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EARLY TEEN MARRIAGE AND FUTURE POVERTY

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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 31 percentage points more likely to live in poverty when she is older. Similarly, a woman who drops out of school is 11 percentage points more likely to be poor. The results are robust to a variety of alternative specifications and estimation methods, including LIML estimation and a control function approach. While grouped OLS estimates for the early teen marriage variable are also large, OLS estimates based on individual-level data are small, consistent with a large amount of measurement error.

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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 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 31 percentage points more likely to live in poverty when she is older. Similarly, a woman who drops out of school is 11 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 and a control function approach. In comparison, the OLS estimates are very sensitive to how the data is aggregated, particularly for the early marriage variable. OLS estimation on individual-level data indicates a small effect for early teen marriage, while OLS estimates using grouped data are also large.

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 specification and robustness checks. It also provides a discussion of measurement error issues and a reconciliation with the teenage childbearing literature. Section 6 concludes.

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

[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).

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 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. 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 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.

While issues of causality 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. Early research using OLS reveals large and significant consequences associated with teenage childbearing (Moore and Waite, 1997). Subsequent approaches which attempt to deal with selection bias reach disparate conclusions. For example, studies using a variety of instrumental variables by Angrist and Evans (1996), Grogger and Bronars (1993) and Klepinger, Lundberg, and Plotnick (1999) conclude that teenage childbearing has negative consequences. However, Geronimus and Korenman's (1992) paper using sister fixed effects and the research by Hotz, McElroy, and Sanders (1997, 2005) and Hotz, Mullin, and Sanders (1997) using random miscarriages as an instrument find little evidence of a negative effect. The debate is ongoing, with recent work by Ashcroft and Lang (2007) and Fletcher and Wolfe (2008) using variations on the miscarriage instrument and finding negative effects.

3 Data and OLS Estimates

3.1 Data

The data for this paper combines information on state-specific marriage, schooling, and labor laws with data from the 1960, 1970, and 1980 U.S. Decennial Censuses.[4] Supplementary data is obtained from Vital Statistics marriage certificate data and the National Fertility Surveys. The U.S. Census data are ideal for obtaining precise information about teenage marriage at the

[4] 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.

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 further restricted to women who were between the ages of 20 and 60 when the Census was taken and who were born in the U.S.[5] Data is also restricted to the 41 states with available data on marriage laws, compulsory schooling laws, and child labor laws (these laws will be discussed in Section 4.1).

The Census data reveal 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, 3.5% of women report first marrying under the age of 16.[6] This compares to 11.9% of women first marrying at ages 16 or 17 (4.5% at age 16 and 7.4% at age 17). The top two series in Figure 1a graph the fraction of women marrying at these ages over time. The fraction of early teen marriages starts out at 3.8% for the 1935 cohort, reaches a peak of 4.9% in the early fifties, and then declines to 1.6% by the end of the sample. This pattern is mirrored for the fraction of women marrying at age 16 or 17.

To put these patterns into perspective, the bottom series in Figure 1a graphs the median

[5] 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.

[6] Age at first marriage is calculated from each woman's date of first marriage and date of birth. In 1980 valid responses included ages as low as 12; in 1960 and 1970, the lowest valid response was age 14.

age at first marriage for a long time horizon. The plot reveals that the time period of interest in this paper (1935 to 1969) corresponds to a period in history when marriage ages were remarkably low by historical standards. The median age at marriage for women fell almost two years from the start of the century to reach a low of 20.1 in 1956. Since the 1970s, the median age has risen dramatically, so that by the end of the nineties, the median age was 25.

Figure 1b plots other well-known secular trends which were also occurring in the middle of the 1900s. There was a sharp decline in overall fertility during WWII, followed by a dramatic increase in the post-war period. Fertility starts to decline by the end of the fifties, reaching a low in the late seventies where the rate is half that of the peak. The trends in the top half of Figure 1b – couples starting their marriages and childbearing sooner in the post-war period – are recognized as being largely responsible for the baby boom between 1946 and 1964. Similarly, the baby bust resulted from delayed marriage and fertility. The same fertility pattern holds for teenage childbearing as well. At the peak in 1957, there were 96 births per 1000 15-19 year old women in the U.S.[7] Comparing Figures 1a and 1b, it is apparent that the trends in early teen marriage coincide with those for teen childbearing.

To add further perspective, the bottom half of Figure 1b plots marriage and divorce rates over time. Marriage and divorce rates fell during the Great Depression and spiked following WWII. Divorce rates also rose sharply starting in the late 1960s before reaching a plateau in the 1980s. Further insights into the changes in and possible causes of these dramatic shifts in marriage and divorce can be found in Stevenson and Wolfers (2007).

These secular trends have several implications for the current study. First, it will be important to allow for different effects by time period. In the regression analyses which follow, separate dummies will be included for year of birth, current age (in year intervals), and census

[7] The fertility rate for 15 to 17 year old women is available starting in 1960. From 1960 to 1998, the 15-17 year old rate is 59% of the 15-19 year old rate on average, and the two series roughly have the same time pattern.

year. Second, the findings should be interpreted in the appropriate historical context, as the cultural, legal, and economic environment is very different today.

In addition to early marriage, another key variable for the analysis is the high school dropout rate. Carter et al. (2006) document that dropout rates have fallen over time. At age 19, 52.3% of women born between 1921 and 1930 have not finished high school. This percentage falls monotonically over time, so that for women born between 1971 and 1980, only 15.1% of 19 year old women have not finished high school.[8] Completion rates rise as women age and have the opportunity to go back to school; for example, women in their thirties who were born between 1921 and 1930 have a dropout rate of 42.9%.

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. Approximately 10% of all observations in my sample are classified as poor, with this rate more than doubling for those who marry before the age of 16.

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. While individuals can enter and exit poverty throughout the lifecycle, Bane and Ellwood (1986) find that "the majority of poor persons at any time are in the midst of a rather long spell of poverty." In addition, Rank and Hirschi (2001) find that once poverty occurs it is likely to occur again.

The large fraction of early teen marriages for the women in our sample is ideal for the

[8] The dropout rate at age 19 for women is 52.3% for the 1921-30 birth cohort, 43.1% for 1931-40, 36.7% for 1941-50, 25.9% for 1951-60, 20.7% for 1961-70, and 15.1% for 1971-80 (Carter et al., 2006).

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.

3.2 OLS Estimates

How are poverty, early teen marriage, and dropping out of high school related? I begin by presenting OLS estimates of the effect of early teen marriage and dropout status on poverty. The top panel of Table 1 displays results for 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 in columns 2-4 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 small effect.

In contrast to the individual-level estimates, the grouped data results in the bottom panel of Table 1 present a very different picture. In the bottom panel, the data is aggregated to state of birth × year of birth × census year cell means. In contrast to OLS, the estimates in columns 1-4 are much larger and the inclusion of controls affects both the dropout and teenage marriage coefficients. After including controls for (i) census year, race, and current age dummies, (ii) state of birth and birth cohort dummies, (iii) and region of birth trends, the coefficient on early teen marriage is 26.4 and on dropout is 12.9. These coefficients are large; they imply a 26% increase in future poverty for early teen brides and a 13% increase for those who do not finish high school. A key question is whether there are additional omitted variables which would drive either of these

coefficients closer to zero, or whether these estimates represent causal effects.

