The Effects of Employment Protections for Married Women: The Case of U.S. Teachers*

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Abstract

Married women's labor force participation continues to lag behind that of both men and unmarried women in many parts of the world today. What role can legislative protections from employment discrimination play in pulling married women into the labor force and what would the broader consequences be for gender parity in the labor force? We answer this question by studying the introduction of employment protections for married women teachers in the 1930s in Kentucky and North Carolina. We find that the employment protections increased the share of teachers who were married women by 4 p.p. (26%) in treated counties relative to control ones, driven both by the entry of married women outside the labor force into teaching and, to a lesser extent, by unmarried women teachers staying in teaching after marriage. However, we find that the influx of married women teachers came entirely at the expense of unmarried women teachers, who became more likely to exit the labor force, with no effects on men. Our results show that in the presence of gendered social attitudes, targeted employment protections can increase married women's employment but may do so at the cost of unmarried women's displacement.

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1 Introduction

One of the most notable labor market shifts of the 20th century was the rise of married women working outside the home.[1] Yet even in the 21st century, married women's LFP continues to lag behind that of married men around the world. The difference is most stark in Western/-Central/Southern Asia and Northern Africa, where in 2019 less than 30% of married women were in the labor force compared to over 94% of married men [UN Women, 2024].[2]

One potential explanation for married women's low LFP is that external factors lead them to face a particular trade-off between marriage and work. In some cases, the trade-off is made explicit on the labor demand side by employers who dismiss women at marriage or refuse to hire married women, a practice that was widespread in the U.S. until the 1950s and remains legal in 82 countries today [Goldin, 1988, World Policy Center, 2024]. In others, the trade-off may arise out of social norms: for example, prior work shows that negative social attitudes towards women working can result in such a trade-off that in turn can help explain married women's low LFP [Bertrand et al., 2015].

In such environments, to what extent can policy prohibiting employment discrimination, independent of widespread changes in social norms, increase married women's LFP? And what consequences might such policies have for other workers, namely men and unmarried women?

This paper provides evidence that, in an environment in which married women are discouraged from working outside the home, a targeted anti-discrimination policy can bring married women into the workforce at the cost of displacing single women and with no effect on men. Our context is the early 20th century U.S., a time and place during which many white married women did not work and, in many white-collar occupations, did not have the option to work due to widespread employment discrimination on the basis of their marital status [Goldin, 1988]. [3] Over the same time period however, debates over tenure protection laws in one particular occupation began to spread across the country, centering *teachers* in a national discussion on married women's employment rights. Amidst court cases of married women suing school boards for wrongful termination and numerous attempts by state legislators to pass employment protections for married women in teaching, only two states—Kentucky and North Carolina—ultimately passed laws in married women's favor. Using this variation in the pas-

[1] In countries like the U.S., the increase was most stark for white college-educated married women, whose labor force participation (LFP) grew from 21% in 1940 to 77% in 2020 and who today comprise 12% of the U.S. labor force. [Ruggles et al., 2024b,a].

[2] The discrepancy in labor force participation for unmarried women and men is considerably smaller, although still large: in the same regions, over 50% of prime-age never-married women and under 90% of prime-age never-married men are in the labor force.

[3] Throughout the paper we focus on white women as non-white married women were significantly more likely than white married women to be working in the early 20th century U.S. See Section 3.1 for further discussion.

sage of state-wide employment protections for married women teachers in the 1930s we evaluate how the removal of institutional barriers affected the employment of married women and by extension the overall labor force.

It is unclear *ex-ante* how introducing employment protections would have affected the employment of married women, especially given the backdrop of gendered social attitudes at the time. Newspapers from this era chronicle an active public debate over the working married woman, deemed by many to be "taking money away from needy unmarried women" and "neglecting home duties" [The New York Herald, 1921, Oakland Tribune, 1921]. If married women (or their households) also held these beliefs then married women would have chosen not to work even if discriminatory hiring practices were made illegal. Our setting thus provides a unique opportunity to assess the impacts of removing barriers to employment for married women and to test whether married women's low LFP was being predominantly driven by non-institutionalized social norms in our context. Furthermore, if married women did in fact enter the labor force as a result of these employment protections, it is not obvious how the labor market would have been affected. In the absence of an overall expansion of the teacher workforce, married women's entry must have come at the cost of other workers' exits—who were these workers and where did they go?

The employment protections took the form of state-wide legislation that made it illegal for school districts to discriminate against teachers based on their marital status in employment decisions and were passed in only two states. We leverage this variation *difference-in-differences* design, which we use to compare outcomes in states that introduced employment protections to outcomes in neighboring Southern states that did not.[4] Using a combination of IPUMS full-count decennial U.S. census data spanning 1910-1950 Ruggles et al. [2024b] and 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], we use our design to evaluate how the policy change affected the composition of the teacher workforce in cross-sections and to track the employment outcomes of individual teachers over time.[5]

Our setting allows us to overcome two key challenges inherent to identifying the effects of removing barriers to employment for married women: (1) data availability and (2) endogeneity

[4]Southern states, including KY and NC, differed from the rest of the country in the extent to which married women worked and in the demand for teachers. See Section 3.3 for further discussion.

[5]It should be noted that our estimation strategy captures the net effect of the employment protections taking into account how the protections may have impacted schools' labor demand as well as married women's labor supply. While our data preclude us from being able to separate these two channels, the lack of coverage of the employment protections in local media suggest that the average married woman would have had limited awareness of the policy, indicating that changes in schools' labor demand likely played the more central role.

in firms' choices of hiring policies. 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 factors that simultaneously affected women's employment, such as economic conditions, national trends in gender equality, or women's wages. 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 provide three pieces of suggestive evidence that the 'parallel trends' assumption holds. First, we find qualitative evidence from historical policy briefs and 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, like the priorities of a particular state legislator. Second, 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]. Third, we show evidence of parallel pre-trends in our outcomes of interest prior to 1930. We perform several robustness checks to validate our results.

Our main finding is that the passing of employment protections in teaching *increased* the employment of married women in teaching. Treated counties experienced a 4 p.p. increase between 1930 and 1940 (a 26% increase) in the share of teachers who were married women. The unconditional likelihood that a married woman worked as a teacher also increased by 17% between 1930 and 1940 in treated counties relative to control ones. Our estimates thus squarely reject the null hypothesis that non-institutionalized social norms were the only driver of married women's low LFP in the U.S. in the early 1900s.[6]

Furthermore, we find that the increase was driven by an extensive margin labor supply response among married women. Using our linked census sample[7] in which we can observe

[6]In fact, a back-of-the-envelope calculation using our estimates suggests that the removal of institutional barriers to employment during the 1940s contributed as much as 24% to the dramatic increase in the LFP of white college-educated married women between 1940 and 1950 (when the bulk of employment discrimination against married women did in fact cease Goldin [1988]). See Appendix C for details.

[7]Although an important occupation for college-educated women, teachers comprised only 6% of the entire female workforce in 1940. The significant but small entries and exits to teaching induced by the policy change are thus small relative to changes in the larger labor force. In the linked sample we can condition on women's employment and marital history, isolating the effects of the policy on specific groups of interest.

individuals moving between occupations as well as in and out of the labor force, we find that the key mechanism underlying the increase was women changing their decision of whether and when to *work as a teacher* rather than whether and when to get married. We find that the laws did not lead unmarried women to become more likely to get married, nor did they induce working women to switch from other occupations into teaching. Instead, the increase in married women teachers was driven by two primary channels: married women who were not in the labor force becoming more likely *to enter teaching* rather than stay out of the labor force.[8], and unmarried women teachers becoming more likely to get married *and remain in teaching* rather than get married and exit the workforce. We therefore conclude that the employment protections pulled married women into teaching from outside the labor force, rather than from other occupations.

However, in pulling married women into the labor force as teachers, the laws also pushed other workers—specifically, other women—out of the teaching occupation. We find that the increased employment of married women in teaching was entirely offset by a decrease in the employment of unmarried women in teaching, with no effect on the total number of teachers or on the share of men in teaching. We interpret the maintained ratio of men-to-women teachers in response to the laws as being consistent with two common beliefs that schools and other firms held at the time: that men and women workers were imperfect substitutes, and that employing men took priority over employing women.[9] Indeed, consistent with imperfect substitution, women teachers in many U.S. cities were paid less than a third of what men teachers were paid and tended to be allocated to subjects like home economics rather than mathematics [Bohan and Null, 2007, Blount, 1996].

Finally, we use our linked census sample to see what happened to the unmarried women who were, or might have eventually become, teachers. We find that the laws did not deter unmarried women from later becoming teachers. However, we also find evidence that the laws pushed some unmarried women who were already teachers out of the labor force or into other occupations.

What, then, was the overall effect of the employment protections? Given that the laws both pulled women into the labor force and pushed some women out, we conclude by assessing the overall effect the laws had on incumbent women teachers. We find that the laws did not

[8]In addition to finding that women who were married prior to the laws being passed became more likely to enter teaching, we also find that women who were not married prior to the laws became more likely to appear as married women in teaching later on. However, our data are not granular enough to examine whether the latter group of women were married before or after the laws were passed.

[9]Gender norms at the time dictated that men were responsible for providing for their families [Goldin, 2021]. Thus, schools that held this belief might have only hired a married woman at the expense of letting go of an unmarried women to uphold to the norm that men needed jobs to provide for their families.

have a significant effect on the labor force participation or the average occupational score of incumbent women teachers, although point estimates are suggestive of a noisy positive effect. Combined, however, with the unambiguously positive effect of the employment protections on women outside the labor force, we conclude that on net the policy change brought women into the labor force.

Overall, our study shows that removing institutional barriers to employment specific to married women can modestly increase their labor force participation. Our context also serves as a cautionary tale: in the presence of gendered social attitudes, the costs of such a policy may be borne by other women who are reshuffled to other occupations or out of the labor force, in line with the concerns expressed in local newspapers at the time. In the context we study however, this concern is outweighed by a positive overall effect on women's labor force participation and earnings.

Related literature. This paper contributes to the study of how anti-discrimination policies impact targeted and non-targeted populations (see e.g., Carrington et al. [2000], Neumark and Stock [2001], Bailey et al. [2024] on the Equal Pay Act of 1963 and Title VII of the Civil Rights Act of 1964, or e.g., DeLeire [1997], Acemoglu and Angrist [2001], Beegle and Stock [2003] on the Americans with Disabilities Act of 1990). We study a policy that targets discrimination against married women, which renders our findings particularly relevant for countries in which married women's employment has remained low even in the 21st century, such as those in Western/Central/Southern Asia and Northern Africa [UN Women, 2024] and the 82 countries that had no explicit laws prohibiting workplace discrimination on the basis of marital status as of 2021 [World Policy Center, 2024]. Our findings suggest that introducing employment protections for married women in a cultural environment that discourages married women from working outside the home can modestly increase their LFP but at the cost of displacing single women, with no cost to men.

