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Source: *American Economic Journal: Economic Policy*, August 2019, Vol. 11, No. 3 (August 2019), pp. 292-324

Published by: American Economic Association

Stable URL: <https://www.jstor.org/stable/10.2307/26754074>

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The Long-Run Effects of Teacher Collective Bargaining[†]

By MICHAEL F. LOVENHEIM AND ALEXANDER WILLÉN*

We analyze how exposure to teacher collective bargaining affects long-run outcomes for students, exploiting the timing of state duty-to-bargain law passage in a cross-cohort difference-in-difference framework. Among men, exposure to a duty-to-bargain law in the first 10 years after passage depresses annual earnings by \$2,134 (3.93 percent), decreases weekly hours worked by 0.42, and reduces employment and labor force participation. The earnings estimate implies that current duty-to-bargain laws reduce earnings by \$213.8 billion annually. Effects grow with time since law passage, are largest among nonwhites, and are not evident for women. Duty-to-bargain laws reduce male noncognitive skills, supporting the labor market findings. (JEL I21, J22, J31, J45, J51, J52, K31)

Teacher collective bargaining is a prevalent and contentious feature of the US education system. Over 60 percent of teachers in the United States currently are covered by a collectively bargained contract (Frandsen 2016), and recently many states have weakened the ability of teachers' unions to negotiate contracts. For example, in 2011, Wisconsin, Indiana, Idaho, and Tennessee passed legislation that greatly reduced the scope of teacher bargaining. Michigan passed a public employee right-to-work law that sought to limit teacher union negotiating power in 2012, and the 2018 Supreme Court decision in *Janus v. AFSCME* nationalized right-to-work rules for public sector employees.¹ 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

The debate over the proper role of teacher collective bargaining in the US education system rests on how such bargaining impacts student outcomes, among other

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[†]Go to <https://doi.org/10.1257/pol.20170570> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

¹Right-to-work laws make it illegal to force employees to join the union or pay union dues as a condition of employment.

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

factors. 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 this 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 when only sparse data were available on student outcomes that could be matched to one's school district. 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 (Hoxby 1996, Lovenheim 2009) or state-level SAT scores (Kleiner and Petree 1988). These analyses reach different conclusions, and their focus on a limited set of performance measures does not yield a complete picture of the effects of teacher collective bargaining on students. More recent studies have better student achievement data but lack exogenous variation in teacher collective bargaining (e.g., Lott and Kenny 2013, Strunk 2011, Moe 2009).

In this paper, we use the timing of the passage of duty-to-bargain DTB laws, which occurred between 1960 and 1987 (see Figure 1), linked with educational and labor market outcomes among 35–49-year-olds in the 2005–2012 American Community Survey (ACS) to provide new evidence on how teacher collective bargaining impacts a broad array of long-run outcomes. The duty-to-bargain laws on which we focus require districts to negotiate with teachers' unions in good faith. Prior work has shown extensive evidence that these laws increase union membership and the probability that a district elects a union to bargain collectively (Frandsen 2016, Lovenheim 2009, Hoxby 1996, Saltzman 1985). Our work is the first, however, to directly study how these laws affect long-run outcomes of students.

We employ cross-cohort difference-in-difference event study models that examine how outcomes changed among students who were differentially exposed to duty-to-bargain laws that had been in place for different lengths of time based on what state and in what year they were born. The sources of variation we exploit come from within-state changes in outcomes across birth cohorts as a function of time since passage of a DTB law and cross-state differences in the timing of when (or whether) these laws were passed. 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.

Our primary results focus on men, for whom we find negative effects of exposure to teacher collective bargaining laws on the long-run labor market outcomes of students who grew up in states with these laws. At 10 years of DTB exposure, annual earnings decline by \$2,134.04 (or 3.93 percent) and weekly hours worked fall by 0.42 (or 1.09 percent). These individuals are 1 percentage point less likely to be employed, are 0.8 percentage points less likely to be in the labor force, and sort into lower skilled occupations. However, collective bargaining laws have only a modest effect on educational attainment, reducing years of education by -0.051 from 10 years of DTB exposure. Our estimates therefore suggest that the effect of teacher collective bargaining on labor market outcomes reflect declines in quality rather than quantity of education. We further substantiate this conclusion by examining

the effect of DTB laws on noncognitive skills using the 1979 National Longitudinal Survey of Youth (NLSY). This analysis shows declines in noncognitive skills due to collective bargaining exposure. Both of these results are consistent with the “rent-seeking” hypothesis of teacher unionization (Hoxby 1996).³

We further demonstrate that the negative effects of duty-to-bargain laws are particularly pronounced among black and Hispanic males: annual earnings decline by \$3,246 (9.43 percent), hours worked per week decline by 0.72 (2.18 percent), the likelihood of being employed is 1.3 percentage points lower, and years of schooling and occupational skill are significantly lower at 10 years of exposure. Collective bargaining laws also lead to worse labor market outcomes among white and Asian men, but the effects are more modest in magnitude.

The results are robust to a range of alternative specifications, suggesting that our results are not driven by other contemporaneous policies, secular trends, or unobserved shocks to the outcomes of interest. First, our models include controls for other important policies during this period to which students may have been exposed. Second, we explicitly test for the existence of pretreatment trends in outcomes across cohorts. Third, the results are robust to directly controlling for pretreatment trends. Fourth, our results are not being driven by the general union environment in the state, are not influenced by the urbanicity of the population, are not correlated with the prevalence of social unrest in the state when our sample was of school age, are not influenced by the political environment in the state, and are robust to accounting for region-specific cohort shocks. Fifth, we perform permutation tests in which we randomly assign the year of duty-to-bargain law passage across states. Finally, our estimates are not biased by cross-state mobility of those with school-age children. Taken together, these results provide extensive evidence that supports the causal interpretation of our estimates.

We do not find consistent effects of collective bargaining law exposure on female labor market and educational attainment outcomes. Most of the point estimates are negative, but they are much smaller than those for men. Further, they show clear evidence of differential pretreatment trends, perhaps reflecting strong secular changes in women’s educational and labor market outcomes among the cohorts we examine (Goldin, Katz, and Kuziemko 2006; Blau and Kahn 2013; Bick, Brüggeman, and Fuchs-Schündeln 2014).⁴ Thus, our empirical approach does not appear valid for women; we cannot draw strong conclusions about how duty-to-bargain laws affect long-run female outcomes with our approach. Importantly, there is no evidence that the secular trends for women produce similar trends among men that would threaten our identification strategy. We do find more evidence of negative effects among black and Hispanic women, which together with the male estimates suggests DTB laws disproportionately affect long-run outcomes among minorities.

Though we are unable to comprehensively examine the mechanisms that drive our results, we show that DTB laws are associated with higher expenditures on teachers

³The rent-seeking hypothesis of teachers’ unions states that unions lead to a reallocation of resources toward teachers while also making educational resources less productive. See Section I for a more in-depth discussion.

⁴These secular trends reflect reduced gender-based discrimination, rising expectations of future labor market participation among women, increased female college attendance, and expanded female labor market opportunities.

and administrators but do not alter total expenditures or teacher-student ratios. This is consistent with prior research, which finds evidence that duty-to-bargain laws reduce hours worked among teachers (Frandsen 2016) and that reduced bargaining power leads to lower fringe benefits among teachers (Litten 2017). However, when examining education policies, we ultimately care about how they impact school quality and the long-run outcomes of students, which we speak to directly in this paper.

Taken together, our results indicate that public sector collective bargaining laws for teachers have a negative effect on male long-run labor market outcomes. The effects we find are economically significant: our estimates suggest that the duty-to-bargain laws that exist in 33 states cumulatively decrease male earnings by \$198.1 billion annually. We underscore that these estimates are from a time period in which the education system was different along many dimensions from today, so caution should be exercised in extrapolating the results to the current education system.

I. Teacher Collective Bargaining in the United States

A. *Duty-to-Bargain Laws*

Prior to 1960, teachers' unions in the United States 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. Beginning with Wisconsin in 1960, states began passing public sector "duty-to-bargain" (DTB) laws, which mandated that districts have to negotiate in good faith with a union that has been elected for the purposes of collective bargaining. These laws gave considerable power to teachers' unions in the collective bargaining process. 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). In states that pass a DTB law, the vast majority of school districts elect a union for the purpose of collective bargaining, and these unions achieve contracts at very high rates (Lovenheim 2009). Thus, passage of a DTB law leads to a high fraction of teachers being covered by a collectively-bargained contract over a short period of time.

Between 1960 and 1987, 33 states passed DTB laws, as shown in Figure 1. Most of these laws were implemented between the late 60s and late 70s. Table 1 shows the year of passage for each state as well as the set of states without such a law.⁵ Of the 17 non-DTB states, 10 allow teachers 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) outlaw collective bargaining. 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.

⁵Note that Washington, DC is excluded both from Table 1 and from our analysis.

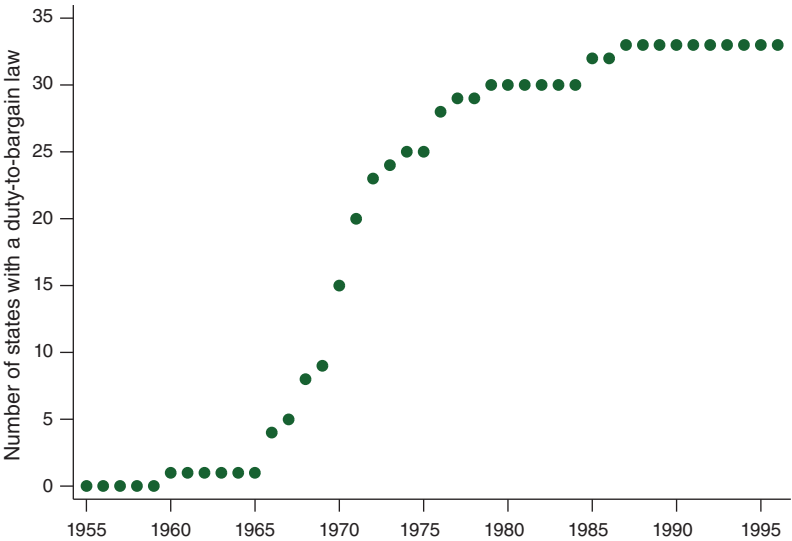


FIGURE 1. THE NUMBER OF STATES WITH TEACHER DUTY-TO-BARGAIN LAWS OVER TIME

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

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	

Note: Blank entries reflect the absence of a teacher duty-to-bargain law in the state.

