

Robustness Appendix for “A Cautionary Tale About the Use of Administrative Data: Evidence from Age of Marriage Laws”

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This online supplement contains additional information which was not included in the main paper due to space constraints. This appendix contains a number of robustness checks and supplementary evidence about the results in the paper.

1. Alternative Estimates of the Effect of Legal Restrictions on Age at Marriage Using the Census

The main paper focuses on the discrepancy between Census and Vital Statistics and the lessons to be learned for researchers about avoidance. Here, we note some evidence regarding the effect of legal restrictions on the age of marriage using the preferred data source, retrospective decennial Census data. We provide estimates of the effect of both non-consent and consent laws. The main paper includes information about variation in non-consent laws. We provide analogous information here regarding consent laws in Table 1, which summarizes the variation across states and overtime in consent laws for each sex.

Using data from the 1980 Census, we test for an effect of legal prohibition on actual age at marriage. We estimate the following equation:

$$Y_{gst}^i(a) = \beta_0 + \beta_1 P_{gst}(a) + \Gamma_b + \Gamma_s + \varepsilon_{gst}^i \quad (1)$$

where g indexes gender, s indexes state, and t denotes birth cohort within the Census. In (1), the vectors Γ_b and Γ_s are, respectively, birth cohort and state fixed effects; and ε_{gst}^i is a random error term. The binary outcome variable Y_{gst}^i indicates whether an individual i of a given gender, state and birth cohort is ever married by age a ; P_{gst} is a binary variable indicating whether, in a given year and state, the person was never able to legally marry before turning age a . So, for example, to assess the impact of non-consent laws on marriage before age 18,

Y measures whether the individual was ever married by age 17, P_{gst} equals 1 if there was no time in the years before they turned 18 that the marriage laws allowed the person to legally marry. The coefficient β_1 measures how much a legal age constraint against marriage lowered the likelihood of marrying. The inclusion of state and cohort effects in (1) means that β_1 is identified from changes in marriage laws within states and across cohorts.

Table 2 presents the results from estimating equation (1). The top panel shows results for men and the bottom panel shows results for women. The first row of the top panel shows the estimate of the effect of laws that do not allow men to marry without parental consent before the age of 21 on the probability of being married by age 20. The results suggest that there is a significant negative effect of these laws on the cumulative probability of marriage at a younger age. The magnitude of the coefficient can be estimated by dividing it by the share of men married by age 20, which is 0.235 in 1970.¹ This suggests that the likelihood of being married by age 20 is reduced by 3.2 percent in a state that has a legal marriage age of 21 (without consent) versus a state with a lower legal marriage age.

We also look at the effect of changing the age of marriage with parental consent. For men, a significant number of states reduced this minimum age from 18 to 16 during the time period studied in the regressions. Hence, we estimate the effect of not being able to marry without consent before age 18 on the probability of marriage by age 17. The point estimate is unexpectedly positive, but small and statistically insignificant.

The bottom panel shows similar estimates for women. When the legal age of marriage without parental consent was 20 or 21 among women, there is a statistically significant and negative effect on younger marriages. We also look at the effect of age limitations on marriage with parental consent for women. The results indicate that imposing a 16-year-old age of consent reduces marriage among women age 15 or younger. Relative to the mean number of marriages at age 15 or younger, our estimate suggests that the legal restriction is associated with a 15.3 percent decline in young marriages among women.² Our estimates suggest that age of marriage laws did impact the marriage choices of the young adult population, with larger effects on women than on

¹ This is the average including data from all states.

² In this respect, our results agree closely with Dahl (2005), who focuses only on age of marriage with consent laws on women.

men. Changes in the age of marriage without parental consent have a significant but not particularly large effect, with about a 2 to 3 percent change in the probability of marriage. This is much smaller than the effect of non-consent laws estimated in the Vital Statistics (with a different specification), as discussed in the main paper.

2. Our Main Results are Robust to Concerns About Recall Bias

One potential reason for differences between contemporaneous Vital Statistics data and retrospective Census data is recall bias. We use the 1980 Census in our main results because a 5% sample is available in that year, whereas earlier samples are only one-fifth that size. If recall bias is a problem, it is because people's answer about their age at first marriage changes depending on when you ask them. To demonstrate that recall bias is not generating our results, we show that data from the 1960 Census and the 1970 Census are statistically indistinguishable from data from the 1980 data we use in our main result.

