

Elect Your Own Employer! Endogenous Election Timing or Interest Group Advantage?

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Abstract

Is public policy affected by the time of year in which elections are held? Yes, say scholars of state and local politics, who routinely assert that “off-cycle” elections depress participation among marginal voters and in turn advantage organized interest groups whose members turnout regardless election date. For example, school districts that use off-cycle elections compensate their employees more generously, it is argued, because teacher union interest groups face significantly less competition in low-turnout elections (Berry and Gersen 2010; Anzia 2011). However, such claims fail to consider the unobserved factors that led politicians to choose off cycle election dates in the first place. If those unobserved factors correlate with the outcomes one wishes to attribute to variation in the election calendar, unbiased identification becomes impossible. We model this type of bias directly—by examining the consequences of a 2004 reform in Michigan election law that *required* school districts to publicly reveal their preferred election date at a single point in time. Using propensity score matching, (PSM) we compare policy outcomes important to teacher interest groups in a sample of school districts that differed only in their choice of on- or off-cycle elections. Although our substantive estimates are similar in magnitude to previous studies, our findings cast doubt on the causal significance of election timing itself. Because we theorize that a school district’s preferred election date reflects its underlying attitude toward teachers unions, we content analyze teacher contracts in our sample of matched districts to see whether a latent “union sympathy” helps explain district choice in election date. We find that teacher contracts negotiated in 2004-2005 by boards that simultaneously revealed a preference for off-cycle elections were significantly more union-friendly than the contracts negotiated by boards in similar districts that voted for on-cycle elections. These results demonstrate that the choice of election timing is far from randomly determined and is often endogenous to a researcher’s main outcome of interest. Consequently, scholars should pay careful attention to the historical and political context of election reform in addition to explaining precisely what they claim to be able to measure when leveraging natural variation in an election calendar to explain policy outcomes.

Background and motivation

Public education in the United States represents our largest collective investment specifically targeting the nation's youth. Both the size of the target population and the total cost is substantial. As of the 2006-07 school year total expenditures on public education exceeded \$562 billion, which represented nearly 4 percent of the total U.S. gross domestic product that year. The public investment in education is large, though the size is justifiable because education is a key determinant of many important societal outcomes including: wages, unemployment, political and civic participation, incarceration rates, and national economic growth (e.g., Nie, Junn, and Stehlik-Barry 1996; Farkas and Vicknair 1996; Heckman and Masterov 2007; Hanushek and Woessmann, 2009).

Despite these extraordinary investments, educational outcomes in the U.S. generally rank poorly among other industrialized countries across multiple dimensions. Recent research has shown that even the highest-performing students and states in the U.S. lag behind other developed countries (Hanushek et al. 2011); which suggests the production of education in the U.S. is relatively economically inefficient. While many different factors contribute to students' cognitive development (and hence, education) over time, formal schooling is a major component in the process of education production. And, of all the various schooling inputs involved, the quality of a child's teacher makes the largest contribution to learning outcomes (Sanders and Rivers 1996; Goldhaber 2002; Rivkin et. al. 2005; Chetty et al. 2011).¹ Not surprisingly, the top-performing school systems in the world share one variable in common: they all recruit 100 percent of their teacher workforce from the most accomplished one-third of their college

¹ Hanushek and Rivkin (2004, 21) note that, "having five years of good teachers in a row (one standard deviation above average) could overcome the average seventh grade mathematics achievement gap between lower income kids and those from higher income families." Chetty et al. (2011) find that, "Students assigned to [highly effective] teachers are more likely to attend college, attend higher-ranked colleges, earn higher salaries, live in higher SES neighborhoods, and save more for retirement. They are also less likely to have children as teenagers."

graduates (McKinsey & Company 2007). Yet, this is no easy task to mirror in the U.S. where nearly half of American teachers are drawn from the bottom third of their graduating class (McKinsey & Company 2010).

To address these shortcomings in the quality of the American teaching workforce, elected officials have begun to spend significant time and political capital reforming the policies that govern the teaching profession (e.g. teacher pay, evaluation, tenure). Moreover, given the current climate of tight budgets and fiscal strain politicians have begun to target long-standing teacher compensation and benefit policies as both costly and inefficient.² For example, in Michigan and New Jersey, Republican governors have proposed that teachers begin to shoulder more of the cost of their own pension and health care benefits. And, G.O.P. leaders in Nevada, New Jersey, Florida, Idaho, South Dakota, and Indiana, recently proposed eliminating or at least radically changing the tenure process to consider a teacher's actual effectiveness in granting permanent job security. Even Democratic lawmakers, traditionally strong allies of the teachers unions, have backed reforms requiring that school districts compensate and evaluate teachers based on classroom effectiveness. As President Obama's Education Secretary, Arne Duncan, recently stated, "School districts pay about \$8 billion each year to teachers because they have masters' degrees even though there is little evidence that teachers with masters degrees improve student achievement more than other teachers..." Meanwhile Secretary Duncan called on school districts to learn how to "do more with less" signaling that the days of automatic raises and free health care benefits for public employees may no longer be here to stay: "Doing more with less

² Compensating America's 4 million-plus teachers accounts for roughly 25 percent of the average state's general fund budget (NASBO, 2008) and shortfalls in public employee pensions (where teachers are almost always the largest cost share) suggest even more long term fiscal pain lies ahead. Teacher pay is almost always a narrow function of experience and education which research suggests are poor proxies for a teacher's actual classroom effectiveness (Goldhaber, 2002; Aaronson et al., 2007; Chingos and Peterson, 2011).

will likely require reshaping teacher compensation to do more to develop, support, and reward excellence and effectiveness, and less to pay people based on paper credentials” (Duncan 2011).

Not surprisingly, the nation’s largest teachers unions have pushed back against the current wave of teacher reform with considerable zeal. After all, as recent events surrounding teacher collective bargaining in Wisconsin illustrate—education reform is at its heart a thorny political issue. Because nearly every major decision about how Americans organize their schools (including teacher policies) is a decision that is ultimately determined by the democratic political process (Chubb and Moe 1990; Moe 2006) teachers and their unions will continue to organize and pursue their interests rather than passively accept major changes to the way their profession is governed without a political fight.