Column 5 expands the sample to include allocated observations. The Census Bureau allocates values for age at first marriage when data are missing or inconsistent. First, a logical edit is performed if possible, using information from other variables and other household members. When this isn't possible, the Census uses a hot deck allocation method to assign a value from an individual with similar characteristics.[9] Allocation rates are much higher for early teen marriages compared to the rest of the sample, especially in 1980. Additionally, the hot deck procedure used in 1980 (and to a lesser extent in 1960 and 1970) suffers from bracketing issues for early teen marriages, with sharp spikes in marriage rates occurring for women whose current age is a multiple of five. When these allocated marriages are included in column 5, the coefficient on early teen marriages drops, particularly in the grouped OLS panel. As we shall see later, these allocated marriages do not have much of an impact on the IV estimates, suggesting that these allocated marriages are largely noise. Therefore, unless otherwise noted, all allocated marriages are dropped from the data.

What explains the different estimates for early teen marriage when comparing the individual versus grouped data in Table 1? Aggregation should not affect the estimated relationship as long as the model is correctly specified and the coefficient of interest is the same for all individuals (Theil, 1954). When the model is not correctly specified, aggregation has the potential to do two things: (i) minimize attenuation bias arising from noisily measured covariates, and (ii) either minimize or exacerbate the effects of selection bias. We will explore these issues, including the role of measurement error and heterogeneous effects, later on in the paper. If appropriate instruments can be found, misspecification due to omitted variables or measurement

[9] For example, in the 1980 Census, entries for the age at first marriage variable "were allocated from one of seven matrices in which reported marital history responses were stored. The matrices contained data on various combinations of characteristics, such as age, marital status, sex, the presence or absence of a spouse, the presence of the person's own children in the household, and the number of times the husband or wife had been married" (Census Bureau, 1981).

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 remarkably similar individual-level IV and aggregate IV estimates.

4 State Laws and Their Effect on Early Marriage and Schooling

4.1 State Marriage, Schooling, and Labor Laws

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.[10]

[10] 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,

There are two sets of laws specifying minimum age requirements for marriage. The first 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 during the period of my data. Prior to 1971, approximately 80% of states specified an age of 18 for marriage *without* parental consent for women and approximately 85% specified an age of 21 for men. In 1971, men and women were granted the right to vote at age 18, which seems to have spurred most states to change their statutes for the legal age without parental consent for both men and women to age 18 (for a discussion of these laws, see Blank, Charles, and Sallee, 2007).

Focusing on the laws with parental consent, 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, in most states, the marriage law specifies that the courts have the right to grant exceptions to women based on "moral" and "welfare" arguments (as explained in the footnotes to tables in the *World Almanac and Book of Facts*, various years). These statutes imply that a judge could grant permission for an early teenage marriage if the teenage woman was pregnant. How often judges actually granted exceptions is hard to know ex post, but given the relatively low rate of illegitimate births and abortions during much of this period, exceptions for pregnancy were probably common.

It is important to note that 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

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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.

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 (i) the required years of schooling before dropping out and (ii) the difference between the minimum dropout age and the maximum enrollment age (lagged 8 years). Child labor is defined as the *maximum* of (i) the required years of schooling before receiving a work permit and (ii) 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 for her birth state which are in force when she would have been age 15. I assess the potential bias of this assignment rule in Section 5.2.

Table 2 summarizes the changes in these laws across five-year time periods (in the regression analysis, year-by-year values are used). For the period 1935 to 1939, 41% of states specified that a woman had 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 3 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 7 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 8 or more.[11] Later on in the paper, we will also investigate the impact of divorce and unilateral divorce laws. These laws are less tightly tied to the marriage laws, partly because few states enacted unilateral divorce until the 1970s (only 4% of the state-years in our sample period).

[11] 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.

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.

4.2 The Impact of State Laws on Early Teen 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 before age 16). In states with a legal minimum of 12-13, 14, 15, and 16+, the percent of women who are early teen brides is, respectively, 6.5%, 4.3%, 3.5%, and 2.9%.[12] 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

[12] 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 Figure 1.

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.[13] 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 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.[14] 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

[13] 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 Table 4.

[14] 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.

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 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.[15] 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,

[15] 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.

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.[16]

Table 4 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 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 4 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 4, 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

[16] 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.

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.[17] 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 the full set of controls appearing in the baseline IV specification in Table 5). 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=0.44), while the F-statistic for the effect of current laws is 14.6 (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

[17] 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.

bottom panel in Table 5 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 mirror those used in column 4 of Table 1, and are described in the table.

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 0.5 to 1.1 percentage points more 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 child labor laws seem to work in the opposite direction—more restrictive child labor laws actually increase the probability of an early marriage. A woman born in a state with a child labor law of 9 or greater has 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 working, are more limited. The third set of laws which deal with compulsory schooling are smaller and less significant.

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. 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.[18] 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

[18] 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.

are not statistically significant.

For all of the estimates, F-statistics are reported for the joint significance of the instruments. The F-statistic is 15.68 for the early teen marriage equation and 5.44 for the dropout equation. 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.[19]

5.2 Baseline IV Results

The top panel of Table 5 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 30.6 percentage point increase in the probability of living in poverty. Dropping out of high school is associated with an 11.2 percentage point increase in poverty.

I now present a series of alternative estimation approaches to assess the robustness of the baseline result. Table 1 revealed that aggregation made a large difference for OLS estimates: the individual-level results suggested a small impact of early teen marriage on poverty, while the grouped-level OLS estimates suggested a large effect. The first column in Table 6 repeats the baseline IV analysis, but this time with grouped data. The data is aggregated at the state of birth × year of birth × census year level. The grouped data IV estimates are remarkably similar to the individual-level IV estimates (0.314 versus 0.306 for early teen marriage and 0.112 versus 0.112 for dropout). The similarity of the coefficient estimates is not surprising, since the instruments

[19] Failure to adjust the standard errors in the first stage and the corresponding F-statistics makes a large difference in the current analysis. Without clustering, the F-statistics for the dropout equation rise to 29.22 and for the marriage equation to 22.35. Clustering at the state of birth × year of birth level, the F-statistic for the dropout equation is 12.96 for the dropout equation and 15.48 for the marriage equation.

are constant for all individuals in a state-cohort group, effectively aggregating both the individual-level and group-level estimates. The standard errors also change very little when using the grouped data, increasing by about 10% for early teen marriage and not at all for dropout. Since the aggregated data produces very similar point estimates and slightly more conservative standard errors, in what follows, I present results for aggregated data unless otherwise noted.[20]

As is well known, 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). The first stage F-statistics appearing in Table 5 are significant, but of moderate size. To help assess whether weak instruments might be biasing the results, the first column in Table 6 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).[21] The LIML results are virtually identical to the grouped IV estimates. This suggests that weak instruments are not a major issue for estimation.

The next task is to assess the impact migration has on the assignment of state laws for marriage, schooling, and work and the subsequent IV estimates. As a reminder, laws are assigned based on a woman's state of birth, although ideally we would like to use the state a woman lived in at age 15. Since some women have migrated out of their birth state and into a state with a different set of laws by age 15, the instruments are measured with error. I assess how this affects the IV estimates in column 3 of Table 6.

[20] Bertrand, Duflo, and Mullainathan (2004) show that clustering does not always do a good job correcting the standard errors if the within-group sample is large. Aggregating the data should produce more conservative standard errors in such situations.

[21] 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.

To see how I examine the issue, notice the expected value of the ideal (but unobserved) state laws can be calculated if migration probabilities are known. Let z_{ij}^{*} be a dummy variable indicating the state law woman *i* faces at age 15, given she was born in state *j*. The asterisk indicates this variable is not observed, since she may have moved from her birth state by age 15. However, if migration probabilities are known, we can calculate the expected value of this variable as

$$E[z_{ij}^{*}] = \sum_{k} p_{jk} w_k$$

where p_{jk} represents the probability that a woman will live in state *k* at age 15 given she was born in state *j* and w_k is the law in force in state *k* for the relevant year. The same logic applies when there are several variables for state laws, such as the instruments used in Table 5.