This paper also contributes to our understanding of the historical factors that led to the rise in women's LFP in the U.S. throughout the 20th century. The bulk of the literature studies large-scale factors in the second half of the century—including 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 anti-discrimination laws [Bailey et al., 2024]—during which married women faced less discrimination and/or were protected by law and allowed to work in most occupations. One notable exception is Goldin [1988], who documents an explicit form of employer discrimination married women faced in the first half of

the century—colloquially known as "marriage bars"[10]—and explores the economic justifications firms made for not employing married women. Our paper complements the existing literature and builds upon Goldin [1988] by studying how the introduction of employment protections for married women in the first half of the century impacted married women's LFP during a time in which married women faced explicit and legal employer discrimination. We provide causal evidence that institutional barriers reduced married women's labor supply above and beyond the restricting social norms of the early 1900s.

Outline. The rest of the paper continues as follows. Section 2 describes the historical context in the U.S., including the justifications used for marriage bars across occupations and the circumstances surrounding the employment protections introduced in teaching. Section 3 describes the data and Section 4 describes the standard difference-in-differences methodology we use. Section 5 describes the effects of the employment protections on the teaching profession and on the LFP of married women, single women, and men. Section 6 concludes.

2 Employment Discrimination against Married Women in the Early 20th Century US

This section provides historical background on the evolution of the institutional barriers to employment that married women faced in the early 20th century US, 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 US, 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

[10] A 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.

or both forms of discrimination, either formally by implementing rules to not employ married women at the firm level or discretionarily 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 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 her. Prominent progressives of the time framed the working married woman as a [11]

Second, it was believed that due to their household responsibilities, married women were less efficient workers than unmarried women and men ("the married women 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 of large firms adopting 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 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 with the business cycle from 1920 to 1950. "Hire bars" in school districts—the practice of not hiring

[11] This 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].

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.[12] 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 US quickly declined and largely ended by the 1950s and 1960s.[13] 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 Employment Protections in Teaching

School districts were the most prominent employers that discriminated against married women throughout the early 20th century. Their use of marriage bars was 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.[14]

School districts rationalized using marriage bars for the same reasons cited by the firms described in the previous section: job scarcity for men, perceived inefficiency of married women workers, and 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 the country from the 1910s onward. By 1922, districts in eleven states offered tenure to teachers with various legislative limitations, but did not explicitly protect married women. As a result, some

[12]There 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].

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

[14]The importance of teaching as an occupation for married women has persisted to the 21st century, too: in 2000, 12% of married women with any college were teachers.

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 shows that some districts hired substantially higher rates of married women than others, confirming that marriage bars were implemented locally. 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 in 1933 was broad in its application: 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 in 1938 was more specific to experienced teachers: 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]. The laws received virtually no coverage in local newspapers at the time: we found no newspaper articles referencing the legislation in North Carolina and only one article mentioning the legislation passed in Kentucky [The Courier Journal, 1938]. The lack of coverage suggests that the laws may have been more salient to school districts that were wary of being sued than to the general public.

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 over time of the fraction of White teachers who were married women 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 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 laws 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 U.S. [Ruggles et al., 2024b]. 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 full-count 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. Nevertheless, 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.[15]

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 20th 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

[15]Enrollment 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 have reason to believe that this omission would vary differentially across counties nor over time in our treated and control groups.

necessity (to support their families) or out of social expectation. In light of these differences and 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.[16] 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 which tend to rely on using last names that typically change 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 and had greater shares of teachers who were married women. Southern counties also had lower population shares in urban areas, more children per married woman, and significantly more white school-aged students per white teacher than non-Southern counties.[17]

Column (3) 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 had higher student-to-teacher ratios, 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 a married woman in KY and NC counties was similar to the national average, despite white

[16] At the time of writing, linkages to 1950 are not yet available.

[17] The last fact is consistent with anecdotal evidence of teacher shortages in the South [Goldin, 2021].

married women being relatively less likely to work in KY and NC than in the rest of the country.

Finally, Column (4) 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 in the analysis that follows.[18] Our final balanced sample thus consists of 217 treated counties and 310 neighboring Southern control counties.[19]

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 a marked 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 evaluate the impact of the introduction of employment protections in teaching on employment outcomes by comparing outcomes over time in counties that passed the 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

[18] See Appendix D for a robustness exercise using an alternate matched county design.

[19] We exclude 16 counties that are either created or consolidated between 1910 and 1950 and are thus missing from the census in at least one year, and we exclude one county with ten or fewer teachers in 1930 or 1940 to prevent bias from small samples. Our results are robust to including all 220 counties in the treated states and all 320 counties in the Southern neighboring states as reported in the 1930 census. See Panel (a) of Appendix Figure A4 for a map of our sample of treated and control counties.

and on men's and women's employment. Our preferred specification is

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

where c indexes county, t indexes year, $s(c)$ is the state county c is in, 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 β_c^{DD} capture year and county 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. We cluster standard errors at the county level.

4.2 Identifying assumption

Causal inference relies on the "parallel trends" assumption: that in the absence of the employment protections in teaching being introduced between 1930 and 1940, our outcomes of interest 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 disproportionate increase in the number of married women employed in teaching in the two states relative to their neighbors.

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 policy reports and newspaper archives show that tenure protections for teachers were being debated across the country, not only in KY and NC. Newspapers throughout the 1930s 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.[20] 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 between the treated and control states.

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 prior to 1930.

5 The Effects of Employment Protections in Teaching

This section presents our main results. Estimates and 95% confidence intervals are presented in Figures 2, 3, 4, A1, and A2 and Tables 2, 3, 4, and A2. Unless otherwise noted, 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 1930. Robustness is discussed at the end.

5.1 Effects on Married Women

Direct Effects. We start by examining the most direct effect of the policy: how did introducing employment protections for married women in teaching affect the employment of married

[20] Note 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.

women as teachers?

First, we find that in response to the policy, schools became more likely to employ married women among their teaching staff. We estimate Equation (1) where our key outcome of interest is the share of teachers in a county who were 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 who were married women increased by 4.0 p.p. in treated counties between 1930 and 1940, roughly 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.

Another way to present this effect is to examine how much more likely married women were to work as teachers in response to the policy. Column (2) of Table 2 shows estimates of Equation (1), where the outcome of interest is the number of white married women teachers per hundred white married women.[21] We find that the employment protections resulted in roughly one additional married woman in every thousand married women working as a teacher. Relative to a baseline mean of 0.57 married women teachers per hundred married women, this estimate suggests that the employment protections in teaching resulted in a 17% increase in white married women's participation in teaching.

In Appendix C we extrapolate our estimates to the larger labor force by using a back-of-the-envelope to approximate how much the end of discriminatory hiring practices against married women in *all clerical and teaching jobs* contributed to the overall growth in married women's labor force participation in white-collar work between 1940 and 1950.[22] We estimate that the end of firms' discriminatory hiring practices against married women accounts for approximately 14% of the overall growth in married women's employment in white-collar jobs between 1940 and 1950.[23] We perform a similar exercise focusing on the growth in college-educated married women's employment and find that the end of employment discrimination against married women accounts for approximately 24% of the total 8.6 p.p. increase in college-educated white married women's labor force participation between 1940 and 1950. [24]

Notably, the effect of employment protections on married women's labor force participation in treated states was relatively short-lived. By 1950, the gap between treated and control

[21] Regressions are weighted by the total number of white married women in the county and year.

[22] We focus on the period between 1940 and 1950 to capture the tightest window around the widespread end of discriminatory hiring practices, which by the majority of accounts occurred during and immediately following World War II [Goldin, 1988].

[23] We define white-collar occupations as all professional/technical, managerial, clerical, and sales occupations. These occupations combined employed roughly 30% of the total labor force in 1940.

[24] Marriage bars were often thought to be targeted at educated married women in particular (see Goldin [1988]). In 1940, 84% of white married women teachers had at least one year of college, compared to only 9.5% of all white married women.

counties in the share of teachers who were married women shrank to be indistinguishable from zero. These results indicate that as employment discrimination against married women faded nationwide during the 1940s, control counties also began employing married women in teaching in larger numbers, effectively 'catching up' to treated counties.

Mechanisms. Having established that the employment protections pulled married women into teaching, we now explore the mechanisms driving our main effect. In particular, did the policy change pull married women into teaching who otherwise would not have worked (an 'extensive margin' effect) or did the policy change only affect women who would have worked even absent the policy, either by inducing unmarried teachers to marry or by inducing married workers to switch to teaching ('intensive margin' effects)?

We answer this question using our linked Census sample, the panel structure of which allows us to trace out the marriage and employment outcomes of individual women around the time that the employment protections were passed, conditioning on both their employment history and their marital status. We examine the outcomes of two groups of women: (1) unmarried women teachers and (2) married women not in the labor force.

Sample 1: Unmarried Women Teachers. We start by looking at unmarried women teachers in 1930, who after passage of the employment protections could get married and continue teaching. We construct our sample using the linked census data described in Section 3 as follows. First, we identify unmarried women under the age of 40 who were working as teachers in 1930.[25] For this group, our outcomes of interest are the county-level share of these women who were still teaching and/or married ten years later, in 1940. Then we identify the same groups in our pre-periods: identifying unmarried women teachers under 40 in 1920 and 1910 and computing their outcomes in 1930 and 1920 respectively. Finally, we use these linked county-level samples to construct a balanced sample of treated (KY and NC) and control (neighboring Southern) counties over time which we use to estimate our same (1) where t indexes the year in which the outcome is measured.[26] Results are shown in Panel 1 of Table 3.

Our first result shown in Column (2) of Table 3 is that the employment protections led to a 2 p.p. increase in the likelihood that unmarried women teachers got married and continued teaching ten years later, indicating that existing teachers were a significant driving force behind the overall effect of the policy. The increase is statistically significant at the 1% level and large

[25]We use the age cutoff of 40 to focus on women for whom the decision to marry is more likely to be a relevant margin. Our results are robust to alternate age cutoffs.

[26]Note that in this analysis, we are only able to measure outcomes for three years: 1920, 1930, and 1940, since linkages between 1940 and 1950 are not yet available as of writing. We also drop counties with fewer than five unmarried women teachers under 40 linked from 1920 to 1930 or from 1930 to 1940.

in context: only 5% of unmarried women teachers in 1920 were both married and teaching in 1930, meaning the protections led to a 40% increase in the propensity for unmarried women teachers to get married and stay in teaching.

Crucially, we find that the increase in unmarried women teachers' propensity to become married women teachers was entirely driven by changes in their decision to work rather than their decision to marry. The protections did not increase the rate at which unmarried women teachers got married, as shown in Column (1) of Table 3 Panel 1. Instead, unmarried women teachers responded to the protections by getting married "as planned" but *keeping their jobs*: conditional on marriage, the increased propensity to stay in teaching was offset by a 3.7 p.p. decrease in propensity to exit the labor force (as shown in Column (4)), with no effect on the likelihood of working outside of teaching (as shown in Column (3)). These results suggest that the policy had an *extensive margin* effect on unmarried women teachers: without changing their propensity to get married, the employment protections kept women who got married in teaching who otherwise would have left the labor force.

