This very interesting paper examines an understudied phenomenon in the Female LFP literature, the case of marriage bars in the teaching occupation that excluded married women from employment. I think it's a very interesting research question to understand how important marriage bars were for excluding women from the labor force, versus just substituting between married and unmarried women. I have a few large comments:

—The definition of outcome variables is strange to me. The first one is share of teachers that are married, and then it's share of married women who are teachers. I would have expected a much more conventional LFP measure for married women first, and then for all women. I don't see why this wouldn't be possible with the census data. One would not even need to know who was a teacher to just look at whether the removal of these bars in one of the most important occupations for women increased married female LFP. Then, looking at overall LFP would answer the question as to whether any gains for married women were just displacing single women.

—As it is, the authors need to make *a lot* of choices / assumptions that to me limit the generalizability of their findings. Increasing the share of teachers that are married women seems like a very direct effect of the policy—kind of like a first stage—but not an actually interesting policy object the way increase LFP of married women and women overall would be.

—The first of these choices I take issue with is the decision to exclude Black women, who, the authors note, were more likely to be employed when married. There is some possibility that marriage bars in teaching were partly about excluding more experienced Black women from these professions in favor of younger, unmarried white women (although I have no idea to what extent teaching was integrated in the relevant time and place). If this is the case, then excluding Black women from the analysis may be missing some of the possible overall effects.

—The next choice is how you define the outcome variable as a "share". The authors do not spend any time discussing their outcome variables or data set construction in the empirical specification. The regression equation is only meaningful when you define what is on the left hand side, how it is constructed, and what universe of data it's being run on. Is the share of white teachers who are married defined as a county-level cell? State level cell? Using a share as an outcome variable requires a number of choices that, again, a simple individual-level LFP would not require. (And even if the authors choose not to change the defendant variable, I do think they should add a description of how it's constructed).

—They then use a matched sample of teachers to do a longitudinal analysis of women who were already teachers to see if they remain in the profession once married women were allowed to enter. This seems overly complex to me, and again makes assumptions that don't seem necessary. I expect the effects were not on pushing women out, but rather on not needing to hire new unmarried women to replace those who were excluded once they became married. I.e., I expect tenure to increase, and I expect a lower share of new graduates to go into teaching. This will not be found by looking at retention of single teachers. The longitudinal analysis should be secondary to the overall LFP analysis, which will catch whether new grads are having a harder time finding jobs.

I think this is a nice empirical setting, and the authors should persevere and "clean up" the analysis to make it more impactful!