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

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A Additional Figures

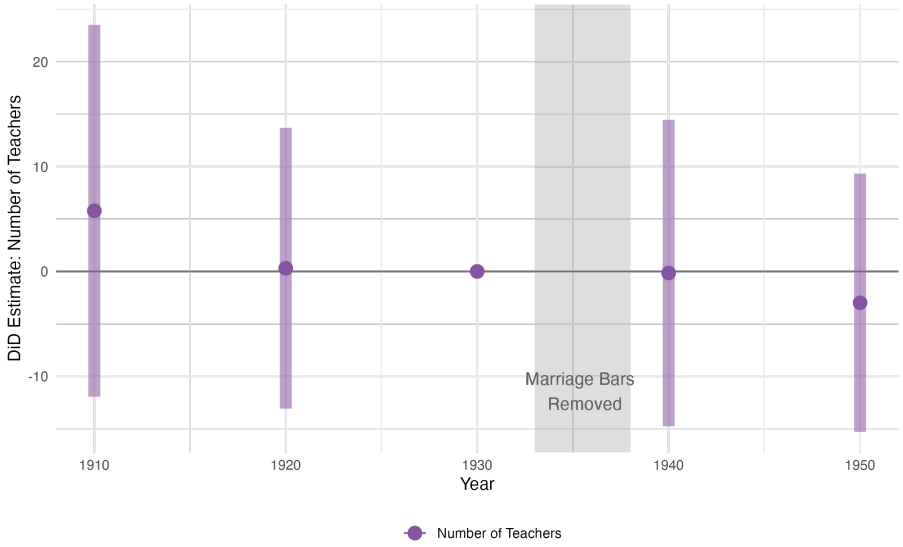


Figure A1: Estimated effects of the introduction of employment protections for married women on the total number of teachers per county.

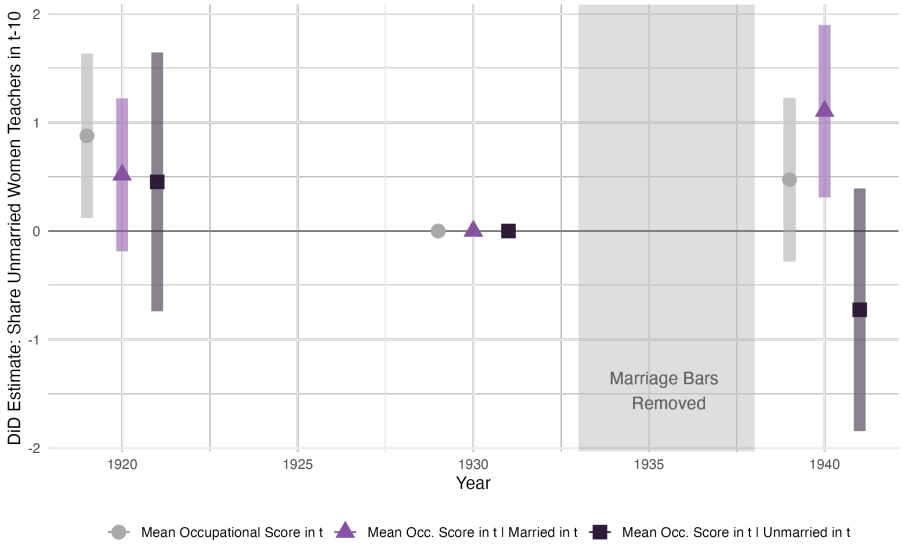


Figure A2: Estimated effects of the introduction of employment protections for married women in teaching on the occupational score of incumbent unmarried teachers. Analysis uses linked sample of unmarried women teachers in $t-10$ and measures mean occupational score for the full sample, conditional on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

