

The Effects of Prohibiting Marriage Bars: The Case of U.S. Teachers^{*}

Amy Kim and Carolyn Tsao[†]

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Abstract

Married women in the early 20th century U.S. faced “marriage bars,” a form of employer discrimination that barred them from paid employment. However, because the end of marriage bar use coincided with shifting social norms and labor market conditions, it is unclear how the end of marriage bars affected women’s employment. We study the effects of the legislative prohibition of marriage bars in teaching during the 1930s. A difference-in-differences design shows that the prohibitions increased the share of married women teachers, partly by pushing unmarried women out of the labor force, and modestly increased women’s labor force participation.

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†Department of Economics, Princeton University, kimamy@princeton.edu and ctsaو@princeton.edu

1 Introduction

One of the most notable labor market shifts of the 20th century was the rise of married women working outside the home, with the percentage of married women in the U.S. labor force growing from 6% in 1900 to 61% in 2010 [Ruggles et al., 2024]. Prior work documents how various factors contributed to the growth in married women’s labor supply from the 1960s onwards, including the introduction of new technologies, such as contraceptives and household appliances, and the shifting of social norms (e.g., Greenwood et al. [2005], Bailey [2006], Fernández and Wong [2014], Bertrand et al. [2015])). However, less is known about the factors that contributed to the initial rise in women’s labor force participation, which began in the first half of the century.

This paper studies how married women’s labor supply in the early 1900s was affected by an important policy change: the prohibition of marriage bars in the teaching profession in two U.S. states in the 1930s. The early 20th century U.S. was 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 firms’ widespread use of marriage bars—a form of employer discrimination based on a woman’s marital status [Goldin, 1988].¹ 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, only two states—Kentucky and North Carolina—ultimately passed laws *prohibiting marriage bars in teaching*. By comparing Kentucky and North Carolina to unaffected neighboring states in the Southern U.S., we evaluate how prohibiting the use of marriage bars in teaching affected the employment of married women and by extension the overall labor force.

It is unclear *ex-ante* how prohibiting marriage bars would have affected the employment of married women, especially given the backdrop of gendered social attitudes at the time.

¹Throughout 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.

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]. Had married women (or their households) also held these beliefs, then married women would have chosen not to work even if discriminatory employment practices were made illegal. Our setting thus provides a unique opportunity to assess whether the removal of barriers to employment for married women increased married women’s LFP in the 1900s, independent of changing norms around married women’s employment. Furthermore, if married women did in fact enter the labor force as a result of the prohibition of marriage bars in teaching, it is not obvious how the labor market would have been affected. For instance, if married women entered teaching but the overall teacher workforce did not expand, married women’s entry must have come at the cost of other workers’ exits. Who were these affected workers and where did they go?

The marriage bar prohibitions we study, which 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, were passed in only two states. We leverage this variation in a *difference-in-differences* design, which we use to compare outcomes in states that prohibited marriage bars to outcomes in neighboring Southern states that did not.² Using a combination of IPUMS full-count decennial U.S. census data spanning 1910-1950 Ruggles et al. [2024] 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.³

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)

²Southern 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.

³It should be noted that our estimation strategy captures the net effect of the marriage bar prohibitions 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 these specific laws prohibiting marriage bars in teaching 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.

endogeneity in firms' choices of hiring policies. Although firms used marriage bars to bar 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 used marriage bars 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 prohibitions of marriage bar use 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 marriage bars had not been prohibited. We provide three pieces of suggestive evidence that the 'parallel trends' assumption holds. First, we show evidence of parallel pre-trends in our outcomes of interest prior to 1930. 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 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 marriage bar prohibitions were passed in some states but not others for seemingly idiosyncratic reasons, like the priorities of a particular state legislator. We perform several robustness checks to validate our results.

Our main finding is that prohibiting marriage bars 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 policy changes aimed at prohibiting employer discrimination against married women were ineffective in increasing married women's low LFP. In fact, a back-of-the-envelope calculation using our estimates suggests that the gradual removal of marriage

bars across all occupations and all states throughout 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]).

Furthermore, we find that the increase was driven by an extensive margin labor supply response among married women. Using our linked census sample⁴ in which we can observe 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 remain out of the labor force⁵, and unmarried women teachers becoming more likely to get married *and remain in teaching* rather than get married and exit the workforce. The prohibition of marriage bars therefore appeared to have 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 before and after 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.⁶ Indeed, consistent with

⁴Although 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.

⁵In 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.

⁶Gender norms at the time dictated that men were responsible for providing for their families [Goldin,

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 investigate 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. Instead, we 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 prohibiting marriage bars on women's LFP? Although the laws pulled women into the labor force as teachers, given that the laws also pushed incumbent women teachers out of the labor force, it is possible that the overall effect of the laws on women's LFP was zero or even negative. To assess these possibilities, we evaluate how the laws affected the LFP of incumbent women teachers. We find that the laws had no effect on the LFP or the average occupational score (as in e.g. Abramitzky et al. [2012]) of incumbent women teachers. We therefore conclude that the policy change had a net positive impact on women's LFP, pulling women into the labor force without pushing incumbent women teachers out of the labor force.

Overall, our study shows that removing institutional barriers to employment specific to married women modestly increased their LFP between 1930 and 1950. Our findings also serve 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. However, in our context, the policy still had a positive overall effect on women's LFP and earnings.

This paper 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 improved [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.

household technologies [Greenwood et al., 2005]—during which married women faced less discrimination and/or were protected by law and allowed to work in most occupations. However, women’s LFP began rising prior to 1940, and few papers study the factors that contributed to the initial rise during which married women faced active, legal employer discrimination. 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”⁷—and explores the economic justifications firms made for not employing married women. Motivated by Goldin [1988], we study how the prohibition of marriage bars impacted married women’s LFP in the first half of the century. Our findings suggest that the prohibition of marriage bars had sizeable effects on married women’s LFP in the short run, falling within the range of effect sizes of changes in the second half of the century like early access to the pill (which accounted for 8% of the rise in women’s LFP between the ages of 26-30, according to Bailey [2006]) and improved household technologies (which accounted for over half of the later rise in women’s LFP, according to Greenwood et al. [2005]).

This paper also contributes to the study of the intended and unintended consequences of anti-discrimination policies. While some policies have been found to affect the targeted population as intended—for example, the Equal Pay Act of 1963 and Title VII of the Civil Rights Act of 1964 resulted in an increase in women’s pay (see e.g., Carrington et al. [2000], Neumark and Stock [2001], Bailey et al. [2024])—others policies have been found to do the opposite. For instance, the American with Disabilities Act (ADA) of 1990 decreased the employment of people with disabilities (e.g. DeLeire [1997], Acemoglu and Angrist [2001], Beegle and Stock [2003]), and policies targeting age discrimination such as the Age Discrimination in Employment Act (ADEA) in 1968 decreased the employment of older workers (e.g., Neumark and Song [2013], Lahey [2008]). The latter group of papers illustrate that when an anti-discrimination policy imposes new costs on firms, firms might take actions to try and minimize said costs and inadvertently negatively impact the workers being discriminated against.⁸ Our paper provides new evidence of another example of an anti-discrimination

⁷A 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.

⁸In the case of the ADA, which imposed that firms must provide accommodations for workers with dis-

policy with unintended consequences: while prohibiting marriage bars led married women's employment to rise in teaching, schools also chose to maintain the gender ratio of teaching staff, leading single women to be pushed out of the profession with no anti-discrimination policy in place to protect their employment.

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 their prohibition 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 marriage bar prohibitions 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 U.S.

This section provides historical background on the evolution of the institutional barriers to employment that married women faced in the early 20th century U.S., both nationwide and specifically in the teaching profession.

2.1 The Evolution of Marriage Bar Use: 1900-1960

Marriage bars, the class of discriminatory employment practices that excluded married women from the workplace, 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 (e.g. school districts).

In practice, marriage bars were implemented in two ways: married women were either

abilities, [Acemoglu and Angrist \[2001\]](#) rationalize their findings by arguing that firms that found introducing such accommodations too costly simply chose not to employ as many workers with disabilities. Similarly, in the case of the ADEA, which lowered the cost of filing age discrimination claims in some states, [Lahey \[2008\]](#) finds that firms avoided the potential increase in litigation costs by employing fewer older workers in the first place.

not hired due to their marital status (“hire bars”), or single working women who got married were fired or expected to quit upon marriage (“retain bars”). Firms practiced one or both forms of discrimination, either 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, 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.

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]. Incidentally, 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.⁹

⁹In 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 surveys show that marriage bar use in schools increased over the course of the Great Depression, a trend that has been attributed to rising unemployment and scarcity of jobs for men.¹⁰ “Hire bars” in school districts, for instance, affected 60% of the urban population in 1928 prior to the Great Depression, 73% in 1930-31, and nearly 80% in 1942.¹¹ 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.¹² 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 made for using marriage bars.

2.2 The Prohibition of Marriage Bars in Teaching

School districts were the most prominent employers that used marriage bars throughout the early 20th century. Their use 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.¹³

¹⁰There 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].

¹¹Similarly, “retain bars” in school districts affected 50% to 60% of the urban population over the same time period.

¹²Note that for some occupations, such as airline stewardess, marriage bars persisted until decades later [Associated Press, 1986].

¹³The 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.

School districts used the same justifications as other firms to rationalize the use of marriage bars. However, unlike in other occupations, discriminatory hiring policies in teaching were contested nationwide in debates over tenure protection for teachers 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. Some of the women who ended up being dismissed on the basis of marriage took the offending school boards to court, but 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, which suggests that the laws may have been more salient to school districts that were wary of being sued than to the general public.¹⁴

We conclude with descriptive evidence that suggests that the state-wide prohibitions 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 elsewhere. 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.¹⁵ Our empirical design described in Section 4 leverages this variation to formally evaluate the effects of the laws in KY and NC on women's employment in teaching.

3 Data

3.1 Cross-Sectional Sample

For the first part of our analysis, we use data on teachers in the repeated cross-sectional full-count U.S. Decennial Censuses from 1910 to 1950, which cover all individuals in the U.S. [Ruggles et al., 2024]. We define teachers as adults between the ages of 18 and 64 who report teaching as their occupation and who are not self-employed. The data do not separately identify public and private school teachers, but because private schools accounted for a small share of schools in the early 1900s, public school teachers likely comprise the bulk

¹⁴We found no newspaper articles referencing the legislation in North Carolina and only one article mentioning the legislation passed in Kentucky [The Courier Journal, 1938].

¹⁵We found no evidence of similar state-wide prohibitions of marriage bars in any other state in later years, suggesting that the increase of married women teachers in other states during the 1940s was due to socioeconomic factors rather than legislation.

of the teachers we identify.¹⁶

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, Black married women were more likely to work outside the home than white married women either out of necessity (to support their families) or out of social expectation. In light of these differences and the relatively small sample of Black teachers at the time, we focus our analysis on white teachers, but also conduct our main analysis for teachers of all races (in Supplemental Figure A5) which does not meaningfully change our results.

3.2 Linked Samples

For the second part of our analysis, we use panel data on the women who can be linked between consecutive years of U.S. Censuses from 1910 to 1940.¹⁷ 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.

Linkage Rates. Supplemental Table B1 shows linkage rates for various populations across censuses. While linkage rates are largely consistent over time, and relatively similar between treated and all states, it is important to note the differences in linkage rates between unmarried and married women. As may be expected, because Census Tree links are based on

¹⁶Enrollment 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].

