

## A Cautionary Tale About the Use of Administrative Data: Evidence from Age of Marriage Laws<sup>†</sup>

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*This paper demonstrates that administrative data may be inferior to survey data under particular circumstances. We examine the effect of state laws governing the minimum age of marriage in the United States. The estimated effects of these laws are much smaller when based on retrospective reports from census versus administrative records from Vital Statistics data. This discrepancy appears due to systematic avoidance behavior of two kinds. Some young people marry in states with less restrictive laws; others appear to have misrepresented their age on their marriage certificate. Our results have important implications regarding legal avoidance and the use of administrative data. (JEL J12 K36)*

This paper presents a cautionary note about the use of administrative data in empirical work, especially work devoted to assessing the behavioral effects of laws. The behavioral changes induced by laws often represent important tests of theory and, because they are plausibly exogenous, may be exploited to identify various causal effects. Increasingly, scholars studying the effects of laws, and other empirical questions, rely on administrative data. These data are drawn from official records and are thus thought to provide more reliable information about the behaviors they record than do general surveys. We argue that this presumption may often be incorrect, with important implications for the validity of empirical analysis.

When a particular behavior has been outlawed, there will be people who modify their behavior as intended by the law and others who attempt to circumvent the law in various ways. In particular, “noncompliers” can either (a) move to an area where the behavior is not outlawed and engage in it there or (b) break the law where they are.<sup>1</sup> Both these actions bias administrative records toward suggesting larger

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<sup>†</sup> To comment on this article in the online discussion forum visit the articles page at: <http://www.aeaweb.org/articles.php?doi=10.1257/app.1.2.128>.

<sup>1</sup> That some agents might migrate to avoid a law to which they would otherwise be subject has been studied in a variety of contexts including migration for tax-related reasons (Michael F. Lovenheim 2008 and Marcus Asplund, Richard Friberg, and Fredrik Wilander 2007); migration to obtain legal abortions (Rebecca M. Blank, Christine C. George, and Rebecca M. London 1996); or migration to avoid age-of-drinking laws (Donald S. Kenkel 1993).

changes in the behavior addressed by the law than actually occurred. In the first case, official records correctly indicate that recorded behavior in an area conforms to the area's laws, but they miss the fact that some of these people would not have been in the area if the laws had been different. In the second case, administrative records might not accurately reflect behavioral changes within a given area. People who break the law have an incentive to conceal that fact, and that incentive is likely especially large when the threat of a sanction is large (as is true in virtually all cases where administrative data are collected).

These potential problems are rarely discussed in studies using administrative data. Moreover, because researchers are predisposed to believe that laws affect behavior in the intended direction, for the intended population, they are unlikely to be vigilant against the possibility that information from an administrative data source suggests larger effects of a law than is accurate. In practice, the consequences of this bias in administrative data may be severe and lead scholars to incorrect conclusions about a law's effects. Interestingly, while they have other limitations, data from general surveys may not be subject to the particular problems of administrative data. Research that combines data from different sources, or uses data from one source to assess the validity of results from another, is thus much more likely to identify parameters of interest than research using one type of data.

To assess the practical importance of these issues, we study how minimum age of marriage laws, which were designed to reduce early marriage, affected young people's marriage behavior. Before the 1970s, adolescents wishing to marry faced minimum age of marriage restrictions which varied widely across states. These laws converged dramatically over a short period in the early 1970s, so that by 1975 the minimum permissible age of marriage was identical across almost all states. These legal changes present an ideal opportunity to study how marriage responds to policy initiatives and could be exploited in instrumental variables or other approaches to provide estimates of how early marriage affects various outcomes.

The "gold-standard" data source for studying the effect of marriage laws would seem to be the official marriage statistics from the National Vital Statistics System of the Center for Disease Control. These data are drawn from a state's actual marriage certificates and represent the official administrative record of marriage collected at the point of marriage. Other surveys, like the decennial census, inquire about marriage history but rely on a person's retrospective report about behavior from years before. Surprisingly, we find that for marriages in the 1950s, the Vital Statistics evidence suggests dramatically larger effects of minimum age of marriage laws than are found in census data.

We examine three possibilities for differences in the results from the two data sources. First, we show that the discrepancy is not the result of mechanical differences between the datasets or recall bias in census records. Second, because the census reports marriages by where people live while the National Vital Statistics System records marriages by where they occur, the different estimates from the two data sources might actually describe the same underlying behavior, if people traveled to more permissive states to marry. We show that while there was some marriage-related "migration" among young people in the relevant cohorts, migration cannot explain the overall discrepancy between the data sources. The third possible explanation is that

young people in the 1950s actually married at younger ages than was legally permitted in their states but misrepresented their ages on official marriage records. Consistent with this interpretation, we show that most of the differences between retrospective census and Vital Statistics records disappear by 1970, before the convergence in legal marriage ages across states but after documentary evidence of proof of age, such as driver's licenses and social security cards, became more common.

Our results suggest that minimum age of marriage laws did lower the incidence of young marriage as intended. However, because in the 1950s many young people seem to have circumvented the laws by moving to other states or by systematically misrepresenting their ages on official records, Vital Statistics records suggest much larger changes in marriage than actually occurred. More generally, our results illustrate how data quality might be affected by the incentives that agents face to give misinformation, and how those incentives may be particularly high for administrative records. Standard regression or instrumental variables estimates of behavioral responses, which rely exclusively on administrative data, may thus be very much at variance with changes that actually occurred in the population.

Below, we review the history of minimum age of marriage laws and describe available information about marriage in the Vital Statistics and census data. Section II presents results from these two data sources, showing that the estimated effects of age of marriage laws differ sharply across them. In Section III, we analyze, in turn, the importance of mechanical differences and recall bias in census data and marriage migration and systematic misrepresentation in the Vital Statistics data for explaining the discrepancy across the two data sources. Section IV concludes.<sup>2</sup>

### I. History of Minimum Age Laws and Data on Early Marriage

In 1950, laws governing the minimum age of marriage varied widely across states and across gender.<sup>3</sup> Over the short period from 1971 to 1975, a burst of legislative activity led to a dramatic convergence in laws across states and gender so that age 18 became the legal age of marriage without parental consent.<sup>4</sup> In 1980, an 18-year-old man or woman wishing to marry could do so without parental consent in all but three states in the country. Merely ten years earlier, in 1970, that young man would not have been free to marry without parental consent in more than 40 states. Minimum marriage ages were historically lower for women in 1970, but, even so, about 10 states prohibited 18-year-old marriage without parental consent for young women.

We study the effects of minimum age of marriage laws in the post-World War II period on marriage behavior.<sup>5</sup> Throughout, we focus on legal age of marriage without

<sup>2</sup> A longer version of this paper is available as a National Bureau of Economic Research (NBER) working paper (Blank, Charles, and Sallee 2007). Many of the details in that paper, not presented here, are in the Web Appendix available at <http://www.aeaweb.org/articles.php?doi=10.1257/app.1.2.128>.

<sup>3</sup> We gathered information on legislative statutes about age of marriage laws in each state by tracing back in time, through successive amendments, from when most of these laws were initially introduced in the early 1800s. For some sense of the variation in laws across states as late as 1950 and sharp convergence in early 1970s, see Appendix Table A1.

<sup>4</sup> These changes were likely occasioned by the constitutional amendment in 1971 making the voting age in national elections 18 years old and by demands from the women's rights' movement for legal parity for women.

<sup>5</sup> Gordon B. Dahl (2005) exploits minimum age of marriage laws as an instrument for adult poverty using census data. His work differs from ours in several ways. First, Dahl focuses exclusively on women and, because

parental consent, which we call the “nonconsent” age. Generally, the minimum age in these laws is above 16 and thus is likely to “bite,” in the sense that nontrivial amounts of people may actually wish to marry at or below the consent age. A legal age of marriage with parental consent as young as 12 years old or 14 years old would likely affect fewer young people. Furthermore, changes in marriage induced by nonconsent laws have nothing to do with the preferences of a third party such as a parent. How these laws affected the marital behavior of young persons across various cohorts can be investigated with data from two different sources: the National Vital Statistics or the decennial census.<sup>6</sup> We discuss these in turn.

