Early Teen Marriage and Future Poverty

Abstract

Both early teen marriage and dropping out of high school have historically been associated with a variety of negative outcomes, including higher poverty rates throughout life. Are these negative outcomes due to pre-existing differences or do they represent the causal effect of marriage and schooling choices? To better understand the true personal and societal consequences, this paper uses an instrumental variables approach which takes advantage of variation in state laws regulating the age at which individuals are allowed to marry, drop out of school, and begin work. The baseline IV estimate indicates that a woman who marries young is 28 percentage points more likely to live in poverty when she is older. Similarly, a woman who drops out of school is 10 percentage points more likely to be poor. The IV results are robust to a variety of alternative specifications and estimation methods, including LIML estimation. In comparison, standard OLS estimates are extremely sensitive to how the data is aggregated, particularly for the early marriage variable.

1 Introduction

Historically, individuals were allowed to enter into a marriage contract at a very young age. In Ancient Rome, the appropriate minimum age was regarded as 14 for males and 12 for females. When Rome became Christianized, these age minimums were adopted into the ecclesiastical law of the Catholic Church. This canon law governed most marriages in Western Europe until the Reformation. When England broke away from the Catholic Church, the Anglican Church carried with it the same minimum age requirements for the prospective bride and groom. The minimum age requirements of 12 and 14 were eventually written into English civil law. By default, these provisions became the minimum marriage ages in colonial America. These common laws inherited from the British remained in force in America unless a specific state law was enacted to replace them.[1]

While Roman, Catholic, English, and early American law may have allowed marriage at 12 for girls and 14 for boys, many questioned the advisability of such an early union. Researchers and policymakers around the turn of the 20th century recognized that teens may be especially ill-prepared to assume the familial responsibilities and financial pressures associated with marriage.[2] As a result of the changing economic and social landscape of the U.S., in the latter part of the 19th century and throughout the 20th century, individual states began to slowly raise the minimum legal age at which individuals were allowed to marry. In the U.S., as in most developed countries, age restrictions have been revised upwards so that they are now between 15 and 21 years of age.

During this same time period, dramatic changes were also occurring in the educational system of the United States (see Goldin, 1998, 1999; Goldin and Katz, 1997, 2003; Lleras-

[1] See "Marriage Law," Encyclopædia Britannica, 2005.

[2] The Russell Sage Foundation commissioned an early study to raise awareness about "child" marriages and document state-specific minimum age laws (May, 1929). Concurrently, Richmond and Hall (1929) harshly criticized early teen marriage as a result of their investigation of 240 women who married before the age of 16. They concluded "the effects of child marriage do not cease with childhood. Both physically and socially the marriage relation can be permanently influenced by immature mating" (p. 124).

Muney, 2002). Free public schooling at the elementary level spread across the U.S. in the middle of the nineteenth century and free secondary schooling proliferated in the early part of the twentieth century. As secondary schooling became more commonplace, states began to pass compulsory schooling laws. States often also passed child labor laws which stipulated minimum age or schooling requirements before a work permit would be granted. These state-specific compulsory schooling and child labor laws are strongly correlated with the legal restrictions on marriage age, indicating that it might be important to consider the impact of all the laws simultaneously.

There are at least two rationales often given for the use of state laws as policy instruments to limit teenagers' choices. The first argument is that teens do not accurately compare short-run benefits versus long-run costs. If teens are making myopic decisions, restrictive state laws could prevent decisions they will later regret. It is also argued that the adverse effects associated with teenagers' choices impose external costs on the rest of society. If these effects can be prevented, external costs (such as higher welfare expenditures) would also argue for restrictive state laws. Both teenage marriage and dropping out of high school are closely associated with a variety of negative outcomes, including poverty later in life. To assess the relevance of either argument, however, it is important to know whether the observed effects are causal.

Any observed negative effects may be due to pre-existing differences, rather than a causal relationship between teen marriage (or schooling choices) and adverse adult outcomes. Women who marry as teens or drop out of school may come from more disadvantaged backgrounds or possess other unobserved characteristics that would naturally lead to worse outcomes. For example, teens choosing to marry young might have lower unobserved earnings ability, making it hard to draw conclusions about the causal relationship between teenage marriage and poverty.

To identify the effect of a teenager's marriage and schooling choices on future poverty, this paper uses state-specific marriage, schooling, and child labor laws as instruments. Variation across states and over time in these laws can be used to identify the causal impact teen marriage

and high school completion have on future economic well-being. While compulsory schooling laws have previously been used as instruments in a variety of settings (e.g., Acemoglu and Angrist, 2000; Black, Devereux, and Salvanes, 2004; Leon, 2004; Lleras-Muney, 2005; Lochner and Moretti, 2004; Oreopoulus, 2005; Oreopoulus, Page, and Stevens, 2003), this appears to be the first time marriage laws have been used as instruments. The idea of the marriage law instrument is that states with restrictive marriage laws will prevent some teenagers from marrying who would have married young had they lived in a state with more permissive laws.

Using the marriage, schooling, and labor laws affecting teens as instruments for early marriage and high school completion, I find strong negative effects for both variables on future poverty status. The baseline IV estimates imply that a woman who marries young is 28 percentage points more likely to live in poverty when she is older. Similarly, a woman who drops out of school is 10 percentage points more likely to be living in a family whose income is below the poverty line. The IV results are robust to a variety of alternative specifications and estimation methods, including LIML estimation. In comparison, the OLS estimates are very sensitive to how the data is aggregated, particularly for the early marriage variable.

The remainder of the paper proceeds as follows. In Section 2, I first briefly review the negative outcomes associated with teenage marriage and dropping out of school and discuss alternative perspectives for why teens might make these decisions. Section 3 describes the data and presents OLS estimates. I then discuss the early marriage, compulsory schooling, and child labor laws which will be used as instruments in Section 4. Section 5 presents the instrumental variable estimates and conducts several robustness checks. Section 6 provides concluding comments.

2 Early Marriage and Dropout Decisions

Previous research points to a variety of social, family, health, and financial outcomes which are strongly correlated with early teen marriage and low education. Women who marry

while in their teens are two-thirds more likely to divorce within 15 years of their wedding compared to women who postpone marriage. In addition, women who marry in their teens tend to have more children and to have those children earlier.[3] Teenage marriage is also associated with much lower education levels; women who marry before the age of 19 are fifty percent more likely to drop out of high school and four times less likely to graduate from college (U.S. Census data tabulations; Klepinger, Lundberg, and Plotnick, 1995; Ribar, 1994). There is an even larger literature documenting the negative outcomes associated with low education, including lower wages and higher unemployment rates (Katz and Autor, 1999), worse health (Berger and Leigh, 1989; Lleras-Muney, 2005), and higher crime rates (Lochner and Moretti, 2004).

The negative outcomes associated with early marriage and dropping out of high school have the potential to affect not only the individual making the decision, but also her children and the rest of society. For example, a high divorce rate combined with low wages and a larger family size increases the number of children living in poverty and receiving state assistance (Bane, 1986; Moffit, 1992). Children of teenage mothers also have lower birth weights, have a higher rate of infant homicide, are often the victims of child abuse and neglect, have academic and behavioral problems in school, and are more likely to engage in crime (Goerge and Lee, 1997; Heinz et al, 1998; Hotz, McElroy, and Sanders, 1997; Hunt, 2003).

Given these negative outcomes, why would an individual choose to marry young or drop out of high school? Traditional economic analysis focuses on rational and forward-looking individuals (Becker, 1974; Becker, Landes, and Michael, 1977). A woman chooses whether to accept a teen marriage offer (or drop out of school) based on the relative attractiveness of her alternatives. In this paradigm, a young woman fully anticipates the future consequences of her decisions, subject to some uncertainty about how things will actually turn out. Women who

[3] Married teen mothers are 40 percent more likely to have a second birth within 24 months of their first birth compared to unmarried teen mothers (Kalmuss and Namerow, 1994). For the sample period used in this paper, twenty-three percent of women who married in their teens gave birth to five or more children, versus eight percent for those who married later in life (U.S. Census tabulations). See also Kiernan (1986).

marry early can have a high likelihood of ending up poor later in life, yet still be optimizing. However, even if the individual is optimizing, society might still be concerned about the effects of poverty on her children and the costs associated with transfer programs.

An alternative perspective for why teens marry young is based on psychological and behavioral economic models. In a discussion of risky behavior among youth, O'Donoghue and Rabin (2001) explore extensions to the traditional approach which can help in modeling the decisions of adolescents. They argue that teens may not accurately compare short-run benefits versus long-run costs because teens discount the future too heavily. Two closely related explanations are that teens have time-inconsistent preferences or projection bias. These models provide an explanation for why teenagers engage in risky teenage behavior such as drinking, smoking, drug use, unprotected sex, and criminal activity, even though these behaviors can have substantial negative consequences in the long run (Gruber, 2001). Looking at schooling decisions, Oreopoulos (2007) argues that myopia helps explain why some teens drop out of school early. The various psychological explanations for poor decision-making by youth generally share the feature that teens make choices they will later regret.

Although teen marriage and low education are associated with a variety of below-average outcomes, it is not necessarily true that these choices caused the bad outcomes. For example, differences may be due to pre-existing characteristics of women who marry young versus later, rather than any causal relationship between teen marriage and negative adult outcomes.[4] To my knowledge, no previous research has studied the causal effect of early marriage. Yet understanding the causal effect of teens' choices is key for understanding whether they are making choices they will later regret or which impose costs on their children and society. If

[4] While such issues have received little attention in the context of teenage marriage, a related line of research attempts to disentangle the effects of teenage childbearing on education and wages from pre-existing differences between those who parent early and those who delay childbearing (Angrist and Evans, 1996; Geronimus and Korenman, 1992; Grogger and Bronars, 1993; Hoffman, 1998; Klepinger, Lundberg, and Plotnick, 1999).

teenage marriage and dropping out of high school are largely driven by unobserved personal characteristics which are the primary cause of negative outcomes, legal interventions to prevent these choices may make little difference. However, if strong causal effects exist, then state laws restricting teenagers' choices have the potential to greatly lessen the chances of future poverty.

3 Data and OLS Estimates

3.1 Data

The data for this paper combines information on state-specific marriage, schooling, and labor laws with individual-level data from the 1960, 1970, and 1980 U.S. Decennial Censuses. [5] Supplementary data is obtained from Vital Statistics marriage certificate data. The U.S. Census data are ideal for obtaining precise information about teenage marriage at the state level due to the large number of individuals in the survey. For 7% of the entire U.S. population in 1980, 3% in 1970, and 1% in 1960, the Census has information regarding age at first marriage, along with limited demographic, educational attainment, and economic variables.

Even though the Census datasets are cross-sectional surveys conducted every ten years, they contain information about women from a variety of cohorts. Since the surveys ask retrospective questions about age at first marriage and women are different ages when the survey is administered, a large dataset with time varying information can be created from the cross-sections. All three census years are combined together to create a dataset for women born between 1920 and 1954. These women were 15-year-old teenagers from 1935 to 1969, which corresponds to the approximate age they were at risk for becoming early teen brides. The sample is restricted to women who are currently between the ages of 20 and 60 and born in the U.S.[6] Data is also restricted to the 41 states with available data on marriage laws, compulsory schooling

[5] Data are taken from the U.S. Census 1960 general sample; the 1970 Form 1 State, Neighborhood, and Metro samples; and the 1980 State, Metro, and Urban/Rural samples (see Ruggles, et al, 2004; and http://www.ipums.org). Information on age at first marriage is not collected in the 1990 or 2000 Censuses.
[6] This age restriction implies the 1970 sample contains women born between 1920 and 1950 and the 1960 sample contains women born between 1920 and 1940.

laws, and child labor laws (these laws will be discussed in Section 4.1).

