MARRIAGE BAR, REPRESENTATION, AND SELECTION[*]

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[PRELIMINARY DRAFT]

Abstract

Women's increased participation in the workforce over the past century was the most significant change in US labor market (Goldin, 2006). An often cited – but understudied – reason for women's increased participation in the labor force was the elimination of a widespread discriminatory policy: the marriage bar. We gathered new data from 1900-1950 to document the prevalence of the marriage bar in the teaching profession - which prohibited married women teachers from working - across US cities. Using Census full-count data from 1880-1940 and a generalized difference-in-differences design around the initial adoption of the marriage bar, we show that the marriage bar decreased the proportion and number of married women teachers by 1.3 and 8.3 percentage points in the following census year (equivalent to 13% and 24% of the control mean). An increase in the representation of single female teachers substitutes this decrease. Additionally, we provide suggestive evidence on lowered retention rates of the teaching population. This paper highlights the cost of discrimination practices in reshaping the occupation and changing workers' incentives.

1 Introduction

Women's increased participation in the labor force over the past century has been the most significant change in the US labor market (Goldin, 2006). Yet, the establishment of legal rights for women is a recent historical progress. Historically, women in the labor market encountered institutional barriers such as unequal payment and discriminatory hiring criteria (Goldin, 2023). Despite the existence of some female-friendly occupations (Li, 2023), many occupations, such as teaching, clerical work, and the civil service, commonly prohibited married women from working – a practice termed the "marriage bar" (Goldin, 1988). This practice raises questions about the impact of discriminatory institutions in reshaping the labor force in the occupation and individuals' occupational choice. Recent work has examined the anti-discrimination legislation (Stevenson, 2010; Bailey et al., 2024) and gender-blind practices in hiring (Goldin and Rouse, 2000; Kuhn and Shen, 2023), finding such policies led to gains in women's representation, job opportunities, and wages. But there is limited understanding of the effects of an actively propagated discriminatory institution.

We explore the impact of marriage bar policies in the early 20th century, a widely adopted practice to prohibit married women from working by firing newly-married women and/or prohibiting the hiring already married women. At its height, marriage bars were adopted by 75% of all American school boards (Goldin, 1988). Despite its prevalence, there is no empirical evidence examining its impact. A major challenge is the availability of data documenting use of such policies[1]. Finally, the adoption often comes with a bylaw passes or simply an announcement, making it informal and hard to track. In this paper, we attempt to establish the first systematic dataset tracking marriage bar practices across cities in the US from 1900 to 1950. We leverage two data sources, a school board journal and newspaper archives `newspapers.com`, and we compile all the information pertinent to the adoption, repeal, and detailed practice of marriage bars if available.

We first examine whether the marriage bar resulted in a decrease in the share and number of married female teachers, as expected. While we can't test directly whether the marriage bar was enforced, we interpret decreases in the share and number of married women teachers as potential evidence that the policy was enforced and was binding. For example, if marriage bars were only implemented in locations where female teachers chose to retire upon marriage, possibly due to social norms, then we would not expect such

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[1]There are few reasons. First, local school boards are usually the decision-maker for hiring practices (Goldhammer, 1964), making the policy documentation decentralized. Second, as far as we know, the most systematic data source is the series of surveys conducted by National Education Association. However, these surveys documented the existence of the policy among school boards with limited capacity for tracing how these policies changed over space and time.

policies to result in any changes to the teaching composition (a non-binding policy). To answer this question, we exploit the natural experiments where marriage bar is adopted at different timing across different cities.

Using Census full-count data from 1880 to 1940 with a generalized difference-in-differences empirical strategy, we find that marriage bars decreased the proportion of married women teachers by 1.3 percentage points (13% of pre-treatment mean), substituted by single female teachers. Without changing the total teacher size, the marriage bar led to a 8.3 percentage points decrease in the total number of married female teachers per hundred children (24% of pre-treatment mean). Substitution within female teachers is aligned with the common reasoning for marriage bar – rationing labor from married women to single women (Goldin, 1988). We test the robustness of the results by (1) using an interaction-weighted estimator estimator (Sun and Abraham, 2021) to account for heterogeneous treatment effects, (2) using baseline population-interacted estimates to account for different population growth across cities, (3) adding state-year fixed effect to control for common shocks, (4) weighting by population at school-age, and (5) restricting sample to the treated group. The results are robust across all specifications, suggesting that the marriage bar did reshape the composition of teacher population.

Next, we explore the impact of the marriage bar on selection into teaching. As marriage becomes more costly under the marriage bar, it might encourage women to enter more family-friendly positions. It could also change the career prospects of women teachers, when they are more likely to treat teaching as a temporary job – they resign upon marriage, take child-rearing leaves, and return to teach until retirement age (Scharf, 1980). Possibly, marriage bars attracted teachers with aligned preferences – later marriage, fewer children, and/or low retention. Since we do not have information on the entry and exit of all the teachers at granular level, we attempt to use the population characteristics of teachers to provide suggestive evidence on this question. We find supportive evidence for lower retention rates - current teachers are less likely to still be teachers in 10 years, but there is no significant changes with respect to the age of the teaching population.

This paper makes several contributions. First, it deepens our understanding of the impact of marriage bar policies. Although frequently documented in historical narratives of women's obstacles to work (Bailey et al., 2024; Goldin, 2023), there is limited empirical evidence on whether and to what extent marriage bars affected women's work. Prior works studying this question have focused on relating individual marriage bar experiences with labor market participation and fertility choice in the Ireland context and is not causal in nature (Mosca and Wright, 2020). We add to this literature by leveraging the staggered adoption of marriage bar policies in the US to causally identify the impact, and showing the occupation structure and dynamics

are also affected. Connecting with the in-depth discussion on the determinants of marriage bar (Goldin, 1988), we expand this literature by building new data source and providing causal evidence.

Secondly, this paper provides a unique setting to understand the cost of discrimination. Current discrimination literature provides evidence on the negative impacts of discrimination or stereotypes on workers' productivity. Given the ethnic challenges for identifying and eliciting discrimination, scholars often use field experiments to indirectly approach the discrimination manner[2] (See review from Bertrand and Duflo (2017)). Instead of using latent discrimination measurement, marriage bar adoption clearly identifies the differential treatment towards married women, making it possible to collect using historical data resources and salient enough to demonstrate discrimination. Although there still exist concerns regarding measuring the policy adoption, we argue that these concerns would underestimate our results, and could be pervasive in any type of discriminatory policy research. We will discuss the measurement error in the Data section 1.3. Several studies use novel natural experiment design and show the impact of practices that suppresses discrimination on diversifying the representation of the labor supply (Goldin and Rouse, 2000; Kuhn and Shen, 2023). This article expands this topic by examining the representation consequence of discrimination.

