A War on Teachers?
Labor Market Responses to Statewide Reform

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Abstract

We examine the effect of Michigan's 2011 reforms to teacher evaluation and tenure policies on teacher retention. Our data are drawn from administrative records containing the population of public school employees from 2005-06 through 2015-16. We utilize both interrupted time series (ITS) and difference-in-differences (DD) models to examine how reform-induced changes in teacher evaluation and tenure policies affect the probability that a teacher permanently exits Michigan schools. Our ITS analysis shows a substantial increase in teacher exits from Michigan schools in the post-reform period. However, our DD analysis reveals no differential rate of attrition for teachers relative to non-instructional professional staff. Important exceptions to this pattern include strong evidence that teachers in high poverty schools and suggestive evidence that teachers in schools with lower performing students were more likely to exit post reform. Thus, our results suggest that although more teachers exited Michigan's schools post reform, teacher-specific reforms alone may have had little impact on overall teacher attrition, and policymakers must consider differential impacts on teachers in traditionally hard to staff schools.

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I. Introduction

In the last five years policymakers in nearly every state have enacted reforms to the public school teaching profession and the teacher labor market. Many of these reforms include changes to long-standing policies that teacher advocates, most notably powerful teachers' unions, have long defended (Marianno 2016). Although the types of reforms introduced and enacted differ across states, new policies include limits on the scope of collective bargaining, changes to the tenure process, and teacher accountability systems based on high-stakes evaluation linked to student outcomes. Proponents argue that these reforms will make it easier to identify and remove ineffective teachers and reduce administrative constraints over human capital and resource allocation decisions, which in turn will lead to increased student performance. Opponents counter that these reforms make teaching a less attractive profession, leading to an increase in attrition from the field as well as a decrease in the supply and/or the quality of individuals who elect to enter the profession in the future. To date, however, there has been little systematic research to inform this debate.

In this paper we provide some of the first systematic evidence on how such reforms have affected teacher attrition. Our analysis focuses on the implementation of a set of policies in Michigan that substantially diminished the rights of teachers and their unions. In July 2011, Michigan established a system of teacher evaluation that provided districts with new ability to identify and remove ineffective teachers (Spalding 2014). These laws reduced teachers' tenure protections, increased the length of the probationary period from 4 to 5 years, increased reliance on multiple measure teacher evaluation systems that include student achievement, and prohibit local collective bargaining of topics including teacher transfer and reassignment, evaluation, performance-based compensation, classroom observations, the length of the school year and discipline (State of Michigan 2011). These comprehensive changes to the rules governing teachers and teaching in the state were followed up in 2012 with the implementation of a Right-to-Work law that prohibited unions from collecting agency fees (membership dues paid to support union professional activity) as a provision of the collective bargaining agreements negotiated in their employing districts. As a result of these reforms Michigan now has one of the most restrictive scopes of bargaining of any state in the country (Zeehandelaar 2012).

To examine whether and how these reforms affected teacher attrition, we use detailed administrative data on the universe of Michigan school employees from 2005-06 through 2013-14. For both teachers and non-instructional staff, these records include information on demographics, years of experience, certification, school and district location, and other measures common to rich administrative unit-level data.

We begin our analysis by using our administrative data to estimate linear probability models in which the dependent variable is an indicator for whether a teacher permanently exits the Michigan public school system. In these interrupted time series (ITS) models we find statistically significant increases in attrition rates of approximately 1 to 2 percentage points in each year following the reforms. These increased attrition rates hold both for the full sample of teachers and for sub-samples of teachers based on years of teaching experience. While our ITS results provide interesting and informative descriptive evidence that teacher attrition rates increased in the post-reform period, they do not shed light on whether the reforms themselves caused the observed increase in attrition rates or some other factor. Indeed, we show that during our sample timeframe real teacher salaries began to decline in 2008 and continued to decline through 2012, raising the possibility that our ITS results are being driven by declines in teacher compensation.

In our main analysis, we employ two different difference-in-differences (DD) strategies to identify the causal effects of Michigan's 2011 reforms. In the first, we exploit the plausibly exogenous timing of reform implementation in individual districts based on the fact that the teacher evaluation and changes to collective bargaining reforms did not take effect until each district's pre-reform collective bargaining agreement (CBA) had expired. Although the majority of districts were subject to reform in 2011, 40 percent of districts had contracts that did not expire until later years. The second DD strategy is based on specifications in which we compare the exit rates of teachers to the exit rates of non-instructional professional staff who work in the same school districts as our sample of teachers. Our sample of non-instructional professional staff is comprised of school staff whose positions require similar educational attainment and certification requirements as teachers (e.g., school guidance counselors, school nurses, and social workers) but who should not have been directly affected by the full slate of 2011 reforms. We show that these professional staff display similar pre-reform trends in both exit rates and salaries to teachers and thus satisfy the parallel trends assumption that underlies causal claims for any post-reform estimates based on difference-in-difference analyses.

In our DD specifications that exploit the plausibly exogenous timing of when teachers were actually exposed to the reforms, we find no evidence of any reform-induced changes in teacher exit rates. Similarly, in our specifications based on teacher-staff DD comparisons we find no statistically significant differences in the exit rates of teachers, relative to non-instructional professional staff, in the post reform period. These results hold for teachers overall and separately for experienced teachers, mid-career teachers and new teachers. Because we find no evidence of any reform-induced changes in teacher attrition rates in either of our DD

identification strategies, our general conclusion is that the teacher-specific reforms associated with tenure and evaluation on average did not induce disproportionate teacher exits from the labor market. However, these overall results may mask important heterogeneity in exit rates if some teachers were more likely to be influenced by the reforms than others. To explore that possibility, we estimate difference-in-differences (DD) models that examine whether: 1) teachers assigned to school districts that are arguably more challenging to teach in and harder to staff (high poverty districts and districts with lower performing students), 2) teachers with potentially better outside-of-teaching career (secondary STEM teachers) and 3) teachers that graduated from more selective colleges and universities, exhibited higher post-reform attrition rates than their professional staff counterparts.

We find no evidence that STEM teachers or teachers that graduated from more selective colleges or universities were more likely to exit post reform relative to professional staff. In contrast, we find strong evidence that teachers in high poverty districts were more likely to exit post reform relative to professional staff working in the same high poverty districts as teachers. We also find suggestive but noisier evidence that teachers in lower performing districts were more likely exit post reform relative to professional staff. Finally, event study estimates confirm that teachers in high poverty or lower performing districts only began exhibiting higher attrition rates relative to professional staff in the years following the implementation of Michigan's reforms.

In what follows, we provide detailed background on the education policy environment in Michigan during our study's timeframe, including a description of the particular law changes that we consider. We proceed with a description of our data and associated descriptive statistics, and then provide an overview of our empirical strategy. We then present the results of our analysis, followed by a discussion of both the implications and limitations of our approach.

II. Background: Teacher Labor Market Reforms in Michigan

After an initial attempt to introduce a performance-based teacher evaluation system in 2010 (State of Michigan 2010), the state of Michigan implemented several laws beginning in the summer of 2011 that substantially reduced teachers' protections and the rights of teachers' unions in collective bargaining. In July 2011 the Michigan legislature implemented Public Acts 100, 101, 102 and 103 (State of Michigan 2011), followed by Public Act 349 in December 2012. The first set of reforms (Public Acts 100-102) diminished teachers' employment protections through the implementation of a high stakes performance-based teacher evaluation system and tying promotion and layoff decisions to evaluation outcomes. Specifically, Public Act 102 required the

immediate establishment of a high-stakes teacher evaluation system, mandating that local districts put into place evaluation systems in the 2011-12 academic year. The key feature of this legislation was the inclusion of student achievement as a "significant" determinant of educator performance ratings, and the eventual dismissal of teachers with multiple (three) "ineffective" ratings.[1] In addition, PA 102 prohibited districts from using seniority as the dominant determinant of layoff decisions (as is the case under typical Last-in-First-Out (LIFO) seniority-based layoff processes) and required districts to base layoff decisions on performance ratings stemming from the new evaluation system, although districts are still allowed to use seniority to determine layoffs between teachers of similar performance ratings. To further aid districts' ability to use the new evaluation system to remove ineffective teachers, the legislature also concurrently passed Public Acts 100 and 101. These acts increased the pre-tenure probationary period from four to five years and required that evidence of teacher effectiveness be the dominant factor in awarding tenure. Specifically, new teachers were required to be rated effective or higher in three consecutive probationary years before receiving tenure (State of Michigan 2011).

To facilitate the implementation of the evaluation reform in particular, PA 103 also passed at the same time in July 2011, prohibited CBAs bargained after that date from governing evaluation, teacher transfer and reassignment (which is traditionally based on seniority rather than performance or school need), performance-based compensation, classroom observations, the length of the school year and discipline (State of Michigan 2011). These changes "radically altered the landscape of bargaining for public school employers and the unions representing their teachers," according to Michigan administrative law judge Julia Stern.[2] In addition, in December 2012, the state passed Public Act 349, which removed requirements for teachers to be members of their local unions. The law prohibited districts from requiring teachers to pay agency fees (funds designated for union activities related to the organization's professional purposes) as a condition of employment, shifting the state from agency shop to Right-to-Work status (State of Michigan 2012a). Importantly for our purposes, although PA 100 and 101 applied to newly hired teachers immediately, PA 102 and 103 did not take effect until the collective bargaining agreements (CBAs) operating for each district expired. We consider this feature of the reform implementation in greater detail in our analysis below.

How might such reforms affect the teacher labor market? The logic behind the tenure and evaluation reforms is that a shift to employment-at-will or performance-based job security will

[1] In November 2015, the state passed subsequent legislation to allow individual districts wide discretion in the implementation of this policy over time, with student achievement remaining an important feature.
[2] See Michigan Association of School Boards Bargaining Toolkit https://www.masb.org/Portals/0/Member_Center/Labor_Relations/Bargaining_Toolkit.pdf accessed 5/9/17

enable administrators to have more information about teacher performance while providing them with increased discretion over teacher retention and promotion. This should ultimately allow administrators to remove ineffective teachers (and provide a signal to ineffective teachers about a poor fit with the profession). However, if such reforms diminish other perceived benefits of public school teaching, they may induce adverse effects on the teacher labor market. Indeed, as research suggests, potential and existing teachers consider tenure part of their compensation package, especially if it is dependent on time-in-service as opposed to performance on the job (e.g., Feinberg, 1981; Brunner & Imazeki, 2010; Rothstein, 2014; Strunk et al., 2016). Other studies have also indicated that teachers value non-pecuniary benefits when they appraise their compensation packages (Loeb and Page 2000), including job stability and mitigated risks to future employment (e.g. Murnane and Olsen 1990; Rothstein 2014). This suggests that weakening tenure protections and/or linking job security to classroom performance may dis-incentivize effective teachers from entering or remaining in the profession, especially without offsetting financial incentives (Rothstein 2014). More generally, it is possible—and teacher advocates have strongly asserted—that such reforms constitute a "war on teachers,"[3] creating employment conditions of lower morale and diminished satisfaction with the profession. As a recent article titled "The Disappearing Educator" in the monthly magazine published by Michigan's largest teachers' union summarized, a "toxic brew of conditions" including "ever-rising job demands, teacher scapegoating, loss of autonomy, budget cuts, and over-testing" have recently driven teachers from the profession early, potential new teachers from teaching in the first place (Ortega 2017).