¹⁷At the time of writing, linkages to 1950 are not yet available.

a genealogical website, linkage rates are substantially higher for married women than for unmarried women. While this is not in and of itself a threat to our identification strategy, the different *trends* in linkage rates between unmarried women teachers and married women teachers is of potential concern. The linkage rates for married women increase substantially over subsequent censuses, while the linkage rates for unmarried women slightly decreases. As a result, our linked analyses, particularly those with small sample sizes, should be interpreted with some degree of caution.

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.¹⁸

Column (3) shows 1930 summary statistics for counties in KY and NC, the states in which marriage bars in teaching were prohibited 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 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,

¹⁸The last fact is consistent with anecdotal evidence of teacher shortages in the South [Goldin, 2021].

they comprise our preferred comparison group in the analysis that follows.¹⁹ Our final balanced sample thus consists of 217 treated counties and 310 neighboring Southern control counties.²⁰

4 Empirical Strategy

4.1 Main specification

It is unclear *a priori* how the prohibition of marriage bars in teaching would have affected women's labor force participation in the 1930s. If marriage bars were the main factor preventing married women from working as teachers, then prohibiting marriage bars would have a marked effect on the share of married women in teaching. On the other hand, if marriage bars played a negligible role relative to e.g. social norms, then married women would continue to self-select out of teaching under the prohibitions, resulting in no discernible effect on the gender composition of teachers.

We evaluate the impact of the prohibitions 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. We use a *difference-in-differences* design to evaluate the effects of the state-wide policy changes on the composition of the teacher workforce and on men's and women's employment.²¹ Our preferred specification is

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

¹⁹See Supplemental Appendix D for robustness exercises using a border county design and a matched county design.

²⁰We 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 Supplemental Appendix Figure D1 for a map of our sample of treated and control counties.

²¹Although treatment timing is staggered in our setting (1933 for NC and 1938 for KY), implementing the estimators recommended in the recent econometrics literature on difference-in-differences for such environments (e.g. Sun and Abraham [2021], Roth et al. [2023]) requires more frequent observations (e.g. yearly) before and after treatment than our data contain. In our state-level analysis in Supplemental Appendix E, we also show results for North Carolina and Kentucky separately. We show that our findings are not being disproportionately driven by one state and that in fact point estimates are remarkably similar for the two states.

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 .

Standard errors are clustered at the county level in the main analysis and at the state level in the supplemental materials. Our preferred specification uses county level clustering, despite the fact that the treatment variation is at the state level, for two reasons. First, we observe significant heterogeneity across counties within state in terms of their baseline level of employment of married women prior to the prohibitions. As such, although the prohibitions were technically applied to all counties within a state at once, there is reason to believe that within-county correlations over time may be more relevant for our outcomes of interest than across-county, within-state correlations over time. Second, there is an insufficiently small number of states in our setting (two treated states and four control states) for standard inference using clustering at the state level to be valid, and solutions to few clusters typically require strong homogeneity assumptions [Canay et al., 2021, Roth et al., 2023]. Regardless, in Supplemental Appendix A (Figure A9), we show standard errors clustered at the state level using the cluster wild bootstrap. We further supplement our county-level analysis with a state-level synthetic difference-in-differences empirical strategy in Supplemental Appendix E. Our main results are robust to both approaches.

4.2 Outcome variables

Two main sets of outcomes are examined. The first set, constructed using the cross-sectional decennial censuses, is used to study the direct effects of the prohibitions on the composition of the teaching workforce, and specifically marital status and gender. The outcomes include: the share of white teachers in county c in year t who are married women, unmarried women, and men, and the number of white married women teachers per hundred white married women in county c in year t .

The second set of outcomes, constructed using the *linked* decennial censuses, is used to study the effects of the prohibitions on women's employment and marital choices. At a

high level, we construct these outcomes by grouping women based on their demographics at “baseline,” i.e. in year $t - 10 \in \{1910, 1920, 1930\}$,²² and defining each group’s outcome in year t as the share of the group that is employed or married in year t . As such, we leverage the linked structure of the data to examine how outcomes change over time for the same group of women. More specifically, we classify women under the age of 40 in each “baseline” year of interest, $t - 10$, into one of six cells based on the interaction between their marital status in $t - 10$ (unmarried or married) and their employment status in $t - 10$ (employed as a teacher, employed but not as a teacher, and not in the labor force).²³ We aggregate the data to the county level by counting the total number of women in each cell in each county. We then construct the following four outcomes for each county-cell combination: the share of women in the $t - 10$ county-cell who are (a) married in year t , (b) married and teaching in year t , (c) not teaching but are in the labor force in year t , or (d) married but not in the labor force in year t . To specifically examine the future outcomes of unmarried women who might choose to stay unmarried, we also construct the following outcomes for each county-cell of unmarried women teachers between the ages of 18 and 40 in $t - 10$: the share who are still unmarried in year t , the share who are still unmarried and teaching in year t , the share who are still unmarried but employed elsewhere in year t , and the share who are still unmarried and are no longer in the labor force in year t .

We define several tertiary outcomes that allow us to explore the effect of the prohibitions on factors related to employment and marriage, namely income and fertility.²⁴ The results corresponding to these outcomes are mentioned throughout the main results section and are shown in Supplemental Appendices A and B, as each outcome has its limitations. To proxy for income, which was not collected in the Census prior to 1940, we follow the economic history literature (see e.g. [Abramitzky et al. \[2012\]](#)) in using occupational scores,

²²Note that 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.

²³We define our outcomes for unmarried women teachers under 40 for whom the decision to marry is more likely to be relevant. We also drop counties with fewer than five unmarried women teachers under 40 linked from 1920 to 1930 or from 1930 to 1940, or counties missing observations in any year.

²⁴Another natural outcome of interest in this setting is the effect of the prohibitions on women’s age at marriage. However, data on age at marriage were only collected for the 1% sample of the census, resulting in too few observations of teachers with age at marriage to be able to construct a meaningful outcome variable for our purposes.

i.e. numerical ratings of occupations ranging from 0 to 80 based on the average income associated with the occupation in 1950. We construct two fertility-related outcomes: the share of teachers in county c in year t who report having any children in the household, and the share of unmarried women teachers in each $t - 10$ county-cell who have children in year t . In theory, these measures allow us to investigate whether the prohibitions affected the timing of childbirth; however, if the prohibitions delayed childbirth by one or two years, our measures, constructed using decennial data, would not capture such variation.

4.3 Identifying assumption

Causal inference relies on the “parallel trends” assumption, i.e. that in the absence of the marriage bar prohibitions being introduced in teaching between 1930 and 1940, the outcomes of interest would have evolved similarly in treated and control counties. The main threat to identification is that the prohibitions 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 in their neighboring states, then such views may have driven both the passing of the laws and an increase in married women teachers in KY and NC relative to their neighbors.

We argue that the parallel trends assumption reasonably holds in our setting for three reasons. First, we find that there are no differential pre-trends in our outcomes of interest between the treated and control counties until 1930 (see 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 on similar trajectories as their neighboring states prior to 1930.

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. In a 1938 Gallup poll [Gallup Organization, 1938], respondents were asked the following question: “Schools in some states only hire unmarried teachers and discharge them if they get married. Do you approve of this rule?” We compare the responses to this question between KY and neighboring control states.²⁵ We find that

²⁵Since this survey took place several years after marriage bars in teaching were prohibited in NC, we do

respondents in KY were weakly less likely to approve of the rule (22.0%, s.e. 4.9%) compared to respondents in TN and WV (27.9%, s.e. 5.9%), but that the gap is statistically indistinguishable from 0 at the 90% confidence level (t-statistic: 0.77). We conclude that there were no meaningful differences in norms regarding the employment of married women in teaching between the treated and control states.

Third, we find suggestive qualitative evidence that the prohibitions were not driven by state-specific trends in sentiments towards married women teachers, but rather were passed due to idiosyncrasies in the priorities and actions of the legislators involved. Historical policy reports and newspaper archives show that tenure protections for teachers were being debated across the country throughout the 1930s, not only in KY and NC. Newspapers describe 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.

5 The Effects of Prohibiting Marriage Bars in Teaching

5.1 Effects on Married Women

We start by examining how the prohibitions of marriage bars in teaching affected the employment of married women as teachers.

not compare the responses from NC. Note also that 1938 was the same year that marriage bars in teaching were prohibited in KY, 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.

Direct Effects. Column (1) of Table 2 (and the triangles in Figure 2) show the estimated effects of the prohibitions on the share of white teachers in a county who were married women, while Column (2) shows the estimated effects on the number of white married women teachers per hundred white married women.²⁶

Our main finding is that the prohibitions increased women's involvement in their local teaching workforce. Column (1) shows that schools became more likely to employ married women among their teaching staff: 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. Column (2) shows that the marriage bar prohibitions 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 prohibitions resulted in a 17% increase in white married women's participation in teaching.²⁷

In Supplemental Appendix C we extrapolate our estimates to the larger labor force by using a back-of-the-envelope calculation 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 1950.²⁸ 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 1950.²⁹ We perform a similar exercise

²⁶Regressions are weighted by the total number of white married women in the county and year. Supplemental Figures A4 and A5 show the corresponding estimates for Black teachers and all teachers respectively—we find no effect of the prohibitions on the county shares of Black teachers who are married women, single women, or men and including non-white teachers does not change the main results. We interpret these findings as being consistent with the fact that Black women were more likely to work during this time period and thus *a priori* would not be expected to be affected by the prohibitions.

²⁷One might be concerned that schools responded to the prohibitions by changing the margin on which they discriminated against married women: for example, by letting go of women if and when they had children instead of if and when they got married. We investigate this possibility in Supplemental Figure A6 by estimating the effect of the prohibitions on the county share of women teachers with children. We find that the share of women teachers with children increased following the prohibitions being passed, suggesting that schools did not simply substitute towards discriminating against women with children.

²⁸We 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].

²⁹We define white-collar occupations as all professional/technical, managerial, clerical, and sales occupa-

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.

Notably, the effect of the prohibition of marriage bars on married women's labor force participation in treated states was relatively short-lived. By 1950, the gap between treated and control counties in the share of teachers who were married women shrank to be indistinguishable from zero. These results indicate that as employment discrimination against married women faded nationwide in the 1940s, control counties effectively 'caught up' to treated counties in employing more married women in teaching.

Mechanisms. What are the mechanisms driving our main effect? Did the prohibitions pull married women into teaching who otherwise would not have worked (an 'extensive margin' effect), or did the prohibitions 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 data, leveraging its panel structure to trace out the individual marriage and employment outcomes over time for three groups of women: (1) unmarried women teachers, (2) married women not in the labor force, and (3) unmarried women not in the labor force.

We start by looking at unmarried women teachers in 1930 who, after the prohibitions, could get married and continue teaching. We look at the outcomes of these teachers in the 1940 census and compare them to the outcomes of unmarried women teachers from 1920 in the 1930 census and 1910 teachers in the 1920 census in treated relative to control states.³⁰ Results are shown in Panel 1 of Table 3. Column (2) shows that the prohibitions

tions. These occupations combined employed roughly 30% of the total labor force in 1940.

³⁰We construct our sample by identifying unmarried women teachers under 40 (for whom the decision to marry is more likely to be relevant) in 1930 (and 1920 and 1910) and computing the county-level share of these women who were still teaching and/or married in the linked 1940 (and 1930 and 1920) census. We then estimate Equation (1) where t indexes the year the outcome is measured. 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, or counties missing observations in any year.

