# THE CAUSES AND CONSEQUENCES OF FAMILY-BASED DISCRIMINATION: EVIDENCE FROM THE TEACHER MARRIAGE BAR*

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**Abstract**

Women's increased participation in the workforce was one of the most significant changes in the US labor market over the past century (Goldin 2006). An often-cited but understudied reason participation rates remained so low during the first half of the twentieth century was the marriage bar, a policy that prohibited married women from working (Goldin 1990, chap. 6). We collect new data on the adoption of marriage bars in teaching across US cities between 1900 and 1940. Using the 1880–1940 full-count censuses and a difference-in-differences design, we show that marriage bars changed the composition of teachers. They decreased the share of married women teachers by 3.9 percentage points (22 percent) and increased the share of single women teachers by 3.1 percentage points. The decline came from fewer married women being hired and fewer teachers remaining after they married. These effects were large enough to decrease the share of married women in local professional and white-collar work overall. We find little evidence that the composition effects persisted after the bars were likely abandoned during World War II. Using linked censuses, we then follow single female teachers to assess how marriage bars affected the trajectory of their work and family lives. We show that while marriage bars did not change whether these women married or worked, they did change whether the two were combined; young single teachers were 20 percent less likely to be married and in the labor force a decade later, driven almost entirely by teaching. Discriminatory employment policies therefore shaped both who worked in an occupation and the family decisions of the women in it.

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# 1 Introduction

Women's labor force participation grew dramatically across the twentieth century, driven largely by women's increasing persistence in the labor force throughout the life cycle, including after marriage (Goldin 2006). Although this trend accelerated nationally after 1940, married women's labor force participation began rising earlier. Between 1900 and 1940, the share of married women in the labor force rose from 3 to 13 percent. However, this early growth was uneven across the United States, and even more so within specific occupations. Place mattered more for whether a married woman could continue teaching than it did for any other occupation (Figure 1). In 1940, 44 percent of female teachers were married in Arkansas, Florida, and Oklahoma, compared with only 8 percent in Iowa, Massachusetts, and Rhode Island.

One likely reason for this variation was the use of marriage bar policies which restricted married women from working (Goldin 1990, chap. 6). While such discriminatory policies were a prominent feature of women's labor markets in the early to mid-twentieth century in the US, gender-based restrictions on economic activity continue to remain a widespread challenge today. Across 190 economies, women have two-thirds of the economic rights afforded to men, and only 4 percent of women live in economies with full legal equality (World Bank 2026). By restricting access to employment and occupations, discriminatory barriers can distort talent allocation and impose substantial aggregate costs (Hsieh et al. 2019). A growing literature studies how legislation and institutional reforms that prohibit discrimination expand women's access to employment and improve their labor market outcomes (Goldin and Rouse 2000; Stevenson 2010; Bailey, Helgerman and Stuart 2024; Kuhn and Shen 2023; Kim and Tsao 2025). Yet we know much less about the consequences discriminatory employment institutions have while they remain in effect.

In this paper, we study the causes and consequences of the marriage bar. Marriage bars prohibited employers from hiring married women or required female employees to leave their jobs upon marriage. These restrictions were particularly common in teaching, clerical work, and the civil service, which provided women with some of their main opportunities for white-collar and professional employment. We focus on teaching, where local school boards generally determined hiring and personnel policies (Goldhammer 1964). Because this policy was adopted in a decentralized

manner, no systematic record exists of where and when marriage bars were adopted and used.[1]

To address the lack of existing data on marriage bars, we construct the first systematic city-level dataset tracking teacher marriage bars across the United States from 1900 to 1940. We manually collect information from the *American School Board Journal* and historical newspaper archives to document each marriage bar's adoption and repeal. We focus on cities with citywide school systems, where a school board's decision applied to every school in the city. We combine these data with full-count U.S. Census data from 1880 to 1940. Our main sample includes 133 cities that adopted a marriage bar between 1911 and 1938 and 460 cities with no recorded bar. Because school boards adopted marriage bars in different years, we estimate the effects of the marriage bar on the teaching workforce and women's labor and family outcomes using a generalized difference-in-differences design implemented with the Callaway and Sant'Anna (2021) estimator.

We first examine the determinants of marriage bars. We show that among our sample of large cities, marriage bars were common in all regions of the country, but were somewhat less common in the West. We also show that the marriage bars were most commonly adopted in the 1920s and 1930s. Before adoption, cities that later adopted a bar were similar to cities that never did in teacher characteristics, though they were more urban. Only one of 18 city and teacher characteristics changed differently between 1880 and 1900, both before any adoption in our sample, and the event-study estimates are flat in the censuses before adoption. In contrast to historical narratives that argue fiscal pressures drove adoption, we find little evidence that, among cities that had not yet adopted marriage bars by the Great Depression, the severity of the Great Depression predicted which cities adopted. This investigation supports our subsequent analysis comparing cities that adopted versus those that did not adopt, using a difference-in-differences framework.

We show that adoption reduced the share of married female teachers by 3.9 percentage points, or 22 percent of the control mean, and reduced the number of married female teachers per 100 children by 0.16, or 30 percent of the control mean. These declines were offset by an increase in single female teachers, with no change in the total number of teachers per student or the gender composition of local teachers. This pattern is consistent with a commonly stated rationale for marriage bars, which was to ration teaching positions away from married women and toward single

[1]The most systematic existing source is a series of surveys conducted by the National Education Association. However, these surveys provide only intermittent snapshots of whether school boards employed marriage bars and offer limited ability to trace how these policies changed across places and over time.

women. Our results are robust to controlling for time-varying city characteristics and region-by-year fixed effects, and to using an interaction-weighted estimator that allows for heterogeneous treatment effects (Sun and Abraham 2021). However, they are smaller with state-by-year fixed effects.

Because teaching was a large share of women's professional work, we show that the teaching marriage bar changed aggregate rates of married women's professional work. Across our sample, teachers make up about a quarter of professional workers and a twentieth of white-collar workers. Adoption decreased the married share of professional workers by 0.98 percentage points (9 percent) and of white-collar workers by 0.61 percentage points (6 percent). When we remove teachers from these groups, the estimates are small and not statistically significant.

However, we find no evidence that the marriage bar policy's effects lasted beyond when it was likely enforced. Marriage bars in teaching were nearly eliminated by the 1950s (Goldin 1990, chap. 6), consistent with the rise of married women filling teacher vacancies during World War II (Briskin 2025). Because the 1950 census does not identify cities, we aggregate our analysis to the county level. Using 1950 county data, we show that counties that adopted a bar had a married share of teachers 3.2 percentage points lower in 1940 but only 0.4 percentage points lower by 1950, and this difference is not statistically significant. This suggests that the marriage bar changed who taught, but only during the period in which it was actively enforced; this suggests that the marriage bar did not work to increase social norms or stigma against married women's work, at least among the group of women with intentions to teach.

Next, we use linked censuses to ask how the composition of teachers changed. By tracing each teacher back to the previous census, we find that the decline in married teachers came from both fewer married women being hired and fewer teachers remaining after they married. Marriage bars therefore operated as both hiring and retention bars, but largely exempted, or "grandfathered," married women who were already teaching. Other characteristics of new hires, such as their age, race, and nativity, changed little, suggesting that the marriage bar had little effect on selection into teaching along other, non-family characteristics.

Finally, we use the linked dataset to follow single women who were already teaching when we first observe them. Because marriage bars raised the cost of combining marriage with a teaching career, women may have responded by leaving teaching, delaying marriage, or leaving the labor

force. We find that overall the bar had little effect on whether they married or whether they worked, but they were 20 percent (1.74 percentage points) less likely to be married and in the labor force a decade later. This decline is driven almost entirely by teaching, with no evidence that these women moved into other work. The effects are concentrated among younger women and in cities with the highest pre-adoption labor force participation among married women. We also find suggestive evidence that single teachers married husbands with higher occupation scores, possibly to compensate for the marriage bar limiting women's own ability to persist in the labor force after marriage.

This paper makes three main contributions. First, we contribute to research on the rise in married women's labor force participation. This rise was driven largely by women's increasing persistence in the labor force after marriage (Goldin 2006). This literature has identified several forces that allowed married women to keep working, including improvements in maternal health (Albanesi and Olivetti 2016), labor-saving household technology (Greenwood, Seshadri and Yorukoglu 2005), the oral contraceptive pill (Goldin and Katz 2002; Bailey 2006), and changing beliefs about women's work (Fernández, Fogli and Olivetti 2004; Fernández 2013). Studies of this rise also point to marriage bars as a key constraint on married women's work before 1950, but they have not been able to measure them.

We collect data that allow us to study this policy institution directly. Goldin (1990, chap. 6) studies the factors associated with the adoption of marriage bars, Kim and Tsao (2025) study their prohibition in Kentucky and North Carolina, and Mosca and Wright (2020) show that Irish women who reported having been affected by a marriage bar had shorter working lives, lower personal income, and more children. We instead study the implementation of marriage bars across US cities. We document the policy's extent and timing, show that it operated through both hiring and retention bars, and find that it reshaped the teaching workforce away from married women, with no effect on workforce size. We also document its effects on the lives of the individual women teachers it affected.

Second, we contribute to the broader literature on discriminatory labor-market institutions. Much of the modern discrimination literature measures latent discrimination using audit and correspondence studies or related experimental approaches (Bertrand and Duflo 2017). Related literature examines changes in laws and personnel practices that prohibit discrimination or reduce discriminatory barriers to employment (Goldin and Rouse 2000; Stevenson 2010; Kuhn and Shen

2023; Bailey, Helgerman and Stuart 2024; Kim and Tsao 2025). Instead, we study the introduction of a discriminatory policy and observe where and when local decisions imposed it. By studying the introduction of a discriminatory policy, we are able to shed light on the determinants of discriminatory policies and test whether the landscape that such policies create persists after their repeal. In our setting, we find limited evidence of this, suggesting that removing discriminatory policies can be powerful in equalizing access to employment.

Third, we contribute to research on how women combine career and family (Goldin 2021), and on how they respond when policies change the costs of combining the two. Much of this work studies policies that lowered these costs, such as access to the contraceptive pill (Goldin and Katz 2002; Bailey 2006), parental leave (Lalive and Zweimüller 2009; Kleven et al. 2024), and child care (Havnes and Mogstad 2011). Marriage bars instead raised the cost of combining marriage with a teaching career and made the trade-off explicit, since a teacher who married in a city with a bar could not continue teaching there. We show that young teachers facing this trade-off did not delay or forgo marriage. Instead, they married at the same rate, and fewer of them continued working after marriage. This suggests that, for most women in this period, marriage decisions did not respond to the loss of a career, and that women's work bore the policy's cost.

The remainder of the paper is organized as follows. Section 2 describes marriage bars in teaching. Section 3 describes our data on marriage-bar adoption and census data. Section 4 presents our empirical strategy and examines its identifying assumptions. Section 5 presents the effects on the composition of the teaching workforce; Section 6 examines the sources of this change, through hiring and leaving at marriage; Section 7 presents the effects on incumbent teachers; and Section 8 concludes.

# 2 Background

Marriage bars were employment policies that prohibited the hiring or continued employment of married women. In the US, marriage bars began in the late nineteenth century and, in some places, lasted until the 1960s, when federal legislation, including the Equal Pay Act and the Civil Rights Act, prohibited sex-based discrimination. More commonly, however, they were relaxed during World War II, when teacher shortages created a need that married women filled (Briskin

2025). Marriage bars were also common in other countries, including the Netherlands, Ireland, and the United Kingdom. Marriage bars were not limited to teaching. Many firms, especially in clerical work, did not hire married women or required women to leave when they married (Goldin 1990, chap. 6). The federal government also restricted married women's employment. Section 213 of the Economy Act of 1932 required that, when staff was reduced, married employees whose spouses also worked for the federal government be dismissed first, and most of those dismissed were women; the provision was repealed in 1937. Unlike rules set by individual firms or the federal government, teacher marriage bars were set by local school boards, giving them more far-reaching effects on the local composition of teachers than a single firm's policy could have on its own workers. Marriage bars were also most common in teaching relative to other occupations (Shallcross 1940). Together, these forces likely contributed to why whether one taught after marriage was highly dependent on place, and more so than in any other occupation (Figure 1).

Marriage bars operated in two forms. Under a hiring bar, school boards refused to hire married women; under a retention bar, they dismissed teachers when they married. The two forms imply different changes in the teaching force. A hiring bar results in fewer married women being hired relative to cities without a bar, while a retention bar results in teachers leaving when they marry. In some cases, we also find accounts of school boards that continued to hire married women, but only as substitute teachers, which would change their contracts rather than the number of married women teaching. Our data do not fully distinguish between these forms, but in Section 6 we use linked census records to separate hiring from exits at marriage.

In the teaching setting, local school boards decided whether to operate a marriage bar. A school board was a state-designated governing body that managed the local school district, subject to the state constitution and regulations. Although their powers varied by state, school boards generally had discretion over contracts, property, and hiring (Goldhammer 1964). Consistent with this, nearly all of the adoptions in our data were made by city or township school boards.

