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The Long-run Effects of Teacher Collective Bargaining

Abstract

This paper presents the first analysis of the effect of teacher collective bargaining on long-run labor market and educational attainment outcomes. Our analysis exploits the different timing across states in the passage of duty-to-bargain laws in a difference-in-difference framework to identify how exposure to teacher collective bargaining affects the long-run outcomes of students. Using American Community Survey (ACS) data linked to each respondent's state of birth, we examine labor market outcomes and educational attainment for 35-49 year olds. Our estimates suggest that teacher collective bargaining worsens the future labor market outcomes of students: living in a state that has a duty-to-bargain law for all 12 grade-school years reduces earnings by \$800 (or 2%) per year and decreases hours worked by 0.50 hours per week. The earnings estimate indicates that teacher collective bargaining reduces earnings by \$199.6 billion in the US annually. We also find evidence of lower employment rates, which is driven by lower labor force participation, as well as reductions in the skill levels of the occupations into which workers sort. The effects are driven by men and nonwhites, who experience larger relative declines in long-run outcomes. Using data from the 1979 National Longitudinal Survey of Youth, we demonstrate that collective bargaining leads to sizable reductions in measured cognitive and non-cognitive skills among young adults. Taken together, our results suggest laws that support collective bargaining for teachers have adverse long-term labor market consequences for students.

JEL-Codes: I200, J450, J580.

Keywords: teachers unions, teacher labor markets, economics of education, collective bargaining.

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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 forthcoming), 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 both Wisconsin and Indiana 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 teachers' unions and 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 case is currently on appeal to 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 forthcoming; 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 policies 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, and we find no evidence that such trends exist. Third, we show that our results are not being driven by the general union environment in the state, are not influenced by the urbanicity of the population, and are not correlated with the prevalence of social unrest in the state when our sample was of school age. Fourth, 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 overwhelmingly 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 seriously influenced 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.

Our estimates point to negative effects of teacher collective bargaining laws on the long-run labor market outcomes of students who grew up in states with these laws, which is consistent with the “rent-seeking” hypothesis of teacher unionization (Hoxby 1996).⁴ Spending all 12 years of grade school in a state with a duty-to-bargain law reduces yearly earnings by \$800 (or 2.0 percent) and hours worked per week by 0.50 (or 1.5 percent). Further, these individuals are 0.9 of a 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 leads to lower wages and causes 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 more pronounced among men and nonwhites. For men, earnings decline by \$1,384 per year and hours worked per week decline by 0.63. For both of these outcomes, the post-treatment trends suggest that the long-run effects are even larger. Men also experience reduced employment, which comes almost entirely from a lower labor force participation rate (1.1 percentage points), and their wages decline by 1.8%. In addition, men exposed to a duty-to-bargain law for the entirety of their K-12 schooling obtain 0.06 fewer years of education compared to men not exposed to a duty-to-bargain law and sort into lower-skilled occupations. Among nonwhites, effects are even

⁴ 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.

larger: earnings decline by \$1,986, hours worked decline by 1.2, employment shares decrease by 2.2 percentage points, and years of education decline by 0.17 years. Estimates for wages and occupational skill also are also large and negative, but they are imprecise.

Among women, there is mixed evidence of the effect of collective bargaining laws on long-run outcomes. However, we also document the existence of secular pre-treatment trends for several outcomes that bias the estimates upward. There is evidence of a negative long-run effect of teacher collective bargaining on employment and labor force participation for women, but this effect takes about 20 years post-law passage to appear. That we find any negative effect on female employment rates is notable because of the upward trend among these birth cohorts in female employment (Blau and Kahn 2013; Bick and Bruggeman 2014).

Finally, we use the NLSY79 in a similar cross-cohort difference-in-difference framework to provide supporting evidence of the effect of duty-to-bargain laws on measured cognitive and non-cognitive skill. We find that these laws significantly reduce both cognitive and non-cognitive outcomes. The large effects on non-cognitive scores helps reconcile the fact that we do not see an educational attainment effect despite sizable impacts on labor market outcomes in the ACS data, since non-cognitive skills are likely to affect labor market outcomes more (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 students. Although the point estimates are modest in magnitude, they are economically significant: increasing earnings in the 33 states with a duty-to-bargain law by 2.0 percent amounts to \$199.6 billion of additional earnings per year. Thus, due to the scope of teacher collective bargaining in the US, the small treatment effects we identify translate into large impacts on the productivity of the workforce.

The rest of this paper is organized as follows: Section 2 provides background on teachers' unions in the US as well as on the prior literature, Section 3 introduces the data used in our analysis, and Section 4 presents our empirical strategy. Results using the ACS data are shown in Section 5, and results on cognitive and non-cognitive skill measures are presented in Section 6. Section 7 concludes.

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.⁵

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 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).

Between 1960 and 1987, 33 states passed DTB laws (see 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.⁶ 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

⁵ See Murphy (1990) for a detailed history of the teachers’ union movement in the United States.

⁶ Note that Washington, DC is excluded both from Table 1 and from our analysis.

collective bargaining rates than did the other forms of union laws (Frandsen forthcoming): 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.⁷

2.2. Theoretical Predictions

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, 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 practically 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. The rent-seeking model predicts that any changes in school inputs

⁷ 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.

induced by teacher collective bargaining should not increase student outcomes and likely will cause them to decrease.⁸

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 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 (Gunderson 2005). A more favorable working environment could further induce more-productive workers to select into teaching.

All models of union behavior predict that teacher 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.⁹ 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 study of this question.

2.3. Prior Research on Teacher Unionization and Collective Bargaining

The majority of the earlier research on teachers unions has examined their effect on resource allocation rather than on student outcomes.¹⁰ While such analyses cannot shed light on which models of union behavior are correct, they are instructive in thinking through some of the core mechanisms through which teachers unions can impact student achievement and long-run outcomes. 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 (Moe 2009; Strunk 2009; West forthcoming).

⁸ 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.

⁹ 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.

¹⁰ See Cowen and Strunk (2015) for a recent review of this literature.

Research examining the effect of teacher collective bargaining on district resources has found mixed results. 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 (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 increased teacher salaries, increased per-student expenditures and reduced student-teacher ratios.¹¹ However, using similar data but a different union measure, Lovenheim (2009) finds little connection between teacher collective bargaining and school district resources. In a recent re-analysis of these data, Frandsen (forthcoming) 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

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 productivity¹³ and the quality of teachers in the workforce, which are two main pathways through which unions can influence student achievement.¹⁴ 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

¹¹ 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.

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 To our knowledge, no estimates exist of the effect of teacher collective bargaining on teacher value-added.

¹⁴ 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.

long-run labor market and educational attainment outcomes,¹⁵ which may differ from any short-run impacts in important ways.¹⁶ 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. 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, data limitations force them to focus on one very narrow educational outcome measure. If unions affect a different part of the ability distribution or if they impact 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. Kleiner and Petree (1988) use state-aggregate data on the percentage of teachers covered by contracts as their union measure, and they find little evidence that variation in this measure over time is related to SAT scores. Eberts and Stone (1987) estimate how education production functions differ across union and non-union districts,

¹⁵ 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. Nonetheless, these findings are not necessarily inconsistent with a negative long-run effect of teacher collective bargaining on student outcomes.

¹⁶ Many studies have found that program effects on student test 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. forthcoming). This evidence underscores the importance of examining long-run effects directly.

using test score growth as their outcome measure. They estimate that unions increase education productivity, but they do not have exogenous variation in union status across schools.

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 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.¹⁷ 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 unions negotiate.

Our contribution to this literature is to estimate how teacher collective bargaining affects longer-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.

¹⁷ 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.

3. Data

The data for this 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.¹⁸ 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 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).¹⁹ 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

¹⁸ These data are available at <http://www.nber.org/publaw/>.

¹⁹ 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.

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.

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, age, calendar year level.²⁰ 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 are shown in Table 3. For educational attainment, we generate indicator variables for the highest level obtained. These levels, which are mutually exclusive, include 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 years, 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.²¹

²⁰ Recall that calendar year and age define one's birth cohort.

²¹ As shown in Online Appendix Table A-1, our results are robust to excluding 2005-2007 ACS years when analyzing years of education as an outcome.

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 two measures of worker skill. The first is the log wage, which is calculated by dividing total earnings by the number of hours worked per year.²² The number of hours worked per year is measured using respondents' reports of average hours worked per week in the past year multiplied by 50. We construct this measure both with and without self-reported earnings, as self-reported earnings may be subject to more measurement error and recall bias. The second measure is occupational skill. Using the 2005-2012 ACS, we 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 teacher collective bargaining exposure 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:

$$Y_{sact} = \beta_0 + \beta_1 DTB_Exposure_{sc} + \gamma X_{sact} + \delta_{at} + \theta_{st} + \varepsilon_{sact}, \quad (1)$$

where Y_{sact} is one of the educational or labor market outcomes listed above for those born in state s , of age a , in birth cohort c and ACS calendar year t . Regressions are weighted by the number of observations that underlie each age-birth state-year 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).

²² We have tested whether our estimates are sensitive to alternative ways of measuring wages, including trimming the top 1% of earnings. Estimates using alternative wage measures are very similar to our baseline estimates and are available upon request.

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 age-by-calendar year (δ_{at}) and birth state-by-calendar year (θ_{st}) fixed effects. The age-by-year fixed effects are akin to birth year fixed effects, because age and calendar year perfectly define a birth cohort. These fixed effects control for any systematic differences across birth years that may be correlated with the prevalence of duty-to-bargain laws and with labor market outcomes. The state-by-year fixed effects control for variation in educational attainment or labor markets that are common across birth cohorts within a state in a given year. Such factors include state-specific macroeconomic shocks, changes to the industrial mix, and labor demand changes more generally in a state. We also control for the proportion of each state-age-year cell that is black, white, Hispanic or “other” as well as the percent of the cell that is male. 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:

$$Y_{sact} = \beta_0 + \pi_{-6}I(C - t_0 + 18 \leq -6)_{sc} + \sum_{\tau=-5}^{24} \pi_{\tau}I(C - t_0 + 18 = \tau)_{sc} + \pi_{25}I(C - t_0 + 18 \geq 25)_{sc} + \gamma X_{sact} + \delta_{at} + \theta_{st} + \varepsilon_{sact} \quad (2)$$

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.²³ 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 -5 and 24. We also include an indicator for whether time relative to a DTB law is less than or equal to -6 and for whether $C - t_0 + 18$ is greater than or equal to 25.²⁴ The π_τ coefficients non-parametrically trace out pre-treatment relative trends (for π_{-5} to π_{-1}) as well as time-varying treatment effects (π_0 to π_{24}).

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 1 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 (Lovenheim 2009) 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, we note that there is much variation in the timing of the passage of these laws (see Figure 1 and Table 1), and combined with the state-by-year fixed effects it is 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.

The existence of alternative policies 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

²³ In the time period we examine, no state repeals a duty-to-bargain law.

²⁴ We choose this event window because the sample sizes become small for relative time indicators less than -5 and greater than 24. 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.

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 (Forthcoming), 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 Micheltore (2013) shows that these policies positively affect educational attainment.²⁵ 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 18.²⁶ Below, we show estimates both with and without these controls. For the most part, they have little effect on our results.

5. Results

5.1. Baseline Estimates

Baseline estimates of the effect of teacher collective bargaining on labor market outcomes for the full sample are shown in Table 4. Each cell in the table comes from a separate estimation of equation (1), and we add controls sequentially across columns. In column (i), we control for birth state, age and calendar year fixed effects as well as race/ethnicity and gender. We add controls for state EITC, school finance reform and food stamp exposure during childhood in column (ii), and column (iii) adds birth state-by-year and age-by-year fixed effects.

Across the columns in Table 4, there is clear evidence of a negative effect of teacher collective bargaining on earnings (Panel A). 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

²⁵ Cohodes et al. (forthcoming) 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.

²⁶ The food stamp data we use 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.

reduces earnings by \$799.73 dollars per year. This represents a decline in earnings of 1.95% relative to the mean, which is shown directly below the estimates in the table. While a 1.95% reduction in earnings is 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 \$199.6 billion dollars *per year* due to individuals having grown up in states that mandate collective bargaining between teachers' unions and school districts.²⁷ Furthermore, the estimates in Table 4 are similar across columns, which is inconsistent with biases from age- and state-specific shocks or from exposure to other policies when young.