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 was economically large given that only 5% of unmarried women teachers in 1920 were both married and teaching in 1930. Strikingly, we also find that the increase was entirely driven by changes in women's decision to work rather than their decision to marry. Column (1) shows that the prohibitions did not increase the rate at which unmarried women teachers got married. Instead, unmarried women teachers responded to the prohibitions 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)). The prohibitions therefore had an *extensive margin* effect on unmarried women teachers, keeping them in the labor force after marriage without changing their propensity to get married.³¹

Next we examine the responses of married women who were not in the labor force prior to the prohibitions, who would have been able to respond to the prohibitions by entering the labor force as teachers and remaining married.³² 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 prohibitions became 0.1 p.p. more likely to become a teacher after the prohibitions were passed. The effect is somewhat remarkable given that so few white married women worked during this time period: only 0.2% of married women outside the labor force in 1920 were married teachers in 1930, implying that the prohibitions led to a 50% increase in the propensity for married women to enter teaching

³¹One potential consequence of the increase in women getting married while still working might have been a decline in childbirth rates, or a delay in childbirth events. Although our data on whether women have children is only available once every ten years, and thus cannot be used to discern delays in childbirth of less than ten years, we can still investigate the effects of the prohibitions on the likelihood that unmarried women teachers in $t - 10$ had children by t . Supplemental Figure A7 shows that using these data, we find no effect of the prohibitions affecting the fertility outcomes of unmarried women teachers.

³²We construct our analysis sample of married women outside the labor force following a similar procedure to our first sample: identifying married women under the age of 50 (for whom the decision to retire was less relevant) who were not in the labor force in 1930 (and 1910 and 1920), computing their outcomes ten years later, and estimating Equation (1).

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 0.5 p.p. in the likelihood of staying married and out of the labor force shown 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.

Finally, we examine how the prohibitions affected unmarried women who were not in the labor force prior to the prohibitions being passed. These women could have contributed to the overall effect through either of the channels discussed above: by first becoming teachers then being induced by the policy change to remain teachers even after marriage, or by first becoming married, then being induced by the policy change to enter teaching. While our data prevent us from disentangling the two separate channels for this particular group, we are able to estimate the overall effects of the prohibitions for these women.

Results for this sample are shown in Panel 3 of Table 3. We find that the prohibitions induced unmarried women outside the labor force to get married and enter teaching at a 0.1 p.p. higher rate. This effect amounts to a 26% increase relative to the low baseline rate of entry into teaching conditional on marriage. Column (4) also suggests that unmarried women outside the labor force became 1 p.p. less likely to get married and exit the labor force, although the effect size is small relative to the baseline mean.

In Supplemental Table B2, we also examine whether other working women (married or unmarried) might have switched into teaching in response to the prohibitions being passed. We find no effect of the prohibitions on the likelihood of getting married, teaching, and/or exiting the labor force for these other working women. We therefore conclude that the increase in married women in teaching was driven by an extensive margin response (women becoming more likely to work) rather than an intensive margin one (other working women switching to teaching).³³

³³As an additional step, we estimate that the overall increase was driven predominantly by women entering teaching from *outside* the labor force (Samples 2 and 3) rather than incumbent teachers (Sample 1). We perform this calculation by scaling the effect for each sample by the total number of women in each group in 1930. While the effect (2 p.p.) was largest for unmarried women teachers (Sample 1), their total population was dwarfed by the population of all married (Sample 2) or unmarried (Sample 3) women outside of the labor force. Thus the overall effect was primarily driven by women who had not been teachers prior to the prohibitions.

5.2 Effects on Men and Unmarried Women

What were the consequences of the prohibitions on the employment of men and unmarried women, who were not directly targeted by the marriage bar prohibitions? We first look at effects within teaching. One possibility is that the influx of married women 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. We rule out this possibility, however, in that we find that the prohibitions had no effect on the total number of teachers per county (see Column (4) of Table 4 and Supplemental Figure A1).

Because the total number of teachers did not change in response to the prohibitions, 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 outcomes the share of teachers that were men and unmarried women.

Results for men and unmarried women are shown in Figure 2 and Table 4. Column (3) shows that the increase in married women teachers was entirely offset by a 4.2 p.p. (7%) decrease in the share of teachers who were unmarried women, while Column (2) shows there was no effect on the share of teachers who were men. We interpret the maintained ratio of men-to-women teachers after the prohibitions 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.

What happened to the unmarried women who were pushed out of teaching following the prohibitions? Were they pushed out of teaching into other occupations or out of the labor force entirely? We explore this question using our linked sample of unmarried women teachers. Because we found in the previous section that the prohibitions had no effect on

the incidence of marriage for women in this sample, we can restrict our analysis to the sample of women who remained unmarried and examine their employment outcomes after the prohibitions were passed.

Table 5 shows the estimated effects of the prohibitions on the share of linked unmarried women teachers in $t - 10$ who remained unmarried (column 1) and conditional on remaining unmarried, the share that stayed teachers (2), stayed in the labor force but changed occupations (3), and left the labor force (4). In line with Table 4, we find a noisy decrease in the share of unmarried women teachers who remained unmarried women teachers. However, we also find a weakly significant increase of 0.017 p.p. (or a 20% increase) in the share of unmarried women teachers who left the labor force and remain unmarried. These estimates suggest that the prohibitions pushed some unmarried women out of the labor force entirely. We also find suggestive evidence that some unmarried women teachers left teaching to other occupations, although the corresponding estimates are not significant.

Finally, we consider the possibility that the prohibitions reduced the number of unmarried teachers by affecting the number of young women who *became* teachers, but find no evidence that this was the case. Supplemental Table B3 shows that the prohibitions had no effect on the likelihood of unmarried women entering teaching conditional on remaining unmarried.

5.3 Evaluating Overall Effects on Women's LFP

Our results thus far show that the prohibitions had a positive effect on the LFP of women who were not in the labor force prior. However, to be able to assess the effect of the prohibitions on women's overall LFP, we also need to establish how the prohibitions affected the LFP of women who *were* in the labor force prior. If, for example, the prohibitions decreased the overall LFP of incumbent teachers (which is plausible, given that the prohibitions kept some incumbent teachers in the labor force yet pushed other incumbent teachers out), then the overall effect of the policy on women's LFP could be small or even negative.

We therefore estimate the effect of the prohibitions on the county-level LFP of women teachers who were unmarried prior to the prohibitions. Supplemental Figure A8 shows that the prohibitions had a noisily positive, but statistically insignificant, effect on the overall la-

bor force participation of incumbent unmarried teachers, suggesting that the increase among unmarried women teachers who stayed in teaching was more or less offset by the decrease among unmarried women teachers who exited the labor force as a result of the prohibitions.

We illustrate this result another way in Supplemental Figure A2) by estimating the effect of the prohibitions on teachers' average *occupational scores*, which function as a proxy for income given income was not collected in the Census prior to 1940. Using occupational scores as the outcome of interest produces similar results: namely, we find no effect of the prohibitions on the average occupational score among incumbent unmarried teachers. These results provide corroborating evidence that incumbent unmarried women's employment was unaffected on net by the prohibitions.³⁴

Taken together, our results suggest that the prohibitions increased women's LFP overall, driven entirely by an extensive margin increase on women's labor supply to teaching. The prohibitions had no effect on the overall LFP of women who were already teachers prior to the laws.

5.4 Robustness Checks

Though the absence of pre-trends and the historical context described in Section 4.3 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 prohibition of marriage bars in teaching. We test the robustness of our results in three ways: implementing a placebo test studying secretaries instead of teachers, varying the control group by using both a matched counties design and a border county design, and conducting a state-level analysis using synthetic difference-in-differences.

Secretary placebo. First, to test whether the bans may have coincided with differential trends in attitudes towards employing married women or economic conditions in treated and

³⁴The occupational scores analysis also supports our finding that few workers switched from teaching into other occupations in response to the prohibitions. If unmarried teachers predominantly entered other similar occupations (e.g. secretarial work) after being pushed out of teaching, we might expect the effect on occupational scores for unmarried women to be relatively small. The fact that we do not see such an effect is further evidence that few unmarried women left teaching to enter occupations as a result of this prohibitions.

control states, we conduct a placebo test by examining whether the prohibitions 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 prohibitions in teaching were induced by differential trends in attitudes or employment of married women in the treated and control states, one might expect to see an increase in the share of secretaries who were married women in treated counties as well. To test this, we estimate Equation (1) using outcomes related to the employment of secretaries rather than our teaching-related outcomes of interest. The results are shown in Supplemental Figure A3, which is analogous to Figure 2 but for secretaries, showing the effects of the prohibitions in teaching on the share of secretaries who were married women (triangles), men (circles), and unmarried women (squares). We find no significant effects of the prohibitions in teaching on the composition of the secretarial workforce, particularly between 1930 and 1940.

Alternate Control Group. Second, we examine whether our main results are driven by our choice of control group. We use neighboring Southern states as our preferred control group because of their geographical proximity and cultural similarity to the treated states, as discussed in Section 4. Regardless, one might be concerned that the neighboring Southern states do not offer as close a comparison as possible to the treated states. We address this concern in Supplemental Appendix D by evaluating whether our results are sensitive to using alternate control groups. In our first approach, we restrict our county sample to only include ‘border’ counties, which are plausibly even more similar to each other than neighboring states are over time.³⁵ In our second approach, we keep all treated counties in our analysis but choose or weight control counties using various matching techniques. Our main results remain similar under both approaches.

³⁵Border counties are those in treated states which border a non-treated state, and those in control states which border a treated state. See Supplemental Figure D1 for a visualization of the border counties on a map.

State-Level Analysis. Finally, we examine whether our main results are driven by our chosen unit of analysis. Our preferred specification uses county-level outcomes with standard errors clustered at the county level for reasons discussed in Section 4. Here, we investigate whether our results are sensitive to using state-level outcomes or state-level clustering of standard errors. To implement a state-level analysis with only two treated states, we use a synthetic difference-in-differences approach, outlined in Supplemental Appendix E. The magnitudes and significance levels of our estimates remain similar in the state-level analysis. We also find that our main results are robust to clustering at the state level (using the wild cluster bootstrap as in Cameron et al. [2008] and Canay et al. [2021]), as shown in Supplemental Figure A9.

6 Conclusion

This paper provides new evidence on the effects of a historical policy 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. Employment 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 KY and NC successfully passed state legislation prohibiting the use of marriage bars in schools. 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 use a difference-in-differences design to estimate the effects of the prohibitions on married women’s employment in teaching.

We find that the protections led to an increase in the share of teachers who were married women, an effect largely driven by changes in women’s decision to work rather than by women’s decision to marry. However, we also find that the increase was offset by a decrease in the share of teachers who were unmarried women, with no effect on men nor on the total number of teachers. We find suggestive evidence that the decrease was driven by incumbent unmarried women teachers being pushed out of the labor force. Overall, our findings suggest that while the policy did displace some unmarried women, the net effect on women’s labor

force participation was positive, as the policy pulled married women into teaching who would have otherwise not been in the labor force. Our results are largely robust to various matching specifications.

Our study provides causal evidence that despite the strong social norm that married women stay out of the labor force in early 1900s U.S., there was demand among women to work while married. Making discriminatory hiring practices against married women illegal in one occupation, even as early as the 1930s, pulled more married women into the labor market in just a few years.

