

The Effects of Prohibiting Marriage Bars: The Case of U.S. Teachers

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 by 3.5 p.p. (20%), partly by pushing unmarried women out of teaching, thus increasing women’s labor force participation.

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 LFP, 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 passage of laws that prohibited the use of discriminatory “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 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—KY and NC—ultimately passed laws *prohibiting marriage bars in teaching*. By comparing KY and NC 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. Newspapers from this era chronicle an active public debate over the working married woman,

¹This was particularly true for white women. See Section 5.1 for further discussion.

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, comparing outcomes in states that prohibited marriage bars to neighboring Southern states that did not. 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 women over time.²

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

²It 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.

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 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.

Notably, our period of study overlaps with the Great Depression and subsequent economic recovery—large-scale economic events that could have affected women’s LFP differently in treated and control states absent the prohibitions. We address this concern using several robustness checks, including border county and matched county designs that allow us to match treated and control counties as closely as possible on pre-trends in economic conditions.

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 likelihood that a married woman worked as a teacher also increased by 17% between 1930 and 1940

in treated counties relative to control ones. The effects were driven by white women with no effects on Black married women’s employment. Our estimates thus squarely reject the null hypothesis that policy changes aimed at prohibiting employer discrimination against married women were ineffective in increasing white married women’s low LFP, and further suggest that marriage bars were not binding for Black women in the early 20th century.

Furthermore, we find that the increase was driven by an extensive margin labor supply response among white married women. Using our linked census sample in which we can observe individuals moving between occupations and in and out of the labor force, we find that the prohibitions led women to change 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 force, and unmarried women teachers becoming more likely to get married *and remain in teaching* rather than get married and exit the workforce. We therefore conclude that the prohibition of marriage bars 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 teaching. 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 [Goldin, 2021]. Indeed, consistent with imperfect substitution, women teachers in many U.S. cities were paid less than a third of what men teachers were paid and tended to be allocated to subjects like home economics rather than mathematics [Bohan and Null, 2007, Blount, 1996].

Finally, we use our linked census sample to 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 into other occupations, without affecting their earnings as proxied by occupational scores. We therefore conclude that the policy change had a net positive impact on women’s labor market outcomes, pulling women into the labor force without pushing incumbent women teachers out of the labor force or to lower paying jobs.

Overall, our study shows that removing institutional barriers to employment specific to married women modestly improved their labor market outcomes between 1930 and 1950. Our findings also serve as a cautionary tale: in the presence of gendered social attitudes, the costs of such policies may be borne by other women who are reshuffled to other occupations. However, in our setting, we find an overall positive effect of prohibiting marriage bars 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. A significant body of work studies the effects of large-scale factors post-1950, 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 household technologies [Greenwood et al., 2005]. However, few papers study the factors that contributed to the initial rise pre-1950, which happened in spite of married women facing active, legal employer discrimination. One notable exception is Goldin [1988], who documents the use of marriage bars in the early 1900s U.S. and explores firms’ economic justifications for using these discriminatory practices.³ 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

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

(which accounted for over half of the later rise in women’s LFP, according to [Greenwood et al. \[2005\]](#)). We also provide the first empirical evidence that marriage bars were not binding for Black women, as posited by [Goldin \[1988\]](#).

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 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.⁴ Our paper provides new evidence of another example of an anti-discrimination 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.

⁴For instance, in the case of the ADA, which imposed that firms must provide accommodations for workers with disabilities, [Acemoglu and Angrist \[2001\]](#) rationalize their findings by arguing that firms that found introducing such accommodations too costly and simply chose not to employ as many workers with disabilities.

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 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. 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 over a quarter of working women in urban centers due to the greater likelihood of large firms adopting formal policies [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, then dropped significantly throughout the 1940s.⁵ “Hire bars” in school districts, for instance, rose from affecting 60% of the urban population in 1920 to nearly 80% in 1942, before a dramatic decline to 17% 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.⁶ Incidentally, rhetoric around the efficiency of married women workers also flipped during this time period, with older women being praised for their “maturity,” “steadiness,” and “relia-

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

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

bility,” 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.⁷

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.

By 1940, only two states—KY and NC—had passed state-level legislation containing employment protections that explicitly prohibited discrimination against married women in teaching [[Cooke and Simms, 1940](#)]. The legislation in NC in 1933 was broad in its application: the NC Public Laws Chapter 562 Section 11 declared that “in the employment of teachers no

⁷The 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.

rule shall be made or enforced on the ground of marriage or nonmarriage” [[North Carolina General Assembly, Regular Session, 1933](#)]. The legislation in KY in 1938 was more specific to experienced teachers: House Bill No. 51 in the KY 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.⁸

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.⁹ 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.¹⁰ Our empirical design in Section 4 leverages this variation to formally evaluate the effects of the laws in KY and NC on women’s employment in teaching.

⁸We found no newspaper articles referencing the legislation in NC and only one article mentioning the legislation passed in KY [[The Courier Journal, 1938](#)].

⁹One potential contributor to the slight increase in married women teachers across states by 1930 was the shift to longer, more formal accreditation. With most teacher training programs changing from local 2-year normal schools to state-approved 4-year teaching colleges by 1930, schools in need of qualified teachers may have chosen to retain/rehire trained and married women teachers as a result ([Harper \[1939\]](#)).

¹⁰The increase in other states was likely due to socioeconomic factors rather than legislation, as we found no evidence of similar state-wide marriage bar prohibitions in other states in later years.

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 individuals between the ages of 16 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 less than 10% of total enrollment in the early 1900s, public school teachers likely comprise the bulk of the teachers we identify [National Center for Education Statistics, 1993].

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 1940.¹¹ 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]. 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 similar over time and between treated and control states, there are two differences of note. First, linkage rates are higher in all years for married women versus unmarried women (65.8% versus 53.9% in 1920) and for white women versus Black women (62.0% versus 32.1% in 1920). These differences are known in the literature, in part due to the Census Tree links coming from a free genealogical website (Buckles et al.