The most common reason given for adopting a marriage bar was to ration teaching positions toward single women.[2] The argument was that married women could be supported by their hus-

[2]For instance, in 1923, the board of education of Quincy, Illinois, ruled against the employment of married women teachers. A local newspaper 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, about the reasons for its action. It was generally believed that the employment of married women was an injustice to single women who sought positions.

bands, while single women needed the income more. This argument grew stronger during economic downturns such as the Great Depression, when many new graduates competed for a limited number of positions. In places with tenure policies, the financial incentive for a marriage bar was even stronger, given the high cost of employing married teachers (Goldin 1990, chap. 6). Conversely, laws loosened when there were economic motivations to hire married women. For example, during teacher shortages such as those in World War I and World War II, school boards encouraged married women to return to teaching by repealing marriage bars; however, because of data limitations, we do not use repeals in our estimation.

Many of the above arguments justifying the marriage bar appear in a cartoon titled "The Married Teacher Problem," published on the cover of the October 1924 *American School Board Journal* (Figure 2). It depicts a representative member of the school board weighing "points for consideration" when deciding between a married and an unmarried teacher, including teaching ability, the need for self-support, outside interests, and family.

Figure 3a shows the timing of adoptions and repeals in our data. Panel (a) includes every adoption and repeal we collected, and panel (b) shows the adoption years of the 133 cities in our estimation sample. The timing in our data matches historical accounts. Most adoptions occurred in the 1920s and 1930s, and most repeals occurred during the teacher shortages of World War I and World War II, peaking in 1918 and 1942. Figure 4 shows the sample cities, with adopting cities marked separately. The adopting cities span 28 states, but almost half are in the Northeast; Massachusetts and Pennsylvania have the most, with 17 cities each.

# 3 Data

## 3.1 Marriage Bar Policy Collection

We collect marriage bar data from two main sources. First, we manually collected marriage bar policies reported in the *American School Board Journal*, a monthly education trade journal. Second, we supplement this data with information from newspaper archives (`newspapers.com`).

The *American School Board Journal* (ASBJ) is a monthly education magazine that began publishing in 1891 and continues to publish today. It covers a broad range of topics related to school

governance and management, policy making, and student achievement, and its main subscribers are school board clerks, board members, and superintendents. We manually searched journals published between 1900 and 1940 and digitized every report of a marriage bar policy, yielding adoption dates for 281 cities.

However, we could not determine how the ASBJ collected this information. In particular, the absence of a marriage bar report in the ASBJ could mean that a city had no marriage bar, or simply that the city did not report to the ASBJ. To address this concern, we systematically searched newspaper archives for information on marriage bars in a selected set of large city school systems, which gives us adoption information for another 97 cities. For cities with multiple adoption dates, or with a repeal followed by a second adoption, we use the date of the first adoption. This lets us interpret our estimates as the effect of first adoption, rather than an effect that depends on earlier adoption practices.

To our knowledge, these are the most comprehensive data on marriage bar policies in the US. Nevertheless, we expect measurement error in our adoption variable. First, some cities in our control group may have adopted a bar that does not appear in the ASBJ or in newspapers. This would bias our estimates toward zero; significant estimates may therefore understate the true effects and some null estimates may reflect measurement error. Second, some cities in our treatment group may be false positives. We investigate this by checking the cities reported in the ASBJ against newspaper archives, and we can verify every city in newspaper sources.[3] However, we do find differences in the year of adoption; newspapers usually report an earlier date than the ASBJ. Finally, as shown in Figure 3a, the number of cities in our data is far below the reported prevalence of marriage bars, which by the 1930s were reported in up to 75 percent of school boards. This is less of a concern for causal interpretation, since our analysis relies on variation within our sample, but it does raise questions about external validity.

## 3.2 Census Data and Crosswalks

Next, we use the IPUMS full-count censuses from 1880 to 1940 to build a panel of city characteristics, including the number, share, and average age of teachers by sex and marital status

[3] So far, we have checked the cities in Massachusetts, one of the two states with the largest number of treated cities in our sample.

(Ruggles et al. 2024). We identify cities using the individual-to-place crosswalks created by the Census Place Project (Berkes, Karger and Nencka 2023), which give consistent place identifiers over time.

We use this dataset to build a panel of cities over time. For each city and census year, we also measure population, whether the city is urban, the share of residents who are Black, the share aged 5 to 16, the share of farmers, the average occupation score of men, the labor force participation of women and of married women, and the married share of professional and white-collar workers.

We also build a similar panel following individual teachers over time. To do this, we use the links created by the Census Tree Project (Price et al. 2021; Buckles et al. 2023). When using the linked panel, our main outcomes are status transitions between censuses. For women teaching at a census, we record whether they were teaching and whether they were married ten years earlier. For never-married women teaching at a census, we record whether they are married, in the labor force, and teaching ten years later.

In Section 7, we also show that the share of a city's single teachers that we can link to the next census does not change when the city adopts a bar.

## 3.3 Sample Selection

We restrict our sample to city school systems that appear in the Commissioner of Education reports and the Biennial Survey of Education (CEBSE). First, limiting our analysis to these cities made data collection feasible because we could focus the newspaper search on these select cities. Second, because these cities operated citywide school systems, school board decisions likely applied to every school in the city, whereas the jurisdiction of town and county school boards is often less clear. In our main analysis, we drop cities that adopted a marriage bar between 1900 and 1910 because our analysis of individual teachers requires a link between a baseline census and a subsequent census, and since the 1890 census is not available, we have no "pre-period" for those adopting before 1910. We also drop cities in Michigan, where the 1937 Teachers' Tenure Act limited dismissals of tenured teachers to reasonable and just cause (Michigan Legislature 1937), and in Kentucky and North Carolina, where state laws prohibited marriage bars (Kim and Tsao 2025). Our final sample includes 593 cities, of which 133 adopted a bar, and 460 did not.

Our individual sample includes women who were never married and worked as teachers in a sample city's school system at one census from 1900 to 1930 and who are linked to the next census. We assign each woman the marriage bar status of the city in which she taught at the earlier census.

# 4 Empirical Strategy

## 4.1 Teacher Composition

Our identifying variation stems from differences in local school boards' decisions to adopt the marriage bar across cities and over time. The following event-study equation characterizes our empirical strategy.

$$y_{ct} = \alpha_0 + \sum_{k=-3, k\neq -1}^{2} \beta_k AdoptMB_{ct}^{k} + D_c + D_t + X_{ct} + \epsilon_{ct} \tag{1}$$

where $y_{ct}$ is the outcome of interest, $c$ denotes the city, $t$ is the census year, and $k$ denotes census years relative to marriage bar adoption. For each city, $k = 0$ is the first census year at or after adoption, and $AdoptMB_{ct}^{k}$ indicates the census year relative to the adoption census. We omit $k = -1$ to interpret each coefficient as the effect of adoption at that event time relative to the census before adoption. We bin event time at three or more censuses before adoption and at two or more censuses after. City fixed effects $D_c$ absorb city-specific differences that are constant over time, for example, prevailing social norms toward married women's work. Year fixed effects $D_t$ absorb shocks common to all cities, such as the macroeconomic environment. We include city population as a control in $X_{ct}$ and cluster standard errors at the city level.

Equation 1 helps clarify the sources of our identifying variation. However, with heterogeneous treatment effects by adoption time, the standard two-way fixed effects estimator yields a biased estimate (Goodman-Bacon 2021; de Chaisemartin and D'Haultfœuille 2020; Borusyak, Jaravel and Spiess 2024). We therefore use the estimator of Callaway and Sant'Anna (2021), which estimates a separate average treatment effect for each adoption cohort $g$ and census year $t$, $ATT(g,t)$, and compares each cohort only to cities that never adopt. We estimate each $ATT(g,t)$ using the doubly robust method of Sant'Anna and Zhao (2020), which combines outcome regression with inverse probability weighting and is consistent if either is correctly specified; we also take long differences

from the census before adoption. Averaging over cohorts and post-adoption periods gives the overall effect we report in tables, and averaging at a fixed distance from adoption gives the event-study estimates we report in figures, which are comparable to the $\beta_k$ above. Because these averages use non-negative weights, neither is affected by the negative weights of two-way fixed effects.

We weight each estimate by the outcome's own denominator; shares of teachers are weighted by the number of teachers, and teachers per 100 children by the number of children. The weighted mean is then the aggregate quantity, such as total married teachers over total teachers, rather than an average of city-level ratios.

We report five robustness checks alongside our main estimates, which we describe in Section 5.4. They include two-way fixed effects with and without controls for city composition, region-by-year and state-by-year fixed effects, and the interaction-weighted estimator of Sun and Abraham (2021).

## 4.2 Sources of Composition Change

To study how teacher composition changed, whether through hiring or retention, we link each female teacher to the previous census. This link allows us to identify three types of married teachers. Some were already married but not teaching at the previous census and were hired within the decade, which a hiring bar would prevent. Others were single at the previous census and married by the current one. This group can reflect either margin; a retention bar applies to women who married while teaching, and for women hired during the decade, we do not observe whether they married before or after they were hired. The rest were already married and teaching at the previous census. We count the teachers in each group by city and census year and estimate the effect of adoption on each group's share of linked female teachers, using the same estimator as in Section 4.1 and weighting by the number of linked female teachers. Because the shares add up to the married share of linked teachers, the estimates decompose the total effect into these sources. We measure new-hire characteristics the same way, defining a new hire as a teacher linked to the previous census who was not teaching then.

## 4.3 Incumbent Teachers

Whereas Section 4.2 links teachers back to the previous census, here we follow incumbent teachers forward. This allows us to estimate the effects of the marriage bar on women's labor and family decisions. Our sample consists of women observed as never-married teachers in a city school at one census, whom we link to the next census to measure their outcomes. We assign treatment based on the city where they taught in the earlier census, the baseline census. The design is the same as in Section 4.1. Cities adopt at different dates, and we compare women whose baseline city adopts a bar to women whose city never does. We divide women into three groups by their age at baseline, 18 to 27, 28 to 37, and 38 to 47. Each group spans ten years, which is the same as the number of years between censuses, to ensure that no woman is counted twice within a group. Our main sample is women aged 18 to 27.

We use the same estimator, Callaway and Sant'Anna (2021), with never-treated cities as the comparison group and the doubly robust method of Sant'Anna and Zhao (2020). Because the unit of observation is now the woman rather than the city, we cluster standard errors at the baseline city. Estimates are unweighted, since each woman is one observation.

## 4.4 Determinants of the Marriage Bar

We first assess which factors are associated with adopting the marriage bar. Table 1 compares treated and control cities in 1880, before any adoption. Treated cities were more likely to be urban (76 percent vs. 66 percent) but otherwise similar to control cities in population, racial composition, men's occupation scores, the share of farmers, and women's labor force participation. Their teachers' characteristics were also similar.

Adopting cities were also spread across regions much like the comparison cities; 47 percent were in the Northeast, 28 percent in the Midwest, 18 percent in the South, and 7 percent in the West, compared with 46, 24, 19, and 11 percent of control cities. Most adoptions took place in the 1920s. Many bars were relaxed during World War II (Briskin 2025), although we record a repeal between 1941 and 1946 for only 13 of the 133 adopting cities. Of the adopting cities, 14 percent adopted a bar between 1911 and 1920, 59 percent between 1921 and 1930, and 28 percent between 1931 and 1939. Timing differed by region; cities in the Northeast and Midwest adopted mainly in

the 1920s, while cities in the South adopted mainly in the 1930s. Only nine of the adopting cities were in the West.

We also examine which city characteristics predicted adoption (Table A1). Larger cities were more likely to adopt a bar; a one-standard-deviation increase in log population in 1910 raises the probability of adoption by 8.0 percentage points, from 23 percent. Among cities without a bar in 1930, cities with a higher married share of teachers were more likely to adopt one in the 1930s. However, the severity of the Great Depression, measured by the fall in county retail sales between 1929 and 1933, does not predict whether a city adopted a bar.

Because our design exploits changes within cities over time, any preexisting differences in levels between adopting and other cities are absorbed by city fixed effects. Instead, our identifying assumption is that, absent the bar, outcomes in adopting cities would have followed non-adoption cities in parallel. To test this, we check whether city and teacher characteristics changed differently between 1880 and 1900, both of which precede every adoption in our sample.

To do so, we estimate a difference-in-differences model restricted to the 1880 and 1900 censuses:

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

Estimates of $\beta_1$ are shown in columns (7) and (8) of Table 1. Of the 18 characteristics we test, only one is significant at the 10 percent level; the share of teachers who are single women grows more slowly in treated cities in the unweighted test. The coefficient is small, 2.9 percentage points on a base of 65 percent, and it is not significant when we weight cities as in our estimation. In Section 5, we also show that the event-study estimates before adoption are not significantly different from zero.

