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A War on Teachers? Labor Market Responses to Statewide Reform

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. Our difference-in-differences identification strategy exploits the plausibly exogenous timing of pre-reform CBA contract expiration dates that governed when teachers were exposed to the reforms to isolate the causal effect of reforms on the probability that a teacher permanently exits Michigan's traditional public schools. We find that, on average, Michigan's teacher accountability reforms had little impact on teacher attrition. However, further analyses provide strong evidence that teachers assigned to hard-to-staff districts (proxied by poverty rates, student performance and dropout rates) were more likely to exit post reform, as well as evidence that pre-tenure teachers were also disproportionately affected. 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 based on experience and teaching location.

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I. Introduction

Over the past eight 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.

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In this paper we provide some of the first empirical 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 public school teachers' workplace protections and the collective bargaining rights of 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 four to five years, increased reliance on multiple measure teacher evaluation systems that include student achievement, and prohibited local collective bargaining of topics including teacher transfer and reassignment, evaluation, performance-based compensation, classroom observations, the length of the school year, and teacher 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 result of these reforms Michigan now has one of the most restrictive scopes of bargaining of any state in the country (Zeehandelaar 2012), and offers an ideal test of the competing hypotheses offered by the advocates for such reforms and the defenders of union and teacher protections.

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 2015-16. 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 ask three research questions: 1) *Do teachers exit the profession at greater rates in the years post-reform?;* 2) *What was the impact of Michigan's teacher reforms on teacher exit rates?;* 3) *Did Michigan's teacher reforms differentially impact teachers who a) worked in in harder-to-staff districts, b) had greater out-of-teaching opportunities, or c) were higher quality?*

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 traditional public school system.[1] 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.

To identify the causal effects of Michigan's accountability reforms on teacher attrition we utilize a difference-in-differences (DD) identification strategy that exploits 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.

In our baseline DD specifications, we find no evidence of any reform-induced changes in teacher exit rates. These results hold for teachers overall and separately for experienced teachers, mid-career teachers and new teachers. As such, we conclude that the teacher-specific reforms, especially those 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 DD models that allow for heterogeneous treatment effects depending on whether teachers: 1) are assigned to a school district that is arguably more challenging to teach in and harder to staff (high poverty districts, districts with lower performing students, and districts with high dropout rates); 2) have potentially better outside-of-teaching career opportunities (secondary STEM teachers); and 3) graduated from more selective colleges and universities.

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[1] We exclude charter school teachers from this analysis as described below.

We find no evidence that STEM teachers or teachers that graduated from more selective colleges or universities were more likely to exit post-reform. In contrast, we find strong evidence that teachers in "harder-to-staff" districts – those with higher poverty rates, lower performing students and higher dropout rates – were more likely to exit post-reform. Finally, additional analyses and falsification tests suggest that these results have a causal interpretation. For example, we find no evidence that our results are being driven by differential trends in teacher compensation among districts with and without high concentrations of disadvantaged students. Falsification tests based on moving the actual timing of pre-reform CBA contract expirations back either three or four years yield estimates that are small in magnitude and statistically insignificant. Finally, in falsification tests where we replace the sample of teachers with a sample of professional staff that work in the same schools and districts as teachers, but who were not impacted by Michigan's 2011 teacher accountability reforms, we find no evidence of any reform-induced increases in attrition rates.

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In what follows, we provide 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 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

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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.[2] In addition, PA 102 prohibited districts from using seniority as the primary determinant of layoff decisions (as is the case under typical Last-in-First-Out (LIFO) seniority-based layoff processes) and required districts instead 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 or professional status. 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, which 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.[3] 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

[2] 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.

[3] 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

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 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, Barrett & Lincove 2017). Other studies have also indicated that teachers value non-pecuniary benefits when they appraise their compensation packages (Loeb & Page 2000), including job stability and mitigated risks to future employment (e.g. Murnane & 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,"[4] 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, and discouraged 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

[4] See, for example, 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

"Our outcome of interest is teacher exit from the Michigan public school system."

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 did induce teacher exit (Strunk, Barrett & Lincove 2017), and evidence from Houston has indicated that low-performing teachers are particularly likely to leave schools after the implementation of rigorous teacher evaluation (Cullen, Koedel & 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.[5] 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[6] teachers, representing 140,000 unique teachers, provide the core analytic sample.

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[5] The Strunk, Barret and Lincove (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 were more likely to exit.

[6] Descriptions of employee assignments were used to identify teachers with primary duty as lead classroom instructors. Individuals with primary assignments in comparatively administrative areas (such as curriculum) and teachers' aides were excluded.

Our outcome of interest is 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 a date of termination indicator in the administrative record and the presence and then absence of a teacher's unique identifier in the time series. Specifically, we define an exit as permanent disappearance after year *t* from the population of Michigan public school teachers.[7] Our exit measure has a number of limitations. First, teachers 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.[8] Using the full extent of our time series, we determined that the vast majority of short-term leaves are for only a single year. As such, we adjust our data to reflect that an individual must be absent for at least two 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.[9]

We also create a number of control variables that capture the demographic characteristics of teachers in our sample. Those variables are: 1) age, age squared and age cubed; 2) an indicator for whether a teacher has a master's degree or higher; 3) an indicator for whether a teacher is female; and 4) indicators for whether a teacher is Black, Hispanic or some other race or ethnicity other than non-Hispanic white.[10]

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

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

[8] We take the position that teacher exits tied to the Spring teaching semester and those known to occur over the summer before the commencement of the next academic year represent the same exit timing. 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 precisely timed spikes in exits associated with retirement incentives based on the experience and age combinations that qualify individuals for early retirement.

[9] 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.

[10] We do not control for years of teaching experience in our specifications due to the fact that years of experience is likely endogenous. Specifically, years of teaching experience is itself a function of whether or not a teacher decides to exit the teaching profession. We have, however, estimated models identical to those reported in the paper that include controls for experience and experience square and all of our results are robust. Results are available upon request.

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 specific groups of teachers were more likely to exit post-reform than other teachers. These groups are: 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 who graduated from more selective colleges and universities. Hanushek, Kain and Rivkin (2004) find that teacher exit rates tend to be substantially higher in schools serving large numbers of disadvantaged students.[11] 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) and high school dropout rates as secondary measures of school districts with challenging teaching environments.[12] We obtain both of these measures from Michigan's Department of Education (MDE) and Center for Educational Performance and Information.

To examine whether teachers with potentially better outside-of-teaching career options were more likely to exit post reform, we use 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 predominantly hold an undergraduate major, minor or non-degree equivalent in math or science—training that typically provides better outside-of-teaching options than other undergraduate majors that teachers typically pursue.[13] 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 from which a teacher graduated. We therefore proxy for teacher quality using the observable

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[11] 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.

[12] 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.

[13] 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.

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. We obtain data on college selectivity from the Integrated Postsecondary Education Data System (IPEDS) maintained by the National Center for Education Statistics (NCES).[14]

We restrict our sample in a number of ways. First, we exclude from the sample all administrators and individuals younger than 21 years of age or older than 70.[15] Second, we restrict the sample to teachers working in traditional K-12 public schools and thus drop teachers who work in charter schools.[16] 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 robustness checks. Table 1 provides summary statistics for the variables used in our analysis. Because we examine the potential differential attrition of teachers with various levels of experience, we present separate summary statistics for teachers based on years of work experience in the Michigan public school system. On average approximately four percent of teachers exit Michigan public schools after any given year. As expected, teachers in the highest experience category (10-plus years) exhibit the highest attrition rates, while teachers in the 6-9 years of experience category exhibit the lowest attrition rates. Furthermore, teachers tend to be predominantly non-Hispanic White and female. Finally, the fraction of teachers with a Master's degree or higher tends to increase monotonically with years of teaching experience.