Vital Statistics marriage data come from information submitted voluntarily by states in the period 1940–1995. The data are from the marriage certificates filled out by couples just prior to marriage as a requirement for having the marriage legally registered. During the years it collected marriage data, Vital Statistics saw a large increase in the number of states providing information, from less than 20 in 1950 to virtually all states in 1980. Vital Statistics marriage information is not available for all states in all years and is, for most years, not available in age-disaggregated form.<sup>7</sup> Age-specific information is available for the 18 reporting states in 1950 (and for several years in the 1940s) but is not available again until 1968. For the remainder of the 1950s and 1960s, Vital Statistics only reports information about the number of marriages for aggregate categories, such as marriages among persons aged 14–19 or 20–24. Because the data are collected at the time of marriage and are official administrative data, the Vital Statistics data are typically considered the authoritative source of information on marriage in the United States.

The decennial censuses represent an alternative source of data about marriage outcomes for the populations of young people affected by marriage laws. In the 1960, 1970, and 1980 censuses, respondents were asked about the date of their first marriage and their date of birth. Individuals’ ages at marriage can be readily calculated from these two pieces of retrospective information. The census contains very large samples and provides age-disaggregated information about marriage. But retrospective census information about marriage has the drawback that reports may be subject to recall error. Further, location information in the census is limited to a person’s state of residence and state of birth. Thus, where a marriage occurred is not perfectly known. These differences notwithstanding, if higher nonconsent ages lowered the likelihood of early marriage, this effect should be evident and of similar magnitude in both surveys. We examine these marriage patterns in the next section.

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he focuses on marriages with parental consent (unlike our focus), he analyzes marriages at much younger ages than we do. Dahl mentions and briefly analyzes the potential for cross-state marriages, but he suggests that this is evidence that state laws are binding. By contrast, we think the primary lesson to be learned from marriage migration is that it can create fundamental differences between administrative and survey data. Dahl does not mention the potential for misrepresentation, which is central to our findings. Jeanne Lafortune (2008) also uses these laws as a first-stage instrument to investigate human capital accumulation.

<sup>6</sup> Another data source is the June supplement to the Current Population Survey (CPS), which includes questions about marriage in some years. Relative to the census, the June CPS samples are smaller and primarily cover more recent years, which are of less interest to us.

<sup>7</sup> Data on marriages before 1968 are available in published annual reports and online at <http://www.cdc.gov/nchs/products/pubs/pubd/vsus/vsus.htm>. Microdata files for 1968 to 1995 are maintained by the NBER at <http://www.nber.org/data/marrdivo.html>.

## II. Effect of Marriage Laws on Early Marriage from Vital Statistics and Census Data

We begin with a regression analysis of marriages from 1951–1979, comparing the results from the two datasets. Vital Statistics information for these years is not age-disaggregated, so we focus on marriage for the 14- to 19-year-old age group. We convert the marriage counts reported by Vital Statistics to marriage rates by estimating the number of 14- to 19-year-olds in each gender/state/year cell using the decennial censuses and either the official intercensal estimates from the Census Bureau or linear interpolation when these are not available. Our census data come from retrospective reports from the 5 percent Integrated Public Use Microdata Series (IPUMS) sample of the 1980 census. To make the results from the two surveys strictly comparable, we collapse age-disaggregated census data in the relevant cohorts into the same 14 to 19 age bin observed in the Vital Statistics, and limit the census data to the set of states (using state of birth) and years for which there is Vital Statistics information.<sup>8</sup> Finally, census marriage rates for 14- to 19-year-olds are the rate in the IPUMS sample.

On these two separate samples, we estimate event-study regressions of the form

$$(1) \quad Y_{gst} = \beta_0 + \sum_l \delta_1^l I_{gs}^l(t - k_l^* \leq -4) + \sum_l \delta_2^l I_{gs}^l(-3 \leq t - k_l^* \leq -1) \\ + \sum_l \delta_3^l I_{gs}^l(1 \leq t - k_l^* \leq 3) + \sum_l \delta_4^l I_{gs}^l(t - k_l^* \geq 4) + \Gamma_t + \Gamma_s + \varepsilon_{gst}.$$

The marriage rate in the 14–19 age group in year  $t$  and state  $s$  for persons of gender  $g$  is  $Y_{gst}$ ;  $\Gamma_t$  and  $\Gamma_s$ , respectively, are vectors of year and state fixed effects, and  $\varepsilon_{gst}$  is a random error term. The variable  $k_l^*$  denotes the year in which a particular “nonconsent” age law  $l$  was changed (generally lowered) for persons of gender  $g$  in state  $s$ .<sup>9</sup> The indicator variables  $I_{gs}^l$  in (1), respectively, denote that the year  $t$  is four or more years before  $k_l^*$ ; between one and three years before  $k_l^*$ ; between one and three years after  $k_l^*$ ; or four or more years after  $k_l^*$ . The excluded time period in the regressions is the year in which the law changed, so the estimated coefficients  $\delta$  represent how, after accounting for factors peculiar to a state and for general time effects, average marriage rates for 14- to 19-year-olds in a given period compare to marriage rates in the year that a law changed.

Table 1 reports the results for regression (1) for women and men separately across the two surveys. The regressions include treatment variables for all of the different nonconsent laws observed in our data, but the upper panel of the table only reports coefficient estimates associated with lowering nonconsent ages from 21 years old to 18 years old for men and women, which is overwhelmingly the most common legal

<sup>8</sup> Results from the census sample using all states are similar. We have also estimated related models in which we use all states and a fully age-disaggregated sample from the census. These results are qualitatively the same as the census results shown here and can be seen in the Web Appendix.

<sup>9</sup> The regressions include dummies for each type of change indicated in the Appendix Table A1.

TABLE 1—COMPARISON OF ESTIMATED EFFECTS OF A CHANGE IN NONCONSENT AGE FROM 21 TO 18 ON MARRIAGE RATES ACROSS DATA SOURCES

Dependent variable	Vital Statistics		Census	
	Women	Men	Women	Men
Marriage rate of 14- to 19-year-olds				
Dummy for 3 years before change	-0.0040 (0.0028)	-0.0007 (0.0010)	-0.0005 (0.0017)	-0.0008 (0.0007)
Dummy for 3 years after change	0.0065 (0.0030)	0.0010 (0.0010)	-0.000003 (0.0017)	0.0002 (0.0007)
Dummy for 4 or more years before change	-0.0075 (0.0027)	0.0012 (0.0012)	-0.0004 (0.0016)	-0.0008 (0.0007)
Dummy for 4 or more years after change	0.0064 (0.0027)	0.0013 (0.0012)	0.0002 (0.0018)	0.0007 (0.0007)
State fixed effects	X	X	X	X
Year fixed effects	X	X	X	X
Years in sample	1951–1979	1951–1979	1951–1979	1951–1979
Observations	958	958	958	958
Difference (3 years after – 3 years before)	0.0105	0.0016	0.0004	0.0009
<i>p</i> -value	[0.000]	[0.071]	[0.590]	[0.082]
Percent effect of legal change on marriage rate of 14- to 19-year-olds	16.5%	6.8%	0.8%	4.2%

*Notes:* Standard errors in parentheses. We assign states based on state of birth in the census and state of marriage in Vital Statistics. Vital Statistics estimates are based only on first marriages (unlike disaggregated Vital Statistics data studied below). The regression also includes a dummy for four or more years before and after the legal change, and corresponding dummies for other nonconsent legal changes. An identical set of states and years are used for both sexes and datasets. Census estimates exclude a small percentage of observations that have imputed age at first marriage or birth place. Robustness checks indicate that this exclusion is not important for our results.

change in the data. In the lower panel of the table, we present the estimated change in the rate of marriage rate among 14- to 19-year-olds between 1 and 3 years before the law to between 1 and 3 years after the law ( $\hat{\delta}_3 - \hat{\delta}_2$ ), and relative to the average rate of marriage among 14- to 19-year-olds in the state. We focus on this estimate because, in our view, only behavioral changes within the six-year period bracketing the change in a law can plausibly be attributed to the law rather than some other factor.