# 5 Composition of the Teaching Workforce

## 5.1 Married and Single Teachers

In this section, we ask how the marriage bar changed the composition of the teaching workforce. Figure 5 plots event-study coefficients and 95 percent confidence intervals for the married

share of teachers and married female teachers per 100 children, and Table 2 and Table 3 report the corresponding difference-in-differences estimates for each sex and marital-status group. The estimates before adoption support our identifying assumption; the coefficients for three and two censuses before adoption are 0.19 and 0.15 percentage points, and neither is statistically different from zero.

City adoption of the marriage bar reduces the share of married female teachers by 3.9 percentage points ($p < 0.001$; Table 2, Column 2); this is a 22 percent decline relative to the control-city mean of 17.3 percent. This effect grows with exposure, from 2.4 percentage points at adoption to 5.0 at ten years and 6.8 at twenty. We also find that the number of married female teachers per 100 children falls by 0.163 ($p < 0.001$), or 30 percent of the control mean of 0.549; note that this estimate is large relative to the baseline, but small relative to the teaching workforce.[4]

The decrease in married female teachers is almost completely offset by a rise in single female teachers. Single female teachers per 100 children rise by 0.204 ($p = 0.001$), and their share of all teachers rises by 3.1 percentage points ($p < 0.001$).

Consistent with evidence on full substitution from married to single teachers, we find no evidence that the teaching workforce size changed. The estimate for total teachers per 100 children is 0.079. It is not statistically different from zero (Table 3, Column 1). Estimates for male teachers are small and not statistically significant in both share and rate. Cities therefore taught the same number of children after adopting a bar, but with fewer married and more single women teachers.

To put these estimates in perspective, the 133 adopting cities had about 1.7 million children aged 5 to 16 in 1940. An effect of 0.163 married women teachers per 100 children implies that these cities had about 2,700 fewer married women teachers in 1940 than they would have had without a bar, compared with the 13,600 married women who were teaching in them.

## 5.2 Professional and White-Collar Work

Teaching was a large occupation. On average, a city in our sample has 285 teachers, 1,124 professional workers, and 5,758 white-collar workers. Therefore, teachers made up about a quarter

[4]Our estimates are similar in size to those of Kim and Tsao (2025), who find that state laws prohibiting marriage bars increased the married share of teachers by 3.5 to 4 percentage points. Our estimates come from the adoption of bars by local school boards in 28 states, rather than from their prohibition in two.

of professional workers and a twentieth of white-collar workers. An effect of the size we estimate should be visible in these aggregates even if nothing outside teaching changed. Table 4 tests this by estimating the same specification for broader occupational groups, each shown twice, once including teachers and once excluding them.

The married share of professional workers falls by 0.98 percentage points ($p = 0.001$) on a control mean of 10.9 percent, and the married share of white-collar workers falls by 0.61 percentage points ($p = 0.07$) on a control mean of 10.4 percent. When teachers are removed, both estimates shrink and are not statistically significant. The estimates are $-0.22$ ($p = 0.40$) for professional work excluding teachers and $-0.46$ ($p = 0.17$) for white-collar work excluding teachers. The aggregate effects therefore come from teaching. Figure 6 shows the same pattern in event time. Professional work, including teachers, falls after the adoption of marriage bars and continues to decline in subsequent decades; in contrast, when teachers are excluded, professional work does not fall immediately after the adoption. We discuss the other occupations in Table 4 in Section 5.4.

## 5.3 Persistence

Marriage bars largely ended during World War II, when teachers left for military service and school boards, facing shortages, could no longer ration jobs (Briskin 2025; Goldin 1990). To test whether the change in composition outlasted the policy, we observe whether the effects of the marriage bar persisted through 1950. The 1950 census allows this, but only at the county level, since the place crosswalk that defines our city panel is not available for 1950. We therefore construct a county-level panel, assigning each sample city to the county containing most of its population. A county is treated if all of its sample cities adopted a bar and is a control if none did. We drop counties with both, which leaves 81 treated and 299 control counties. Because a county contains school districts that never adopted a bar, the county estimates should be smaller than the city estimates.

Figure 7 reports estimates by calendar year rather than event time for both cities and counties, since adoption cohorts reach a given event time in different years and no single event-time coefficient corresponds to 1950. The city estimates are 2.4 percentage points in 1920, 3.4 in 1930, and 4.6 in 1940. The county estimates are similar while bars were in force; 2.6 percentage points in

1920, 2.1 in 1930, and 3.2 in 1940. The county gap disappears in 1950, when the estimate is 0.4 percentage points with a standard error of 1.4. By 1950, the married share of women teachers had risen nationally from about a quarter to over a half (Figure 1), and marriage bar cities appear to have caught up.

## 5.4 Robustness

We conduct a series of checks to show our results are robust to alternative specifications. We summarize the teacher composition results in Figure A1. The first series (in black) is our baseline Callaway–Sant'Anna event study.

The second series is a two-way fixed effects event study with city and year fixed effects. As shown by the recent econometric literature, these estimates may be biased if there is heterogeneity in treatment effects between treated cities and/or across time (de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021; Borusyak, Jaravel and Spiess 2024). In our setting, the two estimators agree closely.

Third, to account for other potentially varying city-level factors, we add city-level and time-varying controls, including the Black share of the population, the share of the population aged 5 to 16, and married women's labor force participation. However, as Callaway and Sant'Anna (2021) note, time-varying controls may also capture treatment effects, which is why this is not our main specification.

Fourth and fifth, we include region-by-year and state-by-year fixed effects to control for common regional or state-level shocks, such as differential effects of the World Wars or the Great Depression. The consistent sign of these results supports our identification strategy. However, we note that the coefficients on the share of married female teachers are substantially absorbed after controlling for state-by-year fixed effects. This is likely because, on average, each state has only 13 cities in the sample, with a range of 1 to 72, so state-by-year fixed effects are quite conservative here.

Finally, we implement the interaction-weighted estimator proposed by Sun and Abraham (2021), which addresses heterogeneous treatment effects by a different route and provides a weighted average of cohort effects that avoids the negative weights of standard two-way fixed effects. These

estimates track our baseline closely. Including the 33 cities that adopted a bar between 1901 and 1910 gives similar composition estimates; the married share of teachers falls by 3.7 percentage points. Our main sample excludes Michigan, Kentucky, and North Carolina because of state laws passed in the 1930s (Section 3.3). Keeping their cities but dropping their 1940 observations also gives similar estimates; the married share of teachers falls by 3.2 percentage points (Table A2).

We also estimate the same specification for other occupations (Table 4 and Figure A2). Nursing shows no effect ($-0.63$, $p = 0.52$). There is some evidence that the married share of clerical work falls, but its effect size is a quarter of the size of the teaching effect ($-1.04$, $p = 0.05$).

# 6 Sources of Composition Change

The results so far show that adopting a bar decreased the share of married teachers in a city. However, there are several different channels through which the share could fall, including married women being dismissed, fewer married women being hired, fewer teachers marrying, or married teachers moving to other districts. Our linked sample, by following the same women over time, allows us to separate these explanations in understanding the overall changes to the teacher composition.

Table 5 decomposes the married share of linked teachers by status at the previous census. Row 1 is women who were married but not teaching in the city, row 2 is women who were single and have since married, and row 3 is women who were married and teaching. These groups are mutually exclusive and therefore sum to the total effect reported in each column.[5] Estimates are weighted by the number of linked female teachers.

At the first census after adoption, the share of married teachers is 2.2 percentage points lower in adopting cities (Table 5, Column 2). This is driven by a 1.1 percentage point decline in female teachers who married and were retained as teachers, and a 0.5 percentage point decline in the share of married new hires. We also see that the share of women who were married and teaching in both censuses declines by 0.6 percentage points but is not statistically significant, potentially consistent with women who were already married and employed being "grandfathered" in before the policy took effect.

[5] About a fifth of teachers cannot be linked to the previous census and are not included; see the note to Table 5.

By the second and third censuses after adoption, the decline grows to 6.5 percentage points (Column 3), of which 1.2 comes from married new hires, 1.0 from teachers who married during the decade, and 4.2 from women who were married and teaching at both censuses. The last component reflects a decline in women who would have taught for ten or more years while married. Marriage bars therefore operated through hiring and retention after marriage and largely exempted married women already teaching when the bar was adopted.

We use this data setup to explore whether the marriage bar affected selection into teaching based on other characteristics. However, apart from having fewer children at home, which follows from fewer married hires, the characteristics of new hires changed little. We find few differences, all small in magnitude, in new hires' age, race, nativity, and fathers' occupations (Table A3). Small differences that do exist in the hiring margin, for example new hires are slightly younger, do not translate into aggregate changes in the age distribution of teachers in the workforce (Table A4).

# 7 Effects on Incumbent Teachers

We next ask how incumbent teachers responded to the marriage bar. A teacher in a city with a bar could respond by leaving teaching for another job when she married, by leaving the labor force altogether, or by marrying less to keep teaching. The bar could also change whom she married. To study these responses, we follow women who were never married and teaching in a city school at one census and observe them again a decade later. The main sample includes women aged 18 to 27 at baseline, and Panels B and C of each table repeat the analysis for women aged 28 to 37 and 38 to 47.

## 7.1 Marriage

We find no evidence that women's decision to marry is affected. The probability of being married within the decade changes by $-0.49$ percentage points, with a control mean of 40.2 percent and a standard error of 1.07 (Table 6, Column 4). This estimate is precise enough to rule out effects larger than about two percentage points in either direction.

## 7.2 Labor Force Participation

Labor force participation is also unaffected. Exposed women are 1.04 percentage points less likely to be in the labor force a decade later, with a standard error of 0.96, on a control mean of 64.0 percent (Table 6, Column 1), and the estimates for teaching and non-teaching work are also not statistically significant. Although the bar removed married women from teaching, it did not, on net, remove these women from the labor force.

## 7.3 Combining Marriage and Work

Table 7 reports joint indicators of marital status and work a decade later. Because the columns partition the sample, none of these outcomes condition on marriage or work, which the bar could itself affect. The bar reduced the probability of being both married and in the labor force by 1.74 percentage points ($p = 0.006$) on a control mean of 8.60 percent, about a fifth of the control mean, and the probability of being both married and teaching by 1.58 percentage points ($p = 0.005$) on a mean of 6.07 percent (Table 7, Columns 1–2). The estimate for being married and working outside teaching is $-0.17$ (0.25). The decline is concentrated in teaching, the occupation the bar covers. Being married and out of the labor force increases by 1.25 percentage points (0.87), which offsets most of the decline in being married and in the labor force, although this estimate is not statistically significant. The estimates for women who remained never married are small and not statistically significant; being never married and in the labor force increases by 0.70 (1.12) and being never married and teaching by 0.99 (1.25).

Figure 8 shows these estimates in event time. Before adoption, the estimates for being married and in the labor force are $-1.24$ (1.12) three censuses before and 0.11 (0.46) two censuses before, and neither is statistically significant. The decline begins at adoption, at $-0.82$ (0.57), and grows to $-2.70$ (0.92) one census later. The estimates for being married and working in another occupation remain close to zero in every census.

Table A5 shows the same pattern, but with children instead of marital status. Having a child and being in the labor force falls by 0.81 percentage points ($p = 0.004$), and having a child and teaching by 0.85 percentage points ($p = 0.001$), while the estimates for women without children are small and not statistically significant. The bar therefore restricted combining family with work,

rather than marriage itself.

Panels B and C of Table 7 and Figure 9 show how the effect varies with age. The effect is concentrated among women aged 18 to 27 at baseline, and it is smaller in the two older groups, at $-0.66$ and $-0.52$, neither of which is statistically significant. This is consistent with the bar mainly affecting the decision to keep teaching after marriage. Women in Panel C are also a selected group, since remaining a never-married teacher to that age is itself partly a result of exposure; their marriage rate in control cities is 5.7 percent, compared with 40.2 percent in Panel A.

## 7.4 Husbands

If the bar raised the cost of marriage without changing whether women married, it may have changed whom they married. Table 8, Column 1, shows that the husbands of exposed women have occupation scores 0.89 points higher ($p = 0.03$) on a control mean of 32.2.

## 7.5 Heterogeneity

Figure A3 reports the effects on the main outcomes within subgroups of the 18-to-27 sample. We focus on being married and in the labor force. The effect is similar for women whose fathers had occupation scores below and above the median, $-2.01$ (0.92) and $-1.47$ (0.76), and across the three age groups. By region, it is largest in the South ($-2.65$, 1.24) and in the West ($-4.79$, 3.04), although the West estimate is imprecise; it is small and not statistically significant in the Northeast and Midwest. The effect is also larger for bars adopted earlier. It is $-2.22$ (0.90) for bars adopted between 1911 and 1920, $-1.69$ (0.81) for bars adopted between 1921 and 1930, and $-1.05$ (1.41) for bars adopted in the 1930s, which we observe for only one census after adoption.