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[14] 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. Results available upon request.

[15] We exclude individuals age 70 or older to reduce variation in exit rates due primarily to normal retirement decisions.

[16] We exclude charter school teachers from our analyses because they are not subject to either the tenure or collective bargaining reforms that are fundamental to the 2011 policy shift. In addition, pre-reform attrition trends for charter school teachers differ substantially from those of traditional public school teachers. We observe less than 1 percent of TPS teachers who ever move to a charter school, and roughly 6 percent of charter teachers who at some point move into TPS (they are counted as TPS teachers once we observe them in that sector).

Table 1: Summary Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		Exp. 10-plus		Exp. 6 - 9		Exp. 1 - 5	
Variables	Mean	St. Dev.	Mean	St. Dev	Mean	St. Dev	Mean	St. Dev
Exit	0.0550	0.228	0.0658	0.248	0.0290	0.168	0.0434	0.204
Teacher Characteristics								
Age	42.90	10.76	47.45	8.938	36.88	8.435	31.77	8.079
Masters Degree or More	0.606	0.489	0.716	0.451	0.544	0.498	0.230	0.421
Female	0.735	0.441	0.739	0.439	0.728	0.445	0.730	0.444
Black	0.031	0.172	0.032	0.175	0.030	0.170	0.027	0.162
Hispanic	0.008	0.091	0.008	0.087	0.009	0.093	0.010	0.101
Other Race	0.008	0.091	0.007	0.085	0.009	0.094	0.012	0.108
Retirement Incentive Eligible	0.014	0.118	0.022	0.147	0	0	0	0
STEM Certified Teacher	0.071	0.256	0.062	0.241	0.053	0.224	0.131	0.337
Attended Selective College (ACT)	26.01	2.053	26.02	2.093	25.95	1.976	26.03	1.976
Attended Selective College (EPI)	80.03	2.518	80.03	2.522	80.00	2.522	80.08	2.491
District Characteristics								
Fraction Free Lunch Eligible	0.276	0.182	0.282	0.183	0.265	0.178	0.269	0.179
Average ACT Math Score	19.22	1.991	19.20	1.995	19.27	1.973	19.23	1.994
Dropout Rate	0.116	0.0859	0.117	0.086	0.113	0.084	0.117	0.089
Observations	628,509		405,594		125,153		97,762	

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

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 2011-12, 2012-13 and 2013-14, respectively, X_{ijt} is a vector of teacher characteristics, δ_j is a vector of locational labor market fixed effects, operationalized as either the intermediate school district (ISD) or local school district fixed effects, and ε_{ijt} is a random disturbance term.[17] 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 or more years of experience, 6-9 years of experience and 1-5 years of experience. Our rationale for estimating separate equations based on teacher experience groupings is twofold.

First, Michigan's reforms had different implications for teachers with more or less experience. While the teacher evaluation reform affected all teachers, only new, pre-tenure teachers (teachers in their first five years in a district) 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.[18] Second, the "cost" associated with exiting the teaching profession may differ by experience level due to the design of Michigan's retirement system. 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, potentially creating less

[17] ISDs, of which there are 56, are usually centered around 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.

[18] In addition, while the reforms implemented in 2011 required districts to base layoff 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.

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

The ITS model given by equation (1) is designed to provide a descriptive examination of teacher exit rates pre- and post-reform, addressing the question: *do teachers exit the profession at higher rates in the years post-reform*? 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 alternative employment options for teachers.

To overcome that limitation, our primary analysis is based on a difference-in-differences (DD) identification strategy that exploits the plausibly 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.[19] 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 senior personnel at the Michigan Department of Education, who 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.[20]

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[19] The state required districts to begin teacher evaluations in the fall of 2011, however the consequences outlined in the reform legislation did not take effect until districts' CBAs expired.

[20] As we discuss further below, we examine the sensitivity of this assumption by dropping all observations associated with districts with a pre-reform CBA that expired prior to 2011. Our results are robust to dropping those districts.

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

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

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. Consistent with (1), we estimate (2) for the full sample of teachers and staff and separately for each experience group discussed previously.21

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 teacher attrition rates vary depending on pre-reform socio-economic or performance level of students in district j, we estimate models of the following form:

$$Exit^{g}_{ijt} = \pi_0 + \pi_1 Exposed_{jt} + \pi_2(Exposed_{jt}{*}HTS_j) + X_{ijt}\theta^3 + \lambda_t + \delta_j + \nu_{ijt}, \quad (3)$$

where, $Exposed_{jt}{*}HTS_j$ is an interaction term between the exposure indicator from (2) and a pre-reform measure of the socio-economic or performance level of students in district *j*, ν_{ijt} is a random disturbance term and all other terms are as defined in (2).

As noted previously, we operationalize HTS_j with three proxies for hard-to-staff districts: 1) the fraction of students eligible for free or reduced price lunch, 2) average student performance on the ACT exam and 3) district dropout rates. Note that because HTS_j does not vary within districts, the level effect of HTS_j in (3) is absorbed by the district fixed effects. The coefficients of primary interest in (3) are π_1 and π_2. Specifically, π_1 is the DD estimate of the effect of full exposure to the 2011 reforms on teacher attrition, while π_2 measures how that DD estimate changes if a teacher is assigned to a hard-to-staff district.

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[21] In addition, over the course of the period under study, 547 traditional public schools were closed in the state (largely during the great recession). This could bias our results if we assume that teacher exit is a response to policy changes rather than simultaneous school closures. In alternate specifications we either drop teachers assigned to schools that closed during our sample time frame or include an indicator for school closure, and find that our results are entirely robust to these specification checks. Results available from the authors upon request.

Finally, we are also interested in whether STEM-certified teachers (our proxy for teachers with better outside-of-teaching career options) or teachers who graduated from a more selective college or university were more likely to exit the profession post reform. Thus, we also estimate models of the following form:

$$Exit^{g}_{ijt} = \alpha_0 + \alpha_1 Exposed_{jt} + \alpha_2\left(Exposed_{jt}{*}S_{ijt}\right) + \alpha_3 S_{ijt} + X_{ijt}\theta^4 + \lambda_t + \delta_j + \omega_{ijt}, \quad (4)$$

where $Exposed_{jt}{*}S_{ijt}$ is an interaction term between the exposure indicator and an indicator for whether or not a teacher is a STEM-certified teacher or an index for the selectivity of the college or university that the teacher attended, ω_{ijt} is a random disturbance term and all other terms are as defined in (3). Since S_{ijt} now varies at the teacher-level, equation (4) also includes the level effect of S_{ijt}. Similar to (3), the coefficients of primary interest are α_1 and α_2.

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 whose CBAs expired at different times several years after the recession itself, the recessionary effects should not contaminate our estimates of the effect of differential CBA expiration times on attrition. In addition, in heterogeneity analyses below, we explore the possibility that teachers in districts hardest hit by the recession were more likely to exit following their CBA expiration and, by way of preview, we note here that we see little evidence of such a pattern.

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).[22] This retirement incentive directly incentivized eligible teachers and staff to retire ahead of the series of teacher-related reforms the following year. We address this by including in

[22] 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.

our empirical specifications an indicator that takes the value of one in 2010 for individuals eligible for the incentive.

