu + \alpha A_{it-1} + \beta X_{it} + \rho C_{ct} + \theta_{jt} + \varepsilon_{it}$$

- A_{it}: post-score
- A_{it-1}: pre-score
- X_{it}: student characteristics
- C_{it}: classroom characteristics
- θ_{jt}: value-added of teacher j in year t
- ε_{it}: error term for student i in year t

The model is estimated by year (2009-2015) and subject (math, ELA, science, social studies).

Following Guarino et al. (2015), the above value-added model can be re-written as:

$$y = X\gamma + Zb + u$$

X includes student demographics and prior test scores. Z includes course taking dummies. u contains the unobserved student-specific effects. b is the vector of teacher effects.

The shrunken value-added estimate for teacher j is then:

$$\hat{b}_j = \left(\frac{\sigma_b^2}{\sigma_b^2 + (\sigma_u^2/N_j)}\right)(\bar{y}_j - \bar{x}_j\hat{\gamma})$$

Let $c \equiv \frac{\sigma_b^2}{\sigma_b^2+(\sigma_u^2/N_j)}$. It represents the shrinkage factor. σ_b^2 is the variance of the teacher effects, b_j. σ_u^2 is the variance of the student-level error, u. N_j is the number of students taught by teacher j. $\bar{y}_j - \bar{x}_j\hat{\gamma}$ is the unshrunken estimate.

Appendix B1: Model Estimates of the Probability of Teacher Exit – Replication with Permanent and Temporary Exits

	Panel A: All Teachers	Panel B: Main Comparison Groups		Panel C: Retirement Sub-groups		
	(1)	(2)	(3)	(4)	(5)	(6)
	All Teachers	**Low** vs. High VAM	**Tenured** vs. Untenured	**Full** vs. 10-19	**Full** vs. Partial	**Partial** vs. 10-19
2006	0.032***			0.019***	0.018***	0.020***
	(0.002)			(0.003)	(0.006)	(0.003)
2007	0.011***			0.009***	0.004	0.010***
	(0.002)			(0.003)	(0.005)	(0.003)
2008	0.009***			0.004	-0.003	0.005**
	(0.002)			(0.002)	(0.005)	(0.002)
2010 (Evaluation)	0.005**	-0.006	-0.029***	0.003	0.010**	0.002
	(0.002)	(0.009)	(0.009)	(0.003)	(0.005)	(0.003)
2011	0.011***	-0.005	-0.002	0.006**	0.012**	0.005**
	(0.002)	(0.009)	(0.010)	(0.003)	(0.005)	(0.003)
2012 (Tenure)	0.024***	0.004	-0.030***	0.015***	0.030***	0.014***
	(0.002)	(0.010)	(0.011)	(0.003)	(0.005)	(0.003)
2013	0.029***	0.014	-0.019*	0.020***	0.049***	0.021***
	(0.002)	(0.010)	(0.010)	(0.003)	(0.006)	(0.003)
2014	0.017***	0.003	-0.037***	0.013***	0.025***	0.014***
	(0.002)	(0.011)	(0.013)	(0.003)	(0.005)	(0.003)
2015	0.012***	0.009	-0.019*	0.007**	0.017***	0.008***
	(0.002)	(0.011)	(0.011)	(0.003)	(0.005)	(0.003)
Responder		0.027*	-0.049***	0.073***	0.060***	0.015***
		(0.014)	(0.009)	(0.003)	(0.004)	(0.004)
Responder*2006				-0.006	-0.007	-0.001
				(0.005)	(0.007)	(0.006)
Responder*2007				-0.009**	-0.006	-0.004
				(0.005)	(0.006)	(0.006)
Responder*2008				0.003	0.009	-0.008
				(0.005)	(0.006)	(0.005)
Responder*2010		0.019	0.009	0.011**	0.005	0.006
		(0.016)	(0.012)	(0.005)	(0.006)	(0.005)
Responder*2011		0.021	0.010	0.029***	0.024***	0.004
		(0.015)	(0.012)	(0.005)	(0.006)	(0.005)
Responder*2012		0.032**	0.057***	0.053***	0.041***	0.011*
		(0.016)	(0.013)	(0.006)	(0.007)	(0.006)
Responder*2013		0.040**	0.041***	0.059***	0.033***	0.026***
		(0.016)	(0.013)	(0.006)	(0.008)	(0.006)
Responder*2014		0.021	0.038***	0.041***	0.032***	0.010*
		(0.016)	(0.014)	(0.005)	(0.007)	(0.006)
Responder*2015		0.007	0.023*	0.033***	0.026***	0.008
		(0.016)	(0.014)	(0.005)	(0.007)	(0.006)
Constant	0.164***	0.203***	0.077	0.083***	0.114***	0.039***
	(0.012)	(0.038)	(0.049)	(0.016)	(0.027)	(0.013)
N	503,479	32,806	29,223	231,388	159,046	149,186
N Schools	1,651	1,179	1,421	1,641	1,632	1,615
N Teachers	84,955	16,034	17,072	46,038	29,555	30,432

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Names in bold in the table headers are the responder groups. Regressions include school fixed effects and controls for time-varying teachers and school characteristics. Standard errors are clustered at the school level.

