

The Effects of Letting Married Women Work: Evidence from Teaching in the U.S., 1900-1950*

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February 22, 2024

PRELIMINARY DRAFT. PLEASE DO NOT CIRCULATE.

Abstract

For much of the early twentieth century, married women faced explicit employer discrimination that prevented many married women from entering the labor force. However, little is known about the extent to which the end of these discriminatory practices, independent of changes in married women’s preferences for work, affected married women’s labor supply. We gain traction on this question by studying the introduction of a unique set of employment protections in the 1930’s that made employer discrimination on the basis of marital status illegal in a specific occupation—teaching—in KY and NC. To identify the effects of the employment protections on women’s employment and marriage decisions, we exploit the timing of the laws in KY and NC and the fact that neighboring states, for plausibly exogenous reasons, did not pass similar legislation. We find that the employer protections increased the share of teachers who were married women by 4 p.p. (26%) in treated counties relative to control ones. In particular, the employer protections altered the trade-off between work and marriage for (1) married unemployed women and (2) single women teachers: in response to the employer protections being passed, married unemployed women became more likely to become teachers, and single women teachers became more likely to get married and stay in teaching rather than get married and leave the workforce.

*We thank Thomas Le Barbanchon, Zach Bleemer, Emilio Borghesan, Leah Boustan, Charissa Jefferson, Ilyana Kuziemko, David Lee, Alex Mas, Garima Sharma, Damian Vergara, and participants of the Industrial Relations Workshop at Princeton University for their helpful comments and suggestions. We are grateful for the financial support of the Industrial Relations Section at Princeton; additionally, Kim acknowledges financial support from the National Science Foundation and Tsao acknowledges financial support from Prize Fellowship in Social Sciences at Princeton and the Social Sciences and Humanities Council of Canada (SSHRC). All errors are our own.

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1 Introduction

One of the most notable labor market shifts of the 20th century was that married women became significantly more likely to work outside the home, with the percentage of married women in the U.S. labor force growing from 6% in 1900 to 61% in 2010. What factors explain the growth in married women’s labor supply? It is well-documented that from the 1960s onwards, the introduction of new technologies such as contraceptives and household appliances changed the within-household cost-benefit analysis underlying married women’s decision to seek work outside the home [Goldin and Katz, 2002, Greenwood et al., 2005, Bailey, 2006, Abou Daher et al., 2023]. However, less is known about the extent to which *the removal of institutional barriers* affected the growth in married women’s labor supply earlier on. Until 1950, many married women did not have the option to work due to the widespread employer practice of not hiring married women and even firing women employees upon marriage [Goldin, 1988]. Yet despite the prevalence of these discriminatory practices, it remains an open question how much the *end* of these practices—that is, firms *letting married women work*—affected married women’s labor supply. The effects are particularly difficult to identify as (1) the use of the discriminatory hiring practices declined gradually rather than all at once, and (2) the decline overlapped with other institutional changes that simultaneously influenced the entry of married women into the labor force in the 1940s, such as higher labor demand during World War II and rising wages [Goldin, 1991, Galor and Weil, 1996, Rose, 2018].

This paper examines the effects of a policy change that allows us to gain insight for the first time into whether, how, and by how much the removal of institutional barriers for married women brought married women into the workforce. Specifically, we measure how married women’s labor force participation was affected by the introduction of employment protections for married women in *teaching* in the 1930’s. The employment protections took the form of state-wide legislation passed in only two states—Kentucky and North Carolina—that made it illegal for school districts to discriminate against teachers based on their marital status when making hiring or firing decisions.

Our main empirical design is a *difference-in-differences* design which we use to compare outcomes in states that introduced employment protections to neighboring Southern states that did not.¹ First, we estimate the overall effect of the passing of employment protections on the employment of married women in teaching. We then examine how the effects of the employment protections may have heterogeneously impacted the trade-off women faced between work and

¹Southern states, including KY and NC, differed from the rest of the country in the extent to which married women worked and the demand for teachers, making it difficult to compare either KY or NC to their non-Southern neighboring states. See Section 3.3 for further discussion.

marriage, especially for those who were already in teaching.

Our approach is made possible by features of our setting that allow us to overcome two key challenges inherent to identifying the effects of removing barriers to employment on women’s labor supply: (1) data availability and (2) selection. Although firms barred the employment of married women across many occupations, there is no systematic data on which firms did so at what points in time. In addition, the timing of individual firms’ decisions to allow married women to work may have been correlated with unobserved trends that simultaneously affected women’s employment. In teaching however, a women-dominated occupation in which an estimated 70-80% of school districts discriminated against married women at one point between 1930 and 1940 [Goldin, 2021], we observe both (1) detailed documentation of employment policies relative to other occupations, and (2) plausibly exogenous introductions of employment protections for married women in several states.

The validity of our empirical design rests on the identifying assumption that married women’s outcomes would have evolved in parallel between the “treated” (KY, NC) and “control” states (neighboring Southern states) if the employment protections had not been passed. We find three pieces of suggestive evidence that the “parallel trends” assumption holds. First, we find qualitative evidence from reports in newspaper archives that show both treated and control states experienced similar policy discussions around protections for teachers at the time, suggesting that the employment protections were passed in some states but not others for seemingly idiosyncratic reasons. Secondly, using Gallup polling data from 1938, we find that the general public in treated and control states held similar views on whether married women should work [Gallup Organization, 1938]. Finally, we find evidence of parallel pre-trends in our outcomes of interest prior to 1930.

Ex-ante, the effect of employment protections on the labor force participation of married women is unclear. If, for instance, the low labor force participation of married women in the first half of the 20th century was entirely driven by the individual household’s cost-benefit analysis, then married women would have chosen not to work even absent firms’ discriminatory hiring practices. In such a world, we would expect the introduction of employment protections to have no effect on married women’s labor force participation.

Our first main finding is that the passing of employment protections in teaching *increased* the employment of married women in teaching overall. Treated counties experienced a 4 p.p. increase between 1930 and 1940 (a 26% increase) in the share of teachers that were married women. We therefore reject the null hypothesis that the discriminatory hiring practices had no effect on the labor supply of married women. These results suggest that the employment protections played a non-trivial role in the growth in the labor force participation of married women,

at least in teaching, during the 1930s and 1940s. By 1950 however, the gap between treated and control counties in the share of teachers that were married women closed, suggesting that control counties quickly ‘caught up’ to treated counties in eliminating the use of discriminatory hiring practices against married women, resulting in similar levels of employment of married women in teaching by 1950.

Additionally, we find that the increase in married women teachers was entirely offset by a decrease in unmarried women teachers, with no change in either the share of teachers that were men or in the total number of teachers. We interpret the maintained ratio of men-to-women teachers as being consistent with two common beliefs that schools and other firms held at the time: (1) that men and women workers were imperfect substitutes, and/or (2) that employing men took priority over employing women.²

In theory, although the protections increased married women’s employment in teaching overall, the effects may have varied by women’s baseline marital status and occupation. Prior to the protections being passed, some single women teachers may have been delaying or forgoing marriage in order to continue working, while others may have been quitting their teaching job upon marriage knowing that they would be forced to leave anyway. For those who were delaying or forgoing marriage, the employment protections may have induced them to get married earlier *and* continue working, while for those who were quitting upon marriage, the employment protections may have induced them to get married as planned *but continue working*. Meanwhile, for all other women not teaching at baseline, the new legal protections may have induced them to enter teaching, potentially joining the workforce at the extensive margin. Our goal is to test which of these effects are non-zero: although the theoretical direction of each effect is unambiguous, the actual magnitudes may be small and insignificant if the original employer discrimination against married women was not actually a primary factor in women’s work and marriage decisions.

Indeed, our second main finding is that the employer protections did have heterogeneous effects on different groups of women. Single women teachers did not become more likely to marry in treated counties relative to control ones, indicating that laws did not induce changes in the incidence of marriage. Instead, we find that conditional on getting married, single women teachers in treated counties became more likely to *stay in teaching* rather than exit the workforce. In addition, we find that married women who were not teaching prior to the laws also became more likely to enter teaching in treated counties relative to control ones: the effect

²Schools that held the former belief might have allocated men and women teachers to different types of teaching positions (e.g. high schools or elementary schools) based on beliefs about comparative advantages, while schools that held the latter belief might have only hired a married woman at the expense of letting go of an unmarried woman to uphold to the norm that men needed jobs to provide for their families.

size, while small, is remarkable given that so few White married women worked during this time period.

Overall, our results provide new empirical evidence that *the removal of institutional barriers* specific to married women contributed to the growth in married women’s labor force participation in the early 20th century. We find that employment protections for married women played an active role in bringing married women into the labor force in an important, albeit single, occupation, by inducing women who would have otherwise gotten married and quit their teaching jobs to instead get married and stay in teaching. Our study provides causal evidence that institutional barriers delayed married women’s entry into the labor force, above and beyond the restricting social norms of the early 1900s.

This paper adds to our understanding of how women in the U.S. have historically been affected by institutional barriers to employment and the subsequent policies that have sought to remove such barriers [Doepke and Tertilt, 2009, Doepke et al., 2012, Cascio et al., 2015, Hyland et al., 2020, Goldin, 2021, Tertilt et al., 2022, Bailey et al., 2023, Marchingiglio and Poyker, 2021]. Few other papers study the type of employer discrimination that married women faced in the early 1900s. Our paper most closely relates to Goldin [1988], in which Goldin provides extensive documentation of employer discrimination (both formal and informal) against married women—often called “marriage bars” colloquially and in the economic history literature.³ In particular, she explores the economic justifications firms made for using marriage bars and compiles survey evidence documenting how widespread marriage bar use was. We build upon Goldin [1988] by providing the first quasi-experimental estimates of how employment protections that effectively banned marriage bars affected the labor market outcomes of U.S. women.

This paper also contributes to our understanding of the factors that led to the rise in women’s labor force participation throughout the twentieth century, such as World War II [Goldin, 1991, Acemoglu et al., 2004, Rose, 2018], the introduction of oral contraceptives [Goldin and Katz, 2002, Bailey, 2006], shifting cultural attitudes [Fernández, 2007], and the 1963 Equal Pay Act and 1964 Civil Rights Act [Bailey et al., 2023]. The bulk of the literature studies large-scale factors in the second half of the century, during which married women faced less discrimination and/or were protected by law and allowed to work in most occupations. In these studies, the induced changes in married women’s labor supply were the result of increasing benefits of working outside the home relative to the costs of doing so. Our paper complements the existing literature by studying the removal of a potentially large-scale factor in the first half

³A few papers study marriage bars in other countries, including Mosca and Wright [2020] and Mosca et al. [2021], who study the long-term effects of the marriage bar on teachers in Ireland.

of the century, during which married women faced explicit and legal employer discrimination.

Finally, we build on a large body of research that focuses on factors affecting women’s labor supply, ranging from child care and distance to family [Connelly, 1992, Compton and Pollak, 2014] to job amenities [Black et al., 2014, Mas and Pallais, 2020] to discrimination [Neumark et al., 1995, Goldin and Rouse, 2000, Balafoutas and Sutter, 2012]. These studies can be broadly thought of as measuring the effect of a given policy change on the *marginal woman*’s propensity to work. However, the marginal woman induced to work by a policy change today may be very different from the marginal woman induced to work by a policy change in the 1930s, when there was far less precedent for married women to work outside the home at all. In this paper, we are able to estimate the causal effect of removing institutional barriers on a particular group of women with a relatively high propensity to work, potentially capturing effects that are more difficult to pick up in a modern setting, where such women are all working already.