2 Background

Marriage bars are discriminatory practices prohibiting the employment, or continued employment, of married women. In the US, marriage bars began in the late 19th century, and lasted until the 1960s when landmark federal legislatures – Equal Pay and Civil Rights Act – were in place to prohibit discrimination based on sex. This practice was widely adopted across sectors, such as firms, school boards, and governments, and across countries, including Netherlands, Ireland, UK, and the US. In this paper, we focus on teaching profession in the US due to our data availability. Nevertheless, schoolteachers are one of the several sectors that are most likely to find bars (Shallcross, 1940).

The format of the bars could be categorized into two types, one is in the form of refusal of hiring married women (hiring bar), the second is to dismiss women upon marriage (retention bar). This indicates different compositional change for teacher population. Theoretically, hiring bar result in fewer new married women

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[2]Audit and correspondence studies help to gauge the extent of discrimination by constructing fictitious job applications. This allows researchers to identify the average effect between minority and majority groups but is not informative about the response from applicants. The more recent method, Implicit association tests (IAT), is based on the speed of response when one is exposed to the minority group. Therefore, it helps to identify individual specific discrimination and its impact on those who exposed to them. However, there are debates about what IAT really measures and to what extent it reflects discrimination.

being hired as compared to cities without any bar. Retention bars, on the other hand, would result in exits of already employed teachers upon their mariage. In some cases, we also find narratives about keeping hiring married women but only as substitute teachers. This would indicate no change in the number of married women teachers but changes in the contract. We cannot fully differentiate these policy types from our data resources. But we will discuss the implications about the policy types based on our results in Results section 1.5.

The decision-making of personnel practices is usually at the discretion of local school boards. The school board, as a governance agency designated by the state, has duties to manage local affairs as well as the power of overseeing local school district, adhere to the state constitution and regulations. Although it varies from state to state, school boards may exert their discretionary power in important decisions such as drafting contracts, acquiring property, and employing personnel (Goldhammer, 1964). Aligned with our dataset, we find nearly all of the marriage bar adoption are initiated by city or township school boards.

The initiatives for adopting or repealing marriage bars could be summarized into two aspects. The most common reason is to ration the labor force by preserving opportunities for single women[3]. The justification for marriage bar is that married women had means of support from their husband, while single women were more in-need in comparison. This conflict is easily escalated during the economic recession such as the Great Depression, where the competition in the labor market is intensified. Abundant graduates are ready to enter the job market, but positions are limited. For places with tenure policies, the economic incentives for the marriage bar were even stronger given the high cost of employing married teachers (Goldin, 1988). We provide empirical evidence on the labor market compositional changes due to marriage bar passage in the Results Section 1.5.

On the other hand, during periods of labor supply shortages, such as during the war times, school boards attempted to encourage married women to return to teaching positions to address the teacher shortage. Therefore, the changes in policies in 1920-1950 are mainly attributed to economic reasons, although this inevitably was rooted along with the social norm against working women. In other cases, it could be driven by statistical-based discrimination with beliefs about the low productivity of married women when they must take care of the family. Occasionally it also comes from the taste-based discrimination, where working

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[3]For instance, in 1923, the board of education of Quincy, Illinois, has ruled against the employment of married women teachers. A local newspaper thereupon interviewed the members of the board, including President J. C. Whitfield, Wilton E. White, J. N. Tibesar, Mrs. Mary Westermann, and C. F. A. Behrensmeyer, as to the reasons for its action. It was generally believed that the employment of married women worked an injustice to single women who sought positions.

married women are viewed as inappropriate. We are not leveraging the variation in repeal for estimating the impact of marriage bar. But those culture incentives make our identification strategy – controlling for city fixed effects – credible as it controls for the culture norms that remain constant over time.

Figure 1 illustrate the timing of the adoption of the marriage bar using the dataset we built. It includes all the adoption and repeal information we collected, highlighting the variation of restricted sample used in empirical analysis in Results section 1.5. The spikes in marriage bar variations are accumulated during the WWII and Great Depression period, aligning with the financial incentives for this policy. Figure 2 show the geographic spread of cities documented to adopt or repeal marriage bar. It suggests the northeast area is where the marriage bar practice changes were most frequent.

3   Data

3.1   Marriage Bar Policy

The policy variation comes from two sources, one is our manual collection of marriage bar policies using a monthly published education magazine, American School Board Journal, the other is the information from newspaper archives `newspapers.com`. *American school board journal* (ASBJ) is a monthly published education magazine starting 1891 until today. It covers a broad range of topics pertinent to school governance and management, equity, policy making, student achievement, and the art of school leadership. It is mostly subscribed to by school board clerks, board members, and superintendents. We manually searched and digitized the marriage bar policies using journals published during 1900-1950, and we obtained the adoption date of 281 cities.

However, it is unclear to us about the information collection process. Hence we might worry about the selection bias from cities reported in ASBJ. To address this concern, we turn to newspaper archives to systematically search for information regarding marriage bar across all cities. Under this approach, we search marriage bar by state and collect the adoption timing and location if available. This approach allows us to comprehensively document the marriage bar discussions in the newspaper archives. We collect adoption information for another 62 cities using `newspapers.com`[4]. For cities with multiple adoption dates or repeal after adoption, we use the date of the first-time adoption. This allows us to interpret the estimates as the impact of first-time adoption and to be less concerned about the heterogeneous treatment effects that are

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[4]The data collection is still ongoing, this is the number of cities we collected so far.

contingent on previous adoption practices.

To our knowledge, this is the most comprehensive data on marriage bar policies in the US. Nevertheless, we expect measurement error in our adoption variable. First, cities in the control group might have adopted the policy but is not captured in our dataset, which could results in underestimation in our estimates. Significant results might be underestimating the real effects, and insignificant results might be masked by the measurement error. Second, on the other hand, cities in treatment group might be false positives. We investigate this by cross validating the treatment information collected from ASBJ in the newspaper archive, and we find that cities collected by ASBJ could be completely verified based on newspaper sources[5]. However, we do find discrepancies in the year of adoption, where the newspapers usually document a time that is earlier than ASBJ. Finally, as shown in Figure 1, the number of cities we collected is far from the prevalent coverage of marriage bar - roughly 75% of all school boards. This is less concerning for interpreting our results as causal, since the pre-trend check will be informative for the differential trend across treated and control groups. Inevitably, this implies limitation on our external validity for extrapolating the results into the whole population. We will show that at least our cities are representative of large population cities in Empirical Strategy section 1.4.

