The Effect of Tenure Laws on Students: Evidence from the Implementation of Tenure Systems in the 20th Century

Nikolai Boboshko

July 2021

Abstract: After working for a given number of years, most US K-12 teachers gain tenure, which grants them substantial job security. These tenure protections are widespread, controversial, and their impact is unclear. I study how student's exposure to tenure systems affects their long-run outcomes. Due to limited contemporaneous variation, I go back to the 20th Century and identify all state implementations of tenure systems. I then implement a difference-in-difference design that exploits differential exposure to tenure across cohorts of students. By studying the impact of a whole tenure system, I identify the joint effect of all channels through which tenure operates. An additional benefit is the ability to identify the long-run effects of tenure policies. Event study models reveal that tenure systems negatively impact student outcomes, but only when they are binding. The first tenure laws, passed between 1910-1927, increased teacher retention and drastically lowered long-run wage income. All later laws, implemented between 1928-1977, did not affect teacher retention or the long-run outcomes of the average student. An exception is a small decline in the educational attainment of Black men and a large reduction in the fraction of Black teachers. The large decrease in Black teachers can potentially explain the negative effects on Black men.

1 Introduction

After working for a given number of years, most US K-12 teachers gain tenure, which grants them substantial job security. These tenure protections are a controversial and widespread aspect of the education system. In all but four states, teachers have tenure protections (National Council on Teacher Quality, 2017). However, the recent trend is to weaken teacher job security, either by legislating the elimination of tenure for all or new teachers, arguing that tenure policies are unconstitutional in the courts, or weakening teacher unions' ability to bargain over dismissal policies. Yet, despite substantial policy interest, there is no academic consensus on if tenure policies harm or help students. My paper attempts to answer this question by studying the impact that state implementations of tenure systems have on long-run student outcomes.

The central debate on tenure laws concerns the relative strength of multiple hypothetical effects. Opponents of tenure emphasize that strong job protections weaken work incentives and encourage shirking. Also, restrictions on dismissals as a management tool might negatively affect workplace culture. Stronger barriers to discharge can also attract lower-quality teachers. On the other hand, tenure proponents emphasize the potential positive effects of tenure policies. Tenure is a non-pecuniary job benefit that allows schools to attract qualified candidates at lower wages. Tenure advocates also point out that while tenure sometimes prevents necessary dismissals, historically, teachers were often dismissed for reasons not based on job performance. Instead, releases that were motivated by petty personal and political reasons were common. Removing such arbitrary firings might encourage teachers to invest in occupation-specific human capital and raise job attachment (National Education Association, 1924).

In this paper, I contribute to the tenure debate by presenting the first evidence on the impact of an entire teacher tenure system on long-run student outcomes. Whether a whole state has a tenure system or not, such variation is difficult to find in the present. By the 21st Century, most states have tenure systems already in place. Therefore, I go back to the 20th Century and code all state enactments of teacher tenure systems. These state tenure implementations began in 1910 and end in 1977. These were large policy changes granted substantial protections in an era where legally mandated job security was virtually nonexistent. Thus, the 20th Century enactments of teacher tenure regimes represent a unique opportunity to study the effect of a whole tenure system on students.

In contrast to studying the impact of entire tenure systems, the literature has focused on exploiting incremental differences in existing tenure regimes. For example, several papers take advantage of states' varying the amount of work experience necessary before teacher can attain tenure (Goldhaber et al., 2016; Jones, 2015; Phillips, 2009; Roberts, 2018). These differences make it possible to compare tenured and untenured teachers who have the same experience level. Understanding the impact of such components of tenure is of policy importance. However, these tenure studies have two drawbacks that affect the conclusions we can draw from them regarding tenure laws. First, the estimates are partial effects that do not estimate the joint impact of all the potential mechanisms of tenure policies. To my knowledge, no tenure paper has identified the impact of tenure on students through the labor supply channel. However, such labor supply effects are important; Kraft et al. (2020) find that reform policies that reduce non-pecuniary benefits (e.g., increased time to tenure) lower the number of new teaching graduates and increase the vacancy rate. This drawback does not apply to variation that I exploit. By studying the impact of a whole tenure system, my estimates capture all the mechanisms through which tenure can affect student outcomes.

The second drawback of prior work is that the results are only short-run effects. We know that there can be large differences between short-run and long-run estimates (Ludwig and Miller 2007, Chetty et al. 2011, Deming et al. 2013, Cohodes et al. 2016). In the case of tenure, this difference can be due to treatment intensity. In many prior tenure studies, students are exposed to a policy change for a limited amount of time, in some cases for only a year. This single year of exposure to a tenured teacher might not result in large effects. Also, a single year of tenure protection for teachers is unlikely to result in behavior changes that immediately affect students. For example, teacher quality can be determined more by long-run investments in human capital than by short-run changes in effort. Other than limited treatment intensity, long-run estimates of tenure can also better capture the effect of slow-moving mechanisms. Anything that operates through the labor supply side, where tenure policies change the composition of teachers, will not significantly impact the short-run estimates. By studying tenure systems, I can overcome these shortcomings. Enough time has passed between the implementation of tenure systems to observe long-run outcomes. The staggered implementation of tenure regimes allows for treatment intensity to vary substantially.

My analysis of student outcomes uses Census data from 1940 to 2000. Cohorts observed in these periods were affected by the implementation of teacher tenure systems in 47 states. Formally, I estimate the impact of tenure systems using a cross-cohort difference-in-difference event study mode. The model compares differences in outcomes across students born in states that passed or did not pass tenure laws and between birth cohorts from the same state exposed to the tenure system while of school age for varying amounts of time. A key assumption is that the state of birth is a reasonable proxy for the state and adult respondent attended school. However, I can relax that assumption for select years in my data by observing the childhood state of residence.

Empirically, I find harmful effects when tenure systems matter. The first wave of tenure laws, passed between 1910 and 1927, drastically harmed student outcomes in 1940. Male students exposed to tenure laws for 12 years (the entirety of K-12) experience a wage reduction of 5.85 percent. Effect for women are remarkably similar – wages decline by 5.55 percent. The negative results are also particularly strong for the disadvantaged as wages of treated immigrant men fall by 8.55 percent. These negative outcomes are robust to a range of alternative specifications and tests. For example, the effect of tenure policies is null for immigrants who moved to the U.S. after completing their schooling. The results also hold in linked census panel data that allows me to include detailed controls, such as the father's occupation. In the linked data, I can also better observe where an individual went to school and no longer have to proxy on the birth state. However, later implementation of tenure systems, beginning in 1937 and continuing up to 1977, had a zero effect on the average student. One exception is that African-American men experience a small reduction in educational attainment.

To reconcile the highly heterogeneous results, I look at the first-stage impact of tenure laws on teacher retention. The effect of tenure systems on students should be proportional to the system's effect on teachers. Binding tenure laws should increase retention by reducing forced exits; it is more difficult for principals to fire teachers. Tenure laws can also reduce voluntary departures; teachers perceive the job as more valuable. I find those early tenure laws that substantially harmed students lead to a large increase in teacher retention. Later laws that had zero effect on the average student did not impact teacher retention. Finally, the small negative impact on Black men is not due to teacher turnover but due to changing racial composition of teachers. Tenure laws passed after school integration reduced the number of black teachers—the reason for this decrease is unclear. As students perform better under same-race teachers (Dee, 2004; Clotfelter & Vigdor, 2007; Egalite, Kisida, & Winters, 2015; Gershenson et al., 2018), a large reduction in Black teachers is a potential mechanism for explaining the small negative impact on Black students' from later tenure laws.

2 Teacher Tenure Laws

2.1 Overview of Teacher Tenure Systems

In the early 20th Century, teachers had few job protections. Consequently, principals could dismiss teachers at any time for any reason. Education reformers were concerns that many of the dismissals were not related to job performance. Instead, too many teachers were being removed for petty personal or political reasons. For example, elected members of the school board or superintends would commonly assign teaching positions as rewards for political support (National Education Association, 1924). Such behavior might place unqualified candidates into teaching positions. It also removes effective and experienced teachers because they backed the wrong candidate. Therefore, starting with New Jersey in 1910, states began to strengthen teacher job protections and pass teacher tenure laws.

I define a teacher tenure law as a state statute where a teacher can only be dismissed for just cause and due process, and enjoys these protections for the full calendar year. The definition of just cause is broad.

It included poor performance on teaching tasks, such as instruction and grading, but also immortal and immodest behavior. Due process protections consisted of four factors. First, the reason for dismissal must have been made available to the teacher. Two, the teacher must have the opportunity to appeal. Three, a strict timetable had to be followed. Four, the final say on the dismissal would belong to a party that did not initiate the teacher's release, such as the district superintendent or the local school board.

I excluded laws that grant teachers some employment protections but do not meet the above tenure definition. These excluded legislations come in three types. First, certain states gave teachers strong protection only during the school year but did not extend them to cover the calendar year. I consider the ability to easily dismiss a teacher outside the school year as a major loophole, especially in the early part of the sample, where school years are short. Second, education reformers also championed legislation to reduce teachers' employment uncertainty. Specifically, the renewal of contracts was to occur at the start of the summer break. I exclude such contracts as they only granted very weak protection during the school year and made it easy to dismiss a teacher at the end of the school year (National Education Association, 1954).[1] Again, easy dismissals at the end of the school year are a major loophole.

Finally, I only code laws enacted by state legislatures and do not include laws passed by municipalities or school districts. Identifying all historical tenure variation school district by school district would be highly time-consuming and impractical. An exact accounting of the amount of these formal local laws is difficult to find, although certain point-in-time estimates suggest that their prevalence was small (National Education Association, 1942). Informal tenure regulations, such as unwritten district policies to only dismiss teachers for merit, were more common. However, these informal policies were not official contracts that had to be honored by the courts. Nor were they backed by standardized rules and regulations (National Education Association, 1942). Note that I do include some tenure laws that target portions of the

[1] A 1954 report by the National Education Association, which was the premier teacher organization during the time tenure laws were passed, states that "the typical continuing-contract law offers limited protection to teachers" (20).

state, but only if the state legislature enacted them. It is common for the first state law to give tenure protections to teachers in the largest cities and later law to extend that protection to cover the whole state.

The resulting variation consists of 47 states passing 64 tenure laws between 1910 and 1978, as shown in Figure 1. The legislative activity occurs in two distinct periods. In the early period, states implement tenure systems between 1910 and 1927. Then, possibly due to the Great Depression, legislative activity substantially declines. In the late 1930s states again began to pass tenure laws and continue to do so at a steady pace until 1978.

Figure 2 provides a visual representation of the cross-sectional variation of tenure laws for two critical periods. Panel A highlights all the states that passed tenure laws between 1910 and 1927. These states are the ones where early tenure laws caused large reductions in teacher turnover and harmed long-run student outcomes. In Panel B, I plot the states that passed tenure laws between 1964 and 1978. These are the states where tenure laws did not affect turnover on average but did lead to large reductions in Black teachers and a small reduction in educational attainment for Black students.

2.2 Potential Mechanisms

There are multiple mechanisms by which tenure can affect student outcomes. The main focus concerns two channels: (1) increased job protections can reduce teacher effort, and (2) stronger job security is a compensating differential and allows schools to attract teachers at lower wages. An optimal contract balances these two effects and leads to maximal output (Prendergast, 1999). It is an empirical question whether the current level of teacher job security and wages are optimal. In addition to those two effects emphasized in the public debate, a system of tenure can affect output through at least three more channels: (3) endogenous selection, (4) joint production, (5) incentives of non-tenured teachers.

Endogenous selection refers to the fact that tenure policies can potentially change who becomes a teacher. A better relative compensation package can attract more qualified candidates. On the other hand, if lower quality candidates are attracted by stronger employment protections, teacher quality can decline.[2] There is empirical evidence for both of those effects. Nagler et al. (2020) argue that in a weak private sector labor market, teaching positions have a relatively better compensation package. They find that individual who sort into teaching in such a weak labor market tend to produce better student outcomes. Kraft et al. (2020) study the impact of teacher accountability reforms, such as extending time to tenure or eliminating tenure protections for new teachers, on teacher labor supply. These accountability reforms appeared to improve the quality of new teachers.

In a system of joint production, the productivity of a single teacher can also serve as an input into the production function of other teachers. Therefore, changes in effort due to tenure protections can have spillovers on the productivity of other teachers. Although teachers work alone in a classroom, there are several reasons why productivity spillovers might exist. Teachers do socialize among themselves. They also maintain discipline outside the classroom and share resources such as teaching knowledge and lesson plans. All these behaviors contribute to school culture, which is an important factor in predicting student outcomes (Lunch et al., 2012; Thapa et al., 2013). Students also form school peer networks that are not limited to their classroom. The productivity of another teacher can influence the behavior of the student's peers, and K-12 peers' behavior is an important predictor of own behavior (Bursztyn and Jensen, 2015; Carrell and Hoekstra, 2010; Hanushek et al., 2003; Lavy et al., 2012). Finally, in a given classroom students apply skills that they learned in other grades or classes. For example, writing skills learned in an English class can be applicable to a wide array of subjects. Opper (2019) finds evidence of such productivity spillovers between teachers who teach the same student in different grades.

Finally, the impact of tenure policies on teachers who are tenure-track, but have not obtained tenure, can be affected by tenure policies. This group does not receive significant attention in tenure debates, but

[2] For example, risk-individuals value job-protections more. However, they also have lower cognitive outcomes (Dohmen et al., 2010). There is evidence that teacher's cognitive ability is correlated with teaching ability (Hanushek et al., 2019). If principals are not able to effectively screen new lower quality candidates, teacher quality can fall.

any behavioral response by this group is important. The closest theoretical literature on the topic is on up-or-out rules in professional organizations, such as public accounting firms and law firms. In an up-or-out regime, after a given number of years, the worker is promoted or dismissed. There is no choice to keep the worker in their current position. Certain papers have emphasized that such rules can encourage worker human capital investment (Gilson and Mnookin, 1989; Kahn and Huberman, 1988). Therefore, paradoxically, among early-career teachers, tenure systems can increase effort. Furthermore, if early career investments yield larger gains in human capital, lifetime productivity can increase.

2.3 Prior Research on Teacher Tenure

Prior work on teacher tenure has not been able to identify the impact of a whole tenure system on student outcomes. New implementations or removals of tenure systems have been rare events. Instead, the focus has been on exploiting incremental policy changes to tenure regimes. The resulting policy variation comes in three types.[3] First, states differ in the amount of experience a teacher must accumulate before tenure. This variation allows a researcher to compare teachers with the same level of experience but with varying tenure protections. The resulting estimate is a combination of two mechanisms. There is a short-run incentive effect due to teachers having tenure for an additional amount of time, typically a year. There is also a selection effect due to differential attrition rates between tenured and non-tenured teachers. The literature finds mixed evidence on teacher effort and student outcomes (Goldhaber et al., 2016; Jones, 2015; Phillips, 2009; Roberts, 2018).

The second policy variation exploited is from score cutoffs that teachers must meet to attain tenure or maintain tenure protections. The score is usually a combination of the teacher's Value-Added estimates

[3] In addition to the papers that I will discuss, there are other studies that cover topics similar to teacher tenure. Teacher unions are often associated with bargaining for tenure like employment protections. Several papers have studied the impact of policies that affect teacher's unions and their impact on student outcomes (Anderson et al. 2019; Baron, 2018; Lovenheim and Willén, 2019; Roth, 2019). Kraft et al. (2020) look at teacher accountability reforms, including ending tenure for new teachers, on the labor supply of new teachers. Loeb et al. (2015) and Rothstein (2015) study the impact of time-to-tenure policies. Additionally, Rothstein (2015) explores whether, once tenure is removed, dismissing a large portion of teachers for poor performance is an effective policy.

and the principals' subjective assessments. Comparing teachers who meet the score cutoff to those who just missed it generates a causal estimate. A consistent finding is that teachers that fall below these cutoffs boost student outcomes (Dee and Wyckoff, 2015; Rodriguez, 2018).

The last set of tenure-related policies are ones where a subset of teachers lose employment protections. These policies cover new teachers in Florida who lost tenure protections (Carruthers et al., 2018), new teachers in Tennessee for whom tenure protections were weakened (Rodriguez, 2018), and pre-tenure teacher in Chicago (Jacob, 2013), who lost substantial job protections despite not having tenure. In each of the three cases, student outcomes improved, although in Chicago the effect was not statistically significant and the Tennessee results were not robust. The results from Florida are of particular interest. In Florida, it was not possible to link students to teachers, only aggregate school outcomes were available. So, to identify student outcomes, the variation came from the fraction of affected teachers across schools. As the number of treated teachers in each school is small, an implication is that either the Treatment on the Treated (TOT) is especially large or there are spillovers between classrooms due to joint production.

The above papers identify important effects. For example, many states extended time to tenure or are considering doing so. Understanding whether there is a short-run productivity change from such policies is important in evaluating them. However, the identified estimates do not capture the full effect of the policies they study. First, the estimates do not include labor supply effects due to changes in the compensating differential that would occur as tenure protections are changed. There is empirical evidence that labor supply effects are important, as Kraft et al. (2020) find that reform policies that reduce non-pecuniary benefits, such as increased time to tenure, lower the number of new teaching graduates, and increased the vacancy rate.

Second, the results are only short-run effects where students are exposed to policy changes for a limited amount of time, in many cases for only a year. It is not evident whether an additional year of tenure protections will significantly change a teacher's effectiveness. For example, if teacher productivity varies due to long-run investments in, say, lesson plans, a year of tenure protection is unlikely to create large modifications to pre-determined lessons. Furthermore, we know that the long-run effects of educational interventions can differ from short-run estimates (Ludwig and Miller 2007, Chetty et al. 2011, Deming et al. 2013, Cohodes et al. 2016). In the case of tenure policies, long-run effects can differ due to treatment intensity, as over a long-run time horizon, students receive a stronger dose of treatment. Alternatively, there can be long-run effects through the labor supply channel. Tenure affects selection into teaching, and consequently, over the long run, will affect the composition of teachers.

