The Long-run Effects of Teacher Collective Bargaining[1]

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Abstract

Teacher collective bargaining is a highly debated feature of the education system in the US. This paper presents the first analysis of the effect of teacher collective bargaining laws on long-run labor market and educational attainment outcomes, exploiting the different timing across states in the passage of duty-to-bargain laws in a difference-in-difference framework. Using American Community Survey data linked to each respondent's state of birth, we examine labor market outcomes and educational attainment for 35-49 year olds, separately by gender. We find robust evidence that exposure to teacher collective bargaining laws worsens the future labor market outcomes of men: living in a state that has a duty-to-bargain law for all 12 grade-school years reduces male earnings by $1,493 (or 2.75%) per year and decreases hours worked by 0.52 hours per week. Estimates for women do not show consistent evidence of negative effects on these outcomes. The earnings estimates for men indicate that teacher collective bargaining reduces earnings by $149.6 billion in the US annually. Among men, we also find evidence of lower employment rates, which is driven by lower labor force participation. Exposure to collective bargaining laws leads to reductions in the skill levels of the occupations into which male workers sort as well. Effects are largest among black and Hispanic men, although white and Asian men also experience sizable negative impacts of collective bargaining exposure. Using data from the 1979 National Longitudinal Survey of Youth, we demonstrate that collective bargaining law exposure leads to reductions in measured cognitive and non-cognitive skills among young adults, and these effects are larger for men.

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1. Introduction

Teacher collective bargaining is one of the most prevalent and contentious features of the US education system. Over 60% of teachers in the United States currently are covered by a collectively-bargained contract (Frandsen 2016), and recently there has been a movement in many states to weaken the ability of teachers' unions to negotiate contracts in K-12 education. For example, in 2011 Wisconsin, Indiana, Idaho and Tennessee passed legislation that greatly reduced the ability of teachers to bargain with school districts, and in 2012 Michigan passed a public employee right-to-work law that sought to limit teacher union negotiating power. In 2014, the ruling in *Vergara v. California* argued that the tenure and teacher retention policies that are a main focus of collective bargaining violated the constitutionally-guaranteed right to an adequate education for each child in California.[2] These court rulings and legislative actions have reignited a debate over the proper role of teacher collective bargaining in the US education system. One of the core factors on which this debate rests is how such collective bargaining impacts student outcomes. Despite the large amount of policy attention directed toward the role of teachers' unions in education, there is a lack of empirical research that credibly and comprehensively addresses this question.

A central hurdle facing the prior teachers' union literature is the lack of student outcome data linked to exogenous variation in teacher collective bargaining. Much of the cross-sectional variation in teacher bargaining is driven by state public sector union laws that determine the obligations of school districts to negotiate with teachers. These laws were passed in the 1960s-1980s, a time period in which there were sparse data available on student outcomes that could be matched to one's state or school district of residence. The small set of studies that have examined the relationship between teacher collective bargaining and student outcomes from this time period have used high school dropout rates from the US Census (Hoxby 1996; Lovenheim 2009) or SAT scores at the state level (Kleiner and Petree 1988). These analyses reach different conclusions, and their focus on contemporaneous impacts for a limited set of performance measures does not yield a complete picture of the effects of teacher collective bargaining on student outcomes.[3] More recent studies have access to better student outcome data but lack

[2] This ruling was reversed in 2016 by the California Court of Appeals, and the reversal was subsequently upheld by the California Supreme Court.

[3] SAT scores in particular are problematic because average state-level scores are affected by changes in the selection of students into taking the test, which can be influenced by teacher unionization.

exogenous variation in teacher collective bargaining (e.g., Lott and Kenny 2013; Strunk 2011; Moe 2009).

In this paper, we present the first evidence in the literature on how teacher collective bargaining laws affect long-run outcomes of students. We focus on duty-to-bargain (DTB) laws, which require districts to negotiate with teachers' unions in good faith. Prior work has shown extensive evidence that duty-to-bargain laws increase teacher union membership and the probability that a school district unionizes for the purpose of collective bargaining (Frandsen 2016; Lovenheim 2009; Hoxby 1996; Saltzman 1985). We use the timing of the passage of these laws, which occurred between 1960 and 1987 (see Figure 1), linked with long-run educational and labor market outcomes among 35-49 year olds in the 2005-2012 American Community Survey (ACS), to provide novel evidence on the extent to which teacher collective bargaining impacts a broad array of long-run outcomes. Critical to our identification strategy is the ability to link ACS respondents to their state of birth, which allows us to account for any endogenous migration of families across states with different collective bargaining laws.

We employ cross-cohort difference-in-difference models that examine how outcomes changed among students who were of school age when a duty-to-bargain law passed compared to outcomes among students who did not experience a change in the public sector bargaining law. The sources of variation we exploit come from within-state changes in outcomes across birth cohorts who were differentially exposed to collective bargaining and cross-state differences in the timing of when these laws were passed.

As with the majority of studies examining long-run program effects, identification is complicated by the potential for other policies, secular trends, and unobserved shocks to affect the outcomes of interest. We show extensive evidence that our estimates are not being driven by such factors. First, our models include a comprehensive set of controls for other important policies during this period to which students may have been exposed. Second, we present event-study results that explicitly test for the existence of pre-treatment trends in outcomes across cohorts. Third, we show that the results are robust to directly controlling for any cross-cohort pre-treatment trends. Fourth, our results are not being driven by the general union environment in the state, are not influenced by the urbanicity of the population, are not correlated with the prevalence of social unrest in the state when our sample was of school age, and are robust to accounting for region-specific cohort shocks. Fifth, we perform permutation tests in which we randomly assign the year of duty-to-bargain law passage across states that ever pass a law in a manner that replicates the distribution of passage years. These estimates support the claim that we are not simply picking up differential secular variation between the treated and untreated states. Finally, we show that our estimates are not biased by cross-state mobility of those with school-age children. Taken together, these results provide extensive evidence that supports the causal interpretation of our estimates, and they are inconsistent with plausible sources of bias from other programs or trends.

Women's educational and labor market outcomes were subject to strong secular changes among the cohorts we examine (Goldin, Katz and Kuziemko 2006; Blau and Kahn 2013; Bick and Bruggeman 2014), and we thus analyze outcomes separately by gender.[4] Among men, our estimates point to negative effects of exposure to teacher collective bargaining laws on the long-run labor market outcomes of students who grew up in states with these laws. These results are consistent with the "rent-seeking" hypothesis of teacher unionization (Hoxby 1996).[5] Spending all 12 years of grade school in a state with a duty-to-bargain law reduces yearly male earnings by $1,492.82 (or 2.75%) and hours worked per week by 0.52 (or 1.34%). Further, these individuals are 1.1 percentage point less likely to be employed and are 0.8 of a percentage point less likely to be in the labor force. We also find evidence that collective bargaining law exposure causes male workers to sort into lower-skilled occupations later in life. However, collective bargaining laws have only a modest effect on educational attainment. Our estimates therefore are consistent with the lack of strong effects on high school graduation rates found in earlier work (Lovenheim 2009) and suggest that union effects on labor market outcomes affect human capital in ways that do not show up in years of educational attainment. This finding motivates our analysis using the 1979 National Longitudinal Survey of Youth (NLSY79) that shows declines in cognitive and non-cognitive skills due to collective bargaining exposure.

We further demonstrate that the negative effects of duty-to-bargain laws are particularly pronounced among black and Hispanic males; earnings decline by $3,640 (10.57%), hours worked per week decline by 1.35 (4.06%), the likelihood of being employed is 2.6 percentage

[4] These secular trends are unlikely to be related to teacher collective bargaining and instead reflect reduced gender-based discrimination, rising expectations of future labor market participation among women, increased female collegiate attendance, and expanded female labor market opportunities.
[5] The rent-seeking hypothesis of teachers' unions states that unions lead to a re-allocation of resources towards teachers while also making educational resources less productive. See Section 2 and Hoxby (1996) for a more in-depth discussion of this hypothesis.

points lower, and years of schooling and occupational skill are significantly lower. Exposure to collective bargaining laws also lead to worse labor market outcomes among white and Asian men, but the effects are more modest in magnitude.

We do not find consistent effects of collective bargaining law exposure on female labor market and educational attainment outcomes. Some of the point estimates are negative, but they are much smaller in absolute value than those for men and typically are not robust to accounting for differential pre-treatment trends. On the whole, therefore, the evidence points to little effect among women. Importantly, we do not find any evidence that the secular trends for women produce similar trends among men that would threaten our identification strategy. Why men would be so adversely affected but not women is an open question that our empirical approach admittedly cannot address, but these findings are consistent with emerging evidence that boys' long-run outcomes are more susceptible than are those of girls to negative shocks that occur in childhood (Autor et al. 2016; Fan et al. 2015; Autor and Wasserman 2013; Bertrand and Pan 2013).

A drawback of our setting, common to many studies that estimate long-run program effects, is that we cannot directly examine the full set of mechanisms through which our results operate. However, when examining education policies, what we ultimately care about is how they impact school quality and the long-run outcomes of students, something that we are able to speak directly to in this paper. That the data do not exist to examine all of the mechanisms potentially at work in determining long-run effects further augments the importance of directly estimating impacts on these long-run outcomes themselves.

Though we are unable to examine the full set of mechanisms driving our results, we do show evidence that passage of duty-to-bargain laws is associated with modest increased expenditures on teachers and a large increase in expenditures on administrators while keeping overall expenditures and teacher-student ratios the same. Prior research also has found evidence that duty-to-bargain laws reduce hours worked among teachers (Frandsen 2016) and that reduced bargaining power leads to lower fringe benefits among teachers (Litten 2017). Given the impossibility of exploring a comprehensive set of mechanisms, the long-run estimates we produce represent new evidence on the impact of duty-to-bargain laws that are very important because of the prevalence of these laws, the contentiousness surrounding them, the recent rise in policies aimed at curbing teacher collective bargaining rights, and the paucity of evidence on how they affect students.

While not direct evidence on mechanisms, we also provide supporting estimates that show how duty-to-bargain laws impact medium-run cognitive and non-cognitive skills among high school students using the NLSY97. These outcomes indicate whether the long-run effects we identify are reflective of changes in human capital. Consistent with the labor market effects, we find that duty-to-bargain law exposure significantly reduces both cognitive and non-cognitive outcomes and that these effects are larger among boys than girls. The sizable impacts on non-cognitive scores helps reconcile the fact that we do not see a strong educational attainment effect despite large impacts on labor market outcomes in the ACS data, since non-cognitive skills are likely to affect labor market outcomes more than they affect education outcomes (Heckman, Stixrud and Urzua 2006; Heckman and Kautz 2012; Heckman, Pinto and Savelyev 2013). These estimates support our long-run findings and indicate that teacher collective bargaining laws reduce the quality of education students receive.

Taken together, our results suggest that there are negative effects of public sector collective bargaining laws for teachers on the long-run labor market outcomes of men. Although the point estimates are relatively modest in magnitude, they are economically significant: increasing male earnings in the 33 states with a duty-to-bargain law by 2.75% amounts to $149.6 billion of additional earnings per year. This is equal to about 137% of annual federal spending on education. Thus, due to the scope of teacher collective bargaining in the US, the treatment effects we identify translate into large impacts on workforce productivity.

2. Teacher Collective Bargaining in the US

2.1. Duty-to-Bargain Laws

Prior to 1960, teachers unions in the US were predominantly professional organizations that had little role in the negotiation of contracts between teachers and school districts. Collective bargaining occurred in only a handful of large, urban school districts (such as New York and Detroit), and there was little recourse other than a strike if the district decided not to negotiate.[6]

Beginning with Wisconsin in 1960, states began passing union-friendly public sector bargaining laws that either gave teachers the right to collectively bargain or explicitly mandated

[6] See Murphy (1990) for a detailed history of the teachers' union movement in the United States.

that districts have to negotiate in good faith with a union that has been elected by teachers for the purposes of collective bargaining. The latter set of laws, called "Duty-to-Bargain" (DTB) laws, gave considerable power to teachers' unions in the collective bargaining process. Not only did it make it illegal for a district to refuse to bargain with a union, but most of these laws have provisions that require state arbitration if the two sides are at an impasse. As a result, duty-to-bargain laws led to a sharp rise in teacher unionization and in the prevalence of collectively-bargained contracts (Lovenheim 2009; Saltzman 1985). Indeed, in states that pass a DTB law, the vast majority of school districts elect a union for the purpose of collective bargaining, and these unions achieve contracts at very high rates (Lovenheim 2009). Thus, passage of a DTB law leads to a high fraction of teachers being covered by a collectively-bargaining contract over a short period of time.

Between 1960 and 1987, 33 states passed DTB laws, as shown in Figure 1. Most of these laws were implemented between the late-60s and late-70s, but there is considerable variation across states in the timing of passage. Table 1 shows the year of passage for each state as well as the set of states without such a law.[7] Of the 17 states without a duty-to-bargain law, 10 have legislation that allows teachers and districts to collectively bargain if both sides agree to do so. Four states (Alabama, Georgia, North Carolina, and Virginia) have no state law governing teacher collective bargaining, while three states (Mississippi, Missouri and Wyoming) explicitly outlaw collective bargaining among teachers. The states that have more restrictive collective bargaining laws tend to be located in the South and the West, which highlights the fact that these laws are not randomly assigned across states.

The focus of this paper is on how the passage of public sector DTB laws affects the long-run outcomes of students who attended elementary or secondary schools in those states. We examine duty-to-bargain laws because these laws led to larger increases in unionization and collective bargaining rates than did the other forms of union laws (Frandsen 2016): non-duty-to-bargain union laws do not explicitly require districts to recognize unions and bargain in good faith, thus allowing them to simply refuse to engage in collective bargaining.[8]

2.2. Theoretical Predictions

[7] Note that Washington, DC is excluded both from Table 1 and from our analysis.

[8] Our results are robust (though somewhat attenuated) when we use a more expansive definition of collective bargaining laws that includes the 10 states that allow but do not require districts to negotiate with teachers unions. These results are available from the authors upon request.

The main way in which duty-to-bargain laws affect student outcomes is by increasing the rate and substance of collective bargaining between teachers and school districts. Changes in collective bargaining, in turn, can impact students through three main channels: 1) by altering the inputs to education production, 2) by affecting teacher effort (and thus effectiveness), and 3) by changing the composition of teachers. The third mechanism in particular implies that the long-run effects may be larger than the short-run effects, as it takes time to alter teacher composition.

Models of public sector union behavior provide ambiguous predictions about how teacher collective bargaining should affect student outcomes. The "rent-seeking" model of teacher unionization argues that teacher collective bargaining is likely to lower student outcomes by distorting the allocation of resources towards teachers and away from other inputs to education production. A key prediction of this model is that teacher collective bargaining should lead to increases in resources going to teachers, but also to lower student achievement: the resource changes induced by teachers unions reduce the efficiency of educational inputs, which negatively impacts students. Furthermore, by protecting teachers from being fired, unions can reduce teacher effort and lower the quality of the teacher workforce, which will lead to worse student outcomes.

Under the rent-seeking model, we should observe an increase in teacher-related resources (such as teacher pay and employment) but a decline in the effectiveness of those resources. Such a decline could lead to worse student outcomes, either in the short or long run. In theory, one can test the predictions of this model using direct productivity measures, such as teacher value-added. Unfortunately, this is not feasible due to data limitations for the time period when there was variation in teacher collective bargaining laws. Alternatively, one can examine student outcomes directly, and this is the focus of our paper. The rent-seeking model predicts that any changes in school inputs induced by teacher collective bargaining should not increase student outcomes and likely will cause them to decrease.[9]

In contrast to the rent-seeking union model, there are several arguments suggesting that teachers unions can improve educational outcomes. First, a reallocation of resources based on teacher preferences could result in higher achievement due to lack of knowledge among

[9] The rent-seeking model does not guarantee that unionization will lead to lower student achievement. The reason is that unionization could increase total resources while also making those resources less effective. The net effect on student outcomes thus is ambiguous.

educational administrators about the education production function. Empowering teachers who are in the classroom therefore might lead to a more efficient allocation of resources. Second, there could be a "union voice" effect, whereby giving teachers a voice with which to influence their working environment makes them more productive (Freeman 1980; Gunderson 2005). A more favorable working environment could further induce more-productive workers to select into teaching.

All models of union behavior predict that teachers unions will alter district resource allocations. Indeed, just examining how unions affect education inputs such as teacher pay, employment and per-student spending will not allow one to distinguish between them.[10] These outcomes have constituted most of the focus of the prior literature, however. Where the union models differ is in their predictions of the direction of any effects on student outcomes. The theoretical ambiguities highlighted above underscore the importance of conducting an empirical investigation on how teacher collective bargaining affects student outcomes.

2.3. Prior Research on Teacher Unionization and Collective Bargaining

The majority of the earlier research on teachers unions examined their effect on resource allocation rather than on student outcomes.[11] While such analyses cannot shed light on which models of union behavior are correct nor on how collective bargaining affects student outcomes, they are instructive in thinking through some of the core mechanisms through which any effects on outcomes might operate. Collective bargaining can influence several dimensions of school resource allocation decisions: teachers typically negotiate over wage schedules, hiring and firing policies, health care and retirement benefits, work rules detailing the hours they are required to be at work and to teach, class assignments, class sizes and non-teaching duties (West 2015; Moe 2009; Strunk 2009).

Research examining the effect of teacher collective bargaining on district resources has found mixed results, although data constrains have only allowed an examination of a small subset of potentially affected outcomes. Much of this literature uses cross-sectional variation in union status that suffers from endogeneity concerns driven by the selection of teachers into unionization based on unobserved factors that also relate to the outcomes of interest. Hoxby

[10] It also is impossible to observe all educational inputs in most datasets. Thus, only examining the effect of unions on measured resources provides a somewhat limited description of their effect on schools and students.
[11] See Cowen and Strunk (2015) for a review of this literature.