Our objective in this paper, then, is to consider whether the package of reforms implemented by the Michigan state legislature in 2011 and 2012 affected the teacher labor market by causing increased attrition as teachers respond to reductions in their job protections and in the ability of their unions to continue extracting job protections and other working conditions through negotiations and lobbying activities. However, such exit might be tempered by teachers who prefer to remain in the profession under circumstances that are more dictated by merit and teacher autonomy. Unfortunately, the existing literature gives us few clues as to whether or not teacher exits have increased in recent years, and particularly whether such exits, if prevalent, are the result of teacher-related reforms. A recent study suggests that the removal of tenure protections in Louisiana does induce teacher exit (see Strunk, et al. (2017), and evidence from Houston has

[3] e.g. https://www.washingtonpost.com/blogs/answer-sheet/post/the-war-on-teachers-why-the-public-is-watching-it-happen/2012/03/11/gIQAD3XH6R_blog.html?utm_term=.dbc6233796ea; http://www.newsweek.com/dismal-toll-war-teachers-379951

indicated that low-performing teachers are particularly likely to leave schools after the implementation of rigorous teacher evaluation (Cullen, Koedel and Parsons 2016). However, there are no papers of which we are aware that estimate plausibly causal effects not only of reductions in teacher employment protections but also the simultaneous onset of teacher evaluation policies and changes to collective bargaining rights.[4] Moreover, such effects can be difficult to disentangle given the timing of these reforms. In particular, the Great Recession preceded the bulk of state policy changes nationally, and changing labor market opportunities in other fields may also contribute to teacher attrition. In what follows, we aim to isolate the impact of the Michigan teacher policy reforms on teacher attrition by leveraging a variety of identification strategies that explicitly account for other confounding factors, such as changing labor market opportunities, that occurred in close temporal proximity to the reforms introduced in Michigan and nationwide.

III. Data

Our primary source of data is administrative records for the population of Michigan's public school employees provided by the Michigan Department of Education (MDE) and the Center for Educational Performance and Information (CEPI) for the school years 2005-2006 through 2015-2016. These data include demographic and credential information for employees as well as descriptions of their assignments. Comprehensively, the data capture approximately 2.8 million employee-years. The 952,000 observations for instructionally-focused teachers, representing 140,000 unique teachers, provide the core analytic sample.

We use occupation and assignment codes for non-instructional staff to develop a comparison group for one of the difference-in-differences analyses described below. Specifically, our comparison group consists of the following non-instructional professional staff: 1) school guidance counselors; 2) social workers; 3) accountants/bookkeepers; 4) nurses and other health services workers; 5) occupational therapists; 6) physical therapists and 7) audiologists.[5]

Our primary outcome is a teacher exit from the Michigan public school system. No description of the reason for exiting the Michigan school system is available in the state record. Instead, we infer teacher exit from the presence and then absence of a teacher's unique identifier

[4] The Strunk, et al. (2017) paper, to which the present study is most analogous, does not have an explicit comparison group and rather compares heterogeneous results across treated teachers, finding those most susceptible to reform indeed were more likely to exit.

[5] Our comparison group is similar to the one suggested by Harris and Adams (2007) who use the Current Population Survey (CPS) to compare turnover rates among teachers to those of nurses, social workers, and accountants; a group of professional they argue is similar along multiple dimensions to teachers. Their results suggest that teachers and individuals in their comparison group exhibit similar turnover rates.

in the time series. Specifically, we define an exit as permanent disappearance after year *t* from the population of Michigan public school teachers. We apply the same definition of exit to school non-instructional professional staff.[6] Our exit measure has a number of limitations. First, teachers or staff moving from the public sector to private schools or to another state are not observable to us and will be interpreted as exits. Second, permanent exit or retirement is especially hard to infer because some teachers and staff may return beyond the length of our panel.[7] Using the full extent of our time series, we determined that the vast majority of short-term leaves are for 1 year. As such, we adjusted our data to reflect that an individual must be absent for at least 2 years from the data before being identified as an exit. The implication of this restriction is that it removes the 2014-15 and 2015-16 exits from our analysis.[8]

We also use the administrative records to create a number of control variables that capture the demographic characteristics of teachers and non-instructional professional staff in our sample. Those variables are: 1) age, 2) years of experience in the Michigan public school system, 3) an indicator for whether an employee is female, and 4) indicators for whether an employee is Black, Hispanic or some other race or ethnicity other than non-Hispanic white. Finally, although exits due to retirement typically occur at well-defined points in an individual's age and experience profile (e.g. age 65 and/or 30 years of experience), in 2010, Michigan introduced a one-time retirement incentive for school employees whose age and experience levels added to 80 or who were age 60 and had at least 10 years of experience. To control for this retirement incentive we also include an indicator for whether an individual was eligible for the retirement incentive in 2010. We discuss the early retirement incentive in more detail in the next section.

In some of our analyses we are particularly interested in whether: 1) teachers assigned to school districts that are arguably more challenging to teach in and harder to staff, 2) teachers with potentially better outside-of-teaching career options and 3) teachers that graduated from more selective colleges and universities, were more likely to exit post reform than other teachers.

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[6] The administrative data allow us to follow teachers and staff from assignment to assignment and school to school. Thus, our measure of exit rates is not contaminated by teachers or staff simply switch schools or moving to a new district within the state.

[7] Nevertheless the administrative data strongly suggest that our measure of exits adequately captures genuine departure from the school system—teachers exit disproportionately when we expect them to do so. For example, we observe large spikes in exits at 30 years of experience, the cutoff for full retirement benefits and spikes in exits associated with retirement incentives based on the experience and age combinations that qualify individuals for early retirement.

[8] Teacher experience itself, which plays a role in exiting decisions, requires some calculation. Though each individual district reports a hire date, prior years' experience for teachers switching jobs before 2005 is not observable to us, causing some degree of understatement of their tenure. However, there are very low rates of inter-district transfer within our time-series, and for all teachers who we do observe having transferred between districts, we simply adjust our experience variable to reflect years in all districts in the state, not just the teacher's current district.

Hanushek, Kain & Rivkin (2004) find that teacher exit rates tend to be substantially higher in schools serving large numbers of disadvantaged students.[9] Consequently, we use the fraction of free or reduced price lunch students in a school district in 2005 as our primary measure of a more challenging teaching environment. In addition, we also use the average district-wide student performance on the American College Test (ACT) in 2009 as a secondary measure of school districts with challenging teaching environments.[10] Both of these measures were obtained from the Michigan Department of Education (MDE).

To examine whether teachers with potentially better outside-of-teaching career options were more likely to exit post reform, we used data from the MDE administrative records to create an indictor variable for whether a teacher was a certified secondary STEM teacher. Our rationale for focusing on certified secondary STEM teachers is that these teachers must hold an undergraduate degree in math or science, majors that typically provide better outside-of-teaching options than other undergraduate majors that teachers typically pursue.[11]

Finally, we are also interested in whether exit rates among teachers differ depending on observable measures of teacher quality. Unfortunately, we do not have any individual-specific pre-reform measures of teacher quality such as a measure of teacher value added. However, in the MDE administrative records we do observe the college or university that a teacher graduated from. We therefore proxy for teacher quality using the observable characteristics of the college or university from which a teacher graduated. Specifically, we use the 75th percentile ACT score of incoming undergraduate students as our primary measure of college selectivity. Data on college selectivity was obtained from the Integrated Postsecondary Education Data System (IPEDS) maintained by the National Center for Education Statistics (NCES).[12]

We restrict our sample in a number of ways. First, we exclude from the sample all administrators, individuals younger than 21 years of age or older than 70, teachers and

[9] Also see Boyd, Lankford, Loeb, & Wyckoff, (2005), Scafidi, Sjoquist, & Stinebrickner (2007), Falch & Strøm (2005) and Ingersoll (2001), among others, for further evidence on factors that affect teacher attrition.

[10] Michigan administers the ACT exam statewide in all districts as part of its student evaluation system. We use data on average district-wide performance on the ACT in 2009, rather than a more recent year to ensure that test results are measured prior to Michigan's 2011 reforms. Starting in 2017, the ACT exam will be replaced by the SAT.

[11] For example, Dolton and van der Klaauw (1999) find that teachers with an education degree rather than a specific college major like mathematics are less likely to exit teaching for a non-teaching job. Similarly, Stinebrickner (1998) finds that science teachers are substantially more likely to exit teaching than other teachers, a finding he attributes to the fact that teachers with bachelor degrees in math and science have better non-teaching wage opportunities.

[12] We also explored several other measures of college selectivity such as the median undergraduate GPA of students selected into a Michigan college or university teacher preparation program and the MDE's Educator Preparation Institution (EPI) Performance Score. Results obtained using these alternative measures were qualitatively and quantitatively similar to those based on the 75th percentile ACT score.

professional staff with more than 25 years of experience and any staff without a college degree.[13] Second, we restrict the sample to teachers and staff working in traditional K-12 public schools and thus drop teachers and staff who work in charter schools. Third, many small school districts employ very few of the non-instructional professional staff that belong to our comparison group. For example, many small school districts in Michigan do not have school nurses or occupational and physical therapists. As a result, we limit our sample to school districts with 500 students or more. We note, however, that even after imposing this restriction, we retain 97.7% of our sample of teachers and 98.6% of our sample of staff. The final restriction is related to the collapse and recovery of the domestic auto industry based in Detroit and the budget crisis that plagued the Detroit school system during our sample time frame. Specifically, in 2009 the Detroit school system faced approximately a $400 million deficit prompting Michigan's governor to replace the local school board with an appointed official and place the Detroit school system in emergency management status. The financial condition of the Detroit school system relative to other school districts in Michigan, and the fact that Detroit is the largest school district in the state, raises the obvious concern that its inclusion in our sample may unduly influence our results. We address this issue by dropping Detroit in our main analysis and then presenting results based on specifications that include Detroit in specification checks.

Table 1 provides summary statistics for the variables used in our analysis. We present separate summary statistics for teachers based on years of work experience in the Michigan public school system. On average approximately 4 percent of teachers exit Michigan public schools after any given year (recall that these are teachers with less than 25 years of experience). Furthermore, teachers tend to be predominantly non-Hispanic White and female. For comparison purposes, Table 2 presents summary statistics for the sample of teachers and for the sample of non-instructional professional staff that comprise our control group. We once again present separate summary statistics for teachers and staff based on years of work experience in the Michigan public school system.[14] On average, the annual exit rate from Michigan public schools is slightly higher for professional staff than for teachers (5 versus 4 percent). Both teachers and staff are disproportionately female, while teachers are slightly younger on average than staff (41

[13] We exclude individuals age 70 or older and those with 25 or more years of experience to reduce variation in exit rates due primarily to normal retirement decisions. We exclude staff without a college degree and individuals younger than 21 to ensure our comparison group is similar to our sample of teachers.

[14] Because the sample of staff is significantly smaller than the sample of teachers, we expand the staff experience groupings in specifications where we focus on more and less experienced teachers to increase the staff sample size in each of the comparison groups. Specifically, for teachers with less than 10 years of experience we use as a comparison group staff with 1-14 (instead of 1-9) years of experience and for teachers with 10 or more years of experience we use staff with 5 or more years of experience.

versus 45 years old) and fewer teachers have a master's degree (which is expected given the professional status of our comparison staff). Finally, as expected, the share of teachers and staff eligible for the retirement incentive program in 2010 drops off substantially as one moves to less experienced categories.

