Technical Report

WHEN TENURE ENDS: THE SHORT-RUN EFFECTS OF THE ELIMINATION OF LOUISIANA'S TEACHER EMPLOYMENT PROTECTIONS ON TEACHER EXIT AND RETIREMENT

EDUCATION RESEARCH ALLIANCE FOR NEW ORLEANS

Katharine O. Strunk, University of Southern California
Nathan Barrett, Tulane University
Jane Arnold Lincove, University of Maryland-Baltimore County

April 12, 2017

EducationResearchAllianceNOLA.org

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

April 11, 2017

Katharine O. Strunk[a], Nathan Barrett[b], Jane Arnold Lincove[c]

The research presented here utilizes confidential data housed at the Education Research Alliance for New Orleans. We gratefully acknowledge the receipt of these data and funding for this study from the Laura and John Arnold Foundation. We wish to thank Josh Cowen, Doug Harris, Andrew McEachin and seminar participants at the Stanford Center for Education Policy Analysis and at Michigan State University for their insights. The views expressed in this paper do not necessarily reflect those of ERA-New Orleans, the University of Southern California, Tulane University, the University of Maryland-Baltimore County or the study's sponsors. Responsibility for any and all errors rests solely with the authors.

[a] Corresponding author. University of Southern California, 3470 Trousdale Parkway, Los Angeles, CA 90089, USA. Email: kstrunk@usc.edu, (213) 740-2190 (o).

[b] Education Research Alliance of New Orleans, Tulane University, 1555 Poydras Street, 7th Floor, Room # 701, New Orleans, LA 70112. Email: nbarret@tulane.edu, (504) 274-3617 (o)

[c] University of Maryland, Baltimore County. School of Public Policy. Email: jlincove@umbc.edu, (512) 779-2045 (o)

I. INTRODUCTION

The large majority of public school teachers, like other public employees, have long-standing employment protections that shield them from arbitrary or capricious dismissal. At the centerpiece of K-12 public school teacher protections are tenure policies, which intentionally erect barriers to dismissing teachers by setting guidelines around the length of time teachers serve in "probationary" positions, after which they only can be dismissed for "just cause." Administrators wishing to dismiss a tenured teacher must provide notice, issue a statement of causes for termination, conduct a hearing, and provide the right for appeal (Christie & Zinth, 2011).

Proponents of public school teacher tenure argue that these laws are necessary to protect teachers from unreasonable job requirements, censure, and arbitrary dismissals. Further, tenure protection might improve teacher recruitment and retention if teachers are willing to accept lower salaries in return for enhanced job security, relative to opportunities in other industries (Chermerinsky, 2014; Public Impact, 2011; Ravitch, 2015; Rothstein, 2015). Opponents of tenure protections argue, on the other hand, that tenure harms schools and students by making it nearly impossible to fire ineffective teachers due to the time and resources required to meet due process obligations (e.g., Hanushek, 2015).

The debate over teacher tenure has been elevated in the media and public discourse through high-profile cases like *Vergara vs. California* and *Wright vs. New York*, in which plaintiffs argued that state tenure laws enable ineffective teachers to teach in public school classrooms, and in particular in the classrooms of low-income and minority students. Many state legislatures have attempted with increasing success to implement reforms to teacher tenure. Prior to 2009, no state's tenure legislation required districts to take performance into account in making tenure decisions. By 2016, 16 states mandated that tenure could not be granted without evidence of teaching effectiveness, and seven states have enacted laws that allow districts to rescind tenure protections from already tenured

teachers if they don't meet performance standards. Three states have passed laws that effectively eliminate tenure and/or due process rights for teachers (NCTQ, 2016; Thomsen, 2016).

The logic behind policy reforms that shift to employment-at-will or make tenure-based employment security dependent on teacher performance, and thus harder to attain, is that they should provide administrators more flexibility to remove ineffective teachers and thus improve the quality of the teaching workforce. However, this theory of action may ignore important benefits of tenure and the potential negative consequences that tenure reform may have on the teacher labor market. Research suggests that public school teachers implicitly value job security, and that the removal of job protections will negatively affect teacher recruitment and retention. In particular, tenure may be viewed by teachers as a part of their overall compensation package, and teachers may be willing to accept difficult working conditions and relatively low pay only when tenure protections provide substantial job security (e.g., Feinberg, 1981; Brunner & Imazeki, 2010; Rothstein, 2015).

Therefore, a potential unintended consequence of removing tenure or tying tenure to teacher performance is increased teacher attrition. Ideally, those who exit as a result of the removal of tenure protections would be the lowest job performers who are either counseled out of the workforce or who choose to leave based on information that they may be at risk of job loss. However, even if some or most of the exiting teachers are lower-performing, their sudden absence requires planning and resources to recruit replacements of adequate quality, possibly resulting in declines in the overall quality of the teacher labor force. However, little research attention has been paid to the impacts of tenure reforms on the teacher workforce, leaving policymakers with inadequate information about the potential effects of reform on which to base policy decisions.

In this study, we measure the effects of the elimination of teacher tenure in Louisiana on teacher attrition. Louisiana is a particularly apt location for study. In 2012, Louisiana effectively eliminated teacher tenure, replacing the lifetime teaching credential (previously earned after three

years on the job) with a temporary license dependent on positive performance appraisal. As we will discuss below, Louisiana was also in the process of developing a statewide teacher evaluation system, known as Compass, which was to be used in part to inform teachers' employment. However, for the first two years of tenure reform, there were no evaluation data available to inform dismissal decisions, leaving teachers in this period to react to the removal of tenure without the complementary threat of information provided by high-stakes evaluations. Using an eight-year panel of teacher-level administrative data, we employ an interrupted time series (ITS) model to examine changes in patterns of teacher attrition in the two years after the removal of tenure. We then attempt to isolate the causal impact of tenure removal by using comparative interrupted time series (CITS) analyses and identifying two sets of comparison groups that should be similarly affected by other conditions but differentially affected by the removal of tenure: 1) third-year teachers who never received tenure versus fourth-year teachers who lost tenure due to the reform; and 2) fully-retirement eligible teachers who had alternative pension-funded options versus partially-eligible or ineligible teachers We also compare traditional public school teachers with charter school teachers, who were always at-will employees and did not benefit from the tenure law before its removal. Last, we examine if there was differential reform-induced attrition for teachers in schools that serve more traditionally disadvantaged student populations. We conduct several falsification tests to confirm our results, including specifications that isolate the passage of the tenure reform from the onset of the Compass evaluation system. Thus we come as close as possible to estimating the causal effects of the tenure policy change alone (and not teacher evaluation or other contextual variables) on short-term teacher exit.

This work is the first to utilize a state-wide administrative dataset to assess the impact of the removal of tenure protections on teacher exits from the state, enabling us to provide the first evidence that the removal of tenure protections causes a substantial increase in teacher attrition. We

find that 8.7 percent of teachers exited teaching in the year of tenure removal, a 20 percent (1.4 percentage points) increase from the year prior to the reform. Exits are concentrated among teachers who either derive a greater benefit from tenure (4th year teachers and traditional public school teachers) or have a more certain alternative income through retirement. We find no evidence of effects on teachers who should not be affected by the tenure reform, including charter school teachers, and no differential effects across groups of teachers who face similar benefits from tenure protections. However, we do find evidence that teachers in Louisiana's lowest-performing ("F"-rated) schools are more likely to exit teaching as a result of the tenure reforms. ITS analyses confirm that, while there was a small increase in attrition at the onset of the Compass evaluation system, the tenure reform itself substantially escalated teacher exit rates net of any evaluation system effect. All of this raises confidence that the removal of tenure protections caused a significant increase in teacher attrition in both the year immediately after the reform and the following year. Estimates suggest that reform-induced attrition was equivalent to losing between 1,500 to 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.

In what follows, we first review Louisiana's education reform context. In Section III, we review the limited research base that currently informs the debate surrounding tenure reforms. Section IV describes our dataset, 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 teacher policy reform.

II. TEACHER TENURE AND POLICY REFORM IN LOUISIANA

Between 2010 and 2015, the Louisiana legislature enacted multiple teacher policy reforms, including major legislation affecting teacher evaluation (Act 54) and teacher tenure (Act 1). Figure 1

displays the timeline of the implementation of these reforms. Act 54 was passed in 2010 and required the Louisiana Department of Education (LDOE) to develop a statewide multiple-measure teacher evaluation system.[1] This system, known as Compass, was developed in 2010-11 and piloted in a small number of districts during 2011-12 school year. Evaluations for all teachers statewide were conducted for the first time in 2012-13, with results available to school districts and principals in the fall of the 2013-14 school year.

Compass would provide new information on teacher performance, but Louisiana's existing tenure protections restricted a district's ability to use performance data in dismissal decisions. Prior to 2012, Louisiana's tenure law looked similar to those in place in most states (NCTQ, 2016).[2] Teachers served a probationary period of three years during which a teacher could be dismissed upon recommendation of the superintendent for "valid reasons."[3] After the third year, dismissal required a hearing by the local school board at which the teacher was found guilty of "willful neglect of duty, incompetency, dishonesty, immorality, or being a member of a group that was legally prohibited from operating in the state" (Act 1, Louisiana H.B No. 974, 2012).

Act 1 (the "Talent Statute"), passed in July 2012, created new rules for dismissal, capitalizing on the teacher performance data that would be available via the Compass evaluation system. Act 1 designated local superintendents and principals as the final authority in personnel decisions, emphasizing that personnel policies and decisions should be based on teacher performance. Although the word "tenure" is still used, the definition is quite different. First, Act 1 extended the time to tenure and made the status contingent on performance. For untenured teachers as of the beginning of the 2012-2013 school year, tenure would be granted only after a teacher received a highly-effective Compass rating for five out of six consecutive years, a stipulation that makes it nearly impossible to gain tenure.[4] Second, Act 1 changed the definition of tenure for all teachers regardless of prior tenure status. Specifically, tenure is immediately revoked if a teacher is rated

ineffective on Compass. A teacher without tenure can be dismissed for a "valid reason" by a principal or superintendent at any time. For these teachers, as well, tenure can be regained only by receiving a highly-effective Compass rating for five of six consecutive years. But since tenure is revoked any time a teacher is ranked ineffective, there is no status that fully protects a teacher from potential dismissal. In other words, the common definition of teacher tenure as a nearly guaranteed job for the remainder of a teacher's employment in a district no longer applies for *any* Louisiana public school teacher.[5] From the teacher's perspective, then, Act 1 profoundly reduced job security, making teaching a higher risk occupation than it had been prior to 2012 by basing job protections and compensation on evaluated performance rather than experience and seniority.

Because both evaluation and tenure reforms were instituted in a relatively condensed time period in Louisiana, it could be hard to disaggregate the impact of the removal of tenure protections on teacher attrition from the effect of high-stakes teacher evaluation. The implementation timeline of these policies, however, provides us with the ability to isolate the effect of tenure reform. There was a two-year gap between the implementation of the tenure reform at the end of the 2011-12 school year and the availability of the first Compass evaluation data for decision making in the fall of the 2013-14 school year. This means that during the 2012-13 and 2013-14 school years, teachers no longer retained traditional tenure protections, but their exit decisions could not have been motivated by information from Compass. That being said, given that the far majority of states that have implemented tenure reforms have done so in conjunction with new high-stakes teacher evaluation systems, the policy reform of interest might also be conceived of as the *combination* of the removal of employment protections resulting from increased teacher-level accountability tied to tenure reforms. In this case, the corresponding Louisiana policy would be the implementation of Act 1, which tied high-stakes consequences to the new multiple measure teacher evaluation system.[6]

Figure 1 illustrates the timeline for passage and implementation of the two policies. Act 54

ordered the creation of Compass in spring 2010, and Compass was designed during the 2010-2011 school year and piloted during the 2011-2012 school year. Although teachers were initially evaluated using the new Compass system in 2012-13, the results of these evaluations were not issued until fall 2013. If we assume that teachers primarily exit in the summer, teacher exits due to anticipation of poor Compass evaluations could have begun as early as the summer of 2010 as teachers reacted to the expectation of high stakes evaluations (which we test for below). However, any exits attributable to actual consequences of Compass would have been delayed until summer 2014.

Act 1 was passed in the spring of 2012, and the tenure reform went into effect right away in the summer of 2012. Teachers worked without traditional tenure protections during the 2012-13 and 2013-14 school years, meaning that we might see evidence of tenure reform-induced attrition in our data in those years, *before* exits resulting from receipt of Compass evaluation scores could have begun. Such exits were unlikely to be driven by principals' removal of underperforming teachers, as during this time they still did not have Compass information about teacher quality. In other words, changes in the rate of teacher exit prior to summer 2014 likely result as a voluntary response by teachers to the loss of employment protections. If teachers exited in response to the *passage* of the teacher evaluation law, that exit would have commenced in summer in 2010. If teachers exited in response to the pure loss of tenure protections, causing employment to be tied to measures of performance, that exit would have commenced in summer 2012 and continued in summer 2013. In this study, we examine exit trends from the summer of 2006 to 2013 to estimate the effect of this decrease in job security on the teacher workforce in Louisiana.[7]

III. TEACHER TENURE, EMPLOYMENT RISK AND THE TEACHER LABOR MARKET

The removal of tenure protections for teachers has been debated both in the courts and state legislatures. Proponents of job protections for teachers argue that tenure is necessary both to attract qualified new teachers to the profession and to retain teachers in public schools (e.g., Chermerinsky,

2014; Ravitch, 2015; Rothstein, 2015). The rationale these scholars and public figures employ in their defense of tenure in part relies on the fact that teachers value tenure – or any employment protections – as a key component of their overall compensation. This may well be the case; extant research illustrates that teachers consider both monetary and non-monetary benefits from their positions in their calculation of total compensation (e.g., Loeb & Page, 2000). Non-pecuniary factors that teachers value include 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). More importantly for this study, teachers, like other workers, also value job stability and the absence of employment risk, or certainty about their likelihood of retention (e.g., Authors, 2016a; Feinberg, 1981; Murnane & Olsen, 1990; Rothstein, 2015).