(1996) is the first study to use more credible identifying variation by exploiting the passage of duty-to-bargain laws within states over time. She finds that increased unionization driven by these laws led to higher teacher salaries, increased per-student expenditures and reduced student-teacher ratios. However, using similar data but a different union measure, Lovenheim (2009) finds little connection between teacher collective bargaining and school district resources. In a re-analysis of these data, Frandsen (2016) finds that duty-to-bargain laws are not associated with a change in teachers' wages on average but do lead to a small decline in earnings and hours worked of about 1-2%.[12] More recent evidence exploiting the 2011 Budget Repair Bill of Wisconsin, which imposed substantial restrictions on collective bargaining rights, finds these restrictions increase teacher wage dispersion (Biasi 2017) and have a modest effect on average wages but a sizable impact on non-wage compensation (Litten 2017).

Of first-order importance in the policy debate over the role of teachers unions in education as well in being able to distinguish between models of union behavior is how collective bargaining affects student outcomes. As discussed above, estimates of the effects on school district resources do not allow us to predict the effects on these outcomes. In addition, data constraints make it virtually impossible to estimate how unions affect teacher productivity and the quality of teachers in the workforce, which are two main pathways through which unions can influence student achievement.[13] Thus, it is important to examine how unions impact student outcomes directly.

There is a small literature on the effect of teachers' unions on student academic achievement. However, none of these studies estimates the effect of collective bargaining on long-run labor market and educational attainment outcomes,[14] which may differ from any short-run impacts in important ways; many studies have found that program effects on student test

[12] An earlier body of research examines how unions affect teacher pay and comes to mixed conclusions as well. Balfour (1974), Zuelke and Frohreich (1977), and Kleiner and Petree (1988) find no effect on teacher pay, while Eberts and Stone (1986), Moore and Raisian (1987) and Baugh and Stone (1982) find evidence of a union wage premium for teachers ranging from 3-12 percent. However, these studies typically lack plausibly-exogenous variation in union status.

[13] Hoxby and Leigh (2004) argue in a Roy model framework that the wage compression which typically follows unionization leads lower-ability workers to select into teaching. Alternatively, the increased worker "voice" combined with changes in human resource policies that often accompany unionization suggest that teacher quality could increase due to teacher collective bargaining.

[14] Freeman et al. (2016) show that children with parents who are union members have higher earnings and that intergenerational mobility is higher in areas with higher union density. This work does not focus on teachers' unions, per se, and the evidence adduced in this paper is correlative rather than causal. Their findings are not necessarily inconsistent with a negative long-run effect of teacher collective bargaining on student outcomes.

scores can be very different from any effects on long-run outcomes (e.g., Ludwig and Miller 2007; Chetty et al. 2011; Deming et al. 2013; Cohodes et al. 2016). This evidence underscores the importance of examining long-run effects directly. One central reason for this lack of prior work is data constraints: the teacher unionization movement took hold before consistent measures of student outcomes were collected. Thus, researchers are forced either to use a small set of outcomes from older data to exploit the law changes that provide plausibly-exogenous variation in teacher collective bargaining or to use more recent data from a time period when there is little exogenous variation in collective bargaining behavior across school districts.

Hoxby (1996) and Lovenheim (2009) both use the passage of duty-to-bargain laws to estimate how teacher collective bargaining affects contemporaneous high school dropout rates. They focus on this outcome because it is the only district-level educational outcome consistently available nationwide from the 1960s-1980s. Hoxby finds that collective bargaining laws lead to an increase in high school dropout rates, which is consistent with the rent-seeking model of union behavior.[15] Using an alternative unionization measure and a smaller set of states, Lovenheim (2009) finds no such effect.

Although the Hoxby (1996) and Lovenheim (2009) studies are the most credible in terms of the use of exogenous variation in union status (and hence collective bargaining), data limitations force them to focus on one very narrow educational outcome measure. If collective bargaining affects a different part of the ability distribution, or if it impacts student human capital accumulation in ways that do not show up in high school dropout rates, these studies will present an incomplete picture of how teacher collective bargaining affects students. Other work that examines the link between teachers' unions and student outcomes uses student test score data, but this research typically suffers from a lack of exogenous variation in union status (e.g., Kleiner and Petree 1988; Eberts and Stong 1987).

Much of the literature that uses more recent data to examine how unions and collective bargaining affect test scores relies on measures of contract restrictiveness or union power to measure the strength of unions in a district. Lott and Kenny (2013) show that states with higher union dues and union expenditures have lower 4th grade proficiency rates. Strunk (2011) finds that contract restrictiveness is negatively correlated with test score level differences across

[15] In contrast, Eberts and Stone (1986, 1987) find that teachers' unions increase school productivity. However, they lack exogenous variation in union status across schools, which complicates the interpretation of their results.

schools but not with differences in test score growth. The cross-sectional nature of these comparisons make it unlikely that these studies isolate the causal effect of union strength on student outcomes, as districts with strong unions tend to be in more urban, lower-income areas.

In order to address the problems associated with a cross-sectional approach, Moe (2009) examines how changes over time in union contract restrictiveness within school districts in California relate to changes in student test scores.[16] He finds that districts with contracts that become more restrictive experience declines in test score growth. Even though this differencing approach handles any cross-sectional selection problems, it is unlikely that the within-district variation in restrictiveness over time is exogenous. Thus, his findings could be driven by unobserved factors that both depress test score growth and lead to an increase in the restrictiveness of the contracts that unions negotiate.

Our contribution to this literature is to estimate how teacher collective bargaining affects long-run educational and labor market outcomes using an identification strategy that incorporates exogenous variation in the prevalence of collective bargaining in the state. By linking adults in different birth cohorts to their state of birth, we can exploit timing differences in the passage of duty-to-bargain laws to overcome the identification problems and data limitations faced by prior research. Our results therefore provide the first comprehensive analysis of the causal effect of teacher collective bargaining on student outcomes, which is of first-order importance given the prevalence of teachers unions and the ongoing policy debate about their proper role in education.

3. Data

The data for our main analysis come from two sources. The first source is the NBER collective bargaining law dataset (Valletta and Freeman 1988) that was updated in 1996 by Kim Reuben.[17] These data contain, for each state and year since 1955, collective bargaining laws for each type of public sector worker. We use the laws for teachers, and we generate an indicator variable equal to 1 if a duty-to-bargain law was in place in each state and year.

We combine the collective bargaining information with 2005-2012 American Community Survey (ACS) data on individuals between the ages of 35 and 49. We focus on these

[16] Moe (2009) defines contract restrictiveness using factor analysis on a set of work rule restrictions that are included in many teacher contracts. In contrast, Strunk (2011) uses partial independence item response methods that are outlined in Strunk and Reardon (2010) to define contract restrictiveness.
[17] These data are available at http://www.nber.org/publaw/.

ages because individuals within this age span typically have completed their education and are on a part of their lifetime earnings profile where yearly earnings are informative about lifetime earnings (Haider and Solon 2006). Furthermore, we are able to observe individuals of each age in each of the eight survey years, leading to a balanced panel of age observations in our data. We construct birth cohorts by subtracting age from calendar year, and we assume each respondent begins school at the age in which his assigned birth cohort turns 6. These assumptions lead to some measurement error in treatment assignment because the ACS is conducted each month and states have different school-age cutoff dates. Using the school-age cutoff dates that prevailed in 1988 (Bedard and Dhuey 2012) and assuming that ACS survey month and birth month are evenly distributed over the year, we calculate about 27% of the sample will enroll in school the year prior to their assigned birth cohort. This is likely to bias our estimates towards zero by generating changes in outcomes in the cohort just prior to DTB passage.

Table 2 presents the birth cohort that underlies each age and year combination; for example, 40-year-olds in 2005 come from the 1965 birth cohort and 40-year-olds in 2012 come from the 1972 birth cohort. As shown in Table 2, the birth cohorts range from 1956 to 1977. These birth cohorts correspond to students who would have been in school from 1962 (when the 1956 birth cohort was 6) to 1995 (when the 1977 birth cohort was 18).[18] These schooling years correspond with the large rise in duty-to-bargain laws across states in the US shown in Figure 1.

One of the main advantages of using the ACS for this analysis is the ability to link adults to their state of birth. This is an important feature of the data because collective bargaining laws might cause families to migrate, especially if they affect schooling quality. In addition, these laws may cause post-schooling migration patterns to differ, as obtaining more or less skill when young could affect one's access to a more national labor market. Using each respondent's state of birth eliminates any problems associated with endogenous mobility. Of course, families can move across states such that one's state of birth differs from the state in which he or she attended school. In Section 5.5, we show any bias resulting from such mobility is small. We also do not find evidence that exposure to DTB laws is correlated with changes in the composition of the population born in a given state and cohort, which suggests parents are not endogenously moving with respect to DTB regulations prior to a child's birth.

[18] Note that the collective bargaining law dataset ends in 1996. Even though there were few public sector bargaining law changes made after 1996, any such changes would not affect the cohorts we consider in this analysis.

Because one's state of birth and birth cohort determine one's exposure to a duty-to-bargain law while in school, we collapse the data to the state-of-birth, year-of-birth, calendar year level. Aggregation to this level is sensible because the effect of duty-to-bargain laws on student outcomes is not necessarily limited to unionized districts: these laws can impact all districts in a state through spillover and "union threat" effects (Farber 2003). The spillover effects come in part from the political activities of teachers' unions that can impact educational resources and policies in all schools in the state. Additionally, union threat effects can cause non-unionized districts to begin behaving like unionized ones in order to stave off a unionization movement among teachers.

The ACS contains detailed information on educational attainment and labor market outcomes. Descriptive statistics of the variables we use, separately by gender, are shown in Online Appendix Table A-1.[19] For educational attainment, we generate mutually exclusive indicator variables for the highest level obtained: high school graduation, some college (but no degree), Associates degree (AA), or at least a Bachelors degree (BA). We also combine these measures into a *years of education* variable. In the 2008-2012 ACS, years of completed schooling are reported directly. In the 2005-2007 ACS waves, we used completed schooling levels to construct this variable in the following way: 0 for no school completion, 4 for fourth grade completion, 6 for 5th or 6th grade completion, 8 for 7th or 8th grade completion, 9-11 for 9th through 11th grade completion, 12 for 12th grade completion and less than 1 year of college, 13 for one or more years of college with no degree, 14 for an AA degree, 16 for a BA degree, 18 for a master's or professional school degree, and 21 for a doctoral degree.[20]

We also use the ACS measures of whether an individual is currently employed, unemployed or not in the labor force, as well as labor income in the previous year and hours worked per week. Labor income is the sum of wage and salary income as well as self-employed income over the past 12 months. Both income and hours worked are set to zero for those who do not report any income or working activity, which typically occurs because the respondent is unemployed or is not in the labor force.

Finally, we construct a measure of occupational skill. Using the 2005-2012 ACS, we

[19] Descriptive statistics by gender and race/ethnicity are shown in Online Appendix Table A-2.
[20] As shown in Online Appendix Table A-3, our results are robust to excluding 2005-2007 ACS waves when analyzing years of education as an outcome.

calculate the proportion of workers in each 4-digit occupation code that has more than a high school degree (i.e., at least some collegiate attainment). This allows us to rank occupations by the skill level of those who engage in the occupation in order to examine whether exposure to teacher collective bargaining leads workers to sort into lower- or higher-skilled occupations.

4. Empirical Methodology

We exploit the different timing across states in the passage of duty-to-bargain laws in a difference-in-difference framework. Specifically, we estimate models of the following form, separately for men and women:

$$Y_{sct} = \beta_0 + \beta_1 DTB\_Exposure_{cs} + \gamma X_{sct} + \delta_{ct} + \theta_s + \phi_t + \varepsilon_{sct}, \quad (1)$$

where Y_{sct} is one of the educational or labor market outcomes listed above for those born in state *s* in birth cohort *c* and in ACS calendar year *t*. Regressions are weighted by the number of observations that underlie each birth year-birth state-calendar year-gender cell, and all standard errors are clustered at the birth state level. The treatment variable of interest, *DTB_Exposure*, varies from 0 to 1 and is defined as the proportion of a cohort's school years in which a duty-to-bargain law was in effect in its state of birth. Thus, when *DTB_Exposure* is equal to 1, it means a duty-to-bargain law had been enacted by the time a cohort was six years old (in time for first grade).[21] Values of *DTB_Exposure* between 0 and 1 reflect partial exposure; for example, when *DTB_Exposure* equals 4/12 it means a duty-to-bargain law was enacted when the cohort was 14 years old (and likely in 9th grade). This treatment measure equals zero for cohorts that were over 18 when a duty-to-bargain law was passed or for those born in states that have not passed such a law.

Equation (1) also includes a set of birth cohort-by-calendar year (δ_{ct}), birth state (θ_s) and calendar year (ϕ_t) fixed effects. The birth cohort-by-year fixed effects are identical to age fixed effects, because birth cohort and calendar year perfectly define age. The cohort-year fixed effects control for any systematic differences across birth cohorts in a given calendar year that may be correlated with both the prevalence of duty-to-bargain laws and with labor market outcomes. The state fixed effects control for variation in educational attainment or labor markets

[21] We exclude kindergarten because for the cohorts we examine public, full-day Kindergarten was much less prevalent than it is today. However, our estimates are robust to including Kindergarten students in the exposure measure.

that are common across birth cohorts within a state, and the year fixed effects account for national shocks that impact all birth cohorts in the same year. We also control for the proportion of each state-cohort-year-gender cell that is black, Asian, Hispanic or "other." These controls are in the vector X in equation (1).

Conditional on the fixed effects and demographic controls in the model, the variation in duty-to-bargain law exposure comes from two sources. The first is within-state differences in exposure over time driven by the state's year of passage of a DTB law. The second is cross-state variation in the timing of when states passed these laws. The assumptions underlying the identification of parameter β_1 are similar to all difference-in-difference analyses: the timing of duty-to-bargain law passage must be uncorrelated with any prior trends in outcomes across birth cohorts within each state, and the timing of the law passage cannot coincide with any state-specific shocks that are isolated to the treated cohorts or with other policies that might influence long-run educational attainment or labor market outcomes.

In order to test for the existence of differential pre-treatment trends across birth cohorts relative to the timing of passage of DTB laws, we estimate the following event-study model:

$$Y_{sct} = \beta_0 + \pi_{-11} I(C - t_0 + 18 \leq -11)_{sc} + \sum_{\tau=-10}^{20} \pi_\tau I(C - t_0 + 18 = \tau)_{sc} + \pi_{21} I(C - t_0 + 18 \geq 21)_{sc} + \gamma X_{sct} + \delta_{ct} + \theta_s + \phi_t + \varepsilon_{sct}. \quad (2)$$

The variable $(C - t_0 + 18)$ is equal to the number of years of exposure a given cohort has had to a duty-to-bargain law, with C being the birth year of the cohort and t_0 being the year of passage of the duty-to bargain law. Thus, a cohort that is 19 when a duty-to-bargain law is passed will have an exposure time of -1, while a cohort that is 10 when it passes will have an exposure time of 8. This variable takes on a value of zero in states that have never had a duty-to-bargain law.[22] Hence, $I(C - t_0 + 18 = \tau)$ are indicator variables equal to 1 for each relative year to passage of a duty-to-bargain law between -10 and 20. We also include an indicator for whether time relative to a DTB law is less than or equal to -11 and for whether $C - t_0 + 18$ is greater than or equal to 21.[23] The π_τ coefficients non-parametrically trace out pre-treatment relative trends (for π_{-11} to

[22] In the time period we examine, no state repeals a duty-to-bargain law.
[23] We choose this event window because the sample sizes become small for relative time indicators less than -10 and greater than 20. Including these "catch-all" relative time indicators allows us to use the same sample as in equation (1), but we caution that it is rather difficult to interpret the coefficients on these two variables.

π_{-1}) as well as time-varying treatment effects (π_0 to π_{21}). In practice, we omit $I(C - t_0 + 18 = -1)$ such that all π estimates are relative to the year prior to DTB passage.

Equation (2) tests for the existence of selection on fixed trends across cohorts as well as for time-varying treatment effects that can come from two sources. The first is that some cohorts are only exposed for part of their schooling years. When $(C - t_0 + 18)$ is between 0 and 12, there may be time-varying treatment effects due to different lengths of exposure to collective bargaining laws across cohorts. The second factor that influences the time pattern of treatment effects is that these laws may have time-varying effects on resource allocation (see Lovenheim (2009) and Appendix Table A-9) as well as on the composition of teachers. We are unable to separate these two factors in our setup, so both are reflected in the post-DTB π coefficient estimates.

The second potential identification problem of unobserved state-cohort specific shocks correlated with the passage of duty-to-bargain laws is more difficult to investigate. However, there is much variation in the timing of the passage of these laws, as shown in both Figure 1 and Table 1, which makes it very unlikely that there are secular shocks that are systematically correlated with the timing of DTB passage and only influence the affected cohorts. Permutation tests further support the contention that unobserved shocks correlated with the timing of the rollout of DTB laws are not biasing our estimates. We also include a robustness check that includes state-by-year fixed effects. While less precise, these estimates indicate that our estimates are not being influenced by state-specific macroeconomic shocks or current statewide policies.

The existence of alternative policies that were passed concurrently with duty-to-bargain laws is a more serious threat to identification. The 1960s-1980s saw many changes to both schooling and social policies that could have affected the birth cohorts we analyze. If the rollout of these policies is correlated with duty-to-bargain passage, it could bias our results. We address this concern by controlling for exposure to three alternative policies that occurred concurrently with the DTB movement that also could impact these students' long-run outcomes: school finance reform, the earned income tax credit (EITC), and food stamps. We know of no other policy changes that could plausibly have impacted the declines in labor market outcomes we document. In the vector X in equations (1) and (2), we control for the number of years each birth cohort would have been exposed to legislative or court-ordered school finance reform (separately) while in school. The timing of legislative and court-ordered school finance reform are taken from Jackson, Johnson and Persico (2015), who show these reforms led to large increases in the outcomes we consider. We also control for average state EITC rates between the ages of 6 and 18 for each cohort, as Bastian and Michelmore (forthcoming) show that these policies positively affect educational attainment.[24] Finally, Hoynes, Schanzenbach and Almond (2016) demonstrate that exposure to the food stamp program when young has long-run effects on health and economic outcomes. We use the population-weighted average proportion of counties eligible for food stamps when each birth cohort-state of birth group was between 6 and 18.[25] Below, we show estimates both with and without these controls; they have little effect on our results.