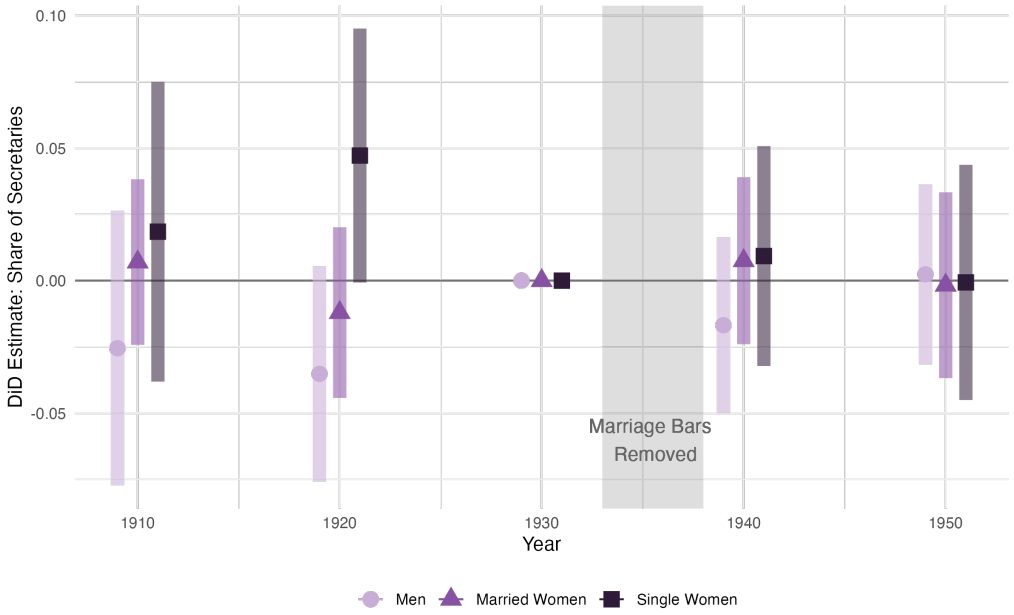


Figure A3: Placebo test: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *secretaries* who are men, unmarried women, and single women.

B Additional Tables

Table B1: Estimated effects of the prohibitions on women's propensity to get married and to teach, work outside of teaching, and exit the labor force.

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

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

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

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

Notes: See notes for Tables 3 and B1.

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:

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

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 $\hat{\gamma}_{1940}^{DD} \equiv \Delta \hat{s}_{teach,1930-1940}^{MW} = 0.9767/1000 = 0.0009767$. The baseline mean in 1930, weighted by the total number of married women in each county, is 0.005724. Therefore the numerator (representing the total contribution of the lifting of marriage bars to the increase between 1930 and 1940 in treated states in married women's likelihood of being a teacher) is 0.171.

In calculating the denominator, note that by 1940 all teachers in treated states

were covered by marriage bar prohibitions ($q^{teach}_{emp,1940} = 0$), regardless of the initial value of $q^{teach}_{emp,1930}$. 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 $\varepsilon^{teach}_{EP} = 0.171$.

C.2 Other Occupations

The key assumption in this back of the envelope calculation is that $\varepsilon^{teach}_{EP} = \varepsilon^{o \in \mathcal{O}}_{EP}$ 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 $\Delta q^{o}_{emp,1940-1950}/q^{o}_{emp,1930} = 1$ for all $o \in \mathcal{O}$. The latter assumption is strong especially as it is known that some occupations like teaching still had marriage bars (although at much lower rates) in 1950, but since marriage bars disappeared by "the 1950s" [Goldin, 1988] and our data is decennial, we take 1950 as the proximate end of marriage bar use.

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

C.3 Calculation

Under these assumptions, we can estimate the total change in white married women's LFP in these occupations between 1940 and 1950 **due to** the removal of institutional

barriers to employment (or equivalently, the removal of marriage bars) as follows:

$$\Delta s^{MW}_{MB,1940-1950} = \sum_{o \in \mathcal{O}} \varepsilon^{o}_{EP} \cdot s^{MW}_{o,1940} = \varepsilon^{teach}_{EP} \sum_{o \in \mathcal{O}} s^{MW}_{o,1940} = \varepsilon^{teach}_{EP} \cdot s^{MW}_{MB,1940} \tag{2}$$

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

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

White-Collar Occupations. The first calculation we present in the body of the paper is the estimated contribution of the removal of institutional barriers to the increase in married women's participation in *all white-collar occupations*, including professional/technical, managerial, clerical, and sales occupations.[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 $\Delta s^{MW}_{MB,1940-1950} = 0.006353$ implies that the removal of institutional barriers accounts for 13.8% of the total growth in white married women working in white-collar occupations.

College-Educated Women. The second calculation we present in the body of the paper is the estimated contribution of the removal of institutional barriers to the increase in *college-educated* married women's total LFP. The total growth in the

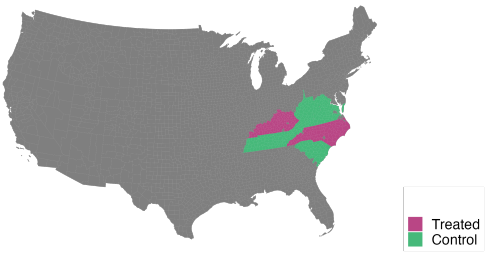
[1] Approximately 30% of the total labor force was employed in a white-collar occupation in 1940.