3.2 Other Data Sources

We obtain city characteristics and teacher outcomes of interests using IPUMS full-count census from 1880 to 1940 (Ruggles et al., 2024). We identify city in census using individual-location crosswalk created by Census Place Project (Berkes et al., 2023) to ensure consistent place identifiers over time. We then identify the teachers using the occupation variable - occ1950 – from census, therefore we can construct city-level variables including the count, proportion and average age of teachers by marital status and gender. To measure teacher retention, we use the crosswalk provided by Census Tree Project (Price et al., 2021; Buckles et al., 2023) to ensure abundant linking available for women, which is usually challenging due to name changes. We then calculate teacher retention at city level as the proportion of linked teachers remain as teacher in the next census year.

Instead of using all the cities from census for further analysis, we restrict our sample to construct a comparable control group. To do so, we limit cities from the Report of the Commissioner of Education and Biennial Survey of Education (CEBSE). These reports contain education expenditure, revenue and

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[5]By now we only checked cities in Massachusetts, the state with the largest number of treatment cities in our analysis group.

enrollment data of city school systems with population of 10,000 and over, and cover all the years within our sample span. We plan on digitizing these data to add control and outcome variables. This provides a list of cities that are city school systems and variables for further usage. Therefore, we keep our cities for which is in these reports. We further keep cities with a balanced panel across all 6 census years. This gives us 624 cities covering 46 states, where we have 132 treated cities and 492 control cities.

4 Empirical Strategy

To identify the causal impact of marriage bar on teacher composition, we employ an event study method with a staggered adoption. Specifically, we estimate the model:

$$y_{ct} = \alpha_0 + \sum_{k \geq -2, k \neq -1}^{k=2} \beta_k AdoptMB_{ct}^k + D_c + D_t + X_{ct} + \epsilon_{ct} \tag{1}$$

Where y_{ct} is the outcome of interest. c denotes the city, t is the census year and k denotes the year relative to marriage bar adoption. For each city, we denote $k = 0$ as the closest census year at or after the year of adoption. $AdoptMB_{ct}^k$ are indicators for each census year relative to the adoption census year. We include the whole set of event time dummies except for the omitted year $k = -1$. In this way, coefficients can be interpreted as the effect of adoption at each event time relative to the census year before the adoption of marriage bar. We included binned event dummy for years that are at least 3 census year before adoption and years that are at least 3 census years after adoption. Following conventional event-study designs, we include city fixed effects D_c to control for any time-constant city-specific factors. This helps control for important factors such as social norms, which could determine the preferences for adoption as well as affect the married teachers' willingness to work after marriage. Year fixed effects D_t control for any year-specific shocks that are common across cities. This accounts for part of the macro-economic shocks that are universal across cities. In all specifications, we cluster at the city level. For city-year level controls X_{ct}, we include the log of city population.

To interpret the results as causal, we need to satisfy two identification assumptions: no anticipation of the treatment and parallel trends (Sun and Abraham, 2021; Borusyak et al., 2024). Under the no anticipation assumption, we assume individuals do not change their behavior in anticipation of the treatment. For example, the debates among school board members might inform teachers about their potential to be rehired,

and hence change their incentive to stay productive. As described in Background section 1.2, the decision to adopt marriage bar was mainly financially driven and was made by school board members in response to the large supply of young graduates. Therefore it is less likely to spread over teachers or even driven by current teachers. Under the parallel trends assumption, we assume that absent the reform, the difference in potential outcomes would be the same across all cities and years conditional on the set of controls, city and year fixed effects. Table 1 illustrates the comparison between treated and control cities. Column (1) and (3) suggest the treated cities have larger population, are more likely to be urban areas, have higher income, and have less proportion of farmers or black population in the baseline year 1880. But all the teacher population characteristics are identical. To mitigate concerns about differential trends, we perform balance tests between treated and control cities and show that there is no difference in changes of city demographics as well as teacher population characteristics between the baseline year 1880 and 1900.

To do so, we estimate a difference-in-differences model, in which we restrict to pre-treatment year 1880 and 1900

$$y_{ct} = \alpha_0 + \beta_1 Treated_c * Post_{1900} + D_c + D_{1900} + \epsilon_{ct} \tag{2}$$

Estimates β_1 are shown in column (5) of Table 1. Out of the 15 outcomes we analyze, just one is significant at the 10% level: the age of single female teachers, which are growing faster in treated cities. However, the direction of the bias is not clear. For instance, an elder single female teacher population might reflect young female has better outside options, hence has a negative impact on proportion of single female teachers. Or it could reflect the demand side force when young single women are not viewed as competitive as elder single women teachers, therefore trigger the marriage bar and push school board to hire more single women. Furthermore, in the Results section 1.5, we will show that the estimated pre-coefficients do not differ significantly from zero.

5 Results

5.1 Effects of Marriage Bar on Teacher Representation

We first consider how marriage bar reshaped the composition of teachers. Figure 3 plots the event study coefficients and their 95% confidence intervals for the married female share of all teachers and the number of married female teacher per 100 children. We show the magnitudes of these findings in Table 2

and Table 3, with estimates for other gender and marital status groups. We first note that the validity of our identification strategy is supported by the lack of pre-trends in the Table 2. The married female share significantly decreased by 1.3 percentage points at the census year post the adoption of marriage bar. Relative to the pre-treatment share of 10.2 %, this indicates that married female teachers decreased by 13% due to marriage bar. Number of married female teachers decreased by 0.08, implying a decrease of 24% given the pre-treatment number of married female teachers is 0.349 per hundred children. The results suggest that the marriage bar, which targeted the employment of married women, did decrease the representation of married women in teaching. A natural question is whether other groups replaced the married women teachers. Based on column (2) of Table 2 column (3) of Table 3, the positive sign and relatively large magnitude on the share of single female teachers suggest married female teachers are substituted by their single counterpart, which is aligned with the rationale for the marriage bar.

Interestingly, based on the estimates for the following census years post marriage bar, the decrease in the representation of married female teachers persists and the magnitude becomes larger. Although it is hard to interpret given our short and unbalanced panel data, this might indicate the persistence discriminatory practice towards married women, or the reinforcement of social norms convinced married women teachers to resign voluntarily. However, since our sample is not balanced, the post periods' coefficients could simply be estimates for early adopted cities, which might apply a stronger practice during the Great Depression.

5.2   Effects of Marriage Bar on Occupation Selection

We next investigate the effects of marriage bar on the selection behavior of teachers. Since we do not have information on the entry and exit of all the teachers at granular level, we attempt to use the population characteristics of teachers to provide suggestive evidence on this question. Specifically, we explore age of the teaching population and teachers' retention rates in ten years.