The Census data reveals that early teen marriage, which I define as marrying before the age of 16, has historically accounted for a nontrivial fraction of all marriages in the United States. In the sample used in this paper, 4.4% of women report first marrying between the ages of 12 and 15.[7] This compares to 4.5% of women first marrying at age 16 and 19.2% at age 17 or 18. Table 1 reports the fraction of women marrying for each of these age categories by 5-year age cohorts. Women are assigned to the cohorts based on the year when she was 15 years old (i.e., the 1935-1939 cohort includes women born between 1920 and 1924). The fraction of early teen marriages starts out at 4.3% for the cohort of women who are 15 in the early thirties, reaches a peak of 5.8% for the 1950-1954 cohort, and then declines to 2.5% by the late sixties.

This pattern by cohort is not unique to early teen marriages. It is also true that rates peak in the late fifties for women marrying between the ages of 16 and 18. More generally, the sample period used in our dataset coincides with a time when the median marriage age reaches its lowest level, consistent with the relatively large number of teenage brides. Looking over the entire century, the median age at first marriage started at 21.9 years at the turn of the century, dropped to a low of 20.3 years in 1950, and rose to a high of 25.1 years by the year 2000.[8]

In contrast, Table 1 shows that education levels rise monotonically with each cohort. For women in the 1935-39 cohort, 40% dropped out of high school, but by the late sixties only 14%

[7] Age at first marriage is calculated from each woman's date of first marriage and date of birth. The Census Bureau allocates some of the values for age at first marriage. The hot deck allocation mechanism used in 1980 (and to a lesser extent in 1960 and 1970) suffers from bracketing issues, with sharp spikes in marriage rates occurring for women whose current age is a multiple of five. To deal with the five-year bracketing, allocated marriages are discarded, and non-allocated marriages are reweighted to preserve the overall marriage rate for the entire sample period (based on both the allocated and non-allocated data). The weights make little difference for the early teen marriage rates in 1960 and 1970 (weights of 1.033 and 1.031, respectively), but are more important for the 1980 sample (weight of 1.41). Bracketing issues have more of an effect in 1980 since a much larger fraction of teenage marriages were allocated in that census year. Alternative methods which smooth the allocated data to eliminate bracketing effects were explored and yielded very similar results.

[8] Median age at first marriage was 22.0 in 1890, 21.9 in 1900, 21.6 in 1910, 21.2 in 1920, 21.3 in 1930, 21.5 in 1940, 20.3 in 1950, 20.3 in 1960, and 20.8 in 1970, 22.0 in 1980, 23.9 in 1990, and 25.1 in 2000 (U.S. Census Bureau).

dropped out. The fraction of high-school graduates remains fairly constant across cohorts, with a substantial increase in the number of women who attend at least some college.

As a summary measure of well-being, I use a variable which indicates whether the woman lives in a poor family according to the government definition of poverty. Whether a woman lives in poverty depends on family income, family size (including the number of children in the family), and whether the householder is over age 65. This poverty variable captures the cumulative impact of a variety of past decisions by a woman. As such, it is a useful summary measure of the consequences of early marriage and dropping out of high school. For example, a woman who marries young may have additional children, gain less work experience, and divorce sooner, all of which likely increase the chances of future poverty. Table 1 reports that poverty rates are roughly 10% for all cohorts in the sample. If a teen marries young or drops out of high school, however, the poverty rate is much higher.[9]

The large fraction of early teen marriages for the women in our sample is ideal for the current paper. There are over 140,000 of these early teen marriages in our combined census sample. The large number of high school dropouts, and the dramatic decrease over time which does not parallel the pattern in early marriage rates, makes the data well-suited to separate out the two effects. Perhaps the biggest advantage of the data, however, is that this era of high teen marriage rates and declining dropout rates coincides with a time period when many states were revising their early marriage, compulsory schooling, and child labor laws. These laws are discussed in the next section, after the OLS estimates are presented.

[9] One thing to keep in mind when looking at Table 1 is that the age range of women in each cohort is not the same since we have restricted women to be between the ages of 20 and 60 for each of the three census years. For example, in the 1935-1939 cohort, women from the 1980 Census are age 55 to 59, from the 1970 Census are 45 to 49, and from the 1960 Census are 35 to 39. For the 1965-1969 cohort, women from the 1980 Census are 30 to 34, and from the 1970 Census are 20 to 24. To account for this fact, the OLS and IV regressions in subsequent tables control for age and census year in addition to a variety of other characteristics. The age differences across cohorts mask the fact that age-adjusted poverty rates actually decline over time. Note that this does not affect the age at first marriage variable, however, since all women in the sample are over age 20 regardless of cohort. It should also not markedly affect the dropout variable, since few women finish high school after age 20.

3.2 OLS Estimates

How are poverty, early teen marriage, and dropping out of high school related? Figure 1 graphs these relationships at the state level using our combined census sample. The top graph plots the poverty rate versus the early marriage rate for each state. There is a clear positive association between the two rates. The graph also indicates that both early teen marriage rates and poverty rates vary greatly across states; the early marriage rate varies from a low of 1.2% in Rhode Island to a high of 9.6% in Mississippi, while the poverty rate varies from 5.6% in Connecticut to 21.9% in Mississippi. In the bottom figure, dropout rates are also shown to be strongly related to poverty at the state level. The dropout rate also varies widely across states, from a low of 13.9% in Washington to a high of 44.4% in Georgia. Although not shown, the early marriage and dropout rates are also positively correlated across states.[10]

To account for other possible determinants of poverty, I begin by presenting OLS estimates of the effect of early teen marriage and dropout status on poverty. The top panel of Table 2 presents the results for the individual-level data, which includes more than 3 million observations. The estimates in column (1) do not include any controls, and indicate that early marriage and dropping out of high school increase the chance of poverty by around 4% and 13%, respectively. Including additional control variables decreases the estimates slightly, to around 3% and 12% respectively. These estimates suggest that dropping out of high school has a sizable impact on future poverty, but that teen marriage has relatively modest effect.

In contrast to the individual-level estimates, the grouped data results in the bottom panel of Table 2 present a very different picture. In the bottom panel, the data is aggregated to state of birth × year of birth × census year cell means. Without controls, early teen marriage is estimated to have a large effect (a 78% increase in the poverty rate) while dropping out of high school has

[10] The rates by state appearing in Figure 1 are an average for women from all age cohorts (1935 to 1969). Cohort-specific rates can be even higher in a state; for example, the early marriage rate was 15.1% in Mississippi for the 1951 cohort and the high school dropout rate was 66.7% in Georgia for the 1935 cohort.

only a modest effect (a 5% increase in the poverty rate). As year, race, and age effects are added in, the early marriage coefficient falls threefold, while the dropout coefficient triples. Unlike the case for the individual level estimates, adding in state of birth and cohort effects has a large impact on the aggregate estimates. Column (3) reveals that the estimate of the early teen marriage coefficient actually becomes negative when these additional variables are included. In column (4), the addition of region-specific birth-year trends makes this coefficient even more negative and statistically significant, so that teens who marry young are predicted to have a 13% *lower* probability of poverty.

What explains the dramatically different estimates for the individual versus grouped data? First, consider the conditions under which there should be no difference in the two sets of estimates. Theil (1954) shows aggregation does not affect the estimated relationship as long as the model is correctly specified and the coefficient of interest is the same for all individuals (although efficiency can be affected with aggregation).

Of course, if there is omitted variable bias or measurement error, the model is not correctly specified. In such cases, the conditions for perfect aggregation will not hold, and differences can arise between the two sets of estimates. When the model is not correctly specified, aggregation has the potential to do two things: (1) minimize attenuation bias arising from noisily measured covariates, and (2) either minimize or exacerbate the effects of selection bias. The effect of aggregation is *a priori* ambiguous, and without further structure it is not possible to know whether it will worsen or improve specification bias.[11] It is also possible that aggregation does not hold because early marriage has heterogeneous effects across women.

There is reason to believe that teen marriage is noisily measured, suggesting aggregation might be a good idea. In our Census data, age at first marriage is calculated from each person's

[11] For another example where aggregation bias appears to be important, see Hanushek, Rivkin, and Taylor (1996). They discuss how aggregation might help explain the contradictory estimates found in the literature for the effect of school resources on student outcomes (which range from negative and significant to positive and significant).

date of first marriage and date of birth. The 1970 and 1980 Censuses have explicit instructions asking for an estimate if individuals cannot remember the exact month and year of their first marriage, suggesting the Census recognized that dates are likely to be remembered imperfectly. While education could also be measured with error, it likely has a higher signal to noise ratio compared to the teen marriage variable.

Looking at just the first two columns of Table 2, measurement error appears to play a role in explaining the large differences between the individual-level and group-level estimates. However, it cannot be the only force at work, as it would require an unreasonably small signal to noise ratio to explain the entire gap (e.g., the early marriage coefficient in column (1) increases from 0.04 to 0.78). Columns (3) and (4) confirm that measurement error is not the only story, as the grouped estimate actually becomes negative after adding additional control variables. It is likely that both measurement error and omitted variable bias play a role, and that the nature of the correlation of the marriage variable with the error term in the poverty equation changes with both aggregation and the set of control variables. A similar, but less severe, combination of measurement error and selection bias plausibly explains the smaller differences in the dropout coefficient estimates as well. In addition, it is possible that differential treatment effects could be driving some of the differences between the individual and aggregate level results.

If appropriate instruments can be found, misspecification due to omitted variables or measurement error can be eliminated at both the individual and aggregate level. As we shall see later, using state marriage, compulsory schooling, and child labor laws as instruments results in similar individual-level IV and aggregate IV estimates. Of course, if there are heterogeneous treatment effects, one should be appropriately cautious to interpret the IV estimates in a local average treatment effect context.

4 State Laws and Their Effect on Early Marriage and Schooling

4.1 State Marriage, Schooling, and Labor Laws Affecting Youth

The OLS estimates presented in the last section potentially suffer from both omitted variable bias and measurement error. One solution to these problems is to use an instrumental variables approach. Ideally, instruments would induce exogenous variation in early teen marriage but be uncorrelated with unobserved characteristics which affect both poverty and the decision to marry young. Similarly, the instruments would induce exogenous variation in high school graduation but be orthogonal to the error term in the poverty equation. I use changes in state marriage, schooling, and labor laws over time as instruments for early marriage and dropping out of high school. By preventing some teens who would like to marry or drop out of high school from doing so, these legal restrictions can help identify the causal effects on poverty free of selection bias.

In the U.S., wide variation has historically existed regarding the minimum age individuals were legally allowed to marry. The laws which regulate teenage marriage have appeared in the *World Almanac and Book of Facts* starting in the late 1800's. Since 1935, information has consistently been reported on the minimum marriage age with parental (or court) consent, separately for males and females. I have collected this information annually for the years 1935 to 1969, for the 41 states with reliable information on marriage laws during this time period.[12]

There are two sets of laws specifying minimum age requirements for marriage. The first

[12] Information was collected from each year's *World Almanac*. If a state's law was missing or changed for one (or at most two) years and then returned to its previous value, that year's law was replaced with the value from the surrounding years. This procedure resulted in 12 changes out of a total of 1,435 state-year laws. If these 12 changes are not made, the results which follow are virtually identical. Alaska and Hawaii are excluded since compulsory schooling and child labor laws are not available. Maine, Massachusetts, Michigan, Minnesota, New Jersey, North Carolina, South Carolina, and West Virginia are excluded since the *World Almanac* reports unstable, noisy data on their state laws (i.e., multiple up and down changes spanning several years in the marriage laws). If these eight states are included in the analysis, the estimates are less precisely estimated, but the general conclusions do not change.

is the minimum age with parental (or court) consent while the other is the minimum age without parental consent. In this paper, I focus on the marriage age laws with parental consent, partly because there is little variation over time or across states in the laws without parental consent (these laws generally specify an age of 18). It should be noted that the laws do not eliminate all early teenage marriages. Some teens may find ways to lie about their age or may travel to states with lower age requirements to get married. In addition, courts have the right to grant exceptions to women based on "moral" and "welfare" arguments (i.e., if the teenage woman is pregnant). The fact that restrictive laws do not prevent 100% of early teenage marriages does not make them invalid instruments. Rather, the strength of the instrument set is that restrictive state laws make it harder to marry young, thereby preventing some fraction of teen marriages that otherwise would have occurred.