The rest of the paper continues as follows. Section 2 describes the historical context of marriage bars and the teaching profession in the early twentieth century, including the justifications for marriage bars across occupations and the circumstances around the unique bans on marriage bars in teaching. Section 3 describes the data we use and the standard differences-in-differences methodology. Section 5 describes the effects of the marriage bar bans on women’s outcomes. Section 6 concludes.

2 Employer Discrimination against Married Women in the Early Twentieth Century

This section provides historical background on the evolution of the institutional barriers to employment that married women faced in the first half of the twentieth century, both nationwide and specifically in the teaching profession.

2.1 The Evolution of Marriage Bar Use from 1900 to 1960

The class of discriminatory employment practices that excluded married women from the workplace are often referred to as ‘marriage bars.’ Marriage bars began to emerge across the world throughout the late 1800s and early 1900s, and have been termed “the most numerically important of all prohibitions in their impact on the employment of married women” [Goldin, 1988]. In the U.S., marriage bars were popular among firms that employed women as clerical workers (e.g. in banking, insurance, etc.) and government agencies that employed women as teachers (i.e. school districts). Women experienced marriage bars in two ways: married women were

either not hired due to their marital status (“hire bars”), or single working women who got married were fired or expected to quit upon marriage (“retain bars”). Firms practiced one or both forms of discrimination, either by implementing formal rules to not employ married women at the firm level or by using their discretion on a case-by-case basis.

Firms viewed marriage bars as favorable personnel policies for three reasons. First, in light of the general social consensus on the appropriate roles of women and men in society and in the household, it was widely believed that men, rather than married women, were the ones meant to support their families. There was therefore a perceived social cost to offering a job to a married woman who had a husband to provide for them.⁴

Second, it was believed that due to their household responsibilities, married women were less efficient workers than unmarried women and men (“the married woman lacks genuine interest in her work” [Cooke and Simms, 1940]). Not employing married women was thus justified on the basis that single women were more reliable workers than married women, though ironically the stereotype was reversed once married women entered the labor force *en masse*.

Third, many firms used internal promotion practices and tenure-based salary schedules, both of which incentivized firms to maintain high turnover of employees. Firing married women was thus a convenient and socially acceptable way to avoid paying the higher salaries associated with longer tenures for a particular subset of workers [Goldin, 1988]. Teaching was a key example of an occupation that featured fixed salary schedules in the majority of school districts as early as the 1920s.

Although marriage bars were widely used, there is no systematic record of marriage bar use across U.S. firms. The available data on firm-level marriage bar use largely comes from a handful of surveys that were carried out between 1931 and 1956 asking non-representative samples of firms about their policies concerning married women. The surveys show that discretionary marriage bar policies were especially common: in 1936, 50-60% of factories and offices in a survey conducted by Purdue University reported using formal or discretionary marriage bar restrictions [Mosca and Wright, 2021]. Formal marriage bar policies were less common, but still affected many working women due to the greater likelihood that large firms adopted formal policies: in 1931, 12% of firms surveyed in five large cities by the U.S. Department of Labor reported having a formal policy in place, affecting 25% of the women employed [Goldin, 1988].

The most comprehensive data on marriage bar use was inadvertently collected by the National Education Association (NEA) in their surveys of school districts in 1928, 1930-31, 1942, and 1950-51 [Goldin, 2021]. The NEA surveys also illustrate how marriage bar use fluctuated

⁴This sentiment was succinctly summarized by the Irish Department of Education in 1932: “married women teachers restricted opportunities for other women and created social tensions if married to a farmer, shopkeeper or teacher” [Mosca and Wright, 2021].

with the business cycle from 1920 to 1950. “Hire bars” in school districts—the practice of not hiring married women—affected 60% of the urban population in 1928 prior to the Great Depression, which increased to 73% in 1930-31 and nearly 80% in 1942. Similarly, “retain bars” in school districts—the practice of firing a woman teacher upon her marriage—affected around 50% to 60% of the urban population over the same time period. Marriage bar use increased over the course of the Great Depression, a trend that has been attributed to rising unemployment and scarcity of jobs for men.⁵ But by 1950, marriage bar use in schools had declined significantly, with the share of the urban population affected by school districts’ hire and retain bars falling to around 17% and 10% respectively in 1950-51.

The steep decline in school districts’ use of marriage bars between 1940 and 1950 mirrored a society-wide trend towards inclusion of married women in the workforce. After World War II, unemployment was near zero and demand for workers was high. It became too costly for firms to continue excluding older, married women from the workforce [Goldin, 1988]. As such, marriage bar use in the U.S. quickly declined and largely ended by the 1960s.⁶ Incidentally, rhetoric around the efficiency of married women workers also flipped during this time period, with older women being praised for their “maturity,” “steadiness,” and “reliability,” in stark contrast with the earlier justifications in favor of using marriage bars.

2.2 The Introduction of Employer Protections in Teaching

School districts were the most prominent employers that discriminated against married women throughout the early twentieth century. School districts’ use of marriage bars is particularly notable as teaching was a women-dominated occupation and one of the few socially-accepted occupations for educated women at the time—in 1940, 31% of married women in the workforce with any college were teachers.⁷

School districts rationalized using marriage bars for the same reasons cited by the firms described in the previous section: job scarcity for men, the perceived inefficiency of married women workers, and the incentives to maintain high turnover to keep labor costs low for salaried workers. However, unlike in other occupations, discriminatory hiring policies in teaching were particularly contested in debates over tenure protection for teachers, which took place across

⁵There was even federal legislation, such as Federal Order 213 in the Federal Economy Act of 1932, that mandated that “executive branch officials... fire workers whose spouses were employed by the federal government,” and was largely used to fire married women [Goldin, 1988].

⁶Note that for some occupations, such as airline stewardess, marriage bars persisted until decades later [Associated Press, 1986].

⁷The importance of teaching as an occupation for married women has persisted to the twenty-first century, too: in 2000, 12% of married women with any college were teachers.

the country from the 1910s onward.⁸ A non-trivial number of women who were dismissed on the basis of marriage took the offending school boards to court. Newspaper archives show that the court decisions were mixed, ranging from indicating that local school boards could use their discretion (e.g. in MA, MN, MI, and SC) to indicating that marriage was not a just cause for dismissal (e.g. in NY, WV, OR, and IN) [[Associated Press, 1934, 1938](#)]. By 1931, localities in nine states had passed tenure legislation for teachers that included protection against dismissal due to marital status; by 1939, the number increased to thirteen, and by 1943 to thirty-three [[Cooke et al., 1943](#)].

Importantly, although the tenure laws that protected teachers from being dismissed upon marriage became more common from 1920 to 1940, the majority of such laws were not statewide in their application. Repeated cross-sectional data on the county-level share of teachers who were married women confirms that marriage bar use was local, as some districts hired substantially higher rates of married women than others. That said, legislators in multiple states attempted to pass state bills declaring it unlawful to discriminate against married women. Some, like that introduced by the sole woman legislator in Virginia in 1932, failed [[Associated Press, 1932](#)].

By 1940, only two states—Kentucky and North Carolina—had passed state-level legislation containing employment protections that explicitly prohibited discrimination against married women in teaching [[Cooke and Simms, 1940](#)]. The legislation in North Carolina was broad in its application: in 1933, the North Carolina Public Laws Chapter 562 Section 11 declared that “in the employment of teachers no rule shall be made or enforced on the ground of marriage or nonmarriage” [[North Carolina General Assembly, Regular Session, 1933](#)]. The legislation in Kentucky was more specific to experienced teachers: in 1938, House Bill No. 51 in the Kentucky General Assembly included an act “to prohibit boards of education or school superintendents from adopting rules preventing marriage of any school teacher who has had five years or more teaching experience” [[Kentucky General Assembly, Regular, 1st and 2nd Special Sessions, 1938](#)].

We conclude the section by providing descriptive evidence that suggests that the state-wide employment protections in KY and NC may have led to greater employment of married women in teaching. Figure 1 shows the distribution of the fraction of White teachers who were married women, from 1910 to 1950, for counties in KY and NC versus other counties across the rest of the country. Married women gradually entered teaching between 1910 and 1930 in all states, as evidenced by the rightward shift in the distribution means. The variances increase over time as well, indicating that some counties still maintained low shares of married women teachers

⁸By 1922, districts in eleven states offered tenure to teachers with various legislative limitations, but did not explicitly protect married women.

even as married women begin to enter teaching overall. In 1940 however, the mass of the KY and NC distributions shifts right relative to other states, indicating that nearly all counties in KY and NC were hiring married women at relatively higher rates. Finally, by 1950, the other states appear to catch up to KY and NC in terms of married women’s employment in teaching. Our empirical design described in Section 4 leverages this variation in exposure to employment protections to formally evaluate the effects of the bans in KY and NC on women’s employment in teaching.

3 Data

For our analysis, we use full-count U.S. Decennial Census data from 1910 to 1950, which covers all individuals in the US [Ruggles et al., 2024]. In addition to cross-sectional census data, we also use a combination of linkages from the Census Tree [Price et al., 2023a,b,c], the Census Linking Project [Abramitzky et al., 2022a,b,c], and the IPUMS Multigenerational Longitudinal Panel [Helgertz et al., 2023, Ruggles et al., 2021] to link individuals across subsequent censuses.

3.1 Cross-Sectional Sample

For the first part of our analysis, we use repeated cross-sectional samples of teachers in the U.S. Census from 1910 to 1950. We define teachers as adults between the ages of 18 and 64 who report teaching as their occupation and who are not self-employed. While we are interested specifically in the effect of the employment protections for married women on public school teachers, the data do not identify public school teachers separately from other types of teachers. That said, because private schools captured a smaller market in the early twentieth century than they do today, public school teachers likely comprise the bulk of the teachers we identify.⁹

We also restrict our attention to white teachers, to whom the employer discrimination practices were most relevant. Black women in teaching were significantly more likely to be married than white women in teaching in the early twentieth century [Goldin, 2021]; indeed, as a consequence of the systemic racism that black men and women experienced, black married women were more likely to work outside the home than white married women either out of necessity (to support their families) or out of social expectation. Thus, in light of the different

⁹Enrollment in private schools in the early 1900s was low, totalling less than 10% of total elementary and secondary school enrollment [National Center for Education Statistics, 1993]. In addition, our definition may exclude some subject-specific teachers (for instance, music teachers are categorized under a separate occupation label). However, we do not believe this omission would differentially vary across counties nor over time in our treated and control groups.

implications of employment protections for married teachers for black and white women in the 1930s and 1940s, and due the relatively small sample of black teachers at the time, we focus our analysis on white teachers.