While some research suggests that policy responsiveness in local education politics is not overly biased in favor of organized teacher interests (Berkman and Plutzer 2005), when evaluated through the lens of participation in school politics, the data suggest that the average American voter is significantly detached from the public officials who govern their schools. Of the nearly 15,000 U.S. school districts, thousands stagger their board elections and hold them apart from other state and national elections (off-cycle) at times of the year when only a fraction, let alone a representative group of the district’s citizens are politically engaged (Anzia 2011). Consequently, most board elections generate limited competition and garner turnout below 15 percent (Kirst 2004). Predictably, organized teacher union interest groups try to exert influence in school district politics since they can typically count on minimal public awareness and participation in school board elections (Wirt and Kirst 2001; Moe 2006).³ For example, in Michigan the state’s largest teachers union, the Michigan Education Association (MEA),

³ Moe (2006) estimates that teachers who live and work in the same district are far more likely to turnout in school board elections than teachers who live and work in different locales.

distributes a voter mobilization guide (see Figure 1 below) during school board election season to all of its local teacher union affiliates entitled, *Elect Your Own Employer: It's As Easy as 1,2,3* providing teacher locals with specific strategies for how they can influence the policy making process in school districts by organizing themselves politically in low turnout off-cycle (May) board elections. Among other items, the manual reminds MEA members that because nearly 1 in 50 registered Michigan voters is either a member or past member of the MEA meaning that locals' activism in low-turnout board elections can oftentimes be the difference between school boards sympathetic to their interests (e.g. boosting wages, benefits) or reform-oriented boards intent on dramatically reforming the profession.

[Insert Figure 1 about here]

Election Reform Comes To Michigan

In 2004, citing the need to increase participation in local democracy and re-direct scarce education dollars back into the classroom, the Michigan legislature passed a series of election law reforms that encouraged districts to consolidate their school board elections by moving them to November. Previously, nearly all school board elections in Michigan were “special elections” held in June and paid for entirely by school districts' own funds.⁴ Public Acts 286-300 *required* that Michigan's 552 school districts select from a set of five potential dates in May and November (May Odd Years, May Annual, November Annual, November Odd Years, and November Even Years). Although the law did not expressly require that districts select “on cycle” election dates, it did shine a public spotlight on an increasingly controversial issue in

⁴ According to the officials I spoke with at Michigan's state department this was true for approximately 99 percent of the state's 552 school districts. Although it is *probable* that a handful of districts held non-June elections before 2005, the state department could not identify them. Therefore, I concede some measurement error in the current analysis. However, I plan to survey districts in the near future about their election histories enabling me to strike this handful of problematic cases from my sample.

Michigan local politics—the persistence of unnecessarily expensive “stealth” elections that typically resulted in less than 10 percent turnout.

In this paper, we suggest that Michigan’s consolidated election reforms provide an important testing ground for a growing research literature in election timing that tries to explain interest group advantage in local politics as a function of low-voter turnout in off-cycle elections. Specifically, contemporary debates over school governance and election law reform are in search of an answer to a question with significant real world policy implications: will moving school board elections to on-cycle dates impact education outcomes (both policy and student achievement)? Michigan is an ideal test case for answering this particular question because its change in election law enables us to observe up close and within a uniform window of time the factors associated with a school district’s decision to adopt on-cycle versus off-cycle election dates. As we explain in greater detail below, the failure of previous research in this area to identify the factors that motivate politicians to adopt particular election dates in the first case precludes researchers from accurately estimating the real world effect of moving an election date on-cycle to increase citizen participation in local school politics.

Previous research and theoretical orientation

There is a vast research literature in political science on election timing including studies that consider the relationship between election dates and: turnout (Patterson and Caldeira 1983; Hajnal and Lewis 2003; Caren 2007; Hajnal 2010; Fukumoto, Horiuchi, and Tanaka 2011), interest group advantage (Anzia 2011; Berry and Gersen 2010; Fukumoto and Horiuchi 2009; Meredith 2009), economic conditions under incumbent governments (Smith 1996; Kayser 2005), and election fraud (Fukumoto and Horiuchi 2011). Providing an overview of even a fraction of

this literature is beyond the scope of our study other than to say that scholars, particularly in comparative politics, are well aware that the timing of elections are a contested if not important feature of democracy. It seems fair to say that American politics scholars have historically thought less about election timing as an important predictor of the quality of nature of American democracy since major state and federal U.S. elections are, by law, held on fixed dates not subject to manipulation by the incumbent government. Indeed, in a recent issue of the *American Political Science Review* (Vol. 105, Issue 3, pp. 586-603, August 2011), comparative scholars Kentaro Fukumoto and Yusaku Horiuchi encourage all political scientists, regardless of field, to harness the explanatory power of election timing as an exogenous predictor for a host of important outcomes:

We argue that our approach—‘election timing as treatment’—can be applied to investigate not only this type of electoral fraud but also electoral connections in other countries... If we can regard the election timing as an as-if randomly assigned treatment, as we do in this article, the setup becomes a natural experiment. The senates of the United States and France are promising examples. Even if not, however, electoral timing is still worth exploiting as long as we can control relevant pre-treatment variables by matching or any other proper statistical method.

As if responding directly to Fukumoto and Horiuchi, in recent years, a growing body of research in American politics has begun to make the argument that naturally occurring variation in the timing of elections influences subsequent policy outcomes via a theory of interest group advantage (Anzia 2011; Berry and Gersen 2010; Berry and Gersen, 2011).

Anzia (2011) develops the most theoretically rigorous statement about why we might expect organized interest groups to obtain significant advantage in the policy making process in jurisdictions that employ off-cycle elections. First, she notes that the composition of the electorate in off-cycle elections should be radically different given the significantly higher costs associated with participating in elections held at odd-times of the year. In light of Downs (1957,

254) famous adage that, “those who stand the most to gain [from voting] are the men who earn their incomes there [from the government],” the hypothesis that off-cycle electorates are more likely to include a disproportionate share of public employees who have a personal stake in staying informed and involved makes good sense. Certainly marginal voters who show up for presidential or other major state and national elections are less likely to engage in local elections when they are held at odd-times of the year because they have less of a rational incentive (than public employees and members of other organized interest groups) to participate. Anzia’s second reason for relating off-cycle election timing to interest group advantage is grounded in prior research demonstrating the importance of voter mobilization by parties and interest groups in spawning mass participation (Rosenstone and Hansen 1993). In short, organized interest groups—like public sector employee unions—have clear incentives to mobilize their members in local elections even when they are oddly timed and the costs of turning out unusually high. If anything, organized interests are more likely to benefit by mobilizing members in such elections because they can take for granted the fact that their opposition (taxpayer watchdog groups, the median voter, marginal voters) are less likely to take part in these off-cycle elections.

