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

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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 on students is unclear. In this paper, I study how students' exposure to tenure systems affects their long-run outcomes. As contemporary variation in tenure regimes is limited, I go back to the 20th Century. This was the period where most states implemented tenure; in 1910, only one state had a tenure regime, but by 1977 every state had a tenure law. I then exploit the passage of tenure laws between 1910-1977 in a difference-in-differences design. Event study models reveal that tenure systems negatively impact student outcomes, but only when they improve teacher job security. Tenure laws that do not increase teacher job security do not affect students. I present evidence that these heterogeneous effects are due to the tightness of teacher labor markets. In a teacher surplus, tenure laws increase teacher job security but negatively impact students. In a teacher shortage, tenure laws do not affect teachers or students.

1 Introduction

Economists have long worried that job protection for workers lowers productivity (e.g., Besley and Burgess 2004; Autor et al., 2007; Aghion et al., 2008; Bassanini et al., 2009; Bjuggren, 2018). One prominent example of employment protection is tenure rights for K-12 schoolteachers, which is now ubiquitous in the US (National Council on Teacher Quality, 2017). The 20th Century started with no states having tenure rights. Tenure rights were then steadily adopted, state by state, over the century. By 1977, all US states had some form of a tenure law for K-12 teachers. As a result, more children today attend a K-12 school with tenured teachers than did their parents.

Today the 20th Century decision to implement tenure regimes is controversial because it is unclear how tenure affects students. The original purpose of tenure was to improve school quality by protecting teachers from dismissals unrelated to productivity (National Education Association, 1924). With tenure, teachers can only be removed for valid reasons (i.e., poor performance) verifiable by a third party. Reducing arbitrary dismissals makes it cheaper to fill teaching vacancies (Rothstein, 2015) and encourages workers to invest in occupation-specific human capital (Wasmer, 2006). However, tenure laws prevent performance-related dismissals because teachers' output is hard to verify. Consequently, tenure can also lower school quality by preventing the removal of low-performing teachers (Hanushek, 2015) and diminish incentives to provide effort on the job (Prendergast, 1999).

In this paper, I present causal evidence on the two central questions relating to tenure – do tenure regimes increase teacher job security, and if they do so, then how do they impact students? Prior research on teacher tenure faced a key challenge; there is limited contemporary variation in tenure systems. By the 21st Century, most states have tenure in place. Therefore, I exploit the 68-year rollout of tenure laws across states over the 20th Century, the period during which every state implemented some form of a tenure regime. These state laws represent a unique opportunity to estimate the effect of an entire tenure regime. Relying

on primary sources, I code all implementations of tenure regimes by state legislatures from 1910-1977.[1] Using a cross-cohort difference-in-differences study design, I then study how tenure regimes affect long-run student outcomes, such as wages.

My main findings indicate that tenure laws that increase teacher job security also diminish long-run student outcomes. However, most tenure laws have no measurable impact on job security and consequently do not affect students. I present suggestive evidence that this is due to teacher shortages – dismissing a teacher is already costly, and tenure laws only marginally change job protections. These results have important policy implications as, presently, there is a teacher shortage (García and Weiss, 2019; Liu, 2020; Walker, 2021). Therefore, before removing or eliminating tenure, it is first necessary to make teaching a more attractive occupation.

The empirical estimates I provide are important as the theoretical impact of tenure regimes on productivity is ambiguous and rests on two closely related questions. One question is a first-stage debate on whether tenure laws are binding. Specifically, do tenure job protections lower teacher dismissal rates and improve retention? Tenured teachers should be difficult to remove; schools can only fire them for a set of valid reasons that they have to prove to a third party. Survey evidence of both teachers and school administrators also suggests that it is hard to fire a tenured teacher (Farkas, 2003; Thomas A. Kersten, 2006; Weisberg et al., 2009). The survey evidence also aligns with case studies. For example, at one point in New York City, dismissals took between two to five years, cost up to $400,000, and had the potential to end in compromise (Brill, 2009). Yet, the impact of tenure protections is difficult to see in the national data – depending on the question and survey year, forced exits for tenured teachers can be either lower, similar, or higher vis a vis non-tenured teachers (National Center for Education Statistics, 2004, 2008, 2012). The fact that strong job protections don't affect dismissal rates is a puzzle. A potential explanation is that fired teachers are difficult to replace in a tight labor market, and dismissal rates are low even without job

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[1] Variation is limited after 1977, as all but two states have a statewide tenure system. The remaining two states are Wisconsin and Texas. Wisconsin implemented a tenure law in Milwaukee. Texas passed a voluntary tenure law that allows school districts to implement a tenure regime if they choose to do so.

protections. Therefore, tenured teachers are difficult to fire (as observed in case studies and surveys), but tight labor markets ensure that few such dismissals are initiated in the first place (as observed in national data).

The second debate on tenure regimes concerns their impact on students if they bind and improve teacher job security. The answer depends on the principal's dismissal function and the relative strength of several different partial effects. Perhaps the most important of these potential mechanisms is the effect of tenure on the composition of teachers, which is *a priori* ambiguous. On the one hand, if principals only fire teachers for poor performance, then constraining dismissals can reduce teacher quality and lower student outcomes. Teacher quality can fall if job protections attract lower-quality candidates (Levine, 1991) or prevent dismissals of low-performing teachers (Rothstein, 2015).[2] On the other hand, the above assumption that principals only dismiss low-performing teachers is not guaranteed to hold empirically. In the past, principals would dismiss female teachers for marriage (Murphy, 1990) and to open vacancies for political allies (National Education Association, 1924). Contemporary examples of unjust dismissals include school districts dismissing a teacher for wearing a Black Lives Matter mask (Elassar, 2020) or teaching critical race theory (Green, 2021). Tenure policies also can protect teachers from such arbitrary dismissals. As these cases are not related to productivity, preventing them does not lower teacher quality. In fact, quality might improve as decreased risk of arbitrary dismissals makes the occupation more attractive.[3]

The theoretical ambiguity of tenure laws has not been answered empirically. Finding the necessary policy variation is the key challenge as most states have tenure regimes in place.[4] Prior work has attempted to solve this problem through two approaches. One set of papers exploits incremental variation within an

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[2] Job protections also can cause firms to hire less risky workers (Lazear, 1995) or to raise hiring standards (Pries and Rogerson, 2005).

[3] The effect of job protections on effort, holding teacher composition, fixed is also ambiguous. For example, job protections incentivize workers to invest in occupation-specific human capital (National Education Association, 1924; Wasmer, 2006). However, a weaker relationship between effort on a productive task and dismissal can encourage shirking, see Prendergast (1999) for a review on the employee incentives literature.

[4] Only one state (Kansas) completely eliminated tenure, while three others (Florida, Louisiana and North Carolina) committed to a slow phase out (Kraft et al., 2021), the impact of which will fully materialize far into the future.

existing tenure regime, finding mixed effects. The most pertinent papers to mine exploit policies that reduce the fraction of teachers with job protections across schools in Chicago (Jacob, 2013) and Florida (Carruthers et al., 2018).[5] The effects these papers estimate can differ from the impact of an entire tenure system for many reasons. First, outcomes are observed only two to three years after policy implementation. Yet, we expect tenure to change the composition of teachers and for this change to take decades to materialize (Hess, 2010; Rothstein, 2015). Second, these studies present effects on test scores that the authors interpret with caution. For example, in Chicago (Jacob, 2013), removing teacher job protections lead to large test score gains, but the point estimates and significance vary with model specifications and samples.[6]

Another set of papers study optimal dismissal rules using structural models (i.e., Rothstein, 2015). These papers relate to tenure policies because job protections make firing teachers more difficult and prevent optimal dismissal rates. However, the models are difficult to estimate empirically due to the lack of observed variation in teacher contracts. The papers also estimate optimal dismissal rules where principals fire teachers only due to poor productivity. Yet, the reason for tenure regimes is that principals were dismissing teachers suboptimally for reasons unrelated to quality (National Education Association, 1924). Due to these arbitrary dismissals, the empirical effect of tenure laws can differ from model estimates. The difference will depend on the principal's dismissal function and the correlation between productivity and the factors that determine dismissal. In my paper, I empirically estimate the effect of tenure regimes, where the observed impacts are due to constraining firing rules that principals implement in practice.

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[5] Other papers identify partial effects of tenure regimes. Some papers study short-run changes in teacher behavior and productivity around tenure receipt, finding mixed effects (Phillips, 2009; Jones, 2015; Goldhaber et al., 2016; Roberts, 2018; Ng, 2021). There is also limited causal work on how tenure can affect the supply of teachers. Kraft et al. (2021) study how teacher accountability reforms affect the supply of new teaching graduates. These reforms include increases in the length of the probationary period and the removal of tenure for new teachers. They find that weakening tenure reduces the number of new teaching graduates and increases vacancies. The literature on the impact of unions is also pertinent (Baron, 2018; Roth, 2019; Lovenheim and Willén, 2019; Anderson et al., 2021), even though unions do not bargain over tenure laws (Hess and loop, 2008).

[6] In Florida (Carruthers et al., 2018), removing tenure protections for new teachers leads to trivial test score increases on average. However, the cross-school policy variation the authors exploit is small, resulting in limited treatment intensity. With limited treatment intensity, small effects are to be expected. Also, several models have divergent pre-trends. The strongest result is that removing tenure leads to a small, short-run increase in test scores of disadvantaged students. In Chicago (Jacob, 2013), while the test score results are interpreted with caution, there is robust evidence that removing employment protections decreases absences.

To study how tenure affects long-run student outcomes, I collect data on the implementation of tenure laws between 1910-1977 and link it with adult outcomes measured in the 1940 to 2000 Decennial Censuses. These are large datasets, with tens of millions of observations providing substantial statistical power. My empirical strategy is a cross-cohort event study model as in Jackson et al. (2016), Lovenheim and Willén (2019), and Rothstein and Schanzenbach (2021). In the model, exposure to tenure systems varies with the potential amount of time an individual spent in the K-12 system with tenure. The variation comes from the differential timing of tenure implementation across states and differences in when the cohort was born relative to when tenure was implemented. Intuitively, I test if treated cohorts (those young enough to have attended K-12 under a tenure regime) have different adult outcomes than untreated cohorts (those too old to be in the K-12 school under a tenure regime).

I find that tenure laws implemented in the early 20th Century lowered men's and women's wage income. For cohorts with 12 years of tenure law exposure, almost the entirety of K-12, wages fall by 7.7 percent for men and 8.2 percent for women. Furthermore, I present evidence that these early tenure laws increased measures of job security, such as teacher retention. Therefore, the first tenure regimes increased teacher job security but led to wage declines for the treated students.

In contrast, later implementations of tenure regimes did not affect long-run student outcomes. The 95 percent confidence interval allows me to rule out a negative wage effect larger than one percent. Consistent with no effects on students, I also find that these tenure laws did not improve teacher job security. There is no significant effect on teacher retention and other teacher outcomes that tenure laws can potentially affect. Therefore, later tenure regimes did not increase teacher job security and did not measurably affect students.

The above analysis demonstrates that whether tenure laws increase teacher job security is an important factor for predicting their impact on student outcomes. However, what factors determine whether tenure laws affect teacher job security? I provide suggestive evidence that teacher labor market conditions are an important determinant. If teachers are scarce, dismissing a teacher is costly because they are difficult

to replace. Therefore, principals dismiss few teachers even without tenure laws. The implementation of tenure leads to only a marginal increase in teacher job security; teachers already enjoy strong protections due to market forces.

To identify teacher labor market conditions in the 20th Century, I exploit that census enumerators asked non-employed individuals to report their previous occupation; thereby, I observe a teacher-specific unemployment rate. This teacher unemployment rate is my measure of teacher labor market tightness. When the unemployment rate is high, there is a teacher surplus, and there is a teacher shortage when it is low. Using this measure, I split the sample of states by whether they have a teacher surplus or a relative teacher shortage. In a teacher shortage, tenure regimes do not increase teacher job security or affect students. However, when there is a large surplus of teachers, tenure regimes increase teacher retention and lower the wage income of treated students.

From a policy perspective, my paper highlights the limits of tenure reforms when there is a teacher shortage. Presently, many schools are struggling to attract and retain teachers, perhaps due to low wages (Temin, 2002; Allegretto and Mishel, 2020), poor working conditions (McCreight, 2000; Geiger and Pivovarova, 2018), and the pandemic (Walker, 2021). These shortages are particularly pronounced in high-demand fields such as STEM (Hansen et al., 2019) and in districts that serve disadvantaged students (García and Weiss, 2019; Liu, 2020). Therefore, more must be done to attract and retain high-quality teachers before reforming or eliminating tenure.

The remainder of this paper is organized as follows. Section 2 describes tenure regimes, the debate around them, and past research in more detail. Sections 3 and 4 go over the data and the empirical model in more detail. In particular, I describe my implementation of an adjustment from Cengiz et al. (2019) to correct for heterogeneous treatment effects in difference-in-difference models. The main student outcomes are presented in Section 5, and several detailed robustness tests are conducted in Section 6. Finally, Section 7 covers the mechanisms, and Section 8 concludes.

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 concerned that many of the dismissals were not related to job performance. Instead, too many teachers were being removed for personal or political reasons. For example, elected members of the school board or superintendents 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 strengthening teacher job protections and implementing teacher tenure laws.

I define a teacher tenure law as a state statute enacted that specifies that a teacher can only be dismissed for just cause and with 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 immoral and immodest behavior. Due process protections consisted of four factors. First, the reason for dismissal must have been made available to the teacher. Second, the teacher must have the opportunity to appeal. Third, a strict timetable had to be followed. Fourth, 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 exclude laws that grant teachers some employment protections but do not meet the above tenure definition. There are three types of such legislation. First, certain states gave teachers strong dismissal protections 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, when 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).[7]

Furthermore, 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 statutes 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 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 a later law to extend that protection to cover the whole state.

Finally, I limit my focus to the lower 48 states. This drops Alaska, District of Columbia (D.C.), and Hawaii. Removing Alaska and Hawaii ensures that I study a consistent group of states in my sample, which spans most of the 20th Century. I remove D.C. as information is difficult to find in many primary sources. The most likely reason for the difficulty, is that, depending on the year, legislation is implemented either bythe federal government or the city council, not by a state legislature.

The resulting variation consists of 47 states passing 64 tenure laws between 1910 and 1977. The exact year of implementation is shown in Table 1.[8] I also present two figures to highlight a few important aspects of the policy variation. The time-series variation in tenure laws is shown in Figure 1. The legislative activity occurs in two distinct periods. In the early period, states implement tenure systems between 1910

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[7] 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).
[8] As best as possible, I code when the tenure law became effective, not when it was first passed by the state legislature. In practice, enactment date and the effective date are at most a year apart and the distinction is trivial in practice.

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 continued to do so at a steady pace until 1977. The cross-sectional variation of tenure regime is available in Figure 2 for two critical periods. Panel A highlights the states that implement tenure regimes between 1910-1922, while Panel B shows tenure variation between 1964-1977.

2.2   Potential Mechanisms

Tenure protections operate by increasing the cost of teacher dismissal as well as by reducing the probability that a dismissal will occur if proceedings are initiated. In turn, these job protections can affect students by: (1) changing within teacher productivity and (2) altering the composition of teachers. Below I will briefly describe each of the mechanisms.