5. Results

Tables 3-5 present baseline estimates of the effect of teacher collective bargaining exposure on labor market outcomes for men (columns i-iii) and women (columns iv-vi). Each cell in each table comes from a separate estimation of equation (1), and we add controls sequentially across columns. In columns (i) and (iv), we control for birth state, birth cohort and calendar year fixed effects as well as race/ethnicity. We add controls for state EITC, school finance reform and food stamp exposure during childhood in columns (ii) and (v), and columns (iii) and (vi) adds cohort-by-year (i.e., age) fixed effects. We discuss the estimates for men and women in turn below.

5.1. Baseline Male Estimates

Table 3 presents results for earnings (Panel A) and hours worked (Panel B). Across the first three columns in Panel A, there is clear evidence of a negative effect of teacher collective bargaining on male earnings. The estimate in column (iii) indicates that attending school in a state with a duty-to-bargain law for all 12 years of elementary and secondary school reduces earnings by $1,492.82 dollars per year. This represents a decline in earnings of 2.75% relative to

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[24] Cohodes et al. (2016) and Brown, Kowalski and Lurie (2015) show that the Medicaid expansions of the 1980s and 1990s had large, positive effects on the educational attainment and eventual earnings of youth exposed to these expansions. However, our birth cohorts are mostly too old to have been impacted by these policy changes. Furthermore, we cannot control for Medicaid eligibility in this study because eligibility policies and rates are not available prior to 1980. If anything, this will cause us to understate (in absolute value) the effect of collective bargaining laws.

[25] The food stamp data come from the publicly-available data used by Hoynes, Schanzenbach and Almond (2016), available at https://assets.aeaweb.org/assets/production/articles-attachments/aer/app/10604/20130375_app.pdf.

the mean, which is shown directly below the estimates in the table. While a 2.75% reduction in earnings is relatively modest for each individual, this estimate translates to a large amount of total earnings lost because of the prevalence of duty-to-bargain laws in the US. Across all 33 states that have a duty-to-bargain law in place, our results suggest a total loss of $149.6 billion dollars *per year* due to male workers having grown up in states that mandate collective bargaining between teachers' unions and school districts.[26] Furthermore, the estimates in Table 3 are similar across columns, which is inconsistent with biases from age-specific shocks or from exposure to other policies when young.

Panel (a) of Figure 2 shows event study estimates for male earnings. We have excluded relative year -1 and have overlaid a linear fit for the pre- and post-treatment periods to help see if there are differential pre-treatment trends and if there are time-varying treatment effects. In Section 5.4, we show estimates that test directly for biases associated with any pre-treatment trends. Each point in the figure is an estimate of π_τ for the given relative year, and the bars show the 95% confidence interval of each estimate using standard errors clustered at the state level. The visual evidence in Panel (a) of Figure 2 strongly supports our identification strategy: there is no evidence of differential trends in earnings across pre-treatment cohorts. When duty-to-bargain laws are passed, earnings begin to decline and continue to do so with the length of exposure. As a result, the effect on earnings 20 years after DTB passage is about -$2,500.

Panel B of Table 3 presents estimates for weekly hours worked (including zeros). Consistent with the reduction in earnings, average hours worked decline by 0.523 due to being exposed to DTB laws throughout one's schooling years. This is a 1.34% decline relative to the mean of 38.96 shown in Table A-1. The estimates are stable across columns and are significant at the 5% level for men. Figure 2, Panel (c) presents event study estimates for this sample and outcome: there is no evidence of differential pre-treatment trends, and similar to earnings the effect grows with exposure. After 20 years, weekly hours worked decline by almost an hour.

The finding that teacher collective bargaining is associated with fewer working hours among men suggests that DTB laws may affect the extensive margin of labor supply. Table 4 examines this question in detail, showing estimates of equation (1) where the proportion

[26] We obtain this estimate using total wage income for each state and the percent of the workforce that is male (53.16%) in 2014, obtained from the Bureau of Labor Statistics. Specifically, we multiply 2014 total income in the 33 states by 0.0275*0.5316.

employed (Panel A), unemployed (Panel B) and not in the labor force (Panel C) are used as the dependent variables. Looking across the panels, it is clear that duty-to-bargain laws reduce male employment and increase the proportion of male workers who are not in the labor force. In Panel A, exposure to a duty-to-bargain law while in grade school lowers the likelihood a male worker is employed by 1.1 percentage point, or 1.34% relative to the mean. The estimates are significant at the 5% level and are similar in magnitude to the hours worked results. Thus, much of the reduction in hours worked is coming from the extensive margin.[27]

There is little evidence of an effect on unemployment. Rather, teacher collective bargaining laws impact labor force participation: 12 years of exposure to a duty-to-bargain law reduces the male labor force participation rate by 0.8 of a percentage point. Relative to the mean labor force participation rate, this represents a reduction of 6.56%.

Event study estimates of employment outcomes are shown in Figure 3. They align closely with the estimates in Table 4: pre-treatment trends are small and in the opposite direction of the treatment effects, and there is a clear effect of DTB law passage that grows over time for employment and labor force participation. There is no evidence of an effect on unemployment.

Table 5 shows results for occupational skill and educational attainment. In Panel A, the dependent variable is the proportion of individuals in one's occupation that has at least some collegiate attainment.[28] Here, the inclusion of state-cohort fixed effects reduces the size of the estimate, but it still is negative and statistically significant at the 10% level in column (iii). The results suggest that being exposed to a duty-to-bargain law for all 12 years decreases the proportion of workers in one's occupation with at least a college degree by 0.003 (or 0.48% relative to the mean) in our preferred model. Panel (a) of Figure 4 shows event study estimates for this outcome. The figure shows no evidence of pre-DTB differential trends, and there is a clear reduction in occupational skill post law passage that accords closely with the difference-in-difference estimates. These results point to collective bargaining negatively affecting the occupational skill level chosen by workers.

The reduced earnings and labor force participation associated with teacher collective

[27] That there is an extensive margin effect makes it difficult to examine wages, because the treatment is correlated with a change in the composition of wage earners among men. We therefore focus on earnings, which can more easily handle changes on the extensive margin.
[28] The regressions in Panel A of Table 5 are estimated using the individual-level, disaggregated ACS data. This was done because the dependent variable does not lend itself simply to aggregation at the state-year-cohort level.

bargaining suggest that human capital accumulation is declining among exposed cohorts. This reduction could show up in changes in the quantity of education completed, although educational attainment is a coarse measure of human capital. We examine how exposure to a DTB law affects years of completed education; estimates on cognitive and non-cognitive test scores that provide alternative measures of human capital are shown in Section 6. Because most people have finished their formal schooling by their mid-30s, the age ranges included in our analysis allow us to accurately measure the total amount of education obtained by each ACS respondent.

Panel B of Table 5 shows results for the total number of years of education. Across columns, the point estimates are negative, modest in magnitude, and are not statistically significantly different from zero at even the 10% level. Taking the point estimates at face value, they suggest a 0.31% decline in educational attainment due to collective bargaining exposure. The event study estimates in Panel (c) of Figure 4 indicate a somewhat stronger result. There is a small upward pre-treatment trend that biases the estimates in Table 5 towards zero. The educational attainment effect post-DTB law also grows over time, such that by 20 years after law passage those in DTB states have 0.1 fewer years of education on average.

How much of the earnings decline can the educational attainment effects explain? The estimate in Table 5 is precise enough to rule out an effect larger than -0.105 years of completed schooling at the 5% level in column (iii), which is 0.78% relative to the mean. Assuming that an additional year of schooling increases earnings by 10% (Card 1999), a decline in educational attainment of 0.275 years could fully explain the 2.75% earnings decline we estimate from teacher collective bargaining. Thus, we can rule out that more than 38% of the earnings effect is driven by changes in completed years of education.[29]

Examining total years of schooling may miss heterogeneous effects across the distribution of schooling levels. In Appendix Table A-4, we estimate equation (1) using the proportion of respondents with different highest levels of educational attainment as the dependent variable.[30] The estimates indicate reductions in postsecondary attainment and an increase in the proportion of students who only have a high school degree, but the point

[29] One concern with the estimates in Table 5 is that the ACS changed the way it asked about the total number of years of schooling in 2008. We estimate equation (1) for the total years of schooling outcome using data only from 2008-2012 in Appendix Table A-3. The results are not statistically significantly different from those in Panel B of Table 5.

[30] Event study estimates for each of the educational attainment levels are available upon request from the authors.

estimates are small in absolute value and none is statistically significant at conventional levels. The small negative effect on reduced years of education appears to be rather evenly distributed throughout the educational attainment distribution.

The lack of strong educational attainment effects is somewhat surprising, especially given the large labor market effects we document. However, these results are consistent with some of the prior literature discussed in Section 2 that has not found an effect of duty-to-bargain law passage on high school dropout rates (e.g., Lovenheim 2009). The implication of the educational attainment results is that collective bargaining law exposure affects human capital in ways that are not fully captured by years of education or degree receipt. Our estimates likely reflect other aspects of human capital accumulation that do not appear in educational attainment measures, such as non-cognitive skills, and they highlight the value of examining labor market measures in order to draw a more complete picture of how teacher collective bargaining affects long-run outcomes. We return to this issue in Section 6 when we discuss effects on educational achievement and non-cognitive outcomes.

Our results suggest that male students experience worse long-run labor market outcomes when exposed to duty-to-bargain laws. As discussed previously, we are unable to fully examine the mechanisms that underlie this result due to lack of information on teacher productivity and only sparse data on schooling inputs from this time period. However, our results are consistent with Frandsen (2016), who shows that DTB law passage leads to fewer work hours among teachers. Litten (2017) also finds evidence from the restriction of collective bargaining rights in Wisconsin that teacher compensation is reduced, with the largest effect coming from non-wage compensation. Using the Census/Survey of Governments from 1972-1991, we estimate parametric event study models of DTB law passage on state average schooling resource allocations that allow for linear pre- and post-DTB trends as well as a level shift in the year of passage. Online Appendix Table A-9 presents suggestive evidence that DTB passage increases the total amount spent on teachers, especially relative to a negative pre-passage trend, but the largest effect is on administrative salary expenditures.[31] These expenditures increase dramatically following law passage, but total expenditures do not change. The shift toward

[31] Prior research using these data examine average teacher salaries, not total spending on teachers. This can account for some of the differences between these estimates and those in Hoxby (1996) and Frandsen (2017) as the composition of teachers also can change due to DTB law passage.

teaching and administrator salaries come at the expense of support service salaries. That these effects grow over time is another reason why the impacts of DTB laws become stronger with years post DTB passage, as shown in the event study results. It is plausible these changes could reduce school productivity, but we are unaware of research demonstrating a clear link between spending on school administration and student achievement. We also find no effect on teacher-student ratios.

5.2. Baseline Female Estimates

Tables 3-5 and Figures 2-4 also show results for women. In general, the estimates for women are attenuated relative to those for men. For example, in Panel A of Table 3, exposure to a duty-to-bargain law for all 12 years of schooling reduces female earnings by -$314.05. This estimate is not statistically significantly different from zero at even the 10% level, however. There also is evidence of reduced hours worked in Table 4 and lower employment and labor force participation in Table 4. The estimates in Table 5 on educational attainment and occupational skill level are close to zero and are not statistically significant.

Though the results in Tables 3-5 are suggestive of a small negative effect of collective bargaining law exposure among women on labor market outcomes, the event study estimates in Figures 2-4 indicate that these effects are biased by cross-cohort pre-DTB trends that are in the same direction as the treatment effects. Unlike the results for men, the pre-trends among women indicate that the small negative effects we find are spurious. The event studies show no evidence of a treatment effect of DTB exposure for women.

These female pre-treatment trends likely reflect strong secular shifts in labor market opportunities that have occurred for women over the cohorts we consider (Blau and Kahn 2013; Bick and Bruggeman 2014). The shifts happen to be negatively correlated with the timing of DTB passage, but it is clear that the forces driving these trends do not affect male outcomes; we find no evidence of a bias from such trends for males either visually or statistically when we control for cross-cohort pre-DTB outcome trends in Section 5.4. Thus, the data are inconsistent with an effect of duty-to-bargain law exposure among women on labor market outcomes, but there is a clear negative effect for men. Motivated by these findings, we focus much of the remainder of the analysis on men but also present female estimates for completeness.

What might explain our findings of strong negative effects among men but ostensibly no effects among women? We argue these results are consistent with a growing body of evidence that boys are more sensitive than girls to educational interventions and adverse shocks they experience during childhood (Autor et al. 2016; Fan et al. 2015; Autor and Wasserman 2013; Bertrand and Pan 2013; Krueger 1999). To the extent that DTB laws degrade the quality of the educational environment, which is consistent with our estimates, the heightened sensitivity of boys relative to girls found in prior research is in line with the negative long-run effects being concentrated among men.

5.3. Estimates by Race/Ethnicity

We show estimates by race and ethnicity in Table 6. Panels A and B present results for black and Hispanic men and white and Asian men, respectively, and Panels C and D present similar results for women. Examining results among blacks and Hispanics separately is of great interest, as urban areas that differentially service minority students were more likely to unionize first and to have stronger unions. Furthermore, the 1980s saw a relative erosion of labor market outcomes of young black men (Bound and Freeman 1992). This was a time period in which many of those exposed to a DTB law were entering the labor market, and examining effects for nonwhites versus whites could reveal substantial heterogeneity in treatment effects.

As shown in Panel A, the impact of duty-to-bargain law exposure is particularly large among black and Hispanic men: 12 years of exposure leads to a decline in earnings of $3,640 (10.6%), hours worked of 1.35 hours (4.1%), employment of 2.6 percentage points (3.7%), and labor force non-participation of 1.6 percentage points (7.6%). We also find a statistically significant decline in years of schooling of 0.21 years and a significant decline in occupational skill. All of these estimates are significant at the 5 or 1 percent levels. Panel (a) of Online Appendix Figures A-1 through A-6 present event study estimates for this sample. For each outcome, pre-DTB trends are either zero or in the wrong direction (i.e., opposite the direction of the treatment effect), and the effect grows with more exposure to a collective bargaining law. In short, these figures mirror the event study estimates for the male sample as a whole but are much larger in magnitude.

Panel B of Table 6 shows that the estimates are not isolated to black and Hispanic men; statistically significant adverse effects are present for white and Asian men as well, though they are more modest in magnitude. Earnings among white and Asian men decline by $1,150 (1.94%) with 12 years of DTB exposure, and employment declines by 0.6 of a percentage point (0.71%). Both of these estimates are significant at the 10% level. The other estimates are consistent with a decline in outcomes and are similar in magnitude to the baseline estimates, but they are not significantly different from zero.

Results in Panels C and D show that all of the negative point estimates identified for females in Tables 3-5 are driven by black and Hispanic women; estimates among white and Asian women are very close to zero and are neither economically nor statistically significant. As in the baseline estimates, event studies in Online Appendix Figures A-1 through A-6 show some evidence of differential pre-treatment trends in the same direction as the treatment effect among black and Hispanic women. These trends are not present for all outcomes, but the results in Panel C of Table 6 should be interpreted with caution given the event study results.

5.4. Robustness Checks

The baseline estimates support the rent-seeking theory of union behavior, whereby unions reduce the productivity of public schools and lead to a reduction in student achievement as well as subsequent long-run labor market outcomes. In this section, we explore evidence on whether our results are driven by other policies, trends or events that are not accounted for by the controls in equation (1).

We first show results from estimates of parametric event study models that directly control for pre-DTB trends. We construct a relative time to DTB law variable ($C - t_0 + 18$) that forms the basis for the relative time indicator variables in equation (2).[32] This variable takes on a value of zero in states that do not pass a duty-to-bargain law. We then estimate models of the following form:

$$Y_{sct} = \alpha_0 + \alpha_1(C - t_0 + 18)_{sc} + \alpha_2 I(DTB)_{sc} + \alpha_3(C - t_o + 18) * I(DTB)_{sc} + \gamma X_{sct} + \delta_{ct} + \theta_s + \phi_t + \varepsilon_{sct}. \quad (3)$$

All other variables are as previously defined. In equation (3), we allow for a level shift (α_2) and a slope shift (α_3) relative to any pre-treatment trend. Thus, this model is not biased by linear pre-DTB trends, so comparing these estimates to baseline provides some evidence of the importance of directly controlling for cross-cohort variation prior to DTB law passage.

Results of estimating equation (3) are shown in Table 7. The results strongly align with

[32] Similar to the event study estimates, we group relative time observations less than -10 and greater than 20 together. We do so to make this model as similar as possible to equation (2) and to avoid the estimates being unduly influenced by observations that are far away from the timing of treatment. This ensures we are identified off the 30 year period surrounding duty-to-bargain law passage.

the event study estimates and indicate that the male estimates are not biased by pre-treatment trends. For only one outcome is there a significant pre-treatment trend estimate, and it is in the opposite direction of the treatment effect. For all but unemployment and years of education, there are level and slope shifts that are of similar magnitudes to those in the baseline tables.[33] We can calculate percent effects after 12 years ($(\alpha_2 + \alpha_3 * 12)/\bar{Y}$), which are directly comparable to the percent effects shown in Tables 3-5. These calculations show an earnings effect of -5.84%, an hours worked effect of -3.40%, an employment effect of -2.68% and an occupational skill effect of -0.94%. Thus, these estimates are similar to, if somewhat larger than, the baseline results.