Appendix B2: Teacher Attrition and the Effect of Tenure Reform 2009-2015 with Teacher Effectiveness by Experience Group – Replication with Temporary and Permanent Exits

	0-3 Years	4-9 Years	10-19 Years	20-24 Years	25+ Years
	(1)	(2)	(3)	(4)	(5)
2010	-0.016	-0.003	0.014	-0.027	0.024
	(0.038)	(0.016)	(0.011)	(0.029)	(0.033)
2011	0.018	-0.004	-0.001	-0.048	0.075**
	(0.039)	(0.018)	(0.011)	(0.029)	(0.036)
2012	0.035	0.006	-0.004	-0.032	0.113***
	(0.041)	(0.018)	(0.012)	(0.032)	(0.040)
2013	0.001	0.009	0.021	-0.004	0.124***
	(0.042)	(0.020)	(0.014)	(0.032)	(0.039)
2014	0.027	0.010	0.002	-0.016	0.107**
	(0.043)	(0.020)	(0.013)	(0.034)	(0.042)
2015	0.046	0.010	0.004	-0.032	0.123***
	(0.042)	(0.020)	(0.015)	(0.035)	(0.043)
Responder	0.047	0.060*	0.005	0.005	0.026
	(0.048)	(0.032)	(0.014)	(0.069)	(0.056)
Responder*2010	0.026	-0.029	0.011	0.028	0.075
	(0.052)	(0.034)	(0.018)	(0.074)	(0.056)
Responder*2011	-0.004	-0.021	0.029*	0.046	0.059
	(0.051)	(0.034)	(0.016)	(0.070)	(0.059)
Responder*2012	-0.014	-0.019	0.040**	0.082	0.064
	(0.052)	(0.035)	(0.017)	(0.074)	(0.062)
Responder*2013	0.019	-0.005	0.036**	0.092	0.108*
	(0.054)	(0.035)	(0.018)	(0.074)	(0.062)
Responder*2014	-0.032	-0.019	0.040**	0.039	0.072
	(0.053)	(0.036)	(0.016)	(0.075)	(0.063)
Responder*2015	-0.063	-0.028	0.040**	0.089	0.008
	(0.052)	(0.035)	(0.019)	(0.077)	(0.063)
Constant	0.101	0.283***	0.149*	1.480	-0.492**
	(0.122)	(0.089)	(0.079)	(1.392)	(0.247)
N	6,256	8,576	9,977	3,095	4,902
N Schools	1,012	1,072	1,058	801	890
N Teachers	4,463	5,080	5,155	1,782	2,492

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Regressions include school fixed effects and controls for time-varying teachers and school characteristics. Standard errors are clustered at the school level.

Appendix B3: Falsification Tests – Replication with Temporary and Permanent Exits

	VAM vs. no VAM	Charter Schools
2007		-0.090**
		(0.035)
2008		0.013
		(0.024)
2010	0.007***	-0.035**
	(0.002)	(0.014)
2011	0.013***	0.035*
	(0.002)	(0.020)
2012	0.027***	-0.006
	(0.003)	(0.015)
2013	0.030***	-0.012
	(0.003)	(0.019)
2014	0.020***	-0.022
	(0.003)	(0.018)
2015	0.013***	-0.013
	(0.003)	(0.019)
Responder	-0.003	
	(0.004)	
Responder*2010	-0.005	
	(0.005)	
Responder*2011	-0.005	
	(0.005)	
Responder*2012	-0.009	
	(0.005)	
Responder*2013	0.002	
	(0.005)	
Responder*2014	-0.007	
	(0.005)	
Responder*2015	-0.001	
	(0.005)	
Constant	0.129***	0.433**
	(0.013)	(0.176)
N	351,048	23,260
N Schools	1,574	132
N Teachers	72,819	8,451

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Regressions include school fixed effects and controls for time-varying teachers and school characteristics. Standard errors are clustered at the school level.