Panel B presents estimates for annual hours worked (including zeros) for the full sample of 35-49 year olds. Consistent with the reduction in earnings, hours worked decline by 0.50 per week on average due to being exposed to DTB laws throughout one's schooling years. This is a 1.46% decline relative to the mean of 34.4 shown in Table 3. The hours worked estimates decline slightly across columns and the standard error increases with the addition of more fixed effects. As a result, this estimate is only significantly different from zero at the 10% level in column (iii). Nonetheless, Panel B of Table 4 suggests that a main driver of the earnings declines shown in Panel A is a reduction in hours worked as a result of teacher collective bargaining.

The finding that teacher collective bargaining is associated with fewer working hours suggests that DTB laws may affect the extensive margin of labor supply. Table 5 examines this question in detail, showing estimates of equation (1) where the proportion 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 employment and increase the proportion of 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 worker is employed by between 0.9 and 1.2 percentage points. This is between a 1.15% and 1.54% reduction in employment relative to the mean, and these estimates are statistically significant at the 5% level in columns (i) and (ii) and at the 10% level in column (iii).

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

²⁷ We obtain this estimate by multiplying total wage income for each state in 2014, obtained from the Bureau of Labor Statistics, by 1.95%.

reduces the labor force participation rate by 0.8 to 0.9 of a percentage point. Relative to the mean labor force participation rate, this represents a reduction of approximately 5%. While the estimate in column (iii) is not statistically significantly different from zero even at the 10% level, this mainly reflects the larger standard error associated with the more demanding specification. The estimates in the first two columns are significant at the 10% level and are similar in magnitude.

In addition to changes in hours worked, the earnings effects shown in Table 4 could be driven by changes in wages. Table 6 presents estimates using log wage as the dependent variable. In Panel A, we use all positive wages and in Panel B we use only wages from non-self-employment income. As the table demonstrates, there is a consistent negative relationship between collective bargaining exposure and wages, but the estimates are not statistically significantly different from zero. We view these results as providing suggestive evidence of a negative effect of teacher collective bargaining on average wages. In Panel C, we show results from estimation of equation (1) that uses the proportion of individuals in one's occupation that has at least some collegiate attainment.²⁸ 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 0.004 (or 1.2% relative to the mean) in our preferred model. This estimate is statistically significantly different from zero at the 10% level, and it points to collective bargaining negatively affecting the occupational skill level chosen by workers.

The reduced earnings and labor force participation associated with teacher collective 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 DTB laws 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 D of Table 6 shows estimates for the total number of years of education. There is little evidence of an effect on the overall number of years of schooling completed. The estimates

²⁸ The regressions in Panel C of Table 6 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.

are precise enough to rule out an effect larger than -0.086 years of completed schooling at the 5% level in Column (iii), which is 0.6% relative to the mean. Assuming that an additional year of schooling increases earnings by 10% (Card 1999), a decline in educational attainment of 0.2 years could fully explain the 2% earnings decline we estimate from teacher collective bargaining. Thus, we can rule out that more than 43% of the earnings effect is driven by changes in completed years of education.

One concern with the estimates in Table 6 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. These results are shown in Appendix Table A-1 and are very similar to those in Panel D of Table 6. The lack of an effect of teacher collective bargaining on educational attainment is not driven by the measurement changes that occurred in the ACS.

Examining total years of schooling may miss heterogeneous effects across the distribution of schooling levels. In Appendix Table A-2, we therefore estimate equation (1) using the proportion of respondents with different highest levels of educational attainment as the dependent variable. The estimates are inconsistent with teacher collective bargaining affecting the proportion of the sample whose highest level of education is a high school degree, some college, an Associates degree, or at least a Bachelors degree.

That teacher collective bargaining laws have no average effect on educational attainment 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 (Lovenheim 2009). 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.

5.2. Event Study Estimates

As discussed in Section 4, a core identification concern with our empirical approach is that the states that pass duty-to-bargain laws are experiencing different cross-cohort trends in outcomes relative to states that do not pass these laws. We therefore estimate equation (2), which allows us to test for differential pre-treatment trends as well as for time-varying treatment

effects. Figures 2-4 show our estimates of π_τ from equation (2) for labor market and educational attainment outcomes, with relative year -1 omitted in order to rescale the estimate to be relative to that year. Each point is an estimate of π_τ for the given year, and the bars show the 95% confidence interval of each estimate using standard errors clustered at the state level.

The results across figures are inconsistent with the existence of pre-treatment trends that could bias our results.²⁹ In general, outcomes are trending similarly across cohorts in the pre-period as a function of future DTB-passage timing, and the resulting changes in outcomes match closely the timing of DTB law passage. We also show that the treatment effects grow substantially over time. As discussed in Section 3, this is due both to the increasing proportion of one's schooling years in which one is exposed to a duty-to-bargain law in relative years 0-12 and any time-varying treatment effects of the laws themselves, including changes in teacher composition. These figures match the results in Tables 4-6 closely in showing reduced earnings, hours worked, employment and labor force participation among cohorts exposed to duty-to-bargain laws. Figures 2-4 make clear that these reductions were not a result of cross-cohort trends that existed prior to the passage of these laws but rather were due to trend breaks that occurred after the laws were passed. As in Table 6, Figure 4 shows there is little impact on wages or educational attainment. That these outcomes are not declining shows there is nothing mechanical about our model specification that leads to the negative effects on other outcomes.

Online Appendix Figures A-1 through A-4 show event study estimates for high school completion, some college, AA completion and BA completion, respectively. They differ from the estimates in Table A-2 in a couple of ways. First, there is a negative effect for the some college outcome in Figure A-2 over the long-run. The magnitude of the effect is notable, with a long-run reduction in college attendance of 3 percentage points (or 13% relative to the mean). Second, Figure A-3 suggests that there could be a 1 percentage point decline in the rate of AA attainment in the long-run. The third difference is in Figure A-4, which shows an increase in BA attainment due to teacher collective bargaining. As we discuss below, this result is driven by

²⁹ For employment and hours worked in Figures 2 and 3, there is a decline in outcomes in the year just prior to DTB passage. As discussed in Section 3, this likely reflects the fact that about 27% of these students are treated. Thus, we interpret this decline as part of the effect of collective bargaining rather than as an indication of selection on fixed trends. That this is a level effect in both cases, not a differential trend, supports this interpretation. We highlight as well that the magnitudes of the declines are small; the overall negative effects are clearly driven by the large relative declines in outcomes that occur for cohorts treated with more than 6 years of DTB exposure.

women, who experienced large secular increases in BA attainment across the cohorts we study (Goldin, Katz and Kuziemko 2006). This result therefore should be interpreted cautiously.

5.3. Estimates by Gender

Due to differences in labor force participation and educational attainment trends across genders, as well as the fact that boys are more affected by adverse events and environmental factors than girls (Bertrand and Pan 2013; Autor et al. 2016), examining the effect of teacher collective bargaining by gender is instructive. We first focus on estimates among men: Panel A of Table 7 shows estimates of equation (1) for this subsample that includes all controls except gender. The results show that the effects of collective bargaining laws on earnings and hours worked are much larger among men. Being exposed to a collective bargaining law during one's grade school years leads to \$1,384 dollars per year lower earnings (2.55% relative to the mean) and 0.63 fewer hours of work per week (1.63% relative to the mean). Both estimates are statistically significantly different from zero at the 5% level.

The labor force effects are similarly larger for men. Collective bargaining laws lead to a reduction in male employment rate of 1.2 percentage points (1.46% relative to the mean), which all can be attributed to lower labor force participation. Exposure to collective bargaining also reduces male wages by a statistically significant 1.8% and decreases occupational skill. Consistent with the negative wage effect for men, teacher collective bargaining laws reduce the total number of years of education by 0.06 (0.42% relative to the mean).³⁰

The first panel of Figures 5-11 show event study estimates for men. As with the main estimates, there is little evidence of pre-treatment cross-cohort trends that can bias our labor force or educational attainment results. Furthermore, there is clear evidence of declining earnings, hours worked, employment, labor force participation, occupational skill and educational attainment for cohorts exposed to duty-to-bargain laws.³¹ These treatment effects grow larger over time as well. As a result, the long-run effects are considerably larger in magnitude than the short-run effects. For example, 20 years after a duty-to-bargain law is passed, earnings are almost \$4,000 lower, hours worked decline by about 1.5, and years of education are 0.15 years lower relative to men in non-duty-to-bargain states.

³⁰ This estimate is very similar when restricting the sample to 2008-2012. Results available upon request.

³¹ Online Appendix Figures A-1 through A-4 show event study estimates by gender for different educational attainment levels.

Panel B of Table 7 shows results for women. In general, these estimates are in the same direction as for men, but they are attenuated and tend not to be statistically different from zero at conventional levels. The one exception is the coefficient on wages, which suggests that exposure to collective bargaining increases female wages by about 2%. Thus, the reason there is no overall wage effect is the fact that the estimates for men and women cancel each other out.

The different estimates for men and women are somewhat surprising. There are two main explanations for this finding. First, it could be that unionization affects boys differently from girls. Boys tend to be more strongly affected by education interventions at young ages more broadly, which is consistent with the results presented here. Second, estimates for women may be biased upward by secular trends. The cohorts we consider experienced large increases in female labor supply, wages, and educational attainment. Although we have time-varying treatments that help us account for any secular trends, the fact that we only see union law passage (versus repeals) in our data means that strong secular trends could bias our results because the union laws always move in the same direction.

The event study estimates in Figures 5-11 and in Appendix Figures A-1 to A-4 provide some evidence to support the second explanation. For log wages as well as college completion among women, there are rather strong pre-treatment trends that suggest that the estimates from equation (1) are biased upward. Particularly for wages, the positive effect shown in Table 7 likely reflects secular wage increases among women (related to the positive secular trend in BA completion) rather than a positive causal effect of collective bargaining on wages.

For non-wage labor market outcomes, we find that there are negative effects of teacher collective bargaining laws on some female labor supply measures in the long run. These estimates are not biased by pre-treatment trends. While there is no evidence of a negative earnings effect, there is a sizable, long-run negative hours worked effect that is similar in magnitude to the male estimates. Mechanically, the reason there is no earnings effect among women is that the increase in wages and decrease in hours work cancel each other out. The long-run effects of teacher collective bargaining on employment and labor force participation are also large and are similar to those of men. Thus, while the effects on female labor market outcomes are mixed in general, on net we find that collective bargaining reduces labor force participation and employment in the long run. Teacher collective bargaining also has at most a small effect on female educational attainment, even in the long run.

5.4. Estimates by Race/Ethnicity

Panels C and D of Table 7 show results for nonwhites and whites. Asians are included with whites, but as Table 3 shows they are a very small proportion of the sample. The nonwhite sample therefore is comprised of black, Hispanic and “other” racial/ethnic categories. This stratification 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.

Table 7 shows that the labor market and educational attainment effects are much larger for nonwhites than they are for whites. Among nonwhites, earnings decline by \$1,986 (6.75%), hours worked are reduced by 1.51 (3.66%), employment declines by 2.2 percentage points (3.13%), and years of education is -0.17 years (2.73%) lower due to 12 years of DTB exposure. We also find negative and sizable effects on log wages and on occupational skill, however these estimates are not statistically significantly different from zero at conventional levels. Among whites and Asians, there is a more muted negative earnings effect of \$513 (1.17%) and a reduction in hours of work of -0.16 hours (0.45%). Both of these estimates are significant at the 5% level, but none of the other outcomes exhibit significant or economically meaningful changes due to DTB exposure.

Panels C and D of Figures 5-11 as well as panels D and E of Online Appendix Figures A-1 through A-4 show event study estimates of all outcomes. Throughout, there is little evidence of differential pre-treatment trends that could bias our estimates. Furthermore, the patterns of post-treatment coefficients match the results in Table 7 closely. As with the baseline results as well as those by gender, there is evidence of much stronger long-run effects than short-run effects. This is particularly the case for years of education and occupational skill among nonwhites. Thus, the difference-in-difference estimates in Table 7 likely understate the long-run effects of duty-to-bargain laws somewhat.