Although this theory of interest group advantage is plausible if not compelling, unfortunately, the empirical evidence used to argue for a link between off-cycle elections and union-favored policy outcomes raises many more questions than it answers. First, it is problematic that the extant research relies exclusively on a *wide* range of natural variation in election timing that cannot be said to have arisen randomly or consistently. For example, both Anzia (2011) and Berry and Gersen (2010) use present-day variation in school board election dates as the chief explanatory variable in a regression model testing whether off-cycle elections are associated with higher pay for teachers on account of teacher union efficacy in school

districts plagued by low-turnout. The causal pathway that both researchers develop runs from 1) the timing of elections to 2) the composition of the electorate to 3) interest group advantage at the ballot box all the way through to 4) more interest group policy victories. Yet, their empirical evidence is drawn from simple observed differences in election timing without the full knowledge of the historical context and decision-making process that went into each school district original decision about when to hold elections. Specifically, looking across 9 states and within 1 state, Anzia (2011) finds that school districts that have off-cycle elections pay experienced teachers 3 percent more than districts where board elections are concurrent with traditional November races. However, nearly all of the variation in election timing that Anzia relies upon to uncover these estimates occurred during the 1930s when progressives moved to reform elections. Such an extreme lag (70 years) between the initial adoptions of off- versus on-cycle elections makes it difficult to pinpoint precisely what Anzia can said to be measuring at present. Similarly, Berry and Gersen (2010, 2011) also uncover a robust, (though admittedly more modest), relationship between off-cycle election timing and more generous teacher compensation by looking at pay scales and natural variation in election timing in California where a 1982 change in state law *allowed* but did not *require* school districts to make a change to on cycle election dates.⁵

In addition to concerns over the uncertain history that gave rise to the scattered variation in election timing used most election timing research, our chief concern is the implausibility of election timing as an exogenous variable in politics and policy making more generally. While neither Anzia or Berry/Gersen are so cavalier as to suggest that the timing of school board election dates observed today arose for purely random reasons, neither are they able to estimate

⁵ See Table 5 in the Appendix for a side-by-side comparison of the empirical strategy and case selection in this study compared to previous research examining school board election timing and interest group advantage.

the degree to which their outcomes of interest (greater teacher pay) may be correlated with factors that influenced districts' choice of election timing at the outset. Because districts in their samples made shifts to on-cycle elections over a decades-long period, they simply have no feasible way of accounting for differences in why districts chose as they did. Importantly, Anzia and Berry/Gersen are drawing entirely on variation in election timing that was self-selected rather than imposed. In contrast, we argue that Michigan's 2004 election reform law provides a superior test case for the following three reasons:

1. All Michigan school districts were compelled to make some sort of switch thereby revealing an election timing preference;
2. All initial election timing preferences were revealed at a uniform point in time; and,
3. The debate over election timing in Michigan was a relatively straightforward debate between saving money/boosting turnout versus preserving a predictable (though small) electorate.

Consequently, we pursue an empirical strategy that is quite literally designed to model the endogenous selection into treatment (treatment being the choice of an on-cycle election) that previous studies either ignore or try and downplay as inconsequential to their causal story.

Data and empirical strategy

In order to explore the link between the timing of school board elections and our outcomes of interest (teacher pay, education achievement), we first gathered data on districts' choice of election dates in 2004-2005. The Michigan Department of State (Elections Division) furnished us with a complete list of election dates by school district. Our dependent variables of interest are teacher pay (the most straightforward test for interest group advantage) and growth in student achievement (testing whether union influence extends to student learning as some scholars, notably Moe 2009, hypothesize). Teacher pay was measured by coding the generosity

of three points on district salary schedules in 2009: the starting salary for a teacher with a BA and no experience, the starting salary for a teacher with an MA and no experience, and finally the highest grade of pay offered to a veteran teacher (the maximum experience) with an MA. To assess the relationship between student performance and union influence we constructed a school-level data set on student achievement with relevant school-level control variables (e.g. percentage of students in poverty, percentage of non white students) using data from the National Center for Education Statistics Common Core Data and the Michigan Department of Education’s Office of Accountability and Student Achievement. When we turn to modeling student achievement, our unit of observation becomes individual schools, which themselves are nested in our 74 matched treatment and control districts.

Because we theorize that a district’s choice of when to hold its election is endogenous to several factors related directly to our outcomes of interest, OLS estimates are unlikely to identify the causal effect of election timing on interest group advantage. Due to endogeneity concerns in an OLS setup, we employ propensity score matching (PSM) to try and account for the admittedly non-random assignment of on-cycle elections. Specifically, we employed “optimal” matching, which allows us to match each treated district with its most similar control, while taking into account the requirement to provide matches for all other treated districts (Hansen 2004). PSM accounts for the non-random assignment of election timing by comparing the outcomes of Michigan school districts in the treatment and control groups who hold similar pre-treatment characteristics. Rosenbaum and Rubin (1983) define the propensity score as the probability of receiving the treatment conditional on pre-treatment covariates. In particular, let $p(X_i)$ be the probability of district i being assigned to treatment, then

$$p(X_i) = Pr(T_i = 1 | X_i) = E(T_i = 1 | X_i) \quad (2)$$

where T_i is a dummy variable indicating if district i is a district that opted for on-cycle elections. We use a host of school district-level covariates obtained from the National Center on Education Statistics and the Michigan Department of Education to predict the probability of a district choosing an on-cycle election date in 2005-2006. To that end, we obtained historical data 1997-2005 (all prior to districts' mandated response to election timing changes) on student academic achievement, the propensity of voters within each school district to support school bond initiatives, the percentage of voters *at the school district level* who supported Democratic candidates in national and state elections, voter turnout in school board elections, the generosity of salary and benefits for public school teachers, district wealth, demographic composition of the school district, as well as a host of revenue and expenditures data that capture the degree to which school districts are reliant on local property taxes versus federal and/or state contributions.⁶ Then, we have the following proposition -

$$(Y_{i0}, Y_{i1}) \perp\!\!\!\perp T_i \mid p(X_i) \quad (3)$$

where Y_{1i} equals the outcome of district i if treatment were received and Y_{0i} equals the outcome of district i if treatment were not received. In theory, equation 3 says we can now treat the adoption of an on-cycle election as randomly assigned, conditional on observable characteristics.⁷ We can then calculate the average effect of treatment on the treated (ATT) as the following -

$$ATT = E(Y_{1i} \mid D_i = 1, p(X_i)) - E(Y_{0i} \mid D_i = 0, p(X_i)) \quad (4)$$

⁶ Covariates include: Free Lunch Percentage, Expenditures per Student, Percent of Expenditures Spent on Teacher Salaries, Percent Spent on Teacher Benefits, Student-Teacher Ratio, District Partisanship, Student Achievement, Voter Turnout in School Board Elections, Number of School Bonds Proposed, Percent of School Bonds Passed by Voters, Student Enrollment, Median Family Income, Percent Special Education and Percent English Second Language.