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

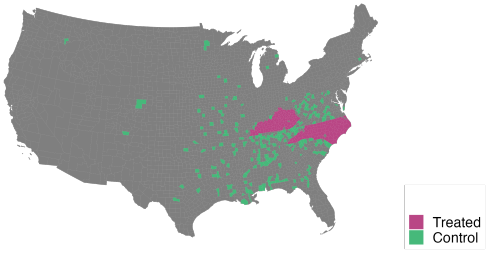
D Matched Counties Design

As discussed in Sections 3.3 and 5.4, 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 a matched counties design which does not rely on the above assumption about the similarity of counties in treated states to counties in neighboring Southern states.

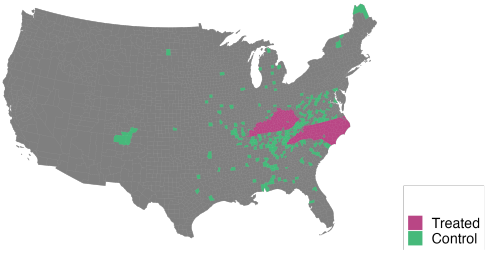
Figure D1: Maps of treated (pink) and control (green) counties for (a) our preferred control specification of neighboring Southern counties and (b)-(d) specifications using a range of matching techniques.



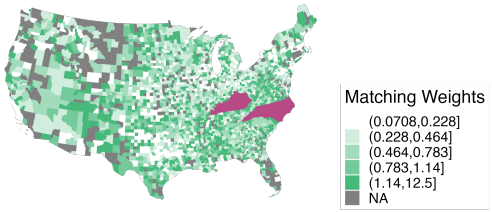
(a) Southern Neighbor States



(b) Match 1: Nearest Neighbor



(c) Match 2: Genetic



(d) Match 3: Optimal Full Matching

We match treatment and control counties using both the 1930 level and change between 1920 and 1930 of an extensive set of county-level variables, including demo-

graphics, urbanization, literacy rate, and workforce composition both for teachers and overall, all obtained from the full-count census [Ruggles et al., 2024]. We also include 1939 retail sales per capita and the growth in retail sales per capita from 1929 to 1939, as obtained from Fishback et al. [2005]. [2] To match counties we use three different methods, all utilizing the MatchIt package in R [Ho et al., 2011]. The first matched sample is constructed by nearest neighbor matching using Mahalanobis distance; the second using genetic matching as developed by Diamond and Sekhon [2013], Sekhon [2011]; the third using optimal full matching as developed by Hansen [2004]. The first two methods are 1:1 matching methods, which produce the same number of control counties as treatment counties, while optimal full matching uses all counties and assigns weights to control counties based on their similarity to treatment counties. Figure 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.

[2] 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.