It is straightforward to show that substituting in $E[z_{ij}^{*}]$ for z_{ij}^{*} yields consistent estimates in an IV framework. The remaining issue is how to consistently estimate the conditional migration probabilities, p_{jk}. While this information is not available for all women, we can estimate the migration patterns for women who are age 15 at the time of the census enumeration. This is because the Census records both state of birth and state of current residence. I use 15-year old women in the 1960 Census to estimate these migration probabilities. I then calculate the expected value of the laws based on the state a women lived in at age 15 as outlined above, and use these expected laws as instruments.[22] The migration adjusted estimates in column 3 of Table 6 are very similar to the baseline estimates, indicating the assignment of state marriage laws based on state of birth is a reasonable approach.

If there is heterogeneity an individual's "returns" to marrying young or dropping out of

[22] This implicitly assumes that migration patterns have not changed over time. As a check on this assumption, I alternatively used 15 year olds from the 1970 Census, and the IV estimates are very similar (.317 for early teen marriage, and .111 for dropout). Expanding the age window to 14-16 year olds also yields similar estimates.

school, the assumptions needed for IV to consistently estimate an average treatment effect are stronger (Björklund and Moffit, 1987; Card, 1999, 2001; Heckman and Vytlacil, 1998; Willis and Rosen, 1979; Wooldridge, 1997). In the current context, a sufficient set of conditions is the instruments are independent of (i) the individual returns to marrying young and dropping out of high school, (ii) any individual-specific intercept term in the outcome equation, and (iii) the reduced form residuals in the first stage early marriage and dropout equations (see Heckman and Vytlacil, 1998).

To assess the impact of heterogeneous returns, I pursue a control function approach similar to the one proposed by Garen (1984) and discussed by Card (2001). The basic idea of a control function approach is to make some assumptions about the relationship between the observed variables (controls and instruments) and the individual-specific returns and individual-specific intercept term. One then includes additional terms in the outcome regression to control for these relationships. The Appendix details the assumptions and estimating equation. The resulting control function estimates appear in column 4 of Table 6. Compared to the baseline IV estimates, the early marriage estimate is approximately 10% smaller and the dropout estimate is approximately 10% larger. These results suggest that heterogeneity across individuals plays a minor role in estimation of the average treatment effect.

5.3 Additional Estimates

To further investigate heterogeneity in the returns to marrying young and dropping out of school, Table 7 presents additional OLS estimates. Panel (A) estimates the poverty equation separately for blacks and whites. The OLS estimates suggest that dropping out of school is associated with a much larger penalty for blacks compared to whites, while the early marriage coefficient is similar for both races. The estimates by region of country in Panel (B) suggest somewhat larger coefficients for both early marriage and dropping out, although the estimates are imprecise.

Panel (C) shows how the estimates differ based on year of birth and age cohorts. The

first column interacts the early teen marriage and dropout variables with three age cohorts. The coefficient for early teen marriage is much larger for the earliest birth cohort (born between 1920 and 1930). In contrast, the dropout coefficient is smallest for the earliest birth cohort. Column 2 performs the same exercise, but with current age cohort interactions instead. Both early marriage and dropping out of school have the largest effect for women in their twenties. Column 3 includes both sets of interactions at the same time. The same general patterns hold in this regression as well.

The first two panels in Table 8 present additional IV estimates by race and region of country. The IV estimate of the early teen marriage effect for the black sample is 0.46, which is much larger than the baseline estimate or the estimate for the white sample (although the estimate for whites is imprecisely estimated and not significant). The marriage instruments also have more power for the black sample than the white sample. The dropout coefficients are similar for whites and blacks, but statistically insignificant for blacks. When looking at estimates by region of the country, it becomes clear that most of the identification is coming from Southern states, which is not surprising given that much of the variation in laws occurs in this region of the country. Interestingly, the dropout coefficients fall for both the black and white samples. Taken together, these OLS and IV estimates suggest heterogeneous returns to marrying young and dropping out of school, although the estimates are sometimes imprecise.[23]

The last three panels in Table 8 present additional robustness checks for the IV estimates. This paper has focused on the laws governing marriage with parental consent for women. There are also laws specifying the minimum marriage age without parental consent for women and laws for men. As discussed earlier, there is little variation in the laws without consent for either women or men before 1970, so we cannot effectively use these to instrument for marriages at later ages. The laws for men with parental consent are highly correlated with the laws for women

[23] While it would be interesting to also have IV estimates by birth and age cohorts as in Table 7, this is not feasible in practice as there is not sufficient variation in the instrument set.

with parental consent, and are usually two years higher. In Panel (C) we use all of the marriage laws, for men and women, with and without consent, as instruments. These additional instruments result in modest increases in the IV estimates for both the early marriage and dropout variables.

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. The effects of early marriage and dropout status on family income are large, presenting a similar picture to the poverty regressions. An additional robustness exercise includes observations where the age at first marriage variable was allocated by the Census Bureau. Including these observations had a large impact on the OLS estimates appearing in the bottom panel of Table 1. In contrast, the IV estimates are robust to the inclusion or exclusion of these allocated observations.

As a final exercise, in Table 9 I investigate the effect of divorce on poverty. I begin by presenting estimates similar to those in column 4 of Table 1, but with an additional variable for whether a woman is currently divorced. The estimated effect is substantial. Current divorce is associated with a 21.5% increase in the probability of poverty, an effect similar in magnitude to the estimated effect of an early teen marriage. In this regression, the early teen marriage coefficient falls slightly compared to Table 1, from 26.4% to 23.5%. The IV estimate in column 2 instruments for early teen marriage and dropout status using the same specification as column 1 of Table 6, but also adds in the currently divorced variable as an additional control. The IV estimate for early teen marriage falls to 26.4% (compared to 31.4% in Table 6), suggesting that at least part of the penalty for an early teen marriage arises through a divorce channel. The divorce coefficient also falls in this specification compared to Table 1, but remains large.

Since divorce might not be exogenous, it would be useful to instrument for this variable

as well. Previous research has analyzed the effect of changes in divorce laws on divorce rates and stocks (Friedberg, 1998; Parkman, 1992; Peters, 1986; Wolfers, 2006). Research in this area has looked at the impact of divorce laws on outcomes such as labor supply, suicide, investment in marriage specific capital, children's well-being, and family distress (Gray, 1998; Gruber, 2004; Stevenson, 2007; Stevenson and Wolfers, 2006). One possibility for the current paper is to use these divorce laws as instruments.

I follow the approach taken by Wolfers (2006) and Gruber (2004) and use unilateral divorce laws as an instrument for the stock of divorces. I assign divorce laws based on current state of residence (using Gruber's coding), which necessitates the use of individual-level data. The divorce coefficient appearing in column 3 of Table 9 is negative, but not statistically significant. The early marriage coefficient rises to 38%, while the dropout coefficient does not change much compared to the baseline estimate. While the results of this exercise are interesting, they should be interpreted with caution, as most of the changes in divorce laws, as well as most of the rise in divorce rates, occurs after my sample period (see Figure 1).

5.4 Discussion

The IV estimates indicate the causal effects of early teen marriage and dropout status on future poverty are substantial. The baseline estimates imply that marrying young increases the chances a young bride will end up in poverty later in life by around 31 percentage points. Dropping out of high school has a somewhat smaller, but still substantial, 11 percentage point effect on future poverty. These results are robust to the level of aggregation, LIML estimation, corrections for migration, and a control function approach. The individual-level OLS estimates for early teen marriage are small, while aggregated OLS estimates yield an estimate which is of the same magnitude as the IV estimates.