5.5. Robustness Checks

The baseline estimates support the rent-seeking theory of union behavior, whereby unions reduce the productivity of education inputs. The reduction in input productivity leads to long-run

declines in earnings and labor market participation among students exposed to teacher collective bargaining while in grade school. 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). Table 8 presents several robustness checks that each examines how our results and conclusions change when we control for additional factors that could be correlated with both duty-to-bargain exposure and long-run outcomes.

In Panel A, we exclude the 14 states that do not have anti-strike penalties associated with their duty-to-bargain laws.³² 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, suggesting that our findings are not driven by the states that allow teachers to strike.

One alternative hypothesis that is consistent with our findings is that teacher collective bargaining alters student motivation or changes cultural norms regarding work behavior. Such an effect still would reflect an effect of collective bargaining on student outcomes, but it would be acting through a different channel than through changes in school district resource allocations, teacher behavior and teacher quality. To investigate this competing hypothesis, we calculate the average proportion of the workforce comprised of teachers, police and firefighters (separately) that those in each birth cohort-birth state would have experienced between the ages of 6 and 18. If changes in cultural norms are responsible for our estimates, these effects should be captured at least partly by the size of the public sector workforce. These proportions could be impacted by collective bargaining policies, so we view these estimates as more suggestive than causal. Panel B of Table 9 shows estimates that control for these proportions. The point estimates are 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 C of Table 9, we control for the total unionization rate at age 18 for

³² These states are Wisconsin, Connecticut, Michigan, Massachusetts, Rhode Island, Maine, Vermont, Alaska, Hawaii, Kansas, Pennsylvania, Idaho, Oregon and Montana.

each birth state-birth cohort.³³ 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 9 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.³⁴ 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 D 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 18.³⁵ This specification is designed specifically to examine the effect that the urban civil unrest in the 1960s and 1970s has on our estimates. Panel E of Table 9 contains the results that include this additional control, and the results are again extremely similar to those in the main analysis.³⁶ Taken together, the robustness of our results to the supplementary control variables included in Table 9 supports our

³³ 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.

³⁴ 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.

³⁵ The Dynamics of Collective Action dataset can be found at: <http://web.stanford.edu/group/collectiveaction/cgi-bin/drupal/>.

³⁶ 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.

preferred interpretation of the evidence as reflecting rent-seeking behavior by teachers' unions in the collective bargaining process.

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 12 for three of our main outcomes: yearly earnings, hours of work, and employment.³⁷ As the figure demonstrates, our estimates are insensitive to excluding any one state: in no case does the qualitative or quantitative results change.

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 suggest that any such trends were not correlated with timing of DTB passage. But, because we only have DTB passages rather than appeals, 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. 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 except log wages, we do not get any simulated results that are smaller (i.e., more negative) than the baseline estimates. Even for log wages less than 5% of the simulated effects are smaller. Note that the labor force participation estimate is positive, so here we do not obtain simulated effects that are larger than the one shown in Table 5. These results suggest that we are not simply picking up aggregate differences between the treatment and control states. What matters is not

³⁷ The results for other outcomes and by gender are extremely similar. We exclude them for parsimony, but they are available from the authors upon request.

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 and the event study figures strongly support the validity of our results.

A final identification issue comes from measurement error driven by post-birth mobility. In the 1990 Census, 78.4% of 17-year-olds live in the state of their birth. Post-birth mobility introduces measurement error into our DTB exposure variable. 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 that should bound the effect. In Panel A of Table 10, we show results that exclude the 37.7% of respondents who do not live in their birth state. 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 large in absolute value than our baseline estimates, although they are close in magnitude.

In Panel B, we estimate equation (1) under the very conservative assumption that those who live in a state at age 17 other than their birth state spent their entire childhood 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).³⁸ The results in Panel B are smaller than baseline in absolute value but are close in magnitude. Together with the results in Panel A these estimates likely bound the true effect. That both panels show results that are similar to each other and to the baseline results suggests that any bias from post-birth mobility is small.

³⁸ Because this method requires aggregated data, we do not estimate this model for occupational skill.

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-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 Forces Qualifying Test (AFQT), which is our measure of cognitive skills. 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 age in 1997 and state of residence at age 14 fixed effects (not age-year and state-year fixed effects). We also control for race, gender, 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. We see consistent evidence that 12 years of exposure to a collective bargaining law negatively impacts both cognitive and non-cognitive scores. AFQT percentile declines by 7.5, a 15.6% effect relative to the mean. All non-cognitive skill measures move in the direction of declining skill as well: the Rotter Locus of Control increases by 1.32 (15.5%), the Rosenberg Self-esteem Scale declines by 1.42 (6.3%) and the Pearlin Mastery Scale

score is reduced by 0.89 (4.0%). The first three estimates are statistically different from zero at the 5% level, and the fourth is significant at the 10% level.

The estimates in Table 11 strongly 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 leads 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 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 the long-run educational and labor market outcomes of affected children. 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 olds were of school-age adversely affects their long-run outcomes.

We find evidence 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 earnings by \$800, or 1.95%. A back-of-the-envelope calculation indicates these laws reduce total labor market earnings by \$199.6 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: hours worked declines due to reductions in employment and decreases in labor force participation.

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 find reflect reductions in human capital that do not show up in years of educational attainment measures.

Our estimates show that the effects are larger among men and nonwhites. In particular, yearly male earnings decline by \$1,384 and hours worked decreases by 0.63 hours per week. We find some evidence of a small decline in educational attainment for men, who also experience a decline in wages due to growing up in a duty-to-bargain state. Among nonwhites, earnings decline by \$1,986 and hours per week are reduced by 1.2. Labor force participation and wages also decline substantially due to DTB exposure. 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. In total, our estimates indicate that state duty-to-bargain laws have sizable, negative labor market consequences for those who attend 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 and Indiana 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. 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: Summary Statistics of Analysis Variables

Variable	Mean	Std. Dev.
Male	0.495	0.039
Age	42.5	4.610
Asian	0.015	0.065
Black	0.115	0.119
Hispanic	0.049	0.082
Other	0.021	0.038
Duty to Bargain	0.610	0.488
Years Exposed	5.332	5.550
Average EITC	0.001	0.011
Court-Ordered School Finance Reform	0.987	3.115
Legislative School Finance Reform	1.569	3.768
Food Stamp Exposure	0.623	0.326
High School Degree	0.272	0.065
Some College	0.230	0.045
Associate Degree	0.097	0.028
Bachelor Degree	0.294	0.064
Years of Education	13.539	0.406
Employed	0.781	0.049
Unemployed	0.050	0.024
Not in Labor Force	0.170	0.041
Total Income	41095.86	6452.665
Hours Worked (weekly)	34.423	2.094
Hourly Wage	22.373	3.688
Occupational Skill Level	0.330	0.240

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-age-year cell.

Table 4: The Effect of Collective Bargaining Laws on Earnings and Hours Worked

Panel A: Earnings			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-894.842** (307.951)	-973.823** (305.080)	-799.730** (314.644)
% Effect	2.12%	2.37%	1.95%
Panel B: Hours Worked			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.564** (0.236)	-0.560** (0.243)	-0.503* (0.297)
% Effect	1.64%	1.63%	1.46%
SFR, EITC and Food Stamp Controls		x	x
Birth State*Year FE			x
Age*Year FE			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-age-year observations. All estimates include birth state, age and year fixed effects, as well as controls for the percent male and the racial/ethnic composition of the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 Labor Market Participation

Panel A: Employed			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.012*** (0.004)	-0.012** (0.005)	-0.009* (0.005)
% Effect	1.54%	1.54%	1.15%
Panel B: Unemployed			
Treatment Measure	(i)	(ii)	(iii)
Exposure	0.003 (0.002)	0.003 (0.002)	0.001 (0.001)
% Effect	6.00%	6.00%	2.00%
Panel C: Not In Labor Force			
Treatment Measure	(i)	(ii)	(iii)
Exposure	0.009* (0.005)	0.009* (0.005)	0.008 (0.006)
% Effect	5.29%	5.29%	4.71%
SFR, EITC and Food Stamp Controls		x	x
Birth State*Year FE			x
Age*Year FE			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-age-year observations. All estimates include birth state, age and year fixed effects, as well as controls for the percent male and the racial/ethnic composition of the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 6: The Effect of Collective Bargaining Laws on Log Wages, Occupational Skill and Educational Attainment

Panel A: Log Wages			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.007 (0.005)	-0.006 (0.006)	-0.003 (0.006)
Panel B: Log Wages Excluding Self-Employed			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.005 (0.006)	-0.005 (0.007)	-0.003 (0.006)
Panel C: Occupational Skill			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.006** (0.002)	-0.006** (0.002)	-0.004* (0.002)
% Effect	1.82%	1.82%	1.21%
Panel D: Years of Education			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.050 (0.031)	-0.057* (0.032)	-0.031 (0.028)
% Effect	0.37%	0.42%	0.23%
SFR, EITC and Food Stamp Controls		x	x
Birth State*Year FE			x
Age*Year FE			x

Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. In Panels A, B and D, regressions are based on 6,000 birth state-age-year observations and include birth state, age and year fixed effects, as well as controls for the percent male and the racial/ethnic composition of the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year cell. In Panel C, 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 C and includes birth state, age and year fixed effects as well as controls for whether the respondent is male and 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 7: The Effect of Collective Bargaining Laws on Long-Run Outcomes, by Gender and Race

Panel A: Men								
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un- Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Log Wage (vii)	Occup. Skill (viii)
Exposure	-1383.547*** (551.648)	-0.634** (0.265)	-0.012** (0.004)	0.001 (0.002)	0.011** (0.005)	-0.056* (0.031)	-0.018** (0.007)	-0.005** (0.002)
% Effect	2.55%	1.63%	1.46%	1.77%	9.02%	0.42%	.	1.33%
Panel B: Women								
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un- Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Log Wage (vii)	Occup. Skill (viii)
Exposure	-317.301 (347.458)	-0.404 (0.399)	-0.009 (0.008)	0.001 (0.002)	0.008 (0.008)	-0.014 (0.035)	0.020** (0.008)	-0.002 (0.003)
% Effect	1.07%	1.36%	1.22%	2.17%	3.68%	0.10%	.	0.71%
Panel C: Nonwhite								
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un- Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Log Wage (vii)	Occup. Skill (viii)
Exposure	-1985.779** (755.061)	-1.151*** (0.326)	-0.022*** (0.006)	0.002 (0.003)	0.020** (0.007)	-0.165*** (0.041)	-0.028 (0.022)	-0.009 (0.006)
% Effect	6.75%	3.66%	3.13%	2.52%	9.16%	1.28%	.	2.38%
Panel D: White								
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Un- Employed (v)	Not in Labor Force (v)	Years of Education (vi)	Log Wage (vii)	Occup. Skill (viii)
Exposure	-513.115*** (254.963)	-0.158** (0.233)	-0.004 (0.004)	0.001 (0.001)	0.003 (0.005)	-0.036 (0.037)	-0.001 (0.007)	-0.003 (0.002)
% Effect	1.17%	0.45%	0.50%	2.30%	1.88%	0.26%	.	0.93%

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-age-year observations. All estimates include birth state-year and age-year fixed effects as well as controls for exposure to school finance reform, food stamps and EITC when of school age. Results in Panels A and B include controls for the racial/ethnic composition of the state-age-year cell. Results in Panels C and D control for the percent male in each state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 8: The Effect of Collective Bargaining Laws on Long-Run Outcomes – Robustness Checks