Panel B shows estimates of equation (3) for women. Consistent with the event studies, there are pre-treatment trends. Relative to those trends, there is little evidence of an effect of duty-to-bargain exposure on female labor market outcomes. Taken together, the results in Table 7 support our preferred interpretation of the baseline results that there are sizable adverse effects among men and no effects among women.

Table 8 presents additional robustness checks that each examines how our results and conclusions for men change when we control for additional factors in equation (1) that could be correlated with both duty-to-bargain exposure and long-run outcomes. Associated estimates for women are in Appendix Table A-6. In Panel A, we exclude the 14 states that do not have anti-strike penalties associated with their duty-to-bargain laws.[34] Teacher strikes may have an independent effect on student outcomes, and there is some evidence that resource effects of unions were larger in such states (Paglayan 2015). This specification produces estimates very similar to our baseline results.

It also could be the case that states becoming more favorable to teachers' unions were becoming more favorable to private sector unions as well. If the passage of public sector duty-to-bargain laws is correlated with the strength of private sector unions, it could bias our labor market estimates. In Panel B of Table 8, we control for the total unionization rate at age 18 for

[33] Although it is somewhat unexpected that there are level changes as well as slope changes in Table 7, these results are consistent with the level changes in resources we present in Appendix Table A-9. It is unlikely these level shifts represent unobserved systematic negative shocks because of the time-varying nature of the treatment and because the level shifts persist and strengthen post law passage.

[34] These states are Wisconsin, Connecticut, Michigan, Massachusetts, Rhode Island, Maine, Vermont, Alaska, Hawaii, Kansas, Pennsylvania, Idaho, Oregon and Montana.

each birth state-birth cohort.[35] The estimates are quite similar to our main results, and the conclusions one draws from the estimates in Panel C are the same as those discussed above.

The next two panels of Table 8 address the possibility that the rollout of duty-to-bargain laws is correlated with inner-city violence and white flight that occurred during the 1960s and 1970s. Such events likely had independent negative effects on long-run outcomes, which could be driving many of our results. First, we control for the average proportion of people in each state living in urban areas during each cohort's schooling years.[36] While we do not know if a respondent grew up in an inner city, the bias stemming from secular shocks occurring within cities should be correlated with the proportion of individuals living in inner-city areas. Furthermore, this control helps account for increasing suburbanization that was occurring when our analysis cohorts were in school. The results in Panel C that control for the percent urban are extremely similar to our baseline estimates.

Next, we use data on all riot and collective action protest events. Using the Dynamics of Collective Action dataset that includes counts of all collective action events from 1955-1995, we count the number of riots as well as the number of protests in which violence occurred in each state over the time period when each cohort was between 6 and 18.[37] This specification is designed specifically to examine the effect that the urban civil unrest in the 1960s and 1970s has on our estimates. Panel D of Table 8 contains the results that include this additional control, and the results are again extremely similar to those in the main analysis.[38]

In Panel E, we estimate models akin to the seminal Card and Krueger (1992a,b) analysis

[35] Unionization rates come from CPS Merged Outgoing Rotation Group data collected by Barry Hirsch and David Macpherson: http://www.unionstats.com. We also have performed this specification using the private sector unionization rate. Private sector union data at the state level are only available post-1982, however, which requires us to drop the 1956-1964 birth cohorts. Estimates from this regression on this sample are similar and are available upon request from the authors.

[36] Urban areas include those living in "urbanized areas" or in "incorporated places"/Census Designated Places (areas with a population of 2,500 or more outside of an urbanized area). This proportion is calculated using the 1960-1990 Decennial Censuses. We use each decennial Census estimate and average across cohorts using the percentage of their school-age years spent in each decade. We also have calculated the urban proportion in each state and in each census and then linearly interpolate across census years using the 1960-2000 Censuses. Using these state-year estimates, we then calculate the state-specific average over ages 6-18 for each cohort in our study. Results using this alternative method are extremely similar but we do not favor them because the Census Bureau changed how they defined urbanicity in the 2000 Census, complicating comparisons with earlier decades. These results are available upon request.

[37] The Dynamics of Collective Action dataset can be found at: http://web.stanford.edu/group/collectiveaction/cgi-bin/drupal/.

[38] We also have controlled for the number of collective action protest events including nonviolent events. Results are unchanged from those reported in the main tables.

of school quality on student earnings. They control for both state-of-birth and current state-of-residence fixed effects. The latter set of fixed effects account for the different labor markets in which workers are located that could be correlated with treatment. This is not our preferred specification because DTB laws might affect how students sort across labor markets later in life, which makes current location an endogenous mediating variable. Nonetheless, this is an instructive model to estimate to determine the empirical relevance of such sorting. We estimate this model with individual-level disaggregated data, and the results are larger in absolute value than baseline. We now find a statistically significant effect (at the 10% level) for years of education, but on the whole the estimates lead to similar conclusions to our baseline model. If anything, not accounting for current state of residence leads to more conservative estimates.

Panel F of Table 8 adds controls for state-by-year fixed effects. These estimates account for any birth state specific shocks or policies that affect all birth cohorts similarly in a state and year. The estimates are noisier than in the baseline models, but they are qualitatively similar. Some of the point estimates are slightly smaller and some are slightly larger, and we now see a statistically significant decline in years of education in this model. On the whole, these results are consistent with our preferred estimates and provide no evidence of bias from state-by-year specific shocks. Finally, in Panel G, we control for Census Region-by-cohort fixed effects. As Table 1 shows, there are strong regional differences in duty-to-bargain law passage. Some regions may be experiencing differential shocks during the time period in which these laws are passed, such as desegregation in the south. The estimates in Panel G use only within-region and cohort variation, and they are extremely similar to the baseline results if somewhat larger in absolute value. These results suggest we are not picking up different regional shocks or trends in our main estimates.

We also examine the sensitivity of our results to outliers by re-estimating equation (1) 50 times for all of our outcomes, each time dropping a different state from the analysis sample. The results from this exercise are shown in Figure 5 for four of our main outcomes: earnings, hours of work, employment, and labor force participation.[39] As the figure demonstrates, our male estimates are insensitive to excluding any one state: in no case do the qualitative or quantitative results change.

[39] The results for other outcomes and for women are extremely similar. We exclude them for parsimony, but they are available from the authors upon request.

As discussed above, of primary concern in our identification strategy is the existence of secular trends that differ across the treated and untreated states. The event study estimates for men suggest that any such trends were not correlated with timing of DTB passage. But, because we only have DTB passages rather than repeals, our results could be influenced by secular trends across never-passing states that differ from ever-passing states. An implication of such trends is that *any* cross-cohort comparisons between the DTB and non-DTB states would generate similar results, regardless of the timing of passage.

To examine this possibility, we perform permutation tests for all of our outcomes that randomly assign passage years to states with a duty-to-bargain law. We do this two ways: first, we randomly assign dates between 1960 and 1987 to all states that ever pass a law, and second we randomly assign dates to states that ever pass a law to match the aggregate passage distribution shown in Figure 1. Table 9 shows the results from these tests for men. We perform the permutation test 300 times for each outcome and calculate the percentage of times the simulated estimate is less than the actual estimate. These results therefore represent p-values of the null hypothesis that any combination of passage dates in the DTB states would generate the same outcome. As is shown in Table 9, we strongly reject such a null in every case. For all outcomes other than non-labor force participation, we do not get any simulated results that are smaller (i.e., more negative) than the baseline estimates. For the labor force non-participation, all estimates are smaller as the treatment effect is positive. These results suggest that we are not simply picking up aggregate differences between the treatment and control states. What matters is not whether a state passes a DTB law but when it does so, and as the event study estimates indicate, there are no differential pre-passage trends in outcomes across treated and control states. Taken together, the results from Table 9, the event study figures, and Table 7 strongly support the validity of our results.

A final identification issue comes from measurement error driven by either pre- or post-birth mobility. To assess the importance of pre-birth mobility, we estimate equation (1) using observed fixed characteristics in the ACS and some state-year level observables that are unlikely to be affected by teacher collective bargaining. Because we focus on state of birth, these estimates show whether the composition of people born in a given state and cohort changed with respect to duty-to-bargain law exposure. Online Appendix Table A-7 shows these results. We find no evidence of a change in the composition of birth cohorts that would indicate parents are systematically moving prior to having a child because of duty-to-bargain laws.

We next examine the relevance of post-birth mobility, which introduces measurement error into our DTB exposure variable. In the 1990 Census, 78.4% of 17-year-olds live in the state of their birth. If the resulting measurement error is classical, it should attenuate our estimates, but it is unlikely that such error is classical. In order to provide information about how serious any mobility-induced bias would be, we re-estimate equation (1) under two assumptions. In Panel A of Table 10, we show results for men that exclude the 37.7% of respondents who do not live in their birth state.[40] This will overstate the true effect if more high-skilled workers are induced to work out of state and if collective bargaining reduces worker skill as our results thus far suggest. Indeed, the estimates in Panel A are typically larger in absolute value than our baseline estimates, although they are close in magnitude.

In Panel B, we estimate equation (1) under the assumption that those who live in a state at age 17 other than their birth state spent all of their schooling years in that other state. Using the 1990 Census, we create a 50x50 matrix that contains the full joint distribution of state-of-birth and state at age 17. We then create a new dataset that contains 50 observations for each age-year-birth-state observation. Within each age-year-birth-state group, there is a separate observation for each potential state a respondent could have lived in at age 17. We then weight each observation by the proportion of the 1990 Census that was in the given birth state-state at 17 combination. All DTB and other state-specific variables are calculated using the assumed state at age 17, not the birth state. Standard errors are two-way clustered at the birth state, state at age 17 level (Cameron, Gelbach and Miller 2011).[41] The results in Panel B are very similar to baseline in magnitude and statistical significance. Taken together, the results in Table 10 suggest that any bias from post-birth mobility is small.

6. Medium-Term Effects on Cognitive and Non-Cognitive Outcomes

The negative effects of teacher collective bargaining on earnings and labor force participation suggest that duty-to-bargain laws lead students to obtain less human capital when in school. We now turn to direct evidence on how collective bargaining influences student cognitive and non-cognitive outcomes using data from the NLSY79. This is a nationally-

[40] Estimates for women are shown in Online Appendix Table A-8.
[41] Because this method requires aggregated data, we do not estimate this model for occupational skill.

representative dataset of students aged 14-22 in 1979, covering the 1957-1965 birth cohorts. These cohorts thus overlap with much of the variation in the passage of teacher collective bargaining laws shown in Figure 1.

Respondents in the NLSY79 data take the Armed Forced Qualifying Test (AFQT), which is our measure of cognitive skill. These scores are reported in age-specific percentiles. Non-cognitive skills come from three measures: the Rotter Locus of Control, the Rosenberg Self-esteem Scale and the Pearlin Mastery Scale. The Rotter Locus of Control measures the extent to which students believe they have control over their own lives. Thus, it is a measure of perceived self-determination, with higher scores indicating *less* internal control. Higher scores on this measure therefore translate into lower non-cognitive skills. The Rosenberg Self-esteem Scale is designed to measure a student's self-worth. Higher scores indicate higher reported self-esteem. Finally, the Pearlin Mastery Scale is a measure of the extent to which individuals perceive themselves in control of forces that significantly impact their lives. Respondents with higher measures report increased ability to determine the course of their own life.

We estimate models using these outcomes that are very similar to equation (1). All outcomes are measured in 1997, so we can only include birth cohort and state of residence at age 14 fixed effects (not birth cohort-year fixed effects). We also control for race and family income. The exposure measure is constructed identically to that in the ACS analysis. Estimates are weighted by the NLSY79 sample weights and standard errors are clustered at the state level.

Table 11 shows results from the estimation of our difference-in-difference model on cognitive and non-cognitive outcomes, separately by gender. We see consistent evidence that 12 years of exposure to a collective bargaining law negatively impacts both cognitive and non-cognitive scores among men. AFQT percentile declines by 10.2, a 20.9% effect relative to the mean. These estimates are consistent with Hansen, Heckman and Muller (2004), who show that AFQT scores can be positively impacted by schooling. All non-cognitive skill measures move in the direction of declining skill as well: the Rotter Locus of Control increases by 1.37 (16.3%), the Rosenberg Self-esteem Scale declines by 1.66 (7.3%) and the Pearlin Mastery Scale score is reduced by 2.27 (10.2%). The first two estimates are statistically different from zero at the 5% level, and the third is significant at the 10% level. The estimates for women tend to be smaller in absolute value though in similar direction to those of men. In particular, the effect on AFQT scores is less than half the size of the male estimate.

The results in Table 11 support the earnings and labor market results presented above. These cognitive and non-cognitive measures have been shown in prior research to be highly correlated with long-run outcomes (Heckman, Stixrud and Urzua 2006), and they provide more direct evidence consistent with the rent-seeking hypothesis. Teacher collective bargaining laws lead to a decline in the productivity of educational inputs, which reduces short-run cognitive and non-cognitive outcomes that are still evident into adulthood. Furthermore, these results help explain why the labor market effects of teacher collective bargaining are larger than the educational attainment effects: non-cognitive skills affect the former more than the latter (Heckman, Stixrud and Urzua 2006). The sum total of the evidence from the ACS and NLSY79 is remarkably consistent in showing that teacher duty-to-bargain laws negatively impact male long-run outcomes through their effects on the quality of education students receive.

7. Conclusion

This paper provides the first comprehensive analysis of the effect of state teacher duty to bargain laws on student long-run educational attainment and labor market outcomes. Prior work in this area has been hampered by the lack of student outcome data from the time period in which these laws were passed as well as by the lack of exogenous variation in collective bargaining laws in more recent years when there are better student outcome data. We overcome these limitations by linking adults from the 2005-2012 ACS to their state of birth and exploiting the timing of passage of duty-to-bargain laws across cohorts within a state and across states over time. Our estimates show that exposure to duty-to-bargain laws when 35-49 year old men were of school-age adversely affects their long-run outcomes. We do not find robust evidence of impacts on women, however.

Our results are consistent with the rent-seeking model of teachers' unions. Exposure to a duty-to-bargain law for all of one's grade school years lowers male earnings by $1,492.82, or 2.75%. A back-of-the-envelope calculation indicates these laws reduce total labor market earnings by $149.6 billion per year, which suggests that this modest marginal effect has large implications for earnings in the US due to the prevalence of duty-to-bargain laws. Our results also point to large impacts of collective bargaining laws on the extensive margin of labor supply among men: hours worked declines due to reductions in employment and decreases in labor force participation. Male occupational skill level also declines due to exposure to DTB laws.

However, overall educational attainment is only marginally affected by exposure to these laws. The negative earnings impacts we identify therefore reflect reductions in human capital that do not show up in educational attainment measures.

The negative effects of exposure to duty-to-bargain laws are largest among black and Hispanic men, although white and Asian men also are adversely impacted. In particular, yearly earnings decline by 10.6% and hours worked decreases by 4.1% among black and Hispanic males. We find more evidence of a decline in educational attainment for this group of men as well. Among white and Asian men, earnings decline by 1.9% and hours worked by 0.6%.

We complement these results with an analysis from the NLSY79 that shows duty-to-bargain laws reduce cognitive and non-cognitive outcomes among young adults, with effects that are larger for males than females. In total, our estimates indicate that state duty-to-bargain laws have sizable, negative labor market consequences for men who attended grade school in states with these laws.

From a policy perspective, these results contribute to the contentious debate occurring in many states about whether to limit the collective bargaining power of teachers. For example, in 2011 Wisconsin, Indiana, Tennessee and Idaho passed legislation that greatly reduced the ability of teachers to bargain with school districts, and in 2014 Michigan passed a public employee right-to-work law that sought to limit union negotiating power. Of first-order concern in this policy debate is how collective bargaining affects student outcomes. Our results provide the most comprehensive information to date on this question. However, there are a couple of caveats to generalizing these findings to current students. First, the cohorts we analyze were exposed to an educational environment very different from the one that exists today. For example school choice as well as teacher, school and student accountability policies that are currently rather ubiquitous were virtually nonexistent during the 1960s-1980s. Some of the effects of teacher collective bargaining we estimate could be driven by how teachers' unions interacted with specific aspects of the educational system that no longer are relevant. Second, the current collective bargaining law changes in many states alter aspects of collective bargaining, not the legality of collective bargaining itself. Examination of these policy changes will lend much insight into whether one can change collective bargaining laws to reduce the negative impacts on students we find while still providing teachers with the bargaining benefits they clearly value. We view this as an important set of questions for future research.

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Table 1: Teacher Duty-to-Bargain Law Passage by State

State	Year of Passage	State	Year of Passage
Alabama		Montana	1972
Alaska	1971	Nebraska	1987
Arizona		Nevada	1970
Arkansas		New Hampshire	1976
California	1977	New Jersey	1969
Colorado		New Mexico	
Connecticut	1966	New York	1968
Delaware	1970	North Carolina	
Florida	1976	North Dakota	1970
Georgia		Ohio	1985
Hawaii	1971	Oklahoma	1972
Idaho	1972	Oregon	1970
Illinois	1985	Pennsylvania	1971
Indiana	1974	Rhode Island	1967
Iowa	1976	South Carolina	
Kansas	1971	South Dakota	1971
Kentucky		Tennessee	1979
Louisiana		Texas	
Maine	1970	Utah	
Maryland	1970	Vermont	1968
Massachusetts	1966	Virginia	
Michigan	1966	Washington	1968
Minnesota	1973	West Virginia	
Mississippi		Wisconsin	1960
Missouri		Wyoming	

Source: NBER Public Sector Collective Bargaining Law Data Set (Valletta and Freeman 1988), updated by Kim Reuben to 1996. Blank entries reflect the absence of a teacher duty-to-bargain law in the state.