To better understand why the IV and aggregated OLS estimates differ so much from the individual level OLS estimates, I now explore the role of measurement error. In Census data, age at first marriage is calculated from the reported date of first marriage and date of birth (month and

year). Note that usually only one person fills in the Census form for the entire household. This is likely to exacerbate measurement error, as the person completing the census may not have accurate information about other household members' dates of birth and first marriage. In 1970 and 1980, the Census form instructs individuals to "give your best estimate" when either of these dates is "not known."[24]

Since the fraction 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 of the early teen marriage coefficient. With just a small amount of measurement error, the incorrectly classified teen brides can outnumber the true teen brides, resulting in substantial attenuation bias.

To better understand the prevalence of measurement error in reported dates, consider the 1975 National Fertility Survey (NFS). This was the fifth in a series of surveys conducted every five year examining marital fertility and family planning. The interesting feature of the survey in 1975 is that the researchers chose to re-interview a selected sample of women from the 1970 survey. The re-interview sample includes 2,355 white women in their first marriages who were continuously married, whose age at marriage was less than 25 years, and whose husbands had also been married only once. Both the 1975 and 1970 surveys ask date of birth and date of first marriage, with both sets of answers being recorded in the 1975 sample. So for this subset of women, I can calculate a lower bound on misreports by comparing the same woman's answers over a 5 year time horizon.

Table 10 tabulates how often the responses from the 1975 survey do not match with the responses from the 1970 survey. For the entire sample, dates of birth do not concur 4% of the time and dates of marriage do not concur 12% of the time. The result is that a woman's age at

[24] Research finds several sources for misreporting of date of birth, including ignorance, miscommunication, distortion to meet preconceived social norms, and errors in processing (Mason and Cope, 1987). These same measurement error issues are likely compounded for reports of date of first marriage.

marriage measured in years will not line up for 11% of the observations in the sample. Mismatch rates are also reported in Table 10 for groupings based on a woman's reported marriage age in the 1970 survey. For those marrying at or below the age of 15 in the 1970 survey, 8% of birth dates and 35% of marriage dates do not align across survey years. For this group, 39% of the implied marriage ages differ across the surveys. While misreporting generally declines as a woman's reported marriage age rises, the amount of error in these other groups is modest in comparison. The amount of measurement error in the Census is likely to be even larger, since the 1975 NFS only samples women who have never divorced and has women answer questions about themselves.

Measurement error can plausibly explain the difference between the individual OLS estimates and the grouped OLS or IV estimates. One remaining question is why the grouped OLS and IV estimates are so large. The results in Table 9 put some perspective on the size of the effect: early teen marriage results in an increase in poverty which is on par with the effect of divorce (when treated exogenously) or almost three times the effect of dropping out of school. These effects are larger than those found in much of the literature for teenage childbearing discussed earlier. How can the current results be reconciled with that literature?

There are at least two reasons why the estimated effect of early teenage marriage might not be comparable to the effects estimated in for teenage childbearing in the literature. First, the sample periods differ greatly. Most of the research on teenage childbearing is for births occurring in the 1970s or later, since these studies often use NLSY or PSID data. In contrast, the current paper focuses on women who were age 15 between 1935 and 1969. Comparing the two time periods, there are large differences in access to birth control and abortion, social norms, and labor market opportunities for married women and women with children. Birth control began to be widely available to young, single women starting in the late 1960s and had large effects on women's career and marriage decisions (Goldin and Katz, 2002). Abortion also became legalized in the early 1970s, first in select states, and then nationwide with Roe v. Wade in 1973. To

highlight one change in what was socially acceptable over time, consider illegitimacy rates, which rose from 3.9% in 1950, to 10.7% in 1970, to 28.0% in 1990 (National Vital Statistics Reports, 2002). Female labor force participation rates for married women also increased dramatically over this time period, steadily increasing from 15% in 1940 to over 50% by 1980 and almost 75% by 1990 (Goldin, 2006). In sum, there were many changes starting in the 1970s which could make teenage motherhood after that period not comparable to my sample of early teen brides earlier in the century.

The second reason for why the estimated effect of early teen marriage is so large compared to the estimates for teen childbearing is that this paper looks at a sample of particularly young teenagers – those marrying at or before the age of 15 – while the teen childbearing literature typically examines the effect of births to teenagers less than or equal to age 19. There may be a large difference between marrying (or having a child) at or before the age of 15 versus between the ages of 16 and 19.

Some of these differences can be highlighted using the 1965 and 1970 National Fertility Surveys.[25] Women who marry at age 15 or younger divorce within five years of marriage 18% of the time, compared to 12% for women who marry at age 16 or 17 and 7% for women who marry at age 18 or 19. Eighty-seven percent of early teen brides have not finished high school, compared to 66% and 29% of brides who marry at age 16-17 and 18-19, respectively.[26] These early teen brides have children early, with 63% having one child by the age of 16 and 25% having two children by the age of 18. This compares to rates of 10% and 8% for those marrying between 16-17, and 1% and 0.5% for those marrying between 18-19.

Moreover, early teen brides marry men who are also relatively young and less educated.

[25] In the combined 1965 and 1970 NFS sample, there are 654 marriages before age 16, 2,080 marriages at age 16-17, and 3,458 at age 18-19.

[26] For tabulations of dropout status, I only include women (or men, when considering husband's education) currently over the age of 25 so as to focus on individuals who are more likely to have completed their education.

Twenty percent of women marrying at age 15 or younger marry a man who is 17 or younger. In contrast, only 10% of women marrying between 16-17 and 3% of women marrying between 18-19 do so. The dropout rates of early teen brides' husbands is 65%, compared to 53% for women marrying between 16-17 and 36% marrying between 18-19. These tabulations from the NFS show that those who marry very young have substantially different divorce, fertility, schooling, and husbands, even in comparison to women who marry just a few years later.

One other interesting comparison can be drawn from the NFS data. In 1965, the survey asked respondents if they would encourage a daughter to marry at a younger age, the same age, or an older age as they did. Ninety-one percent of early teen brides answered "older", compared to 48% for the rest of the sample. This provides some indication that early teen brides would not necessarily make the same decision to marry so young if they had it to do over again.

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 31 percentage points more likely to live in poverty when they are older. Similarly, women who drop out of school are 11 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, including LIML estimation and a control function approach. In comparison, OLS estimates are sensitive to how the data is aggregated; regressions on individual level data estimate small effects for early teen marriage, while aggregated data estimate large effects. I argue the difference is due to a large amount of

measurement error in the early marriage variable, resulting in substantial attenuation bias in the individual-level OLS regressions, but not the aggregated OLS or IV regressions.

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 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.

Appendix

This appendix describes the control function approach taken for the estimate appearing in Table 6. Consider the following outcome equation, which allows for individual-specific coefficients and an individual-specific intercept term:

(1) $$y_i = \bar{\alpha} + \bar{\gamma} m_i + \bar{\delta} d_i + (\alpha_i - \bar{\alpha}) + (\gamma_i - \bar{\gamma}) m_i + (\delta_i - \bar{\delta}) d_i + e_i$$

where y_i is a dummy for poverty, m_i is early teen marriage, and d_i is divorce. For ease of exposition, other control variables are excluded and coefficients have been written as deviations from their averages.

The equations for early teen marriage and divorce are

(2) $$m_i = \varphi z_i + u_i$$

(3) $$d_i = \rho z_i + v_i$$

where z_i is the vector of marriage, schooling, and work laws for individual *i*'s state.