The results from the Vital Statistics indicate that marriage laws significantly affected young marriage rates. For young women, the estimates imply that a change in a state's female nonconsent age from 21 years old to 18 years old was associated with a 16.5 percent increase in the marriage rate of young women aged 14–19. For men, the estimated effect of a 6.8 percent increase in rates of early marriage is not as large, but is not at all trivial. These estimates are precisely what we would expect if minimum age laws strongly affected young people's marriage decisions. The difference between the estimates for men and women is also not surprising. Young men marry later than women so laws lowering the age at which they could legally marry might be expected to have a smaller effect. These patterns differ dramatically from what we find with retrospective census data. For women, census results indicate that marriage laws appear to have had little to no effect on the rate of early marriage. The estimates for men are positive, although they are substantially smaller than Vital Statistics results.<sup>10</sup>

<sup>10</sup> We have tried several alternative specifications to the ones reported, all of which lead to the same conclusion as the results in Table 1.

Recall that in addition to the aggregated information used in Table 1, the Vital Statistics reports age-disaggregated marriage data in 1950 and some years after 1968.<sup>11</sup> We use this information to graphically examine differences in the impact of marriage laws across states in 1950 and 1970. To understand the figures, suppose that the number of women (men) of age  $a$  wishing to marry in state  $s$  at time  $t$  is  $n_{st}^a$ . Let  $\pi_a$  be the share of people who can always get married because of some dispensation such as parental or judicial consent. If  $\hat{a}_s$  is the age at which persons within the state can marry without parental consent, and if marriage laws are binding, then the share of all marriages occurring in state  $s$  in year  $t$  to persons of age  $a$  is

$$(2) \quad f_{st}(a) = \begin{cases} \pi_a n_{st}^a / \left( \sum_{a < \hat{a}_s} \pi_a n_{st}^a + \sum_{a \geq \hat{a}_s} n_{st}^a \right) & \text{if } a < \hat{a}_s, \\ n_{st}^a / \left( \sum_{a < \hat{a}_s} \pi_a n_{st}^a + \sum_{a \geq \hat{a}_s} n_{st}^a \right) & \text{if } a \geq \hat{a}_s. \end{cases}$$

Equation (2) implies that, barring the unrealistic assumption that every young person below the state minimum age can receive some exception or dispensation, the probability density function of ages at marriage should display a discrete jump or “spike” at a state’s legal nonconsent age. These “spikes” should occur at different ages in different states, depending upon the marriage laws in effect in the state.

Using Vital Statistics and retrospective census data, Figure 1A shows the distribution of age at marriage for women in 1950 in the 15 states where the age of marriage without parental consent was 18 years old. The sharp spike in marriage at the age of 18 in the Vital Statistics is entirely absent from the census data. Census data also show higher marriage rates at earlier ages but, from age 19 onward, the two series are quite similar. The six-point discrepancy for marriage at the age of 18 across the two surveys is substantial. Figure 1B shows a similar comparison for the four states in 1950 where the age of marriage without consent was 21 years old for women. Again, there is a much higher incidence of early marriage in 1950 in the census data. Vital Statistics shows a peak at age 21 which is totally absent in the census data. The census and Vital Statistics data also show different results for men’s marriages in 1950. Figure 1C shows less of a discrepancy between data sources in states where the nonconsent age was either 18 years old or 20 years old. Even so, there is a spike in the Vital Statistics at 21 years of age, and census data show a higher prevalence of marriage at younger ages. Figure 1D plots the distribution of age at first marriage in states with a nonconsent age of 21 years old. The census data does not show the spike at age 21 found in the Vital Statistics data. Like the women’s data, the census shows

<sup>11</sup> The age disaggregated data from 1950 includes both first marriages and remarriages, and we (necessarily) compare this to first marriage data from the census. Including remarriages, which are rare at younger ages, has a predictable effect of overestimating marriages at older ages. In the Web Appendix, we perform the best available interpolation of first marriages from the data, and show that, consistent with our expectations, using the remarriage data biases our main estimates against our findings. Thus, we report the remarriage-inclusive data in our main results because we believe it is the conservative choice.

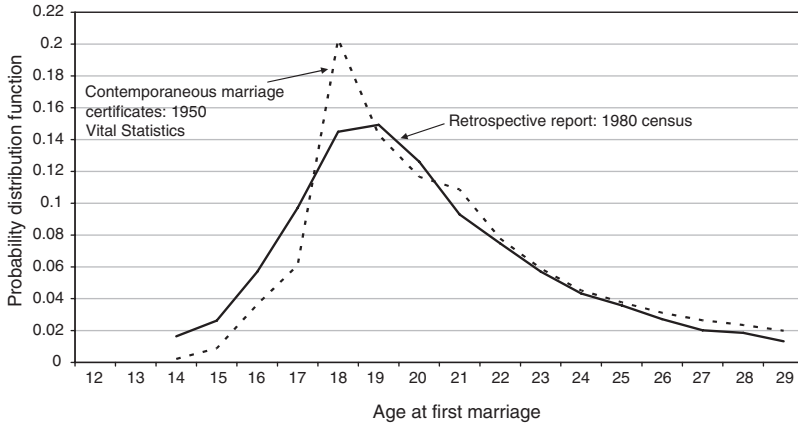


FIGURE 1A. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR WOMEN MARRYING IN 1950 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 18

Note: Based on the following 15 states with female nonconsent age equal to 18: DE, IA, ID, KS, ME, MI, MS, MT, ND, NH, OR, SD, TN, VT, and WY.

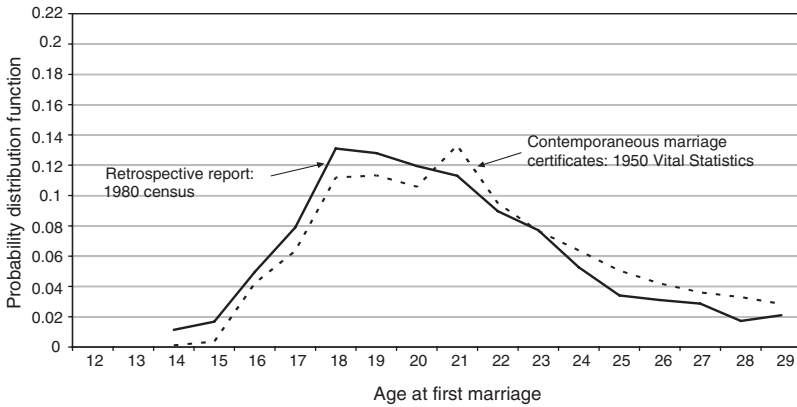


FIGURE 1B. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR WOMEN MARRYING IN 1950 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 21

Note: Based on the following 3 states with female nonconsent age equal to 21: CT, FL, and NE.

a higher incidence of younger marriages among all men who marry. For the ages 22 years old and older, the census and Vital Statistics data are identical.<sup>12</sup>

In Figures 2A–2D, we repeat the exercise above but, this time, for marriages in 1970. Strikingly, the figures show that the differences for men and women found between the two surveys in 1950, do not exist in 1970, or are substantially reduced.

<sup>12</sup> As noted above, we have the age of marriage data from the Vital Statistics System available within states for several years in the 1940s as well as in 1950. We have compared census and Vital Statistics data for these years and see the same pattern of discrepancy as is shown here for 1950.