I also use the compulsory schooling and labor laws originally collected by Acemoglu and Angrist (2000), and subsequently modified by Goldin and Katz (2003). These laws typically specify a minimum age or amount of schooling before a youth can drop out of school or obtain a work permit. Using Goldin and Katz's approach, compulsory school attendance is defined as the *minimum* of (1) the required years of schooling before dropping out and (2) the difference between the minimum dropout age and the maximum enrollment age (lagged 8 years). Child labor is defined as the *maximum* of (1) the required years of schooling before receiving a work permit and (2) the difference between the minimum work age and the maximum enrollment age (lagged 8 years). The value of the marriage, schooling, and labor laws assigned to a woman are based on the set of laws in force when she was age 15.[13]

Table 3 summarizes the changes in these laws across the same five-year time periods used to define cohorts in Table 1. From 1935 to 1939, 41% of states specified that a woman had

[13] Using the set of laws corresponding to when a woman was age 14 or age 16 yields similar results. Ideally, the state laws would be assigned based on where a woman lived in her early teen years; since this information is not available in the Census, I assign laws based on a woman's state of birth.

to be 16 or older before marrying. Over time, several states raised their age requirements, so that by 1965-1969, 70% of states required a woman to be at least 16 before marrying. Summarizing the law changes another way, the average minimum marriage age across states was 14.6 years at the beginning of the sample period, but rose by approximately one year to 15.7 years by the end of the sample. There have also been similar increases in the requirements governing school attendance and child labor. In 1935-1939, 24% of states required at least 9 years of compulsory schooling; by 1965-1969, this rose to 63% of states. Similarly, in 1935-1939, only 2% of states had a child labor requirement of 9 years or more; by 1965-1969, 38% of states had such a requirement.

Figure 2 shows the geographical distribution of the legal minimum marriage age at the beginning and end of the sample period. In 1935, there is a fair amount of variation in the laws, with 25 out of 41 states specifying an age of 15 or less. While southern states generally have lower minimums to begin with, there is a mix of age minimums in all regions of the country. By 1969, a substantial fraction of states had revised their marriage law upwards, with only 11 states specifying a legal minimum of 15 years or less. The states with relatively permissive laws regulating marriage are scattered throughout the country at the end of the sample period. In 1969, New Hampshire had a requirement of 13; Alabama, New York, Texas, and Utah had a requirement of 14; Idaho, Mississippi, Missouri, North Dakota, Oklahoma, and Oregon had a requirement of 15. A more detailed listing of the early marriage laws across states can be found in the Appendix Table.

Previous work has documented the patterns of compulsory schooling and child labor laws across states and over time, and hence is not repeated here (Acemoglu and Angrist, 2000; Goldin and Katz, 2003; Lleras-Muney, 2002; Lochner and Moretti, 2004; Margo and Finegan, 1996). What has not been documented, however, is the strong correlation between these laws and the early marriage laws. Table 4 shows the relationship between these three sets of laws by tabulating the relative frequencies of various combinations of laws.

Consider the first panel, which tabulates the marriage laws versus the schooling laws. The rows indicate compulsory attendance requirements and the columns indicate minimum marriage age requirements. The values for these two sets of laws are clearly interrelated; the chi-square test for the independence of the rows and the columns is strongly rejected. States with relatively low required compulsory attendance laws are generally more likely to have a low marriage age law, although the relationship is not always monotonic. The marriage laws are also intertwined with the child labor laws in a state; for example, states with a child labor law of 6 years or less are considerably less likely to prohibit marriages at very early ages (before age 16) compared to states with child labor laws of 9 or more (43% versus 68%, respectively). Finally, the compulsory schooling laws are highly correlated with the child labor laws. States with a child labor law of 6 or less require 7 or fewer years of school 24% of the time, while no state with a child labor law of 9 or more ever has such a lax compulsory schooling law.[14]

Since the marriage, schooling, and labor laws affecting youth are so highly correlated, it could be important to account for all three simultaneously when estimating instrumental variable regression models. Past research has used the compulsory schooling and child labor laws as instruments for education in models describing human capital externalities (Acemoglu and Angrist, 2000), crime (Lochner and Moretti, 2004), mortality (Lleras-Muney, 2005), intergenerational transmission of human capital (Oreopoulus, Page, and Stevens, 2003), and fertility (Black, Devereux, and Salvanes, 2004; Leon, 2004). In many of these applications, there may not be a need to instrument for early teen marriage. However, for some outcomes, part of the observed effects might be due to changes in marriage laws (and early marriage rates) but mistakenly attributed to changes in compulsory schooling laws (and education levels) instead. In the IV regressions which follow in Section 5, I use all three sets of laws in poverty regressions which instrument for early marriage and high school completion.

[14] The interrelated nature of the marriage and schooling/labor laws cannot be attributed solely to trends over time. After regressing out time trends in the laws, the state laws are still highly related.

4.2 The Impact of State Laws on Early Marriage

How effective are state-specific marriage laws at restricting the age individuals marry? Other work has examined the effectiveness of compulsory schooling and child labor laws on high school graduation, and is not repeated here (See Acemoglu and Angrist, 2000; Goldin and Katz, 2003; Lleras-Muney, 2002; Lochner and Moretti, 2004; Margo and Finegan, 1996). The combined census samples reveal that restrictive laws are associated with a smaller number of early teen marriages (i.e., marriages occurring between the ages of 12 and 15). In states with a legal minimum of 12-13, 14, 15, and 16+, the percent of women who are early teen brides is, respectively, 7.9%, 5.4%, 4.4%, and 3.6%.[15] Of course, these differentials could partly be due to time trends or variation across states with differing laws. In the IV regressions appearing in the next section, these factors will be accounted for.

Are the laws actually reducing the number of teen marriages or would states with restrictive laws naturally have lower teen marriage rates anyway? If states laws actually prevent early teen marriages, one would expect to see a jump in the number of marriages occurring immediately after the specified minimum age. I use the 1968 and 1969 Vital Statistics Marriage Detail files, which collect data from marriage certificates, to examine the timing of teen marriages.[16] For women who married between the ages of 14 and 16 in 1968 or 1969, Figure 3 plots the fraction of women marrying at different ages (measured in two-month intervals) who are residents of states with different legal age minima.

Sharp increases in the fraction marrying occur where expected assuming the laws are

[15] Although not shown, there is a persistent difference in early teen marriage rates over time. The trends across states with restrictive or permissive laws both follow the same general pattern shown in Table 1 (i.e., a peak in 1950-54, and a decline by 1965-1969).

[16] Data from 1968 and 1969 is used because earlier years are not readily available. Data is collected for the 42 registered Marriage Reporting Areas (MRAs). Marriage certificate data is not reported in 1968 and 1969 for the non-MRA states of Arizona, Arkansas, Nevada, New Mexico, North Dakota, Oklahoma, South Carolina, Texas, and Washington. In addition, 6 MRA states collect marriage age data in years, but do not record age in months (District of Columbia, Idaho, Indiana, Maryland, Massachusetts, and Ohio) and hence are excluded from the analysis in Figure 3. MRA states for which marriage law information is unavailable are also excluded from Figure 3 and Table 5.

enforced. For example, in states where the legal minimum is 14 years, a fair number of women actually marry at this young age. Moreover, there is not much of a jump in marriages once women turn age 15. In contrast, in states where the legal minimum is 15 years, there a sudden rise in the number of marriages immediately after women reach the minimum age of 15. For another example, consider women marrying at age 16. In the third graph where the legal minimum is 16, there is a sharp and large increase in the number of marriages occurring immediately after women turn 16. In comparison, the rise surrounding age 16 is much less pronounced in states with minimum ages of 14 or especially 15.[17] Most states also record the state where the marriage takes place on the marriage certificate. A graph of the timing of marriages by marriage state instead of residence state, although not shown, yields a similar picture. The graphs suggest that restrictive state laws effectively delay or prevent at least some early teen marriages.

Another way to test whether state laws impact the probability of marrying young is to see whether teens travel to a state with a lower age requirement to get married. If so, this is an indication that restrictive laws impose costs on those wishing to marry before the law in their state of residence allows. Some young teens will cross state lines, while others will be deterred by these costs. The extent to which teens cross state lines to marry in states with more permissive laws can be examined using the residence state and marriage state information in the Vital Statistics datasets.

Before looking at the entire U.S., first consider the case for women residing in Tennessee. Tennessee is a long, narrow state, with population centers scattered throughout the state. Tennessee has an age requirement of 16 years for women to marry in 1968 and 1969, the period

[17] There are also noticeable rises surrounding the time a young women has a birthday regardless of the legal restriction. For example, there are moderate jumps at age 16 even in states where the legal minimum is 14 or 15 years old. Two possible explanations are that parents or the courts may not give their consent to let young women marry until they reach their 16th birthday or that young women themselves do not wish to marry until they turn 16.

for which Vital Statistics data is available. As Figure 2 shows, Tennessee is bordered by 8 states with varying age minimum. Six of these states have valid marriage certificate and marriage law information.[18] If the marriage age law is binding in Tennessee, we might expect those who want to marry earlier than the law allows in Tennessee to travel to Alabama, Mississippi, or Missouri, where the age minimum is lower. However, we should not see as many prospective teen brides travel to Georgia, Kentucky, or Virginia, where the age requirement of 16 is the same as in Tennessee.

The pattern of out-of-state marriages strongly supports the idea that Tennessee teens travel to bordering states with more permissive laws in order to marry young. Twenty-two percent of women from Tennessee who marry before the age of 16 travel to Alabama, Mississippi, or Missouri to marry compared to only 4% who travel to Georgia, Kentucky, or Virginia. This is not because Alabama, Mississippi, and Missouri are more convenient or attractive places to get married in general, however. For Tennessee brides who marry at age 16, 4% travel to Alabama, Mississippi, or Missouri; this compares to 18% who travel to Georgia, Kentucky, or Virginia. It appears that the set of neighboring states with an age requirement identical to Tennessee's are the preferred marriage destinations, but that brides wishing to marry below the age of 16 go out of their way to marry in a state with a lower age requirement.[19]

Table 5 extends the Tennessee analysis of out-of-state marriages to all of the states in the sample. I categorize women based on the earliest age they can marry in their state of residence with their parent's consent. I then tabulate the percentage of women who marry (1) in their state of residence, (2) in a state with a lower minimum age compared to their residence state, and (3) in a state with an equal or higher minimum age compared to their residence state. For women who

[18] Arkansas is not in a Marriage Reporting Area, so no marriage certificate data is available; North Carolina does not have information available on marriage laws.

[19] Jerry Lee Lewis, the rock singer, and Myra Gale Brown are perhaps the most famous example of a Tennessee couple traveling across state lines to marry. In 1957, Lewis took his 13-year-old second cousin to Hernando, Mississippi, where they were married. At the time, the minimum marriage age was only 12 in Mississippi, while it was 16 in Tennessee.

married between the ages of 12 and 15, 22% of those living in states with a legal minimum of 16 years of age went to states with lower age limits to marry. In contrast, individuals living in states with legal minima of 13, 14, or 15 years were much more likely to remain in their residence state to marry (only 5% travel outside their residence state to marry).

Of course, the patterns observed in the top panel of Table 5 could be the result of the location of states with various laws or the general attractiveness of marrying in different states. To control for this possibility, in the bottom panel of Table 5, I tabulate marriage patterns for women who married at age 16. For these women, the marriage laws should not be binding. Indeed, fewer of the women facing an age minimum of 16 leave their residence state to marry. In contrast to the top panel, women in states with laws specifying a legal minimum of 16 who choose to marry outside their state of residence are much more likely to marry in states with an equal or higher minimum age law.