Tenure job protections can affect teacher productivity by changing teacher effort on productive tasks. If we assume that teachers exert effort in order to avoid dismissals, then the effect of tenure is ambiguous and depends on the principals dismissal function and the operation of the tenure regime. On the one hand, if principals dismiss teachers for low productivity and tenure regimes make such dismissal more difficult, then the return to effort is reduced, effort decreases, and productivity falls. On the other hand, consider a scenario where principals dismiss teachers for measures unrelated to productivity and teachers can exert effort on these unrelated measures. If a tenure regime only prevents such dismissals, then the returns to the unproductive task fall to zero and teachers will allocate more effort to the productive task.

Tenure policies can also affect student outcomes by altering the composition of teachers and, like teacher effort, the effect on students is ambiguous. The fixed component of teacher quality can decline either because low-performing teachers are more difficult to dismiss (Rothstein, 2015), or job protections induce negative selection by attracting workers who are more likely to shirk (Levine, 1991). Alternatively, composition of teachers can improve by increasing the compensating differential. If workers are risk averse and there is an idiosyncratic component to dismissals, then tenure protections reduce risk and make the

occupation more attractive. There is empirical evidence that changing the relative teaching compensation and job security package generates both positive and negative selection effects. Nagler et al. (2020) find that during a period of high private sector unemployment, when teaching is a relatively more attractive occupation, the quality of new hires improves. So, tenure policies that increase the value of teaching can lead to higher-quality teachers. Alternatively, Kraft et al. (2020) study teacher accountability reforms, like increasing the probationary period before teachers acquire tenure. They find that the reforms potentially improved teacher quality by reducing the number of new teachers from nonselective undergraduate colleges. Therefore, there is some evidence that potentially less qualified candidates more value increased job security.

There are several other channels through which tenure operates. First, as job protections make teaching a more attractive occupation, principals can lower teacher wages and hold the value of the position fixed. The resulting savings can be invested in other educational inputs, such as lower class sizes (Rothstein, 2015). So, tenure policies can affect other school inputs, not only teachers. Second, tenure can affect the productivity of non-tenured teachers because tenure systems follow up-or-out rules in that teachers who are not granted tenure are dismissed. Certain theoretical papers argue that such up-or-our rules exist in part to ensure higher investment in human capital during the probationary period (Gilson and Mnookin, 1989; Kahn and Huberman, 1988). Increasing effort among non-probationary teachers is potentially a non-trivial effect as this group is large. For example, non-tenure teachers make up a sizable portion of teachers in Chicago and are the majority in certain schools (Jacob, 2013). Third, tenure policies can reduce productivity by preventing dismissals of effective classroom, but who nonetheless are disruptive to a proper function organization due insubordination or fraud (Martin, 2009). In a school context, disruptive or uncooperative teachers can harm school culture, and prior work finds that school culture is an important factor in predicting student performance (Lunch et al., 2012; Thapa et al., 2013).

2.3   Prior Research on Teacher Tenure

Prior empirical research on teacher tenure attempted to solve the lack of contemporary variation by exploiting incremental changes within existing tenure regimes. Below I will argue that the available policy variation is suited for identifying a partial effect of tenure, namely short-run changes in teacher effort. However, short-run changes in teacher effort is not the only mechanism through which tenure operates. There are at least two other partial effects the literature has not been able to identify causally; changes in teacher composition and long-run changes in teacher human capital investments.

Prior research has taken two approaches to studying teacher tenure. One set of papers exploits policies that change the fraction of teachers with job protections across schools in Chicago (Jacob, 2013) and Florida (Carruthers et al., 2018). These designs tend to find that job protections negatively affect test scores but have two shortcomings. First, the papers observe student outcomes only two to three years after policy implantation. Yet, we expect tenure to change the composition of teachers, which can take more time to materialize (Hess, 2010). Therefore, the observed effects are likely due to changes in short-run effort (Rothstein, 2015), but not changes in teacher composition or human capital investments. Second, the authors interpret their estimates with caution because they vary across samples and model specifications (Jacob, 2013) or are potentially endogenous due to divergent pre-trends (Carruthers et al., 2018).

The second approach taken by prior researchers exploits within-teacher variation in job protections using several different sources of variation. One source of variation is differences in the probationary period across or within states to identify the productivity effects around tenure receipt (Goldhaber et al., 2016; Jones, 2015; Phillips, 2009; Roberts, 2018; Ng, 2021).[9] Alternatively, state teacher evaluation policies generate cutoffs around which job protections for teachers vary for one year (Dee and Wyckoff, 2015; Dee et al., 2021; Ng, 2021). The within-teacher aspect of these designs ensures that they identify incentive

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[9] These papers are difference-in-differences designs where teachers with the same years fo experience can be compared with and without tenure. Technically, the literature identifies effects that is from two channels; within teacher changes in productivity and a selection effect that is due to screening of low-quality teachers during the probationary period. The papers either include controls for teacher characteristics or teacher fixed effects to partial such variation out and to identify the within teacher productivity effect.

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effects, but not selection effects. Furthermore, in all these papers the observed effects are due to short-run changes in effort, as in most designs differences in job protections vary for only a year. The resulting changes in teacher productivity might not be indictive of effects due to longer exposure to job protections – consider models where workers' output is determined by investments in human capital (Becker, 1964; Ben-Porath, 1967; Heckman, 1976). Tenure protections can also affect these human capital investments, but the stock of accumulated human capital changes slowly. Therefore, the short-run within-teacher estimates can differ from the long-run within-teacher effects.

My contribution is that I estimate the effect of an entire tenure system. As previously discussed, measuring the effect of a tenure regime from partial effects is difficult because the available variation is not suited to identify the impact of changing teacher composition or human capital investments. In contrast to the previous literature, I forgo estimating all the relevant partial effects directly. Instead, I identify a reduced form effect of the state tenure regime through a difference in differences design. Importantly, I estimate effects up to 19 years after tenure implementation, allowing for a substantial amount of time for changes to teacher composition, school inputs, and within teacher productivity to occur.

In addition to the empirical literature on tenure, my paper related to the empirical literature on teacher unions. I will start with the debate on the impact of teacher unions (Baron, 2018; Anderson et al. 2019; Lovenheim and Willén, 2019; Roth, 2019). My paper differs from the union literature in that I study a policy that only affects teacher job protections. In contrast, teacher unions bargain over a much larger set of educational inputs, such as teacher compensation and working conditions. Furthermore, analysis of Collective Bargaining Agreements (CBAs) find that unions do not bargain for employment protections, instead the CBA dismissal procedures often defer to the state tenure law (Hess and Loop, 2008).[10] Therefore, the union literature estimates an effect that is due to a different policy than teacher tenure. A contribution

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[10] Unions can affect job protections through other channels. For example, the Chicago teacher's union bargained for strong job protections for non-tenured teachers (Jacob, 2013).

of my study is to estimate the effects of teacher job protections separately from unions or collective bargaining.

My paper also contributes to the literature on optimal teacher dismissal rules (e.g., Rothstein, 2015).[11] The goal of these papers is to identify the optimal fraction of low-performing teachers that the principal should dismiss in a given year. These optimal dismissal policies relate to teacher tenure because tenure job protections make high dismissal rates difficult. However, they differ in that they only model performance-related dismissals that are due to low productivity. An important aspect of the tenure debate is if principals are dismissing teachers only for poor performance. Therefore, my paper differs in that I estimate the effect of tenure policies due to contrasting dismissal rules that principals implement.

3 Data

When looking at student outcomes I use Decennial Census IPUMS data (Ruggles et al., 2021) for 1940-2000. The data start 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 exposure to tenure systems becomes sparse. 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. Limiting the sample to those born in the US is necessary, 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 by birth-state and birth-year, I collapse the data to the birth-state, birth-year, and calendar-year levels.

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[11] Also, see Staiger and Rockoff (2010), Boyd et al. (2011), and Winters and Cowen, (2013).

Besides the main public data, I use additional information to explore the 1940 results in more detail. As previously discussed, early tenure laws lead to lower wage income in 1940. So, to obtain 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 balance 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 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. The finer geographic data allows me to observe the earliest county of residence during school ages (6-18). For the linked census sample, I collapse the data to calendar-year, birth-year, and earliest county of residence during school ages.[12]

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 contains multiple important variables, I focus on wage income (including zeroes) and years of education. These two variables are important outcomes and are highly correlated with other outcomes of

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[12] The linked student outcome data is only available for 1940, therefore the only available calendar year is 1940.

interest. 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

4.1 Cross-Cohort Event Study Model

To measure the impact of tenure exposure on student adult outcomes I estimate a cross-cohort event study model. In this model, treatment variation comes from two sources. First, there is differential timing of tenure implementation across states. Second, within a tenure state, cohorts are exposed to the regime for varying amounts of time. Cohorts that are younger when tenure is implemented will spend more time in a K-12 school system with a tenure regime and have more exposure. In contrast, cohorts that are older when tenure is implemented will have relatively less exposure. In fact, they might no longer be in the K-12 school system when a tenure law is implemented and have no exposure at all. These cross-cohort models have been frequently used to estimate the impact of childhood interventions on adult outcomes (e.g. Jackson et al., 2016; Lovenheim and Willén, 2019; Rothstein and Schanzenbach, 2021).

I deviate from prior literature in the exact specification because recent work has highlighted concerns with in such difference-in-differences models that exploit treatments implemented at different times.[13] Specifically, bias arises when the model makes comparisons between switchers; which are states that change treatment status in the sample period. I implement an adjustment from Cengiz et al. (2019). For each treated state, I construct a dataset that consists of said state and states that did not change treatment status. I thus estimate the effects of tenure within each of the datasets using as controls only the states that did not switch treatment.

In my baseline specification I estimate the following equation separately for each Census decade:

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[13] This bias arises if treatment is staggered, and effects of tenure laws are heterogeneous across time. In my setting treatment is staggered as states implement tenure regimes at different times. Furthermore, as discussed in the introduction, the effects of tenure laws vary significantly across time.

$$Y_{scdt} = \sum_{\tau \in T} \pi_\tau I(C - t_0 + 18 = \tau)_{sct} \times x_{st} + \delta_{sd} + \gamma_{R(s),c,d} + \epsilon_{scdt}, \forall\, t \in [1940, 1950, \dots, 2000] \quad (1)$$

where the dependent variable, Y_{scdt}, is measured for individuals born in state s, belonging to cohort c,

belonging to dataset d and observed in census decade t. The key independent variable is exposure, which is

measured by $C - t_0 + 18$, 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. The π_τ coefficients allow me to non-parametrically

trace out the effect of tenure laws for different levels of exposure. I scale the π_τ coefficients by x_{st}, which

is the change in the fraction of state population covered by a tenure regime.[14]

The baseline model controls for birthplace-by-dataset fixed effects δ_{sd} and birth-year-by-census-

region-by-dataset fixed effects $\gamma_{R(s),c,d}$. 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. These fixed effects ensure

that that model has a difference-in-differences interpretation. The birth-year-by-census-region-by-dataset

fixed effects $\gamma_{R(s),c,d}$ deviate from the most parsimonious model. I include them in my main specification

to primarily to account for differential economic conditions in the US South vis-à-vis the US Northeast.

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.

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[14] This fraction is calculated based on the closest Census decade when a tenure regime is implemented. The fraction
can vary across census decades as a state can pass more than one tenure law.

Although in my baseline equation I estimate equation (1) separately for each decade, I also present results from a model where several decades are estimated jointly. In such a case, I stack the individual by decade event studies by allowing the fixed effects to vary across calendar years:

$$Y_{scdt} = \sum_{\tau \in T} \pi_\tau I(C - t_0 + 18 = \tau)_{sct} \times x_{st} + \delta_{sdt} + \gamma_{R(s),c,d,t} + \epsilon_{scdt} \quad (2)$$

The assumptions behind my empirical strategy are identical to a difference-in-differences 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.

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 and the fraction who have an immigrant father. Additionally, when using the 1940 linked data, I control for 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 Willén, 2019).[15]

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

Finally, as the Great Depression is of particular concern in the 1940 data, I construct a Bartik measure (Bartik, 1991) of economic conditions that affected each cohort during the worst Depression years.[16] The variable is created 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).

I also conduct a placebo exercise using linked 1940 census data. I know the exact date of migration to the US 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 US. However, those who immigrated later in life – and did not attend US schools, were not affected by tenure laws.

4.2   Intent to Treat Interpretation

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 dropped out early. Therefore, exposure is imperfectly assigned. Such exposure coding implies that the estimates have an intent-to-treat (ITT) interpretation. In the early Census years, high school graduation rates are low, and the ITT and treatment-on-the-treated (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.

Note that 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.

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[16] I construct a Bartik measure to avoid including an endogenous control in my model.

Figure 3 provides a visual representation of the ITT nature of the estimates. It shows the fraction of individuals who 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. 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 grade nine, and a minority, 32 percent, finished grade 12. Note that the initial effect of tenure regimes is estimated based on exposure during high school. Due to low high school attendance, these effects are likely to be understated.

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 who move out of their birth state before they turn 18 but who are coded as treated. Also, it is 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 grandfather experience and grant eligible teachers immediate tenure protections upon tenure law implementation.

4.3   Tenure Regime Variation

In each Census decade I estimate the impact of tenure regimes for which I observe the outcomes of cohorts before treatment and the outcomes of cohorts after treatment. Therefore, I do not estimate the impact of every tenure law in every decade. For example, I do not estimate the effect of a 1977 tenure law in the 1940 Census as I only observe pre-treatment variation. Conversely, I do not estimate the effect of a 1910 tenure law in 2000 as I only observe post-treatment variation.

The exact rule that I use to assign tenure laws to Census decades is the following; I must observe pre-treatment outcomes at least five years before a tenure regime and post-treatment outcomes for at least

ten years after a tenure law. This rule ensures that, for at least five years before treatment the observed parallel trends are not due to differential attrition of treated states from the sample. Furthermore, it ensures that for at least ten years after treatment the observed dynamics are also not due to sample attrition. States that implement a tenure regime and have some pre- and post-variation but do not meet my above rule are dropped from the sample for the Census decade. Following Cengiz et al. (2019), states that have only pre tenure regime variation or only post tenure regime variation in a given decade can act as controls in said decade. Their inclusion does not alter the assumptions for a difference-in-differences design to be valid; parallel trends must hold and there must be no contemporaneous differential shocks. The list of state tenure laws that are switches in each Census decade are available in Appendix Table 2.

5   Effects on Long-Run Outcomes

5.1   Long-Run Student Outcomes

Figure 4 summarizes the main results of the paper. There is a structural break in the effect of tenure regimes on wage income. Early tenure laws negatively affect men's and women's wage income in 1940 and 1950. However, by 1960 the negative effects disappear and, if anything, are slightly positive. Most tenure regimes do not affect wage income for either gender. Finally, tenure regimes have only modest effects on educational attainment. Most effects are positive, but extremely small in magnitude. The broad pattern of results is that most tenure laws do not affect long-run student outcomes, with the early tenure laws implemented between 1910-1937 being the sole exception. In Section 7, I will argue that the reason that only early laws affect student outcomes is because they are binding, while later tenure laws are not.