Sample 2: Married Women Outside the Labor Force. Married women who were not in the labor force prior to the employment protections would have been able to respond to the policy change by entering the labor force as teachers while remaining married. To explore this potential channel, we construct our sample of married women outside the labor force following a similar procedure to our first sample: identifying married women under the age of 50 who were not in the labor force in 1930 (and 1910 and 1920), computing their outcomes ten years later, and estimating Equation (1) as above.[27] Results are shown in Panel 2 of Table 3.

As shown by the dependent variable means, the vast majority of married women in our linked sample who are not in the labor force in 1920 are still married (94%) and not in the labor force (88%) in 1930. Yet relative to control counties, married women in treated counties who were not in the labor force prior to the protections became 0.1 p.p. more likely to become a teacher after the protections were passed. The effect is significant at the 1% level, and is somewhat remarkable given that so few white married women worked during this time period: only 0.2% of married and women outside the labor force in 1920 were married teachers in 1930, implying that the employment protections led to a 50% increase in the propensity for married women to enter teaching from outside the labor force. Column (3) shows that conditional on remaining married, there is no corresponding decrease in the likelihood of entering the labor force in a non-teaching occupation. Instead, there is a small but weakly significant (at the 10% level) decrease of 0.6 p.p. in the likelihood of staying married and out of the labor force shown

[27] We use the age cutoff of 50 to focus on married women for whom the decision to retire was less likely to be a relevant margin. Our results are robust to alternate age cutoffs.

in column (4), further suggesting that married women were actually being induced to join the labor force as opposed to being diverted from other occupations.

Comparison of Effects. Having established two primary channels through which married women entered teaching, we pause now to compare the magnitudes of the channels. Scaling each of the effects by the total number of women in each group in 1930 allows us to compare the approximate *number* of women each group contributed to the overall increase in married women in teaching induced by the employment protections.

In our linked sample,[28] there were 7,612 unmarried women teachers in the treated counties in 1930. The protections thus led to approximately 149 of said women (a 2 p.p. increase, as in panel 1 of Table 3) becoming married women teachers. Meanwhile, there were substantially more women outside the labor force: in our linked sample there were 437,888 married women outside the labor force in treated counties in 1930. This corresponds to roughly 285 married women (a 0.06 p.p. increase, as in panel 2 of Table 3) entering teaching.

In summary, the observed increase in married women teachers was driven predominantly by married women entering teaching from outside the labor force and to a smaller extent (with a total effect size roughly half that of the first channel) by unmarried teachers choosing to remain teachers even after getting married, rather than leaving the labor force.

Sample 3: Unmarried Women Not in the Labor Force. There is one more significant group that may have contributed to the increase in married women teachers: unmarried women who were not in the labor force prior to the employment protections being passed. We view these women as potentially contributing to our overall effect through both of the two channels discussed above: they could have become married women teachers either by first becoming teachers, then being induced by the policy change to remain teachers even after marriage (as with Sample 1), or by first becoming married, then being induced by the policy change to enter teaching (as with Sample 2). Although it is difficult to disentangle the two separate channels for this particular group without further information on their marriage and employment between decennial censuses, we can estimate overall effects for this group. To explore whether and by how much this group was affected by the policy change, we construct our sample of unmarried women outside the labor force following a similar procedure to our other two samples: identifying unmarried women between the ages of 8 and 40 who were not in the labor force in 1930

[28] Our preferred approach is to scale the estimates using population counts from the linked sample, given the estimates used in this exercise were obtained using the linked sample. Note that estimated population counts using the linked sample are attenuated by imperfect linkage—approximately 74% of women were linked between the 1930 and 1940 censuses in the Census Tree [Buckles et al., 2023]. As a robustness check, we also scale the estimates using the population counts from the full Census, and obtain qualitatively similar results.

(and 1910 and 1920), computing their outcomes in 1940 (and 1920 and 1930), and estimating Equation (1).[29] Results are shown in Panel 3 of Table 3.

Our main finding for this sample is that we observe that the employment protections induce unmarried women outside the labor force to get married and enter teaching at a 0.1 p.p. higher rate. While small, this estimates amounts to a 26% increase relative to the low baseline rate of entry into teaching conditional on marriage, and is significant at the 1% level.

The remainder of these results should be interpreted with caution, since the baseline effects of the employment protections, while large within teaching, are small relative to the entire population of unmarried women and are thus easily obscured by noise. We observe once again that the employment protections do not induce unmarried women to marry at higher rates.[30] Instead, our results are again suggestive of unmarried women, conditional on marriage, entering teaching at higher rates *rather than leaving the labor force*. The employment protections are associated with a small but significant 1.1 p.p. decrease in unmarried women outside the labor force getting married and staying out of the labor force, with no effect on their propensity to get married and enter the labor force in occupations other than teaching. These results are again suggestive of an extensive margin effect, i.e. of the employment protections inducing these women, conditional on marriage, to work as teachers rather than remaining outside the labor force.

Note that this group did contribute substantially to the total effect: in our linked sample, there were 324,444 unmarried women who were not in the labor force in the treated counties in 1930. The protections therefore led to approximately 400 of said women (a 0.12 p.p. increase, as in panel 2 of Table 3) becoming married women teachers.

Other Working Women. Two additional groups of women that may have contributed to the increase in married women in teaching were unmarried and married women who were working but not teachers prior the employment protections. Results for these groups are presented in Table A1. We find that the employment protections had no significant effects on the likelihood of marriage, the likelihood of teaching, or the likelihood of teaching, working, and exiting the labor force conditional on marriage. This further emphasizes that the increase in married women in teaching was driven by an extensive margin response (women becoming less likely to not work) rather than an intensive margin response (other working women switching to teaching).

[29] We use the lower bound on age to ensure our sample will be 18 or older in the year the outcome is measured. Our results are robust to alternate age cutoffs.

[30] If anything, we observe that unmarried women outside the labor force in treated counties are slightly *less* likely to marry relative to in control counties (although significant at the 5% level, the effect amounts to a 1% decrease relative to the 1930 treated mean).

5.2 Effects on Men and Unmarried Women

Direct Effects. What were the consequences of the laws on the employment of men and unmarried women, who were not directly targeted by the employment protections? First, we look at effects within teaching. One possibility is that the influx of married women simply led to an overall expansion of the teacher labor force, resulting in larger teacher populations overall with no effect on men and single women teachers. 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 4 and Appendix Figure A1).

As such, because the total number of teachers did not change in response to the policy, it must be the case that the increased share of married women teachers resulted in a corresponding decrease in the share of men and/or single women teachers. To confirm this, we estimate Equation (1) using as our outcomes the share of teachers that were men and unmarried women. The results for men and unmarried women are shown in Figure 2 and Table 4 as light purple circles (Column (2)) and black squares (Column (3)) respectively.

We find that the employment protections affected the employment of single women in teaching, but not of men. The increase in married women teachers was entirely offset by a 4.3 p.p. (7%) decrease in the share of teachers who were unmarried women, significant at the 1% level, with no effect on the share of teachers who were men.

Why was men's employment in teaching unaffected by the policy? 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 women to uphold to the norm that men needed jobs to provide for their families.

Mechanisms. Given that the entry of married women into teaching came at the expense of the exit of unmarried women, we now unpack where unmarried women may have been pushed out of teaching to. In particular, did the policy change push unmarried women out of teaching into other occupations (an 'intensive margin' effect) or out of the labor force entirely (an 'extensive margin' effect)?

We begin with our linked sample of unmarried women teachers (Sample 1). Having es-

tablished that the employment protections had no effect on the incidence of marriage for these women, we investigate outcomes for this sample of women *conditional on remaining unmarried* by estimating Equation (1).

Figure 3 and Panel (1) of Table A2 show the estimated effects of the laws on the share of linked unmarried women teachers in $t-10$ who both remained unmarried and stayed teachers (light purple circles and Column (2)), stayed in the labor force but changed occupations (purple triangles and Column (3)), and left the labor force (black squares and Column (4)). We find a decrease, although insignificant, in the share of unmarried women teachers who remained unmarried women teachers. However, we also find a weakly significant (at the 10% level) increase in the share of unmarried women teachers who leave the labor force and remain unmarried. These estimates suggest that there may have been an extensive margin effect of the policy on unmarried women's labor force participation: some unmarried women are pushed out of teaching and out of the labor force entirely. There is also an increase in the share of unmarried women teachers who remain unmarried and leave teaching but remain in the labor force: this is suggestive of a potential intensive margin channel for the decrease in unmarried women teachers, but the estimate is not significant at the 10% level.

Finally, it is also possible that the employment protections reduced the number of unmarried teachers by limiting the number of young women who *became* teachers, in addition to pushing out incumbent teachers. We find no evidence, however, that this was the case. When looking at both the sample of unmarried women who were previously not in the labor force (Panel 2 of Table A2) and the sample of unmarried women who were previously working outside of teaching (Panel 3 of Table A2), we find no significant effect of the employment protections on the probability of either of these groups entering teaching conditional on remaining unmarried.

Evaluating Overall Effects Our estimates thus far have shown clear evidence of the *positive* extensive margin effects of the employment protections for women initially outside the labor force and no evidence of any negative effects for women outside the labor force or in non-teaching jobs. We can therefore conclude that for women outside the labor force, the employment protections had a net positive effect.

For incumbent teachers, however, our evidence is more mixed. We have shown that while the employment protections led to incumbent teachers remaining in teaching after marriage at higher rates, it may have also pushed incumbent teachers who remained unmarried out of teaching. To evaluate the overall effect of this policy on incumbent teachers, we once again estimate Equation (1) with our linked Sample 1 of unmarried women teachers in $t-10$ and with the outcome of the county-level labor force participation of these women. The result for

the entire sample, regardless of marital status, is presented in grey circles in Figure 4. There is a small increase in the share of unmarried women teachers who remain in the labor force, approximately 2 p.p. relative to a baseline mean of 38% in treated counties in 1930, but the effect is not significant.

When we decompose the overall effect by separately analyzing the share of unmarried women teachers who remain in the labor force conditional on marriage (purple triangles) and conditional on remaining unmarried (black squares), we find that the effect is largely driven by the significant positive effect of the employment protections on unmarried women teachers who **become married**, with a slight negative effect for unmarried women teachers who remain unmarried. On net, the effect on incumbent teachers' labor force participation is indistinguishable from zero.