A simple difference-in-differences estimate makes clear that women are crossing state lines to marry young. To construct the estimate, first compare the fraction of women who marry in a state with a lower minimum versus a higher minimum. Subtracting this difference for women who marry between 12 and 15 from the difference for women who marry at age 16 yields the estimate. For states with a marriage requirement of 13 or 14, the difference in difference is close to 0 and not significant, as expected. For states with an age minimum of 15, the estimated difference in difference is 4.6% and significantly different from zero. An even greater contrast shows up for the states specifying a minimum age of 16, with a large and significant estimate of 14.0%. These results imply that restrictive marriage laws increase the costs to potential teen brides and likely prevent some desired early teen marriages.

As a final check on the validity of the laws as instruments, I explore the timing of law changes. One potential concern is that states which pass more restrictive laws would have experienced larger reductions in early teen marriage rates even in the absence of a law change. However, if law changes are exogenous, then future values of the laws should not affect current

early marriage rates conditional on current laws.[20] To check this, I added the state laws in place ten years in the future into a regression describing early teen marriage rates, where the regression also includes the current set of laws (and controls for year, race, age, state of birth, cohort of birth effects, and region of birth trends). The results from this exercise indicate that future laws do not significantly determine current early marriage rates, while current laws do. The F-statistic for the effect of future laws is 0.92 (p-value=0.53), while the F-statistic for the effect of current laws is 8.52 (p-value=.01).

5 Instrumental Variable Estimates

5.1 First Stage Results

To investigate the effects of teenage marriage and high school completion on subsequent poverty, this paper uses state marriage, schooling, and labor laws as instrumental variables. The bottom panel in Table 6 presents the first stage estimates. Since I am instrumenting for both early marriage and dropout status, there are two sets of regression estimates. Column 1 regresses a dummy variable for early teen marriage on the set of marriage, schooling, and labor laws. Additional controls include year, race, age, state of birth, cohort of birth effects, and region of birth trends as described in the footnote to Table 2.

The marriage laws significantly reduce the number of teens who marry before the age of 16; *ceteris paribus*, states with a legislated minimum of 13 or less have between 0.7 to 1.4 percentage points fewer early marriages compared to states with more restrictive marriage laws. In states without a legislated minimum, common law (which specifies a minimum of 12 years) prevails; the estimated effect of a common law is similar to a legislated minimum of 13 or less. Interestingly, the compulsory schooling laws seem to work in the opposite direction—more restrictive schooling laws actually increase the probability of an early marriage. These effects are

[20] Black, Devereux, and Salvanes (2004) and Lochner and Moretti (2004) perform similar analyses for compulsory schooling laws and find that future laws do not affect current dropout rates in a state.

jointly, and sometimes individually, statistically significant. A woman born in a state with a compulsory attendance law of 10 or greater has nearly a one percentage point higher probability of marriage at an early age. One possible explanation is that early marriage becomes more attractive to a young woman if her other options, such as dropping out of school, are more limited.[21] The third set of laws which deal with child labor are smaller and statistically insignificant.

Column 2 presents the same set of coefficient estimates for the first-stage dropout regressions. As expected, the compulsory schooling laws have a relatively large and jointly significant effect on whether a young woman finishes high school. The marriage laws have nontrivial coefficient estimates, but are imprecisely estimated and therefore not significant. As discussed earlier, one reason why dropout status might project onto the marriage laws is that the marriage laws are highly correlated with the compulsory schooling laws. The marriage laws are measured every year but the schooling laws are only measured intermittently.[22] In the years for which schooling laws are interpolated noisily, effects may load onto the marriage laws instead. More restrictive child labor laws seem to discourage some women from dropping out of school, but the estimates are not statistically significant.

For all of the estimates, F-statistics are reported for the joint significance of the instruments. The F-statistic is 13.5 for the early teen marriage equation and 7.3 for the dropout equation (both have 10 numerator degrees of freedom). Weak instruments can lead to biased IV estimates; under general conditions and finite samples, weak instruments bias the estimates in the same direction as OLS estimates (See Bound, Jaeger, and Baker, 1995; and Staiger and Stock, 1997). While the F-statistics appearing in Table 6 are sizable and statistically significant, one might still be concerned about weak identification. Researchers have argued that limited

[21] In some states, individuals are exempted from the compulsory schooling requirement if they are married or pregnant.

[22] The compulsory school and child labor laws were collected approximately every five years. I adopt the approach of Acemoglu and Angrist (2000) and interpolate by extending older data.

information maximum likelihood estimates (LIML) are more robust than least squares IV with moderately weak instruments (see, for example, Stock, 2002). Therefore, in the results which follow, I present LIML estimates in addition to standard IV estimates.

All of the standard errors reported in Table 6 (and throughout the paper) are adjusted for clustering by state of birth to account for arbitrary correlation over time. Bertrand, Duflo, and Mullainathan (2004) show that failure to account for such correlation can lead to severely biased confidence intervals for the estimated coefficients. This is particularly likely to be important in IV analyses which use laws over time as instruments, since there is typically a long time component and plausible serial correlation. The current paper illustrates that failure to adjust the standard errors in the first stage and the corresponding F-statistics can make a large difference. In Table 6, I also report the F-statistics which result from cluster adjustments at the state of birth × year of birth level. This is the level many researchers cluster at, since instruments based on state laws usually vary at this level. One can see that the F-statistics are larger for the dropout equation when clustering at this narrower grouping, but actually somewhat smaller for the early marriage equation.

One message from Table 6 is that the compulsory schooling laws which have been widely used as instruments are not necessarily as strong as previously thought.[23] There are three reasons why the F-statistics for the dropout equation are relatively small. First, the level of clustering matters, as discussed in the previous paragraph. Second, region of birth trends knock out some of the predictive power of the schooling laws, but have not generally been included in past studies. Finally, the minor modifications and corrections to Acemoglu and Angrist's (2000) compulsory schooling law variables suggested by Goldin and Katz (2003) result in somewhat smaller first stage F-statistics. To see the combined impact of these three reasons more clearly, consider similar first stage regressions for dropout status (which, for simplicity, exclude the

[23] Notable exceptions include Goldin and Katz (2003) and Black, Devereux, and Salvanes (2004) who cluster at the state-year level.

marriage law and child labor law instruments). With state of birth × year of birth clustering, no region of birth trends, and Acemoglu and Angrist's original set of schooling laws, the first stage F-statistic is 50.4 (3 d.f.). With state of birth clustering, region of birth trends, and Goldin and Katz's set of revised laws, the first stage F-statistic drops to 5.8 (3 d.f.). This does not mean, of course, that schooling laws shouldn't be used, but it does suggest that alternative estimation techniques such as LIML which are more robust to weak instruments should at least be investigated.

5.2 Baseline IV Results

The top panel of Table 6 presents the baseline results for the instrumented poverty regression. Early teen marriage and dropping out of high school both have sizable effects on the probability a woman will end up in poverty. The estimates imply that marrying young is associated with a 28 percentage point increase in the probability of living in poverty. Dropping out of high school is associated with a 10 percentage point increase in poverty.

To help assess whether weak instruments might be biasing the results, the first panel in Table 7 reports LIML estimates for the baseline model. The consensus in the literature is that when there are many instruments / weak instruments, LIML tends to exhibit less bias compared to least squares IV, and LIML confidence intervals typically also have better coverage rates (Stock, 2002).[24] The LIML results are very similar to the IV estimates in Table 6, with a slight increase in the early marriage estimates. This suggests that weak instruments are not a major issue for estimation.

Aggregation bias was shown to be a major issue for the OLS estimates appearing in Table 2. As previously discussed, the individual-level and grouped-level OLS estimates

[24] Of course if the instruments are weak enough, both the least squares IV and the LIML confidence intervals can have the wrong coverage rates. With a single endogenous variable, solutions include inverting the Anderson-Rubin test statistic or implementing the conditional likelihood ratio test of Moreira (2003). These approaches do not readily extend to the case where there are two or more endogenous variables, which is the situation in the current paper.

presented very different pictures, especially for the relationship between poverty and early teen marriage. A combination of measurement error, omitted variables, and heterogeneous effects were discussed as likely culprits for the lack of perfect aggregation. In addition, Bertrand, Duflo, and Mullainathan (2004) show that clustering does not always do a good job in correcting the standard errors if the within-group sample is large. Aggregating the data should produce more conservative standard errors in such situations. The bottom two panels of Table 7 repeat the baseline IV and LIML analyses based on individual-level data, but this time with grouped data. As a reminder, the data is aggregated at the state of birth × year of birth × census year level.

The grouped data estimates, using either IV or LIML, are virtually identical to the individual-level estimates. In no instance do the estimated coefficients on either early marriage or dropout vary by more than a percentage point. The similarity of the coefficient estimates is not surprising, since the instruments are constant for all individuals in a state-cohort group, effectively aggregating both the individual-level and group-level estimates. Perhaps less surprisingly, the standard errors change very little when using the grouped data, indicating the cluster-corrected standard errors are not sensitive to the level of aggregation.

To summarize, Tables 6 and 7 indicate the causal effect of early teen marriage and dropout status on future poverty is substantial.[25] Regardless of the method or aggregation, they imply that marrying young increases the chances a young bride will end up in poverty later in life by around 28 percentage points. Dropping out of high school has a somewhat smaller, but still substantial, 10 percentage point effect on future poverty. The conclusion is that early marriage and schooling choices have a strong impact on well-being several years down the road.

[25] To interpret the estimates as causal, I am implicitly assuming the laws do not differentially affect level of schooling and age of marriage within the dropout and early teen marriage indicator categories. The basic conclusions do not change when adding continuous measures for age at first marriage and years of schooling into the regressions (and instrumenting for them as well). In particular, the estimated coefficient on the early teen marriage variable remains very close to the baseline IV estimate. The estimates are 0.276 (s.e.=0.089) for early teen marriage, -0.001 (s.e.=.004) for age at first marriage, 0.034 (s.e.=0.095) for dropout, and -0.015 (0.023) for years of education. While the dropout dummy is no longer individually significant in this regression, the dropout and the years of education variables are jointly significant.

The IV estimates can be more easily understood in a local average treatment context (Imbens and Angrist, 1994). Consider the early teen marriage variable, and for simplicity, think of state laws regarding marriage as being dichotomous, i.e., either permissive or restrictive. The IV estimate can be interpreted as the average effect on poverty for those women who married young because a permissive state law did not prevent them from doing so. That is, the estimate captures the effect of early marriage on those women who would be prevented from marrying if their state law changed from permissive to restrictive.[26]

While we cannot directly observe this subpopulation, we can speculate on the type of women whose marriage decisions might be affected by state marriage laws. Note that it is *not* likely to be composed primarily of the set of teens that get pregnant and marry as a consequence, since most state laws allow for such exceptions. Rather, it is likely to be those young teenagers who would like to marry, perhaps because they believe they are in love or think it will solve their problems. It is likely that restrictive state laws are preventing these young girls from making a very myopic decision. Viewed in this light, it is not so surprising that the baseline IV estimate is a 28 percentage point effect. Marrying young may result in outcomes which have long-lasting effects; forcing this group of women to wait to marry until they are older will likely delay childbearing, increase the chances of graduation from high school, and result in a more stable family situation once they do marry.

Why are the individual-level OLS estimates so small in comparison to the IV estimates? As a reminder, the individual-level OLS estimates appearing in Table 2 suggest only a modest increase in poverty of 3 to 4 percentage points if a teen marries early. One possibility relates to the discussion above: those individuals affected by the instruments may have a larger treatment effect compared to the remaining early teen brides. While it would be interesting to know the

[26] Of course, the estimation uses a set of state marriage, compulsory schooling, and child labor laws as instruments. A similar idea to LATE still holds with multiple instruments, albeit with a slightly more involved interpretation of the IV estimate as a weighted average of the various treatment effects.

impact for all early teen brides, the subsample affected by the instrument is of particular interest, since these young women can be affected by policy changes in marriage laws.