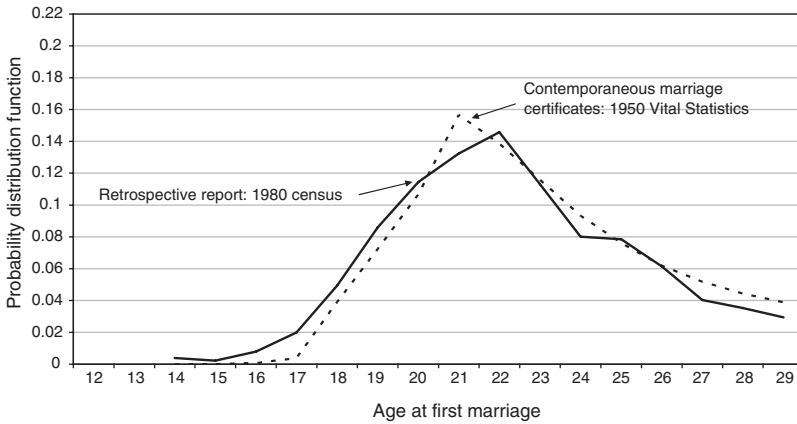


FIGURE 1C. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR MEN MARRYING IN 1950 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 18 or 20

Note: Based on the following 4 states with male nonconsent age equal to 18 (ID, MI, TN) or 20 (NH).

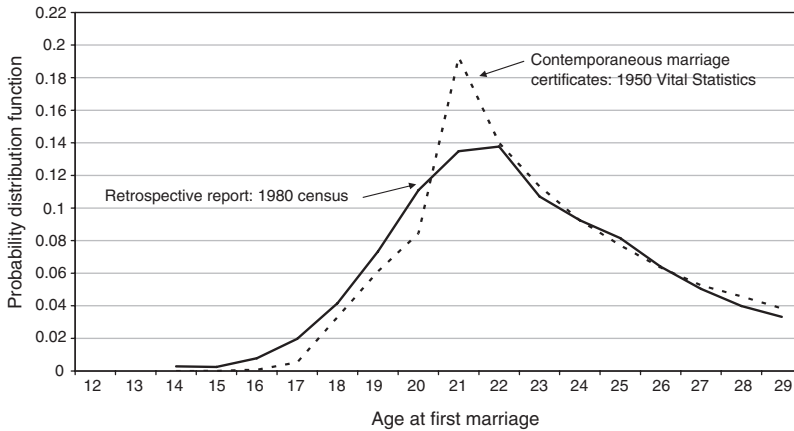


FIGURE 1D. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR MEN MARRYING IN 1950 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 21

Note: Based on the following 14 states with male nonconsent age equal to 21: CT, DE, FL, IA, KS, ME, MS, MT, ND, NE, OR, SD, VT, and WY.

In Appendix Table A2, we conduct a series of formal tests for the differences in the probability distributions shown in Figures 1 and 2. The results strongly confirm what is clear in the graphs, census and Vital Statistics age distributions are statistically different from each other for marriages in 1950, but are statistically the same for marriages in 1970.<sup>13</sup>

<sup>13</sup> Treating these age groups as discrete bins, the data on age of marriage in Figure 1 may be thought of as a set of binomial distributions. We report a set of pairwise tests of the equality of proportions across legal regimes, for different age bins. Since we have large samples, the binomial distribution is well approximated by the normal

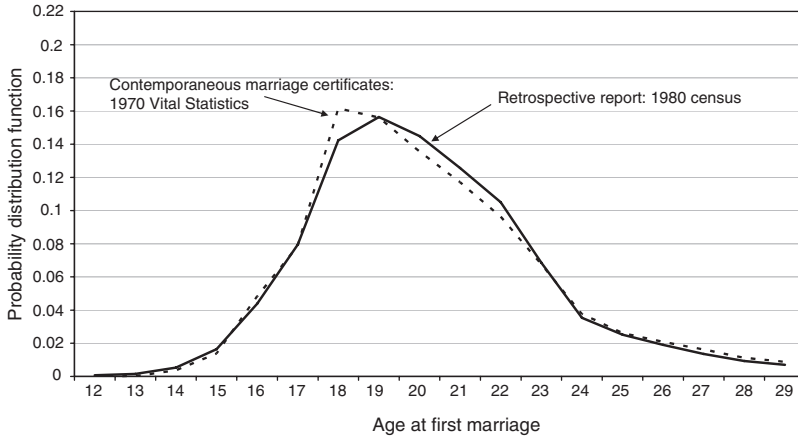


FIGURE 2A. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR WOMEN MARRYING IN 1970 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 18

Note: Based on the following 36 states with female nonconsent age equal to 18: AK, AL, AR, CA, CO, DE, HI, IA, ID, IL, IN, KS, KY, MA, MD, ME, MI, MN, MO, MT, NC, ND, NH, NJ, NV, OH, OR, SC, SD, TN, TX, UT, VT, WA, WI, and WY.

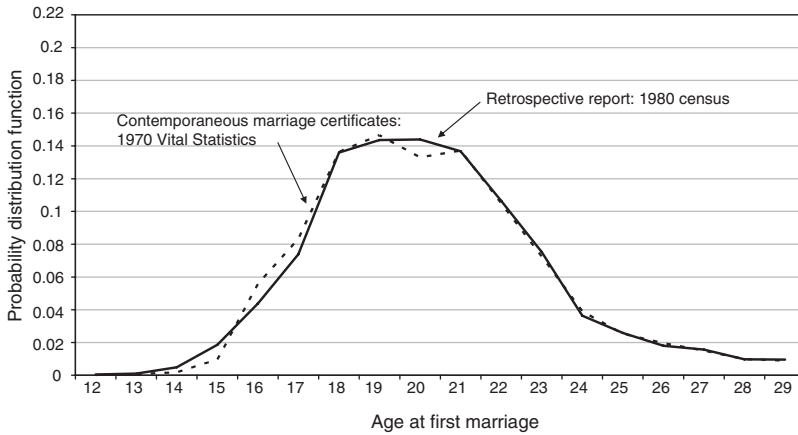


FIGURE 2B. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR WOMEN MARRYING IN 1970 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 19, 20, or 21

Note: Based on the following 8 states with female nonconsent age equal to 19 (GA), 20 (NE), and 21 (CT, FL, PA, RI, VA, WV).

### III. Alternative Explanations for Differences across Data Sources

Before exploring our preferred explanations for the difference across the two surveys, we briefly explore whether mechanical factors, having to do with how the two datasets are constructed, may account for these discrepancies. The most obvious

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distribution, which implies that the differences are also approximately normal. The difference statistics have a z-distribution.



FIGURE 2C. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR MEN MARRYING IN 1970 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 17, 18, 19, or 20

Note: Based on the 14 states with male nonconsent age equal to 17 (MS), 18 (KY, MI, NC, SC, TN, WA), 19 (AK, GA, TX) and 20 (HI, ME, NE, NH).