¹¹At the time of writing, linkages to 1950 are not yet available.

[2023]), and persist over time. As a result, women who get married or move between decennial Censuses are less likely to appear in our linked samples, which may attenuate our estimated effects of the prohibitions on unmarried women teachers towards zero.

Second, linkage rates increase over time for married women but remain stable for unmarried women. One potential explanation for these trends could be that reports on the FamilySearch website are more frequent in more recent years, driven by descendants of married women. However, given that linkage rates rise similarly in both treated and control states, we are not concerned that the differential trends bias our results.

3.3 County Sample Selection

Our analysis focuses on the counties within and surrounding the states that passed prohibitions (KY/NC). To motivate this focus, Table 1 provides summary statistics comparing counties in KY/NC to counties in other states in 1930.

KY/NC were highly comparable with their neighboring Southern states, SC, VA, TN, and WV, as shown in Columns (3) and (4). Compared to the national average (Column (1)), counties in KY/NC and neighboring states had higher student-to-teacher ratios, exhibited a lower share of the population living in urban areas, and saw more children per married woman on average.¹² Within the U.S. South (Column (2)), counties in KY/NC and neighboring states tended to have lower rates of LFP among white married women, particularly in teaching. There are a few small but notable differences between KY/NC and the neighboring states—in 1930, neighboring states were more urban and had higher LFP among married women, higher unemployment rates, and greater shares of single women in teaching—which warrant robustness exercises using controls and different empirical strategies.¹³ However, given the overall cultural and statistical similarities between KY/NC and their neighboring states, the neighboring states comprise our preferred comparison group.

Our main analysis sample includes all 220 counties in treated states and all 320 counties in the neighboring Southern states. In parts of our analysis where we examine county-

¹²These statistics also highlight that our findings are most relevant for the U.S. South. The higher student-teacher ratios in particular in Southern states are consistent with anecdotal evidence of teacher shortages in the South, as in Goldin [2021].

¹³See Supplemental Appendix C.

level outcomes (rather than individual-level outcomes), we restrict to a balanced sample consisting of 217 treated counties and 310 neighboring Southern control counties.¹⁴ We make two further restrictions for analyses by race: when we examine county-level outcomes for white teachers, we exclude one control county with fewer than ten white teachers in 1930 or 1940; similarly, when we examine county-level outcomes for Black teachers, we exclude 109 treated and 129 control counties with fewer than ten Black teachers in 1930 or 1940. Our results are robust to alternate sample specifications.

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 LFP 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—KY and NC—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.^{15,16}

¹⁴We exclude 13 counties that are either created or consolidated between 1910 and 1950 and are thus missing from the Census in at least one year. See Panel (a) of Supplemental Appendix Figure C1 for a map of our sample of treated and control counties.

¹⁵Although treatment timing is staggered in our setting (1933 for NC and 1938 for KY), implementing the estimators recommended in the recent 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.

¹⁶In our state-level analysis in Supplemental Appendix D, we also show results for NC and KY separately. Our findings are not being disproportionately driven by one state; in fact, point estimates are remarkably similar for both states.

Our outcomes of interest, which are outlined in the following section, include region-level outcomes (such as the share of teachers in a county who are married women) and individual-level outcomes (such as the likelihood that an unmarried woman in 1930 becomes a married woman teacher in 1940). We therefore use both county- and individual-level specifications. Our preferred county-level specification is:

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

where c indexes county, t indexes Census 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.¹⁷

Analogously, our preferred individual-level specification is:

$$y_{it} = \alpha_t^{DD} + \beta_{c(i)}^{DD} + \sum_{k \in \{1910, 1920, 1940, 1950\}} \gamma_k^{DD} \times \text{Treat}_{s(i)} \times \text{Year}_{k=t} + \varepsilon_{it}, \quad (2)$$

where i indexes individual and the notation follows from the county-level specification above. As above, we include county and year fixed effects and cluster standard errors at the county level.

¹⁷Our preferred specification uses county level clustering 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 whole states 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 (six in total) 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 Figure A4, we show standard errors clustered at the state level using the cluster wild bootstrap. We also use a state-level synthetic difference-in-differences empirical strategy in Supplemental Appendix D. Our main results are robust to both approaches.

4.2 Outcome variables

We examine two sets of outcomes. The first set of outcomes allows us to explore the effects of the marriage bar prohibitions on the local teacher workforce. Since the closest geographic approximation we have to the school district is the county, we use cross-sectional decennial Census data to construct the following *county-level* outcomes for local teacher workforces: the share of teachers in county c in year t who are married women, unmarried women, and men; the share of teachers in county c in year t who are women with children; the number of married women teachers per hundred married women in county c in year t ; and the total number of teachers in county c in year t .

The second set of outcomes allows us to understand the mechanisms by which the prohibitions affected the composition of teachers. These outcomes are constructed at the *individual level* using the linked decennial Censuses, allowing us to investigate women’s transitions in and out of marriage, teaching, and the workforce. For each woman i observed in year $t - 10 \in \{1910, 1920, 1930\}$, our main outcomes of interest include: an indicator for her marital status in year t , and an indicator for the interaction between her marital status in year t and her occupational status in year t (either teaching, not teaching but in the labor force, or not in the labor force). Treatment status is defined based on the county in which the person lives in year $t - 10$.

Additional linked outcomes include a woman’s occupational score in year t and indicators for whether, in year t , she has any children, is in the labor force, or is living in a different state.^{18,19}

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

¹⁸Another natural outcome of interest is women’s age at marriage. However, age at marriage was only collected for sample-line individuals (roughly 5% of the population) in the 1940 census, resulting in too few teachers with age at marriage recorded to be able to capture meaningful variation for our purposes.