Appendix C1: Model Estimates of the Probability of Teacher Exit – Additional Covariates

	Panel A: All Teachers	Panel B: Main Comparison Groups		Panel C: Retirement Sub-groups		
	(1)	(2)	(3)	(4)	(5)	(6)
	All Teachers	**Low** vs. High VAM	**Tenured** vs. Untenured	**Full** vs. 10-19	**Full** vs. Partial	**Partial** vs. 10-19
Experience	-0.007***	-0.007***				
	(0.000)	(0.001)				
Experience Squared	0.000***	0.000***				
	(0.000)	(0.000)				
Female	-0.005***	-0.016***	-0.003	-0.005***	-0.005**	-0.001
	(0.001)	(0.006)	(0.004)	(0.002)	(0.002)	(0.002)
Black	-0.018***	-0.033***	-0.021***	-0.005**	-0.003	-0.016***
	(0.001)	(0.005)	(0.005)	(0.002)	(0.003)	(0.002)
Other Race	0.011***	-0.004	0.019**	0.002	-0.001	0.005
	(0.003)	(0.011)	(0.010)	(0.005)	(0.009)	(0.005)
Bachelors	-0.001	-0.007**	0.003	-0.013***	-0.016***	0.000
	(0.001)	(0.003)	(0.004)	(0.001)	(0.002)	(0.001)
SPED Certification	-0.010***	-0.011**	-0.002	-0.015***	-0.022***	-0.003***
	(0.001)	(0.004)	(0.004)	(0.001)	(0.002)	(0.001)
STEM Certification	0.005***	-0.009*	0.008*	-0.009***	-0.014***	-0.001
	(0.001)	(0.004)	(0.005)	(0.002)	(0.003)	(0.001)
Alt. Prep.	0.090***	0.120***	0.137***	0.078	0.803***	-0.039***
	(0.009)	(0.023)	(0.019)	(0.123)	(0.022)	(0.010)
Bachelors Instate	-0.068***	-0.079***	-0.074***	-0.027***	-0.047**	-0.023***
	(0.002)	(0.008)	(0.006)	(0.006)	(0.022)	(0.006)
School Pct. Black	0.032**	0.015	0.168**	0.022	0.035	0.031*
	(0.016)	(0.059)	(0.070)	(0.022)	(0.027)	(0.018)
School Pct. Other	0.020	0.031	-0.005	-0.009	-0.002	-0.000
	(0.028)	(0.106)	(0.125)	(0.039)	(0.052)	(0.033)
School Pct. FRPL	-0.015	-0.035	-0.009	-0.018	-0.023	-0.012
	(0.011)	(0.038)	(0.047)	(0.015)	(0.019)	(0.012)
School Pct. LEP	0.110***	0.214	0.287	0.157***	0.207***	0.096**
	(0.042)	(0.160)	(0.192)	(0.055)	(0.070)	(0.046)
School Pct. SPED	-0.011	-0.086*	0.003	-0.019	-0.019	0.025
	(0.017)	(0.050)	(0.074)	(0.022)	(0.024)	(0.018)
School Pct. Gifted	-0.043	-0.088	0.126	-0.110**	-0.136**	0.046
	(0.032)	(0.147)	(0.225)	(0.049)	(0.064)	(0.038)
School SPS Z-score	-0.006***	-0.008	-0.004	-0.005**	-0.004	-0.006***
	(0.002)	(0.006)	(0.005)	(0.002)	(0.003)	(0.002)
N	503,479	32,806	29,223	231,388	159,046	149,186
N Schools	1,651	1,179	1,421	1,641	1,632	1,615
N Teachers	84,955	16,034	17,072	46,038	29,555	30,432

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Names in bold in the table headers are the responder groups. Regressions include school fixed effects. Standard errors are clustered at the school level.

Appendix C2: Teacher Attrition and the Effect of Tenure Reform 2009-2015 Teacher Effectiveness by Experience Group – Additional Covariates