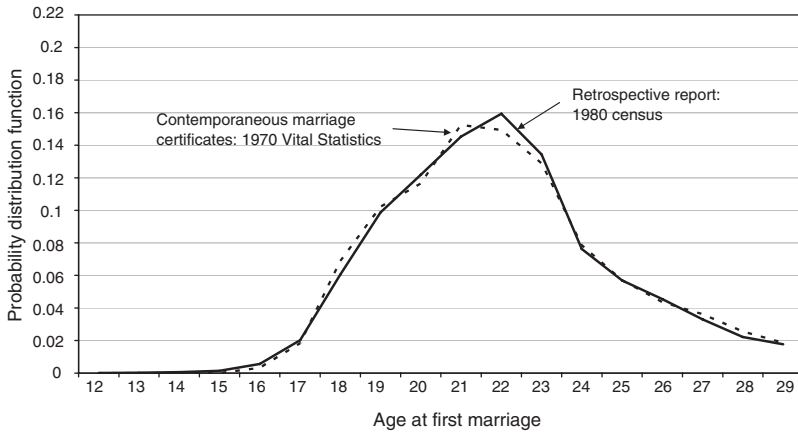


FIGURE 2D. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR MEN MARRYING IN 1970 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 21

Note: Based on the following 31 states with male nonconsent age equal to 21: AL, AR, CA, CO, CT, DE, FL, IA, ID, IL, IN, MA, MD, MN, MO, MT, ND, NJ, NV, KS, OH, OR, PA, RI, SD, UT, VA, VT, WI, WV, and WY.

difference between Vital Statistics and census marriage data is that census information about marriage is retrospective. Might recall error associated with these reports explain the patterns documented above? An argument that recall error in census data explains our findings would have to take one of two forms. First, it could be that, when retrospectively reporting their early marriage, census respondents systematically report having gotten married at younger ages than they actually did. But it is not obvious why the tendency to report an earlier marriage should change discontinuously at particular ages as would have to be true for the census and Vital Statistics

data to line up at older ages, and as we find. Moreover, a variety of considerations suggests that if people systematically misreport their age at marriage in a particular direction, they likely underreport very young marriages in the census.<sup>14</sup>

A second and more plausible recall error concern is that people make random errors when retrospectively reporting their age at marriage. These errors would tend to flatten the distribution of marriage ages in the census reports relative to Vital Statistics. Since we use the 1980 census for both 1950 and 1970 marriage, the recall problems, with the attendant flattening, would be worse for those who married in 1950 than for those who married in 1970, possibly leading to the patterns we estimate.

We address this concern in a variety of robustness exercises, the details of which can be seen in the Web Appendix. Summarizing the results, when we estimate the 1950 age of marriage distribution using the 1960 and 1970 censuses (instead of 1980), the patterns are graphically very similar to, and statistically no different than, those shown in Figure 1. Indeed, if anything, the point estimates from these alternative exercises suggest that using the 1980 census may actually lead to a slight underestimate of the difference in early marriages in the census and Vital Statistics data (probably because people are less likely to report early, unsuccessful marriages as time passes). Finally, we think the fact that all our analyses reveal that the largest differences between census and Vital Statistics data occur exactly at states' nonconsent ages is persuasive evidence that recall bias does not explain the differences across the surveys.

A second mechanical concern is that the census results are faulty because we have to assume that state of birth is the same as state of marriage.<sup>15</sup> If people moved randomly prior to marriage, the errors we make in assigning people's state of marriage would be random, which would tend to smooth the census data relative to Vital Statistics data. However, this should have been a larger problem in 1970 than in 1950 because of higher income levels and more mobility in 1970. Instead, we find that the discrepancies between the two data series vanish rather than increase between 1950 and 1970. Note that we distinguish this point from the systematic migration to which we devote much attention below.

Two final points about the possible importance of mechanical differences between the surveys warrant some discussion. In the census our estimates of people's age at marriage are based on their reports about the month and year they married, rather than the exact day. We assess the sensitivity of our results to any resulting imprecision in the estimated age of marriage and find that our results are robust to these tests.<sup>16</sup> The other issue is the possibility of attrition bias in the census because of

<sup>14</sup> These factors include the fact that people: may choose not to report a short-duration marriage from their early life, as the 1980 census directs them to do for annulled marriages; and may erroneously describe the date of their current marriage despite clear instructions to discuss their first marriage.

<sup>15</sup> We only include people in our census sample who report being born in the United States. This excludes US marriages among those who were born outside the United States but immigrated prior to marriage. These immigrant marriages are included in the Vital Statistics data, however. We ignore this discrepancy, given the years we are focusing on are years when immigration into the United States was relatively low.

<sup>16</sup> Specifically, we use electronic Vital Statistics records for the 1970s and estimate age of marriage in precisely the same way as the census (thereby causing some imprecision in the estimated age). We compare the distribution of actual age at marriage (which is known precisely in the Vital Statistics) with the less precise census-like calculation, and estimate virtually the same age of marriage distribution.

death or emigration from the United States since their marriage. Census data contain no marriage information for these missing persons. Notice that the longer the retrospective period of recall, the greater the attrition this will produce in the census marriage reports. Since we are looking at teenage marriages reported no more than 30 years later, we assume this is not a major problem in our data.

### A. *Systematic Marriage Migration*

If differences in survey construction do not explain the observed differences in age of marriage across surveys, what does? We speculate that some young people systematically evaded the laws, possibly by traveling to more permissive states to marry. How important a factor is this migration in explaining the patterns shown above? In 1950, where we find the largest differences across the two data sources, available Vital Statistics data does not permit a comparison of out-of-state marriage rates across different age categories. In addition, the set of reporting states is very limited in the early years of the Vital Statistics data. These shortcomings notwithstanding, we can nonetheless provide an intuitively appealing assessment of migration's influence.

To see the logic of our approach, consider a state which was a likely destination for marriage migration in 1950, and suppose that all cross-border migration is motivated by legal avoidance. Census records for the state would show the age of marriage distribution of state residents, while Vitals Statistics records would show the age of marriage distribution for residents and in-migrants. But since all in-migrants would be at least as old as the nonconsent age in the state, the state's Vital Statistics marriage records should show that a discretely larger fraction of marriages in the state occur at precisely the state's nonconsent age than do census data, which measure marriages only for the state's residents. Consider, next, a state which is a likely source of marriage migration. In this case, since some people younger than the state's nonconsent age leave the state to marry, and since state residents who are at least as old as the state's nonconsent age have no legal reason to leave the state, the distribution of age of marriage in the Vital Statistics data should, again, show a discrete jump at the state's nonconsent age relative to that in the census. Finally, in states in which there is neither in- nor out-migration, the distribution of age of marriage should be the same across census and Vital Statistics data.

One indicator of whether a state in 1950 was a likely in-migration state, an out-migration state, or neither, is how its laws compare to those of neighboring states. In the available Vital Statistics data, there are seven states with female nonconsent ages of 18 that have at least one neighboring state with a higher nonconsent age. These are likely in-migration states. There are eight states with female nonconsent ages of 18 and for which neighbors all also have nonconsent ages of 18. These states, to a first approximation, are neither in- nor out-migration states. For men, we can identify six states with male nonconsent ages of 21 that have at least one neighboring state with a lower nonconsent age. These are likely out-migration states for men. And, we can identify eight states with nonconsent ages of 21 that are all surrounded by states for which the nonconsent age is also 21. These states are likely neither in- nor out-migration states.

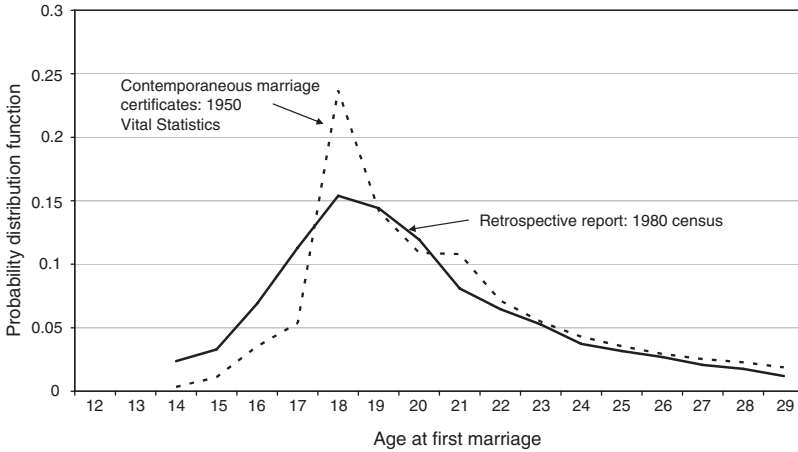


FIGURE 3A. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR WOMEN MARRYING IN 1950 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 18 AND NEIGHBORING STATES HAVE MORE RESTRICTIVE LAWS

Note: Based on the following 7 states with female nonconsent age equal to 18 and with at least one adjacent state with a higher nonconsent age: DE, IA, KS, MS, SD, TN, and WY.