7 Figures

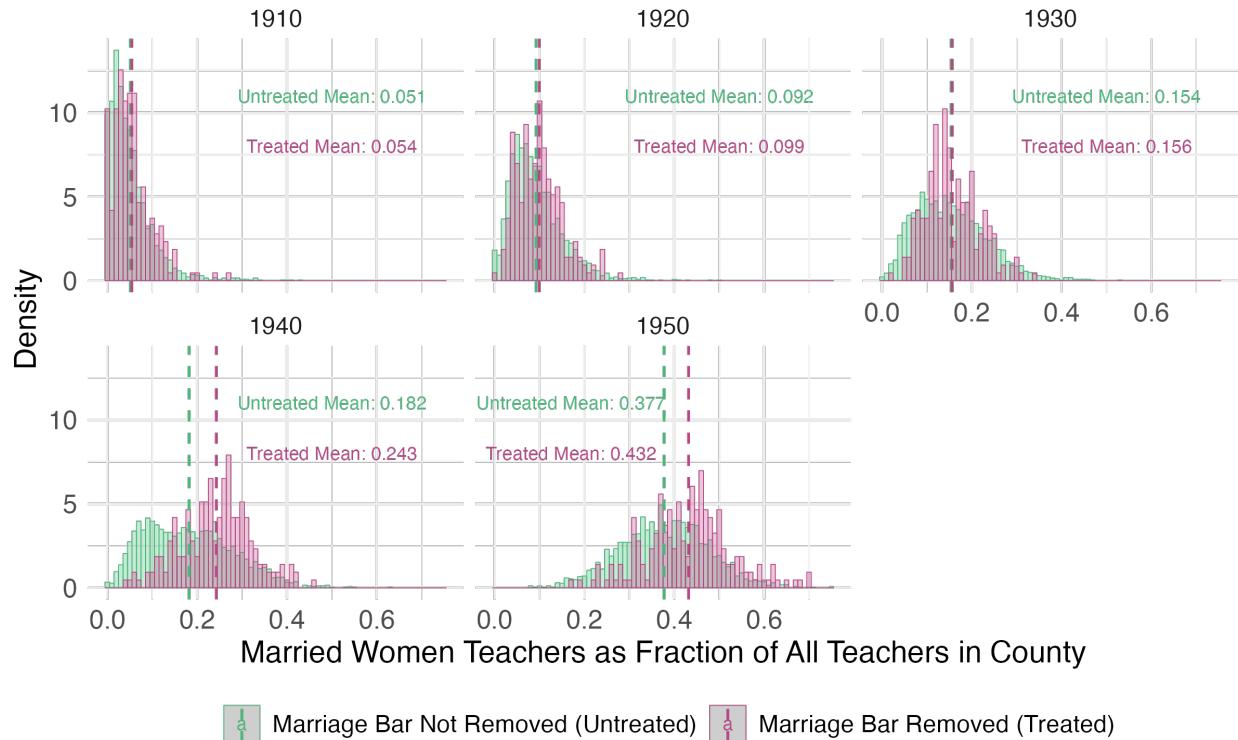


Figure 1: Density plots from 1910 to 1950 of the county-level fraction of white teachers who are married women. Separate distributions are shown for (1) counties in states where marriage bars were prohibited in teaching in the 1930s (KY, NC), and (2) all other counties in the country. Vertical dashed lines are group means.

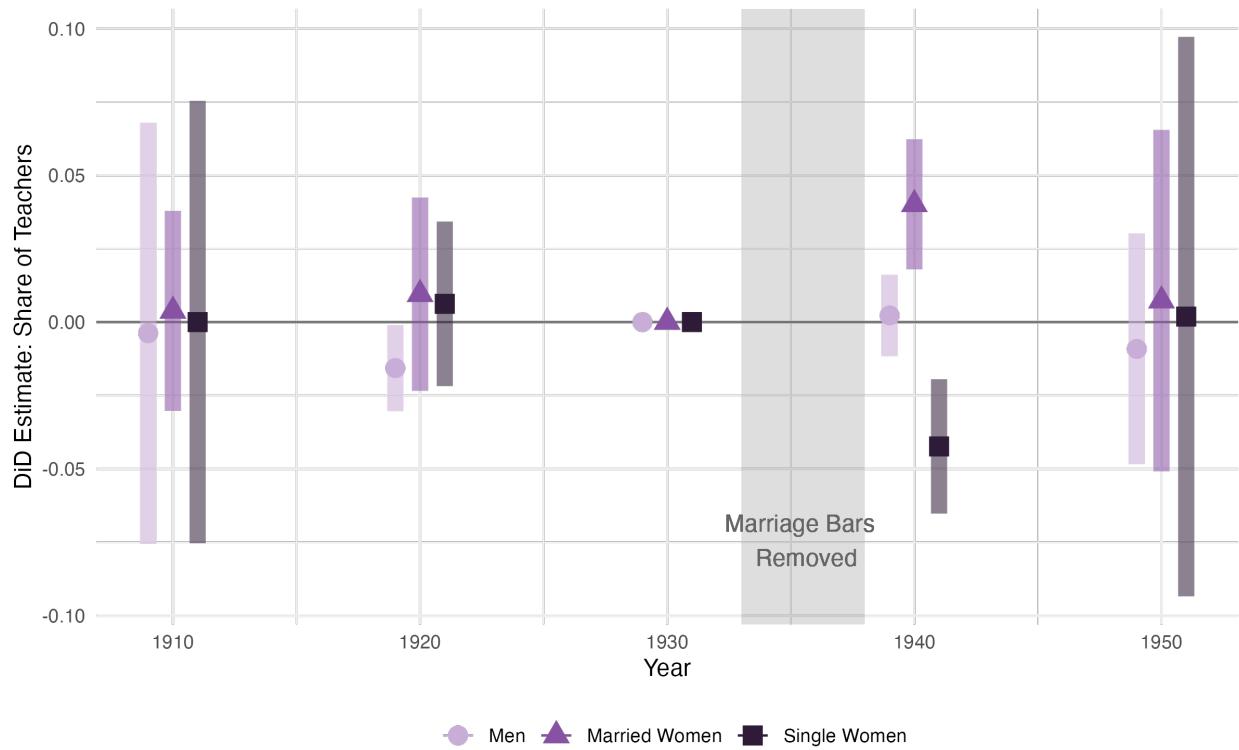


Figure 2: Estimated effects of the prohibition of marriage bars in teaching on the *gender composition* of teachers, at the county level. Sample includes KY, NC, and neighboring Southern states.

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)
<i>N</i> (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., 2024]. Panel A presents means and standard errors of county-level variables for the whole county population, including population in thousands, the percentage of the county population living in an urban area, the percentage of married women and white married women in the county between the ages of 18 and 64 who are in the labor force, and the average number of children for married women. Panel B presents means and standard errors of county-level variables related to teachers, including the white school-age population divided by the number of white teachers in a county, and the share of white teachers in a county that are men, unmarried women, and married women.

Table 2: Estimated effects of the prohibition of marriage bars on married women teachers

	Dependent Variable:	
	Share Teach Mar. Wom.	MW Teach/100 MW
	(1)	(2)
Treated \times 1940 (γ_{1940}^{DD})	0.040*** (0.007)	0.098*** (0.036)
Treated \times 1950 (γ_{1950}^{DD})	0.007 (0.008)	-0.014 (0.032)
Dep. Var. 1930 Treated Mean	0.156	0.572
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., 2024].

Table 3: Estimated effects of the prohibitions 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 \times 1940 (γ_{1940}^{DD})	-0.016 (0.014)	0.020*** (0.007)	0.001 (0.005)	-0.037*** (0.014)
Dep. Var. 1930 Mean	0.623	0.051	0.035	0.537
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 \times 1940 (γ_{1940}^{DD})	-0.001 (0.001)	0.001*** (0.0002)	0.004 (0.003)	-0.005* (0.003)
Dep. Var. 1930 Mean	0.935	0.002	0.050	0.884
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 \times 1940 (γ_{1940}^{DD})	-0.007** (0.003)	0.001*** (0.0003)	0.003 (0.002)	-0.011*** (0.003)
Dep. Var. 1930 Mean	0.528	0.005	0.039	0.485
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 1920, 1930, and 1940. Within these counties, we identify women whom we are able to link over consecutive Census years (i.e. between 1910 and 1920, 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 1910, 1920, and 1930; Sample 2, containing linked women who were aged 18-50, married, and not in the labor force in 1910, 1920, and 1930; and Sample 3, containing linked women who were aged 8-40, unmarried, and not in the labor force in 1910, 1920, and 1930. All regressions use the 1910-1920, 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 prohibitions on the gender composition of teachers

	Dependent Variable:			
	% Teach Married Women	% Teach Men	% Teach Unmar. Women	Number of Teachers
	(1)	(2)	(3)	(4)
Treated \times 1940 (γ_{1940}^{DD})	0.040*** (0.007)	0.002 (0.007)	-0.042*** (0.009)	-0.182 (7.410)
Treated \times 1950 (γ_{1950}^{DD})	0.007 (0.008)	-0.009 (0.008)	0.002 (0.011)	-3.063 (6.246)
Dep. Var. 1930 Treated Mean	0.156	0.217	0.627	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., 2024].

Table 5: Estimated effects of the prohibitions on unmarried women's teachers propensity to remain unmarried and conditional on remaining unmarried, 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)
Treated \times 1940 (γ_{1940}^{DD})	0.016 (0.014)	-0.014 (0.013)	0.014 (0.009)	0.017* (0.009)
Dep. Var. 1930 Mean	0.378	0.208	0.084	0.086
Observations	1,545	1,545	1,545	1,545
Adjusted R ²	0.471	0.527	0.072	0.085

Notes: Estimation follows equation 1. See table notes for Table 3 for details on sample construction for Sample 1. All regressions use the 1920-1930 and 1930-1940 linked full-count Census samples.

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Supplementary Materials For “The Effects of Prohibiting Marriage Bars: The Case of U.S. Teachers”

Amy Kim and Carolyn Tsao*

October 18, 2024

*Department of Economics, Princeton University, kimamy@princeton.edu and ctsaو@princeton.edu

A Additional Figures

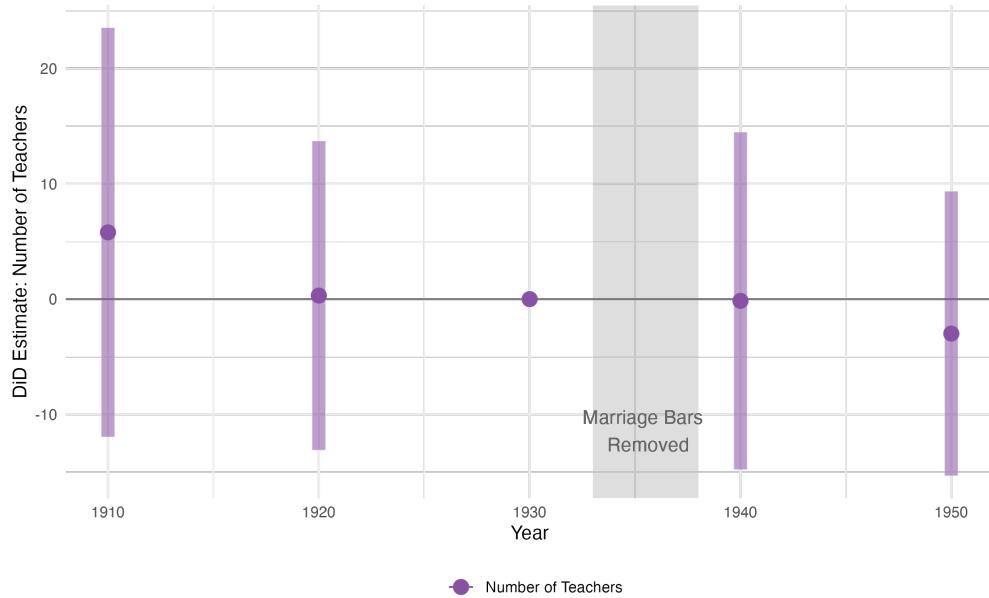


Figure A1: Estimated effects of the introduction of employment protections for married women on the total number of teachers per county.