IV. Empirical Framework

We begin our analysis of the impact of Michigan's 2011 teacher evaluation and tenure reforms on teacher attrition rates by estimating a series of interrupted time series (ITS) models that take the following form:

$$Exit_{ijt}^{g} = \beta_0 + \beta_1 Trend_t + \beta_2 2011_t + \beta_3 2012_t + \beta_4 2013_t + X_{ijt}\theta^1 + \delta_j + \varepsilon_{ijt}, \quad (1)$$

where, $Exit_{ijt}^{g}$ equals one if teacher *i*, a member of experience group *g*, in school district *j* exits teaching in year *t*, $Trend_t$ is a linear time trend centered at zero in the year prior to the enactment of the reforms (2010), 2011_t, 2012_t and 2013_t are indicators for the post reform years of 2011, 2012 and 2013, respectively, X_{ijt} is a vector of teacher characteristics, δ_j is a vector of locational (labor market, operationalized as the intermediate school district, or local school district) fixed effects, and ε_{ijt} is a random disturbance term. The coefficients of primary interest in (1) are β_2, β_3 and β_4, which measure deviations from the pre-reform trend in teacher exit rates in the years subsequent to the adoption of the reforms. All of the analyses below consider a teacher's behavior after time *t* conditional on the information set at *t*. For example, a coefficient on an indicator variable for 2011 is the estimated probability that a teacher leaves after the 2011-12 academic year.

In the empirical work that follows, we estimate (1) using the full sample of teachers and separately for teachers with 10-25 years of experience and 1-9 years of experience. We estimate separate equations based on teacher experience groupings to account for the possibility that the "cost" associated with exiting the teaching profession may differ by experience level due to the design of Michigan's retirement system. Specifically, as noted by Koedel et al. (2013) and Costrell and Podgursky (2009), because defined benefit retirement formulas tend to be heavily back-loaded, they create an incentive for mid-career teachers to remain in the profession to maximize their pension wealth. Similarly, retirement formulas tend to be relatively flat in the initial years. As a result, accrual of pension wealth tends to be low for less experienced teachers,

creating less of an incentive for early-career teachers to remain in the profession relative to mid-career teachers.

The ITS model given by (1) is designed to provide a descriptive examination of teacher exit rates pre- and post-reform, simply addressing the question: *have teachers been exiting the profession at higher rates over time*? While these ITS results are interesting and informative, they are unlikely to have a causal interpretation since it is impossible to separate the effect of the reforms on teacher exit rates from the effect of other factors that may have changed coincidentally with the adoption of the reforms. For example, as noted previously, the timing of Michigan's adoption of the reforms coincides quite closely with the end of the Great Recession. As a result, based simply on the ITS results, it is impossible to know whether any increase in teacher attrition post reform was due to the adoption of the reforms or due to improvements in labor market conditions that led to better alterative employment options for teachers.

To overcome that limitation, our primary analysis employs two distinct identification strategies based on difference-in-differences (DD) approaches. Our first identification strategy exploits the plausible exogenous timing of when a school district's pre-reform CBA expired. Specifically, recall that to facilitate the implementation of the evaluation reform, the state prohibited CBAs bargained after July of 2011 from governing evaluation, teacher transfer and reassignment, performance-based compensation, classroom observations, the length of the school year and discipline. Thus, the exact timing of when teachers were fully subjected to Michigan's 2011 reforms depended on when the pre-reform CBA negotiated by a school district expired. Teachers in districts where the pre-reform contract did not expire until a year after 2011 were not subject to the teacher evaluation reforms or other reforms until the pre-reform contract expired. Approximately 40% of districts had pre-reform CBAs that expired in 2011, making teachers in those districts susceptible to the reforms immediately. An additional 29% of districts had pre-reform CBAs that expired in 2012, 11% that expired in 2013 and 2% that expired in either 2014 or 2015. The remaining districts had CBAs that either expired sometime in 2010, the year prior to reform (10%) or had contracts that expired pre-reform but had no new contracts (approximately 9%). After consulting with the Michigan Department of Education, which advised us that districts with earlier expirations may have been operating under anticipated law changes, we add both groups to the 40% of districts with CBAs expiring immediately in 2011, such that 59% of our districts in total are coded as treated in 2011.

We exploit the plausibly exogenous timing of pre-reform CBA contract expiration dates to estimate DD models of the following form:

$$Exit_{ijt}^{g} = \kappa_0 + \kappa_1 Exposed_{jt} + X_{ijt}\theta^2 + \lambda_t + \delta_j + \mu_{ijt}, \quad (2)$$

where $Exposed_{jt}$ is an indicator variable that takes the value of unity for all years following the expiration of a pre-reform CBA in district j, λ_t and δ_j are vectors of year and district fixed effects respectively, μ_{ijt} is a random disturbance term and all other terms are as defined in (1). The coefficient of primary interest in (2) is κ_1; the DD estimate of the effect of full exposure to the 2011 reforms on teacher attrition rates.[15] Consistent with (1), we estimate (2) for the full sample of teachers and staff and separately for each experience group discussed previously.

Our identification strategy that exploits the plausibly exogenous timing of pre-reform CBA contract expiration dates implicitly assumes that teachers only respond to the 2011 reforms once they become fully exposed to those reforms (i.e. when their district's pre-reform CBA contract expires). However it is also possible that teachers, knowing that they would be subject to the reforms in the near future, simply reacted to the reforms immediately in 2011. In that case, the DD model given by (2) would incorrectly lead us to the conclusion that the reforms had no impact on teacher attrition since the impact of the reforms would all load on the post-reform year indicators (the year fixed effects for 2011 through 2013) and not on $Exposed_{jt}$. We therefore turn to our second identification strategy that utilizes a comparison group, namely professional staff that work in the same schools and districts as teachers but who were not subject to the 2011 teacher reforms. Specifically, our second identification strategy is based on DD models that take the following form:

$$Exit_{ijt}^{g} = \gamma_0 + \gamma_1 Teacher_i + \gamma_2 Post_t * Teacher_i + X_{ijt}\theta^3 + \lambda_t + \delta_j + \eta_{ijt}, \quad (3)$$

where $Teacher_i$ is an indicator equal to one if individual i is a teacher and zero if the individual is a non-instructional professional, $Post_t * Teacher_i$ is the interaction between the teacher indicator and an indicator for whether the observation is for a year in the post reform period (i.e. 2011 – 2013), η_{ijt} is a random disturbance term and all other terms are as defined in (2). The coefficient of primary interest in (3) is γ_2, which is the DD estimate of the effect of Michigan's teacher evaluation and tenure reforms on the attrition rate of teachers relative to non-instructional professional staff. As before, we estimate (3) for the full sample of teachers and staff and separately for each experience group discussed previously.

[15] Note that κ_1 is in essence an intent-to-treat parameter, indicating when the law should have exposed teachers in individual districts to the policy change.

We also conduct analyses designed to examine whether certain sub-groups of teachers were more likely to exit the teaching profession post reform than other teachers. Specifically, to examine whether teachers in high poverty districts or a district with lower performing students were more likely to exit the profession post reform than their professional staff counterparts, we estimate models identical to those in equation (3), except we restrict the sample of school districts to those above the 50th or above the 75th percentile of free lunch students or below the 50th or below the 25th percentile of ACT math scores. Similarly, to examine whether STEM certified teachers or teachers that graduated from a more selective college or university were more likely to exit the profession, we again estimate models identical to those in equation (3) except we limit the sample of teachers to either STEM certified teachers or teachers that graduated from a more selective college or university and compare those teachers to all professional staff in our sample.

Finally, to provide further evidence that the results based on (3) have a causal interpretation, we also estimate event study models where we replace the $Post_t * Teacher_i$ indicator in (3) with a series of lead and lag treatment indicators that span the years both before and after the 2011 reforms. Specifically, we estimate models of the following form:

$$Exit_{ijt}^{g} = \sum_{j=-3}^{2} \alpha_j T_{j,it} + \pi_1 Teacher_i + X_{ijt}\theta^4 + \lambda_t + \delta_j + \nu_{ijt}, \qquad (4)$$

where, $T_{j,ist}$ represents a series of lead and lag treatment indicator variables that span the policy adoption year of 2011, ν_{ijt} is a random disturbance term and all other terms are as defined above. We include treatment-year interactions for each of the post-reform years and for the three years just prior to the adoption of the reforms (2008 - 2010). The omitted category is therefore four or more years prior to the adoption of the reforms. The post-reform interactions allow the effect of the 2011 reforms on attrition rates to evolve slowly over time while the pre-reform interactions provide evidence on whether the attrition rates of teachers were trending higher prior to the introduction of the reforms. If our results have a causal interpretation the pre-reform interactions should all be relatively small in magnitude and statistically insignificant.

There are several potential confounding events that occurred during our sample time frame that deserve further discussion. The first is that our data straddle the Great Recession. We note, however, that while the Great Recession represents a significant threat to our ITS analysis, since we are simply mapping out the exit rates of teachers pre and post reform, it is less of a threat to our DD specifications. For example, as long as the Great Recession had similar effects on exit rates for teachers and non-instructional professional staff, our DD analyses in (3) should properly control for any effects of the Great Recession on exit rates. Recall that in model (3), our

comparison group consists of non-instructional professionals that also work in the public school system but were not subject to the teacher evaluation or tenure reforms. As noted in the data section, these non-instructional professionals consist of individuals working in occupations such as guidance counselors, school nurses, social workers, etc. –- occupations that require similar educational attainment to teachers and also are directly involved in servicing students. As a result, broad labor market conditions and other coincidental factors that affect attrition, other than Michigan's teacher evaluation and tenure reforms, should presumably have similar effects on the attrition rates of teachers and non-instructional professionals. Indeed, as we show in the next section, we find that trends in exit rates between teachers and non-instructional professional staff were quite similar both before the Great Recession and during the Great Recession.

The second event relates to a series of reforms the state of Michigan implemented to its public school employee pension system. As noted previously, in May 2010 the legislature passed a retirement incentive program adding additional bonuses for school employees whose age and experience levels added to 80 or more or who were 60 years of age or older with at least 10 years of experience and who retired between July 1 and September 1 of 2010 (State of Michigan 2010).[16] This retirement incentive directly incentivized eligible teachers and staff to retire ahead of the series of teacher-related reforms the following year. As noted previously, we address this by dropping individuals who are at or near full retirement eligibility (those with 25 years or more experience) and including an indicator that takes the value of one in 2009 and 2010 for individuals eligible for the incentive in our empirical specifications.

V. Results

A. Interrupted Time Series Analysis

To provide some initial context on the evolution of teacher attrition rates both before and after Michigan's adoption of teacher evaluation and tenure reforms, Figures 1 plots annual average attrition rates from 2005-06 through 2013-14. We present attrition rates for all teachers (squares) and for less experienced teachers (triangles). For both groups of teachers, Figure 1 reveals relatively stable attrition rates prior to 2011, the main reform year, with substantial

[16] In addition, an attempt by the state legislature to increase the pension contributions of existing employees in May 2010 was delayed and ultimately ruled unconstitutional by the state Court of Appeals in 2012. In response, the legislature passed new pension changes in 2012 that offered a 401k-style hybrid option to all teachers, a change to 401k systems for new employees, and the choice to do so or remain in the defined benefit system at a cost of additional employee contributions for those hired by 2010 (State of Michigan 2012b). The 2012 pension changes were not upheld by the Michigan Supreme Court until April 2015 (Egan 2015), which is outside our sample timeframe.

increases in attrition rates from 2011 on.[17] Furthermore, Figure 1 reveals that attrition rates across the experience distribution were relatively similar in the post-reform period.