Panel A: Excluding States that Allow Teachers to Strike						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-925.045*** (324.482)	-0.660** (0.286)	-0.011** (0.005)	0.010* (0.006)	-0.034 (0.031)	-0.004* (0.002)
Panel B: Controlling for Percent of Teachers, Firefighters and Police						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-640.938** (285.443)	-0.413 (0.297)	-0.007 (0.005)	0.006 (0.006)	-0.028 (0.028)	-0.003* (0.002)
Panel C: Controlling for Total Union Membership at Age 18						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-695.935** (277.510)	-0.410 (0.265)	-0.007 (0.005)	0.006 (0.005)	-0.032 (0.029)	-0.004* (0.002)
Panel D: Controlling for Proportion Living in Urban Areas						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-848.695*** (249.211)	-0.540** (0.252)	-0.010** (0.005)	0.009* (0.005)	-0.032 (0.027)	-0.004* (0.002)
Panel E: Controlling for Riots and Violent Protests						
	Earnings (i)	Hours Worked (ii)	Employed (iv)	Not in Labor Force (v)	Years of Education (v)	Occup. Skill (vi)
Exposure	-868.809*** (309.244)	-0.495 (0.301)	-0.010* (0.006)	0.009 (0.006)	-0.035 (0.028)	-0.004* (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 birth state-by-year and age-by-year fixed effects. Occupational skill results are based on individual data and control for gender and race/ethnicity. Other outcomes are estimated using aggregated data and control for the percent male and the racial/ethnic composition of the state-age-year cell. Regressions using aggregated data are weighted by the number of individual observations that are used to calculate the averages in each state-age-year cell. In Panel (A) we exclude the the 14 states that allow teachers to strike. Panel (B) controls for the percent of teachers, firefighters and police in the labor force during one's schooling years in the birth state, calculated from CPS MORG data. Union membership data used in Panel (C) come from CPS MORG. In Panel (D), we control for the average proportion of individuals in one's birth state living in a metro area during one's schooling years, calculated from the 1970-1990 US Census. Panel (E) 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

Panel A: Randomly Assigning Passage Dates							
	Earnings	Hours Worked	Employed	Not in Labor Force	Years of Education	Log Wage	Occup. Skill
	(i)	(ii)	(iv)	(v)	(v)	(vi)	(vii)
% Less than Baseline	0.000	0.000	0.000	1.000	0.000	0.043	0.000
Panel B: Randomly Assigning Passage Dates to Match Passage Timing Distribution							
	Earnings	Hours Worked	Employed	Not in Labor Force	Years of Education	Log Wage	Occup. Skill
	(i)	(ii)	(iv)	(v)	(v)	(vi)	(vii)
% Less than Baseline	0.000	0.000	0.000	1.000	0.000	0.046	0.000

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-by-year and age-by-year fixed effects, as well as controls for the percent male, the racial/ethnic composition of the state-age-year 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-age-year 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 duty-to-bargain law. In Panel (B), we run 300 simulations in which we randomly assign passage dates to states that ever pass a duty-to-bargain 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 – Accounting for Mobility

Panel A: Dropping Those Who do not Live in State of Birth						
	Earnings	Hours Worked	Employed	Not in Labor Force	Years of Education	Occup. Skill
	(i)	(ii)	(iv)	(v)	(v)	(vi)
Exposure	-1229.852** (498.101)	-0.590 (0.386)	-0.010 (0.007)	0.010 (0.008)	-0.060 (0.038)	-0.004 (0.003)
Panel B: Weighting by Childhood Mobility						
	Earnings	Hours Worked	Employed	Not in Labor Force	Years of Education	Occup. Skill
	(i)	(ii)	(iv)	(v)	(v)	(vi)
Exposure	-766.813*** (181.344)	-0.425** (0.168)	-0.008** (0.003)	0.007** (0.003)	-0.035** (0.017)	

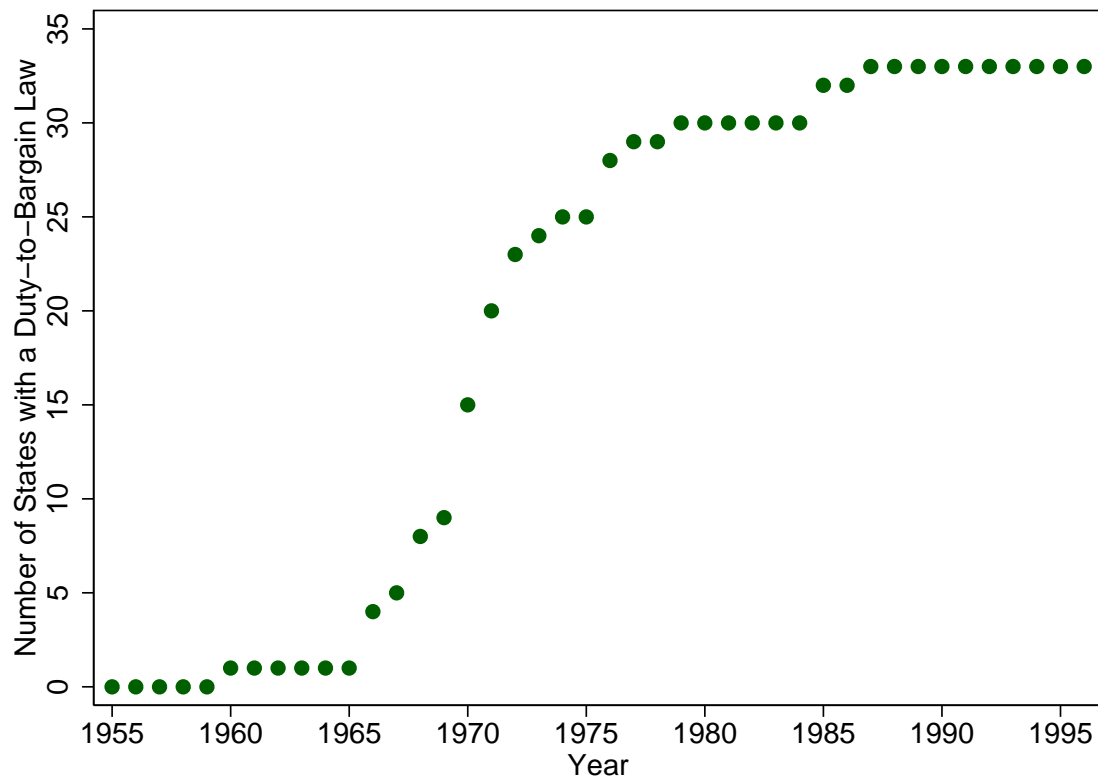
Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. All estimates include state-by-year and age-by-year fixed effects, as well as controls for the percent male, the racial/ethnic composition of the state-age-year cell, exposure to 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-age-year 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-age-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

Dep. Var.	1997 AFQT Percentile	Rotter Locus of Control	Rosenberg Self-Esteem Scale	Pearlin Mastery Scale
Exposure	-7.52** (3.43)	1.32** (0.031)	-1.42** (0.63)	-0.89* (0.053)
% Effect	-15.6%	15.5%	-6.3%	-4.0%
Mean	48.16	8.50	22.53	22.20
N	11719	12355	11826	8818

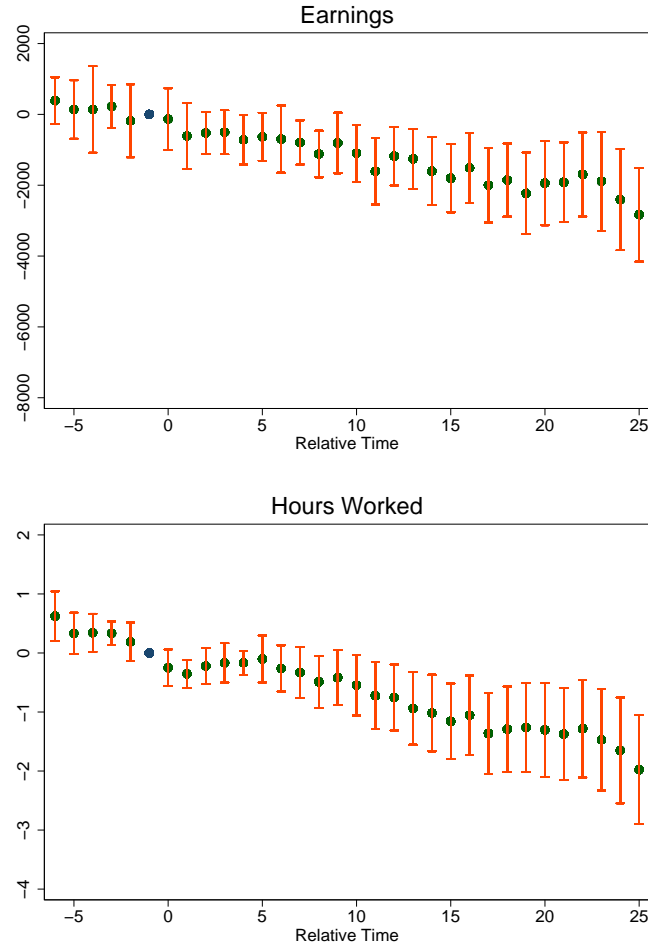
Notes: Data come from NLSY79 (1957-1965 birth cohorts). All outcomes are measured in 1979. Models include controls for race, gender, and family income as well as state at age 14 and age fixed effects. All estimates are weighted by the NLSY79 sample weights. The Rotter Locus of Control measures the extent 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



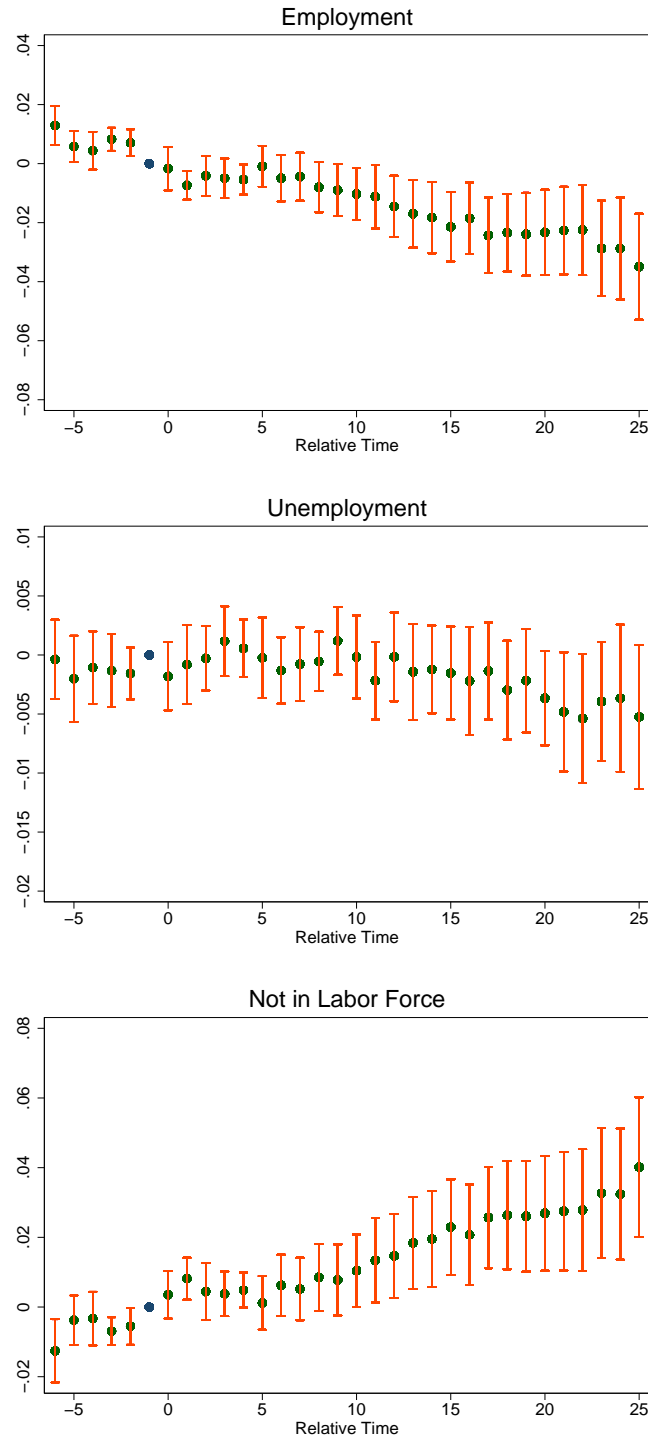
Source: NBER Public Sector Collective Bargaining Law Data Set (Valletta and Freeman 1988), updated by Kim Reuben to 1996.