Here, we replicate the measures used in our main analysis for marriages in 1950 in selected states and compare these across waves of the Census. The results, reported in Table 3, show that (with one exception) the age distributions are statistically indistinguishable at the 95% level.³ There are a variety of ways to test the equivalence of two data sets. We chose to report these tests of the equivalence between waves of the Census because we think that they are most relevant to interpreting the potential importance of recall bias for the particular tests that we emphasize in the paper.

3. Our Main Results are Robust to Concerns About the Inclusion of Remarriages in 1950 Vital Statistics Data

The age disaggregated data in the Vital Statistics in 1950 includes both first marriages and remarriages, and we compare these, out of necessity, to first marriages from the Census in the main text of the paper. Since there are very few first marriages at the youngest ages, and proportionately more at older ages, the inclusion of remarriages will have a predictable effect on the distribution: including remarriages will exaggerate the older ages and undercount the younger ages. The inclusion of remarriages, therefore, could potentially explain part of the discrepancy between Vital Statistics and Census estimates of marriages in 1950.

³ The pairwise comparison that we use here is the same that we describe in the text of the main paper.

In this appendix, we perform the best possible interpolation of remarriages using additional information from the 1950 Vital Statistics to demonstrate that the inclusion of remarriages, rather than explaining our findings, works against us. The 1950 Vital Statistics records indicate, in a separate table, the rates of first marriage and remarriage by state for several age categories (14-19, 20-24, etc.). We identify the set of states that match our sample as closely as possible (there is not perfect overlap in availability across the tables) and calculate average remarriage rates for the available age categories. Then, we take the same set of states and calculate the mean age within each category (e.g., the mean age of women 14 to 19 who marry in 1950 is not 16.5, it is closer to 19). This provides three pairs of data (age and remarriage rate) for an appropriate set of states in 1950. We then use these three points to interpolate the estimated remarriage rate at each age.

Table 4 repeats the comparison emphasized in the main paper using 1950 Vital Statistics data, upon which this interpolation has been performed, to the Census estimates. On the left, the results reported in the main paper, using the original Vital Statistics data, are included for reference. On the right, we report interpolated estimates using a third degree polynomial to fit the remarriage data function exactly.⁴ The interpolation has the expected effects. The adjustment closes the gap between data sources at younger ages, but only to a very small degree. The pronounced difference in Census estimates of the youngest marriages remains after the interpolation. The adjustment has a more significant effect on the spikes at middle ages in the Vital Statistics, which makes the strategic discrepancy between data sources look much larger. The adjustment also causes Vital Statistics estimates of marriages at relatively old ages to shrink, bringing Vital Statistics closer to Census at these ages, which is consistent with our hypotheses about how the data sets should differ if individuals avoided restrictive laws.

In sum, the best available adjustment for remarriages in the 1950 data appears to strengthen the data discrepancy that we emphasize in the paper. We therefore conclude that the conservative approach is to use the unadjusted data, which also has the added benefit of not requiring subjective decisions about how to perform an imperfect interpolation. We believe that the true discrepancy between first marriages reported in 1950 is probably larger than we estimate in the main paper.

⁴ We have experimented with other forms of interpolation and found consistent results.

4. Additional Information About the Quantity of Marriage Migration

The first type of legal avoidance that we analyze in the paper is the strategic movement of young people from their state of residence to nearby states with less restrictive laws, which we have called marriage migration. Here, we provide several pieces of evidence which suggest that marriage migration was relatively small in later years using disaggregated Vital Statistics data that is available starting in 1968. Estimates of the extent of marriage migration in 1968 to 1971 provide a plausible upper bound on migration in 1950 because (a) rising incomes likely lowered the cost of traveling to marry in another state, (b) the pronounced convergence of non-consent laws had not yet taken place and (c) documentation requirements made age misrepresentation (a substitute for migration) more costly in 1968 – 1971, as compare to 1950. Here, we provide several pieces of evidence that migration was of modest proportions in this later period, which we interpret as suggesting that some other factor (age misrepresentation) must have driven the data discrepancy in 1950.

Table 5 provides evidence about the extent to which people married outside their state of residence during the years 1968-71. As the first row indicates, between 1968 and 1971, 15.7 percent of all men and 10.3 percent of all women who marry, marry outside their state of residence. Men under the age of 21 are those most likely to be affected by legal age limits. The results show that these men are somewhat less likely to marry outside their state of residence (13.6 percent), while younger women marry away from home at about the same rate as all women (10.6 percent.) If we break this down by age, for younger teens we find relatively higher rates of marriage outside one's state of residence (at times exceeding 20 percent), and relatively lower rates among older teens.