3.2 Linked Samples

For the second part of our analysis, we use panel data on samples of women who can be linked between consecutive years of U.S. Censuses from 1910 to 1940.¹⁰ We use the links provided by the Census Tree, which is based on linkages obtained directly from a genealogical website called FamilySearch [Buckles et al., 2023]. Additional linkages are added using a machine learning algorithm trained on the FamilySearch linkages [Price et al., 2021], the Census Linking Project [Abramitzky et al., 2021], and the IPUMS Multigenerational Longitudinal Panel [Helgertz et al., 2023]. By using links reported by family members, the Census Tree data has the added advantage of linking more women than previous methods, since other linkage approaches generally rely at least in part on the last name, which typically changed for women after marriage. To retain as many observations as possible, we only link between adjacent censuses. We also drop the few linkages for which the sex or race is different between Censuses, or for which the implied year of birth varies by more than five years.

3.3 County Sample Selection

Our analysis focuses on Southern states, since as demonstrated by Table 1, there is significant heterogeneity in the baseline characteristics of counties across the country in 1930. Columns (1) and (2) show that in 1930, relative to non-Southern counties, Southern counties had higher labor force participation rates for married women across all occupations (mostly driven by black married women) and had greater shares of teachers who were married women. In addition, Southern counties had lower shares of the population living in urban areas, and married women in Southern counties had more children on average than in non-Southern counties. Southern counties also had significantly more white school-aged students per white teacher, consistent with anecdotal evidence of teacher shortages in the South [Goldin, 2021].

Column (4) shows 1930 summary statistics for counties in KY and NC, the states in which employment protections for married women in teaching were passed in the mid-1930s. Like the average Southern county, counties in KY and NC experienced teacher shortages, exhibited a low share of the population living in urban areas, and saw more children per married woman on average. Unlike the average Southern county however, the probability that a teacher was

¹⁰At the time of writing, linkages to 1950 are not yet available.

a married woman in KY and NC counties was similar to the national average, despite white married women being relatively less likely to work in KY and NC than in the rest of the country.

Finally, Column (3) shows 1930 summary statistics for counties in Southern states that neighbored KY and NC: namely, counties in South Carolina, Virginia, Tennessee, and West Virginia. While these counties were on average largely similar to all Southern counties, they are notably more similar to KY and NC in terms of their labor force participation of white married women and their shares of teachers who were married women in 1930. Because these “Southern neighbor” counties are most similar to KY and NC culturally and statistically, they comprise our preferred comparison group throughout. Our final balanced sample thus consists of 217 treated counties and 310 neighboring Southern control counties.

4 Empirical Strategy

4.1 Main specification

It is unclear *a priori* how much the introduction of employment protections for married women in teaching would have affected women’s labor force participation in the 1930s. Prior work suggests that the relatively low labor force participation of married women in teaching at the time was due to some combination of institutional barriers, such as marriage bars, and household preferences informed by social norms. If marriage bars were the key factor preventing married women from working as teachers, then we would expect employment protections for married women to have some effect on the share of married women in teaching. On the other hand, if institutional barriers played a negligible role, then even with employment protections married women would continue to self-select out of teaching, resulting in no discernible effect of employment protections on the demographic composition of teachers.

We aim to evaluate the impact of the introduction of employment protections in teaching on women’s outcomes by comparing outcomes over time in counties that passed the plausibly exogenous laws—Kentucky and North Carolina—with counties in neighboring Southern states that did not, namely South Carolina, Tennessee, Virginia, and West Virginia. We use a *difference-in-differences* design to evaluate the effects of the state-wide policy changes on the composition of the teacher workforce. Our preferred specification is

$$y_{ct} = \alpha_t^{DD} + \beta_{s(c)}^{DD} + \sum_{k \in \{1910, 1920, 1940, 1950\}} \gamma_k^{DD} \times \text{Treat}_{s(c)} \times \text{Year}_{k=t} + \varepsilon_{ct}, \quad (1)$$

where c indexes county, t indexes year, $s(c)$ is the state of county c , y_{ct} is the outcome variable

of interest, $\text{Treat}_{s(c)}$ is an indicator for whether a county is in a treated state, and α_t^{DD} and $\beta_{s(c)}^{DD}$ capture year and state fixed effects respectively. The main parameter of interest is γ_k^{DD} , which under certain assumptions captures the effect of being in a treated state in year k on county-level outcome y .

4.2 Identifying assumption

Causal inference relies on the “parallel trends” assumption: in the absence of the employment protections in teaching being introduced between 1930 and 1940, our outcomes of interest pertaining to women’s labor force participation in the treated and control counties would have evolved similarly. The main threat to identification is that the passing of the employment protections and the outcomes of interest might have been jointly determined by some omitted variable. For example, if school districts in KY and NC held more progressive views on employing married women on average than neighboring states, then such views may have both driven the passing of the laws in KY and NC and a steady increase in the number of married women employed in teaching in the two states before and after the protections were passed.

We argue that the parallel trends assumption reasonably holds in our setting for three reasons. First, we find suggestive qualitative evidence that state-specific trends in sentiments towards married women teachers did not drive the passing of bills in Kentucky and North Carolina, meaning the main determinant of the bills passing was more likely due to idiosyncrasies in the specific legislators involved. Historical newspaper archives show that tenure protections for teachers were being debated across the country, not only in KY and NC. Throughout the 1930’s, newspapers reported on school districts that explicitly resolved to not renew teaching contracts for married women teachers in e.g. OH, MN, and TN. Court decisions on whether it was just for women to be dismissed on the basis of marital status were mixed, with some courts in MA, MN, WI, SC, CA, KS, and FL upholding the school boards’ right to dismiss while other courts in NY, AL, CA, FL, IL, IN, KY, LA, NJ, NY, OR, TN, and WV did the opposite [Associated Press, 1934, 1938]. Furthermore, KY and NC were not the only states in which bills protecting married women against dismissal were introduced: in Virginia, a Mrs. Emma Lee White introduced a similar bill in 1932 to the Virginia General Assembly which was ultimately unsuccessful [Associated Press, 1932]. We take these data as evidence that the policy discussion and sentiments towards married women teachers were similar in KY and NC and the mix of neighboring Southern states, suggesting that the passing of the laws was the result of as-good-as-random variation in the priorities and actions of the legislators involved.

Second, using public opinion polls, we find suggestive evidence that there were no meaningful differences in public opinion on the employment of married women in teaching between

our treated and control states. Our data come from a 1938 Gallup poll [Gallup Organization, 1938] that asked respondents the following question: “Schools in some states only hire unmarried teachers and discharge them if they get married. Do you approve of this rule?” Since this survey took place several years after employment protections for married women teachers were implemented in North Carolina, we compare responses from respondents in Kentucky with control states only.¹¹ We find that while respondents in Kentucky were slightly less likely to support discriminatory employment policies for married teachers (22.0%, s.e. 4.9%), compared to respondents in neighboring Southern states Tennessee and West Virginia (27.9%, s.e. 5.9%), this gap is not distinguishable from 0 at the 90% confidence level (t-statistic: 0.77). We conclude that there were likely no meaningful differences in norms regarding the employment of married women in teaching that could have violated the parallel trends assumption.

Third, we find that there are no differential pre-trends in our outcomes of interest between the treated and control counties until 1930, as shown throughout Section 5. While a lack of pre-trends is neither necessary nor sufficient evidence that the parallel trends assumption holds, it is re-assuring for our identification strategy that KY and NC were not on dramatically different trajectories from the neighboring states.

5 The Effects of Employment Protections in Teaching

This section presents our main results. Estimates and 95% confidence intervals are presented in Figures 2 and 3 and Tables 2 and 3. In each case, we include as many pre-periods as possible in the regressions and plots to be transparent as to how outcomes trended in treated versus control counties prior to the 1930. Robustness is discussed at the end.

5.1 Effects on Teacher Workforce Composition

We start by estimating the effect of introducing employment protections for married women on the demographic composition of the teacher workforce. Our key outcome of interest is the share of teachers in a county who are married women, which we interpret as a measure of the relative involvement of married women in the local teaching workforce. Column (1) of Table 2, and the purple triangles in Figure 2, show that relative to control counties, the share of teachers that were married women increased by 4.0 p.p. in treated counties between 1930 and 1940, roughly

¹¹Note that 1938, the year that this Gallup poll was conducted, was the same year that employment protections for married women teachers were implemented in Kentucky, which could bias the results if conversations around the policy change were salient for the average person. These results should therefore be interpreted with caution.

a 26% increase from 1930 when the mean share of teachers who were married women was only 15.6%. The effect is significant at the 1% level.

However, we also find that the effect of the employment protections on married women teachers was relatively short-lived. By 1950, the gap between treated and control counties in the share of teachers who were married women shrank to be indistinguishable from zero. These results indicate that as employer discrimination against married women faded nationwide during the 1940's, control counties also began employing married women in teaching in larger numbers, effectively 'catching up' to treated counties. The positive and sizable impact of the employment protections on married women's participation in teaching, at least in the short run, suggests that institutional barriers up until 1950 had played a non-negligible role in keeping married women out of the labor force.

What were the consequences of the influx of married women for the teaching workforce at large? One possibility is that the influx of married women translated into an overall expansion of the teacher labor force, resulting in larger teacher populations overall. Our data, however, rule out this possibility: estimating equation (1) with the total number of teachers in a county as the outcome variable, we find that the employment protections had no effect on the total number of teachers per county (see Column (4) of Table 2 and Appendix Figure A1).

As such, in the absence of a change in the total number of teachers, it must be the case that the increased share of married women teachers resulted in a corresponding decrease in the number of men and/or single women in teachers. To confirm this, we estimate equation (1) using as our outcomes the share of teachers that were men and unmarried women.

The results are shown in Figure 2 and Table 2 as light purple circles (Column (2)) and black squares (Column (3)) respectively. We find that the increase in married women teachers was entirely offset by a 4.3 p.p. (7%) decrease in the share of teachers that were unmarried women, significant at the 1% level.

Notably, the employment protections had no effect on the share of teachers who were men. We interpret the maintained ratio of men-to-women teachers as being consistent with two common beliefs that schools and other firms held at the time: (1) that men and women workers were imperfect substitutes, and/or (2) that employing men took priority over employing women. For example, schools that held the former belief might have allocated men and women teachers to different types of teaching positions (e.g. high schools or elementary schools) based on beliefs about comparative advantages, while schools that held the latter belief might have only hired a married woman at the expense of letting go of an unmarried woman to uphold to the norm that men needed jobs to provide for their families.

5.2 Mechanisms: Effects on Marriage and Labor Force Participation

Our main finding above—that the employment protections increased the share of married women teachers in treated counties—suggests that the employment protections changed the trade-off between work and marriage for some subset of women. We expect theoretically that any effects on marriage or labor supply decisions would have been most apparent for women who were on the margin of getting married or teaching prior to the employment protections being passed. In this section, we explore how the employment protections affected the decisions of two such types of women: (1) those who were single teachers prior to the protections, and (2) those who were married but not working as teachers prior to the protections.

We first focus on single women who were teaching prior to the protections being passed. In the absence of the employment protections being passed, these women may have delayed or forgone marriage in order to continue working, or they may have gotten married and quit their job knowing that they would be forced to leave by the school districts upon marriage anyway. For single women teachers who were planning on delaying or forgoing marriage, the employment protections may have induced them to get married earlier while continuing to work. On the other hand, for single women teachers who were about to quit upon marriage, the employment protections may have induced them to get married as planned *but continue working*.