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

The focus of this paper is on how the passage of public sector DTB laws affects the long-run outcomes of students who attended elementary or secondary schools in those states. We examine duty-to-bargain laws because these laws led to larger increases in unionization and collective bargaining rates than did the other forms of union laws (Frandsen 2016): non-duty-to-bargain union laws do not explicitly require districts to recognize unions and bargain in good faith, thus allowing them to simply refuse to engage in collective bargaining.⁶

B. *Theoretical Predictions*

One of the main ways in which duty-to-bargain laws affect students is by increasing the rate and substance of bargaining between teachers and school districts. Changes in collective bargaining, in turn, can impact students through three main channels: (i) by altering the inputs to education production, (ii) by affecting teacher effort (and thus effectiveness), and (iii) 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 students. The “rent-seeking” model argues that by distorting the allocation of resources toward teachers, student outcomes may decline. The key predictions of this model are that teacher collective bargaining should lead to increases in resources going to teachers and to reductions in the returns to those resources: the resource changes induced by teachers’ unions reduce the efficiency of educational inputs, which negatively impacts students. By protecting teachers from being fired, unions also can reduce teacher effort and lower the quality of the teacher workforce. Under the rent-seeking model, the decline in effectiveness of teacher-related resources can produce worse student outcomes.⁷

In contrast to the rent-seeking model, there are several arguments suggesting that teachers’ unions can improve educational outcomes. Empowering teachers could result in higher achievement from a more efficient allocation of resources, since educational administrators do not have full knowledge of the education production function. There also could be a “union voice” effect, whereby giving teachers a voice with which to influence their working environment makes them more productive (Freeman 1980, Gunderson 2005). A more favorable working environment could further induce more productive workers to enter teaching.

All models of union behavior predict that teachers’ unions will alter district resource allocations; just examining how unions affect education inputs such as teacher pay, employment, and per-student spending will not allow one to distinguish between them.⁸ Where the union models differ is in their predictions of the direction

⁶Our results are similar (though somewhat attenuated) when we use a more expansive definition of collective bargaining laws that includes the ten states that allow but do not require districts to negotiate with teachers’ unions.

⁷The 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.

⁸It 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.

of any effects on achievement. The theoretical ambiguities highlighted previously underscore the importance of conducting an empirical investigation on how teacher collective bargaining affects student outcomes.

Duty-to-bargain laws also can affect outcomes through mechanisms other than unionization and bargaining, *per se*. Teachers' unions engage in statewide advocacy that can influence all school districts, and there can be union threat effects (Farber 2003) that make nonunionized districts behave like unionized ones to stave off a union vote.

C. Prior Research on Teacher Unionization and Collective Bargaining

The majority of research on teachers' unions focuses on resource allocation effects. Collective bargaining can influence several dimensions of school resource allocation decisions: teachers typically negotiate over wage schedules, hiring and firing policies, health care and retirement benefits, work rules detailing the hours they are required to be at work and to teach, class assignments, class sizes, and non-teaching duties (West 2015, Moe 2009, Strunk 2012). Research examining the effect of teacher collective bargaining on district resources has found mixed results, although data constraints have only allowed an examination of a small subset of education inputs. Studies that have exploited the rollout of DTB laws have either found positive effects on teacher salaries and per-student expenditures (Hoxby 1996) or no effects (Lovenheim 2009, Frandsen 2016).⁹ Recent evidence exploiting the substantial restrictions on collective bargaining rights in Wisconsin in 2011 finds increases in teacher wage dispersion and exit (Biasi 2018, Roth 2017) as well as modest effects on average wages but a sizable impact on non-wage compensation (Litten 2017). Results from the 2011 ban on teacher collective bargaining in Tennessee indicates a reduction in teacher compensation in the form of wages and health care and shrinkage in the size of the teacher workforce (Quinby 2017).

Of first-order importance in the policy debate over the role of teachers' unions in education is how collective bargaining affects student outcomes. The effects on resource allocation discussed above yield ambiguous predictions for effects on students. There currently is only a small literature on the effect of teachers' unions on academic achievement. None of these studies estimate the effect of collective bargaining on long-run labor market and educational attainment outcomes, which may differ from any short-run impacts (Ludwig and Miller 2007, Chetty et al. 2011, Deming et al. 2013, Cohodes et al. 2016). One central reason for this lack of existing 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 during the period of DTB law passage or to use data from more recent time periods that lack exogenous variation in collective bargaining across schools.

⁹An earlier body of work finds mixed evidence on how unions affect teacher pay. Balfour (1974), Zuelke and Frohreich (1977), and Kleiner and Petree (1988) find no effect. Eberts and Stone (1986), Moore and Raisian (1987), and Baugh and Stone (1982) find evidence of a union wage premium ranging from 3 percent–12 percent. These studies typically lack plausibly exogenous variation in union status. See Cowen and Strunk (2015) for a review of this literature.

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. 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.¹⁰ Using an alternative unionization measure and a smaller set of states, Lovenheim (2009) finds no such effect.¹¹

Much of the literature that uses more recent data to examine how unions and collective bargaining affect test scores focuses on measures of contract restrictiveness or union power. Lott and Kenny (2013) show that states with higher union dues and union expenditures have lower fourth grade proficiency rates. Strunk (2011) shows that contract restrictiveness is negatively correlated with test score levels but not with 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. Moe (2009) examines how changes over time in union contract restrictiveness within school districts in California relate to changes in student test scores. While he finds that districts with contracts that become more restrictive experience declines in test score growth, it is unlikely that the within-district variation in restrictiveness over time is exogenous.¹²

Our contribution to this literature is to estimate how teacher collective bargaining affects long-run educational and labor market outcomes using an identification strategy that incorporates exogenous variation in the prevalence of collective bargaining in the state. By linking adults in different birth cohorts to their state of birth, we exploit timing differences in the passage of duty-to-bargain laws combined with variation in whether states ever pass such a law 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.

II. Data

The collective bargaining data we use come from the NBER collective bargaining law dataset (Valletta and Freeman 1988).¹³ 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 to create an indicator variable for whether a duty-to-bargain law was in place in each state and year.

¹⁰In 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.

¹¹Some prior work examines the link between teachers' unions and student outcomes using student test score data, but it typically lacks exogenous variation in union status (e.g., Kleiner and Petree 1988, Eberts and Stone 1987).

¹²Evidence from how Wisconsin's collective bargaining changes (Act 10) affected student outcomes are mixed. Biasi (2018) and Roth (2017) find increases in student test scores, while Baron (2018) finds large declines.

¹³These data are available at <http://www.nber.org/publaw/>.

We combine the collective bargaining information with 2005–2012 American Community Survey (ACS) data on individuals aged 35–49. Individuals within this age span typically have completed their education and are on a flat part of their lifetime earnings profile (Haider and Solon 2006). We observe individuals of each age in each of the eight survey years, leading to a balanced panel of age observations in our data. Birth cohorts are constructed by subtracting age from calendar year, and we assume each respondent begins school at the age in which his assigned birth cohort turns six.¹⁴ The birth cohorts range from 1956 to 1977 and 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). These schooling years align with the large rise in duty-to-bargain laws across states in the United States shown in Figure 1.

A main advantage of using the ACS is the ability to link adults to their state of birth, because collective bargaining laws might cause families to migrate across states. These laws also 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 IVD, we show that any bias resulting from such mobility is small. We also do not find evidence that parents are endogenously moving in response to DTB laws prior to a child's birth using changes in the observed composition of those born in a given state and cohort.

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

The ACS contains detailed information on educational attainment and labor market outcomes. Descriptive statistics of the variables we use are shown in online Appendix Table A-1.¹⁵ For educational attainment, we construct a *years of education* variable. In the 2008–2012 ACS, years of completed schooling are reported directly. In the 2005–2007 ACS waves, we use completed schooling levels to construct this variable.¹⁶ We also use the ACS measures of whether an individual is

¹⁴These 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 percent of the sample will enroll in school the year prior to their assigned birth cohort. This is likely to bias our estimates toward zero by generating changes in outcomes in the cohort just prior to DTB passage.

¹⁵Descriptive statistics by gender and race/ethnicity are shown in online Appendix Table A-2.

¹⁶We code educational attainment as follows: 0 for no school completion, 4 for fourth grade completion, 6 for fifth or sixth grade completion, 8 for seventh or eighth grade completion, 9–11 for ninth through eleventh grade

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, salary, and 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.

Finally, we construct a measure of 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 exposure to teacher collective bargaining leads workers to sort into lower or higher skilled occupations.