V. Results

A. Interrupted Time Series Analysis

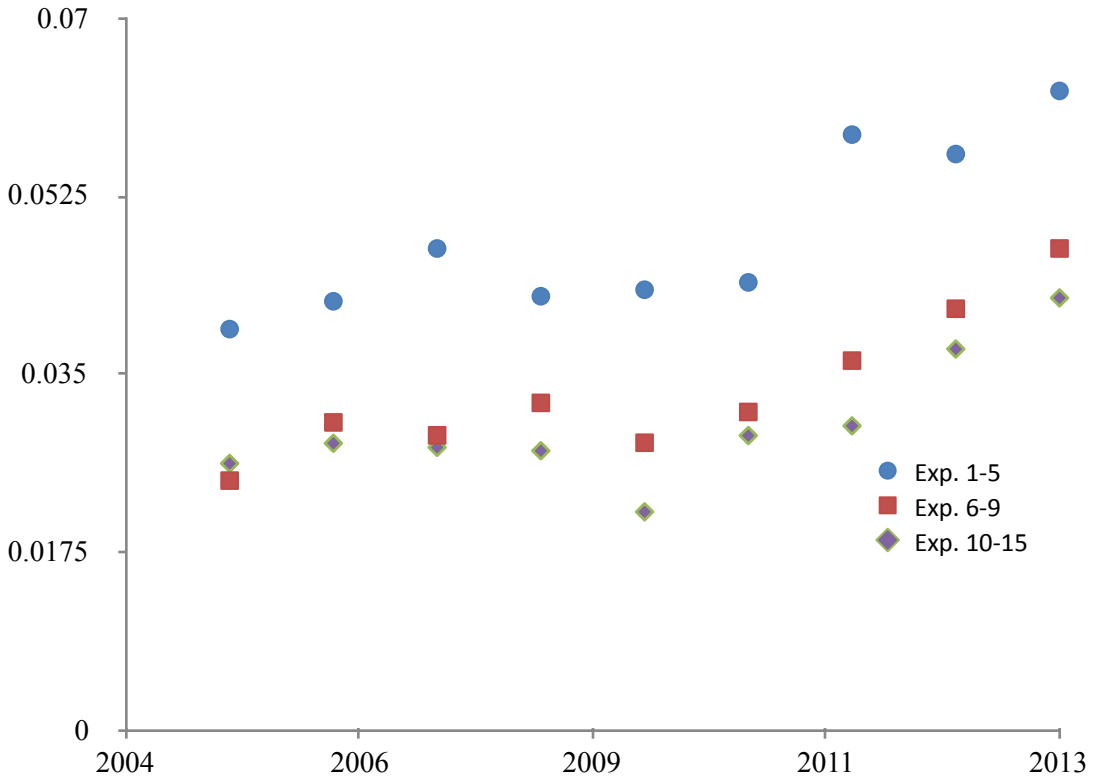
To provide some initial context on the evolution of teacher attrition rates before and after Michigan's adoption of teacher evaluation and tenure reforms, Figure 1 plots annual average attrition rates from 2005-06 through 2013-14. We present attrition rates for teachers with 1-5 years of experience (circles), with 6-9 years of experience (squares) and with 10-15 years of experience (triangles). For all three groups of teachers, Figure 1 reveals relatively stable attrition rates prior to 2011, the main reform year, with substantial increases in attrition rates from 2011 on.[23]

Interrupted time series (ITS) results based on the estimation of (1) are presented in Table 2. In the interest of brevity, Table 2 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 2 include the full set of individual control variables listed in Table 1. Complete regression results for all the control variables for the specifications shown in the top panel of Table 2 are presented in Appendix Table 1A. The top panel of Table 2 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 effects. The standard errors reported in Table 2 are clustered at the district-level to allow for within-district autocorrelation of the disturbance term.

The results reported in Table 2 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 0.6 to 1.7 percentage points, especially after the first post-policy year of 2011. The fact that attrition rates were lower in the first reform year of 2011 may not be surprising given that few teachers would have had time to react to the policy, which passed less than two months before the school year began.

[23] The dip in attrition rates for the sample of all teachers with 10-15 years of experience that occurs in 2009 is due primarily to our decision to drop teachers eligible for early retirement in that year when creating the figure. If we left those teachers in the figure, one would observe a substantial spike in exit rates in 2009. 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. In the empirical work that follows we control for teachers eligible for early retirement by including an indicator that takes the value of one if a teacher was eligible for early retirement in 2009. We chose to present attrition rates for teachers with 10-15 years of experience, rather than teachers with 10 or more years of experience, to reduce to influence of the early retirement program (which mainly affected more senior teachers) on our graphical illustration of exit rates.

Figure 1: Teacher Exits by Experience 2005-2013



B. Difference-in-Differences Analyses

The results reported in Table 2 and Figure 1 provide evidence that teacher attrition increased in the post-reform period. However, the ITS results do not allow us to conclude that reforms themselves induced greater exit, 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 we therefore turn to our DD identification strategy to isolate the causal effect of Michigan's reforms on teacher attrition.

Table 2: Interrupted Time Series Estimates of Teacher Attrition

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6-9	Exp. 1 - 5
		Panel A: ISD Fixed Effects		
Trend	-0.000	-0.001*	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.001)
2011	0.007***	0.009***	0.004	0.003
	(0.002)	(0.002)	(0.002)	(0.004)
2012	0.014***	0.018***	0.003	0.010**
	(0.002)	(0.003)	(0.003)	(0.004)
2013	0.014***	0.017***	0.006*	0.010**
	(0.002)	(0.003)	(0.003)	(0.005)
		Panel B: District Fixed Effects		
Trend	0.000	-0.001	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.001)
2011	0.007***	0.009***	0.004	0.002
	(0.002)	(0.002)	(0.002)	(0.004)
2012	0.014***	0.018***	0.003	0.009**
	(0.002)	(0.003)	(0.003)	(0.004)
2013	0.014***	0.017***	0.006*	0.009**
	(0.002)	(0.003)	(0.003)	(0.004)
Observations	646,516	417,198	128,392	100,926

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 individual control variables listed in Table 1. Bottom panel includes school district fixed effects. Robust standard errors clustered at the district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3: DD Estimates of Probability of Teacher Attrition: Pre-Reform CBA Expiration Timing

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
		Panel A: Baseline		
Exposed	-0.000	-0.001	0.004	0.003
	(0.002)	(0.003)	(0.003)	(0.005)
		Panel B: Heterogeneous Treatment Effects		
Exposed 1st year	0.001	0.000	0.005*	0.005
	(0.002)	(0.003)	(0.003)	(0.005)
Exposed 2nd year	-0.007**	-0.009**	-0.003	-0.004
	(0.003)	(0.004)	(0.004)	(0.007)
Exposed 3rd Year	-0.007	-0.011*	-0.002	0.007
	(0.005)	(0.006)	(0.006)	(0.010)
Observations	642,401	414,832	127,533	100,036

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 listed in the top row. All specifications include the full set of individual control variables plus district and year fixed effects. Bottom panel replaces the exposed indicator with separate treatment year indicators. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Results based on the DD specification given by equation (2), which exploits the plausibly exogenous timing of pre-reform CBA expirations, are presented in the top panel of Table 3.