The impact on the age of teacher population could be multifaceted, with a mixed of compositional effect, substitution effects, as well as teachers' incentives to enter the profession. First, if the marriage bar is mostly adopted in the form of hiring bar, then prohibiting hiring newly married teachers will likely increase the average age of the married female teacher. The impact on the average of all population depends on whether substitution to single women is driven by young or elder single teachers. Table 4 shows our estimates for age of all teachers and by gender and marital status. Overall, there is no significant effect on the age of the teaching population. Column (2) suggests the effect on the age of married female teachers is positive but not

statistically significant. And in column (3) the coefficient for single female teacher is negative, suggesting the single teacher group comprises of younger teachers, but also not statistically significant. If marriage bar is mainly in the form of retention bar, it implies prohibiting renewing the contract with currently married teachers, therefore the average age of female teacher should decrease as married teachers are usually elder. We estimated the effect on the age of female teachers (not shown here), and the results suggest the age decreases but also not statistically significant, and the magnitude of the estimates are small. Appendix Figure A1 shows the plot for the event-study coefficients.

For teacher retention rate, we hypothesize that the marriage bar may increase teacher retention. On one hand, it may change teachers' perception of the profession (Scharf, 1980), single women may treat teaching as a part-time job where they resign to get married, have children and then return back to teaching when children grow up. Therefore single female teachers could have a higher retention rates. On the other hand, the discriminatory policy could directly encourage all women selecting out of teaching and enter other professions. Figure 4 and Table 5 plot the estimates on the retention rates of teacher population and by gender and marital status groups. The coefficients of β_0 are neither statistically nor economically significant, but the coefficients of β_1, β_2 are significant. In column (1), the marriage bar adoption decreases the retention rate of all teachers by 3.4 percentage points at the second census year post the adoption of marriage bar. Relative to the pre-treatment retention rate of 53.6%, this indicates a 6.3% decrease in retention rate due to marriage bar. Decomposing the drop in retention rates by gender and marital status in columns (2) to (5), both single and married women experience a significantly lower retention rates in later census years. Appendix Figure A2 shows the plot for the event-study coefficients.

5.3 Robustness Checks

We conduct a series of checks to show our results are robust to alternative specifications. The results for teacher representation and teacher retention are summarized in Figure 5.

The first check shows that our estimate is robust to using the interaction term between event-study dummy and baseline population in 1880 as estimates[6]. This accounts for the heterogeneous treatment effects due to the differential growth of population across cities. The second check shows the estimates using an alternative interaction-weighted estimator, proposed by (Sun and Abraham, 2021). As highlighted by the recent econometric literature, our estimates may be biased if there is heterogeneity in treatment effects

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[6]To make the estimates visually comparable with estimates from other specifications, we divide the interaction term by 100,000.

between treated cities or across time (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2024). We conduct the robustness checks using (Sun and Abraham, 2021) as their estimator allows a flexible event-study specification. It provides a weighted average of the treatment effects in a way that's more interpretable than the estimates from a standard two-way fixed effects estimator (Sun and Abraham, 2021). The results remain robust. The third check adds state by year fixed effects to control for common state-year level shocks, such as World War I and Great Depression. The consistent results provide support for our identification strategy. Fourth, the baseline estimates treat cities of different sizes as equally contributing to the treatment effect, i.e. the interpretation of the coefficient is that on average, the adoption of marriage bar has certain level of impact for a city. We can also adjust the estimates by re-weighting on the size of school age children of each city, so that we can interpret the estimates as the effect on a weighted-average city. The estimates stay robust. Finally, we limit our analysis to cities that are eventually treated. The estimates remain similar, hence lending support for the effect is not driven by the differences in control cities.

6 Conclusion and Discussion

This paper presents new evidence on the effects of marriage bar on occupation representation and selection. Exposure to marriage bar decreased the representation of married women in teaching. We also provide suggestive evidence on lowered retention rates of the teaching population.

On the whole, this paper highlights the cost of discrimination practice in reshaping the occupation and changing incentives of workers. While the number of married women affected may be small in magnitude, due to there being few married female teachers as baseline, it is possible that this policy served to change female student's expectations about work and reinforce social norms. A future avenue for research is to understand if the practice had spillover effect on students, and in particular female students. It will also be interesting to understand what factors predict the adoption of the marriage bar policy, including the dynamics between local labor organisations and decision-makers at the school board, and local economics conditions. This question could be of great relevance today given the push for gender equality and identity equality in education.
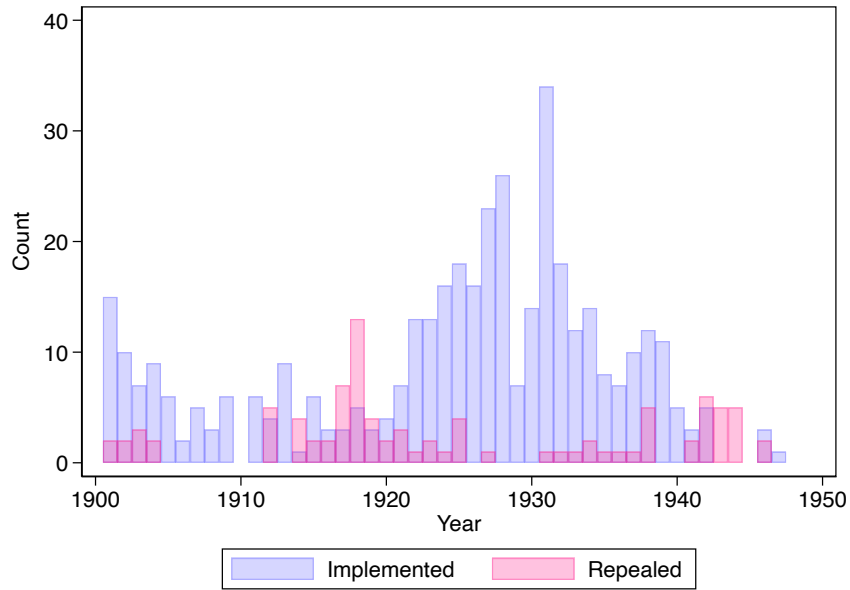
Figure 1: Timing of Marriage Bar