Figure 2: Event Study Estimates - Earnings and Hours Worked



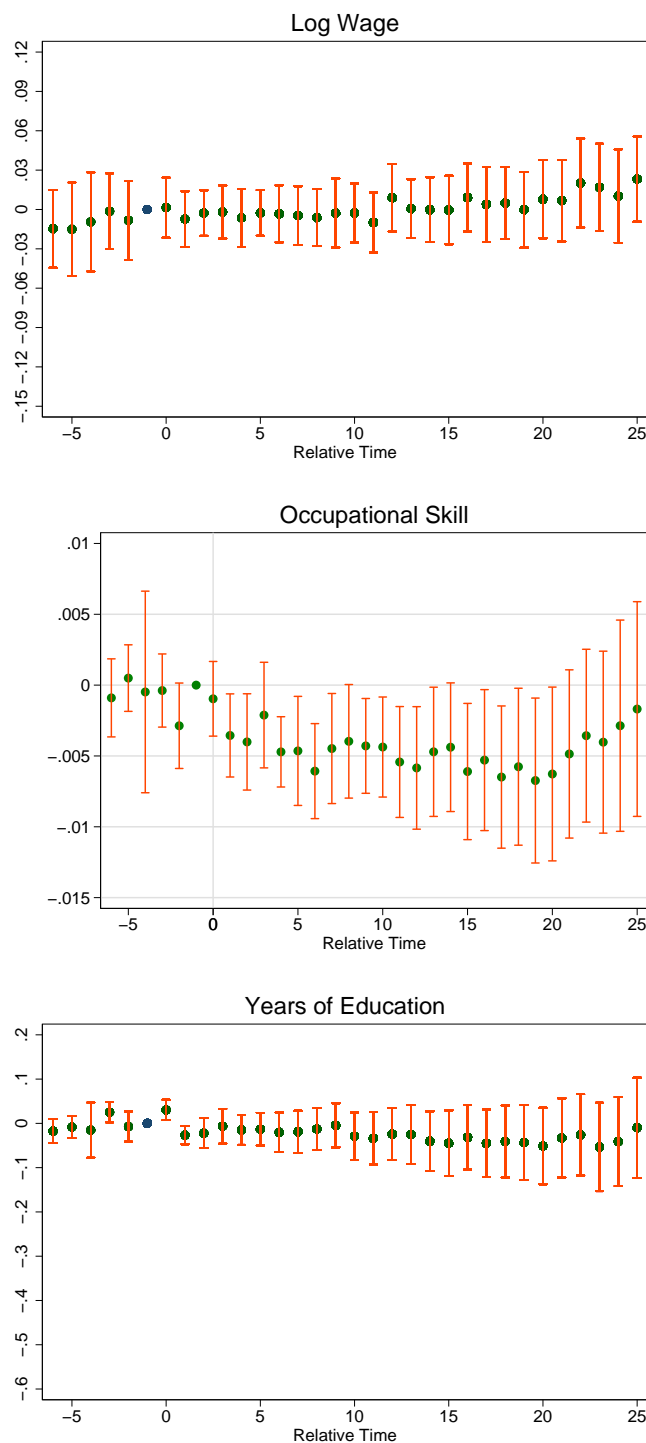
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year and birth state-by-year fixed effects as well as controls for the percent male, the racial/ethnic composition of the state-age-year 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-age-year 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



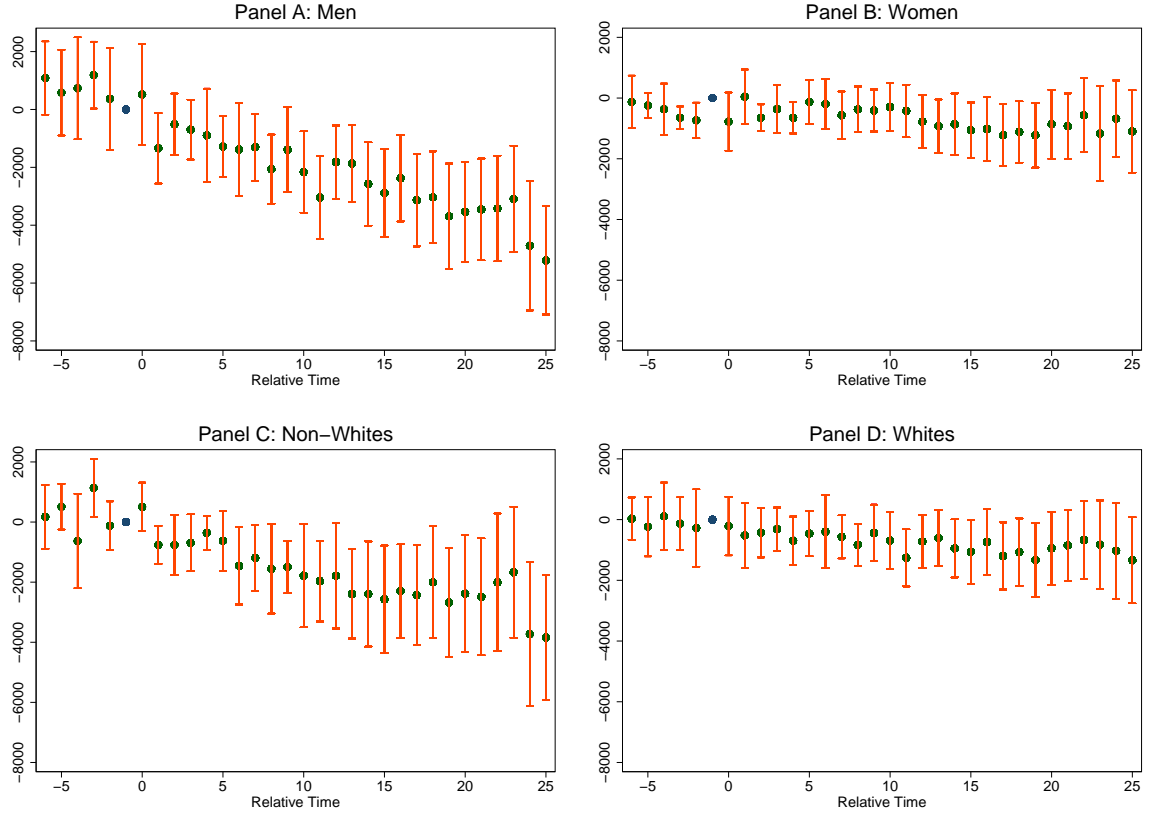
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year and birth state-by-year fixed effects as well as controls for the percent male, the racial/ethnic composition of the state-age-year 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-age-year 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 - Wages, Occupational Skill and Years of Education



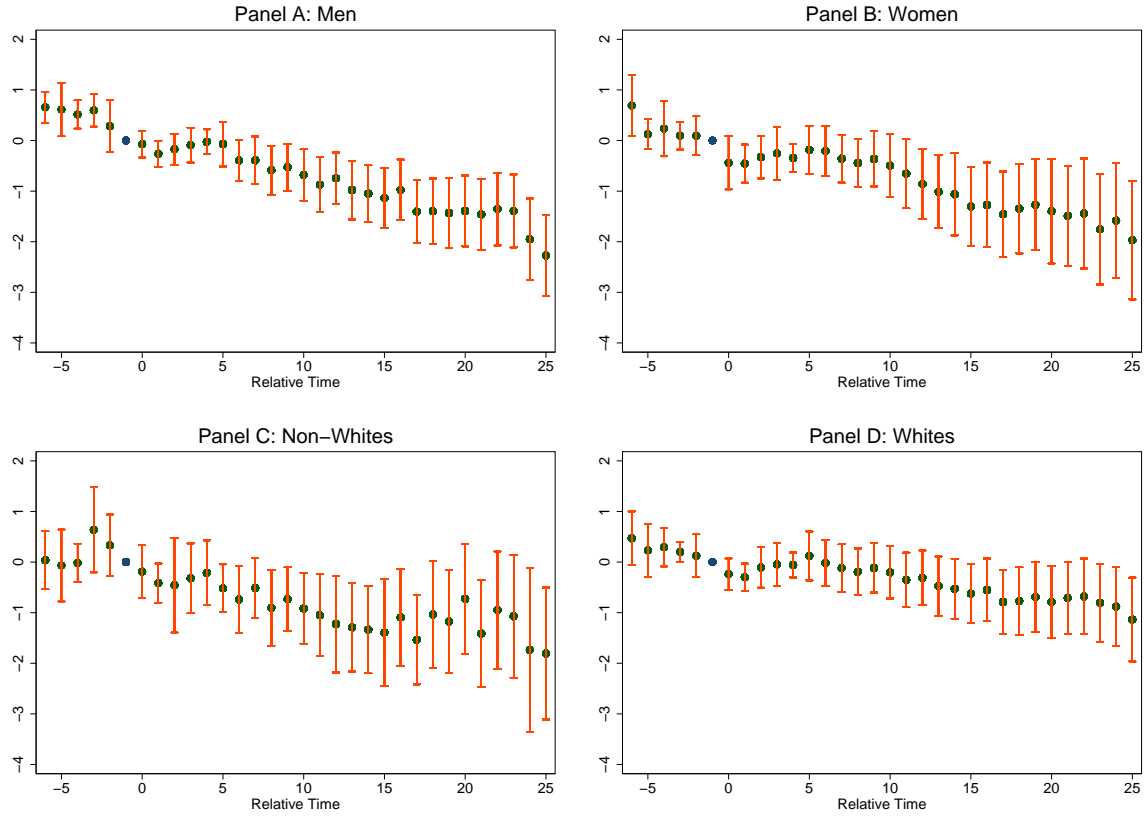
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year and birth state-by-year fixed effects as well as controls for the percent male, the racial/ethnic composition of the state-age-year 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-age-year 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 5: Event Study Estimates by Gender and Race/Ethnicity - Earnings



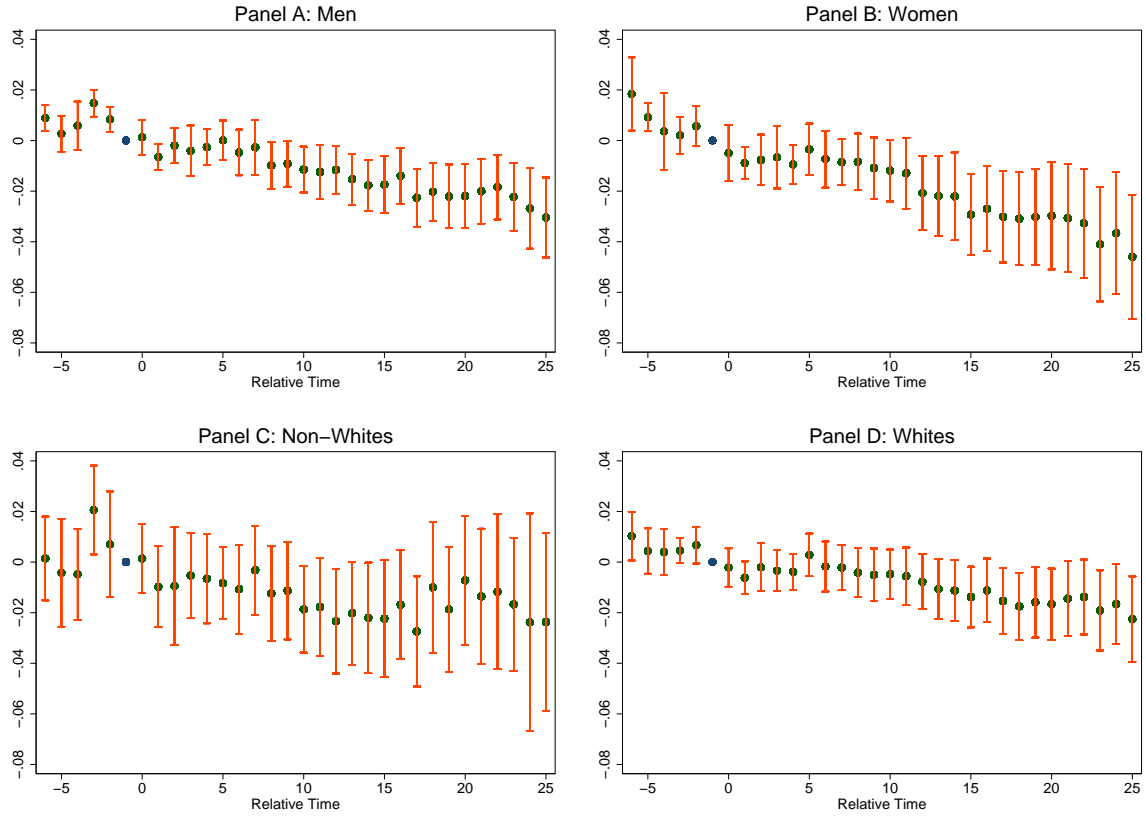
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panels A and B include controls for racial/ethnic composition of the state-age-year cell, while those in Panels C and D control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 6: Event Study Estimates by Gender and Race/Ethnicity - Hours Worked