¹⁹Occupational scores were numerical ratings of occupations ranging from 0 to 80 based on the average income associated with the occupation in 1950—a common proxy for income in the economic history literature, see e.g. [Abramitzky et al. \[2012\]](#).

of interest would have evolved similarly in treated and control counties. A potential threat to identification would be if the prohibitions and the outcomes of interest had been jointly determined by some omitted variable. For example, if school districts in KY and NC held more progressive views on employing married women compared to 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. Another important factor to consider is that the prohibitions took place against the backdrop of many economic and policy changes that could have affected women’s marriage and employment outcomes, such as changes in compulsory schooling laws, child labor laws, teacher training requirements, or the Great Depression and New Deal programs. Parallel trends could also be violated if any of these policies differentially impacted treated and control states.

In this section, we present three pieces of evidence that suggest the parallel trends assumption reasonably holds in our setting. We then discuss how the assumption is affected by the Great Depression and New Deal, and how we approach the biases these economic and policy changes may introduce in our analysis.

Support for parallel trends. 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.²⁰ We find that respondents in KY were weakly less likely to approve of the rule (22.0%, s.e. 4.9%) com-

²⁰Since this survey took place several years after marriage bars in teaching were prohibited in NC, we do 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.

pared to respondents in TN and WV (27.9%, s.e. 5.9%), but that the gap is statistically indistinguishable from zero 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. Local 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 1932, a Mrs. Emma Lee White introduced a similar bill 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 neighboring Southern states.

The Great Depression and New Deal. Despite the many economic and policy changes that took place between 1910 and 1950 that could have affected women's marriage and employment outcomes, many of these changes did not differentially impact our treated and control states. For instance, changes in compulsory schooling laws and child labor laws, which could have affected demand for teachers, did not occur in most of our treated and control states between 1910 and 1950.²¹ One of the key changes in teacher training in the early 1900s was the shift from 2-year "normal schools" to 4-year college programs, but these

²¹The only change that occurred was that VA, one of our control states, required one more year of compulsory schooling in the 1920s, which would have led to increased demand for teachers and bias our estimates downwards.

changes largely took place before 1930.

However, we do find two notable differences in how our treated and control states were affected by the Great Depression and New Deal. Here, we reference prior work on the effects of the Great Depression to assess the potential biases these differences may cause in our design, and develop strategies that allow us to address these concerns.

The first difference is that the average treated county experienced a more severe economic downturn and a more muted recovery than the average control county during the Great Depression.²² Two control states in particular experienced comparatively mild effects of the Great Depression: in Supplemental Table B2, Column (2) shows that while KY/NC experienced similar decreases in retail sales per capita between 1929 and 1933 as control states TN and WV, SC and VA experienced much smaller declines. At the same time, Column (3) shows that with the exception of WV, control states experienced slightly larger recoveries in retail sales per capita between 1933 and 1939 than KY/NC.

How might these differential trends bias our results? Prior literature finds that the Great Depression delayed women’s decisions to marry (Hill [2015]) and contributed to more widespread implementation of marriage bars, to preserve job openings for men (Goldin [1988]). At the same time, Bellou and Cardia [2021] find that more severe economic downturns pushed more white women into the labor market, with suggestive evidence that married women were affected as well—although this “added worker” effect was tempered by New Deal programs (Finegan and Margo [1994]).²³

The direction of potential bias is therefore ambiguous. In the absence of the marriage bar prohibitions, worse economic conditions in treated states relative to control states would have increased the use of marriage bars, delayed marriage among women, and decreased resources for schools, all of which would have reduced the relative likelihood that women got married or taught in treated states compared to control ones. At the same time, the “added worker” effect may have resulted larger inflows of married women into the labor force in

²²There is data on New Deal spending by county from 1933 to 1939, but given the time period and decennial nature of the census, the data cannot be used to construct time-varying measures of New Deal spending in 1930 and 1940. We proxy for both recession severity and economic recovery using changes in retail sales per capita instead.

²³Similarly, while prior work finds that birthrates decreased during the Great Depression (Schaller et al. [2020]), the New Deal countered these effects by decreasing infant mortality and increasing birthrates (Fishback et al. [2007]).

treated states compared to control states. Our estimates could thus be biased downward by the reduced likelihood of treated women getting married or teaching in treated states, or biased upward by the “added worker” effect in treated states.

We use several strategies to address these potential biases. Our primary strategy involves comparing treated states to alternate control groups that were more similarly affected by the Great Depression. Supplemental Appendix C outlines these specifications in detail, including a border counties design, where we only compare counties along state borders, and a matched county design, where we match counties based on 1920 and 1930 characteristics. We also include controls for unemployment rates and industry shares in several specifications.

The second notable difference between treated and control states was their exposure to New Deal policies. A large number of control counties in TN, but only a small number of treated counties, were directly impacted by the Tennessee Valley Authority (TVA), a major New Deal program that aimed to provide the first federal electrification program and stimulate the economy in the hard-hit Tennessee Valley. Prior work finds that the TVA had large impacts on the local economy, particularly in shifting work away from agriculture to higher-paying manufacturing jobs (Kline and Moretti [2014]). Combined with the aforementioned studies, these results suggest that including TVA-affected counties in our analysis could bias our estimates upwards, as increasing manufacturing jobs in TVA-affected counties would have decreased the need for married women to work in more control counties than treated ones. To address this potential bias, we conduct robustness checks that exclude the counties affected by the TVA.

5 The Effects of Prohibiting Marriage Bars in Teaching

5.1 Direct Effects on Married Women

We start by examining how the prohibitions of marriage bars in teaching affected the employment of married women as teachers. Results from estimating Equation (1) are shown in Table 2. Column (1) (and triangles in Figure 2) shows the estimated effects of the prohibitions on the share of teachers in a county who were married women, while Column (2)

shows the estimated effects on the number of married women teachers per hundred married women.

Our main finding is that the prohibitions increased married women’s involvement in their local teaching workforce. 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 3.5 p.p. in treated counties between 1930 and 1940, roughly a 20% increase from 1930 when the mean share of teachers who were married women was only 17.8%. The effect is significant at the 1% level. The prohibitions also resulted in roughly one additional married woman per hundred married women working as a teacher in treated versus control counties, a 15% increase relative to the baseline mean of 0.64 married women teachers per hundred married women.