III. Empirical Methodology

We exploit within-state, cross-cohort differences in exposure to DTB laws driven by cross-state variation in the timing of when or whether states passed these laws in a difference-in-difference framework. The effect of collective bargaining laws on student achievement is likely to vary across cohorts for two reasons. The first is that some cohorts are only exposed for part of their schooling years, which can generate time-varying treatment effects based on the length of exposure to collective bargaining laws. The second factor that influences the time pattern of treatment effects is that the laws themselves may have time-varying impacts on resource allocation (see Lovenheim 2009 and online Appendix Table A-9), the composition of teachers, and teacher effort from unions becoming more powerful or effective over time. There also can be immediate impacts of DTB law passage on student outcomes. Thus, our main empirical approach is to estimate event study models separately for men and women that allow us to nonparametrically identify time-varying treatment effects:

$$(1) \quad Y_{sct} = \beta_0 + \pi_{-11} I(C - t_0 + 18 \leq -11)_{sc} + \sum_{\tau=-10}^{20} \pi_{\tau} I(C - t_0 + 18 = \tau)_{sc} \\ + \pi_{21} I(C - t_0 + 18 \geq 21)_{sc} + \gamma X_{sct} + \delta_{ct} + \theta_s + \phi_t + \varepsilon_{sct},$$

where Y_{sct} is one of the educational or labor market outcomes listed above for those born in state s in birth cohort c and in ACS calendar year t . Regressions are weighted by the number of observations that underlie each birth year–birth state–calendar year–gender cell, and all standard errors are clustered at the birth state level.

The variable $(C - t_0 + 18)$ is equal to the number of years of exposure a cohort has had to a duty-to-bargain law, with C being the birth year and t_0 being the year of passage of the duty-to-bargain law. For example, 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

completion, 12 for twelfth 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.

value of zero in states that have never had a duty-to-bargain law.¹⁷ Hence, $I(C - t_0 + 18 = \tau)$ are indicator variables equal to 1 for each relative year to passage of a duty-to-bargain law between -10 and 20 . We also include an indicator for whether time relative to a DTB law is less than or equal to -11 and for whether it is greater than or equal to 21 .¹⁸ The π_τ coefficients nonparametrically trace out pretreatment relative trends (for π_{-10} to π_{-1}) as well as time-varying treatment effects (π_0 to π_{20}). In practice, we omit $I(C - t_0 + 18 = -1)$ such that all π estimates are relative to the year prior to DTB passage.

Equation (1) also includes birth cohort-by-calendar year (δ_{ct}), birth state (θ_s), and calendar year (ϕ_t) fixed effects. The birth cohort-by-year fixed effects are identical to age fixed effects because birth cohort and calendar year perfectly define age. The cohort-year fixed effects control for any systematic differences across birth cohorts in each calendar year that may be correlated with both the prevalence of duty-to-bargain laws and labor market outcomes. The state fixed effects control for variation in outcomes that are common across birth cohorts within a state, and the year fixed effects account for national shocks that impact all birth cohorts in the same year. We also control for the proportion of each state-cohort-year-gender cell that is black, Asian, Hispanic, or “other.” These controls are in the vector X in equation (1).

The parameters of interest in equation (1) are π_0 to π_{20} , which show the long-run effects of DTB laws among cohorts who are first exposed to these laws in relative years 0 to 20. We show a full set of π estimates in the figures below, but to summarize our findings in a parsimonious way we present effects at 5 (π_5), 10 (π_{10}), and 15 (π_{15}) years in the tables. Effects at ten years are our preferred estimates because they show the effects among the first cohort that spent nearly the entirety of its schooling years in a DTB environment.

Conditional on the 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 DTB law passage. The second is cross-state variation in the timing of when or whether states passed these laws. The assumptions underlying the identification of parameters π_0 to π_{21} are similar to all difference-in-difference analyses: the decision of whether and when to pass a duty-to-bargain law 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.

The π_{-11} to π_{-2} estimates in equation (1) allow us to test the assumption that there is no selection on fixed trends across cohorts. If outcomes are trending in the direction of the estimated treatment effects prior to passage of DTB laws, it suggests a bias from secular trends. As a further check on the credibility of this assumption, we estimate parametric event study models in which the treatment effect is

¹⁷In the time period we examine, no state repeals a duty-to-bargain law.

¹⁸We choose this event window because the sample sizes become small for relative time indicators less than -10 and greater than 20 . Including these “catch-all” relative time indicators allows us to use the full analysis sample, but we caution that it is rather difficult to interpret the coefficients on these two variables.

identified relative to a linear pretreatment trend. The estimates are very similar to those from equation (1).

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

The existence of alternative policies that were passed concurrently with duty-to-bargain laws is a more serious threat to identification. The 1960s–1980s saw many changes to both schooling and social policies that could have affected the birth cohorts we analyze. If the rollout of these policies is correlated with duty-to-bargain passage, it could bias our results. We address this concern by controlling for exposure to three alternative policies that occurred concurrently with the DTB movement that also could impact these students' long-run outcomes: school finance reform, the earned income tax credit (EITC), and food stamps. We know of no other policy changes that could plausibly have impacted the declines in labor market outcomes we document. In the vector X in equation (1), 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 (2016), who show these reforms led to large increases in the outcomes we consider. We also control for average state EITC rates between the ages of 6 and 18 for each cohort, as Bastian and Micheltore (2018) shows that these policies positively affect educational attainment.¹⁹ Finally, Hoynes, Schanzenbach, and Almond (2016) demonstrates 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 18.²⁰ Below, we show estimates both with and without these controls; they have little effect on our results.

IV. Results

Tables 2–4 present baseline estimates of the effect of teacher collective bargaining exposure on labor market outcomes for men (columns 1–3) and women

¹⁹Cohodes et al. (2016) and Brown, Kowalski, and Lurie (2015) show that the Medicaid expansions of the 1980s and 1990s had large, positive effects on the educational attainment and eventual earnings of youth exposed to these expansions. However, our birth cohorts are mostly too old to have been impacted by these policy changes. Furthermore, we cannot control for Medicaid eligibility in this study because eligibility policies and rates are not available prior to 1980. If anything, this is likely to attenuate our estimates.

²⁰The food stamp data come from the publicly available data used by Hoynes, Schanzenbach, and Almond (2016), available at https://assets.aeaweb.org/assets/production/articles-attachments/aer/app/10604/20130375_app.pdf.

(columns 4–6) in exposure years 5 (π_5), 10 (π_{10}), and 15 (π_{15}). These estimates show changes in outcomes relative to the year prior to DTB passage, which is set to zero in the event study models. Each column in each panel comes from a separate estimation of equation (1), and we add controls sequentially across columns. In columns 1 and 4, we control for birth state, birth cohort, and calendar year fixed effects, as well as race/ethnicity. We add controls for state EITC, school finance reform and food stamp exposure during childhood in columns 2 and 5, and columns 3 and 6 add cohort-by-year (i.e., age) fixed effects. We discuss the estimates for men and women in turn below.

A. Baseline Male Estimates

Table 2 presents results for earnings (panel A) and hours worked (panel B), both of which include zeros. Across the first three columns in panel A, there is clear evidence of a negative effect of teacher collective bargaining on male earnings that grows with exposure time. The estimate in column 3 indicates that attending school in a state with a duty-to-bargain law for 5 years reduces earnings by \$1,728.95 per year. The effect grows to $-\$2,134.04$ in year 10 and $-\$2,666.71$ in year 15. We focus on the effect at year 10 because it represents exposure for nearly all schooling years. The reduction in earnings among the 10-year cohorts is 3.93 percent relative to the mean, which is shown directly below the estimates in the table. The 3.93 percent reduction in annual earnings for each individual translates to a large amount of total earnings lost because of the prevalence of duty-to-bargain laws in the United States. Across all 33 states that have a duty-to-bargain law in place, our results suggest an *annual* loss of \$213.8 billion due to male workers having grown up in states that mandate collective bargaining between teachers' unions and school districts.²¹ As the 15 year estimates show, this is likely a conservative estimate of earnings losses due to duty-to-bargain exposure. Furthermore, the estimates in Table 2 are similar across columns, which is inconsistent with biases from age-specific shocks or from exposure to other policies when young.

Panel A of Figure 2 shows the full set of event study estimates for male earnings.²² We overlay a linear fit for the pre- and post-treatment periods to see if there are differential pretreatment trends and if there are time-varying treatment effects. In Section IVD, we show estimates that test directly for biases associated with any pretreatment trends. The visual evidence in panel A of Figure 2 supports our identification strategy: there is no evidence of differential trends in earnings across pretreatment cohorts. When duty-to-bargain laws are passed, earnings decline rather linearly as a function of exposure time. The 5, 10, and 15 year treatment effect

²¹ We obtain this estimate using total wage income for each state and the percent of the workforce that is male (53.16 percent) in 2014, obtained from the Bureau of Labor Statistics. Specifically, we multiply 2014 total income in the 33 states by 0.0393×0.5316 .

²² The event study estimates are based on an unbalanced panel of states due to the timing of when our outcomes are measured and the timing of DTB passage. In results available upon request, we have estimated event studies using the small set of states for which we have sufficient pre- and post-DTB observations. The estimates continue to show no signs of pretreatment trends, and the effect sizes are somewhat larger. There is no evidence that the unbalanced panel we use throughout drives our results and conclusions.

TABLE 2—THE EFFECT OF COLLECTIVE BARGAINING LAWS ON EARNINGS AND HOURS WORKED

Exposure time	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Earnings</i>						
At 5 years	−1,738.43 (476.59)	−1,749.87 (475.46)	−1,728.95 (475.38)	−348.99 (355.43)	−215.31 (366.35)	−214.25 (367.02)
At 10 years	−2,145.54 (597.60)	−2,160.91 (601.33)	−2,134.04 (601.74)	−357.26 (376.29)	−238.37 (367.46)	−238.48 (368.80)
At 15 years	−2,665.56 (699.07)	−2,698.04 (707.30)	−2,666.71 (708.22)	−899.26 (412.58)	−842.71 (405.98)	−839.93 (409.05)
Percent effect at 10 years	−3.95	−3.98	−3.93	−1.18	−0.79	−0.79
<i>Panel B. Hours worked</i>						
At 5 years	−0.065 (0.164)	−0.030 (0.168)	−0.022 (0.167)	−0.238 (0.220)	−0.152 (0.219)	−0.156 (0.221)
At 10 years	−0.459 (0.183)	−0.433 (0.189)	−0.424 (0.189)	−0.514 (0.267)	−0.446 (0.273)	−0.452 (0.276)
At 15 years	−0.676 (0.222)	−0.681 (0.219)	−0.668 (0.217)	−1.148 (0.324)	−1.134 (0.340)	−1.140 (0.343)
Percent effect at 10 years	−1.18	−1.11	−1.09	−1.74	−1.51	−1.53
Other policy controls		X	X		X	X
Birth cohort × survey year FE			X			X

Notes: This table shows the authors’ estimation of equation (1) as described in the text using 2005–2012 ACS data on 35–49-year-old respondents. The table also shows 5-, 10-, and 15-year estimates from the full event study model. Regressions are based on 6,000 birth state–birth cohort–year observations. All estimates include birth state, birth cohort, and year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell. Other policy controls include school finance reform, EITC, and food stamp measures as described in the text. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Percent effect at 10 years shows the 10-year effect relative to the mean presented in online Appendix Table A-1. Standard errors clustered at the birth state level are in parentheses.

patterns shown in Table 2 thus provide an accurate depiction of how DTB law exposure affects earnings.