Of course, people marry out of their state of residence for many reasons. How many of these young "marriage migrants" might have been seeking to avoid age of marriage laws? As Table 5 indicates, 66.8 percent of young men and 73.7 percent of young women who marry out of state did so in an adjacent state. Among these men, 25.8 percent of them were too young to marry without consent in their own state, but could marry legally in the adjacent state where their marriages actually took place. Among women, this rate is 19.4 percent. Since these persons were all too young to marry in their own state but could legally marry in an adjacent state, it can reasonably be argued that they were all migrating to avoid their home state's minimum marriage age. These marriages constitute only 2.4 percent of all marriages among men under age 21 and only 1.5 percent of all marriages among women under age 21. While this is only an approximate

estimate of marriage-related migration (some movers could have gone to non-adjacent states; some going to adjacent states may not have been consciously avoiding the laws, etc.), it suggests that a relatively small share of those under age 21 are likely to be migrating as a way to avoid age of marriage laws.

To further explore the importance of marriage migration, we compare migration in the period before and after age of non-consent laws converge across states. Figure 1 looks at these patterns. The solid dark line in Figure 6 shows the percentage of younger male migrants who move from more restrictive to less restrictive states, as classified by 1968 laws. The denominator is the number of men under age 21 who live in a state where the 1968 age of consent for marriage is 21 but who marry out of state; this is the number of ‘marriage migrants’ who are too young to marry in historically restrictive states. The numerator is the number of these men who marry in a state where the 1968 age of consent law would have allowed them to marry legally. The ratio represents the share of younger marriage migrants who could plausibly be avoiding the law, *if the 1968 laws were still in effect*. We show this percentage for all years from 1968 to 1979, using the 1968 state laws to define restrictive and less restrictive states. If marriage migration is important, there should be more movement in the late 1960s between these states (when the restrictions were actually in place) than in the late 1970s (when almost all states had adopted age 18 as the legal age for marriage without parental consent). The dashed line shows the same data for women under age 21. Both of these lines decline during the period when marriage consent laws converge.⁵

As one final check on the extent of marriage migration prior to convergence in age-of-marriage laws, we estimate difference-in-difference regressions, which are reported in Table 6. Our sample consists of all marriages among men (women) under age 25 in the periods 1968-71 and 1976-79. The dependent variable is a binary variable which denotes whether the man (woman) migrates to a state where the male (female) non-consent law is less than age 21 in 1968. We difference between the early and late period, and between men younger than age 21 and those ages 21-25. This implicitly compares changes over time (before and after the laws bind) in migration rates to states with historically lower non-consent laws among men who are of an age to be affected by these laws versus changes over time in migration rates among men who are too

⁵ If we redo Figure 6 using age of marriage with parental consent (rather than age of nonconsent), we find a decline among men but no decline among women in the propensity to migrate to a state with a lower age of nonconsent. The age of parental consent laws change less over this period and fewer marriages are affected by them.

old to be affected.

We find that there was a statistically significant 1.8 percent higher incidence of marriages among younger men in less restrictive states in the early period than in the late period. This is quite consistent with our estimate of marriage migration in Table 5, suggesting a relatively small (but significant) marriage migration effect before the laws converge. Similar estimates among women find slightly larger effects. We estimate a statistically significant 2.9 percent greater rate of marriage among younger women in less restrictive states in the earlier period than in the later period.

In short, we find clear evidence of migration to states with less restrictive age of marriage laws among those who marry before age 21 in the period when there are significant cross-state differences in these laws. “Marriage migration” appears to be regularly used as a way to avoid state age of marriage laws. The magnitude of this effect is relatively small, however, and seems to have affected only somewhere between 1 and 3 percent of all younger marriages. Unfortunately, we can say nothing about the trend over time in legal avoidance through marriage migration before the late 1960s, but we strongly suspect the ability of teens to go out of state to avoid marriage laws would have been no greater and probably smaller in earlier years. If we take our estimate of marriage migration from the 1970s as a maximal estimate of this phenomenon in 1950, it will explain less than half of the discrepancy between Census and Vital Statistics data in 1950, suggesting that *both* migration and misrepresentation were occurring in this year.