Before going into the mechanisms, I would like to explore the main results in more detail, show that event study assumptions hold and that the effects are likely to be causal. For brevity, I combine the separate estimate for each Census decade into two periods. In the first period I study the early tenure laws, implemented between 1910-1922 whose impact I measure in 1940. In the second period I study tenure regimes implemented between 1927-1977 whose impact I measure in 1960-2000 using the stacked event

study model. As these later tenure laws have the same effects (see Figure 4), such a pooling is appropriate. I do not focus on the impact of the 1927-1937 laws that I measure in 1950. The 1950 Census sample only measures wage income for 0.2 percent of the population, leading to noisy effects. Nevertheless, for completeness they are available in Appendix Figure 1 and Appendix Table 3. The point estimates are consistent with the 1940 results; tenure laws reduce adult wage income.

5.2   Wage Income in 1940

Figure 5 displays the impact of early tenure laws (1910-1922) on wage income for men (Panel A) and women (Panel B). Note that income is inflated to 2000 dollars. Event study estimates are plotted for cohorts expected to exit K-12 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 tenure law passage. This omitted period (-1) 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.

For all variables and genders, the estimated effects for years -11 to -2 are null. These estimates correspond to cohorts that were between the ages 20 (for -2) and 29 (for -11) at the time of the tenure law passage. Observations between ages 20 and 29 were not in school when tenure laws were implemented. Therefore, we expect the effects to be zero. This confirms that parallel trends hold and one of the necessary assumption of the difference in differences design is valid.

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 and not statistically significant. These null effects can occur for multiple reasons. For example, in none of the these early tenure states did teacher immediately acquire tenure. Instead, after passage it took at least two more years of employment for tenure protections to be acquired. Furthermore, analysis of past interventions for children, ranging from

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

In contrast to the short-run outcomes, the long-run effects of tenure systems on adulthood income are large and negative. For men, the effects begin to turn negative after four years – these are cohorts for whom treatment begins before high school. 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 eventually to stabilize after 15 years of exposure to tenure regimes.

Table 2 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 -$1042 (-7.68%) percent and female earnings by -$565 (8.28%). The result is robust to the inclusion of division-by-cohort fixed effects (Column 3) and demographic and policy controls (Column 4).[17] In Column (1) I remove the region-by-cohort-fixed effects; all estimates to decrease in size by about 50 percent. In results available upon request, I show that this is due to the Southern Census region. When the South is removed, region-by-cohort effects are unimportant. Furthermore, the estimates without the South are qualitatively similar to specification that include both the South and birth-region-by-cohort fixed effects.[18]

5.3   Educational Attainment in 1940

Figure 5 shows event study estimates of tenure laws on years of education for men (Panel C) and women (Panel D). There is no pre-treatment trend, and, but there is some evidence of a very small positive effect. These effects are further explored in Table 2 Panels C and D. The effect is null in the most parsimonious model. However, when division-by-cohort fixed effects are included there is a positive and significant

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[17] There are four census regions and nine census divisions.
[18] The change in estimates is due to region-year indicators is specifically because the South is a poor control group for the Northeast region.

increase in educational attainment of about 1/10th of a school year. This is an extremely small change that is significant only in half of the specifications. For example, taking the estimates of returns to education for this period (Feigenbaum and Tan, 2020), we would expect a 0.1 increase in years of schooling to raise wages by 0.4 percent. The effect could be spurious or due to bias if tenure reforms are packaged with other interventions that increase years of schooling. Prior papers that study long-run effects of other educational interventions found often find income effects larger than what would be expected by changes in educational attainment (Jackson et al., 2016; Lovenheim and Willén, 2019). However, in those papers the effects on income and educational attainment move in the same direction.

5.4 Wage Income and Educational Attainment in 1960-2000

Figure 6 present the event study estimates of the impact of tenure laws between 1960-2000 for income (Panels A to B) and years of education (Panels C to D). In these figures, I pool all the decades from 1960-2000. Such pooling is appropriate as the effect is identical for each period (Figure 4). The separate estimates for each decade are available upon request. The pooling has the added benefit of increasing power. For each of the two variables, the event study estimates are null and are not statistically significant at conventional levels. Tenure laws have no impact on income and educational attainment for nearly the entire second half of the 20th Century.

The point estimates for 1960-2000 are available in Table 3 for income (Panels A and B) and education (Panels C and D). 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 -\$291 to \$507 for wage income and between -0.01 to 0.07 for years of education. Estimates for women are similar in magnitude and statistical significance. The zero effect is robust to adding demographic and policy controls as well as birth-division-by-cohort fixed effects. Furthermore, unlike the 1940 results removing birth-region-by-cohort fixed effects has no effects on the results.

These null effects are consistent with the available qualitative evidence. From the 1970s a small survey of teachers' opinions regarding tenure laws is available. Experienced teachers generally felt that tenure had no impact on the profession, either positively or negatively.

6 Robustness Using Linked Census Data

In this section I focus on the negative wage outcomes that I observe in the 1940 Census and perform several robustness tests on them. In one test, I include detailed additional controls for parental characteristics and use more detailed geographic information to assign treatment. In another, I identify a placebo group that moved to the state after they finished K-12 schooling and should not be affected by tenure laws. I describe each of these in turn.

The above two robustness tests are possible to conduct for men using linked census data. After 72 years, personally identifying information such as first and last name are no longer restricted information and are publicly available. The use of personally-identifying data allows researchers to track the same individuals across several census decades, thereby creating a large historical panel. In this section I use the cross-census links provided by Abramitzky et al. (2021).

For estimation, I use a version of equation (1). Except that I no longer collapse the data to birth-state-cohort cell. Instead, the data is collapsed to the earliest county of residence during schools ages by cohort level. The new detailed geographic controls allow me to assign treatment more directly and I no longer have to estimate state treatment that is adjusted for the fraction of the population affected. The fixed effects that dependent on birthplace are now re-calculated using 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).

6.1 Detailed Geographic Treatment and Parental Controls

The new information that these links provide allows me to address 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.[19] 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.

The results using individual-level data are shown in Table 4. Wage effects are presented in Panel A. All the estimates are negative, significant and similar in magnitude to the non-linked Census data. The inclusion of the detailed parental background characteristics (Column 5) leaves the results unchanged. The effects on educational attainment are in Panel C. The estimate are also similar to ones that use non-linked census data.

6.2 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 US 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 use a second group, those who immigrated to the US below age seven (early movers). I show that the early movers (treated group) and the late movers

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[19] The controls for early life circumstances are cell values (either the mean or the fraction) for the father's occupation, childhood urban residence, presence of a father, and presence of a mother, father's age, mother's age, and the number of siblings.

(placebo group) are affected by similar economic shocks, but only the early movers are affected by tenure laws.

Using immigrants for the placebo test is ideal for three reasons. 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 US 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.[20] 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 US. 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

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[20] 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.

determine treatment. This state is analogous to the birth-state used for the natives. For 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 residence 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 US 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. 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 immigrant children being more sensitive to school inputs. Perhaps because they learn English language skills at school instead of at home. Point estimates and robustness tests are available in Table 5 and confirm the results from the event study figures.

7 Mechanisms

The previous results imply that the impact of tenure regimes is very heterogeneous. In this section I will present evidence that early tenure laws had an impact on students because they were binding. Specifically, these binding tenure laws increased teacher retention. Retention is an important outcome of effective tenure regimes as mechanically job protections should lower the number of forced exists (and increase retention). In contrast, I will also show proof that later tenure regimes did not bind and impact teacher retention.

In addition to evidence on whether tenure laws bind, I will also present suggestive results on the determinants of tenure bite. I will argue that tenure laws only bind when there is a large surplus of teachers and do not bind when there is a relative teacher shortage. That teacher labor market tightness determines

the effect of tenure regimes is based on the following argument; (1) for tenure policies to impact students they must constraint a significant number of dismissals (Rothstein, 2015), (2) in a shortage the number of dismissals is low because teachers are difficult to replace, (3) if the number of dismissals is low then there is limited scope for tenure laws to matter. For example, if the number of dismissals is low before a tenure regime is implemented, then even if a tenure law prevents all dismissals the resulting total change in forced exists is trivial. The resulting limited change in turnover is then too small to measurably affect student outcomes.

Empirically, I will demonstrate the effect of teacher labor market conditions through two pieces of evidence. First, I will demonstrate that the observed effect of teacher tenure regimes on teacher retention scales with teacher surpluses. In a surplus, tenure laws increase teacher retention. Second, I will also show that the effect of tenure systems on student outcomes also scales with teacher surpluses. In a surplus, tenure regimes have a large negative effect on wage income. In a contrast the effects are small or not sigifcant in a teacher shortage.

To measure the state of the teacher labor market, I exploit the fact that census enumerators asked non-employed individuals to report their previous occupation; thereby, I observe a teacher-specific unemployment rate. This teacher unemployment rate is my measure of teacher labor market tightness.[21] I plot by decade teacher unemployment rate in Figure 8. Consistent with qualitative evidence, I find that the teacher labor market conditions can be split into two distinct periods. First, there was a large teacher surplus in the early 20th Century.[22] In 1910, more than 5 percent of individuals who report a teaching occupation were unemployed. Second, beginning with WWII (Cumbee et al., 1942; National Education Association, 1942b; Swanson, 1942), and continuing in the 1950s and 1960s (National Education Association, 1957;

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[21] The value of this measures depends on two flows: the flow from employment as a teacher to unemployment and the flow from unemployment to employment. In a shortage, transitions to unemployment for teachers are likely low and transition from unemployment to employment are likely high. Therefore, the unemployment rate for teachers will be low. The reverse holds in a surplus.

[22] During this early period the unemployment rate for teachers declines. This is consistent with an observed increase in demand for teachers during the 1920s (Sedlak and Schlossman, 1986), likely due to the increased demand for high school education (Goldin and Katz, 1999).

Graybeal, 1971), there is a relative shortage. For example, in 1950 and 1960 the teacher unemployment rate drops to below one percent. Finally, by 1971 enough new teachers are being trained to fill new vacancies (Lang, 1975) and the unemployment rate for teachers increases, but never again is it higher than two percent and does not come close to the heights reached in the early 20th Century.

7.1   The Effects of Late Tenure Laws on Teachers

I begin this section by studying the impact of later tenure laws on teacher retention. These later tenure laws had no measurable impact on long-run student outcomes and were implemented during a period of a relative teacher shortage (see Figure 8). The unemployment rate for teachers was low, below one percent for most of the period. The one exception is 1940, but that number is unlikely to be representative of labor market conditions in the 1940s because US entered WWII one year after. The start of WWII was associated with large teacher shortages, therefore the 1940 measure is unlikely to correspond to the actual labor market conditions in the 1940s. As these laws have no effect on students and are implemented under a relative teacher shortage, they should not bind and affect teacher retention.

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. Although not ideal, the tenure laws passed between 1964-1977 do overlap with an important result; that later laws have a null effect on the average student.

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

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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 groups. The resulting cross-sectional unit 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.

Formally, I estimate a standard event study model and exploit variation across years and state groups. 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).

The results for teacher retention are presented in Figure 9. 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 Panel A of Table 6 and are close to zero in all specifications. The results are unchanged to the inclusion of census-region-by-year fixed effects and controlling for other policies that can affect teacher retention, such as duty-to-bargain laws and education finance reforms. The inclusion of census-division-by-year fixed effects is difficult as the regression is estimated off 21 state groups, many of which overlap with the Census definition of a division. Nevertheless, including the division-by-year controls does not lead to significant effects. Although the point estimates in those specification are negative and large. For example, with division-year (columns 5 and 6) tenure regimes lead to a non-statistically significant 1.5 percentage point decrease in retention each year. The overall pattern is that these tenure regimes do not affect teacher retention. These null effects are consistent with survey evidence from the 1970s which found that teachers felt that tenure laws had no positive or negative effects on their profession (Cobb, 1981).

The above retention analysis suggests that later tenure laws were not binding, but is not conclusive. In part because the retention data is only available for a portion of the later period. Therefore, I will supplement the retention analysis by studying other teacher outcomes using Decennial Census data. The Decennial Census is a large dataset that collects important information on teachers for the years 1940 to 2000. The years 1940-2000s do overlap with a large portion of later tenure laws (1927-1977) that do not affect students.

The Census data affords the opportunity to test if tenure regimes affect teacher wages, educational attainment, reported weekly work hours and teacher's race. I study wages because binding job protections make teaching a more attractive occupation and principals can take advantage of it by lowering teacher wages. Alternatively, if wages are held fixed the higher value of the job can allow principals to hire more qualified candidates on dimensions they observe. Therefore, I look at whether tenure laws affect teacher educational attainment. Furthermore, binding job protections reduce the incentive to provide effort, so I test if tenure laws change reported weekly hours worked. Finally, I test if tenure laws protected African-American teachers from dismissals and do so by measuring if the fraction of Black teachers changed. Finding no effect on these outcomes would further support the teacher retention results, which as previously discussed are not perfect, but highly suggestive.

The event studies are plotted in Figure 10 and the point estimates are available in Table 6 panels B to E. Across most of these dimensions tenure laws have no significant effects. There are only two consistent patterns that I will briefly discuss. One is that, in some specifications, the fraction of Black teachers declines (Table 6, panel B), but Figure 10 suggests that this is due to divergent parallel trends. Another is that in a model with only division-by-year fixed effects teachers reported weekly hours worked fall by 45 minutes and the effect is significant. However, the effect is most likely spurious as it is not significant in any other specifications.

The results present a preponderance of evidence that later tenure laws do not bind. These tenure laws have no measurable impact on teacher retention and no significant effects on teachers hours worked,

wages and educational attainment. That these tenure laws do not bind is consistent with these tenure laws not affecting student outcomes. This evidence is suggestive that tenure laws do not bind under a teacher shortage but is not close to being conclusive. In the next section, I will strengthen the evidence by demonstrating that under a teacher surplus tenure laws do bind and that the effect scales with the extent of the teacher surplus.

7.2   The Effects of Early Tenure Laws on Teacher Retention and Other Variables

In this section I study the impact of earlier tenure laws on teacher retention. It is these early tenure laws that have a large negative effect on wage income. The teacher markets in this period were characterized by a heavily feminized workforce with limited outside options.[23] At the start of the period women were paid about 70 percent that of men (The US Bureau of Education, 1901), and were often dismissed upon marriage (Sedlak and Schlossman, 1986). In the unemployment data this translates to a large teacher surplus (Figure 8). In 1910 the teacher unemployment rate was more than 5 percent, the highest in the 20th Century. As the century progressed, rising school enrolment (Goldin and Katz, 1999) increased demand for teachers (Sedlak and Schlossman, 1986). Consequently dismissals for marriage became less common (Sedlak and Schlossman, 1986) and the unemployment rate for teachers fell to about 3.2 percent in 1930. Nonetheless, relative to the rest of the 20th Century the market for teachers appears to be in a relative surplus. Therefore, if tenure laws bind in a relative teacher surplus, then these tenure laws should increase teacher retention.

The high unemployment rate in this period also generates large variation in state teacher labor market conditions across states that I can exploit. If tenure laws increase retention in a teacher surplus, then the effect should be larger in teacher labor markets with large surpluses. Conversely, there are also labor markets with a relative teacher shortage; tenure regimes should not affect teacher retention there. I do so by splitting the treated states into three groups (low, medium and high rate) based on their teacher labor market conditions. There are only 12 such states and therefore, I prefer to use an "eyeball test" for the split.