Panel B of Table 2 presents estimates for weekly hours worked. Consistent with the reduction in earnings, average hours worked decline by 0.424 due to being exposed to DTB laws for 10 years. This is a 1.09 percent decline relative to the mean of 38.96 shown in online Appendix Table A-1. The estimates are stable across columns and are significant at the 5 percent level for men. As with earnings, the negative effect grows linearly in magnitude with years of exposure from a small and not statistically significant effect at year 5 to −0.668 hours in year 15. Panel C of Figure 2 shows event study estimates for this sample and outcome: there is no evidence of differential pretreatment trends, and the effect grows linearly with relative treatment time.²³

²³Event study estimates for hours worked in Figure 2, as well as for employment outcomes in Figure 3, show evidence of a shift in the year just prior to DTB passage. As discussed in Section III, some of this shift is due to misclassification of treatment timing across cohorts because we do not know the year in which respondents entered school. It is unlikely these level shifts represent systematic shocks because of the time-varying nature of the treatment. Importantly, all of our event study estimates reported in the tables are relative to year −1, which is set to 0. Thus, our estimates reflect the change in slope at DTB passage rather than any level shift that occurs prior to passage. Furthermore, the changes between relative years −2 and −1 are not indicative of broader pretreatment trends in the direction of the treatment effect.

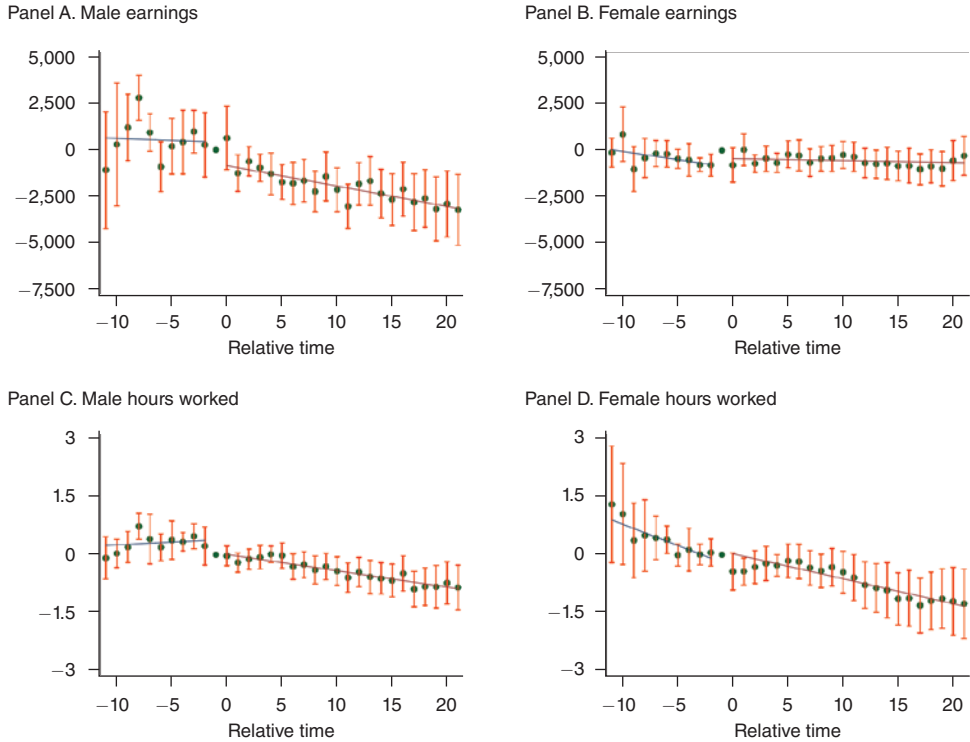


FIGURE 2. EVENT STUDY ESTIMATES—EARNINGS AND HOURS WORKED

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

The finding that teacher collective bargaining is associated with fewer working hours among men suggests that DTB laws may affect the extensive margin of labor supply. Table 3 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. Duty-to-bargain laws reduce male employment and increase the proportion of male workers who are not in the labor force. In panel A, 10 years of exposure to a duty-to-bargain law while in school lowers the likelihood a male worker is employed 1 percentage point, or 1.19 percent relative to the mean. The estimates are significant at the 5 percent or 10 percent levels and are similar in magnitude to the hours worked results. Thus, much of the reduction in hours worked is coming from the extensive margin.²⁴

²⁴That there is an extensive margin effect makes it difficult to examine wages because the treatment is correlated with a change in the composition of wage earners among men. We, therefore, focus on earnings, which can

TABLE 3—THE EFFECT OF COLLECTIVE BARGAINING LAWS ON LABOR MARKET PARTICIPATION

Exposure time	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Employed</i>						
At 5 years	−0.003 (0.003)	−0.002 (0.004)	−0.002 (0.004)	−0.005 (0.005)	−0.004 (0.005)	−0.004 (0.005)
At 10 years	−0.011 (0.005)	−0.010 (0.005)	−0.010 (0.005)	−0.013 (0.005)	−0.011 (0.005)	−0.011 (0.005)
At 15 years	−0.014 (0.005)	−0.014 (0.005)	−0.013 (0.005)	−0.027 (0.006)	−0.027 (0.006)	−0.027 (0.006)
Percent effect at 10 years	−1.33	−1.21	−1.19	−1.76	−1.55	−1.57
<i>Panel B. Unemployed</i>						
At 5 years	0.002 (0.003)	0.002 (0.003)	0.002 (0.003)	0.004 (0.002)	0.004 (0.002)	0.004 (0.002)
At 10 years	0.002 (0.003)	0.001 (0.004)	0.002 (0.004)	0.004 (0.003)	0.004 (0.003)	0.004 (0.003)
At 15 years	0.002 (0.004)	0.002 (0.004)	0.002 (0.004)	0.004 (0.004)	0.003 (0.004)	0.003 (0.004)
Percent effect at 10 years	3.09	2.64	2.71	9.09	8.38	8.49
<i>Panel C. Not in labor force</i>						
At 5 years	0.001 (0.003)	0.000 (0.003)	−0.000 (0.003)	0.002 (0.005)	0.000 (0.005)	0.000 (0.005)
At 10 years	0.009 (0.004)	0.008 (0.004)	0.008 (0.004)	0.008 (0.006)	0.007 (0.006)	0.007 (0.006)
At 15 years	0.012 (0.005)	0.012 (0.005)	0.011 (0.005)	0.024 (0.008)	0.023 (0.008)	0.023 (0.008)
Percent effect at 10 years	7.51	6.93	6.74	3.81	3.29	3.33
Other policy controls		X	X		X	X
Birth cohort × survey year FE			X			X

Notes: This table shows the authors’ estimation of equation (1) as described in the text using 2005–2012 ACS data on 35–49-year-old respondents. The table also shows 5-, 10-, and 15-year estimates from the full event study model. Regressions are based on 6,000 birth state-birth cohort-year observations. All estimates include birth state, birth cohort, and year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell. Other policy controls include school finance reform, EITC, and food stamp measures as described in the text. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Percent effect at 10 years shows the 10-year effect relative to the mean presented in online Appendix Table A-1. Standard errors clustered at the birth state level are in parentheses.

There is little evidence of an effect on unemployment. Rather, teacher collective bargaining laws impact labor force participation: 10 years of exposure to a duty-to-bargain law reduces the male labor force participation rate by 0.8 of a percentage point. Relative to the mean labor force nonparticipation rate, this represents a reduction of 6.74 percent. As with the results in Table 2, effects at year 15 are even larger than those at year 10.

Full event study estimates of employment outcomes are shown in Figure 3. Pretreatment trends are small and if anything are in the opposite direction of the treatment effects. As with hours worked in Figure 2, there is a level shift that occurs

more easily handle changes on the extensive margin due to the inclusion of zeros.

two years before treatment. But the estimates in Table 3 reflect only the post-DTB trend break. The figure shows clear effects of DTB passage on employment and labor force participation that grow over time, but there is no evidence of an effect on unemployment.

Table 4 presents results for occupational skill and educational attainment. In panel A, the dependent variable is the proportion of individuals in one's occupation that has at least some collegiate attainment.²⁵ The results suggest that being exposed to a duty-to-bargain law for 10 years decreases the proportion of workers in one's occupation with at least a college degree by 0.003 (or 0.46 percent relative to the mean) in our preferred model. While the year 10 effect is not statistically significantly different from zero at conventional levels (it is significant at the 11 percent level), both the year 5 and year 15 estimates are of similar magnitude and are significant at the 10 percent level. Panel A of Figure 4 shows full event study estimates for this outcome. The figure shows little evidence of pre-DTB differential trends, and there is a reduction in occupational skill post law passage that accords closely with the estimates in Table 4.²⁶ These results point to collective bargaining laws 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 a DTB law affects years of completed education; estimates on noncognitive outcomes that provide alternative measures of human capital are shown in Section V. Because most people have finished their formal schooling by their mid-30s, the age ranges included in our analysis allow us to accurately measure the total amount of education obtained by each ACS respondent.