Table 2: Birth Cohorts by Age in Each ACS Year

Age	2005	2006	2007	2008	2009	2010	2011	2012
35	1970	1971	1972	1973	1974	1975	1976	1977
36	1969	1970	1971	1972	1973	1974	1975	1976
37	1968	1969	1970	1971	1972	1973	1974	1975
38	1967	1968	1969	1970	1971	1972	1973	1974
39	1966	1967	1968	1969	1970	1971	1972	1973
40	1965	1966	1967	1968	1969	1970	1971	1972
41	1964	1965	1966	1967	1968	1969	1970	1971
42	1963	1964	1965	1966	1967	1968	1969	1970
43	1962	1963	1964	1965	1966	1967	1968	1969
44	1961	1962	1963	1964	1965	1966	1967	1968
45	1960	1961	1962	1963	1964	1965	1966	1967
46	1959	1960	1961	1962	1963	1964	1965	1966
47	1958	1959	1960	1961	1962	1963	1964	1965
48	1957	1958	1959	1960	1961	1962	1963	1964
49	1956	1957	1958	1959	1960	1961	1962	1963

Notes: Authors' tabulations from 2005-2012 ACS data on 35-49 year old respondents. Birth cohorts are calculated by subtracting birth year from calendar year.

Table 3: The Effect of Collective Bargaining Laws on Earnings and Hours Worked

Panel A: Earnings						
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	-1542.93**	-1514.72**	-1492.82**	-355.66	-315.96	-314.05
	(602.44)	(611.11)	(599.72)	(281.66)	(286.00)	(290.66)
% Effect	-2.84%	-2.79%	-2.75%	-1.17%	-1.04%	-1.04%
Panel B: Hours Worked						
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	-0.534***	-0.529***	-0.523***	-0.475	-0.478	-0.480
	(0.173)	(0.177)	(0.177)	(0.333)	(0.334)	(0.337)
% Effect	-1.37%	-1.36%	-1.34%	-1.61%	-1.62%	-1.62%
Other Policy Controls		x	x		x	x
Birth Cohort*Survey Year FE			x			x

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Regressions are based on 6,000 birth state-cohort-year observations. All estimates include birth state, birth cohort and year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell. Other Policy Controls include school finance reform, EITC and food stamp measures as described in the text. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. % Effects show effects relative to the means presented in Table 3. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 4: The Effect of Collective Bargaining Laws on Labor Market Participation

			Panel A: Employed			
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	-0.011***	-0.011**	-0.011**	-0.012*	-0.011*	-0.011*
	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.006)
% Effect	-1.34%	-1.34%	-1.34%	-1.64%	-1.51%	-1.51%
			Panel B: Unemployed			
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	0.003	0.003	0.003	0.004*	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
% Effect	5.30%	5.30%	5.30%	8.31%	6.23%	6.23%
			Panel C: Not In Labor Force			
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	0.009**	0.008**	0.008**	0.008	0.008	0.008
	(0.003)	(0.003)	(0.003)	(0.007)	(0.007)	(0.007)
% Effect	7.38%	6.56%	6.56%	3.61%	3.61%	3.61%
Other Policy Controls		x	x		x	x
Birth Cohort*Survey Year FE			x			x

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Regressions are based on 6,000 birth state-cohort-year observations. All estimates include birth state, birth cohort and year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell. Other Policy Controls include school finance reform, EITC and food stamp measures as described in the text. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. % Effects show effects relative to the means presented in Table 3. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 5: The Effect of Collective Bargaining Laws on Occupational Skill and Educational Attainment

	Panel A: Occupational Skill					
	Men			Women		
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	-0.006**	-0.006**	-0.003*	-0.006	-0.006	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
% Effect	-0.97%	-0.97%	-0.48%	-1.07%	-0.97%	-0.16%
	Panel B: Years of Education					
	Men			Women		
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	-0.045	-0.042	-0.042	-0.011	-0.009	-0.009
	(0.032)	(0.031)	(0.032)	(0.030)	(0.030)	(0.030)
% Effect	-0.33%	-0.31%	-0.31%	-0.08%	-0.07%	-0.07%
Other Policy Controls		x	x		x	x
Birth Cohort*Survey Year FE			x			x

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. In Panel B, regressions are based on 6,000 birth state-birth cohort-year observations and include birth state, birth cohort and year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Other Policy Controls include school finance reform, EITC and food stamp measures as described in the text. In Panel A, the dependent variable is the percent of those in each respondent's occupation with more than a high school degree. Estimation of equation (1) is done using disaggregated data in Panel A and includes birth state, birth cohort and year fixed effects as well as controls for respondent race/ethnicity. % Effects show effects relative to the means presented in Table 3. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 6: The Effect of Collective Bargaining Laws on Long-Run Outcomes, by Race/Ethnicity

	Panel A: Black and Hispanic Men						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un-Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Occup. Skill (vii)
Exposure	-3640.12***	-1.347***	-0.026***	0.009*	0.016**	-0.205***	-0.007**
	(1056.59)	(0.339)	(0.009)	(0.005)	(0.007)	(0.045)	(0.003)
% Effect	-10.57%	-4.06%	-3.69%	10.64%	7.58%	-1.62%	-1.07%

	Panel B: White and Asian Men						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un-Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Occup. Skill (vii)
Exposure	-1149.61*	-0.223	-0.006*	0.002	0.004	-0.043	-0.002
	(583.15)	(0.167)	(0.003)	(0.003)	(0.003)	(0.037)	(0.002)
% Effect	-1.94%	-0.55%	-0.71%	4.05%	4.02%	-0.32%	-0.33%

	Panel C: Black and Hispanic Women						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un-Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Occup. Skill (vii)
Exposure	-1308.87**	-0.997**	-0.024***	0.010**	0.014	-0.140***	-0.004
	(592.54)	(0.373)	(0.008)	(0.004)	(0.009)	(0.050)	(0.004)
% Effect	-5.01%	-3.32%	-3.41%	13.52%	6.30%	-1.07%	-0.68%

	Panel D: White and Asian Women						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un-Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Occup. Skill (vii)
Exposure	-52.34	-0.074	-0.004	0.003	0.001	-0.010	-0.001
	(352.72)	(0.232)	(0.005)	(0.002)	(0.006)	(0.041)	(0.002)
% Effect	-0.17%	-0.25%	-0.54%	7.35%	0.45%	-0.07%	-0.18%

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Regressions are based on 6,000 birth state-cohort-year observations. All estimates include birth state, year and birth cohort-by-year fixed effects as well as controls for exposure to school finance reform, food stamps and EITC when of school age. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-birth cohort-year-gender-race cell. % Effects show effects relative to the mean of each variable. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 7: Parametric Event Study Estimates of the Effect of Collective Bargaining Laws on Long-Run Outcomes

Panel A: Men							
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un-Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Occup. Skill (vii)
Relative Years to DTB Law	42.69	0.025	0.0005	-0.00003	-0.0004	0.004	0.0005***
	(71.43)	(0.023)	(0.0006)	(0.0003)	(0.0004)	(0.004)	(0.0002)
I(DTB Law)	-1228.81***	-0.486***	-0.010***	0.001	0.009***	0.0001	-0.001
	(457.18)	(0.116)	(0.003)	(0.001)	(0.003)	(0.0188)	(0.001)
Relative Years to DTB Law* I(DTB Law)	-161.82**	-0.070***	-0.001**	-0.00003	0.0012***	-0.0003	-0.0004**
	(72.37)	(0.022)	(0.0005)	(0.00029)	(0.0004)	(0.0037)	(0.0002)

Panel B: Women							
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un-Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Occup. Skill (vii)
Relative Years to DTB Law	-105.24**	-0.061	-0.001	-0.0001	0.001	-0.0004	-0.0005**
	(49.35)	(0.043)	(0.001)	(0.0004)	(0.001)	(0.0043)	(0.0002)
I(DTB Law)	448.56	-0.074	-0.006	0.004**	0.002	0.015	0.002
	(311.89)	(0.199)	(0.004)	(0.002)	(0.004)	(0.024)	(0.001)
Relative Years to DTB Law* I(DTB Law)	92.51*	-0.002	-0.001	0.0001	0.001	0.002	0.0006***
	(50.50)	(0.043)	(0.001)	(0.0003)	(0.001)	(0.004)	(0.0002)

Notes: Authors' estimation as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative Years to DTB Law is the number of years relative to the passage of a duty-to-bargain law, which is set to zero for states that never pass such a law. I(DTB Law) is an indicator for whether a duty-to-bargain law has been passed in the state. Regressions are based on 6,000 birth state-birth cohort-year observations. All estimates include birth state, year and birth cohort-by-year fixed effects as well as controls for racial/ethnic composition of the state-cohort-year-gender cell, exposure to school finance reform, food stamps and EITC when of school age. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 8: The Effect of Collective Bargaining Laws on Long-Run Outcomes for Men – Robustness Checks

	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Panel A: Excluding States that Allow Teachers to Strike						
Exposure	-1869.16**	-0.680***	-0.013***	0.010***	-0.050	-0.002
	(699.55)	(0.187)	(0.003)	(0.003)	(0.036)	(0.002)
Panel B: Controlling for Total Union Membership at Age 18						
Exposure	-1466.11**	-0.456***	-0.010***	0.007*	-0.044	-0.003*
	(587.52)	(0.154)	(0.003)	(0.003)	(0.033)	(0.002)
Panel C: Controlling for Proportion Living in Urban Areas						
Exposure	-1515.62***	-0.529***	-0.011***	0.008***	-0.044	-0.003*
	(586.50)	(0.167)	(0.004)	(0.003)	(0.031)	(0.002)
Panel D: Controlling for Riots and Violent Protests						
Exposure	-1656.31***	-0.520***	-0.011***	0.009**	-0.052*	-0.003*
	(515.30)	(0.182)	(0.004)	(0.003)	(0.031)	(0.002)
Panel E: Controlling for Current State Fixed Effects (Individual-level Data)						
Exposure	-1907.53***	-0.512***	-0.011***	0.008**	-0.074*	-0.003*
	(629.25)	(0.183)	(0.003)	(0.004)	(0.038)	(0.002)
Panel F: Including Birth State-by-Year Effects						
Exposure	-1383.55**	-0.634**	-0.012**	0.011**	-0.056*	-0.003*
	(551.65)	(0.265)	(0.004)	(0.005)	(0.031)	(0.002)
Panel G: Including Census Region-by-Cohort Fixed Effects						
Exposure	-1522.31**	-0.575**	-0.012***	0.009**	-0.046	-0.003**
	(567.77)	(0.221)	(0.004)	(0.004)	(0.032)	(0.001)

Notes: All estimates include birth state, year and birth cohort-by-year fixed effects. Occupational skill results and estimates in Panel E are based on individual data and control for race/ethnicity. Other outcomes are estimated using aggregated data and control for racial/ethnic composition of the state-cohort-year-gender cell. Regressions using aggregated data are weighted by the number of individual observations that are used to calculate the averages in each state-year-cohort-gender cell. In Panel (A) we exclude the the 14 states that allow teachers to strike. Union membership data used in Panel (B) come from CPS MORG. In Panel (C), we control for the average proportion of individuals in one's birth state living in a metro area during one's schooling years. Panel (D) controls for the number of riots and violent protests that occurred in one's birth state during one's schooling years. The riot/protest data come from Dynamics of Collective Action Dataset: http://web.stanford.edu/group/collectiveaction/cgi-bin/drupal/. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 9: P-Values of Permutation Tests for Men

Panel A: Randomly Assigning Passage Dates

	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
% Less than Baseline	0.000	0.000	0.000	1.000	0.000	0.000

Panel B: Randomly Assigning Passage Dates to Match Passage Timing Distribution

	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
% Less than Baseline	0.000	0.000	0.000	1.000	0.000	0.000

Notes: All estimates include birth state, year and birth cohort-by-year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell, exposure to school finance reforms, average state EITC and average food stamp availability during school years. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. The table shows the proportion of times the estimates from the permutation tests are smaller than the baseline estimates. In Panel (A), we run 300 simulations in which we randomly assign passage dates to states that ever pass a DTB law. In Panel (B), we randomly assign passage dates to states that ever pass a DTB law in a way that matches the overall date-of-passage distribution shown in Figure 1.

Table 10: The Effect of Collective Bargaining Laws on Long-Run Outcomes for Men – Accounting for Mobility

Panel A: Dropping Those Who do not Live in State of Birth

	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-2162.96**	-0.625**	-0.009	0.008*	-0.085*	-0.004***
	(1011.36)	(0.261)	(0.006)	(0.004)	(0.043)	(0.002)

Panel B: Weighting by Childhood Mobility

	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-1745.66***	-0.511**	-0.012***	0.007***	-0.052**	
	(417.85)	(0.110)	(0.002)	(0.002)	(0.021)	

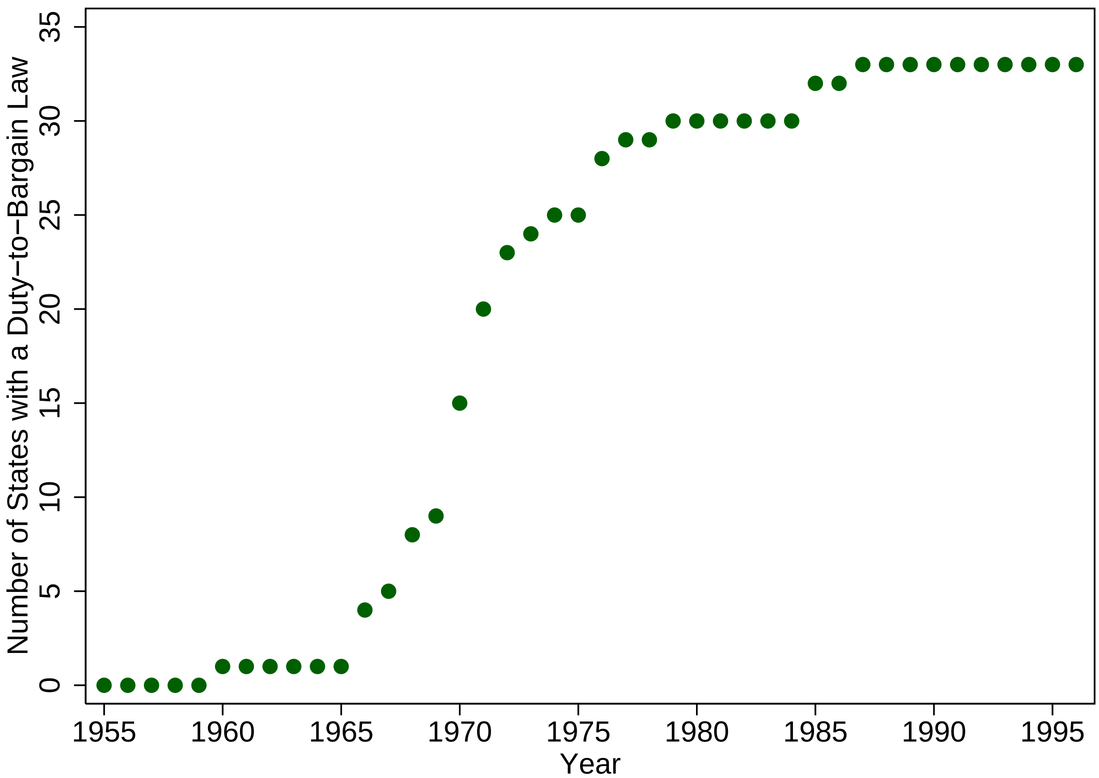
Notes: All estimates include state, year and birth cohort-by-year fixed effects, as well as controls for the racial/ethnic composition of the state-cohort-year-gender cell, exposure to school finance reforms, average state EITC and average food stamp availability during school years. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. In Panel (A), we exclude the 37.7% of respondents who do not live in their state of birth. In Panel (B), we expand the data to be at the state of birth-cohort-potential migration state level and weight each observation by the proportion of 17 year olds in the 1990 census who were born in the birth state and lived in the migration state. All variables are defined using the migration state, assuming students went to school in the migration state for all 12 years. Standard errors clustered at the birth state level in Panel (A) and two-way clustered at the birth state and migration state in Panel (B) are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table 11: The Effect of Teacher Collective Bargaining on Cognitive and Non-Cognitive Student Outcomes, NLSY79

	Panel A: Men			
Dep. Var.	1997 AFQT Percentile	Rotter Locus of Control	Rosenberg Self-Esteem Scale	Pearlin Mastery Scale
Exposure	-10.15**	1.37**	-1.66*	-2.27
	(4.17)	(0.044)	(0.92)	(1.49)
% Effect	-20.9%	16.3%	-7.3%	-10.2%
Mean	48.54	8.41	22.68	22.29
	Panel B: Women			
Dep. Var.	1997 AFQT Percentile	Rotter Locus of Control	Rosenberg Self-Esteem Scale	Pearlin Mastery Scale
Exposure	-4.53	1.24**	-1.24	-1.55**
	(7.02)	(0.41)	(0.80)	(0.63)
% Effect	-9.5%	14.4%	-5.5%	-7.0%
Mean	47.78	8.59	22.37	22.12

Notes: Data come from NLSY79 (1957-1965 birth cohorts). All outcomes are measured in 1979. Models include controls for race and family income as well as state at age 14 and birth cohort fixed effects. All estimates are weighted by the NLSY79 sample weights. The Rotter Locus of Control measures the extent to which students believe they have control over their lives: higher scores indicate less internal control (i.e., self-determination). The Rosenberg Self-Esteem Scale measures questions of self-worth, with higher scores associated with higher self-esteem. The Pearlin Mastery Scale measures the extent to which individuals perceive themselves in control of forces that significantly impact their lives, with higher scores indicating more control. Standard errors are clustered at the state level: ** indicates significance at the 5% level and * indicates significance at the 10% level.