These effects were completely driven by white teachers (Columns (3) and (4), and Figure 3a), with no effects on the composition of Black teachers (Columns (5) and (6), and Figure 3b).²⁴ We interpret these findings as evidence that marriage bars were binding for white women but not Black women, as posited by Goldin [1988]. Our findings are consistent with the fact that white and Black women in this setting worked in segregated schools due to Jim Crow era laws, and the fact that Black married women were more likely to work during this time period in general (Costa [2000]).

One 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. However, Supplemental Figure A1 shows that the share of women teachers with children increased by 2.1 p.p. following the prohibitions being passed, suggesting that we can reject the null hypothesis that schools fully substituted toward discriminating against women with children.

Notably, the effect of the prohibitions on married women’s participation in teaching 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

²⁴Note that sample sizes differ across columns of Table 2 due to our sample construction discussed in Section 3.3. However, our results are not dependent on the sample: Column (3) of Supplemental Table B3 shows that our results for all teachers are robust even when using the restricted set of counties we use to study Black teachers.

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.

5.2 Mechanisms Behind Direct Effects

What are the mechanisms driving our main effect? Did the prohibitions pull married women into the labor force (an ‘extensive margin’ effect), or did the prohibitions only affect women who would have worked even absent the policy (an ‘intensive margin’ effect), by either inducing unmarried teachers to marry or by inducing married workers to switch to teaching?

We answer this question using our linked Census data, which allows us to trace out individual transitions into marriage and employment for three groups of white women, grouped by their marital and employment status in year $t - 10$: (1) unmarried women teachers, (2) married women not in the labor force, and (3) unmarried women not in the labor force.²⁵ Results from estimating Equation (2) for each group are shown in Table 3.

We start by looking at the effects for women who were unmarried and teaching in 1930 in Panel 1. Column (2) shows that the prohibitions led to a 2.2 p.p. increase in the likelihood that unmarried women teachers got married and continued teaching ten years later—an economically large effect, given that only 4.5% of unmarried women teachers in our linked sample 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. The prohibitions did not increase the marriage rate for unmarried women teachers (Column (1)). Instead, unmarried women teachers responded to the prohibitions by getting married “as planned” but *keeping their jobs*: their propensity to exit the labor force fell by 3.0 p.p. (Column (4)), with no change in their likelihood of working outside of teaching (Column (3)). The prohibitions therefore had an *extensive margin* effect on the LFP of unmarried women teachers by keeping them in the labor force when they would have

²⁵Given our findings that the marriage bars had no direct effects on Black teachers, we focus on white teachers in our analysis of mechanisms. In addition, in Supplemental Table B4, we examine outcomes for other working women. We restrict our linked samples of unmarried women to be between the ages of 8 and 40 (16-40 for teachers) in $t - 10$, and we restrict our linked samples of married women to be between the ages of 18 and 50 in $t - 10$, to focus on the populations for whom the decisions to marry and work were the most relevant. Our results are robust to alternate cutoffs.

left after marriage absent the prohibitions.²⁶

Next we examine the effects for married women who were not in the labor force prior to the prohibitions. Results are shown in Panel 2. The majority of married women in our linked sample who were not in the labor force in 1920 stayed married (93%) and out of the labor force (88%) in 1930. Yet the prohibitions led to a 0.06 p.p. increase in the likelihood that married women who were previously not in the labor force became teachers (Column (2))—a 33% increase relative to the small baseline share of white married women who entered teaching after marriage—and led women to be less likely to stay out of the labor force (Column (4)).²⁷

Finally, we examine the effects on unmarried women who were not in the labor force prior to the prohibitions. Panel 3 shows that these women became more 0.13 p.p more likely to get married and enter teaching following the prohibitions (a 30% increase, shown in Column (2)), as opposed to getting married and staying out of the labor force (Column (4)). While our lack of intercensal data prevents us from disentangling the explicit mechanisms underlying this result, the increase could have been driven by either of the channels discussed above: by women becoming teachers and then staying in teaching post-marriage, or by entering teaching for the first time after marriage.

Interestingly, Column (3) seems to suggest that the prohibitions weakly increased the likelihood that married women worked in non-teaching jobs as well. The magnitude of the effect is small relative to the other estimates (a 10 to 12% increase), and could be indicative of spillover effects of the prohibitions in teaching to other occupations. However, another interpretation could be that married women’s LFP was rising faster in treated states than in control states in all occupations, which would violate the parallel trends assumption. We find evidence that this is not a concern, as the estimate is not robust to controlling for unemployment, excluding counties affected by the Tennessee Valley Authority (TVA), or

²⁶One 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 ability to measure small delays in childbirth is hampered by only having decennial Census data, Column (1) of Supplemental Table B5 shows that the prohibitions had no significant effect on the likelihood that unmarried women teachers in $t - 10$ had children by t .

²⁷Note that the magnitude of the decrease in Column (4) is larger than the magnitude of the increase in Column (2). This is to offset the increase in Column (3) which we discuss below.

restricting to only counties bordering neighboring states.²⁸

Our results suggest that the increase in married women in teaching was driven entirely by changes in extensive margin labor supply, both by increasing *retention* of incumbent teachers who would otherwise have exited the labor force, and by increasing *hiring* of women who would have otherwise remained out of the labor force. We find no evidence of negative effects on the LFP of women who were already in the labor force or who would have entered the labor force even absent the prohibitions.²⁹

Furthermore, our estimates suggest that new hiring contributed more to the increase in married women teachers than the retention of incumbent teachers did. To see this, we scale the estimated effects in Table 3 by the total number of treated white women in each group in 1930. Although the effect size in percentage points is largest for unmarried women teachers (2.2 p.p.), there were fewer than 8,000 such women in our treated linked sample in 1930, suggesting the prohibitions led to the retention of an additional 168 married women teachers. In contrast, there were more than 434,000 married women outside of the labor force in our treated linked sample in 1930, meaning our effect size of 0.06 p.p. translates to an increase of 258 married women teachers due to new hiring. We conclude that roughly 60% of the overall increase in married women teachers was due to the hiring of new married women.³⁰

5.3 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 with no effects

²⁸See Columns (4), (5), and (6) respectively, in Supplemental Appendix Table B6.