Panel B of Table 4 shows results for the total number of years of education. Across columns, the point estimates are negative, modest in magnitude, and are only statistically significantly different from zero at 15 years. Taking the estimates at face value, they suggest a 0.38 percent decline in educational attainment at 10 years that increases in magnitude at 15 years of collective bargaining exposure.²⁷ The event study estimates in panel C of Figure 4 align with the prior results in showing no pre-DTB trends and post-DTB effects that increase linearly in magnitude with exposure time.

²⁵ The regressions in panel A of Table 4 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.

²⁶ Figure 4 shows that much of the effect of occupational sorting is a level shift with much smaller growth in the magnitude of the effects over time since DTB passage than we document for other outcomes. Thus, the pattern of effects for this outcome differs from the other labor market outcomes we examine. This likely reflects somewhat different mechanisms driving the occupational sorting results than the other labor market effects.

²⁷ Examining total years of schooling may miss heterogeneous effects across the distribution of schooling levels. We have estimated equation (1) using the proportion of respondents with different highest levels of educational attainment as the dependent variable to check whether total years of schooling is masking shifts at particular parts of the educational-attainment distribution. The estimates for all schooling levels are small in absolute value, and only the effect on "some college" is significant at the 5 percent level. The small negative effect on reduced years of education thus predominantly reflects lower college enrollment, but we cannot rule out small declines that are distributed evenly throughout the educational-attainment distribution. These results are available from the authors upon request.

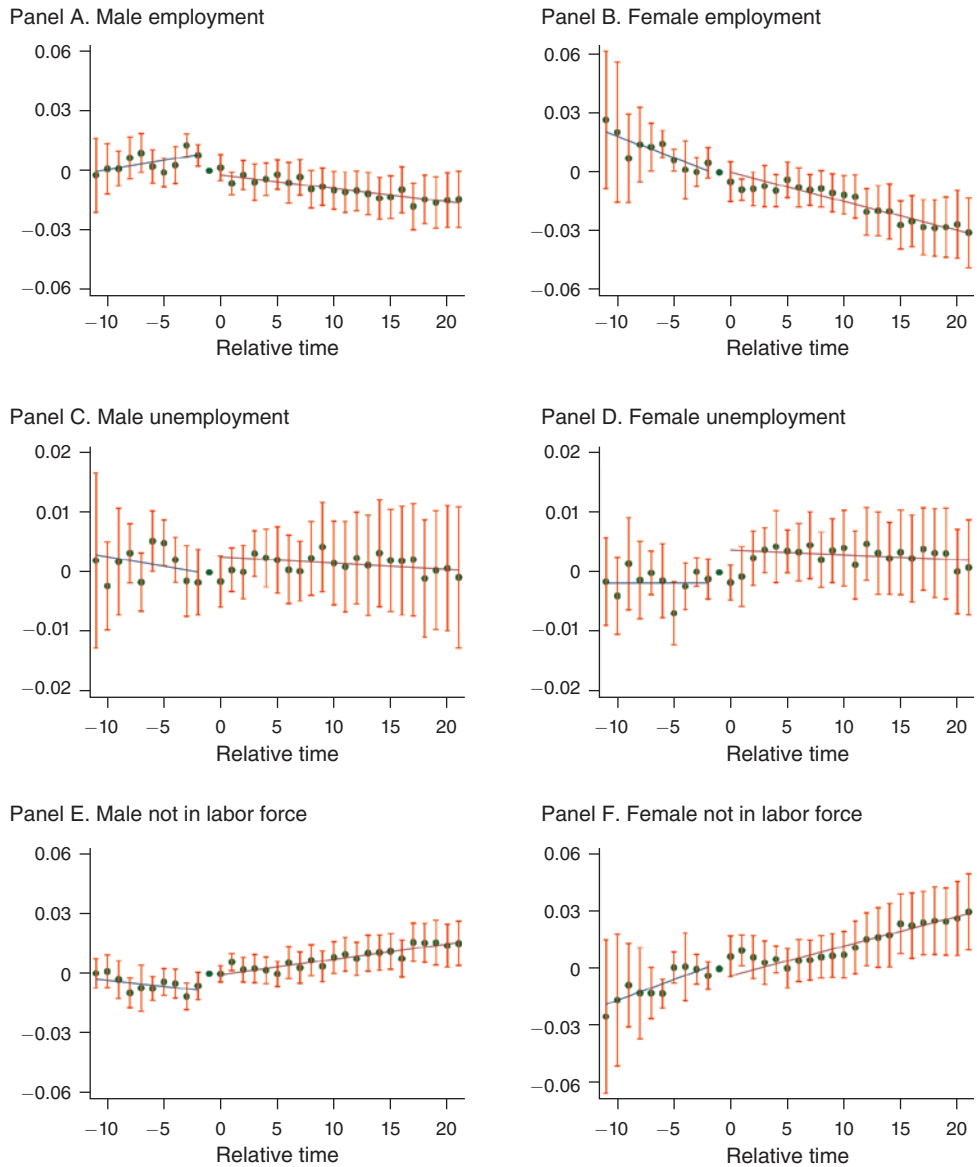


FIGURE 3. EVENT STUDY ESTIMATES—EMPLOYMENT OUTCOMES

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

TABLE 4—THE EFFECT OF COLLECTIVE BARGAINING LAWS ON OCCUPATIONAL SKILL AND EDUCATIONAL ATTAINMENT

Exposure time	Men			Women		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Occupational skill</i>						
At 5 years	−0.003 (0.002)	−0.003 (0.002)	−0.003 (0.002)	−0.001 (0.002)	−0.001 (0.002)	−0.001 (0.002)
At 10 years	−0.003 (0.002)	−0.003 (0.002)	−0.003 (0.002)	−0.002 (0.002)	−0.002 (0.002)	−0.002 (0.002)
At 15 years	−0.004 (0.002)	−0.004 (0.002)	−0.004 (0.002)	−0.002 (0.003)	−0.002 (0.002)	−0.002 (0.002)
Percent effect at 10 years	−0.45	−0.46	−0.46	−0.36	−0.32	−0.32
<i>Panel B. Years of education</i>						
At 5 years	−0.029 (0.025)	−0.026 (0.024)	−0.025 (0.024)	−0.020 (0.027)	−0.019 (0.027)	−0.019 (0.027)
At 10 years	−0.054 (0.039)	−0.051 (0.037)	−0.051 (0.037)	−0.019 (0.033)	−0.017 (0.033)	−0.017 (0.033)
At 15 years	−0.091 (0.038)	−0.088 (0.038)	−0.089 (0.038)	0.001 (0.043)	0.003 (0.043)	0.002 (0.043)
Percent effect at 10 years	−0.40	−0.38	−0.38	−0.14	−0.12	−0.13
Other policy controls		X	X		X	X
Birth cohort × survey year FE			X			X

Notes: This table shows the authors’ estimation of equation (1) as described in the text using 2005–2012 ACS data on 35–49-year-old respondents. The table also shows 5-, 10-, and 15-year estimates from the full event study model. In panel B, regressions are based on 6,000 birth state-birth cohort-year observations and include birth state, birth cohort, and year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Other policy controls include school finance reform, EITC, and food stamp measures as described in the text. In panel A, the dependent variable is the percentage of those in each respondent’s occupation with more than a high school degree. Estimation of equation (1) is done using disaggregated data in panel A and includes birth state, birth cohort, and year fixed effects, as well as controls for respondent race/ethnicity. Percent effect at 10 years shows the 10-year effect relative to the mean presented in online Appendix Table A-1. Standard errors clustered at the birth state level are in parentheses.

How much of the earnings decline can the educational attainment effects explain? The 10-year estimate is precise enough to rule out an effect larger than −0.124 years of completed schooling at the 5 percent level in column 3, which is 0.92 percent relative to the mean. Assuming that an additional year of schooling increases earnings by 10 percent (Card 1999), changes in completed schooling can explain at most 31 percent (0.0124/0.0393) of the earnings effect we find.²⁸ The earnings effect also is likely driven to some extent by reductions in teacher quality, both from changes in who becomes a teacher and in teacher effort. Chetty, Friedman, and Rockoff (2014) shows that having a one standard deviation higher value-added teacher in one grade increases earnings at age 28 by 1.3 percent. Under the assumption that

²⁸ One concern with the estimates in Table 4 is that the ACS changed the way it asked about the total number of years of schooling in 2008. We estimate equation (1) for the total years of schooling outcome using data only from 2008–2012 in online Appendix Table A-3. The estimate is somewhat larger in absolute value but qualitatively similar to the baseline estimate in Table 4. The estimate in Table 4 also is within the 95 percent confidence interval of the estimate in online Appendix Table A-3.