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[23] For example, in the 1900 Census women make up 74 percent of the teaching workforce and 73 percent of women in professional occupations are teachers.

The unemployment rate for the states that make up the three groups is available in Figure 11, Panel A while the geographic distribution is available in Panel B.

To measure the differences in teacher labor market conditions across states in this period I use the 1910 state unemployment rate. I use the 1910 measure as it is the earliest unemployment rate I have for tenure laws implemented between 1910-1922 (there is no data for 1920). I use the earliest measure before tenure implementation because the teacher unemployment rate is potentially endogenous with respect to employment protection legislation (Autor et al., 2006). Finally, I use a state level measure as opposed to a county measure because there is evidence that the state is the more relevant labor market. Using linked Census data for male teachers, I find that across two decades only 12 percent switch states, but over 70 percent switch counties.

The retention measure for this period is constructed using linked Census files from 1900-1940. Intuitively, due to the availability of personally identifying information such as first and last name, it is possible to track teachers across decades. So, I can observe if they move counties or switch occupations. The exact links and a description of the linking procedure are from Helgertz et al. (2020).[24] Unfortunately, as women change names across marriage, these links are only available for male teachers.[25] So, for men, I construct a measure of decennial retention. 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 event study for the effects of tenure laws on the retention of male teachers is available in Figure12; parallel trends hold and tenure laws cause teacher retention to increase. The point estimates from several different specifications are in Table 7, Panel A. Tenure laws significantly increase retention in a parsimonious event study model (Column 1) by about 4 percentage points. Adding region and division by-

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[24] I use the links from Helgertz et al. (2020) when I link older adult individuals. I use the link from Abramitzky et al. (2021) when I link adults to their childhood selves. Helgertz et al. (2020) makes use of additional information, such as family structure that increase the number of adult to adult matches and makes the resulting links more accurate.
[25] I have also constructed a retention measure for women using a restricted version of Census files, but do not have permission to share the results yet.

year fixed effects causes the estimates to decrease in magnitude and loose significance – choosing an appropriate control group is important for the effects. I explore this further and limit the comparison group for each treated states to control states whose 1910 teacher unemployment rate is within one percentage point of the treated state. This restriction should generate a comparison sample that is more similar to the treated states that one generated by region and division by-year fixed effects. The results are presented in columns (4) to (6). The estimated retention effect is significant in every specification and increases in size with region and division fixed effects.

In Table 12 Panel B, I present retention effects by teacher labor market conditions. Tenure regimes implemented in states with low teacher unemployment rate (relative shortage) do not affect teacher retention. None of the effects are significant and in two of the specifications the effects are the wrong sign (negative). In contrast, tenure regimes implemented in states with a middle level of teacher unemployment result in a large and significant increase in teacher retention in every specification. At 10 to 19 years after tenure, retention increases by between 6.4 pp to 8.6 pp depending on the specification. In Panel C I display the results of an F-Test for the equality of the coefficients between the three state unemployment groups. I can reject that the estimates are equal between the low unemployment and the middle unemployment states in five out of six specifications at the five percent significance level. Now turning to states with a high unemployment rate, it is clear that the retention point estimates do not scale linearly with teacher labor market conditions. The effects are positive, larger than that for the low teacher unemployment states, but our only significant in three specifications and smaller that the effects for the middle group. Furthermore, only in the specification with division-by-year fixed effects do the estimates for the low and high unemployment rate group differ significantly (Panel C, Column 3).

The above results support the hypothesis that in a relative teacher shortage tenure laws do not bind, but the effect does not necessary scale linearly with the teacher unemployment rate. The non-linearities can be due to several reasons. First, the 1910 state teacher unemployment rate is an imperfect measure of labor

markets and might not be accurate enough to identify more fine differences.[26] For example, labor market conditions could have changed between 1910 and when a state implemented a tenure regime and states in the high rate group tend to implement tenure regimes later. Alternatively, while the estimates within each group are causal, there is no guarantee that the differences across these groups are due to teacher labor market conditions. Importantly, the extent of the bias can scale with teacher unemployment rate. If labor markets are tight and dismissals are limited, then any potential differences between states that can affect retention do not matter. The principals in both states dismiss very few teachers and there is limited scope for any other unobserved heterogeneity to matter. In contrast, if the unemployment rate is high enough, there are more opportunities for unobserved differences between states to affect the results.[27] That can explain why the effects for low rate states are close to zero, but the effects does not perfectly scale with teacher labor market conditions.

The previous results show that tenure regimes affected retention of male teachers, but there is no guarantee that retention of female teachers increased too. To provide suggestive evidence that tenure regimes affected women, I study whether the proportion of married female teachers increased after a tenure law. Tenure laws can increase the size of this group if dismissals for marriage were not a valid category under a tenure law. 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 effect of tenure regimes on the proportion of teachers who are married and female is available in Figure 13 (event study) and Panel A of Table 8 (point estimates). For the average state there is only some

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[26] For example, another potential measure of teacher labor market conditions is the teacher retention rate. The low unemployment rate states have a much higher retention rate than the middle or high-rate states. However, the retention rate is similar between the middle and high unemployment rate states.

[27] For example, suppose there are two states, *a* and *b*, both with a very low dismissal rate. State *a* passes an onerous tenure law while state *b* passes a relatively weak tenure law where its easy to dismiss teachers. The decline in dismissals should be larger in state *a* than in *b*. However, because the dismissals are low the potential change in retention is small in both states; even if in state *a* dismissals drop completely and in state *b* the dismissal rate is unchanged. In contrast, now suppose that the dismissal rate is very high; dismissals in state *a* drop completely but are unchanged in state *b*. The resulting difference in retention rate is now larger.

evidence that tenure regimes increase the number of married female teachers. Point estimates are statistically significant in four out of the six specification. However, in a baseline model with division-year fixed effects the impact is a reduction in married female teachers. Among the three significant and positive estimates the effects range from a 3.5 pp to 4.5 pp increase. The event study (Figure 13) plots the most parsimonious model corresponding to Column (1) of Table 8, Panel A. The parallel trends hold, but like the table the effects are not statistically significant.

The heterogeneity analysis by state 1910 state teacher unemployment rate is presented in Panels B and C of Table 8. The most striking result is that states with a high teacher unemployment rate experience large increases in the proportion of married female teachers. The effect is significant in all specifications and ranges from a 4pp to 7.88 pp increase. However, the overall pattern does not scale with teacher labor market conditions as the estimates are small and not significant for states in the middle group, but are positive and significant in some specifications for states in the low teacher unemployment rate group. This can be because, like discussed above, the 1910 unemployment rate is not a perfect measure of labor market tightness. Alternatively, marriage related dismissals might be only weakly related to labor market conditions, but heavily related to social stigma against married women working. So, it could be that the stigma is stronger in the low unemployment rate group that the middle group. Therefore, the estimated effects are larger in the low-rate group.

7.3   The Impact of Tenure Laws on Wages by Teacher Labor Market Conditions

Finally, I test if the impact of tenure regimes on students varies with teacher labor market conditions. As previously discussed, in Figure 8 we see that the early tenure laws were implemented under a teacher surplus while the later laws were implemented under a teacher shortage. This time series variation in teacher labor markets matches the time series variation in student outcomes; early tenure laws negatively affected student wages while later tenure laws had no measurable effects. The trend across time is consistent with the hypothesis that labor market conditions matter, but it is not conclusive.

Next, I turn to the cross-section and show that the negative effects of early tenure laws scale with the teacher labor market conditions in each state. Figure 14 plots event study estimates for each of the three state groups. These treated states were split based on their 1910 unemployment rate (low, medium and high). We see that parallel trends hold for each of the three states groups. Students in treated states with low unemployment rate are not affected by tenure laws. The effects of tenure are negative for students who reside in states that have a medium unemployment rate. Then, the negative effects further increase in size for students who reside in high teacher unemployment rate states.

The point estimates for the impact of tenure laws on male wage income are available in Table 9. In every specification the impact of tenure regimes scales with teacher labor market conditions. As an example, take the model with cohort-by-region indicators (Column 2). At 10 years of exposure, wages fall by $544 in states with a low teacher unemployment rate, by $1,128 in states with a middle rate, and by $1,636 in states with a high rate. The one semi-inconsistent result is the parsimonious model (Column 1). The effects do scale with teacher surpluses, but the effect for states with a low unemployment rate is still large and negative, about a $1,114 wage decline.[28]

8 Conclusion

My paper is the first to study the implementation 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 1940-2000. I then exploit the implementations of these tenure systems in a cross-cohort difference-in-differences design. The results suggest that tenure laws negatively affect student outcomes, but only when they are binding.

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[28] Cohort-by-region indicators matter because the Southern region appears to be a poor control for the Northeast region. Once the South is dropped from the sample, estimates from the parsimonious specification closely match that of one with cohort-by-region fixed effects with the South included. Furthermore, if the South is excluded that cohort-by-region fixed effects don't alter the estimates. These results are available upon request.

The main findings are that early tenure laws passed between 1910-1922 drastically lowered adult wage income in 1940. After 12 years of exposure, male wage income declined by 7.68 percent and female wage income fell by 8.28 percent. These large negative effects are specific to the early 20th Century context as tenure laws passed in later years do not affect the average student's income or educational attainment. To reconcile these divergent results, I show that early tenure laws have policy bite and protect teachers, while later tenure laws do not bind and do not measurably affect teachers. A potential explanation for why some tenure laws bind and others do not is teacher shortages. I show that if there is a relative teacher shortage tenure laws do not bind and consequently do not affect student outcomes. Therefore, given the recent relative scarcity of teachers, my paper suggests that removing tenure protections will not lead to measurable changes to student outcomes.

9 Works Cited

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10 Figures

Figure 1. Time Series Variation in Teacher Tenure Systems
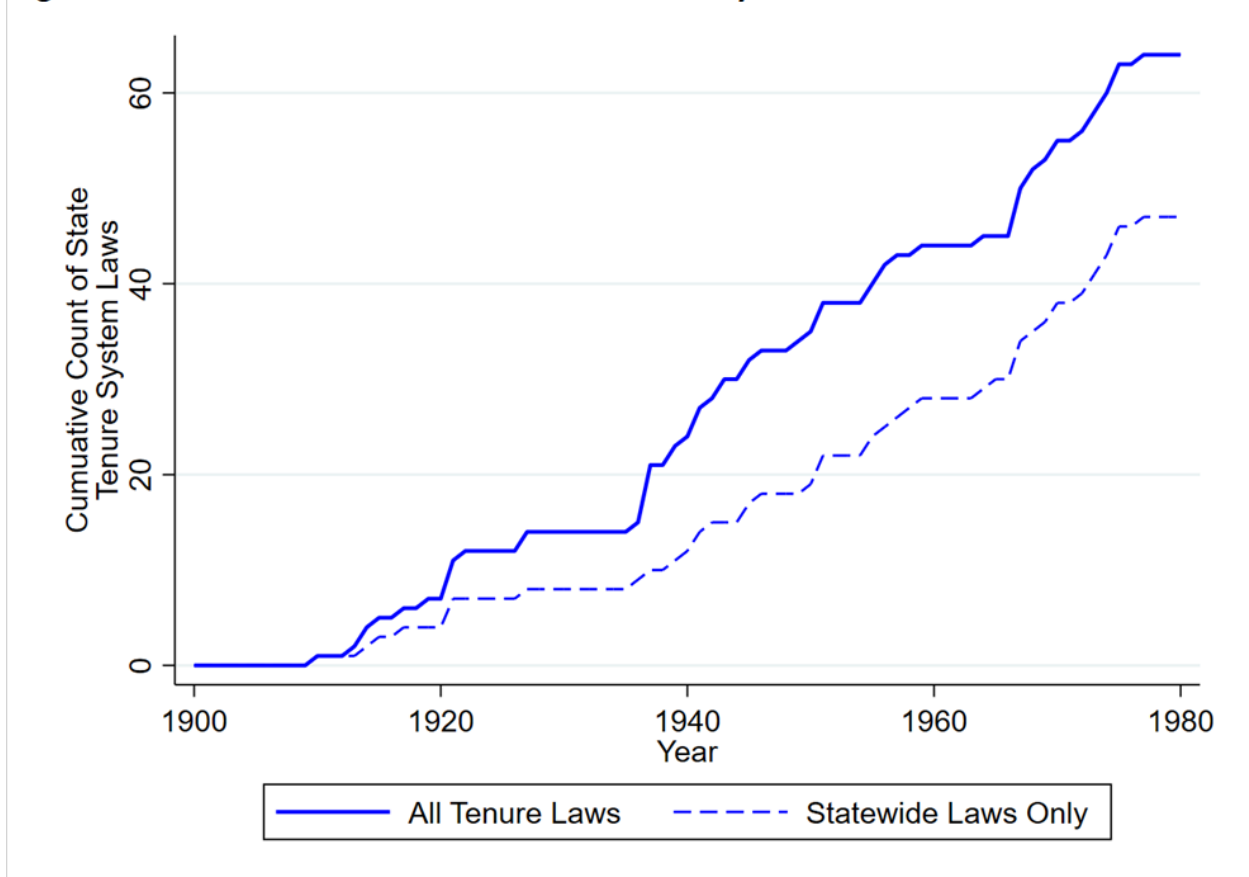
Note: This figure displays the cumulative count of state tenure laws passed by a given year. All Tenure Laws includes local laws passed by state legislatures and statewide laws. As a state can first pass a local law, and then a statewide law, the cumulative count can exceed the number of states. Statewide Laws Only includes only tenure laws that cover the whole state.