⁷ Given evidence we present later in the paper on the relationship between teacher contracts and the adoption of on-cycle elections we are skeptical that our matching results end up approximating randomization.

Once every school district is assigned a propensity score, the data is sorted by the propensity score and strata of observations with similar propensity scores are created. Within each stratum, we test whether the means of the covariates are equal in the treatment and control groups. If they are equal, the balancing property is satisfied. For this study, the data were divided into 6 strata in order for the balancing property to be satisfied. Figure 2 demonstrates that the 37 districts which moved to a November election in the immediate two-year aftermath of the law going into affect (2005 or 2006) all have a highly similar May election district from which we can draw on to provide suitable “control” districts. These matched control districts are those districts that share very similar covariate values to those districts which differ only on account of having moved their election date from off cycle (previously in June) to on cycle (either November Annual or November Even).

[Insert Figure 2 about here]

We assume that the choice between November and May election dates among these matched districts is stochastic. Figure 2 suggests this is a tenable assumption, whereby every November district has a well-matched May election district. The support for the matched districts across all estimated propensity scores supports our assumption that the decision to move your election time *between these matched cohorts* is approximately stochastic.

Figure 3 (below) provides specific indications of the covariate balance before and after matching. These results assure us two things. First, the matching has been largely successful, since matching has resulted in only one large decrease in balance (specifically, the amount of students in a district receiving free or reduced price lunches). All other covariate balances are either superior after matching or insignificantly worse.

[Insert Figure 3 about here]

Second, this plot provides strong evidence for our account of districts engaging in strategic behavior when deciding upon when to conduct their future school board elections. Districts that opted to use May elections (off cycle) spend over \$3000 more per pupil on student instructions. Counter our expectations, though, the districts with the wealthiest families were much more likely to shift to a November election. While each individual match is only a bivariate relationship, and this wealth effect might be simply reflecting some other political expectation common in wealthy school districts, this plot suggests the impulse to preserve district resources by moving to a November election has a muted effect. The important conclusion for our current investigation, though, is that our matching has resulted in a well-balanced set of observations, in which the treatment indicator (movement to a November even year or annual election) is uncorrelated with these relevant controls. Armed with these propensity scores, we proceed with our analysis as follows. We restrict our observations to those 37 treatment districts and their 37 matched controls. We then use our binary treatment indicator as a covariate in a series of ordinary least squares models. While our propensity score matching at the first stage already allows us to defensibly attribute residual differences between the groups to election timing, we can make our models more robust by including additional control covariates.

Before turning to the results of our post-matching regression models, there is one final piece to our analytic strategy for modeling district selection of an on-cycle “treatment” election date. Because we wish to isolate the effect of election timing on interest group advantage we run an additional model for each outcome of interest adding a term on the right hand side that is intended to capture each matched school districts’ underlying attitude toward teachers unions. Specifically, we coded teacher union contracts on two dimensions of restrictiveness for our 37

treatment districts and the 37 matched controls. Appendix B contains the complete codebook summarizing precisely how these measures of restrictiveness were calculated; however, the important point to take away is that this variable is designed to capture each school board's orientation (positive or negative) towards the local teachers union *at nearly the same point in time* that each board revealed its preference in election timing. Table 1 (below) shows the correlation matrix between districts' election timing preference and these factor scores measuring the degree to which boards negotiated teacher-friendly contracts in 2005-2006. The construction of these factor scores was based on theory drawn from existing studies of teacher union contract restrictiveness (Moe 2009) and confirmed through empirics (principal components analysis) so as to ensure that the various items hung sufficiently well together (Cronbach's alpha approaching .7).

[Insert Table 1 about here]

The positive and significant correlations for negotiated items relating to two dimensions of teacher union contracts including teacher working conditions and staffing procedures confirms our overarching point—namely, that school boards which revealed a preference for union-favored off-cycle election dates did so, in part, because these districts were *already* sympathetic to teacher union priorities (as evidenced by the contracts they negotiated). By adding the factor score measuring board support for teachers unions in contract negotiations *prior* to our observed outcomes of interest (teacher pay, student achievement) on the right hand side of our estimating equation, we raise the bar on election timing and reduce the chance that it achieves statistical significance as a predictor of downstream policy outcomes. Again, this decision is based on theory about the endogenous selection into on-cycle treatments: districts that go on cycle, we

hypothesized, already have an underlying propensity to sympathize with the union. And, we suspect that it is precisely that underlying tendency that will explain any differences in our outcomes of interest across treatment and control districts.⁸

Results and discussion

Our first set of post-matching regression models estimate the generosity of school districts' teacher salary schedules as a function of our treatment variable (on-cycle elections) and the median family income for the school district (the baseline model). Table 2 (below) presents the results of our baseline model alongside the more fully specified model incorporating the factor score measuring teacher contract restrictiveness.

[Insert Table 2 about here]

Consistent with prior studies of election timing in school board politics, our treatment districts (those that hold elections on-cycle) are less generous in paying senior teachers on the order of \$5,000 fewer dollars (or about 7 percent less for teachers at the same level of education and experience). Note that although the coefficients for more junior teachers (those with only a BA and zero years experience as well as teachers with masters degrees and no experience), do not quite (they are close) reach statistical significance the effect sizes are fairly large (roughly 3 percent less pay in on-cycle districts). These results seem to confirm the theory that interest groups are advantaged in school districts that hold elections off cycle and suggest that just 4 short years after Michigan school boards revealed their preferred election date, districts that

⁸ One might ask the following: if we hypothesize that a district's underlying attitude toward the teachers union is a predictor of the propensity for a school board to choose an on-cycle election, why did we not incorporate this contract measure as one of the matching covariates? This is a reasonable point, however, given the need to code 552 separate contracts on 10 dimensions each, we opted to explore the power of teacher contracts on the smaller matched sample first. We may eventually decide to run matching on the full population of contracts.

signaled a preference for on-cycle elections were much less generous in compensating their teachers.