We supplement this analysis in Figure A2 by using the same sample to compute average *occupational scores* in year t in lieu of income data in the Census prior to 1940.[31] We find remarkably similar relative effect magnitudes and confidence intervals as we do for labor force participation, suggesting that the bulk of the effect of the policy happens on the extensive margin (effectively a tradeoff between an occupational score of 0 from being outside of the labor force and an occupational score of 27 from being a teacher). Particularly for teachers who remain unmarried, if they predominantly enter other similar occupations (e.g. secretarial work) after leaving teaching, we might expect the effect on occupational scores for unmarried women to be smaller in magnitude. The fact that we do not see such an effect is further evidence that few unmarried women leave teaching to enter occupations as a result of this policy.

5.3 Robustness Checks

We close the results section with a series of robustness checks. Though the absence of pre-trends and the historical context described in Section 4.2 lend credibility to our identifying assumption of parallel trends in treated and control counties, one may still be concerned that the estimates 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

[31] Occupational scores are numerical ratings of occupations, based on the average income associated with the occupation in 1950, ranging from 0 to 80. They are commonly used in the economic history literature as proxies for income (see e.g. Lukas's JMP – FILL IN CITATION).

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 dominated by women during the early 1900s and 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 attitudes or 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 A3 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.

Matching. We use neighboring Southern states as our preferred control group for two reasons. First, due to geographical proximity, neighboring states were likely most similar in terms of culture. Second, 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. Historical newspapers show that there were cases in each of the control states in which rulings came down in favor of married women in some cases but school boards in others. Legislators in VA even proposed a similar state-wide bill as was passed in KY and NC, but it did not pass. Although anecdotal, we interpret these reports as evidence that the control states were engaged in similar discussions.

Regardless, one might be concerned that the neighboring Southern states are not as close a control as possible. We therefore evaluate in Appendix D whether our results are sensitive to using other counties as our control group obtained through various matching techniques. We 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 married women as well as both unmarried women and men. We find that the protections led to an increase in the share of teachers who were married women. This effect was driven by changes in women's decision to work rather than by women's decision to marry—while we see no significant increase in marriage rates for any group of women, we do see both incumbent unmarried teachers remaining in teaching after marriage rather than leaving the labor force as well as married women originally outside of the labor force entering teaching after the employment protections were implemented.

We find, however that this increase in married women's participation in teaching was offset by a decrease in the share of teachers who were unmarried women, with no effect on men or the total number of teachers. We find suggestive evidence that this decrease was driven by incumbent teachers who remained unmarried and left the labor force rather than stay teachers. Overall, our findings suggest that while the policy did displace some unmarried women, the net effect on women's labor force participation was positive. Our 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, suggesting that this type of policy may also be effective globally in a modern-day context.

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7 Figures

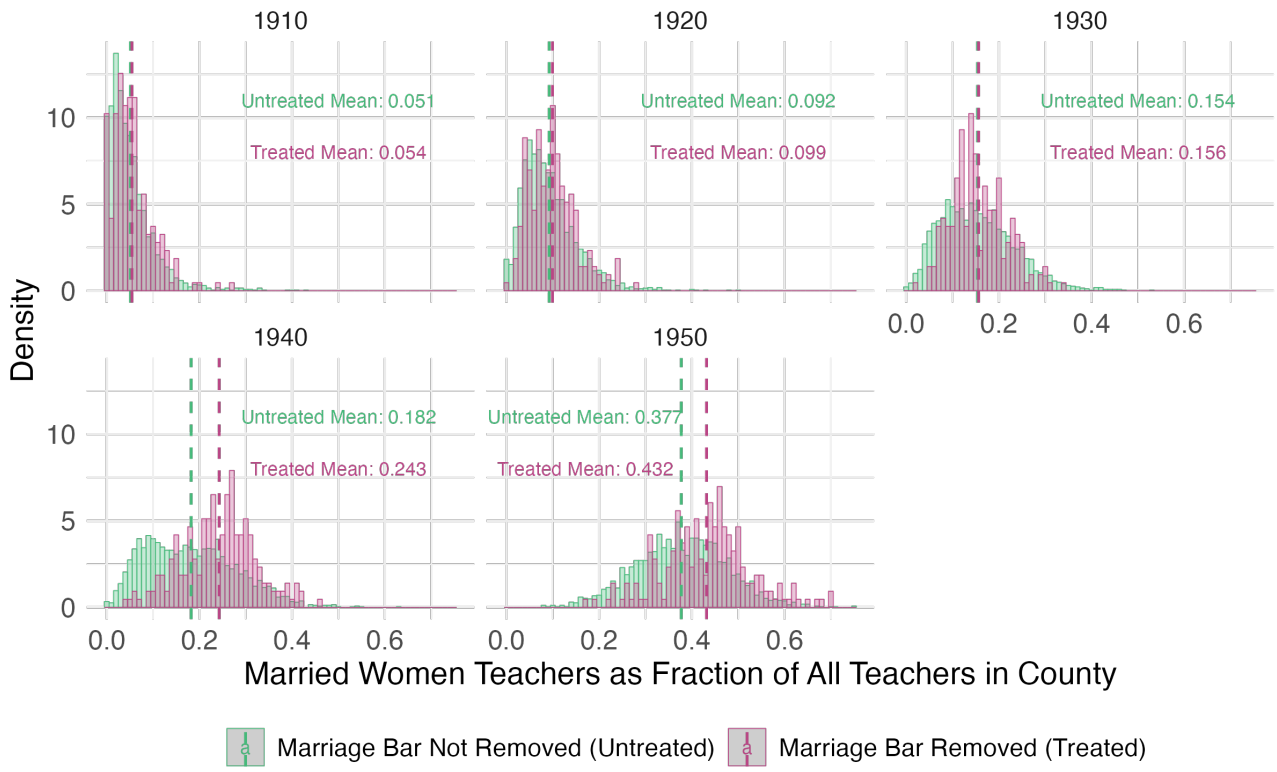


Figure 1: Density plots from 1910 to 1950 of the county-level fraction of white teachers who are married women. Separate distributions are shown for (1) counties in states where employment protections were passed in the 1930s (KY, NC), and (2) all other counties in the country. Group means are indicated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

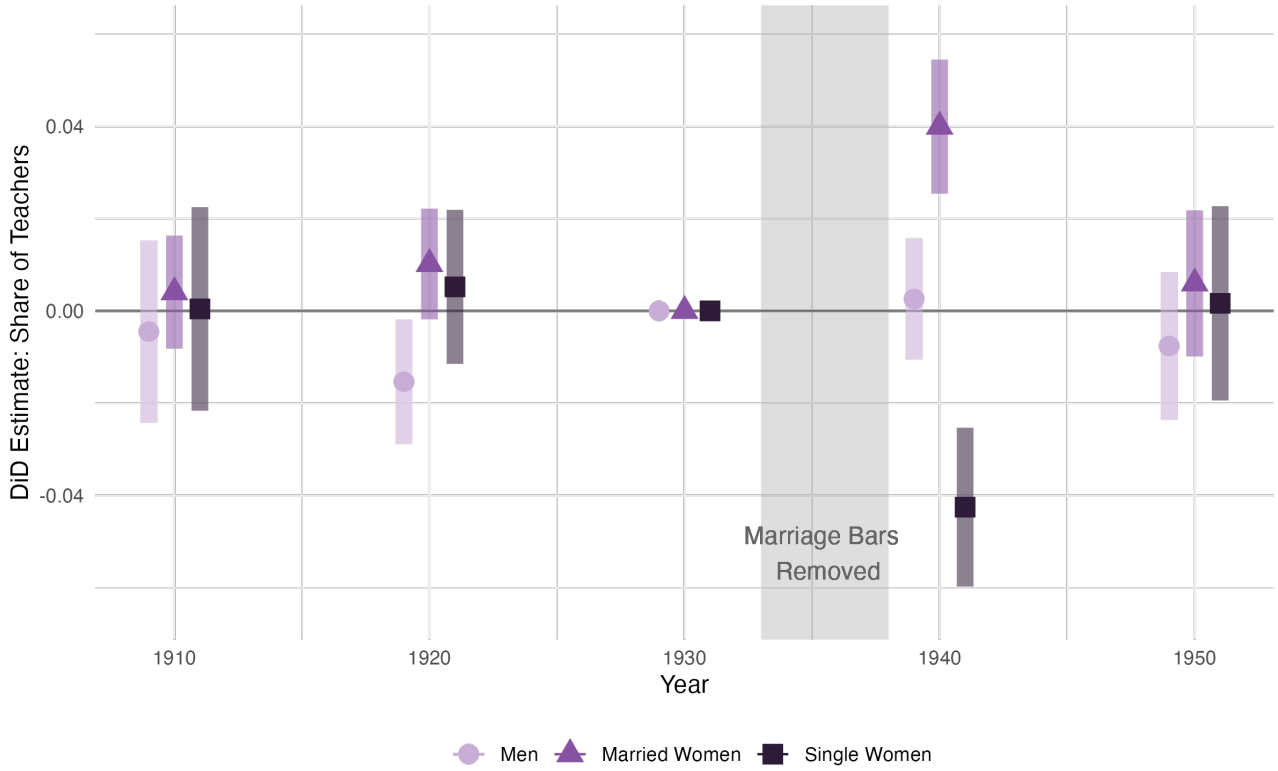


Figure 2: Estimated effects of the introduction of employment protections for married women in teaching on the *gender composition* of teachers, at the county level. Estimates are from a difference-in-differences specification where the dependent variable is the share of teachers in a county that are 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.