Absent tenure protections, there is a greater degree of risk involved in teaching. Teachers face a risk of dismissal due to factors both within and beyond their control, which introduces uncertainty into teachers' assessments of future compensation. Because teachers value job security (the absence of risk) as part of their total compensation package, the removal of tenure effectively diminishes their overall compensation. As a result, a risk-averse teacher with a positive risk of dismissal will accept an alternative position that a teacher with zero probability of dismissal (or a risk-neutral teacher) would not accept. Therefore, to the extent that such job opportunities exist, the teacher without job security is more likely to exit in any given year.

Although there is a great deal of public discourse about the benefits and drawbacks of tenure protections, there is little research that tells us how tenure policies influence the workforce through individual decisions to enter or exit the profession. This is due in large part to the fact that tenure policies have existed for over a century in the United States, but only recently have policy changes enabled rigorous evaluation of their impacts. In the absence of sufficient policy variation in teacher tenure, two recent studies have utilized simulations to test the labor market impacts of diminished

tenure protections (Staiger & Rockoff, 2010; Rothstein, 2015). Both suggest that productivity benefits could be accrued by lessening tenure protections, either by delaying the provision of tenure and/or decreasing the fraction of teachers who are awarded tenure. However, Rothstein (2015), who models teachers' behavioral responses to the diminution of tenure protections, finds that more rigorous tenure processes would necessitate substantial salary increases in order attract new teachers to a high-risk employment environment (Rothstein, 2015). Specifically, Rothstein (2015) examines the implementation of alternative tenure processes (denying more teachers tenure and lengthening the time before the tenure decision) in a simulated labor market. He shows that denying tenure to more teachers (56-57%) and potentially pushing the tenure decision out an extra year would require a 30 to 33% increase in average teacher compensation.

Some empirical evidence also supports the notion that teachers view tenure as a part of their total compensation. For instance, Brunner and Imazeki (2010) illustrate that beginning teacher salaries are positively associated with the length of time to tenure; in states with longer probationary periods, districts must pay beginning teachers higher salaries to compensate for the increased risk associated with teaching without tenure. Similarly, in the only study of which we know that examines a tenure policy change, Loeb, Miller and Wyckoff (2015) show that, under a policy in New York City that extended the pre-tenure probationary period for a subset of less effective teachers, teachers who had their probationary time period extended were significantly more likely to voluntarily exit teaching in New York City public schools than were teachers who were granted tenure.[8]

To date, we know of no published research that empirically examines the impact of a state's tenure reform on teacher attrition. Empirical studies of the elimination of similar employment protections in other sectors of government suggest that reforms that remove job protections alter employer-manager relations and perceptions (Battaglio, 2010; Battaglio & French, 2016) and might negatively influence recruitment (Bowman & West, 2006). Our study provides a new source of

evidence to help inform policymakers and researchers about the effects of tenure reform on teacher attrition.

IV. DATA AND EMPIRICAL STRATEGY

Data

We use an eight-year panel of teacher- and school-level data, spanning the 2005-06 through 2012-13 school years. Our full analytic dataset consists of 406,782 teacher–year observations from 76,629 teachers and 1,643 schools. Data were provided by the Louisiana Department of Education (LDOE) and include elements from the state's Profile of Educational Personnel (PEP) and Student Information System (SIS) datasets. PEP includes de-identified teacher observations for all traditional public school (TPS) and charter school teachers in Louisiana. Variables include demographics, teaching certificates, college degrees, whether or not a teacher graduated from a Louisiana college, whether or not a teacher ever held a probationary certificate (a proxy for entering the profession through an alternative pathway), school assignments, and district hire dates. We can observe teachers as they move across public schools in the state, and we identify teacher exit through departure from the PEP dataset. If a teacher does not appear in the data the following year, he is coded as exiting during the summer. Thus, with data through the 2013-14 school year, we are able to observe exits spanning the summers of 2006 through 2013.[9]

As will become clear in our discussion of our methods, accurately identifying teacher experience is particularly critical to our analysis. Unfortunately, the PEP 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 because teacher salaries are dictated by teaching experience and education levels, and are codified in the published negotiated teacher salary schedules available for each district. Using PEP data on teacher salary and education levels, we are able to identify a teacher's experience level

by working backwards from the district's reported salary schedule. Salary data are available for 92% of teachers in our sample. We then used district hire date to confirm experience derived from the salary schedules and to impute experience for remaining teachers. Hire dates report the first time a teacher worked for a school district but not necessarily as a teacher. Estimating experience based on hire date involves first measuring the time between the hire date and the initial observation of a teacher in our dataset. We then add one year for each additional year of teaching observed in the PEP data.[10] Hire date is available for 88 percent of all teachers, but because hire dates are underreported prior to 2010-11, missing values are concentrated among more experienced teachers.

Our final experience measure is based on derived experience from the salary data. We use this measure for the 80% of teachers who have both salary and hire date-derived experience measures[11] and for the 12% of teachers who are missing their hire date but for whom we can generate a salary-based measure. Seven percent of teachers had a valid post-2011 initial hire date, but we could not confirm experience through a published salary schedule, and we use the hire-date measure of experience for these teachers. (Almost all of these cases were teachers at charter schools, which do not publish salary schedules.)[12] The remaining teachers, only 1.3 percent of the sample, are excluded due to missing values across both measures of experience. We provide summary statistics of teacher experience and all other teacher-level covariates by analytic group in Table 1.

We also include school-level measures in our analyses to control for school conditions that change over time and might influence a teacher's propensity to exit. We generate these measures by aggregating student-level SIS data that describe student demographics and educational needs (race/ethnicity, English proficiency, special education status, and free/reduced price lunch eligibility). We also include state-reported school performance scores (SPS), which aggregate student proficiency on state standardized tests and are used to determine the school's accountability status. SPS calculations change across the time period under study, so SPS scores in this study are

normalized within year and school level.[13] In some of our analyses, we separately examine the impact of the tenure reform on schools with high or low proportions of historically disadvantaged students, defined as those in the top or bottom quartile of schools by proportion minority, low-income (qualify for free- or reduced-price lunches), and with low or high SPS levels and SPS growth. We also examine schools with high or low performance on the state's A-F accountability system. Finally, we also identify all charter schools and use charter school teachers as a comparison to TPS teachers. Louisiana charter school teachers are employed by private charter management organizations. They were not covered by the Louisiana tenure law prior to 2012, and therefore their employment protections were not affected by the tenure reform.[14] Charter school teachers were covered by the teacher evaluation policy and received Compass evaluations beginning in fall 2013 similar to TPS teachers. Charter schools are located primarily in New Orleans and most opened after hurricane Katrina in 2005, so any analysis using charter school teachers begins in 2006-07 instead of 2005-06.[15]

Empirical Strategies

First, we use an interrupted time series (ITS) model to estimate teacher exit before and after the Louisiana tenure reform, controlling for teacher and school characteristics. Second, we use comparative interrupted time series (CITS) models to test for differential exit propensities over time for teachers who should be more affected by the loss of tenure. We compare teacher exit before and after the tenure reform. The timing of the policy implementation is very important to our estimation strategies, because of the timeline described in Figure 1. If the concept of performance-based evaluation caused teachers to perceive teaching as a riskier (or simply less friendly) occupation, any resulting exit would have commenced during the summer of 2010, when Act 54 was passed. Teacher exits in response to the tenure policy change (Act 1) itself would likely have begun in the summer of 2012, and response to actual evaluation data coupled with tenure reform would be delayed until summer of 2014. Given the staged implementation of Compass, Louisiana's outcomes in 2011-12

and 2012-13 offer the best opportunity to isolate the effects of the removal of tenure on the teacher workforce as a whole through the labor market choices of teachers. Throughout our analysis, we identify a teacher as an *exiter* in a specific school year if she did not return to teaching the following fall (e.g. a teacher "exits" in 2011-12 if she does not return to teaching in 2012-13). Thus, we identify 2010-11 as the final baseline pre-tenure reform year, 2011-12 as the first post-tenure reform year, and 2012-13 as the second post-tenure reform year. All years are identified by the spring of the academic year in our tables so that, for example, 2010-11 will be labeled 2011.

Interrupted Time Series Models

We begin with a descriptive estimation of changes in the probability of teacher exit after tenure reform. At the teacher level, we first estimate an ITS model of the probability that a teacher exits for a period of time from summer 2006 (exits after the 2005-06 school year) to summer 2013 (exits after the 2012-13 school year). We employ the model recommended by Somers, Zhu, Jacob, and Bloom (2013) for ITS models with two years of post-intervention data, as follows:

$$EXIT_{ijt} = \alpha_0 + \delta_0 YEAR_t + \alpha_1 2012_t + \alpha_2 2013_t + X_{it} + Z_{jt} + \varphi_j + \mu_j + \varepsilon_{it} \quad [1]$$

where $EXIT_{ijt}$ is equal to one if a teacher *i* in school *j* exits teaching in year *t*. $EXIT_{ijt}$ is equal to zero if the teacher returns to any public school teaching position in Louisiana.[16] We run all models as linear probability models.[17] $YEAR_t$ is a time trend variable that is centered at zero in the final baseline year before tenure reform (2010-11). 2012_t is a dichotomous indicator equal to 1 in the first year after the passage of tenure reform (2011-12 school year). 2013_t is a dichotomous indicator equal to 1 in the second year after the passage of tenure reform (2012-13 school year). Our data allow us to observe TPS teacher exit for six pre-tenure reform years (2005-06 to 2010-11), and two years post-tenure reform (2011-12 and 2012-13). δ_0 estimates the time trend in teacher exit prior to tenure reform, and α_1 and α_2 estimate deviations from that trend in the first and second years after tenure reform, respectively. In an alternate specification we also run the ITS with separate indicators

for the 2010, 2011, 2012 and 2013 school years, thus identifying the individual attrition levels in each year after the passage of Act 54, relative to the pre-Compass trend. This enables us to alleviate concerns that we are solely capturing changes in teacher attrition resulting from the Compass reform.

Because school conditions common to multiple teachers influence exit decisions, we estimate specifications controlling for time-varying school characteristics (Z_{jt}) and time-invariant school fixed effects (γ_j). Because exit is also influenced by teacher characteristics (such as qualifications for other jobs and eligibility for retirement), we also include teacher-level controls (X_{it}) Thus, in our full specification, we are estimating changes in propensities to exit in the post-tenure reform years for similar teachers within schools. We decompose the error into μ_j, which is unexplained variance correlated at the school level, and ε_{ijt}, which is random error. In our tabled results, we adjust for μ_j with clustered standard errors within schools, and results are robust to wild bootstrapped standard errors.[18]

Comparative Interrupted Time Series Analysis

The ITS model in equation [1] examines whether the average teacher was more likely to exit after the implementation of tenure reform. However, the patterns shown in the ITS framework cannot identify if teacher exit is *caused* by the tenure reform, as both α_1 and α_2 might be influenced by other factors. For instance, these coefficients might capture teachers' responses to other circumstances related to the policy (in particular to the perceived escalating unfriendliness of the Louisiana legislature towards teachers, above and beyond the onset of the Compass reform) or to other concurrent events, such as shifts in the instructional curriculum (e.g. Louisiana began an incremental shift to the Common Core curriculum in 2010).

To identify the causal effect of the removal of tenure on teacher mobility, ideally we would be able to compare teachers affected by the tenure policy change to a group of teachers who were

not. However, given the policy design, all traditional public school teachers in Louisiana were affected by the removal of tenure protections. Instead, we identify comparison groups of otherwise similar teachers whom we expect to be differentially affected by the loss of tenure. If exit responses post-policy are stronger in groups that we expect to suffer a greater loss from the policy change relative to groups of similar teachers who we expect to suffer a smaller loss, it raises confidence that the measured response predicted in our ITS model is due to the tenure policy change rather than to other policy or contextual factors.

Comparison Groups. We identify three groups of teachers that we expect to be differentially affected by the tenure reform that we can use as comparisons in our analyses. These comparisons are intended to confirm that any increases in attrition shown in the ITS models are due to the removal of tenure protections rather than Compass.

We first consider how groups of TPS teachers might have been differentially impacted by the removal of tenure protections. In order to measure the effect of removing tenure protections, we exploit the idea that not all teachers value the initial tenure policy equally. In particular, we identify two groups of teachers who likely place a greater value on tenure than a similar group of comparison teachers. We first consider teachers who have not yet achieved tenure compared to teachers who already have tenure-based employment security. There is still an inherent value of tenure for untenured teachers, as traditional public school teachers likely enter the profession in part because they incorporate the high degree of job security (that comes with tenure) into their compensation calculations. As such, for not-yet-tenured teachers, tenure is valued as an expectation of *future* job certainty. We compare this with an already-tenured teacher who values tenure as a *current* benefit. As long as there is a positive risk premium attached to a lack of job security and a positive discount rate (e.g., Ainslie & Haslam (1992) and Frederick, Loewenstein & O'Donoghue (2003)), the reversal of tenure protections is unambiguously a greater loss for a tenured teacher than an

untenured teacher, and we should expect a greater response to the policy change among tenured than pre-tenure teachers. Put another way, a teacher who never had tenure has less to lose if tenure is removed, because her overall utility from tenure protections is discounted by the fact that she had yet to benefit from tenure (even if she expected future protection).