Begin by assuming that $E(e_i \mid m_i, d_i, z_i) = 0$, $E(u_i \mid z_i) = 0$, and $E(v_i \mid z_i) = 0$ and that the three individual-specific terms α_i, γ_i, and δ_i are mean independent of z_i. Following Garen (1984) and Card (2001), further assume that

$$E(\alpha_i - \bar{\alpha} \mid m_i, d_i, z_i) = \pi_m m_i + \pi_d d_i + \pi_z z_i$$

$$E(\gamma_i - \bar{\gamma} \mid m_i, d_i, z_i) = \lambda_m m_i + \lambda_d d_i + \lambda_z z_i$$

$$E(\delta_i - \bar{\delta} \mid m_i, d_i, z_i) = \tau_m m_i + \tau_d d_i + \tau_z z_i .$$

Under these linearity assumptions, it follows that

$$E(y_i \mid m_i, d_i, z_i) = \bar{\alpha} + \bar{\gamma} m_i + \bar{\delta} d_i + \pi_m u_i + \pi_d v_i + \lambda_m m_i u_i + \lambda_d d_i v_i + \tau_m m_i u_i + \tau_d d_i v_i .$$

This equation can be consistently estimated using the estimated residuals $\hat{u}_i$ and $\hat{v}_i$ from equations (2) and (3) in place of u_i and v_i.

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Table 1. 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)	(5)
	OLS Estimates				
Early teen marriage	0.043**	0.037**	0.033**	0.032**	0.029**
	(0.005)	(0.003)	(0.002)	(0.002)	(0.002)
High school dropout	0.134**	0.125**	0.122**	0.121**	0.123**
	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Control variables:					
Census year, race, and age dummies		X	X	X	X
State of birth and birth cohort dummies			X	X	X
Region of birth trends				X	X
Allocated observations included					X
Observations	3,256,434	3,256,434	3,256,434	3,256,434	3,489,385
R-squared	0.041	0.079	0.082	0.082	0.084
	Grouped OLS Estimates				
Early teen marriage	1.065**	0.453**	0.329**	0.264**	0.071*
	(.135)	(0.090)	(0.063)	(0.054)	(0.041)
High school dropout	0.026	0.119**	0.166**	0.129**	0.136**
	(0.022)	(0.029)	(0.024)	(0.035)	(0.033)
Control variables:					
Census year, race, and age dummies		X	X	X	X
State of birth and birth cohort dummies			X	X	X
Region of birth trends				X	X
Allocated observations included					X
R-squared	0.421	0.815	0.871	0.874	0.884
Observations (number of cells)	3,567	3,567	3,567	3,567	3,567

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 (14 or 15 in the 1970 and 1980 Censuses) and high school dropout is defined as fewer than 12 years of completed schooling. Year dummies are indicators for each of the three census years, race is a dummy for whether the respondent is white, and current age dummies are single year indicators for a woman's current age. State of birth dummies are indicators for each of the 41 states, and cohort of birth dummies are single year indicators for each birth cohort. Region of birth trends are separate linear cohort year trends for each of the four birth regions. In the second panel, the data is aggregated to state of birth × year of birth × census year cell means. Allocated observations refer to observations whose value for the variable age at first marriage has been logically edited or hot decked by the Census Bureau.

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

Table 2. 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: 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.

Table 3. Relationship between Early Marriage and Compulsory Schooling, Child Labor, and Divorce Laws across States.

	Marriage Laws (minimum marriage age)					
	common law	≤ 13	14	15	≥ 16	Row total
Compulsory Schooling Laws						
compulsory attendance ≤ 7 (24.5%)	6.3	8.2	6.5	16.2	62.8	100%
compulsory attendance = 8 (46.1%)	6.2	2.4	9.1	22.7	59.7	100%
compulsory attendance = 9 (18.1%)	0.0	0.0	49.8	7.7	42.5	100%
compulsory attendance ≥ 10 (11.3%)	0.0	0.0	16.7	24.7	58.6	100%
Column average	4.3	3.2	16.7	18.6	57.2	100%
χ^2 test of independence [p-value]	324.3 [0.0001]					

	Marriage Laws (minimum marriage age)					
	common law	≤ 13	14	15	≥ 16	Row total
Child Labor Laws						
child labor ≤ 6 (11.4%)	11.6	11.0	9.2	14.6	53.7	100%
child labor = 7 (22.8%)	11.9	3.4	16.8	35.2	32.7	100%
child labor = 8 (44.0%)	0.6	2.5	15.5	16.0	65.3	100%
child labor ≥ 9 (21.8%)	0.3	0.0	22.7	8.6	68.4	100%
Column average	4.3	3.2	16.7	18.6	57.2	100%
χ^2 test of independence [p-value]	266.6 [0.0001]					

	Marriage Laws (minimum marriage age)					
	common law	≤ 13	14	15	≥ 16	Row total
Divorce Laws						
no unilateral divorce (96.0%)	4.6	3.3	17.4	18.2	56.6	100%
unilateral divorce (4.0%)	0.0	0.0	0.0	29.3	70.7	100%
Column average	4.3	3.2	16.7	18.6	57.2	100%
χ^2 test of independence [p-value]	20.3 [0.0001]					

Notes: 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. Percentages in parentheses represent marginal percentages averaged over the marriage laws.

Table 4. 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 ≤ 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. See footnote 13 in the text for a list of available MRA states. 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 5. 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	0.306**	
	(0.096)	
Dropout	0.112**	
	(0.035)	
Observations	3,256,434	
	First Stage	
	Dependent Variable	
	Early Teen Marriage	Dropout
Marriage Laws (≤13 excluded)		
common law	0.001	0.010
	(0.003)	(0.011)
minimum marriage age = 14	-0.08*	-0.004
	(0.004)	(0.018)
minimum marriage age = 15	-0.011**	-0.014
	(0.003)	(0.015)
minimum marriage age ≥ 16	-0.005	-0.007
	(0.004)	(0.017)
Compulsory Attendance Laws (7 excluded)		
compulsory attendance = 8	0.000	-0.008
	(0.001)	(0.006)
compulsory attendance = 9	-0.005**	-0.028**
	(0.003)	(0.006)
compulsory attendance ≥ 10	0.003	-0.012
	(0.002)	(0.009)
Child Labor Laws (6 excluded)		
child labor = 7	0.005**	-0.002
	(0.002)	(0.008)
child labor = 8	0.007**	-0.005
	(0.002)	(0.006)
child labor ≥ 9	0.010**	-0.014
	(0.003)	(0.009)
F-statistic (state of birth clustering)	15.68	5.44
[p-value]	[.0001]	[.0001]
R-squared	0.019	0.102

Notes: Standard errors in parentheses, adjusted for clustering by state of birth. All regressions include census year, race, age, state of birth, and cohort of birth dummies, and region of birth trends. See notes to Table 1.
**Significant at the 5% level, *Significant at the 10% level

Table 6. Alternative Estimators for the Baseline Model.

	Dependent Variable = Poor (1 = poor, 0 = not poor)			
	Grouped IV	**Grouped LIML**	**Migration Adjusted**	**Control Function**
Early teen marriage	0.314**	0.315**	0.318**	0.281**
	(0.107)	(0.109)	(0.111)	(0.108)
Dropout	0.112**	0.112**	0.112**	0.123**
	(0.034)	(0.035)	(0.033)	(0.036)
$\hat{u}$				-.0152
				(0.165)
$\hat{u}$ × early teen marriage				3.648*
				(2.045)
$\hat{u}$ × dropout				-0.375
				(0.527)
$\hat{v}$				0.015
				(0.106)
$\hat{v}$ × early teen marriage				3.405**
				(1.510)
$\hat{v}$ × dropout				-0.440*
				(0.267)
F-statistic for control function [p-value]				3.58 [0.0062]
F-statistic for early marriage 1st stage	14.52	14.52	13.09	
[p-value]	[0.0001]	[0.0001]	[0.0001]	
F-statistic for dropout 1st stage	5.15	5.15	5.81	
[p-value]	[0.0001]	[0.0001]	[0.0001]	
Observations	3,567	3,567	3,567	3,567

Notes: Standard errors in parentheses, adjusted for clustering by state of birth. All regressions include census year, race, age, state of birth, and cohort of birth dummies, and region of birth trends. Data is aggregated to state of birth × year of birth × census year cell means. See notes to Table 1. The migration adjusted approach is described in the text and the control function approach is described in the text and Appendix.
**Significant at the 5% level, *Significant at the 10% level

Table 7. Heterogeneity in the Return to Marrying Young and Dropping Out of School, OLS Estimates.