	0-3 Years	4-9 Years	10-19 Years	20-24 Years	25+ Years
	(1)	(2)	(3)	(4)	(5)
Experience	0.000	-0.012	-0.005	-0.146	0.040***
	(0.014)	(0.012)	(0.007)	(0.123)	(0.011)
Experience Squared	-0.004	0.001	0.000	0.003	-0.000**
	(0.004)	(0.001)	(0.000)	(0.003)	(0.000)
Female	-0.007	-0.013	-0.024**	-0.024	-0.020
	(0.013)	(0.009)	(0.011)	(0.031)	(0.030)
Black	-0.070***	-0.026***	-0.026***	-0.010	-0.037*
	(0.012)	(0.007)	(0.008)	(0.023)	(0.020)
Other Race	0.041	-0.030*	-0.047***	0.045	-0.069
	(0.030)	(0.017)	(0.012)	(0.116)	(0.068)
Bachelors	-0.015	-0.007	-0.009*	-0.009	0.010
	(0.013)	(0.006)	(0.005)	(0.017)	(0.014)
SPED Certification	-0.003	-0.004	-0.002	-0.030	-0.028*
	(0.016)	(0.008)	(0.006)	(0.018)	(0.016)
STEM Certification	-0.002	-0.003	-0.013	0.003	-0.045**
	(0.012)	(0.008)	(0.008)	(0.023)	(0.021)
Alt. Prep.	0.105***	0.047	-0.005		
	(0.029)	(0.039)	(0.007)		
Bachelors Instate	-0.107***	-0.059***	-0.036	0.063	-0.031
	(0.014)	(0.012)	(0.023)	(0.099)	(0.045)
School Pct. Black	0.188	-0.146	-0.093	0.054	0.003
	(0.159)	(0.128)	(0.067)	(0.220)	(0.263)
School Pct. Other	-0.037	-0.091	0.139	-0.005	-0.157
	(0.307)	(0.173)	(0.154)	(0.328)	(0.391)
School Pct. FRPL	0.052	-0.030	0.011	0.007	-0.193
	(0.098)	(0.059)	(0.050)	(0.181)	(0.168)
School Pct. LEP	0.106	0.163	-0.151	0.494	1.345**
	(0.426)	(0.264)	(0.217)	(0.824)	(0.599)
School Pct. SPED	-0.443***	0.057	-0.038	-0.004	-0.036
	(0.155)	(0.095)	(0.068)	(0.130)	(0.220)
School Pct. Gifted	-0.068	0.158	0.214	0.223	-1.005*
	(0.490)	(0.244)	(0.155)	(0.407)	(0.551)
School SPS Z-score	0.002	-0.009	-0.010	-0.019	-0.014
	(0.018)	(0.009)	(0.008)	(0.020)	(0.023)
N	6,256	8,576	9,977	3,095	4,902
N Schools	1,012	1,072	1,058	801	890
N Teachers	4,463	5,080	5,155	1,782	2,492

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Names in bold in the table headers are the responder groups. Regressions include school fixed effects. Standard errors are clustered at the school level.

Appendix C3: Falsification Tests – Additional Covariates

	VAM vs. no VAM	Charter. Schools	Student Enrollment
Experience	-0.007***	-0.006***	-0.176***
	(0.000)	(0.001)	(0.055)
Experience Squared	0.000***	0.000***	0.002*
	(0.000)	(0.000)	(0.001)
Female	-0.003**	-0.006	1.067**
	(0.001)	(0.005)	(0.459)
Black	-0.020***	-0.061***	-1.025
	(0.002)	(0.008)	(0.645)
Other Race	0.009***	-0.021*	-1.982
	(0.003)	(0.011)	(1.272)
Bachelors	-0.001	-0.010*	0.523
	(0.001)	(0.005)	(0.362)
SPED Certification	-0.012***	-0.011**	-0.554*
	(0.001)	(0.005)	(0.298)
STEM Certification	0.004***	0.019***	-0.663
	(0.001)	(0.006)	(0.469)
Alt. Prep.	0.105***	0.102***	-5.242
	(0.009)	(0.011)	(4.049)
Bachelors Instate	-0.062***	-0.040***	-0.953
	(0.003)	(0.008)	(0.785)
School Pct. Black	0.051**	-0.142	1.463
	(0.021)	(0.115)	(72.910)
School Pct. Other	-0.007	-0.304*	-43.318
	(0.036)	(0.180)	(100.375)
School Pct. FRPL	-0.012	0.046	15.699
	(0.012)	(0.042)	(39.324)
School Pct. LEP	0.217***	0.006	-73.526
	(0.056)	(0.200)	(143.549)
School Pct. SPED	-0.006	0.125	3.128
	(0.021)	(0.142)	(33.366)
School Pct. Gifted	-0.064	-0.240*	14.063
	(0.046)	(0.126)	(175.510)
School SPS Z-score	-0.004**	-0.006	2.770
	(0.002)	(0.007)	(4.801)
N	351,048	23,260	495,064
N Schools	1,574	132	1,553
N Teachers	72,819	8,451	84,082

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Names in bold in the table headers are the responder groups. Regressions include school fixed effects. Standard errors are clustered at the school level.