A Additional Figures

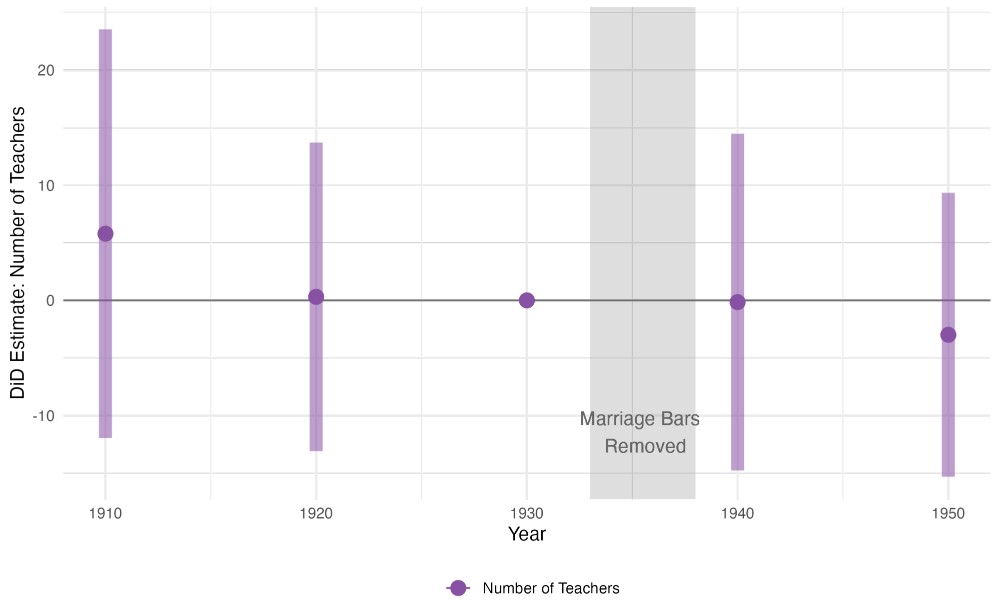


Figure A1: Estimated effects of the introduction of employment protections for married women on the total number of teachers per county.

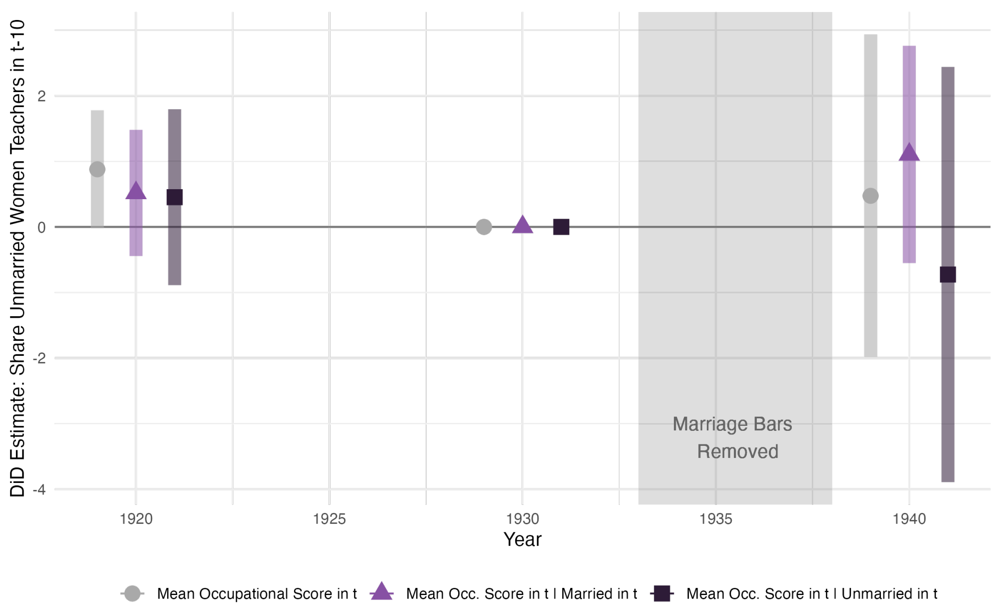


Figure A2: Estimated effects of the introduction of employment protections for married women in teaching on the occupational score of incumbent unmarried teachers. Analysis uses linked sample of unmarried women teachers in $t-10$ and measures mean occupational score for the full sample, conditional on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