However, in looking at the more fully specified models (Election timing plus contract restrictiveness) on the three far right columns of Table 2, it appears that election timing itself may not be the cause behind higher teacher pay. Specifically, when we add our teacher contract restrictiveness variable to the model, the coefficients on our on-cycle election timing treatment predictor are significantly attenuated in magnitude and none reach levels of conventional statistical significance. Note also, that the term for contract restrictiveness does a far better job predicting the generosity of teacher pay than does election timing when the two variables are forced to compete. We think the most straightforward interpretation of these results is that election timing in and of itself is not the main driver of interest group advantage that we observe in teacher pay outcomes. Rather, districts' revealed preference in election timing is simply a latent measure of the local school board's prior disposition toward teacher union interests within the district. In short, we have uncovered the bias that is ignored in prior studies relating to the factors that pre-dispose some school districts to choose on-cycle election dates. Because we are able to isolate that bias (propensity to favor the union) at precisely the same time districts were required to reveal a preference in election timing, we can conclude that election timing is highly endogenous to the very outcomes that concern scholars most: policy outcomes of interest to organized interests.

Finally, we attempt to estimate the effect of districts' revealed preferences for on-cycle election times on growth in student performance. Table 3 replicates the approach used to estimate teacher pay in Table 2, however in addition to running models with and without the teacher contract variable, we add a host of appropriate control variables that are standard in any

education production function. In short, even though we have already matched school districts on nearly all the major variables likely to influence student achievement outcomes, we need to control for additional school-level factors in these models because our unit of analysis has changed to the school building level. In these models, we estimate growth in student proficiency rates on Michigan's 4th grade math and readings tests (known as MEAP) between 2005 and 2010. Although most of the demographic control variables are self-explanatory for any standard education production function (i.e., the inclusion of student poverty in a school), it is important to take note of our control for baseline achievement—a control variable that significantly reduces the chance that our main explanatory variables of interest (election timing and contract restrictiveness) reach statistical significance.

[Insert Table 3 about here]

The results in table 3 are wholly consistent with the evidence presented in table 2 on teacher pay, yet even stronger. In our “timing alone” models, we uncover an impressive boost in nearly 2 percentage points of growth in student proficiency in our treatment districts. That is, out of our sample of roughly 300 schools, those schools that reside in districts that revealed a preference for an on-cycle election improved their 4th grade test scores 2 percentage points from 2005 to 2010 (roughly one quarter of a standard deviation effect size). Now, we wish to be clear that we do not think students learned more *as a result of* their school board members being elected in November (as opposed to May) elections. Such a claim would be rather absurd. Rather, we think that the evidence presented in the more fully specified models in Table 3, which adds the contract restrictiveness variable, helps explain this relationship. Notice that in this more fully specified model, the contract restrictiveness variable partials out the effect of election

timing consistent with our theory that school districts' revealed preference for on-cycle elections signaled an underlying tendency of board members to oppose teacher union policy demands. That underlying animus toward the union is apparent in the much less restrictive (less union-friendly) contracts districts these districts negotiated in 2004-2005. To the extent that election timing preferences revealed in 2004-2005 merely reflect the school boards' propensity to feel sympathetic to teachers unions, the fact that sympathizing districts witnessed slower growth in student achievement five years downstream is more likely due to board decision-making in the district departing from what is best for students. One measure for such decision-making is, in fact, our measure of contract restrictiveness, which Moe (2009) demonstrates has a significant and negative impact on school performance, especially for schools that serve a disproportionate share of low-income students.

Figures 4-6 provide visual evidence of the link between districts' revealed preference for off-cycle elections and the correlated measure of contract restrictiveness for growth in student achievement. In addition, the figures include interaction terms (interacting the election treatment and contract restrictiveness with school poverty) to show how these negative effects on student performance are powerfully conditioned by school poverty (consistent with Moe 2009; Lindy 2011).

[Insert Figures 4-6 here]

High poverty schools are where school district sympathy to organized interests most impact student performance. If the causal mechanism at work that runs indirectly from election timing to student achievement are more teacher friendly and restrictive union contracts then it seems entirely consistent with Moe's research that we would find heterogeneous effects of correlations in election timing and weak growth in student achievement depending upon the composition of

the schools themselves. While we can only speculate at this point about the mechanism at work, future research will need to account for the relationship between school performance and union influence in school district electoral politics. Nevertheless, our results go a long way toward demonstrating that a school district's choice in election timing is far from randomly determined and, in point of fact, it is often endogenous to researchers' main outcomes of interest (here teacher pay *and* student achievement). Consequently, scholars will need to pay careful attention to the historical and political context of election reform in addition to explaining precisely what they claim to be able to measure when leveraging natural variation in an election calendar to explain policy outcomes.

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Appendix A: Tables & Figures

Table 1: The Relationship Between School Board Members' Preferred Election Date and Members' Willingness to Negotiate Teacher Union-friendly Contracts

	School Board Selects "On-Cycle" Election (2005-2006)	School Board Negotiates Union- favored Staffing Policies (2005-2006)	School Board Negotiates Union- favored Working Conditions (2005-2006)	Factor Score of Teacher Contract (2005-2006)
School Board Selects "On-Cycle" Election (2005-2006)	1.000			
School Board Negotiates Union- favored Staffing Policies (2005-2006)	-.290*	1.000		
School Board Negotiates Union- favored Working Conditions (2005-2006)	-.292*	.255*	1.000	
Factor Score of Teacher Contract (2005-2006)	-.379*	.792*	.792*	1.000

N = 70

Cell entries are pair-wise correlation coefficients.

* denotes $p < .05$.

Table 2: Results for models predicting district wages

	<i>Election Timing Models</i>			<i>Election Timing + Contracts</i>		
	BA Teacher Salary, no experience	MA Teacher Salary, no experience	Maximum Teacher Salary	BA Teacher Salary, no experience	MA Teacher Salary, no experience	Maximum Teacher Salary
Revealed “On Cycle” Election Preference (2005)	-1045.796 [796.101]	-1055.903 [855.073]	-4957.767* [2951.667]	-475.468 [839.793]	-318.570 [882.806]	-3204.033 [3161.39]
Median District Family Income	.097*** [.018]	.097*** [.020]	.304*** [.069]	.091*** [.018]	.095*** [.019]	.297*** [.068]
Revealed “Union friendly” Contract Preference (2005)	-	-	-	789.73** [389.091]	1001.91** [409.012]	2473.27* [1364.72]
Intercept	30907*** [1227.999]	33668*** [1318.964]	56714*** [4552.995]	30734*** [1204.739]	33435*** [1266412]	56207*** [4535.109]
R ²	.37	.38	.43	.47	.49	.42

*Significant at the p<.1 level, **significant at the p<.05 level, ***significant at the p<.01 level

Table 3: The relationship between election preference and student achievement growth (2010)