Figure 2. Cross-Sectional Variation in Teacher Tenure Systems

Panel A. Tenure Laws, 1910-1922

□ No Tenure Variation
■ Local Tenure Law
■ Statewide Tenure Law
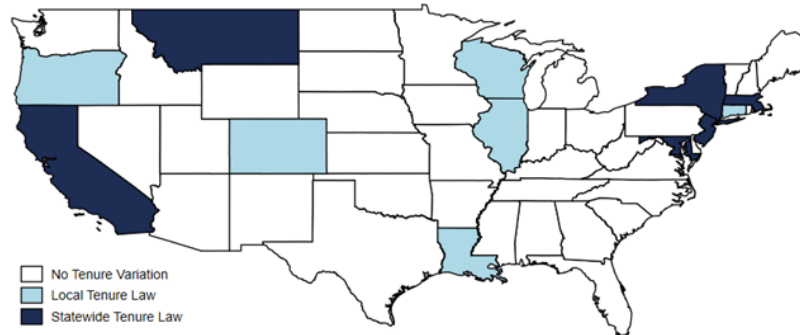
Panel B. Tenure Laws, 1964-1977

□ No Tenure Variation
■ Statewide Expansion of Local Law
■ Statewide Tenure Law
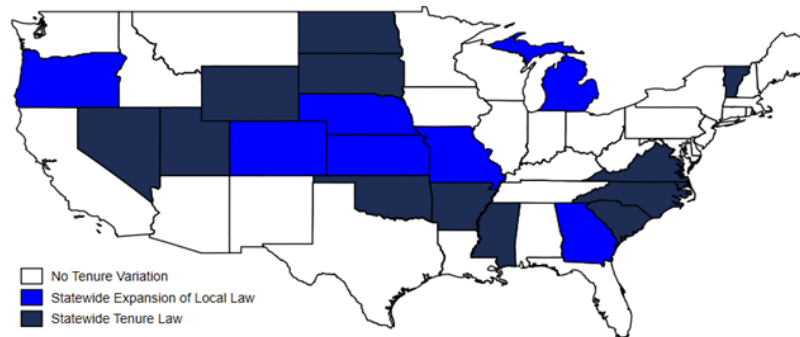
Note: This figure displays cross-sectional variation in teacher tenure systems for two key periods. Statewide Tenure Law refers to a state passing a tenure system where previously none existed. Local Tenure Laws are those passed by a state legislature, but only cover a portion of the state. Statewide 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
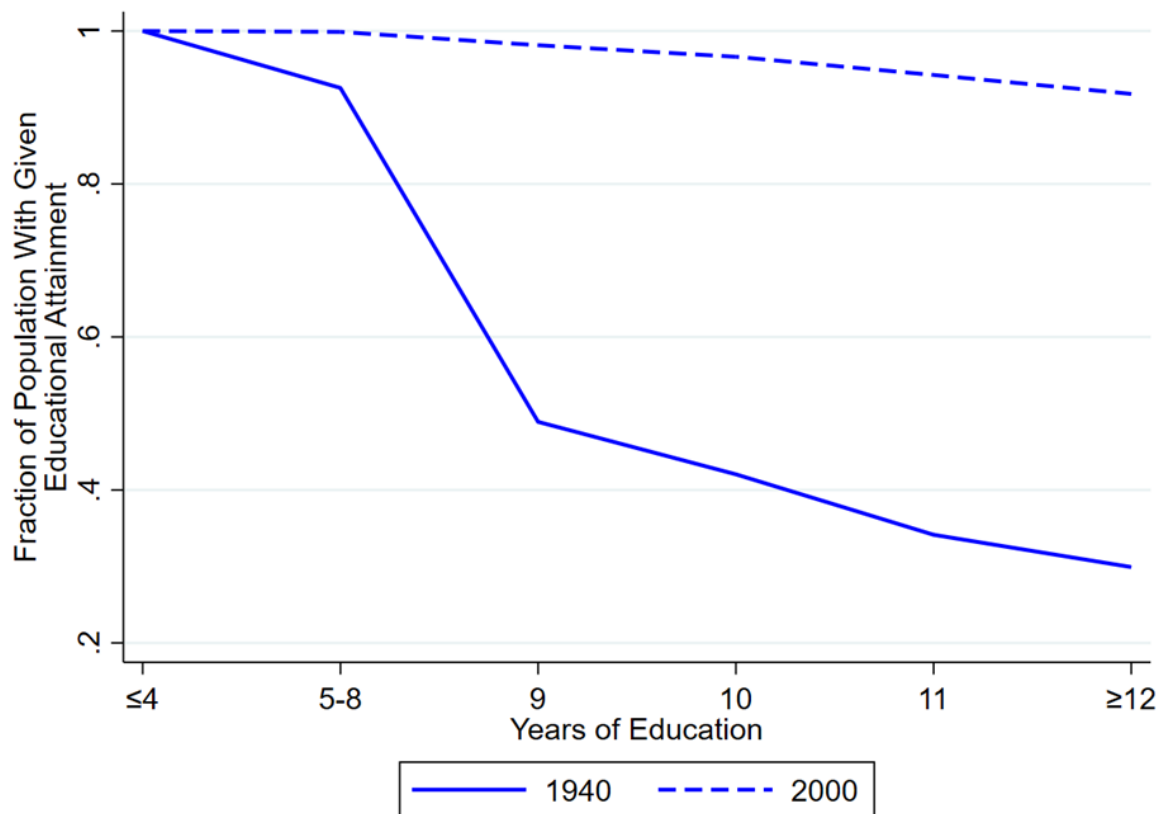
Note: The cross-cohort event study estimates an Intent to Treat effect. One of the reasons for the ITT nature of the estimates is educational attainment. Not everyone attends school for 18 years. This figure highlights the relationship between educational attainment and the ITT estimate. The figure displays educational attainment of the sample in 1940 and 2000.

Figure 4. Summary of Main Results

Panel A: Effect on Male Income at 10 to 14 Years of Treatment

Panel B: Effect on Female Income at 10 to 14 Years of Treatment

Panel C: Effect on Male Education at 10 to 14 Years of Treatment

Panel D: Effect on Female Education at 10 to 14 Years of Treatment

Note: This figure plots the main results of the paper. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. The baseline model is equation (1) with birth-region-by-cohort fixed effects and demographic controls the fraction of each state-cohort-gender cell that is Black, Hispanic, Other Race. Percent change in wage income is calculated by taking the pooled estimate for 10 to 14 years of treatment and dividing it by the mean. On the x-axis, the bold number represents the Census decade that I use to estimate the regression. The italicized number represents the tenure laws that I evaluate in that decade.

Figure 5. Student Outcomes in 1940
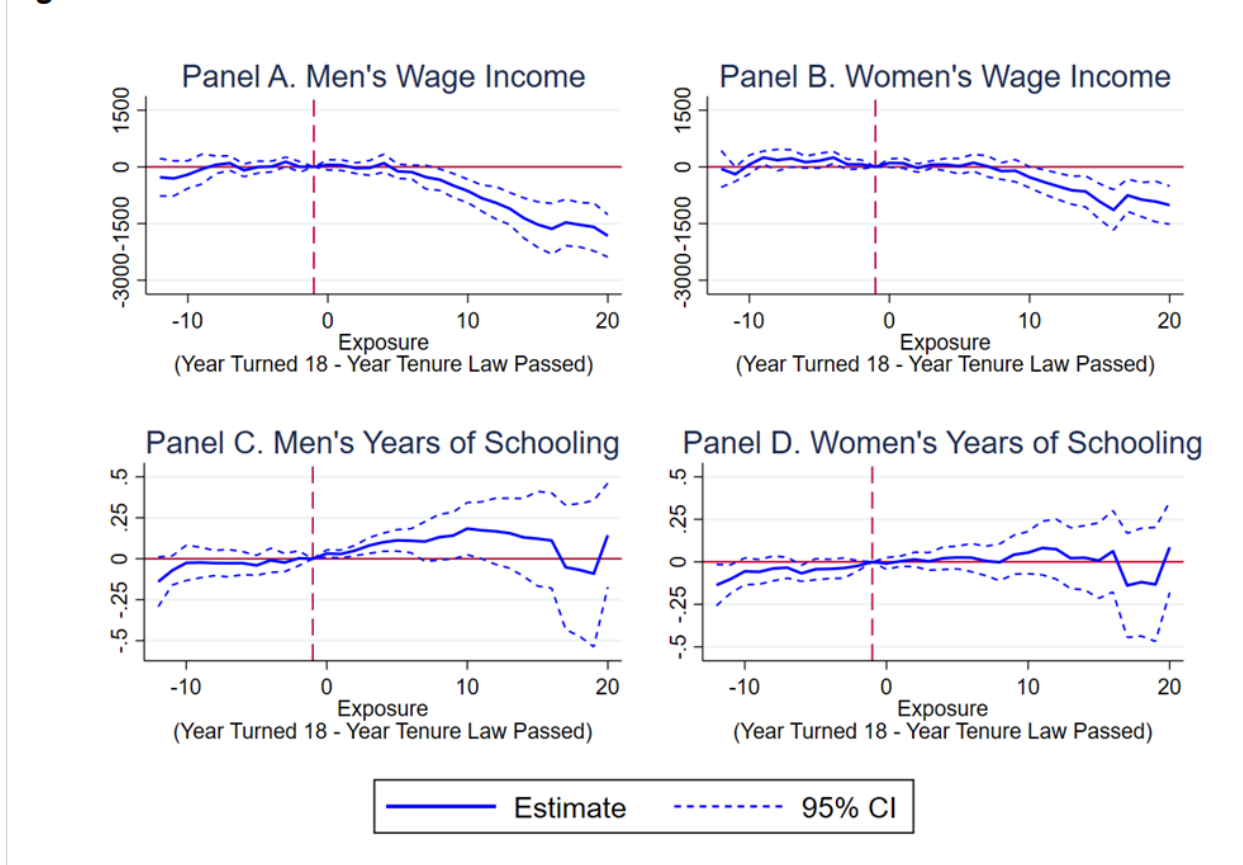
Note: This figure plots coefficients and the 95 percent confidence interval from an event-study model that estimates the effect of tenure laws on adult wage income and educational attainment. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. The model controls for the proportion of individuals in each cell who are black, Hispanic, or other and birth region by cohort fixed effects. Standard errors clustered at the birth state and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Figure 6. Student Outcomes in 1960-2000
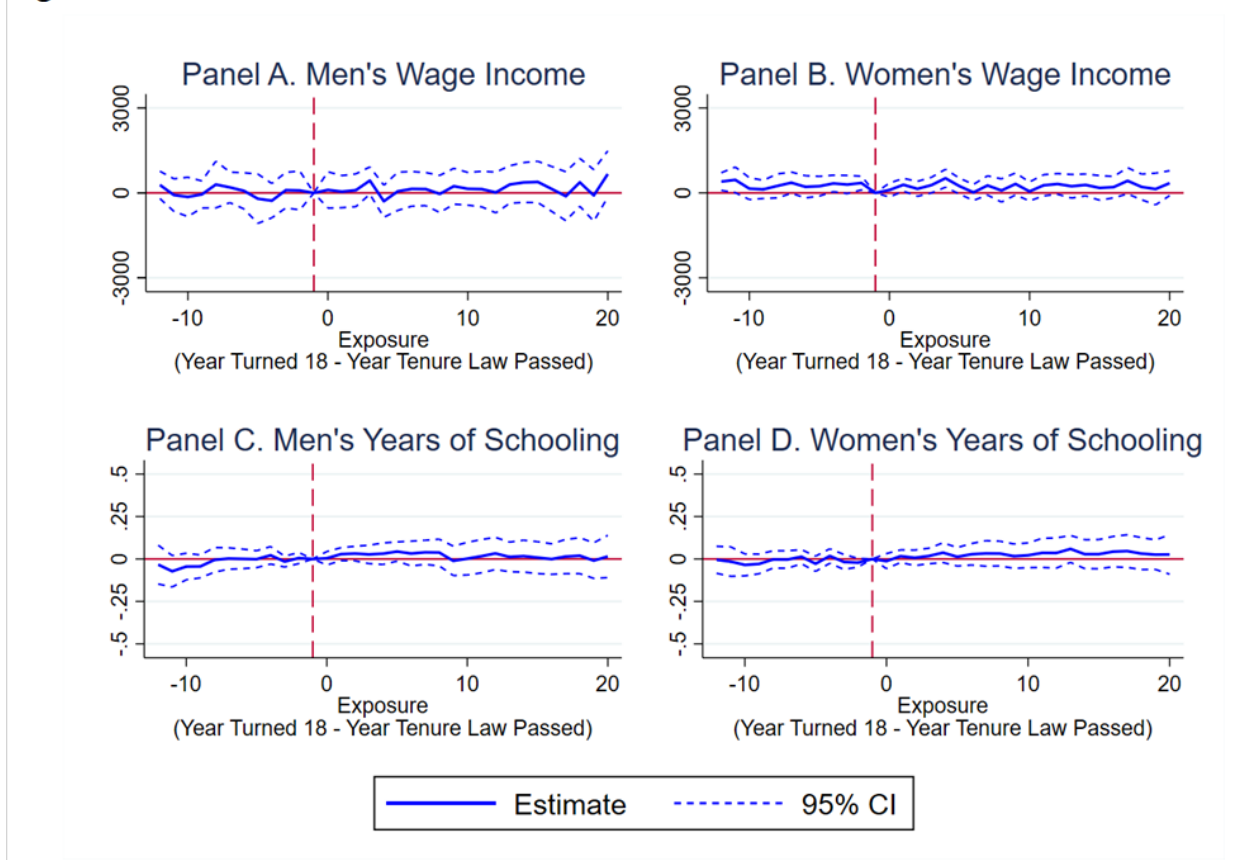
Note: This figure plots coefficients and the 95 percent confidence interval from an event-study model that estimates the effect of tenure laws on adult wage income and educational attainment. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. The model controls for the proportion of individuals in each cell who are black, Hispanic, or other and birth region by cohort fixed effects. Standard errors clustered at the birth state and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

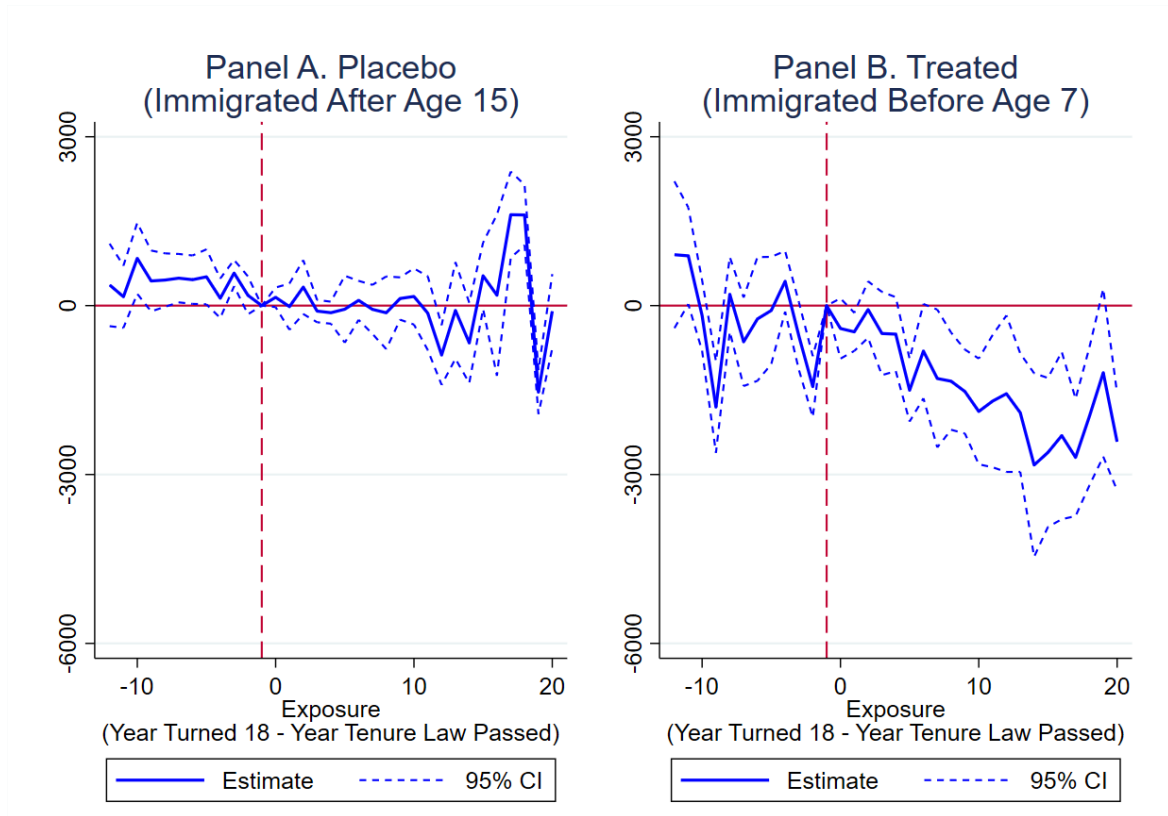**Figure 7. Effect on Immigrants by Age of Arrival**