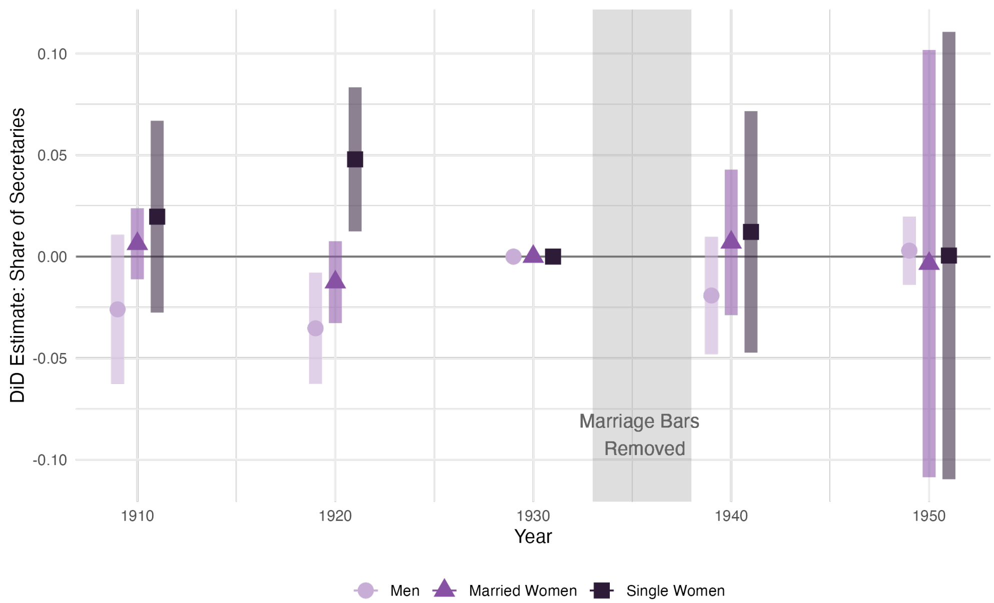


Figure A3: Placebo test: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *secretaries* who are men, unmarried women, and single women.

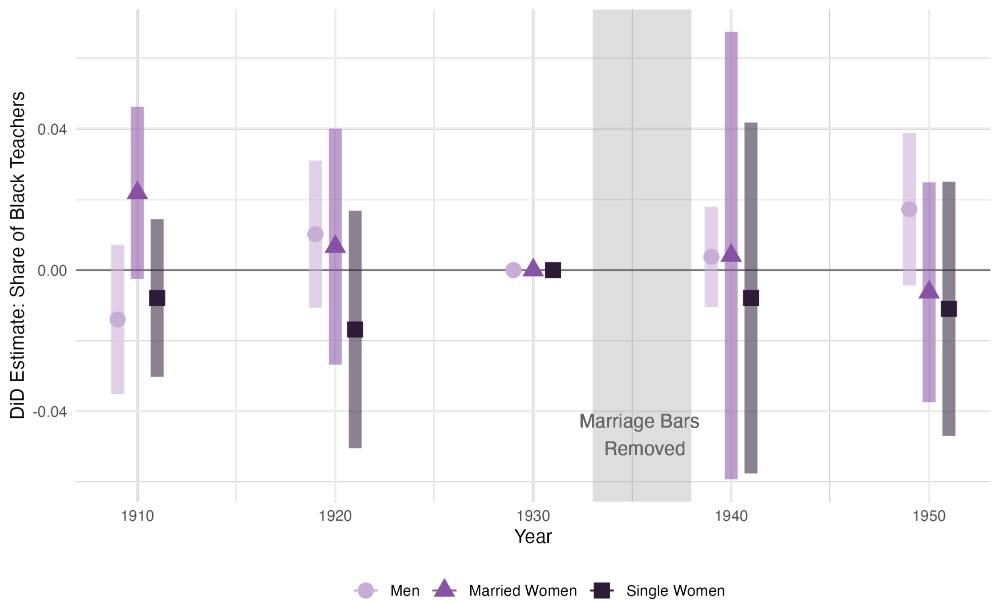


Figure A4: Heterogeneity in main results: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *Black teachers* who are men, unmarried women, and single women.

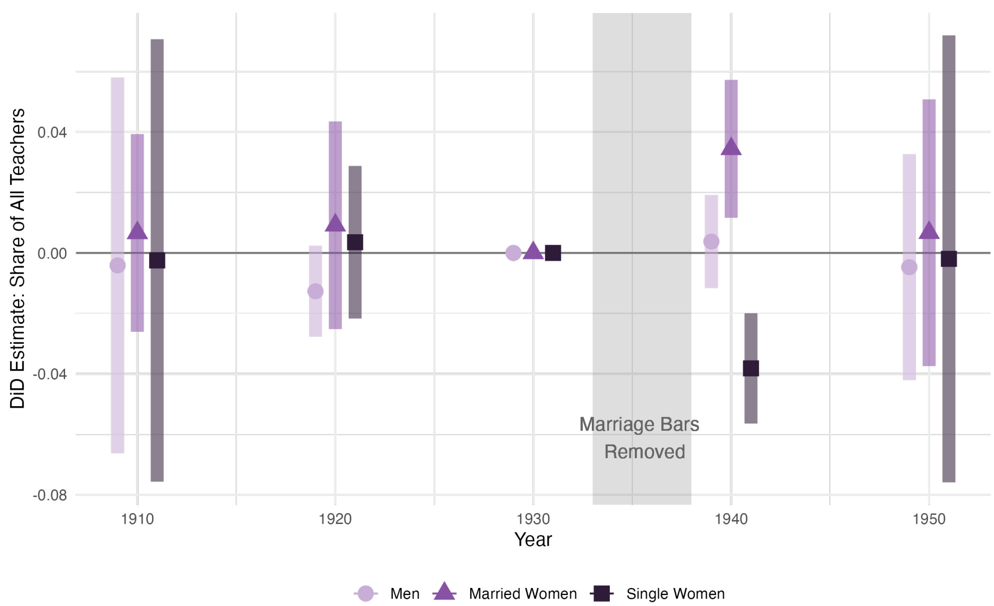


Figure A5: Heterogeneity in main results: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *all teachers* (white and Black) who are men, unmarried women, and single women.