²⁹See Supplemental Table B4 for the estimated null effects on married and unmarried non-teachers in the labor force in $t - 10$.

³⁰While women who were unmarried and outside the labor force in $t - 10$ also contributed to the increase, recall that we cannot disentangle what shares of these women became married women teachers through hiring or retention channels. However, we can apply a similar argument: of the unmarried women out of the labor force in $t - 10$, a much larger number will get married by t than become teachers by t ; hence, if the percentage point effect sizes are similar in magnitude, then the effect of the prohibitions on unmarried women outside of labor force will likely be driven by hiring effects.

on men and single women teachers. We are able to conclusively rule out this possibility in Table 4: Column (4) (and Supplemental Figure A2) shows that the prohibitions had no effect on the total number of white teachers per county. It must therefore 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 all teachers are shown in Figure 2, with results split by race in Figure 3 and for white teachers only in Table 4. Consistent with our earlier finding that marriage bars were not binding for Black married women, Figure 3b shows that the prohibitions had no effect on Black men or Black unmarried women teachers, either. For white teachers, however, Figure 3a and Table 4 show 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, with no effect on the share of teachers who were men. We interpret the maintained ratio of men-to-women white 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 woman to uphold to the norm that men needed jobs to provide for their families.

What happened to the white unmarried women who were pushed out of teaching following the prohibitions? Were they pushed to 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 these women became no more likely to marry due to the prohibitions, 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 likelihood that unmarried women teachers in $t - 10$ remained unmarried (Column 1) and either 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 likelihood that unmarried women teachers remain unmarried women teachers. However, we also find that the prohibitions significantly increased the likelihood of unmarried women teachers switching occupations by 1.9 p.p. (22%), with no effect on the likelihood of leaving the labor force. These estimates suggest that the prohibitions pushed some unmarried women to other occupations, but not out of the labor force all together.

These displaced unmarried women teachers were not pushed to lower-paying jobs, but we find some evidence that they may have been pushed to move. Supplemental Table B5 examines the effect of the prohibitions on the *occupational scores* (Columns 2 and 3) and mobility (Columns 4 and 5) of unmarried women teachers, where occupational scores function as a proxy for earnings given income was not collected in the Census prior to 1940. Although the prohibitions increased occupational scores on average across all incumbent unmarried teachers (Column 2), we find the increase is entirely driven by those in treated states who got married and kept their jobs (Column 3), suggesting that unmarried teachers who switched to other occupations were not paid significantly less than they would have been as teachers. Similarly, although the prohibitions increased movement out of state on average across all incumbent unmarried women teachers (Column 4), we find that this difference is almost entirely driven by those who remained unmarried (Column 5). These results are consistent with incumbent teachers (in particular unmarried women, who experienced no positive retention effects from the prohibitions) pursuing other jobs or wanting to teach in states that were unaffected by the prohibitions.

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 B7 shows that the prohibitions had no effect on the likelihood of unmarried women entering teaching and stayed unmarried.

Taken together, our results suggest that the prohibitions not only increased women's LFP overall, but also weakly increased women's earnings.

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 four 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, conducting a state-level analysis using synthetic difference-in-differences, and including controls for factors that may have varied by county and year. We also discuss the relationship between our results and the Great Depression and New Deal.

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 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 Groups. 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. However, one might be concerned that the neighboring Southern states do not offer the closest comparison possible to the treated states.

We address this concern in Supplemental Appendix C 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.³¹ 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.

State-Level Analysis. Our preferred specifications use county- or individual-level outcomes for the 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 D. The magnitudes and significance levels of our estimates remain similar. 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 A4.

Additional Controls. Our main specifications include year and county fixed effects without additional controls. However, particularly due to the Great Depression, there were also county-level time-varying factors that are not accounted for by county fixed effects. To address this, Column (4) of Supplemental Table B3 shows the effect of the prohibitions on the share of teachers that are married women including controls for county-level unemployment rates (to account for the effects of the Great Depression), shares of workers in manufacturing and in agriculture (to account for possible changes in demand for education), and log county population and restricts the sample to only 1930 and 1940 (when data on unemployment is available). We find that our main results do not change with the inclusion of these controls.

³¹Border counties are those in treated states which border a non-treated state, and those in non-treated states which border a treated state. See Supplemental Figure D1 for a map of the border counties.

The Great Depression and New Deal. Finally, we discuss the robustness of our results to the impacts of the Great Depression and New Deal. Our alternate control group designs allow us to address the potential bias caused by treated states being more severely impacted overall by the Great Depression than control states. In our border counties design, we compare counties that share similar industry composition and other unobservable characteristics before 1933, and hence are more likely to experience similar effects of the Great Depression and more similar New Deal support. In our matched counties design, we match counties on retail sales per capita in 1929 and growth over time. We also examine how our results are affected by including controls for county unemployment rates and industry composition. Finally, in Column (5) of Supplemental Table B6, we present our results excluding counties in the TVA. As discussed above, our results are robust to all these specifications, suggesting that any biases caused by the Great Depression are minimal.