Across all experience groups, we find no evidence that reform exposure (based on the expiration of the pre-reform CBAs) led to higher teacher attrition rates. It is possible that teachers did not react to the reform exposure immediately but rather responded to the policy reforms only after they had been exposed to the reforms for a year or more. For example, teachers may have taken a “wait and see” approach to the conditions of teaching in their district post-reform and then decide to exit only after determining the new regime was untenable. Similarly, the consequences of leaving an established career before retirement age may have been substantial enough for individual teachers to wait as long as possible to do so. To examine the possibility of a delayed reform effect regardless of the reason, we replaced the exposure

indicator from equation (2) with a set of indicator variables that take the value of one for each year a teacher was exposed to the policy reforms. For example, if a district's pre-reform CBA ended in 2011, we include indicators for the 2011 year (1st year of exposure), the 2012 year (2nd year of exposure) and the 2013 year (3rd year of exposure). Results are reported in the bottom panel of Table 3. Similar to the our standard DD results reported in the top panel, we once again find little evidence that reform exposure led to higher teacher attrition rates.[24] Across all specifications only one of the estimated coefficients is positive and statistically significant (column 3, 1st year of exposure), but even in that case the estimated coefficient is relatively small in magnitude and only marginally significant.

Finally, ten percent of the districts in our sample had pre-reform CBAs that expired prior to 2011 and, as noted previously, for these districts we set the expiration date of pre-reform CBAs to 2011 due to the fact that they may have been operating under anticipated law changes. In appendix Table 3A, we examine the sensitivity of our Table 3 results to this issue by dropping all observations associated with districts with a pre-reform CBA that expired prior to 2011. As shown in Table 3A, our results are not sensitive to dropping those districts.

C. Heterogeneous Effects

Although we find no evidence that teachers exited Michigan public schools at higher rates following exposure to the 2011 reforms, it is possible that while on average teachers were no more likely to exit post reform, the average masks heterogeneity in attrition rates across sub-groups of teachers. We explore that possibility in this section by turning to estimates based on equations (3) and (4) which interact the indicator for exposure to the 2011 reforms with other covariates that may had led to heterogeneous treatment effects. The first issue we investigate is whether there was heterogeneity in exit rates depending on the impact of the Great Recession on local labor markets. Specifically, one possibility is that higher post-reform attrition rates we observe in the ITS results reported in Table 2, are being driven by districts that were hardest hit by the Great Recession. Teachers in those districts may have delayed their decision to exit the teaching profession until after the Great Recession, when improvements in labor market conditions may have led to better alternative employment options. To examine that possibility, we collected data on county-level unemployment rates in 2009 and created a standardized unemployment rate with a mean of zero and a standard deviation of one. We then interacted county unemployment rates with the exposure index to estimate specifications based on equation (3). Results are reported in the top panel of Table 4.

[24] Table 2A of the Appendix reports estimates identical to those reported in Table 3 except we add the Detroit school district to our sample. Adding Detroit to the sample has little effect on our results. All of the estimated coefficients on the exposure indicator remain small in magnitude and statistically insignificant.

Table 4: DD Estimates for Various Subgroups Based on CBA Expiration Timing

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
	Panel A: Standardized 2009 County Unemployment Rate			
Exposed	-0.000	-0.001	0.004	0.003
	(0.002)	(0.003)	(0.003)	(0.005)
Exposed * Unemployment Rate	0.000	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.002)
	Panel B: Fraction Free Lunch			
Exposed	-0.010***	-0.012***	-0.007*	-0.007
	(0.003)	(0.004)	(0.004)	(0.006)
Exposed * Free Lunch	0.038***	0.038***	0.042***	0.035**
	(0.008)	(0.010)	(0.010)	(0.014)
	Panel C: Standardized ACT Test Scores (District Students)			
Exposed	0.000	-0.001	0.005	0.003
	(0.002)	(0.003)	(0.003)	(0.005)
Exposed * Test Scores	0.006***	0.006***	0.007***	0.009***
	(0.001)	(0.002)	(0.002)	(0.002)
	Panel D: Dropout Rates			
Exposed	-0.008***	-0.009**	-0.008**	-0.006
	(0.003)	(0.004)	(0.004)	(0.006)
Exposed * Dropout Rate	0.072***	0.064***	0.113***	0.070**
	(0.019)	(0.024)	(0.021)	(0.034)
	Panel E: Secondary STEM Teachers			
Exposed	-0.000	-0.001	0.004	0.003
	(0.002)	(0.003)	(0.003)	(0.005)
Exposed * Secondary STEM Teacher	0.003	-0.000	-0.002	-0.001
	(0.004)	(0.005)	(0.011)	(0.006)

	Panel F: Standardized 75th Percentile ACT Score Colleges			
Exposed	0.001	-0.000	0.004	0.002
	(0.002)	(0.003)	(0.003)	(0.006)
Exposed * ACT75th	-0.000	-0.000	0.001	0.002
	(0.001)	(0.001)	(0.002)	(0.002)
Observations	642,401	414,832	127,533	100,036

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. Each column presents results from a separate regression for the sample of teachers listed in the top row. District student ACT scores (panel C) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables plus district and year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

There is little evidence of heterogeneity in exit rates that stems from differences in county unemployment rates: the estimated coefficients on the interaction term are all small in magnitude and statistically insignificant.[25]

The second issue we investigate is whether teachers assigned to "hard-to-staff" school districts were more likely to exit post reform. As noted previously, we proxy for hard-to-staff districts using pre-reform measures of the fraction of free or reduced price lunch students, average student performance on the ACT exam, and dropout rates. Panel B of Table 4 reports results based on specifications where we interact the exposure indicator with the fraction of free or reduced-price lunch students. There we find strong evidence that teachers assigned to districts with higher shares of free- or reduced-price lunch students were more likely to exit post exposure to the reforms: all of the estimated coefficients on the interaction terms are positive, relatively large in magnitude and statistically significant. In terms of magnitude, the results reported in column 1 suggest that teachers assigned to districts where all students were eligible for a free- or reduced-price lunch were 2.8 percentage points (-0.01+0.038) more likely to exit post reform than similar teachers located in districts with no free- or reduced-price lunch students.

In panel C we replace the interaction between the exposure indicator and the fraction of free- or reduced-price lunch students with an interaction between the exposure indicator and standardized average district ACT scores. For ease of interpretation we multiply the

[25] We also estimated models that replace the 2009 county unemployment rates with the average county unemployment rate between 2008 and 2010. Results based on this alternative specification yielded results that were qualitatively and quantitatively similar to those reported in Table 4. Results are available upon request.

"Teachers assigned to districts with lower-performing students were more likely to exit Michigan schools post exposure to the reforms."

standardized ACT scores by minus one to ensure that larger values of the index represent districts with lower performing students. Once again, we find that all of the estimated coefficients on the interaction terms are positive and statistically significant, suggesting that teachers assigned to districts with lower-performing students were more likely to exit Michigan schools post exposure to the reforms. Finally, as shown in panel D, we find similar results when we proxy for hard-to-staff districts using district dropout rates.

One concern with the results reported in Panels B-D of Table 4 is that we are interacting the exposure indicator with a continuous variable and thus imposing linearity on how higher exposure to free or reduced-price lunch students, lower performing students or higher dropout rates affects attrition rates. As a sensitivity check of our results, we therefore also estimated models where we interacted the exposure index with an indicator variable that takes the value of unity if a district is at or above the 75th percentile of free- or reduced-price lunch students or dropout rates or at or below the 25th percentile of standardized average district ACT scores. The results of this exercise are reported in Table 4A of the Appendix. Similar to the results reported in Table 4, we find that all of the estimated coefficients on the interaction terms are positive and statistically significant. In terms of magnitude our results suggest that teachers assigned to districts at or above the 75th percentile of free or reduced price lunch students, were between 1.1 (column 1) and 1.7 (column 3) percentage points more likely to exit post reform than teachers in districts with fewer disadvantage students. Specifications based on average ACT scores or dropout rates yield estimates of similar magnitude.