We operationalize this by comparing TPS teachers who are either in their final pre-tenure year (3rd year teachers) or their first post-tenure year (4th year teachers). Third and fourth year teachers should have similar characteristics (shown in Table 1) and alternative job opportunities, and any differences in exit rates between the two teacher groups should not change pre-post tenure reform *unless* one group has a greater response to the change in tenure policy. However, 4th year teachers effectively *lost* tenure, since before the policy shift they would have had it, while 3rd year teachers never had tenure and only lost an expected future value of tenure. Therefore, we expect these two groups to be similar in their response to other factors, but different in the utility effects of tenure reform.[19]

Next, we consider otherwise similar teachers who vary in the value of their next-best job alternative. A teacher with a certain alternative income places a lower value on the security of the teaching job than a teacher with an uncertain alternative income. Thus, uncertainty about alternative opportunities unambiguously increases the value of tenure (job security) to a tenured teacher. The teacher who has an equivalent certain alternative has little need for job security. A primary example of employees with alternative opportunities for compensation absent risk is those who are eligible for retirement. Public employees in general, and teachers in particular, are often eligible to draw down a set and substantial income upon retirement as a result of state-mandated defined benefit pension systems (Backes et al., 2015). Thus, retirement-eligible teachers face a certain alternative income and should be *more likely* to exit teaching when tenure protections are removed than should teachers who do not yet qualify for pension payments from a defined benefit system. Accentuating

this response from retirement-eligible teachers, it is possible that the onset of performance-based retention and promotion reforms could cause teachers to feel attacked or disrespected by the state, and especially teachers who had put in decades of work as teachers. This feeling of disrespect might further diminish veteran teachers' total utility received as a function of teaching in Louisiana, making them even more likely to exit upon the removal of tenure protections.

We operationalize this comparison group by examining attrition for teachers who are eligible for retirement benefits as a certain alternative to teaching relative to those who are not yet eligible. Louisiana provides its traditional public school teachers with a public retirement system—the Teacher Retirement System of Louisiana (TRSL)—that offers a generous defined benefit upon retirement. Teachers with 25 or more years of experience have a certain alternative to a teaching position in guaranteed retirement income.[20] We use as a comparison group TPS 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). We expect these 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 group who can exit with certain income.[21]

Last, we turn to what at first glance seems the most intuitive comparison group; charter school teachers. Charter school teachers in Louisiana were never given tenure, and so the removal of tenure protections in the state should not impact their likelihood of exit relative to TPS teachers, who suffered a loss in total compensation with the removal of tenure. Because both charter and TPS teachers were equally affected by the implementation of the teacher evaluation system but only TPS teachers were affected by tenure reform, this comparison should isolate the impact of tenure reform net the effect of Compass on teacher attrition. In other words, if tenure reform is the source of observed increases in teacher exits, this effect should be concentrated among TPS teachers since

charter school teachers never had tenure to lose. We note, however, that there are several limitations to the use of charter teachers as a comparison. First, Table 1 shows that charter school and TPS teachers are quite different on observable characteristics. Moreover, charter school teachers are concentrated in a single district – New Orleans – while the TPS teachers are in the remainder of the state. In addition, as can be seen in Figure 3c, the pre-reform attrition trajectories vary substantially between charter and TPS teachers. Because of these limitations, we view the charter school comparison analysis as suggestive, and pair it with our ITS and additional CITS analyses to provide evidence about the effects of the removal of tenure protections on teacher attrition.

We also perform a series of sub-analyses intended to examine if specific populations of students are more affected by reform-induced attrition. To do so, we again use CITS models, this time comparing teachers who teach in schools with more or less disadvantaged populations. Specifically, we include four measures of disadvantage: the proportion of students who qualify for free- or reduced-price lunches (a measure of poverty), the proportion of students who are under-represented minorities, and two measures of school performance scores (SPS) -- level and growth. We divide these groups into quartiles, such that the schools in the top quartile of low-income and minority students are labeled "most disadvantaged" and the schools in the bottom quartile are the "least disadvantaged." Similarly, teachers in schools that are performing the worst in terms of SPS measures are labeled "most disadvantaged," consisting of teachers in the bottom quartile of schools by SPS level or SPS growth. "Least disadvantaged" schools are those in the highest quartile of SPS level or growth. In addition, we compare reform-induced attrition for teachers in high- ("A") and low-performing ("F") schools, as rated by the state's A-F accountability system. Accountability grades are given to schools by the state based on their SPS. Because these schools are substantially different on observable and likely unobservable characteristics, we view these results as painting a descriptive picture of which kinds of students may be most affected by the reforms.

While these sub-analyses are intended to help us assess which groups of students (schools) may be most impacted by the reform, we can also conceive of these analyses as a further test of our hypothesis that certain groups of teachers will be more likely to exit when tenure protections are removed. In this case, teachers who teach in particularly disadvantaged schools under a salary schedule that does not compensate them for doing so (as is generally the case in Louisiana), may already view their overall compensation as lower than teachers in more advantaged schools. If the tenure reform further reduced their overall compensation, they may be more likely to exit teaching relative to teachers whose overall compensation was offset by having "easier" working conditions, attributable to higher-income, lower-minority, and/or higher-performing student populations. In particular, teachers in F-rated schools may have felt particular accountability pressure in Louisiana, and the increased risk associated with the removal of employment protections may have been more likely to drive them out of the profession.

The CITS Model. Empirically, we use CITS models to test these differential effects. This allows us to separate the effect of tenure removal on teacher mobility from other hard-to-observe factors that might have simultaneously influenced teacher exit. 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). Following Somers et al. (2013), our CITS analysis estimates the following linear probability model:

$$EXIT_{ijt} = \alpha_{0i} + \delta_0 YEAR_t + \gamma_{0i} RESPONDER_{it} + \beta_0 YEAR_t * RESPONDER_{it} + \alpha_1 2012_t + \beta_1 2012_t * RESPONDER_{it} + \alpha_2 2013_t + \beta_2 2013_t * RESPONDER_{it} + X_{it} + Z_{jt} + \varphi_j + \mu_j + \varepsilon_{it} \quad [2]$$

$RESPONDER_{it}$ 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 (4th year

relative to 3rd year teachers, retirement-eligible relative to ineligible teachers, and TPS as opposed to charter school teachers). In our disadvantaged schools analyses, we classify as the "responder" groups TPS teachers in schools serving the highest and lowest proportions of disadvantaged students, as defined above.

As in equation [1], $YEAR_t$ is a trend variable centered at zero in the final baseline year before tenure reform (2010-11). In the CITS model, α_0 is the mean probability of exit for non-responders during the last year of full employment protections (2010-2011), $\alpha_0 + \beta_0$ is the baseline mean probability of exit for responders in that year. Thus, β_0 measures pre-reform differences between the groups. γ_0 is the pre-reform time-trend for exit probabilities of non-responders, and $\gamma_0 + \delta_0$ is the pre-trend for responders. α_1 and α_2 estimate deviations from the pre-reform trend for non-responders in the first and second years of tenure reform, respectively. β_1 and β_2 are our variables of interest for causal analysis, measuring differential deviations from the baseline trend for responders relative to non-responders. In this case, we are testing for a differential changes 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. As above, CITS specifications also control for time-varying teacher characteristics (X_{ij}), time-varying school characteristics (Z_j), school fixed effects (μ_j), and decomposed school and random errors are estimated as described above for the ITS. [22]

Falsification Analyses

It is possible that other unknown 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 several falsification tests. A primary concern is that, in the Louisiana case, teacher exit due to an unfriendly legislative climate might have begun prior to the first year of tenure reform. To assess if this is the case, we first conduct a test of the presence of an Ashenfelter or pre-intervention dip (Ashenfelter, 1978). To do

this, we falsely identify 2011 as the first post-tenure reform year by adding variables to equation [2] for 2011_t and 2011_t * $RESPONDER_{it}$ and re-centering our trend variable. This tests whether we are estimating post-reform effects that actually began prior to reform.[23] Next, we conduct a second falsification test to identify whether teacher exits were affected by exogenous changes in the demand for teachers. Here we replace the dependent variable in equation [2] with time-varying measures of the number of students (total school enrollment) and student demographics (percent white students in the school). These two falsification tests are run for all three of our CITS model comparisons.

For comparisons of TPS school teacher by experience groups, we add two more falsification tests. First, we test the differential effects of tenure reform on samples of charter school teachers within the same experience groups. Again, using the charter school group as a counterfactual enables us to confirm that any differences we see in attrition rates between TPS teachers of different experience levels are most likely driven by changes to tenure protections (which did not apply to charter school teachers) than by the onset of Compass (which applies to charter teachers and TPS teachers). If increased exit is caused by the loss of tenure, we should see no differential effects by experience for charter school teachers. Second, we test for potentially confounding effects of teacher experience by conducting the CITS analysis for teachers who are one-year post-tenure (4^{th} year teachers) compared to two years post-tenure (5^{th} year teachers). These teachers have the same experience gap as 3^{rd} and 4^{th} year teachers, but they have little or no difference in the theoretical value of tenure. We would expect to see no differential effects of tenure reform across 4^{th} and 5^{th} year teachers. For the more experienced teacher groups, we conduct a similar test by comparing teachers eligible for partial retirement (with 20 to 24 years of experience) to both the non-eligible and fully-eligible teachers. If a differential response to tenure reform is driving our results, we would expect to see smaller, but still significant differences, between these groups that vary more subtly in their alternatives to teaching.[24]

V. RESULTS

Descriptive Analysis

Table 1 reports descriptive statistics for the six teacher subgroups that form the core of our causal analysis as well as for A- and F-rated schools. We report average characteristics in the pre-reform years of our panel (from 2005-06 through 2010-11, in Panel A) and in the post-reform years (from 2011-12 through 2012-13, in Panel B). The exit rates shown in the top rows of each panel of Table 1 begin to preview our results. In the pre-reform years, 6.6 percent of all TPS teachers exited, increasing to 9.4 percent after tenure was removed. This is also plotted year-by-year in Figure 2, Panel A, which again shows the marked increase in attrition in the post-reform years. At the same time, there are few substantial differences in average teacher characteristics or average student make-up between the two time periods.

The exit rates for our subgroup comparisons bolster this descriptive story. The changes in exit rates for the responder groups (4^{th} year teachers, retirement eligible teachers, and teachers in F-rated schools) are greater than the changes in exit rates for comparison groups across the two time periods. The differences in the differences of the unadjusted exit rates shown in Table 1 suggest that 4^{th} year teachers had a 1.7 percentage point greater rate of exit after relative to before the implementation of Act 1 compared to the same difference for 3^{rd} year teachers, and the same difference for retirement eligible versus ineligible teachers was 0.8 percentage points. The differences in the differences of the unadjusted exit rates for teachers in F-rated relative to A-rated schools was 2.4 percentage points. The remaining summary statistics provided in Table 1 show that the responder and associated comparison groups were similar in terms of teacher and school characteristics, and that any changes over time appear for the most part consistent across groups.

The descriptive picture is less clear when comparing charter and TPS teachers. Exit rates increased by 4.1 percentage points for charter teachers and by 2.8 percentage points for TPS

teachers after the reform, suggesting both groups might have been influenced by an exogenous change. However, the averages in Table 1 conceal a time trend of increasing exit rates among charter teachers that will be discussed below. In addition, the make-up of charter school teachers in terms of experience and demographics was changing rapidly during this time period. For example, average experience among charter teachers declined by nearly two years from 11.4 years in the pre-reform period (Panel A) to 9.5 years in the post-reform period (Panel B). Thus the full CITS estimation controlling for changing teacher characteristics is necessary to assess the difference between charter and TPS teachers, and even then we view these results as suggestive.

Based on these simple descriptive analyses, it appears likely that the tenure reform increased exit rates for TPS teachers overall, and for specific groups of TPS teachers who theoretically should be most impacted by the removal of employment protections. We now turn to our ITS and CITS analyses to substantiate these results.

Interrupted Time Series Analysis

Table 2 provides results from our ITS model examining the trajectory of TPS teacher exit rates before relative to after the removal of employment protections. Column 1 provides results estimating the probability of teacher exit for a very basic model that includes controls only for the trend and post-tenure reform year dummies (2012 and 2013). Column 2 adds school fixed effects. Column 3 adds teacher and school covariates without school fixed effects, and column 4 provides our fully specified model including covariates and school fixed effects. Column 5 replicates the model in column 4, but this time counts 2010 (the passage of Act 54) as the first intervention year. The top five rows show our trend and year variables of interest, and we see that they change little with the inclusion of covariates or fixed effects. Estimates from the fully specified model in column 4 show that TPS teachers were 2.2 percentage points more likely to exit teaching in the first year following the removal of tenure protections, and 3.8 percentage points more likely to exit in the

second year of reform implementation, all relative to the pre-reform trend. The estimates in column 5 provide evidence that this increase in attrition largely occurs in 2012, after the passage of Act 1 rather than in 2010, after the passage of Act 54. In particular, while it appears that teacher attrition increased in 2010 by approximately 0.9 percentage points and by 2.0 percentage points in 2011 – in essence a one percentage point growth year-over-year – the majority of the increase in attrition occurred in the 2012 and 2013 school years, with an additional two percentage point increase in each subsequent year – a doubling in the rate of growth in attrition.

These results bolster our purely descriptive results discussed above. Estimated exit probabilities over time from the specification in Table 2, columns 4 and 5 are illustrated in Figure 2, Panels B and C. These results suggest that tenure reforms were associated with an increase in the probability of teacher exit, and that the removal of tenure protections had an immediate and sustained effect above and beyond prior levels, even when accounting for any growth in attrition that may have been due to the passage and pilot implementation of Compass. We cannot, however, determine from these results if tenure reform *caused* an increase in teacher exits. To do so, we turn to results from our comparative analyses.