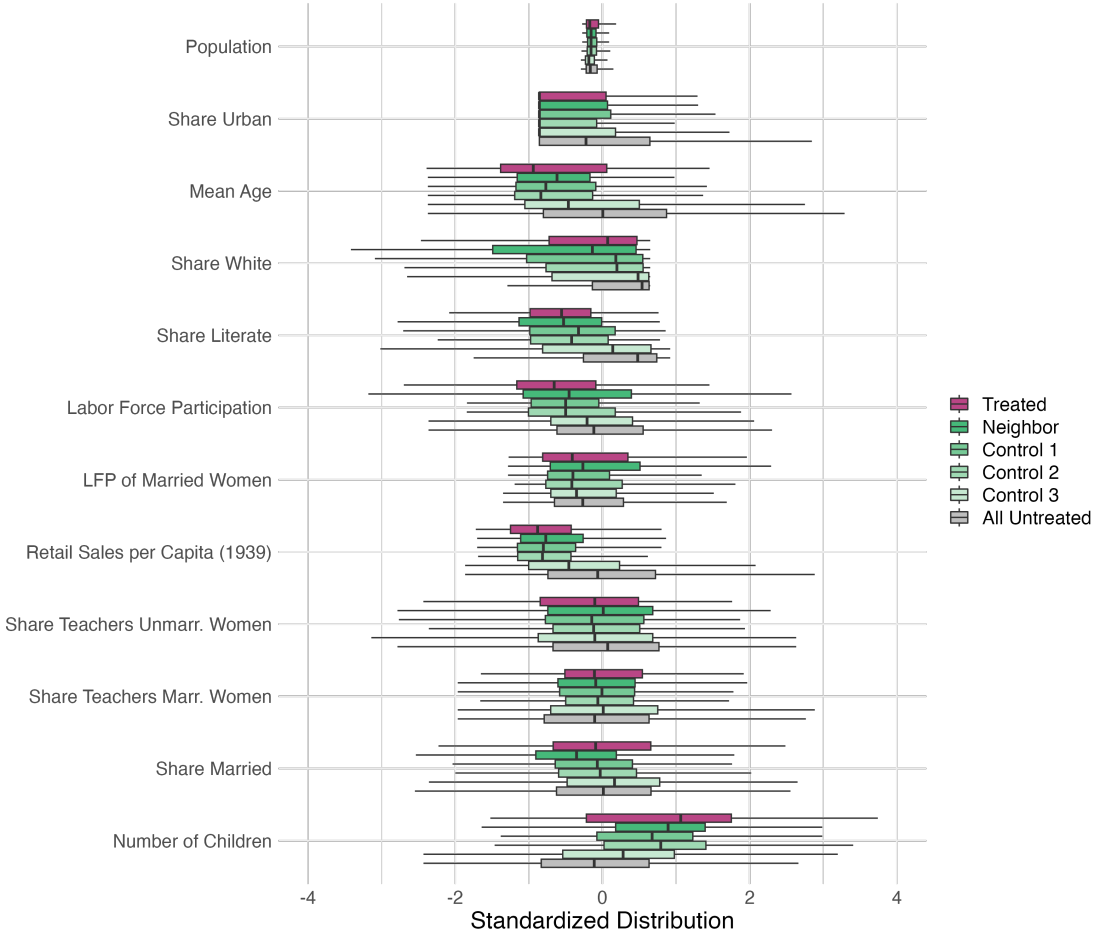


Figure D2: Boxplot of standardized 1930 values of various covariates by treatment or control group. The center bar represents the median, the edges of the box represent the 25th and 75th percentiles, and the edges of the whiskers represent extrema, with outliers removed (see R function geom_boxplot for further details). Distributions are weighted for control group 3. Covariates are outlined in detail in Footnote 2, 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 2, as well as two additional variables not used for matching (share of women married and average number of children for married women). Motivating the need to

identify an appropriate control group for KY and NC, the boxplots show that the average untreated county is quite distinct from the treated counties. Importantly, the neighoring Southern counties are very similar to the treated counties, and on some dimensions (e.g. share of teachers married women, number of children) even outperform the matched county groups in terms of similarity. While the first and second control groups are very similar in distribution to the treatment counties across nearly all covariates, the third control group is much less similar.

D.1 Results

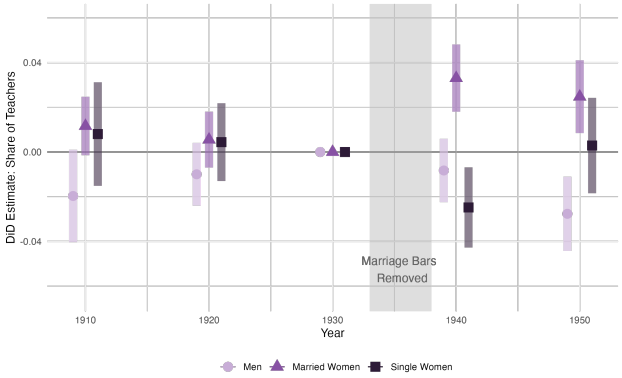
We re-estimate our key analyses using the three matched samples. We begin by estimating the effect of the marriage bar prohibitions on the composition of the teacher workforce (i.e. the share of teachers that are married women, men, and unmarried women) and present results in Figure D3. For matched samples 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 albeit attenuated. Matched sample 3, in panel (c), shows similar results, but suggests a decrease in the share of men in teaching. Recall that Figure D2 suggests that the control group for matched sample 3 is the least comparable to the treatment group. We also see, as in our primary specification, no evidence of pre-trends in the share of married women, unmarried women, or men teachers prior to 1930.