My contribution to this literature is that I estimate how an entire teacher tenure system affects student outcomes, while previous literature focused on identifying the impact of incremental changes to tenure systems. Although understanding the impact of modifications to tenure system are important, the empirical strategy of those papers only allowed for the identification of short-run impacts and shut down any effects that operate through labor supply. In contrast, by studying the implementation of tenure systems, my empirical design allows for the identification of important long-run effects that are due to tenure policies. Furthermore, the geographical variation that I exploit in tenure systems overlaps local labor markets. Therefore, my estimates also capture the impact on labor supply effects due to changes in compensating differential. The final contribution concerns the scope of the policy variation I exploit, tenure policies were implemented over a period of 68 years. It is well known that modern causal methods calculate a localized treatment effect (LATE). By studying the same policy implemented in different environments, I can identify multiple different LATE estimates and infer more about the mechanisms through which different estimates are generated.

3 Data

I use Decennial Census IPUMS data (Ruggles et al., 2021) for 1940 and 1960-2000. The data starts with 1940 as it is the first Census to contain information on wage income and educational attainment, two key variables. The data ends in 2000; after that year, the variation across birth cohorts in tenure systems becomes sparse. I omit 1950 as the resulting sample is small, and there is limited tenure policy variation to exploit in the decade.[4] Finally, to ensure a consistent sample of states in each decade, the sample only includes the lower 48 states.

The sample is limited to native individuals between the ages 25 and 54. This restriction ensures that individuals have likely completed their schooling and are not near retirement age. It is necessary to limit the sample to those born in the U.S., as I assume that a person's birth state is also the state they attended school in K-12. Although this state of birth imputation does lead to some misspecification error, it has the advantage of avoiding endogenous mobility across states due to tenure laws. Finally, as treatment varies only by birth-state and birth-year, I collapse the data to birth-state, birth-year, and calendar-year level.

Besides the main public data, I use additional information to explore the 1940 results in more detail. As previously discussed, only in 1940 did tenure laws significantly lower wage income for all individuals. So, to gain additional information, I link 1940 to previous 20th-century Censuses. This linking is possible as personally identifying information for the 1940 and earlier census decades is publicly available. The addition of variables such as first and last name allows researchers to track a single individual across several census decades using automated methods. The specifics of the matching procedure are available in Abramitzky et al. (2020), and the links from the procedure have been made publicly available. One concern with such links is that the resulting sample is not fully representative of the population. For example, individuals with more unique names are more likely to be matched. To account for this, I construct inverse propensity weights that ensure covariate balanced between the full count data and the linked sample (Bailey et al., 2020).

Supplementing the main Census results with linked data has three advantages. Additional variables based on parental characteristics, such as father's occupation, can be included as controls. Second, it is no

[4] Most tenure laws passed by 1950 are either too recent or too old. The problem with too rec ent laws is that not enough time has elapsed to accurately measure long run outcomes. The problem with laws that are too old laws is that all the available cohorts within a state are treated. Therefore, there is no within state comparison group for a difference-in-difference design to work.

longer necessary to assume that the individual attended school in their state of birth. I observe the earliest state of residence between the ages of 6 and 18 and assume they went to school in said state. Third, I can identify a more detailed level of geography than the state. As previously discussed, certain states pass tenure laws only in certain counties or cities. With better geographic data, individuals who reside in these treated sub-state areas can be directly identified.

The resulting Census datasets contain important information on educational attainment and labor market outcomes. Descriptive statistics for the Census data are available in Appendix Table 1. Although the Census constraints multiple important variables, in the interest of brevity: I focus on three: wage income (inflated to year 2000), employment, and years of education. These three variables are important outcomes and are highly correlated with other outcomes of interest. For example, the employment rate is highly correlated with the unemployment rate. Note that wage income is set to zero for those who did not work and did not have any wage income. It is set to missing for those who are non-wage workers (e.g. self-employed).

4 Empirical Strategy

This paper aims to causally estimate the impact of teacher tenure systems on student's long-run outcomes. Empirically, I exploit cross-cohort differences in tenure exposure that arise from two sources; whether the state implemented a tenure system and whether the individual was exposed to a tenure regime before exiting the K-12 school system. Formally I estimate:

$$Y_{sct} = \pi_{-12} I(C - t_0 + 18 \leq -11)_{sc} + \sum_{\tau=-11}^{19} \pi_\tau I(C - t_0 + 18 = \tau)_{sc} + \pi_{20} I(C - t_0 + 18 \geq 20)_{sc}$$

$$+ \delta_s + \theta_c + \gamma_{R(s),c} + \epsilon_{sc} \tag{1}$$

where the dependent variable, Y_{sct}, is measured for individuals born in state *s*, belonging to cohort c and observed in census decade *t*. The key independent variable is exposure, which is $C - t_0 + 18$, where C is the year of birth and t_0 is the year the tenure system is enacted. For example, an individual who is 17 at the time when a tenure system is passed will have an exposure equal to one, while someone who is six has an exposure of 12. Individuals who are too old to be in school during a tenure regime serve as a within-state control group.

It is important to emphasize that for the exposure variable, it does not matter whether the individual attended school up to the age 18. Instead, I code the exposure variable as if everyone was in school up to their 18th birthday, even if they have not a dropped out early.[5] Therefore, exposure is imperfectly assigned and understated. Such exposure coding implies that the estimates have an intent-to-treat interpretation (ITT). In the early Census years, high school graduation rates are low, and the ITT and TOT are likely to differ greatly. In later years, high school graduation rates are high and the ITT and the TOT are likely to be very similar.

Figure 3 provides a visual representation of the ITT nature of the estimates. It shows the fraction of individuals that have attained a given year of schooling for two decades, 1940 and 2000. These two very different decades highlight the two extremes of the spectrum. As in 1940, the TOT and the ITT are likely to be very different, but in 2000 they are much more similar. Indeed, that is what the graph shows. In 2000 grade attainment for all years, even high school, is very high. Individuals who are assumed to be treated by tenure in high school were likely to be treated.

In contrast, in 1940, grade attainment is below 2000 levels. For grades one through eight, the difference between 1940 and 2000 is marginal, as most students attend up to middle school. However, high school attendance for the 1940 cohorts is low. Only 48 percent completed grad nine, and a minority, 32 percent, finished grade 12. Such low attendance figures strongly imply, absent a large TOT, that the short-run effect of tenure systems is likely to be null.[6]

---

[5] I do not use actual years of completed education to determine exposure. The primary reason for not doing so is that tenure laws can potentially change the quantity of education an individual receives. Therefore, years of schooling is an endogenous variable. Calculating treatment based on an endogenous variable complicates the interpretation of the results.

[6] This is likely to be the case. Individuals in 1940 who attended high school are likely positively selected. Such selected individuals are typically less affected by educational interventions.

Note that schooling is one of several factors that can cause divergence between actual exposure and assigned exposure. The resulting ITT interpretation of the estimates can also be due to some individuals move out of their birth state before they turn 18 but are coded as treated. It is also possible that the treatment from tenure laws should start when teachers can attain tenure. In most states, tenure laws require teachers to accumulate two to three years of new experience after the passage of a tenure system. Although very few states do grandfather experience and grant eligible teacher immediate tenure protections with a tenure law. teachers are required to accumulate two to three years of experience after a tenure law.

Returning to the estimation equation, I also control for birthplace fixed effects δ_s and birth-year-by-census-region fixed effects $\gamma_{R(s),c}$. The birthplace fixed effects control for all variation that is common to all individuals who are born in the same state. The birth-year-by-census-region fixed effects account for all the common shocks experienced by each birth cohort from a given region. The census-region interaction is included in my main specification primarily to account for differential economic conditions in the U.S. South. Due to a historical slave/cotton economy and the resulting civil war, the cohort economic shocks are different in the South than in the rest of the country. Nevertheless, I do present results without the census-region interaction and show that the qualitative conclusions hold. Lastly, the coefficients π_τ are of main interest as they non-parametrically trace the impact of a tenure system on the dependent variable.

Intuitively, the above specification has a difference-in-difference interpretation. The first difference is between treated and untreated cohorts within a state; remember that untreated cohorts are too old to be in school under a tenure regime. The second difference is between cohorts in a treated state that passed a tenure law and an untreated state with no tenure law. One key difference is that the within-state exposure is not dichotomous as individuals can receive varying levels of the tenure dose.

The assumptions behind my empirical strategy are identical to a difference-in-difference design. First, the outcomes of cohorts in treated and untreated states must be on parallel trends before treatment. Second, there must be no birthplace shocks that differentially affect the cohorts in the state and whose timing coincides with states implementing a tenure system. The parameters π_{-12} to π_{-1} allow for a visual inspection of the parallel trends assumption. To verify that correlated shocks do not drive the estimates, I test the robustness of my results to additional controls.

First, for each state-cohort cell, I include demographic variables; these are fraction Black, fraction Hispanic, and fraction other race. The 1940 data also allows me to include the fraction who have an immigrant mother, have an immigrated father. Additionally, when using the 1940 linked data, a wide set of parent characteristics. These parental controls include detailed indicators for father's occupation, whether a mother works, father's age, and mother's age. To account for potential cohort-specific shocks occurring with tenure laws, as a robustness exercise, I include birth-census-division-by-cohort fixed effects and a wide range of other policies passed during my sample period. These are school-age laws (Stephens and Yang, 2014), court-mandated and legislative school finance reforms (Jackson et al., 2016), food stamps (Hoynes et al., 2016), exposure to the Earned Income Tax Credit (Bastian and Michelmore, 2018), and Duty to Bargain laws (Lovenheim and Willen, 2019).[7] Finally, as the Great Depression is of particular concern in the 1940 data, I construct a Bartik measure of economic conditions that affected each cohort during the worst Depression years.[8]

Besides including the above controls, I also conduct a placebo exercise using linked 1940 census data. I know the exact date of migration to the U.S. for immigrants in the sample. I show that those who immigrated at young ages were affected by tenure laws – because they likely attended school in the U.S. However, those who immigrated later in life – and did not attend U.S. schools, were not affected by tenure

---

[7] My construction of these policy variables is based on publicly available data released by Stephens and Yang (2014) and Lovenheim and Willen (2019).

[8] I construct a Bartik measure to avoid including an endogenous control in my model. The variable is constructed by first calculating the fraction of each state-cohort-gender cell that is employed in each industry at the start of the Great Depression. Then, I combine these employment-by-industry shares with national level industry wage changes between 1929-1933. I chose the years 1929-1933 as that corresponds to the height of the Great Depression. The 1929 occupation data is from the 1930 Census and the industry level wage information is from Lebergott (1964).

laws. Furthermore, I show that these two immigrant groups can serve as a good comparison group for each other as they are both equally affected by state economic conditions.

Equation (1) is estimated separately for each census decade, *t.* I am hesitant to pull 60 years of data into a single model as the impact of a tenure system can vary in each decade. However, after an initial inspection, if tenure laws have similar effects, I do pool them for power and brevity. The pooled specification is estimated using the following model:

$$Y_{sct} = \pi_{-12} I(C - t_0 + 18 \leq -11)_{sct} + \sum_{\tau=-5}^{19} \pi_\tau I(C - t_0 + 18 = \tau)_{sct} + \pi_{20} I(C - t_0 + 18 \geq 20)_{sct}$$

$$+ \delta_{st} + \theta_{ct} + \gamma_{R(s),c,t} + \epsilon_{sct} \qquad (2)$$

The key difference from equation (1) is that the main fixed effects, $\delta_{st}, \theta_{ct}, \gamma_{R(s),c,t}$, can now vary across each Census decade. For example, I now no longer estimate birth-state fixed effects but birth-state-by-decade fixed effects. Such a modification is appropriate to capture large changes that can occur across decades.

Another complication from exploiting more than half a Century of empirical variation is the difficulty of identifying the impact of each tenure law in every decade. The inclusion of laws passed far into the future would not contribute to estimating treatment effects, as none of the cohorts affected by these laws are old enough. Alternatively, laws passed far into the past will not contribute to the estimation of pre-treatment coefficients. Furthermore, such laws, which occurred far into the past or the future, complicate the difference-in-difference interpretation of the estimates. For example, a tenure law can be implemented so far in the past that in a particular decade, only cohorts treated by the law are observed. So, there is no within state comparison group that is unaffected by the law.

To avoid these problems, in each Census decade, I only include tenure laws for which at least one pre-treatment cohort exists. Later, I show that the results are robust to other inclusion rules, such as including laws for which at least five pre-treatment cohorts exist. The list of state laws estimated in each census decade is available in Appendix Table 2.

5 Effects on Long-Run Outcomes in 1940

5.1 Income in 1940

Figure 4 shows the impact of tenure laws on the 1940 wage income of men and women. Note that the income is inflated to 2000 values. Event study estimates are plotted for cohorts expected to graduate 12 years before a tenure law and for cohorts that might graduate 20 years after a tenure law. The event study coefficients are estimated in relation to the year right before the tenure law passage. This omitted year corresponds to a cohort that was 19 when a tenure system was implemented. Consequently, they have exited the K-12 school system by that point and just missed out on tenure treatment.

The estimates effects for years -12 to -2 are null. These estimates correspond to cohorts that were between the ages 20 (for -2) and 30 (for -12) at the time of the tenure law passage. The null estimates imply that cohorts from tenure and non-tenure states are on a parallel trajectory before the implementation of a tenure system. Therefore, any effects on the treated cohorts are not due to diverge trends before treatment actually occurs. The parallel trends contribute to the credibility of the research design.

The short-run effects of tenure laws, for cohorts that have exposure ranging from zero (age 18 when a tenure law is passed) to five (age 13 when a tenure law is passed) are small, negative and not statistically significant. These null effects can occur for multiple reasons. For example, in none of the treated states did teacher immediately acquire tenure. Instead, after passage it took at least two more years of employment for tenure protections to be acquired. Consequently, no teacher benefited from stronger employment protections and any effect operating through such channel would not exist.[9] Furthermore, analysis of past

[9] The estimates can also be null due to low high school attendance. Remember that the event study coefficients, π_0 to π_5, are estimated using variation from individuals who are between the ages of 13 and 18 when a tenure law is passed. As previously discussed, more than half of this group was unlikely to attend high school. Therefore, a large portion of individuals who are coded as having exposure between zero and five likely were not in school and not affected by tenure laws. This would attenuate estimates. Also, the individuals who attended grade nine and above in

interventions for children, ranging from Medicaid to school spending often found no initial effects (Goodman-Bacon, 2016; Jackson et al. 2016; Lafortune et al. 2018), but significant long-run results.

It is clear that even though the short-run estimates are not significant, the long-run effects of tenure systems on adulthood income are large and negative. For men, estimates are negative three years after exposure and become significant six years after a tenure system is passed. Negative effects for women take longer to materialize. Income begins to fall after nine years and the impact is only significant after twelve. For both genders there is a clear pattern; once income begins to fall, the decline scales with amount of exposure to tenure systems. Effects do appear to stabilize after 15 years. However, by that point the sample composition of treated states also begins to change significantly. It is not clear if the resulting stabilization is causal or due to changing composition of treated states.

Table 1 presents the income results for both men (Panel A) and women (Panel B). There is a consistent pattern of a large negative effect across all models. My preferred specification includes birth-region-by-cohort fixed effects (Column 2). After 12 years of exposure, male earnings decline by -$848.82 (5.85%) percent and female earnings by -$424 (5.55%). The result is robust to the inclusion of demographic and policy controls (Column 3) and division-by-cohort fixed effects (Column 4).[10]

Removing the region-by-cohort fixed effects causes all estimates to increase in size by about 50 percent. The increased size of the estimates is due to the South. Without the South (Column 5), region-by-cohort effects are unimportant (Column 6). Furthermore, the no-South results are qualitatively similar to specifications that include both the South and birth-region-by-cohort fixed effects.[11]

---

the early 20th century and should be affected by tenure laws were likely advantaged. Childhood intervention typically have smaller impact on advantaged children. For example, see Dahl and Lochner, 2012.

[10] There are four census regions and nine census divisions.

[11] Recent work has highlighted potential problems in event study designs that exploit staggered treatment (e.g., Abraham and Sun, 2018). I follow Cengiz et al. (2019) and run stacked event-by-event models to account for any potential problems. The estimates are very similar to ones from standard event studies and are currently available upon request.

Intuitively, the stacked event-by-event estimates are event-study regressions that separately estimate the impact of all tenure laws passed in the same year. As the variation only comes from tenure laws passed simultaneously, there is no staggered treatment. Then, these separate event-by-event regressions are stacked on top

5.2 Educational Attainment in 1940

Figure 5 shows event study estimates of tenure laws on years of education. There is no pre-trend, and post-treatment estimates are all zero. These effects are further explored in Table 1 Panels C and D. The null is highly robust; it holds in all the specifications and for both men and women. These zero effects in 1940 are somewhat surprising; in the previous section tenure laws were found to drastically reduce wage income.. However, that can be reconciled; years of education is a coarse measure of human capital attainment. Tenure laws can affect human capital by changing the quality of schooling, note so much the quantity. Furthermore, other work that studied the impact of schooling interventions often found that changes in years of schooling do not fully account for the observed income effects (Jackson et al., 2016; Lovenheim and Willen, 2019). All the prior work also used more contemporaneous data where the returns to schooling are much higher than in 1940. Causal estimates of the modern returns to schooling vary between 9% and 27% (Jackson et al., 2016). Meanwhile, in 1940 the return to a school year is only 4 percent (Feigenbaum and Tan, 2020). Small returns will further attenuate any relationship between income and years of educational attainment.

5.3 Employment in 1940

The effect of tenure laws on employment in 1940 is available in Figure 6. All the estimated event study coefficients for men are zero. These null effects also occur in 1940, a decade when tenure laws drastically

---

of each other into one model and are estimated jointly. This procedure ensures that I align all the events in event time, as opposed to calendar time.