Note: This figure plots coefficients and the 95 percent confidence interval from an event-study model that estimates the effect of tenure laws on adult wage income. The model is estimated using linked decennial Census data (Helgertz et al., 2020). The sample in Panel A is limited to individuals who immigrated to the US after age 15. The sample in Panel B is limited to individuals who immigrated to the US before age seven. Treatment is assigned to both groups based on the state they resided in the 1930 Census. The model controls for census division by birth cohort fixed effects, birthplace indicators and a Bartik measure of exposure to the great depression between 1929-1933. Standard errors are clustered at the 1930 state of residence. $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 8. Teacher Unemployment Rate by Decade

Note: This figure displays the unemployment rate for teachers in each census decade. I calculate the variable using individuals who are in the labor force and report a teaching occupation. Those who are not presently employed report their occupation in the previous year. Those who are currently employed report their present occupation. No unemployment information is available in the 1920 census.
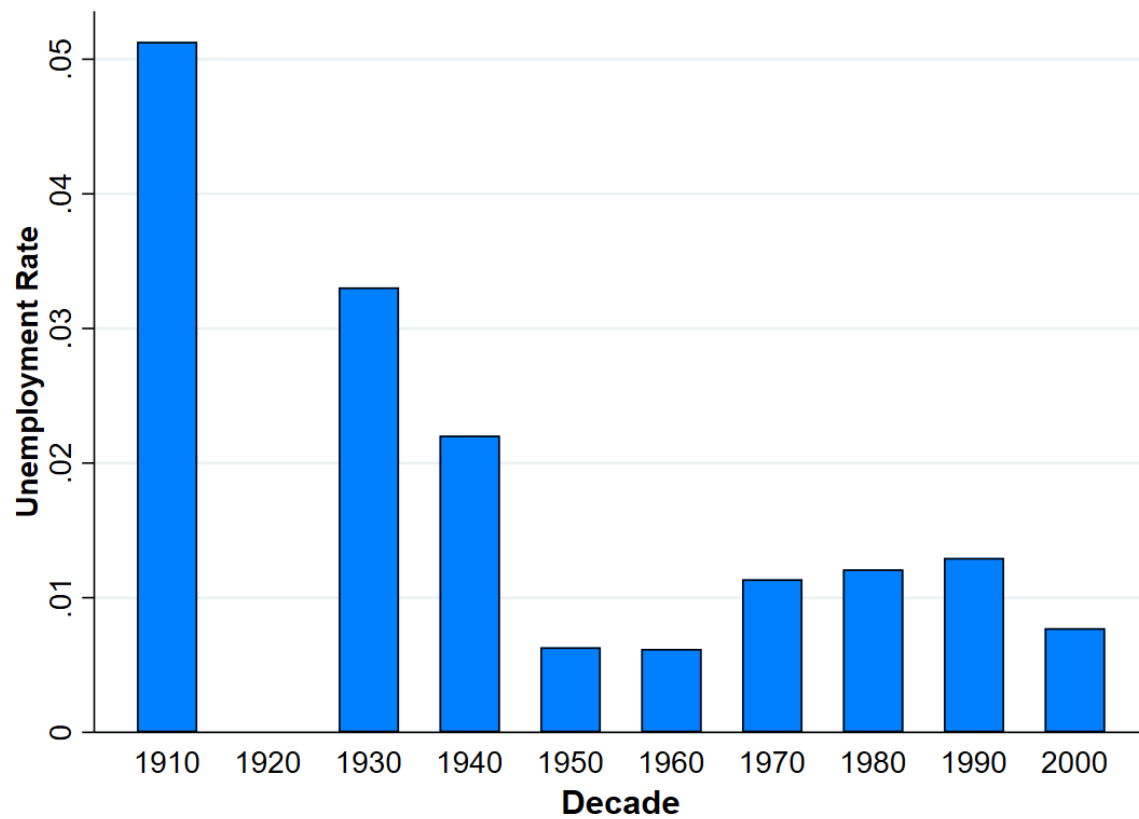
Figure 9. Teacher Retention in 1963-2000

Note: This figure plots coefficients and the 95 percent confidence interval from an event-study model that estimates the effect of tenure laws on teacher retention. The data is from the Current Population Survey Annual Social and Economic Supplement for the years 1963-2000. The outcome variable is an indicator equal to one if an individual, who was previously a teacher, is currently a teacher. The sample is limited to individuals who were public school teachers in the previous year. Standard errors are clustered at the state-group level.

Figure 10. Teacher Outcomes in 1960-2000
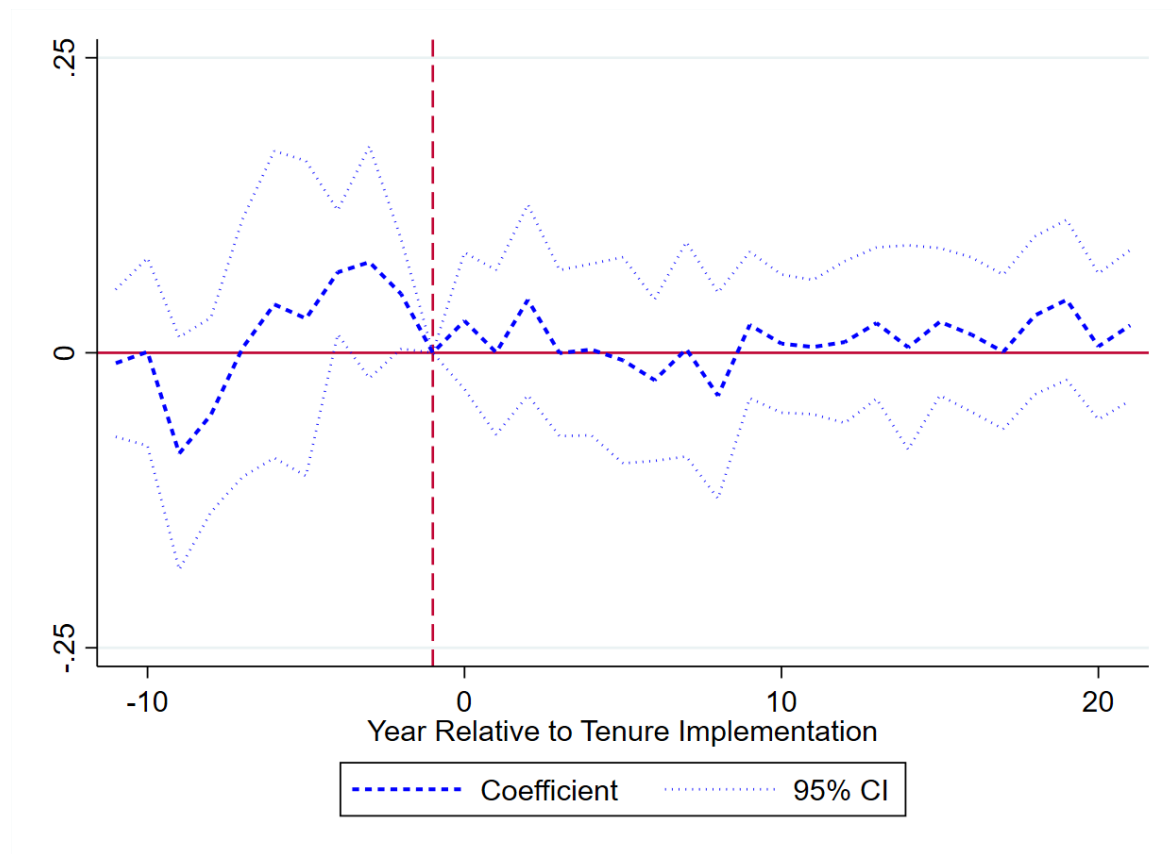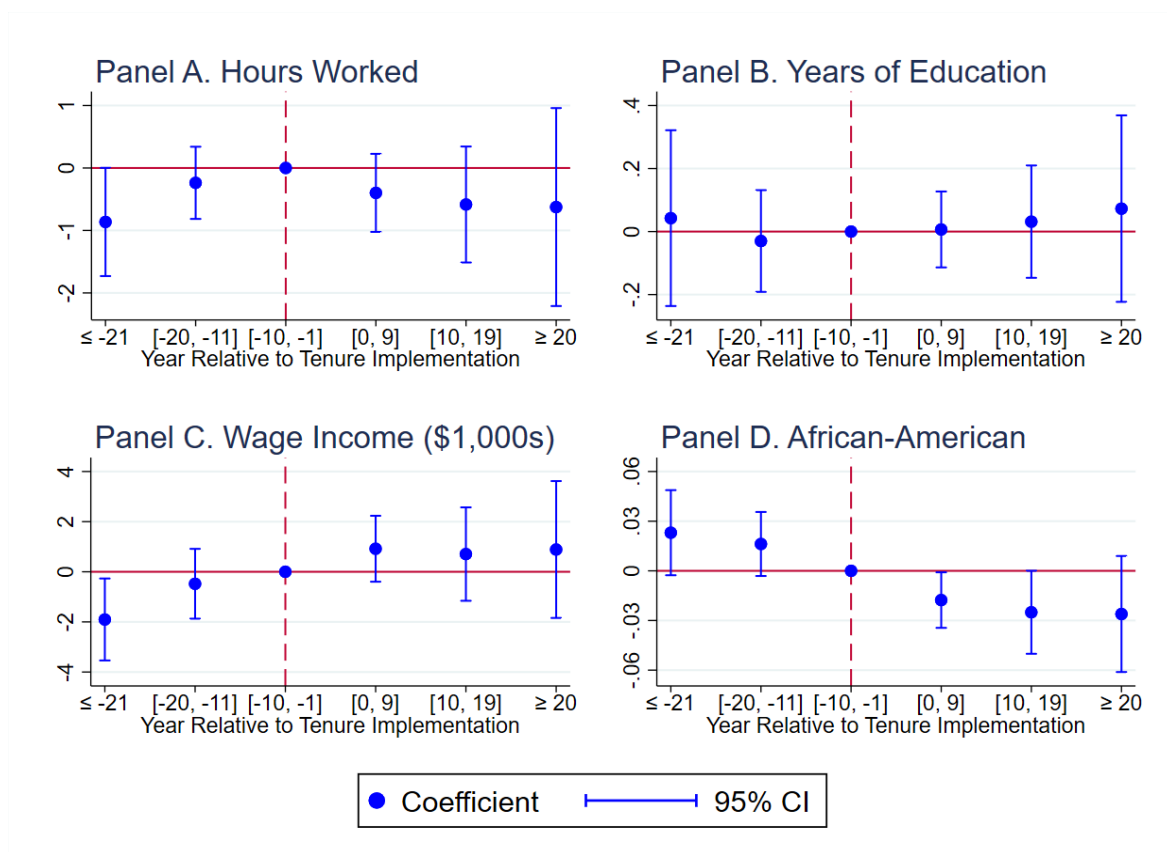

Note: This figure plots coefficients and the 95 percent confidence interval from an event-study model that estimates the effect of tenure laws on a variety of teacher outcomes. The data is from the Decennial Census for the years 1940-2000. The outcome variables are weekly hours worked (Panel A), educational attainment (Panel B), total wage income (Panel C), and an indicator that is equal to one if the teacher is African American (Panel D). The sample consists of teachers who are presently in the labor force. Standard errors are clustered at the state level.

Figure 11. Early Tenure Laws and Teacher Unemployment Rate

Note: This figure shows the distribution of teacher labor market conditions in 1910 for the early tenure laws implemented between 1910-1922. The states are split into three groups based on the 1910 teacher unemployment rate. The average unemployment in each group is displayed in the legend. Panel A presents information on the unemployment rate while Panel B plots the geographic distribution.

Figure 12. Teacher Retention in 1910-1940
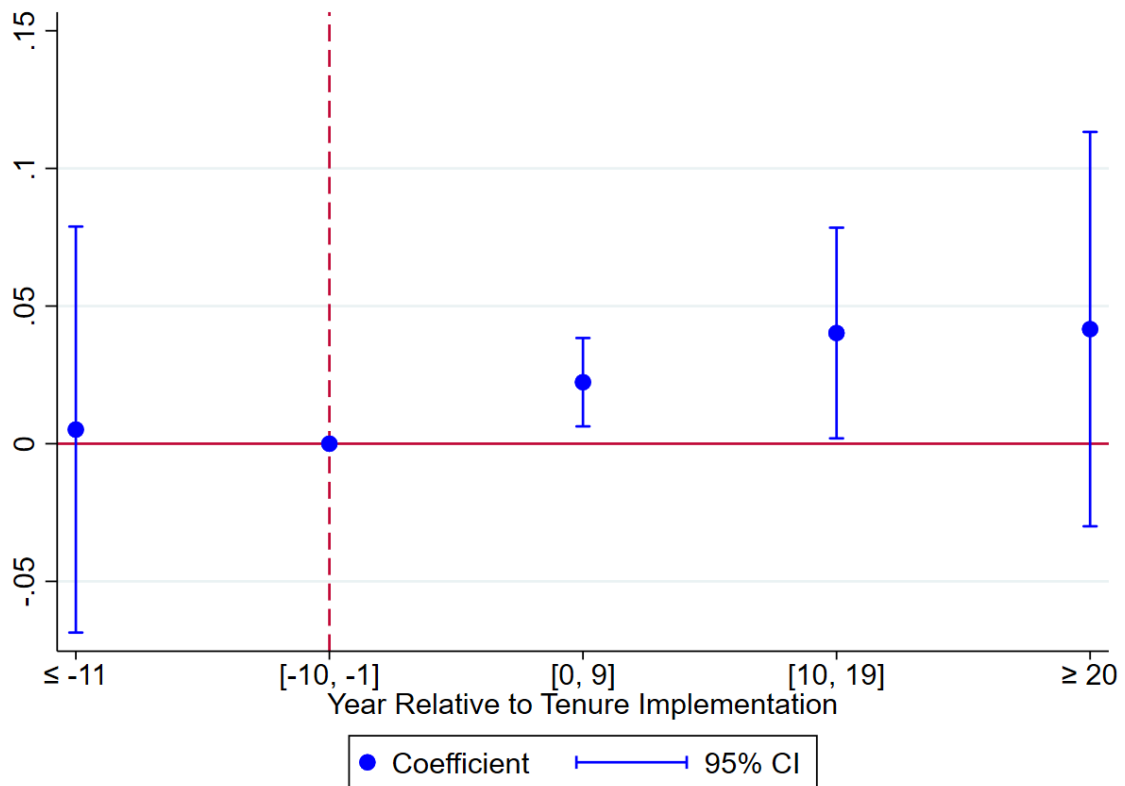
Note: This figure plots coefficients and the 95 percent confidence interval from an event-study model that estimates the effect of tenure laws on teacher retention. The model is estimated using linked decennial Census data (Helgertz et al., 2020). The sample is limited to men who reported a teaching occupation in the previous decade. The outcome variable is an indicator equal to one if the individual is still a teacher in the same county. Standard errors are clustered at the state of residence reported in the previous decade. $p<0.01$, ** $p<0.05$, * $p<0.1$.

