RE: The Journal of Economic History - Decision on Manuscript ID JEH-2024-0189.R1

Dear Prof. Moehling,

Thank you so much for moving our paper to "conditional accept" status at *The Journal of Economic History*!

In what follows, we respond point-by-point to your comments and suggested edits (original comments from you are in *blue italics*, our reply in regular font).

Our draft also incorporates all of the careful line edits that you provided directly in our draft, and we have converted the document to Word.

Thank you again.

Sincerely,

Amy Kim and Carolyn Tsao

Editor's Comments

Here are the issues I would like you to address in response to the reviewers' comments:

1. Inclusion of age fixed effects – You should note in the text whether you include age fixed effects or other controls for age. Given that marriage is a key outcome variable (or component of the outcome variable) and the risk of marriage is strongly related to age, it seems important to control for age in your models. This is especially true for the models using the linked individual data.

Authors' reply:

Thank you for pointing this out. We have now updated all our main linked specifications to include age fixed effects. The only specifications we did not update were our cross-sectional results, which are at the county level, where age controls would have to be fairly coarse. We do, however, include a version of our cross-sectional analysis at the individual level which does include controls for age (see column (9) of Table B3). Our results do not meaningfully change with the inclusion of age fixed effects.

2. 1910 – Referee 1 is still concerned about the inclusion of 1910 in the estimated models. What happens when you drop 1910? I imagine that the effects are unchanged. If that is the case, you can provide a footnote to that effect.

Authors' reply:

Thank you for this suggestion. When we drop 1910, the effects are indeed unchanged. We have now added a footnote that says this explicitly in the specification discussion in Section 4.1, which we replicate below for your convenience:

> We include 1910 in our main specification in order to show difference-in-differences estimates for as long a pre-period as possible. The results do not change when we exclude 1910 and focus only on the twenty-year period before and after the prohibition of the marriage bars.

3. Bias due to linkage loss – If possible, it would be useful for you to provide some bounds on the possible bias due to the linkage loss.

Authors' reply:

Thank you for this suggestion. We agree that trying to bound the possible bias caused by linkage loss is a worthwhile exercise.

We have added a discussion on potential bias due to linkage rates to a new appendix on linkage, which now also contains all of our discussion on linkage rates from the previous

version of the draft. While our conclusion is that it is not possible to provide numerical bounds, we provide intuition on the different possible biases that may arise.

We replicate the new discussion on bias below for your convenience:

> Linkage loss could introduce bias to our results if linkage rates differed by women's treatment status, marital status, or employment status. We find similar linkage rates between treated and control states, so the first case is not a concern. However, the second and third cases may both be at play. On one hand, because single women who get married between censuses are less likely to be linked, single women who get married are underrepresented in our linked samples. As a result, using the linked data, our estimated effects of the prohibitions on the likelihood that a woman in $t - 10$ is married in t are attenuated towards zero. On the other hand, single women who stop working because they got married between censuses are also underrepresented in our linked samples. As a result, using the linked data, our estimated effects of the prohibitions on the likelihood that a woman in $t - 10$ is working in t are upward biased.

> It is difficult to bound these biases, as doing so would require estimating the number of single women who get married whom we are unable to link, which is inherently unobserved. However, what we can say is that our main estimate of interest in our linked analyses—that is, the effect of the prohibitions on the likelihood that women in $t - 10$ become married women teachers in t—is ambiguously biased due to linkage loss, given it is subject to two sources of bias that go in opposite directions and could even negate each other.

4. Broader implications of your results – Referee 2 asks that you provide "brief discussion in the conclusion on how their findings relate to the broader demise of marriage bars in other professions beyond teaching, or to the evolution of women's labor market opportunities on the eve of World War II."

Authors' reply:

Thank you for this suggestion. We have added a short discussion to the end of the conclusion on how we relate our findings to the broader trends in women's LFP during the mid-1900s, and particularly to the shock to labor demand brought on by WWII. We replicate the paragraph below for your convenience:

> More generally, our findings suggest that the broader decline of marriage bar use across occupations may have been an important force behind the rise in women's LFP by 1950. Marriage bars were already in steep decline by the 1950s and had largely disappeared by the 1960s ([3]). At the same time, women's LFP rose sharply between 1940 and 1960, and existing work highlights the role of WWII war production in driving the increase. More recent research, however, shows that while war production initially drove higher women's LFP, the war itself had "little direct impact" on women's LFP in 1950 ([4]). Given how quickly prohibiting

marriage bars in a single occupation raised LFP among women at the margin of teaching, our results suggest that the nationwide demise of marriage bars by 1950 meaningfully contributed to the broader rise in women's LFP, compounding the labor demand shock driven by WWII.