Comparative Interrupted Time Series Analysis

Early Career Teachers

The estimated effects of tenure reform on early-career TPS teachers over time are illustrated in Figure 3, Panel A, based on coefficients from the CITS analysis that are presented in Table 3. 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. In particular, Figure 3, Panel A, which plots the exit trend lines for 4th year (solid) and 3rd year (dashed) teachers, shows that, prior to the tenure policy reform, the two experience groups had similar trends in attrition, with 3rd year teachers having consistently higher

exit rates throughout the pre-reform period. In 2012, we see that exit rates for 3rd year teachers remained at virtually the same level as in the year pre-reform, while the exit rate for 4th year teachers jumped substantially. In the second year post-reform (2013), we see that both groups' attrition rates increased, following a similar trajectory.

The partner estimates from these models are shown in Table 3, column 1 (Base Model). The results echo the graphic depiction in Figure 3, Panel A. The removal of tenure policies was associated with no significant average increase in non-responder (3rd year) teacher exit in the 2011-12 school year, but with a 2.6 percentage point increase in the probability of exit for 4th year teachers. There is no significant difference between the slopes of the two groups between 2012 and 2013 (indicated by the statistically insignificant coefficient on the interaction between 2013 and responder). This suggests that the removal of tenure protections may cause an immediate shock to the teacher labor force among early-career teachers, mostly seen in the first year after the reform, with little or no additional increase in exit rates in subsequent years. This implementation-year effect on attrition is expected given that 4th-year teachers experienced the reform as a one-time shock. After the first year, it would seem reasonable to expect that teachers would continue to exit as a result of the reform (indeed, this is shown in both Figure 2 and Figure 3A), but there would be no reason to think that 4th year teachers would be any more likely to exit at higher rates than 3rd year teachers in the out years.

Table 3 also reports the results of falsification tests for the CITS analysis. Column 2 replicates the analyses in Column 1, this time examining 3rd and 4th year charter school teachers rather than TPS teachers. We find no effect of tenure removal for 4th relative to 3rd year charter school teachers. This is a particularly critical test because it provides further evidence that the effects shown in column 1 stem from the removal of tenure protections rather than from the onset of the Compass evaluation system. Column 3 substitutes in 4th and 5th year traditional public school

teachers, and we again find no effect of tenure removal for this comparison. For the Ashenfleter dip test (Column 4), we simply move our first "treatment" year up to the 2010-11 school year, and we see no differential "impact" of the reform for 4th versus 3rd year teachers in the pre-year. To further probe the effect found in Column 1, we replace the Ashenfelter dip test with a Granger test that assess the difference in attrition in all pre-reform years, not just in 2011. The Granger test specifies all of the years in our panel and interacts each year with the 4th-year indicator. Our results are displayed in Table A2 of the Online Appendix. Joint F-tests of all of the pre-policy year by responder interactions suggest that there is no indication that our primary results are biased in this way. The final falsification tests (Columns 5-6) replace the dependent variable of teacher exit with two measures of school-level student characteristics that should not be impacted by the tenure reform. If we see differential "effects" of tenure reform on 4th- relative to 3rd-year teachers on school size (enrollment) or the proportion of students who are white, we may be capturing something about the post-reform period that impacts schools as a whole. However, we find no effect of tenure removal on either student enrollment outcome (shown by the insignificant coefficients on the relevant interaction terms). All together, these results suggest that the estimates from our base model CITS analysis are picking up genuine effects of tenure reform rather than confounding teacher, student, or policy effects.

Later Career Teachers

We next compare teachers with a certain income upon exit (those with 25-30 years of experience who are eligible for full retirement benefits) and teachers with significant experience, but not enough to access certain income through retirement (teachers with 10-19 years of experience). We also compare teachers with full retirement eligibility to teachers with only partial eligibility (20-24 years of experience), and partially-eligible teachers to ineligible teachers. The theory outlined in Section III predicts that teachers with a guaranteed alternative with known compensation

(retirement-eligible) will offset the loss of tenure and assumed risk by demonstrating higher exit rates than those teachers who do not have this option. Table 4 presents results for this analysis.

Column 1 presents the results of our base model specification for these experience groups, with fully retirement-eligible teachers as the responder group that we expect to be more affected by tenure reform and ineligible teachers as the control. We show that the removal of tenure protections increased both retirement-eligible and ineligible teachers' exit rates in the year of reform implementation, but that the retirement-eligible teachers were 2.6 percentage points more likely to exit in the 2011-12 school year than their retirement ineligible counterparts. In the 2012-13 school year, both sets of teachers were again more likely to exit relative to the base year before the reform, but retirement-eligible teachers remained 1.5 percentage points more likely to exit than their ineligible colleagues. These results are illustrated in Figure 3, Panel B.

Columns 2-7 of Table 4 report CITS results for the falsification tests: comparing fully versus partially retirement-eligible teachers (those with 20-24 years of experience, column 2), partially eligible versus ineligible teachers (column 3), using the sample of charter school teachers (column 4), examining the potential for a pre-intervention dip (column 5), and using alternative outcomes (columns 6 and 7). Our first robustness checks shown in columns 2 and 3, are actually more confirmation tests than falsification tests, or they can alternately be viewed as tests of a dose response. In column 2 we compare teachers who are eligible for partial retirement benefits (20-24 years of experience) to a responder group that is eligible for full retirement benefits (25-30 years of experience). In column 5, we compare teachers who are not eligible for any retirement benefits (10-19 years of experience) to a responder group that is eligible for partial benefits (20-24 years of experience).

Our results in column 2 are consistent with the idea that retirement eligibility acts as a dosage treatment; both partially and fully retirement-eligible teachers are more likely to exit in the

year of the reform, but fully retirement-eligible teachers are 2.0 percentage points even more likely to exit than their partially eligible colleagues relative to the year before the reform. However, in the 2012-13 school year, the two groups do not exit at differential rates (both groups have approximately a 3.5 percentage point increase in exit relative to the pre-year). Column 3 shows that the pattern is reversed when we compare partially eligible to ineligible teachers: both groups are approximately 1.3 percentage points more likely to exit in the 2011-12 school year, with no differential rate of exit between the two groups, but partially-eligible teachers are 1.5 percentage points more likely to exit than are ineligible teachers in the 2012-13 school year. These results confirm our hypotheses; as expected, the effect of the tenure-reform is significantly larger for teachers whose certainty of alternative income is greater than the comparison group.[25]

The remainder of the robustness tests provide evidence that our base model estimates capture the causal impact of the removal of tenure protections on the differential attrition of teachers who are and are not eligible for retirement benefits. We again find no effects of the tenure policy change on charter school teachers from either experience group (column 4). In addition, the interaction between retirement eligibility and the 2010-11 "pre-year" is small in magnitude and insignificant, suggesting that there is no evidence of our results being driven by a shift in the year before the reform, including due to changes to employment continuation policies affecting only the retirement eligible group (column 5). Finally, there is no evidence that tenure reform is related to exogenous changes in student populations (columns 6 and 7). In short, there is no evidence that our results are related to plausible factors other than the causal impact of the removal of tenure protections on retirement-eligible teachers' propensities to exit teaching.

Traditional Public School vs. Charter School Teachers

Table 5 and Figure 3 (Panel C) provide results of our CITS models predicting TPS teacher exit rates relative to those of charter school teachers in the years after the tenure reform. Column 1

(base model) shows that, controlling for teachers and school characteristics, charter school teachers' ("non-responders") propensities to exit teaching in Louisiana were unaffected by the removal of tenure protections. By contrast, exit rates for teachers in TPS schools ("responders") increased approximately three percentage points relative to the baseline year, compared to changes in exit rates for charter teachers. In other words, in the first year of the reform, TPS teachers' exit rates increased relative to charter teachers, and then remained consistent, relative to charter teachers, in the second year of the reform. This pattern can also be seen in Figure 3, Panel C. TPS teachers' exit rates clearly increase in each of the two years after the reform (2012 and 2013), but charter school teachers' exit rates also increase in the second year post-reform such that the relative (to charter teachers) increase in TPS teachers' exit rates remains consistent in each of the two years. [26]

These results support our hypothesis that TPS teachers are more likely to respond to the removal of tenure protections through increased exit than are charter teachers, since TPS teachers *lost* employment protections in 2012 and charter school teachers had no protections to lose. Table 5 includes three falsification tests to ensure that that these results do not reflect a deviation from previous years' exit propensities stemming from an Ashenfelter's dip in the year before the implementation of the reform (Column 2), or a reaction to some larger shift that occurred simultaneously with the tenure reform (Columns 3 and 4). All three tests confirm that our base model estimates appear to be unbiased. The coefficient on the 2011 school year x responder interaction term is neither substantial nor significant and there are no significant coefficients on our variables of interest in either of the falsification tests predicting school enrollment and proportion of white students. The significant coefficients on the responder indicator in both columns 3 and 4 are expected given the differences between charter and TPS populations (see Table 1).

Differential Impacts on Teachers in Disadvantaged Schools

Last, we examine whether or not the tenure reform appeared to drive up exit rates

differentially for teachers who teach in traditionally disadvantaged schools. Table 6 provides results for teachers in schools with high (top 25%) or low (bottom 25%) proportions of low-income and minority students, as well as schools that achieve in the top and bottom quartiles in terms of SPS level and growth. We find that there are no differential rates of attrition for teachers in the least or most disadvantaged schools, where disadvantage is defined as the proportion of minority students in the school or by SPS level or growth, relative to teachers in the schools in the middle two quartiles of disadvantage. However, we do find suggestive evidence that teachers in schools with the most low-income students are less likely to exit as a result of the tenure reform, which would be counterintuitive. However, when we run F-tests to compare the "effects" of the reform on teachers in the most and least disadvantaged schools, we see that there are no significant differences in attrition between teachers in these schools. These results suggest that teachers in the "hardest-to-staff" schools do not exhibit differential attrition rates relative to teachers in less disadvantaged schools in the state. As such, it does not appear that students in these schools are disproportionately harmed by the tenure reform.

Table 7 compares teachers in A-rated schools to those in F-rated schools. Our base model shows that, in 2012, teachers in F-rated schools were approximately two percentage points more likely to exit the workforce compared to teachers in A-rated schools. This persisted in 2013, with teachers in F-rated schools still approximately two percentage points more likely to exit. The following three columns show our falsification test results, which all uphold our base model results. These findings indicate that teachers who face the most accountability pressure (they teach in F-rated schools) are more likely to leave when the removal of tenure effectively increases accountability even more without some sort of remediating compensation. If this is the case, then the most disadvantaged students – those who are already subjected to the worst schools in the state – are harmed most by tenure-induced churn.

VI. DISCUSSION AND POLICY IMPLICATIONS

While many vocal opponents of teacher employment protections have advocated for reform or removal of state tenure laws, the widespread and long-term existence of stable tenure protections makes it difficult to anticipate the potential impact on school districts through mass teacher exit. Using Louisiana's rapid and comprehensive removal of tenure protections as a case study, we are the first to provide empirical evidence about the effects of tenure reform on teacher attrition. We find that teachers responded to the loss of tenure much as critics of these reforms predicted they would. First, the removal of tenure led to an increase in teacher exit immediately after implementation. Second, we find predictably larger jumps in exit rates for teachers who have more to lose in terms of overall compensation. These findings are robust to three specific teacher populations with differential benefits from tenure: teachers who lost tenure protections compared to similar teachers who never had tenure; teachers who had alternative options such as defined income retirement eligibility compared to similar teachers who did not; and TPS teachers, who were at all impacted by the policy change, relative to charter school teachers, who were not. These patterns are most clearly shown in Figure 3. First, teachers with three years of experience, who never received tenure protections, are no more or less likely to exit in 2012 than in 2011. However, teachers with four years of experience, teachers with 10-19 years of experience and teachers with 25-30 years of experience are all more likely to exit in 2012. Teachers with 10-19 years of experience, who are closing in on retirement eligibility (and teachers with 20-24 years of experience, who are partially-retirement eligible), are less likely to exit than teachers who are fully retirement eligible. In addition, TPS teachers are more likely to exit post-reform than are charter teachers. Last, we find suggestive evidence that the effects of the reform are particularly severe for teachers in schools facing the greatest accountability pressures (F-rated schools). These latter results indicate that the most disadvantaged students – those who are already subject to the worst schools in the state – are the

most impacted by tenure reform-induced churn.

The primary limitation to this work is that it is difficult to separate the effect of the removal of tenure from the implementation of the Compass teacher evaluation system. Although we attempt to show that the patterns reported in our findings do, in fact, identify the impacts of tenure reform net of the effect of the implementation of the new teacher evaluation policy, this empirical issue may be less relevant for policy. So far, every state that has diminished or removed tenure protections has done so in combination with the implementation of teacher evaluation systems that have high stakes for teacher employment. In this sense, if we are indeed capturing the combined effect of the two reforms, similar effects should be expected in other states given that all states have implemented teacher evaluation and tenure reform as a package. If we instead are isolating the impact of the removal of tenure protections, net of the two percentage point increase in attrition stemming from the passage of the Act 54, we may be underestimating the effects that may be expected when states implement teacher accountability systems that incorporate both tenure and evaluation reforms.

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, then there may have been pent-up demand for teacher retirements in the 2011-12 and 2012-13 school years, as recessionary pressures eased. If this is the case, then our overall ITS and CITS 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.

Another potential limitation of this work stems from our use of charter school teachers as a comparison group for TPS teachers, given that the two populations differ in both observable (see

Table 1) and likely unobservable characteristics. The use of a CITS model as opposed to a more traditional difference-in-difference approach enables us to relax some of the necessary assumptions about parallel pre-trends and baseline similarities. Nonetheless, given the differences between TPS and charter teachers, we do not rely on charter school teachers as our sole comparison group.