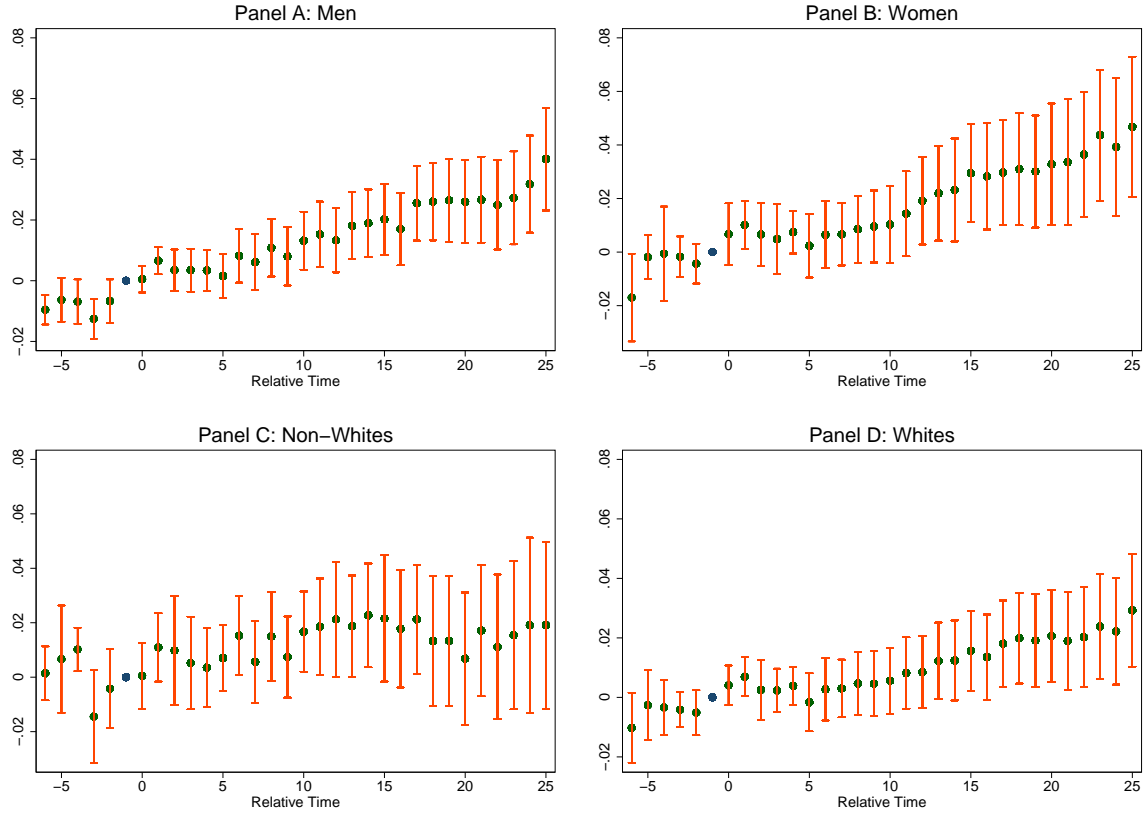
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panels A and B include controls for racial/ethnic composition of the state-age-year cell, while those in Panels C and D control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 7: Event Study Estimates by Gender and Race/Ethnicity - Employment



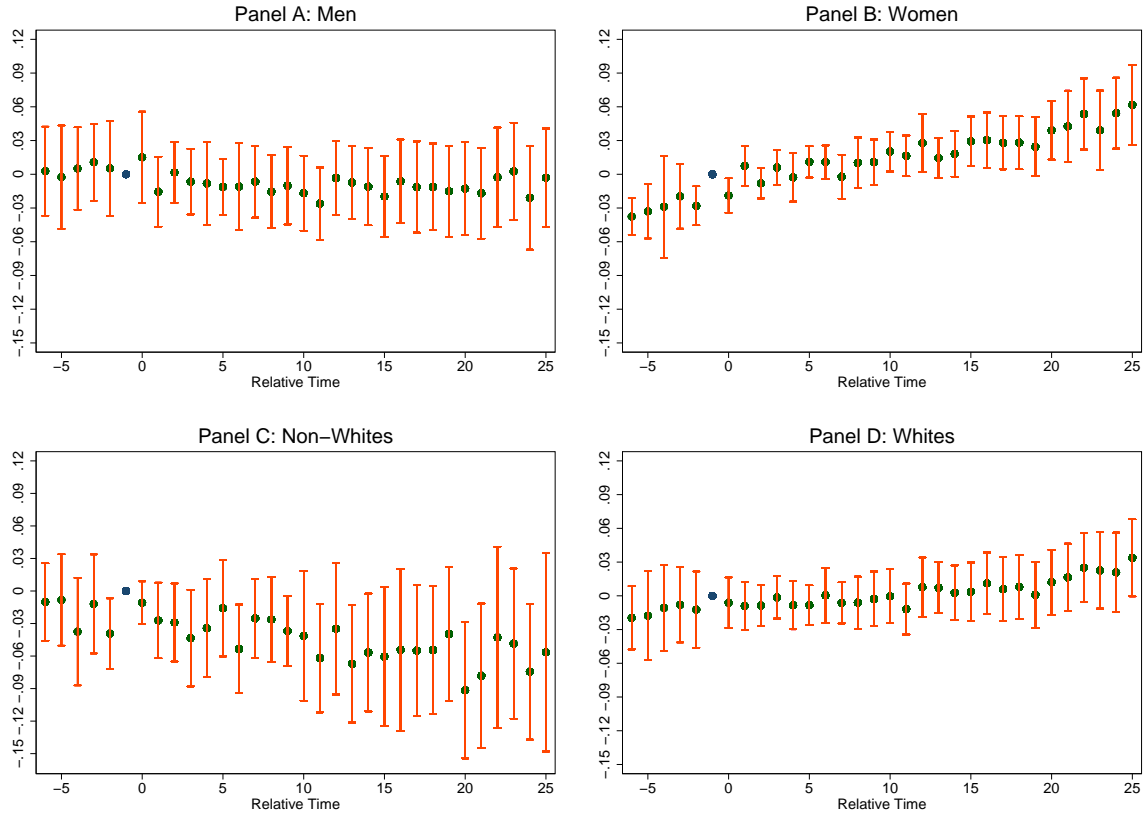
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panels A and B include controls for racial/ethnic composition of the state-age-year cell, while those in Panels C and D control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 8: Event Study Estimates by Gender and Race/Ethnicity - Not in Labor Force



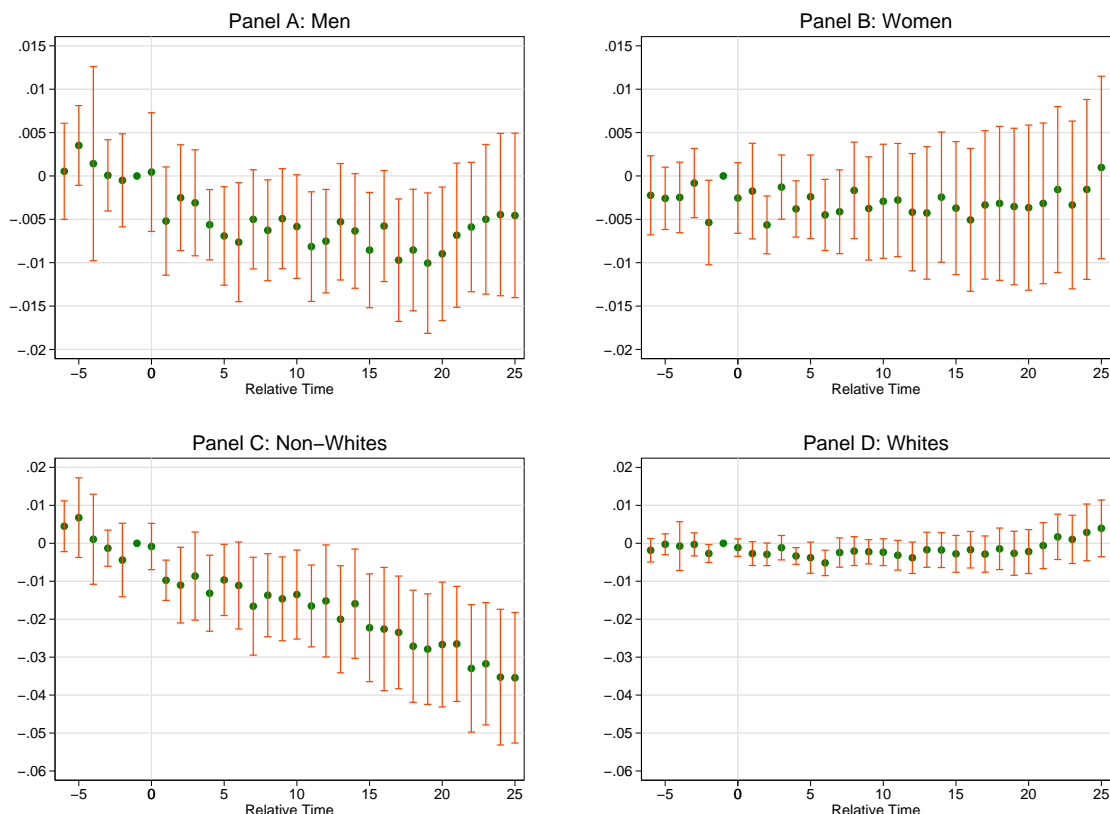
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panels A and B include controls for racial/ethnic composition of the state-age-year cell, while those in Panels C and D control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 9: Event Study Estimates by Gender and Race/Ethnicity - Log Wage



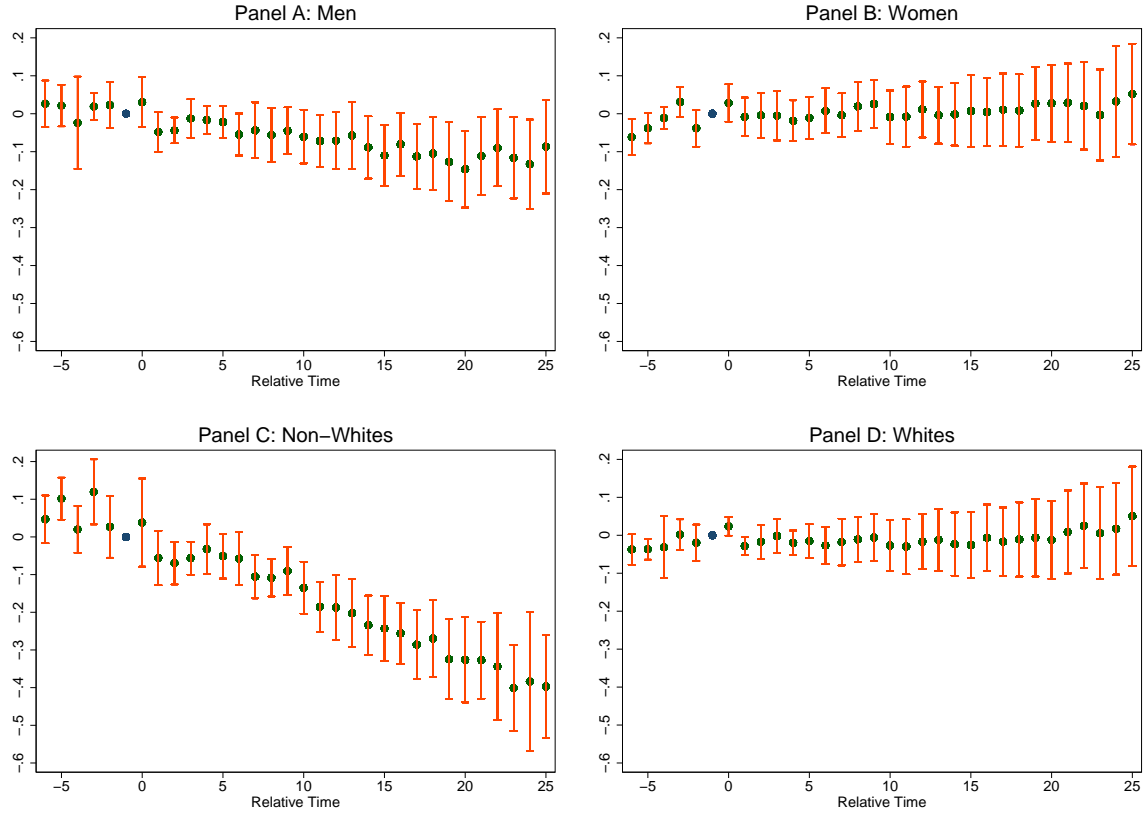
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panels A and B include controls for racial/ethnic composition of the state-age-year cell, while those in Panels C and D control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 10: Event Study Estimates by Gender and Race/Ethnicity - Occupational Skill



