



Carolyn Tsao &lt;ctsao@princeton.edu&gt;

## Fwd: The Journal of Economic History - Decision on Manuscript ID JEH-2024-0189

1 message

Amy Kim <kimamy@princeton.edu>  
To: Carolyn Tsao <ctsao@princeton.edu>

Thu, Jan 16, 2025 at 7:36 PM

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From: **The Journal of Economic History** <[onbehalf@manuscriptcentral.com](mailto:onbehalf@manuscriptcentral.com)>  
Date: Fri, Jan 17, 2025 at 8:13 AM  
Subject: The Journal of Economic History - Decision on Manuscript ID JEH-2024-0189  
To: <[yk0581@princeton.edu](mailto:yk0581@princeton.edu)>

16-Jan-2025

Dear Ms. Kim:

Manuscript ID JEH-2024-0189 entitled "The Effects of Prohibiting Marriage Bars: The Case of U.S. Teachers", which you submitted to The Journal of Economic History, has been reviewed. The comments of the reviewers are included at the bottom of this letter.

Both reviewers believe that the manuscript has the potential to advance scholarship on the effects of institutionalized discrimination and the rise of female labor participation in the twentieth century U.S. They do, however, raise some concerns and make suggestions for improving the paper.

Based on the reviewers' recommendations and my own reading of the paper, I have decided to invite you to revise and resubmit the paper. I believe your paper has promise, but I must make clear that resubmitting your manuscript does not guarantee eventual acceptance. I will send the resubmission to the same reviewers, and I will make my decision based on their evaluation of how you have addressed the concerns raised in this email and their full reports (which are provided below).

Here are the key issues I would like you to address in the revision:

### 1. Provide more historical context

Both referees make this request. The current manuscript provides a discussion of the history of marriage bars in teaching but not the conditions in which they operated.

The glaring omission for me -- and noted by both referees -- is a discussion of how the Great Depression and the New Deal may have affected the outcomes you study. As Referee 2 notes, there is an extensive literature on how the Great Depression and New Deal affected labor supply, household formation, and fertility decisions. At the very least, you need to discuss whether any of your results, which are based on variation across states, could be explained by the geographic variation in intensity of the economic downturn or the level of New Deal expenditures. I believe it would also be helpful to think through whether being in the midst of the Great Depression dampened or amplified the effects of prohibiting marriage bars. Would we have expected bigger or smaller shifts from single to married women teachers if the country had been in more prosperous times?

I am also interested in whether the Great Depression played a role in the politics around marriage bars and their prohibition. For instance, did the economic crisis lead to the intensification of efforts to eliminate marriage bars because of the role women's wages played in the family economy?

Referee 1 wants to know how teacher training varied over your study period and if and how it varied across states. Training to be a teacher was an investment decision; marriage bars imposed constraints on the returns to that investment. How then did marriage bars impact who trained to be teachers and how long they remained in the profession? Did this impact vary across time and space in a way that may affect your analysis?

### 2. Analyze individual-level data

As Referee 2 notes, using the individual-level data would allow you "to control for rich personal and household characteristics available in the census data, addressing omitted variable biases more effectively." They would also allow

you to "capture heterogeneity in the effects of the prohibition on various subgroups, such as those defined by age, county population size, and economic structure." Particularly for the linked census data, I do not see the value of limiting the analysis to county-level aggregates. Those aggregates are difficult to interpret in any case because they only include the linked individuals.

3. Describe how you deal with geographic mobility in the linked samples

Referee 2 raises this question and is one of the reasons they argue for using the individual-level data. Do you define an individual's county as where they lived in year t-1 or t?

4. Discuss whether the effects of the prohibition of marriage bars differed between NC and KY

Based on your description on page 11 of the laws, my expectation is that the effects would differ across these states. NC passed a law in 1933 that was according to your description, "broad in its application." KY passed a law in 1938 that prohibited "rules preventing marriage of any school teacher who has had five years or more teaching experience." I would expect a much smaller effect for KY given its narrower scope and the fact that it had only been in effect for 2 years in 1940.