The clearest differences are by the city's characteristics in 1910, before any adoption. The effect is concentrated in cities where married women were already more likely to teach and work. In cities with a married share of teachers above the median in 1910, the effect is $-3.31$ (0.88), while in cities below the median it is $0.35$ (0.79). The pattern is similar for married women's labor force participation, $-2.36$ (1.03) versus $-1.02$ (0.72), and for married women's work in white-collar occupations other than teaching, $-2.66$ (0.79) versus $0.16$ (0.91). These results suggest that bars may have mattered most in cities where social norms permitted married women to work.

## 7.6 Robustness

We examine how sensitive our estimates for individual teachers are to several alternative choices. Table A6 reports each check for the four main outcomes, and its first row repeats our main estimates.

Our main estimates use only never-treated cities as controls. Adding cities that adopt a bar later to the comparison group gives nearly the same estimate for being married and in the labor force, $-1.77$ (0.63). Controlling for age at baseline also gives a similar estimate, $-1.79$ (0.63), and clustering standard errors by state rather than by city increases the standard error to 0.77, but the estimate remains statistically significant at the 5 percent level. Two-way fixed effects regressions with city and census-year fixed effects give $-1.45$ (0.63) for being married and in the labor force and $-1.34$ (0.57) for being married and teaching. With state-by-year fixed effects, both estimates fall to $-0.40$ and are no longer statistically significant. As with the composition results in Section 5.4, this specification is demanding in our setting, since each state has on average only 13 sample cities.

Our main sample pools all linked pairs, including women whose baseline census came after their city adopted a bar. Restricting to women whose baseline census came before adoption gives smaller estimates, $-0.82$ (0.57) for being married and in the labor force and $-0.90$ (0.50) for being married and teaching. In this restricted sample, every treated woman's outcome census is the first census after adoption; the estimate therefore equals the event-time-zero estimate in Figure 8. Since these women were exposed to the bar for less than a decade, and often for only a few years, a smaller effect is expected. However, we cannot rule out that part of the larger pooled estimate reflects differences in which women became single teachers after a bar was adopted.

A final concern is that the bar changed which women we can link. Women often changed their surnames at marriage. If the bar changed who married or when, it could also change the share of single teachers we find at the next census. We test this directly by estimating the effect of adoption on the share of a city's never-married women teachers who are linked to the next census, weighting each city by its number of never-married women teachers. The estimate is $-0.35$ percentage points (1.03) on a mean of 58.5 percent. The bar therefore did not change the share of single teachers we can link.

Finally, keeping Michigan, Kentucky, and North Carolina but dropping their 1940 observations gives similar estimates; being married and in the labor force falls by 1.53 percentage points and being married and teaching by 1.30 percentage points, both significant at the 5 percent level (Table A2).

# 8 Conclusion

This paper presents new evidence on how the marriage bar affected both the composition of the teaching labor force and the lives of women teachers. Exposure to the marriage bar decreased the share of teachers who were married women, with an offsetting increase in single women teachers, and these effects were large enough to decrease the share of married women in local professional and white-collar work overall. The decline came from fewer married women being hired and fewer teachers remaining after they married, while married women who were already teaching were largely exempted. We find little evidence that these composition effects persisted after the bars were likely abandoned during World War II. Using linked censuses, we show that marriage bars did not change whether young single teachers married or whether they worked, but they did change how the two were combined; young single teachers were 20 percent less likely to be married and in the labor force a decade later. The policy made women choose between a teaching career and marriage, and the women affected largely chose marriage.

Discriminatory policies can reshape both who works in an occupation and the lives of the workers in it. A future avenue for research is to understand whether marriage bars affected students, and in particular female students. We show which cities were more likely to adopt marriage bars, but these decisions were made by local school boards, and we know little about how board members and local labor organizations shaped them. Records of who sat on these boards, and of how teachers' associations and unions responded, could help explain why some cities adopted bars and others did not. These questions remain relevant today, since gender-based restrictions on women's work are still common in many countries (World Bank 2026).

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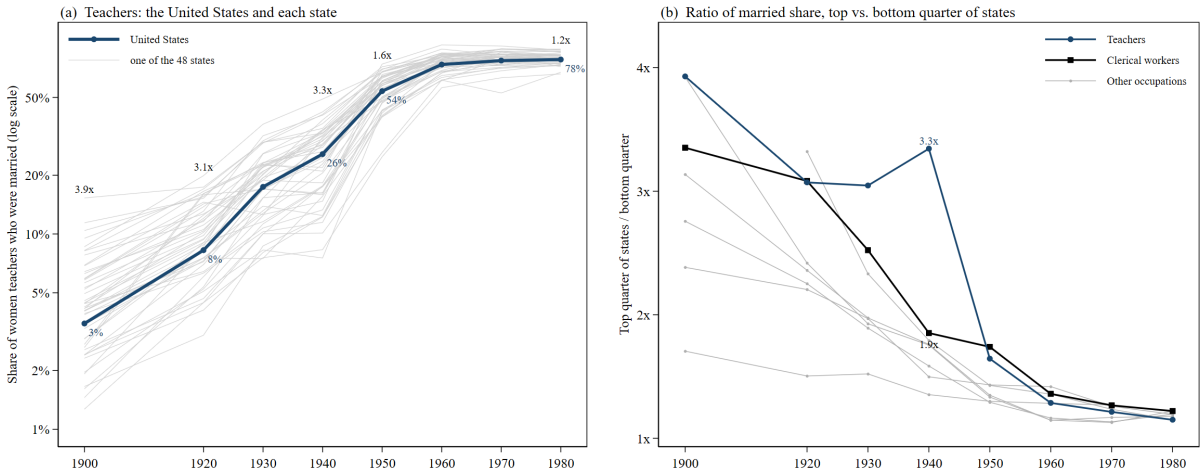
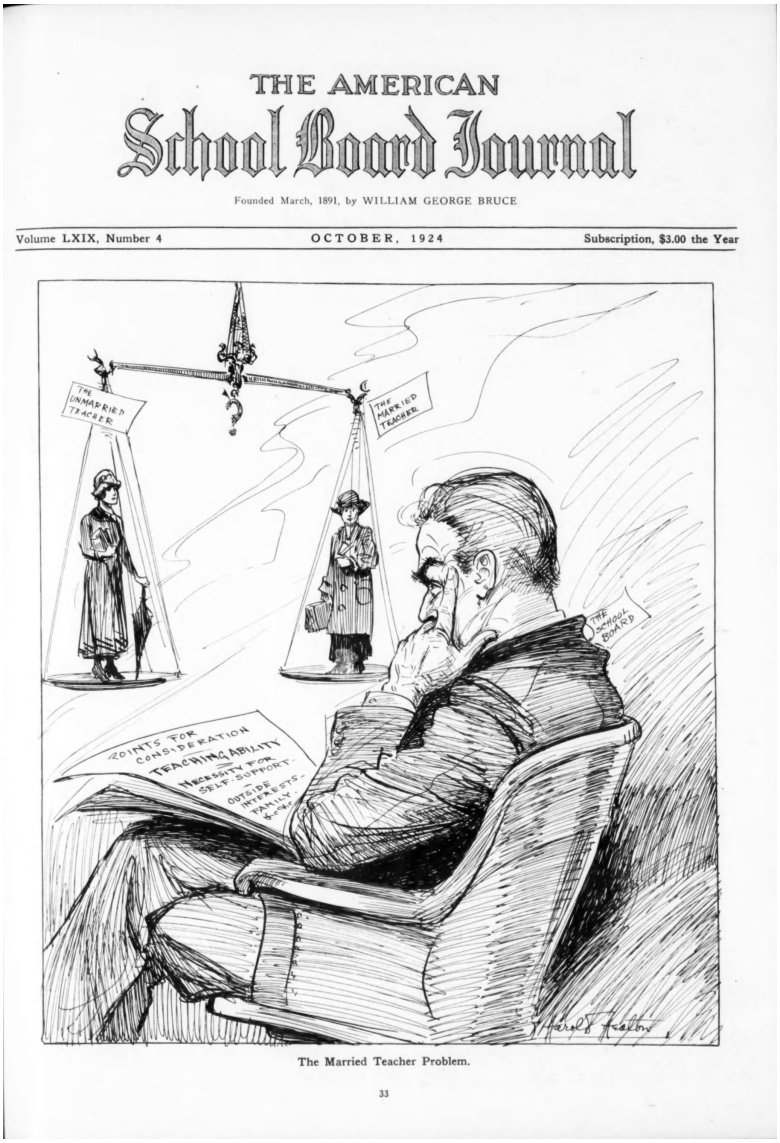
Figure 1: Where Married Women Taught, 1900–1980



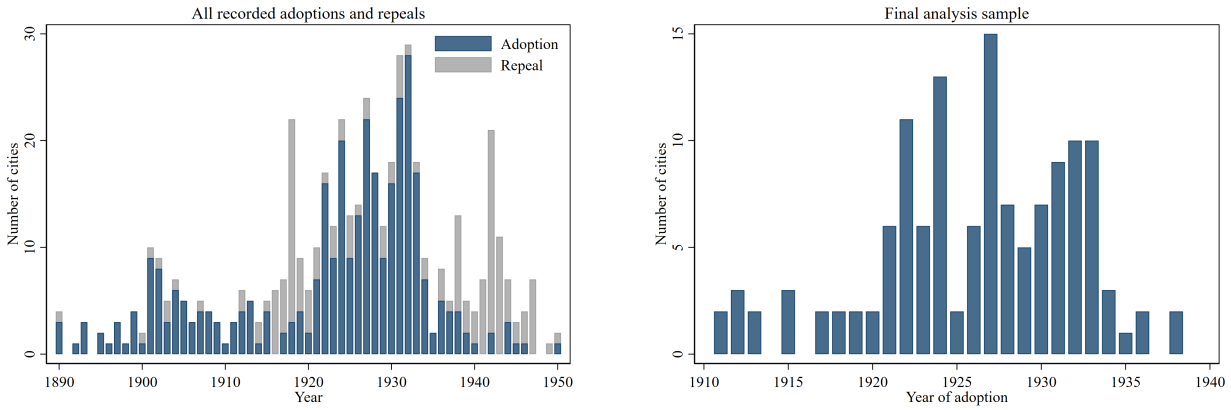

*Source:* Complete-count censuses 1900–1950 and IPUMS samples for 1960 (1 percent), 1970 (1 percent) and 1980 (5 percent), weighted by person weights.
*Note:* White women aged 18–64 in the labor force. The married share of an occupation is the number of its women workers who are married with spouse present divided by the number who are married or never married. Panel (a) is the married share of women teachers. The bold line is the 48 conterminous states together and each thin line is one state, on a logarithmic scale. The label above each marked year is the married share in the twelve states with the highest shares divided by the married share in the twelve with the lowest. Panel (b) reports that ratio for each occupation and census year; other occupations are nurses, other professional workers, managers, sales workers, operatives and service workers. 1910 is omitted because that census over-counts women's work.

Figure 2: The Married Teacher Problem




*Source:* Cover of the *American School Board Journal*, October 1924 (Volume 69, Number 4).
*Note:* A representative member of the school board looks at "points for consideration" when deciding between a married and an unmarried teacher, considering teaching ability, necessity for self-support, outside interests, and family.

Figure 3: Timing of Marriage Bar Adoption and Repeals

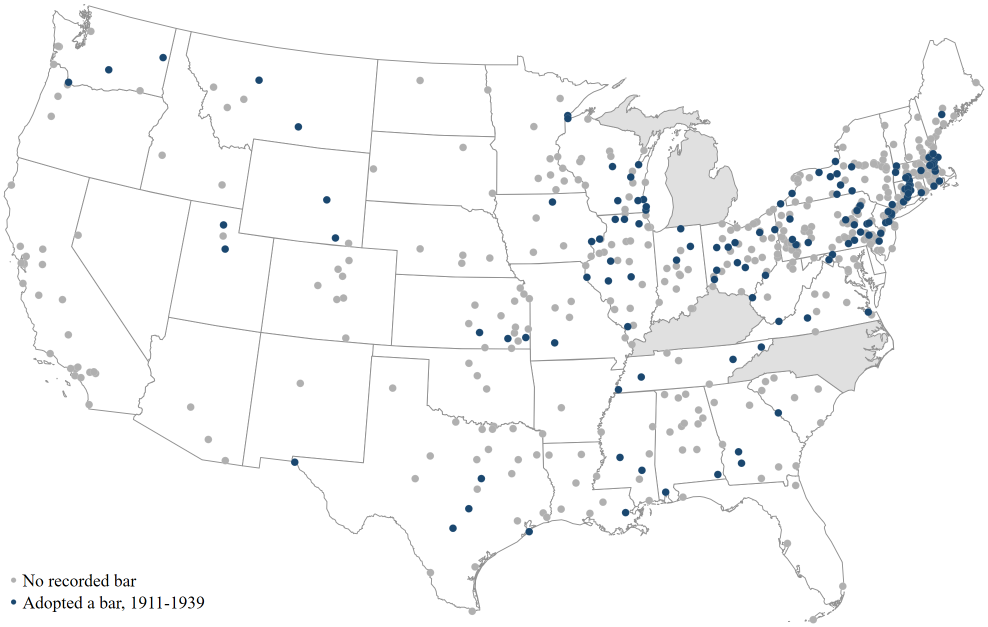



(a) All recorded adoptions and repeals

(b) Final analysis sample

*Source:* Our collection from the *American School Board Journal* and newspapers.com.
*Note:* Panel (a) counts every adoption and repeal we recorded between 1890 and 1950, with repeals stacked on adoptions. Panel (b) is the first recorded adoption year of the 133 treated cities in the estimation sample.