	Dependent Variable = Poor (1 = poor, 0 = not poor)		
(A) By race	**Black**		**White**
Early teen marriage	0.100**		0.093**
	(0.033)		(0.047)
Dropout	0.242**		0.097**
	(0.034)		(0.024)
R-squared	0.737		0.780
Observations	3,304		3,567
(B) By region of country	**South**		**Non-South**
Early teen marriage	0.194**		0.15*
	(0.078)		(0.082)
Dropout	0.110**		0.095**
	(0.034)		(0.029)
R-squared	0.901		0.749
Observations	1,218		2,349
(C) By birth year and age cohort			
Early teen marriage × birth cohort 1920-1930	0.391**		0.425**
	(0.082)		(0.179)
Early teen marriage × birth cohort 1921-1941	0.151**		0.123
	(0.053)		(0.125)
Early teen marriage × birth cohort 1942-1954	0.168**		---
	(0.109)		
Dropout × birth cohort 1920-1930	0.109**		0.314**
	(0.034)		(0.119)
Dropout × birth cohort 1921-1941	0.165**		-0.108
	(0.040)		(0.123)
Dropout × birth cohort 1942-1954	0.134**		-0.396**
	(0.043)		(0.162)
Early marriage * age cohort 20-29		0.370**	0.066
		(0.109)	(0.045)
Early marriage * age cohort 30-44		0.120*	0.075**
		(0.067)	(0.032)
Early marriage * age cohort 45-60		-0.112	---
		(0.099)	
Dropout * age cohort 20-29		0.258**	0.156**
		(0.042)	(0.039)
Dropout * age cohort 30-44		0.156**	0.078**
		(0.037)	(0.036)
Dropout * age cohort 45-60		0.089**	0.004
		(0.034)	(0.042)
R-squared	0.875	0.897	0.902
Observations	3,567	3,567	3,567

Notes: Standard errors in parentheses, adjusted for clustering by state of birth. All regressions include census year, race, age, state of birth, and cohort of birth dummies, and region of birth trends. Data is aggregated to state of birth × year of birth × census year cell means. See notes to Table 1. There are no observations in the 20-30 year age cohort who are also in the 1920-30 birth year cohort and there are no observations in the 45-60 age cohort who are also in the 1942-1954 birth year cohort. This explains the omitted interaction terms in column 3 of Panel (C).
**Significant at the 5% level, *Significant at the 10% level

Table 8. Additional IV Estimates.

	Dependent Variable = Poor (1 = poor, 0 = not poor)	
(A) By race	**Black**	**White**
Early teen marriage	0.464**	0.227
	(0.205)	(0.332)
Dropout	0.085	0.109**
	(0.114)	(0.057)
F-statistic for early marriage eqn.	52.54	5.92
F-statistic for dropout equation	5.53	4.84
Observations	3,304	3,567
(B) By region of country	**South**	**Non-South**
Early teen marriage	0.384**	0.377
	(0.112)	(0.440)
Dropout	0.061*	0.063
	(0.032)	(0.060)
F-statistic for early marriage eqn.	178.44	2.40
F-statistic for dropout equation	27.45	39.19
Observations	1,218	2,349
(C) Adding marriage laws for women without consent and for men with and without consent as instruments		
Early teen marriage	0.368**	
	(0.113)	
Dropout	0.157**	
	(0.022)	
F-statistic for early marriage equation	47.98	
F-statistic for dropout equation	35.34	
Observations	3,567	
(D) Using total family income as the dependent variable		
Early teen marriage	-25.596**	
	(10.380)	
Dropout	-5.470	
	(4.495)	
F-statistic for early marriage equation	14.52	
F-statistic for dropout equation	5.15	
Observations	3,567	
(E) Including allocated observations		
Early teen marriage	0.334**	
	(0.103)	
Dropout	0.110**	
	(0.042)	
F-statistic for early marriage equation	15.96	
F-statistic for dropout equation	5.27	
Observations	3,567	

Notes: Standard errors in parentheses, adjusted for clustering by state of birth. All regressions include census year, race, age, state of birth, and cohort of birth dummies, and region of birth trends. Data is aggregated to state of birth × year of birth × census year cell means. Family income measured in thousands of dollars. See notes to Table 1.
**Significant at the 5% level, *Significant at the 10% level

Table 9. OLS and IV Estimates of the Effect of Divorce on Poverty.

	Dependent Variable = Poor (1 = poor, 0 = not poor)		
	Grouped OLS	**IV**	**IV**
Early teen marriage	0.235**	0.264**	0.380**
	(0.059)	(0.133)	(0.129)
Dropout	0.128**	0.112**	0.125**
	(0.035)	(0.033)	(0.043)
Currently divorced	0.215**	0.159**	-0.114
	(0.037)	(0.003)	(0.117)
Instrumenting for early marriage and dropout?		X	X
Instrumenting for divorce?			X
F-statistic for early marriage 1st stage		13.67	14.83
[p-value]		[0.0001]	[0.0001]
F-statistic for dropout 1st stage		5.32	6.61
[p-value]		[0.0001]	[0.0001]
F-statistic for divorce 1st stage			14.68
[p-value]			[0.0001]
R-squared	0.877		
Observations	3,567	2,870,390	2,870,390

Notes: Standard errors in parentheses, adjusted for clustering by state of birth. All regressions include census year, race, age, state of birth, and cohort of birth dummies, and region of birth trends. Data is aggregated to state of birth × year of birth × census year cell means. See notes to Table 1.
**Significant at the 5% level, *Significant at the 10% level

Table 10. Marriage Age Misreports in the 1975 and 1970 National Fertility Surveys.

	Percent of 1975 Responses Not Matching with 1970 Responses			
	Date of Birth	**Date of Marriage**	**Marriage Age (in Years)**	**Obs.**
Reported Age of Marriage in 1970 Survey				
≤ 15	7.7†	34.6††	38.5††	52
16	3.5	15.9†	12.4	113
17	5.7††	16.1††	17.1††	211
18	3.5	14.0††	11.5	400
19-24	3.0	10.2	9.1	1,579
Any age	3.5	12.2	11.0	2,355

Notes: Standard errors in parentheses. Data come from the subsample of women the 1970 National Fertility Survey who were re-interviewed in the 1975 National Fertility Survey. Marriage age is calculated from date of marriage and date of birth.

††Significantly different from the 19-24 coefficient at the 5% level, †Significantly different at the 10% level

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

State	Year							
	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 1a. Fraction of Women Marrying Young and Women's Median Age at First Marriage over Time.