Source: Data comes from our manually collected data from ASBJ and `newspapers.com`
Note: This figure shows the extensive margin of number of cities adopted marriage bar by the year of adoption.
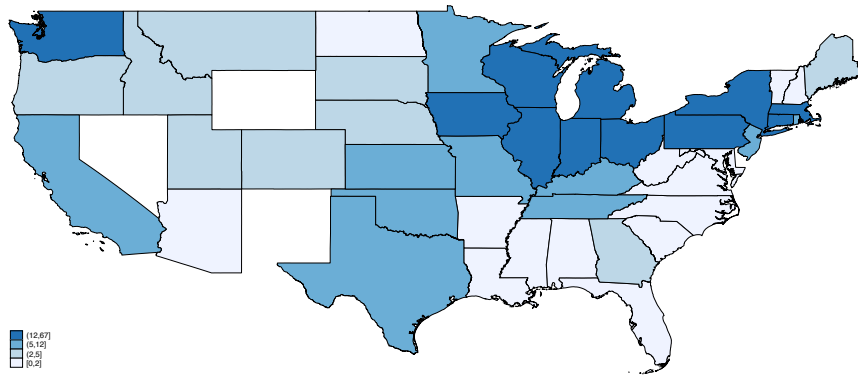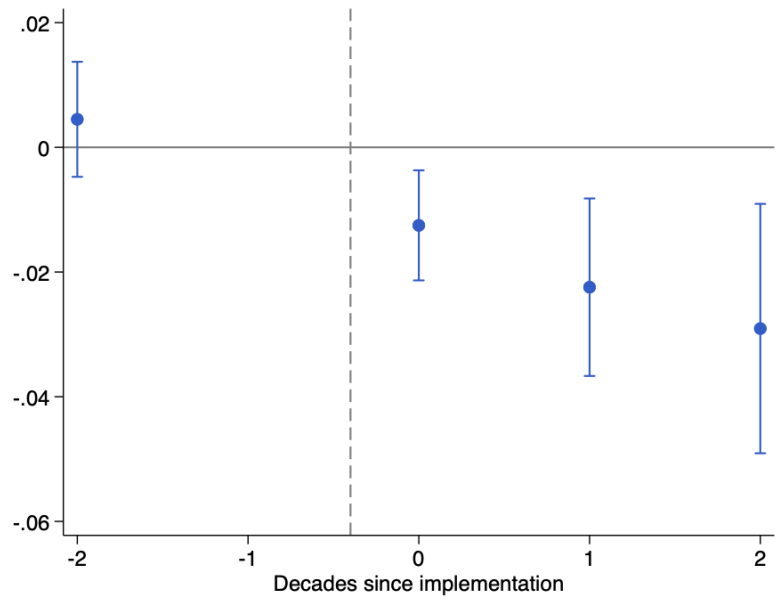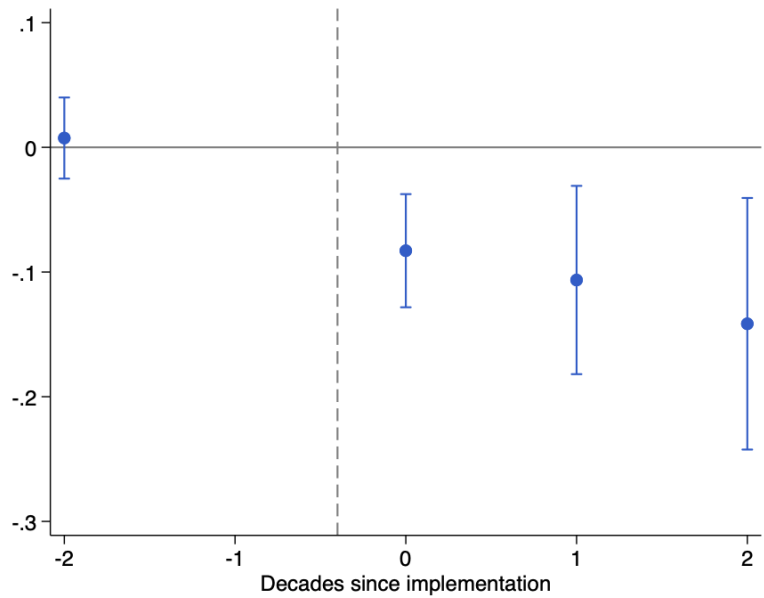

Figure 2: Spacial Variation of Marriage Bar

Source: Data comes from our manually collected data from ASBJ and `newspaper.com`
Note: This figure shows the frequency of appearance of marriage bar (both repeal and adoption) across cities in the US.

(a) % of Married Female Teachers

(b) # of Married Female Teachers per 100 Children

Figure 3: Effects of Marriage Bar on Teacher Representation

Source: Data comes from 1880-1940 US full count census.

Note: These figures show the event-study coefficients that analysis the effect of marriage bar on % of married female teachers and # of married female teachers per 100 Children. All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel.
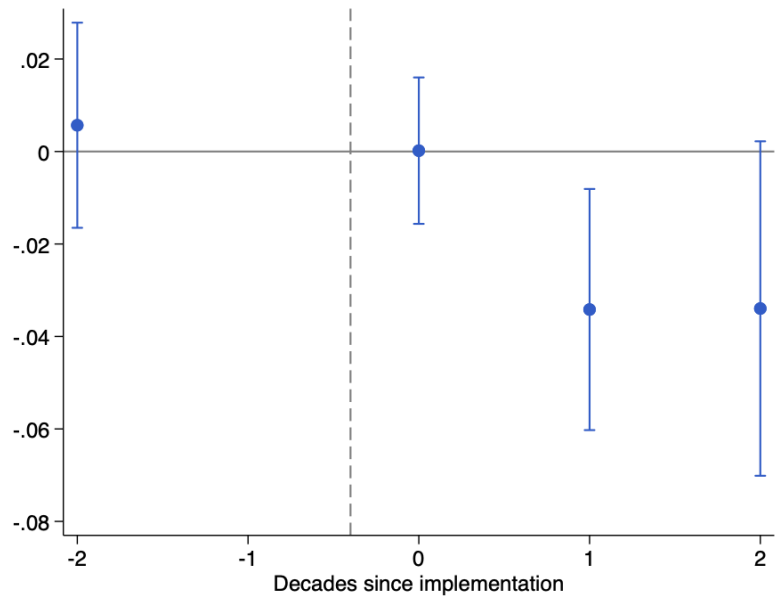
Figure 4: Effects of Marriage Bar on Teacher Retention Rates