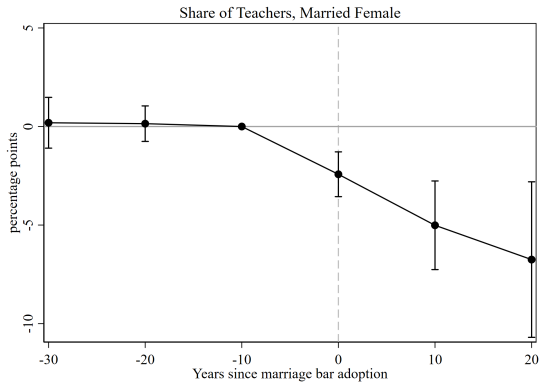
Figure 4: Sample Cities



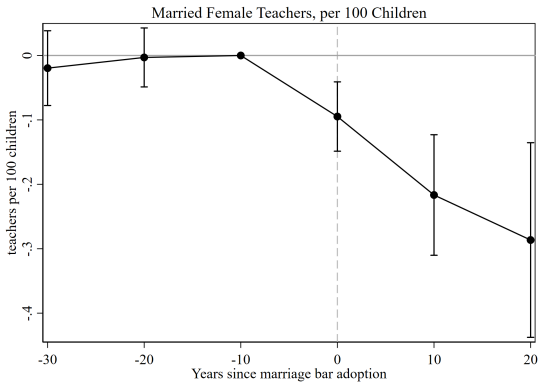

*Source:* Our collection from the *American School Board Journal* and newspapers.com.
*Note:* Each dot is one of the 593 cities in our estimation sample. Navy dots are the 133 cities that adopted a marriage bar between 1911 and 1939, and gray dots are the 460 cities with no recorded bar. Shaded states are excluded; Michigan because of its 1937 Teachers' Tenure Act, and Kentucky and North Carolina because state laws prohibited marriage bars.

Figure 5: Effects of Marriage Bar on Teacher Composition

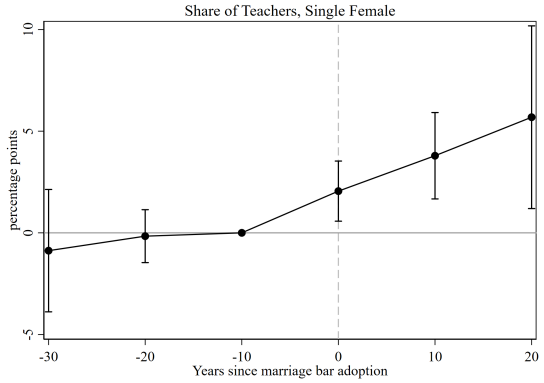
(a) Share of Teachers, Married Female



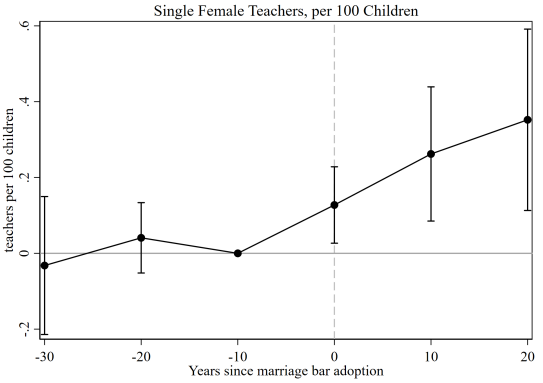

(b) Married Female Teachers, per 100 children




(c) Share of Teachers, Single Female




(d) Single Female Teachers, per 100 Children



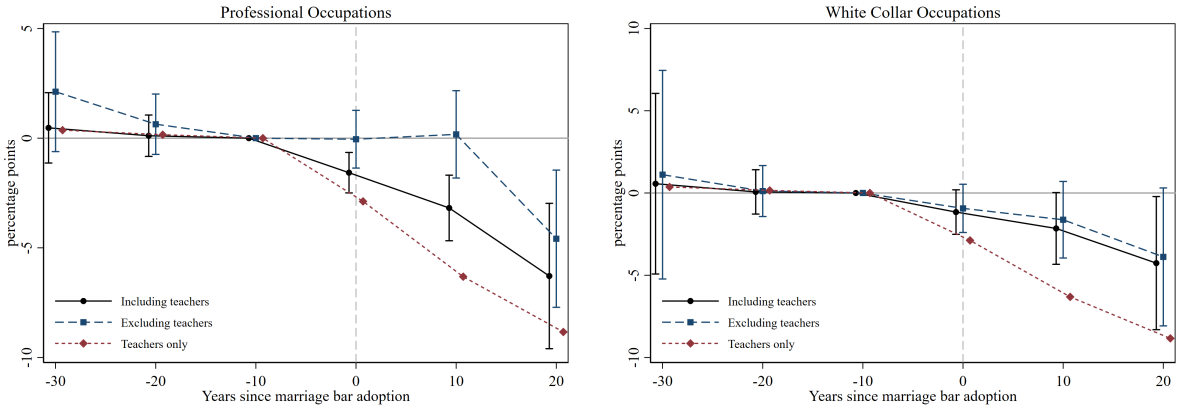

*Source:* 1880–1940 full-count censuses.

*Note:* Event-study effects of the marriage bar, in decades relative to adoption, on (a) the share of teachers who are married women, (b) married women teachers per 100 children, (c) the share of teachers who are single women and (d) single women teachers per 100 children. Estimates are Callaway–Sant'Anna event-time effects relative to the census before adoption, using never-treated cities as controls with doubly-robust inverse-probability weighting, weighted by each outcome's own denominator. Bars are 95 percent confidence intervals; standard errors are clustered at the city level.

Figure 6: Married Share of Women in Professional and White-Collar Work

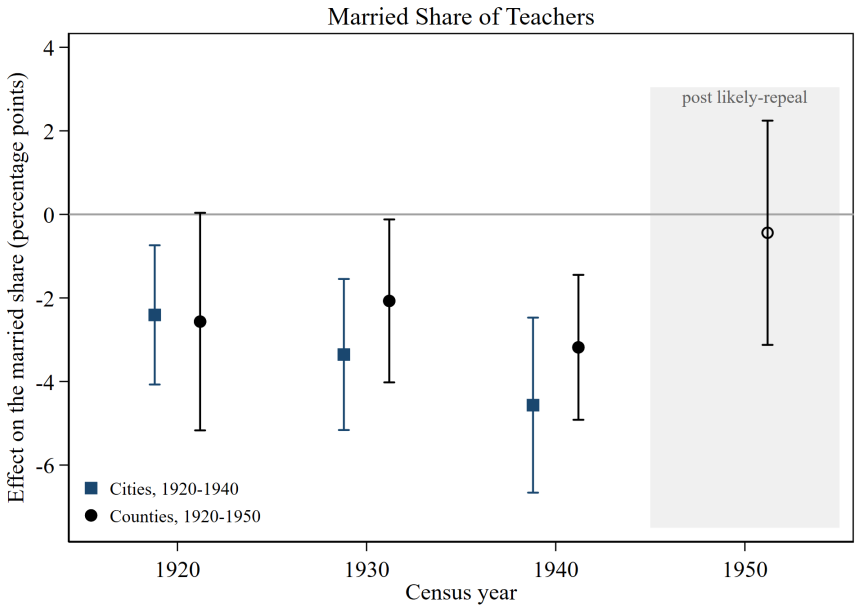
(a) Professional occupations

(b) White-collar occupations



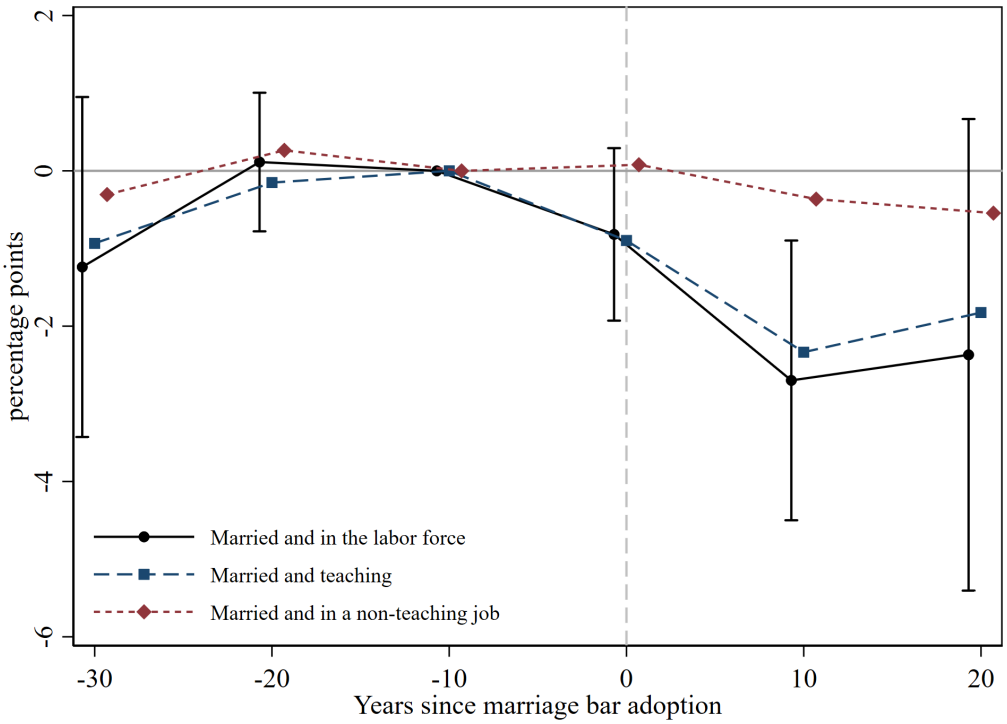

*Note:* Event-study estimates of the effect of the marriage bar on married women as a share of women working in each occupation group, relative to the census before adoption. Each panel shows the group with and without teachers, and the series for teachers alone. Professional work is occupation codes 0–99, and white-collar work adds codes 200–490. The series excluding teachers are flat through ten years after adoption; the estimate twenty years after adoption comes only from the earliest adopters and is imprecise. Bars are 95 percent confidence intervals. Estimates are Callaway–Sant'Anna, weighted by the number of women in the group, with standard errors clustered at the city level.

Figure 7: Effects by Census Year, Cities and Counties, 1920–1950



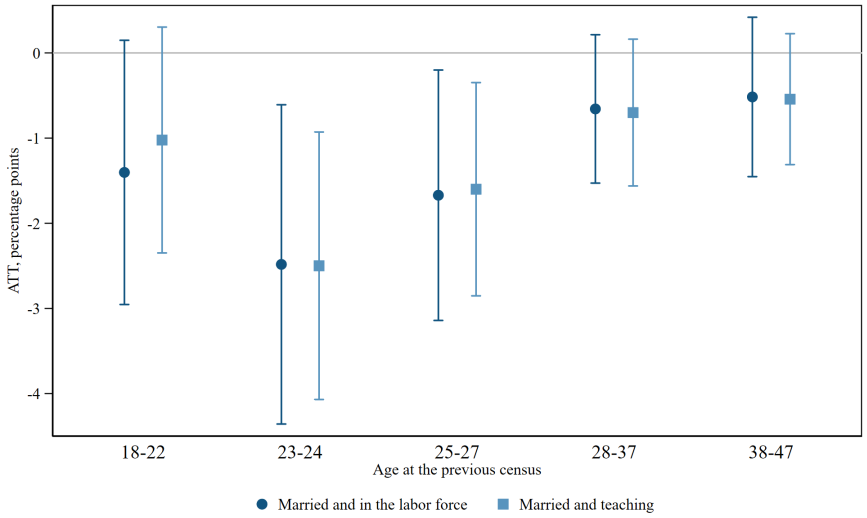

*Note:* Callaway–Sant'Anna estimates of the effect on the married share of teachers by calendar year rather than by event time. Squares are the 133 treated and 460 control cities, through 1940, and circles are 81 treated and 299 control counties, through 1950. Each sample city is assigned to the county containing most of its population. A county is treated if all of its sample cities adopted a bar and is a control if none did, and counties with both are dropped. Where a treated county has several adopting cities, we use the earliest adoption date. Because each county also contains districts that never adopted a bar, the county estimates are smaller than the city estimates. Calendar-year estimates average only over cohorts already treated by that year. The series therefore begin in 1920 and have no estimates before adoption. The shaded area and the hollow marker mark 1950, after most bars were abandoned. The city series stops in 1940 because 1950 is the only census without a place crosswalk. Bars are 95 percent confidence intervals, and standard errors are clustered at the city or county level.