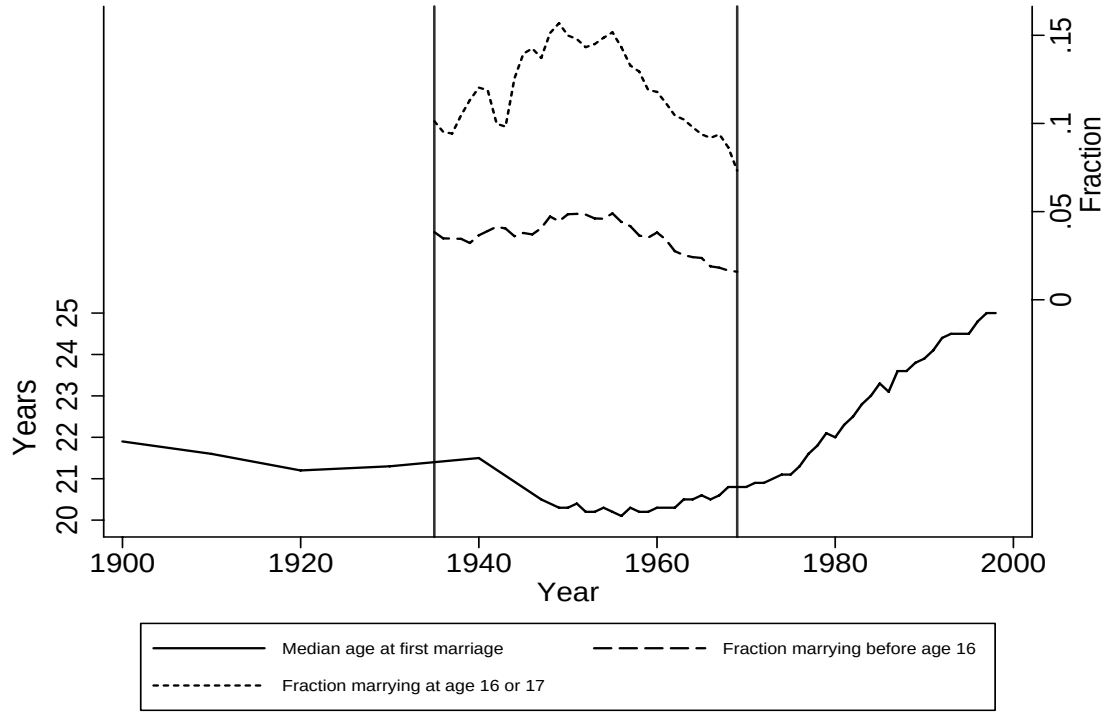
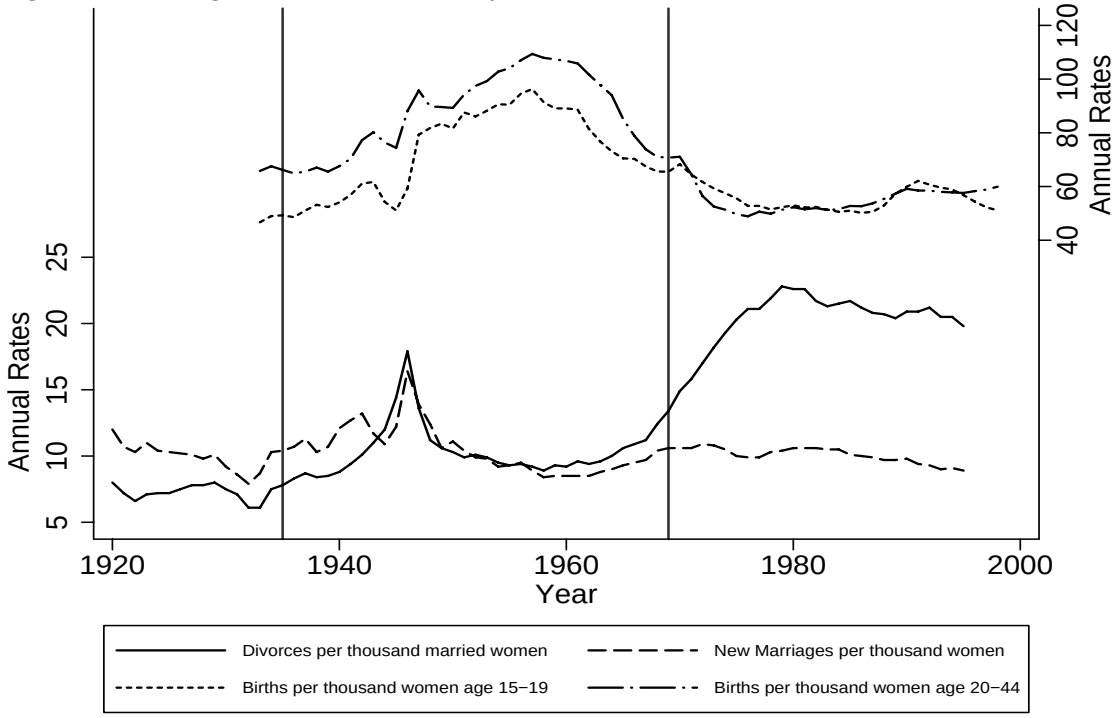


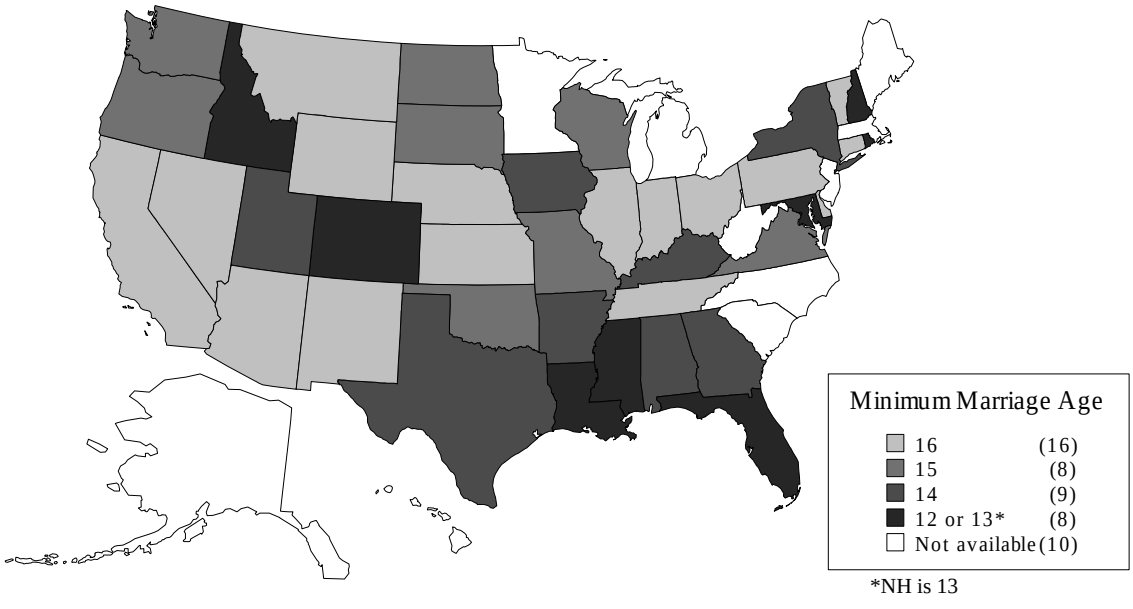
Figure 1b. Marriage, Divorce, and Fertility Rates over Time.



Notes: Data on fraction of teenage marriages from author's tabulations of U.S. Census data. All other data series from various tables in Carter et al. (2006). Vertical lines denote the time period being analyzed in this paper (1935-1969).