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 driven by white women who changed their decisions to work rather than their decisions 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 evidence that the decrease was driven by incumbent unmarried

women teachers being pushed out of teaching to other similar-paying occupations. Overall, our findings suggest that while the policy did displace some unmarried women, the net effect on women’s LFP 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

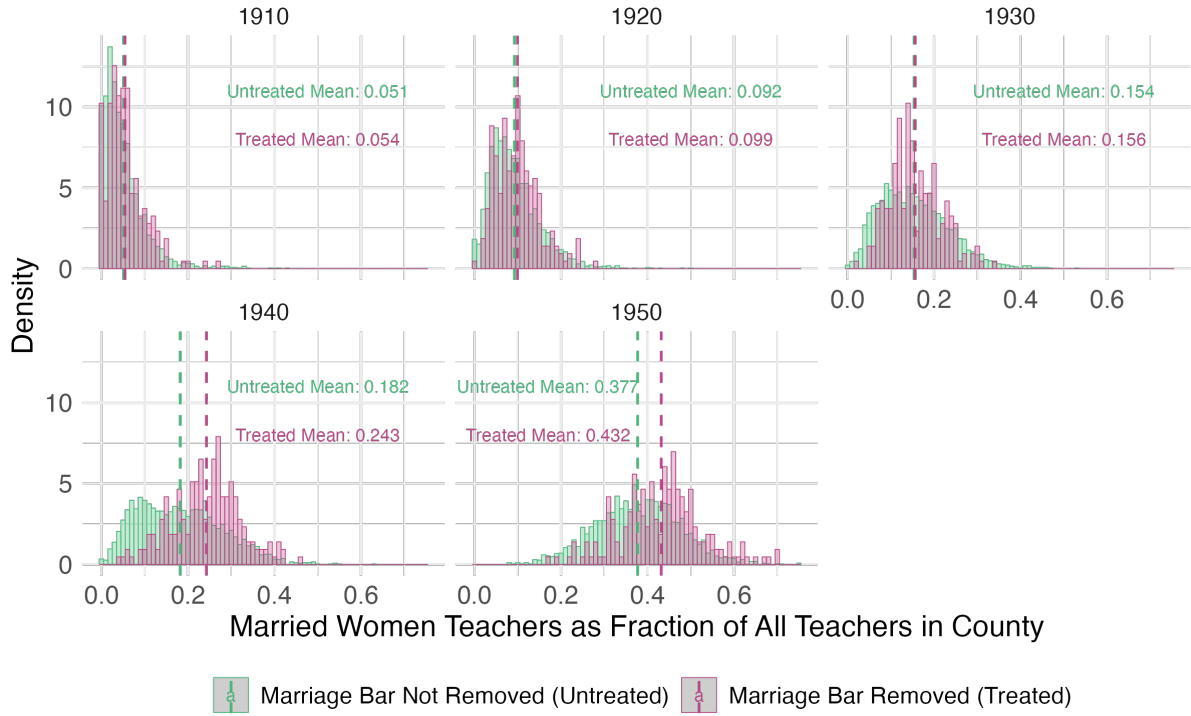


Figure 1: Density plots from 1910 to 1950 of the county-level fraction of white teachers who are married women. Separate distributions are shown for (1) counties in states where marriage bars were prohibited in teaching in the 1930s (KY, NC), and (2) all other counties in the country. Vertical dashed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