These estimates also compare quite closely to the attrition rates we would predict from Table 4. Specifically, the average fraction of students eligible for free- or reduced-price lunch in districts at or above the 75th percentile of free lunch eligible districts is 0.53. Applying that fraction to the estimates reported in column 1 of Table 4 yields a predicted post-reform attrition rate among teachers in high free lunch districts of 0.015, an estimate identical to the one reported in column 1 of Table 4A. Similarly, the average dropout rate in districts at or above the 75th percentile of dropout rates is 0.24. Applying that fraction of dropouts to the estimates reported in column 1 of Table 4, yields a predicted post-reform attrition rate among teachers in high dropout districts of 0.015, an estimate quite similar to the estimate of 0.013 reported in column 1 of Table 4A.

Panels E and F of Table 4 report estimates based on equation (4) that examine whether certified STEM teachers or teachers that graduated from more selective colleges or universities

were more likely to exit post reform. Specifically, in Panel E we interact the exposure indicator with an indicator for whether a teacher is a certified secondary STEM teacher and in Panel F we interact the exposure indicator with a standardized (mean 0, standard deviation 1) measure of the 75th percentile ACT score of incoming undergraduate students at the college or university that a teacher attended. In both panels E and F we find little evidence that STEM teachers or teachers that attended more selective colleges were more likely to exit the teaching profession post reform. All of the estimated coefficients in panels E and F are small in magnitude and statistically insignificant.

In Table 5A of the Appendix we provide a further robustness check for the results presented in Table 4 by adding district-specific linear time trends to all of the specifications reported in Table 4. Turning first to the results based on the fraction of free- and reduced-price lunch students, we find that all of our estimates are robust to the inclusion of district-specific linear time trends. Specifically, all of the estimates reported in Panel B of Table 5A are similar in magnitude to those reported in Table 4 and statistically significant. In panels C (average district ACT scores) and D (dropout rates) all of the estimated coefficients on the interaction terms remain positive, although the magnitude of the estimates for the 1-5 experience groups decline slightly in magnitude and become statistically insignificant. This is likely due to the fact that including district-specific trends substantially increases the standard errors with more than 450 additional parameters to estimate, but in general the results of this check is quite supportive of the results in Table 4.

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 whether or how our results change if we include Detroit. In Table 6A of the Appendix we present results based on models identical to those reported in Table 4 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 interaction terms in the high poverty and low-performing district specifications are larger in magnitude in Table 6A. Again, these results are not surprising given both the high concentration of low-income students in Detroit and the financial problems the district faced.

D. Falsification Tests

In this section we present falsification tests for our core findings related to post-reform exit rates in hard-to-staff districts. In our first falsification test we create a pseudo CBA

Table 5: Falsification Estimates for CBA Expiration Timing Based on Moving Back Expiration Date 4 Years

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
		Panel A: Fraction Free Lunch		
Exposed	0.003	0.003	-0.002	0.007
	(0.003)	(0.005)	(0.003)	(0.005)
Exposed * Free Lunch	0.002	0.002	0.010	0.001
	(0.007)	(0.009)	(0.008)	(0.010)
		Panel B: Standardized ACT Test Scores		
Exposed	0.003	0.003	0.001	0.007**
	(0.003)	(0.004)	(0.002)	(0.004)
Exposed * Test Scores	0.000	0.000	0.002	0.001
	(0.001)	(0.002)	(0.002)	(0.002)
		Panel C: Dropout Rates		
Exposed	0.003	0.004	-0.001	0.005
	(0.003)	(0.004)	(0.003)	(0.005)
Exposed * Dropout Rate	-0.000	-0.008	0.017	0.020
	(0.016)	(0.020)	(0.018)	(0.027)
Observations	480,709	297,149	102,407	81,152

Notes: Table presents falsification estimates for results reported in Table 4. We create a placebo CBA expiration date by moving the actual CBA expiration date back 4 years and then dropping all observations for actual post CBA expiration dates. Each column presents results from a separate regression for the sample of teachers listed in the top row. District student ACT scores (panel B) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables plus district and year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

expiration date by moving the actual expiration date of pre-reform CBA contracts back four years and then dropping all observations associated with years when a pre-reform CBA actually expired. We then re-estimate the specifications reported in Table 4 treating the pseudo CBA expiration date as the actual date. Results from this falsification test are reported in Table 5.

Consistent with our Table 4 results having a causal interpretation, the estimates from these falsification tests are statistically insignificant and small in magnitude. Our second falsification test is identical to our first test except rather than moving the actual expiration date of pre-reform CBA contracts back four years, we move it back three years (and drop all

observations associated with years when a pre-reform CBA actually expired).[26] Results are reported in Table 7A of the Appendix. Similar to the results reported in Table 5, the estimates from these falsification tests are small in magnitude relative to those reported in Table 4 and all but two are statistically insignificant. Furthermore, in the two cases where we do find a positive and statistically significant estimate (column 3 panels B and C), the estimates are small in magnitude and only marginally statistically significant.

In our third falsification test we once again move the actual expiration date of pre-reform CBA contracts back three years but this time add district-specific linear time trends to all specifications. As shown in Table 8A of the Appendix, across all columns and panels the estimates from this falsification test are statistically insignificant and mostly of the opposite sign compared to those presented in Table 4.

In our final falsification test, we replace the sample of teachers used in our Table 4 results, with a sample of professional staff that work in the same schools and districts. Specifically, we focus on non-instructional professional staff from the following occupations: 1) school guidance counselors; 2) social workers; 3) accountants/bookkeepers; 4) nurses and other health services workers; 5) occupational and physical therapists; 6) school psychologists and 7) audiologists.[27] All of these occupations require similar educational attainment and certification requirements as teachers and also are directly involved in servicing students. Importantly, however, professional staff were not subject to the 2011 teacher reforms. Using this sample of professional staff we then estimate models identical to those reported in Table 4.

Results are reported in Table 6. In the interest of brevity we once again only report estimated coefficients on the exposure indicator and that indicator interacted with our various proxies for hard-to-staff school districts but note that all of the specifications reported in Table 6 include the full set of individual control variables listed in Table 1.[28] As shown in Table 6, all of the estimated coefficients on the interaction terms are small in magnitude relative to the results reported in Table 4 and all of the estimates are statistically insignificant. Thus, we find no evidence that professional staff who work in the same school districts as teachers were more likely to exit post-reform, even if they were assigned to a hard-to-staff district.

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[26] We conduct this second falsification test to ensure that results from our first test were not being driven by the year we chose for the falsification test and to allow a long enough pre-reform period to estimate falsification models that include district-specific linear time trends as in Table 5A.

[27] 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.

[28] Because the sample of staff with the least years of experience is significantly smaller than the sample of teachers, we expand the sample of professional staff for the 1-5 years of experience group to include staff with 1-7 years of experience.