The first step in the procedure is to identify all tenure laws passed in the same year, t. For these tenure laws, I then construct a clean control group. These clean controls are states for which there is no tenure variation to exploit in a given decade. For example, in the 1940 Census, I exploit the 1921 tenure laws passed in Colorado and Wisconsin. Next, I drop all other states for whom I code tenure variation in the 1940 Census to construct a clean control group. This creates a single dataset for all tenure laws passed in year t and exploited in a given decade. Then, I stack each of the resulting datasets on top of each other and estimate the following event study model:

$$Y_{sch} = \pi_{-12} I(C - t_0 + 18 \leq -11)_{sc} + \sum_{\tau=-5}^{19} \pi_\tau I(C - t_0 + 18 = \tau)_{sc} + \pi_{20} I(C - t_0 + 18 \geq 20)_{sc} + \delta_{sh} + \theta_{ch} + \gamma_{R(s),c,h} + \epsilon_{sch}$$

The outcome variable Y_{sch} is measured for individuals born in state s belonging to cohort c and observed in dataset h. Remember that each dataset consists of treated states that passed a tenure law in the same year and the clean controls. Overall, the model is similar to equation (1), except that the fixed effects can vary across datasets (h). It is also close to equation (2) but with two exceptions. The regression is estimated separately for each decade. Also, the stacking is by datasets instead of decades. Standard errors are clustered at the state of birth.

reduced wage income. A potential explanation for the 1940 null effect is that the early 20^{th} Century had few welfare programs, lower returns to non-work, and a strong social stigma against idleness. All these factors contribute to a very low reservation wage and a weak relationship between employment and human capital. The lack of an employment effect is also encouraging for identification purposes. A major confounder in 1940 is the tail end of the Great Depression. As the depression is best known for drastically lowering employment, a zero employment effect strongly suggests that the treated cohorts are not differentially affected by the depression.

In contrast to men, the 1940 employment effect for women is positive. For women, treatment increases employment, and the effect increases with treatment size. It is also clear that the positive impact is primarily not due to divergent trends, although they are not perfectly parallel. A positive employment effect can simultaneously exist with a negative wage earnings effect under certain conditions. For example, the cross-elasticity of women's employment with respect to the income of their husband would have to be negative, and the husband's income would have to fall. It is not unreasonable for those two conditions to hold. Alternatively, tenure systems can reduce marriage rates by hurting the labor market prospects of potential partners and unmarried women are more likely to work.

The point estimates are available in Table 1 Panels E and F. The male result is null in all specifications. For women, at 12 years of exposure, the fraction employed increases by 0.27 percentage points. The 0.27 pp increase is small in absolute terms. However, due to low female employment rates in 1940, it translates to an 11 percent employment increase. The estimate is nearly identical in all specifications, except that it is slightly larger without cohort-by-region fixed effects.

5.4 Robustness Using Linked Data

One may worry that the negative outcomes are due to early childhood shocks unrelated to tenure laws. Alternatively, the measurement error due to using the birth state as a proxy might be non-classical and cause negative results. To help address these concerns, I link the individuals in the 1940 census to their childhood selves in earlier decades. The new information that these links provide allows me to address the two concerns. First, I now observe detailed information on parental characteristics that allow me to account for differences in childhood circumstances between treated and untreated cohorts.[12] Second, I also observe the state and the county of residence during school ages. So, I no longer have to rely on the birth state to determine treatment. The observed location is potentially more accurate and is more geographically detailed. As previously discussed, certain states passed tenure laws targeting only select areas in a state. With more detailed data, I no longer code the whole state as treated; I can now observe such local treatment directly.

For estimation, I use a version of equation (1). Except that I no longer collapse the data to state-cohort cell, but estimate the equation the individual level. The fixed effects that dependent on birthplace are now re-calculated using either the state of residence during school ages or the county of residence during school ages. Standard errors are clustered at the earliest state of residence. The rest of the specification is identical to equation (1).

The results using individual-level data are shown in Table 2. In Panel A, the specification exploits the more detailed county-level residence information. After 12 years, male wages decline by 10.58 percent (Column 1). Including region-by-year fixed effects attenuates the estimate, but only slightly, to – 8.46 percent (Column 2). For comparison, in Table 1, using public Census data and relying on birth-state, the baseline effect is a 10.71 percent wage decline (Table 1, Column 1) that attenuates to – 5.85 percent with region-by-year controls. That the attenuation in linked data is smaller implies that region-by-year fixed effects mattered in part due to misspecification of treatment. All additional controls, including the new early childhood circumstance variables, leave the estimates from linked data essentially unchanged. So, the negative income effects are not due to other childhood shocks correlated with tenure laws.

[12] The controls for early life circumstances are detailed indicator variables for father's occupation, childhood urban residence, presence of a father, presence of a mother, father's age, mother's age, whether mother is employed and the number of siblings.

As we move down the table, the geographic variation that I exploit to assign treatment becomes less detailed. In Panel B, the state of residence during childhood is used to calculate exposure, while in Panel C, it is the state of birth. There is a clear pattern; as we move down from panel to panel, the estimates attenuate. In my preferred region-by-year fixed effects specification (Column 2), the estimate declines from -10.58 percent (Panel A) to -9.36 percent (Panel B), to -8.78 percent (Panel C). Therefore, using individual level data, it is apparent that less detailed geographic information attenuates the effects. Furthermore, in every panel, the addition of early childhood circumstance controls leaves the results unchanged.

5.5 Placebo Test

If the estimates are causal and not due to state-cohort-specific shocks, only individuals who went to school in a tenure regime should be affected. A natural falsification test is that the impact must be non-negative for those who moved to the state after completing their education. To conduct this test, I use two groups of immigrants. The placebo group migrated to the U.S. when they were 16 and older (late movers), and consequently, are unlikely to be taught in a tenure regime. This group of immigrants is very different from the natives used to estimate baseline regressions. Therefore, I bring a second group, those who immigrated to the U.S. below age seven (early movers). I show that the early movers (treated group) and the late movers (placebo group) are affected by similar economic shocks, but only the early movers are affected by tenure laws.

Using immigrants for the placebo test allows me to address three potential problems of such research designs. First, a good placebo test must be able to identify treated and non-treated individuals accurately. In my data, detailed mobility information is only available for immigrants. So, only for non-natives can I be fully confident in my assignment of individuals to the placebo group. Second, the placebo group must be also be affected by the same underlying confounders that affect the treated group. These underlying confounding shocks are the potential sources of bias. We want to use the placebo group to demonstrate that correlation between treatment and such shocks is not correlated with treatment. However, there is an important concern; the placebo group is often different from the treated group on many dimensions. Therefore, they might not be affected by the same underlying shocks as is the treated group. For example, I show that late-mover immigrants (the placebo group) and immigrants who moved to the U.S. when young (the treated group) are both affected by an important confounder. Specifically, the Great Depression appears to have equally affected the labor market outcomes of both late and early-mover immigrants.[13] Finally, a placebo test might fail if there are spillovers from the treatment to the untreated groups. In my context, the majority of treated individuals are natives. As natives differ from immigrants on several characteristics, there could be minimal spillover between the two groups.

To implement the placebo test, I need to know the age an individual migrated to the U.S. This information is not available in the 1940 census but does exist in the 1930 Census. Therefore, I link the two censuses together. The linked data contains the year of birth and migration; two variables necessary to identify placebo immigrants (late movers) and treated immigrants (early movers). To re-iterate, those who immigrated at age 16 and older (late movers) are the placebo group, and those who immigrated at age six and younger (early movers) are the comparison group. Next, immigrants must be assigned a state that will determine treatment. This state is analogous to the birth-state used for the natives. For these immigrants, I use the state of residence in 1930. For the early movers, the 1930 state of residence is a reasonable proxy for the state they were likely educated in. For the placebo sample of late movers, there is naturally no treated state. Instead, by coding "treatment" based on 1930 I test whether state-cohort economic shocks between 1930 and 1940 can explain the negative impact of tenure laws. Such shocks are of particular concern as the period overlaps with the Great Depression.

The results for the placebo test are presented in Figure 7. We see that those who immigrated to the U.S. after 16 are not affected by state tenure laws. The event study coefficients are zeroes until 14 years after tenure passage. After at 14 years of placebo treatment, the outcomes for this group appear to improve.

---

[13] In results available upon request, I construct a state-cohort measure of Great Depression exposure between 1929 and 1933 and show that the negative impact from such exposure is nearly identical for the two groups. This needs more polish and then I will add it to the paper in an Appendix.

However, this is unlikely to be causal. Due to restrictive immigration laws, only very few individuals contribute to estimating those event study coefficients.

In contrast to the placebo results, there is a strong negative effect of tenure laws on those who immigrated at age six or younger. The negative effect is larger than the native estimates, possibly due to disadvantaged groups being more sensitive to school inputs. Point estimates and robustness tests are available in Table 3 and confirm the results from the event study figures.

6 Effects on Long-Run Outcomes in 1960-2000

6.1 The Effect on the Average Student

Figures 8 to 10 present the event studies estimates on the impact of tenure laws between 1960-2000 for income (Figure 8), years of education (Figure 9), and employment (Figure 10). In these figures, I pool all the decades from 1960-2000 and estimate Equation (2). Such pooling is appropriate as the effect is identical for each period. The separate estimates for each decade are available upon request. The pooling has the added benefit of increasing power and providing brevity. For each of the three variables, the event study estimates are null and not statistically significant. Tenure laws have no impact on income, educational attainment and employment for nearly the entire second half of the 20^{th} Century.

The point estimates for 1960-2000 are available in Table 4 for income (Panels A and B), education (Panels C and D), and employment (Panels E and F). Consistent with the graphical evidence presented above, the estimated effects of exposure at 12 years are null in almost every specification. The effect of tenure laws on men after 12 years ranges between -\$230 to \$543 for wage income, 0.04 to 0.07 for years of education, and a consistent 0.02pp effect for employment. Estimates for women are similar. The zero effect is robust to adding demographic and policy controls as well as birth-division-by-cohort fixed effects. Furthermore, unlike the 1940 result removing birth-region-by-cohort fixed effects makes no difference.

These null effects are consistent with the available qualitative evidence. From the 1970s a small survey of teacher's opinions regarding tenure laws is available. Experience teachers generally felt that tenure had no impact on the profession. In that, there were no negative effects from tenure laws, but also no positive ones (Cobb, 1981).

6.2 The Impact on Black Men

In 2012 a group of students sued California, arguing that the state's teacher tenure law was unconstitutional because it negatively impacted minority students. The lawsuit was ultimately unsuccessful due to limited evidence. In this section, I present the first evidence on the impact of a whole tenure system on Black students.

Initial analysis for African Americans is complicated due to divergent trends between treated and untreated groups. These divergent trends only occur in 1990 and 2000 when studying the impact of tenure systems on the educational attainment of Black men. To address the problem, I follow Goodman-Bacon (2016, 2021). Intuitively the adjustment procedure rotates the event study estimates so that the parallel trends look flat. Practically, this is a two-step procedure. The first step is to estimate the following equation:

$$Y_{sc} = \delta_s + \theta_c + \gamma_{R(s),c} + \beta(C - t_0 + 18) + \epsilon_{sc},\ \forall\ (C - t_0 + 18) \leq 0 \tag{3}$$

Where $(C - t_0 + 18)$ is a linear exposure variable that should account for divergent trends. Including the linear exposure variable in the full event study specification, equation (1), leads to endogeneity. The exposure variable is endogenous because it captures not only pre-treatment trends but also post-treatment effects due to tenure laws. Therefore, only cohorts with an exposure of zero or less are used to estimate equation (3). This restriction ensures that the linear cohort trend is identified only from the pre-treatment trend. The last step is to calculate the residual from equation (3) for all exposure levels. The residual is the new dependent variable, and the pre-treatment trends are mechanically purged from it.

Generally, the results for African-Americans are null, with one exception. The educational attainment of black men declines in 1990 and 2000. The by-decade event study estimates for each of the three dependent variables are available upon request.[14] Below I focus on the significant education results.

The event study estimates for the educational attainment of black men in 1990 and 2000 are available in Figure 11. The pre-treatment trends are parallel. However, as previously noted, the parallel trend result is mechanical. The data has been adjusted such that the pre-treatment coefficients should be close to zero.

In the short-run tenure laws do not have an effect on the educational attainment of Black men. At four years of treatment there is a trend break; the estimate become consistently negative and increase in size with exposure. After nine years tenure laws have a negative and statistically significant effect on the years of schooling completed by Black men.[15] Therefore, even though the short-run effect is not-significant, in the long-run tenure laws decrease educational attainment. The time lag points to the importance of studying the long-run effects of educational interventions.

The above event study estimates show that tenure laws can have a negative impact on educational attainment. To better understand the size of the negative effect, point estimates for Black men are available in Table 5. After 12 years of exposure to tenure laws, educational attainment for Black men declines by 0.45 years in 1990 and by 0.29 years in 2000 (see Column 1). These are high declines. For example, suppose we want to raise school spending to compensating for falling schooling and increase educational attainment.

[14] I should put them into the Appendix.

[15] The lack of a short run effect, but significant long run results could occur due to several factors. There is potentially a treatment intensity effect that scale with the time of exposure to tenure laws. A few years of exposure of a tenure system might just be too weak of a treatment. Alternatively, there could be heterogenous effects related to student's age, where tenure systems have a stronger impact on younger students. Finally, the long-run effects could be due to changes in the composition of teachers due to tenure laws, and such mechanisms that operate through labor supply are slow moving. No matter what the mechanisms is, the results point to the importance of studying the long-run effects of educational interventions.

We would then need to increase per-pupil spending by about 15 percent for the 1990 cohorts and by 9 percent for the 2000 cohorts.[16]

The 1990 effect is robust to the inclusion of birth-region-by-cohort fixed effects (Column 2) and adding policy controls (Column 3). However, that slightly attenuates with the inclusion of birth-region-by-cohort fixed effects (Column 2) and additionally adding policy controls (Column 3). However, birth-division-by-cohort fixed effects do noticeably attenuate the results. For example, going from a parsimonious event study model (Column 1) to a specification with policy variables and birth-division-by-cohort fixed effects (Column 4), causes 12-year estimate to decline from -0.45 to -0.27. Such a decline could be due to correlated changed occurring at the census division level. Alternatively, useful variation can be lost by estimating only within census division.

7 Mechanisms

To explain why early laws negatively affect student's long-run outcomes, but later laws generally do not, I look at the impact of tenure laws on teachers. Specifically, I focus on the first stage impact of tenure systems on teacher retention, as this captures the potential bite/impact/strength of a tenure law. Retention is a natural measure of tenure system impact. Tenure should increase retention mechanically by affecting forced exits; stronger job protections make it difficult to remove teachers. Retention can also increase through less voluntary exits; the non-pecuniary benefits of the job have increased, and teachers are less likely to exit the occupation.

7.1 Retention Estimates for Male Teachers in 1900-1940

I begin the analysis by studying the impact of early tenure laws, passed between 1910-1927, on teacher retention. These early implementations of tenure systems potentially had a large impact on teacher retention for several reasons. First, other than tenure laws, no other formal job protection measures existed. The very

[16] Estimates are based on Jackson et al. (2016).

first piece of employment protection can have a stronger marginal effect than additional protection legislation. Second, the impact of tenure systems should be proportional to the amount of involuntary turnover in a period. For example, if there is no turnover, then passing tenure legislation should make no difference. However, if turnover is high, then there is a large potential for tenure laws to matter. We know that turnover was particularly large before 1930 (Carter and Savoca, 1992). Therefore, due to high turnover and lack of other protections, the early tenure laws should significantly impact teacher retention.

The retention data is constructed using linked Census files from 1900-1940. Intuitively, due to the availability of personally identifying information, it is possible to track teachers across decades. So, I can observe if they move counties or switch occupations. This knowledge of teachers location and occupation across different decades is what allows me to construct retention measures.

The use of linked data also generates a complication where it is difficult to jointly study the retention of male and female teachers. I rely on personally identifying information, such as first and last name, to link individuals across the Census decades. For women, this task is difficult, as they tend to change their last name upon marriage. Consequently, the linking procedure that I implement differs by gender. The different linking procedures also generate different measures of retention. Therefore, although the outcomes capture qualitatively similar information, they do vary quantitatively. As the outcome variables differ by gender, it is necessary to estimate effects separately for men and women. Therefore, I begin by analyzing male teachers and will present results for female teachers in the next section.

I begin the retention analysis by looking at the retention of male teachers but will present results for female teachers in the next section. For male teachers, I use 1900-1940 data to construct a retention measure and link teachers in each Census decade to the two available adjacent decades.[17] For men, such

[17] I cannot do both forward and backwards links in 1900 and 1940. The 1890 census microdata does not exist and the 1950 census is restricted by privacy laws. Furthermore, because I cannot construct a backward link with the 1900 census, I do not have retention information for that decade. However, I use the 1900 census to construct retention variables for the 1910 census.

linking procedures are relatively standard and commonly used in historical work. The exact links and a description of the linking procedure are from Helgertz et al. (2020).

With the linked census data, I construct two measures that relate to retention. The first, decennial retention, calculates teacher retention across census decades within counties. It is an indicator equal to one if a male teacher in county j in year *t*, is still a teacher in the same county j in year *t+10*. The second, which I term occupational attachment, quantifies a teacher's attachment to the occupation. It is an indicator variable equal to one if a current teacher was also a teacher in the previous decade. Neither decennial retention nor occupation attachment are perfect measures of retention. I am not aware of such information, even in modern datasets. However, the two variables I construct are highly instructive of actual retention and are of policy relevance. For example, one of the early tenure laws' goals was to increase teachers' attachment to their occupation (National Education Association, 1924).

I identify the impact of tenure on decennial retention and occupational attachment through a difference-in-difference design by estimating an event study model with the following equation:

$$Y_{cti} = \pi_{-2} I(t - t_0 \leq -21)_{st} + \sum_{\gamma=-1}^{2} \pi_\gamma I(t - t_0 \in [-10 + 10 * \gamma, -1 + 10 * \gamma])_{ct}$$

$$+ \pi_3 I(t - t_0 \geq 20)_{ct} + \delta_c + \theta_t + \epsilon_{pt} \qquad (4)$$

The outcome, Y_{cti}, is measured for individual *i*, residing in county c and observed in decade *t*, δ_c are county fixed effects and θ_t are decade fixed effects. The π_γ are event study dummies that measure the impact of tenure system on the outcome a given number of years before or after the implementation of a tenure system. Note that the admittedly elaborate $\pi_\gamma I(t - t_0 \in [-10 + 10 * \gamma, -1 + 10 * \gamma])_{ct}$ term has a very simple intuitive explanation. It simply means that I pool each ten event study variables together. For example, π_0 identifies the average impact of the tenure system from zero to nine years after its implementation.[18]

[18] This pooling of ten event study variables together is due to the decennial nature of the data. Consider an example where a state passes a tenure law in 1917. I only observe post treatment outcomes for the state in 1920, 1930 and

When estimating equation (4) for decennial retention, I code both treatment and county fixed effects (δ_c) using the prior decade county of residence. Additionally, the sample is limited to individuals who were teachers in the previous decade. These modifications ensure the following interpretation of the estimates: If a state or county passes a tenure law, what is the change in the probability that they can retain a given teacher over the next decade?