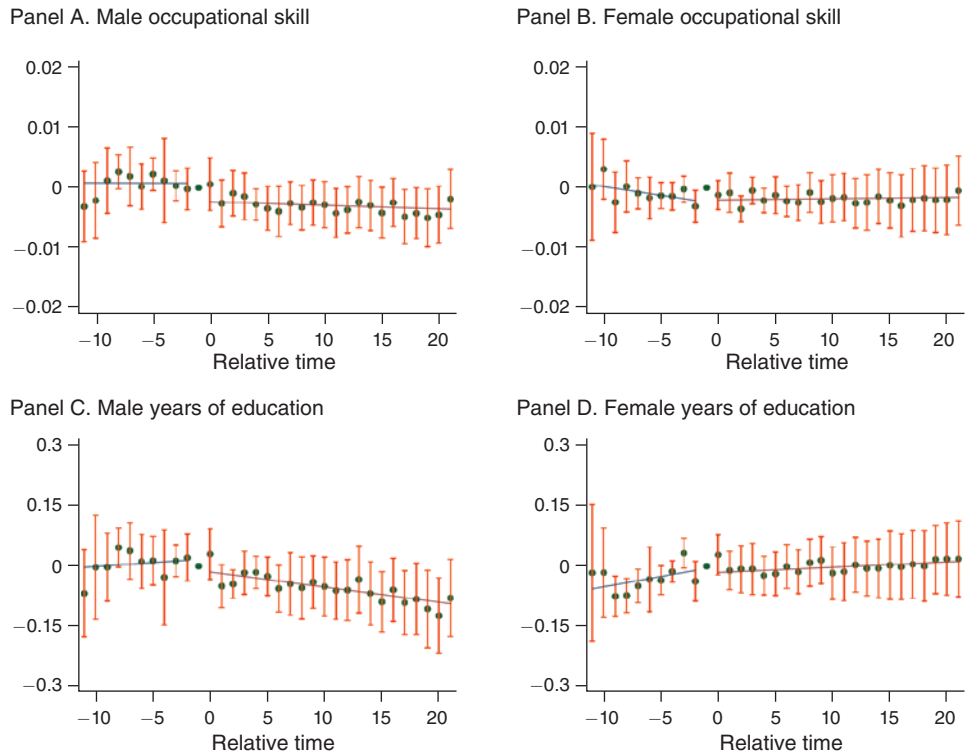


FIGURE 4. EVENT STUDY ESTIMATES—OCCUPATIONAL SKILL AND YEARS OF EDUCATION

Notes: This figure shows the authors' estimation of equation (1) as described in the text using 2005–2012 ACS data on 35–49-year-old respondents. Relative year –1 is omitted; all estimates are in relationship to this year. Relative year –11 includes observations with relative time ≤ -11 , and relative year 21 includes observations with relative time ≥ 21 . All estimates include birth cohort-by-year, birth state, and year fixed effects, as well as other controls for school finance reform, EITC, and food stamp measures. In panels C and D, regressions are based on 6,000 birth state-birth cohort-year observations and include controls for racial/ethnic composition of the state-cohort-year-gender cell. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. In panels A and B, estimation of equation (1) is done using disaggregated data and includes controls for respondent race/ethnicity. Each point is a relative time parameter estimate, while the bars extending from each point show the bounds of the 95 percent confidence interval calculated from standard errors clustered at the state level.

teacher value-added effects are cumulative across grades, our earnings effect is consistent with a 0.30 ($3.93/(10 \times 1.3)$) reduction in teacher value added.

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

long-run outcomes. We return to this issue in Section V when we discuss effects on noncognitive outcomes.

Our results suggest that male students experience worse long-run labor market outcomes when exposed to duty-to-bargain laws. As discussed previously, we are unable to fully examine the mechanisms that underlie this result due to lack of information on teacher productivity and only sparse data on schooling inputs from this time period. Our results are consistent with Frandsen (2016), who shows that DTB law passage leads to fewer work hours among teachers. Litten (2017) also finds evidence from the restriction of collective bargaining rights in Wisconsin that teacher non-wage compensation is reduced. Using the Census/Survey of Governments from 1972–1991, we estimate models of DTB law passage on state average schooling resource allocations that allow for linear pre- and post-DTB trends, as well as a level shift in the year of passage (see equation (2)). Online Appendix Table A-9 presents evidence that DTB passage increases the total amount spent on teachers, especially relative to a negative pre-passage trend, but the largest effect is on administrative salary expenditures.²⁹ These expenditures increase dramatically following law passage, but total expenditures do not change. The shift toward teaching and administrator salaries come at the expense of support service salaries. That the effect grows over time matches the pattern of results in the event study models closely. It is plausible that these changes could reduce school productivity, but we are unaware of research demonstrating a clear link between spending on school administration and student achievement. We also find no effect on teacher-student ratios.

B. Baseline Female Estimates

Tables 2–4 and Figures 2–4 show results for women as well. The results presented in the tables are suggestive of a small negative effect of collective bargaining law exposure among women on labor market outcomes. Importantly, the event study estimates in Figures 2–4 indicate that these effects are biased by cross-cohort pre-DTB trends that are in the same direction as the treatment effects. Unlike the results for men, the pre-trends among women indicate that any negative effects we find are spurious. We therefore urge caution in lending a causal interpretation to these findings.

The pretreatment trends among women likely reflect strong secular shifts in female labor market opportunities across the cohorts we consider (Blau and Kahn 2013; Bick, Brüggeman, and Fuchs-Schündeln 2014). The shifts happen to be negatively correlated with the timing of DTB passage, but it is clear that the forces driving these trends do not affect male outcomes; we find no evidence of a bias from such trends for males either visually or statistically when we control for cross-cohort pre-DTB outcome trends in Section IVD. Thus, our empirical design leads to inconclusive evidence on the effect of duty-to-bargain law exposure on labor market outcomes among women due to the existence of pretreatment trends. There is, however, a clear negative effect for men, for whom we find no evidence of such trends.

²⁹Prior research using these data examines average teacher salaries, not total spending on teachers. This can account for some of the differences between these estimates and those in Hoxby (1996) and Frandsen (2016).

TABLE 5—THE EFFECT OF COLLECTIVE BARGAINING LAWS 10 YEARS POST-DTB PASSAGE ON LONG-RUN OUTCOMES BY RACE/ETHNICITY

Exposure time	Earnings (1)	Hours worked (2)	Employed (3)	Unemployed (4)	Not in labor force (5)	Years of education (6)	Occup. skill (7)
<i>Panel A. Black and Hispanic men</i>							
At 10 years	−3,245.77 (1,571.03)	−0.724 (0.525)	−0.013 (0.013)	−0.000 (0.007)	0.013 (0.008)	−0.196 (0.059)	−0.009 (0.005)
Percent effect	−9.43	−2.18	−1.87	−0.25	6.34	−1.55	−1.33
<i>Panel B. White and Asian men</i>							
At 10 years	−1,660.73 (721.34)	−0.226 (0.214)	−0.007 (0.004)	0.002 (0.003)	0.005 (0.005)	−0.037 (0.039)	−0.002 (0.002)
Percent effect	−2.80	−0.56	−0.83	4.02	5.07	−0.27	−0.31
<i>Panel C. Black and Hispanic women</i>							
At 10 years	−781.05 (619.64)	−1.111 (0.532)	−0.033 (0.015)	0.021 (0.008)	0.012 (0.012)	−0.066 (0.045)	−0.007 (0.003)
Percent effect	−2.99	−3.70	−4.69	28.94	5.23	−0.51	−1.13
<i>Panel D. White and Asian women</i>							
At 10 years	48.03 (453.81)	0.008 (0.282)	−0.001 (0.007)	0.001 (0.002)	0.000 (0.007)	−0.017 (0.044)	−0.001 (0.002)
Percent effect	0.15	0.03	−0.17	1.98	0.20	−0.12	−0.23

Notes: This table shows the authors’ estimation of equation (1) as described in the text using 2005–2012 ACS data on 35–49-year-old respondents. Ten-year estimates from the full event study model are shown. Regressions are based on 6,000 birth state–birth cohort–year observations. All estimates include birth state, year, and birth cohort–by–year fixed effects, as well as controls for exposure to school finance reform, food stamps, and EITC when of school age. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state–birth cohort–year–gender–race cell. Percent effects show effects relative to the mean of each dependent variable. Standard errors clustered at the birth state level are in parentheses.

Motivated by these findings, we focus much of the remainder of the analysis on men but also present female estimates for completeness.

C. Estimates by Race/Ethnicity

We show estimates by race and ethnicity at 10 years in Table 5. Panels A and B present results for black and Hispanic men and white and Asian men, respectively, and panels C and D present similar results for women. Examining results among blacks and Hispanics separately is of great interest, as urban areas that differentially service minority students were more likely to unionize first and to have stronger unions.³⁰ 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

³⁰Urban districts were more likely to be represented by the more confrontational AFT rather than the NEA, which could drive some of our results. It also could be that teachers’ unions themselves have different effects on nonwhite children. Unions could exacerbate racial differences in disciplinary behavior or otherwise lead to differences in how African American and Hispanic children are treated relative to white and Asian children. Investigating this mechanism is beyond the scope of the paper, but the reductions in noncognitive skills we show in Section V are consistent with this mechanism.

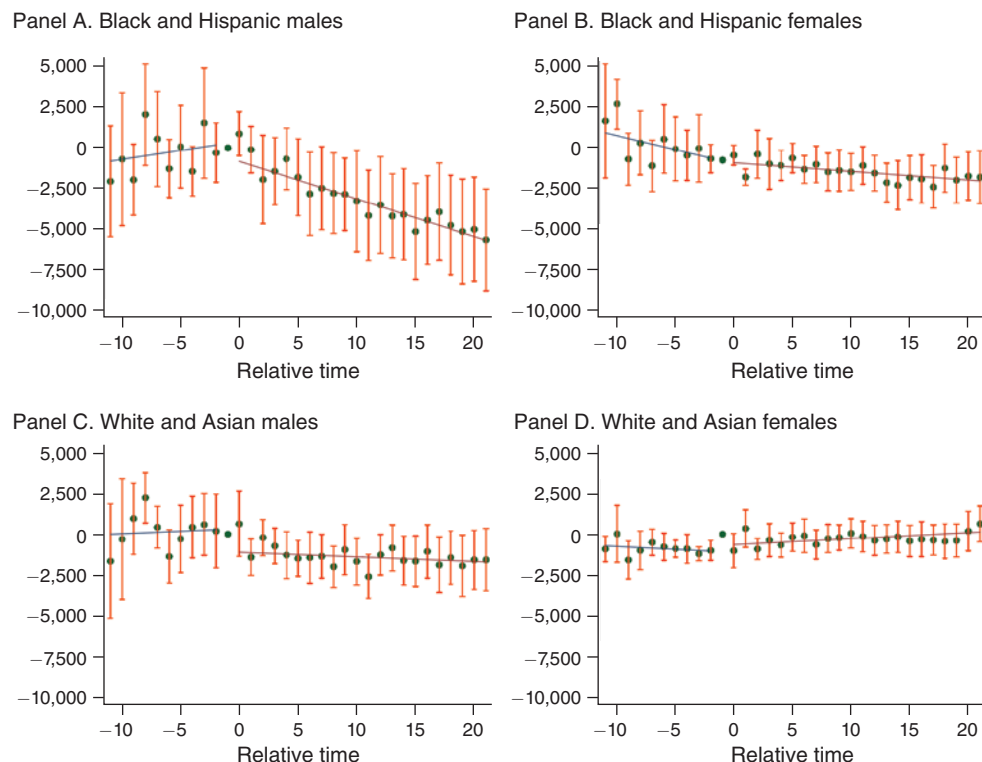


FIGURE 5. EVENT STUDY ESTIMATES BY GENDER AND RACE/ETHNICITY—EARNINGS

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

effects for nonwhites versus whites could reveal substantial heterogeneity in treatment effects.