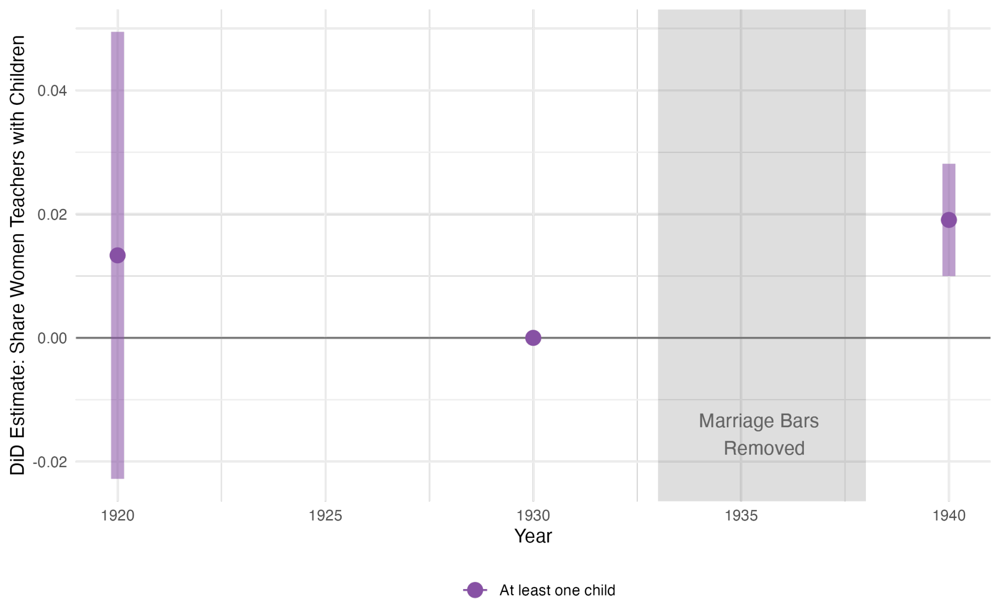


Figure A6: Results on fertility: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *white women teachers* with children.

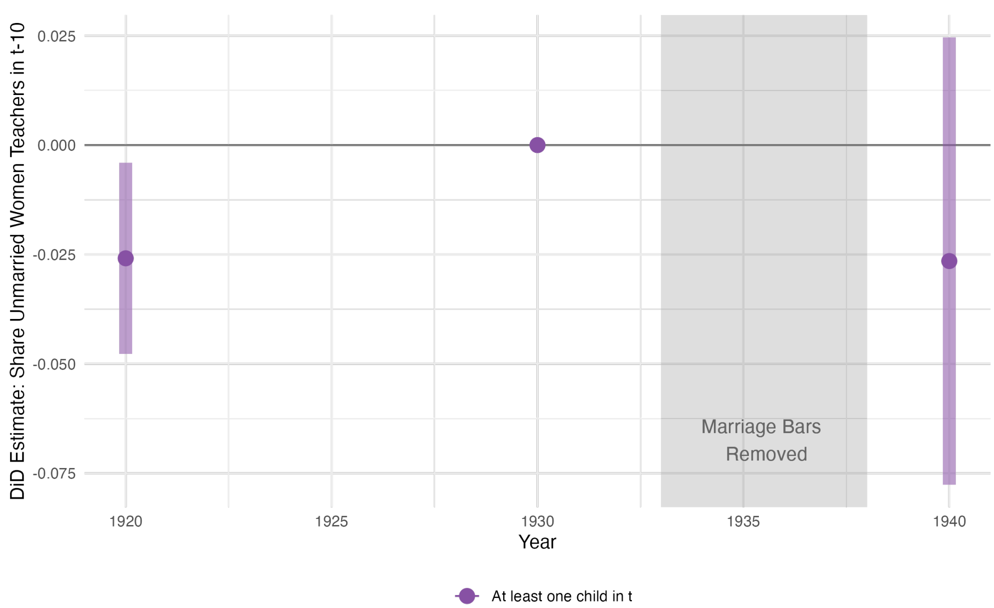


Figure A7: Results on fertility: Estimated effects of the introduction of employment protections for married women in teaching on likelihood of having children in the future among unmarried women teachers. Analysis uses linked sample of unmarried women teachers in $t - 10$ and uses as an outcome the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