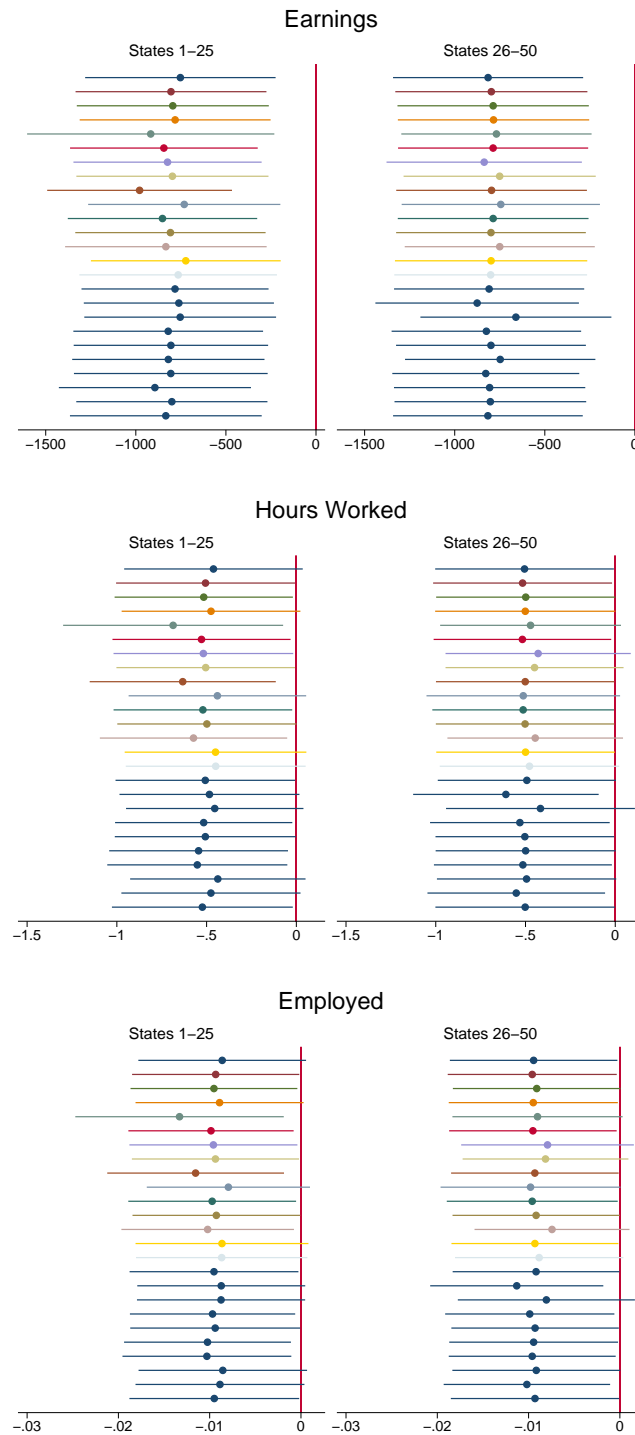
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 individual ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panels A and B include controls for race/ethnicity, while those in Panels C and D control for gender. 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 11: Event Study Estimates by Gender and Race/Ethnicity - Years of Education



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 -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panels A and B include controls for racial/ethnic composition of the state-age-year cell, while those in Panels C and D control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 12: Sensitivity of Results to Excluding Each State



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.

**Table A-1: The Effect of Collective Bargaining Laws
on Years of Education, 2008-2012 ACS
Years Only**

Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.014 (0.028)	-0.022 (0.026)	-0.016 (0.027)
SFR, EITC and Food Stamp Controls		x	x
Birth State*Year FE			x
Age*Year FE			x

Notes: Authors' estimation of equation (1) as described in the text using 2008-2012 ACS data on 35-49 year old respondents. All estimates include birth state, age and year fixed effects, as well as controls for the percent male and the racial/ethnic composition of the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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-2: The Effect of Collective Bargaining Laws on Educational Attainment Levels

Panel A: HS Grad			
Treatment Measure	(i)	(ii)	(iii)
Exposure	0.006 (0.007)	0.006 (0.007)	0.000 (0.009)
Panel B: Some College			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.008 (0.005)	-0.007 (0.005)	-0.002 (0.005)
Panel C: AA Completion			
Treatment Measure	(i)	(ii)	(iii)
Exposure	0.002 (0.003)	0.002 (0.003)	0.001 (0.003)
Panel D: BA Completion			
Treatment Measure	(i)	(ii)	(iii)
Exposure	-0.003 (0.007)	-0.006 (0.007)	-0.003 (0.007)
SFR, EITC and Food Stamp Controls		x	x
Birth State*Year FE			x
Age*Year FE			x

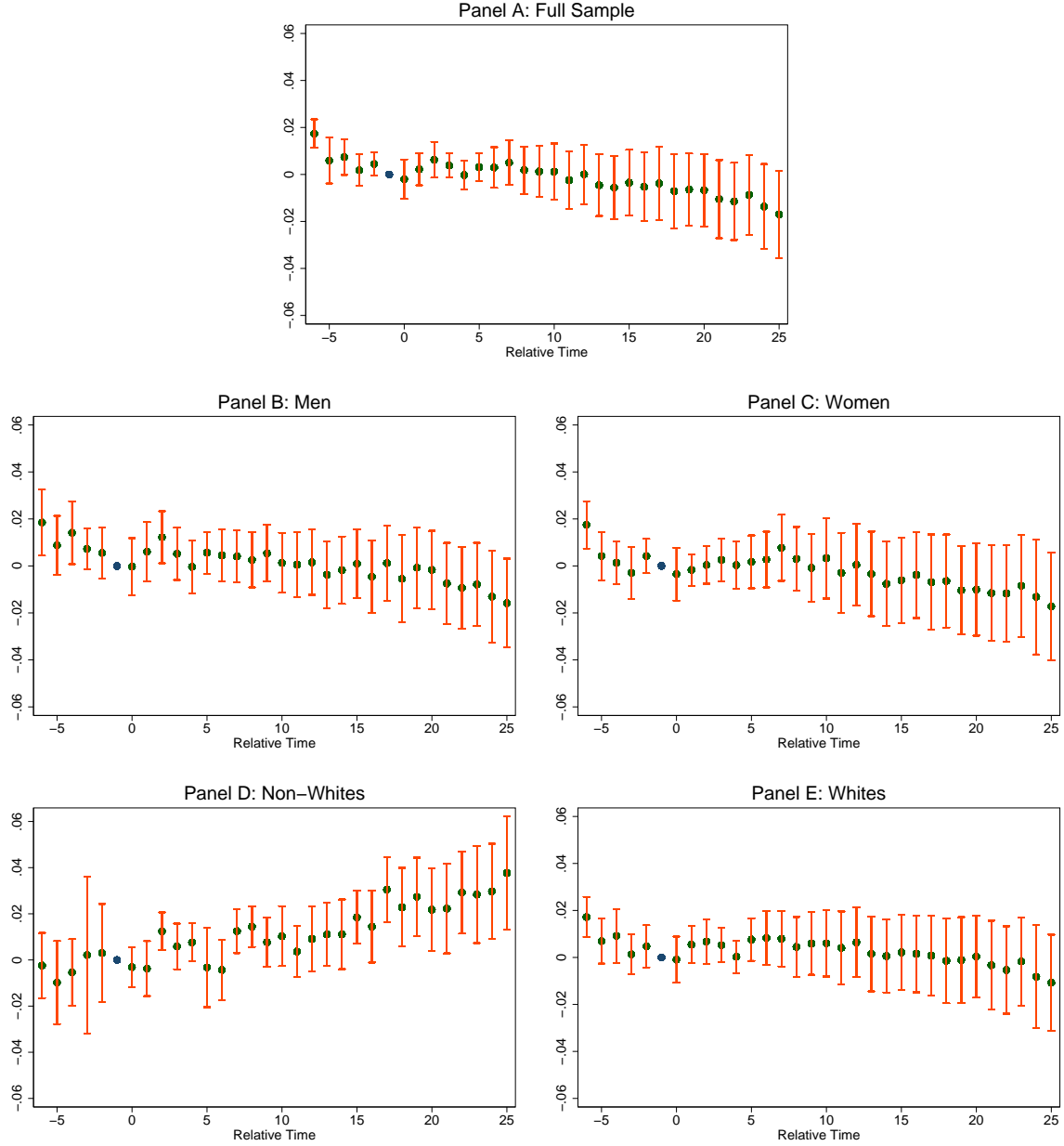
Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Regressions are based on 6,000 birth state-age-year observations. All estimates include birth state, age and year fixed effects, as well as controls for the percent male and the racial/ethnic composition of the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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-3: The Effect of Collective Bargaining Laws on Educational Attainment Levels, by Gender and Race/Ethnicity

Panel A: Men				
	HS	Some		
Treatment Measure	Grad	College	AA	BA
	(i)	(ii)	(iii)	(iv)
Exposure	-0.000	-0.002	-0.000	-0.006
	(0.009)	(0.009)	(0.004)	(0.006)
Panel B: Women				
	HS	Some		
Treatment Measure	Grad	College	AA	BA
	(i)	(ii)	(iii)	(iv)
Exposure	0.003	-0.005	0.001	-0.001
	(0.011)	(0.005)	(0.004)	(0.008)
Panel C: Nonwhites				
	HS	Some		
Treatment Measure	Grad	College	AA	BA
	(i)	(ii)	(iii)	(iv)
Exposure	0.005	-0.015**	-0.005	-0.009
	(0.007)	(0.006)	(0.004)	(0.006)
Panel D: Whites				
	HS	Some		
Treatment Measure	Grad	College	AA	BA
	(i)	(ii)	(iii)	(iv)
Exposure	0.006	-0.003	0.001	-0.006
	(0.010)	(0.006)	(0.005)	(0.007)