Figure 8: Effects of the Marriage Bar on Marriage and Work, among Never-Married Teachers




*Note:* Event-study estimates for women who were never married and teaching in a city school at the previous census, aged 18–27 at that census. Each outcome is a joint indicator for both conditions holding a decade later. Confidence intervals at 95 percent are shown for being married and in the labor force only; the other two series are its components, married and teaching and married in a non-teaching job. Labor force participation is having an occupation code below 971. Estimates are Callaway–Sant'Anna with never-treated cities as controls, with standard errors clustered at the city level.

Figure 9: The Exit at Marriage by Age at Baseline




*Note:* Each point is the effect on being married and in the labor force, or married and teaching, a decade later. Women aged 18–27 at baseline are split into three age groups, and the 28–37 and 38–47 groups are the older samples in Table 7. The sample is women who were never married and teaching in a city school at the previous census. Estimates are Callaway–Sant'Anna average treatment effects on the treated, in percentage points, using never-treated cities as controls with doubly-robust inverse-probability weighting, with standard errors clustered at the city level. Bars are 95 percent confidence intervals.

Table 1: City Characteristics Before Adoption

| | Treated | | Control | | Level Coefficient | | Trend Coefficient | |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Unweighted | Weighted | Unweighted | Weighted |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Population | 16,852 | 24,750 | 16,371 | 54,471 | 481 | – | 2,735 | – |
| Urban | 76% | 0.36 | 66% | 0.42 | 0.101** | 0.040 | 0.020 | 0.021 |
| % Black | 7% | 0.15 | 9% | 0.16 | -0.020 | 0.015 | -0.002 | -0.003 |
| % Children (5–16) | 26% | 0.03 | 26% | 0.03 | 0.003 | 0.004 | -0.003 | -0.002 |
| Average Occupation Score, Men | 24.7 | 2.9 | 24.7 | 2.6 | -0.055 | -0.076 | 0.194 | 0.015 |
| % Farmers | 13% | 0.18 | 13% | 0.15 | -0.000 | -0.014 | -0.012 | 0.000 |
| % Female LFP | 20% | 0.09 | 21% | 0.09 | -0.002 | -0.004 | -0.000 | 0.003 |
| % Married Female LFP | 9% | 0.09 | 10% | 0.09 | -0.008 | -0.000 | -0.003 | 0.002 |
| Teacher # per 100 Children | 1.94 | 0.89 | 2.03 | 0.95 | -0.096 | 0.033 | -0.132 | -0.060 |
| - % of Married Female | 8% | 0.08 | 9% | 0.09 | -0.010 | -0.002 | 0.008 | 0.001 |
| - % of Single Female | 67% | 0.18 | 65% | 0.19 | 0.028 | 0.006 | -0.029* | 0.007 |
| - % of Married Male | 15% | 0.11 | 15% | 0.12 | -0.002 | 0.006 | 0.009 | -0.006 |
| - % of Single Male | 10% | 0.08 | 11% | 0.10 | -0.016* | -0.010 | 0.013 | -0.002 |
| Average Age of Teachers | 29.3 | 3.5 | 29.4 | 3.7 | -0.035 | 0.244 | 0.273 | -0.355 |
| - Married Female | 38.7 | 6.3 | 37.7 | 10.7 | 0.970 | 1.292 | -1.125 | -1.340 |
| - Single Female | 26.1 | 2.5 | 26.3 | 3.7 | -0.134 | 0.091 | 0.424 | -0.147 |
| - Married Male | 40.6 | 6.0 | 39.6 | 6.8 | 0.984 | 0.021 | -0.922 | -0.385 |
| - Single Male | 26.4 | 5.1 | 27.1 | 5.6 | -0.644 | -0.661 | 0.519 | 0.503 |
| Region | | | | | | | | |
| - Northeast | 47% | 0.50 | 46% | 0.50 | 0.011 | – | – | – |
| - Midwest | 28% | 0.45 | 24% | 0.43 | 0.039 | – | – | – |
| - South | 18% | 0.39 | 19% | 0.39 | -0.011 | – | – | – |
| - West | 7% | 0.25 | 11% | 0.31 | -0.039 | – | – | – |
| Year of Adoption | | | | | | | | |
| - 1911–1920 | 14% | 0.34 | – | – | – | – | – | – |
| - 1921–1930 | 59% | 0.49 | – | – | – | – | – | – |
| - 1931–1939 | 28% | 0.45 | – | – | – | – | – | – |
| # of Cities | 133 | | 460 | | | | | |
| # of States | 28 | | 46 | | | | | |

*Note:* The sample is our estimation sample of 133 treated and 460 control cities. Columns (1)–(4) report city means and standard deviations in 1880; cities first observed in 1900 are not included in these columns. Columns (5) and (6) report the coefficient from regressing the 1880 value on treatment. Columns (7) and (8) report the coefficient from regressing the change between 1880 and 1900 on treatment; both censuses come before every adoption in the sample, the earliest of which is 1911. The first column of each pair weights cities equally, and the second weights each outcome by its own denominator, as in our estimation (teachers for the teacher shares and ages, children for the teacher rate, and population for the city characteristics). Population is tested unweighted only. Children are ages 5–16. The occupation score is the city mean among men, and married female labor force participation is among ever-married women. Teacher shares are of all teachers, and teacher ages are city means. Region and year of adoption are fixed characteristics, measured once for every city; the region coefficient is unweighted, and year of adoption is shown for treated cities only. Standard errors are robust. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 2: Effects on Teacher Composition

| | % Female | % Married Female | % Single Female | % Married Men | % Single Men |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| ATT | -0.008 | -0.039*** | 0.031*** | 0.003 | 0.005 |
| *SE* | (0.006) | (0.008) | (0.009) | (0.005) | (0.003) |
| Control mean | 0.810 | 0.173 | 0.637 | 0.133 | 0.057 |
| # of Treated Cities: 133 | | | | | |
| # of Control Cities: 460 | | | | | |

Table 3: Effects on Teacher to Children Ratios (# of Teachers per 100 Children)

| | All | Married Female | Single Female | Married Men | Single Men |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| ATT | 0.079 | -0.163*** | 0.204*** | 0.018 | 0.020* |
| *SE* | (0.077) | (0.036) | (0.063) | (0.031) | (0.010) |
| Control mean | 3.176 | 0.549 | 2.024 | 0.422 | 0.181 |
| # of Treated Cities: 133 | | | | | |
| # of Control Cities: 460 | | | | | |

Table 4: Married Share of Women in Teaching and Other Occupations

| | White Collar, Including Teaching | White Collar, Excluding Teaching | Clerical | Professional, Including Teaching | Professional, Excluding Teaching | Nurses | All Other |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| ATT | -0.006* | -0.005 | -0.010** | -0.010*** | -0.002 | -0.006 | -0.004 |
| *SE* | (0.003) | (0.003) | (0.005) | (0.003) | (0.003) | (0.010) | (0.003) |
| Control mean | 0.104 | 0.100 | 0.129 | 0.109 | 0.088 | 0.325 | 0.122 |
| Workers per city | 5,758 | 5,473 | 1,889 | 1,124 | 839 | 87 | 12,087 |

*Note:* Each column is married women as a share of everyone working in the occupation, weighted by the number of workers. The effect on teachers is in Table 2, column (2). Professional is occupation codes 0 to 99, and white collar adds codes 200 to 490, so both include teachers; each is shown with and without teachers. Clerical is clerical and kindred workers, codes 300 to 390. The control mean is the mean in never-treated cities across all years. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting, with standard errors clustered at the city level. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 5: Decomposing the Married Share of Teachers

| | Before Adoption | First Census After Adoption | Subsequent Censuses | Control Mean |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| New hires already married | -0.007 | -0.005* | -0.012** | 0.075 |
| *SE* | (0.005) | (0.003) | (0.005) | |
| Single at previous census, married now | -0.003 | -0.011*** | -0.010 | 0.060 |
| *SE* | (0.006) | (0.004) | (0.006) | |
| Married at previous census, still teaching | 0.001 | -0.006 | -0.042*** | 0.061 |
| *SE* | (0.003) | (0.004) | (0.009) | |
| Total: married share of teachers | -0.009 | -0.022*** | -0.065*** | 0.196 |
| *SE* | (0.011) | (0.007) | (0.015) | |
| # of Treated Cities: 133 | | | | |
| # of Control Cities: 460 | | | | |

*Note:* Each female teacher observed in a census is looked up in the previous census. The three rows divide married female teachers by what they were doing at the previous census. Row 1 is women who were not teaching in the city and were already married, row 2 is women who were single and had married by the current census, and row 3 is women who were teaching and already married. All rows are shares of linked female teachers, so they sum to the total in every column. Column (1) is the average estimate before adoption, column (2) is the first census after adoption, and column (3) averages the second and third censuses after adoption, with a delta-method standard error. About a fifth of teachers cannot be linked to the previous census and are not included. Linkage does not change with adoption, but the married share among unlinked teachers falls by a further 1.2 percentage points, likely because women who married changed their surnames, so the decomposition understates the total effect. The panel begins in 1910 because 1900 has no earlier census to link to. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting, weighted by linked female teachers, with standard errors clustered by city. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 6: Effects of the Marriage Bar on Never Married Teachers in $t-10$: Work and Marriage in $t$

| | In Labor Force | Teaching | Non-Teaching Job | Ever Married |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A. Aged 18–27 at baseline* | | | | |
| ATT | -1.04 | -0.59 | -0.45 | -0.49 |
| *SE* | (0.96) | (1.08) | (0.64) | (1.07) |
| Control mean | 63.99 | 52.48 | 11.51 | 40.22 |
| Observations | | 121,679 | | |
| *Panel B. Aged 28–37 at baseline* | | | | |
| ATT | 1.22 | 1.84 | -0.61 | -0.73 |
| *SE* | (1.37) | (1.59) | (0.61) | (0.78) |
| Control mean | 83.04 | 72.82 | 10.22 | 15.25 |
| Observations | | 79,130 | | |
| *Panel C. Aged 38–47 at baseline* | | | | |
| ATT | 2.59 | 2.70 | -0.11 | 0.04 |
| *SE* | (1.78) | (1.99) | (0.78) | (0.73) |
| Control mean | 86.89 | 77.72 | 9.17 | 5.65 |
| Observations | | 43,231 | | |
| # of Treated Cities: 133 | | | | |
| # of Control Cities: 460 | | | | |

*Note:* The sample is women who were never married and teaching in a city school at the previous census. Panels A, B, and C include women aged 18–27, 28–37, and 38–47 at that census. Each age window is narrower than the ten-year gap between censuses, so no woman appears twice in a panel, although a woman may appear in more than one panel. Women in the older panels are more selected, since remaining a never-married teacher to older ages is partly a result of exposure. Each column is a separate regression. The control mean is the mean of the outcome in never-treated cities across all years. Each outcome is measured on its own; the labor force columns do not account for marital status, and the marriage column does not account for work. Teaching and non-teaching job sum to the labor force column. Ever married means not never married at the outcome census, so it includes women who were widowed, divorced, or separated. Labor force participation is having an occupation code below 971. Effects are in percentage points. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting and standard errors clustered at the city level. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 7: Effects of the Marriage Bar on Never Married Teachers in $t-10$: Work by Marital Status in $t$

| | Ever Married × | | | | Never Married × | | | |
|---|---|---|---|---|---|---|---|---|
| | In Labor Force | Teaching | Non-Teaching Job | Out of Labor Force | In Labor Force | Teaching | Non-Teaching Job | Out of Labor Force |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel A. Aged 18–27 at baseline* | | | | | | | | |
| ATT | -1.74*** | -1.58*** | -0.17 | 1.25 | 0.70 | 0.99 | -0.29 | -0.21 |
| *SE* | (0.63) | (0.56) | (0.25) | (0.87) | (1.12) | (1.25) | (0.60) | (0.89) |
| Control mean | 8.60 | 6.07 | 2.53 | 31.62 | 55.39 | 46.41 | 8.98 | 4.39 |
| Observations | 121,679 | | | | | | | |
| *Panel B. Aged 28–37 at baseline* | | | | | | | | |
| ATT | -0.66 | -0.70 | 0.04 | -0.07 | 1.88 | 2.54 | -0.66 | -1.15 |
| *SE* | (0.44) | (0.44) | (0.20) | (0.65) | (1.39) | (1.60) | (0.59) | (1.08) |
| Control mean | 4.66 | 3.44 | 1.22 | 10.59 | 78.38 | 69.38 | 9.00 | 6.37 |
| Observations | 79,130 | | | | | | | |
| *Panel C. Aged 38–47 at baseline* | | | | | | | | |
| ATT | -0.52 | -0.54 | 0.03 | 0.55 | 3.11* | 3.24 | -0.14 | -3.14* |
| *SE* | (0.48) | (0.39) | (0.24) | (0.66) | (1.87) | (2.01) | (0.81) | (1.75) |
| Control mean | 2.61 | 1.76 | 0.84 | 3.04 | 84.29 | 75.96 | 8.33 | 10.07 |
| Observations | 43,231 | | | | | | | |
| # of Treated Cities: 133 | | | | | | | | |
| # of Control Cities: 460 | | | | | | | | |

*Note:* The sample is women who were never married and teaching in a city school at the previous census. Panels A, B, and C include women aged 18–27, 28–37, and 38–47 at that census. Each age window is narrower than the ten-year gap between censuses, so no woman appears twice in a panel, although a woman may appear in more than one panel. Women in the older panels are more selected, since remaining a never-married teacher to older ages is partly a result of exposure. Each column is a separate regression. The control mean is the mean of the outcome in never-treated cities across all years. Each outcome is a joint indicator equal to one when both conditions hold a decade later. Within each marital status, teaching, non-teaching job, and out of the labor force sum to the marital total, and in labor force is the sum of the first two. Each outcome is a share of the whole sample, so none conditions on marriage or work. Effects are in percentage points. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting and standard errors clustered at the city level. *** p<0.01, ** p<0.05, * p<0.10.