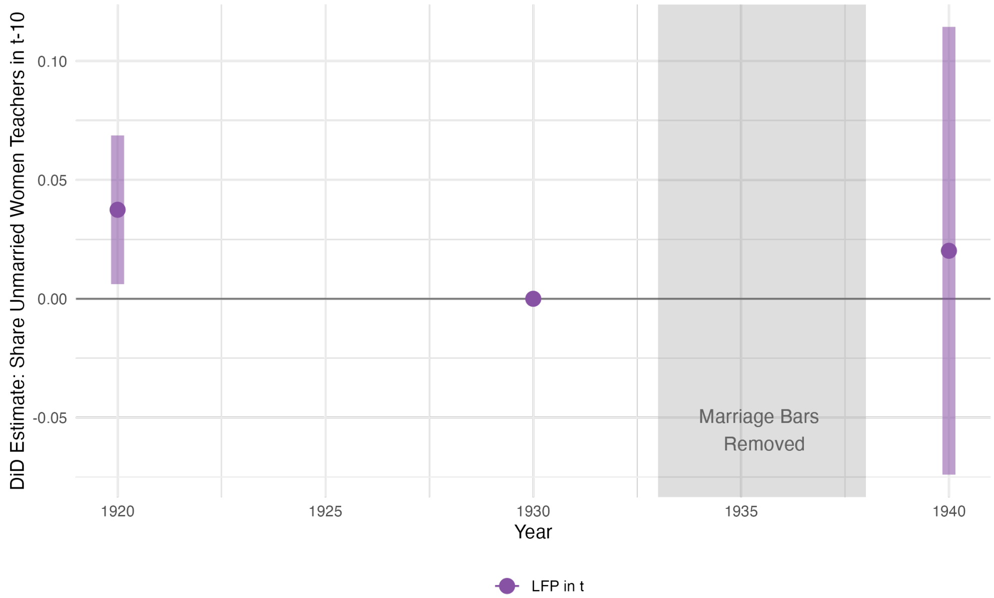


Figure A8: Overall effects on incumbent women teachers' LFP: Estimated effects of the introduction of employment protections for married women in teaching on the LFP of women who were incumbent unmarried teachers prior to the prohibitions.

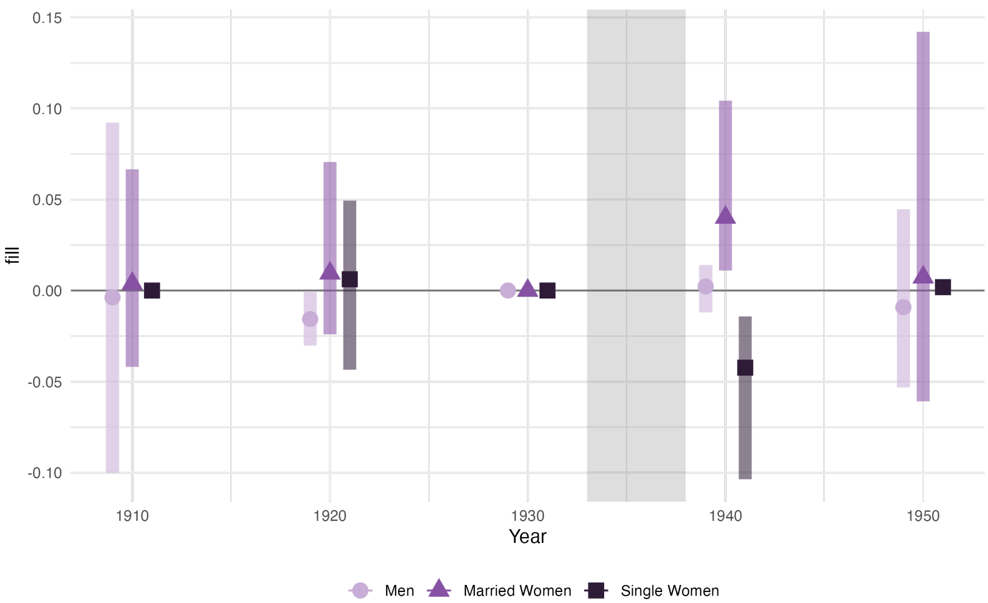


Figure A9: Main results, with state-level clustering of standard errors using wild cluster boostrapping Fischer and Roodman [2021]: Estimated effects of the introduction of employment protections for married women in teaching on the county shares of *teachers* who are men, unmarried women, and single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

B Additional Tables

Table B1: Census linkage rates by year and group.

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

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

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

Dependent Variable:	Pr(Married in t)	Pr(Teacher $\mid$ Married in t)	Pr(Non-Teacher in LF $\mid$ Married in t)	Pr(Not in LF $\mid$ 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 B3: Estimated effects of the prohibitions on women's propensity to remain unmarried and to teach, work outside of teaching, and exit the labor force.

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 B2.