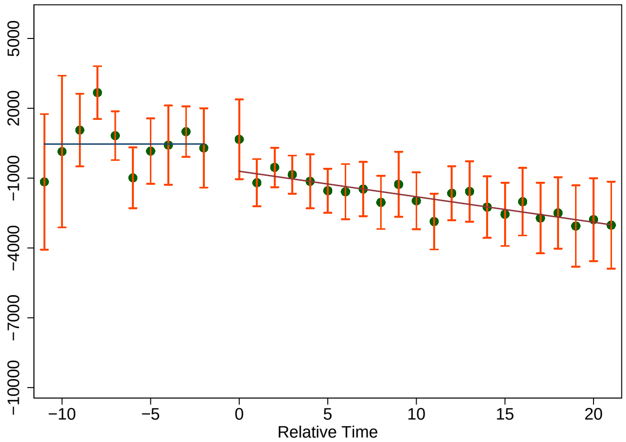
Figure 1: The Number of States with Teacher Duty-to-Bargain Laws over Time



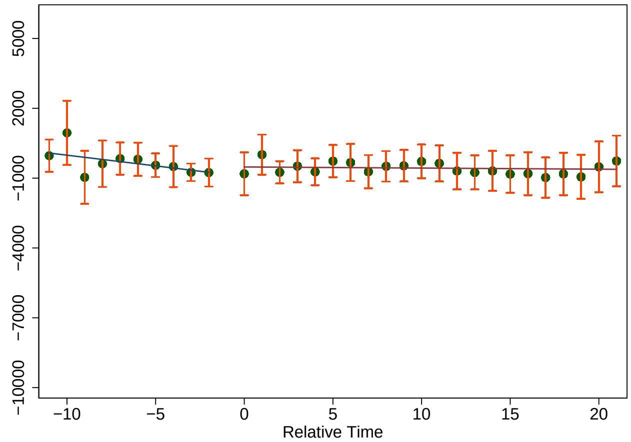
Source: NBER Public Sector Collective Bargaining Law Data Set (Valletta and Freeman 1988), updated by Kim Reuben to 1996.

Figure 2: Event Study Estimates - Earnings and Hours Worked

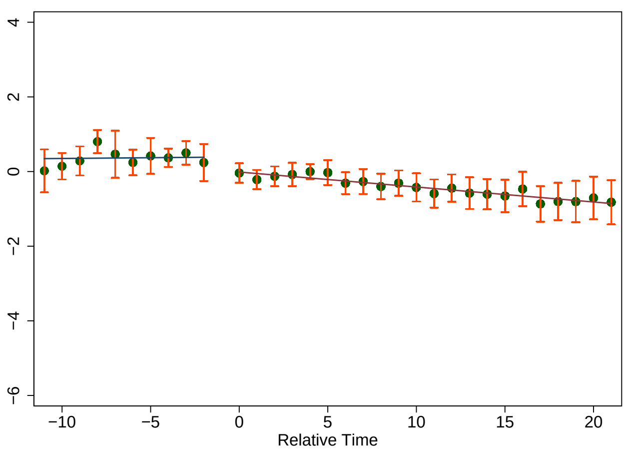
(a) Male Earnings



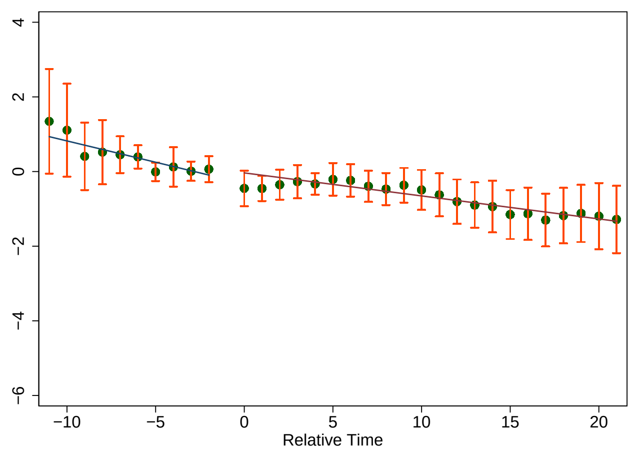
(b) Female Earnings



(c) Male Hours Worked

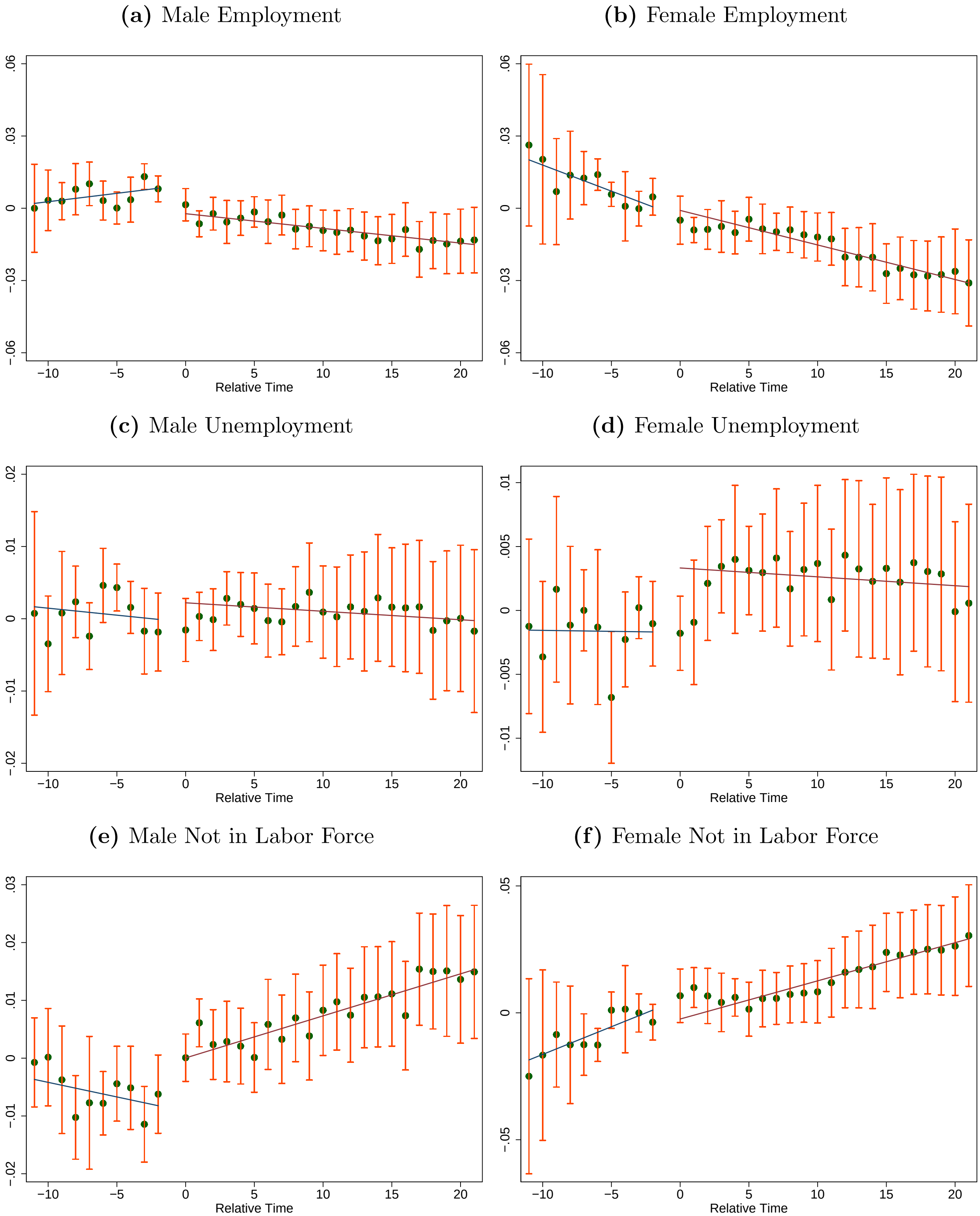


(d) Female Hours Worked



Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -11 includes all observations with relative time ≤-11 and relative year 21 includes all observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects as well as controls for the racial/ethnic composition of the state-cohort-year-gender cell and exposure to school finance reforms, state EITC rates, and food stamps. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the state level.

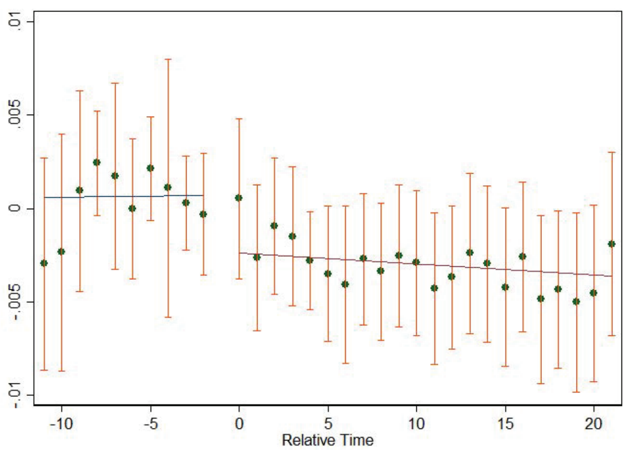
Figure 3: Event Study Estimates - Employment Outcomes



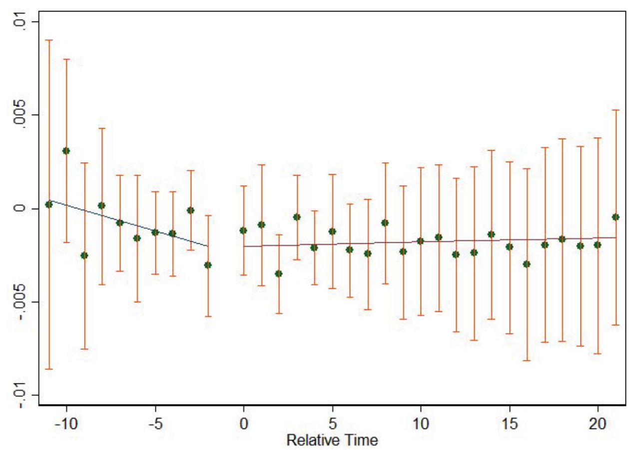
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -11 includes all observations with relative time ≤-11 and relative year 21 includes all observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects as well as controls for the racial/ethnic composition of the state-cohort-year-gender cell and exposure to school finance reforms, state EITC rates, and food stamps. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the state level.

Figure 4: Event Study Estimates - Occupational Skill and Years of Education

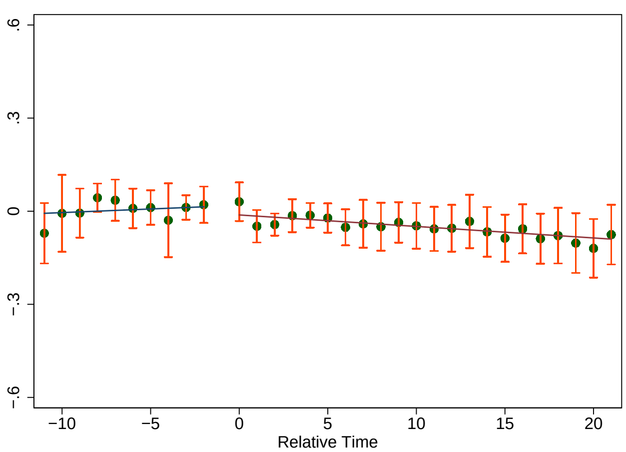
(a) Male Occupational Skill



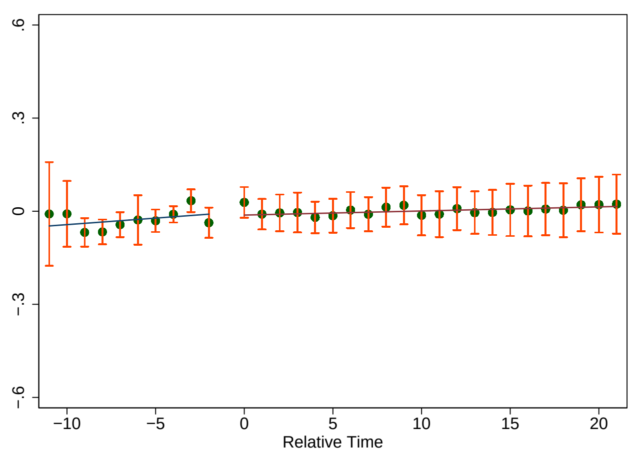
(b) Female Occupational Skill



(c) Male Years of Education



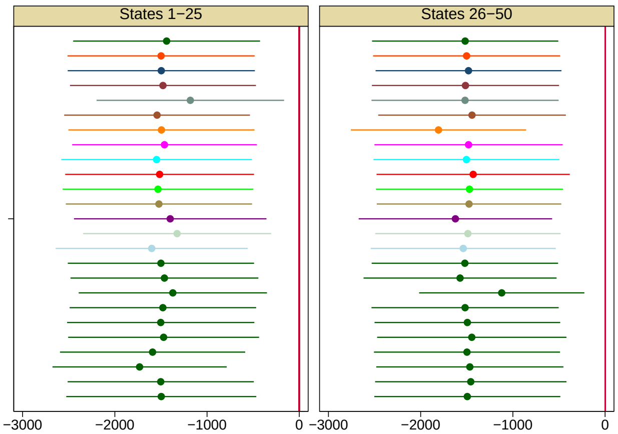
(d) Female Years of Education



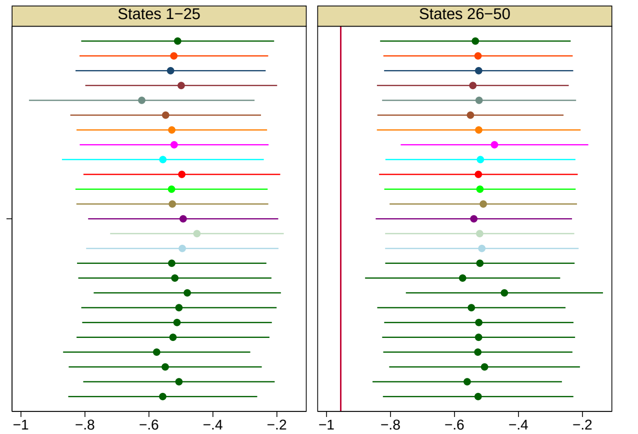
Notes: Relative year -1 is omitted; all estimates are in relationship to this year. Relative year -11 includes observations with relative time ≤-11 and relative year 21 includes observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects, controls for the racial/ethnic composition of the state-cohort-year-gender cell, and exposure to school finance reforms, state EITCs, and food stamps. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors clustered at the state level.

Figure 5: Sensitivity of Results to Excluding Each State - Men

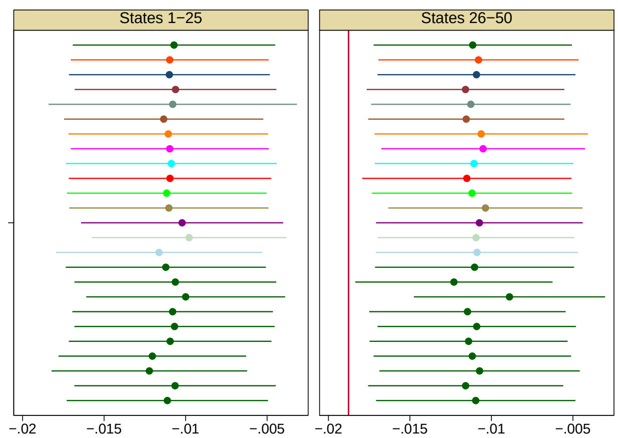
(a) Income



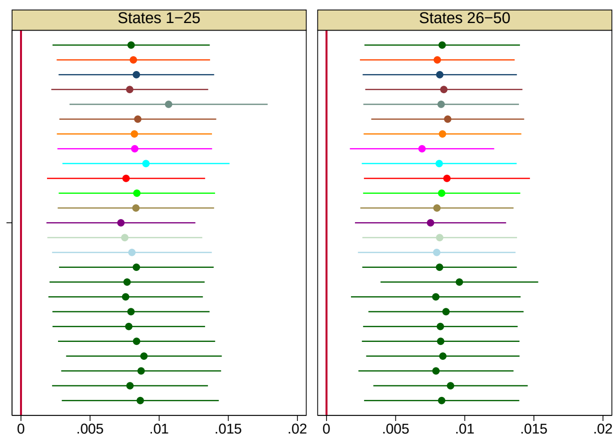
(b) Hours Worked



(c) Employment



(d) Not in Labor Force



Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Each point represents a point estimate excluding a given state from the regression and the lines extending from each point show the 95% confidence interval calculated using standard errors that are clustered at the state level.

Online Appendix: Not for Publication

Table A-1: Summary Statistics of Analysis Variables

	Men		Women	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Age	42.426	4.307	42.456	4.308
Asian	0.010	0.033	0.010	0.033
Black	0.128	0.096	0.144	0.106
Hispanic	0.064	0.088	0.063	0.088
Other	0.010	0.021	0.010	0.023
DTB	0.625	0.484	0.619	0.486
Years Exposed	4.710	5.613	4.646	5.599
Average EITC	0.001	0.011	0.001	0.011
Court-Ordered School Finance Reform	0.993	3.125	0.981	3.106
Legislative School Finance Reform	1.585	3.785	1.554	3.752
Food Stamp Exposure	0.625	0.325	0.621	0.326
Total Income	54,295.50	8,562.10	30,332.68	4,561.59
Hours Worked	38.964	2.112	29.552	1.685
Employed	0.822	0.046	0.730	0.043
Unemployed	0.057	0.025	0.048	0.020
Not in Labor Force	0.122	0.036	0.222	0.038
Years of Education	13.443	0.391	13.689	0.393
Occupational Skill Level	0.619	0.154	0.559	0.130
High School Degree	0.292	0.062	0.250	0.061
Some College	0.217	0.041	0.238	0.044
Associates Degree	0.081	0.023	0.109	0.026
Bachelors Degree	0.286	0.060	0.313	0.065

Notes: Authors' tabulations from 2005-2012 ACS data on 35-49 year old respondents. Tabulations are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell.