We test for evidence of these two stories using our linked Census sample. We construct our sample and outcomes as follows. Within the linked sample (described in Section 3) we identify single women under the age of 40 who report teaching in e.g. 1910. We then compute our outcome variables, at the county level, as (1) the share of these women who were married in 1920, (2) the share who were teaching in 1920, (3) the share who were married and teaching in 1920, and (4) the share who were married and not working anymore in 1920. We repeat this process for single women in teaching in 1920 (to compute outcome variables in 1930) and 1930 (to compute outcome variables in 1940). We then estimate equation (1) using as outcomes the share of single women teachers in the past who we observe (1) being married, (2) teaching, (3) being married and teaching, or (4) being married and not working, in the future.

Our main estimates are shown in the first panel of Table 3 and Figure 3. Column (1) and panel (a) of Figure 3 show that, for women who were single and teaching prior to the employment protections being passed, the protections did not lead them to be more likely to get married treated counties relative to control counties. It therefore appears that on average, single women teachers did not respond to the protections by getting married earlier.

Instead, our estimates suggest that on average, single women teachers responded to the protections by getting married “as planned” but *keeping their jobs*. Column (3) shows that the protections led to a 2 p.p. increase in the likelihood that single women teachers were both

married *and* teaching in the future. The increase is economically significant: in 1930, only 5% of single women teachers in 1920 were both married and teaching in 1930, meaning the protections led to a 40% increase in the propensity for single women teachers to get married and stay in teaching. Finally, Column (4) shows that the increased propensity to get married and stay in teaching was offset by a 3.7 p.p. decrease in propensity to get married and exit the workforce—again, an economically significant magnitude given that 88% of single women teachers in 1920 had gotten married and quit their teaching jobs by 1930. These results indicate that the increase in married women in teaching was at least in part driven by existing single women teachers who, in the absence of employment protections, would have left teaching to get married, but became able to remain teachers due to the new laws.

One result of this shift is that we might expect to see an overall increase in the likelihood of staying in teaching among women who were single teachers prior to the employment protections being passed. However, we see no such effect (see Column (2) of Table 3 and Figure A4), likely due to an offsetting force—that is, single women teachers who never planned on getting married being *pushed out* of teaching by the newly married teachers who stay. While we lack the statistical power to detect evidence of this channel, the idea that single women teachers were being pushed out of teaching by newly married teachers is consistent with the decrease in the share of single women teachers we observed in Section 5.1.

We then turn our attention to the women who were married but not working as teachers prior to the protections being passed. For these women, the new legal protections may have induced them to enter teaching, potentially joining the workforce at the extensive margin. To investigate the extent to which married women may have entered teaching in response to the protections being passed, we follow a similar procedure to define analysis samples, compute outcomes, and estimate effects using our linked panel data.¹²

The resulting estimates are shown in the second panel of Table 3. The dependent variable means show that the vast majority of married women we identify who are not working remain married (93%) and not working (88%) ten years later. However, we also find that relative to control counties, married women in treated counties who were not working prior to the employment protections became 0.1% more likely to become a teacher after the employment protections were passed. The effect is significant at the 1% level, and somewhat remarkable given that so few white married women worked during this time period: indeed, only 0.2% of

¹²More specifically, within the linked sample, we identify married women under the age of 50 who report not working in e.g. 1910. We then compute our outcome variables, at the county level, as (1) the share of these women who were married in 1920, (2) the share who were teaching in 1920, (3) the share who were married and teaching in 1920, and (4) the share who were married and not working in 1920. We repeat this process for married women not working in 1920 (to compute outcome variables in 1930) and 1930 (to compute outcome variables in 1940).

married and not working women in 1920 were teaching in 1930, implying that the employment protections led to a 50% increase in the propensity for married women to enter teaching.

Taken together, our results suggest that the employment protections induced single women teachers who would have otherwise gotten married and quit to get married and stay in teaching, while simultaneously inducing the extensive margin entry of previously married, unemployed women into the labor force as teachers.

5.3 Robustness Checks

We close the section with a series of robustness checks. Though the absence of pre-trends and the historical context (as explained in Section 4.2) lend credibility to our identifying assumption of parallel trends in treated and control counties, we still may be concerned that our results are being driven by factors other than the introduction of employment protections for married women in teaching, we test the robustness of our results in two ways: a placebo test using secretaries and an alternate empirical specification using a matched counties design.

Placebo tests. To test whether the bans may have coincided with differential trends in attitudes towards employing married women in treated and control states, we conduct a placebo test by examining whether the employment protections in *teaching* affected workers in a different occupation: secretarial/clerical work. Much like teaching, secretarial/clerical work was an occupation that was heavily dominated by women during the early 1900s, yet simultaneously an occupation in which firms regularly discriminated against women based on their marital status [Goldin, 1988]. Were it the case that the employment protections in teaching were induced by differential trends in, or attitudes towards, the employment of married women in the treated and control states, we would expect to see an increase in the share of secretaries who were married women in treated counties as well.

We estimate Equation 1, following our main specification, with outcomes related to the employment of secretaries rather than our teaching-related outcomes of interest. Figure A2 replicates Figure 2 for secretaries, showing the effects of the employment protections on the share of secretaries that were married women (triangles), men (circles), and unmarried women (squares). We see no significant effects of the employment protections in teaching on the composition of the secretarial workforce, particularly between 1930 and 1940.

We also replicate our mechanisms analysis using linked data on secretaries instead of teachers. Figure A3 replicates Figure 3 for secretaries instead of teachers, showing the effects of the employment protections on the propensity for women who were single secretaries prior to the protections to become (1) married and still working as secretaries, and (2) married but no longer

in the labor force, in the future. Again, we find no evidence that the employment protections in teaching affected the marriage or working decisions for single women secretaries.

Matching. We use neighboring Southern states as our preferred control group for two reasons. Firstly, due to geographical proximity, neighboring states were likely most similar in terms of culture. Secondly, as discussed in Section 2, anecdotal evidence from newspapers in the 1930s suggest that courts had conflicting views on whether it was just to dismiss women once married. Newspapers record cases in each of the control states—TN, WV, SC, and VA—where court decisions with rulings coming down in favor of married women in some cases but school boards in others. Although anecdotal, we interpret these reports as evidence that the control states were engaged in similar discussions. Legislators in VA even proposed a similar state-wide bill, but it did not pass.

Regardless, one might be concerned that the neighboring Southern states are not as close a control as possible. We therefore evaluate in Appendix B whether our results are sensitive to using other counties as our control group obtained through various matching techniques, and find that our results remain similar.

6 Conclusion

This paper provides new evidence on the effectiveness of historical policies that sought to prevent U.S. firms from discriminating against women on the basis of marital status, during a time period when married women were largely kept out of the labor market. Employer discrimination against married women in school districts and debates over tenure protection for teachers were both at their height in the 1930s. In the midst of this policy environment, legislators in Kentucky and North Carolina successfully passed state legislation in the 1930s enforcing employment protections for married women in teaching. The fact that only two states passed such legislation in the 1930s, along with the fact that neighboring states never passed similar legislation, allows us to estimate the effect of employment protections for married women in teaching.

We employ a difference-in-differences strategy to estimate the effect of introducing employment protections for married women in teaching on (1) the composition of the teacher workforce and (2) the trade-offs that women faced between marriage and teaching. On composition, we find that the protections led to an increase in the share of teachers who were married women. This increase was offset by a decrease in the share of teachers who were unmarried women, with no effect on men. On mechanisms, we find that the employment protections altered the

trade-off between marriage and teaching for two sets of women: those who were single and already teaching prior to the protections, and those who were married and not teaching prior to the protections. For the single women teachers, the employment protections increased the likelihood that they got married and stayed in teaching, as opposed to leaving the workforce. For married women non-teachers, the employment protections increased the likelihood that they entered the workforce, specifically as teachers. The results are largely robust to various matching specifications.

Overall, the results provide causal evidence that despite the strong social norm that married women stay out of the labor force in the early 1900s, there was demand among women to work while married. Making discriminatory hiring practices against married women illegal, even as early as the 1930s and in only one occupation, pulled more married women into the labor market in just a few years, long before the larger-scale societal transformations of the 1950s and 1960s.

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7 Figures

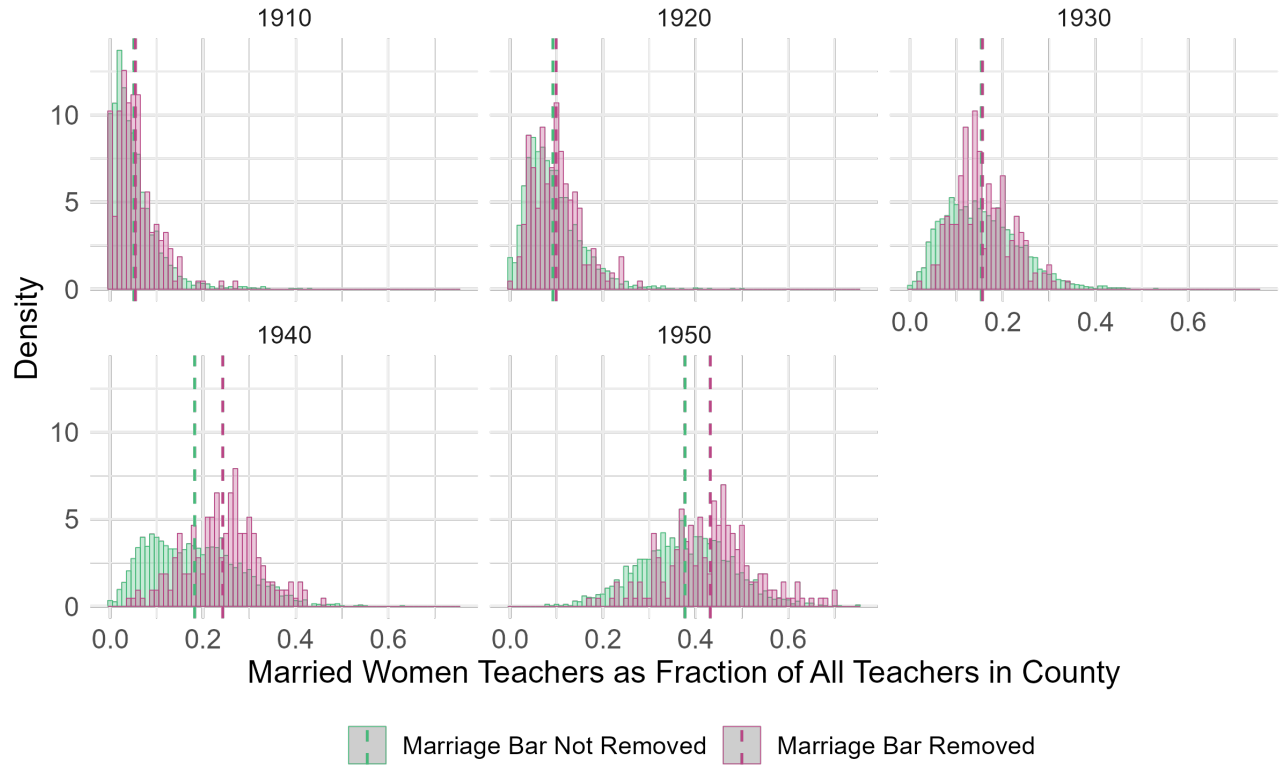


Figure 1: Density plots from 1910 to 1950 of the county-level fraction of white women teachers who are married. Separate distributions are shown for (1) counties in states where employer protections were passed in the 1930's (KY, NC), and (2) all other counties in the country. Group means are indicated by vertical dashed lines. Counties with ten or fewer teachers in 1930, 1940, or 1950 are excluded to prevent bias from small samples.