Table 6: Falsification Estimates for CBA Expiration Timing Based on Sample of Professional Staff

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
		Panel A: Fraction Free Lunch		
Exposed	-0.006	-0.008	-0.009	0.007
	(0.007)	(0.010)	(0.014)	(0.017)
Exposed * Free Lunch	-0.012	-0.013	-0.016	-0.016
	(0.017)	(0.019)	(0.033)	(0.036)
		Panel B: Standardized ACT Test Scores		
Exposed	-0.010	-0.012	-0.012	0.003
	(0.006)	(0.007)	(0.012)	(0.014)
Exposed * Test Scores	-0.002	-0.003	0.002	0.000
	(0.003)	(0.004)	(0.005)	(0.006)
		Panel C: Dropout Rates		
Exposed	-0.013*	-0.016*	-0.012	0.004
	(0.007)	(0.009)	(0.014)	(0.016)
Exposed * Dropout Rate	0.028	0.034	-0.008	-0.007
	(0.039)	(0.042)	(0.067)	(0.074)
Observations	40,616	25,258	10,166	10,856

Notes: Table presents falsification estimates for results reported in Table 4. We replace the sample of teachers in each experience bin with the sample of school professional staff in each experience bin. Professional staff include staff working in the following occupations: 1), guidance counselor, 2) school nurse, 3) social worker, 4) accounting, 5) physical and occupational therapist, 6) school audiologist and school psychologist. Each column presents results from a separate regression for the sample of teachers listed in the top row. District student ACT scores (panel B) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables plus district and year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Overall, the results of our falsification tests provide compelling evidence that our Table 4 results have a causal interpretation. Across the 48 falsification tests reported in Tables 5, 6, 7A and 8A, two of the estimated coefficients on the interaction terms are statistically significant and even in those cases they are small in magnitude and only marginally significant at the 10 percent level.

E. CBA Expiration Timing or Reform Timing?

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. To examine that possibility, we first dropped all observations from 2011 through 2013 for teachers in districts that had a pre-reform CBA that expired in 2011 and thus were immediately treated by the reforms. We then created a pseudo expiration date for all other pre-reform CBA expirations by moving the expiration dates back to 2011. Thus, we are essentially assuming that all teachers were exposed to the reforms immediately in 2011 rather than being exposed only when their district's pre-reform CBA expired.

Results based on this falsification test are reported in Table 7.[29] In columns 1-3, which pertain to teachers with six or more years of experience, we find no evidence that teachers responded to the reforms immediately in 2011: the estimated coefficients on the interaction terms in Table 7 are relatively small in magnitude and statistically insignificant for these experience groups. In contrast, for teachers with fewer than six years of experience (column 4) we find estimates that, while noisy, are of comparable magnitude to those reported in column 4 of Table 4.

Thus, our results suggest that early career (pre-tenure) teachers appear to have responded to the reforms immediately when they were implemented in 2011. Recall that 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. More importantly, the teacher evaluation reforms only affected teachers once a district's pre-reform CBA expired. In contrast, pre-tenure teachers were immediately affected by 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, in contrast to more experienced teachers, pre-tenure teachers actually were immediately impacted by the 2011 reforms. As such, it is not surprising that it is among these teachers that we observe an immediate response to the policy reforms.

[29] Note that the specifications reported in Table 7 do not include an indicator for the direct effect of exposure since we now assume that all teachers were exposed in 2011. As a result, the direct effect of exposure is absorbed by the year fixed effects.

Table 7: Falsification Estimates for CBA Expiration Timing Based on Moving 2012 and 2013 CBA Expirations back to 2011

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5	Exp. 1 - 5	Exp. 1 - 5
			Panel A: Fraction Free Lunch			
Exposed * Free Lunch	0.011	0.011	0.003	0.026	0.034***	-0.005
	(0.008)	(0.013)	(0.012)	(0.024)	(0.013)	(0.013)
			Panel B: Standardized ACT Test Scores			
Exposed * Test Scores	0.001	-0.000	0.001	0.006	0.008***	0.001
	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)	(0.003)
			Panel C: Dropout Rates			
Exposed * Dropout Rate	0.017	0.005	0.016	0.084	0.076**	0.005
	(0.024)	(0.023)	(0.021)	(0.099)	(0.038)	(0.026)
Observations	480,709	297,149	102,407	81,152	100,036	76,860

Notes: Table examines whether teachers responded to the reforms only once their district's pre-reform CBA expired or immediately upon implementation of the 2011 reforms. We drop observations from 2011-2013 for pre-reform CBAs that expired in 2011. We then create a placebo CBA expiration date for the pre-reform CBAs that expired in 2012 or later by moving those pre-reform CBA expiration dates back to 2011. Finally, we drop 2012 and 2013 observations for CBAs that really expired in 2012 and we drop 2013 observations for CBAs that expired in 2013. Column 5 sets the year of exposure to reforms to 2011 for all districts. Column 6 presents falsification estimates for the column 5 results by moving the date of the reforms back to 2008. District student ACT scores (panel B) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables plus district and year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

To provide further evidence that pre-tenure teachers responded immediately to the reforms, in column 5 of Table 7, we set the year of exposure to reforms to 2011 for teachers in all districts, regardless of whether their district's pre-reform CBA expired in 2011 or some later year. In this specification we find strong evidence that pre-tenure teachers were more likely to exit the teaching profession post-reform if they were assigned to a hard-to-staff school district. In column 6 we present falsification estimates for our column 5 results by moving the date of the reforms back to 2008 and dropping all observations after the true reform date of 2011. There we once again find no evidence that teachers in hard-to-staff districts were more likely to exit post the pseudo reform year. Again, these results provide relatively strong evidence that our column 5 results have a causal interpretation.

F. Teacher Compensation

The results reported in Tables 3 and 4 provide evidence that Michigan's teacher accountability reforms had little impact on teacher attrition on average, but they led to increased attrition among teachers assigned to hard-to-staff districts. In this section we explore a final explanation for these results. Specifically, one possible explanation for why, on average, teachers were no more likely to exit the teaching profession in the post-reform period, is that 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.[30]

Salary step summary statistics for the districts in our sample are presented in Table 8. The first column of the table lists 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, and Flint areas.

Finally, columns 5 and 6 present the pre/post-reform change in nominal and real salaries. Even when measured in nominal dollars, four out of eight 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 of the changes in the salary steps are negative ranging from a decline 4.5% of to a decline of 6.5%. Thus, we find no evidence that school districts increased teacher compensation in the post-reform period; if anything, our results suggest that teacher compensation actually declined post-reform.

[30] The salary schedules are contained in each district's collective bargaining agreement (CBA) which we obtained from the Mackinac Center for Public Policy. CBA contracts typically remain in effect for two to three years before being renegotiated. The majority of pre-reform CBAs in our sample ended in 2010 or 2011.

Table 8: Mean Salary Steps for Teachers Pre- and Post-Reform

	(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.

Nonetheless, it is still possible that our Table 4 results are being driven by the fact that hard-to-staff districts saw even larger reductions in teacher compensation post-reform than those reported in Table 8, which could lead to relative increases in attrition. To examine that possibility, we estimated DD models that are identical to those reported in Table 4, except we used the natural log of teacher salaries as the dependent variable.[31]

Results are reported in Table 9. Across all columns and panels of Table 9, the estimated coefficients on the interaction terms are small in magnitude and with one exception, statistically insignificant. Thus, we find little evidence that teachers in hard-to-staff districts experienced larger reductions in teacher compensation than teachers in other districts.

[31] The salary data we employ come from the administrative records provided by the Michigan Department of Education (MDE) and represent a teacher's "base salary" for a given credential level on the district's salary schedule. We also added teacher experience and experience squared to the vector of control variables to account for variation in teacher salaries related to years of teaching experience.