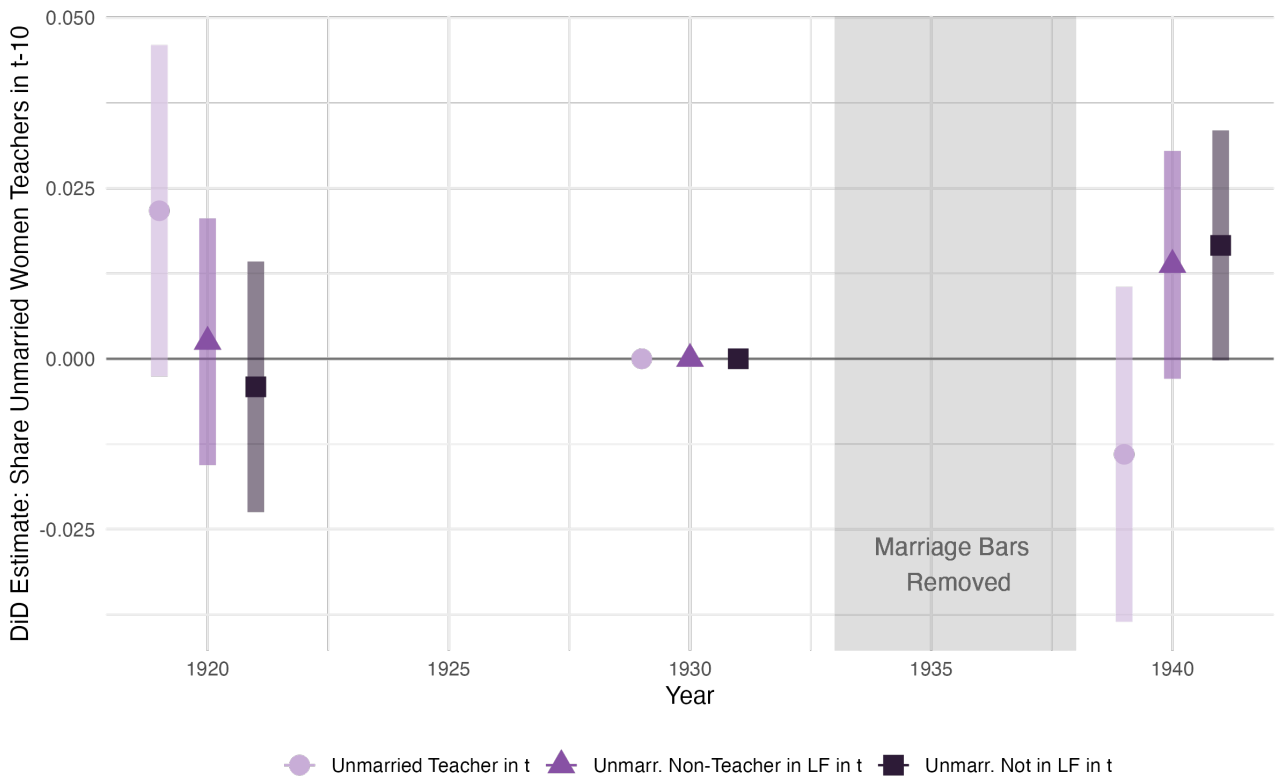


Figure 3: Estimated effects of the introduction of employment protections for married women in teaching on the *employment outcomes of unmarried women teachers* who remain unmarried. Analysis uses linked sample of unmarried women teachers in $t-10$ and measures the share that remain unmarried teachers (light purple circles), remain unmarried and leave teaching but stay in the labor force (purple triangles), and remain unmarried and leave the labor force (black squares) in year t.

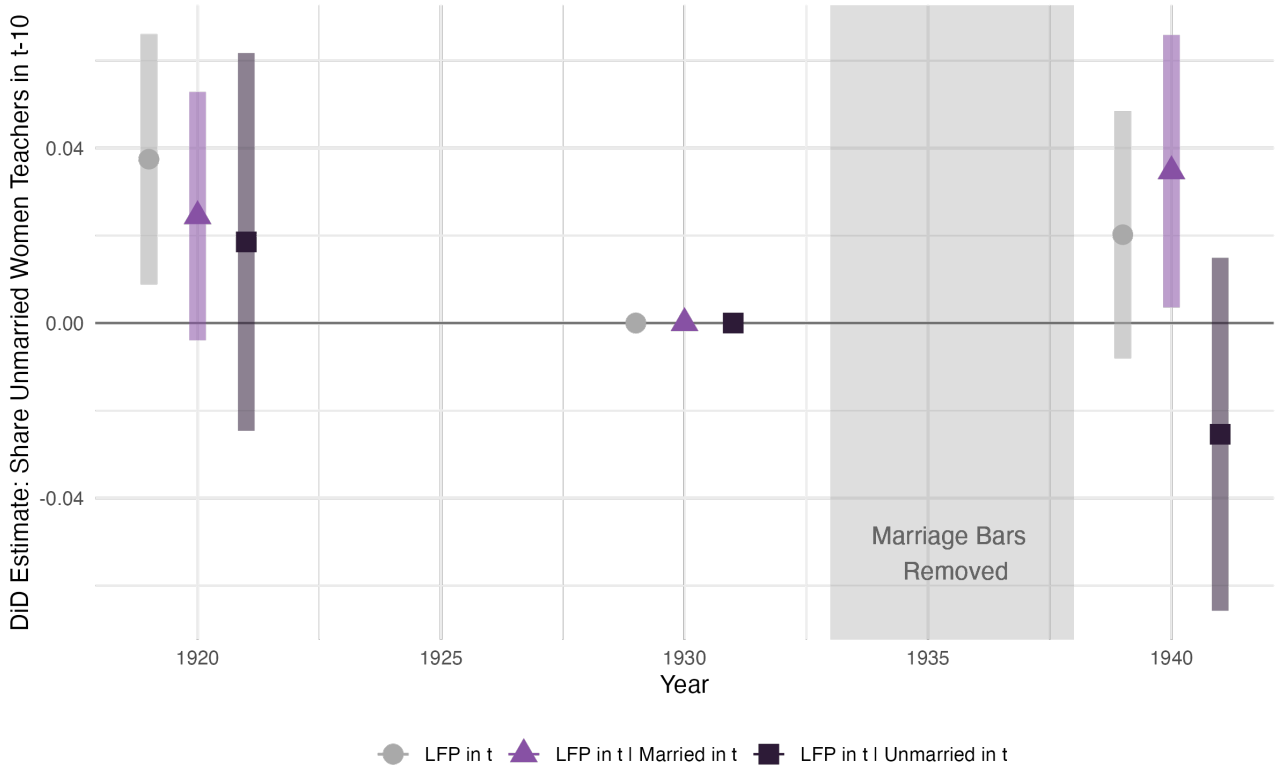


Figure 4: Estimated effects of the introduction of employment protections for married women in teaching on labor force participation of incumbent unmarried teachers. Analysis uses linked sample of unmarried women teachers in $t-10$ and measures mean occupational score for the full sample, conditional on marriage, and conditional on remaining unmarried, in year t.

8 Tables

Table 1: Summary of key county-level statistics by county group in 1930

	All	South	Treated	Neighb. Sth.
	(1)	(2)	(3)	(4)
Panel A: General County Statistics				
Population (Thous.)	39.61	26.69	26.3	26.59
	(2.427)	(1.322)	(2.061)	(1.736)
White School-Age Pop. (Thous.)	9.013	5.605	6.288	5.945
	(0.514)	(0.25)	(0.416)	(0.34)
Share Urban	0.214	0.155	0.134	0.173
	(0.005)	(0.008)	(0.013)	(0.016)
LFP of Married Women	0.102	0.136	0.092	0.118
	(0.001)	(0.003)	(0.004)	(0.005)
LFP of White Married Women	0.081	0.082	0.067	0.078
	(0.001)	(0.002)	(0.003)	(0.003)
Num. Children of Marr. Wom.	2.036	2.247	2.355	2.308
	(0.007)	(0.012)	(0.029)	(0.02)
Panel B: County Statistics on White Teachers				
Students/Teachers	30.61	38.52	44.53	36.23
	(0.203)	(0.404)	(0.937)	(0.591)
Share Men	0.197	0.192	0.218	0.205
	(0.002)	(0.003)	(0.007)	(0.006)
Share Single Women	0.645	0.639	0.627	0.645
	(0.002)	(0.004)	(0.007)	(0.007)
Share Married Women	0.158	0.169	0.156	0.15
	(0.002)	(0.002)	(0.004)	(0.004)
N (Counties)	3100	944	220	320

Notes: All statistics are measured using the full count 1930 census data, aggregated to the county level [Ruggles et al., 2024b]. 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 married women teachers

	Dependent Variable:	
	Share Teach Mar. Wom.	MW Teach/100 MW
	(1)	(2)
Treated × 1940 (γ^{DD}_{1940})	0.040***	0.098***
	(0.007)	(0.036)
Treated × 1950 (γ^{DD}_{1950})	0.007	−0.014
	(0.008)	(0.032)
Dep. Var. 1930 Treated Mean	0.1559	0.5721
Observations	2,635	2,635
Adjusted R^2	0.839	0.719

Notes: Estimation follows equation 1. The estimation sample includes counties in treated states (KY, NC) and neighboring southern states (VA, SC, TN, WV) in 1910-1950. The outcome in Column (1) is the share of white teachers that are married women and the outcome in Column (2) is the share of white married women (ages 18-64) that are working as teachers, multiplied by 1000. All regressions use the 1910, 1920, 1930, 1940, and 1950 IPUMS full count Censuses [Ruggles et al., 2024b].

Table 3: Estimated effects of the employer protections on women's propensity to get married and conditional on marriage, to teach, work outside of teaching, and exit the labor force.

Dependent Variable:	Pr(Married in t)	Pr(Married Teacher in t)	Pr(Married Non-Teacher in LF in t)	Pr(Married Not in LF in t)
	(1)	(2)	(3)	(4)
Sample 1: Women who were unmarried and teaching in $t-10$				
Treated × 1940 (γ^{DD}_{1940})	−0.016	0.020***	0.001	−0.037***
	(0.014)	(0.007)	(0.005)	(0.014)
Dep. Var. 1930 Mean	0.6225	0.05053	0.0351	0.5369
Observations	1,545	1,545	1,545	1,545
Adjusted R^2	0.471	0.199	0.007	0.501
Sample 2: Women who were married and not in the labor force in $t-10$				
Treated × 1940 (γ^{DD}_{1940})	−0.001	0.001***	0.004	−0.005*
	(0.001)	(0.0002)	(0.003)	(0.003)
Dep. Var. 1930 Mean	0.9352	0.001903	0.04982	0.8835
Observations	1,584	1,584	1,584	1,584
Adjusted R^2	0.648	0.291	0.402	0.565
Sample 3: Women who were unmarried and not in the labor force in $t-10$				
Treated × 1940 (γ^{DD}_{1940})	−0.007**	0.001***	0.003	−0.011***
	(0.003)	(0.0003)	(0.002)	(0.003)
Dep. Var. 1930 Mean	0.5284	0.004728	0.03895	0.4847
Observations	1,584	1,584	1,584	1,584
Adjusted R^2	0.848	0.252	0.587	0.885

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 Census Tree linkages. From these women, we construct three samples: Sample 1, containing linked women who were under 40, unmarried, and teaching in 1920 and 1930, Sample 2, containing linked women who were aged 8-40, unmarried, and not in the labor force in 1920 and 1930, and Sample 3, containing linked women who were aged 18-50, married, and not in the labor force 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.

Table 4: 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 × 1940 (γ^{DD}_{1940})	0.040***	0.002	−0.042***	−0.182
	(0.007)	(0.007)	(0.009)	(7.410)
Treated × 1950 (γ^{DD}_{1950})	0.007	−0.009	0.002	−3.063
	(0.008)	(0.008)	(0.011)	(6.246)
Dep. Var. 1930 Treated Mean	0.1559	0.2167	0.6274	155.1
Observations	2,635	2,635	2,635	2,635
Adjusted R^2	0.839	0.680	0.826	0.840

Notes: Estimation follows equation 1. The estimation sample includes counties in treated states (KY, NC) and neighboring southern states (VA, SC, TN, WV) in 1910-1950. 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 1910, 1920, 1930, 1940, and 1950 IPUMS full count Censuses. [Ruggles et al., 2024b].