Table A-2: Summary Statistics of Analysis Variables By Gender and Race/Ethnicity

	Black & Hispanic Men		White & Asian Men		Black & Hispanic Women		White & Asian Women	
Variable	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Age	42.039	4.319	42.506	4.300	42.100	4.336	42.537	4.298
Duty-to-Bargain Law	0.503	0.500	0.650	0.477	0.491	0.500	0.648	0.478
Years Exposed	3.340	5.216	4.993	5.650	3.254	5.173	4.962	5.644
Average EITC	0.000	0.007	0.001	0.012	0.000	0.006	0.001	0.012
Court-Ordered School Finance Reform	1.271	3.535	0.936	3.030	1.199	3.439	0.932	3.023
Legislative School Finance Reform	1.264	3.469	1.651	3.844	1.218	3.409	1.630	3.821
Food Stamp Exposure	0.665	0.317	0.617	0.326	0.654	0.321	0.613	0.327
Total Income	34,434.89	7,630.40	59,326.87	9,233.07	26,149.84	5,273.62	31,486.16	4,861.68
Hours Worked	33.196	3.863	40.386	1.948	29.990	3.100	29.420	1.907
Employed	0.704	0.089	0.851	0.042	0.704	0.075	0.737	0.046
Unemployed	0.085	0.052	0.049	0.024	0.074	0.045	0.041	0.019
Not in Labor Force	0.211	0.074	0.100	0.032	0.222	0.069	0.222	0.043
Occupational Skill Level	0.652	0.151	0.614	0.154	0.585	0.133	0.553	0.129
Years of Education	12.641	0.485	13.644	0.397	13.042	0.486	13.870	0.411
High School Degree	0.332	0.098	0.281	0.064	0.277	0.088	0.243	0.065
Some College	0.239	0.077	0.212	0.043	0.275	0.073	0.227	0.047
Associates Degree	0.072	0.043	0.083	0.025	0.098	0.047	0.112	0.028
Bachelors Degree	0.155	0.063	0.319	0.063	0.201	0.069	0.344	0.072

Notes: Authors' tabulations from 2005-2012 ACS data on 35-49 year old respondents. Tabulations are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell.

Table A-3: The Effect of Collective Bargaining Laws on Years of Education, 2008-2012 ACS Years Only

Treatment Measure	All Men (i)	Black & Hispanic Men (ii)	White & Asian Men (iii)	All Women (iv)	Black & Hispanic Women (v)	White & Asian Women (vi)
Exposure	-0.045	-0.189**	-0.042	0.011	-0.146**	0.016
	(0.032)	(0.055)	(0.032)	(0.033)	(0.062)	(0.044)

Notes: Authors' estimation of equation (1) as described in the text using 2008-2012 ACS data on 35-49 year old respondents. Regressions are based on 6,000 birth state-cohort-year observations. All estimates include birth state, year, and birth cohort-by-year fixed effects as well as controls school finance reform, EITC and food stamp measures as described in the text. Estimates in columns (i) and (iv) include controls for race/ethnicity. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A-4: The Effect of Collective Bargaining Laws on Educational Attainment Levels

	Panel A: HS Grad					
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	0.002	0.002	0.002	0.004	0.005	0.005
	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)
	Panel B: Some College					
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	-0.004	-0.005	-0.005	-0.007	-0.008	-0.008
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
	Panel C: AA Completion					
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	-0.00001	-0.00003	-0.0001	0.002	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
	Panel D: BA Completion					
		Men			Women	
Treatment Measure	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Exposure	-0.003	-0.002	-0.002	-0.00003	0.001	0.001
	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.008)
Other Policy Controls		x	x		x	x
Birth Cohort*Survey Year FE			x			x

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Regressions are based on 6,000 birth state-cohort-year observations for each gender. All estimates include birth state, birth cohort and year fixed effects as well as controls for race/ethnicity, school finance reform, EITC and food stamp measures as described in the text. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A-5: The Effect of Collective Bargaining Laws on Educational Attainment Levels, by Race/Ethnicity

Panel A: Black & Hispanic Men				
	HS	Some		
	Grad	College	AA	BA
Treatment Measure	(i)	(ii)	(iii)	(iv)
Exposure	0.008	-0.022***	-0.004	-0.013**
	(0.008)	(0.007)	(0.005)	(0.005)

Panel B: White & Asian Men				
	HS	Some		
	Grad	College	AA	BA
Treatment Measure	(i)	(ii)	(iii)	(iv)
Exposure	0.006	-0.003	-0.002	-0.004
	(0.009)	(0.006)	(0.005)	(0.006)

Panel C: Black & Hispanic Women				
	HS	Some		
	Grad	College	AA	BA
Treatment Measure	(i)	(ii)	(iii)	(iv)
Exposure	0.017*	-0.014*	-0.010**	-0.008
	(0.009)	(0.007)	(0.004)	(0.007)

Panel D: White & Asian Women				
	HS	Some		
	Grad	College	AA	BA
Treatment Measure	(i)	(ii)	(iii)	(iv)
Exposure	0.008	-0.008	0.004	-0.002
	(0.010)	(0.006)	(0.005)	(0.008)

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Regressions are based on 6,000 birth state-cohort-year observations for each gender and race. All estimates include birth state, year and birth cohort-by-year fixed effects as well as controls for school finance reform, EITC and food stamp measures as described in the text. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender-race cell. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A-6: The Effect of Collective Bargaining Laws on Long-Run Outcomes for Women – Robustness Checks

	Earnings	Hours Worked	Employed	Not in Labor Force	Years of Education	Occup. Skill
	(i)	(ii)	(iv)	(v)	(v)	(vi)
Panel A: Excluding States that Allow Teachers to Strike						
Exposure	-377.12	-0.590*	-0.014**	0.009	-0.012	-0.000
	(319.58)	(0.324)	(0.005)	(0.007)	(0.032)	(0.002)
Panel B: Controlling for Total Union Membership at Age 18						
Exposure	-206.61	-0.389	-0.009*	0.006	-0.010	-0.001
	(261.08)	(0.305)	(0.005)	(0.006)	(0.031)	(0.002)
Panel C: Controlling for Proportion Living in Urban Areas						
Exposure	-326.54	-0.499	-0.012**	0.008	-0.009	-0.001
	(277.19)	(0.299)	(0.005)	(0.006)	(0.030)	(0.002)
Panel D: Controlling for Riots and Violent Protests						
Exposure	-303.15	-0.460	-0.011*	0.008	-0.008	-0.001
	(291.05)	(0.338)	(0.006)	(0.007)	(0.030)	(0.002)
Panel E: Controlling for Current State Fixed Effects (Individual-level Data)						
Exposure	-509.82*	-0.482	-0.012**	0.009	-0.032	-0.001
	(275.80)	(0.323)	(0.006)	(0.007)	(0.034)	(0.002)
Panel F: Including Birth State-by-Year Effects						
Exposure	-317.30	-0.404	-0.009	0.008	-0.014	-0.001
	(347.46)	(0.399)	(0.008)	(0.008)	(0.033)	(0.002)
Panel G: Including Census Region-by-Year Fixed Effects						
Exposure	-319.74	-0.450	-0.010	0.007	-0.007	-0.001
	(316.78)	(0.347)	(0.006)	(0.007)	(0.030)	(0.002)

Notes: All estimates include birth state, year and birth cohort-by-year fixed effects. Occupational skill results and estimates in Panel E are based on individual data and control for race/ethnicity. Other outcomes are estimated using aggregated data and control for racial/ethnic composition of the state-cohort-year-gender cell. Regressions using aggregated data are weighted by the number of individual observations that are used to calculate the averages in each state-year-cohort-gender cell. In Panel (A) we exclude the the 14 states that allow teachers to strike. Union membership data used in Panel (B) come from CPS MORG. In Panel (C), we control for the average proportion of individuals in one's birth state living in a metro area during one's schooling years. Panel (D) controls for the number of riots and violent protests that occurred in one's birth state during one's schooling years. The riot/protest data come from Dynamics of Collective Action Dataset: http://web.stanford.edu/group/collectiveaction/cgi-bin/drupal/. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A-7: The Correlation of Duty-to-Bargain Exposure with Fixed Individual Characteristics and State Observables Unrelated to Collective Bargaining

	Panel A: Men						
	Age	Black	Hispanic	Asian	Other Race	Fraction Homeowner	Fraction State Male
	(i)	(ii)	(iv)	(v)	(v)	(vi)	(vii)
Exposure	0.000	0.009	0.037	0.001	0.001	-0.000	0.001
	(0.000)	(0.009)	(0.028)	(0.002)	(0.001)	(0.003)	(0.002)
	Panel B: Women						
	Age	Black	Hispanic	Asian	Other Race	Fraction Homeowner	Fraction State Male
	(i)	(ii)	(iv)	(v)	(v)	(vi)	(vii)
Exposure	0.000	0.009	0.031	0.003	0.001	0.003	0.000
	(0.000)	(0.010)	(0.026)	(0.003)	(0.001)	(0.003)	(0.002)

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. All estimates include state, year and birth cohort-by-year fixed effects. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. State-specific outcomes are averaged over the individual ACS observations, which is why the male and female estimates differ numerically for these outcomes. Standard errors clustered at the birth state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Table A-8: The Effect of Collective Bargaining Laws on Long-Run Outcomes for Women – Accounting for Mobility

	Panel A: Dropping Those Who do not Live in State of Birth					
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-539.19	-0.427	-0.013*	0.009	-0.030	-0.001
	(336.74)	(0.378)	(0.007)	(0.008)	(0.045)	(0.003)

	Panel B: Weighting by Childhood Mobility					
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-349.73*	-0.405**	-0.011**	0.006	-0.012	
	(187.61)	(0.204)	(0.004)	(0.004)	(0.020)	

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. All estimates include state, year and birth cohort-by-year fixed effects, as well as controls for the racial/ethnic composition of the state-cohort-year-gender cell, exposure to school finance reforms, average state EITC and average food stamp availability during school years. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. In Panel (A), we exclude the 37.7% of respondents who do not live in their state of birth. In Panel (B), we expand the data to be at the state of birth-cohort-potential migration state level and weight each observation by the proportion of 17 year olds in the 1990 census who were born in the birth state and lived in the migration state. All variables are defined using the migration state, assuming students went to school in the migration state for all 12 years. Standard errors clustered at the birth state level in Panel (A) and two-way clustered at the birth state and migration state in Panel (B) are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

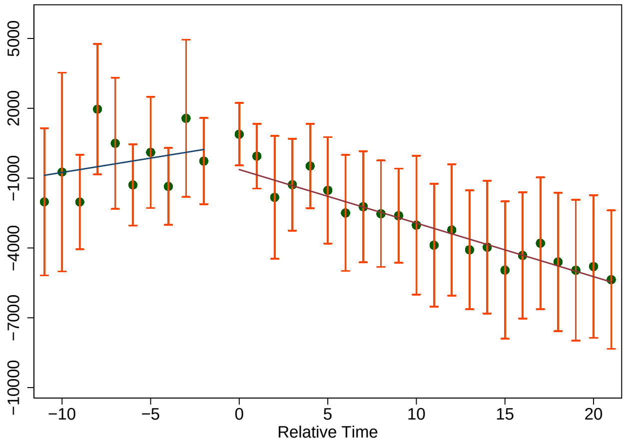
Table A-9: The Relationship Between Duty-to-Bargain Laws and School Resources

	Dependent Variable: Log of				
	Teacher Salary Expenditures (i)	Administrative Salary Expenditures (ii)	Other Salary Expenditures (iv)	Teacher-Student Ratio (v)	Operating Expenditures per Student (v)
Relative Years to DTB	-0.038***	-0.064	0.053***	-0.005	-0.005
	(0.011)	(0.038)	(0.012)	(0.003)	(0.020)
I(DTB)	0.080	0.470*	-0.196***	0.044	0.057
	(0.066)	(0.269)	(0.066)	(0.031)	(0.059)
(Relative Years to DTB)*I(DTB)	0.038***	0.089**	-0.044***	-0.005	0.008
	(0.010)	(0.022)	(0.007)	(0.009)	(0.009)

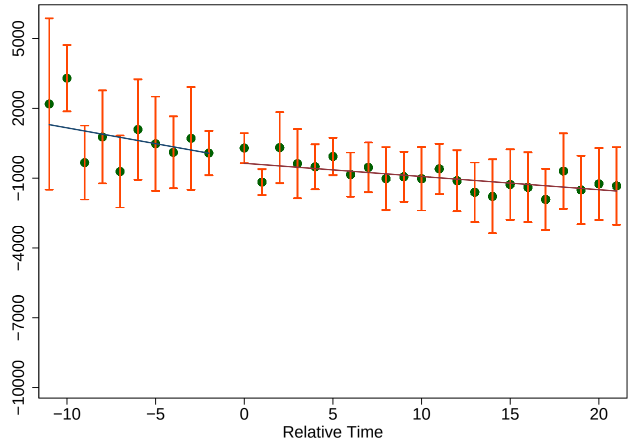
Notes: Authors' estimation as described in the text using 1972-1991 Census/Survey of Governments Data. The data vary at the state-year level and all estimates include state and fixed effects. Regressions are weighted by total enrollment in each state. Relative Years to DTB is a variable that shows the number of years since or to the passage of a DTB law, and I(DTB) is an indicator variable equal to one if a DTB law has passed in the state. All outcome variables are in logs, and salary expenditures reflect total expenditures on each category including part-time and full-time teachers. Standard errors clustered at the state level are in parentheses: *** indicates significance at the 1% level, ** indicates significance at the 5% level and * indicates significance at the 10% level.

Figure A-1: Event Study Estimates by Gender and Race/Ethnicity - Earnings

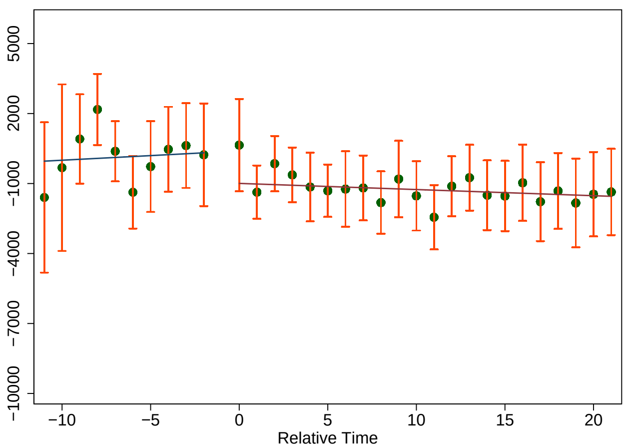
(a) Black & Hispanic Males



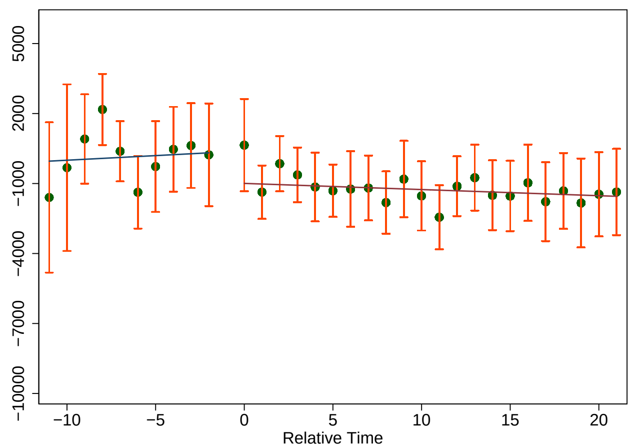
(b) Black & Hispanic Females



(c) White & Asian Males



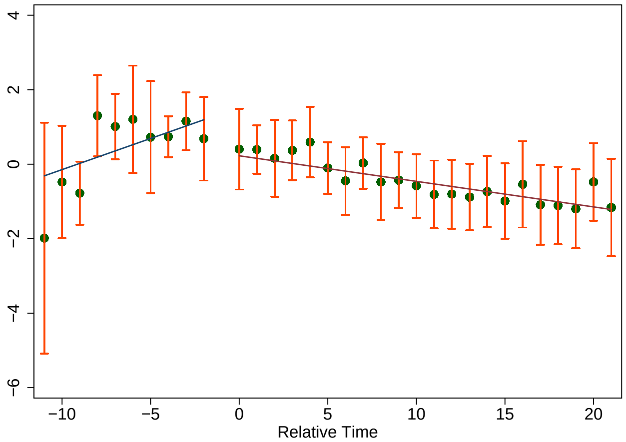
(d) White & Asian Females



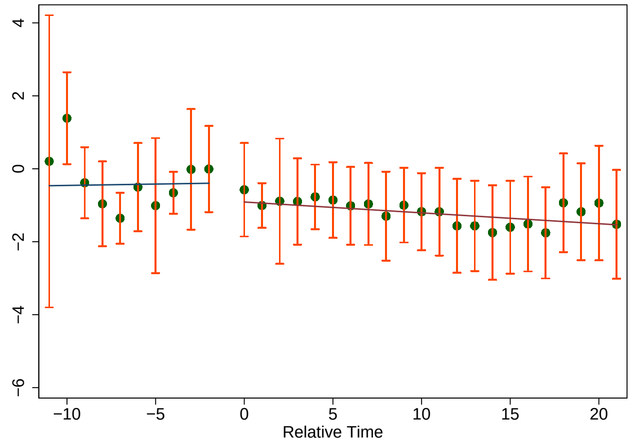
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -11 includes all observations with relative time ≤-11 and relative year 21 includes all observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects as well as controls for exposure to school finance reforms, state EITC rates, and food stamps. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-race-gender cell. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the state level.