However, unlike the results from our preferred specification (Figure 2), the gap in married women's share of teachers persists to 1950. A potential reason for this continued divergence is the effect of World War II – while it is a reasonable assumption that nearby states had similar exposure to World War II, the same may not be true for counties that were similar in terms of *pre-war demographic and*

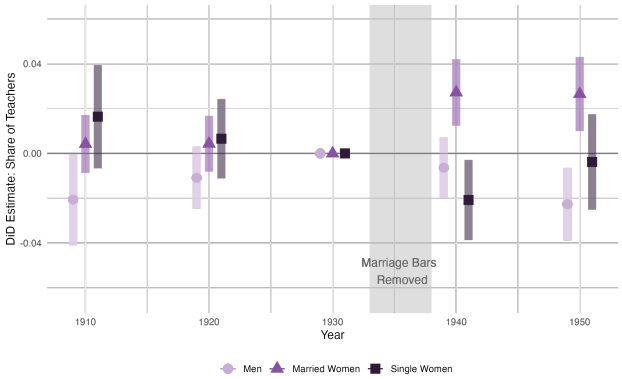
economic characteristics but not constrained to be in nearby states (as is the case for our matched samples). Higher exposure to World War II, in the form of more men being drafted, would have resulted in fewer men available to work as teachers and a substitution towards married women (the primary source of replacement labor during World War II). This difference would also explain why the gap in the share of unmarried women teachers returns to zero in 1950, and why the persistent gap in the share of married women teachers in instead offset by men.

Finally, we reproduce our final figure showing the overall effects of the marriage bar prohibitions on the labor force participation of all incumbent women teachers.[3] The results are shown in Figure D4, and are overall qualitatively similar to the results from our main specification, in Figure 3. In particular, in all three matched samples, we estimate at least weakly positive effects of the prohibitions on the likelihood of the unmarried women teachers remaining in the labor force ten years later, *particularly* if they become married.

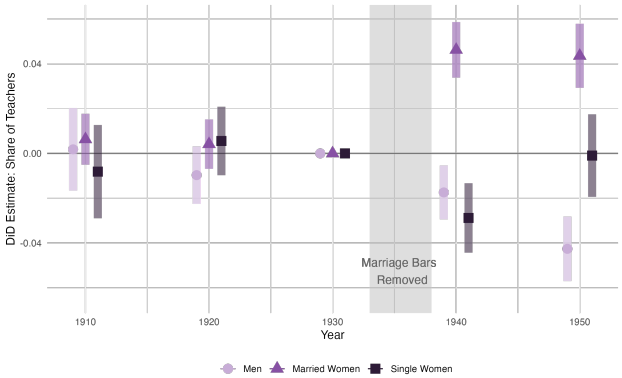
[3]For brevity we do not include the tables showing the mechanism results. The results are qualitatively similar and available upon request.



(a) Match 1: Nearest Neighbor



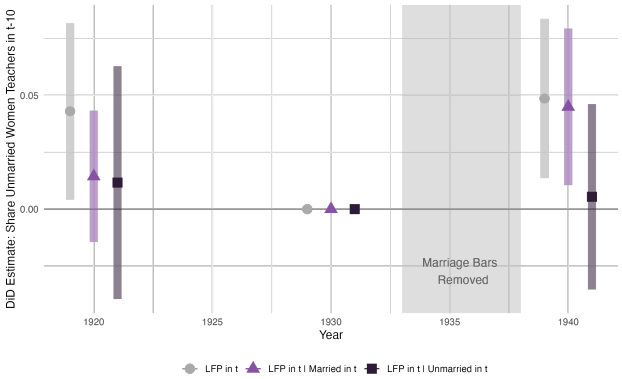
(b) Match 2: Genetic



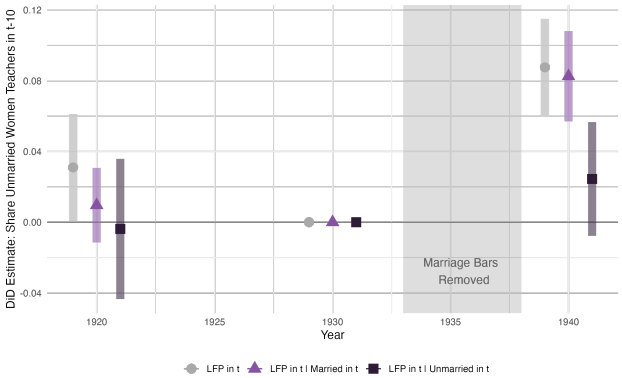
(c) Match 3: Full Optimal

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 includes KY, NC, and matched control counties, as determined by various methods. Standard errors are clustered at the county level. 95% confidence intervals are shown.

(a) Match 1: Nearest Neighbor



(b) Match 2: Genetic



(c) Match 3: Full Optimal

Figure D4: Estimated effects of the prohibition of marriage bars in teaching on the LFP of incumbent unmarried teachers, following Figure 4. The sample includes KY, NC, and matched control counties, as determined by various methods. Standard errors are clustered at the county level. 95% confidence intervals are shown.

References

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