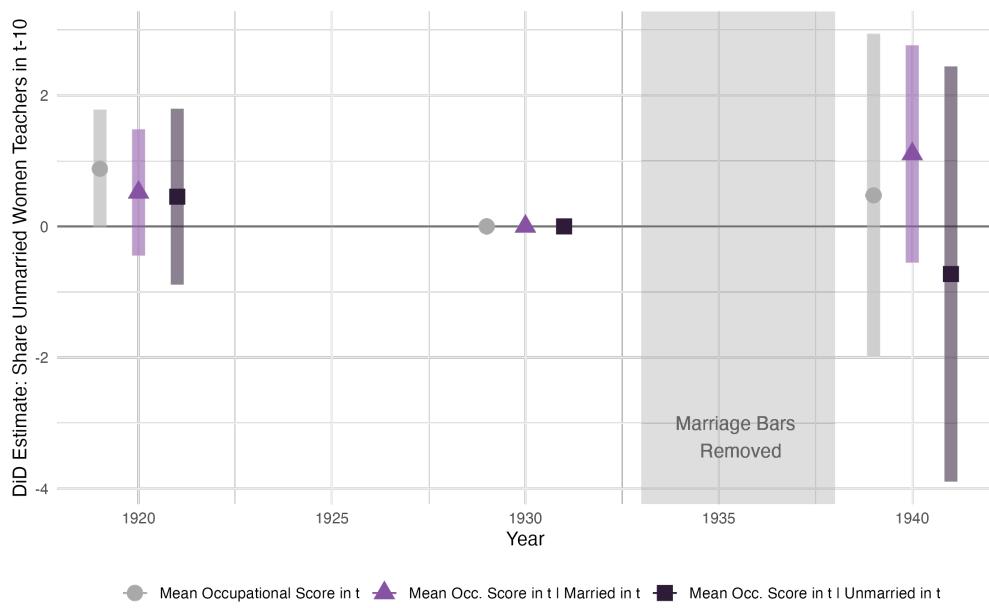


Figure A2: Estimated effects of the introduction of employment protections for married women in teaching on the occupational score of incumbent unmarried teachers. Analysis uses linked sample of unmarried women teachers in $t - 10$ and measures mean occupational score for the full sample, conditional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

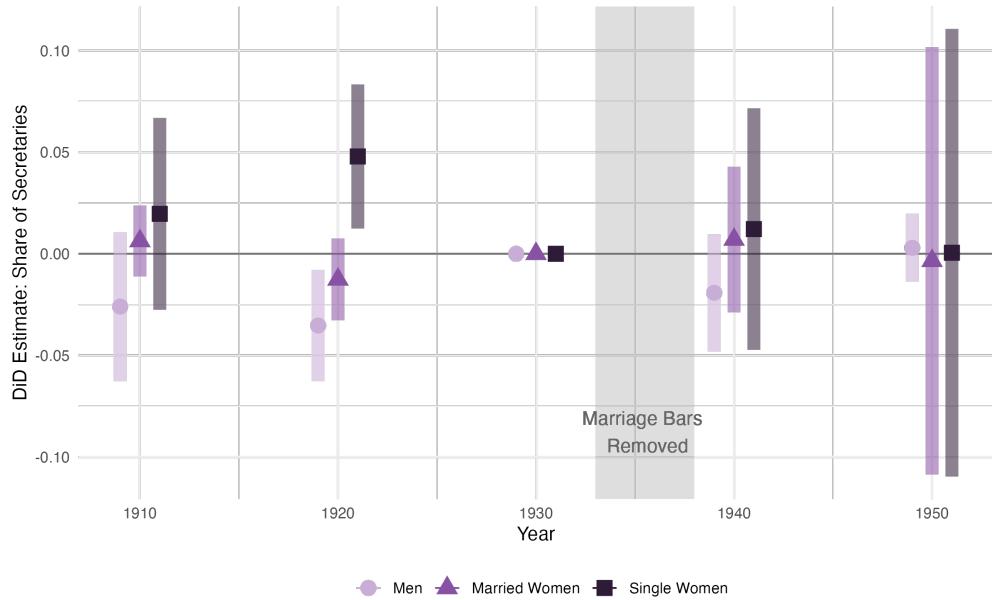


Figure A3: Placebo test: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *secretaries* who are men, unmarried women, and single women.

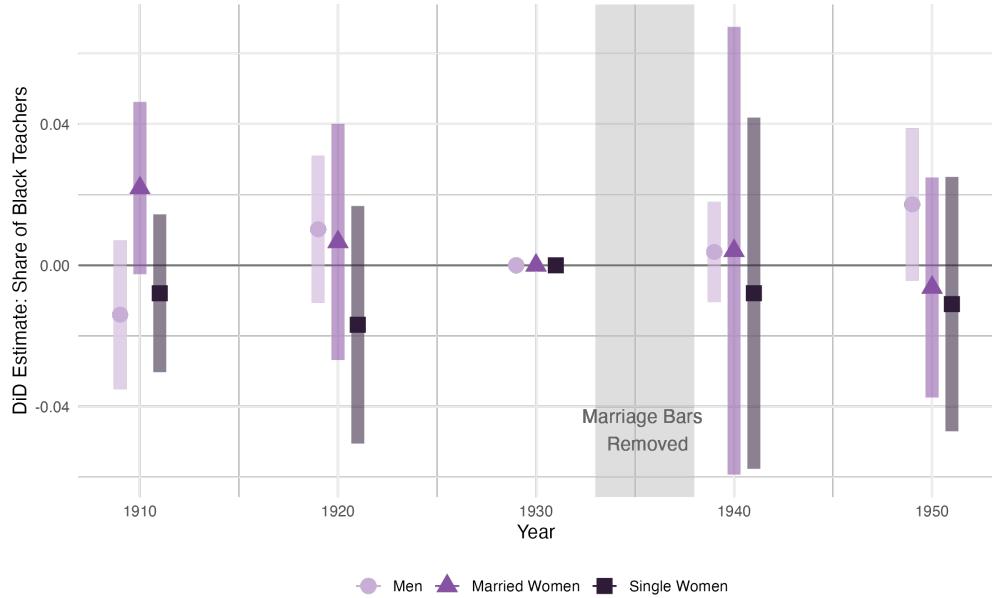


Figure A4: Heterogeneity in main results: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *Black teachers* who are men, unmarried women, and single women.

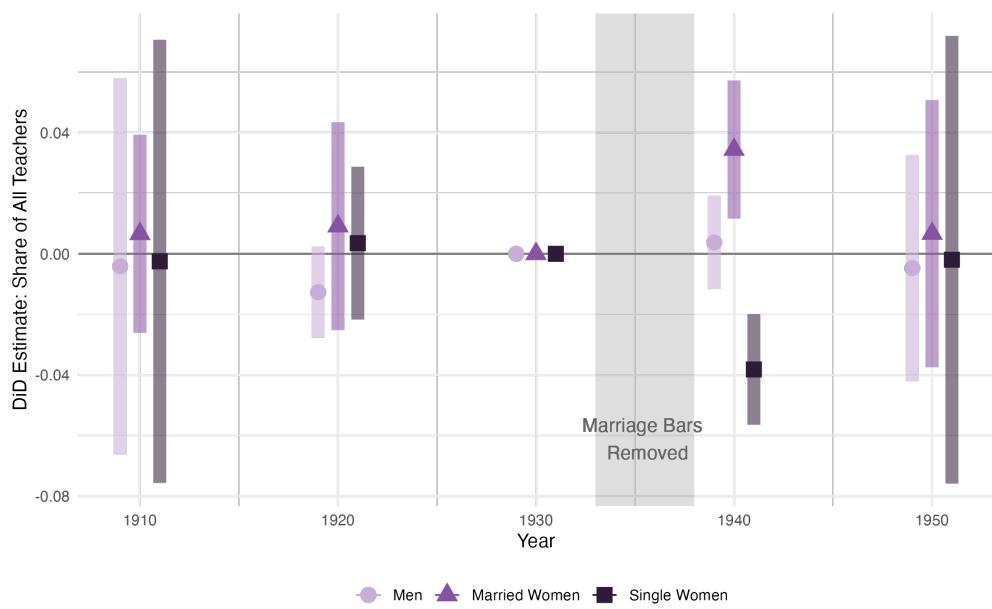


Figure A5: Heterogeneity in main results: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *all teachers* (white and Black) who are men, unmarried women, and single women.

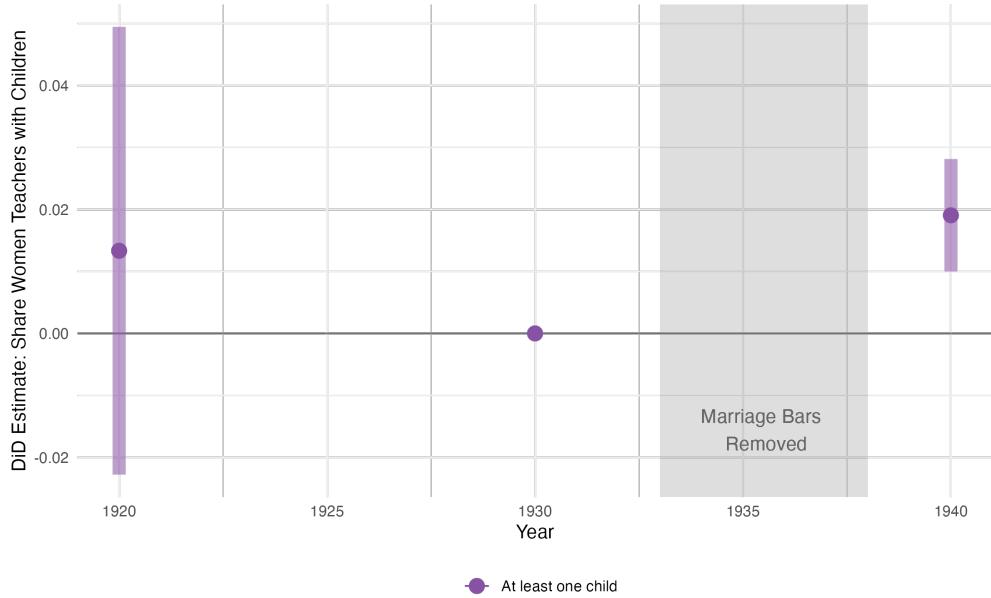


Figure A6: Results on fertility: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *white women teachers* with children.