Another explanation for the sizeable differences between the OLS and IV estimates is measurement error. Since the number of early teen marriages is so small, any mismeasurement of date of birth or date of marriage – the two variables used to construct age at first marriage – is likely to lead to a very large downward bias in the OLS estimate. With just a small amount of measurement error, the incorrectly classified teen brides will outnumber the true teen brides, resulting in substantial attenuation bias.

To see why this is so, consider the following hypothetical example. Suppose that 2% of women marry early and the poverty rates are 10% and 35% for those who marry late and early, respectively. Then the true effect of early marriage is a 25 percentage point increase in the poverty rate (assuming, for simplicity, that no selection bias exists). Now consider what happens if just 3% of all women are randomly misclassified as either marrying late or marrying early. Since there are so few true teen brides to begin with, almost all of the misclassifications are women who married later in life but are mistakenly recorded in the survey as teen brides. In this measurement error example, the observed poverty rates are now approximately 10% and 20% for women who marry late and early, respectively. In other words, the estimated effect of a teenage marriage is reduced from 25 to 10 percentage points. While this example is admittedly simple, it illustrates the important role of measurement error as an explanation for why the OLS coefficient estimates are so small. Systematic underreporting of women's ages in the Census or higher amounts of measurement error would bias the OLS coefficients even closer to zero.

5.3 Robustness Checks

Table 8 provides a variety of specification checks. Panel A instruments for early teen marriage as before, but treats dropout status as exogenous. The resulting IV estimates for both early teen marriage and dropout are very similar to the baseline results appearing in Table 6. This

indicates that the coefficient estimate on dropout status does not suffer markedly from selection bias once early marriage is properly instrumented for. In comparison, when neither early marriage or dropout status are instrumented for in Table 2, the coefficient estimate on dropout is 20% larger. The second panel (Panel B) instruments for dropout status, but treats early marriage as exogenous. The failure to instrument for early marriage dramatically shrinks the estimated coefficient on early marriage. The dropout coefficient falls somewhat, and its associated standard error increases enough that the effect is now only significant at the 10% confidence level.

What happens if we change the set of geographic controls or add cohort-specific age effects? Panel C adds in region of current residence dummies, i.e., dummies indicating where the woman currently lives. These dummies are in addition to the state of birth dummies which are included in every specification. These residence dummies have little effect on the IV estimates. Panel D adds in both region of residence dummies and interactions of region of residence dummies with the three census year dummies. This addition reduces the estimates for both the early marriage and dropout variables somewhat compared to baseline, although the effects are still large and significant. Panel E probes what happens when region of birth trends are *not* included in the regression. This exclusion increases the estimates for both the early marriage and dropout variables compared to baseline. Finally, Panel F allows for different age effects for each 5-year birth cohort. This robustness check does not affect the basic conclusion either.

The next two panels in Table 8 explore what happens when the set of instruments is expanded or contracted. I also collected state laws governing the age at which young men are allowed to marry with parental consent. The legislated minimum marriage age in a state for young men is usually higher than for young women (about 2 years higher on average), and the legal minimum for both has trended up over time. As might be expected, the laws governing early marriage for men and women are highly correlated within a state. Including these marriage laws for young men does not appreciably change the IV estimates in Panel G relative to baseline. Panel H removes the state laws regarding child labor from the instrument set, so that only the

early marriage laws for women and compulsory schooling laws remain. Using this restricted instrument set, the coefficient estimates on both the early teen marriage and dropout variables rise only slightly. In summary, the alternative specifications reported in Table 8 are generally supportive of the baseline results.

In the results presented so far, the dependent variable has been poverty, a binary outcome. I now explore the effect of early marriage and dropping out of high school on family income, a continuous outcome. While this variable arguably does not capture a family's financial well-being as accurately (since it does not account for family size or the number of children), it provides a useful robustness check. Table 9 begins by presenting individual-level and group-level OLS estimates similar to those found in column (4) of Table 2. Using individual-level data, OLS predicts that early teen marriage reduces family income by approximately $1,400 and that dropping out of high school reduces family income by approximately $6,700. In comparison, the OLS estimate based on grouped data switches sign (as it did for the poverty regressions reported in Table 2), but is not significantly different from zero. The aggregate OLS dropout estimate rises to approximately $8,300.

Turning to the IV results, the estimates for the dropout variable are similar for both levels of aggregation, and fairly close to the OLS estimates. In contrast, the IV estimates for early teen marriage (at either level of aggregation) are much larger than the OLS estimates. The general pattern of OLS versus IV estimates is similar to the findings presented in Tables 6 and 7, which use poverty as the outcome variable. This is perhaps not too surprising, since poverty and family income are closely related.

Before concluding, I briefly explore some possible mechanisms which might lead to increased poverty as a result of early teen marriage. Two outcomes linked to early marriage are an increased number of children and a higher divorce rate, both of which would lead to increased poverty, *ceteris paribus*. To document this association, I present OLS regressions which control for a variety of factors. A simple OLS regression of the number of children ever born to a women

yields an estimated coefficient of 0.88 (s.e.=0.02) on the early marriage variable and 0.71 (s.e.=0.04) on the dropout variable. This regression is based on individual-level data and controls for the full set of variables used in column (4) of Table 2. Similarly, an OLS regression with an indicator for whether a woman is currently divorced as the outcome variable yields an estimated coefficient of 0.044 (s.e.=0.003) on the early marriage variable and 0.020 (s.e.=0.002) on the dropout variable. These are just two factors; it is likely that several factors combine to increase the chances a woman who marries young ends up poor later in life.[27]

6 Conclusion

Do the negative effects associated with early teen marriage and dropping out of school reflect unmeasured characteristics or the true consequences of a teen's choices? To better understand the effect of women's early decisions on future life outcomes, this paper uses variation over time and across states in the laws which regulate early marriage, school attendance, and child labor. Using these laws as instruments for early marriage and high school completion, the results indicate strong negative effects on poverty status which are not due to self selection. The baseline IV estimates imply that women who marry young are 28 percentage points more likely to live in poverty when they are older. Similarly, women who drop out of school are 10 percentage points more likely to be in families below the poverty line. The IV results are robust to a variety of alternative specifications and estimation methods. In comparison, the OLS estimates are extremely sensitive to how the data is aggregated. For the early marriage variable, the individual-level OLS estimates likely suffer from substantial attenuation bias due to measurement error.

The results suggest that the decisions women make early in life can have long-lasting consequences. The IV estimates suggest that legal restrictions which prevent early marriage and

[27] IV regressions using the marriage, schooling, and labor laws as instruments yield imprecise, but somewhat larger estimates for the early marriage variable and estimates near zero for the dropout variable.

mandate high school completion have the potential to greatly reduce the chances of future poverty for a woman and her family. The implication is that legal restrictions on teenager's choices can reduce external costs imposed on society, and it is possible that they also prevent some teens from making decisions they will later regret.

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Table 1. Summary Statistics on Early Marriage, Education, and Poverty by Age Cohort.

		Cohort (when a woman was 15 years old)						
	All cohorts (1)	1935-1939 (2)	1940-1944 (3)	1945-1949 (4)	1950-1954 (5)	1955-1959 (6)	1960-1964 (7)	1965-1969 (8)
Age at First Marriage (%)								
12 to 15	4.4	4.3	4.8	5.1	5.8	5.1	3.7	2.5
16	4.5	4.0	4.3	5.7	5.7	5.3	3.9	3.4
17 to 18	19.2	15.3	17.5	21.8	22.8	21.5	18.7	17.6
Education (%)								
Dropout	26.8	40.2	36.8	32.5	28.0	22.7	17.4	14.3
High School graduate	41.4	39.6	41.2	43.0	44.3	43.3	40.8	38.2
Some college or more	31.8	20.2	22.0	24.5	27.7	34.0	41.8	47.5
Poverty Rate (%)								
Entire sample	10.2	10.3	9.3	9.6	10.1	9.9	10.6	11.4
If early marriage (age 12-15)	20.2	22.6	20.1	19.5	18.8	17.6	21.5	24.2
If dropout	20.4	17.7	16.9	18.6	20.8	21.8	25.0	29.1
Age (in years)	38.8	53.3	48.4	43.4	38.5	34.7	30.0	27.4
Sample size	3,256,434	448,307	448,470	417,182	417,666	461,183	565,595	498,031

Notes: Data are from the 1960, 1970, and 1980 U.S. Censuses. The sample is restricted to women between the ages of 20 and 60 who were born in one of the 41 states with available marriage, compulsory schooling, and child labor laws (see Appendix Table).

Table 2. OLS Estimates of the Effect of Early Teen Marriage and Dropping Out of High School on Poverty Using Individual and Grouped Data.

	Dependent Variable = Poor (1 = poor, 0 = not poor)			
	(1)	(2)	(3)	(4)
	OLS Estimates			
Early teen marriage	0.042**	0.036**	0.032**	0.031**
	(0.005)	(0.003)	(0.002)	(0.002)
High school dropout	0.134**	0.124**	0.122**	0.121**
	(0.009)	(0.007)	(0.007)	(0.007)
Control variables:				
Year, race, and age effects		X	X	X
State of birth and cohort of birth effects			X	X
Region trends				X
Observations	3,256,434	3,256,434	3,256,434	3,256,434
R-squared	0.042	0.079	0.082	0.082
	Grouped Data OLS Estimates			
Early teen marriage	0.784**	0.240**	-0.056	-0.127**
	(.127)	(0.080)	(0.037)	(0.043)
High school dropout	0.050**	0.146**	0.201**	0.150**
	(0.025)	(0.031)	(0.025)	(0.032)
Control variables:				
Year, race, and age effects		X	X	X
State of birth and birth cohort effects			X	X
Region of birth trends				X
Observations (number of cells)	3,567	3,567	3,567	3,567
R-squared	0.387	0.796	0.858	0.864

Notes: Standard errors in parentheses, adjusted for clustering by state of birth to account for arbitrary autocorrelation over time. Data are from the 1960, 1970, and 1980 U.S. Censuses. The sample is restricted to women between the ages of 20 and 60 who were born in one of the 41 states with valid marriage, compulsory schooling, and child labor laws (see Appendix Table). The dependent variable, poor, is a dummy equal to one if the woman currently lives in a family which is at or below the poverty line. Early teen marriage is defined as marrying between the age of 12 and 15 and high school dropout is defined as fewer than 12 years of completed schooling. Year effects are dummies for each census year (1960, 1970, 1980), race is a dummy for whether the respondent is white, and age effect is a cubic in the woman's current age. State of birth effects are dummies for each of the 41 states, and cohort of birth effects are seven dummies corresponding to five-year birth cohort intervals. Region of birth trends are separate linear birth-year trends for each of the four regions. In the "Grouped Data" panel, the data is aggregated to state of birth × year of birth × census year cell means.

**Significant at the 5% level, *Significant at the 10% level

Table 3. Summary of State Laws by Time Period.