Table 6, Panel A displays the impact of the tenure system on decennial retention.[19] The dependent variable is equal to one if the individual is still a teacher in the next decade and the same county. We see that the parallel trends assumption holds; before treatment, tenure laws have no statistically significant effect. In the first decade of the tenure law, the retention of male teachers increases by 0.26, and in the second decade, the retention increase further to 0.51 (Column 1). Adding region-by-year fixed effects attenuates the result somewhat. Including division-by-year fixed effects further attenuates the results, and they are no longer statistically significant, although still positive.

The impact of tenure laws on occupational attachment is estimated using equation (4). Treatment status and city fixed effects (δ_c) are coded using the current place of residence. The sample consists of individuals who are presently a teacher. You can interpret the regression results as measuring the impact of teacher tenure on teacher's occupational attachment or experience. It is similar to the decennial retention measure. For example, occupation attachment should also improve if tenure systems cause local teachers to stay and teach in the same county for the next decade. However, the occupational attachment measure is

---

1940. So, results can be identified for 3, 13 and 23 years after a tenure law. It is technically possible to estimate a non-pooled event study. However, doing so has three drawbacks. One, each coefficient will be estimates using variation from very few states - leasing to noisy estimates. Two, trends within each decade will be in part due to changing sample of treated states. Three, certain coefficients will not be identifiable because a tenure law has not been implemented in every year ending from zero to nine.

[19] I do not show event studies figures for decennial data for simplicity. Due to the decennial nature of the data these figures would only plot five coefficients. One of the coefficients is constrained to be zero. Another measures the impact of tenure laws 11 or more years before their implementations. It is estimated from very few observations. Furthermore, such end caps are difficult to interpret.

also different. It also includes any impact of tenure laws attracting experienced teachers from outside the county.

The impact of tenure laws on occupational attachment is available in Panel B. The sample is limited to current teachers. The dependent variable is equal to one if a current teacher was also a teacher in the previous census decade. The parallel trends assumption holds, and it is evident that tenure systems increase occupational attachment. In the first decade of a tenure system, in the parsimonious model (Column 1), occupational attachment increases by 0.32 pp. In the second decade, attachment further increases by 0.45 percent. The estimates attenuate with additional controls, but they also cause divergent trends, complicating their interpretation. Overall, the early tenure laws achieved the two intended goals for male teachers; retention increased, and teachers became more attached to their positions.

7.2 Retention Estimates for Female Teachers in 1900-1940

To ascertain the impact of tenure systems on female teachers, I use two measures. The first is the marriage rate of female teachers. In the first half of the 20th Century, it was common to dismiss female workers if they married. Tenure laws could curb this behavior by allowing dismissal only for cause. Although the wording of the early laws was ambiguous on whether marriage is a valid reason for dismissal (National Education Association, 1942a), court rulings clarified that it was not (Garber, 1934).

The second measure is currently a work in progress, and results are not presently available. Below I will outline how it will be constructed. This new measure, which I term finding rate, will use automated census linkage procedures to construct a measure of female teacher turnover at the county level. It measures the fraction of female teachers in county *j* in year *t* who can be found as a teacher in the same county in year *t + 10*. Usually, applying such automated linkage procedures to women is difficult as they change last name at marriage. Without the last name, there is not enough identifying information to guarantee a unique match. However, I am only interested in using automated linking methods to construct a measure of teacher retention. This limited goal, constructing a teacher retention measure, has three important benefits for linking purposes. One, I am interested in finding potential matches within counties. Therefore, one can use the county of residence to reduce the number of potential matches greatly. Two, in a typical automated linking procedure, the match must be distinct. That is not the case in this context; I only need to know if a potential match exists.[20] Three, most female teachers are unmarried and last name can be used to determine a potential match for most of them.

The first step to contrast the finding rate for year *t* is to limit the sample to teachers in year *t* and *t + 10*. For a teacher in year *t + 10* to be a potential match for one in year *t*, the following characteristics have to be identical between the two: the place of birth, race, the first letter of the first name, and the county of residence. Additionally, the difference in the reported birth year cannot be more than three years. Finally, the reported first names must be similar enough. The first name similarity is measured using Jaro-Winkler string distance and must be above 0.8.

I make use of marital status and last name for matching in certain cases. For married teachers in year *t*, I assume that they are likely to be married in year *t + 10*. This is a reasonable assumption due to low divorce rates. So, the potential matches must also be married or widowed. Furthermore, as name change is no longer an issue, I use the women's last name to determine matches.

For an unmarried teacher in year t, I do not generally use marital status to determine matches. Instead, I only use marital status to determine if the last name provides useful information. The last name provides useful information in cases where the potential match in *year t + 10* is not married, and then the last name is used to determine matches. However, if the possible match in year *t + 10* is married, I do not use the last name.

[20] Consider the following example. An observation A_{1900} in the 1900 Census can have two potential matches in the 1910 Census, C_{1910} and D_{1910}. Both individual C_{1910} and D_{1910} are very similar to A_{1900} and we cannot distinguish which is the correct match. Most automated linking methods would then drop A_{1900} and consider her unmatched. In contrast, I would consider A_{1900} as having a match.

The last step is to aggregate all the potential matches for teachers in year *t* in county *j* to the county level. A naïve approach would be to calculate the fraction of such teachers who have a potential match. However, this will likely overstate the finding rate. For example, consider the case where multiple teachers in year *t* have only one $t + 10$ match, and that matched person is the same for each teacher. The naïve approach would code all the year *t* teachers as having been matched, but a match exists for only one of them. Thus, the aggregation method should ensure that each matched pair is unique when determining the number of links at the county level. So, I aggregate the data up to the county level by finding the maximum number of unique matches possible within each county.[21]

The resulting finding rate variable is analogous to the county turnover measure calculated for men. The finding rate should approximate county retention. A high finding rate implies high retention, and a low match rate indicates low retention. However, there is a key difference between the decennial retention measure used for men and the finding rate measure used for women. The decennial retention variable for male teachers exists only for those individuals who are linked across census decades. In contrast, the finding rate for women captures both true retention and the probability that an individual can be linked.

The finding rate has an advantage in that it is a retention measure for the whole population, not only the linked sample. However, a disadvantage is that it can conflate changes in matching probability with changes in retention. For example, if tenure laws cause an increase in the number of individuals with unique names, the finding rate will increase, even though retention might not.

Table 6 Panel C presents evidence on the impact of tenure laws on the probability that a teacher is both married and female. Short-run estimates in the first decade of tenure law are all null. However, results are significant in the next decade. In the specification with region-by-year and division-by-year fixed effects, tenure systems increase the fraction of married female teachers by 0.033 and 0.026, respectively.

[21] For example, an observation A_{1900} in the 1900 Census can have two potential matches in the 1910 Census, C_{1910} and D_{1910}. Both individuals C_{1910} and D_{1910} are very similar to A_{1900} and we cannot distinguish which is the correct match. Another 1900 Census observation, B_{1900}, has only one potential 1910 match, C_{1910}. The maximum number of unique matches is two if A_{1900} is paired with D_{1910} and B_{1900} is paired with C_{1910}.

These are large effects given that only 10 percent of teachers are married women. The only specification in which the long-run effect is not significant is the baseline (Column 1), although it is still large and positive. Therefore, tenure laws increase the number of married women teachers, but the response is delayed.

A possible reason for the delayed reaction is from two factors; (1) the shorter career length of female teachers and (2) the decennial nature of the data. For example, suppose a tenure law was passed in 1917. I observe female teachers in 1910. Due to short career lengths, many of the 1910 female teachers would have exited the profession by 1917. Therefore, they would not have been affected by the tenure system. In contrast to women, men work for longer periods. Therefore, more of the 1910 male teachers would still be employed by 1917. Consequently, men would be more affected by tenure in the short run. Then, when I calculate retention between 1920 and 1930, all the female teachers I observe in 1920 are affected by the 1917 tenure law. That is a potential reason why long-run effects exist for both genders, but there are short-run effects only for men.[22]

7.3 Retention Estimates for Teachers in 1962-1990

To examine the bite of later tenure laws, I use the March Current Population Survey for 1962-1990. Note that the available March CPS years do not allow me to study the impact of tenure laws passed between 1937-1959, only those passed after.[23] Although not ideal, the tenure laws passed between 1964-1977 do overlap with two important results; that later laws have a null effect on the average student and a small negative impact on the educational attainment of Black men.

---

[22] An alternative explanation has to do with the impact of tenure on selection. The observed long-run increase in retention could be due to tenure laws attracting more committed teachers to the occupation For example, a 1917 law would not affect the decisions of women to become teachers in 1910 and likely have a limited impact in 1920. However, by 1930 the composition of teachers can have significantly changed. These teachers, attracted by the benefits of tenure, are more committed to the occupation. Consequently, they are less likely to exit teaching. Note that this selection mechanism operates by decreasing the number of voluntary exits from more committed teachers. Retention can also decrease through decreased forced exits. Unfortunately, my data does not allow me to distinguish the type of exit. This laternative explanation would also require for the selection channel to operately differently for men and for women.

[23] There are no new state tenure system implemented between 1960 and 1963.

Although commonly thought of as a cross-sectional dataset, the CPS is a two-year panel. This limited panel structure allows for the construction of a retention measure. I can observe if, in a state, a person who reported being a teacher in the previous year is a teacher in the present year. A state-level measure of retention is not perfect, as retention can change at the school level but not at the state level. For example, consider a dance-of-the-lemons style scenario where poorly performing teachers move to new schools (Staiger and Rockoff, 2010). In such a scenario, low-performing dismissed teachers can consistently find employment in different schools. A school-level retention measure will change under such a scenario, but a state-level measure will not. As the low-performing teachers are still employed, the state-level measure captures an important effect that the school-level variable will not. So, even though a state-level measure is not ideal, it is of interest.

A complication with the CPS data is that between 1968-1976 the Bureau of Labor Statistics combines certain states into state groups. The state groups can be as small as a single state or large enough to include seven small states. Therefore, in my baseline specification, I combine all the states into 21 state groups that are consistent over time. For robustness, I will also estimate an unbalanced panel regression where the cross-sectional observation unit can change over time.[24]

Formally, I estimate a standard event study model and exploit variation across years and state groups. For power, I pool event times together by estimating the joint impact of five-year periods. To account for the pooling of states into state groups, I scale each event study variable by the proportion of the state group treated by a tenure law. Also, there can be multiple states that pass tenure laws in different years in a state group. To account for this, in a treated state group, each event study variable can be turned on (equal to one) multiple times (Krolikowski, 2018; Sandler and Sandler, 2014).

[24] For example, in the unbalanced panel, I can observe a state between 1962-1967, then I observe it only as a part of state group between 1968-1976, and finally again as a state from 1977 onward. The estimates from such an unbalanced panel are difficult to interpret, but it does make use of all the available geographic information.

The results for teacher retention are presented in Figure 12. The parallel trends assumption holds. These later tenure laws have a zero impact on teacher turnover, although the confidence intervals are wide. Point estimates are available in Table 7 and are close to zero in almost all specifications. Estimates are qualitatively similar between balanced and unbalanced panel models, although the unbalanced panel results tend to be negative. The results are robust to including census-region-by-year fixed effects, additional policy controls. Additionally, the findings hold in a triple-difference model where other public sector workers serve as the within-state control group.[25] These null effects are consistent with survey evidence from the 1970s. In the survey, teachers felt that tenure laws had no positive or negative effects on their profession (Cobb, 1981).

7.4 Discussion of the Retention Results

The teacher retention results suggest that the first stage impact of tenure laws is a potential explanation for the heterogeneous impact of tenure systems. Early tenure laws passed between 1910-1927 had significant policy bite. Consequently, the early laws also negatively affected student outcomes. In contrast, later laws did not affect the behavior of teachers or principals and did not affect student outcomes. Of course, the above conclusions have two important caveats. First, the retention regressions for early and later tenure laws differ. For example, the estimates for early laws calculate retention over decades, while the estimates for later laws measure yearly changes to retention. Second, it is impossible to study the retention impact of tenure laws passed between 1937-1959 as there is no retention data. At best, I can hypothesize that the null effects from the 1937-1959 tenure laws are for the same potential reason as later laws. Namely that, like the 1967-1977 legislation, there are no retention effects.

Despite the above concerns, qualitative evidence on teacher shortages supports the conclusion that later laws likely did not impact retention. Teacher shortages are an important factor in determining the bite

[25] The triple difference specification includes two types of workers; teachers and other public sector workers. The fixed effects are type-by-year, state-group-by-year and state-group-by-type fixed. For public sector workers, the retention variable is equal to one if they are still employed in the public sector. For teachers, it is equal to one if they are still employed as a public sector teacher.

of tenure laws. In a thin teacher labor market, the option value of dismissing a teacher is low; it is difficult to replace a dismissed teacher. Qualitative work has documents the impact of teacher shortages on tenure. For example, a 1956 tenure report by the National Education Associated stated that “the country has been suffering from a shortage of teachers; therefore, boards of education have not always taken the necessary steps to remove incompetent teachers” (9).

The timing of teacher shortages generally lines up with the passage of later tenure laws. Starting in WWII, wartime industries and military conscription demands lead to a serious teacher shortage (Cumbee et al., 1942; National Education Association, 1942b; Swanson, 1942). The lack of teachers continued into the 1950s and 1960s (Graybeal, 1971; National Education Association, 1957), possibly due to the baby boom that caused female teachers to exit the profession and increased demand for teachers due to rising student populations.

These shortages have turned into a large surplus by 1971 (Lang, 1975) and it is not clear why tenure systems passed in the 1970s did not change retention.[26] One possible explanation is that the teacher shortages data is measured at the national level. There could still have been teacher shortages occurring in states that implemented the 1970s tenure laws. Another explanation is that the norms that determine forced exit behavior take time to adjust. It might take several years for school boards to update their behavior to be optimal under a period of teacher shortages. This update of behavior could also have been slow due to limited past experience with teacher surpluses. The country has been under a teacher shortage since WWII, for almost 30 years. Few superintendents, school board members or principals who were employed in those positions before WWII remained in them by the 1970s.

[26] Tenure laws passed in the 1960s are unlikely to have large effects on retention once the teacher shortage lifts. In the states that implemented the 1960 laws, most of the teachers would already have tenure. Therefore, the impact of the surplus teachers would only be felt by non-tenured teachers. These non-tenured teachers make a small portion of the total teaching force.

7.5 The Impact of Tenure Laws on Black Teachers in 1962-1990

The teacher retention results, although a potential mechanism for explaining the heterogeneous outcomes over time, do not explain the decline in educational attainment for Black men in 1990 and 2000. In this section, I present direct evidence that the passage of later laws sharply reduced the fraction of Black teachers. A reduction in Black teachers can harm Black student outcomes; large literature has documented the students benefit from same-race teachers (Dee, 2004; Clotfelter & Vigdor, 2007; Egalite, Kisida, & Winters, 2015; Gershenson et al., 2018).

I estimate a standard event study model using the same CPS dataset used to measure retention. The sample is limited to individuals who are presently employed as teachers and work in the public sector. The outcome is an indicator variable that is equal to one if the teacher is Black.

Event study estimates are available in Figure 13. Trends between treated and control state groups are parallel before the passage of the tenure laws. The short-run effects of a tenure law, up to four years after its passage, are also zero. However, four years after implementing a tenure system, there is a large statistically significant decline in the fraction of black teachers. Later estimates attenuate somewhat and eventually lose statistical significance, but the effect sizes are qualitatively large.

Point estimates computed using a balanced are presented in Table 8, Panel A. In the parsimonious event study model (Column 1), the short-run effect is negative but not statistically significant. Then, 5 to 9 years after tenure, the fraction of black teachers falls by 0.42. Finally, at ten to fourteen years of treatment, the effect somewhat attenuates to 0.35 and is no longer statistically significant. Point estimates in the balanced panel specification are robust to the inclusion of region-by-year fixed effects. However, in the triple-differences specification, none of the estimates are significant.

In Panel B, the regressions are computed using an unbalanced panel of state groups. The pattern is qualitatively similar to that of the balanced panel. There are no short-run effects. Then there is a large and significant fall in the fraction of Black teachers at five to nine years. The decline ranges from -0.076 to -

0.083 across different specifications. This effect is almost twice the size of the one in the balanced panel. The estimated effect after ten or more years of treatment attenuates. Although this time, they are significant at the ten percent level when using triple-differences or region-by-year fixed effects.

These large reductions in Black teachers can account for the decline in educational attainment among Black men. As to why tenure laws, which are supposed to protect minority teachers, caused a large loss of Black teachers, there are several possible reasons. It could be that discriminatory school boards value the option of firing teachers more for Black teachers and hire less of them. Such school boards could also have denied them tenure at higher rates. Alternatively, there could be a labor market channel where white teachers value tenure protections more than Black teachers. Understanding the exact reason of great interest but is beyond the scope of this paper.

8 Conclusion

My paper is the first to study the impact of tenure systems on long-run student outcomes. As policy variation in the 21st century is limited, I identify the passage of all tenure systems between 1910-1977 and study outcomes in 1940 and 1960-2000. I then exploit the implementations of these tenure systems in a cross-cohort difference-in-differences design. The results state that tenure laws negatively affect student outcomes, but only when they are binding.

The main findings are that early tenure laws passed between 1910-1927 drastically lowered adult wage income in 1940. After 12 years of exposure, male income declined by 5.85 percent, and female wages fell by 5.55 percent. These large negative effects exist only due to the early laws. Tenure laws passed in later years do not affect the average student's income, employment, or educational attainment. One exception is Black male students, whose educational attainment decreases in 1990 and 2000.

To reconcile these divergent results, I show that early tenure laws have policy bite. Between 1900-1940 the implementations of the tenure system increased the retention of male and female teachers, teachers' occupational attachment, and protected married female teachers. In contrast, later laws did not affect teacher turnover but did reduce the fraction of Black teachers. This reduction in Black teachers can potentially explain the decline in the educational attainment of Black male students; prior work has consistently found benefits from same-race teachers (Dee, 2004; Clotfelter & Vigdor, 2007; Egalite, Kisida, & Winters, 2015; Gershenson et al., 2018).