Table 8: Effects of the Marriage Bar on Never Married Teachers in $t-10$: Husband Characteristics in $t$

| | Occupation | Husband's Occupation | | | | | | | | Age Gap |
|---|---|---|---|---|---|---|---|---|---|---|
| | Score | Professional | Manager | Clerical/Sales | Farmer | Craftsman | Operative | Service/Laborer | Teacher | (H − W) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel A. Aged 18–27 at baseline* | | | | | | | | | | |
| ATT | 0.89** | 1.46 | -0.35 | 1.01 | 0.73 | 0.34 | -0.80 | -0.76 | -0.17 | 0.04 |
| *SE* | (0.42) | (1.06) | (1.06) | (1.12) | (0.85) | (0.92) | (0.64) | (0.58) | (0.52) | (0.15) |
| Control mean | 32.23 | 25.40 | 19.68 | 21.39 | 7.12 | 11.76 | 5.51 | 4.32 | 4.76 | 2.99 |
| Observations | | | | | 121,679 | | | | | |
| *Panel B. Aged 28–37 at baseline* | | | | | | | | | | |
| ATT | 2.95*** | 3.41 | 3.65 | -2.85 | -0.37 | -0.23 | 0.90 | -2.96** | -0.67 | 0.49 |
| *SE* | (0.93) | (2.24) | (2.29) | (2.26) | (1.76) | (2.13) | (1.36) | (1.27) | (1.16) | (0.39) |
| Control mean | 31.25 | 23.60 | 20.24 | 19.41 | 8.06 | 12.25 | 5.48 | 4.60 | 4.85 | 3.19 |
| Observations | | | | | 79,130 | | | | | |
| *Panel C. Aged 38–47 at baseline* | | | | | | | | | | |
| ATT | -1.07 | -1.08 | -0.37 | 3.65 | 10.03*** | -13.16* | 3.73** | -3.70 | -3.41 | 0.38 |
| *SE* | (2.56) | (5.56) | (5.22) | (4.78) | (3.64) | (7.52) | (1.90) | (5.47) | (3.14) | (1.70) |
| Control mean | 26.98 | 16.63 | 17.78 | 18.16 | 9.41 | 13.78 | 4.75 | 5.80 | 3.80 | 4.49 |
| Observations | | | | | 43,231 | | | | | |
| # of Treated Cities: 133 | | | | | | | | | | |
| # of Control Cities: 460 | | | | | | | | | | |

*Note:* The sample is women who were never married and teaching in a city school at the previous census. Panels A, B, and C include women aged 18–27, 28–37, and 38–47 at that census. Each age window is narrower than the ten-year gap between censuses, so no woman appears twice in a panel, although a woman may appear in more than one panel. Women in the older panels are more selected, since remaining a never-married teacher to older ages is partly a result of exposure. Each column is a separate regression. The control mean is the mean of the outcome in never-treated cities across all years. Each column is conditional on having married, for women married by the outcome census whose husband is present and has an occupation score. Column (1) is the husband's occupation score, column (10) is his age minus hers in years, and columns (2) to (9) are the shares of husbands in each occupation group, in percentage points. Professional (codes 0 to 99) includes teachers; managers are codes 200 to 290, clerical and sales 300 to 490, farmers 100 to 123, craftsmen 500 to 595, operatives 600 to 690, and service workers and laborers 700 to 970. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting and standard errors clustered at the city level. *** p<0.01, ** p<0.05, * p<0.10.

# Online Appendix: Additional Tables and Figures

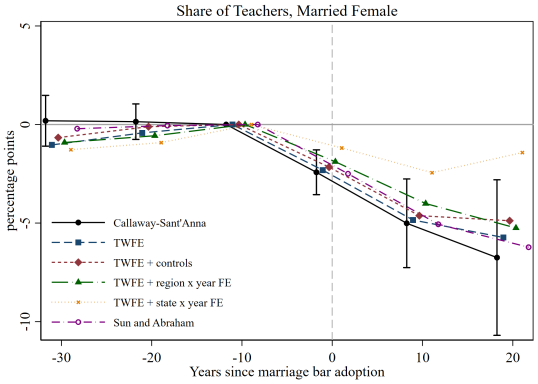
Table A1: Predictors of Marriage Bar Adoption

| | No fixed effects | | Region fixed effects | |
|---|---|---|---|---|
| | Estimate | *SE* | Estimate | *SE* |
| | (1) | (2) | (3) | (4) |
| *Panel A. Ever adopted, 1910 characteristics* | | | | |
| Log population | 8.00*** | (2.35) | 7.92*** | (2.38) |
| Urban | 0.61 | (2.25) | 0.74 | (2.21) |
| % Black | -1.10 | (3.39) | -5.64* | (3.34) |
| Occupation score, men | -0.80 | (2.29) | -1.35 | (2.14) |
| % Female LFP | 2.71 | (5.11) | 2.19 | (5.23) |
| Teachers per 100 children | -1.45 | (4.09) | -0.31 | (4.24) |
| Married share of teachers | 2.92 | (3.75) | 4.98 | (3.30) |
| Log distance to normal school | -0.75 | (2.37) | -0.99 | (2.47) |
| Growth in married share | 1.34 | (1.63) | 0.65 | (1.68) |
| Observations | | 504 | | |
| *Panel B. Adopted 1931–39, among cities untreated in 1930* | | | | |
| Log population | 6.09** | (2.58) | 6.25** | (2.50) |
| Urban | 3.19*** | (0.96) | 3.13*** | (0.93) |
| % Black | -4.28* | (2.16) | -6.24*** | (2.21) |
| Occupation score, men | -4.76** | (1.92) | -4.89** | (1.83) |
| % Female LFP | 5.40 | (3.77) | 3.34 | (3.49) |
| Teachers per 100 children | 2.37 | (2.13) | 2.83 | (2.31) |
| Married share of teachers | 9.65** | (4.37) | 9.62** | (4.24) |
| Log distance to normal school | -0.10 | (2.22) | 0.16 | (2.17) |
| Growth in married share | -3.72 | (3.56) | -4.35 | (3.46) |
| Retail sales growth, 1929–33 | -0.05 | (1.44) | -0.70 | (1.51) |
| County unemployment, 1930 | -3.04* | (1.51) | -3.07* | (1.58) |
| School spending per pupil | -2.71 | (2.06) | -1.67 | (2.18) |
| Trend in spending per pupil | -1.07 | (1.26) | -1.51 | (1.40) |
| Local share of school revenue | 1.12 | (2.09) | 1.08 | (1.96) |
| Trend in enrollment, 1915–28 | -3.38** | (1.27) | -3.18** | (1.25) |
| Observations | | 421 | | |

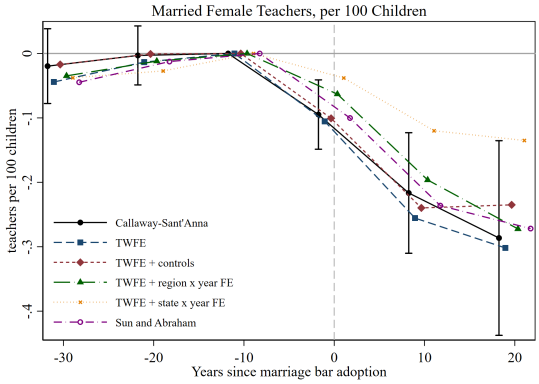
*Note:* Linear probability models for whether a city adopted a marriage bar. Every regressor is standardized on the estimation sample, so each estimate is the change in the probability of adoption, in percentage points, for a one standard deviation increase. Standard errors are clustered by state. Panel A uses 1910 city characteristics, which come before every adoption in the sample; 23 percent of these cities adopted a bar. Panel B includes cities without a bar in 1930 and asks which adopted one in the 1930s; 8.3 percent did. Its city characteristics are from 1930, school finance comes from the Biennial Survey and ends in 1928, and the fall in retail sales is measured over 1929–33. School spending per pupil and the local share of revenue are averages over 1925–28, and the spending and enrollment trends are yearly slopes over 1915–28. Both panels also control for married women's labor force participation, and Panel A for the share of children. Distance to the nearest state normal school open by the baseline year is from Ogren (2005). County retail sales and unemployment are from Fishback, Kantor and Wallis (2017). *** p<0.01, ** p<0.05, * p<0.10.

Figure A1: Robustness to Alternative Specifications (Teacher Composition)

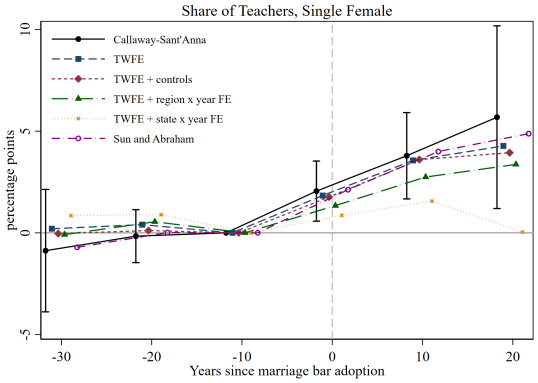
(a) Share of Teachers, Married Female



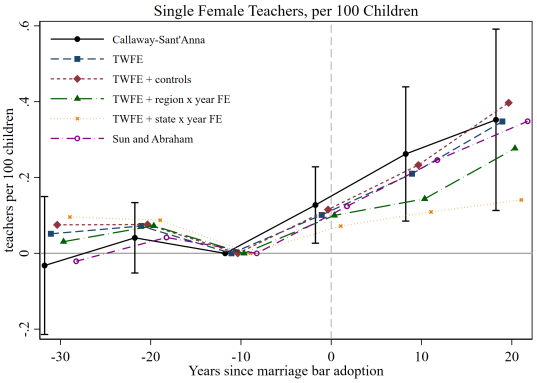

(b) Married Female Teachers, per 100 children




(c) Share of Teachers, Single Female




(d) Single Female Teachers, per 100 Children




*Source:* 1880–1940 full-count censuses.

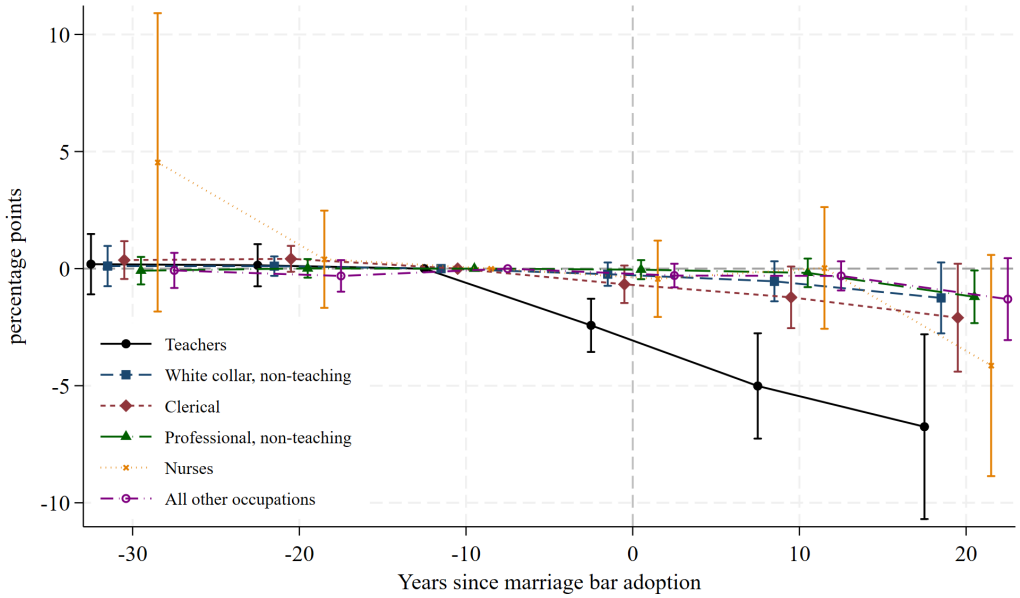
*Note:* Each panel repeats the event study of Figure 5 under alternative estimators. Callaway–Sant'Anna is the main specification, with 95 percent confidence intervals. TWFE is a two-way fixed effects event study with city and year fixed effects and city population; event time is binned at three or more censuses before and two or more after adoption, with the census before adoption omitted. TWFE + controls adds the Black share of the population, the share aged 5–16 and married women's labor force participation. The next two series replace year fixed effects with region-by-year and state-by-year fixed effects. Sun and Abraham is the interaction-weighted estimator of Sun and Abraham (2021) with never-treated cities as the control cohort. All estimates are weighted by the outcome's own denominator, with standard errors clustered at the city level.