Explanatory variables (After matching)	(Timing Alone)		(Timing plus Contract)	
	Grade 4 Read	Grade 4 Math	Grad 4 Read	Grade 4 Math
Revealed “on cycle” preference	1.891***	1.710**	1.491*	1.031
	[.653]	[.852]	[.753]	[.871]
Union-friendly contract (2005)	-	-	-.363	-.498*
			[.293]	[.281]
Baseline Achievement (2005)	-.516***	-.564***	-.564***	-.592***
	[.056]	[.033]	[.056]	[.030]
Per Pupil Spending	-.001*	.005	-.001*	.006
	[.000]	[.000]	[.000]	[.000]
Class Size (Student-teacher)	.083	.174	.106	.209
	[.128]	[.173]	[.127]	[.183]
School Size (Enrollment)	-.002*	.006	-.002**	.001
	[.001]	[.000]	[.001]	[.000]
Percent Free/Reduced Lunch	-13.603***	-7.612**	-13.823	-7.151***
	[2.663]	[3.136]	[2.514]	[2.914]
Constant	52.91***	50.87***	56.091***	51.506***
	[6.213]	[5.596]	[5.832]	[5.193]
N	306	306	297	297
R ²	.37	.67	.38	.69

Note: Robust standard errors clustered by school district. Dependent variable is growth in student achievement as measured by the change from 2005 to 2010 in the percentage of 4th graders proficient on Michigan’s MEAP exam at the school building level.

*Denotes significant at the p<.1 level, **significant at the p<.05 level, ***significant at the p<.01 level.

Table 4: Summary of Previous Studies on Election Timing in School Districts

Author	<i>Anzia (2011a)</i>	<i>Berry and Gersen (2010)</i>	<i>Berry and Gersen (2011)</i>	<i>Hartney and Nickerson</i>
Sample	9 States where timing varies within; MN focus	California	California	Michigan
Election Timing History	Variation dating back to Progressive Period (1930s)	Variation dating back to 1982 legal change	Variation dating back to 1982 legal change	Variation observed in 2005-2006
Empirical Strategy	Natural Variation	Natural Variation with invite to switch	Same as 2010, use IV and matching	State Mandated Switch (Timing Identified)
DV Outcomes	Teacher \$ and Sup Salaries	Teacher \$ and Turnout	Teacher \$ and Student Achievement	Teacher \$; Student Achievement
Key Finding(s)	+3% ~Sup	+\$2000 (3%); turnout +20%	+2-4%; ~student achievement	+2.0 student achievement gains; senior teachers rewarded with roughly 7% more pay in off-cycle districts

Figure 1: Michigan Education Association GOTV Manual on School Board Elections

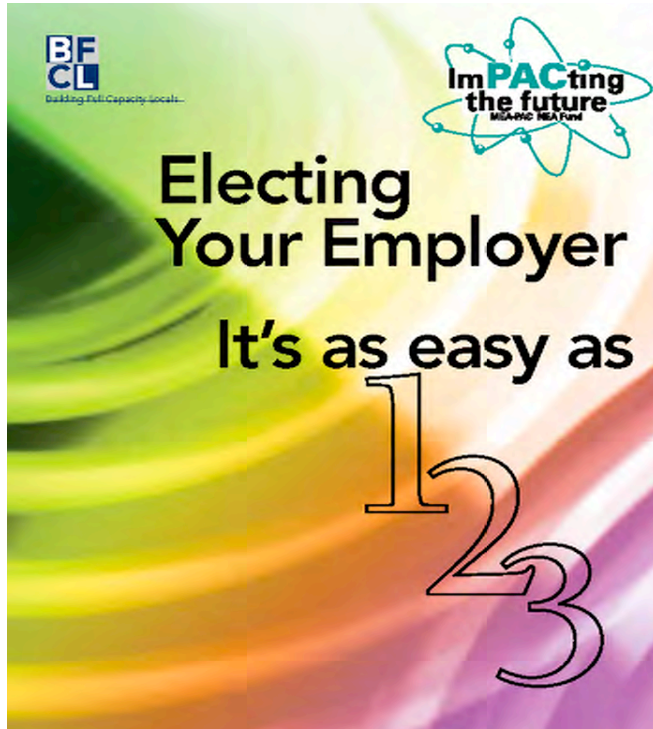


Figure 2: Distribution of Propensity Scores

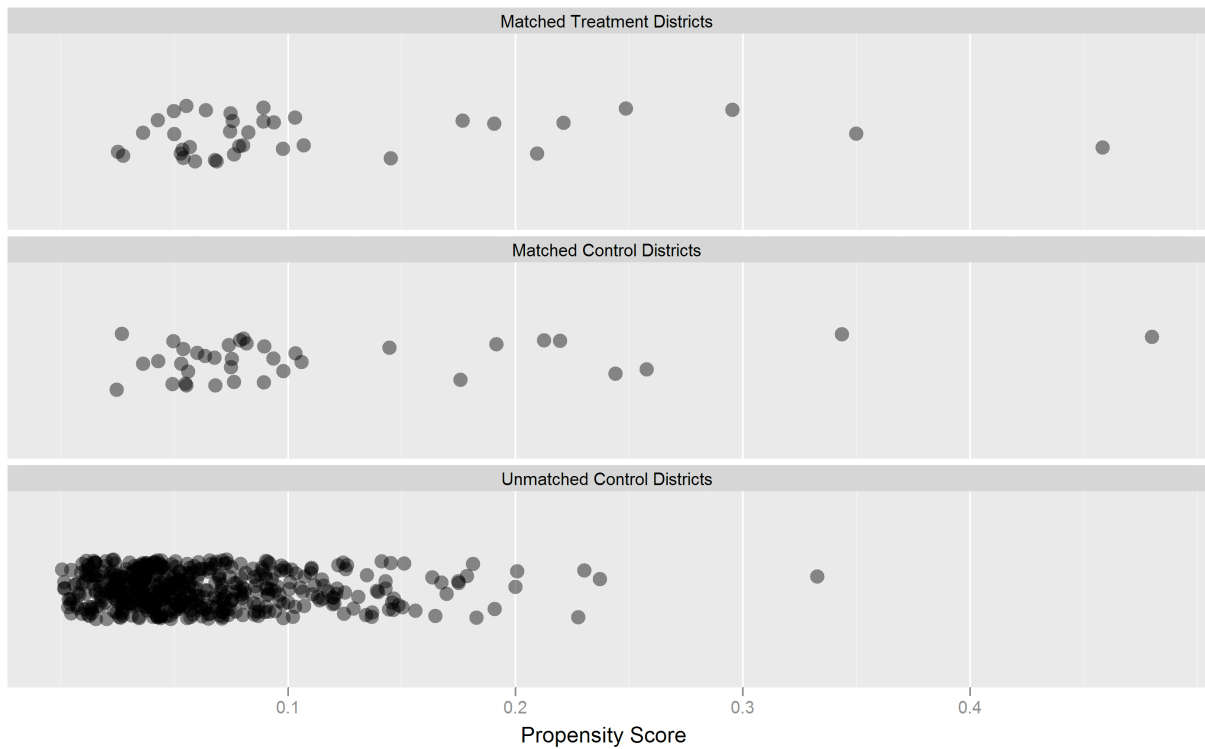


Figure 3: Pre and post matching balance in covariates. (Red points are pre-matching mean differences, blue points are post-matching)