Regarding the modern debate about tenure laws, the estimates presented here allow us to learn a few lessons from the past. First, it is important to consider if tenure laws are binding. Many disadvantaged districts struggle to attract and retain qualified teachers, resulting in tenure laws likely having a limited bite. Furthermore, data suggest a looming teacher shortage. It is not a given that principals will remove a substantial portion of teachers when their replacements are difficult to find. Second, if modern tenure laws are binding, there is no guarantee that the negative 1940 results will repeat. It is important to note that education in the early 20th Century was very different from the present. Then the teaching workforce was much less professional, with a lower career attachment. Many female teachers entered the occupation not as a first career choice but because it was their only choice. The teachers and students were subject to much fewer accountability systems or competition from private and charter schools. Measuring teacher quality dependent on classroom observations and sophisticated methods, such as Value-Added models, were not available. Thus, while the early experience with tenure laws detailed in this paper is of considerable interest both historically and for delineating possible negative effects, it is not a given that such effects will hold in the present.

9 Works Cited

Abramitzky, Ran, Leah Boustan and Myera Rashid. Census Linking Project: Version 1.0 [dataset]. 2020. https://censuslinkingproject.org

Anderson, Kaitlin P., J. M. Cowen, and K. O. Strunk. "The impact of teacher labor market reforms on student achievement: Evidence from Michigan." *Education Policy Innovation Collaborative (EPIC) Working Paper* 1 (2019).

Autor, David H. "Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing." *Journal of labor economics* 21.1 (2003): 1-42.

Autor, David H., John J. Donohue III, and Stewart J. Schwab. "The costs of wrongful-discharge laws." *The review of economics and statistics* 88.2 (2006): 211-231.

Autor, David H., William R. Kerr, and Adriana D. Kugler. "Does employment protection reduce productivity? Evidence from US states." *The Economic Journal* 117.521 (2007): F189-F217.

Bailey, Martha, Connor Cole, Morgan Henderson. "How well do automated linking methods perform? Lessons from U.S. historical data." *Journal of Economic Literature* 58.4 (2020): 997-1044.

Baron, E. Jason. "The effect of teachers' unions on student achievement in the short run: Evidence from Wisconsin's Act 10." *Economics of Education Review* 67 (2018): 40-57.

Bastian, Jacob, and Katherine Michelmore. "The long-term impact of the earned income tax credit on children's education and employment outcomes." *Journal of Labor Economics* 36.4 (2018): 1127-1163.

Bursztyn, Leonardo, and Robert Jensen. "How does peer pressure affect educational investments?. " *The quarterly journal of economics* 130.3 (2015): 1329-1367.

Carrell, Scott E., and Mark L. Hoekstra. "Externalities in the classroom: How children exposed to domestic violence affect everyone's kids." *American Economic Journal: Applied Economics* 2.1 (2010): 211-28.

Carruthers, Celeste, David Figlio, and Tim Sass. "Did tenure reform in Florida affect student test scores " *Evidence Speaks Reports* 2.52 (2018): 1-14.

Carter, Susan B., and Elizabeth Savoca. "The "teaching procession"? another look at teacher tenure, 1845–1925." *Explorations in Economic History* 29.4 (1992): 401-416.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. "The effect of minimum wages on low-wage jobs." *The Quarterly Journal of Economics* 134.3 (2019): 1405-1454.

Chetty, Raj, et al. "How does your kindergarten classroom affect your earnings? Evidence from Project STAR." *The Quarterly journal of economics* 126.4 (2011): 1593-1660.

Cobb, Joseph J. *An Introduction to Educational Law, for Administrators and Teachers*. Thomas, 1981.

Cohodes, Sarah R., et al. "The effect of child health insurance access on schooling: Evidence from public insurance expansions." *Journal of Human Resources* 51.3 (2016): 727-759.

Cumbee, Carroll Fleming, Byron B. Harless, and Arthur Raymond Mead. *Our Schools in War Time: Can We Maintain Adequate Personnel?*. University of Florida, College of Education, 1942.

Dahl, Gordon B., and Lance Lochner. "The impact of family income on child achievement: Evidence from the earned income tax credit." *American Economic Review* 102.5 (2012): 1927-56.

Dee, Thomas S. "Teachers, race, and student achievement in a randomized experiment." *Review of economics and statistics* 86.1 (2004): 195-210.

Dee, Thomas S., and James Wyckoff. "Incentives, selection, and teacher performance: Evidence from IMPACT." *Journal of Policy Analysis and Management* 34.2 (2015): 267-297.

Deming, David J., et al. "School accountability, postsecondary attainment, and earnings." *Review of Economics and Statistics* 98.5 (2016): 848-862.

Dohmen, Thomas, et al. "Are risk aversion and impatience related to cognitive ability?." *American Economic Review* 100.3 (2010): 1238-60.

Feigenbaum, James J., and Hui Ren Tan. *The return to education in the mid-20th Century: evidence from twins*. No. w26407. National Bureau of Economic Research, 2019.

Garber, Lee O. "The Law Governing the Dismissal of Teachers on Permanent Tenure." *The Elementary School Journal* 35.2 (1934): 115-122.

Gershenson, Seth, et al. "The long-run impacts of same-race teachers." No. w25254. National Bureau of Economic Research, 2018.

Gilson, Ronald J., and Robert H. Mnookin. "Coming of age in a corporate law firm: The economics of associate career patterns." *Stanford Law Review* (1989): 567-595.

Goldhaber, Dan, Michael Hansen, and Joe Walch. "Time to Tenure: Does Tenure Reform Affect Teacher Absence Behavior and Mobility? Working Paper 172." *National Center for Analysis of Longitudinal Data in Education Research (CALDER)* (2016).

Goodman-Bacon, Andrew. "Difference-in-differences with variation in treatment timing". *American Economic Review*, forthcoming (March 2021)

Goodman-Bacon, Andrew. "The Long-Run Effects of Childhood Insurance Coverage: Medicaid Implementation, Adult Health and Labor Market Outcomes." No. w22899. National Bureau of Economic Research, 2016.

Graybeal, William S. "Teacher surplus and teacher shortage." The Phi Delta Kappan 53.2 (1971): 82-85.

Hanushek, Eric A., et al. "Does peer ability affect student achievement?." *Journal of applied econometrics* 18.5 (2003): 527-544.

Hanushek, Eric A., Marc Piopiunik, and Simon Wiederhold. "The value of smarter teachers international evidence on teacher cognitive skills and student performance." *Journal of Human Resources* 54.4 (2019): 857-899.

Helgertz, Jonas, Joseph R. Price, Jacob Wellington, Kelly Thompson, Steven Ruggles, and Catherine R. Fitch. "A New Strategy for Linking Historical Censuses: A Case Study for the IPUMS Multigenerational Longitudinal Panel." (2020).

Hoynes, Hilary, Diane Whitmore Schanzenbach, and Douglas Almond. "Long-run impacts of childhood access to the safety net." *American Economic Review* 106.4 (2016): 903-34.

Jackson, C. Kirabo, Rucker C. Johnson, and Claudia Persico. "The effects of school spending on educational and economic outcomes: Evidence from school finance reforms." *The Quarterly Journal of Economics* 131.1 (2016): 157-218.

Jacob, Brian A. "The effect of employment protection on teacher effort." *Journal of Labor Economics* 31.4 (2013): 727-761.

Jones, M. D. (2015). "How do teachers respond to tenure?" IZA Journal of Labor Economics, 4(1), 8.

Kahn, Charles, and Gur Huberman. "Two-sided uncertainty and 'up-or-out'" contracts." *Journal of Labor Economics* 6.4 (1988): 423-444.

Kraft, Matthew A., et al. "Teacher accountability reforms and the supply and quality of new teachers." *Journal of Public Economics* 188 (2020): 104212.

Kraft, M. A., & Gilmour, A. F. (2017). Revisiting the Widget Effect: Teacher Evaluation Reforms and the Distribution of Teacher Effectiveness. Educational Researcher, 46(5), 234–249.

Krolikowski, Pawel. "Choosing a control group for displaced workers." *ILR Review* 71.5 (2018): 1232-1254.

Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach. "School finance reform and the distribution of student achievement." *American Economic Journal: Applied Economics* 10.2 (2018): 1-26.

Lang, Theodore H. "Teacher tenure as a management problem." *The Phi Delta Kappan* 56.7 (1975): 459-462.

Lavy, Victor, Olmo Silva, and Felix Weinhardt. "The good, the bad, and the average: Evidence on ability peer effects in schools." *Journal of Labor Economics* 30.2 (2012): 367-414.

Lebergott, Stanley. *Manpower in economic growth: The American record since 1800*. New York: McGraw-Hill, 1964.

Loeb, Susanna, Luke C. Miller, and James Wyckoff. "Performance screens for school improvement: The case of teacher tenure reform in New York City." *Educational Researcher* 44.4 (2015): 199-212.

Lovenheim, Michael F., and Alexander Willén. "The long-run effects of teacher collective bargaining." *American Economic Journal: Economic Policy* 11.3 (2019): 292-324.

Ludwig, Jens, and Douglas L. Miller. "Does Head Start improve children's life chances? Evidence from a regression discontinuity design." *The Quarterly journal of economics* 122.1 (2007): 159-208.

Lynch, Alicia Doyle, Richard M. Lerner, and Tama Leventhal. "Adolescent academic achievement and school engagement: An examination of the role of school-wide peer culture." *Journal of youth and adolescence* 42.1 (2013): 6-19.

Nagler, Markus, Marc Piopiunik, and Martin R. West. "Weak markets, strong teachers: Recession at career start and teacher effectiveness." *Journal of Labor Economics* 38.2 (2020): 453-500.

National Council on Teacher Quality. (2017). Tenure national results. State Teacher Policy Database. [Data set]. Retrieved from: https://www.nctq.org/yearbook/national/Tenure-79

National Education Association. *The 1957 Teacher Supply and Demand Report.* (1957).

---. *Analysis of Teacher Tenure Provisions: State and Local.* (1954).

---. *Teacher Tenure: Its Status Critically Appraised.* (1942a).

---. *Current Teacher Supply-Demand Situation.* (1942b).

---. *The Problem of Teacher Tenure.* (1924).

Opper, Isaac M. "Does helping john help sue? Evidence of spillovers in education." *American Economic Review* 109.3 (2019): 1080-1115.

Ost, Ben, and Jeffrey C. Schiman. "Workload and teacher absence." *Economics of Education Review* 57 (2017): 20-30.

Papay, John P., Eric S. Taylor, John H. Tyler, and Mary E. Laski. "Learning job skills from colleagues at work: Evidence from a field experiment using teacher performance data." *American Economic Journal: Economic Policy* 12.1 (2020): 359-88.

Phillips, Elizabeth. "The effect of tenure on teacher performance in secondary education." (2009).

Prendergast, Canice. "The provision of incentives in firms." *Journal of economic literature* 37.1 (1999): 7-63.

Roberts, Michael Alvin. *Essays on Regime Change and Education Policy Reform*. Diss. University of Kansas, 2018.

Rodriguez, Luis Alberto. *An Examination of Teacher Tenure Reform in Tennessee: Turnover, Performance, and Sense-Making*. Diss. 2018.

Roth, Jonathan. "Union Reform and Teacher Turnover: Evidence from Wisconsin's Act 10." (2019).

Rothstein, Jesse. "Teacher quality policy when supply matters." *American Economic Review* 105.1 (2015): 100-130.

Sandler, Danielle H., and Ryan Sandler. "Multiple event studies in public finance and labor economics: A simulation study with applications." *Journal of Economic and Social Measurement* 39.1-2 (2014): 31-57.

Staiger, Douglas O., and Jonah E. Rockoff. "Searching for effective teachers with imperfect information." *Journal of Economic perspectives* 24.3 (2010): 97-118.

Steven Ruggles, Sarah Flood, Sophia Foster, Ronald Goeken, Jose Pacas, Megan Schouweiler and Matthew Sobek. IPUMS USA: Version 11.0 [dataset]. Minneapolis, MN: IPUMS, 2021. https://doi.org/10.18128/D010.V11.0

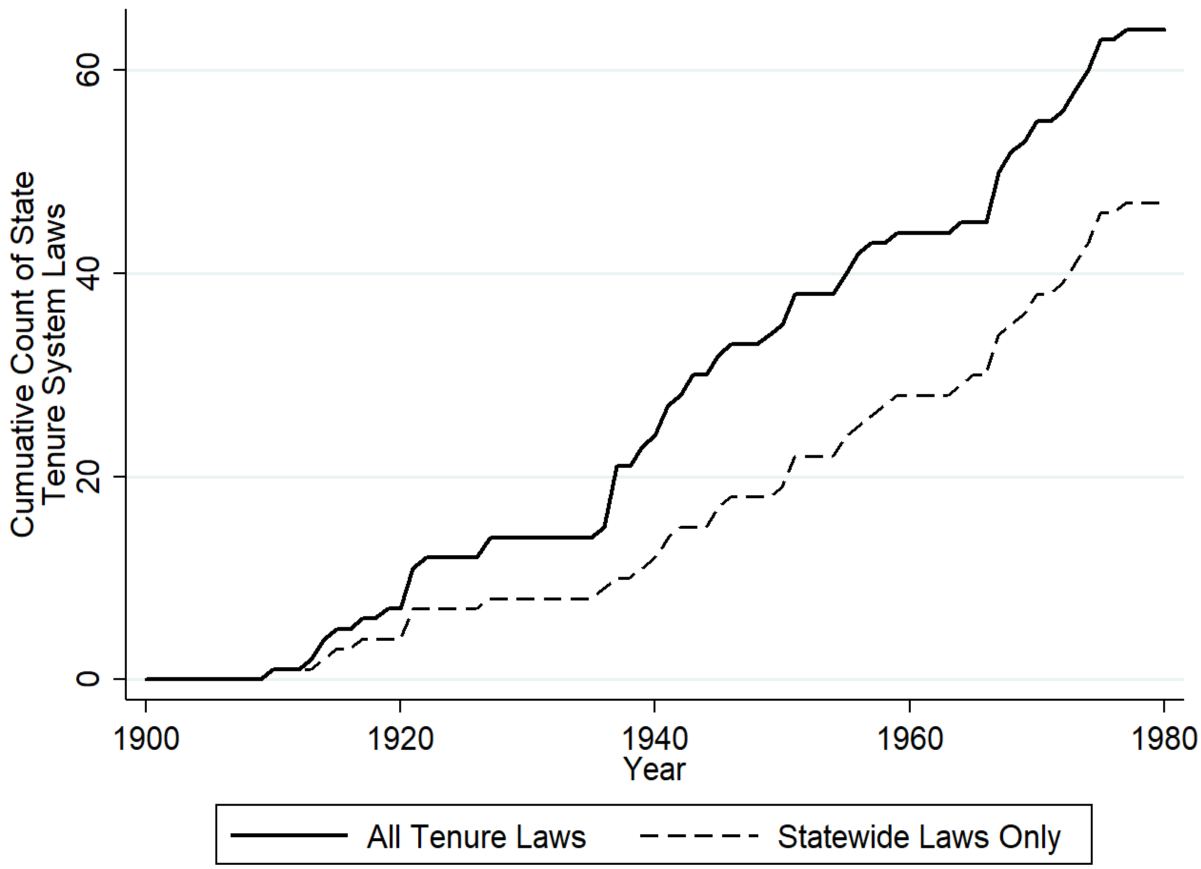
Stephens Jr, Melvin, and Dou-Yan Yang. "Compulsory education and the benefits of schooling." *American Economic Review* 104.6 (2014): 1777-92.

Thapa, Amrit, et al. "A review of school climate research." *Review of educational research* 83.3 (2013): 357-385.

U.S. Office of Education. *Teacher Shortages and Surpluses in 45 States.* (1942).