I also have a few issues that I would like you to address in the revision:

1. "Employer" rather than "firm" – Since you are looking at marriage bars imposed by school districts, it is important that throughout the paper, you talk about "employers" rather than "firms." In the attached marked up version, I did a universal "find and replace" of ""firm" to "employer."

Authors' reply:

Thank you for this note, and for implementing the find and replace already! We have checked and confirmed that all references to "firms" have been changed to "employer." (Some instances of "confirm" also become "conemployer", but we caught them all!)

2. White married women – When you discuss the social norm of married women's place being in the home rather than the labor market, you should make clear that this is about white married women. Black married women had much higher labor force participation rates during this period. You do note this, but sometimes after statements that seem to suggest that the social norm was for all married women. I would be more upfront about the fact that this is really a story about white women. As an aside, I think you should emphasize more the difference in your findings by race.

Authors' reply:

Thank you for this suggestion. We agree that it is very important that we are clear about how the context differed by race, and how those differences factor into our findings. We have adjusted the language throughout to clarify that the social norm of married women not working was specific to white women and not Black women, and to emphasize that the effects we find are driven by white women and not Black women.

We add three main clarifications in the text to address this point, and replicate them below for your convenience.

1. When first describing our main results in the introduction, we write:

   > Our main finding is that prohibiting marriage bars in teaching *increased* the employment of married women in teaching, specifically by increasing white women's employment, with no effect on Black women.

2. At the beginning of describing our context in Section 2.1, we write:

   > Importantly, marriage bars were mainly a discriminatory barrier to employment for white married women but not for Black women, as Black women were signifi-

cantly more likely to work during this time period due to Jim Crow era laws ([2]), and tended to work in highly segregated occupations in agriculture, manufacturing, or in the case of education, in segregated schools ([1]). As such, the details that follow largely describe employers' perceptions of white married women in the workplace.

3. Before describing our results on mechanisms in Section 5.2, we write:

> Given our findings that the marriage bars had no direct effects on Black teachers, from here on, we focus on white teachers in our analysis of mechanisms.

We also clarify that we are referring to "white married women" and not just "married women" in many places throughout the text.

3. Labor force participation rates – In the introduction of the paper, you describe your findings in terms of what they imply about labor force participation rates. But you do not present any estimates of the effects of the legislation on labor force participation rates per se. You examine at shares of married women in the teaching labor force and then examine at the likelihood different groups of women were teachers or *not* in the labor market, but you never examine – at least in the manuscript submitted – labor force participation. I recommend you add results for married women's and all women's LFP to the paper for completeness.

Authors' reply:

Thank you for this suggestion. We have updated two tables in the paper with results that speak to how the prohibitions affected women's LFP, and now mention these results explicitly in the introduction and in the results section.

In short, we detect no statistically significant effect of the prohibitions on all women's LFP, which is unsurprising, given the prohibitions targeted teaching specifically and only a small share of women were teachers. However, we do find a statistically significant effect of the prohibitions on the LFP of women who were unmarried and teaching prior to the prohibitions, suggesting that the prohibitions *did* increase women's LFP, albeit for a small and specific population.

We describe these findings in our introduction in the following paragraph, which we replicate below for your convenience.

> Finally, we use our linked census sample to investigate what happened to the unmarried women who were, or might have eventually become, teachers. We find that the laws did not deter unmarried women from later becoming teachers. Instead, we find evidence that the laws led to unmarried women who were already teachers to switch into other occupations, without affecting their earnings as proxied by occupational scores. In addition, we show that the laws increased overall LFP among unmarried women who were already teachers, where the increase was entirely driven by those who became married and remained in the labor force. We therefore conclude that the policy change had a net positive

impact on women's labor market outcomes, pulling women into the labor force without pushing incumbent women teachers out of the labor force or to lower paying jobs.[1]

We also replicate the updated tables (Supplemental Table B5, and a new Supplemental Table B8) and our discussion of the new results in Section 5.3 here, for your convenience:

These displaced unmarried women teachers did not leave to lower-paying jobs, but we find some evidence that they may have been induced to move. Supplemental Table B5 examines the effect of the prohibitions on the *occupational scores* (Columns 2 and 3), LFP (Columns 4 and 5), and mobility (Columns 6 and 7) of unmarried women teachers, where occupational scores function as a proxy for earnings given income was not collected in the Census prior to 1940. Although the prohibitions increased occupational scores and labor force participation on average across all incumbent unmarried teachers (Columns 2 and 4), we find these increases were entirely driven by those in treated states who got married and kept their jobs (Columns 3 and 5). These findings suggest that unmarried teachers who stayed unmarried kept working at a similar rate following the prohibitions, and that those who switched jobs switched to to occupations that paid similarly to teaching. Similarly, although the prohibitions increased movement out of state on average across all incumbent unmarried women teachers (Column 6), we find that this difference is almost entirely driven by those who remained unmarried (Column 7). These results are consistent with incumbent teachers (in particular unmarried women, who experienced no positive retention effects from the prohibitions) pursuing other jobs or wanting to teach in states that were unaffected by the prohibitions.[2]

Taken together, our results suggest that the prohibitions increased women's LFP and weakly increased women's earnings, albeit only in the occupation that the prohibitions targeted.[3] Unmarried women teachers who stayed unmarried maintained the same rate of LFP, either in teaching or in jobs that paid similarly to teaching (Supplemental Table B5, Columns 2 and 4). However, unmarried women teachers who got married became significantly more likely to stay in the labor force, and thus earned significantly more than they would have otherwise (Supplemental Table B5, Columns 3 and 5).

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[1]That said, we detect no effect of the prohibitions on all women's LFP. This is unsurprising, given that that the prohibitions targeted teaching specifically and only a small share of women were teachers.

[2]We also consider the possibility that the prohibitions reduced the number of unmarried teachers by affecting the number of young women who *became* teachers, but find no evidence that this was the case. Supplemental Table B7 shows that the prohibitions had no effect on the likelihood of unmarried women entering teaching and stayed unmarried.

[3]We detect no effect of the prohibitions on the overall LFP of women, as shown in Supplemental Table B8. However, this is unsurprising, given that the prohibitions only targeted teaching, and—despite teaching being an important occupation for women—teachers comprised only a small share of all women (2.2% of working-age women in 1930).

Table B5: Estimated effects of marriage bar prohibitions on fertility, occupational scores, LFP, and mobility for women who were unmarried and teaching in $t-10$

Dependent Variable:	Has child in t	Occupational Score in t		In Labor Force in t		Moves state in t	
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × 1940	-0.0251**	0.8418**	-0.0814	0.0266**	-0.0033	0.0391***	0.0589***
	(0.0118)	(0.3655)	(0.2838)	(0.0128)	(0.0100)	(0.0095)	(0.0119)
Treated × 1940 × Married in t			1.526***		0.0442***		-0.0429***
			(0.4302)		(0.0151)		(0.0131)
Dep. Var. 1930 Treated Mean	0.4819	10.39	10.39	0.4091	0.4091	0.1421	0.1421
County fixed effects	Yes	Yes	Yes	Yes	Yes		
Age fixed effects	Yes	Yes	Yes	Yes	Yes		
Observations	59,542	59,542	59,542	59,542	59,542		
Adjusted R^2	0.10860	0.13437	0.46280	0.11917	0.47197	0.02193	0.04099

Notes: All columns show results from estimating Equation (2) for women in linked Sample 1, defined in the notes of Table 3. The outcome in Column (1) is an indicator for whether the woman has child in year t. The outcome in Column (2) is the woman's occupational score in year t. We include women who are unemployed, for whom their occupational score is 0. The outcome in Column (4) is an indicator for whether the woman is in the labor force in t. The outcome in Column (6) is an indicator for whether the woman lives in a different state in year t compared to year $t-10$. Columns (3), (5), and (7) add to the previous column an interaction term for whether the woman is married in year t. Standard errors are clustered at the county level. All specifications include county and age fixed effects.

Table B8: Estimated effects of prohibitions on the labor force participation of women.