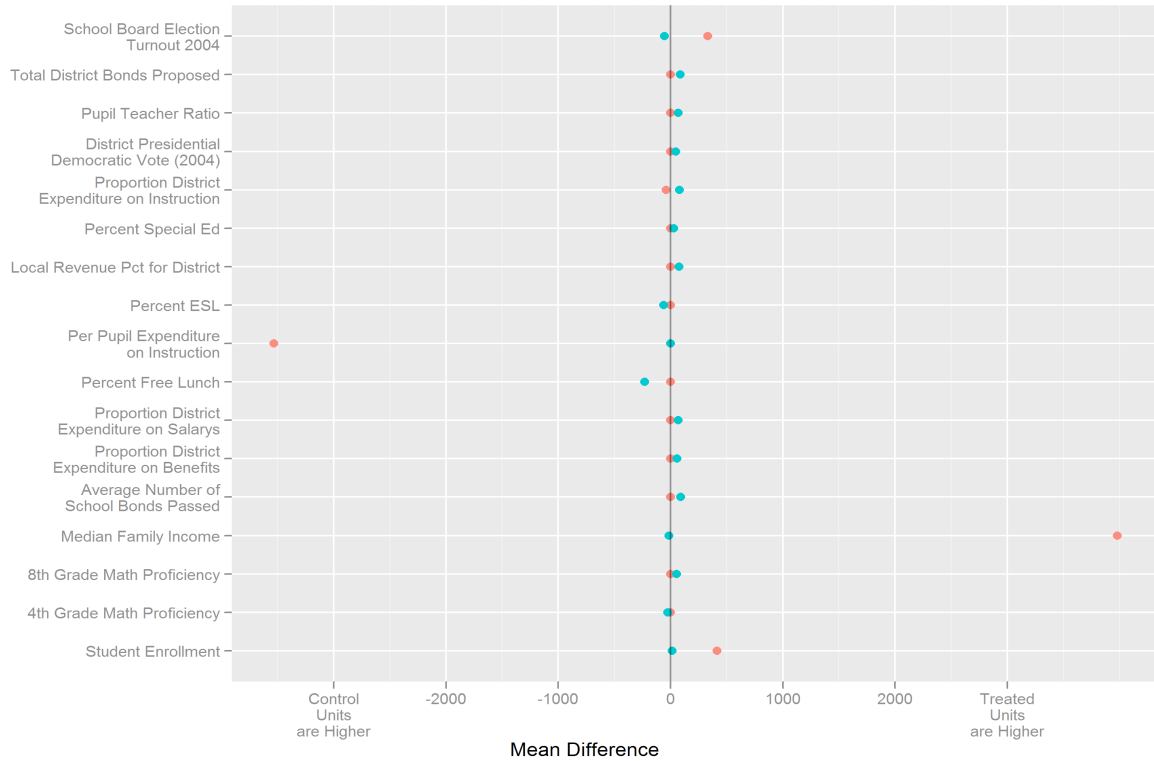


Figure 4: Predicting Growth in Student Achievement as a Function of Election Timing at Various Levels of Student Poverty

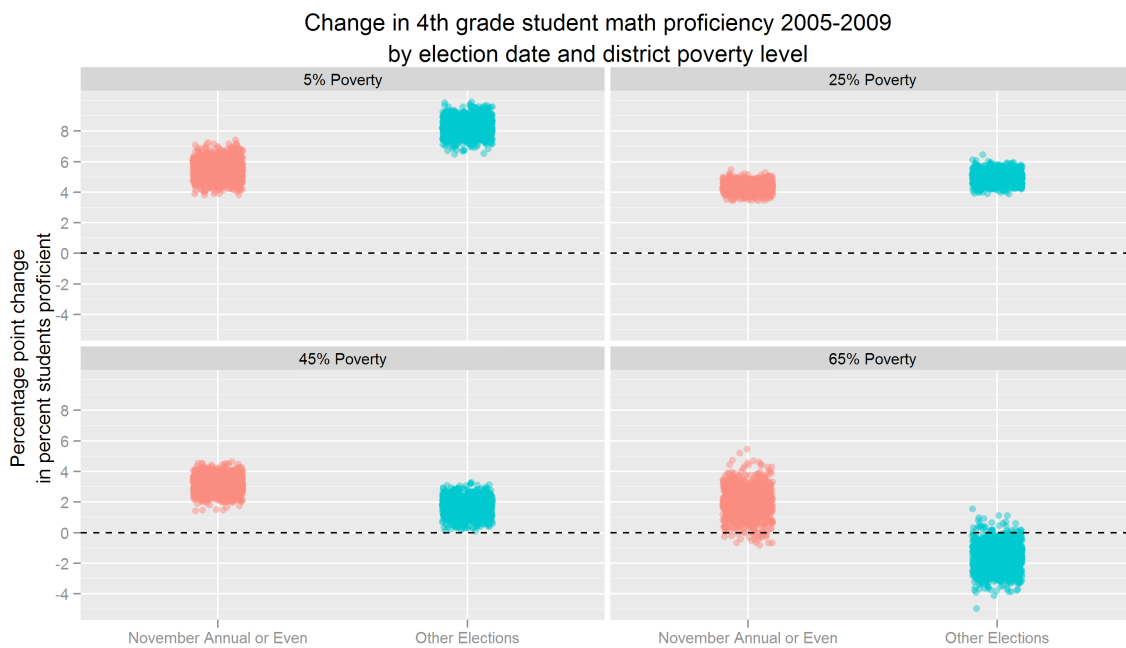


Figure 5: Predicting Growth in Student Achievement as a Function of Election Timing with Poverty Interaction