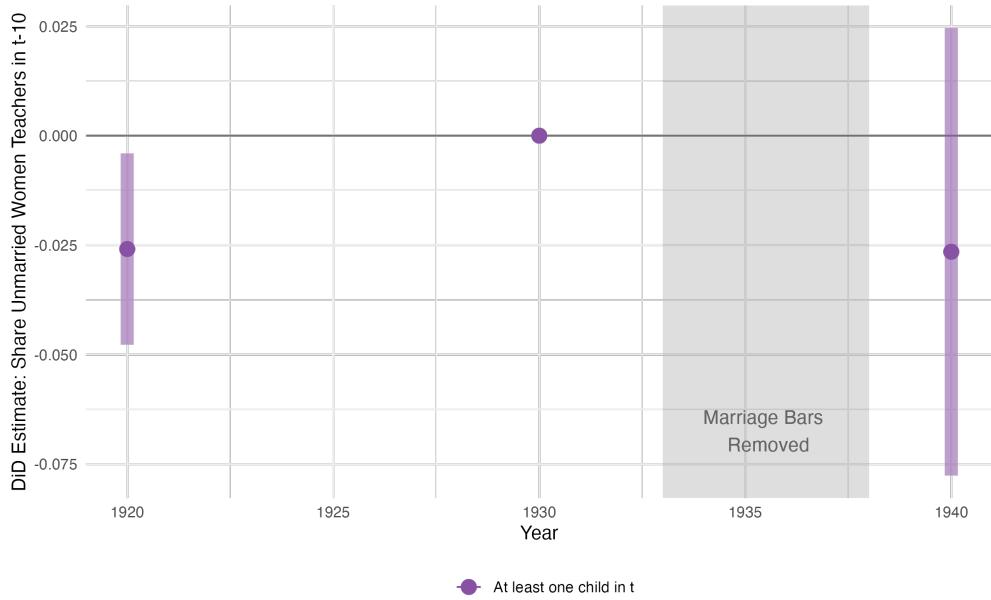


Figure A7: Results on fertility: Estimated effects of the introduction of employment protections for married women in teaching on likelihood of having children in the future among unmarried women teachers. Analysis uses linked sample of unmarried women teachers in $t - 10$ and uses as an outcome the share of said teachers with at least one child in t .

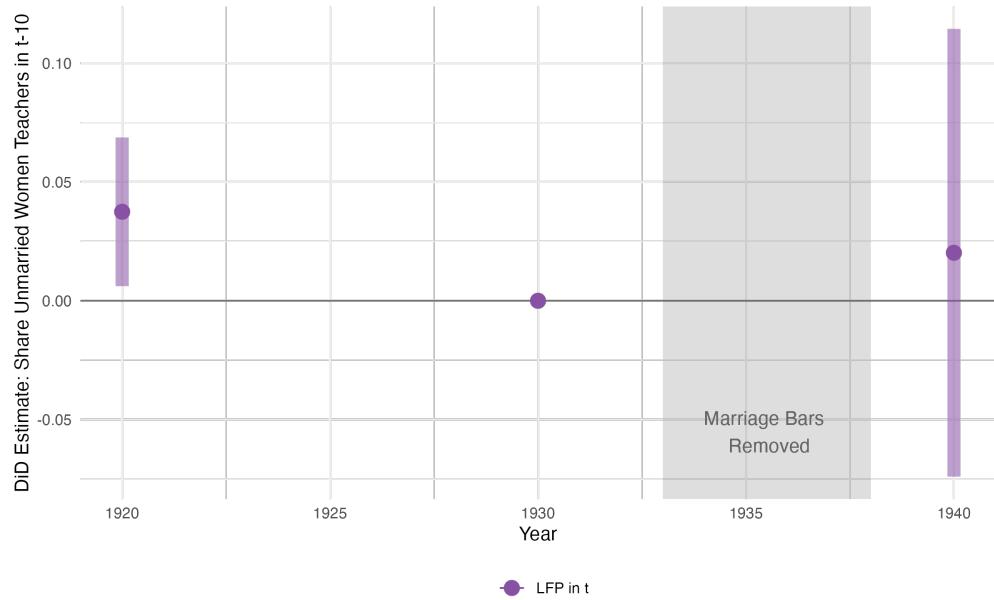


Figure A8: Overall effects on incumbent women teachers' LFP: Estimated effects of the introduction of employment protections for married women in teaching on the LFP of women who were incumbent unmarried teachers prior to the prohibitions.

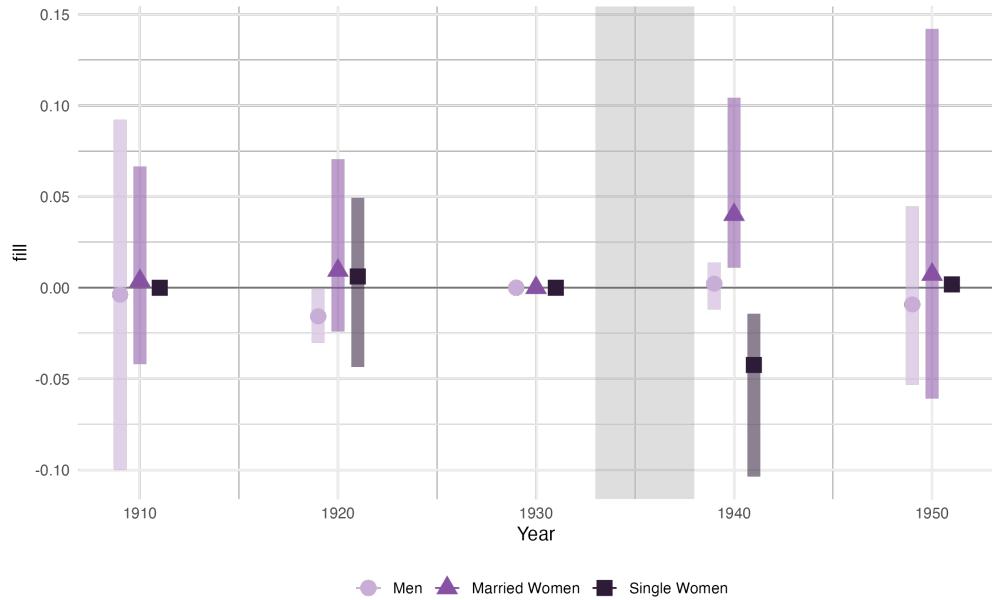


Figure A9: Main results, with state-level clustering of standard errors using wild cluster bootstrapping [Fischer and Roodman \[2021\]](#): Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *teachers* who are men, unmarried women, and single women.

∞

B Additional Tables

Table B1: Census linkage rates by year and group.

	All	Treated States	Teachers	Unmarried Women	Married Women	Unmarried Women Teachers	Married Women Teachers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<u>1910-1920 Linked Sample</u>							
% 1910 Individuals Linked to 1920	62.1%	64.9%	54.5%	55.8%	67.2%	50.2%	64.5%
Number of 1910 Individuals (Thous.)	92,043.6	4,500.8	541.7	27,002.2	17,689.2	409.9	24.5
<u>1920-1930 Linked Sample</u>							
% 1920 Individuals Linked to 1930	63.8%	65.5%	54.5%	55.3%	68.9%	49.4%	69.8%
Number of 1920 Individuals (Thous.)	105,731.0	4,978.4	697.8	30,334.6	21,490.5	531.1	58.2
<u>1930-1940 Linked Sample</u>							
% 1930 Individuals Linked to 1940	65.3%	65.4%	56.3%	54.4%	71.5%	47.1%	74.5%
Number of 1930 Individuals (Thous.)	122,777.5	5,784.9	1,013.0	34,453.1	26,242.3	676.5	150.5

Notes: Linkage rates are computed as the share of a given population in the base year (e.g. 1910) that are successfully linked to the following census (e.g. 1920). Groups (e.g. marital status) are based on base year characteristics. Treated states are NC and KY. We use Census Tree linkages from 1910-1920, 1920-1930, and 1930-1940 [Price et al., 2023a,b,c].

Table B2: Estimated effects of the prohibitions on women's propensity to get married and to teach, work outside of teaching, and exit the labor force.

	(1)	(2)	(3)	(4)
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)				
Sample 4: Women who were unmarried and working as non-teachers in $t - 10$				
Treated \times 1940 (γ_{1940}^{DD})	-0.011 (0.009)	0.001 (0.002)	-0.006 (0.011)	0.005 (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 \times 1940 (γ_{1940}^{DD})	0.001 (0.007)	-0.001 (0.002)	0.001 (0.012)	0.00001 (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 B3: Estimated effects of the prohibitions on women's propensity to remain unmarried and to teach, work outside of teaching, and exit the labor force.

	(1)	(2)	(3)	(4)
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)				
Sample 3: Women who were unmarried and not in the labor force in $t - 10$				
Treated \times 1940 (γ_{1940}^{DD})	0.011 (0.009)	0.002 (0.002)	-0.007 (0.009)	0.016* (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 \times 1940 (γ_{1940}^{DD})	0.007** (0.003)	0.001 (0.001)	-0.003 (0.003)	0.009** (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 B2.

C Estimating the Role of Marriage Bar Removal in the Overall Increase in Married Women’s LFP

C.1 Elasticity Calculation

To estimate the role that the removal of marriage bars 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 marriage bar prohibitions (ε_{EP}^{teach}) using the following formula:

$$\varepsilon_{EP}^{teach} = \frac{\Delta s_{teach, 1930-1940}^{MW} / s_{teach, 1930}^{MW}}{\Delta q_{emp, 1930-1940}^{teach} / q_{emp, 1930}^{teach}} \quad (1)$$

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 marriage bar prohibitions (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 marriage bar prohibitions 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 $\hat{\gamma}_{1940}^{DD} \equiv \Delta s_{teach, 1930-1940}^{MW} = 0.9767 / 1000 = 0.0009767$. 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 marriage bar prohibitions ($q_{emp, 1940}^{teach} = 0$), regardless of the initial value of $q_{emp, 1930}^{teach}$. The denominator of equation (1) is thus equal to 1.

We therefore estimate that the elasticity of married women’s employment in teaching to the prohibition of marriage bars 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 prohibition of marriage bars in teaching is equivalent to the change in married women's employment in any occupation due to the elimination of discriminatory 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 $\Delta q_{emp, 1940-1950}^o / q_{emp, 1930}^o = 1$ 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 removal of marriage bars) as follows:

$$\Delta s_{MB, 1940-1950}^{MW} = \sum_{o \in \mathcal{O}} \varepsilon_{EP}^o \cdot s_{o, 1940}^{MW} = \varepsilon_{EP}^{teach} \sum_{o \in \mathcal{O}} s_{o, 1940}^{MW} = \varepsilon_{EP}^{teach} \cdot s_{MB, 1940}^{MW} \quad (2)$$

where $s_{MB, 1940}^{MW}$ 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 $s_{MB, 1940}^{MW} = 0.03724$, implying that $\Delta s_{MB, 1940-1950}^{MW} = 0.006353$. The total growth in the share of married women in these occupations between 1940 and 1950 is 0.02996, implying that the removal of insti-

tutional 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.¹ 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 $\Delta s_{MB,1940-1950}^{MW} = 0.006353$ 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 $\Delta s_{MB,1940-1950}^{MW} = 0.02075$ (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.

¹Approximately 30% of the total labor force was employed in a white-collar occupation in 1940.

D Matched and Border Counties Designs

As discussed in Sections 3.3 and 5.4, 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 two alternate empirical designs. The first is a border county design which narrows the sample of counties to treatment and control counties that are geographically adjacent and therefore more likely to be similar and satisfy the parallel trends assumption. The second is a matched counties design which uses data on counties across the country and does not rely on the assumption that geographically close counties are similar.

D.1 Border Counties Design

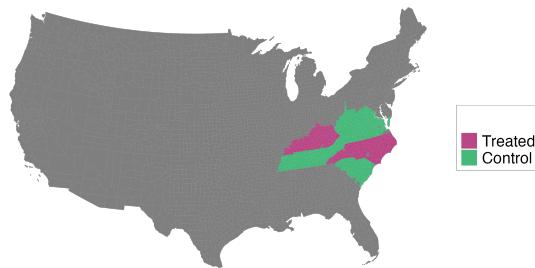
The reasoning underlying the border counties design is that counties in different states that border each other are highly likely to have followed similar historical trends, such that the only difference between counties on opposite sides of a border is their exposure to their states' state-wide prohibitions. Following this logic, we construct our alternate border county samples as follows. For our sample of treated border counties, we select all counties in treated states that border some other (non-treated) state. For our sample of control border counties, we select all counties in neighboring (non-treated) states that border a treated state.² Panel (b) of Figure D1 highlights the resulting samples of bordering counties.