Interrupted time series (ITS) results based on the estimation of (1) are presented in Table 3. In the interest of brevity, Table 3 reports only the estimated coefficients on the linear time trend and the post-reform period year indicators but we note that all the specifications reported in Table 3 include the full set of control variables listed in Table 1. Complete regression results for all the control variables for the specifications shown in the top panel of Table 3 are presented in Appendix Table 1A. The top panel of Table 3 reports results based on specifications with intermediate school district (ISD) fixed effects while the bottom panel reports results based on specification where we replace the ISD fixed effects with finer local school district fixed effect.[18] The standard errors reported in Table 3 are clustered at the ISD-level to allow for within-labor market autocorrelation of the disturbance term.

The results reported in Table 3 confirm the general pattern depicted for teachers in Figure 1. In each post-reform period, teachers were more likely to exit, with the change in exit probability ranging approximately from 1 to 2 percentage points, especially after the first post-policy year of 201. The fact that attrition rates were lower in the first reform year of 2011 is not too surprising given that few teachers may have had time to react to the policy, which passed less than two months before the school year began. While in most of the analysis that follows we focus on teachers in three broad experience groups, all teachers, teachers with 10 or more years of experience and teachers with less than 10 years of experience, in Table 3 we also split the last experience group into two sub-groups: teachers with 6 to 9 years of experience and those with five or fewer years of experience.

We present separate results for these two groups of teachers for several reasons. First, while the teacher evaluation reform affected all teachers, only new, pre-tenure teachers were affected by the increase in the length of the probationary period and the requirement that new teachers receive three consecutive performance ratings of "effective" to earn tenure protections. Thus, it is instructive to examine whether the most junior, pre-tenure teachers were more sensitive to the reforms. Second, while the reforms implemented in 2011 required districts to base layoff

[17] The dip in attrition rates for the sample of all teachers that occurs in 2009 and 2010 is due primarily to our decision to drop teachers eligible for early retirement in those years when creating the figure. If we left those teachers in the figure, one would observe a substantial spike in exit rates in those years. We have chosen to present the figure without early retirement eligible teachers to avoid having a large spike in exit rates that would make the figure harder to interpret. Of course, in the empirical work that follows we control for teachers eligible for early retirement by including and indicator that takes the value of one if a teacher was eligible for early retirement in 2009 or 2010.

[18] ISD's typically follow county lines and are generally regarded as good proxies for local labor markets in Michigan. Each ISD is comprised of a varying number of local school districts.

decisions on performance ratings stemming from the new evaluation system rather than seniority, districts were still allowed to use seniority to determine layoffs between teachers of similar performance ratings. Given that over 98% of teachers received a rating of "effective" of higher in the post-reform period, most layoffs were still tied to seniority with pre-tenure teachers being the most vulnerable.

The results for teachers with 6 to 9 and 1 to 5 years of experience are presented in columns 4 and 5 of Table 3. In general the attrition rates between the two groups are quite similar. The one exception is the attrition rate in 2011 where we find that teachers with the fewest years of experience were more likely to exit the teaching profession than their slightly more experienced counterparts. However, for 2012 and 2013, the attrition rates for the newest teachers are quite similar to those of teachers with slightly more experience.

B. Difference-in-Differences Analyses

The results reported in Table 3 provide evidence that teacher attrition increased in the post-reform period, but do not allow us to conclude that reforms themselves induced greater exit. As noted previously, the ITS results reported in Table 3 are unlikely to have a causal interpretation since it is impossible to separate the effect of the reforms on teacher attrition rates from the effect of other factors that may have changed coincidentally with the adoption of the reforms (e.g. the end of the Great Recession). In this section were therefore turn to our two DD identifications strategies to isolate the causal effect of Michigan's reforms on teacher attrition.

Table 4 reports results based on the difference-in-differences specification given by equation (2), which exploits the plausibly exogenous timing of pre-reform CBA expirations. The first panel presents results for specifications that include district and year fixed effects but no control variables, while the second panel adds all the control variables listed in Table 2. Panels 3 and 4 present results for specifications similar to those in panels 1 and 2 except we now replace the year fixed effects with ISD-by-year fixed effects. We include results based on specifications with ISD-by-year fixed effects to account for unobserved annual shocks to local labor markets, such as reductions-in-force induced by other post-reform factors across ISDs, or other ISD-specific changes in economic or labor market conditions. Columns 1 – 3 present results based on our three main experience groups. Across all of the specifications reported in Table 4 we find no evidence that reform exposure (based on the expiration of the pre-reform CBAs) led to higher teacher attrition rates. Rather, consistent with our ITS results, we find that all teachers started

exiting at higher rates in 2011, regardless of when they were directly exposed to the full slate of reforms.[19]

There are possible explanations for the null results in Table 4. The first is that teachers were forward-looking and knew that in the near future they would be subjected to the reforms so they simply responded to the 2011 reforms once they were enacted in the state, regardless of exactly when the reforms became binding for their district. The second possibility is that the reforms simply had no effect on teacher attrition on average and our ITS results are just capturing a post-reform trend in teacher attrition that is due to other factors like the end of the Great Recession.

To distinguish between these two explanations we turn now to our second DD approach by bringing in a new comparison group, namely non-instructional professional staff. If the explanation for our Table 4 results is that teachers were forward-looking then we should see positive post-reform exit rates for teachers relative to staff, since staff were not exposed to the full slate of reforms. On the other hand, if the explanation is that our ITS results were simply capturing a post-reform trend in teacher attrition that is due to other labor market factors that would presumably affect both staff and teachers then we should find no post-reform bump in attrition relative to staff. We begin by simply comparing the raw (covariate unadjusted) attrition rates of teachers and professional staff from 2005-06 through 2013-14. Figure 2 displays attrition rates for all teachers and professional staff in our sample while Figure 3 displays the same information for teachers and professional staff with less than 10 years of experience. For each experience level and occupation category, the figures clearly indicate relatively stable attrition rates prior to 2011, the main reform year, with substantial increases in attrition rates from 2011 on. Perhaps more importantly, Figures 2 and 3 provide evidence that the main assumption underlying difference-in-differences models, namely the parallel trend assumption, is met. Specifically, in both Figure 2 and 3 we observe very similar pre-reform trends in attrition for both teachers and non-instructional professional staff.

Table 5 reports results based on the estimation of (3). We include the same baseline, baseline-with-controls, and ISD-by-year fixed effects as in Table 4. However, in Table 5 we also include as a robustness check, results in columns 4 and 5 based on specifications that are identical to those reported in columns 2 and 3 except we make the comparison group all non-instructional professional staff, rather than just staff with the same experience level as teachers, in order to maximize the sample size of our comparison group. The coefficients of interest in these models

[19] Specifically, we find that the post-reform increases in teacher attrition rates that we observe in our ITS results, load on the post-reform year fixed effects in DD specifications shown in Table 4.

are the teacher-post interactions, which provide the DD estimates of the differential change in attrition rates for teachers in the post-reform period.

Across nearly all specifications, the estimated coefficients on the teacher-post interaction are close to zero and statistically insignificant; a result anticipated by Figures 2 and 3. The one exception is the teacher-post interaction in column 4 of panels 1 and 3 (specifications without controls) which is actually negative but relatively small in magnitude. Thus, we find little evidence that teachers were more likely to exit Michigan public schools than their professional staff counterparts in the post-reform period.[20]

C. Compensating Differentials

The results reported in Table 5 suggest that in the post-reform period teachers were no more likely to exit Michigan schools than their non-teaching counterparts. One possible explanation for this result is that in the post reform period, school districts increased teacher salaries in order to compensate them for the less secure and arguably more challenging teaching environment that accompanied the advent of a high-stakes teacher evaluation system and other reforms. To examine that possibility, we collected the last pre-reform salary schedule and the first post-reform salary schedule negotiated by districts and their teacher unions. Thus, for each district we observe the salary schedule in place just prior to the enactment of the reforms and the first salary schedule adopted after the enactment of reforms.[21]

Salary step summary statistics for the districts in our sample are presented in Table 6. The first column of the table list the various steps included in the salary schedule, we present summary statistics for salary steps ranging from a Bachelor's degree with no experience (Base BA) to the salary step for a teacher with a Master's degree and 20 years of experience (Step 20 MA). Columns 1 and 2 present the salary steps measured in nominal dollars both pre- and post-reform while columns 3 and 4 present the same information but deflate the salary steps to real 2016 dollars using the CPI for the Detroit, Ann Arbor, Flint areas. Finally, columns 5 and 6 present the pre/post reform change in nominal and real salaries. Even when measured in nominal dollars, 4 out of 8 of the salary steps actually declined between the pre- and post-reform contracts and the largest increase in nominal terms was only 0.6%. When measured in real 2016 dollars, all

[20] We also estimated specifications in which we allowed for heterogeneous teacher-staff differences for each post-reform year. These also indicated no significant effects.

[21] The salary schedules are contained in each districts collective bargaining agreement (CBA) which we obtained from the Mackinac Center for Public Policy, a nonpartisan research and educational institute. CBA contracts typically remain in effect for 2 to 3 years before being renegotiated. The majority of pre-reform CBAs in our sample ended in 2010 or 2011.

of the changes in the salary steps are negative ranging from a decline 4.5% of to a decline of 6.5%.

In Figures 4 – 7 we plot trends in average annual salaries for both teachers and the professional non-instructional staff that comprise our comparison group. These are drawn from administrative wage data available for both employee groups.[22] Figures 4 and 5 present salary trends for all teachers and staff measured in nominal and real 2016 dollars, respectively. Figures 6 and 7 present the same information for teachers and staff with less than 10 years of experience. In all four figures, nominal and real salaries began to decline for both teachers and staff starting around the beginning of the Great Recession. When measured in nominal dollars, salaries for both teachers and staff began to recover somewhat starting in 2013 but never returned to pre-recession levels. In real dollars, the figures indicate only marginal increases in salaries starting in 2013 and large real declines in salary over the sample time frame. Most importantly, all four figures reveal that the salary trends of teachers and staff were nearly identical over the sample time frame.[23]

D. Heterogeneous Effects

The descriptive salary results presented in Table 6 and Figures 4-7, suggest that school districts did not increase salaries in order to compensate teachers for the less secure and arguably more challenging teaching environment that accompanied the 2011 reforms. Furthermore, we find no evidence of a differential trend in teacher salaries relative to staff; casting doubt on whether changes in teacher compensation can explain why we find little evidence that teachers were more likely to exit Michigan public schools than their professional staff counterparts in the post-reform period.

[22] We prefer the salary schedule data for this analysis because these are based on legal documents dictating step and row compensation for all teachers in each district based on education and experience and because there is variation in the ways districts report wage data for individual teachers and staff in the administrative data. In the administrative data, hourly wage is calculated by districts from employee FTE salary and the number of district-specific contracted hours for that employee. Using estimates of contracted hours worked sourced from district collective bargaining agreements, an FTE annual salary is reconstructed from reported hourly wages. This reconstructed measure is available for 29% of records. Title 1 reporting measures are also available for which districts enter employee "base salary" for a given credential level on the district's salary schedule. Where the first, unadjusted measure was available, it was used to identify employee salaries. When not available, the Title I "base salary," which consists of a mix of adjusted and unadjusted reported salaries was used in its stead. This estimated salary measure is available for over 99% of records.

[23] In Table 2A of the Appendix we confirm that teacher salaries did not increase relative to staff salaries in the post-reform period by estimating DD models similar to those reported in Table 4 except the dependent variable is the log of teacher salaries.