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

C.1 Elasticity Calculation

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

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

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 ($q_{emp,1940}^{teach} = 0$), regardless of the initial value of $q_{emp,1930}^{teach}$. The denominator of equation (1) is thus equal to 1.

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

C.2 Other Occupations

The key assumption in this back of the envelope calculation is that $\varepsilon_{EP}^{teach} = \varepsilon_{EP}^{o\in\mathcal{O}}$ for all occupations $o \in \mathcal{O}$ subject to marriage bars: that is, that the change in married women's employment in teaching due to the prohibition of marriage bars in teaching is equivalent to the change in married women's employment in any occupation due to the elimination of discriminatory hiring practices in that occupation. We also assume that for all occupations subject to marriage bars, no married women were subject to discriminatory hiring practices by 1950, i.e. that $\Delta q^o_{emp,1940-1950}/q^o_{emp,1930} = 1$ 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 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.

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

D Matched and Border Counties Designs

As discussed in Sections 3.3 and 5.4, our preferred specification relies on the assumption that in the absence of the laws passed in North Carolina and Kentucky, the composition of the teaching workforce would have evolved similarly in the treated states and neighboring southern states of South Carolina, Tennessee, Virginia, and West Virginia. To test whether our results are robust to alternative specifications, we employ two alternate empirical designs. The first is a border county design which narrows the sample of counties to treatment and control counties that are geographically adjacent and therefore more likely to be similar and satisfy the parallel trends assumption. The second is a matched counties design which uses data on counties across the country and does not rely on the assumption that geographically close counties are similar.

D.1 Border Counties Design

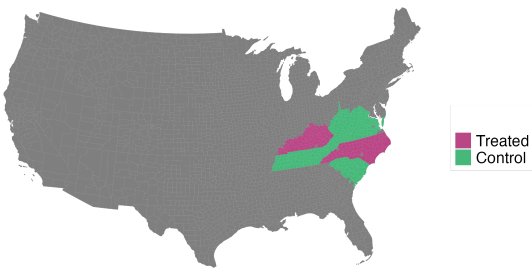
The reasoning underlying the border counties design is that counties in different states that border each other are highly likely to have followed similar historical trends, such that the only difference between counties on opposite sides of a border is their exposure to their states' state-wide prohibitions.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

D.2 Matched Counties Design

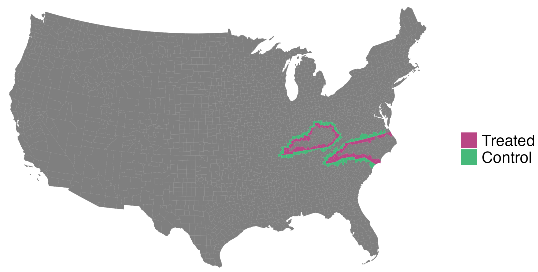
We match treatment and control counties using both the 1930 level and change between 1920 and 1930 of an extensive set of county-level variables, including demographics, urbanization, literacy rate, and workforce composition both for teachers and overall, all obtained from the full-count census [Ruggles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

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

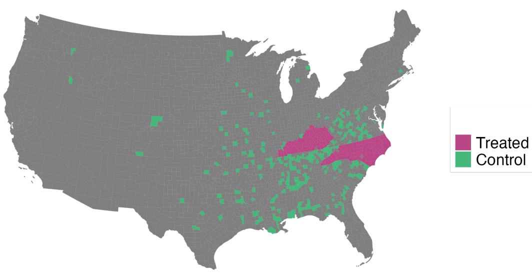
Figure D1: Maps of treated (pink) and control (green) counties for (a) our preferred control specification of neighboring Southern counties, (b) our border county specification and (c)-(e) specifications using a range of matching techniques.