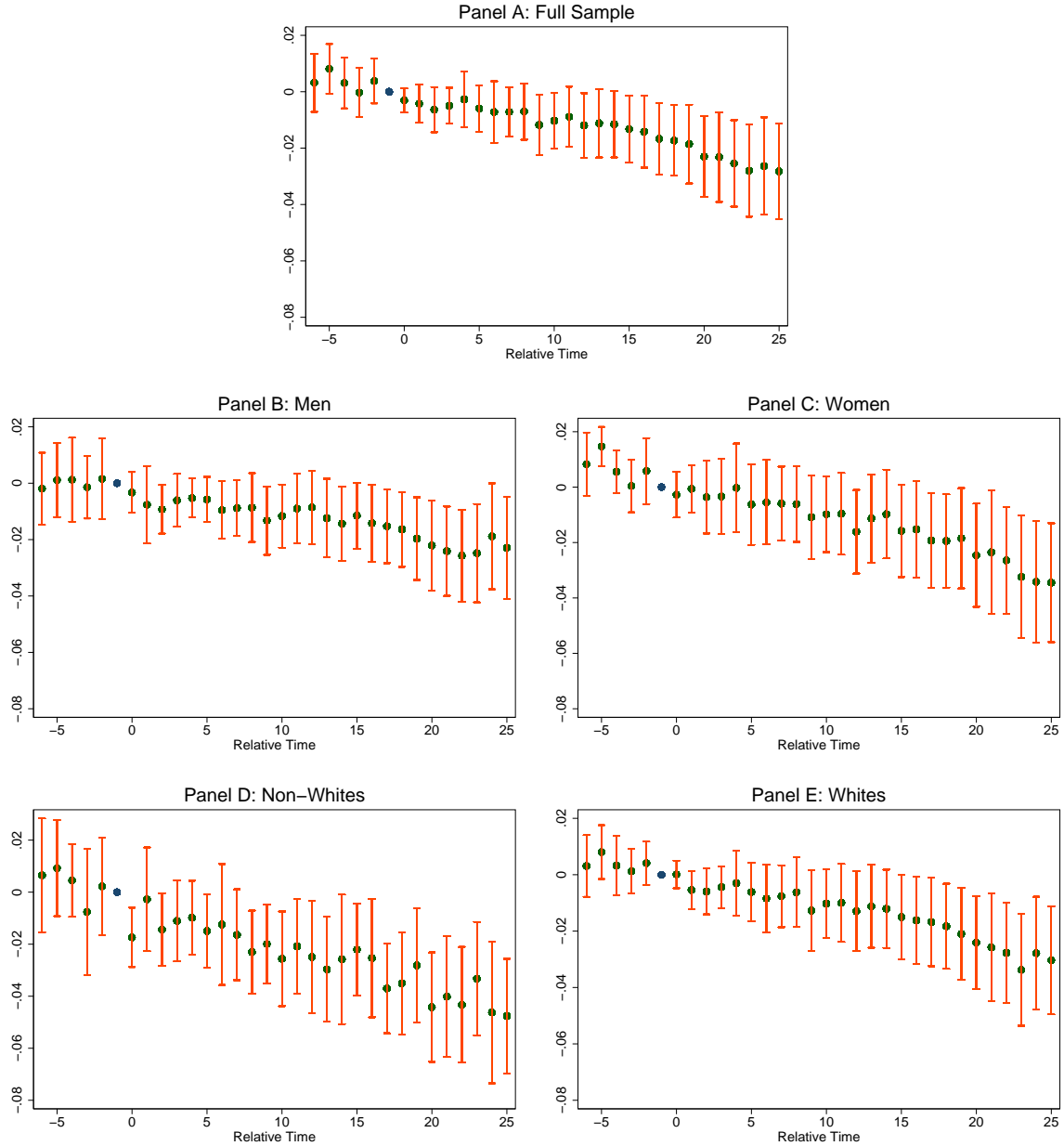
Notes: Authors' estimation of equation (1) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Regressions are based on 6,000 birth state-age-year observations. All estimates include birth state, age and year fixed effects. Results in Panels A and B include controls for the racial/ethnic composition of the state-age-year cell, while those in Panels C and D control for the percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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.

Figure A-1: Event Study Estimates - High School Completion



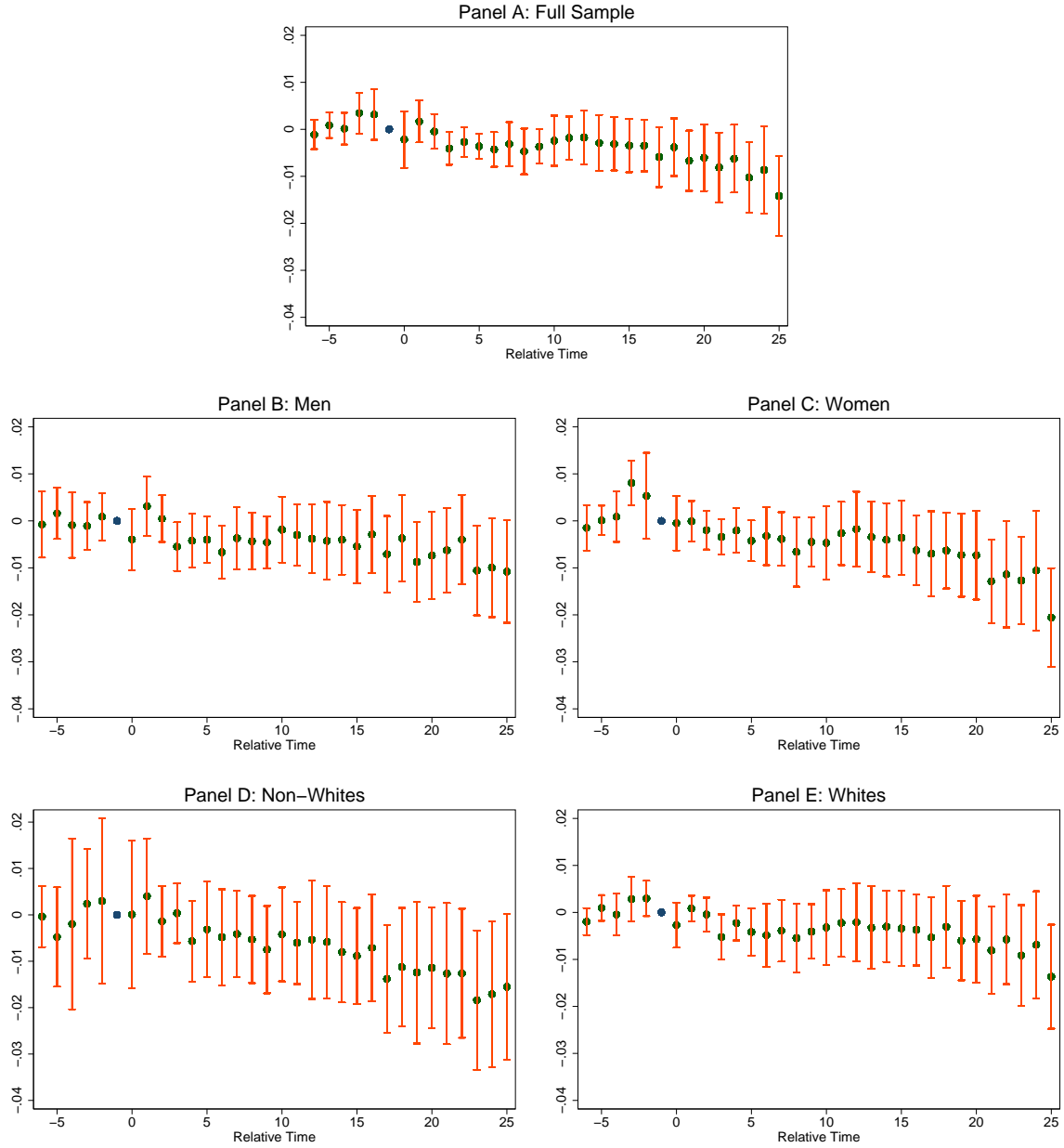
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panel A include controls for racial/ethnic composition and the percent male in of the state-age-year cell, estimates in Panels B and C include controls for racial/ethnic composition of the state-age-year cell, and those in Panels D and E control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 - Some College



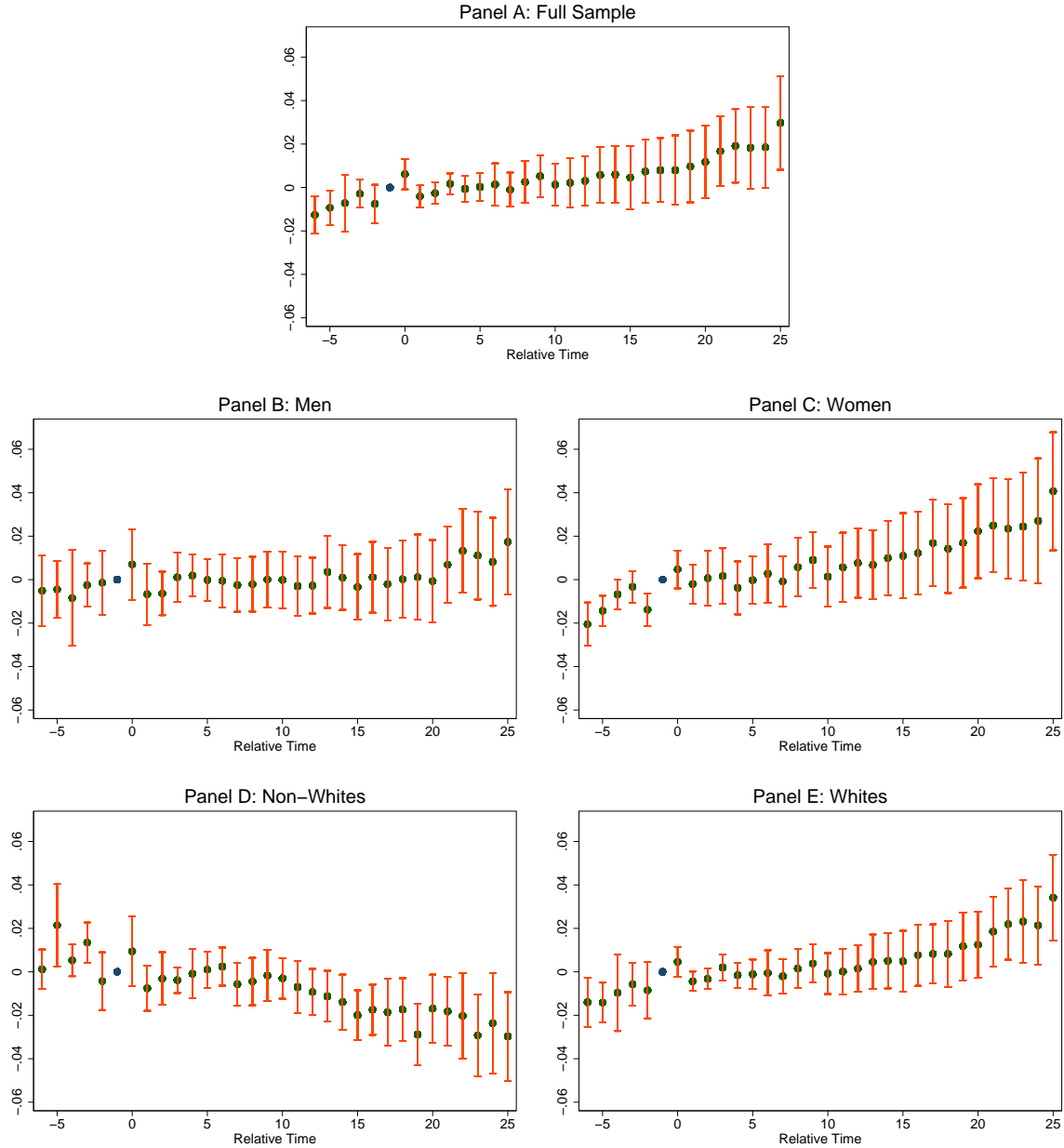
Notes: Authors' estimation of equation (2) as described in the text using 2005-2012 ACS data on 35-49 year old respondents. Relative year -1 is omitted, so all estimates are in relationship to this year. Relative year -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panel A include controls for racial/ethnic composition and the percent male in of the state-age-year cell, estimates in Panels B and C include controls for racial/ethnic composition of the state-age-year cell, and those in Panels D and E control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 - AA Completion



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 -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panel A include controls for racial/ethnic composition and the percent male in of the state-age-year cell, estimates in Panels B and C include controls for racial/ethnic composition of the state-age-year cell, and those in Panels D and E control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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 - BA Completion



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 -6 includes all observations with relative time ≤ -6 and relative year 25 includes all observations with relative time ≥ 25 . All estimates include age-by-year fixed effects, birth state-by-year fixed effects, and exposure to school finance reforms, state EITC rates, and food stamps. Estimates in Panel A include controls for racial/ethnic composition and the percent male in of the state-age-year cell, estimates in Panels B and C include controls for racial/ethnic composition of the state-age-year cell, and those in Panels D and E control for percent male in the state-age-year cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-age-year 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.