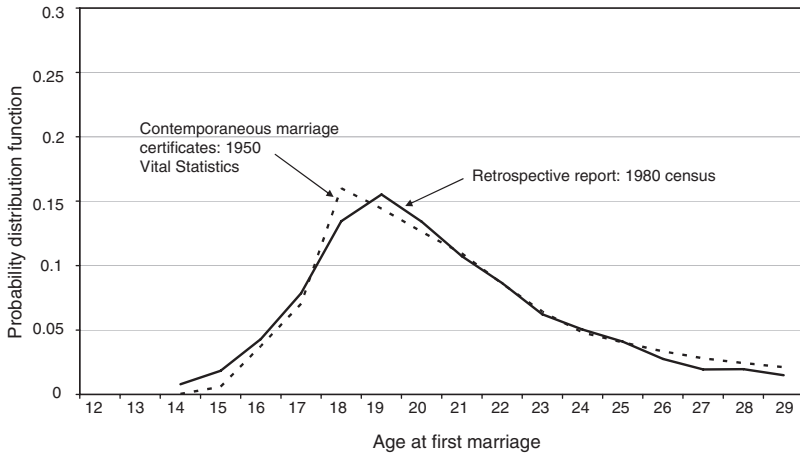


FIGURE 3B. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR WOMEN MARRYING IN 1950 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 18 AND NEIGHBORING STATES HAVE SAME LAWS

Note: Based on the following 8 states with female nonconsent age equal to 18, and for which all adjacent states also have a nonconsent age equal to 18: ID, ME, MI, MT, ND, NH, OR, and VT.

Figure 3 shows the distribution of age of marriage in these different states as estimated in the census and Vital Statistics data. The results are quite striking. Figures 3A and 3B show that women’s age of marriage in in-migration states differs sharply across census and Vital Statistics data in precisely the direction hypothesized, while there is little difference in the age of marriage distribution for nonmigration states.

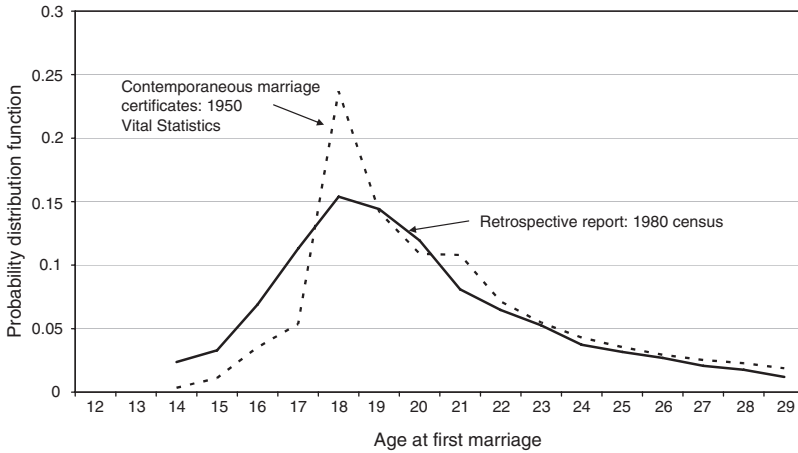


FIGURE 3C. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR MEN MARRYING IN 1950 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 21 AND NEIGHBORING STATES HAVE LESS RESTRICTIVE LAWS

Note: Based on the following 6 states with male nonconsent age equal to 21 years old and with at least one adjacent state with a higher nonconsent age: ME, MS, MT, OR, VT, and WY.

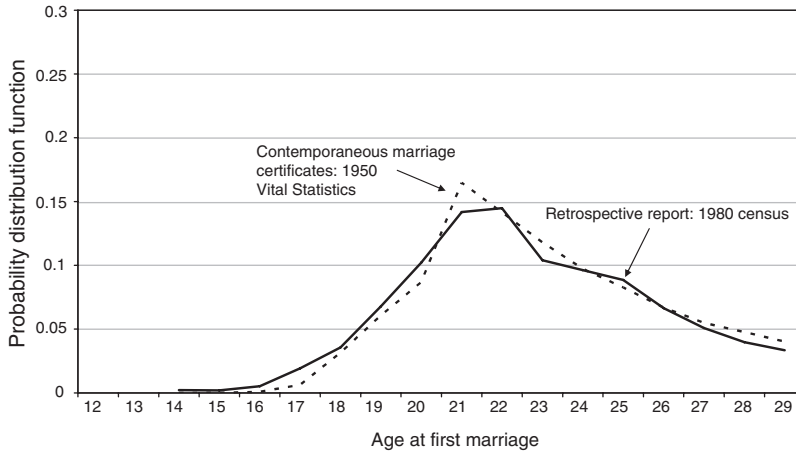


FIGURE 3D. DISTRIBUTIONS OF AGE AT MARRIAGE FROM CONTEMPORANEOUS VERSUS RETROSPECTIVE REPORTS FOR MEN MARRYING IN 1950 IN STATES WHERE AGE WITHOUT PARENTAL CONSENT = 18 AND NEIGHBORING STATES HAVE SAME LAWS

Note: Based on the following 8 states with male nonconsent age equal to 21, and for which all adjacent states also have a nonconsent age equal to 21: CT, DE, FL, IA, KS, ND, NE, and SD.

Similarly, the two data sources reveal the sharp differences outlined above for men in out-migration states. These differences are totally absent in the nonmigration states.<sup>17</sup>

<sup>17</sup> See Appendix Table A3 for formal tests of these differences.

We think the foregoing evidence is strongly suggestive that there was some systematic marriage migration in 1950.<sup>18</sup> However, there are reasons to believe that migration is not the primary explanation for the differences in the data. In particular, observe that in Figure 1, the census records, across all types of states and for both sexes, show significantly more marriages at young ages than the Vital Statistics, indicating an aggregate difference that cannot be explained by mobility. Additionally, we use disaggregated Vital Statistics records from the period 1968–1971 (which predate the convergence of age of marriage laws in the mid-1970s) to measure the extent of migration. In this time period, cross-state variation in nonconsent ages meant that the incentive to migrate should have been as high as in 1950, whereas higher incomes and greater mobility should have made it easier to migrate. We find that only a small share of marriages to young men and women occurred outside of their state of residence in this later period—a probable upper bound on marriage-related migration in 1950.<sup>19</sup> In summary, some factor apart from migration must explain the 1950 disparity and 1970 convergence in census and Vital Statistics data.

### *B. Systematic Age Misrepresentation and Difference across Data Sources*

One important possible explanation for the difference across the surveys is deliberate age misrepresentation. Specifically, we suspect that some young people, intent on not delaying their marriages to satisfy their state's minimum age rules, simply lied about their ages when filling out marriage certificates. Thirty years later, facing no possible sanction for reporting the truth to the Census Bureau, they honestly reported their actual age at marriage. The plausibility of an important role for systematic misrepresentation of age depends on how easy it was to misrepresent age on a marriage certificate for this generation of young people. Clearly, if a state required that an individual show a birth certificate, driver's license, or some other document, it would have been harder to evade the law. Misrepresentation should have been easiest when age was self-declared, with no external verification.<sup>20</sup> Common experience today suggests that lying without forged documents is difficult because proof of age is routinely required for many things. But was this true in 1950? In earlier decades, a much larger share of the population did not hold a driver's license, either because they did not drive (especially younger women) or because states did not require people to carry licenses. The use of social security numbers for identification (other than employment) was relatively uncommon and many younger people did not have a social security number. Some in the population (especially black Americans in rural areas) did not even have birth certificates.

We do not have detailed information for 1950, but in 1929 the Russell Sage Foundation commissioned a document detailing marriage regulations in all states

<sup>18</sup> The only two references to possible marriage migration that we could find in the literature are an early paper by Ira Rosenwaike (1967) and Dahl's (2005) paper. Dahl indicates that in the 1968–1969 data there is evidence of marriage migration, showing that women who marry out of state are more likely to marry in less-restrictive states. Unfortunately, the data to do this type of analysis in earlier years are not available.