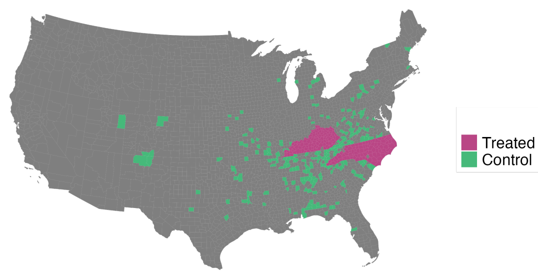
(a) Southern Neighbor States



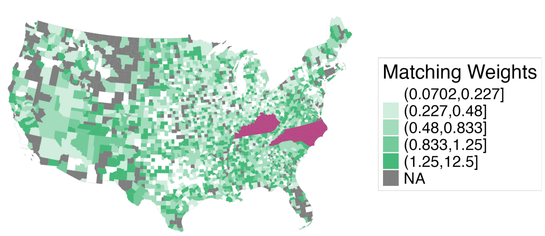
(b) Border Counties



(c) Match 1: Nearest Neighbor



(d) Match 2: Genetic



(e) Match 3: Optimal Full Matching

growth in retail sales per capita from 1929 to 1939, as obtained from Fishback et al. [2005]. [3] To match counties we use three different methods, all utilizing the MatchIt package in R [Ho et al., 2011]. The first matched sample is constructed by nearest neighbor matching using Mahalanobis distance; the second using genetic matching as developed by Diamond and Sekhon [2013] and Sekhon [2011]; the third using optimal full matching as developed by Hansen [2004]. The first two methods are 1:1 matching methods, which produce the same number of control counties as treatment counties. The third method, optimal full matching, uses all counties and assigns weights to control counties based on their similarity to treatment counties. Figure D1 compares the control counties selected by the various matching methods to the neighboring southern states in our preferred specification. Matched samples 1 and 2 are geographically concentrated in the neighboring Southern states, reinforcing the fact that the neighboring Southern counties are indeed similar to our treated counties. Panel (d) of Figure D1 maps the weights of the control counties as determined by the optimal full matching method, which are not as closely concentrated in the neighboring states as with the other matching methods.

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

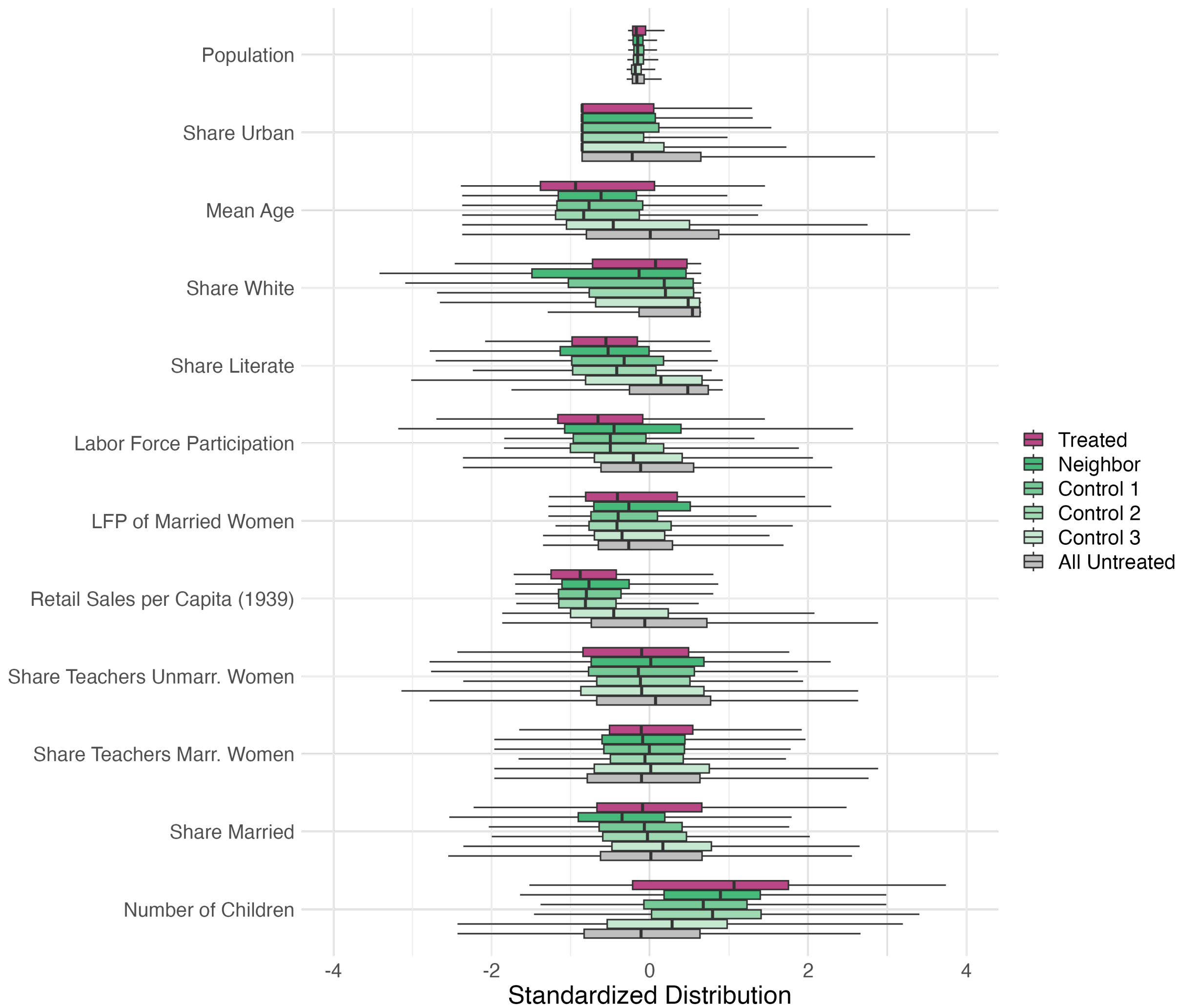


Figure D2: Boxplot of standardized 1930 values of various covariates by treatment or control group. The center bar represents the median, the edges of the box represent the 25th and 75th percentiles, and the edges of the whiskers represent extrema, with outliers removed (see R function geom_boxplot for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