There are important implications of these results for other states considering a similar removal of teacher tenure. First, states should recognize that teachers value employment protections such as tenure as a component of their compensation, and that this value will be greater in fields, like teaching, where employees are particularly risk averse. Public employers such as school districts undergoing reforms to employee protections should expect and plan for a substantial jump in employee exit immediately following this policy change.

Advocates of these reforms will say that turnover is not always a bad thing if they cause the profession to lose less effective employees who theoretically face the greatest risk under policy changes that, like Louisiana's, replace tenure protections with performance-based employment schemes. For example, recent evidence provided by Loeb et al. (2015) suggests that in New York City's tenure reform, the less-effective teachers exited the system. Although we do not have access to teacher-level quality measures for Louisiana, there is little reason to believe that ineffectiveness was more prevalent among any group within our analytic comparison groups – i.e. that 4th year teachers are more likely to be ineffective than 3rd year teachers or that retirement-eligible teachers are more likely to be ineffective than highly-experienced but non-eligible teachers.

Regardless of teacher effectiveness, there should be some concern that the elimination of tenure policies appears to result in the exit of so many teachers. Teacher turnover on its own is harmful to teachers who remain in their schools and to students (e.g., Guin, 2004; Ronfeldt, Loeb & Wyckoff, 2013; Ost, 2014). As such, a policy that increases teacher turnover without substantial attention paid to the retention of high quality or particularly committed teachers and the removal of

low-quality teachers should be carefully considered. In particular, a policy that causes so many teachers to exit the profession will force schools and districts to hire a substantial number of new teachers, and it is unlikely that all of these teachers will be of equal or greater quality as those who exited. This may result in a net decrease in teacher quality, and in the most impacted schools and districts, even if some subset of the lowest-performing teachers do exit the system. This is particularly concerning given our results showing that teachers in F-rated schools are even more likely to exit post-reform.

Our results also suggest implications for costs to schools, districts, and the state. It is expensive to recruit, hire and train new teachers. Some estimates place these costs at $4,366 to $17,872 per new teacher, depending on the geographic context (NCTAF, 2007). In addition, by pushing teachers to retire earlier than they otherwise would, Louisiana loses contributions to pension funds, and must pay out defined benefits for longer than it otherwise would have to. 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, the implication for costs given policy-induced exits may also be positive for districts, as it appears from our analyses that the most experienced, and therefore the most costly, teachers retire earlier. If these teachers are replaced with new or even newer teachers, as they likely are, districts' salary costs will decrease.

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 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 to remain in public schools and 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. At a minimum, the current results buttress Rothstein's (2015) simulation study outcomes, suggesting that, at least in the short term, employees will respond to the loss of tenure by exiting the profession unless states implement counteracting policies to stem attrition.

Along these line, states might implement a suite of reforms – not just employee evaluation and the removal of tenure, but also policies that reward both effectiveness and retention. In the case of teacher compensation, charter schools might provide a better model. By design, charter schools shift employment risk to teachers to enable more flexible hiring, compensation, and firing. However, charter schools may provide non-monetary compensating differentials to account for this employment risk by, for instance, providing teachers with greater autonomy and shared governance, or offering a faster track for raises and promotion. In related work, we find that charter schools in Louisiana compensate teachers for school-level productivity. Although teachers may face greater risk in employment in charter schools, they also experience greater rewards for school effectiveness – something that is not often rewarded in TPS compensation structures (Authors, 2016b).

Overall, evidence from Louisiana suggests that there is indeed cause for concern when states implement wholesale changes in teacher employment policy without enacting complementary policies to ward off unintended consequences. As more states follow suit, policymakers will do well to consider employment protections in the context of overall compensation and employee motivation. Teachers, like everyone else, will respond when their overall compensation is decreased. Efforts to improve efficiency should include attention to costs of turnover, the pipeline of potential replacement teachers, and the implications for public employee pensions.

Endnotes

1. 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.
2. Given that teachers' unions are heavily invested in protecting employment protections for their teachers, it is worthwhile to note that Louisiana is not a particularly strong teachers' union state; bargaining is not mandated in Louisiana and unions are prohibited from collecting agency shop dues (Winkler, Scull & Zeehandelaar, 2012). In addition, tenure is dictated by state policy (in all states, not just Louisiana), so teachers' unions or associations can only impact tenure policies through lobbying at the state level – they cannot dictate tenure provisions through collective negotiations (Thompsen, 2016).
3. The old statute does not specify what would be considered valid reasons.
4. Although there are not yet sufficient data to examine the difficulty of meeting this requirement in Louisiana, using estimates from Koedel and Betts (2010) and assuming that Compass identifies the top 20 percent of teachers as highly effective, the probability of a teacher obtaining a highly effective VAM rating for five of six consecutive years is only 2.8 -5.5 percent.
5. Act 1 also instituted other policy changes that further diminished protections usually afforded to teachers regardless of their performance in the classroom. In particular, Act 1 prohibits teachers rated as ineffective from receiving pay raises, although it reaffirms that compensation cannot be decreased and retirement benefits cannot be reduced due to poor performance. In addition, teachers who lose "tenure" status due to poor performance are no longer eligible to receive compensated sabbatical or compensated medical leaves. Finally, Act 1 prohibits districts from executing reductions-in-force (RIFs, or layoffs) based primarily on teachers' tenure status.
6. In addition, Louisiana introduced two policy changes for the 2011 school year that only affected retirement-eligible teachers. Specifically, Louisiana made changes to the Deferred Retirement Option Program (DROP) and the retire-rehire program. These changes limited teachers' ability to continue to work or return to work while also drawing retirement benefits. Thus, any likely impact of the pension reforms would cause fewer retirement-eligible teachers to exit the workforce in 2011 and beyond (the year before tenure reform and in the years of the reform). Because one of our set of sub-analyses relies on assessing changes to teacher retirements as a result of the tenure reforms, we explore this possibility and discuss the findings in the results section.
7. Because administrators did not have sufficient data from Compass yet provided to them in the 2012 and 2013 school years, all teacher exits attributable to the reform should be voluntary in the post-reform years under study. In particular, in spring/summer 2012, principals would not have had Compass observations or value-added measures of teachers' performance on which to base decisions about teachers' dismissals. In spring/summer 2013, principals would have had teachers' observation ratings, but they would not have had VAMs, and therefore the final Compass ratings, in time for dismissal decisions to be made. Given the way that the law was written, it seems unlikely that administrators would have risked due process proceedings to dismiss a teacher based on incomplete evidence during that time. As a result, we believe that the first year post-reform attrition was entirely due to voluntary exits, and most if not all of the attrition seen in the second year post-reform was also due to voluntary exits.
8. As the authors note, they cannot determine if extended 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 (Loeb, Miller & Wyckoff, 2015).
9. Some teachers temporarily exit the PEP for one or more years before returning to a teaching position. Because our data end in 2014, we cannot track returns for more than one year post-tenure reform. Therefore, 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. In the years for which we can track teachers for more than two years post exit (through 2011), we observe 11 percent of exiting teachers returning to teaching.

10. Temporary gaps in teaching prior to 2002 are not observed.
11. The experience measures are highly correlated. Both the salary- and hire date-derived experience measures are available for 80% of teachers in our sample. In 92% of these cases, there is agreement within $100 between the two experience measures. For the teachers for whom there is not a high degree of agreement, we default to the salary-based measure.
12. As a final confirmation we cross-checked each teacher's district hire date with his undergraduate completion year to ensure that no teacher was "hired" prior to graduating. There were 2,567 (0.63 percent) instances when this was the case. In these instances, we imputed the hire date as the year after the employee received his degree.
13. Details on the calculations for each year are reported at https://www.louisianabelieves.com/resources/library/performance-scores.
14. A small number of teachers transfer between TPS and charter schools during the period of analysis. We omit these teacher from the analysis to avoid using a single teacher in both groups. Thus, our definition of exit applies only to teacher who exited all types of Louisiana public schools and not to teachers who exited to charter schools (or vice versa).
15. All New Orleans schools were closed for at least part of 2005-06 during the evacuation of the city. At that time, the state's Recovery School District took over the majority of the city's public schools and eventually transitioned them to charter schools. This process generated new charter schools each year and also caused the closure of many schools, both TPS and charter. To avoid bias from teacher exit due to school closure, we exclude all teachers whose school was closed in the subsequent school year.
16. Our statewide data set 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.
17. We also run all models presented in this paper as logistic regressions and get the same results. We provide our LPMs for ease of interpretation. Logit results are available upon request.
18. 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.
19. If less effective teachers are more likely to exit each year, 4th year teachers may be more effective on average than 3rd year teachers. However, there is no reason to believe that, conditional on effectiveness, they will vary in their response to a policy like Compass that provides information about effectiveness.
20. Although TRSL rules of eligibility changed in 1999 for teachers who began teaching in Louisiana after that year, we are concerned with the TRSL regulations in effect when retirement-eligible teachers began teaching – well before 1999. The old defined benefit plan guaranteed teachers a set yearly income upon retirement, calculated by multiplying the teacher's total number of years of service as a Louisiana public school teacher upon retirement by the average of the highest consecutive three years of salary and by a "benefit factor." The benefit factor depends on the teacher's age and years of in-state teaching service. In Louisiana, fixed-income benefits begin following 25 years of teaching. Specifically, a teacher at age 60 with five years of service or a teacher at any age with 20 years of service is eligible for a two percent benefit factor. A teacher at age 65 with 20 years of service, a teacher at age 55 with 25 years of service, or a teacher at any

age with 30 years of service is eligible for a 2.5 percent benefit factor. Unfortunately, we do not know the age of the teachers in our sample. Therefore, using several classifications of retirement eligibility, we examine the effect of tenure reform on teacher exit for those teachers eligible for some type of retirement benefit. We group teachers into a "partial" retirement group which includes teachers with 20-24 years of experience and a "full" retirement group which includes teachers with 25-30 years of experience. Because we do not have age, it is likely that we are including some nonretirement-eligible teachers in the sample of retirement eligible teachers and this inclusion would likely bias our results downward.

21. Other teachers with less risky alternative opportunities might be teachers who are credentialed to teach in shortage areas such as special education or science and math fields. We test these groups' propensities to exit relative to non-shortage area teachers, as well. We do not find evidence that the removal of tenure differentially impacts shortage subject teachers. This may be because we only can include subject *credential* rather than college major, and a credential in math or science may not indicate better non-education alternative opportunities. Nonetheless, certification in high-needs areas should make the teacher more employable in other states or the private sector. Results are available in Online Appendix Table A1.
22. Note that for the comparison of charter and TPS teachers, we include school fixed effects in our estimation of equation [2]. However, because charter or TPS status does not vary within schools over time, we cannot include the direct effect of the TPS (Responder). We run the models without school fixed effects and with a host of school-level controls and find the same results. Available from the authors upon request.
23. In addition, we run a Granger test on the early career teacher analysis to examine pre-reform differences between comparison groups across pre-reform years. This analysis is intended to bolster our Ashenfelter dip falsification test.
24. This also might be considered a test of a "dosage" response, rather than a robustness test. Partial retirement-eligible teachers can be viewed as having a partial dose of the "treatment" in that they do not have as clear of an alternative option as teachers who are eligible for their full retirement income, but they do have less incentive to remain in the teaching force when tenure is removed than teachers who are not at all eligible for retirement benefits. In either case, we would expect to see that, after the removal of tenure benefits, teachers who are fully retirement eligible are more likely to exit teaching in Louisiana than are teachers who are partially eligible, and partially eligible teachers are more likely to exit than are teachers who are ineligible.
25. In addition, we specify models that compare fully eligible teachers (with 25 or more years of experience) with the remainder of teachers with 10 to 24 years of experience (ineligible and partially eligible teachers) and that compare ineligible teachers (10-19 years of experience) to teachers that have any retirement eligibility (20 years of more of experience). As expected, we find significant positive impacts of the reform on attrition of the more eligible groups relative to the partially/ineligible groups. Results can be found in the Online Appendix Table A3.
26. We observe in Figure 3, Panel C that charter school teacher exit rates were increasing steadily to 2012. What we observe in 2012 is a pause in an upward trend that continues in 2013. It is unclear whether it was indirectly related to tenure reform, perhaps if charter school teachers began to value their untenured position more as tenured positions in TPS were eliminated, and the negative 2012 effect depicted in the graph for charter teachers is not significantly different than zero. More importantly for the current study, there is no evidence of an increase in the exit probability of charter school teachers at the time of tenure reform that suggests an alternative reason for the increased exit of TPS teachers.

References

Act 1, Louisiana H.B. No. 974 (2012). Retrieved from http://www.legis.la.gov/legis/ViewDocument.aspx?d=793654 on May 4, 2012.

Ainslie, G., and Nick Haslam. (1992), "Hyperbolic Discounting." In George Loewenstein and Jon Elster (eds.), Choice Over Time, New York: Russell Sage Foundation, pp. 57–92

Antos, J.R. & Rosen, S. (1975). Discrimination in the Market for Public School Teachers. *Journal of Econometrics, 3(2),* 123-150.

Ashenfelter, O. (1978). Estimating the effect of training programs on earnings. *The Review of Economics and Statistics*, 47-57.

Backes, B., Goldhaber, D., Grouth, C., Koedel, C.., Ni, S., Podgursky, M., Xiang, P.B., & Xu, Z. (2015). Benefit or Burden? On the Intergenerational Inequity of Teacher Pension Plans. Working Paper #148. CALDER.

Battaglio, R.P. Jr. (2010). Public service reform and motivation: Evidence from an employment at-will environment. *Review of Public Personnel Administration*, 30(3), 341-363.

Battaglio, R. P. Jr & French, P.E. (2016). Public service motivation, public management reform, and organizational socialization: Testing the effects of employment at-will and agency on PSM among municipal employees. *Public Personnel Management*, 1-25.