<sup>19</sup> See the Web Appendix for details of these calculations.

<sup>20</sup> While it may always be possible to forge such a document, we doubt that many teenagers have the knowledge or capacity to do forgeries.



in the late 1920s (Geoffrey May 1929). The first column of Appendix Table A4 summarizes the information in this document. In that year, 15 states indicated that information on the marriage certificate had to be certified by the oath of the parties involved, while another 13 states accepted an affidavit (essentially, a signature). Most of the remaining states did not specify that any testimony of age be offered or indicated merely that such testimony could be requested at the clerk's discretion. In short, age was self-reported and certified by the signature or oath of the potential marriage partners.

We collected information on current requirements in all 50 states and summarize this information in the second column of Appendix Table A4. By the mid-2000s, virtually all states required persons applying for licenses to provide some type of identification, usually in the form of a social security number or birth certificate. Only a few states still have statutes that require only affidavits, and even these states appear to enforce standard practices that require marriage license applicants to show identification with proof of age.<sup>21</sup> We attempted to trace the statutory history of policies requiring documentary verification of age for a marriage license, but the complexity and thinness of the documentary record made this a prohibitively time-intensive activity to conduct across all states. We therefore selected a set of 14 geographically and demographically diverse states and tried to determine when these states started to require documentary proof of age to grant marriage licenses. In a few cases, we could verify that such requirements were in place before or after a specified date, but we could not find the year they were initially implemented. Column 3 of Appendix Table A4 provides this information for these 14 states. In all cases except Massachusetts, these "identifying documents" requirements appeared to have gone into effect sometime after 1960.

In short, our (admittedly fragmentary) evidence suggests that few states in 1950 appeared to require individuals to do more than swear to their stated age in order to receive a marriage license. By 1970, a growing number of states required that documentary proof of age be presented for a license. Lying about one's age to a county clerk almost surely became more difficult over the time period we study. This evidence is indirect, but when the incentives of young people wishing to avoid state laws are taken into account it suggests that much of the difference we observe between Vital Statistics and census data is the product of young people in earlier cohorts having misrepresented their ages on official documents.

#### IV. Discussion and Conclusions

The results in this paper show the massively different conclusions a researcher might draw about the actual effect of marriage laws on marriage delay, depending on the data source used. In particular, we suspect that most researchers would have expected Vital Statistics data to more accurately reflect the effect of marriage laws

<sup>21</sup> Our research assistant called at least one county office in each state requiring only an affidavit and asked what he would need to bring with him to apply for a marriage license. In every case, he was told to bring a driver's license or birth certificate. Being somewhat persistent, he responded saying "You know that state law doesn't require this." The response was inevitably an out-of-patience clerk who replied, "You want a marriage license, you bring your driver's license!"

on delayed marriage, but this interpretation ignores the much greater susceptibility of administrative data to problems from systematic avoidance behavior.<sup>22</sup> Obviously, our results suggest important cautions for future work on questions about early marriage, especially if that work uses Vital Statistics data or exploits cross-state variation in marriage rules.<sup>23</sup>

Our results suggest a broader lesson for all empirical scholars and especially those wishing to use information about laws as instrumental variables in their analyses. What we have found in the case of marriage laws is likely true for other behavior and laws; a law changes behavior among both compliers and noncompliers. Noncompliers—that is, persons whose actual behavior is not changed by the law—have an incentive to report information to administrative bodies in such a way as to suggest that their behavior has changed. The researcher who naïvely assumes that the direct effects of a law can be readily estimated in administrative data, with little attention paid to the agents' efforts to evade the law's effects, may obtain deeply misleading estimates of the law's actual effect on the targeted behavioral change in affected populations. Moreover, when avoidance itself is of interest, researchers can study it by comparing the administrative data to survey data.

Examples of the use of administrative data abound in empirical economics, and several previous authors have hinted at some of the concerns raised in this paper.<sup>24</sup> When, in general, is the quality of such data and the effects estimated using them likely to accurately reflect true changes in underlying behavior and when is it not? Of course, the answer will vary across contexts, but at a minimum it seems reasonable to argue that administrative data is surely better when it is directly and impartially observed (such as when height/weight information is directly measured rather than self-declared), or if information is verified as part of the data collection process (such as when a birth certificate is required along with a declaration of age).

One final point illustrated by our results is that even when administrative data accurately record behavior or outcomes in a state, the likelihood that individuals may systematically move across states to avoid particular laws means that the population within which the behavior is being recorded may differ in important ways from the population researchers believe themselves to be studying.<sup>25</sup> Administrative data may indeed be superior to general survey data for answering many questions, but whether this is actually so in a given instance is something the researcher should carefully assess by considering both types of data and by contemplating the likely

<sup>22</sup> For instance, Martin O'Connell (1980) benchmarks the accuracy of the retrospective census data by comparing it to Vital Statistics data. He notes that the CPS reports a "more youthful distribution of women by age at first marriage" than the Vital Statistics in the 1940s and 1950s, while the two are more similar in later years, but he does not pursue this discrepancy.

<sup>23</sup> A growing literature studies various questions closely related to the issue of early marriage, including the effect of teenage childbearing on women's life outcomes, and the incidence of low education, higher levels of criminal activity, higher poverty, and other negative outcomes among persons born to teens. See David C. Ribar (1994), Daniel Klepinger, Shelly Lundberg, and Robert Plotnick (1999), Saul B. Hoffman (1998), Jennifer Hunt (2006), and Dahl (2005).

<sup>24</sup> For example, differences between reported income and earned income have long been a concern in the study of taxation. Recognizing some of the issues we discuss here, Joel Slemrod (1992) proposes a hierarchy of responses to tax policy and emphasizes that both timing and reporting are likely to be more responsive than actual economic behavior.

<sup>25</sup> This effect has been discussed in other contexts, such as the research on welfare migration (Terra McKinnish 2007), tax avoidance (Asplund, Friberg, and Wilander 2007), and abortion access (Phillip B. Levine et al. 1999).

contamination in each generated by agents' systematic efforts to avoid laws or rules aimed at preventing them from engaging in activities they wish to engage in.

## APPENDIX

TABLE A1—LEGAL AGE OF MARRIAGE WITHOUT PARENTAL CONSENT,  
1950, 1970, AND 1980

State	1950		1970		1980	
	Women	Men	Women	Men	Women	Men
Alabama	18	21	18	21	18	18
Alaska	18	21	18	19	18	18
Arizona	18	21	18	21	18	18
Arkansas	18	21	18	21	18	21
California	18	21	18	21	18	18
Colorado	18	21	18	21	18	18
Connecticut	21	21	21	21	18	18
Delaware	18	21	18	21	18	18
Florida	21	21	21	21	18	18
Georgia	18	21	19	19	18	18
Hawaii	20	20	18	20	18	18
Idaho	18	18	18	21	18	18
Illinois	18	21	18	21	18	18
Indiana	18	21	18	21	18	18
Iowa	18	21	18	21	18	18
Kansas	18	21	18	21	18	18
Kentucky	21	21	18	18	18	18
Louisiana	21	21	21	21	18	18
Maine	18	21	18	20	18	18
Maryland	18	21	18	21	18	18
Massachusetts	18	21	18	21	18	18
Michigan	18	18	18	18	18	18
Minnesota	18	21	18	21	18	18
Mississippi	18	21	15	17	15	17
Missouri	18	21	18	21	18	18
Montana	18	21	18	21	18	18
Nebraska	21	21	20	20	19	19
Nevada	18	21	18	21	18	18
New Hampshire	18	20	18	20	18	18
New Jersey	18	21	18	21	18	18
New Mexico	18	21	18	21	18	18
New York	18	21	18	21	18	18
North Carolina	18	18	18	18	18	18
North Dakota	18	21	18	21	18	18
Ohio	18	21	18	21	18	18
Oklahoma	18	21	18	21	18	18
Oregon	18	21	18	21	18	18
Pennsylvania	21	21	21	21	18	18
Rhode Island	21	21	21	21	18	18
South Carolina	14	18	18	18	18	18
South Dakota	18	21	18	21	18	18
Tennessee	18	18	18	18	18	18
Texas	18	21	18	19	18	18
Utah	18	21	18	21	18	18
Vermont	18	21	18	21	18	18
Virginia	21	21	21	21	18	18
Washington	18	21	18	18	18	18
West Virginia	21	21	21	21	16	18
Wisconsin	18	21	18	21	18	18
Wyoming	18	21	18	21	19	19

Note: Data on legal age requirements by state and year collected by the authors from state statutes.