As shown in panel A, the impact of duty-to-bargain law exposure is particularly large among black and Hispanic men: at 10 years earnings decline by \$3,246 (9.43 percent), hours worked decline by 0.72 (2.18 percent), employment declines by 1.3 percentage points (1.87 percent), and labor force participation is reduced by 1.3 percentage points (6.34 percent). We also find a statistically significant decline in years of schooling of 0.20 years (1.55 percent) and a decline in occupational skill. Panel A of Figure 5 presents earnings event study estimates for this sample. Event studies for other outcomes are presented in online Appendix Figures A-1 through A-5. For each outcome, pre-DTB trends are either zero or in the wrong direction (i.e., opposite to the direction of the treatment effect), and the effect grows with more exposure to a collective bargaining law. In short, these figures mirror the event study estimates for the male sample as a whole but are larger in magnitude.

Panel B of Table 5 shows that the estimates are not isolated to black and Hispanic men; statistically significant adverse effects are present for white and Asian men at 10 years as well, though they are more modest in magnitude. Earnings among white and Asian men decline by \$1,661 (2.80 percent) at 10 years of exposure, and hours worked decline by 0.23 (0.56 percent). The other estimates are consistent with a decline in outcomes and are similar in magnitude to the baseline estimates.

Comparing panels A and C of the race-specific event study figures shows that duty-to-bargain laws lead to worse labor market outcomes among blacks and Hispanics that grow over time, while for whites and Asians the effect is more immediate for several of the outcomes. Hence, the growth in effect sizes with DTB exposure in the baseline estimates is driven predominantly by black and Hispanic men.

Results in panels C and D of Table 5 show suggestive evidence of DTB exposure on outcomes of black and Hispanic women. However, for several outcomes, there are differential pretreatment trends in the same direction as the treatment effect among these women. These trends are not present for all outcomes, but the results in panel C of Table 5 should be interpreted with caution given the event study results. That there is evidence of a negative effect of DTB laws among black and Hispanic women indicates that duty-to-bargain laws have large negative impacts on non-whites. The evidence of effect heterogeneity across race/ethnicity for both men and women suggests collective bargaining laws exacerbate long-run racial inequality in outcomes.

D. Robustness Checks

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

We first show results from estimates of parametric event study models that directly control for pre-DTB trends. We construct a relative time to DTB law variable ($C - t_0 + 18$) that forms the basis for the relative time indicator variables in equation (2).³¹ This variable takes on a value of zero in states that do not pass a duty-to-bargain law. We then estimate models of the following form:

$$(2) \quad Y_{sct} = \alpha_0 + \alpha_1 (C - t_0 + 18)_{sc} + \alpha_2 I(DTB)_{sc} \\ + \alpha_3 (C - t_0 + 18) \times I(DTB)_{sc} + \gamma X_{sct} + \delta_{ct} + \theta_s + \phi_t + \varepsilon_{sct}.$$

All variables are as previously defined. In equation (2), we allow for a level shift (α_2) and a slope shift (α_3) relative to any pretreatment trend (α_1). Thus, this model

³¹ Similar to the event study estimates, we group relative time observations less than -10 and greater than 20 together. We do so to make this model as similar as possible to equation (1) and to avoid the estimates being unduly influenced by observations that are far away from the timing of treatment. This ensures we are identified off the 30-year period surrounding duty-to-bargain law passage.

TABLE 6—PARAMETRIC EVENT STUDY ESTIMATES OF THE EFFECT OF COLLECTIVE BARGAINING LAWS ON LONG-RUN OUTCOMES

	Earnings (1)	Hours worked (2)	Employed (3)	Unemployed (4)	Not in labor force (5)	Years of education (6)	Occup. skill (7)
<i>Panel A. Men</i>							
Relative years to DTB law	56.33 (78.54)	0.031 (0.020)	0.001 (0.001)	−0.0001 (0.0003)	−0.0004 (0.0003)	−0.005 (0.004)	−0.0005 (0.0002)
<i>I</i> (DTB law)	−1,404.81 (509.42)	−0.442 (0.116)	−0.010 (0.003)	0.0010 (0.0015)	0.0085 (0.0028)	−0.003 (0.020)	−0.0005 (0.0010)
Relative years to DTB law × <i>I</i> (DTB law)	−176.36 (80.02)	−0.077 (0.019)	−0.001 (0.001)	0.0001 (0.0003)	0.0012 (0.0003)	0.001 (0.004)	0.0004 (0.0002)
Percent effect at 10 years	−5.84	−3.11	−2.74	2.92	17.11	0.02	−0.61
<i>Panel B. Women</i>							
Relative years to DTB law	−122.39 (57.99)	−0.072 (0.038)	−0.001 (0.001)	−0.0001 (0.0004)	0.0010 (0.0008)	0.0001 (0.0043)	−0.0005 (0.0002)
<i>I</i> (DTB law)	579.21 (323.73)	0.027 (0.167)	−0.004 (0.004)	0.0044 (0.0015)	−0.0001 (0.0038)	0.0120 (0.0254)	0.0019 (0.0011)
Relative years to DTB law × <i>I</i> (DTB law)	105.56 (60.16)	0.006 (0.038)	−0.001 (0.001)	0.0000 (0.0004)	0.0005 (0.0008)	0.0012 (0.0044)	0.0005 (0.0002)
Percent effect at 10 years	5.39	0.30	−1.30	9.83	2.15	0.17	−1.27

Notes: This table shows the authors’ estimation of equation (2) as described in the text using 2005–2012 ACS data on 35–49-year-old respondents. Relative years to DTB law is the number of years relative to the passage of a duty-to-bargain law when each cohort was six years old, which is set to zero for states that never pass such a law. Additionally, *I*(DTB Law) is an indicator for whether a duty-to-bargain law has been passed in the state when each cohort was six years old. Regressions are based on 6,000 birth state-birth cohort-year observations. All estimates include birth state, year, and birth cohort-by-year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell, exposure to school finance reform, food stamps, and EITC when of school age. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Percent effect at 10 years shows the calculated effect 10 years post-DTB passage divided by the mean of each dependent variable. Standard errors clustered at the birth state level are in parentheses.

is not biased by linear pre-DTB trends, so comparing these estimates to baseline provides some evidence of the importance of directly controlling for cross-cohort variation prior to DTB law passage.

Results of estimating equation (2) are shown in Table 6. The results align with the event study estimates and indicate that our results for men are not biased by pretreatment trends. For only one outcome is there a significant pretreatment trend estimate. Aside from unemployment and years of education, there are both level and slope shifts that are of similar magnitudes to those in the baseline tables and that mirror the event study plots. We calculate percent effects after 10 years $((\alpha_2 + \alpha_3 \times 10)/\bar{Y})$, which are directly comparable to the percent effects shown in Tables 2–4. These calculations show estimates that are similar to, if somewhat larger than, the baseline results.³²

Panel B shows estimates of equation (2) for women; similar to the event studies there are pretreatment trends that undermine the validity of the analysis for women.

³² The estimates using equation (2) are larger because they include, to some extent, the changes in outcomes that occur between relative periods −2 and −1, which are evident in the event study figures. This illustrates the value of using a less parametric model (such as equation (1)) that can better disentangle changes that occur posttreatment from those that occur just prior to treatment. In results available upon request, we also have estimated a version of equation (1) that controls for linear pre-DTB trends. The estimates are in-between those in Table 6 and baseline.

Conditional on these linear trends, there is little evidence of an effect of DTB laws on female labor market outcomes.

Online Appendix Tables A-4 and A-5 present additional robustness checks that each examines how our results and conclusions for men and women, respectively, change when we control for additional factors in equation (1) that could be correlated with both duty-to-bargain exposure and long-run outcomes. Throughout, we focus on the 10-year estimates; full event study results for each specification are available upon request.

In panel A, we exclude the 14 states that do not have anti-strike penalties associated with their duty-to-bargain laws.³³ 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 2019). It also could be the case that states becoming more favorable to teachers' unions were becoming more favorable to private sector unions. In panel B, we control for the total unionization rate at age 18 for each birth state-birth cohort.³⁴

The next two panels 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.³⁵ 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. Second, we use data on all riot and collective action protest events from the Dynamics of Collective Action dataset that includes counts of all such 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 18.³⁶ This specification is designed specifically to examine the effect that the urban civil unrest in the 1960s and 1970s has on our estimates. Panel D contains the results that include this additional control. All of the results in panels A–D are extremely similar to baseline.

In panel E, we control for both state-of-birth and current state-of-residence fixed effects (Card and Krueger 1992a, b). The latter set of fixed effects account for the different labor markets in which workers are located that could be correlated with treatment. We estimate this model with individual-level disaggregated data, and the

³³ These states are Wisconsin, Connecticut, Michigan, Massachusetts, Rhode Island, Maine, Vermont, Alaska, Hawaii, Kansas, Pennsylvania, Idaho, Oregon, and Montana.