Bloom, H. S. (1999). Estimating program impacts on student achievement using "short" interrupted time series. *Washington, DC: MDRC.*

Bowman, J.S. & West, J.P. (2006). Ending civil service protections in Florida government. *Review of Public Personnel Administration*, 26(2), 139-157.

Brunner, E.J. & Imazeki, J. (2010). Probation Length and Teacher Salaries: Does Waiting Pay Off? *Industrial and Labor Relations Review, 64(1),* 164-180.

Cameron, A. C., & Miller, D. L. (2015). A practitioner's guide to cluster-robust inference. *Journal of Human Resources*, 50(2), 317-372.

Chambers, J. G. (1981). The hedonic wage technique as a tool for estimating the costs of school personnel: A theoretical exposition with implications for empirical analysis. *Journal of Education Finance*, 330-354.

Chemerinsky, E. (October 23, 2014). Teacher tenure: Wrong Target. The New York Daily News. Retrieved on March 4, 2016 at: http://www.nydailynews.com/opinion/teacher-tenure-wrong-target-article-1.1983826

Christie, K. & Zinth, J.D. (2011). Teacher tenure or continuing contract laws. Education Commission of the States. Denver, CO. Retrieved on April 19, 2016 from:

http://www.ecs.org/clearinghouse/94/93/9493.pdf

Duflo, E. (2001). Schooling and labor market consequences of school construction in Indonesia: Evidence from an unusual policy experiment. *American Economic Review*, 1-60.

Frederick, S., George Loewenstein, and T. O'Donoghue. (2003). "Time discounting and time preference: a critical review." In Loewenstein, Read, and Baumeister, (eds.), Time and Decision, 13–86.

Feinberg, R. M. (1981a) Employment risk, earnings, and market structure. *Applied Economics*, 13, 257-265.

Feinberg, R. M. (1981b) Earnings risk as a compensating differential. Southern Economic Journal, 48, 156-163.

Guin, K. (2004). Chronic Teacher Turnover in Urban Elementary Schools. *Educational Policy Analysis Archives, 12*(42).

Hanushek, E. (March 2, 2015). Ending Tenure to More Easily Fire Bad Teachers Helps Everyone. *The New York Times.* Retrieved on May 12, 2016 from: http://www.nytimes.com/roomfordebate/2014/06/11/does-tenure-protect-bad-teachers-or-good-schools/ending-tenure-to-more-easily-fire-bad-teachers-helps-everyone

Horng, E. (2009). Teacher Tradeoffs: Disentangling Teachers' Preferences for Working Conditions and Student Demographics. *American Educational Research Journal, 46(3),* 690-717.

Kenny, L.W. & Denslow, D.A. (1980). Compensating Differentials in Teacher Salaries. *Journal of Urban Economics, 7(2),* 198-207.

Koedel, C., & Betts, J. (2010). Value added to what? How a ceiling in the testing instrument influences value-added estimation. *Education Finance and Policy*, 5(1), 54-81.

Levinson, A. (1988). Reexamining Teacher Preferences and Compensating Wages. *Economics of Education Review, 7(3),* 357-364.

Loeb, S., Miller, L. C., & Wyckoff, J. (2015). Performance Screens for School Improvement: The Case of Teacher Tenure Reform in New York City. *Educational researcher*, *44*(4), 199-212.

Loeb, S. & Page, M. (2000). Examining the Link between Teacher Wages and Student Outcomes: the Importance of Alternative Labor Market Opportunities and Non-Pecuniary Variation. *Review of Economics and Statistics, 82(3),* 393-408.

Murnane, R., & Olsen, R. (1990). The effects of salaries and opportunity costs on length of stay in teaching: Evidence from North Carolina. *Journal of Human Resources, 25(1),* 106-124.

National Center on Teaching Quality (2016). State Policy Issues: Tenure. Retrieved on April 21, 2016 from http://www.nctq.org/statePolicy/2015/statePolicyIssues.do#9_34.

National Commission on Teaching and America's Future (2007). The High Cost of Teacher Turnover.

Ost, B. (2014). How do teachers improve? The relative importance of specific and general human capital. *American Economic Journal: Applied Economics, 6(2),* 127-151.

Public Impact (2011). Teacher tenure reform: Applying lessons from civil service and higher education. Chapel Hill, NC.

Ravitch, D. (February 2, 2015). Tenure is a Guarantee of Due Process to Prevent Capricious Firings. The New York Times. Retrieved on March 4, 2016 at: http://www.nytimes.com/roomfordebate/2014/06/11/does-tenure-protect-bad-teachers-or-good-schools/tenure-is-a-guarantee-of-due-process-to-prevent-capricious-firings

Ronfeldt, M., Loeb, S., & Wyckoff, J. (2013). How teacher turnover harms student achievement. *American Educational Research Journal, 50*(1), 4-36.

Rothstein, J. (2015). Teacher Quality Policy When Supply Matters. *American Economic Review, 105(1),* 100-130.

Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). Quasi-experiments: interrupted time-series designs. *Experimental and quasi-experimental designs for generalized causal inference*, 171-205.

Somers, Marie-Andrée, Zhu, Pei, Jacob, Robin, & Bloom, Howard (2013). The validity and precision of the comparative interrupted time series design and the difference-in-difference design in educational evaluation. MDRC Working Paper on Research Methodology. New York: MDRC. Retrieved on May 1, 2016 at: http://www.mdrc.org/sites/default/files/validity_precision_comparative_interrupted_time_series_design.pdf.

Staiger, D.O. & Rockoff, J.E. (2010). Searching for Effective Teachers with Imperfect Information. *Journal of Economic Perspectives, 24(3),* 97-118.

Thompsen, J. (2016). 50-State Comparison: Teacher Tenure/Continuing Contract Policies. Education Commission of the States. Denver, CO. Retrieved on March 4, 2016 at: http://www.ecs.org/teacher-tenure-continuing-contract-policies/

Webb, M. D. (2013). *Reworking wild bootstrap based inference for clustered errors* (No. 1315). Queen's Economics Department Working Paper.

Winkler, A., Scull, J. & Zeehandelaar, D. (2012). *How Strong are Teachers' Unions? A State-by-State Comparison*. The Thomas Fordham Institute. Washington, D.C.

Figure 1: Timeline of teacher policy reforms in Louisiana

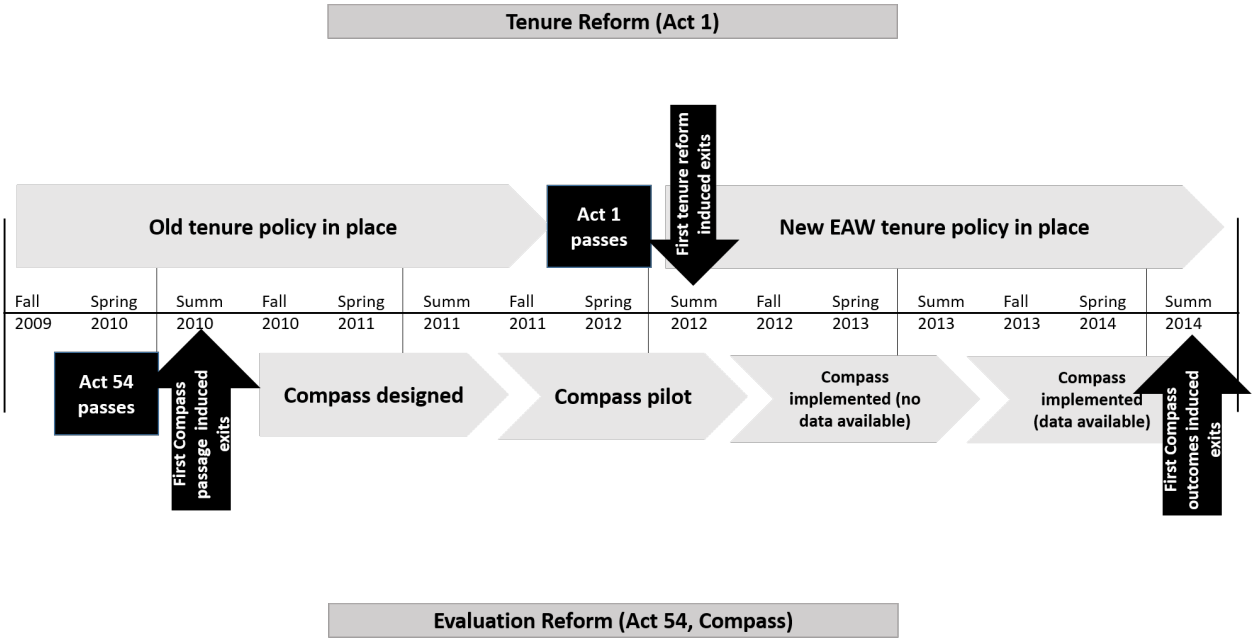
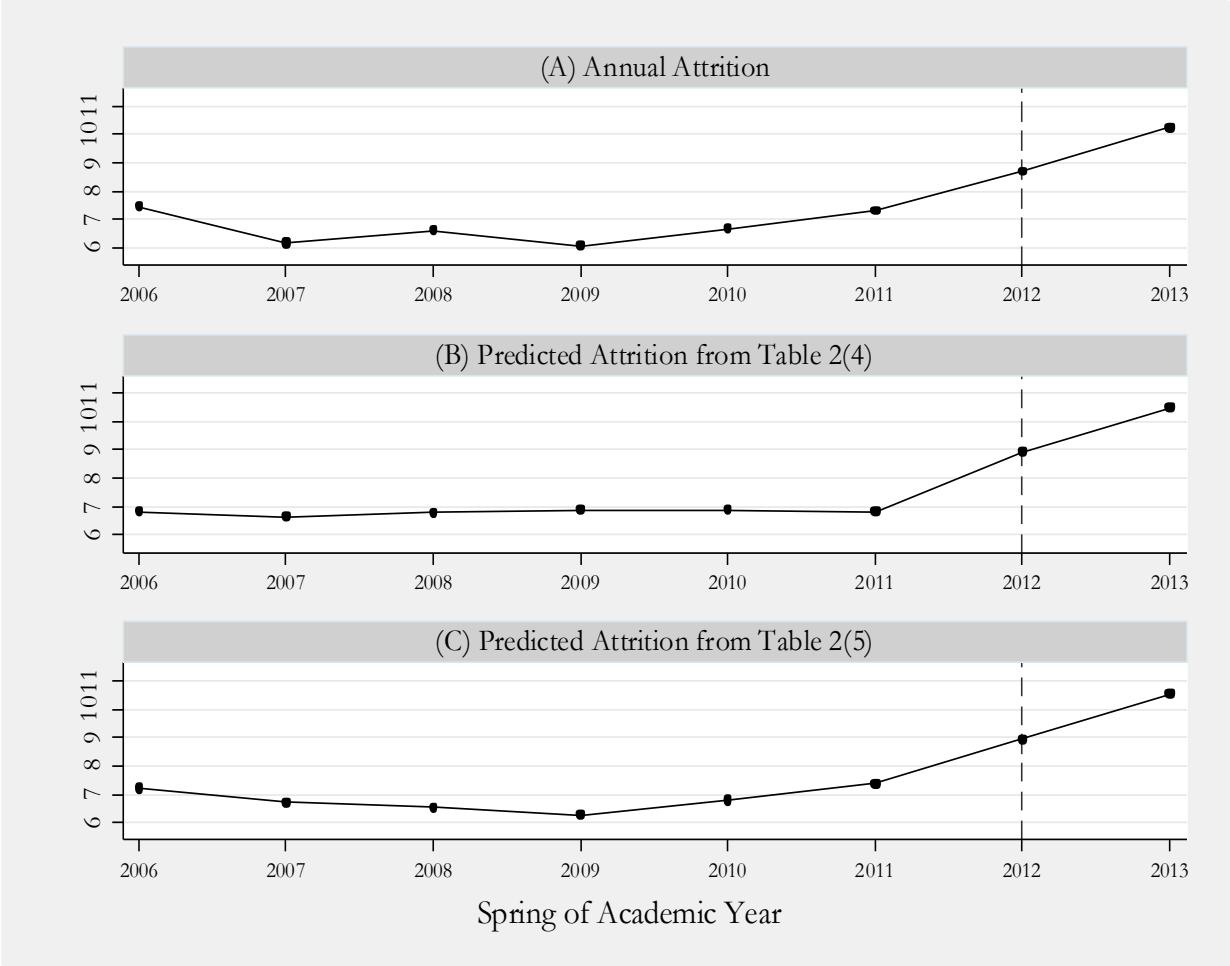
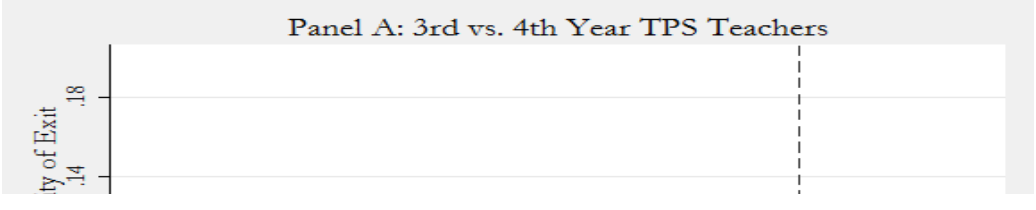


Figure 2: Teacher Attrition in Louisiana 2006-2013



Notes: Teacher exit rates are only reported for teachers in traditional public schools. Panel B displays the estimated teacher exit rates from Column 4 of Table 2 and Panel C displays the estimated teacher exits rates from Column 5 of Table 2.