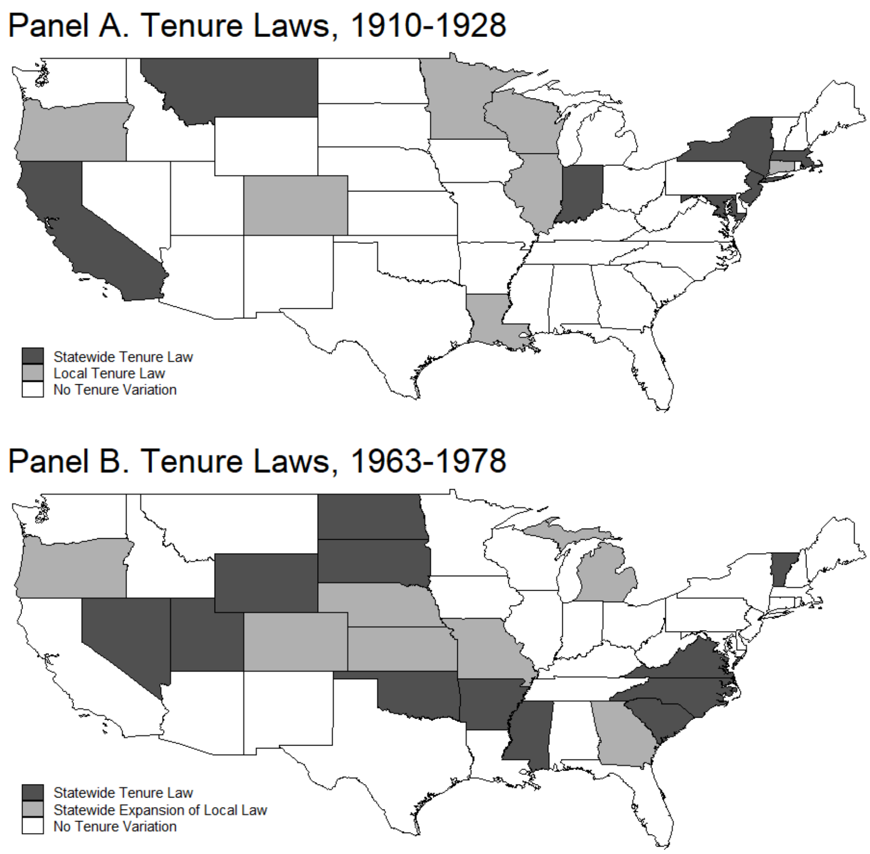
FIGURES

Figure 1. Time Series Variation in Teacher Tenure Systems



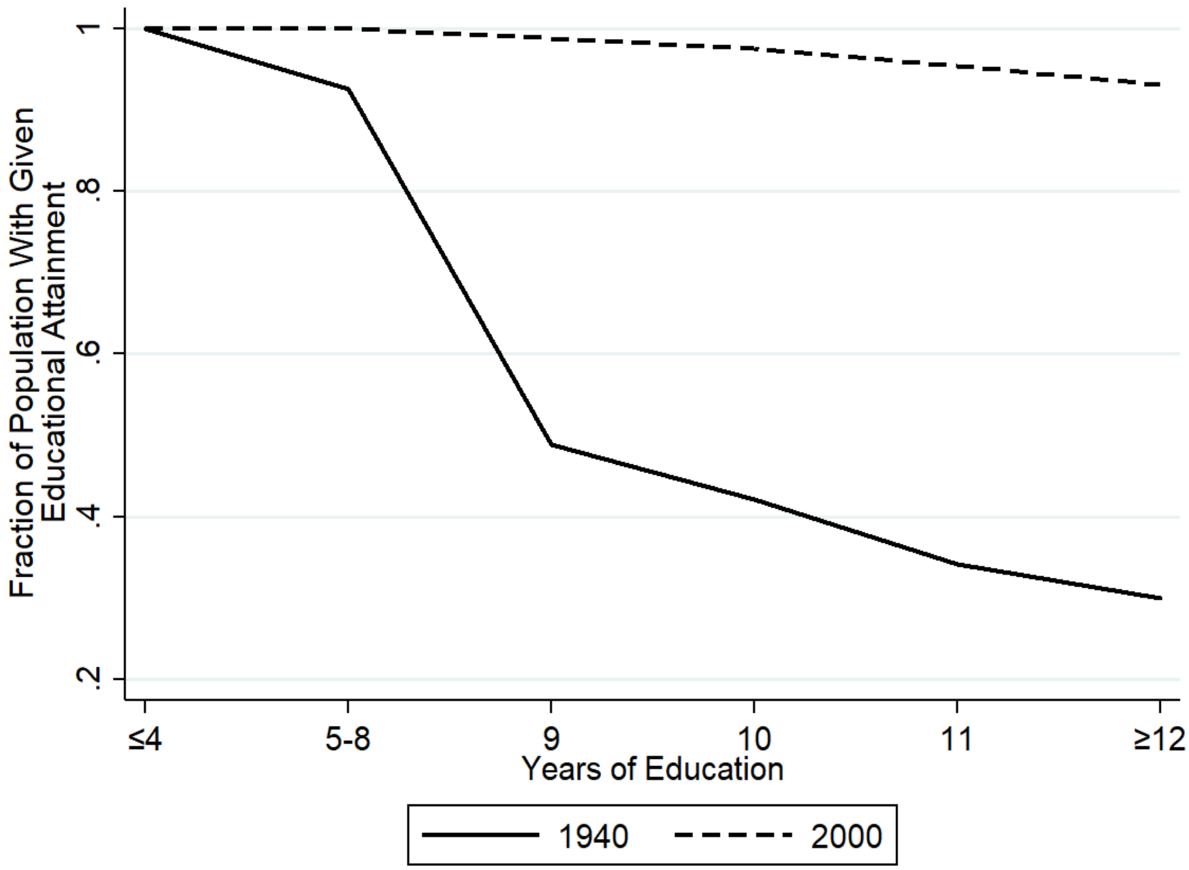
Note: This figure displays the cumulative count of state tenure laws passed by a given year. All Tenure Laws includes local laws passed by state legislatures and statewide laws. As a state can first pass a local law, and then a statewide law, the cumulative count can exceed the number of states. Statewide Laws Only includes only tenure laws that cover the whole state.

Figure 2. Cross-Sectional Variation in Teacher Tenure Systems



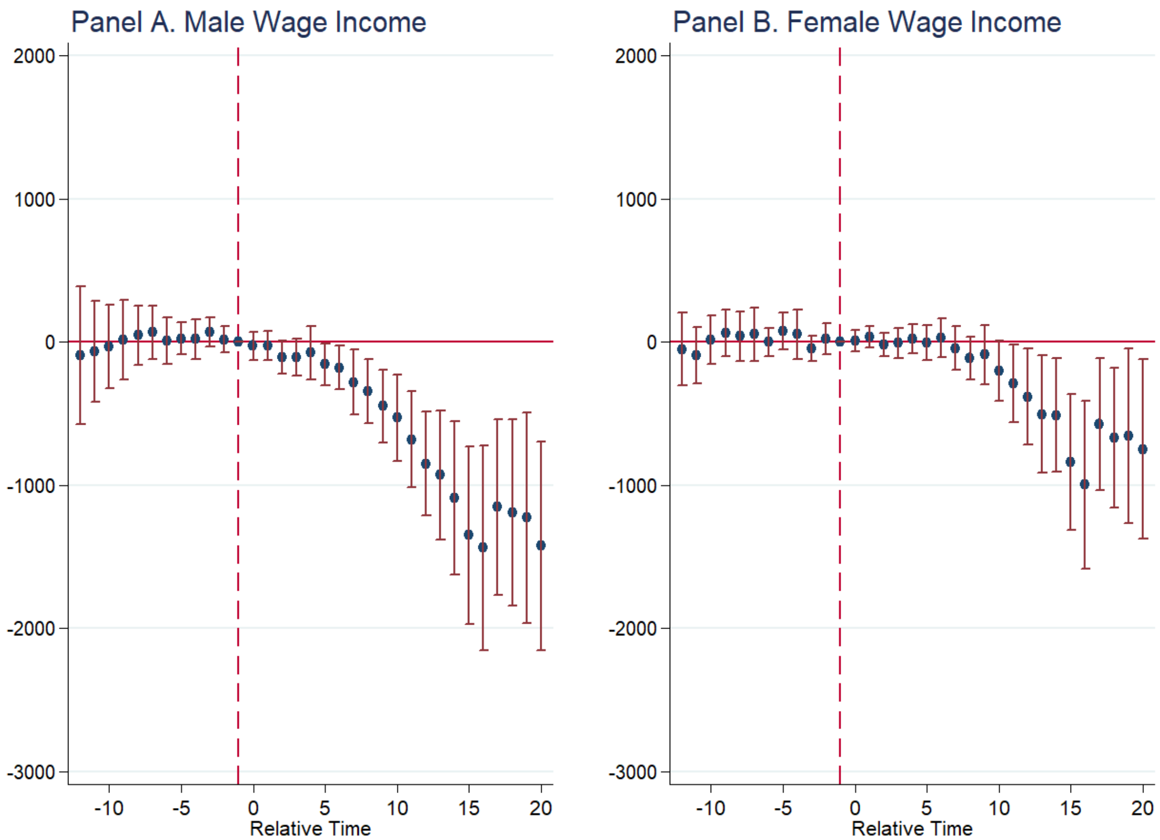
Note: This figure displays cross-sectional variation in teacher tenure systems for two key periods. Statewide Tenure Law refers to a state passing a tenure system where previously none existed. Local Tenure Laws are those passed by a state legislature, but only cover a portion of the state. Statewide Expansion of Local Law is a state implementing a tenure system for the whole state, but previously a portion of the state was already covered by a local law.

Figure 3. Educational Attainment in the 1940 and 2000 Samples



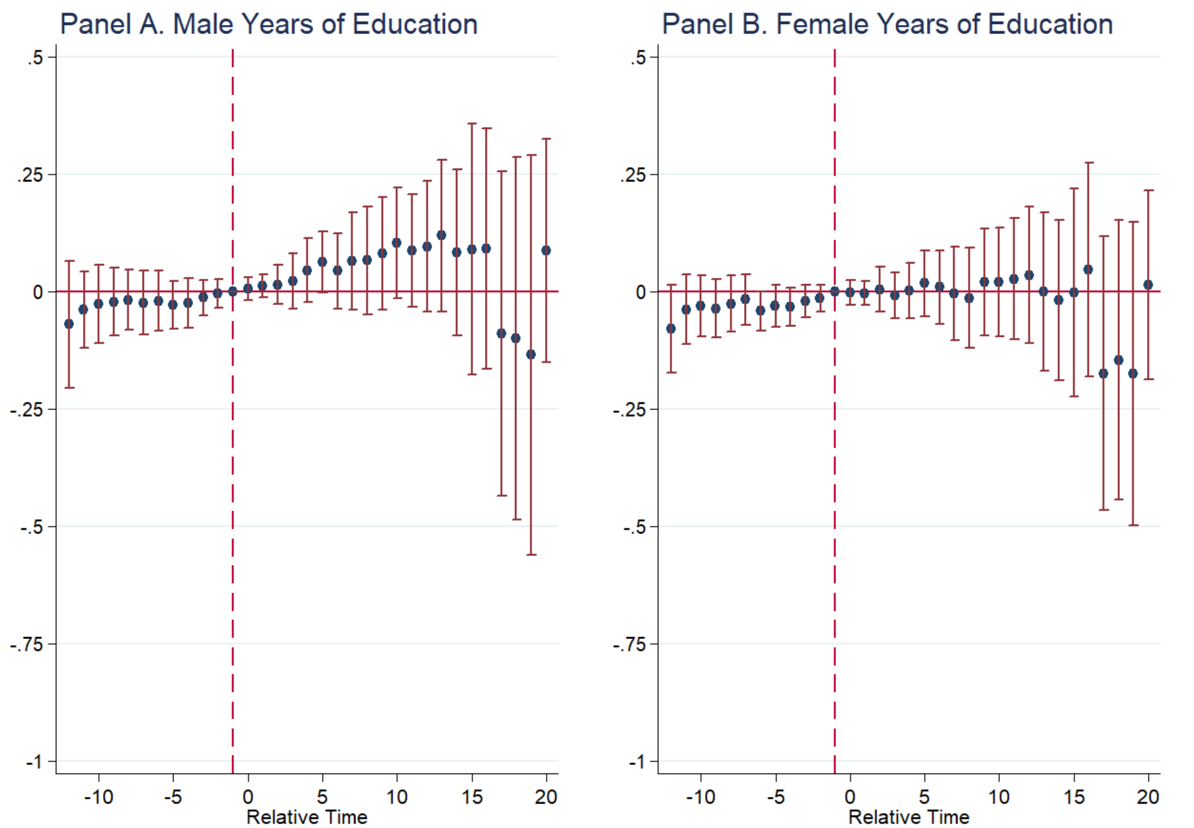
Note: The cross-cohort event study estimates an Intent to Treat effect. One of the reasons for the ITT nature of the estimates is educational attainment. Not everyone attends school for 18 years. This figure highlights the relationship between educational attainment and the ITT estimate. The figure displays educational attainment of the sample in 1940 and 2000.

Figure 4. Event Study Estimates: Wage Income in 1940



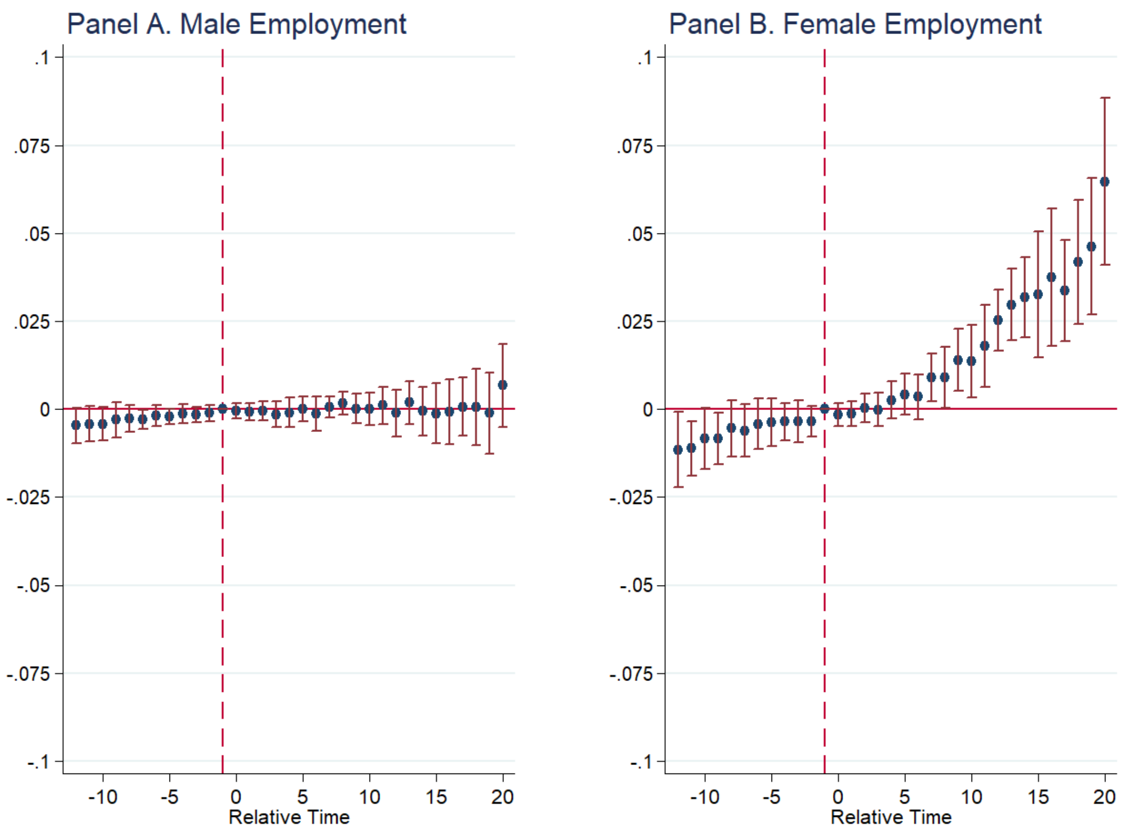
Note: The figure estimates equation (1) using 1940 Census data for individuals between the ages 25-54 who are born in the United States. The dependent variable is wage income. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the birth state level.

Figure 5. Event Study Estimates: Years of Education in 1940



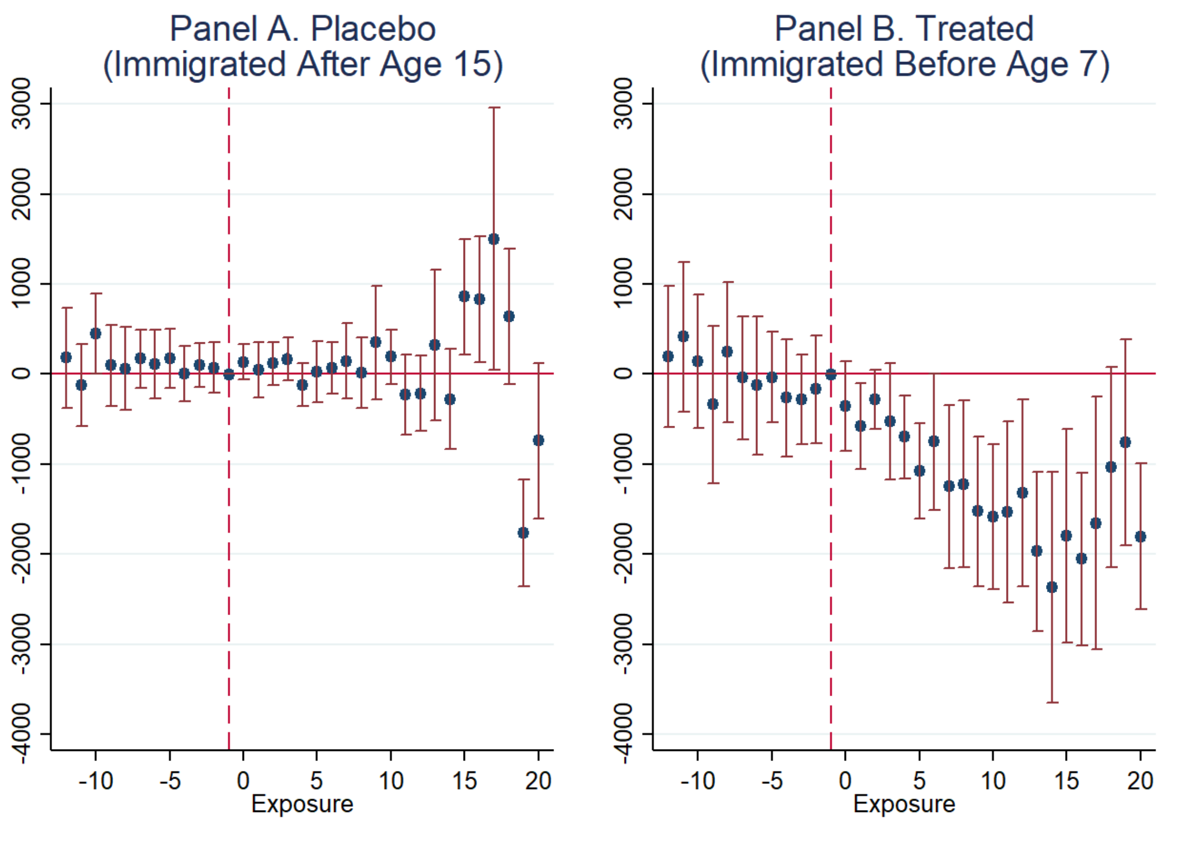
Note: The figure estimates equation (1) using 1940 Census data for individuals between the ages 25-54 who are born in the United States. The dependent variable is years of education. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the birth state level.

Figure 6. Event Study Estimates: Employment in 1940



Note: The figure estimates equation (1) using 1940 Census data for individuals between the ages 25-54 who are born in the United States. The dependent variable is an indicator that is equal to one if the individual is employed. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the birth state level.