It is possible, however, that while on average teachers were no more likely to exit Michigan schools than their non-teaching counterparts, the average masks heterogeneity in attrition rates across sub-groups of teachers. We explore that possibility in Table 7. Panels 1 and 2 of Table 7 report results based on specifications identical to those in equation (2), except we restrict the sample of school districts to those above the 50th or above the 75th percentile of free lunch students. Similarly, panels 3 and 4 present results based on specifications where we restrict the sample to districts below the 50th or below the 25th percentile of ACT math scores. Finally, in panels 5 and 6 we limit the sample of teachers to either STEM certified teachers (panel 5) or teachers that graduated from a more selective college or university (panel 6) and compare those teachers to all professional staff in our sample.

The results reported in panels 1 and 2 of Table 7 provide consistent evidence that teachers in high poverty schools were more likely to exit in the post-reform period than professional staff assigned to the same high poverty schools. Specifically, in the top panel all of the DD estimates are positive and statistically significant at the 5% level. In terms of magnitude, the point estimates suggest that teachers in districts where the fraction of free lunch eligible students is above the median are approximately 1.1 to 1.7 percentage points more likely to exit the Michigan public school system in the post reform period than their professional staff counterparts. In panel 2 (above 75th percentile free lunch districts) all of the DD estimates are once again positive, but the estimates tend to be noisier, which is not too surprising given the small sample sizes.

In panels 3 and 4, where compare teachers in districts with below median or below 25th percentile ACT math scores to professional staff in those same districts, we once again find that all of the estimated coefficients on the post-reform interaction are positive. However, the point estimates reported in panels 3 and 4 tend to be smaller in magnitude than those reported in panels 1 and 2 and most are statistically insignificant. The fact that our test score results tend to be nosier than our poverty results is perhaps not too surprising given, as shown in Table 1, that there is significantly less variation in average district-wide ACT math scores than in poverty rates.[24] Finally, as shown in panels 5 and 6, in contrast to our poverty results, we find no evidence that STEM teachers or teachers that attended a more selective college or university were more likely to exit post-reform than professional staff.

[24] For example, a significant fraction of districts have average ACT math scores that are clustered around the 50th percentile score of 18.8. Furthermore, the 75th percentile score is only one point lower than the 50th percentile score (17.8 versus 18.8).

As noted previously, in our main analysis we have omitted the Detroit school district from our sample due to the financial crisis that ensued in that district around the time of the policy reforms. However, Detroit is the largest school district in Michigan and also contains the highest concentration of free and reduced price lunch students. Thus, it is instructive to examine how our results change if we include Detroit. In Table 3A of the Appendix we present results based on models identical to those reported in Table 7 except we add Detroit to the sample. All of our core results regarding high poverty and low performing schools are robust to the inclusion of Detroit. The biggest difference between the two sets of results is that the estimated coefficients on the treatment indicators in the high poverty and low-performing district specifications are larger in magnitude in Table 3A and all but one of the estimated coefficients in the specification for districts below the 25th percentile of ACT math scores is statistically significant. Again, these results are not too surprising given both the high concentration of low-income students in Detroit and the financial problems the district faced.

As a robustness check for the results reported in Table 7, in Table 8 we report event study results based on (4). Columns 1-3 and 4-6 of the top panel report results based on specifications where we compare teachers in districts where the fraction of free or reduced price lunch students is above the 50th or 75th percentile respectively, to professional staff in those same districts.[25] The bottom panel repeats the analyses in the top panel but uses districts with average district-wide ACT math scores below the 50th or below the 25th percentiles. In columns 1-3 of the top panel of Table 8 (above 50th percentile poverty), all of the estimated coefficients on the post-reform interactions are positive and starting in 2012, all the estimates are statistically significant. In columns 4-6 of the top panel (above 75th percentile poverty) we obtain similar results, although the estimates are noisier. Importantly, across the entire top panel the estimated coefficients on the pre-reform interactions are all statistically insignificant and tend to be relatively small in magnitude. The fact that we find no evidence that exit rates were trending higher prior to the adoption of the policy provides further evidence that our Table 7 results have a causal interpretation.

The results reported in the bottom panel of Table 8 (districts below 50th and 75th percentile of ACT math scores) tend to be noisier than those in the top panel but display a similar pattern. Starting in 2012, all of the estimated coefficients on the teacher-year interactions are positive but only two of the estimates are statistically significant. In terms of magnitude, the

[25] In the interest of brevity we report only the estimates for the main comparison groups and omit the estimates for the alternative comparison groups for teachers with 10-25 and 1-9 years of experience. Results for these alternative comparison groups are similar to those for the respective comparison groups reported in Table 8.

results reported in columns 4-6 of the bottom panel suggest that in 2012 and 2013, teachers were between 0.7 and 1.8 percentage points more likely to exit lower performing school districts in Michigan than their professional staff counterparts. Furthermore, similar to the results in the top panel, there is no evidence of a consistent trend in the relative exit rates of teachers in the pre-reform period. There is some evidence that teachers were more likely to exit in 2008 relative to professional staff but in 2009 and 2010, the point estimates decline sharply in magnitude and none are statistically significant.[26]

VI. Conclusion

States across the country have made substantial changes to basic conditions of teacher employment in public schools. Rights to collectively bargain, tenure protections, seniority-based staffing routines, and walls between student outcomes and assessments of teacher performance have all weakened in recent years, with teachers in some states essentially serving as long-term at will employees in their local school districts. The warnings of teacher advocates that such fundamental changes to the profession would result in a mass exodus from public schools have given way to conjectured assessments that these results have already come to pass. There remains, however, little systematic evidence to support this assertion.

In this paper, we examine nearly ten years of data on the population of teachers in Michigan, where reforms introduced midway through our timeframe added new hurdles to achieving tenure, a new system of teacher evaluation, and changes to teachers' collective bargaining rights and union dues collection. Our main analyses focus on two different difference-in-differences approaches to identifying reform effects. In the first, we exploit the plausibly exogenous timing of reform implementation in each district based on the pre-reform expiration dates of each district's collective bargaining agreement (CBA). Our results from this analysis indicate that teachers did not exit Michigan schools differently depending on when the reforms themselves took effect in their districts; instead all teachers began to leave at higher rates around 2011. In the second, we compare teachers and non-instructional professional staff in the same school districts, both of whom were subject to a change in the state's public education retirement

[26] As an additional robustness check, we also estimated models where we restrict the sample to only teachers and compare teachers in high versus low poverty districts and low versus high student achievement districts. Consistent with the results in Table 7, we find that teachers in high poverty and lower performing districts were significantly more likely to exit post reform relative to teachers in other districts. We then repeat this analysis using only the sample of staff. Given that staff were not subject to the teacher-specific reforms, this analysis serves as a falsification test. There we find no significant differences in the exit rates of staff in high poverty or lower performing districts compared to staff in other districts. We present these results in Appendix tables 4A and 5A.

system, and both were plausibly affected by economic or budgetary forces facing districts in the wake of the Great Recession, but only the teachers encountered significant changes to tenure, evaluation, and teacher-specific layoff/transfer rules during this time. Our results from this analysis indicate that both groups of employees left Michigan public schools at greater rates in the years following the introduction of these reforms, but our estimates detect no overall differences in attrition. Thus, our primary conclusion is that although teachers (and staff) have been exiting Michigan schools at higher rates, the teacher-specific reforms had no particular impact on teacher exits apart from whatever forces were affecting employees in public schools more generally.

Additional analyses, however, reveal two important exceptions to our general finding. First, we find strong evidence that that teachers in high poverty schools (defined as schools at the 50th or 75th percentile of free/reduced lunch proportions in alternate specifications) were more likely to exit the public school system after reforms than their professional staff counterparts. Second, we find some additional evidence that teachers in lower performing districts experienced higher attrition rates than professional staff post reform. There are two potential and non-exclusive explanations for this pattern. The first is that teachers in high poverty or lower performing schools, where working conditions are plausibly more difficult, felt more threatened than other teachers by the teacher evaluation and tenure reforms. Alternatively, or perhaps in addition to this explanation, these are some of the same schools where the pressure to improve outcomes may have been greatest, and principals and other administrators may have counseled out or even dismissed ineffective teachers. Although we do not have access to their exact evaluation scores, we know that the vast majority (around 97.5 percent) of teachers statewide were given post-reform ratings above "effective" (i.e. few were actually formally rated poorly) each year from 2011 onward. As a result, if the latter explanation is driving our results teachers would have to have exited either of their own accord or via school-specific processes we cannot observe. Regardless, from a simple staffing standpoint, these results strongly suggest that students in the poorest schools were disproportionately impacted by the reform, for better or worse.

Our results have several important policy implications. First and foremost, our results cast doubt on the claims made by opponents of high-stakes teacher evaluation systems and other recent reforms that such reforms would lead to a mass exodus of teachers. Michigan serves as an important case to test this hypothesis because the package of "anti-teacher" and/or "anti-union" reforms implemented in quick succession was greater than those passed in most other states. Therefore, we might expect to see a greater response in Michigan than in other states that only

implemented evaluation *or* tenure reform *or* limited the scope of collective bargaining. In fact, we find compelling evidence that relative to other non-instructional professional staff, teachers were no more likely to exit Michigan's schools post reform. This suggests that the reforms labeled part of a "war on teachers" may not depress teacher morale to the point where we lose large numbers of teachers from the profession.

However, our results regarding teachers in high-poverty schools also raise the concern that teacher labor market reforms like those implemented in Michigan may disproportionately impact the poorest schools and school districts—those already facing staffing constraints. Thus perhaps our most important conclusion from this work may be that policymakers should be attuned to the ways in which any major changes to the public education system affect different teachers and different children in different ways. Policymakers may wish to consider ways to minimize exit responses from high-need teachers such as those willing to teach in high-poverty or low-performing schools by pairing reforms with other ways to compensate such educators.

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Table 1
Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample		Exp. 10-25		Exp. 1-9	
Variables	Mean	St. Dev.	Mean	St. Dev	Mean	St. Dev
Exit	0.041	0.199	0.043	0.204	0.039	0.192
Teacher Characteristics						
Experience	11.69	6.14	15.74	4.30	5.70	2.44
Age	41.04	10.04	45.38	8.46	34.64	8.66
Masters Degree or More	0.591	0.492	0.716	0.451	0.406	0.491
Female	0.740	0.439	0.747	0.435	0.729	0.445
Black	0.029	0.167	0.029	0.168	0.029	0.167
Hispanic	0.009	0.092	0.008	0.089	0.009	0.097
Other Race	0.009	0.093	0.008	0.088	0.010	0.100
Retirement Incentive Eligible	0.013	0.111	0.021	0.143	0.000	0.000
STEM Certified Teacher	0.152	0.359	0.130	0.336	0.184	0.387
Attended Selective College	0.410	0.492	0.404	0.491	0.419	0.493
District Characteristics						
Fraction Free Lunch Eligible	27.50	18.10	28.00	18.20	26.70	17.90
Average ACT Math Score	19.23	1.98	19.22	1.97	19.25	1.98
Observations	555,488		331,088		224,400	

Notes: Table presents summary statistics for the sample of teachers. Sample means and standard deviations are for the years 2005 - 2013.

Table 2
Comparison of Teacher and Professional Staff Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample		Exp. 10-25		Exp. 1-9	
Variables	Teachers	Staff	Teachers	Staff	Teachers	Staff
Exit	0.041	0.053	0.043	0.052	0.039	0.047
Experience	11.69	11.39	15.74	12.80	5.70	8.58
Age	41.04	45.18	45.38	46.47	34.64	42.96
Masters Degree or More	0.591	0.839	0.716	0.853	0.406	0.827
Female	0.740	0.838	0.747	0.833	0.729	0.848
Black	0.029	0.060	0.029	0.060	0.029	0.060
Hispanic	0.009	0.009	0.008	0.010	0.009	0.010
Other Race	0.009	0.011	0.008	0.010	0.010	0.012
Retirement Incentive Eligible	0.013	0.019	0.021	0.023	0	0.008
Observations	555,488	31,778	331,088	27,325	224,400	19,099

Notes: Table presents means for the characteristics of the sample of teachers and professional staff that comprise our control group. Sample means are for years 2005 - 2013.