Figure 3: The Effect of Tenure Reform on Attrition (CITS) – Various Subgroups



Notes: Teachers with 25-30 years of experience comprise the retirement eligible group. Teachers with 10-19 years of experience comprise the not eligible for retirement group.

Table 1. Summary Statistics by Teacher Group, 2006-2013

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

	By Tenure Eligibility		By Retirement Eligibility		By School Type		By School Type	
	Comparison 3 years Not tenured	Responder 4 years Tenured	Comparison 10-19 years Not eligible	Responder 25-30 years Eligible	Comparison Charter	Responder TPS	Comparison A-Rated	Responder F-Rated
Teacher exit	0.089	**0.061**	0.027	**0.065**	0.138	**0.066**	0.058	**0.082**
Teacher characteristics								
Years of experience	3.0	**4.0**	14.3	**27.3**	11.4	**15.1**	15.5	**14.7**
Female	0.808	0.816	0.834	0.836	0.733	**0.823**	0.818	**0.792**
Black	0.207	0.208	0.159	**0.190**	0.611	**0.204**	0.114	**0.395**
Other minority race	0.043	**0.025**	0.012	**0.006**	0.042	**0.018**	0.031	**0.023**
Bachelors degree only	0.828	**0.805**	0.705	**0.542**	0.693	**0.679**	0.510	**0.679**
SPED certification	0.203	**0.218**	0.262	**0.300**	0.202	**0.255**	0.196	**0.262**
STEM certification	0.150	0.144	0.142	**0.100**	0.146	**0.132**	0.148	**0.137**
Probationary certification (ever)	0.293	**0.256**	0.160	**0.100**	0.289	**0.210**	0.155	**0.233**
In-state college graduate	0.771	**0.822**	0.884	**0.837**	0.594	**0.849**	0.776	**0.829**
School characteristics								
Percent black	49.1	**47.0**	39.1	**40.8**	91.9	**44.1**	25.9	**74.0**
Percent other minority	4.8	4.7	4.9	4.9	3.6	**5.2**	8.5	**4.6**
Percent free/reduced price lunch	65.7	**64.2**	59.9	**59.1**	82.9	**62.9**	30.0	**81.2**
Percent limited English proficient	1.7	**1.6**	1.6	**1.7**	2.0	**1.9**	1.6	**1.9**
Percent special education	12.6	12.7	13.1	**13.3**	8.2	**13.2**	3.9	**14.1**
Percent gifted	2.9	2.9	3.2	**3.5**	2.2	**2.9**	12.9	**1.2**
School performance z-score	-0.026	**0.022**	0.198	**0.157**	-0.913	**0.100**	2.145	**-0.896**
Number of observations	14,489	13,740	65,285	33,782	10,387	259,559	7,050	96,547

Panel B: 2012-2013 (Post-Tenure Reform)

	By Tenure Eligibility		By Retirement Eligibility		By School Type		By School Type	
	Comparison 3 years Not tenured	Responder 4 years Tenured	Comparison 10-19 years Not eligible	Responder 25-30 years Eligible	Comparison Charter	Responder TPS	Comparison A-Rated	Responder F-Rated
Teacher exit	0.106	0.095	0.044	**0.090**	0.179	**0.094**	0.062	**0.114**
Teacher characteristics								
Years of experience	3.0	**4.0**	14.3	**26.7**	9.5	**15.0**	16.2	**14.1**
Female	0.801	0.803	0.829	**0.850**	0.724	**0.823**	0.826	**0.793**
Black	0.173	0.185	0.185	**0.143**	0.528	**0.192**	0.098	**0.382**
Other minority race	0.041	**0.075**	0.015	**0.007**	0.055	**0.023**	0.029	0.034
Bachelors degree only	0.777	**0.753**	0.683	**0.601**	0.683	**0.668**	0.549	**0.656**
SPED certification	0.187	**0.210**	0.266	**0.295**	0.202	**0.259**	0.210	**0..263**
STEM certification	0.176	**0.194**	0.143	**0.130**	0.157	0.150	0.159	0.162
Probationary certification (ever)	0.327	**0.357**	0.160	**0.142**	0.338	**0.233**	0.178	**0.271**
In-state college graduate	0.736	0.732	0.887	**0.868**	0.502	**0.845**	0.808	0.812
School characteristics								
Percent black	49.0	48.9	39.4	**38.0**	90.5	**42.8**	23.2	**73.9**
Percent other minority	7.5	7.4	7.6	7.8	4.9	**7.6**	10.3	**6.7**
Percent free/reduced price lunch	69.1	69.1	63.4	**61.8**	87.0	**65.0**	33.9	**85.4**
Percent limited English proficient	2.2	2.3	2.1	**2.3**	2.2	2.2	0.9	**2.4**
Percent special education	11.7	11.7	12.6	12.9	10.2	**12.5**	4.7	**12.7**
Percent gifted	3.0	3.1	3.4	**3.6**	2.4	**3.0**	11.1	**1.4**
School performance z-score	0.031	0.017	0.224	**0.273**	-0.551	**0.165**	1.977	**-0.765**
Number of observations	4,233	4,377	22,963	9,250	5,533	99,801	3,471	28,064

Source: Author calculations from Louisiana administrative data. Data available for charter school teachers beginning in 2006-2007. 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 2: ITS Model Estimates of the Probability of Teacher Exit from 2006 to 2013

	(1)	(2)	(3)	(4)	(5)
Year	-0.000	0.000	0.000	0.000	-0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
2010					0.009***
					(0.002)
2011					0.020***
					(0.002)
2012	0.020***	0.021***	0.021***	0.022***	0.041***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
2013	0.035***	0.036***	0.038***	0.038***	0.060***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
Experience			-0.008***	-0.008***	-0.008***
			(0.000)	(0.000)	(0.000)
Experience Squared			0.000***	0.000***	0.000***
			(0.000)	(0.000)	(0.000)
Female			-0.003***	-0.005***	-0.005***
			(0.001)	(0.001)	(0.001)
Black			-0.012***	-0.014***	-0.014***
			(0.001)	(0.001)	(0.001)
Other Race			-0.005	-0.007*	-0.007*
			(0.003)	(0.004)	(0.004)
Bachelors			-0.001	-0.002**	-0.002**
			(0.001)	(0.001)	(0.001)
SPED Certification			-0.010***	-0.010***	-0.010***
			(0.001)	(0.001)	(0.001)
STEM Certification			0.005***	0.006***	0.006***
			(0.001)	(0.001)	(0.001)
Probationary Ever			-0.008***	-0.009***	-0.009***
			(0.001)	(0.001)	(0.001)
In-State Undergrad			-0.055***	-0.053***	-0.053***
			(0.001)	(0.002)	(0.002)
Percent of school			0.009***	0.006	0.008
enroll black			(0.002)	(0.014)	(0.014)
Percent of school			0.017**	0.007	-0.043
enroll other			(0.009)	(0.037)	(0.038)
Percent of school			0.003	-0.001	-0.003
enroll frp			(0.003)	(0.009)	(0.008)
Percent of school			-0.068***	0.032	0.097*
enroll lep			(0.015)	(0.052)	(0.053)
Percent of school			-0.020***	-0.012	-0.014
enroll sped			(0.004)	(0.011)	(0.012)
Percent of school			-0.024**	0.000	0.002
enroll gifted			(0.010)	(0.038)	(0.038)
SPS z-score			-0.009***	-0.004**	-0.004**
			(0.001)	(0.002)	(0.002)
Constant	0.067***	0.065***	0.143***	0.142***	0.152***
	(0.001)	(0.001)	(0.003)	(0.008)	(0.008)
No. of observations	406,782	406,782	406,782	406,782	406,782
No. of teachers	76,629	76,629	76,629	76,629	76,629
No. of schools	1,643	1,643	1,643	1,643	1,643
Fixed Effects	no	yes	no	yes	Yes

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Coefficients from OLS linear probability models estimating the probability of teacher exit in the following school year. Sample includes all teachers at traditional public schools from 2005-06 to 2012-13 with exit observed in the 2006-07 to 2013-14. Standard errors are clustered at the school level and are shown in parentheses.

Table 3: Teacher Attrition and the Effect of Tenure Reform 2006-2013 (CITS) – Early Career Teachers

	Base Model[a]	Falsification Test 1 Charter School Teachers[b]	Falsification Test 2 4th and 5th Year Teachers[c]	Falsification Test 3 Ashenfelter Dip Test[a]	Falsification Test 4 Student Enrollment[a]	Falsification Test 5 Percent White Students[a]
Year	-0.004***	0.001	0.001	-0.006***	-2.120*	-0.005***
	(0.002)	(0.014)	(0.001)	(0.002)	(1.193)	(0.000)
Responder	-0.014*	-0.077*	0.007	-0.013	0.961	0.001
	(0.007)	(0.044)	(0.006)	(0.008)	(2.048)	(0.001)
Year * Responder	-0.002	0.002	-0.001	-0.002	-0.341	-0.000
	(0.002)	(0.010)	(0.002)	(0.002)	(0.500)	(0.000)
2012	0.012	0.045	0.015**	0.020*	21.258***	0.002
	(0.009)	(0.040)	(0.007)	(0.011)	(4.339)	(0.001)
2012 * Responder	0.026**	0.036	-0.004	0.028**	1.606	-0.000
	(0.012)	(0.052)	(0.010)	(0.014)	(3.410)	(0.001)
2013	0.037***	0.026	0.040***	0.046***	28.791***	0.000
	(0.010)	(0.054)	(0.009)	(0.012)	(5.686)	(0.002)
2013 * Responder	0.020	0.082	-0.014	0.022	1.808	0.000
	(0.013)	(0.064)	(0.012)	(0.016)	(3.477)	(0.002)
2011				0.011		
				(0.008)		
2011 * Responder				0.002		
				(0.011)		
Constant	0.183***	0.315	0.123***	0.188***	711.216***	0.553***
	(0.027)	(0.217)	(0.023)	(0.027)	(44.846)	(0.010)
No. of observations	36,839	2,668	33,485	36,839	35,869	35,869
No. of teachers	8,165	870	5,745	8,165	7,900	7,900
No. of schools	1,517	220	1,523	1,517	1,452	1,452

a – Sample includes TPS teachers with four years of experience compared to TPS school teachers with three years of experience from 2006-2013. Responder is equal to one for 4th year teachers (who lost tenure protections) and equal to zero for 3rd year teachers (who never had tenure protections).
b – Sample includes charter school teachers with four years of experience compared to charter school teachers with three years of experience from 2007-2013. Responder is equal to one for 4th year teachers and equal to zero for 3rd year teachers.
c – Sample includes TPS teachers with four years of experience compared to TPS teachers with five years of experience from 2006-2013. Responder is equal to one for 5th year teachers and equal to zero for 4th year teachers.
Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Standard errors are clustered at the school level and are shown in parentheses. Teacher covariates include: gender, race, degree, probationary certification, certification area, and instate undergrad. School covariates include: percent Black, percent other race, percent free or reduced price lunch, percent special education, percent gifted, and school performance zscore. All specifications also include school fixed effects.

Table 4: Teacher Attrition and the Effect of Tenure Reform 2006-2013 (CITS) – Later Career Teachers

	Base Model[a]	Full Compared to Partial Retirement[b]	Partial Retirement Compared to 10-19[c]	Falsification Test 1 Charter School Teachers[d]	Falsification Test 2 Ashenfelter Dip Test[a]	Falsification Test 3 Student Enrollment[a]	Falsification Test 4 Percent White Students[a]
Year	-0.001*	0.004***	0.000	0.006	-0.002***	-1.476	-0.005***
	(0.000)	(0.001)	(0.000)	(0.007)	(0.001)	(1.011)	(0.000)
Responder	0.038***	0.036***	0.010**	0.065	0.038***	0.282	0.002*
	(0.003)	(0.005)	(0.004)	(0.040)	(0.004)	(1.979)	(0.001)
Year * Responder	-0.001	-0.005***	0.002**	-0.005	-0.001	-0.366	-0.000**
	(0.001)	(0.001)	(0.001)	(0.009)	(0.001)	(0.478)	(0.000)
2012	0.013***	0.015***	0.012***	0.013	0.018***	14.452***	-0.001
	(0.002)	(0.005)	(0.002)	(0.021)	(0.003)	(2.932)	(0.001)
2012 * Responder	0.026***	0.024***	0.006	0.042	0.027***	0.997	0.000
	(0.006)	(0.008)	(0.006)	(0.044)	(0.006)	(1.798)	(0.001)
2013	0.028***	0.034***	0.026***	0.032	0.035***	23.303***	-0.004**
	(0.003)	(0.006)	(0.003)	(0.030)	(0.004)	(3.929)	(0.002)
2013 * Responder	0.015**	0.009	0.015**	0.019	0.017**	0.347	0.000
	(0.006)	(0.009)	(0.007)	(0.051)	(0.008)	(2.358)	(0.001)
2011					0.008***		
					(0.002)		
2011 * Responder					0.003		
					(0.005)		
Constant	0.059***	0.053***	0.049***	0.544***	0.063***	681.049***	0.605***
	(0.009)	(0.013)	(0.008)	(0.177)	(0.009)	(25.261)	(0.009)
No. of observations	131,280	73,271	118,487	3,789	131,280	126,397	126,397
No. of teachers	20,622	12,029	17,627	960	20,622	19,620	19,620
No. of schools	1,626	1,594	1,611	221	1,626	1,519	1,519

a – Sample includes TPS teachers with full retirement eligibility compared to TPS teachers with no retirement eligibility and 10-19 year of experience from 2006-2013. Responder is equal to one for fully retirement eligible teachers and equal to zero for ineligible teachers.
b – Sample includes TPS teachers with full retirement eligibility compared to TPS teachers with partial retirement eligibility from 2006-2013. Responder is equal to one for fully retirement eligible teachers and equal to zero for partially eligible teachers.
c – Sample includes TPS teachers with partial retirement eligibility compared to ineligible TPS teachers with 10-19 years of experience from 2006-2013. Responder is equal to one for partially retirement eligible teachers and equal to zero for ineligible teachers.
d – Sample includes charter school teachers with 25 or more years of experience compared to charter school teachers with 10-19 years of experience from 2007-2013. Responder is equal to one for teachers with 25 or more years of experience and equal to zero for 10-19 years of experience.
Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Standard errors are clustered at the school level and are shown in parentheses. Teacher covariates include: gender, race, degree, probationary certification, certification area, and instate undergrad. School covariates include: percent Black, percent other race, percent free or reduced price lunch, percent special education, percent gifted, and school performance zscore. All specifications also include school fixed effects.