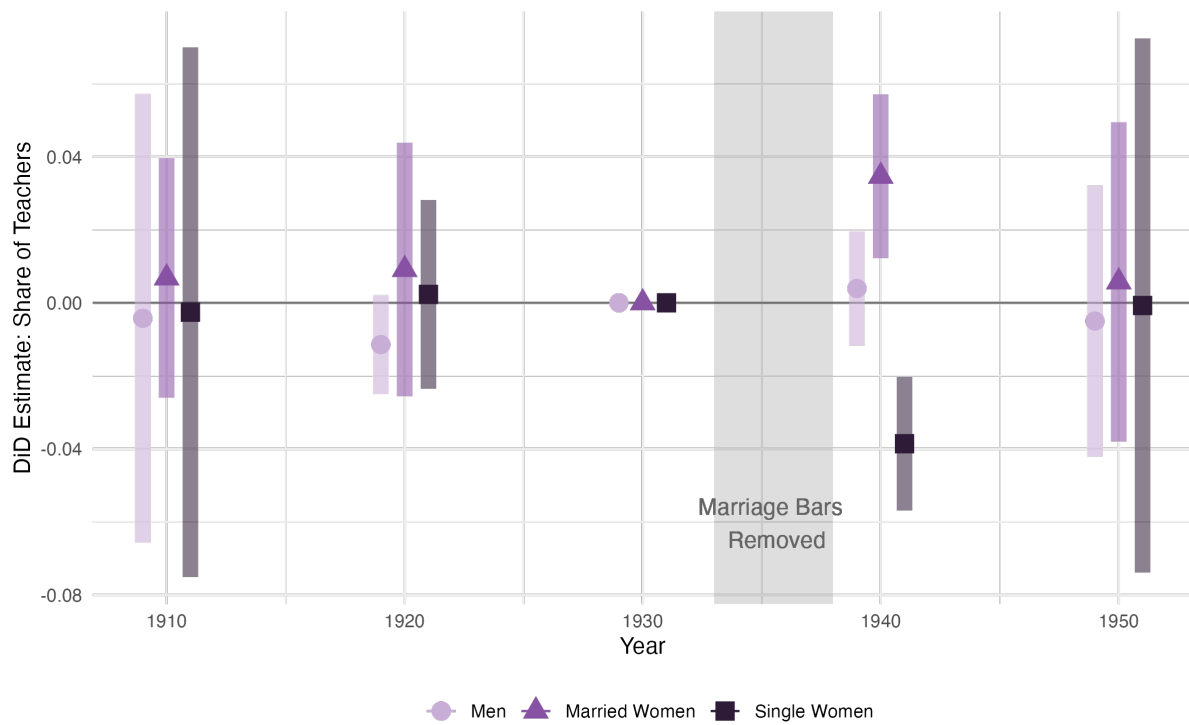
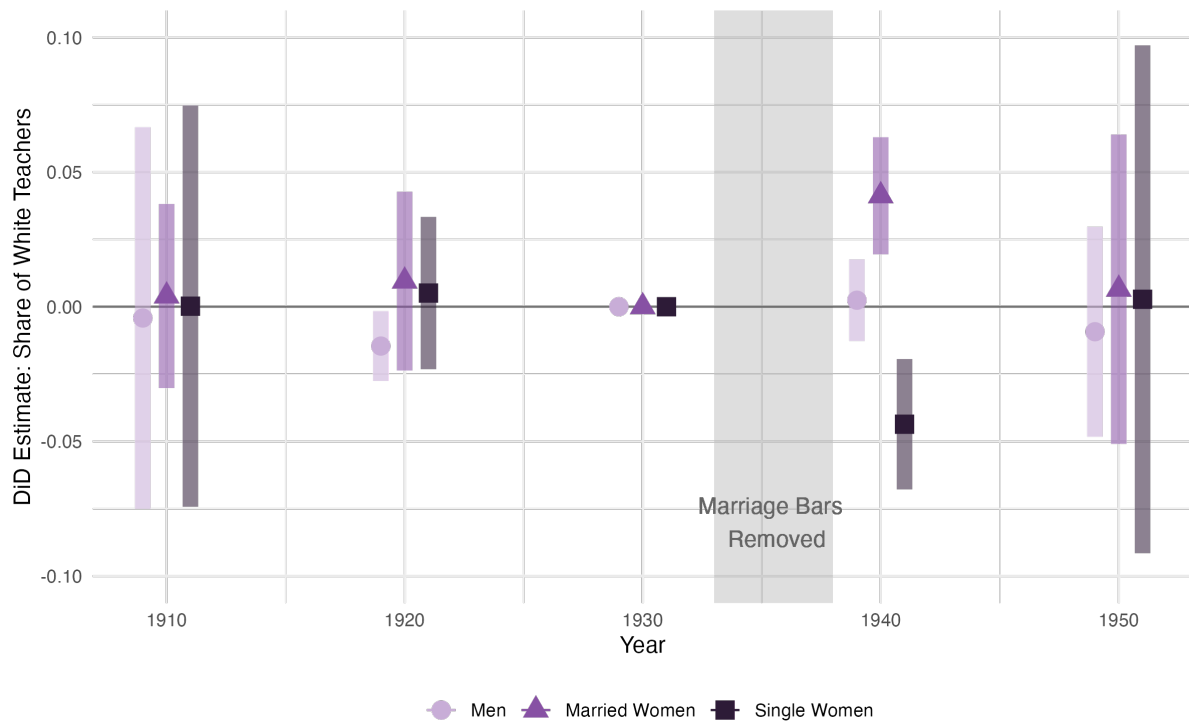
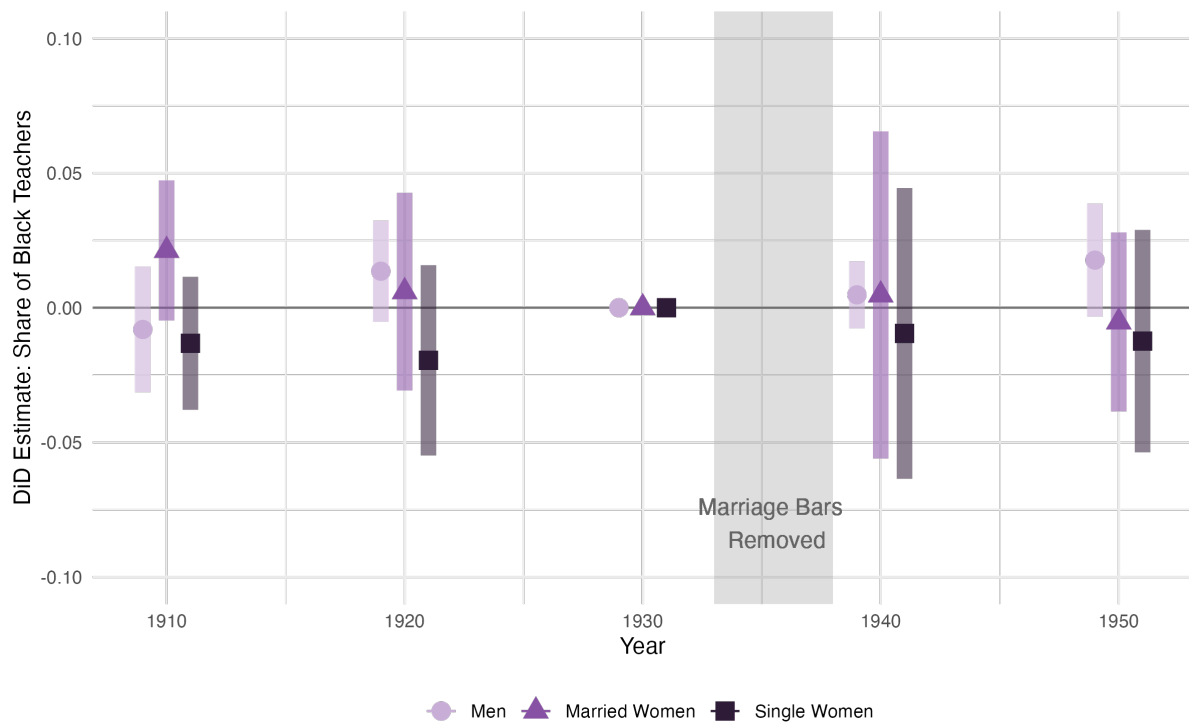


Figure 2: Estimated effects of the prohibition of marriage bars in teaching on the *gender composition* of all teachers, at the county level. Sample includes KY, NC, and neighboring Southern states.



(a) Effects on composition of white teachers.



(b) Effects on composition of Black teachers.

Figure 3: Estimated effects of the prohibition of marriage bars in teaching on the gender composition of (a) white and (b) Black teachers at the county level.