³⁴ Unionization rates come from CPS Merged Outgoing Rotation Group data collected by Barry Hirsch and David Macpherson: <http://www.unionstats.com>.

³⁵ 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.

³⁶ This dataset can be found at <http://web.stanford.edu/group/collectiveaction/cgi-bin/drupal/>. We obtain similar results if we control for the number of collective action protest events, including nonviolent events.

results are mostly larger in absolute value than baseline: not accounting for current state of residence leads to more conservative estimates.

Panel F adds controls for state-by-year fixed effects. These estimates account for any birth state specific shocks or policies that affect all birth cohorts similarly in a state and year. The estimates are noisier than in the baseline models, but they are qualitatively similar and somewhat larger. These results are consistent with our preferred estimates and provide no evidence of bias from state-by-year specific shocks. Panel G complements these findings by showing estimates in which we control for census region-by-cohort fixed effects. Some regions may be experiencing differential shocks during the time period in which these laws are passed, such as desegregation in the south. The estimates in panel G use only within-region and cohort variation, and they are extremely similar to the baseline results if somewhat larger in absolute value. Finally, in panel H, we control for the proportion of time in each cohort's schooling years and state that Democrats had majority control of the state legislature. We do this in order to account for the potential correlation between political control of the state legislature and unionization. The similarity of the estimates suggests we are not picking up political trends of shifts that drive long-run labor market outcomes.

We also examine the sensitivity of our results to outliers by reestimating 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 online Appendix Figure A-6 for four of our main outcomes: earnings, hours of work, employment, and labor force participation. Our male estimates are insensitive to excluding any one state.³⁷

As discussed above, of primary concern in our identification strategy is the existence of secular trends that differ systematically with treatment exposure. The event study estimates for men suggest such trends are not biasing our estimates. As an additional test of whether the timing pattern of DTB passage is driving our results, we perform permutation tests for all of our outcomes that randomly reassign DTB passage years across states. We do this in two ways: first, we randomly assign DTB passage dates between 1960 and 1987 across states, and second, we randomly assign DTB passage dates to match the timing distribution shown in Figure 1.

Table 7 shows the permutation test results for men. We perform the permutations 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 across states would generate the same pattern of treatment effects. We reject such a null at either the 5 or 10 percent level for every outcome in both panels. These results suggest that our baseline estimates are not identified off of secular trends or endogenous timing of DTB passage. That the effects we estimate are linked strongly to both whether a state passes a DTB law and when it does so supports the validity of our estimation strategy.

³⁷ Because of the geographic concentration of DTB rollout, we lack the power to estimate models separately by census region or that drop specific regions. We also lack the power to drop states that never pass DTB laws (many of which are in the South). The estimates in panel G of online Appendix Table A-4 indicate that our results are not driven by region-specific trends or shocks or by the inclusion of any specific region in our sample, and the estimates in online Appendix Figure A-6 suggest our results are not being driven by the inclusion of any one state.

TABLE 7—*p*-VALUES OF PERMUTATION TESTS AT 10 YEARS FOR MEN

	Earnings (1)	Hours worked (2)	Employed (3)	Not in labor force (4)	Years of education (5)	Occup. skill (6)
<i>Panel A. Randomly assigning passage dates</i>						
Percent less than baseline	0.000	0.080	0.053	0.947	0.096	0.080
<i>Panel B. Randomly assigning passage dates to match passage timing distribution</i>						
Percent less than baseline	0.000	0.030	0.027	0.993	0.070	0.097

Notes: All estimates include birth state, year, and birth cohort-by-year fixed effects, as well as controls for racial/ethnic composition of the state-cohort-year-gender cell, exposure to school finance reforms, average state EITC, and average food stamp availability during school years. Regressions are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. The table shows the proportion of times the estimates from the permutation tests on the 10-year estimate are smaller than the baseline estimate. In panel A, we run 300 simulations in which we randomly assign passage dates to states. In panel B, we randomly assign passage dates to states in a way that matches the overall date-of-passage distribution shown in Figure 1.

A final identification issue comes from measurement error driven by either pre- or post-birth mobility. To assess the importance of pre-birth mobility, we estimate equation (1) using observed fixed characteristics in the ACS and some state-year level observables that are unlikely to be affected by teacher collective bargaining. Because we focus on state of birth, these estimates show whether the composition of people born in a given state and cohort changed with respect to duty-to-bargain law exposure. Online Appendix Table A-6 shows these results. We find little evidence of a change in the composition of birth cohorts that would indicate parents are systematically moving prior to having a child because of duty-to-bargain laws. While there is a small number of statistically significant coefficients, they are quite close to zero and thus are not economically significant. The point estimates are also not in a consistent direction that would indicate a bias from changes in composition driven by DTB law passage.

We next examine the relevance of post-birth mobility, which introduces measurement error into our DTB exposure variable. In the 1990 census, 78.4 percent of 17-year-olds live in the state of their birth. In order to provide information about how serious any mobility-induced bias would be, we reestimate equation (1) under two assumptions. In panel A of online Appendix Table A-7, we show results for men that exclude the 37.7 percent of respondents who do not live in their birth state.³⁸ These estimates are extremely similar to the baseline results.

In panel B, we estimate equation (1) under the assumption that those who live in a state at age 17 other than their birth state spent all of their schooling years in that other state. Using the 1990 census, we create a 50 × 50 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

³⁸ Estimates for women are shown in online Appendix Table A-8.

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).³⁹ The results in panel B are similar to baseline but are somewhat attenuated. This is expected because we are making the extreme assumption that all mobility occurs prior to school entry, which introduces measurement error into the exposure measure. Taken together, the results in online Appendix Table A-7 suggest that any bias from post-birth mobility is small.

V. Medium-Term Effects on Noncognitive 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 noncognitive 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.

Noncognitive skills are measured in three ways: 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, with higher scores indicating *less* internal control (i.e., lower noncognitive skills). The Rosenberg Self-Esteem Scale is designed to measure a student's self-worth; higher scores indicate higher self-esteem. Third, 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 similar to equation (2) but that omit the pre-DTB relative time control due to a lack of a sufficient number of observations. We restrict our analysis to men because of prior evidence of lack of pretreatment trends and because it is among men where we observe negative labor market effects of DTB law exposure. All outcomes are measured in 1997, so we can only include birth cohort and state of residence at age 14 fixed effects (not birth cohort-year fixed effects). We also control for race, family income, and indicators for both mother's and father's educational attainment. Estimates are weighted by the NLSY79 sample weights, and standard errors are clustered at the state level.

We see consistent evidence in Table 8 that exposure to a collective bargaining law negatively impacts noncognitive scores among men. All noncognitive skill measures move in the direction of declining skill: after 10 years, the Rotter Locus of Control increases by 12.9 percent, the Rosenberg Self-Esteem Scale declines by 4.6 percent, and the Pearlin Mastery Scale score is reduced by 1.6 percent. The years of exposure estimates are statistically different from zero at the 5 percent level for the first two measures, while Pearlin Mastery Scale estimates are not significant at even the

³⁹ Because this method requires aggregated data, we do not estimate this model for occupational skill.

TABLE 8—THE EFFECT OF COLLECTIVE BARGAINING LAWS ON MALE NONCOGNITIVE SKILL MEASURES, NLSY79

	Rotter Locus of Control	Rosenberg Self-Esteem Scale	Pearlin Mastery Scale
<i>I</i> (DTB law)	0.147 (0.203)	0.316 (0.489)	−0.148 (0.249)
Years post-DTB law	0.094 (0.034)	−0.135 (0.063)	−0.020 (0.026)
Mean	8.41	22.68	22.29
Percent effect at 10 years	12.9	−4.6	−1.6

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

10 percent level. Together, these results show that students exposed to collective bargaining laws experience reductions in noncognitive skills in adolescence and early adulthood.

The results in Table 8 support the earnings and labor market results presented above. These cognitive and noncognitive measures have been shown in prior research to be highly correlated with long-run outcomes (Heckman, Stixrud, and Urzua 2006), and they provide more direct evidence consistent with the rent-seeking hypothesis. Teacher collective bargaining laws lead to a decline in the productivity of educational inputs, which reduces short-run noncognitive 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: noncognitive skills affect the former more than the latter (Heckman, Stixrud, and Urzua 2006). The sum total of the evidence from the ACS and NLSY79 is remarkably consistent in showing that teacher duty-to-bargain laws negatively impact male long-run outcomes through their effects on the quality of education students receive.

VI. Conclusion

This paper provides the first comprehensive analysis of the effect of state teacher duty-to-bargain laws on student long-run educational attainment and labor market outcomes. We link adults from the 2005–2012 ACS to their state of birth and exploit the timing of passage of duty-to-bargain laws across cohorts within a state and across states over time. Our estimates show that exposure to duty-to-bargain laws when 35–49 year old men were of school-age adversely affects their long-run outcomes. We do not find robust evidence of impacts on women, however.

Our results are consistent with the rent-seeking model of teachers’ unions. Men in cohorts who were exposed to a duty-to-bargain law in the 10 years after passage

earn \$2,134.04 (or 3.93 percent) less per year. A back-of-the-envelope calculation indicates these laws reduce total labor market earnings by \$213.8 billion per year, which suggests our findings have large implications for earnings in the United States due to the prevalence of duty-to-bargain laws. Our results also point to collective bargaining laws reducing hours worked, as well as lowering employment and labor force participation rates. The negative effects of exposure to duty-to-bargain laws are largest among black and Hispanic men, although white and Asian men also are adversely impacted. In particular, yearly earnings decline by 9.43 percent among black and Hispanic males. We find more evidence of a decline in educational attainment for this group of men as well. Among white and Asian men, earnings decline by 2.80 percent. We complement these results with an analysis from the NLSY79 that shows duty-to-bargain laws reduce noncognitive outcomes among young men. In total, our estimates indicate that state duty-to-bargain laws have sizable, negative labor market consequences for men who attended grade school in states with these laws.

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

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