5. Provide more discussion of your findings in terms of retention versus hiring practices

I share Referee 1's confusion about whether your results indicate changes in retention or hiring practices: "I had a difficult time understanding why many of the results were attributed to hiring of married women as opposed to retention of married women. On page 22, you find that retention is important. Similarly, are you sure that unmarried teachers are being "let go" (p.24) as opposed to not hired?"

6. Drop, or better defend, the back of the envelope calculations presented on pages 20-21 and Appendix C

I found these calculations unconvincing. In my opinion, they require some heroic assumptions: (1) that KY and NC are representative of the experience in all states across the country; (2) that the teaching labor market is representative of the labor market for all white collar female workers; and (3) that your estimate of the "treatment" effect of prohibiting marriage bars was not dampened or amplified by being observed during the Great Depression.

The referees also raise a number of other minor questions and concerns. When you resubmit your manuscript, please provide a document that responds point-by-point to the issues raised by me and the referees.

Thank you for submitting your paper to the JEH. I look forward to a resubmission.

Regards,  
Carolyn

Dr. Carolyn Moehling  
Co-Editor, The Journal of Economic History  
[carolyn.moehling@rutgers.edu](mailto:carolyn.moehling@rutgers.edu)  
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Reviewer(s)' Comments to Author:

Referee: 1

Comments to the Author

Summary: This paper compares the teaching employment of single women, married women, and all men before and after the introduction of laws in two states that prohibited schools from discriminating against married women in employment compared to surrounding states. It finds that overall the teaching employment of married women increases while that of unmarried women decreases. There's no effect on men's teaching employment. These effects do not seem to be driven by single teachers getting married, but by married women substituting being out of the labor force for being

teachers.

Major: I have no major concerns.

Minor:

The biggest minor concern that I have is that I'm not sure that 1910 should be in your main specifications. It makes more sense to have two decades before if you're having two decades after. (Or it might make sense just to compare 1930 to 1940, since 1950 isn't in the linked data.) I don't think removing 1910 will affect your results given Figure 2 (and it should still be shown in Figure 2). Similarly in the linked data, I don't understand why you have 1910-1920 when you only have 1930-1940 in the post. (And then Table 5 doesn't use 1910-1920?)

For footnote 1, I don't think that this is necessarily a good reason to omit non-White women (1. It isn't teacher specific, 2. More detail is needed about why the laws wouldn't apply to married Black women—do Black school districts just not use them?). I think instead you should elevate A4 and A5 to be part of Figure 2. The odd pre-trends for Black women seem reason enough, and the fact that putting them in with the White women doesn't affect the results (presumably the sample is smaller) helps mitigate concerns about p-hacking. I would also find some historical evidence that Black and White teachers are not substitutes in the Jim Crow South so the patterns for Black teachers are unlikely to be affecting patterns for White teachers as they are different schools. (Maybe Carruthers and Wanamaker have a paper?)

In the background, provide a little information on how difficult it was to get a teaching degree. How long does it take to complete normal school (do they need to complete normal school?)? Does this change during your time period in your states? (The historical examples I know take 1-3 years, but are not in the South.) (It might be helpful to have a section in the lit review on the teaching environment—what did these schools look like? Were there one-room school houses? Were there separate grades? Did this change over time? Were schools segregated? Etc.)

Think about other things that might be going on in your treatment states compared to your control states that could affect married vs. single women in the labor force and argue or demonstrate that they are not problematic. What have you come across historically? (Ex. did time to degree change differentially? Did marital property laws change differentially? Child labor laws?—this is probably irrelevant because you find no effects on fertility. Compulsory schooling laws? Any depression-era projects? I'm not sure what else might be relevant.) I think it is unlikely that there's anything else relevant in the 1930s that just affects your two treatment states or affects your control but not treatment states, but it would be helpful to double check and then state what you checked.

Are your results different for KY and NC?

Footnote 4: Be more clear about what you do when you say you condition on women's employment and marital history. When I first read this comment, my thought was, isn't that endogenous to the laws? But I think you mean you're using a matched sample following the same women to condition on their employment and marital history prior to the laws, which would not be endogenous.