A Additional Figures

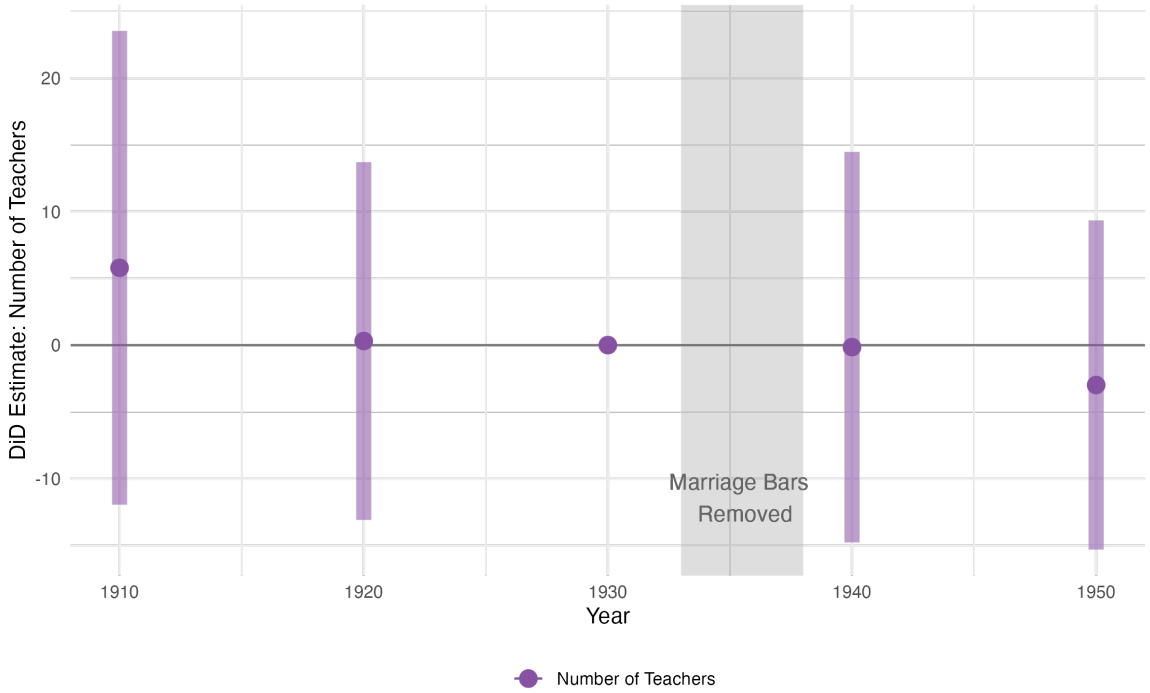


Figure A1: Estimated effects of the introduction of employment protections for married women on the total number of teachers per county.

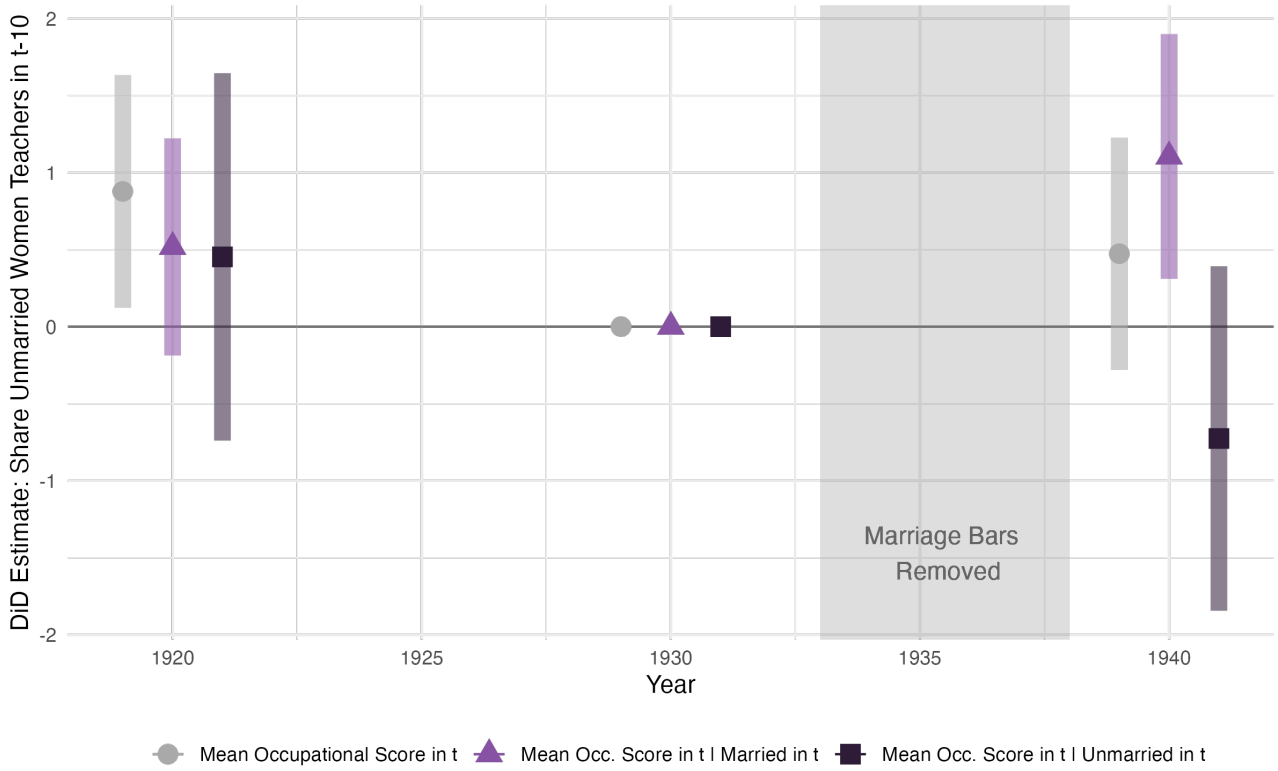


Figure A2: Estimated effects of the introduction of employment protections for married women in teaching on the occupational score of incumbent unmarried teachers. Analysis uses linked sample of unmarried women teachers in $t-10$ and measures mean occupational score for the full sample, conditional on marriage, and conditional on remaining unmarried, in year t.

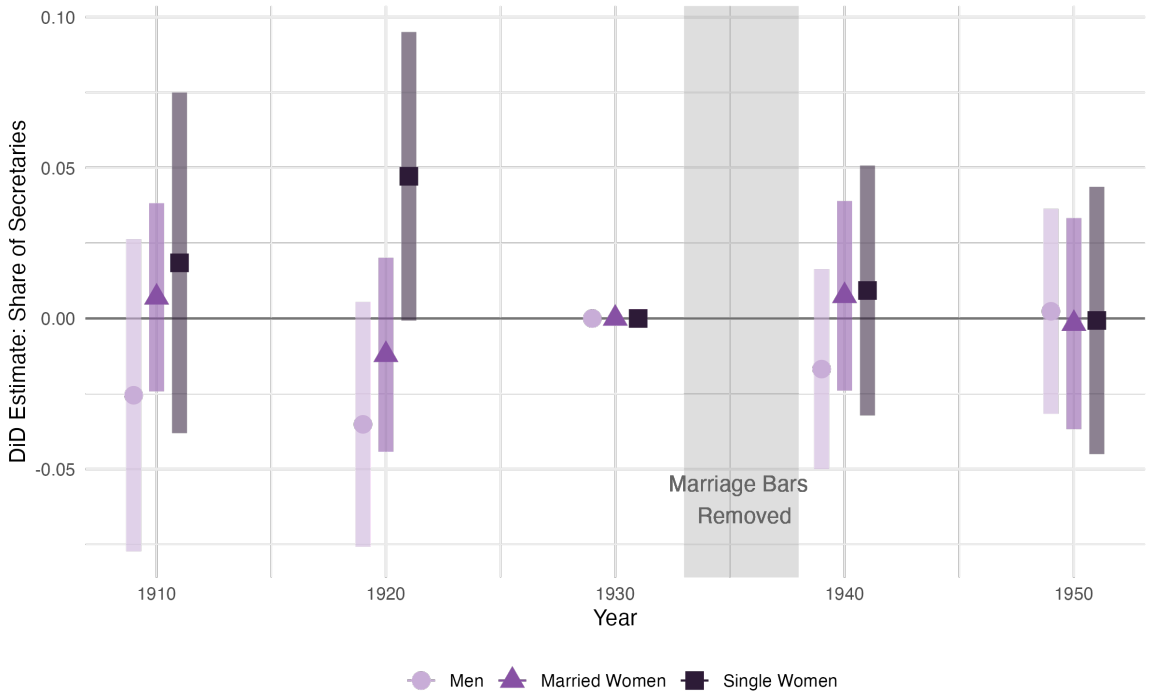


Figure A3: Placebo test: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *secretaries* who are men, unmarried women, and single women.

B Additional Tables

Table A1: Estimated effects of the employer protections on women's propensity to get married and to teach, work outside of teaching, and exit the labor force.

Dependent Variable:	Pr(Married in t)	Pr(Teacher | Married in t)	Pr(Non-Teacher in LF | Married in t)	Pr(Not in LF | Married in t)
	(1)	(2)	(3)	(4)
Sample 4: Women who were unmarried and working as non-teachers in $t-10$				
Treated × 1940 (γ^{DD}_{1940})	−0.011	0.001	−0.006	0.005
	(0.009)	(0.002)	(0.011)	(0.011)
Dep. Var. 1930 Mean	0.5268	0.007552	0.121	0.8715
Observations	1,527	1,527	1,527	1,527
Adjusted R^2	0.468	0.002	0.436	0.434
Sample 5: Women who were married and working as non-teachers in $t-10$				
Treated × 1940 (γ^{DD}_{1940})	0.001	−0.001	0.001	0.00001
	(0.007)	(0.002)	(0.012)	(0.012)
Dep. Var. 1930 Mean	0.8807	0.00702	0.1506	0.8424
Observations	1,494	1,494	1,494	1,494
Adjusted R^2	0.127	0.044	0.511	0.503

Notes: See notes for Table 3. Sample 4 contains linked women who were aged 8-40, unmarried and in the labor force but not working as teachers in 1920 and 1930, and Sample 5 contains linked women who were aged 18-50, married and in the labor force but not working as teachers in 1920 and 1930.

Table A2: Estimated effects of the employer protections on women's propensity to remain unmarried and to teach, work outside of teaching, and exit the labor force.