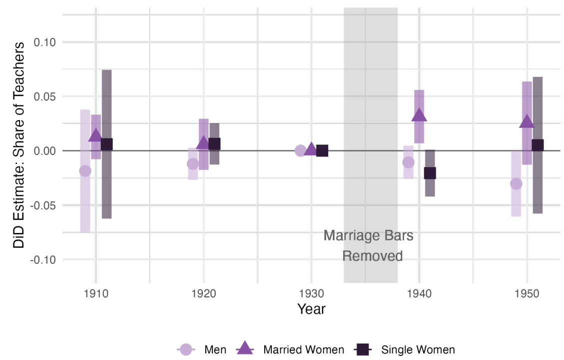
In Figure D2, we graph boxplots for the treated and various control groups of the standardized 1930 values of the matching covariates listed in Footnote 3, as well as two additional variables not used for matching (share of women married and average number of children for married women). Motivating the need to identify an appropriate control group for KY and NC, the boxplots show that the average untreated county is quite distinct from the treated counties. Importantly, the 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.3 Results

We re-estimate our key analyses using the border county and three matched samples and present the results in Figure D3. For the border counties and for matched samples 1 and 2, in panels (a) and (b), our estimated coefficients in 1940 are consistent with our main results—the marriage bar prohibitions caused an increase in the share of married women teachers, at the expense of a decrease in the share of unmarried women teachers, with no change in the share of men in teaching—and significant at the 99% level. Matched sample 3, in panel (d), shows similar results, but suggests a decrease in the share of men in teaching and no convergence by 1950. The slight difference in results for matched sample 3 is consistent with the fact that, as shown in Figure D2, the control group for matched sample 3 is also the least comparable out of all the alternate control groups to the treatment group.

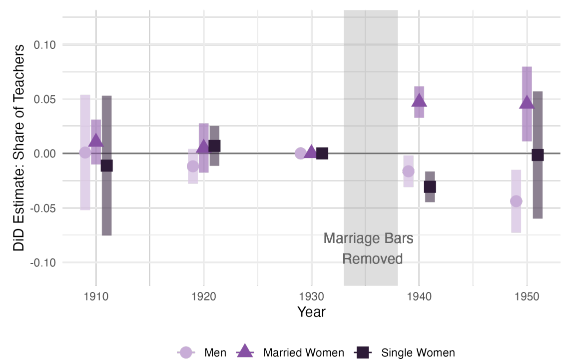
Similarly to our main results, we find no significant evidence of pre-trends in the share of married women, unmarried women, or men teachers prior to 1930 under any of the alternate control groups. Other results are also qualitatively similar and available upon request.

(a) Border Counties



(b) Match 1: Nearest Neighbor

(c) Match 2: Genetic



(d) 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 for (a) includes border counties in KY, NC and adjacent states while the sample for (b)-(d) includes all counties in KY and NC and matched control counties, as determined by various methods. Standard errors are clustered at the county level. 95% confidence intervals are shown.

E State-Level Synthetic Difference-in-Differences

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

E.1 Empirical Strategy

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

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

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

E.2 Estimation and Results

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

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

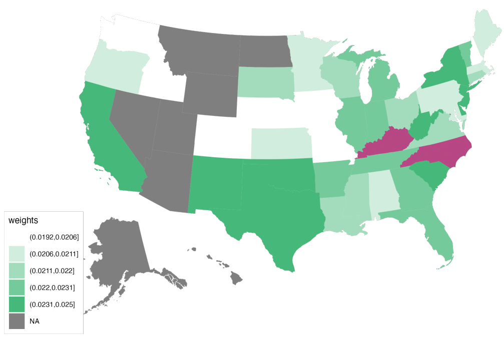
in Panel (a) of Figure E1. Computed time weights for all analyses are 1 for 1930 and 0 for other pre-period years, simplifying our setup to a two-period setting.

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

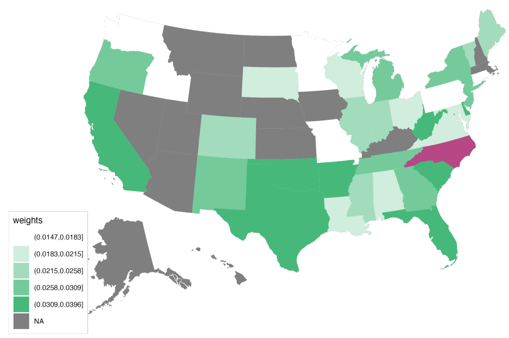
An additional benefit of using the synthetic DiD approach is that it also allows us to perform analysis with only one treated unit. As such, we estimate the effects of the prohibitions on married women teachers in North Carolina and Kentucky separately. Panels (b) and (c) of Figure E2 map control unit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