8 Tables

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

	All	South	Treated	Neighb. Sth.	p-value ((3) & (4))
	(1)	(2)	(3)	(4)	(5)
Panel A: General County Statistics					
Population (Thous.)	39.61 (2.427)	26.69 (1.322)	26.3 (2.061)	26.59 (1.736)	0.912
School-Age Pop. (Thous.)	10.15 (0.541)	7.92 (0.32)	7.998 (0.516)	8.012 (0.447)	0.984
Share Urban	0.214 (0.005)	0.155 (0.008)	0.134 (0.013)	0.173 (0.016)	0.059*
LFP of Married Women	0.102 (0.001)	0.136 (0.003)	0.092 (0.004)	0.118 (0.005)	0.000***
LFP of White Married Women	0.081 (0.001)	0.082 (0.002)	0.067 (0.003)	0.079 (0.003)	0.011**
Num. Children of Marr. Wom.	2.036 (0.007)	2.247 (0.012)	2.355 (0.029)	2.308 (0.02)	0.181
Unemployment Rate	0.054 (0.001)	0.042 (0.001)	0.042 (0.002)	0.047 (0.002)	0.062*
Panel B: County Statistics on White Teachers					
Students/Teachers	33.85 (0.26)	46.37 (0.466)	47.3 (0.934)	42.14 (0.723)	0.000***
Share Men	0.196 (0.002)	0.191 (0.003)	0.219 (0.006)	0.206 (0.006)	0.136
Share Single Women	0.636 (0.002)	0.615 (0.003)	0.604 (0.007)	0.62 (0.006)	0.075*
Share Married Women	0.168 (0.002)	0.194 (0.002)	0.177 (0.004)	0.174 (0.004)	0.561
<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. Column (5) presents p-values from *t*-tests for differences in means between the Treated and Neighboring South counties.

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

Dependent Variable:	All teachers			White teachers only			Black teachers only		
	Share Teach Mar. Wom.	MW Teach per 100 MW		Share Teach Mar. Wom.	MW Teach per 100 MW		Share Teach Mar. Wom.	MW Teach per 100 MW	
	(1)	(2)		(3)	(4)		(5)	(6)	
Treated \times 1940 (γ_{1940}^{DD})	0.035*** (0.007)	0.095*** (0.031)		0.041*** (0.008)	0.122*** (0.035)		0.005 (0.018)	0.068 (0.079)	
Treated \times 1950 (γ_{1950}^{DD})	0.006 (0.009)	-0.083** (0.034)		0.007 (0.010)	-0.074* (0.038)		-0.005 (0.021)	-0.121 (0.095)	
Dep. Var. 1930 Treated Mean	0.1775	0.6442		0.1557	0.6055		0.3168	1.192	
Observations	2,640	2,640		2,635	2,635		1,445	1,445	
Adjusted R ²	0.839	0.709		0.840	0.680		0.386	0.555	

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 100. 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 either teach, work outside of teaching, or 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})	-0.0107 (0.0137)	0.0217*** (0.0058)	-0.0023 (0.0034)	-0.0302** (0.0139)
Dep. Var. 1930 Mean	0.5891	0.0453	0.0347	0.5092
Observations	59,542	59,542	59,542	59,542
Adjusted R ²	0.05822	0.01861	0.00315	0.06448
Sample 2: Women who were married and not in the labor force in $t - 10$				
Treated \times 1940 (γ_{1940}^{DD})	-0.0003 (0.0010)	0.0006*** (0.0002)	0.0065* (0.0035)	-0.0073** (0.0034)
Dep. Var. 1930 Treated Mean	0.9331	0.0018	0.0534	0.8779
Observations	3,125,563	3,125,563	3,125,563	3,125,563
Adjusted R ²	0.00244	0.00058	0.01488	0.01246
Sample 3: Women who were unmarried and not in the labor force in $t - 10$				
Treated \times 1940 (γ_{1940}^{DD})	-0.0015 (0.0038)	0.0013*** (0.0003)	0.0044** (0.0021)	-0.0072* (0.0037)
Dep. Var. 1930 Treated Mean	0.5076	0.0044	0.0433	0.4600
Observations	2,217,852	2,217,852	2,217,852	2,217,852
Adjusted R ²	0.02200	0.00093	0.01319	0.03141

Notes: Estimation follows Equation (2). To construct our estimation samples, we identify women in treated states (KY, NC) and neighboring Southern states (VA, SC, TN, WV) in 1920, 1930, and 1940 whom we are able to link over consecutive Census years (i.e. between 1910 and 1920, 1920 and 1930, or between 1930 and 1940) using the Census Tree linkages. From these women, we construct three samples: Sample 1, containing linked women who were aged 16-40, white, unmarried, and teaching in 1910, 1920, and 1930; Sample 2, containing linked women who were aged 18-50, white, married, and not in the labor force in 1910, 1920, and 1930; and Sample 3, containing linked women who were aged 8-40, white, unmarried, and not in the labor force in 1910, 1920, and 1930. All regressions are at the individual level and use the 1910-1920, 1920-1930, and 1930-1940 linked full-count Census samples. All regressions included county fixed effects and are clustered at the county level. 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 Mar. Wom.	% Teach Men	% Teach Unmar. Wom.	# Teachers
	(1)	(2)	(3)	(4)
Treated \times 1940 (γ_{1940}^{DD})	0.041*** (0.008)	0.002 (0.006)	-0.044*** (0.009)	-0.916 (3.941)
Treated \times 1950 (γ_{1950}^{DD})	0.007 (0.010)	-0.009 (0.008)	0.003 (0.011)	-2.872 (5.166)
Dep. Var. 1930 Treated Mean	0.1775	0.2185	0.604	188.2
Observations	2,635	2,635	2,635	2,635
Adjusted R ²	0.840	0.680	0.827	0.842

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 either teach, work outside of teaching, or 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})	0.0107 (0.0137)	-0.0158 (0.0127)	0.0188*** (0.0064)	0.0077 (0.0064)
Dep. Var. 1930 Treated Mean	0.4244	0.2520	0.0907	0.0817
Observations (Thous.)	59,542	59,542	59,542	59,542
Adjusted R ²	0.05822	0.07253	0.00727	0.00934

Notes: Estimation follows equation 1. The sample definition is provided in the table notes for Table 3: see construction of Sample 1. All regressions use 1910-1920, 1920-1930 and 1930-1940 linked full-count Census samples.

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