Figure A-2: Event Study Estimates by Gender and Race/Ethnicity - Hours Worked

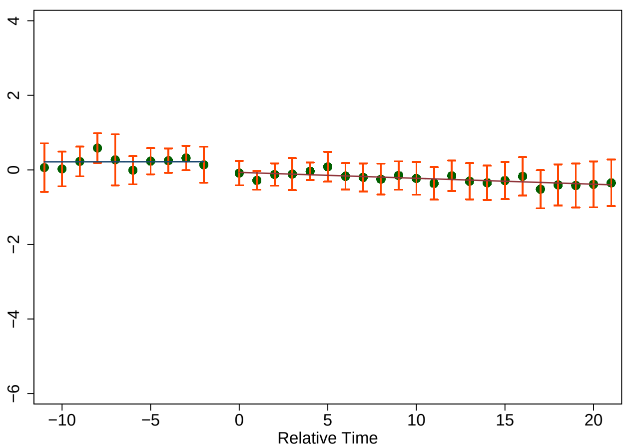
(a) Black & Hispanic Males



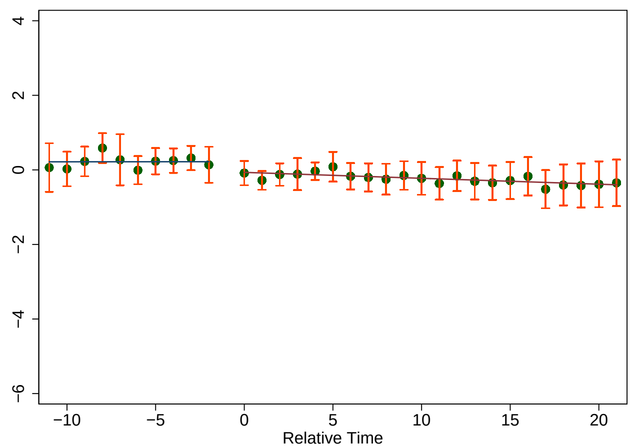
(b) Black & Hispanic Females



(c) White & Asian Males



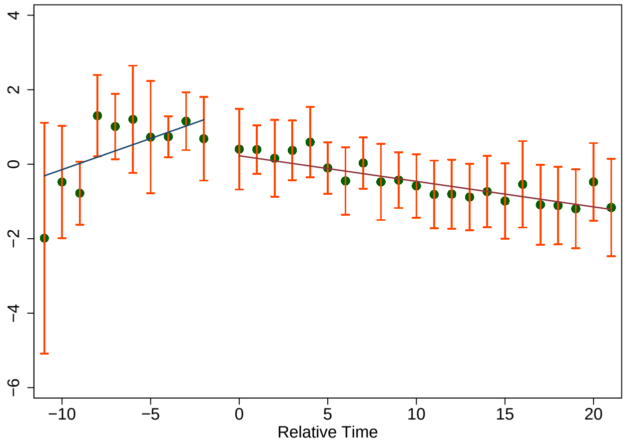
(d) White & Asian Females



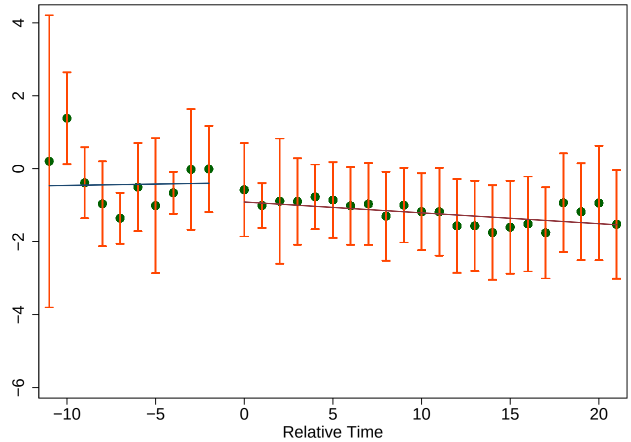
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -11 includes all observations with relative time ≤-11 and relative year 21 includes all observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects as well as controls for exposure to school finance reforms, state EITC rates, and food stamps. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-race-gender cell. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the state level.

Figure A-3: Event Study Estimates by Gender and Race/Ethnicity - Employment

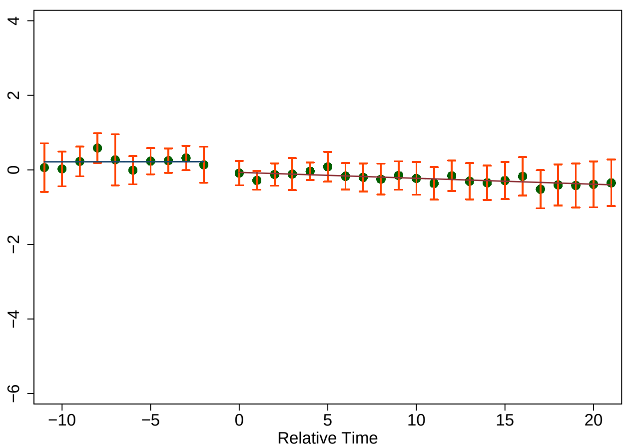
(a) Black & Hispanic Males



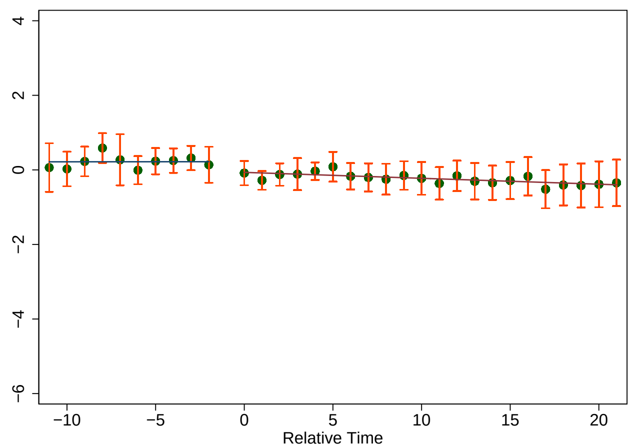
(b) Black & Hispanic Females



(c) White & Asian Males

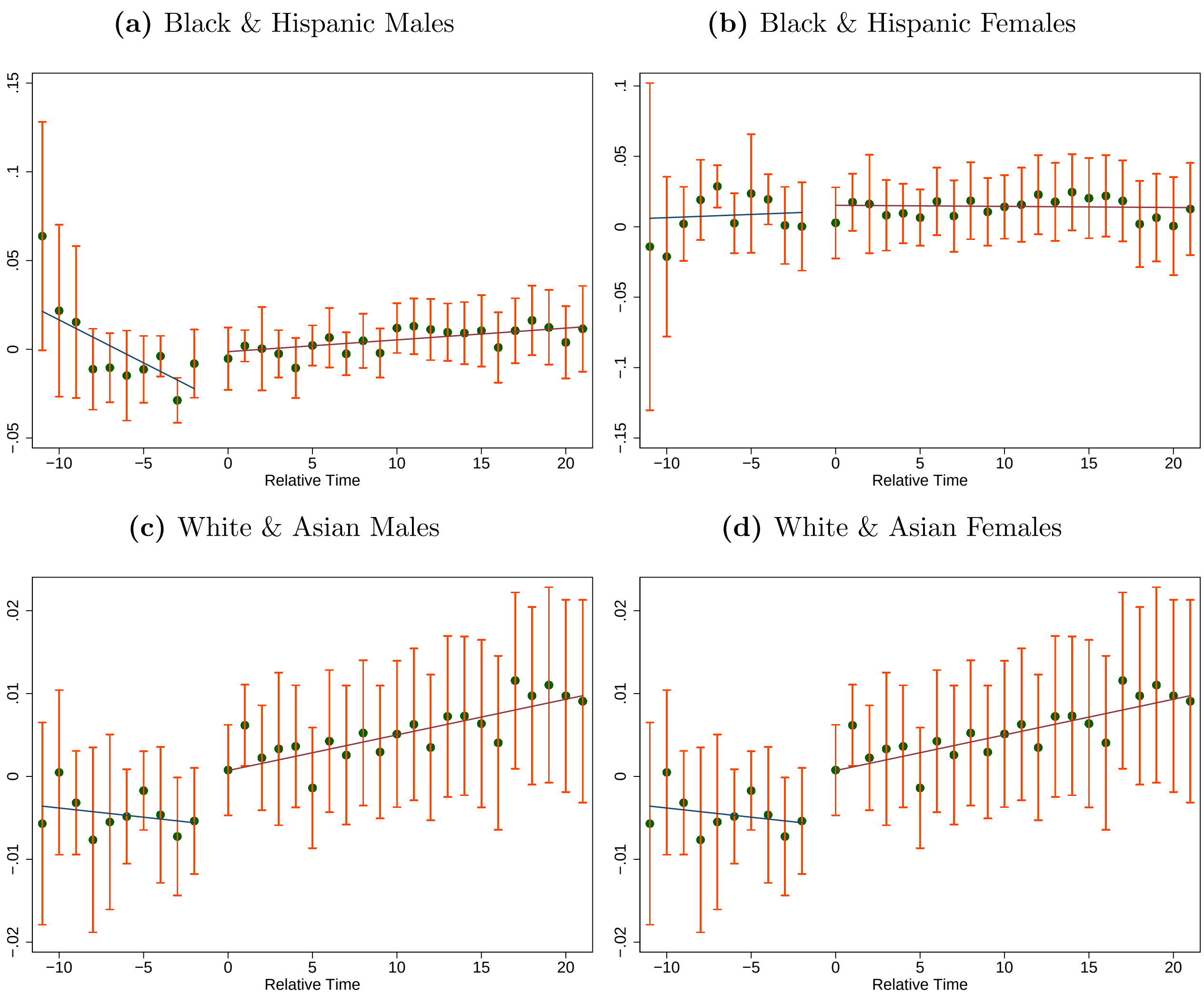


(d) White & Asian Females



Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -11 includes all observations with relative time ≤-11 and relative year 21 includes all observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects as well as controls for exposure to school finance reforms, state EITC rates, and food stamps. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-race-gender cell. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the state level.

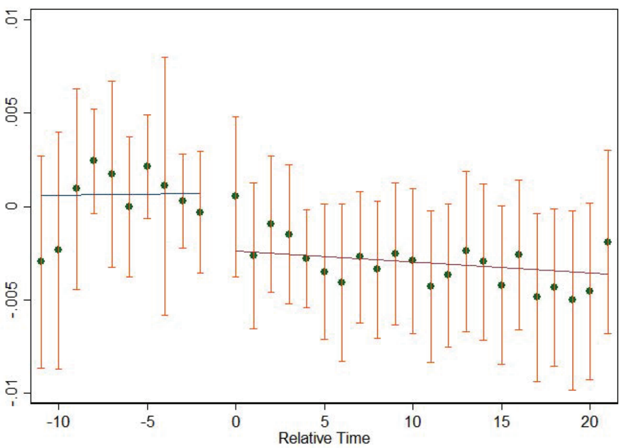
Figure A-4: Event Study Estimates by Gender and Race/Ethnicity - Not in Labor Force



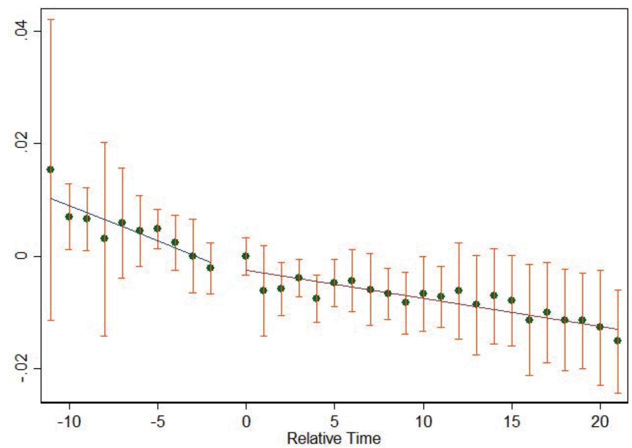
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -11 includes all observations with relative time ≤-11 and relative year 21 includes all observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects as well as controls for exposure to school finance reforms, state EITC rates, and food stamps. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-race-gender cell. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the state level.

Figure A-5: Event Study Estimates by Gender and Race/Ethnicity - Occupational Skill

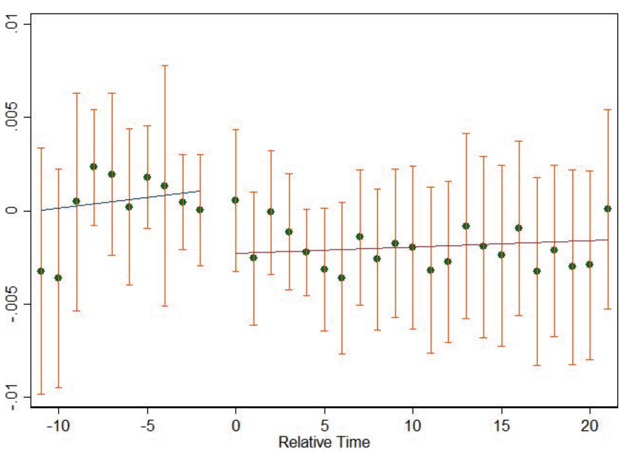
(a) Black & Hispanic Males



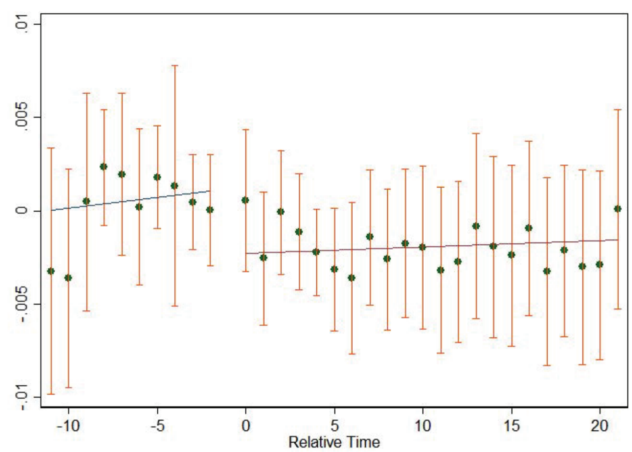
(b) Black & Hispanic Females



(c) White & Asian Males



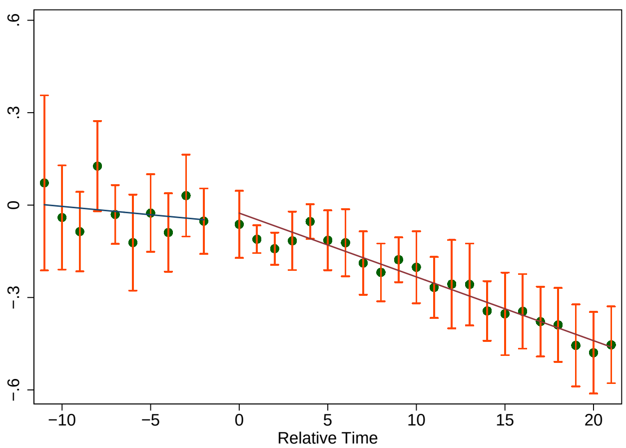
(d) White & Asian Females



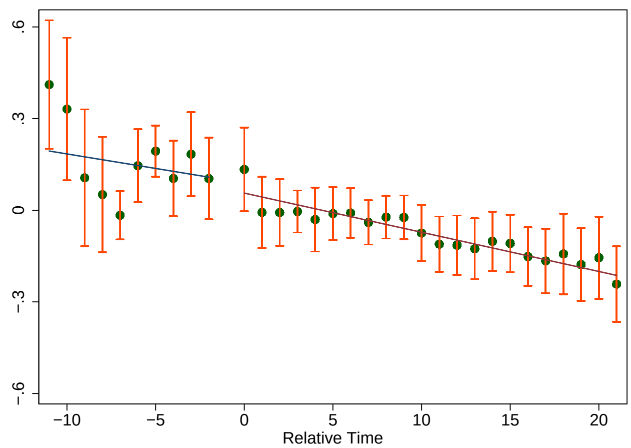
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -11 includes all observations with relative time ≤-11 and relative year 21 includes all observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects as well as controls for exposure to school finance reforms, state EITC rates, and food stamps. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the state level.

Figure A-6: Event Study Estimates by Gender and Race/Ethnicity - Years of Education

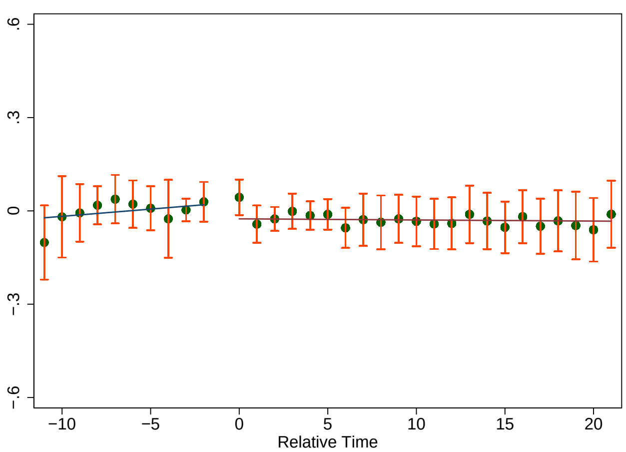
(a) Black & Hispanic Males



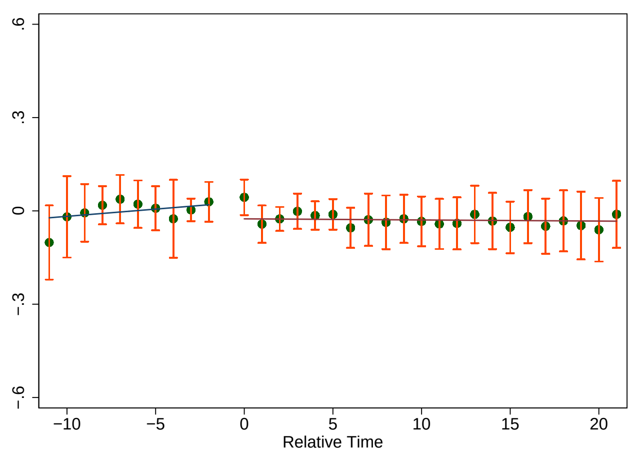
(b) Black & Hispanic Females



(c) White & Asian Males



(d) White & Asian Females



Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -11 includes all observations with relative time ≤-11 and relative year 21 includes all observations with relative time ≥21. All estimates include birth cohort-by-year, birth state, and year fixed effects as well as controls for exposure to school finance reforms, state EITC rates, and food stamps. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-race-gender cell. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95% confidence interval calculated from standard errors that are clustered at the state level.