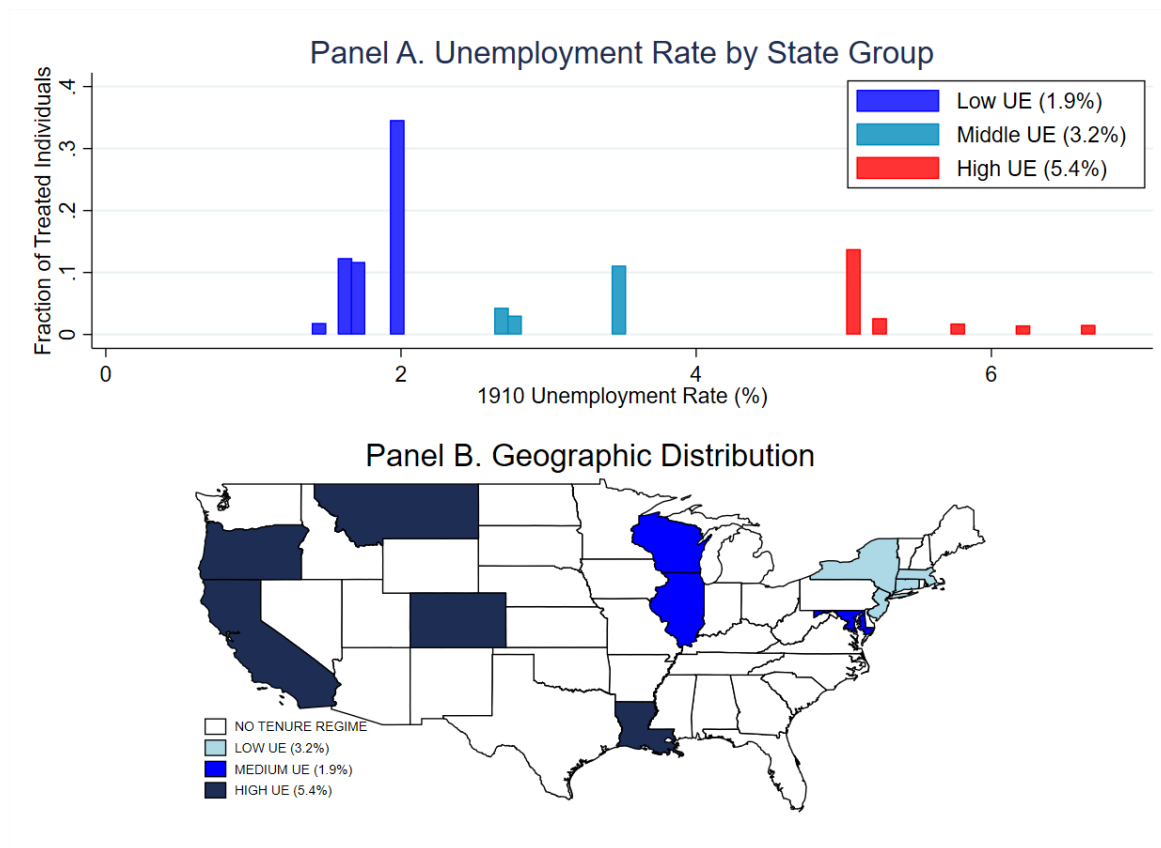**Figure 13. Proportion Married & Female Teachers in 1900-1940**
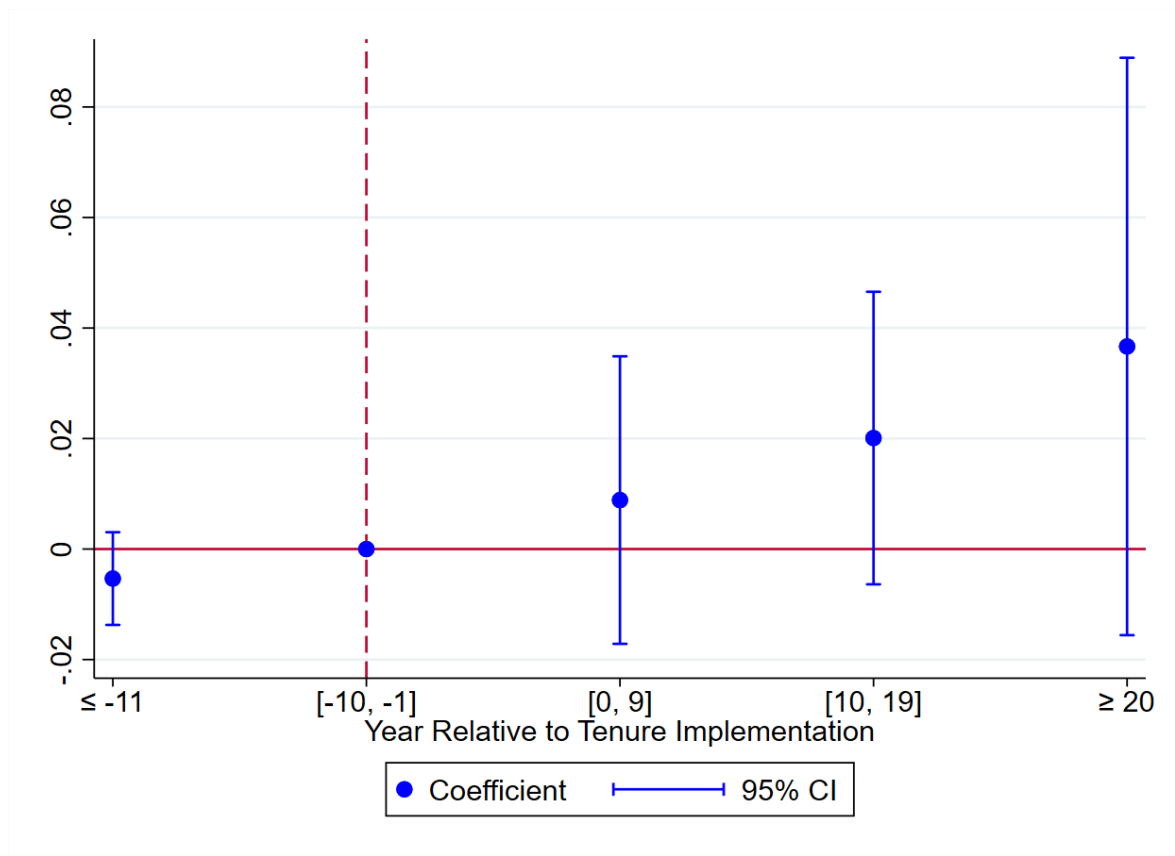
Note: This figure plots coefficients and the 95 percent confidence interval from an event-study model that estimates the effect of tenure laws on teacher retention. The model is estimated using decennial Census data. The sample is limited to individuals who report a teaching occupation. The outcome variable is an indicator equal to one if the individual is a married female teacher. Standard errors are clustered at the state of residence. p<0.01, ** p<0.05, * p<0.1.

Figure 14. The Effect of Tenure by Teacher Labor Market Conditions

Male Wage Income in 1940 (Adjusted to 2000 Values)

Legend:
- Low UE (1.9%)
- Middle UE (3.2%)
- High UE (5.4%)

X-axis: Exposure (Year Turned 18 - Year Tenure Law Passed)
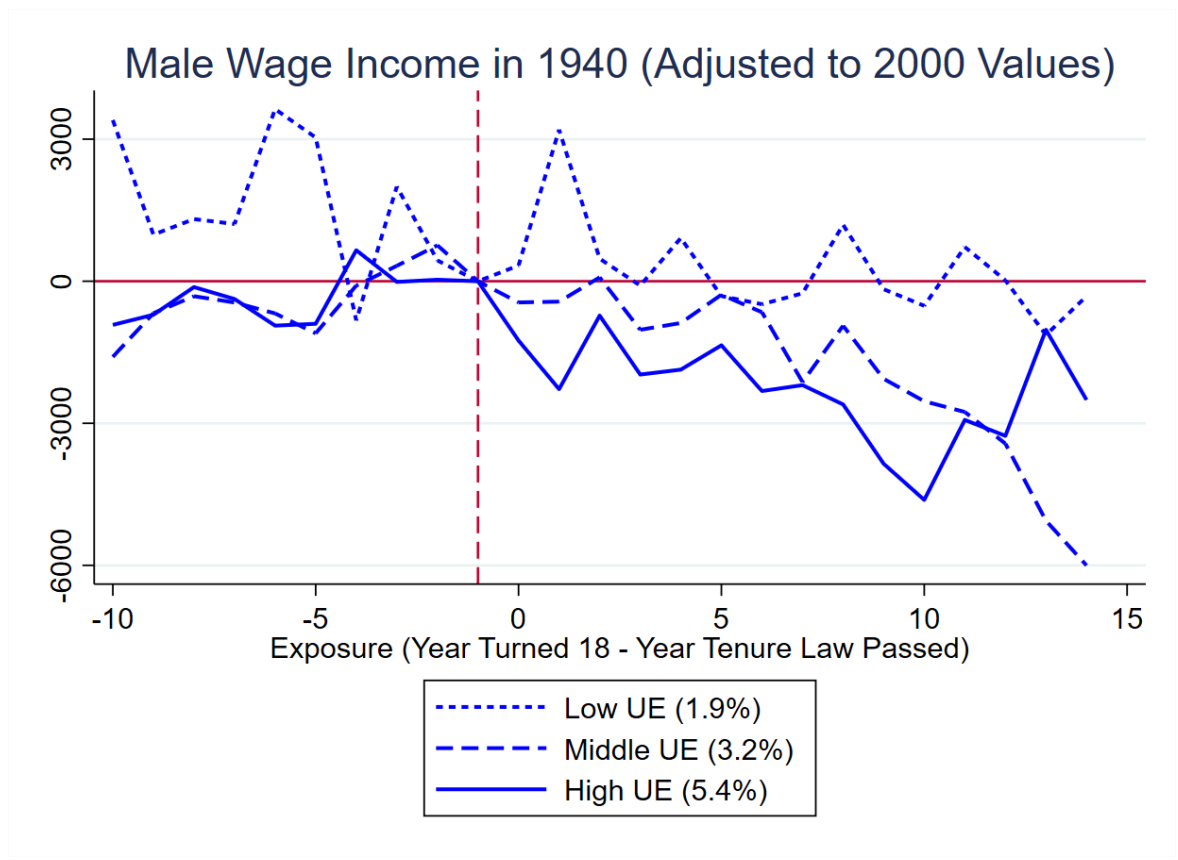
Note: This figure displays the relationship between the 1910 teacher unemployment rate and the impact of tenure laws on men's wage income. The reported coefficients are from an event-study model estimates separately for three state groups. The three-state groups are split based on their 1910 teacher unemployment rate (low, medium, high). The sample is limited to adult men in the 1940 Census. Furthermore, these men must have been linked to an earlier Census in which they are of school age. The control samples for eacht reated state is limited to states that have a teacher unemployment rate that is +/- one percent of the treated group. Standard errors are clustered at the earliest observed state of residence. p<0.01, ** p<0.05, * p<0.1.

Additional Controls: The model includes division-by-cohort fixed effects. Additionally, it also includes a Bartik measure of exposure to the Great Depression between 1929-1933, exposure to school-age laws, and the proportion of individuals in each cell who are black, Hispanic, or other. Childhood family characteristics are accounted for with indicators for father's occupation, dummy variables for if the individual resided with the father, if they resided with the mother, whether the father reported an occupation, whether the mother reported an occupation if they resided in an urban residence and linear terms in the number of siblings and the age of the mother and father.

11 Tables

Table 1. Teacher Tenure Implementation by States

State	Year of Implementation	State	Year of Implementation
Alabama	1939	Nebraska	1943†, 1975
Arizona	1950	Nevada	1968
Arkansas	1970	New Hampshire	1957
California	1921	New Jersey	1910
Colorado	1921†, 1949*, 1967	Nex Mexico	1945
Connecticut	1914ˢ, 1939ˢ, 1955	New York	1917
Delaware	1956	North Carolina	1972
Florida	1937-1941†, 1951	North Dakota	1967
Georgia	1937-1939ˢ, 1968ˢ, 1975	Ohio	1941
Idaho	1955	Oklahoma	1967
Illinois	1919†, 1941	Oregon	1913†, 1973
Indiana	1927	Pennsylvania	1937
Iowa	1945	Rhode Island	1946
Kansas	1937ˢ, 1974	South Carolina	1941ˢ, 1974
Kentucky	1942	South Dakota	1969
Louisiana	1922†, 1936	Tennessee	1937-1941ˢ, 1951
Maine	1959	Texas	1967*
Maryland	1921	Utah	1973
Massachusetts	1914	Vermont	1975
Michigan	1937*, 1964	Virginia	1968
Minnesota	1927†, 1951	Washington	1956
Mississippi	1977	West Virginia	1940
Missouri	1943†, 1970	Wisconsin	1921
Montana	1915	Wyoming	1967

Note: The table displays the year a state implemented a teacher tenure regime. Each state can pass several tenure laws. First, states implement tenure regimes that cover the whole state. The years that represent such tenure laws have no superscripts. Then, there are three types of local tenure laws that a state can implement. These are regional tenure regimes that cover a sizable portion of the state (†), or a trivial percentage (typically less than 12) of the state (ˢ). Finally, states also pass tenure laws that allow school districts to implement a voluntary tenure system (†), where previously they were prohibited from doing so. Certain table entries represent a range of years. These represent a spate of local laws implemented during a period, and I post the range for brevity.