Table 9: DD Salary Estimates for Various Subgroups Based on CBA Expiration Timing

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
		Panel A: Fraction Free Lunch		
Exposed	-0.024	-0.025	-0.012	-0.003
	(0.028)	(0.031)	(0.020)	(0.009)
Exposed * Free Lunch	0.053	0.058	0.010	-0.004
	(0.055)	(0.059)	(0.044)	(0.023)
		Panel B: Standardized ACT Test Scores		
Exposed	-0.010	-0.009	-0.011	-0.004
	(0.019)	(0.022)	(0.013)	(0.006)
Exposed * Test Scores	-0.001	-0.002	-0.008	-0.008*
	(0.009)	(0.010)	(0.007)	(0.004)
		Panel C: Dropout Rates		
Exposed	-0.007	-0.008	-0.004	0.004
	(0.024)	(0.027)	(0.017)	(0.008)
Exposed * Dropout Rate	-0.020	-0.011	-0.054	-0.067
	(0.104)	(0.114)	(0.087)	(0.052)
Observations	496,722	323,196	99,153	74,356

Notes: Dependent variable in all specifications is the log of teacher salaries. Each column presents results from a separate regression for the sample of teachers listed in the top row. District student ACT scores (panel B) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables plus district and year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

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.

"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…"

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 initial ITS analyses show that teachers are more likely to exit the profession in the years following Michigan's 2011 reform package. However, when we turn to our main analyses, which focus on a variety of difference-in-differences specifications exploiting 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), we conclude that overall, teachers did not exit Michigan schools differently depending on when the reforms themselves took effect in their districts. Thus, our primary conclusion is that, although teachers 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 hard-to-staff schools (proxied by poverty rates, student performance and dropout rates) were more likely to exit the public school system after the reforms than their peers in wealthier and higher-performing districts. Second, we also find suggestive evidence that early career, pre-tenure teachers appear to have responded to the reforms immediately when they were implemented in 2011, rather than when their CBAs were renegotiated to include the contract-specific reforms. Most of these early career teachers were immediately affected by an increase in the length of the tenure period and the requirement that new teachers receive three consecutive performance ratings of "effective" to earn tenure protections. Thus, in contrast to more experienced teachers, pre-tenure teachers actually were immediately impacted by the 2011 reforms and it is among these teachers that we observe a corresponding response to the policy reforms.

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 teachers overall 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 they result in a large loss (at least in the short run) of teachers from the profession.

"… (and) 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. "

However, our results regarding teachers in hard-to-staff 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. Although we do not have data describing the direct effectiveness of exiting teachers, we do find suggestive evidence that these reforms do not differentially affect teachers who come from higher- or lower-quality educator preparation programs.

This suggests that the Michigan reforms increased attrition from hard-to-staff school districts, perhaps without shifting the quality of the teacher pool in these districts. If so, then 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. This is especially important as the Supreme Court tackles the issue of collective bargaining rights and dues collection in the current docket.[32] 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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[32] The U.S. Supreme Court is currently taking up the question of agency fees in the case Janus vs. American Federation of State, County and Municipal Employees Council 31.

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APPENDIX

Table 1A: Complete Regression Results CBA Expiration Timing

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
Trend	-0.000	-0.001*	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.001)
2011	0.007***	0.009***	0.004	0.003
	(0.002)	(0.002)	(0.002)	(0.004)
2012	0.014***	0.018***	0.003	0.010**
	(0.002)	(0.003)	(0.003)	(0.004)
2013	0.014***	0.017***	0.006*	0.010**
	(0.002)	(0.003)	(0.003)	(0.005)
Age	0.030***	0.016***	0.039***	0.019***
	(0.002)	(0.005)	(0.005)	(0.005)
Age Squared	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Age Cubed	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Masters Degree or More	-0.003***	-0.002*	-0.004***	0.006***
	(0.001)	(0.001)	(0.001)	(0.002)
Female	0.001**	-0.001	0.006***	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)
Black	0.013***	0.009**	0.017***	0.023***
	(0.003)	(0.004)	(0.004)	(0.008)
Hispanic	0.005*	0.001	0.004	0.012*
	(0.003)	(0.004)	(0.005)	(0.006)
Other Race	0.011***	0.003	-0.001	0.039***
	(0.004)	(0.005)	(0.005)	(0.010)

Retirement Incentive Eligible	0.242***	0.240***		
	(0.010)	(0.010)		
Observations	646,516	417,198	128,392	100,926

Notes: Table presents complete set of estimates for results presented in Panel 1 of Table 2. 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 of Probability of Teacher Attrition: Pre-Reform CBA Expiration Timing, Including Detroit

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
		Panel A: Baseline		
Exposed	-0.010	-0.014	0.000	-0.001
	(0.009)	(0.011)	(0.004)	(0.006)
		Panel B: Heterogeneous Treatment Effects		
Exposed 1st year	-0.010	-0.014	0.002	0.000
	(0.010)	(0.012)	(0.004)	(0.007)
Exposed 2nd year	-0.015**	-0.019**	-0.006	-0.006
	(0.008)	(0.009)	(0.005)	(0.008)
Exposed 3rd Year	-0.020*	-0.027*	-0.006	-0.002
	(0.012)	(0.014)	(0.007)	(0.013)
Observations	687,566	449,208	135,421	102,937

Notes: Table presents DD linear probability model estimates for the probability of teacher exit. Sample includes Detroit school district. Exposed is an indicator variable that takes the value of unity for all years after a school district's pre-reform CBA expired. 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 individual control variables plus district and year fixed effects. Bottom panel replaces the exposed indicator with separate treatment year indicators. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 3A: DD Estimates of Probability of Teacher Attrition: Pre-Reform CBA Expiration Timing Dropping Districts with Pre-Reform CBA Expiration Prior to 2011

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
		Panel A: Baseline		
Exposed	-0.001	-0.002	0.003	0.001
	(0.002)	(0.003)	(0.003)	(0.006)
		Panel B: Heterogeneous Treatment Effects		
Exposed 1st year	0.000	-0.001	0.004	0.003
	(0.002)	(0.003)	(0.003)	(0.006)
Exposed 2nd year	-0.007**	-0.008**	-0.004	-0.006
	(0.003)	(0.004)	(0.005)	(0.008)
Exposed 3rd Year	-0.007	-0.010	-0.004	0.005
	(0.005)	(0.006)	(0.007)	(0.011)
Observations	492,660	317,162	97,902	77,596

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. Sample is restricted to districts with pre-reform CBAs that expired after 2010. 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 individual control variables plus district and year fixed effects. Bottom panel replaces the exposed indicator with separate treatment year indicators. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 4A: DD Estimates for Various Subgroups
Based on CBA Expiration Timing, Using Treatment Indicators for 75th Percentile

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
	Panel A: At or Above 75th Percentile of Fraction Free Lunch			
Exposed	-0.004	-0.005*	0.000	0.000
	(0.002)	(0.003)	(0.003)	(0.005)
Exposed * Free Lunch	0.015***	0.015***	0.017***	0.011*
	(0.003)	(0.004)	(0.004)	(0.006)
	Panel B: At or Below 25th Percentile of ACT Test Scores			
Exposed	-0.003	-0.005*	0.001	-0.001
	(0.002)	(0.003)	(0.003)	(0.006)
Exposed * Test Scores	0.013***	0.014***	0.013***	0.014**
	(0.003)	(0.004)	(0.004)	(0.006)
	Panel C: At or Above 75th Percentile of Dropout Rates			
Exposed	-0.003	-0.004	0.000	-0.000
	(0.002)	(0.003)	(0.003)	(0.005)
Exposed * Dropout Rate	0.013***	0.012***	0.021***	0.013*
	(0.004)	(0.004)	(0.005)	(0.007)
Observations	642,401	414,832	127,533	100,036

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. Exposed * (X) denotes interaction between Exposed and indicators for being at or above the 75th (or at or below 25th) percentile of X. 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 individual control variables plus district and year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 5A: DD Estimates for Various Subgroups
Based on CBA Expiration Timing with District-Specific Linear Time Trends