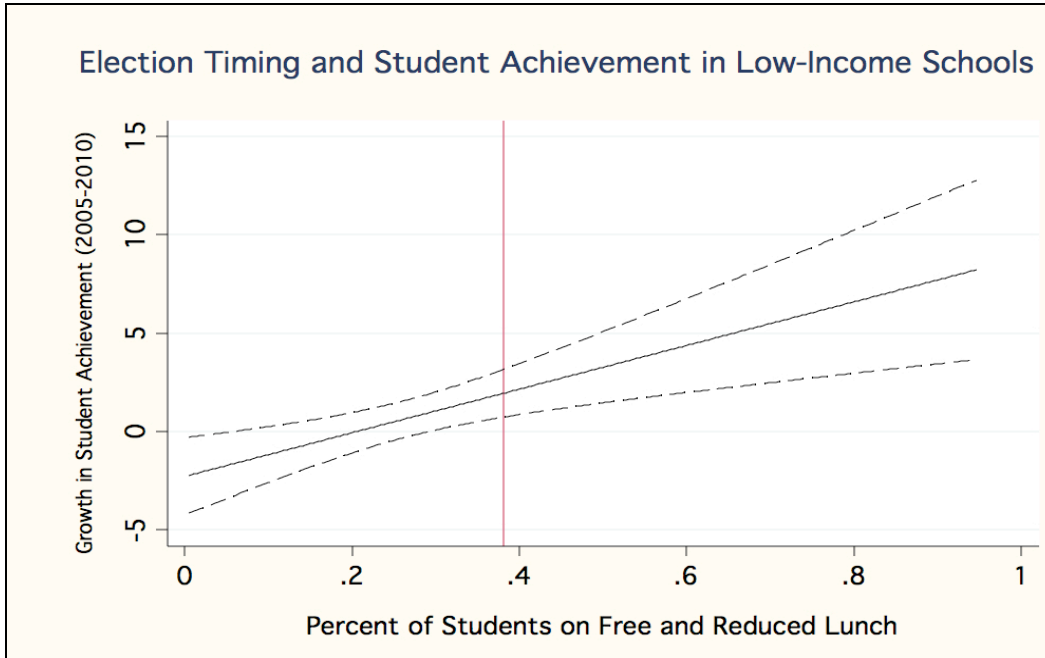
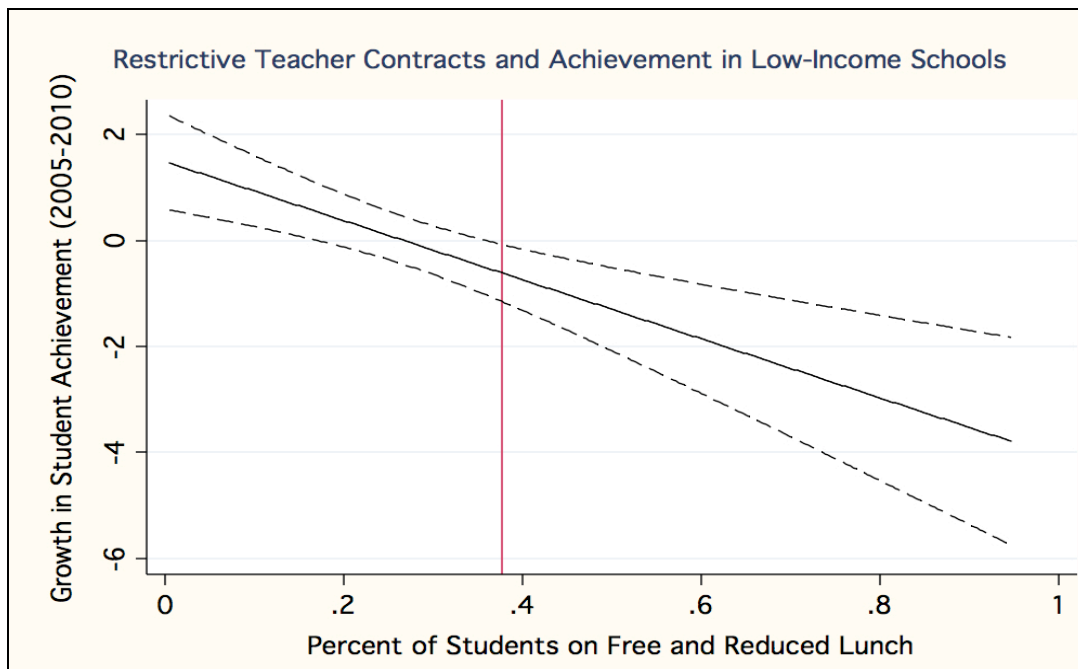


Figure 6 Predicting Growth in Student Achievement as a Function of Contract Restrictiveness with Poverty Interaction



Appendix B: Teacher Contract Codebook

Concept	Indicator	Exact Question	Coding Scheme			
Teacher Staffing Rules	Seniority for Voluntary Transfers	1. What role does seniority play in voluntary transfer teacher assignments?	None 0	A Factor 1	Definitive 2	Bumping 3
	Seniority for Sending Involuntary Transfers	2. What role does seniority play in selecting a teacher to involuntarily transfer?	None 0	A Factor 1	Definitive 2	
	Seniority for Receiving Involuntary Transfers	3. What role does seniority play in receiving a teacher who is being involuntarily transferred?	None 0	A Factor 1	Definitive 2	Bumping 3
	Seniority for Job cutbacks	4. What role does seniority play when the district must layoff teachers	None 0	A Factor 1	Definitive 2	
	Teacher effectiveness for Job cutbacks	5. What role does teacher quality or effectiveness play, if any, in determining which teachers to lay off	None 0	A Factor 1	Definitive 2	
Teacher Working Conditions	Restrictions on Teacher meetings	6. Agreements often contain restrictions on when, how frequently, and for how long principals may hold faculty meetings. Does this one?	None 0	Yes (Requires advanced notice) 1	Yes (Limits meeting frequency) 2	Yes (Limits length of meeting) 3; Code 4 if all restricted
	Restrictions on classroom observation (evaluation)	7. What restrictions are placed on principals/administrators evaluating teachers?	Advanced notice required? Y/N	Must limit time of actual evaluation period (e.g. 60 minutes)? Y/N	Must limit time in between two consecutive evaluations? Y/N	Certain days are precluded from being evaluation days (dates adjacent to Holidays)? Y/N
	Teacher involvement in evaluation	8. Does the contract require teachers or teacher representatives (the union) to be involved in designing the evaluation instrument?	None 0	Yes 1	Yes and it must be a subject of bargaining 2	
	Teacher preparation time	9. Does the contract guarantee preparation time for teachers	No 0	Yes and it specifies number of prep periods 1	Yes and it specifies number and length of prep periods 2	Yes AND it explicitly says teacher cannot be assigned other duties during 3