Table 2. The Effect of Tenure Systems in 1940

	(1)	(2)	(3)	(4)
Panel A. Male Wage Income				
At 12 Years	-1,881.70***	-1,047.22***	-1,130.74***	-1,000.36***
	(321.08)	(208.76)	(204.16)	(198.95)
Percent effect at 12 years	-13.81	-7.68	-8.30	-7.34
Panel B. Female Wage Income				
At 12 Years	-1,170.65***	-565.49***	-542.81***	-503.43***
	(169.37)	(173.79)	(143.76)	(140.75)
Percent effect at 12 years	-17.14	-8.28	-7.95	-7.37
Panel C. Male Years of Education				
At 12 Years	0.12	0.13	0.11**	0.08**
	(0.12)	(0.09)	(0.05)	(0.04)
Percent effect at 12 years	1.48	1.56	1.26	0.92
Panel D. Female Years of Education				
At 12 Years	-0.11	0.02	-0.01	0.01
	(0.09)	(0.07)	(0.05)	(0.06)
Percent effect at 12 years	-1.21	0.24	-0.08	0.17
Region-Year FE		X		
Division-Year FE			X	X
Demographic and Policy Controls				X

Note: This table display results from an event-study model that estimates the effect of tenure laws on adult wage income and educational attainment. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. Demographic and Policy Controls include a Bartik measure of exposure to the Great Depression between 1929-1933, exposure to school-age laws, and the proportion of individuals in each cell who are black, Hispanic, or other. Standard errors clustered at the birth state and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. The Effect of Tenure Systems in 1960 to 2000

	(1)	(2)	(3)	(4)
Panel A. Male Wage Income				
At 12 Years	507.12	383.02	67.78	-291.85
	(579.88)	(349.37)	(343.51)	(403.46)
Percent effect at 12 years	1.30	0.98	0.17	-0.75
Panel B. Female Wage Income				
At 12 Years	371.77	411.29**	189.56	147.82
	(246.61)	(201.29)	(227.05)	(195.19)
Percent effect at 12 years	2.12	2.34	1.08	0.84
Panel C. Male Years of Education				
At 12 Years	0.07	0.07	0.03	-0.00
	(0.07)	(0.05)	(0.05)	(0.05)
Percent effect at 12 years	0.57	0.53	0.26	-0.03
Panel D. Female Years of Education				
At 12 Years	0.10	0.08	0.02	-0.01
	(0.06)	(0.05)	(0.05)	(0.05)
Percent effect at 12 years	0.77	0.61	0.20	-0.05
Region-Year FE		X		
Division-Year FE			X	X
Demographic and Policy Controls				X

Note: This table display results from an event-study model that estimates the effect of tenure laws on adult wage income and educational attainment. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. 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 Early Tenure Regimes on Immigrants

	(1)	(2)	(3)	(4)
Panel A. Placebo (Immigrated After Age 15)				
At 8 to 18 Years	40.89	-4.69	-156.43	-111.82
	(341.16)	(0.00)	(313.52)	(192.78)
Percent Effect at 8 to 16 Years	0.27	-0.03	-1.04	-0.74
Panel B. Treated (Immigrated Before Age 15)				
At 10 to 19 Years (Low UE)	-2,043.28***	-2,277.43***	-1,906.92***	-1,896.50***
	(344.84)	(274.38)	(356.66)	(505.14)
Percent Effect at 8 to 16 Years	-14.32	-15.97	-13.37	-13.30
Region-Year FE		X		
Division-Year FE			X	X
Controls				X

Note: This table display results from an event-study model that estimates the effect of tenure laws on adult wage income. The model is estimated using linked decennial Census data (Helgertz et al., 2020). The sample in Panel A is limited to individuals who immigrated to the US after age 15. The sample in Panel B is limited to individuals who immigrated to the US before age seven. Treatment is assigned to both groups based on the state they resided in the 1930 Census. The model controls for census division by birth cohort fixed effects, birthplace indicators and a Bartik measure of exposure to the great depression between 1929-1933. Standard errors are clustered at the 1930 state of residence. p<0.01, ** p<0.05, * p<0.1.

Table 6. The Effect of Later Tenure Regimes on Teachers

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Teacher Retention						
At 10 to 19 Years	-0.010	-0.005	-0.003	-0.004	-0.015	-0.015
	(0.015)	(0.016)	(0.020)	(0.018)	(0.017)	(0.018)
Panel B. Weekly Hours						
At 10 to 19 Years	-0.585	-0.627	-0.146	-0.121	-0.747**	-0.777
	(0.461)	(0.396)	(0.250)	(0.286)	(0.308)	(0.475)
Panel C. Years of Education						
At 10 to 19 Years	0.031	0.007	0.080	0.076	-0.092	-0.121
	(0.089)	(0.070)	(0.084)	(0.072)	(0.081)	(0.084)
Panel D. Wage Income						
At 10 to 19 Years	705.961	156.898	493.681	599.613	-681.579	-595.495
	(926.284)	(993.753)	(913.213)	(874.155)	(1,174.450)	(1,116.443)
Panel E. Teacher is African American						
At 10 to 19 Years	-0.025*	-0.015	-0.019*	-0.017*	-0.027	-0.025
	(0.012)	(0.010)	(0.011)	(0.010)	(0.026)	(0.024)
Policy Controls		X		X		X
Region-Year FE			X	X		
Division-Year FE					X	X

Note: The table displays results based on equation (1). Panel A uses the Current Population Survey Annual Social and Economic Supplement for the years 1963-2000. The outcome variable is an indicator equal to one if an individual, who was previously a teacher, is currently a teacher. The sample is limited to individuals who were public school teachers in the previous year. Panels B to D use the Decennial Census for the years 1940 to 2000. The outcome variables are either weekly hours (Panel B), reported years of education (Panel C), wage income (Panel D) or an indicator equal to one if the teacher is African American (Panel E). The sample consists of teachers who are presently in the labor force. The policy controls are indicator variables for duty-to-bargain laws, court-mandated school finance reforms, and legislated school finance reforms. Standard errors are clustered at the state-group level (Panel A) or state level (Panels B to E). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 7. The Effect of Early Tenure Regimes on Teacher Retention

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Teacher Retention						
At 10 to 19 Years	0.040**	0.020	0.021	0.026*	0.051***	0.062***
	(0.019)	(0.022)	(0.023)	(0.013)	(0.017)	(0.012)
Panel B. Teacher Retention by UE						
At 10 to 19 Years (Low UE)	0.028	-0.015	-0.016	0.009	0.017	0.020
	(0.025)	(0.023)	(0.024)	(0.012)	(0.019)	(0.017)
At 10 to 19 Years (Medium UE)	0.082***	0.086***	0.064***	0.071***	0.081***	0.075***
	(0.016)	(0.010)	(0.013)	(0.018)	(0.010)	(0.007)
At 10 to 19 Years (High UE)	0.034*	0.047	0.072**	0.015	0.044	0.055**
	(0.019)	(0.032)	(0.028)	(0.020)	(0.036)	(0.027)
Panel C. Prob > F						
Low UE = Medium UE	0.063	0.000	0.005	0.006	0.005	0.005
Low UE = High UE	0.830	0.123	0.021	0.782	0.518	0.276
Region-Year FE		X			X	
Division-Year FE			X			X
Similar 1910 Teacher UE				X	X	X

Note: The table displays results based on equation (1) using linked decennial Census data (Abramitzky et al., 2020). The sample is limited to men who reported a previous occupation in the previous decade. The outcome variable is an indicator equal to one if the individual is still a teacher in the same county. In Panel B, the event study variables are interacted with an indicator variable for whether the state had a low, medium, or high teacher unemployment rate in 1910. Panel C reports the p-value from an F-test for whether the reported coefficients from Panel B are equivalent. Similar 1910 Teacher UE limits each state's comparison group to states whose 1910 teacher unemployment rate is within one percentage point of the treated state. Standard errors are clustered at the state of residence reported in the previous decade. $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 8. The Effect of Early Tenure Regimes on Teacher Demographics

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Married and Female						
At 10 to 19 Years	0.020	0.036***	-0.020**	0.035*	0.045***	0.024
	(0.013)	(0.011)	(0.009)	(0.019)	(0.013)	(0.018)
Panel B. Married and Female by UE						
At 10 to 19 Years (Low UE)	0.015	0.029**	0.032**	0.013	0.045***	-0.003
	(0.019)	(0.012)	(0.012)	(0.020)	(0.014)	(0.017)
At 10 to 19 Years (Medium UE)	0.011	0.014	-0.001	0.020	0.012	-0.002
	(0.015)	(0.010)	(0.013)	(0.020)	(0.012)	(0.013)
At 10 to 19 Years (High UE)	0.040***	0.077***	0.067**	0.051***	0.078***	0.070***
	(0.012)	(0.025)	(0.027)	(0.014)	(0.024)	(0.025)
Panel C. Prob > F						
Low UE = Medium UE	0.870	0.312	0.069	0.753	0.082	0.960
Low UE = High UE	0.257	0.087	0.237	0.086	0.250	0.022
Region-Year FE		X			X	
Division-Year FE			X			X
Similar 1910 Teacher UE				X	X	X

Note: The table displays results based on equation (1) using linked decennial Census data (Abramitzky et al., 2020). The sample is limited to individuals who report a teaching occupation. The outcome variable is an indicator equal to one if the individual is a married female teacher. In Panel B, the event study variables are interacted with an indicator variable for whether the state had a low, medium, or high teacher unemployment rate in 1910. Panel C reports the p-value from an F-test for whether the reported coefficients from Panel B are equivalent. Similar 1910 Teacher UE limits each state's comparison group to states whose 1910 teacher unemployment rate is within one percentage point of the treated state. Standard errors are clustered at the state of residence. $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 9. The Effect of Early Tenure Regimes on Male Wage Income by Teacher Unemployment Rate

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Male Wage Income						
At 10 Years (Low UE)	-1,114.85***	-544.22***	-429.70***	-607.70***	118.44*	327.98
	(195.31)	(185.05)	(137.46)	(193.40)	(60.20)	(293.90)
At 10 Years (Medium UE)	-1,429.49**	-1,128.60**	-924.25***	-987.76	-1,262.63***	-1,005.03***
	(651.52)	(437.54)	(336.92)	(622.83)	(444.53)	(366.92)
At 10 Years (High UE)	-1,697.86***	-1,636.57***	-1,714.80***	-1,496.52***	-1,534.12***	-1,733.24***
	(270.50)	(223.26)	(288.28)	(239.51)	(190.99)	(218.61)
Panel B. Prob > F						
Low UE = Medium UE	0.635	0.222	0.186	0.562	0.004	0.006
Low UE = High UE	0.078	0.000	0.000	0.009	0.000	0.000
Region-Year FE		X			X	
Division-Year FE & Additional Controls			X			X
Similar 1910 Teacher UE				X	X	X

Note: This table displays the relationship between the 1910 teacher unemployment rate and the impact of tenure laws on men's wage income. The reported coefficients are from an event-study model estimates separately for three state groups. The three-state groups are split based on their 1910 teacher unemployment rate (low, medium, high). The sample is limited to adult men in the 1940 Census. Furthermore, these men must have been linked to an earlier Census in which they are of school age. Similar 1910 UE refers to limiting the states that make up the control group to have a teacher unemployment rate that is +/- one percent of the treated group. Standard errors are clustered at the earliest observed state of residence. $p<0.01$, ** $p<0.05$, * $p<0.1$.

Additional Controls: The model includes division-by-cohort fixed effects. Additionally, it also includes a Bartik measure of exposure to the Great Depression between 1929-1933, exposure to school-age laws, and the proportion of individuals in each cell who are black, Hispanic, or other. The controls for early life circumstances are cell values (either the mean or the fraction) for the father's occupation, childhood urban residence, presence of a father, presence of a mother, father's age, mother's age, and the number of siblings.

12 Appendix Tables and Figures

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.

Table A-3 The Effect of Tenure Systems in 1950

	(1)	(2)	(3)	(4)
Panel A. Male Wage Income				
At 12 Years	-2,016.94	-2,210.82	-2,050.58	-2,154.23
	(1,275.61)	(1,420.35)	(1,613.31)	(1,774.32)
Percent effect at 12 years	-9.72	-10.66	-9.89	-10.38
Panel B. Female Wage Income				
At 12 Years	-3,090.89**	-2,813.90*	-3,079.02*	-3,125.35*
	(1,172.30)	(1,654.89)	(1,702.84)	(1,740.48)
Percent effect at 12 years	-28.92	-26.33	-28.81	-29.25
Panel C. Male Years of Education				
At 12 Years	-0.13	0.10	-0.52*	-0.60*
	(0.17)	(0.36)	(0.29)	(0.31)
Percent effect at 12 years	-1.31	1.06	-5.48	-6.29
Panel D. Female Years of Education				
At 12 Years	-0.02	0.21	0.05	0.03
	(0.19)	(0.26)	(0.19)	(0.15)
Percent effect at 12 years	-0.16	2.19	0.52	0.35
Region-Year FE		X		
Division-Year FE			X	X
Demographic and Policy Controls				X

Note: This table display results from an event-study model that estimates the effect of tenure laws on adult wage income and educational attainment in 1950. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. Demographic and Policy Controls include exposure to school-age laws, and the proportion of individuals in each cell who are black, Hispanic, or other. Standard errors clustered at the birth state and reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix Figure 1. Long-run Outcomes in 1950
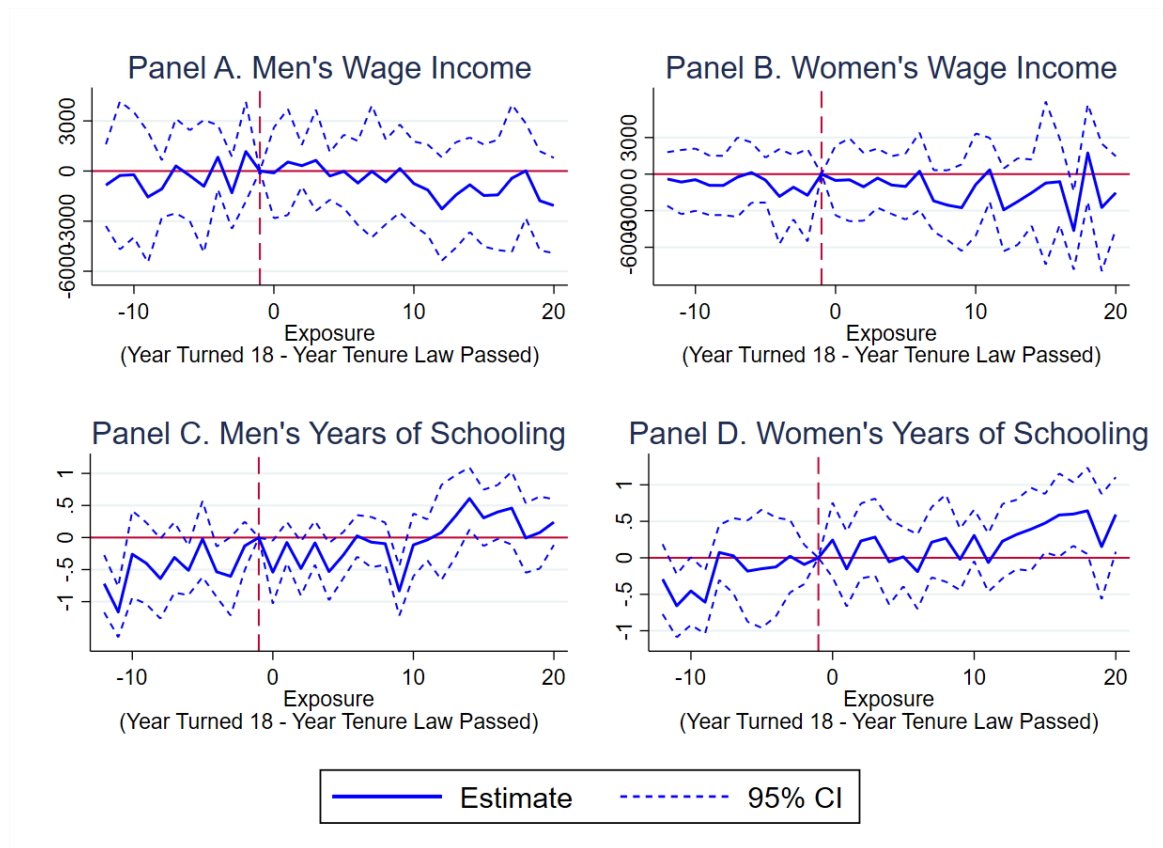
Note: This figure plots coefficients and the 95 percent confidence interval from an event-study model that estimates the effect of tenure laws on adult wage income and educational attainment in the 1950 Census. The sample is limited to individuals who are born in the continental United States and are between the ages 25 to 54. The specification controls for the proportion of individuals in each cell who are black, Hispanic, or other as well as birth region by cohort fixed effects. Standard errors clustered at the birth state and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.