Table 3
Interrupted Time Series Estimates of Teacher Attrition

	(1) All	(2) Exp. 10 - 25	(3) Exp. 1 - 9	(4) Exp. 6-9	(5) Exp. 1 - 5
			ISD Fixed Effects		
Trend	0.000	-0.000	0.001***	0.001***	0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2011	0.005***	0.004***	0.007***	0.004	0.012***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
2012	0.012***	0.013***	0.008***	0.008***	0.009***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
2013	0.017***	0.018***	0.013***	0.012***	0.014***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)
			District Fixed Effects		
Trend	0.000*	-0.000	0.002***	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
2011	0.005***	0.004***	0.007***	0.004*	0.011***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
2012	0.012***	0.013***	0.008***	0.008***	0.009**
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
2013	0.017***	0.019***	0.013***	0.012***	0.013***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
Observations	555,488	331,088	224,400	125,912	98,488

Notes: Table presents linear probability model estimates for the probability of teacher exit. Each column presents results from a separate regression for the sample of teachers listed in the top row. All specifications include the full set of control variables listed in Table 2. The top panel includes ISD fixed effects while the bottom panel includes school district fixed effects. Robust standard errors clustered at the ISD level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4
DD Estimates of Teacher Attrition Based on Pre-Reform CBA Expiration Timing

	(1) All	(2) Exp 10 - 25	(3) Exp 1 - 9
		Baseline No Controls	
Exposed	-0.002	0.000	-0.006
	(0.002)	(0.003)	(0.004)
		Baseline with Controls	
Exposed	-0.002	0.001	-0.005
	(0.002)	(0.003)	(0.004)
		Baseline + ISD-by-Year Fixed Effects No Controls	
Exposed	-0.000	0.002	-0.004
	(0.002)	(0.003)	(0.004)
		Baseline + ISD-by-Year Fixed Effects with Controls	
Exposed	0.000	0.003	-0.004
	(0.002)	(0.002)	(0.004)
Observations	552,555	329,622	222,933

Notes: Table presents DD linear probability model estimates for the probability of teacher exit. Exposed is an indicator variable that takes the value of unity for all years after a school district's pre-reform CBA expired, implying a teacher in the district was fully impacted by the reforms. Each column presents results from a separate regression for the sample of teachers and staff listed in the top row. Panel 1 includes district and year fixed effects with no control variables. Panel 2 adds the full set of control variables listed in Table 2 to the specification in Panel 1. Panels 3 and 4 replace the year fixed effects in panels 1 and 2 with ISD-by-year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5
DD Estimates of Probability of Attrition: Teachers and Professional Staff

	(1)	(2)	(3)	(4)	(5)
	All	Exp. 10 - 25	Exp. 1 - 9	Exp. 10 - 25 Alt.	Exp. 1 - 9 Alt.
			Baseline No Controls		
Teacher	-0.010***	-0.007***	-0.007***	-0.007***	-0.013***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Teacher Post	-0.003	-0.004	0.001	-0.007**	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
			Baseline with Controls		
Teacher	0.001	-0.000	-0.003	0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Teacher Post	-0.003	-0.001	-0.003	-0.004	0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
			Baseline + ISD-by-Year Fixed Effects No Controls		
Teacher	-0.010***	-0.007***	-0.006***	-0.007***	-0.013***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Teacher Post	-0.004	-0.005	0.001	-0.008***	0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
			Baseline + ISD-by-Year Fixed Effects with Controls		
Teacher	0.001	-0.000	-0.003	0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Teacher Post	-0.003	-0.001	-0.003	-0.004	-0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	587,266	358,413	248,150	362,866	256,178

Notes: Table presents DD linear probability model estimates for the probability of teacher/staff exit. Each column presents results from a separate regression for the sample of teachers and staff listed in the top row. Panel 1 includes district and year fixed effects with no control variables. Panel 2 adds the full set of control variables listed in Table 2 to the specification in Panel 1. Panels 3 and 4 replace the year fixed effects in panels 1 and 2 with ISD-by-year fixed effects. Columns 4 and 5 use all staff as the comparison group. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6
Pre and Post Reform Teacher Salary Schedule Comparison

	(1)	(2)	(3)	(4)	(5)	(6)
	Nominal Salary		Real Salary		Change In Salary	
Salary Variables	Pre-Reform	Post-Reform	Pre-Reform	Post-Reform	Nominal	Real
Base BA	35,800	35,950	38,187	36,317	169.9	-1,844
Base MA	38,940	39,079	41,538	39,482	160.9	-2,025
Step 5 BA	43,181	42,981	46,062	43,418	-119.5	-2,601
Step 5 MA	47,302	46,955	50,460	47,438	-235.5	-2,901
Step 10 BA	54,432	53,596	58,072	54,153	-687.8	-3,766
Step 10 MA	59,931	58,869	63,938	59,489	-818.2	-4,202
Step 20 BA	61,607	61,768	65,706	62,373	298.8	-3,163
Step 20 MA	68,531	68,767	73,093	69,475	420.4	-3,411
Observations	432		432		432	

Notes: Table presents means of salary steps for school districts. Pre-Reform corresponds to the last salary schedule negotiated in a CBA prior to 2011 reforms. Post-Reform corresponds to first salary schedule negotiated in a CBA post 2011. Change in Salary denotes change in salary between pre- and post-reform periods.

Table 7
DD Estimates for Various Sub-Groups

	(1)	(2)	(3)	(4)	(5)
	All	Exp. 10 - 25	Exp. 1 - 9	Exp. 10 - 25 Alt.	Exp. 1 - 9 Alt.
			Above 50th Percentile Free Lunch Districts		
Post FL 50th	0.012**	0.013**	0.014**	0.011**	0.017***
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)
Observations	217,544	137,851	86,393	139,363	89,646
			Above 75th Percentile Free Lunch Districts		
Post FL 75th	0.011*	0.015*	0.011	0.009	0.015*
	(0.007)	(0.008)	(0.009)	(0.007)	(0.008)
Observations	126,163	80,510	49,612	81,463	51,488
			Below 50th Percentile ACT Math Score Districts		
Post ACT 50th	0.002	0.004	0.005	0.000	0.008*
	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)
Observations	254,404	155,926	107,064	157,886	110,683
			Below 25th Percentile ACT Math Score Districts		
Post ACT 25th	0.008	0.006	0.012	0.003	0.016**
	(0.006)	(0.007)	(0.008)	(0.006)	(0.007)
Observations	128,004	79,041	53,480	80,047	55,534
			STEM Teachers		
STEM Post	-0.003	-0.006	0.001	-0.010	0.001
	(0.005)	(0.008)	(0.006)	(0.008)	(0.005)
Observations	68,397	44,367	43,321	48,822	51,353
			Teachers From Above Median 75th Percentile ACT Score Colleges		
Post 75th Percentile ACT	-0.002	-0.002	-0.001	-0.006*	0.002
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Observations	259,453	161,027	117,718	165,479	125,749

Notes: Table presents DD linear probability model estimates for the probability of teacher/staff exit. Each column presents results from a separate regression for the sample of teachers/staff listed in the top row. Top panel limits the sample to above 50th percentile free or reduced price lunch districts. Second panel limits the sample to above 75th percentile free or reduced price lunch districts. Third and fourth panels limit sample to below 50th and below 25th percentile ACT math score districts. Fifth panel compares STEM certified teachers to all staff. Bottom panel compares teachers that graduated from MI colleges with 75th percentile ACT scores above the median to all staff. All specifications include the full set of control variables listed in Table 2 and district and ISD-by-year fixed effects. Robust standard errors clustered at the district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8
Event Study Estimates

	(1) All	(2) Exp. 10 - 25	(3) Exp. 1 - 9	(4) All	(5) Exp. 10 - 25	(6) Exp. 1 - 9
	Above 50th Percentile Free Lunch Districts			Above 75th Percentile Free Lunch Districts		
Treat * 2008	0.010	0.011	0.008	0.010	0.008	0.010
	(0.007)	(0.009)	(0.008)	(0.010)	(0.013)	(0.009)
Treat * 2009	-0.004	-0.001	-0.000	-0.009	-0.006	-0.005
	(0.008)	(0.009)	(0.009)	(0.011)	(0.011)	(0.012)
Treat * 2010	0.005	0.009	0.006	0.003	0.011	0.005
	(0.008)	(0.008)	(0.009)	(0.011)	(0.010)	(0.013)
Treat * 2011	0.005	0.007	0.008	0.000	0.003	0.005
	(0.008)	(0.009)	(0.010)	(0.010)	(0.012)	(0.014)
Treat * 2012	0.020**	0.021**	0.023**	0.013	0.016	0.023
	(0.009)	(0.010)	(0.010)	(0.012)	(0.014)	(0.014)
Treat * 2013	0.018**	0.020**	0.018**	0.024**	0.032***	0.009
	(0.009)	(0.010)	(0.009)	(0.009)	(0.011)	(0.011)
Observations	217,544	137,851	86,393	126,163	80,510	49,612
	Below 50th Percentile ACT Math Score Districts			Below 25th Percentile ACT Math Score Districts		
Treat * 2008	0.012*	0.010	0.011*	0.010	0.006	0.007
	(0.007)	(0.009)	(0.007)	(0.009)	(0.011)	(0.009)
Treat * 2009	-0.004	-0.002	-0.005	-0.016	-0.013	-0.012
	(0.007)	(0.007)	(0.008)	(0.011)	(0.011)	(0.012)
Treat * 2010	0.002	0.004	0.001	-0.001	-0.004	0.000
	(0.007)	(0.006)	(0.007)	(0.009)	(0.009)	(0.010)
Treat * 2011	-0.003	-0.002	-0.001	-0.001	-0.005	0.003
	(0.007)	(0.007)	(0.009)	(0.009)	(0.010)	(0.012)
Treat * 2012	0.011	0.015*	0.013	0.007	0.007	0.015
	(0.007)	(0.008)	(0.008)	(0.009)	(0.011)	(0.013)
Treat * 2013	0.003	0.005	0.008	0.013	0.010	0.018*
	(0.008)	(0.009)	(0.010)	(0.010)	(0.012)	(0.011)
Observations	254,404	155,926	107,064	128,004	79,041	53,480

Notes: Table presents event study estimates for the probability of teacher/staff exit. Each column presents results from a separate regression for the sample of teachers/staff listed in the top row. Top panel limits sample to above 50th (columns 1-3) and above 75th (columns 4-6) percentile of free or reduced price lunch districts. Bottom panel limits sample to below 50th (columns 1-3) and below 25th (columns 4-6) percentile of ACT math score districts. All specifications include the full set of control variables listed in Table 2 and district and ISD-by-year fixed effects. Robust standard errors clustered at the district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Figure 1
Trends in Teacher Attrition Rates