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

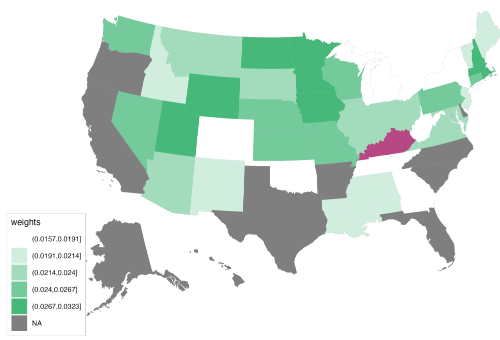
Figure E1: Maps of treated (pink) and control (green) states shaded by unit weights computed in our synthetic difference-in-differences empirical strategy (computed to match pre-trends between control and treatment states for outcome variable share teachers married women). Panel (a) uses both Kentucky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



(a) Control Unit Weights: North Carolina and Kentucky



(b) Control Unit Weights: North Carolina Only



(c) Control Unit Weights: Kentucky Only

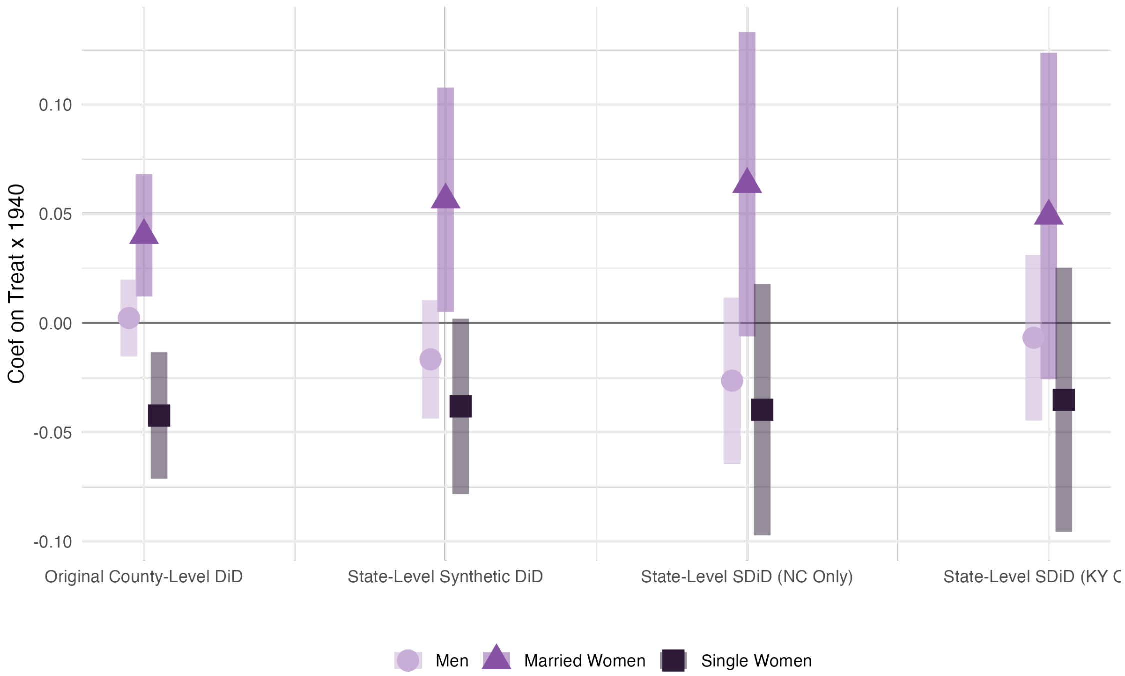


Figure E2: Estimates of the effect of the prohibition of marriage bars in teaching on the gender composition of teachers. The first column uses the standard difference-in-differences setup from our main specification at the county level, with standard errors clustered at the county level. The other columns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

Table 4: State-Level Synthetic DiD: Estimated effects of the prohibitions on women's propensity to get married and conditional on marriage, to teach, work outside of teaching, and exit the labor force.

Dependent Variable:	Pr(Married in t)	Pr(Married Teacher in t)	Pr(Married Non-Teacher in LF in t)	Pr(Married Not in LF in t)
	(1)	(2)	(3)	(4)
Sample 1: Women who were unmarried and teaching in $t-10$				
Treated × 1940 (γ_{1940}^{DD})	-0.038	0.035^{***}	-0.003	-0.088^{**}
	(0.031)	(0.014)	(0.006)	(0.035)
Dep. Var. 1930 Mean	0.5839	0.04572	0.03478	0.5034
Sample 2: Women who were married and not in the labor force in $t-10$				
Treated × 1940 (γ_{1940}^{DD})	-0.003	0.001^{***}	0.009	-0.013^{*}
	(0.002)	(0.0006)	(0.009)	(0.010)
Dep. Var. 1930 Mean	0.9328	0.001822	0.054	0.8761
Sample 3: Women who were unmarried and not in the labor force in $t-10$				
Treated × 1940 (γ_{1940}^{DD})	-0.022	0.002^{**}	0.013^{***}	-0.038^{*}
	(0.017)	(0.0007)	(0.004)	(0.021)
Dep. Var. 1930 Mean	0.5136	0.004338	0.04511	0.4641

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

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