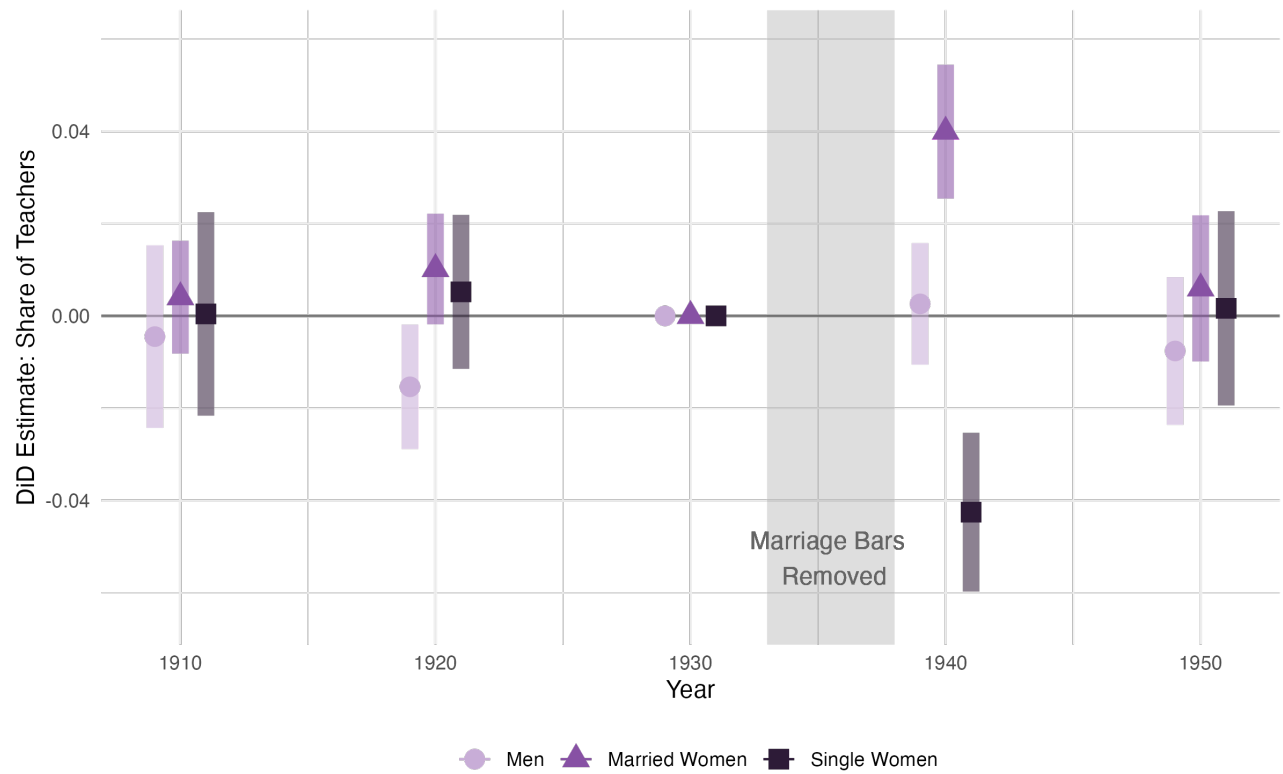
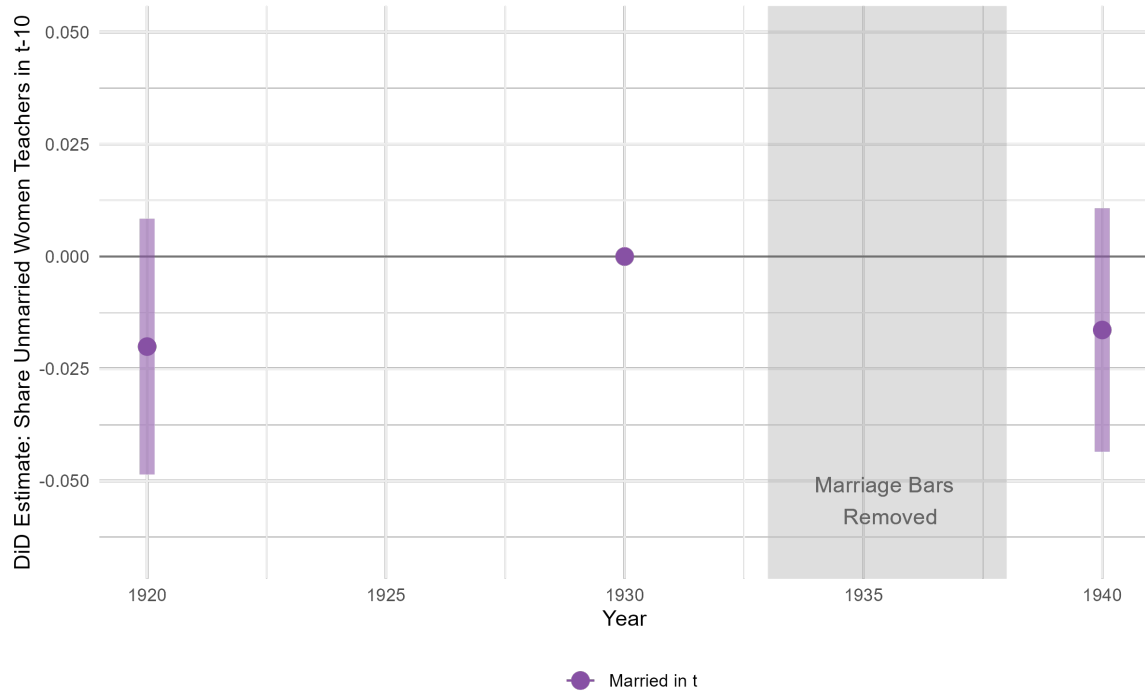
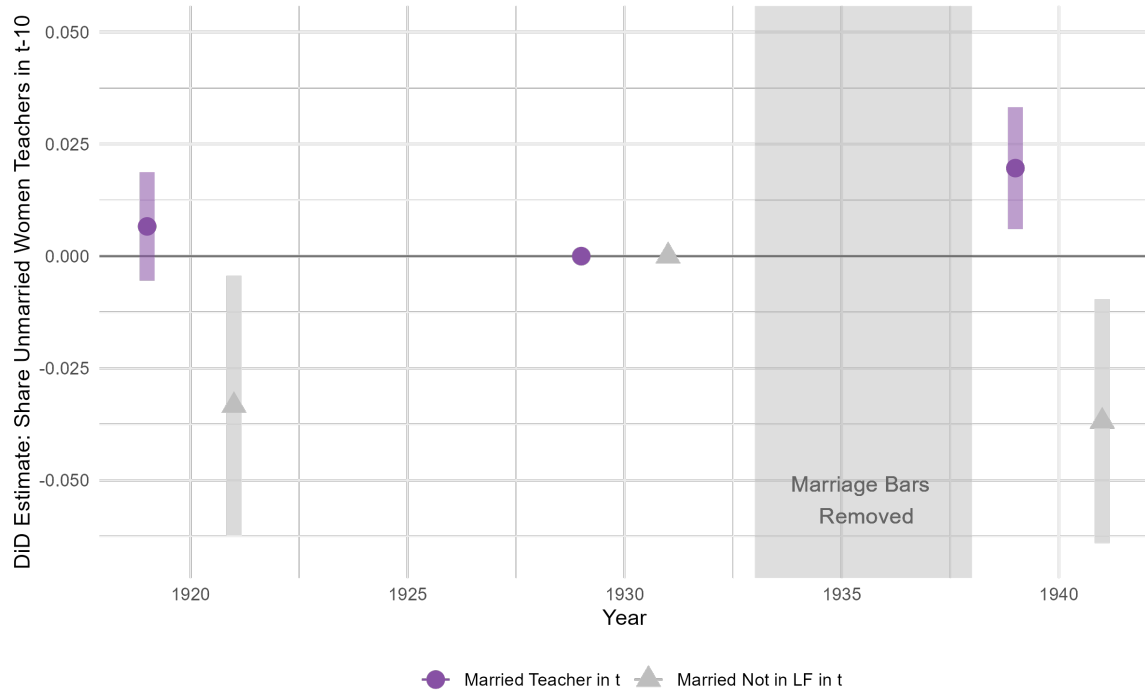


Figure 2: Estimated effects of the introduction of employer protections for married women in teaching on the *gender composition* of teachers, at the county level. Estimates are from a difference-in-differences specification where the dependent variable is the share of teachers in a county that are married women, unmarried women, and men. The sample includes KY, NC, and neighboring states. Standard errors are clustered at the county level. 95% confidence intervals are shown.



(a) Outcome: $\Pr(\text{married in year } t)$



(b) Outcomes: $\Pr(\text{married in year } t \times \text{working status in } t)$

Figure 3: Panel (a) shows the estimated effects of the introduction of the employment protections on the likelihood of being married in year t , among women teachers who were previously unmarried. Panel (b) shows the estimated effects on the likelihood of being a married teacher in year t , and of being married but not working in year t , among women teachers who were previously unmarried. The outcome variable in each year t is calculated using the sample of **unmarried women teachers** below the age of 40 in year $t - 10$.

8 Tables

Table 1: Summary of key county-level statistics by county group

	All (1)	South (2)	Neighb. South (3)	Treated (4)
Panel A: General County Statistics				
Population (Thous.)	41.25641 (2.541)	27.88893 (1.403)	27.30214 (1.791)	26.65849 (2.102)
White School-Age Pop. (Thous.)	9.387786 (0.538)	5.871092 (0.265)	6.126877 (0.349)	6.368358 (0.424)
Share Urban	0.2212502 (0.005)	0.1544175 (0.008)	0.1638445 (0.015)	0.1369042 (0.014)
LFP of Married Women	0.10135790 (0.001)	0.13409351 (0.003)	0.11485380 (0.005)	0.09291305 (0.004)
LFP of White Married Women	0.08014614 (0.001)	0.08118647 (0.002)	0.07738165 (0.003)	0.06779610 (0.003)
Num. Children*	2.029512 (0.007)	2.256121 (0.013)	2.313770 (0.02)	2.345371 (0.029)
Panel B: County Statistics on White Teachers				
Teachers/Students	30.72109 (0.204)	38.52828 (0.415)	36.37310 (0.6)	44.39159 (0.943)
Share Men	0.1974399 (0.002)	0.1942015 (0.003)	0.2080979 (0.006)	0.2158630 (0.007)
Share Single Women	0.6484210 (0.002)	0.6404770 (0.004)	0.6414652 (0.007)	0.6278785 (0.007)
Share Married Women	0.1541391 (0.001)	0.1653215 (0.002)	0.1504369 (0.004)	0.1562585 (0.004)
<i>N</i> (Counties)	2915	883	308	215

Notes: All statistics are measured using the full count 1930 census data, aggregated to the county level [Ruggles et al., 2024]. Panel A presents means and standard errors of county-level variables for the whole county population, including population in thousands, the percentage of the county population living in an urban area, the percentage of married women and white married women in the county between the ages of 18 and 64 who are in the labor force, and the average number of children for married women. Panel B presents means and standard errors of county-level variables related to teachers, including the white school-age population divided by the number of white teachers in a county, and the share of white teachers in a county that are men, unmarried women, and married women.