Source: Data comes from 1880-1940 US full count census, and linking across census uses Census Tree Project.
Note: These figures show the event-study coefficients that analysis the effect of marriage bar on % of current teachers remain as teachers in 10 years. All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel.
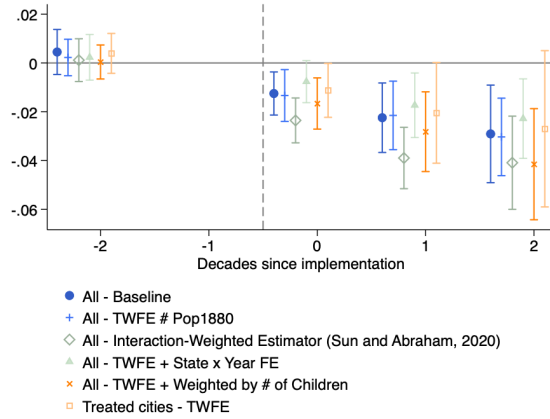
(a) % of Married Female Teachers

(b) # of Married Female Teachers per 100 Children
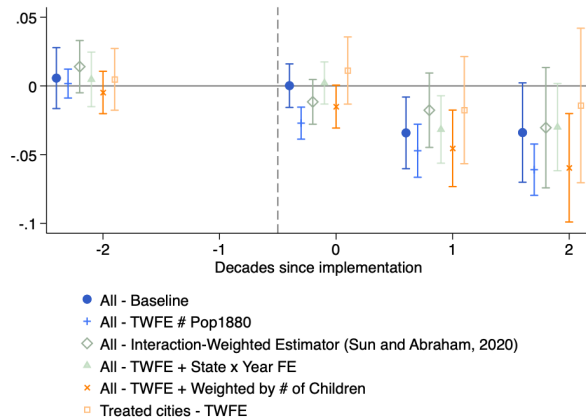
(c) Retention Rates of All Teachers

Figure 5: Robustness to Alternative Specifications

Source: Data comes from 1880-1940 US full count census.

Note: These figures show the robustness checks of event-study coefficients that analysis the effect of marriage bar. All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel.
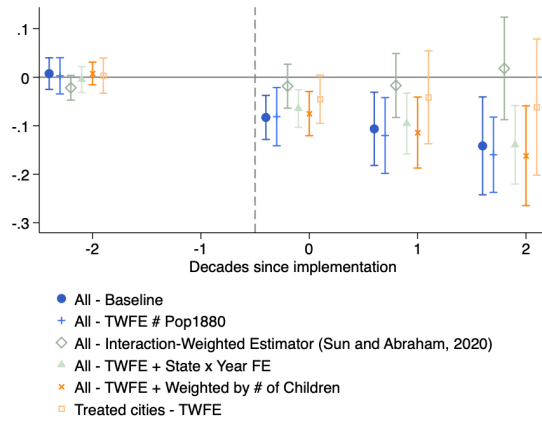
Table 1: Trend in City Characteristics Prior to Marriage Bar

| | Treated | | Control | | Trend |
| | Mean | SD | Mean | SD | Coefficient |
	(1)	(2)	(3)	(4)	(5)
Population	28,833	53,421	14,834	49,936	13,986
Urban	61%	0.49	39%	0.49	0.17
% Black	6%	0.13	10%	0.16	0.006
% Children(5-17)	18%	0.32	23%	0.43	0.002
Average Occupation Score	23	1.86	22	2.45	-0.15
% Farmers	5%	0.08	8%	0.10	0.006
% Female LFP	22%	0.09	21%	0.10	0.006
% Married Female LFP	10%	0.08	8%	0.09	0.002
Teacher # per 100 Children	1.93	0.85	1.97	0.88	0.006
- % of Married Female	9%	0.07	8%	0.09	-0.02
- % of Single Female	67%	0.18	65%	0.19	-0.011
- % of Married Male	14%	0.10	14%	0.11	0.033
- % of Single Male	13%	0.14	14%	0.11	0.011
Average Age of Teachers	30	3.34	29	3.55	0.333
- Married Female	38	6.04	37	3.73	-0.619*
- Single Female	26	25.75	40	6.07	0.687*
- Married Male	41	6.08	40	6.09	-1.075
- Single Male	27.5	4.82	27	5.36	0.579
# of Cities	132		492		
# of States	32		45		

Notes: This table shows the balance between treated and control cities. Full sample includes all cities identified under Census Place Project, including 343 cities with marriage bar adoption date between 1900-1950. We restrict our sample to cities in the Report of the Commissioner of Education and Biennial Survey of Education (CEBSE), with a balanced panel across all 6 census years. Column (1) to (4) shows baseline statistics in 1880. Column (5) shows estimated trend coefficients between 1800 and 1900, as discussed in Section 1.4.

Table 2: TWFE Estimates on Teacher Composition(%)

	(1) % Married Women	(2) % Single Women	(3) % Married Men	(4) % Single Men
Years before -30	0.002	0.044***	-0.019*	-0.027***
	(0.007)	(0.014)	(0.011)	(0.006)
Years -20	0.004	0.003	-0.003	-0.006
	(0.005)	(0.008)	(0.005)	(0.005)
Years 0	-0.013***	0.008	0.000	0.001
	(0.005)	(0.007)	(0.004)	(0.004)
Years 10	-0.022***	0.005	0.003	0.014***
	(0.007)	(0.010)	(0.006)	(0.005)
Years 20	-0.029***	0.022*	-0.001	0.011*
	(0.010)	(0.012)	(0.007)	(0.006)
Years after 30	-0.038***	0.027*	-0.003	0.019***
	(0.014)	(0.014)	(0.008)	(0.007)
log(pop)	0.009*	0.030***	-0.019***	-0.014***
	(0.005)	(0.008)	(0.006)	(0.004)
Constant	0.031	0.413***	0.311***	0.214***
	(0.047)	(0.078)	(0.060)	(0.038)
Observations	3,740	3,740	3,740	3,740
R-squared	0.739	0.639	0.483	0.415
City FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
# of Cities	624	624	624	624
t-1 Mean	0.102	0.757	0.101	0.0574

Source: Data comes from 1880-1940 US full count census.

Note: This table reports the average effects of marriage bar from estimating equation (1). All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3: TWFE Estimates on Teacher Count(#)

	(1) All	(2) Married Women	(3) Single Women	(4) Married Men	(5) Single Men
Years before -30	0.397***	0.053	0.349***	0.019	-0.019
	(0.110)	(0.033)	(0.093)	(0.021)	(0.015)
Years -20	-0.009	0.007	-0.014	-0.004	-0.004
	(0.073)	(0.017)	(0.057)	(0.012)	(0.016)
Years 0	-0.071	-0.083***	0.003	-0.009	-0.004
	(0.087)	(0.023)	(0.066)	(0.026)	(0.011)
Years 10	0.087	-0.106***	0.121	0.017	0.054***
	(0.123)	(0.039)	(0.082)	(0.044)	(0.019)
Years 20	-0.064	-0.142***	0.078	-0.020	0.026
	(0.107)	(0.051)	(0.089)	(0.028)	(0.019)
Years after 30	-0.058	-0.174**	0.124	-0.036	0.047*
	(0.148)	(0.081)	(0.117)	(0.038)	(0.028)
log(pop)	-0.020	0.099***	-0.040	-0.004	-0.044***
	(0.068)	(0.018)	(0.054)	(0.017)	(0.011)
Constant	3.533***	-0.540***	2.752***	0.446***	0.679***
	(0.677)	(0.175)	(0.538)	(0.171)	(0.114)
Observations	3,744	3,744	3,744	3,744	3,744
R-squared	0.711	0.753	0.654	0.675	0.348
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
# of Cities	624	624	624	624	624
t-1 Mean	3.137	0.349	2.354	0.319	0.178

Source: Data comes from 1880-1940 US full count census.