Dependent Variable:	Pr(Unmarried in t)	Pr(Unmarried Teacher in t)	Pr(Unmarried Non-Teacher in LF in t)	Pr(Unmarried Not in LF in t)
	(1)	(2)	(3)	(4)
Sample 1: Women who were unmarried and teaching in $t-10$				
Treated × 1940 (γ^{DD}_{1940})	0.016	−0.014	0.014	0.017*
	(0.014)	(0.013)	(0.009)	(0.009)
Dep. Var. 1930 Mean	0.3775	0.2075	0.0844	0.08557
Observations	1,545	1,545	1,545	1,545
Adjusted R^2	0.471	0.527	0.072	0.085
Sample 3: Women who were unmarried and not in the labor force in $t-10$				
Treated × 1940 (γ^{DD}_{1940})	0.011	0.002	−0.007	0.016*
	(0.009)	(0.002)	(0.009)	(0.009)
Dep. Var. 1930 Mean	0.4732	0.01315	0.2887	0.1714
Observations	1,527	1,527	1,527	1,527
Adjusted R^2	0.468	0.076	0.592	0.388
Sample 4: Women who were unmarried and working as non-teachers in $t-10$				
Treated × 1940 (γ^{DD}_{1940})	0.007**	0.001	−0.003	0.009**
	(0.003)	(0.001)	(0.003)	(0.004)
Dep. Var. 1930 Mean	0.4716	0.03327	0.1402	0.2981
Observations	1,584	1,584	1,584	1,584
Adjusted R^2	0.848	0.620	0.895	0.576

Notes: See notes for Tables 3 and A1.

C Estimating the Role of Employment Protections in the Overall Increase in Married Women's LFP

C.1 Elasticity Calculation

To estimate the role that the implementation of employment protections played in the overall increase in married women's LFP, we begin by calculating the elasticity of the likelihood of a married women working as a teacher with respect to the passing of employment protections (ε_{EP}^{teach}) using the following formula:

$$\varepsilon_{EP}^{teach} = \frac{\Delta s_{teach,1930-1940}^{MW} / s_{teach,1930}^{MW}}{\Delta q_{emp,1930-1940}^{teach} / q_{emp,1930}^{teach}} \tag{2}$$

where $s_{teach,t}^{MW}$ represents the share of married women who were working as teachers in treated states in year t and $q_{emp,t}^{teach}$ represents the share of teachers in treated states in year t who were not covered by employment protections (and therefore potentially subject to discrimination on the basis of their marital status). $\Delta s_{teach,t-r}^{MW}$ and $\Delta q_{emp,t-r}^{teach}$ represent the changes in the respective variables between year t and year r.

The first term in the numerator can be taken directly from our empirical estimate of the effect of the employment protections on the likelihood of a married woman in a treated county working as a teacher, as shown in column (5) of Table 4. The estimated coefficient $\hat{\gamma}_{1940}^{DD} \equiv \Delta \hat{s}_{teach,1930-1940}^{MW} = 0.9767/1000 = 0.0009767$. The baseline mean in 1930, weighted by the total number of married women in each county, is 0.005724. Therefore the numerator (representing the total contribution of the lifting of marriage bars to the increase between 1930 and 1940 in treated states in married women's likelihood of being a teacher) is 0.171.

In calculating the denominator, note that by 1940 all teachers in treated states were covered by employment protections ($q_{emp,1940}^{teach} = 0$), regardless of the initial value of $q_{emp,1930}^{teach}$. The denominator of equation (2) is thus equal to 1.

We therefore estimate that the elasticity of married women's employment in teaching to the implementation of employment protections in teaching is $\varepsilon_{EP}^{teach} = 0.171$.

C.2 Other Occupations

The key assumption in this back of the envelope calculation is that $\varepsilon_{EP}^{teach} = \varepsilon_{EP}^{o \in \mathcal{O}}$ for all occupations $o \in \mathcal{O}$ subject to marriage bars: that is, that the change in married women's employment in teaching due to the implementation of employment protections in teaching is equivalent to the change in married women's employment in any occupation due to the elimination of dis-

criminatory hiring practices in that occupation. We also assume that for all occupations subject to marriage bars, no married women were subject to discriminatory hiring practices by 1950, i.e. that $\Delta q^o_{emp,1940-1950}/q^o_{emp,1930} = 1$ for all $o \in \mathcal{O}$. The latter assumption is strong especially as it is known that some occupations like teaching still had marriage bars (although at much lower rates) in 1950, but since marriage bars disappeared by "the 1950s" [Goldin, 1988] and our data is decennial, we take 1950 as the proximate end of marriage bar use.

Goldin [1988] refers to marriage bars as broadly covering 'clerical workers and teachers'. For this reason, our preferred definition of 'marriage bar occupations' includes all clerical workers and teachers. For robustness, we also include estimates for a more conservative estimate of occupations affected by marriage bars, which only includes occupations specifically named as being subject to marriage bars (teachers, secretaries/attendants, and bank tellers).

C.3 Calculation

Under these assumptions, we can estimate the total change in white married women's LFP in these occupations between 1940 and 1950 **due to** the removal of institutional barriers to employment (or equivalently, the introduction of employment protections) as follows:

$$\Delta s^{MW}_{MB,1940-1950} = \sum_{o \in \mathcal{O}} \varepsilon^o_{EP} \cdot s^{MW}_{o,1940} = \varepsilon^{teach}_{EP} \sum_{o \in \mathcal{O}} s^{MW}_{o,1940} = \varepsilon^{teach}_{EP} \cdot s^{MW}_{MB,1940} \tag{3}$$

where $s^{MW}_{MB,1940}$ represents the total share of married women working in all marriage bar-related occupations in 1940.

Under our preferred definition of marriage bar occupations, we have $s^{MW}_{MB,1940} = 0.03724$, implying that $\Delta s^{MW}_{MB,1940-1950} = 0.006353$. The total growth in the share of married women in these occupations between 1940 and 1950 is 0.02996, implying that the removal of institutional barriers accounts for **21.2%** of the growth in married women's LFP in clerical work and teaching. Our more conservative definition of marriage bars suggests that the removal of institutional barriers accounts for 33.8% of the total growth in the specific occupations known to be directly affected by marriage bars.

White-Collar Occupations. The first calculation we present in the body of the paper is the estimated contribution of the removal of institutional barriers to the increase in married women's participation in *all white-collar occupations*, including professional/technical, managerial, clerical, and sales occupations.[32] The total growth in the share of white married women in white-collar occupations between 1940 and 1950 is 0.04609. Using our preferred estimate of

[32] Approximately 30% of the total labor force was employed in a white-collar occupation in 1940.

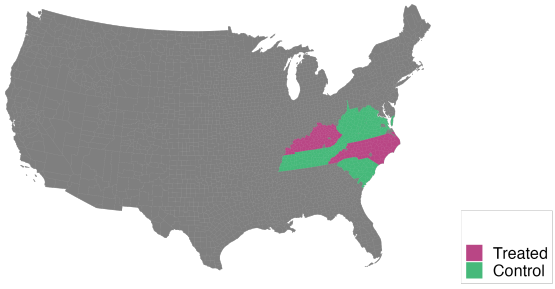
$\Delta s^{MW}_{MB,1940-1950} = 0.006353$ implies that the removal of institutional barriers accounts for 13.8% of the total growth in white married women working in white-collar occupations.

College-Educated Women. The second calculation we present in the body of the paper is the estimated contribution of the removal of institutional barriers to the increase in *college-educated* married women's total LFP. The total growth in the LFP of white married women between 1940 and 1950 is 0.08576. Re-computing the marriage bar removal-induced increase in LFP for college-educated married women gives $\Delta s^{MW}_{MB,1940-1950} = 0.02075$ (since the initial share of college-educated white married women working in marriage bar-related occupations in 1940 was 0.1216). Therefore we calculate that the removal of institutional barriers accounts for 24.2% of the total growth in college-educated white married women's LFP.

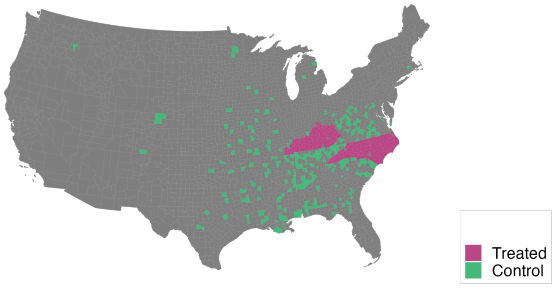
D Matched Counties Design

As discussed in Sections 3.3 and 5.3, our preferred specification relies on the assumption that in the absence of the laws 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.

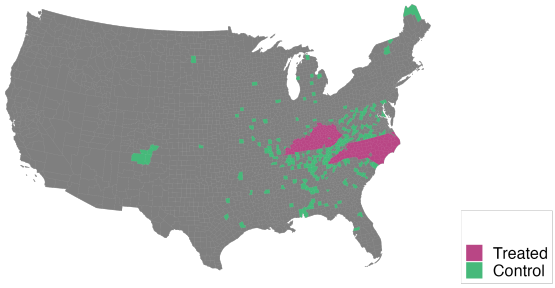
Figure A4: Maps of treated (pink) and control (green) counties for (a) our preferred control specification of neighboring Southern counties and (b)-(d) specifications using a range of matching techniques.



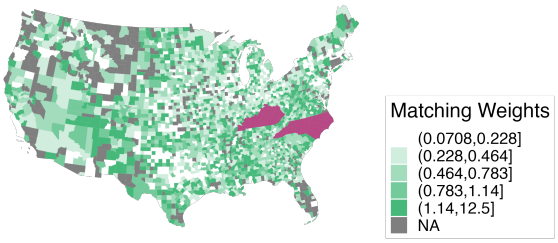
(a) Southern Neighbor States



(b) Match 1: Nearest Neighbor



(c) Match 2: Genetic



(d) Match 3: Optimal Full Matching

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., 2024b]. 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].

[33] 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 using genetic matching as developed by Diamond and Sekhon [2013], Sekhon [2011]; the third using 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 assigns weights to control counties based on their similarity to treatment counties. Figure A4 compares the control counties selected by the various matching methods to the neighboring southern states in our preferred specification. Matched samples 1 and 2 are geographically concentrated in the neighboring Southern states, reinforcing the fact that the neighboring Southern counties are indeed similar to our treated counties. Panel (d) of Figure A4 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.

[33] 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.