Table 2: Estimated effects of the employer protections on the gender composition of teachers

	Dependent Variable:			
	% Teach Mar. Wom.	% Teach Men	% Teach Unmar. Wom.	# Teachers
	(1)	(2)	(3)	(4)
Treated \times 1940 (γ_{1940}^{DD})	0.040*** (0.008)	0.003 (0.006)	-0.043*** (0.009)	-0.150 (3.909)
Treated \times 1950 (γ_{1950}^{DD})	0.006 (0.008)	-0.008 (0.007)	0.002 (0.010)	-2.990 (4.426)
Dep. Var. 1930 Treated Mean	0.1563	0.2159	0.6279	156.2
Observations	1,569	1,569	1,569	1,569
Adjusted R ²	0.787	0.731	0.824	0.966

Notes: Estimation follows equation 1. The estimation sample includes counties in treated states (KY, NC) and neighboring southern states (VA, SC, TN, WV) in 1930, 1940, and 1950. The outcomes in Columns (1), (2) and (3) are the share of white teachers that are married women, men, and unmarried women respectively (note that these categories are exhaustive). The outcome in Column (4) is the total number of white teachers in a county. All regressions use the 1930, 1940, and 1950 IPUMS full count Censuses. [Ruggles et al., 2024].

Table 3: Estimated effects of the employer protections on women’s propensity to get married and/or teach

	Dependent Variable			
	Pr(Married in t)	Pr(Teacher in t)	Pr(Married and Teacher in t)	Pr(Married and Not Working in t)
	(1)	(2)	(3)	(4)
Sample A: Women who were single and teaching in $t - 10$				
Treated \times Post-Ban (γ_{1940}^{DD})	-0.016 (0.013)	0.006 (0.013)	0.020*** (0.007)	-0.037*** (0.013)
Dep. Var. 1930 Mean	0.6225	0.2581	0.05053	0.5369
Nr. of counties	1,030	1,030	1,030	1,030
Adjusted R ²	0.561	0.593	0.115	0.586
Sample B: Women who were married and not teaching in $t - 10$				
Treated \times Post-Ban (γ_{1940}^{DD})	-0.0004 (0.001)	0.001*** (0.0002)	0.001*** (0.0002)	-0.004 (0.004)
Dep. Var. 1930 Mean	0.9341	0.002444	0.001963	0.8786
Nr. of counties	1,056	1,056	1,056	1,056
Adjusted R ²	0.681	0.383	0.330	0.676

Notes: Estimation follows Equation (1). To construct our estimation samples, we start with counties in treated states (KY, NC) and neighboring southern states (VA, SC, TN, WV) in 1930 and 1940. Within these counties, we identify women whom we are able to link over consecutive Census years (i.e. between 1920 and 1930, and between 1930 and 1940) using the CensusTree linkages. From these women, we construct two samples: Sample A, containing linked women who were under 40, single, and teaching in 1920 and 1930, and Sample B, containing linked women who were under 40, married, and not teaching in 1920 and 1930. All regressions use the 1920-1930 and 1930-1940 linked full-count Census samples. See Section 3 for details and full citations for data.

A Additional Figures

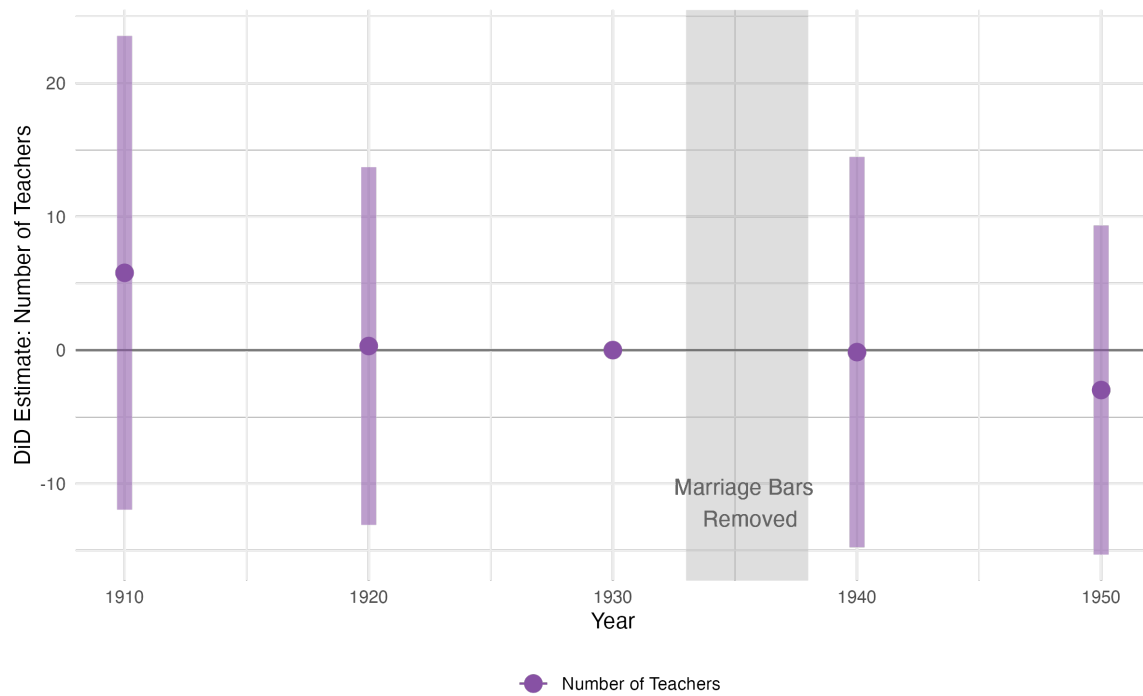


Figure A1: Estimated effects of the introduction of employment protections for married women on the total number of teachers per county.

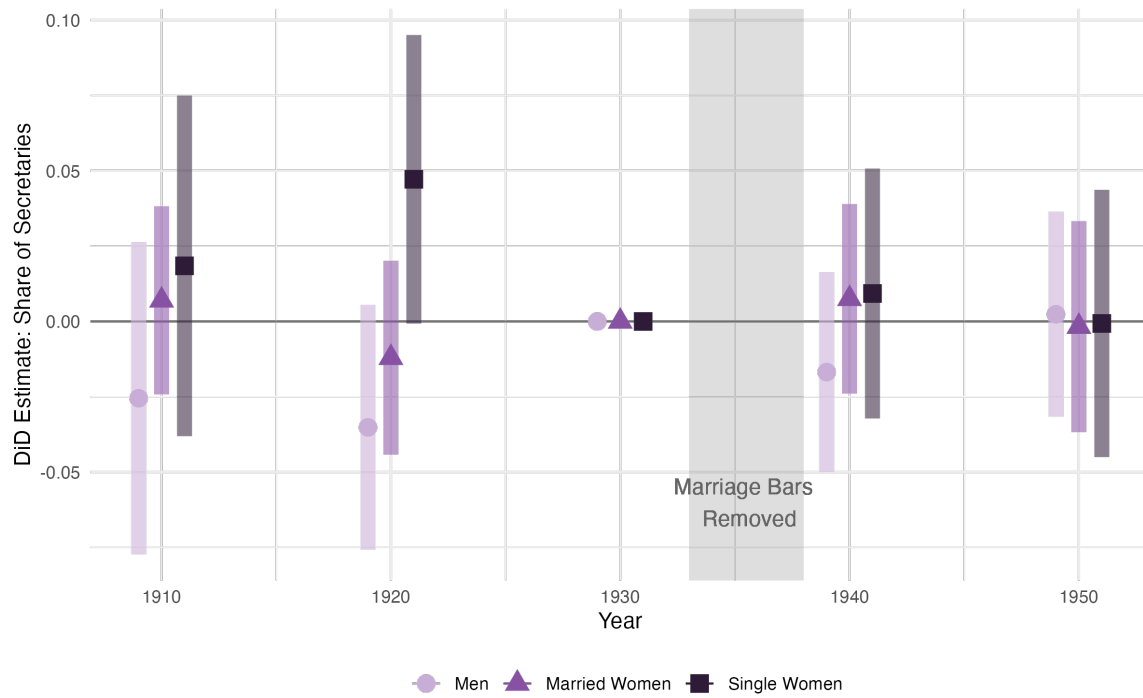


Figure A2: Placebo test: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *secretaries* who are men, unmarried women, and single women.

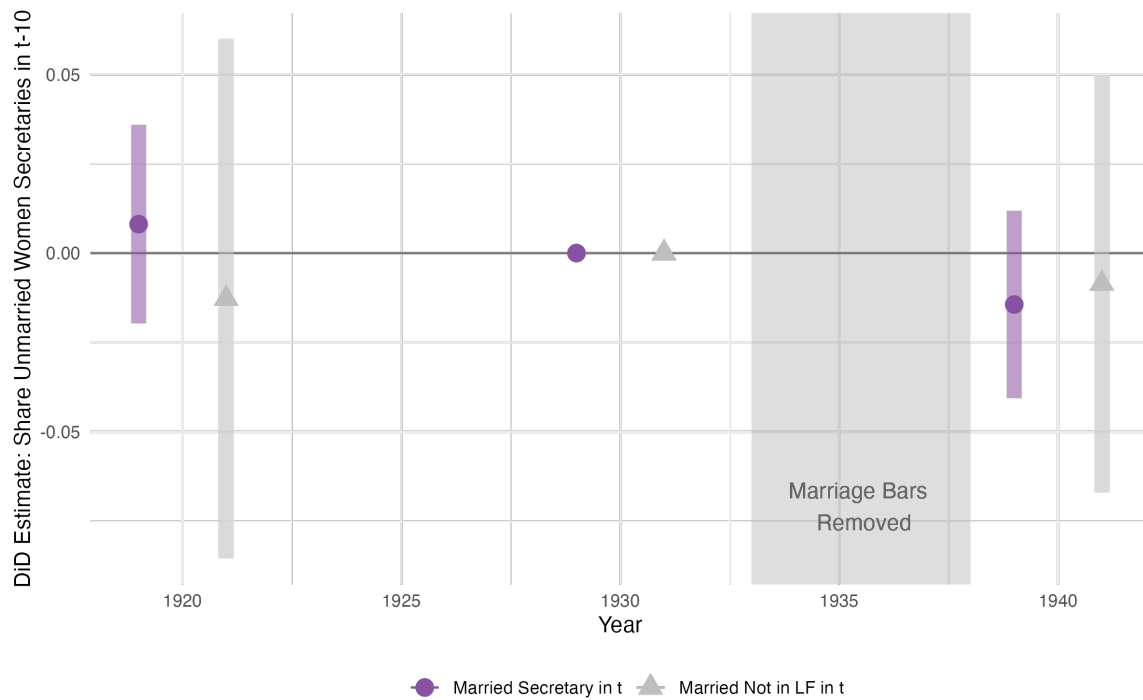


Figure A3: Placebo test: Estimated effects of the introduction of employment protections for married women in teaching on the likelihood that single women secretaries (in year $t - 10$) were still working as secretaries in the following Census (in year t).