	Time Period							
	All years (1)	1935-1939 (2)	1940-1944 (3)	1945-1949 (4)	1950-1954 (5)	1955-1959 (6)	1960-1964 (7)	1965-1969 (8)
Marriage Laws (%)								
common law	4.3	14.1	8.8	5.8	1.5	0	0	0
minimum marriage age ≤ 13	3.2	3.9	3.4	2.4	3.4	4.4	2.4	2.4
minimum marriage age = 14	16.7	21.5	18.1	17.1	17.1	17.1	14.2	11.7
minimum marriage age = 15	18.6	19.5	18.5	18.1	19.5	20.0	19.0	15.6
minimum marriage age ≥ 16	57.2	41.0	51.2	56.6	58.5	58.5	64.4	70.2
Compulsory Schooling Laws (%)								
compulsory attendance = 7	10.0	17.6	18.5	9.3	6.8	5.4	7.3	4.9
compulsory attendance = 8	47.3	58.0	57.1	57.6	47.8	42.4	36.6	31.7
compulsory attendance = 9	30.1	12.7	14.6	23.4	34.1	35.6	41.5	48.8
compulsory attendance ≥ 10	12.6	11.7	9.8	9.8	11.2	16.6	14.6	14.6
Child Labor Laws (%)								
child labor = 6	13.5	19.5	19.5	17.6	16.1	11.2	6.8	3.9
child labor = 7	21.5	22.0	22.0	22.4	21.0	25.9	19.5	18.0
child labor = 8	44.7	56.1	56.1	48.8	38.0	34.6	39.5	40.0
child labor ≥ 9	20.2	2.4	2.4	11.2	25.9	28.3	34.1	38.0
Sample size	1,435	205	205	205	205	205	205	205

Notes: The entries are the fraction of states with a specified law averaged over the five-year time interval. Sample size is the number of state-years; there are 41 states with laws available and 35 years, for a total of 1,435 observations. In the absence of a state law, common law prevails, which specifies a default minimum age of 12.

Table 4. The Relationship between Early Marriage, Compulsory Schooling, and Child Labor Laws across States.

	Marriage Laws (minimum marriage age)					
	common law	≤ 13	14	15	≥ 16	Row total
Compulsory Schooling Laws (%)						
compulsory attendance ≤ 7	8.4	15.4	16.1	30.1	30.1	100
compulsory attendance = 8	5.9	3.2	9.1	21.5	60.2	100
compulsory attendance = 9	1.9	0.0	29.4	8.8	60.0	100
compulsory attendance ≥ 10	1.1	1.1	14.9	22.1	60.8	100
Column Total	4.3	3.2	16.7	18.6	57.2	100
χ^2 test of independence [p-value]	225.0 [0.0001]					

	Marriage Laws (minimum marriage age)					
	common law	≤ 13	14	15	≥ 16	Row total
Child Labor Laws (%)						
child labor ≤ 6	12.4	13.9	12.9	17.5	43.3	100
child labor = 7	10.7	3.6	15.9	34.0	35.9	100
child labor = 8	0.6	1.3	14.5	16.8	66.8	100
child labor ≥ 9	0.3	0.0	24.8	6.9	67.9	100
Column total	4.3	3.2	16.7	18.6	57.2	100
χ^2 test of independence [p-value]	299.8 [0.0001]					

	Compulsory Schooling Laws (compulsory attendance)				
	≤ 7	8	9	≥ 10	Row total
Child Labor Laws (%)					
child labor ≤ 6	24.2	72.7	0	3.1	100
child labor = 7	17.8	47.6	23.0	11.7	100
child labor = 8	6.4	54.5	24.5	14.6	100
child labor ≥ 9	0	14.1	70.3	15.5	100
Column total	10.0	47.3	30.1	12.6	100
χ^2 test of independence [p-value]	441.3 [0.0001]				

Notes: The entries are the fraction of states with a specified combination of laws over all years. Sample size is the number of state-years; there are 41 states with laws available and 35 years, for a total of 1,435 observations. χ^2 test of independence is a test for the independence of the rows and the columns. In the absence of a state law, common law prevails, which specifies a default minimum age of 12.

Table 5. Pattern of Out-of-State Marriages by Restrictiveness of State Laws, 1968 and 1969 Vital Statistics Marriage Certificate Data.

	Married Outside State of Residence (%)				
Earliest Age a Woman Can Marry in Residence State with Parental Consent	Married in State of Residence (1)	State with Lower Minimum Age (2)	State with Equal or Higher Minimum Age (3)	Difference Column (2) – (3)	Observations [Weighted Obs.]
			$12 \leq$ Age at 1st Marriage ≤ 15		
13 or 14 years	94.6	0	5.4	-5.4	482
	(1.2)		(1.2)	(1.2)	[3,889]
15 years	94.7	3.9	1.4	2.5	581
	(1.0)	(0.9)	(0.5)	(1.0)	[3,842]
16 years	77.8	15.3	6.9	8.4	1,919
	(1.0)	(0.9)	(0.7)	(1.0)	[16,654]
			Age at 1st Marriage = 16		
13 or 14 years	94.1	0	5.9	-5.9	1,160
	(0.9)		(0.9)	(0.9)	[9,935]
15 years	93.7	2.1	4.2	-2.1	1,133
	(1.0)	(0.5)	(0.9)	(1.0)	[7,701]
16 years	88.0	3.2	8.8	-5.6	7,128
	(0.4)	(0.2)	(0.4)	(0.5)	[69,042]
			Difference in Difference		
13 or 14 years				0.5	
				(1.5)	
15 years				4.6	
				(1.4)	
16 years				14.0	
				(1.2)	

Notes: Standard errors in parentheses. Data collected from marriage certificates by the National Center for Health Statistics. The sample is restricted to first marriages of women who are residents of and get married in one of the 32 states which are in a Marriage-Reporting Area (MRA) and have information on marriage laws. The marriage certificate data includes all records for small states and a random sample for larger states; the probabilities above are weighted (unweighted probabilities are very similar).

Table 6. Baseline Instrumental Variables Estimates of the Effect of Early Teen Marriage and Dropping Out of High School on Poverty.

Second Stage	
	Dependent Variable = Poor (1 = poor, 0 = not poor)
Early teen marriage	0.282**
	(0.105)
Dropout	0.097**
	(0.028)
Observations	3,256,434

First Stage		
	Dependent Variable	
	Early Teen Marriage	Dropout
Marriage Laws (≤13 excluded)		
common law	0.001	0.010
	(0.004)	(0.011)
minimum marriage age = 14	-0.010**	-0.004
	(0.004)	(0.018)
minimum marriage age = 15	-0.014**	-0.014
	(0.003)	(0.015)
minimum marriage age ≥ 16	-0.007**	-0.007
	(0.003)	(0.017)
Compulsory Attendance Laws (7 excluded)		
compulsory attendance = 8	0.005*	-0.008
	(0.003)	(0.006)
compulsory attendance = 9	0.003	-0.028**
	(0.003)	(0.006)
compulsory attendance ≥ 10	0.009**	-0.012
	(0.003)	(0.009)
Child Labor Laws (6 excluded)		
child labor = 7	-0.003	-0.002
	(0.003)	(0.008)
child labor = 8	0.000	-0.005
	(0.001)	(0.006)
child labor ≥ 9	0.0025	-0.014
	(0.004)	(0.009)
F-statistic (state of birth clustering)	13.50	7.30
[p-value]	[.0001]	[.0001]
F-statistic (state of birth × year of birth clustering)	9.01	15.88
[p-value]	[.0001]	[.0001]
R-squared	0.021	0.102

Notes: Standard errors in parentheses, adjusted for clustering by state of birth to account for arbitrary autocorrelation over time. Two F-statistics are reported to illustrate the impact autocorrelation can have on this statistic; the first adjusts for birth state clustering and the second adjusts for state of birth × year of birth clustering. All regressions include year, race, age, state of birth, cohort of birth effects, and region of birth trends. See the notes to Table 2 for a description of the sample and variable definitions.

**Significant at the 5% level, *Significant at the 10% level

Table 7. LIML, Grouped IV, and Grouped LIML Estimates of the Baseline Model.

	Dependent Variable = Poor (1 = poor, 0 = not poor)
	LIML Estimates (using individual-level data)
Early teen marriage	0.297**
	(0.102)
Dropout	0.098**
	(0.035)
F-statistic for early marriage equation	13.50
[p-value]	[0.0001]
F-statistic for dropout equation	7.30
[p-value]	[0.0001]
Observations	3,256,434
	Grouped Data IV Estimates
Early teen marriage	0.283**
	(0.100)
Dropout	0.105**
	(0.034)
F-statistic for early marriage equation	12.36
[p-value]	[0.0001]
F-statistic for dropout equation	7.05
[p-value]	[0.0001]
Observations (cells)	3,567
	Grouped Data LIML Estimates
Early teen marriage	0.294**
	(0.105)
Dropout	0.105**
	(0.035)
F-statistic for early marriage equation	12.36
[p-value]	[0.0001]
F-statistic for dropout equation	7.05
[p-value]	[0.0001]
Observations (cells)	3,567

Notes: Standard errors in parentheses, adjusted for clustering by state of birth to account for arbitrary autocorrelation over time. First stage F-tests are also adjusted for state of birth clustering. All regressions include year, race, age, state of birth, cohort of birth effects, and region of birth trends. In the "Grouped Data" panels, the data is aggregated to state of birth × year of birth × census year cell means. See the notes to Table 2 for a description of the sample and variable definitions.

**Significant at the 5% level, *Significant at the 10% level

Table 8. IV Estimates of Alternative Specifications.

	Dependent Variable = Poor (1 = poor, 0 = not poor)
(A) Instrumenting for early teen marriage but not dropout	
Early teen marriage (treated as endogenous)	0.282**
	(0.099)
Dropout (treated as exogenous)	0.097**
	(0.010)
F-statistic for early marriage equation	17.34
(B) Instrumenting for dropout but not early teen marriage	
Early teen marriage (treated as exogenous)	0.048**
	(0.018)
Dropout (treated as endogenous)	0.081*
	(0.041)
F-statistic for dropout equation	10.44
(C) With region of residence effects	
Early teen marriage	0.282**
	(0.092)
Dropout	0.089**
	(0.035)
F-statistic for early marriage equation	13.84
F-statistic for dropout equation	7.43
(D) With region of residence and region of residence × year effects	
Early teen marriage	0.232**
	(0.108)
Dropout	0.084**
	(0.035)
F-statistic for early marriage equation	13.81
F-statistic for dropout equation	7.40
(E) Without region of birth trends	
Early teen marriage	0.387**
	(0.135)
Dropout	0.144**
	(0.035)
F-statistic for early marriage equation	4.37
F-statistic for dropout equation	7.20

Table 8 (continued). IV Estimates of Alternative Specifications.

	Dependent Variable = Poor (1 = poor, 0 = not poor)
(F) Birth cohort × age effects	
Early teen marriage	0.280**
	(0.091)
Dropout	0.098**
	(0.034)
F-statistic for early marriage equation	13.68
F-statistic for dropout equation	7.33
(G) Expanded instrument set (+ marriage laws for men)	
Early teen marriage	0.257**
	(0.091)
Dropout	0.093**
	(0.035)
F-statistic for early marriage equation	22.56
F-statistic for dropout equation	7.05
(H) Smaller instrument set (– labor laws)	
Early teen marriage	0.297**
	(0.099)
Dropout	0.110**
	(0.036)
F-statistic for early marriage equation	17.57
F-statistic for dropout equation	3.59

Notes: Standard errors in parentheses, adjusted for clustering by state of birth to account for arbitrary autocorrelation over time. First stage F-tests are also adjusted for state of birth clustering. All regressions include year, race, age, state of birth, cohort of birth effects, and region of birth trends. See the notes to Table 2 for a description of the sample and variable definitions.

**Significant at the 5% level, *Significant at the 10% level

Table 9. Estimates of the Effect of Early Teen Marriage and Dropping Out of High School on Family Income.