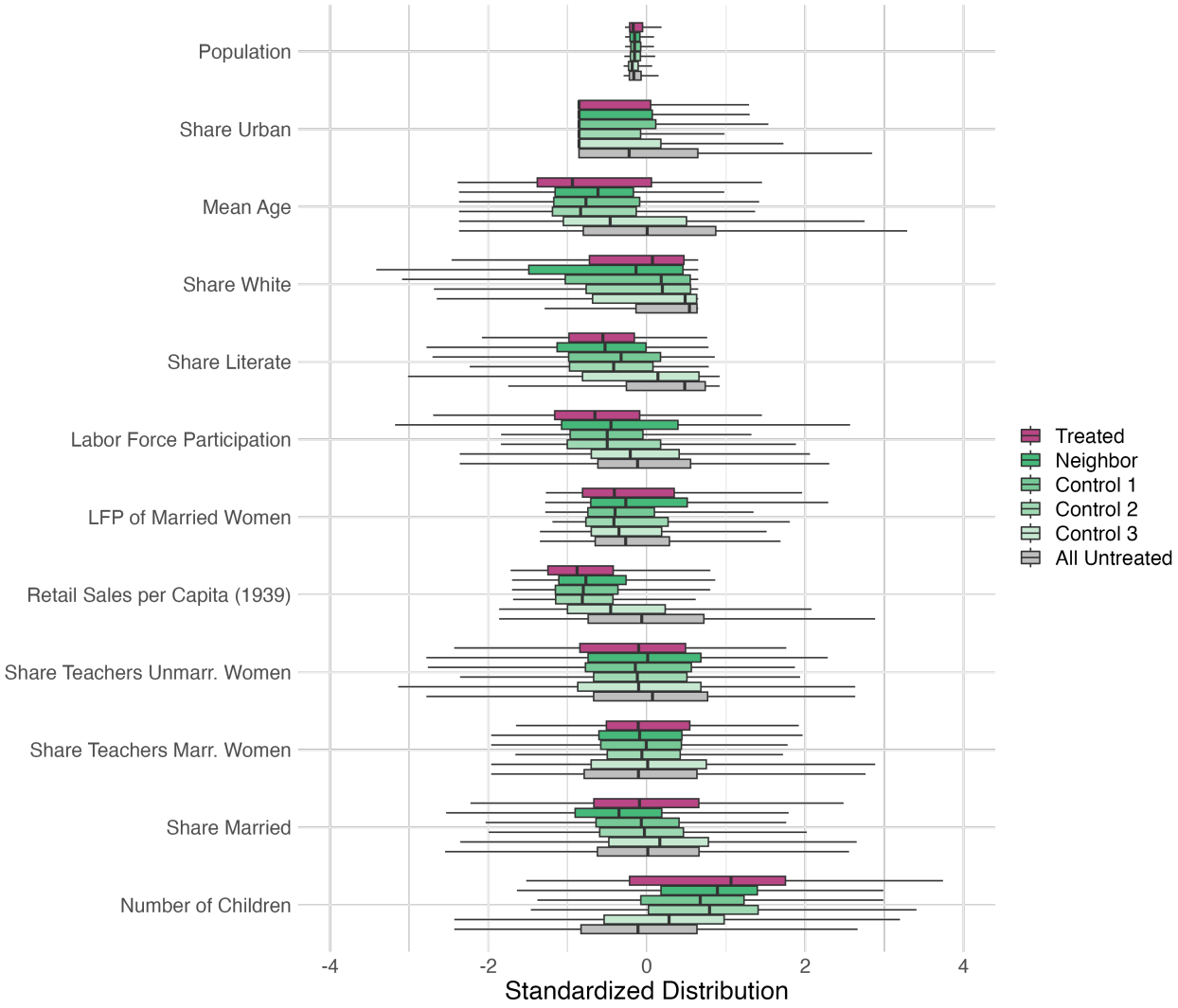


Figure A5: Boxplot of standardized 1930 values of various covariates by treatment or control group. The center bar represents the median, the edges of the box represent the 25th and 75th percentiles, and the edges of the whiskers represent extrema, with outliers removed (see R function geom_boxplot for further details). Distributions are weighted for control group 3. Covariates are outlined in detail in Footnote 33, 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. [2024b] with the exception of 1939 Retail Sales per Capita, which is obtained from Fishback et al. [2005].

In Figure A5, we graph boxplots for the treated and various control groups of the standardized 1930 values of the matching covariates listed in Footnote 33, as well as two additional variables not used for matching (share of women married and average number of children for married women). Motivating the need to identify an appropriate control group for KY and NC, the boxplots show that the average untreated county is quite distinct from the treated counties. Importantly, the neighoring Southern counties are very similar to the treated counties, and on

some dimensions (e.g. share of teachers married women, number of children) even outperform the matched county groups in terms of similarity. 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.

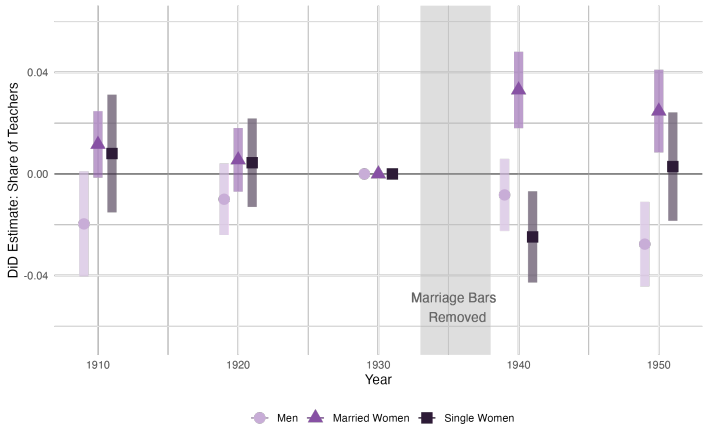
D.1 Results

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 A6. 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 A5 suggests that the control group for matched sample 3 is the least comparable to the treatment group.

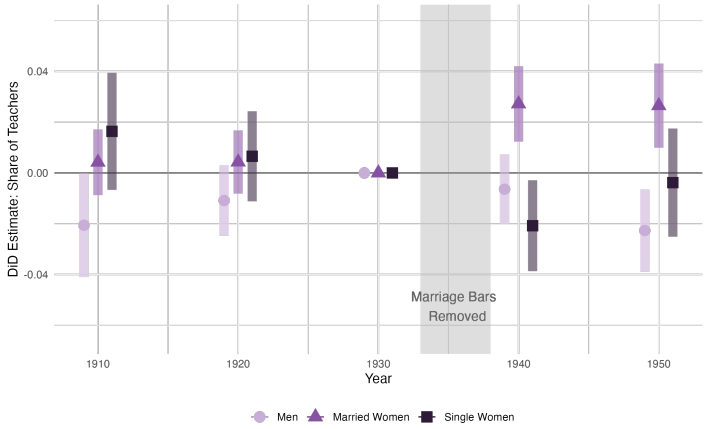
However, unlike the results from our preferred specification (Figure 2), the gap in married women's share of teachers persists to 1950. A potential reason for this continued divergence is the effect of World War II – while it is a reasonable assumption that nearby states had similar exposure to World 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 in 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.[34] The results are shown in Figure A7, 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.

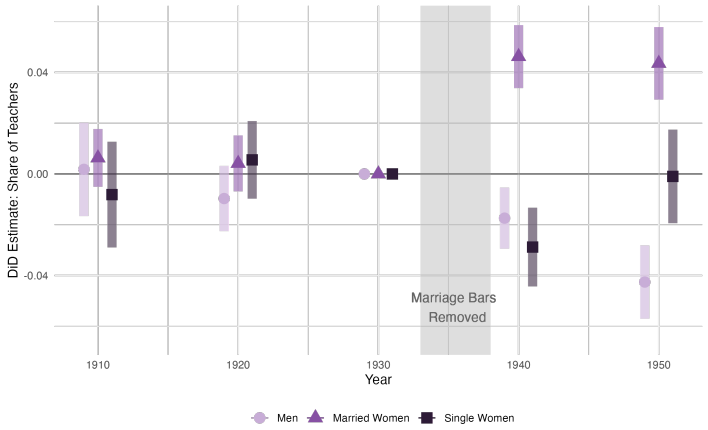
[34]For brevity we do not include the corresponding figure for women who were married and not teaching prior to the employments. The results are qualitatively similar and available upon request.



(a) Match 1: Nearest Neighbor

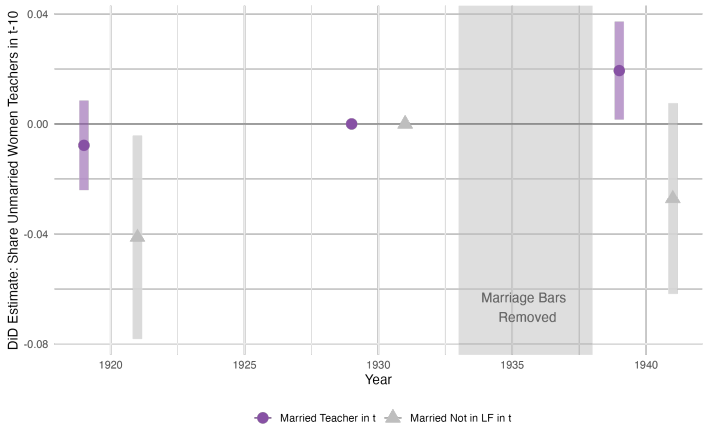


(b) Match 2: Genetic

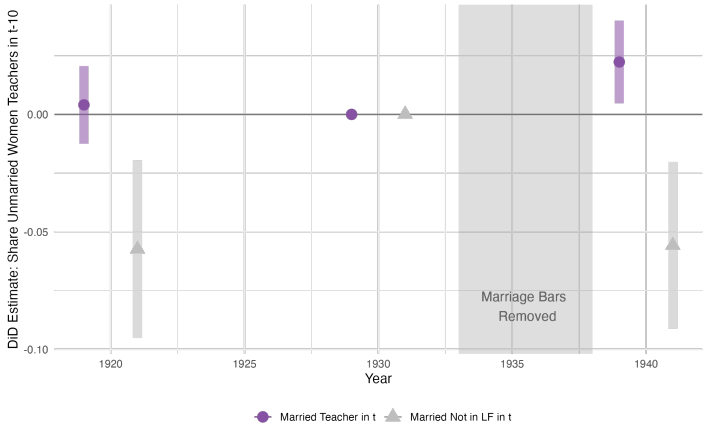


(c) Match 3: Full Optimal

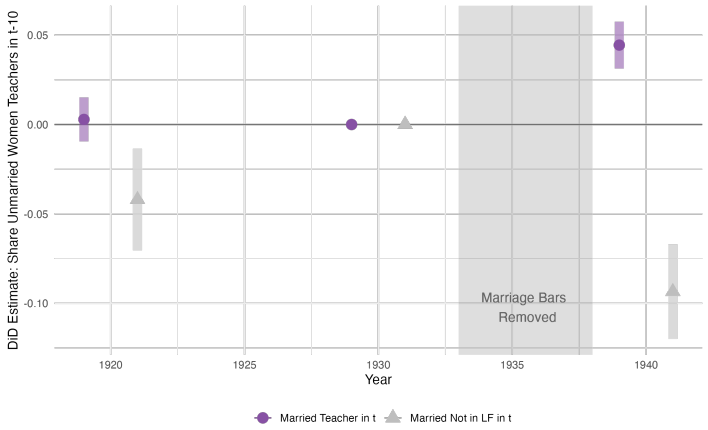
Figure A6: Estimated effects of the introduction of employer protections for married women in teaching on the gender composition of teachers, at the county level. Estimates are from a difference-in-differences specification where the dependent variable is the share of teachers in a county that are married 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 A7: 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.