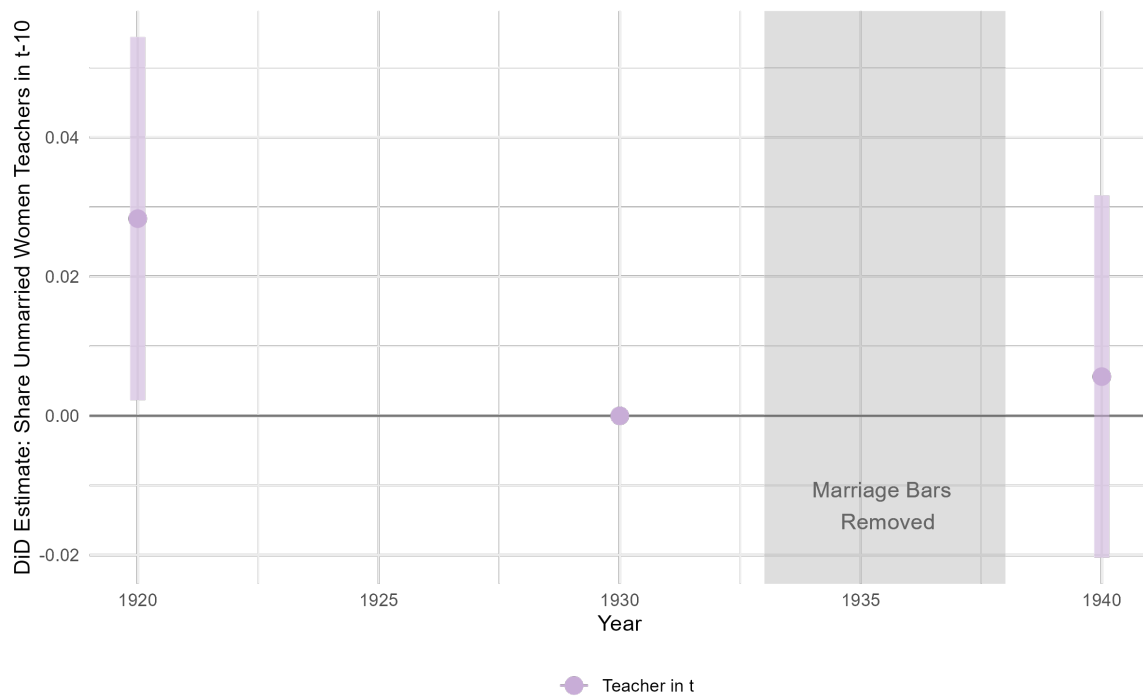
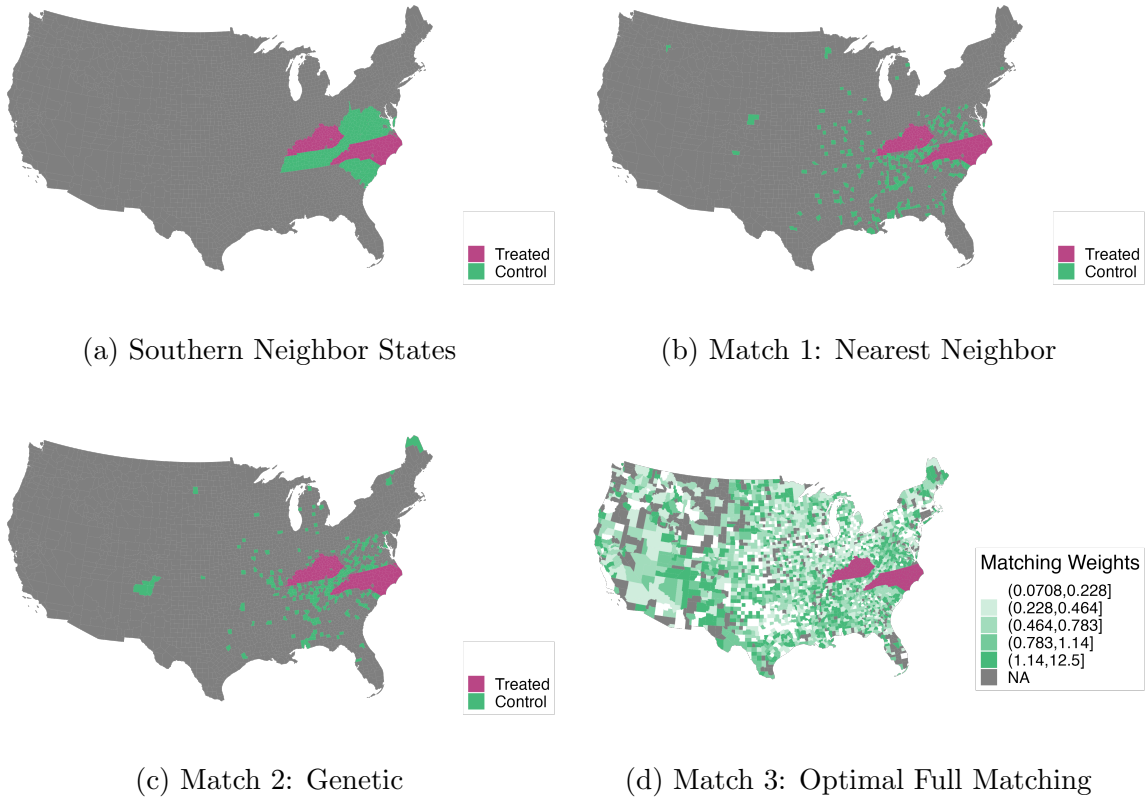


Figure A4: Estimated effects of the employer protections on the likelihood that single women teachers (in year $t - 10$) were still teaching in the following Census (in year t).

B Matched Counties Design

As discussed in Sections 3.3 and 5.3, our preferred specification relies on the assumption that in the absence of the specific marriage bar regulations having been passed in North Carolina and Kentucky, the composition of the teaching workforce would have evolved similarly in the treated states and neighboring southern states of South Carolina, Tennessee, Virginia, and West Virginia. To test whether our results are robust to alternative specifications, we employ a matched counties design which does not rely on the above assumption about the similarity of counties in treated states to counties in neighboring Southern states.

Figure A5: Maps of treated (pink) and control (green) counties for (a) our preferred control specification of neighboring Southern counties and (b)-(d) specifications using a range of matching techniques.



We match treatment and control counties using both the 1930 level and change between 1920 and 1930 of an extensive set of county-level variables, including demographics, urbanization, literacy rate, and workforce composition both for teachers and overall, all obtained from the full-count census [Ruggles et al., 2024]. We also include 1939 retail sales per capita and the growth in retail sales per capita from 1929 to 1939, as obtained from Fishback et al.

[2005].¹³ To match counties we use three different methods, all utilizing the MatchIt package in R [Ho et al., 2011]. The first matched sample is constructed by nearest neighbor matching using Mahalanobis distance, the second matched sample uses genetic matching as developed by Diamond and Sekhon [2013], Sekhon [2011], and the third matched sample uses optimal full matching as developed by Hansen [2004]. The first two methods are 1:1 matching methods, which produce the same number of control counties as treatment counties, while optimal full matching uses all counties and simply assigns weights to control counties based on their similarity to treatment counties. Figure A5 compares the control counties selected by the various matching methods to the neighboring southern states in our preferred specification. We can see that matched samples 1 and 2 are geographically concentrated in the neighboring southern states, reinforcing the fact that the neighboring southern counties are similar to our treated counties. Panel (d) of Figure A5 maps the weights of the control counties as determined by the optimal full matching method, which are not as closely concentrated in the neighboring states as with the other matching methods.

¹³ Complete variable list: population, share living in urban areas, share under age 20, share aged 20-39, share aged 40-59, share aged 60 or older, share white, share literate, share of 18-64-year-olds in the labor force, share of 18-64-year-old married women in the labor force, retail sales per capita in 1939 (in 1967\$), share of teachers that are unmarried women, and share of teachers that are married women. 1920-1930 change is calculated as $g_x = \frac{x_{1930} - x_{1920}}{x_{1920}}$, where x_t represents the value of the relevant variable x in year t , except for 1920-1930 change in share living in urban areas and share of teachers that are unmarried/married women, which are calculated as $g_x = \frac{x_{1930} - x_{1920}}{x_{1920} + 0.01}$ to avoid division by zero.