	Dependent Variable = Family Income (in thousands of dollars)
	OLS Estimates
Early teen marriage	-1.388**
	(0.074)
Dropout	-6.673**
	(0.134)
Observations	3,256,434
	Grouped Data OLS Estimates
Early teen marriage	0.554
	(2.814)
Dropout	-8.275**
	(1.286)
Observations	3,567
	IV Estimates
Early teen marriage	-28.804**
	(9.029)
Dropout	-6.850*
	(3.952)
F-statistic for early marriage equation	13.50
F-statistic for dropout equation	7.30
Observations	3,256,434
	Grouped Data IV Estimates
Early teen marriage	-27.874**
	(9.914)
Dropout	-6.683**
	(3.954)
F-statistic for early marriage equation	12.36
F-statistic for dropout equation	7.05
Observations	3,567

Notes: Standard errors in parentheses, adjusted for clustering by state of birth to account for arbitrary autocorrelation over time. First stage F-tests are also adjusted for state of birth clustering. All regressions include year, race, age, state of birth, cohort of birth effects, and region of birth trends. In the "Grouped Data" panels, the data is aggregated to state of birth × year of birth × census year cell means. See the notes to Table 2 for a description of the sample and variable definitions.

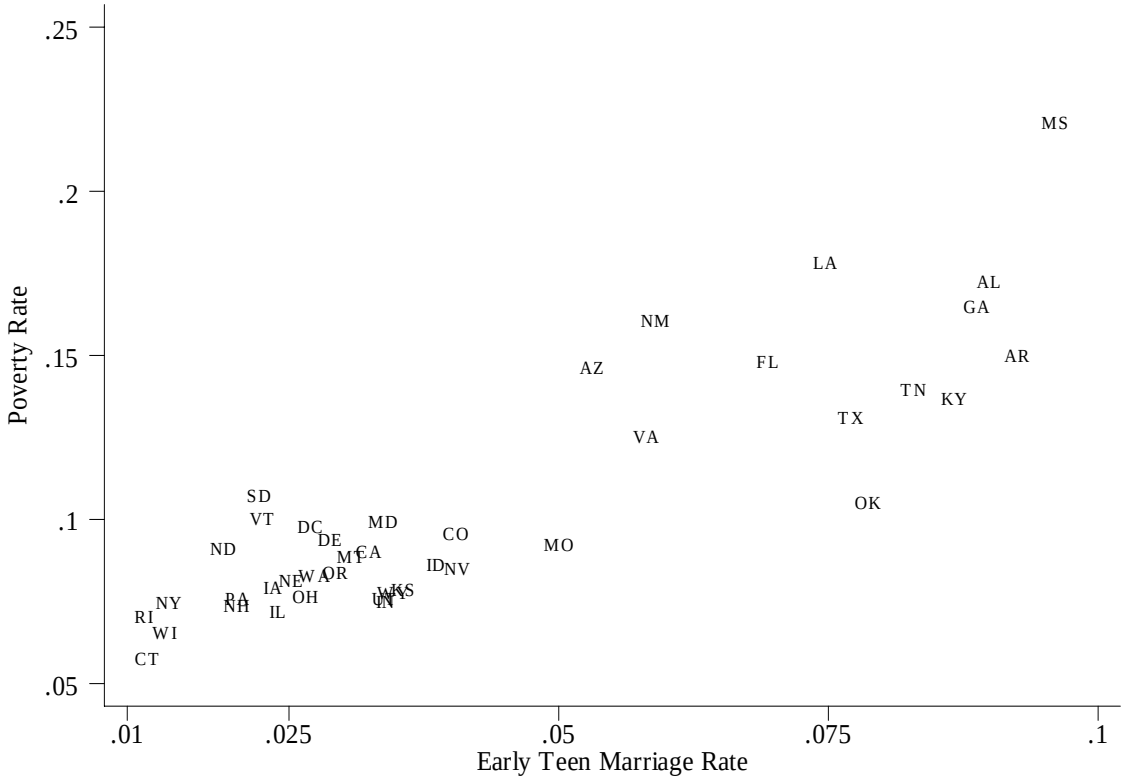
**Significant at the 5% level, *Significant at the 10% level

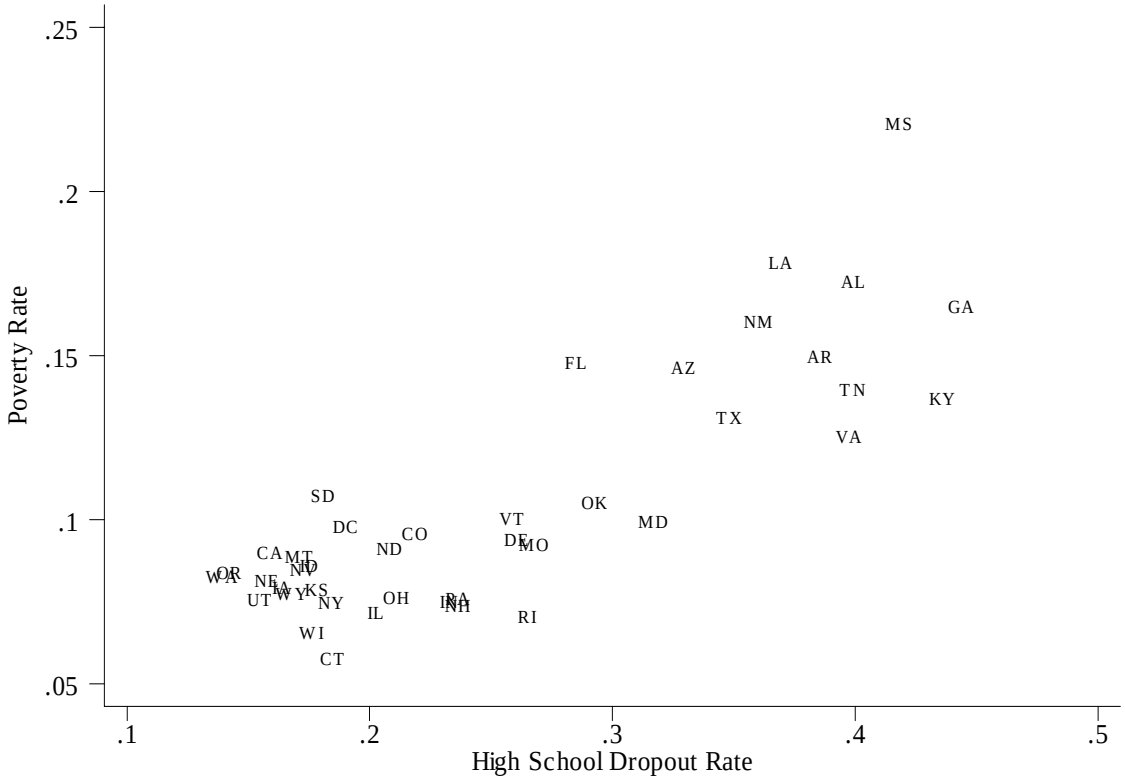
Appendix Table. Legal Minimum Marriage Age by State and Year.

	Year							
State	1935	1940	1945	1950	1955	1960	1965	1969
Alabama	14	14	14	14	14	14	14	14
Arizona	16	16	16	16	16	16	16	16
Arkansas	14	14	16	16	16	16	16	16
California	16	16	16	16	16	16	16	16
Colorado	12	12	12	16	16	16	16	16
Connecticut	16	16	16	16	16	16	16	16
Delaware	16	16	16	16	16	16	16	16
D.C.	14	16	16	16	16	16	16	16
Florida	12	16	16	16	16	16	16	16
Georgia	14	14	14	14	14	14	14	16
Idaho	12	12	12	15	15	15	15	15
Illinois	16	16	16	16	16	16	16	16
Indiana	16	16	16	16	16	16	16	16
Iowa	14	14	14	14	14	14	16	16
Kansas	16	16	16	16	16	16	16	18
Kentucky	14	14	14	14	14	14	16	16
Louisiana	12	12	12	16	16	16	16	16
Maryland	12	16	16	16	16	16	16	16
Mississippi	12	12	12	12	12	15	15	15
Missouri	15	15	15	15	15	15	15	15
Montana	16	16	16	16	16	16	16	16
Nebraska	16	16	16	16	16	16	16	16
Nevada	16	16	16	16	16	16	16	16
New Hampshire	13	13	13	13	13	13	13	13
New Mexico	16	16	16	16	16	16	16	16
New York	14	14	14	14	14	14	14	14
North Dakota	15	15	15	15	15	15	15	15
Ohio	16	16	16	16	16	16	16	16
Oklahoma	15	15	15	15	15	15	15	15
Oregon	15	15	15	15	15	15	15	15
Pennsylvania	16	16	16	16	16	16	16	16
Rhode Island	12	16	16	16	16	16	16	16
South Dakota	15	15	15	15	15	15	16	16
Tennessee	16	16	16	16	16	16	16	16
Texas	14	14	14	14	14	14	14	14
Utah	14	14	14	14	14	14	14	14
Vermont	16	16	16	16	16	16	16	16
Virginia	15	15	16	16	16	16	16	16
Washington	15	15	15	15	15	15	15	17
Wisconsin	15	15	15	15	15	15	16	16
Wyoming	16	16	16	16	16	16	16	16
Average	14.61	14.95	15.02	15.29	15.29	15.37	15.51	15.66

Note: Entries indicate the minimum marriage age for women with parental (or court) consent in the specified year. States which revised their minimum marriage age law are underlined. The following ten states do not appear in the table since they do not have available or consistent information on marriage laws: Alaska, Hawaii, Maine, Massachusetts, Michigan, Minnesota, New Jersey, North Carolina, South Carolina, and West Virginia.

Figure 1. The Relationship between Early Teen Marriage, High School Completion, and Poverty at the State Level.

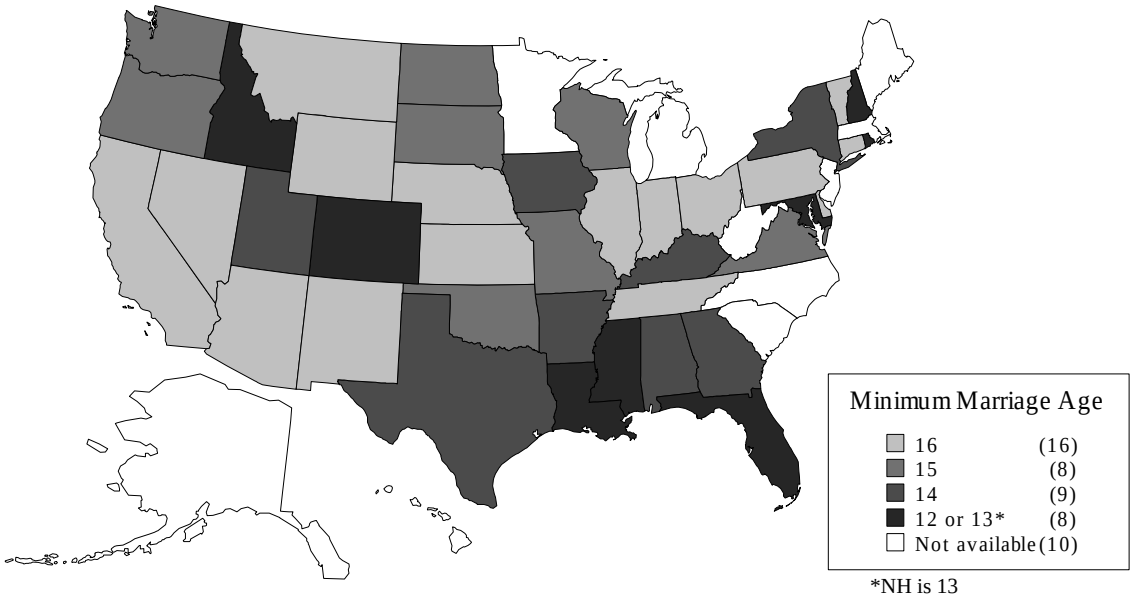




Note: Each state-level observation is an average from the combined 1960, 1970, and 1980 U.S. Census samples (see the notes to Table 2).