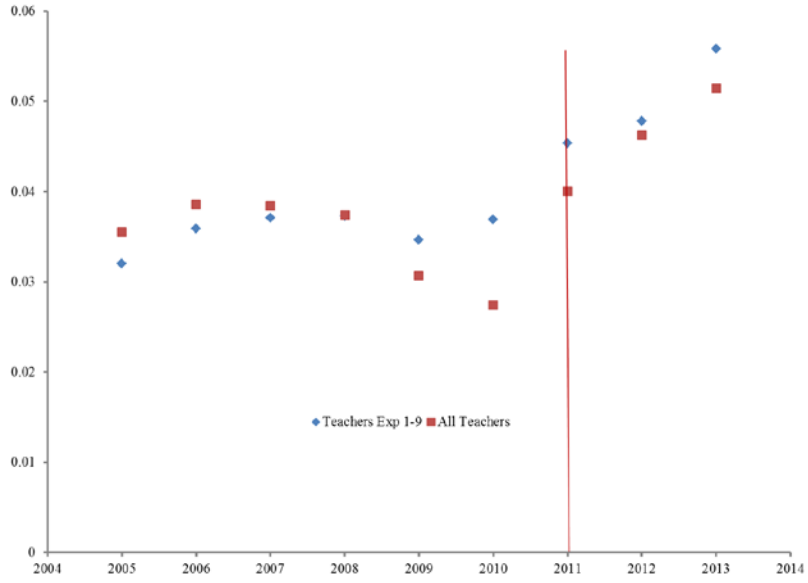


Figure 2
Attrition Trends: All Teachers and Staff

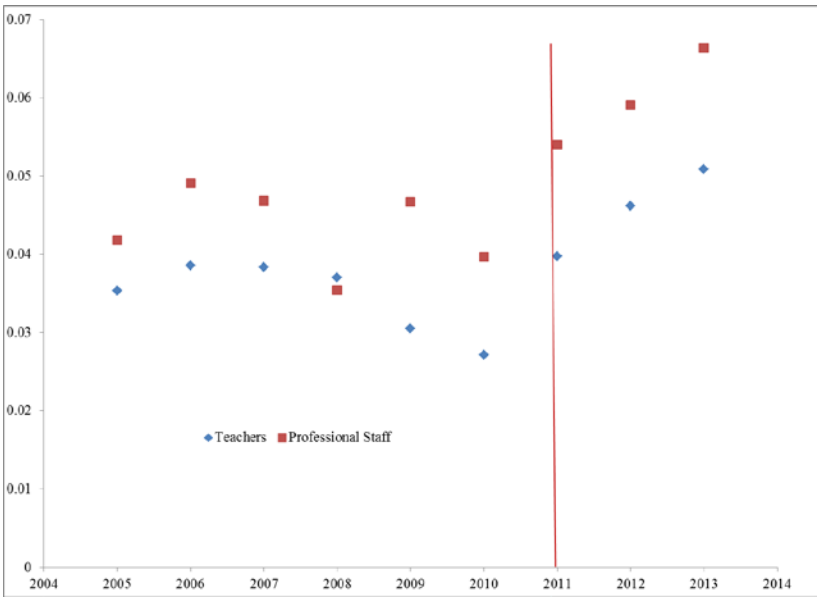


Figure 3
Attrition Trends: Teachers and Staff with 1-9 Years of Experience

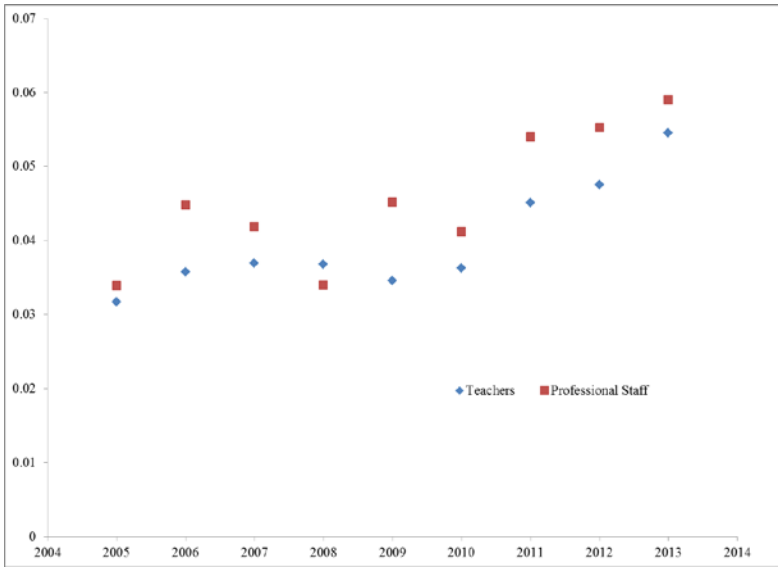


Figure 4
Nominal Salary Trends: All Teachers and Staff

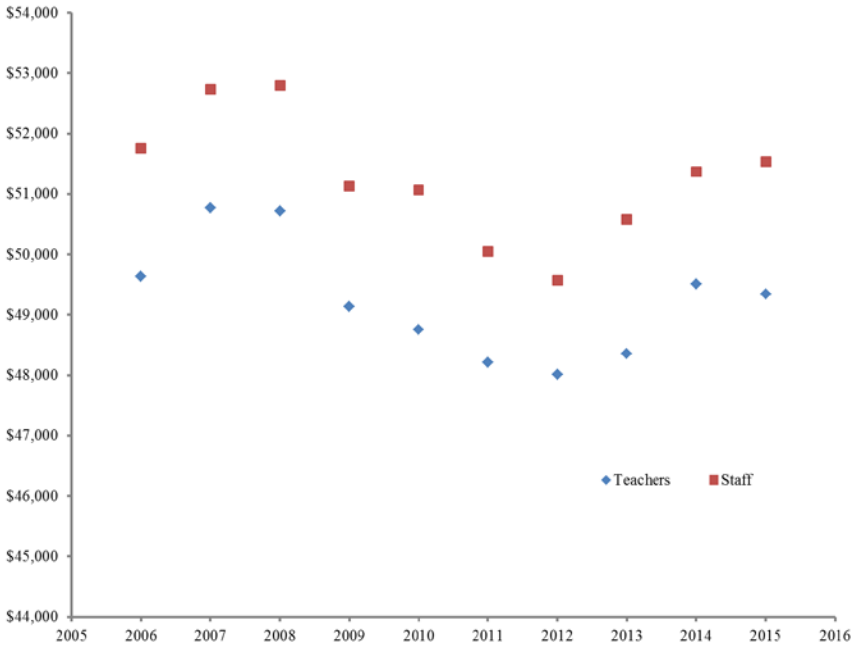


Figure 5
Real Salary Trends: All Teachers and Staff

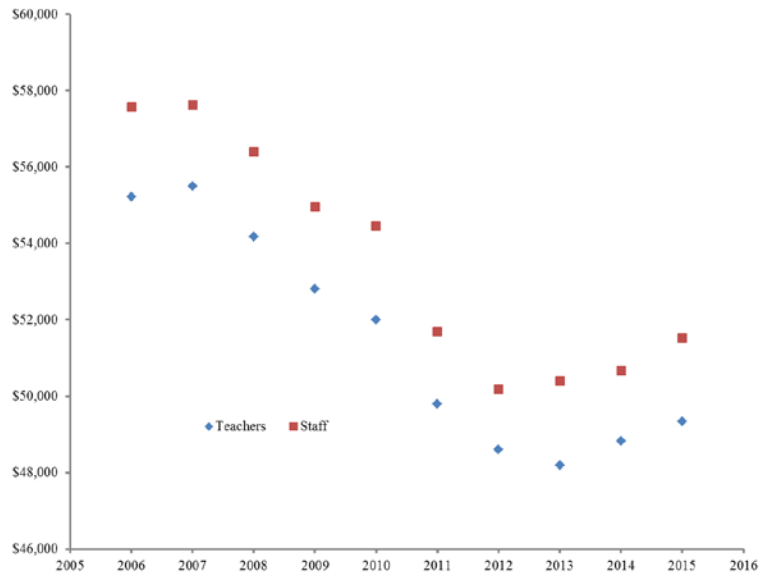


Figure 6
Nominal Salary Trends: Teachers and Staff with 1-9 Years of Experience

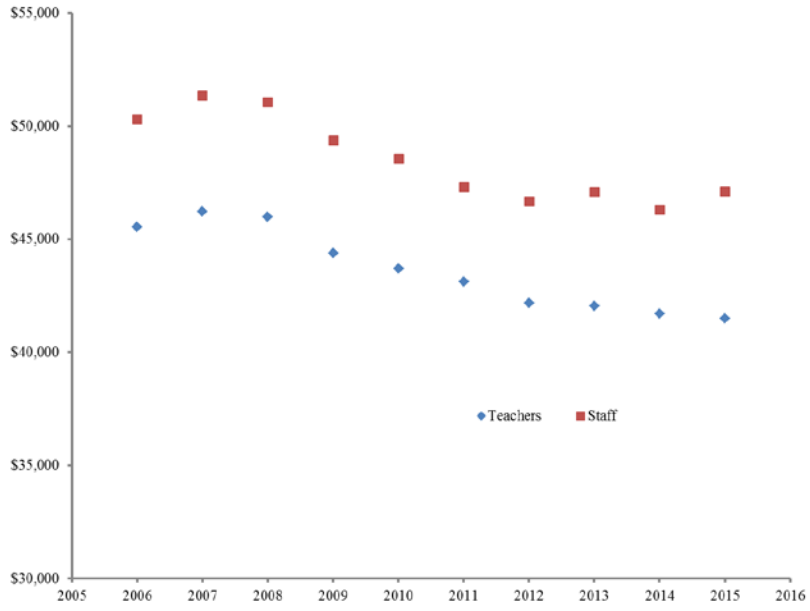
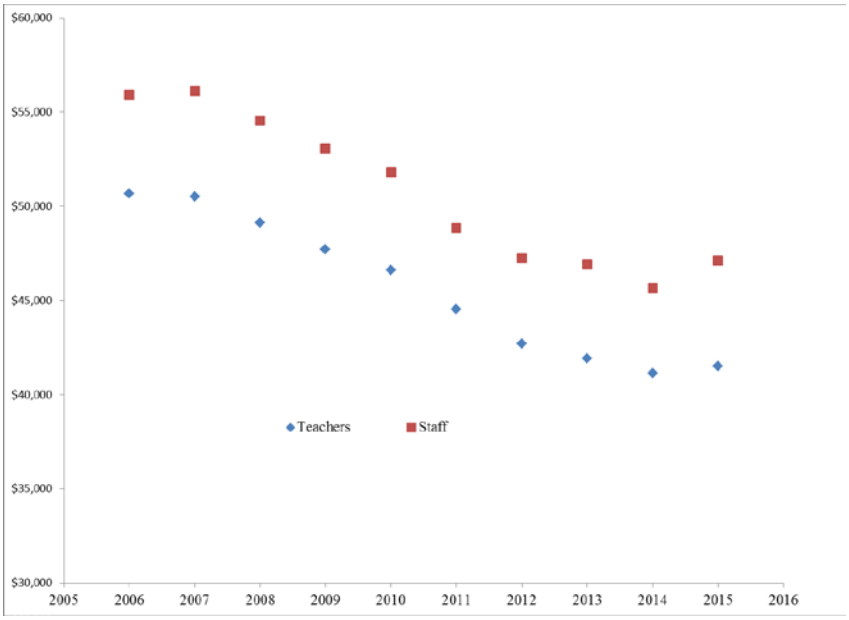