Figure 7. Event Study Estimates: Wage Income of Placebo and Treated Immigrants

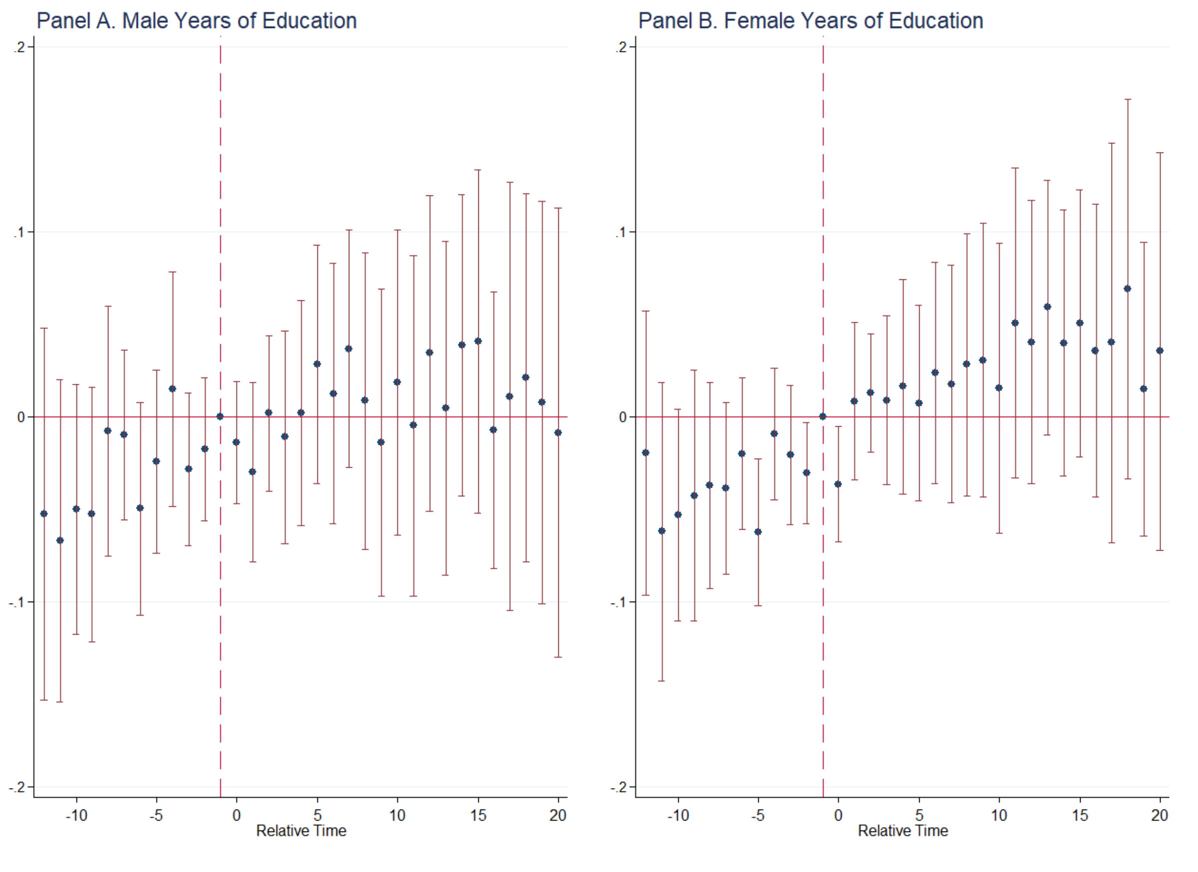


Note: The figure estimates equation (1) using 1940 matched Census data for immigrants between the ages 25-54 who resided in the United States by 1930. The dependent variable is wage income. The placebo group are those who immigrated to the US after age 15. The treated are those who immigrated to the US before age 7. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at 1930 state of residence.

Figure 8. Event Study Estimates: Wage Income in 1960-2000

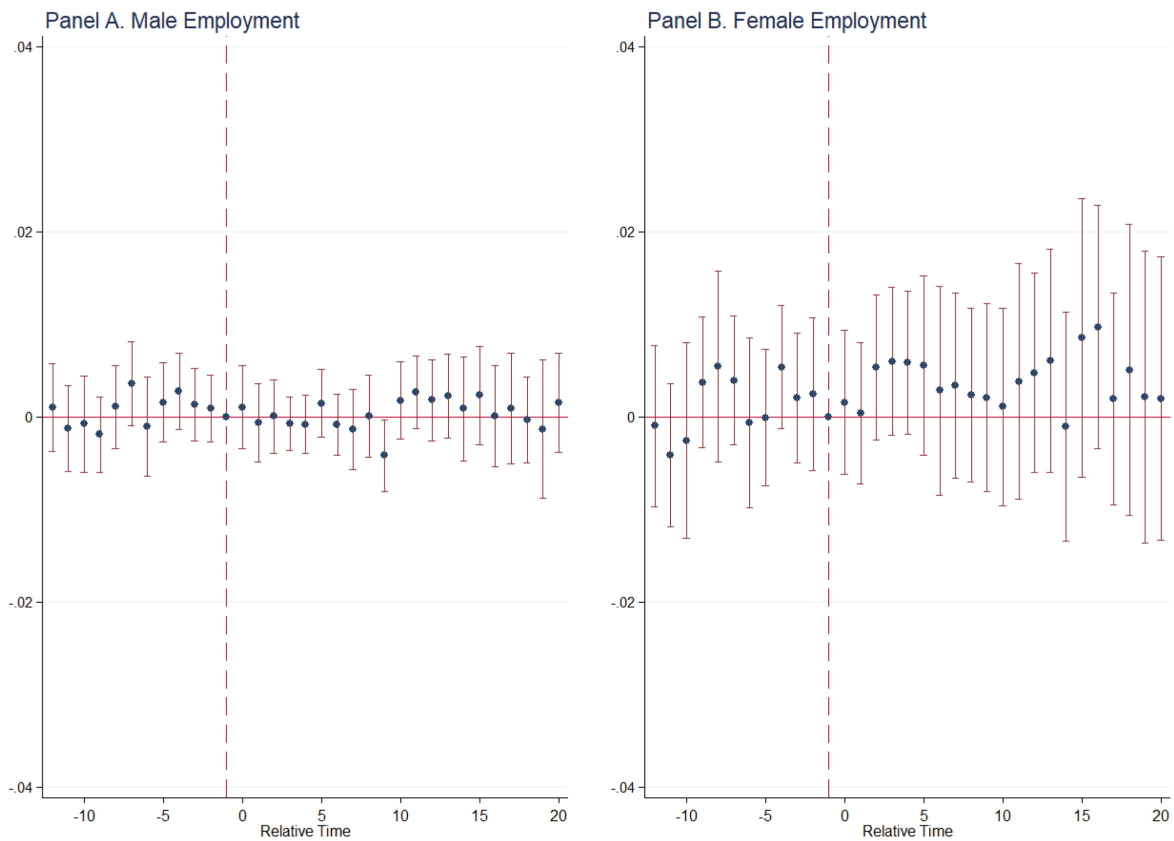
Note: The figure estimates equation (2) using 1960-2000 Census data for individuals between the ages 25-54 who are born in the United States. The dependent variable is wage income. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the birth state level.

Figure 9. Event Study Estimates: Years of Education in 1960-2000



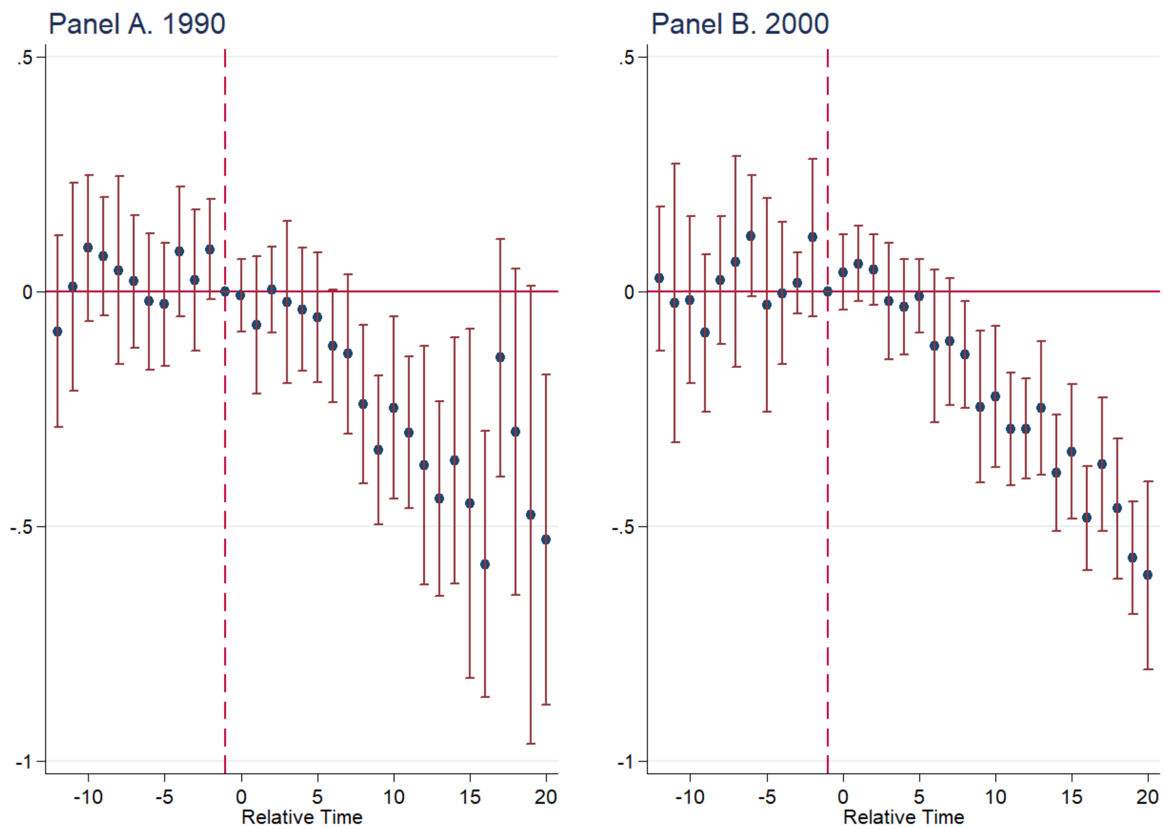
Note: The figure estimates equation (2) using 1960-2000 Census data for individuals between the ages 25-54 who are born in the United States. The dependent variable is years of education. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the birth state level.

Figure 10. Event Study Estimates: Employment in 1960-2000



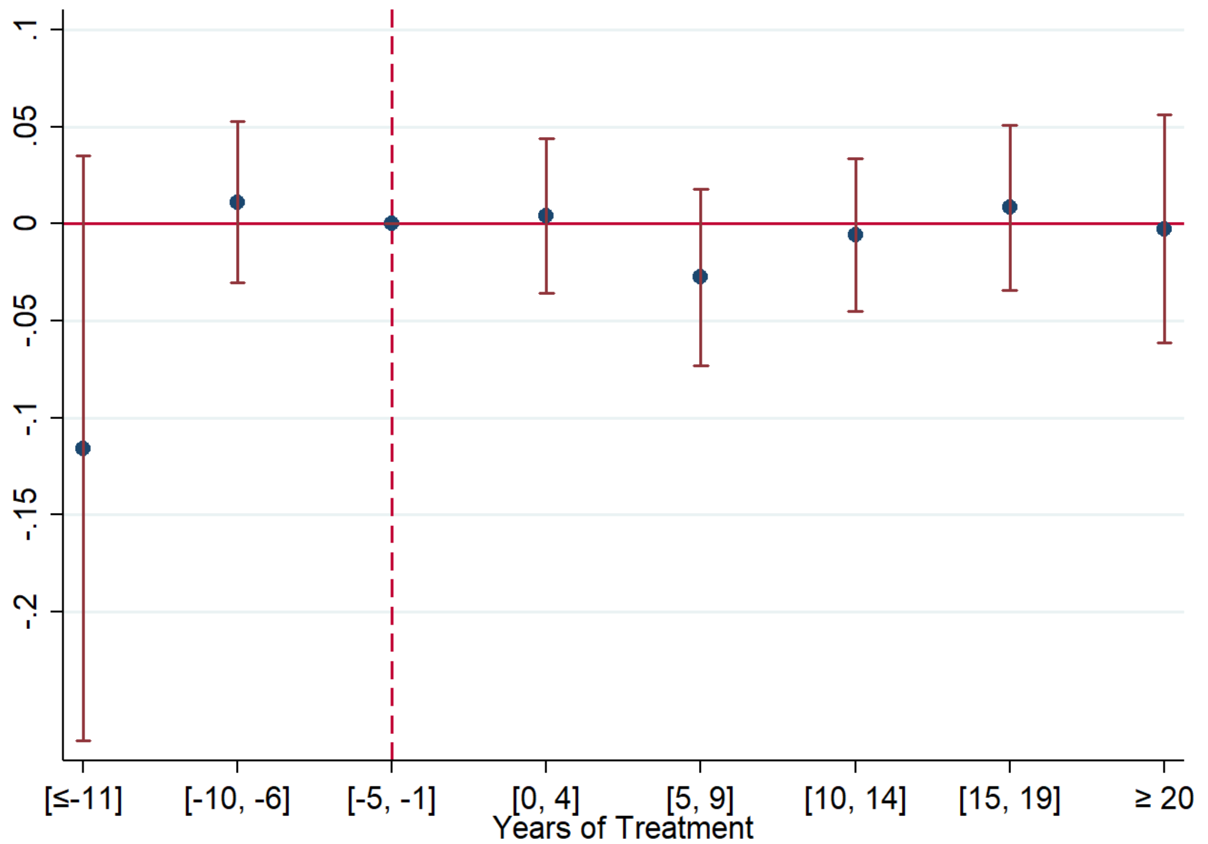
Note: The figure estimates equation (2) using 1960-2000 Census data for individuals between the ages 25-54 who are born in the United States. The dependent variable is an indicator that is equal to one if the individual is employed. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the birth state level.

Figure 11. Event Study Estimates: Black Men Years of Education in 1990 and 2000



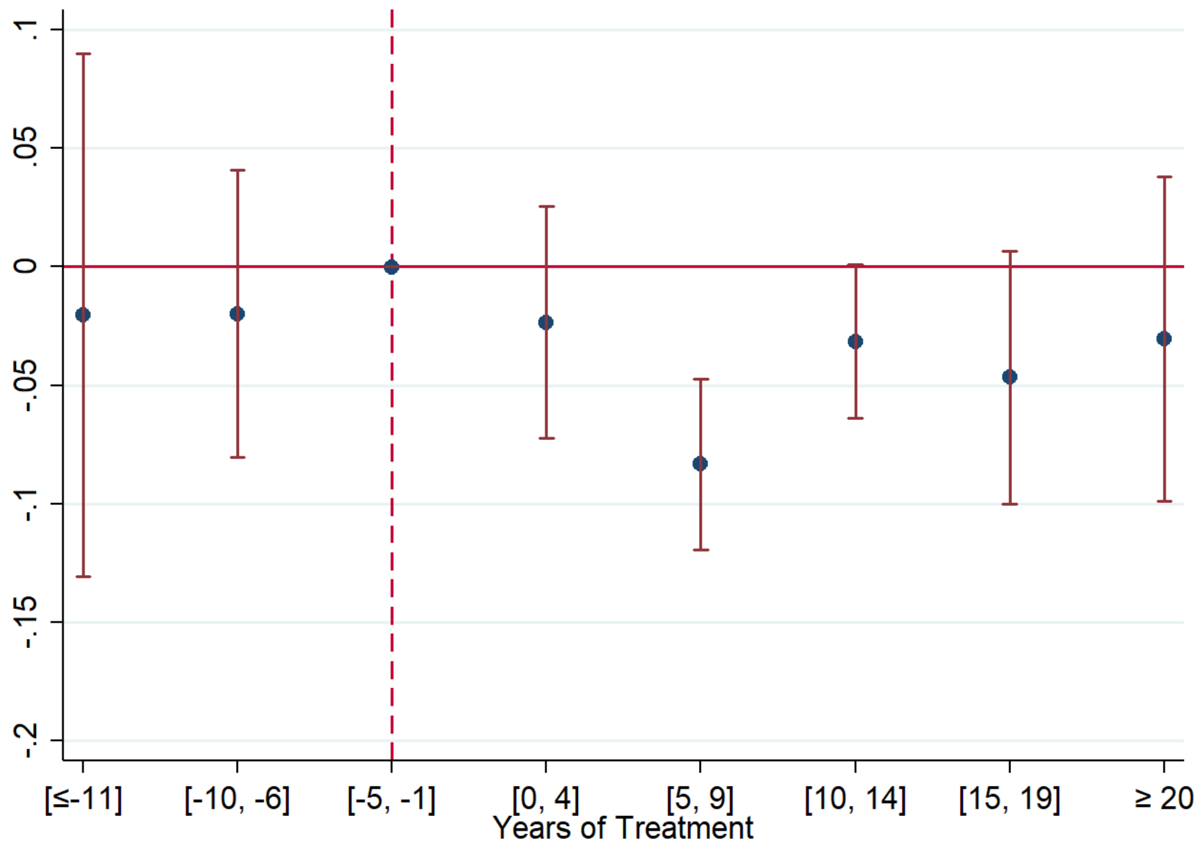
Note: The figure estimates equation (1) using 1960-2000 Census data for Black men between the ages 25-54 who are born in the United States. The dependent variable is years of education. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the birth state level.

Figure 12. Event Study Estimates: Teacher Retention in 1962-1990



Note: The figure estimates a modified version of Equation (4) using 1962-1990 CPS data for individuals who report a public sector teaching occupation in the previous year. The dependent variable is equal to one if they are an employed teacher in the current year. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the state level.

Figure 13. Event Study Estimates: Fraction Black Teachers in 1960-2000



Note: The figure estimates a modified version of Equation (4) using 1962-1990 CPS data for individuals who report a public sector teaching occupation in the current year. The dependent variable is equal to one if they Black. The bars around each point estimate represent the 95 percent confidence interval. Standard errors are clustered at the state level.