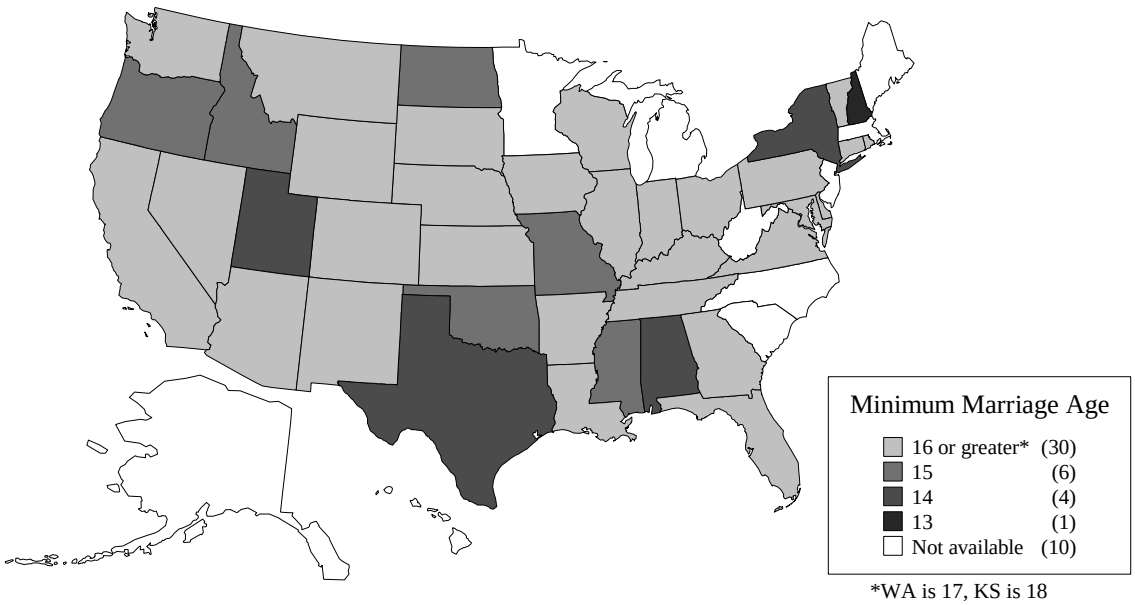
Figure 2. Minimum Legal Marriage Age by State for Women with Parental Consent, 1935 and 1969.

1935



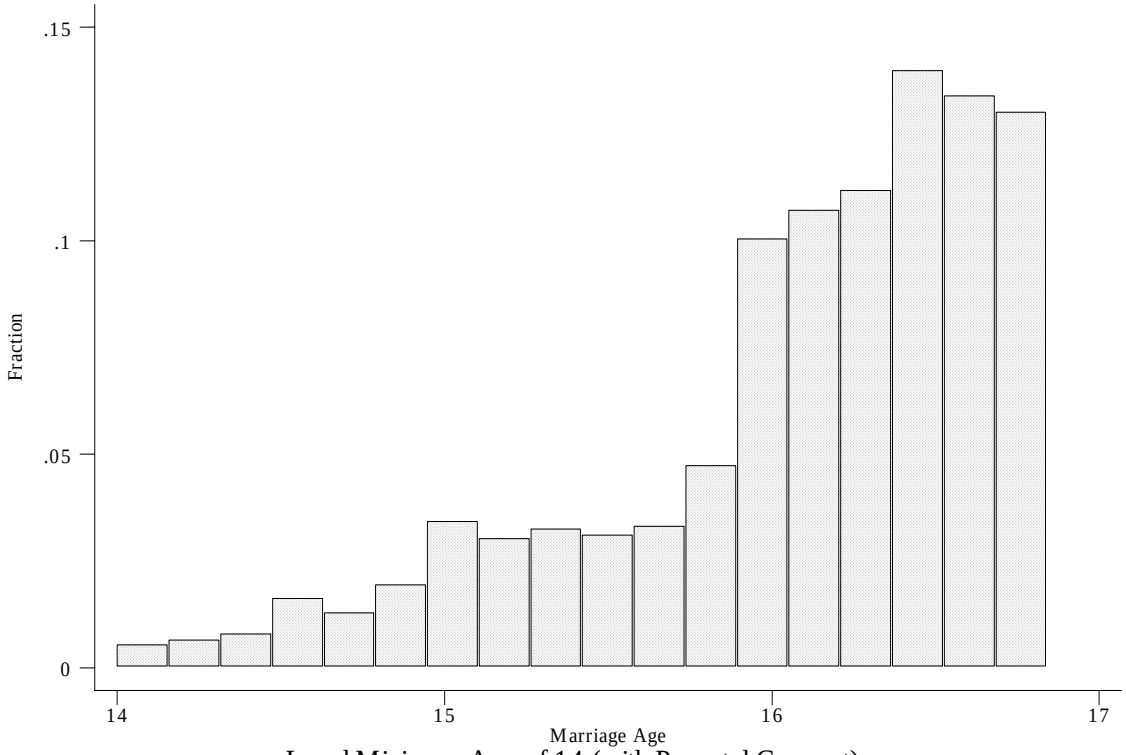
*NH is 13

1969

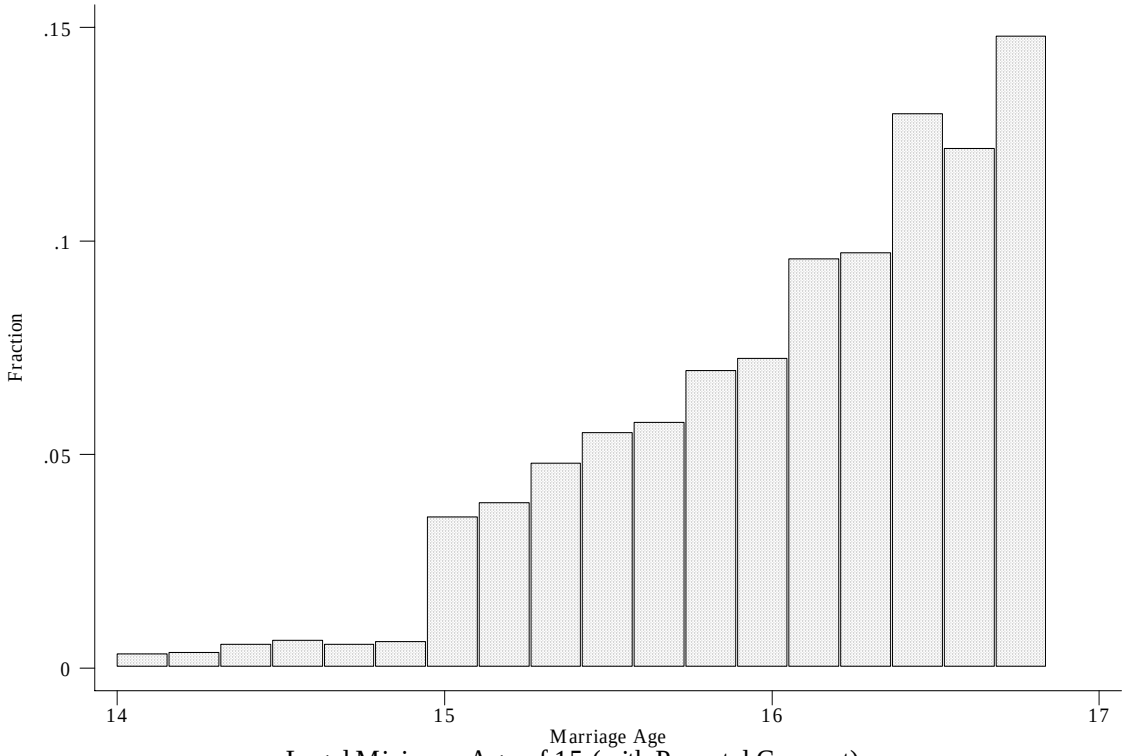


*WA is 17, KS is 18

Figure 3. The Timing of Marriages for Women by Type of State Marriage Law, 1968 and 1969 Vital Statistics Marriage Certificate Data.

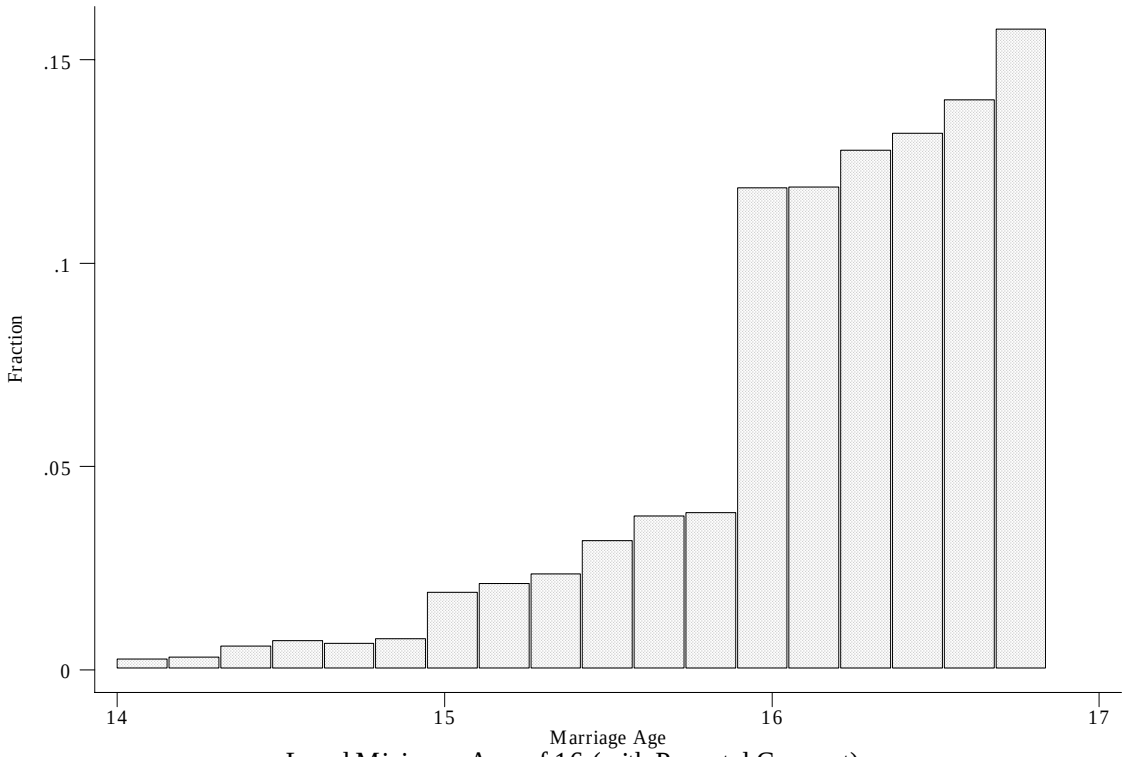


Legal Minimum Age of 14 (with Parental Consent)



Legal Minimum Age of 15 (with Parental Consent)

Figure 3 (continued). The Timing of Marriages for Women by Type of State Marriage Law, 1968 and 1969 Vital Statistics Marriage Certificate Data.



Legal Minimum Age of 16 (with Parental Consent)

Notes: Data collected from marriage certificates by the National Center for Health Statistics. Marriage rates are grouped in two month intervals. The sample is restricted to women who are marrying for the first time, who marry between the ages of 14 and 16, and who are residents of and get married in one of the 32 states which are in a Marriage-Reporting Area (MRA) and have information on marriage laws. The marriage certificate data includes all records for small states and a random sample for larger states; the probabilities above are weighted (unweighted probabilites are very similar). The 32 states included in this figure have the following minimum marriage age with parental consent in 1968 and 1969 for women: 13 years: New Hampshire (included with the 14-year age minimum states in the first graph); 14 years: Alabama, New York, Utah; 15 years: Idaho, Mississippi, Missouri, Oregon; 16 years: California, Connecticut, Delaware, District of Columbia, Florida, Georgia, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Montana, Nebraska, Ohio, Pennsylvania, Rhode Island, South Dakota, Tennessee, Vermont, Virginia, Wisconsin, Wyoming.