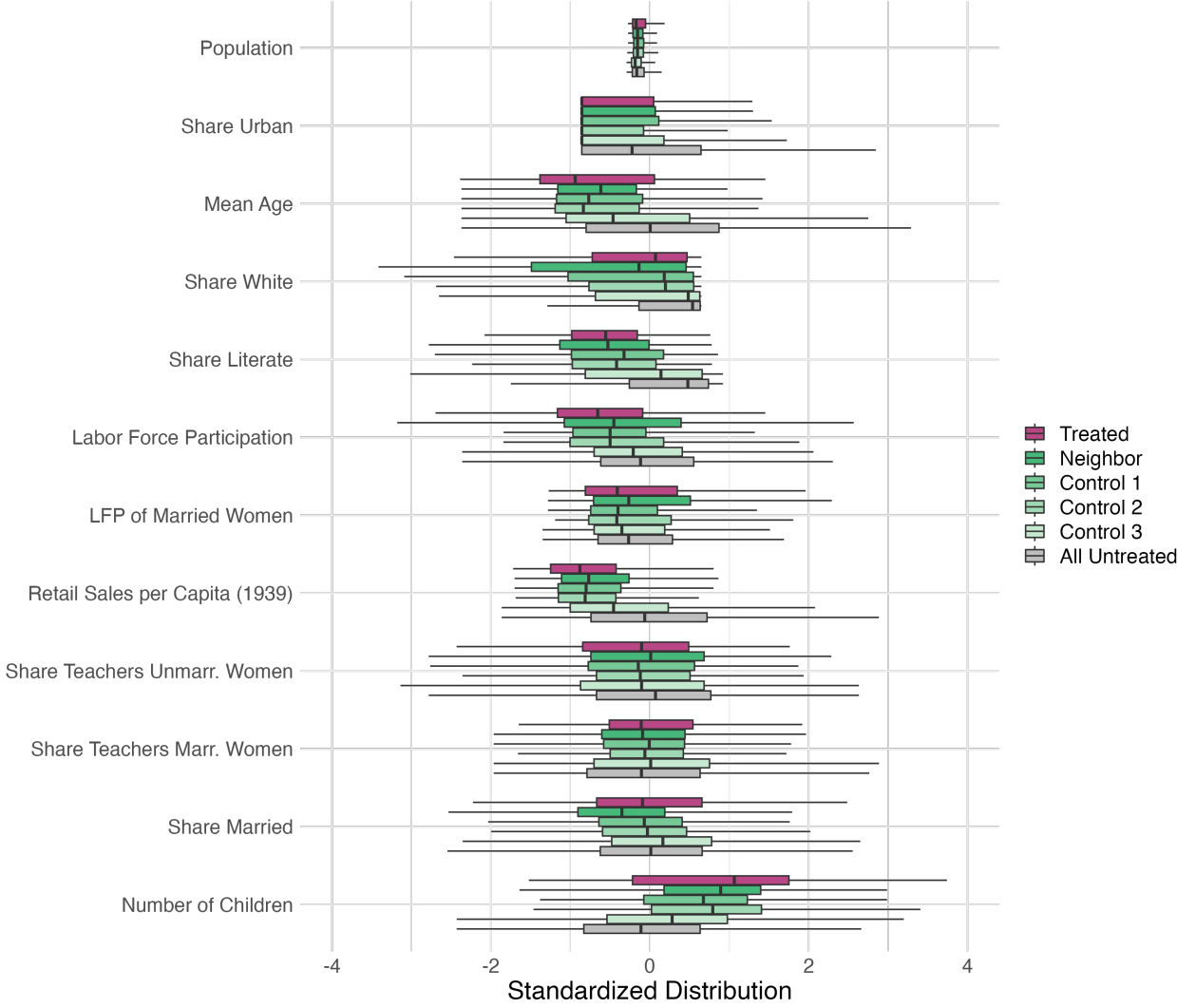


Figure A6: Boxplot of standardized 1930 values of various covariates by treatment or control group. The center bar represents the median, the edges of the box represent the 25th and 75th percentiles, and the edges of the whiskers represent extrema, with outliers removed (see R function `geom_boxplot` for further details). Distributions are weighted for control group 3. Covariates are outlined in detail in Footnote 13, and also include the share of women over the age of 18 that are married, and the average number of children for married women. All data is obtained from [Ruggles et al. \[2024\]](#) with the exception of 1939 Retail Sales per Capita, which is obtained from [Fishback et al. \[2005\]](#).

In Figure A6, for the various treatment and control groups we graph boxplots of the standardized 1930 values of the matching covariates listed in Footnote 13, as well as two additional variables not used for matching: share of women married and average number of children for married women. We note first that while all untreated counties are quite distinct from the treated counties, the neighboring southern counties are actually quite similar to the treated counties and on some dimensions (e.g. share of teachers married women, number of children)

actually outperform the matched county groups. While the first and second control groups are very similar in distribution to the treatment counties across nearly all covariates, the third control group is much less similar.

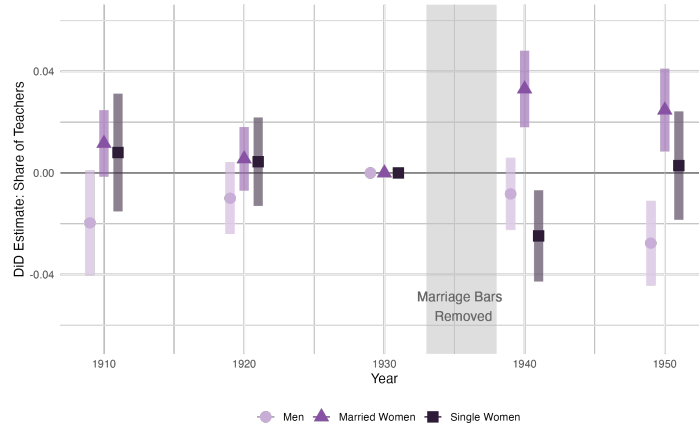
B.1 Results

We re-estimate our key analyses using the three matched samples. We begin by estimating the effect of the employment protections on the composition of the teacher workforce (i.e. the share of teachers that are married women, men, and unmarried women) and present results in Figure A7. For matched samples 1 and 2, in panels (a) and (b), our estimated coefficients in 1940 are consistent with our main results—the employment protections caused an increase in the share of married women teachers, at the expense of a decrease in the share of unmarried women teachers, with no change in the share of men in teaching—and significant at the 99% level albeit attenuated. Matched sample 3, in panel (c), shows similar results, but suggests a decrease in the share of men in teaching. Note that Figure A6 suggests that the control group for matched sample 3 is the least comparable to the treatment group.

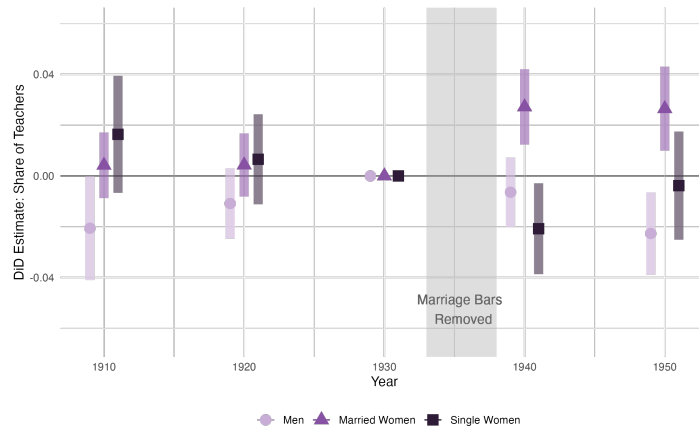
However, unlike the results from our preferred specification (Figure 2), the gap in married women’s share of teachers persists to 1950. A potential reason for this continued divergence is the effect of World War II – while it is a reasonable assumption that nearby states had similar exposure to World War II, the same may not be true for counties that were similar in terms of *pre-war demographic and economic characteristics* but not constrained to be in nearby states (as is the case for our matched samples). Higher exposure to World War II, in the form of more men being drafted, would have resulted in fewer men available to work as teachers and a substitution towards married women (the primary source of replacement labor during World War II). This difference would also explain why the gap in the share of unmarried women teachers returns to zero in 1950, and why the persistent gap in the share of married women teachers is instead offset by men.

Finally, we repeat our mechanisms analysis examining the effects of the employment protections on the marriage and teaching decisions of women who, prior to the employment protections being passed, were single women teachers.¹⁴ The results are shown in Figure A8, and are overall qualitatively similar to the results from our main specification. In particular, in all three matched samples, we estimate a strong positive effect of the employer protections on the likelihood that single women teachers eventually become married women teachers.

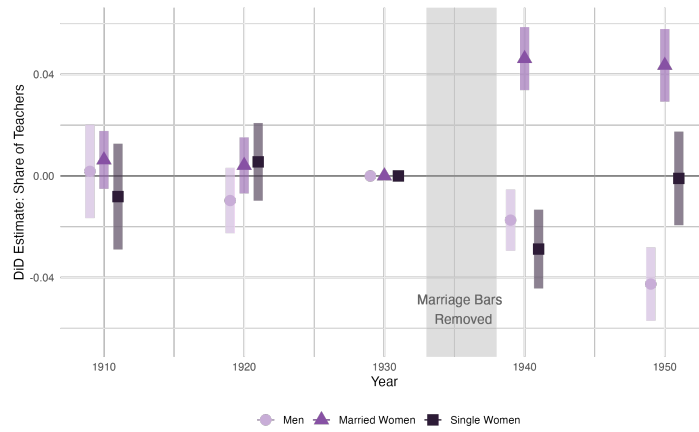
¹⁴For brevity we do not include the corresponding figure for women who were married and not teaching prior to the employments. The results are qualitatively similar and available upon request.



(a) Match 1: Nearest Neighbor

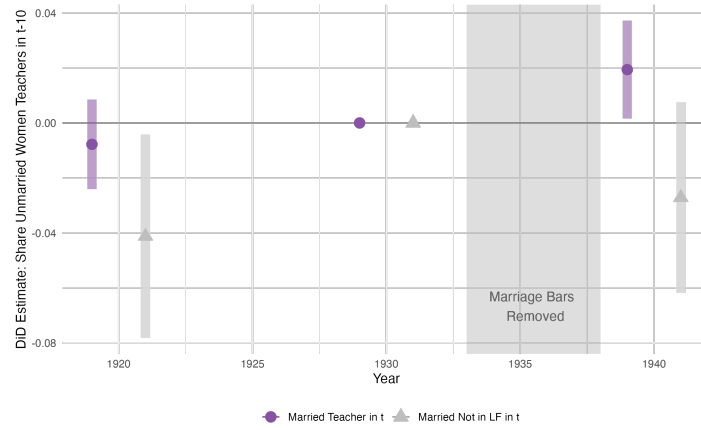


(b) Match 2: Genetic

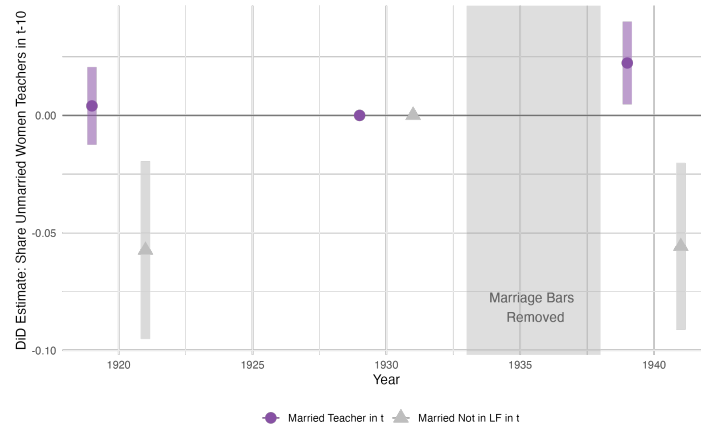


(c) Match 3: Full Optimal

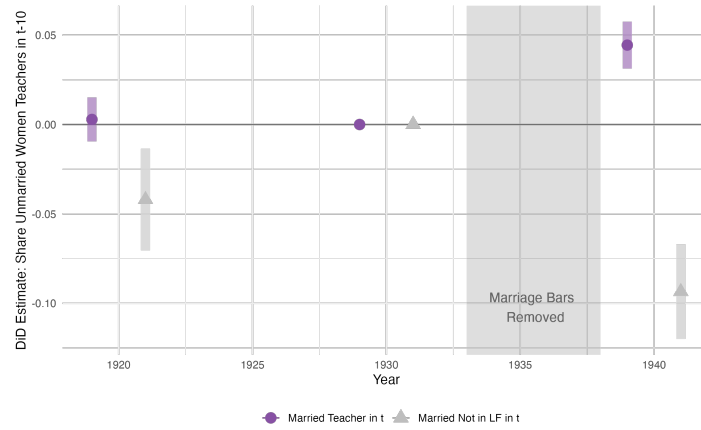
Figure A7: Estimated effects of the introduction of employer protections for married women in teaching on the gender composition of teachers, at the county level. Estimates are from a difference-in-differences specification where the dependent variable is the share of teachers in a county that are married women, unmarried women, and men. The sample includes KY, NC, and matched control counties, as determined by various methods. Standard errors are clustered at the county level. 95% confidence intervals are shown.



(a) Match 1: Nearest Neighbor



(b) Match 2: Genetic



(c) Match 3: Full Optimal

Figure A8: Estimated effects of the introduction of employer protections for married women in teaching on the likelihood of being a married teacher in year t , and of being married but not working in year t , among women teachers who were previously unmarried. The sample includes KY, NC, and matched control counties, as determined by various methods. Standard errors are clustered at the county level. 95% confidence intervals are shown.