Figure 7
Real Salary Trends: Teachers and Staff with 1-9 Years of Experience



Appendix

Table 1A
Complete Regression Results for ITS Specifications

	(1)	(2)	(3)	(4)	(5)
	All	Exp. 10 - 25	Exp. 1 - 9	Exp. 6 - 9	Exp. 1 - 5
Trend	0.000	-0.000	0.001***	0.001***	0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2011	0.005***	0.004***	0.007***	0.004	0.012***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
2012	0.012***	0.013***	0.008***	0.008***	0.009***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
2013	0.017***	0.018***	0.013***	0.012***	0.014***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)
Experience	-0.002***	0.001	0.001*	-0.006	0.026***
	(0.000)	(0.001)	(0.001)	(0.007)	(0.002)
Experience Squared	0.000***	0.000	-0.000***	0.000	-0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	-0.020***	-0.038***	-0.009***	-0.014***	-0.008***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Masters Degree or More	-0.000	-0.002*	-0.001	-0.006***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Female	0.003***	0.001**	0.003***	0.005***	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Black	0.021***	0.016***	0.026***	0.023***	0.030***
	(0.004)	(0.004)	(0.007)	(0.006)	(0.011)
Hispanic	0.009***	0.006	0.013***	0.011	0.015**
	(0.002)	(0.004)	(0.005)	(0.007)	(0.006)
Other Race	0.012***	0.006	0.018***	-0.001	0.037***
	(0.003)	(0.004)	(0.006)	(0.005)	(0.012)
Retirement Incentive Eligible	0.137***	0.115***			
	(0.007)	(0.007)			
Observations	555,488	331,088	224,400	125,912	98,488

Notes: Table presents complete set of coefficient estimates for results presented in Panel 1 of Table 3. Each column presents results from a separate regression for the sample of teachers listed in the top row. Robust standard errors clustered at the ISD level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2A
DD Estimates for Salary Regressions

	(1)	(2)	(3)
	All	Exp. 10 - 25	Exp. 1 - 9
		ISD Fixed Effects	
Teacher	-0.000	0.007	-0.001
	(0.009)	(0.009)	(0.009)
Teacher Post	-0.007	-0.008	0.010
	(0.012)	(0.014)	(0.009)
		District Fixed Effects	
Teacher	-0.002	0.006	-0.003
	(0.008)	(0.008)	(0.009)
Teacher Post	-0.006	-0.008	0.009
	(0.011)	(0.011)	(0.008)
		ISD-by-Year Fixed Effects	
Teacher	0.001	0.009	0.000
	(0.008)	(0.009)	(0.009)
Teacher Post	-0.010	-0.012	0.005
	(0.012)	(0.014)	(0.008)
Observations	593,831	385,435	224,213

Notes: Table presents DD estimates for specifications where the depended variable is the natural log of annual salaries. Each column presents results from a separate regression for the sample of teachers and staff listed in the top row. All specifications include the full set of control variables listed in Table 2. Panel 1 includes year and ISD fixed effects, Panel 2 includes year and district fixed effects and Panel 3 includes ISD-by-Year fixed effects. Robust standard errors clustered at the district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3A
DD Estimates for Various Sub-Groups: Including Detroit

	(1)	(2)	(3)	(4)	(5)
	All	Exp. 10 - 25	Exp. 1 - 9	Exp. 10 - 25 Alt.	Exp. 1 - 9 Alt.
	Above 50th Percentile Free Lunch Districts				
Post FL 50th	0.013***	0.016***	0.012**	0.014***	0.014***
	(0.004)	(0.005)	(0.006)	(0.005)	(0.005)
Observations	258,919	168,297	99,089	169,949	103,792
	Above 75th Percentile Free Lunch Districts				
Post FL 75th	0.013**	0.018***	0.008	0.014**	0.011*
	(0.005)	(0.007)	(0.007)	(0.007)	(0.006)
Observations	167,538	110,956	62,308	112,049	65,634
	Below 50th Percentile ACT Math Score Districts				
Post ACT 50th	0.004	0.008	0.004	0.005	0.006
	(0.004)	(0.006)	(0.005)	(0.006)	(0.004)
Observations	295,779	186,372	119,760	188,472	124,829
	Below 25th Percentile ACT Math Score Districts				
Post ACT 75th	0.011**	0.013*	0.011*	0.011	0.014**
	(0.005)	(0.008)	(0.006)	(0.007)	(0.005)
Observations	169,379	109,487	66,176	110,633	69,680
	STEM Teachers				
STEM Post	-0.004	-0.007	0.001	-0.010	0.001
	(0.004)	(0.007)	(0.005)	(0.007)	(0.005)
Observations	73,410	48,530	45,938	53,125	55,420
	Teachers From Above Median 75th Percentile ACT Score Colleges				
Post 75th Percentile ACT	-0.000	0.001	-0.002	-0.002	0.000
	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)
Observations	285,016	180,695	125,380	185,287	134,861

Notes: Table presents DD linear probability model estimates for the probability of teacher/staff exit. Each column presents results from a separate regression for the sample of teachers/staff listed in the top row. Sample includes the Detroit school district. Top panel limits the sample to above 50th percentile free or reduced price lunch districts. Second panel limits the sample to above 75th percentile free or reduced price lunch districts. Third and fourth panels limit sample to below 50th and below 25th percentile ACT math score districts. Fifth panel compares STEM certified teachers to all staff. Bottom panel compares teachers that graduated from MI colleges with 75th percentile ACT scores above the median to all staff. All specifications include the full set of control variables listed in Table 2 and district and ISD-by-year fixed effects. Robust standard errors clustered at the district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Alternative DD Estimates for the Sample of Teachers and Sample of Professional Staff

As a robustness check, we also estimated models designed to examine whether certain sub-groups of teachers were more likely to exit the teaching profession post reform than other teachers. Specifically, using only the sample of teachers, we estimate DD models that take the following form:

$$Exit_{ijt}^{g} = \rho_0 + \rho_1 HP_i + \rho_2 Post_t * HP_i + X_{ijt}\theta^5 + \lambda_t + \delta_j + \mu_{ijt}, \quad (1A)$$

where HP_i is an indicator that equals one if teacher *i* belongs to a group that potentially has a higher propensity to exit the teaching profession than other teachers, $Post_t * HP_i$ is the interaction between HP_i and an indicator for whether the observation is from the post reform period, μ_{ijt} is a random disturbance term and all other terms are as defined in (3). As in Table 7, we operationalize HP_i in (1A) using several measures: whether a teacher is assigned to a high poverty district or a district with lower performing students, whether a teacher is STEM certified and whether a teacher graduated from a more selective college or university. As in (3), the coefficient of primary interest is ρ_2 which represents the DD estimate of the effect of Michigan's teacher evaluation and tenure reforms on the attrition rate of teachers who are potentially more sensitive to the reforms.

Results based on the estimation of equation (1A) are presented in Table 4A. Similar to Table 7, panels 1 and 2 of Table 4A report results where we compare teachers in districts where the fraction of free or reduced price lunch students is above the 50th or above the 75th percentile in the sample to all other teachers. Panels 3 and 4 repeat the analyses in panels 1 and 2 but use average district-wide ACT math scores to create the comparison groups rather than free or reduced price lunch students. Panel 5 reports results where we compare secondary STEM teachers to all other teachers and the bottom panel presents results where we compare teachers that attended a college or university where the 75th percentile ACT score was above the median among the colleges and universities in our sample to all other teachers.

The results reported in panels 1 and 2 of Table 4A provide consistent evidence that compared to teachers in lower poverty schools, those in the high poverty schools were more likely to exit in the post-reform period. Specifically, in both panels 1 and 2 all of the DD estimates are positive and statistically significant at the 1% level. In terms of magnitude, the point estimates suggest teachers in districts where the fraction of free lunch eligible students is above the median are approximately 1.1 to 1.7 percentage points more likely to exit the teaching

profession post reform relative to other teachers. As seen in panels 3 and 4 we find similar results when we compare teachers in districts with below 50th or below 75th percentile ACT math scores. As shown in panels 5 and 6, in contrast to our free lunch and test score results, we find no evidence that STEM teachers or teachers that attended a more selective college or university were more likely to exit post-reform relative to other teachers. These results are generally consistent with the DD estimates reported in Table 7 that compare teachers to professional staff.

In Tables 5A we report results based on specifications identical to those reported in Table 4A except we now limit the sample to professional staff rather than teachers. Since professional staff were not subject to the teacher evaluation and tenure reforms, the estimated coefficients on the post-reform high-poverty/low test score interactions should all be small in magnitude and statistically insignificant. As shown in Table 5A this is generally what we find: all of the estimated coefficients on the post-reform high-poverty interactions are statistically insignificant and with the exception of the estimates for more experienced staff (column 2) in panels 3 and 4 are generally small in magnitude.

Table 4A
Teacher Comparisons for Various Sub-Groups

	(1)	(2)	(3)
	All	Exp. 10 - 25	Exp. 1 - 9
	Above 50th Percentile Free Lunch Districts		
Post FL 50th	0.011***	0.011***	0.013***
	(0.002)	(0.003)	(0.004)
	Above 75th Percentile Free Lunch Districts		
Post FL 75th	0.014***	0.014***	0.017***
	(0.004)	(0.004)	(0.006)
	Below 50th Percentile ACT Math Score Districts		
Post ACT 50th	0.009***	0.006**	0.014***
	(0.002)	(0.003)	(0.003)
	Below 25th Percentile ACT Math Score Districts		
Post ACT 75th	0.015***	0.013***	0.025***
	(0.004)	(0.004)	(0.005)
	STEM Teachers		
STEM Post	-0.001	-0.008	0.004
	(0.004)	(0.007)	(0.005)
	Teachers From Above Median 75th Percentile ACT Score Colleges		
Post 75th Percentile ACT	-0.001	-0.002	0.000
	(0.001)	(0.002)	(0.002)
Observations	555,449	331,065	224,384

Notes: Table presents DD linear probability model estimates for the probability of teacher exit. Each column presents results from a separate regression for the sample of teachers listed in the top row. Panels 1 and 2 compare teachers in above 50th and above 75th percentile free or reduced price lunch districts to all other teachers. Third and fourth panels compare teachers in below 50th and below 25th percentile ACT math score districts to all other teachers. Fifth panel compares STEM certified teachers to all other teachers. Bottom panel compares teachers that graduated from MI colleges with 75th percentile ACT scores above the median to all other teachers. All specifications include the full set of control variables listed in Table 2 and district and year fixed effects. Robust standard errors clustered at the district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5A
Staff Comparisons for Various Sub-Groups

	(1)	(2)	(3)
	All	Exp. 10 - 25	Exp. 1 - 9
	Staff in Above 50th Percentile Free Lunch Districts		
Post FL 50th	-0.007	-0.003	-0.015
	(0.006)	(0.007)	(0.010)
	Staff in Above 75th Percentile Free Lunch Districts		
Post FL 75th	0.000	0.002	0.004
	(0.007)	(0.009)	(0.014)
	Staff in Below 50th Percentile ACT Math Score Districts		
Post ACT 50th	0.003	0.007	-0.001
	(0.006)	(0.007)	(0.010)
	Staff in Below 25th Percentile ACT Math Score Districts		
Post ACT 75th	0.005	0.010	0.000
	(0.007)	(0.008)	(0.012)
Observations	31,770	18,448	13,306

Notes: Table presents DD linear probability model estimates for the probability of staff exit. Each column presents results from a separate regression for the sample of staff listed in the top row. Panels 1 and 2 compare staff in above 50th and above 75th percentile free or reduced price lunch districts to all other staff. Third and fourth panels compare staff in below 50th and below 25th percentile ACT math score districts to all other staff. All specifications include the full set of control variables listed in Table 2 and district and year fixed effects. Robust standard errors clustered at the district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.