TABLE A2—STATISTICAL TESTS OF THE EQUIVALENCE OF MARRIAGE PROPORTIONS

Age group	1950				1970			
	Vital Stats	Census	Difference	s.e.	Vital Stats	Census	Difference	s.e.
<i>Women: States with nonconsent age of 18 (Figures 1A and 2A)</i>								
≤17	0.108	0.197	-0.088	0.004	0.145	0.148	-0.003	0.002
18	0.203	0.145	0.058	0.004	0.162	0.142	0.019	0.002
≥19	0.689	0.658	0.030	0.005	0.694	0.710	-0.016	0.002
N	201,564	8,051			121,687	46,480		
<i>Women: States with nonconsent age of 21 (Figures 1B and 2B)</i>								
≤20	0.441	0.535	-0.094	0.012	0.567	0.566	0.001	0.006
21	0.133	0.113	0.020	0.008	0.137	0.137	0.000	0.004
≥22	0.425	0.352	0.074	0.012	0.295	0.297	-0.002	0.005
N	45,623	1,672			24,365	11,691		
<i>Men: States with nonconsent age of 18 to 20 (Figures 1C and 2C)</i>								
≤17	0.005	0.034	-0.029	0.003	0.038	0.044	-0.006	0.002
18-20	0.218	0.250	-0.032	0.008	0.362	0.330	0.033	0.004
21	0.157	0.132	0.024	0.006	0.137	0.142	-0.005	0.003
≥22	0.621	0.584	0.037	0.009	0.463	0.485	-0.021	0.004
N	71,012	3,059			43,662	17,209		
<i>Men: States with nonconsent age of 21 (Figures 1D and 2D)</i>								
≤20	0.185	0.259	-0.074	0.006	0.310	0.309	0.000	0.003
21	0.192	0.135	0.057	0.005	0.152	0.145	0.007	0.002
≥22	0.623	0.606	0.017	0.007	0.538	0.545	-0.007	0.003
N	196,015	5,263			99,671	36,639		

Notes: Please see corresponding figures for a list of the states included in each sample. The table reports the proportion of marriages that occur in several discrete age bins in each year for each sex. Each proportion may be thought of as having a binomial distribution, so that a test of the equivalence of the distributions across datasets can be done by taking the difference of the proportions. This difference will have an approximate z-distribution.

TABLE A3—STATISTICAL TESTS OF THE EQUIVALENCE OF MARRIAGE PROPORTIONS IN 1950 VITAL STATISTICS DATA ACROSS STATES WITH VARYING OPPORTUNITIES FOR MIGRATION

Age group	States with high migration potential				States with low migration potential			
	Vital Stats	Census	Difference	s.e.	Vital Stats	Census	Difference	s.e.
<i>Women: States with nonconsent age of 18 (Figures 3A and 3B)</i>								
≤17	0.103	0.239	-0.135	0.007	0.114	0.147	-0.033	0.006
18-20	0.488	0.417	0.071	0.008	0.431	0.424	0.007	0.008
≥21	0.409	0.344	0.065	0.007	0.455	0.429	0.026	0.008
N	112,550	4,341			89,014	3,710		
<i>Men: States with nonconsent age of 21 (Figures 3C and 3D)</i>								
≤17	0.009	0.042	-0.033	0.005	0.007	0.029	-0.022	0.003
18-20	0.150	0.271	-0.121	0.011	0.179	0.206	-0.026	0.007
≥21	0.841	0.687	0.155	0.012	0.814	0.766	0.048	0.007
N	65,068	1,638			87,110	3,625		

Notes: Please see corresponding figures for a list of the states included in each sample. The table reports the proportion of marriages that occur in several discrete age bins in each year for each sex. Each proportion may be thought of as having a binomial distribution, so that a test of the equivalence of the distributions across datasets can be done by taking the difference of the proportions. This difference will have an approximate z-distribution. States with high migration potential are those states that have an abutting neighbor with a different legal regime. For women, high migration potential states have a neighboring state with a nonconsent law of 18 years old. For men, high migration potential states have a neighboring state with a nonconsent law of 18, 19, or 20 years old. For both sexes, a state has a low migration potential if all of its abutting neighbors have the same law as that state.

TABLE A4—STATUTORY PROVISIONS FOR PROOF OF AGE AMONG THOSE APPLYING FOR A MARRIAGE LICENSE

State	Late 1920s	Current	Date of statutory change requiring documentation
Alabama	Not specified <sup>a</sup>	SSN	
Alaska	NA	SSN	1997
Arizona	Oath	Affidavit and SSN	
Arkansas	Affidavit <sup>a</sup>	BC	
California	Oath may be requested	Photo ID	Before 1988
Colorado	Affidavit	“Satisfactory proof” of age	
Connecticut	Oath	SSN	
Delaware	Oath	Affidavit and SSN	
Florida	Affidavit	Affidavit and SSN	
Georgia	Oath	BC, DL, or PP	1975
Hawaii	NA	SSN	
Idaho	Affidavit	BC	1967
Illinois	Affidavit	“Satisfactory proof” of age	
Indiana	Not specified	BC or DL	
Iowa	Affidavit or certificate of age	SSN	1961
Kansas	Oath	Affidavit	
Kentucky	Not specified <sup>a</sup>	BC or DL	
Louisiana	Not specified	BC	
Maine	Not specified	Oath and SSN	
Maryland	Oath	Affidavit and SSN	
Massachusetts	Oath	BC, DL, or PP	1931
Michigan	Affidavit	BC	1968
Minnesota	Oath	Affidavit	
Mississippi	Affidavit	BC or DL	
Missouri	Not specified	SSN	
Montana	Not specified	BC	
Nebraska	Not specified	Photo ID	
Nevada	Oath may be requested	Affidavit	
New Hampshire	Not specified	BC, DL, or PP	
New Jersey	Oath	Oath by witness and SSN	
New Mexico	Not specified	Affidavit	
New York	Affidavit	BC, DL, or PP	Before 1974
North Carolina	Oath may be requested	BC	1957
North Dakota	Oath	BC	1981
Ohio	Oath	Affidavit and SSN	
Oklahoma	Evidence can be requested	BC	1961
Oregon	Affidavit	“Reasonable proof” of age	
Pennsylvania	Oath	Affidavit	
Rhode Island	Oath		
South Carolina	Affidavit	BC	1962
South Dakota	Testimony of witnesses	BC, DL, or PP	
Tennessee	Not specified <sup>a</sup>	Affidavit	
Texas	Not specified	BC, DL, or PP	Before 1997
Utah	Affidavit		
Vermont	Oath		
Virginia	Not specified		
Washington	Affidavit	Affidavit	
West Virginia	Not specified	BC or DL	
Wisconsin	Oath	BC	
Wyoming	Testimony of witnesses	Affidavit	

Notes: SSN: Social Security Number; BC: Birth Certificate; DL: Driver’s License; PP: Passport. Late 1920s data from May (1929); current data and data on changes in statutes collected by authors.

<sup>a</sup>Financial penalty specified for misinformation.

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