Note: This table reports the average effects of marriage bar from estimating equation (1). All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4: TWFE Estimates on Teacher Average Age

	(1)	(2)	(3)	(4)	(5)
	All	Married Women	Single Women	Married Men	Single Men
Years before -30	-0.693**	0.139	-0.465*	-0.133	-0.927*
	(0.301)	(0.682)	(0.263)	(0.667)	(0.483)
Years -20	-0.027	0.036	-0.164	0.428	-0.343
	(0.233)	(0.569)	(0.159)	(0.362)	(0.509)
Years 0	-0.071	0.117	-0.255	0.264	0.030
	(0.179)	(0.424)	(0.190)	(0.326)	(0.401)
Years 10	0.156	0.679	0.092	0.533	-0.841
	(0.237)	(0.467)	(0.257)	(0.402)	(0.523)
Years 20	0.738**	1.430**	0.934***	0.797	-0.463
	(0.297)	(0.618)	(0.321)	(0.529)	(0.606)
post	0.875*	1.071	1.125**	0.432	-0.627
	(0.458)	(0.740)	(0.467)	(0.661)	(0.702)
log(pop)	-0.266	-0.446	0.291	0.464	0.216
	(0.256)	(0.341)	(0.227)	(0.293)	(0.271)
Constant	35.286***	43.962***	28.356***	36.090***	27.408***
	(2.543)	(3.407)	(2.257)	(2.913)	(2.695)
Observations	3,740	3,570	3,732	3,673	3,624
R-squared	0.752	0.328	0.826	0.304	0.360
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
# of Cities	624	624	624	624	624
t-1 Mean	32.90	39.90	31.48	41.57	30.06

Source: Data comes from 1880-1940 US full count census.

Note: This table reports the average effects of marriage bar from estimating equation (1). All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel. *** p<0.01, ** p<0.05, * p<0.1

Table 5: TWFE Estimates on Teacher Retention Rates(%)

	(1) All	(2) Married Women	(3) Single Women	(4) Married Men	(5) Single Men
Years before -30	0.024	0.017	0.017	-0.007	0.107***
	(0.016)	(0.031)	(0.018)	(0.034)	(0.037)
Years -20	0.006	0.015	0.011	-0.035	0.009
	(0.011)	(0.027)	(0.012)	(0.024)	(0.027)
Years 0	0.000	0.015	-0.009	0.001	0.014
	(0.008)	(0.025)	(0.009)	(0.022)	(0.027)
Years 10	-0.034**	-0.048*	-0.031**	-0.023	-0.041
	(0.013)	(0.028)	(0.014)	(0.030)	(0.035)
Years 20	-0.034*	-0.045	-0.033*	-0.035	-0.144***
	(0.018)	(0.037)	(0.019)	(0.037)	(0.046)
Years after 30	-0.080**	-0.033	-0.091**	0.056	-0.100
	(0.031)	(0.092)	(0.035)	(0.051)	(0.082)
log(pop)	0.019**	-0.012	0.026**	0.007	0.023
	(0.008)	(0.018)	(0.010)	(0.015)	(0.015)
Constant	0.263***	0.505***	0.241**	0.355**	0.078
	(0.081)	(0.185)	(0.102)	(0.151)	(0.148)
Observations	3,104	2,706	3,075	2,978	2,794
R-squared	0.741	0.386	0.697	0.416	0.404
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
# of Cities	624	619	624	622	622
t-1 Mean	0.536	0.415	0.585	0.476	0.340

Source: Data comes from 1880-1940 US full count census.

Note: This table reports the average effects of marriage bar from estimating equation (1). All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Online Appendix: Additional Tables and Figures

(a) All Teachers

(b) Married Female Teacher

(c) Single Female Teacher

(d) Married Male Teacher
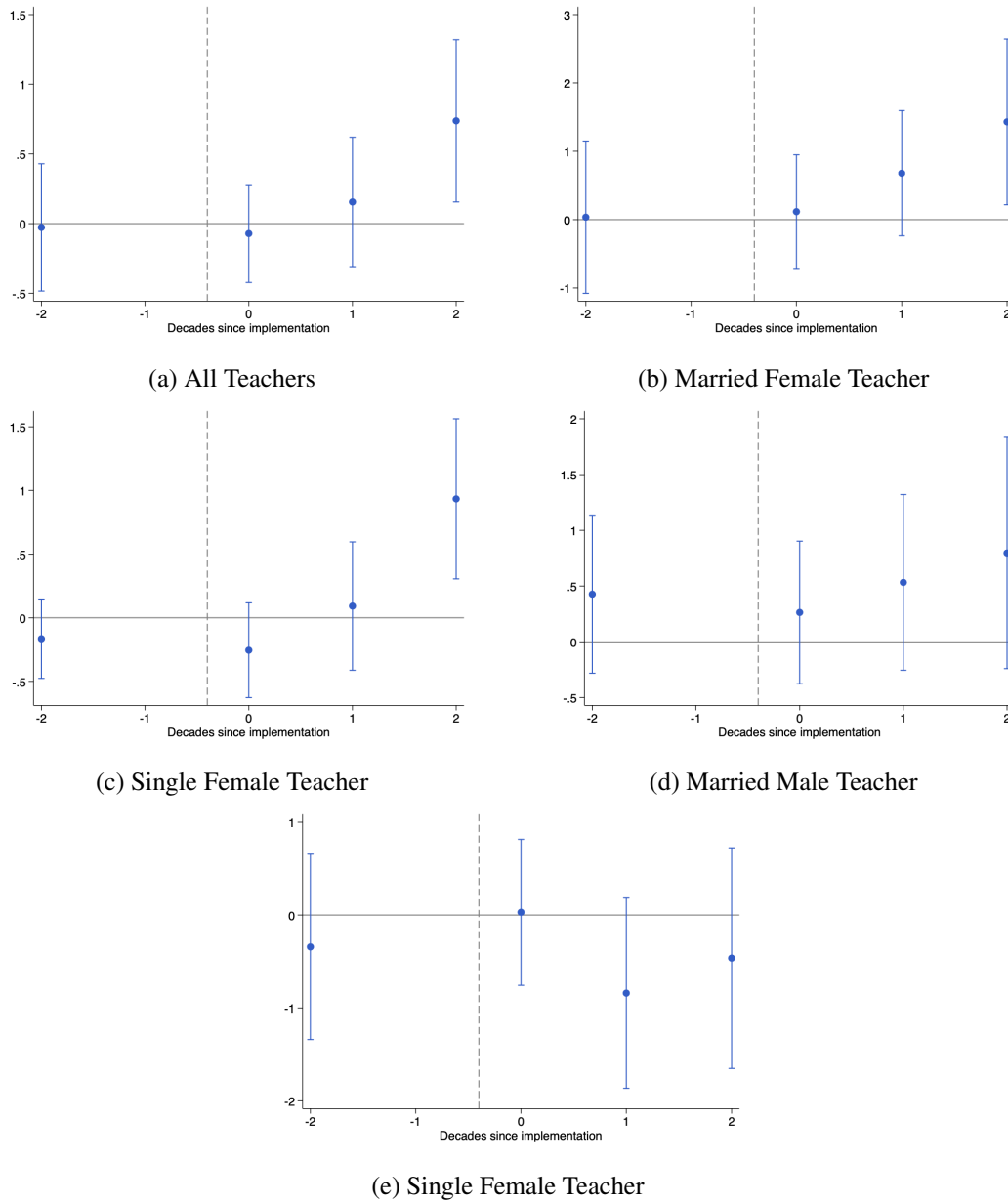
(e) Single Female Teacher