Do the laws affect educational attainment of affected women at all (that is, are girls in high school(?) more likely to invest in education if they know they can be employed long-term as a teacher, whether or not they actually become a teacher?) This would be separate from the question of if teacher retention affects quality.

Speaking of quality—20<sup>th</sup> century literature suggests that there are large returns to experience for the first few years of teaching. Could students have better outcomes with less teacher turnover as a result of this law or do your results preclude that possibility?

p.11 What kinds of bans did KY and NC have before? Do you have any (suggestive) evidence? Do you have any way of knowing if the effects are different by type of ban (maybe at the county level?) or would that be too much/impossible data collection?

I had a difficult time understanding why many of the results were attributed to hiring of married women as opposed to retention of married women. On page 22, you find that retention is important. Similarly, are you sure that unmarried teachers are being "let go" (p.24) as opposed to not hired?

On page 13 where you say small numbers of Black teachers, provide the actual numbers.

p.14 top: Linkage rate problems—do you have any thoughts on the potential direction of biases?

When you discuss Table 1, I think you're trying to do two things: 1. Show how well the southern states you've chosen work as a control group for KY/NC, and 2. What is the external validity of your results to other states. I think making the motivation more clear up front (why are you providing this information) would help when you discuss the summary statistics. Also, I would start with discussing your preferred comparison group rather than ending with it (since economists generally care about internal validity first and external second).

p.15 Explain why you are doing things at the county level.

P.16 top: do you mean year or decade?

Good job on explaining why you're doing what you're doing re: clustering. I had the question you answered in the next sentence.

Footnote 23: Dropping: Why? How many do you lose? Is there evidence of selection? (Also are these dropped in your Table 1?)

P. 19: TN, WV—are these not treated by the court decisions (they are in your control group)? When did the court decisions happen?

Put footnote 27 in the paper proper.

When you discuss your mechanisms results, be a little more clear and remind people what you're doing for each of these specifications. They're more complicated than the previous results and it is difficult to understand what you're doing without looking at the tables at the same time.

Do you have any findings on wages?

Put in numbers for results that you are currently just stating as directions (ex. 5.3)

Figure 1: Is the untreated mean already starting to move in 1930?

Figure 2 is really compelling

Tables: Make number of decimal places consistent. Are the treated and control states significantly different for any of the variables in Table 1?

Figures A7 and A8 look weird with the SE, particularly given Table B1 Column (7)—could you put in sample sizes? Explain more about the dataset, the universe.

Minutia:

"prohibition of marriage bars" is difficult to parse the first time it comes up—is it the prohibition of marriage? Or are marriage bars being prohibited? (It's the latter, but is there a way to make that immediately clear either with the use of a hyphen or some rephrasing?) My preference would be the longer, "laws that prohibited schools from discriminating against married teachers (prohibition of marriage bars)" or something similar the first time you introduce it

p.4 What do you mean by "unconditional likelihood" that a married woman worked... unconditional on what?

Top of page 5, you note that the bulk of employment discrimination against married women did in fact cease—I think you mean legislated employment discrimination or statutory employment discrimination? (Audit studies and lab experiments still show discrimination against married women, particularly those with children, for high level positions in the US and many positions in China.)

Top of page 7 is kind of repetitive and could be cut or consolidated with previous paragraphs (paragraph that discusses Goldin)—I think you've already said most of this by this point.

p.14 "counties in KY and NC had higher student-to-teacher ratios" compared to who?

What is "Sample 1" in Table 5?

Table B2 would be more clear I think if you used and (the upside down U) instead of |, since I think you're saying that they're a teacher in T and married in T, whereas when you have the conditional you're thinking teacher in T+1 given married in T. (What you have is correct, it's just going against a convention and making it more confusing than it needs to be.)

Referee: 2

Comments to the Author

"The effects of prohibiting marriage bars: the case of U.S. teachers" studies the effects of the legislative prohibition of marriage bars in 1933 North Carolina and 1938 Kentucky on the labor market. Two county-level panel data sets are constructed, one using full-count decennial census data and another using the Census Tree linked census data. The identification strategy is a difference-in-differences analysis between two treated states (NC and KY) and neighboring

Southern states (SC, VA, TN, and WV). The result shows that the prohibition increased the share of married women teachers and decreased the share of unmarried women teachers. The manuscript concludes that the prohibition modestly increased women's labor force participation. I believe that, using novel data, the manuscript carefully studies important issues in economic history, namely the rise of female labor supply, gender discrimination in the labor market, and the role of legal institutions on human behavior. The findings are intriguing, and the results are presented with thoughtful discussions. My remaining questions and comments for revision are as follows.