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
	Panel A: Standardized 2009 County Unemployment Rate			
Exposed	0.002	0.000	0.005	0.004
	(0.003)	(0.004)	(0.004)	(0.006)
Exposed * Unemployment Rate	-0.000	0.001	-0.001	-0.004
	(0.001)	(0.002)	(0.002)	(0.003)
	Panel B: Fraction Free Lunch			
Exposed	-0.012***	-0.014***	-0.005	-0.006
	(0.003)	(0.005)	(0.005)	(0.007)
Exposed * Free Lunch	0.048***	0.051***	0.042***	0.037*
	(0.010)	(0.013)	(0.014)	(0.019)
	Panel C: Standardized ACT Test Scores (District Students)			
Exposed	0.002	0.000	0.006	0.004
	(0.003)	(0.004)	(0.004)	(0.006)
Exposed * Test Scores	0.007***	0.008***	0.005*	0.005
	(0.002)	(0.003)	(0.003)	(0.004)
	Panel D: Dropout Rates			
Exposed	-0.008**	-0.010**	-0.004	-0.001
	(0.003)	(0.004)	(0.005)	(0.007)
Exposed * Dropout Rate	0.087***	0.096***	0.092***	0.040
	(0.027)	(0.033)	(0.032)	(0.041)
	Panel E: Secondary STEM Teachers			
Exposed	0.001	0.000	0.005	0.004
	(0.003)	(0.004)	(0.004)	(0.006)
Exposed * Secondary STEM Teacher	0.002	0.000	-0.003	-0.001
	(0.004)	(0.006)	(0.011)	(0.006)

	Panel F: Standardized 75th Percentile ACT Score Colleges			
Exposed	0.002	0.002	0.006*	0.002
	(0.003)	(0.004)	(0.004)	(0.006)
Exposed * ACT75th	0.000	-0.000	0.001	0.003
	(0.001)	(0.001)	(0.002)	(0.002)
Observations	642,401	414,832	127,533	100,036

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. Each column presents results from a separate regression for the sample of teachers listed in the top row. District student ACT scores (panel C) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables, district and year fixed effects and district-specific linear time trends. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 6A: DD Estimates for Various Subgroups
Based on CBA Expiration Timing Including Detroit

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 -9	Exp. 1 - 5
	Panel A: Standardized 2009 County Unemployment Rate			
Exposed	-0.010	-0.014	0.000	-0.001
	(0.009)	(0.011)	(0.004)	(0.006)
Exposed * Unemployment Rate	0.001	0.001	0.001	0.002
	(0.001)	(0.001)	(0.002)	(0.003)
	Panel B: Fraction Free Lunch			
Exposed	-0.021**	-0.025**	-0.010**	-0.015
	(0.010)	(0.012)	(0.005)	(0.009)
Exposed * Free Lunch	0.038***	0.036***	0.042***	0.048***
	(0.006)	(0.007)	(0.008)	(0.017)
	Panel C: Standardized ACT Test Scores (District Students)			
Exposed	-0.010	-0.014	0.002	-0.000
	(0.010)	(0.012)	(0.004)	(0.006)
Exposed * Test Scores	0.007***	0.006***	0.008***	0.011***
	(0.001)	(0.002)	(0.002)	(0.003)
	Panel D: Dropout Rates			
Exposed	-0.019*	-0.022*	-0.012**	-0.013
	(0.010)	(0.012)	(0.005)	(0.009)
Exposed * Dropout Rate	0.075***	0.064***	0.111***	0.096**
	(0.016)	(0.019)	(0.019)	(0.038)
	Panel E: Secondary STEM Teachers			
Exposed	-0.010	-0.014	0.000	-0.001
	(0.009)	(0.011)	(0.004)	(0.006)
Exposed * Secondary STEM Teacher	0.002	-0.002	0.001	-0.001
	(0.004)	(0.006)	(0.011)	(0.006)
	Panel F: Standardized 75th Percentile ACT Score Colleges			

Exposed	-0.009	-0.012	0.000	0.001
	(0.009)	(0.011)	(0.004)	(0.006)
Exposed * ACT75th	0.000	0.000	-0.000	0.004
	(0.001)	(0.001)	(0.002)	(0.003)
Observations	687,566	449,208	135,421	102,937

Notes: Table presents DD linear probability model estimates for the probability of teacher exit. Sample includes Detroit school district. Exposed is an indicator variable that takes the value of unity for all years after a school district's pre-reform CBA expired. Each column presents results from a separate regression for the sample of teachers listed in the top row. District student ACT scores (panel C) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables plus district and year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7A: Falsification Estimates for CBA Expiration Timing Based on Moving Back Expiration Date 3 Years

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
		Panel A: Fraction Free Lunch		
Exposed	0.002	0.004	-0.003	-0.004
	(0.004)	(0.006)	(0.004)	(0.006)
Exposed * Free Lunch	-0.003	-0.004	0.008	-0.010
	(0.006)	(0.008)	(0.007)	(0.012)
		Panel B: Standardized ACT Test Scores		
Exposed	0.001	0.003	-0.000	-0.006
	(0.004)	(0.006)	(0.003)	(0.004)
Exposed * Test Scores	-0.000	-0.001	0.003*	0.001
	(0.001)	(0.001)	(0.001)	(0.003)
		Panel C: Dropout Rates		
Exposed	0.001	0.004	-0.003	-0.007
	-0.004	-0.006	-0.004	-0.006
Exposed * Dropout Rate	-0.002	-0.011	0.026*	0.009
	-0.014	-0.018	-0.014	-0.034
Observations	480,709	297,149	102,407	81,152

Notes: Table presents falsification estimates for results reported in Table 4. We create a placebo CBA expiration date by moving the actual CBA expiration date back 3 years and then dropping all observations for actual post CBA expiration dates. Each column presents results from a separate regression for the sample of teachers listed in the top row. District student ACT scores (panel B) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables plus district and year fixed effects. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8A: Falsification Estimates for CBA Expiration Timing Based on Moving Back Expiration Date 3 Years with District-Specific Linear Time Trends

	(1)	(2)	(3)	(4)
	All	Exp. 10 plus	Exp. 6 - 9	Exp. 1 - 5
		Panel A: Fraction Free Lunch		
Exposed	0.002	0.003	-0.001	-0.001
	(0.005)	(0.008)	(0.004)	(0.007)
Exposed * Free Lunch	-0.006	-0.001	0.000	-0.028
	(0.019)	(0.026)	(0.016)	(0.023)
		Panel B: Standardized ACT Test Scores		
Exposed	-0.000	0.002	-0.001	-0.008*
	(0.004)	(0.006)	(0.004)	(0.005)
Exposed * Test Scores	-0.003	-0.004	0.000	-0.003
	(0.003)	(0.004)	(0.003)	(0.004)
		Panel C: Dropout Rates		
Exposed	0.001	0.002	-0.000	-0.006
	(0.005)	(0.007)	(0.004)	(0.007)
Exposed * Dropout Rate	-0.009	-0.001	-0.007	-0.018
	(0.041)	(0.055)	(0.034)	(0.056)
Observations	480,709	297,149	102,407	81,152

Notes: Table presents falsification estimates for results reported in Table 4. We create a placebo CBA expiration date by moving the actual CBA expiration date back 3 years and then dropping all observations for actual post CBA expiration dates. Each column presents results from a separate regression for the sample of teachers listed in the top row. District student ACT scores (panel B) are multiplied by -1 to reflect the same sign as free lunch and dropout rates. All specifications include the full set of individual control variables, district and year fixed effects and district-specific linear time trends. Robust standard errors clustered at the school district level in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.