D.2 Matched Counties Design

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

²Note that for our border counties design, unlike our preferred specification, we do not require control counties to be in a Southern state.

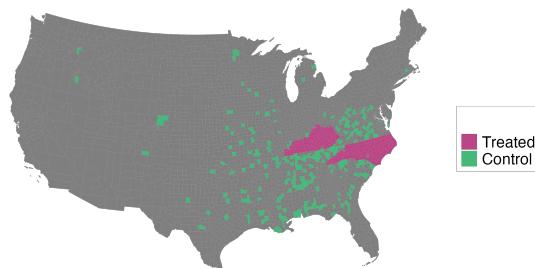
Figure D1: Maps of treated (pink) and control (green) counties for (a) our preferred control specification of neighboring Southern counties, (b) our border county specification and (c)-(e) specifications using a range of matching techniques.



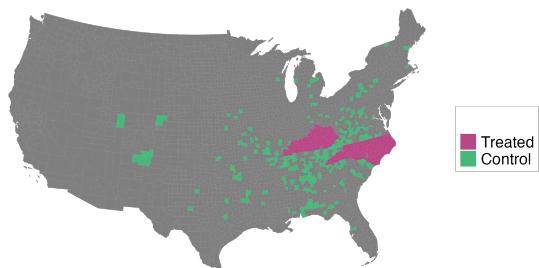
(a) Southern Neighbor States



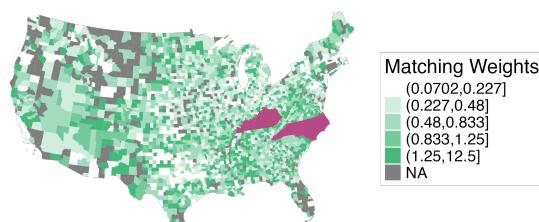
(b) Border Counties



(c) Match 1: Nearest Neighbor



(d) Match 2: Genetic



(e) Match 3: Optimal Full Matching

growth in retail sales per capita from 1929 to 1939, as obtained from [Fishback et al. \[2005\]](#).

³ 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 \[2013\]](#) and [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. The third method, optimal full matching, uses all counties and assigns weights to control counties based on their similarity to treatment counties. Figure D1 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 D1 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.

³ 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.

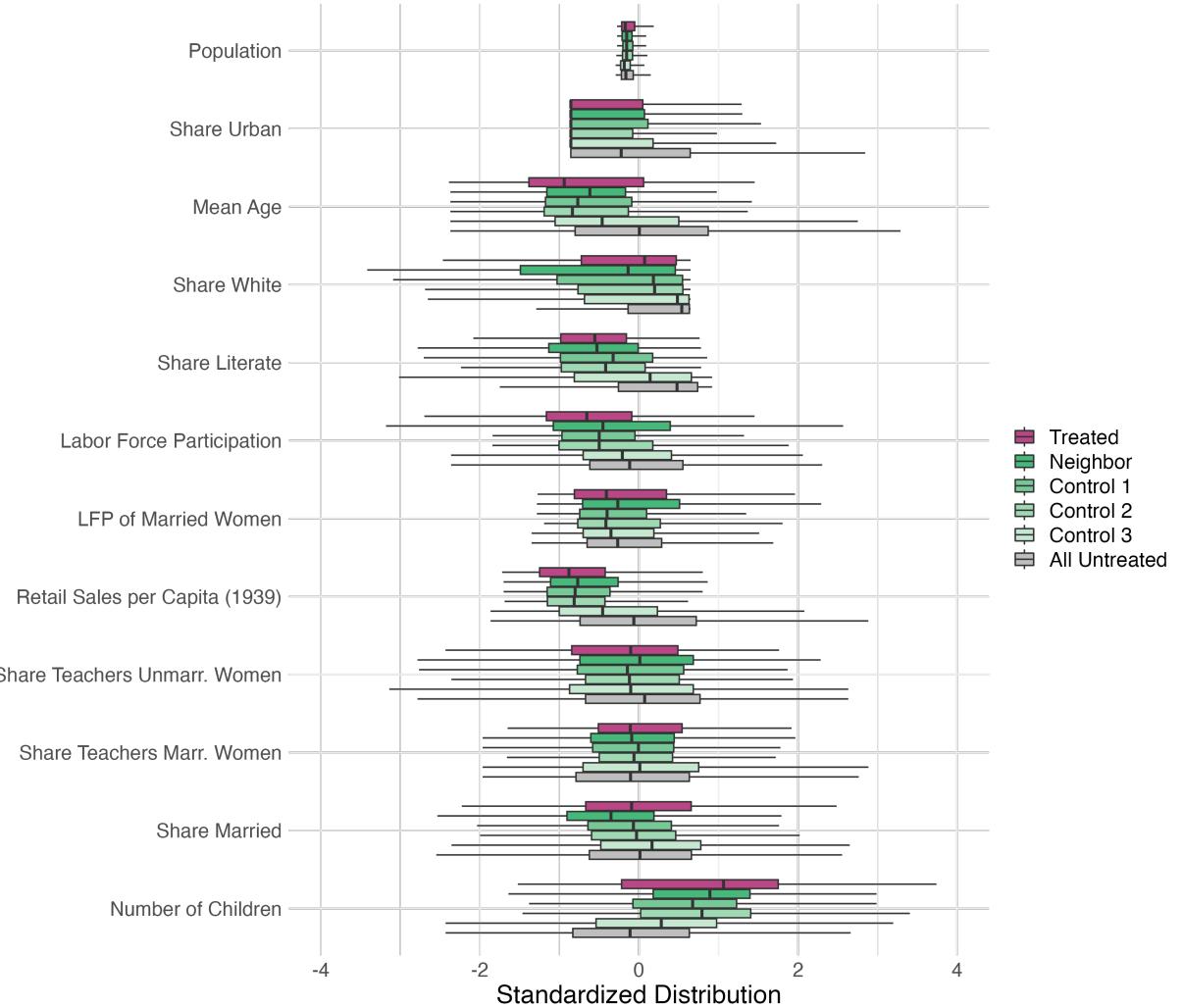


Figure D2: Boxplot of standardized 1930 values of various covariates by treatment or control group. The center bar represents the median, the edges of the box represent the 25th and 75th percentiles, and the edges of the whiskers represent extrema, with outliers removed (see R function `geom_boxplot` for further details). Distributions are weighted for control group 3. Covariates are outlined in detail in Footnote 3, and also include the share of women over the age of 18 that are married, and the average number of children for married women. All data is obtained from [Ruggles et al. \[2024\]](#) with the exception of 1939 Retail Sales per Capita, which is obtained from [Fishback et al. \[2005\]](#).

In Figure D2, we graph boxplots for the treated and various control groups of the standardized 1930 values of the matching covariates listed in Footnote 3, 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 neighboring 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.3 Results

We re-estimate our key analyses using the border county and three matched samples and present the results in Figure D3. For the border counties and for matched samples 1 and 2, in panels (a) and (b), our estimated coefficients in 1940 are consistent with our main results—the marriage bar prohibitions 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. Matched sample 3, in panel (d), shows similar results, but suggests a decrease in the share of men in teaching and no convergence by 1950. The slight difference in results for matched sample 3 is consistent with the fact that, as shown in Figure D2, the control group for matched sample 3 is also the least comparable out of all the alternate control groups to the treatment group.

Similarly to our main results, we find no significant evidence of pre-trends in the share of married women, unmarried women, or men teachers prior to 1930 under any of the alternate control groups. Other results are also qualitatively similar and available upon request.

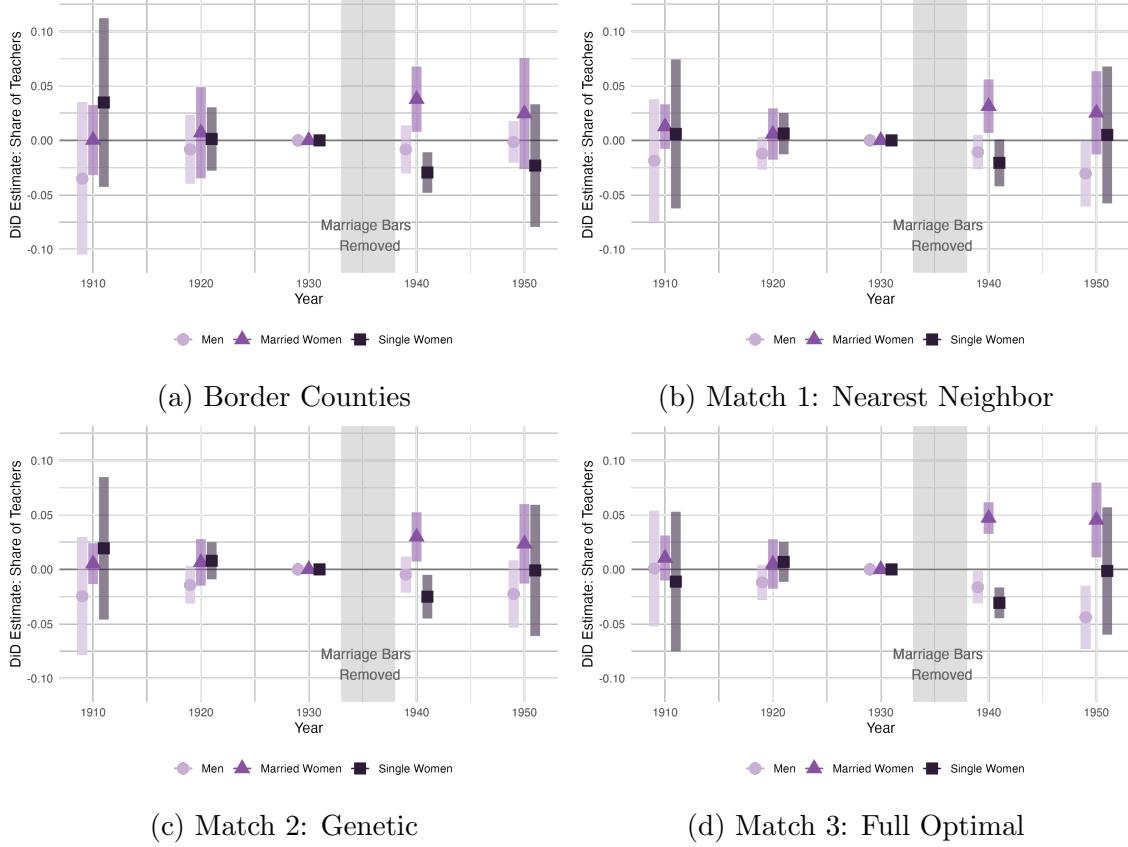


Figure D3: Estimated effects of the prohibition of marriage bars in teaching on the gender composition of teachers, at the county level. Estimates are from a difference-in-differences specification where the dependent variable is the share of teachers in a county that are married women, unmarried women, and men. The sample for (a) includes border counties in KY, NC and adjacent states while the sample for (b)-(d) includes all counties in KY and NC and matched control counties, as determined by various methods. Standard errors are clustered at the county level. 95% confidence intervals are shown.

E State-Level Synthetic Difference-in-Differences

As discussed in Section ??, our preferred specification involves analysis at the county level as there is reason to believe that counties with different initial norms and policies surrounding married women teachers may have responded heterogeneously to the uniform ‘treatment’ of the state-level marriage bar prohibitions. That said, given that the treatment of interest was implemented at the state level, in this section we conduct an alternate state-level analysis to confirm that our results are not driven by our choice of the unit of analysis.