Table A2: Keeping Michigan, Kentucky, and North Carolina Before 1940

| | Teacher Composition | | | | Never-Married Teachers Aged 18–27 | | | |
|---|---|---|---|---|---|---|---|---|
| | % Married Female | % Single Female | Married Female per 100 Children | Single Female per 100 Children | Married and In Labor Force | Married and Teaching | In Labor Force | Ever Married |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel A. Main sample: Michigan, Kentucky, and North Carolina excluded* | | | | | | | | |
| ATT | -3.87*** | 3.10*** | -0.163*** | 0.204*** | -1.74*** | -1.58*** | -1.04 | -0.49 |
| *SE* | (0.81) | (0.87) | (0.036) | (0.063) | (0.63) | (0.56) | (0.96) | (1.07) |
| Treated / control cities | | | | 133 / 460 | | | | |
| *Panel B. Michigan, Kentucky, and North Carolina kept, their 1940 observations dropped* | | | | | | | | |
| ATT | -3.15*** | 2.47*** | -0.147*** | 0.155** | -1.53** | -1.30** | -1.21 | -0.09 |
| *SE* | (0.89) | (0.88) | (0.036) | (0.067) | (0.62) | (0.56) | (0.92) | (1.04) |
| Treated / control cities | | | | 149 / 496 | | | | |

*Note:* Panel A repeats the main estimates (Table 2, Table 3, Table 6, and Table 7). Panel B adds the cities in Michigan, Kentucky, and North Carolina, where marriage bars were overturned in the 1930s, and drops their 1940 observations, so these cities enter only for census years before their bars were overturned. For never-married teachers, the 1940 observations are women whose outcome census is 1940. Columns (1)–(4) are city-level estimates weighted by the outcome's own denominator, and columns (5)–(8) are person-level estimates for women aged 18–27 at the previous census, in percentage points. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting, with standard errors clustered at the city level. *** p<0.01, ** p<0.05, * p<0.10.

Figure A2: Married Share of Women, Teachers and Other Occupations




*Note:* Event-study estimates of the effect of the marriage bar on married women as a share of everyone working in each occupation, relative to the census before adoption. Teachers are the group the bar applied to. White collar and professional both exclude teachers, and clerical is clerical and kindred workers, occupation codes 300–390. Bars show 95 percent confidence intervals. Estimates are Callaway–Sant'Anna, weighted by the number of workers in the occupation, with standard errors clustered at the city level.

Table A3: Characteristics of Newly Hired Teachers

| | Share Male | Age | Share Black | Share Foreign Born | Children in Household | Father's Occupation Score |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ATT | 0.36 | -0.39* | 0.19 | 0.39 | -0.03*** | -0.46* |
| *SE* | (0.67) | (0.22) | (0.56) | (0.27) | (0.01) | (0.27) |
| Control mean | 21.97 | 29.31 | 6.06 | 2.23 | 0.19 | 27.30 |
| # of Treated Cities: 133 | | | | | | |
| # of Control Cities: 460 | | | | | | |

*Note:* Each column is a characteristic of women hired into teaching in a city over the previous decade, meaning teachers observed in a census who were linked to the previous census and were not teaching then. Share male is measured over all hires and the other columns over female hires. Shares are in percentage points. Children in household is the number of own children in the household. Father's occupation score is the household head's score at the previous census, for hires who were then the child of a male head, about three-quarters of female hires. The control mean pools all years for never-treated cities. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting, weighted by the number of hires, with standard errors clustered at the city level. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A4: Effects on the Age Distribution of Female Teachers

| | Under 25 | 25–34 | 35–44 | 45–54 | 55+ |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| ATT | 0.004 | 0.002 | 0.003 | -0.004 | -0.004 |
| *SE* | (0.012) | (0.010) | (0.008) | (0.005) | (0.005) |
| Control mean | 0.232 | 0.344 | 0.220 | 0.132 | 0.072 |
| # of Treated Cities: 133 | | | | | |
| # of Control Cities: 460 | | | | | |

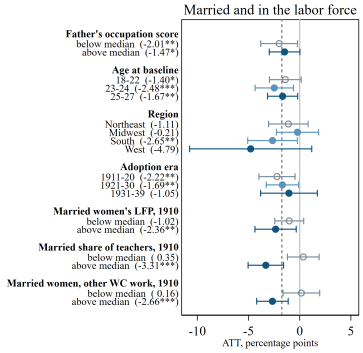
*Note:* The five columns divide female teachers by age, so the coefficients sum to zero. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting and weighted by the outcome's own denominator. Standard errors are clustered at the city level. The control mean pools all years for never-treated cities. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table A5: Effects of the Marriage Bar on Never Married Teachers in $t-10$: Children and Work in $t$

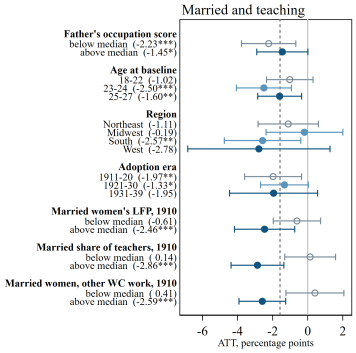
| | Has a Child × | | | | No Child × | | | |
|---|---|---|---|---|---|---|---|---|
| | In Labor Force | Teaching | Non-Teaching Job | Not Working | In Labor Force | Teaching | Non-Teaching Job | Not Working |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel A. Aged 18–27 at baseline* | | | | | | | | |
| ATT | -0.81*** | -0.85*** | 0.04 | 1.04 | -0.23 | 0.27 | -0.50 | 0.00 |
| *SE* | (0.28) | (0.25) | (0.16) | (0.70) | (0.98) | (1.09) | (0.63) | (0.97) |
| Control mean | 3.70 | 2.52 | 1.18 | 23.87 | 60.29 | 49.96 | 10.33 | 12.14 |
| Observations | 121,679 | | | | | | | |
| *Panel B. Aged 28–37 at baseline* | | | | | | | | |
| ATT | -0.18 | -0.41* | 0.23* | -0.90* | 1.41 | 2.25 | -0.84 | -0.32 |
| *SE* | (0.28) | (0.24) | (0.12) | (0.50) | (1.46) | (1.65) | (0.61) | (1.16) |
| Control mean | 2.52 | 1.92 | 0.60 | 6.57 | 80.52 | 70.90 | 9.62 | 10.39 |
| Observations | 79,130 | | | | | | | |
| *Panel C. Aged 38–47 at baseline* | | | | | | | | |
| ATT | -0.20 | -0.32 | 0.12 | 0.57** | 2.79 | 3.02 | -0.23 | -3.16* |
| *SE* | (0.42) | (0.38) | (0.20) | (0.29) | (1.83) | (2.01) | (0.74) | (1.73) |
| Control mean | 1.66 | 1.32 | 0.34 | 1.16 | 85.23 | 76.40 | 8.83 | 11.95 |
| Observations | 43,231 | | | | | | | |
| # of Treated Cities: 133 | | | | | | | | |
| # of Control Cities: 460 | | | | | | | | |

*Note:* The sample is women who were never married and teaching in a city school at the previous census. Panels A, B, and C include women aged 18–27, 28–37, and 38–47 at that census, and women in the older panels are more selected. This table repeats Table 6 with children in place of marital status. Has a child means any own child in the household, so it measures children living at home rather than fertility. Teaching, non-teaching job, and not working sum to the child total, and in labor force is the sum of teaching and non-teaching job. Labor force participation is having an occupation code below 971. Effects are in percentage points. The control mean is the mean in never-treated cities across all years. Estimates are Callaway–Sant'Anna average treatment effects on the treated, using never-treated cities as controls with doubly-robust inverse-probability weighting and standard errors clustered at the city level. *** p<0.01, ** p<0.05, * p<0.10.

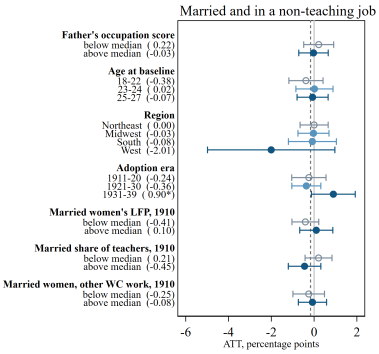
Figure A3: Heterogeneity in the Effects on Never-Married Teachers



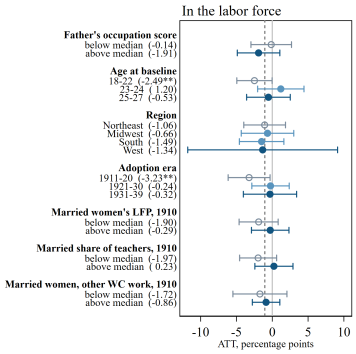

(a) Married and in the labor force



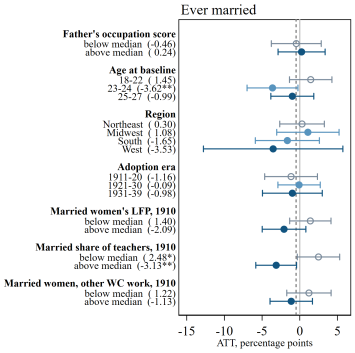

(b) Married and teaching




(c) Married and in a non-teaching job




(d) In the labor force




(e) Ever married

*Note:* Each panel estimates one outcome within subgroups of women who were never married and teaching in a city school at the previous census, aged 18–27 then. Father's occupation score is the household head's score at the baseline census, for women who were then the daughter of a male head, split at the median. The age groups each hold about a third of the sample. Region is the four Census regions. Adoption era divides treated cities by decade of adoption and keeps every control city in each group. The three 1910 city measures (married women's labor force participation, the married share of women teachers, and married women's work in white-collar occupations other than teaching) come before every adoption in the sample and are split at the median across sample cities. Because these splits are by place, they may reflect other differences between cities. Hollow markers are the first group of each split and filled markers the others; bars are 95 percent confidence intervals, and the dashed line is the pooled estimate in Table 7. Estimates are Callaway–Sant'Anna average treatment effects on the treated, in percentage points, using never-treated cities as controls with doubly-robust inverse-probability weighting, with standard errors clustered at the city level.

Table A6: Robustness of the Effects on Never Married Teachers Aged 18–27 at Baseline

| | Married and In Labor Force | Married and Teaching | In Labor Force | Ever Married |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Baseline | -1.74*** | -1.58*** | -1.04 | -0.49 |
| | (0.63) | (0.56) | (0.96) | (1.07) |
| Not-yet-treated cities as controls | -1.77*** | -1.61*** | -1.03 | -0.57 |
| | (0.63) | (0.56) | (0.98) | (1.07) |
| Baseline census before adoption only | -0.82 | -0.90* | -0.72 | -0.01 |
| | (0.57) | (0.50) | (0.93) | (1.01) |
| Controlling for age at baseline | -1.79*** | -1.61*** | -0.82 | -0.77 |
| | (0.63) | (0.56) | (0.97) | (1.03) |
| Standard errors clustered by state | -1.74** | -1.58** | -1.04 | -0.49 |
| | (0.77) | (0.69) | (0.78) | (1.07) |
| Two-way fixed effects | -1.45** | -1.34** | -0.70 | -0.11 |
| | (0.63) | (0.57) | (0.85) | (1.00) |
| Two-way fixed effects, state-by-year | -0.40 | -0.40 | -0.53 | 0.33 |
| | (0.49) | (0.46) | (0.81) | (0.79) |

*Note:* The sample is women who were never married and teaching in a city school at the previous census, aged 18–27 then. Each row re-estimates the four outcomes of the first row, which repeats Table 6 and Table 7 Panel A. The second row adds cities that adopt later to the comparison group. The third row drops women whose baseline census came after their city adopted a bar. The two-way fixed effects rows regress each outcome on an indicator for the outcome census being at or after adoption, with city and census-year fixed effects (or state-by-year fixed effects in the last row). Effects are in percentage points. Standard errors, clustered by city unless stated, are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.