TABLES

Table 1-The Effect of Tenure Systems in 1940

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Male Wage Income						
At 12 Years	-1,554.73***	-848.82***	-782.35***	-802.55***	-894.41***	-867.21***
	(270.34)	(179.61)	(166.42)	(147.90)	(182.08)	(195.98)
Percent effect at 12 years	-10.71	-5.85	-5.39	-5.53	-6.16	-5.97
Panel B. Female Wage Income						
At 12 Years	-849.02***	-424.37**	-372.85**	-370.04**	-491.83**	-438.96**
	(197.97)	(162.79)	(150.42)	(148.28)	(224.05)	(186.57)
Percent effect at 12 years	-11.11	-5.55	-4.88	-4.84	-6.44	-5.74
Panel C. Male Years of Education						
At 12 Years	0.11	0.10	0.06	0.06	-0.01	0.07
	(0.08)	(0.07)	(0.07)	(0.07)	(0.10)	(0.07)
Percent effect at 12 years	1.28	1.12	0.64	0.64	-0.14	0.81
Panel D. Female Years of Education						
At 12 Years	-0.08	0.02	0.01	0.01	-0.14	-0.02
	(0.07)	(0.07)	(0.07)	(0.07)	(0.09)	(0.07)
Percent effect at 12 years	-0.86	0.22	0.16	0.16	-1.55	-0.27
Panel E. Male Employment						
At 12 Years	-0.006	-0.001	-0.001	-0.001	0.000	-0.001
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Percent effect at 12 years	-0.72	-0.13	-0.10	-0.10	0.03	-0.16
Panel F. Female Employment						
At 12 Years	0.036***	0.027***	0.024***	0.024***	0.024**	0.027***
	(0.010)	(0.005)	(0.005)	(0.005)	(0.011)	(0.006)
Percent effect at 12 years	14.64	10.92	9.90	9.90	9.72	10.98
Region-Year FE		X	X			X
Demographic and Policy Controls			X	X		
Division-Year FE				X		
No South					X	X

Note: The table displays results based on equation (1) using 1940 Full Count Census data. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. Regressions are based on 1,440 state-cohort observations and are weighted based on cell size. Demographic and policy controls consist of the fraction of each state-cohort-gender cell that is Black, Hispanic, Other Race, has an Immigrant Mother, and has an Immigrant Father, the number of years the cell has been exposed to school-age laws and a bartik measure of exposure to the Great Depression. Standard errors clustered at the birth state and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2-The Effect of Tenure Systems on Male Wage Income in 1940 Using Linked Data

	Wage Income				
	(1)	(2)	(3)	(4)	(5)
Panel A. Treatment Based on Earliest County of Residence					
At 12 Years	-1,605.84***	-1,283.00***	-1,297.98***	-1,330.68***	-1,330.68***
	(281.62)	(264.02)	(269.93)	(259.11)	(259.11)
Percent effect at 12 years	-10.58	-8.46	-8.56	-8.77	-8.77
Panel B. Treatment Based on Earliest State of Residence					
At 12 Years	-1,419.81***	-1,013.20***	-1,107.25***	-1,047.05***	-1,047.05***
	(238.03)	(159.62)	(162.21)	(180.26)	(180.26)
Percent effect at 12 years	-9.36	-6.68	-7.30	-6.90	-6.90
Panel C. Treatment Based on State of Birth					
At 12 Years	-1,331.46***	-927.26***	-949.07***	-924.00***	-924.00***
	(239.83)	(174.28)	(155.28)	(184.14)	(184.14)
Percent effect at 12 years	-8.78	-6.11	-6.26	-6.09	-6.09
Region-Year FE		X	X	X	
Addtional Controls			X	X	X
Division-Year FE					X
Early Life Circumstances Controls				X	X

A treatment is assigned based on the earliest county of residence and fixed effects for said county are included. In Panel B treatment is assigned using the earliest State of residence and fixed effects for such a state are included. Finally, Panel C is the baseline equation (1) that uses state of birth. The sample is limited to men who are born in the continental United States, are between the ages 25 to 54 and are linked to an earlier census in which they are between the ages of 6 and 18. Additional controls are the number of years an individual has been exposed to school-age laws and a Bartik measure of exposure to the Great Depression. The controls for early life circumstances are indicator variables for father's occupation, childhood urban residence, presence of a father, presence of a mother, father's age, mother's age, whether mother is employed and the number of siblings. Standard errors clustered at the either earliest state of residence (Panel's A and B) or birth state (Panel C) and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3-The Effect of Tenure Systems on Male Immigrant Wage Income in 1940

	Wage Income					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Placebo, Immigrated After Age 15						
At 12 Years	-543.34**	-296.90	-259.09	-309.21	-625.08**	-307.16
	(261.85)	(359.12)	(190.44)	(306.60)	(269.20)	(357.97)
Percent effect at 12 years	-3.50	-1.91	-1.67	-1.99	-4.03	-1.98
Panel B. Treated, Immigrated Before Age 7						
At 12 Years	-1,430.57***	-1,287.35**	-1,157.96**	-958.27*	-1,438.73**	-1,255.33**
	(520.75)	(532.97)	(453.88)	(483.04)	(525.91)	(546.16)
Percent effect at 12 years	-9.51	-8.55	-7.69	-6.37	-9.56	-8.34
Region-Year FE		X	X			X
Demographic and Policy Controls			X	X		
Division-Year FE				X		
No South					X	X

Note: The table displays results based on equation (1) using 1940 Full Count Census data. The sample is limited to men who are between the ages of 25 to 54 in the 1940 census and can be matched to the 1930 census. In Panel A the sample is further restricted to those who immigrated after age 15. In Panel B the additional restriction is instead that the men must have immigrated before age 7. Treatment is assigned based on the state of residence in 1930 and fixed effects for these states are also included. Demographic controls are indicator variables for the region of birth, while policy controls are the number of years the cell has been exposed to school-age laws and a Bartik measure of exposure to the Great Depression. Standard errors clustered at the 1930 state of residence and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4-The Effect of Tenure Systems in 1960 to 2000

	(1)	(2)	(3)	(4)
Panel A. Male Wage Income				
At 12 Years	543.83	119.45	-65.33	-230.75
	(512.56)	(284.27)	(246.76)	(265.47)
Percent effect at 12 years	1.42	0.31	-0.17	-0.60
Panel B. Female Wage Income				
At 12 Years	123.47	308.27*	300.01*	77.18
	(231.14)	(164.57)	(167.18)	(176.57)
Percent effect at 12 years	0.73	1.82	1.77	0.45
Panel C. Male Years of Education				
At 12 Years	0.07	0.05	0.03	0.04
	(0.06)	(0.04)	(0.04)	(0.04)
Percent effect at 12 years	0.61	0.40	0.27	0.31
Panel D. Female Years of Education				
At 12 Years	0.08	0.06	0.04	0.02
	(0.05)	(0.04)	(0.04)	(0.04)
Percent effect at 12 years	0.65	0.49	0.30	0.19
Panel E. Male Employment				
At 12 Years	0.02**	0.02**	0.02	0.02
	(0.01)	(0.01)	(0.00)	(0.00)
Percent effect at 12 years	2.19	2.10	2.07	2.17
Panel F. Female Employment				
At 12 Years	0.00	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.00)
Percent effect at 12 years	0.76	1.29	1.22	0.68
Region-Year FE		X	X	
Demographic and Policy Controls			X	X
Division-Year FE				X

Note: The table displays results based on equation (2) using 1960-2000 Census data. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. Regressions are based on 7200 state-cohort-year observations. The weights are based on cell size and are normalized so that each decade has equal weight. Demographic controls consist of the fraction of each state-cohort-gender cell that is Black, Hispanic, Other Race. The policy variables control for exposure to mandatory school-age laws, the EITC, court and legislation mandated school finance reforms, food stamps and duty-to-bargain laws. Standard errors clustered at the birth state and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5-The Effect of Tenure Systems on Educational Attainment of Black Men

	Years of Education			
	(1)	(2)	(3)	(4)
Panel A. 1990				
At 12 Years	-0.45***	-0.41***	-0.37***	-0.27***
	(0.11)	(0.12)	(0.13)	(0.10)
Percent effect at 12 years	-3.67	-3.29	-3.00	-2.16
Panel B. 2000				
At 12 Years	-0.29***	-0.24***	-0.29***	-0.25***
	(0.05)	(0.05)	(0.05)	(0.09)
Percent effect at 12 years	-2.29	-1.90	-2.30	-1.99
Region-Year FE		X	X	
Policy Controls			X	X
Division-Year FE				X

Note: The table displays results based on equation (1) using 1990 and 2000 Census data. The sample is limited to black men who are born in the continental United States and are between the ages 25 to 54. Regressions are based on 2800 state-cohort-year observations and are weighted based on cell size. The policy variables control for exposure to mandatory school-age laws, the EITC, court and legislation mandated school finance reforms, food stamps and duty-to-bargain laws. Standard errors clustered at the birth state and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6-The Effect of Early Tenure Laws on Teachers

	(1)	(2)	(3)
Panel A. Retention (1910-1940)			
At -1 to -10 Years	0.002	0.008	0.009
	(0.016)	(0.014)	(0.018)
At 0 to 9 Years	0.026***	0.023**	0.014
	(0.008)	(0.011)	(0.011)
At 10 to 19 Years	0.051**	0.044*	0.031
	(0.020)	(0.022)	(0.022)
Observations	192,271	192,271	192,271
R-squared	0.155	0.155	0.157
Dep. Var. Mean	0.263	0.263	0.263
Panel B. Job Attachment (1910-1940)			
At -1 to -10 Years	0.013	-0.014	-0.013
	(0.028)	(0.023)	(0.022)
At 0 to 9 Years	0.032*	0.017	0.022**
	(0.019)	(0.013)	(0.010)
At 10 to 19 Years	0.045***	0.023	0.032**
	(0.013)	(0.015)	(0.013)
Observations	208,231	208,231	208,231
R-squared	0.048	0.049	0.049
Dep. Var. Mean	0.385	0.385	0.385
Panel C. Married & Female (1900-1940)			
At -1 to -10 Years	-0.001	-0.004	-0.002
	(0.008)	(0.007)	(0.007)
At 0 to 9 Years	0.010	0.011	0.007
	(0.010)	(0.008)	(0.008)
At 10 to 19 Years	0.015	0.033***	0.026**
	(0.011)	(0.011)	(0.010)
Observations	3,661,979	3,661,979	3,661,979
R-squared	0.059	0.061	0.062
Dep. Var. Mean	0.101	0.101	0.101
Region-Year FE		X	
Division-Year FE			X

Note: The table displays results for teachers based on equation (4). Panel A and B use linked Census data and consequently limit the sample to men. In Panel A the sample is further limited to individuals who were a teacher in the previous decade. In Panel B the sample is limited to those who are presently a teacher. Panel C uses Full Count Census data for both male and female teachers in years 1900 to 1940. Standard errors clustered at the state and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7-The Effect of Tenure Laws on Teacher Retention, 1962 to 1990

	Techer is Retained		Worker is Retained
	(1)	(2)	(3)
Panel A. Balanced Panel			
At 0 to 4 Years	-0.009	0.002	0.000
	(0.019)	(0.018)	(0.014)
At 5 to 9 Years	0.004	0.017	0.001
	(0.014)	(0.014)	(0.011)
At 10 to 14 Years	0.004	0.009	0.022**
	(0.019)	(0.020)	(0.010)
Observations	51,923	51,923	306,364
R-squared	0.017	0.019	0.133
Panel B. Unbalanced Panel			
At 0 to 4 Years	-0.006	0.004	0.004
	(0.017)	(0.019)	(0.015)
At 5 to 9 Years	-0.042	-0.028	-0.050**
	(0.027)	(0.022)	(0.018)
At 10 to 14 Years	-0.007	-0.006	-0.001
	(0.016)	(0.019)	(0.010)
Observations	51,919	51,919	307,963
R-squared	0.015	0.017	0.015
Region-Year FE		X	
Triple-Differences			X

Note: The table displays results for teachers based on a modified version of equation (4) using 1962-1990 CPS data. In columns (1) and (2) the sample is limited to those who reported a teaching occupation in the previous year. In column (3) the sample is limited to those who reported a teaching or a public sector occupation in the previous year. The Balanced Panel specification (Panel A), pools states into consistent state-groups for all years. The unbalanced panel specification allows for the cross-sectional unit to vary over time. For triple-differences public sector workers serve as the within-state control group. The outcome variable in the triple-differences specification is an indicator variable (Worker is Retained). For teachers, it is equal to one if they retained a teaching occupation this calendar year. For public sector workers, it is equal to one if they retained a public sector position this calendar year. Standard errors clustered at the state-group and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8-The Effect of Tenure Laws on Teacher Race, 1962 to 1990

	Teacher is Black		
	(1)	(2)	(3)
Panel A. Balanced Panel			
At 0 to 4 Years	-0.022	-0.022	-0.020
	(0.029)	(0.027)	(0.000)
At 5 to 9 Years	-0.042**	-0.043**	-0.029
	(0.021)	(0.021)	(0.000)
At 10 to 14 Years	-0.035	-0.035	-0.032
	(0.026)	(0.026)	(0.000)
Observations	49,378	49,378	273,452
R-squared	0.095	0.098	0.082
Panel B. Unbalanced Panel			
At 0 to 4 Years	-0.021	-0.023	-0.039
	(0.030)	(0.023)	(0.026)
At 5 to 9 Years	-0.076***	-0.083***	-0.074***
	(0.020)	(0.017)	(0.019)
At 10 to 14 Years	-0.027	-0.031*	-0.052*
	(0.021)	(0.015)	(0.025)
Observations	49,372	49,372	273,424
R-squared	0.081	0.083	0.070
Region-Year FE		X	
Triple-Differences			X

Note: The table displays results for teachers based on a modified version of equation (4) using 1962-1990 CPS data. In columns (1) and (2) the sample is limited to those who report a teaching. In column (3) the sample is limited to those who reported a teaching or a public sector occupation. The Balanced Panel specification (Panel A), pools states into consistent state-groups for all years. The unbalanced panel specification allows for the cross-sectional unit to vary over time. For triple-differences public sector workers serve as the within-state control group. Standard errors clustered at the state-group and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX TABLES

Table A-1 Summary Statistics of Key Variables

	Men		Women	
	Mean	Std. Dev.	Mean	Std. Dev.
Panel A. 1940				
Wage Income	14519.771	3568.538	7640.798	2355.259
Years of Education	8.658	1.145	8.961	1.063
Employed	0.878	0.026	0.246	0.062
Any Tenure Treatment	0.212	0.409	0.229	0.420
Years Treatment | Treated	8.892	5.190	9.487	5.326
Panel B. 1960				
Wage Income	29917.747	5424.463	8630.269	2366.103
Years of Education	10.464	1.263	10.515	0.999
Employed	0.925	0.026	0.391	0.066
Any Tenure Treatment	0.233	0.423	0.237	0.425
Years Treatment | Treated	8.420	5.496	8.780	5.561
Panel C. 1970				
Wage Income	39993.826	6744.185	12533.206	2558.448
Years of Education	11.621	1.091	11.388	0.869
Employed	0.931	0.031	0.467	0.066
Any Tenure Treatment	0.343	0.475	0.336	0.472
Years Treatment | Treated	11.755	7.018	12.063	7.136
Panel D. 1980				
Wage Income	37634.149	7237.294	15637.914	1337.884
Years of Education	12.720	0.923	12.387	0.760
Employed	0.896	0.033	0.604	0.047
Any Tenure Treatment	0.161	0.368	0.168	0.374
Years Treatment | Treated	10.341	7.126	10.450	7.166
Panel E. 1990				
Wage Income	40409.158	9512.819	21186.467	3058.729
Years of Education	13.221	0.546	13.092	0.475
Employed	0.893	0.028	0.721	0.046
Any Tenure Treatment	0.212	0.409	0.216	0.411
Years Treatment | Treated	9.463	6.134	9.457	6.164
Panel F. 2000				
Wage Income	44309.560	9718.471	25468.170	3996.619
Years of Education	13.385	0.391	13.415	0.335
Employed	0.876	0.031	0.747	0.039
Any Tenure Treatment	0.204	0.403	0.208	0.406
Years Treatment | Treated	11.971	6.839	11.922	6.862

Note: Census data from 1940, 1960-2000 for 25–54-year-old individuals. Tabulations are weighted by the number of individual observations that are used to calculate the averages in each state-cohort-year-gender cell. Years Treatment | Treated refers to the number of years of tenure treatment that the average individual with any tenure treatment received.

Table A-2 Teacher Tenure Variation in Each Census Decade

State	Census Decade	State	Census Decade
Alabama	1960, 1970	Nebraska	1960*, 1970*, 1990†, 2000†
Arizona	1970, 1980	Nevada	1980, 1990, 2000
Arkansas	1990, 2000	New Hampshire	1970, 1980, 1990
California	1940,	New Jersey	1940
Colorado	1940*, 1980†, 1990†, 2000†	Nex Mexico	1960, 1970, 1980
Connecticut	1940*, 1960*, 1970†, 1980†, 1990†	New York	1940
Delaware	1970, 1980, 1990	North Carolina	1990, 2000
Florida	1960*, 1970†, 1980†	North Dakota	1980, 1990, 2000
Georgia	1960*, 1970*, 1990†, 2000†	Ohio	1960, 1970
Idaho	1970, 1980, 1990	Oklahoma	1980, 1990, 2000
Illinois	1940*, 1960†, 1970†	Oregon	1940*, 1990†, 2000†
Indiana	1940, 1960	Pennsylvania	1960, 1970
Iowa	1960, 1970, 1980	Rhode Island	1960, 1970, 1980
Kansas	1960*, 1970*, 1990†, 2000†	South Carolina	1960*, 1970*, 1990†, 2000†
Kentucky	1960, 1970	South Dakota	1990, 2000
Louisiana	1940*, 1960†, 1970†	Tennessee	1960*, 1970†, 1980†
Maine	1980, 1990	Texas	
Maryland	1940	Utah	1990, 2000
Massachusetts	1940	Vermont	1990, 2000
Michigan	1980, 1990	Virginia	1980, 1990, 2000
Minnesota	1940*, 1960*, 1970†, 1980†	Washington	1970, 1980, 1990
Mississippi	1990, 2000	West Virginia	1960, 1970
Missouri	1960*, 1970*, 1990†, 2000†	Wisconsin	1940*
Montana	1940	Wyoming	1980, 1990, 2000

Note: Not all state laws contribute to tenure variation in every Census decade. In order to be included a given decade, the tenure system must have a minimum exposure value of minus one and a maximum exposure value that is equal to or greater than five. Remember that exposure is calculated as the year an individual turns 18 minus the year a tenure system is passed. *local law, †statewide expansion of local law, entries with no superscript are statewide laws and blank entries have no tenure variation.