Figure A1: Effects of Marriage Bar on Teacher Age

Source: Data comes from 1880-1940 US full count census.

Note: These figures show the event-study coefficients that analysis the effect of marriage bar on average age of all teachers and by marital status and gender groups. All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel.
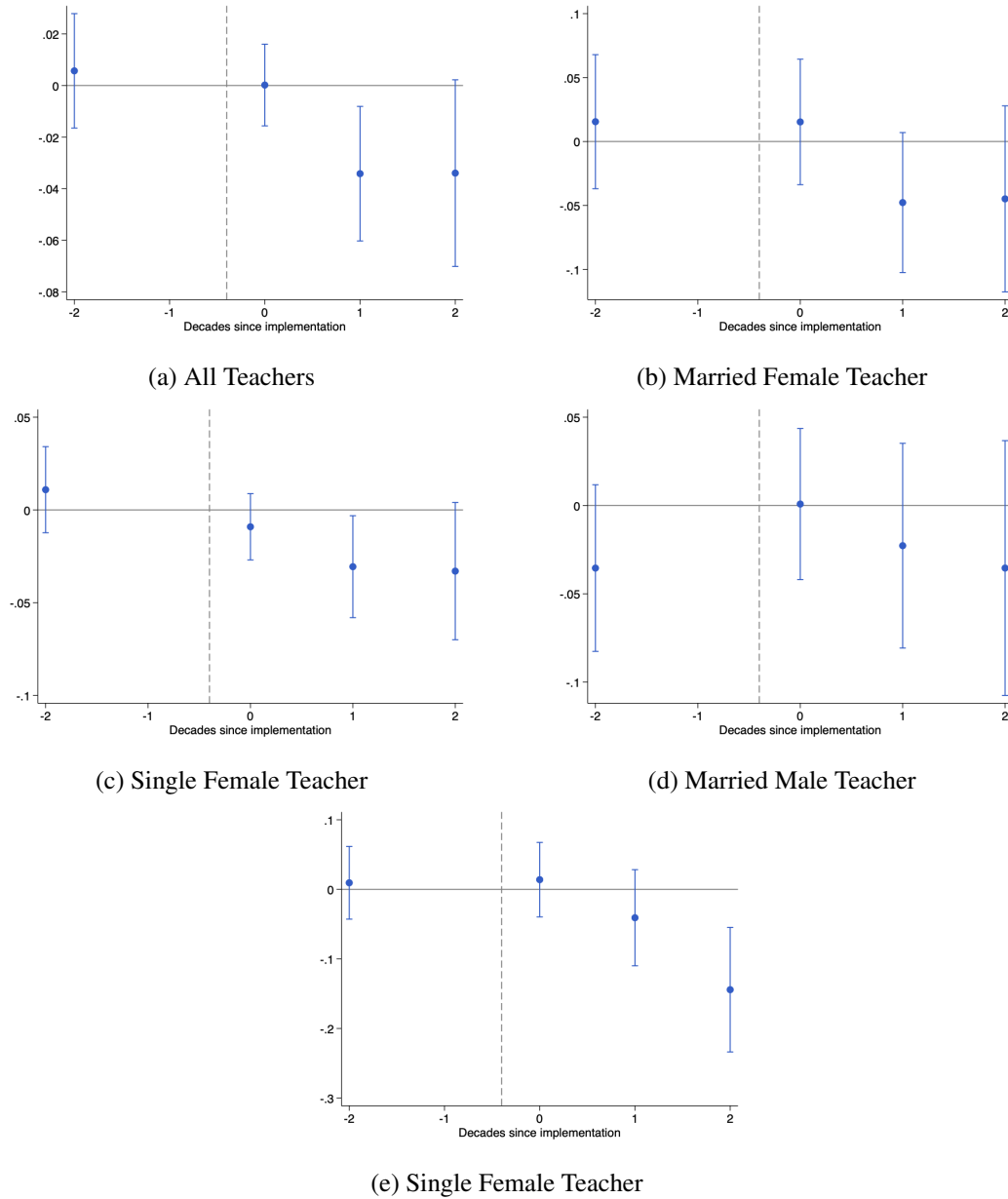
(a) All Teachers

(b) Married Female Teacher

(c) Single Female Teacher

(d) Married Male Teacher

(e) Single Female Teacher

Figure A2: Effects of Marriage Bar on Teacher Retention Rates by Group

Source: Data comes from 1880-1940 US full count census, and linking across census uses Census Tree Project.
Note: These figures show the event-study coefficients that analysis the effect of marriage bar on retention rates of all teachers and by marital status and gender groups. All regressions include controls for log of city population, city and year fixed effects. Standard errors are clustered at city level. The sample consists of all cities in CEBSE, more than 1,000 population in 1900, and with 6 census-year balanced panel.

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