E.1 Empirical Strategy

At the state level, we have only two treated units—Kentucky and North Carolina—which motivates our decision to use a synthetic difference-in-differences empirical strategy. We follow [Arkhangelsky et al. \[2021\]](#) in our implementation of synthetic difference-in-differences by finding both unit weights $\hat{\omega}^{sdid}$ that balance pre-treatment trends in our outcome variables for treated and control units as well as time weights $\hat{\lambda}^{sdid}$ that balance trends in control unit outcomes in the pre-treatment and post-treatment periods.⁴ We then use the computed weights to solve the following problem:

$$\left(\hat{\gamma}^{sdid}, \hat{\alpha}, \hat{\beta}\right) = \arg \min_{\gamma, \alpha, \beta} \left\{ \sum_{i=1}^N \sum_{t=1}^T (y_{st} - \alpha_t^{sdid} - \beta_s^{sdid} - \gamma_{1940}^{sdid} \times Treat_s \times Year_{t=1940})^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\}, \quad (3)$$

which boils down to estimating a weighted two-way fixed effects regression to obtain an estimate of our coefficient of interest γ_{1940}^{sdid} , the effect of the prohibition of marriage bars on state-level outcome y_{st} in treated relative to control states. We define our state-level outcomes analogously to the county-level outcomes described in Section ??.

E.2 Estimation and Results

We begin by computing the unit and time weights. State weights $\hat{\omega}^{sdid}$ as computed for our main outcome variable (the share of women in state s who are married women) are shown

⁴Because it is unclear which units are treated in 1950, all estimation in this section uses only data from 1910-1940.

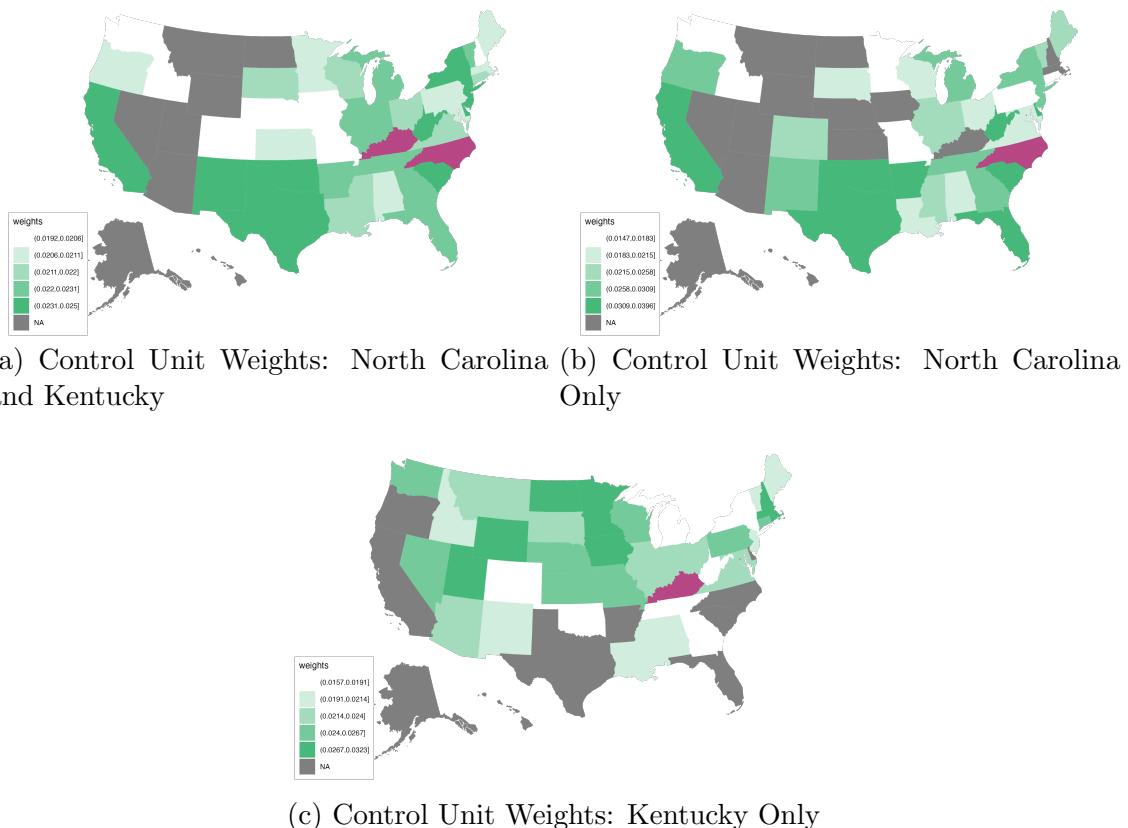
in Panel (a) of Figure E1. Computed time weights for all analyses are 1 for 1930 and 0 for other pre-period years, simplifying our setup to a two-period setting.

First, we use the synthetic DiD setup to estimate the state-level effect of the marriage bar prohibitions on the composition of the teacher workforce. Figure E2 plots estimates of γ_{1940}^{sdid} for the share of teachers who were married women (triangles), men (circles), and unmarried women (squares) respectively. The first two groups of estimates juxtapose the results our preferred specification (at the county level with county-level clustered standard errors) with the state-level synthetic DiD estimates. Standard errors are computed using a permutation approach ('placebo' option in R). The point estimates are very similar between the county and state-level analyses, and our primary finding that the share of married women teachers increased in treated relative to control states remains significant at the 95% confidence level despite widened confidence intervals.

An additional benefit of using the synthetic DiD approach is that it also allows us to perform analysis with only one treated unit. As such, we estimate the effects of the prohibitions on married women teachers in North Carolina and Kentucky separately. Panels (b) and (c) of Figure E2 map control unit weights for NC and KY individually, and we see that much of the control unit weighting in the joint analysis is driven by North Carolina. Nevertheless, the third and fourth groups of estimates in Figure E2 show that, while noisier, the point estimates for the effects of the marriage bar prohibitions on NC and KY are remarkably similar to the overall effects, suggesting that results are not being driven by one state alone.

Finally, we replicate our results on mechanisms using the state-level synthetic DiD and linked data aggregated to the state level. Table 4 replicates Table 3 from the body of the paper, presenting synthetic DiD estimates and standard errors computed using the placebo method. Results are qualitatively similar to our preferred estimates, with the only difference being slightly larger point estimates at the state level. We therefore conclude that our main results are not sensitive to the chosen unit of analysis.

Figure E1: Maps of treated (pink) and control (green) states shaded by unit weights computed in our synthetic difference-in-differences empirical strategy (computed to match pre-trends between control and treatment states for outcome variable share teachers married women). Panel (a) uses both Kentucky and North Carolina as treated units, panel (b) uses only North Carolina as a treated unit, and panel (c) uses only Kentucky as a treated unit.



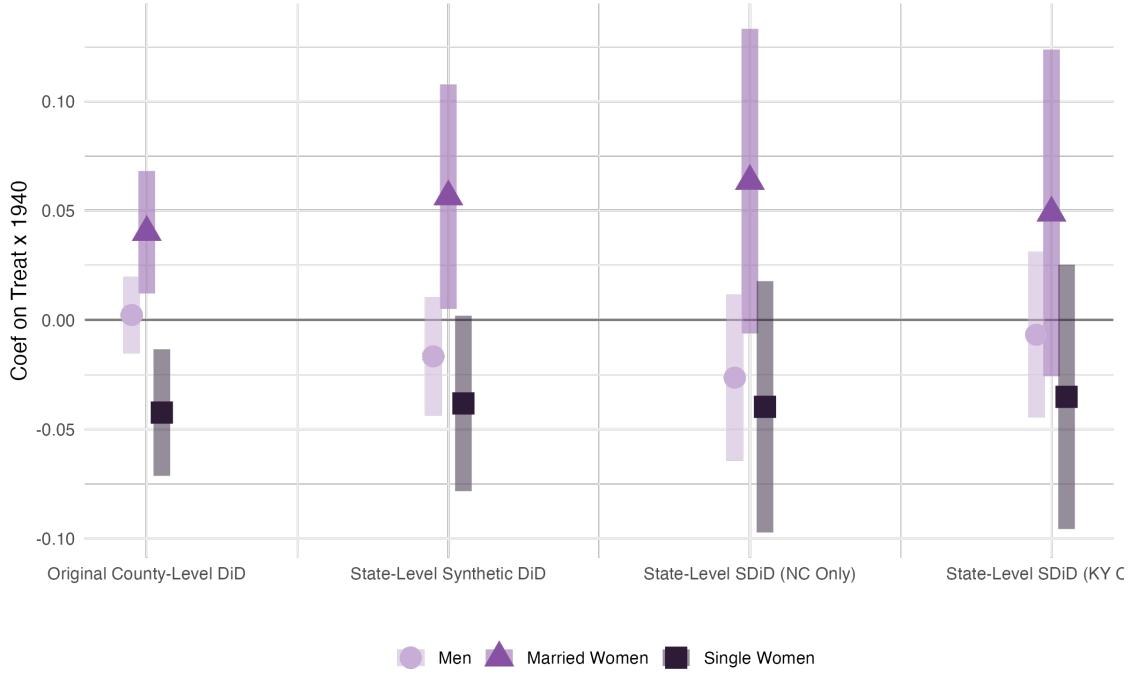


Figure E2: Estimates of the effect of the prohibition of marriage bars in teaching on the gender composition of teachers. The first column uses the standard difference-in-differences setup from our main specification at the county level, with standard errors clustered at the county level. The other columns use state-level synthetic difference-in-differences, with standard errors computed using a ‘placebo’ method. The second column includes both KY and NC as treated units, while the third and fourth only include NC and KY respectively. 95% confidence intervals are shown.

Table 4: State-Level Synthetic DiD: Estimated effects of the prohibitions 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 \times 1940 (γ_{1940}^{DD})	-0.038 (0.031)	0.035*** (0.014)	-0.003 (0.006)	-0.088** (0.035)
Dep. Var. 1930 Mean	0.5839	0.04572	0.03478	0.5034
Sample 2: Women who were married and not in the labor force in $t - 10$				
Treated \times 1940 (γ_{1940}^{DD})	-0.003 (0.002)	0.001*** (0.0006)	0.009 (0.009)	-0.013* (0.010)
Dep. Var. 1930 Mean	0.9328	0.001822	0.054	0.8761
Sample 3: Women who were unmarried and not in the labor force in $t - 10$				
Treated \times 1940 (γ_{1940}^{DD})	-0.022 (0.017)	0.002** (0.0007)	0.013*** (0.004)	-0.038* (0.021)
Dep. Var. 1930 Mean	0.5136	0.004338	0.04511	0.4641

Notes: Estimation follows Equation (3). Construction of these state-level linked samples exactly follows the construction of county-level linked samples (see Section 4.2 and Table Notes from Table 3) except that we use all states and weight according to the procedure outlined above. Sample 1 contains linked women who were under 40, unmarried, and teaching in 1910, 1920, and 1930, Sample 2 contains linked women who were aged 18-50, married, and not in the labor force in 1910, 1920, and 1930, and Sample 3 contains linked women who were aged 8-40, unmarried, and not in the labor force in 1910, 1920, and 1930. All regressions use the 1910-1920, 1920-1930, and 1930-1940 linked full-count Census samples. Standard errors are computed using a ‘placebo’ method. See Section ?? for details and full citations for data.

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