First of all, I am wondering why the county-level aggregate analysis is primarily reported, rather than analyzing rich individual-level data. The prohibition of marriage bars shifts labor demand, and the outcomes can be captured along the supply curve. Ultimately, individuals make decisions about whether to work and what to do. In principle, county-level aggregate analysis using population weights could yield identical results to an uncontrolled individual-level analysis. However, individual-level analysis allows us to control for rich personal and household characteristics available in the census data, addressing omitted variable biases more effectively.

An individual-level analysis is especially desirable when using linked census data. Analyzing individual panel data would reveal detailed information about labor market transition affected by the prohibition between pre-treatment and post-treatment years, after controlling for personal and household characteristics. This approach would also help us capture heterogeneity in the effects of the prohibition on various subgroups, such as those defined by age, county population size, and economic structure.

Another question concerns women's mobility in the linked census data. Marriage may have led to geographical relocation across counties or states. Married women might also have moved across counties and states between two adjacent census years. I wonder how the current county-level analysis addresses this issue. I believe this issue could be better addressed by analyzing individual-level data rather than county aggregates.

Urban-rural gaps also need to be considered. In the 1930s and 1940s, especially in the South, a considerable share of the school-age population did not attend or graduate from high school. The gap in both the demand and supply of public education may have been significant between rural and urban areas, leading to differences in the demand and supply of school teachers. This is a county-level time-varying factor, not captured by fixed effects. While detailed data on the provision of local public schools may not be available, controlling for urban status should not be challenging. Local industrial structures are frequently used as a proxy for local demand for education and could be easily added to the regressions.

Additionally, it should be noted that during the period between 1930 and 1940, or the treatment period defined in the analysis of the manuscript, the Great Depression swept across the United States. The nationwide common impact of the Great Depression and subsequent public interventions can be separated using a difference-in-differences approach. However, state-specific and county-specific impacts may remain and complicate the analysis. Considering the local variation in public intervention in the labor market, identifying the causal effects of the prohibition on labor supply using a state-level difference-in-differences approach with county-level aggregate data could be challenging. I do not have any immediate suggestions to address this issue. A good starting point might be a careful discussion of the possible relationship between the regression results and the Great Depression or New Deal. If easily accessible county-level data capturing local variation in New Deal policies exist, conducting robustness checks using such data would be desirable. To address the heterogeneous impacts of the Great Depression on local labor markets, local unemployment rates could be added as control variables.

Finally, in addition to the research papers by Claudia Goldin and Martha Bailey cited in the manuscript, there is a wealth of economic history literature studying the rise of female labor force participation during the same period. For example, Bellou and Cardia (2021) examines the rise of female employment during the Great Depression. Costa (2000) provides a good review of the long-run change in U.S. female labor supply compared to international counterparts. Finegan and Margo (1994) argues that the labor supply of married women in this period was closely associated with the spouse's employment. Hill (2015)'s findings on the relationship between economic conditions and marriage during the Great Depression also warrant attention.

#### References

- Bellou, Andriana and Emanuela Cardia. 2021. The Great Depression and the Rise of Female Employment: A New Hypothesis, *Explorations in Economic History*, 80: 101383.
- Costa, Dora L. 2000. From Mill Town to Board Room: The Rise of Women's Paid Labor, *Journal of Economic Perspectives*, 14(4): 101-122.
- Finegan, T. Aldrich and Robert A. Margo. 1994. Work Relief and the Labor Force Participation of Married Women in 1940, *Journal of Economic History*, 54(1): 64-84.
- Hill, Matthew J. 2015. Love in the Time of the Depression: The Effect of Economic Conditions on Marriage in the Great Depression, *Journal of Economic History*, 75(1): 163-189.