Table 5: Teacher Attrition and the Effect of Tenure Reform 2007-2013 (CITS) – TPS Teachers vs. Charter School Teachers

	(1)	(2)	(3)	(4)
	Base Model	Falsification Test 1 Policy Year	Falsification Test 2 Enrollment	Falsification Test 3 Percent White
Year	0.016***	0.014***	18.328***	0.003**
	(0.002)	(0.003)	(2.410)	(0.001)
Year * Responder	-0.014***	-0.013***	-17.044***	-0.002
	(0.002)	(0.003)	(2.441)	(0.001)
2012	-0.017**	-0.009	3.314	-0.007
	(0.008)	(0.011)	(9.141)	(0.005)
2012 * Responder	0.031***	0.028**	14.690	0.009
	(0.008)	(0.011)	(9.354)	(0.006)
2013	-0.003	0.006	2.067	-0.039***
	(0.009)	(0.014)	(10.879)	(0.006)
2013 * Responder	0.030***	0.026*	24.625**	0.029***
	(0.009)	(0.014)	(11.117)	(0.007)
2011		0.008		
		(0.009)		
2011 * Responder		-0.002		
		(0.009)		
Constant	0.113***	0.116***	1,314.212***	0.896***
	(0.003)	(0.003)	(3.199)	(0.002)
No. of observations	375,280	375,280	366,004	366,004
No. of teachers	78,239	78,239	76,154	76,154
No. of schools	1,739	1,739	1,603	1,603

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Standard errors are clustered at the school level and are shown in parentheses. Responder is equal to one for all TPS teachers and equal to zero of all charter school teachers. Teacher covariates include: gender, race, degree, probationary certification, certification area, and instate undergrad. School covariates include: percent Black, percent other race, percent free or reduced price lunch, percent special education, percent gifted, and school performance zscore. All specifications include school fixed effects.

Table 6: Teacher Attrition in Louisiana 2006-2013-By Various Measures of School Disadvantage

	FRPL	Minority	SPS Level	SPS Growth
Year	0.000	-0.000	0.000	0.000
	(0.001)	(0.000)	(0.001)	(0.000)
Most Disadvantaged	-0.003	-0.004	-0.008**	-0.003
	(0.004)	(0.004)	(0.004)	(0.003)
Least Disadvantaged	0.005	-0.000	0.008**	-0.001
	(0.004)	(0.003)	(0.004)	(0.003)
Year * Most Disadv.	0.003***	0.002**	0.002**	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Year * Least Disadv.	-0.002*	0.000	-0.002***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
2012	0.022***	0.026***	0.023***	0.020***
	(0.003)	(0.003)	(0.003)	(0.002)
2012 * Most Disadv.	-0.005	-0.009	-0.004	0.002
	(0.005)	(0.005)	(0.005)	(0.005)
2012 * Least Disadv.	0.001	-0.010**	-0.002	0.006
	(0.005)	(0.004)	(0.004)	(0.005)
2013	0.041***	0.040***	0.037***	0.037***
	(0.003)	(0.003)	(0.004)	(0.003)
2013 * Most Disadv.	-0.013**	-0.005	-0.007	0.010
	(0.006)	(0.006)	(0.006)	(0.006)
2013 * Least Disadv.	-0.007	-0.004	0.004	0.003
	(0.005)	(0.005)	(0.005)	(0.005)
N	396,676	396,676	396,676	372,674
F-Tests				
2012*Most=2012*Least	0.321	0.824	0.752	0.393
2013*Most=2013*Least	0.339	0.967	0.071	0.337

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Most disadvantaged is identified by the lowest quartile in SPS score and SPS growth, the highest quartile in FRPL and minority, and "F" schools for SPS grade. Least disadvantaged is identified by the highest quartile in SPS score and SPS growth and the lowest quartile in FRPL and minority. The reference group for disadvantage is comprised of schools that fall into the second and third quartiles.

Table 7: Teacher Attrition and the Effect of Tenure Reform 2007-2013 (CITS) - Teachers in F-Rated Schools vs. Teachers in A-Rated Schools

	Base Model	Falsification Test 1 Policy Year	Falsification Test 2 Enrollment	Falsification Test 3 Percent White
Year	-0.002	0.000	22.331	0.012
	(0.002)	(0.005)	(16.701)	(0.017)
Responder	-0.011	-0.002	288.747**	0.426***
	(0.013)	(0.018)	(121.285)	(0.082)
Year * Responder	0.002	-0.002	-23.677	-0.010
	(0.003)	(0.005)	(16.690)	(0.017)
2012	0.007	-0.002	19.175	-0.017
	(0.008)	(0.016)	(28.566)	(0.034)
2012 * Responder	0.019**	0.033**	8.386	0.014
	(0.009)	(0.017)	(29.375)	(0.034)
2013	0.016	0.006	6.336	0.031
	(0.011)	(0.022)	(52.523)	(0.058)
2013 * Responder	0.024**	0.045**	26.292	-0.041
	(0.011)	(0.022)	(52.987)	(0.058)
2011		-0.012		
		(0.015)		
2011 * Responder		0.024		
		(0.015)		
Constant	0.149***	0.145***	982.008***	0.520***
	(0.014)	(0.019)	(136.805)	(0.081)
No. of observations	135,132	135,132	130,646	130,646
No. of teachers	26,023	26,023	25,848	25,848
No. of schools	723	723	697	697

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Standard errors are clustered at the school level and are shown in parentheses. Responder is equal to one for all TPS teachers in F-Rated schools and equal to zero for all TPS teachers in A-rated schools. Teacher covariates include: gender, race, degree, probationary certification, certification area, and instate undergrad. School covariates include: percent Black, percent other race, percent free or reduced price lunch, percent special education, percent gifted, and school performance zscore

Online Appendix

Table A1: Teacher Attrition and the Effect of Tenure Reform 2006-2013 (CITS) - STEM and SPED Certification

	SPED Certification	STEM Certification	SPED & STEM Certification	SPED or STEM Certification
Year	0.000	0.001**	0.001	0.001*
	(0.000)	(0.000)	(0.000)	(0.000)
Responder	-0.017***	0.002	-0.010***	-0.026***
	(0.002)	(0.003)	(0.002)	(0.006)
Year * Responder	0.001*	-0.001	0.000	0.002
	(0.001)	(0.001)	(0.001)	(0.002)
2012	0.020***	0.021***	0.021***	0.020***
	(0.002)	(0.002)	(0.002)	(0.002)
2012 * Responder	0.001	-0.007	-0.003	-0.005
	(0.004)	(0.005)	(0.003)	(0.009)
2013	0.034***	0.034***	0.034***	0.035***
	(0.003)	(0.002)	(0.003)	(0.002)
2013 * Responder	-0.000	0.003	0.002	-0.007
	(0.004)	(0.005)	(0.004)	(0.011)
Constant	0.123***	0.121***	0.122***	0.118***
	(0.008)	(0.008)	(0.008)	(0.008)
N	410,539	410,539	410,539	410,539

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Sample includes all TPS teachers. Responder is equal to one for STEM and/or SPED certified teachers. All models include school fixed effects. Standard errors are clustered at the school level and are shown in parentheses. Teacher covariates include: gender, race, degree, probationary certification, and instate undergrad. School covariates include: percent Black, percent other race, percent free or reduced price lunch, percent special education, percent gifted, and school performance zscore.

Table A2: Teacher Attrition and the Effect of Tenure Reform 2006-2013 (CITS) – Granger Tests to Confirm Early Career Teacher Effects

	Base Model	Ashenfelter Dip Test	Granger Test 1	Granger Test 2
Year	-0.004***	-0.006***		
	(0.002)	(0.002)		
Responder	-0.026***	-0.028***	-0.025***	-0.020**
	(0.005)	(0.008)	(0.007)	(0.008)
Year * Responder	-0.002	-0.002		
	(0.002)	(0.002)		
2006			0.031***	
			(0.009)	
2006 * Responder			0.005	
			(0.011)	
2007			-0.015*	-0.046***
			(0.008)	(0.008)
2007 * Responder			0.025**	0.020*
			(0.010)	(0.011)
2008			0.004	-0.028***
			(0.009)	(0.009)
2008 * Responder			-0.004	-0.009
			(0.010)	(0.012)
2009			0.010	-0.021**
			(0.009)	(0.009)
2009 * Responder			-0.013	-0.019
			(0.010)	(0.011)
2010			-0.007	-0.039***
			(0.008)	(0.009)
2010 * Responder			0.012	0.007
			(0.010)	(0.011)
2011		0.011		-0.031***
		(0.008)		(0.009)
2011 * Responder		0.002		-0.005
		(0.011)		(0.011)
2012	0.012	0.020*	0.003	-0.028***
	(0.009)	(0.011)	(0.009)	(0.010)
2012 * Responder	0.026**	0.028**	0.025**	0.021**
	(0.012)	(0.014)	(0.011)	(0.011)
2013	0.037***	0.046***	0.024***	-0.008
	(0.010)	(0.012)	(0.009)	(0.010)
2013 * Responder	0.020	0.022	0.015	0.010
	(0.013)	(0.016)	(0.012)	(0.013)
Constant	0.159***	0.155***	0.168***	0.199***
	(0.029)	(0.029)	(0.029)	(0.028)
N	36,839	36,839	36,839	36,839
No. of teachers	8,165	8,165	8,165	8,165
No. of schools	1,517	1,517	1,517	1,517
F-Test				
Pre-Policy Year*Responder All Jointly Significant	--	--	0.528	0.875

Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Sample includes TPS teachers with four years of experience compared to TPS school teachers with three years of experience from 2006-2013. Responder is equal to one for 4th year teachers (who lost tenure protections) and equal to zero for 3rd year teachers (who never had tenure protections). All models include school fixed effects. Standard errors are clustered at the school level and are shown in parentheses. Teacher covariates include: gender, race, degree, probationary certification, certification area, and instate undergrad. School covariates include: percent Black, percent other race, percent free or reduced price lunch, percent special education, percent gifted, and school performance zscore.

Table A3: Entire late career teacher population comparisons

	Base Model[a]	Full Compared to Partial Retirement[b]	Partial Retirement Compared to 10-19[c]	Full Compared to No and Partial Retirement[d]	Partial and Full Retirement Compared to 10-19[e]
Year	-0.001*	0.004***	0.000	0.001	-0.001*
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Responder	0.038***	0.036***	0.010**	0.042***	0.029***
	(0.003)	(0.005)	(0.004)	(0.003)	(0.003)
Year * Responder	-0.001	-0.005***	0.002**	-0.003***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2012	0.013***	0.015***	0.012***	0.014***	0.013***
	(0.002)	(0.005)	(0.002)	(0.002)	(0.002)
2012 * Responder	0.026***	0.024***	0.006	0.023***	0.016***
	(0.006)	(0.008)	(0.006)	(0.006)	(0.004)
2013	0.028***	0.034***	0.026***	0.030***	0.028***
	(0.003)	(0.006)	(0.003)	(0.003)	(0.003)
2013 * Responder	0.015**	0.009	0.015**	0.011*	0.014***
	(0.006)	(0.009)	(0.007)	(0.006)	(0.005)
Constant	0.059***	0.053***	0.049***	0.053***	0.054***
	(0.009)	(0.013)	(0.008)	(0.008)	(0.008)
No. of observations	131,280	73,271	118,487	161,519	161,519
No. of teachers	20,622	12,029	17,627	26,570	25,219
No. of schools	1,626	1,594	1,611	1,634	1,634

a – Sample includes TPS teachers with full retirement eligibility compared to TPS teachers with no retirement eligibility and 10-19 year of experience from 2006-2013. Responder is equal to one for fully retirement eligible teachers and equal to zero for ineligible teachers.
b – Sample includes TPS teachers with full retirement eligibility compared to TPS teachers with partial retirement eligibility from 2006-2013. Responder is equal to one for fully retirement eligible teachers and equal to zero for partially eligible teachers.
c – Sample includes TPS teachers with partial retirement eligibility compared to ineligible TPS teachers with 10-19 years of experience from 2006-2013. Responder is equal to one for partially retirement eligible teachers and equal to zero for ineligible teachers.
d – Sample includes TPS teachers with full retirement eligibility compared to TPS teachers with partial retirement eligibility and no retirement eligibility with 10-19 years of experience from 2006-2013. Responder is equal to one for fully retirement eligible teachers and equal to zero for all others.
e – Sample includes TPS teachers with full or partial retirement eligibility compared to TPS teachers with no retirement eligibility and 10-19 years of experience. Responder is equal to one for full and partial retirement eligible teachers and zero for ineligible teachers.
Note: * $p<0.1$; ** $p<0.05$; *** $p<0.01$. Standard errors are clustered at the school level and are shown in parentheses. Teacher covariates include: gender, race, degree, probationary certification, certification area, and instate undergrad. School covariates include: percent Black, percent other race, percent free or reduced price lunch, percent special education, percent gifted, and school performance zscore. All specifications also include school fixed effects.