Dependent Variable:	Pr(In Labor Force | Woman)							
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Treated ×1940 (γ_{1940}^{DD})	0.0062	0.0091**	0.0001	0.0042	0.0019	0.0083*	0.0057	0.0095
	(0.0046)	(0.0039)	(0.0087)	(0.0045)	(0.0057)	(0.0049)	(0.0054)	(0.0069)
Treated ×1940 × *Married*			0.0072					
			(0.0108)					
Treated ×1940 × *Nonwhite*				-0.0023				
				(0.0132)				
Treated ×1940 × *Urban*					0.0141			
					(0.0118)			
Inverse Weighted by County	No	Yes	No	No	No	No	No	No
Observations	21,423,773	21,423,773	21,423,773	21,423,773	21,423,773	16,739,277	16,533,201	5,859,581
Adjusted R²	0.07115	0.06639	0.16767	0.10433	0.08549	0.07904	0.07183	0.07169

Notes: Estimation of Equation (2) for the cross-sectional sample of all women in our balanced sample of counties in treated (KY, NC) and neighboring Southern control states (VA, SC, TN, WV). The outcome variable for all regressions is an indicator for whether an individual is in the labor force. Column (1) includes all women with no weights or controls. Column (2) includes inverse weighting of each observation by the number of women in a county and year, effectively imitating a county-level regression. All remaining columns do not use weights. Columns (3), (4) and (5) include indicators for whether a woman is married (*Married*), whether a woman is non-white (*Nonwhite*), and whether a woman lives in an urban area (*Urban*) respectively as additional interaction terms. Column (6) includes county-level controls for the share of workers in manufacturing, the share of workers in agriculture, unemployment rate, and log population. Note that the sample size is smaller because full-count Census data on employment status was only available in 1930 and 1940. Column (7) excludes counties in the Tennessee Valley Authority (see Section 4.3 for further discussion). Column (8) includes only border counties, defined in Supplemental Appendix C. All regressions include county, year, and age fixed effects and use 1910-1950 full-count cross-sectional decennial Census data unless otherwise stated [5]. Standard errors are clustered at the county level.

4. "Push" of unmarried women out of teaching – I am not sure that "push" is the right verb to describe your results. I think if unmarried women were "pushed" out of teaching, we would observe them to be less likely to be in the labor market or in occupations that had lower occscores. Instead, you find that they are in the labor market and in occupations with similar occscores. Also, to make the "pushed" argument, I think you have to be able to tell a story about how this took place. Do you think that school districts fired unmarried women teachers to hire married women teachers? I think it is more likely that unmarried women left teaching jobs for other white collar jobs and that married women were more likely than unmarried women to fill those vacancies. "Push" doesn't seem the write verb for that story. But maybe you have another story in mind that leads you to use the term "push." If so, you should present that story and explain how your data support it.

Authors' reply:

Thank you for this thoughtful note. While we have used the verb "push" to really mean "changing jobs," we agree with you that it is not clear that "push" is the right word to use here, as readers may interpret the verb "push" to mean "pushed out of the labor force entirely" or to lower-paying jobs. It is also not possible using our data to get at the story that you describe, and distinguish whether women are fired versus leave teaching of their own volition.

As such, in all instances in the manuscript where we describe unmarried women going from teaching to other occupations, we now use the language "leaving/switching from teaching to other occupations." We keep occasional usage of the verb "push" to refer to women exiting the labor force, and clarify in these cases that we are referring to women being "pushed out of the labor force."

References

[1] Leah Platt Boustan and William J Collins. The origin and persistence of black-white differences in women's labor force participation. In *Human capital in history: The American record*, pages 205–240. University of Chicago Press, 2014.

[2] Dora L Costa. From mill town to board room: The rise of women's paid labor. *Journal of Economic Perspectives*, 14(4):101–122, 2000.

[3] Claudia Goldin. Marriage bars: Discrimination against married women workers from the 1920s to the 1950s. In Patrice Higonnet Henry Rosovsky, David Landes, editor, *Favorites of Fortune: Technology, Growth, and Economic Development since the Industrial Revolution*, pages 511–536. Harvard University Press, 1991.

[4] Evan K. Rose. The rise and fall of female labor force participation during world war ii in the united states. *The Journal of Economic History*, 78(3):673–711, 2018.

[5] Steven Ruggles, Matt A. Nelson, Matthew Sobek, Catherine A. Fitch, Ronald Goeken, J. David Hacker, Evan Roberts, and J. Robert Warren. IPUMS Ancestry Full Count Data: Version 4.0. Dataset, Minneapolis, MN: IPUMS, 2024.