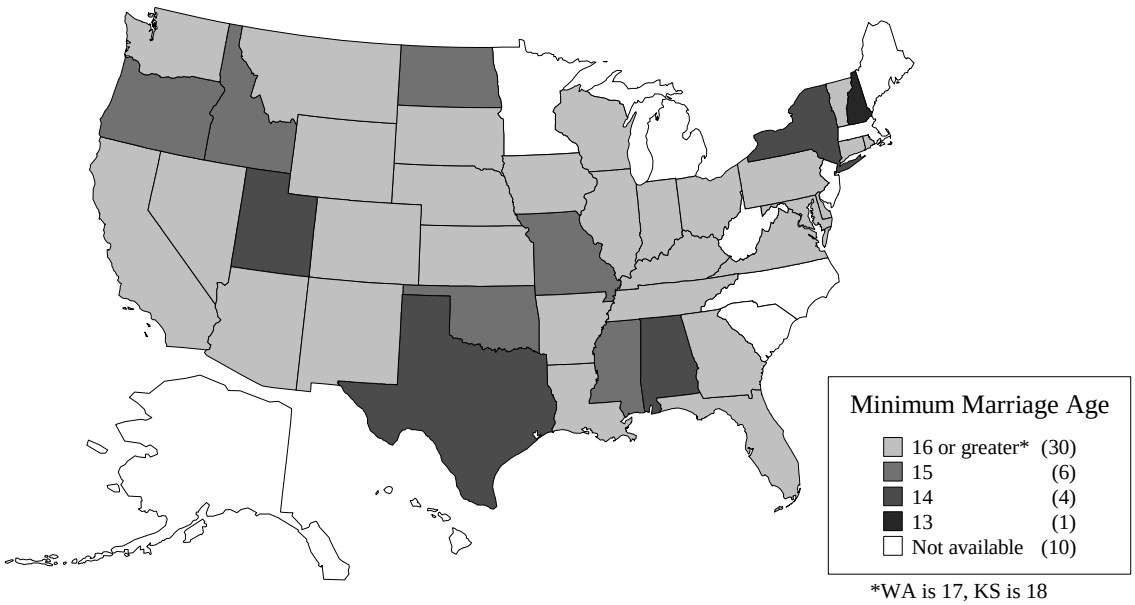
Figure 2. Minimum Legal Marriage Age by State for Women with Parental Consent, 1935 and 1969.

1935



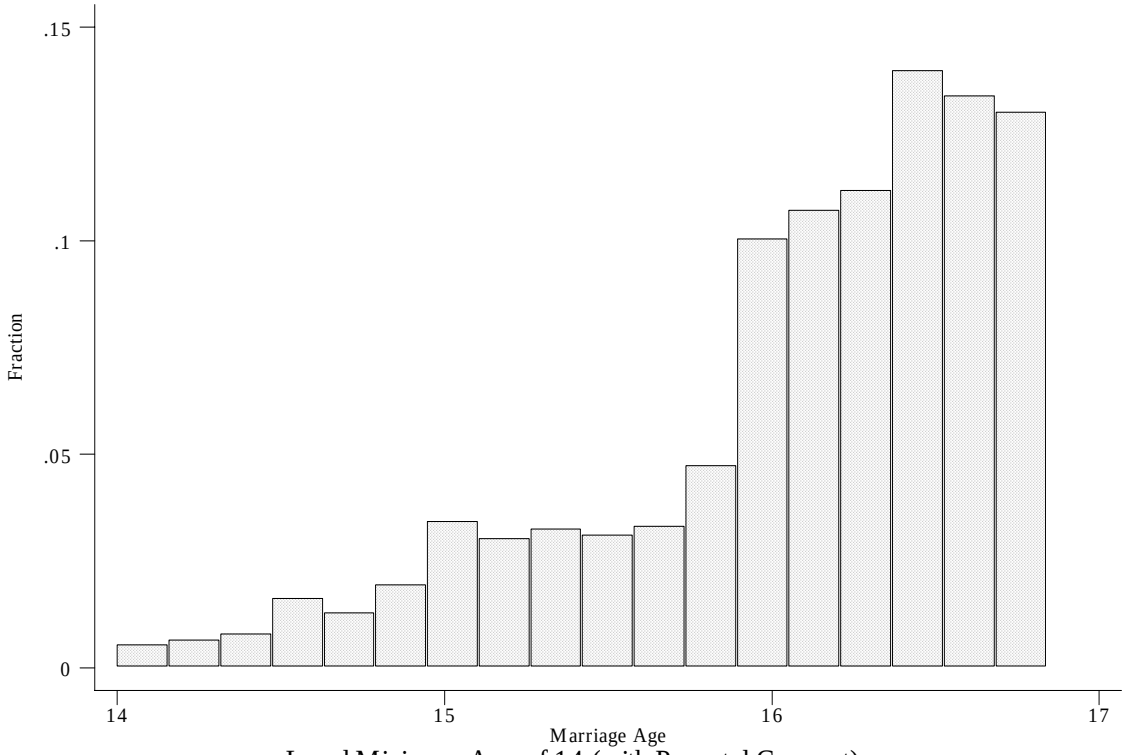
*NH is 13

1969

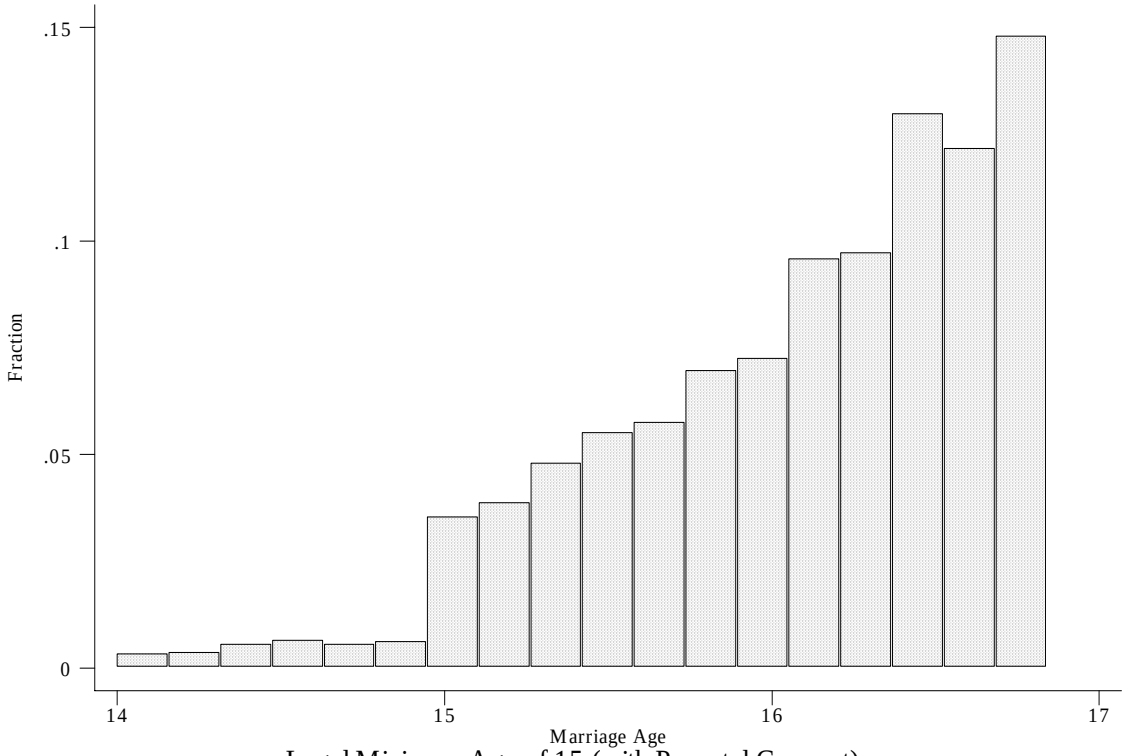


*WA is 17, KS is 18

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

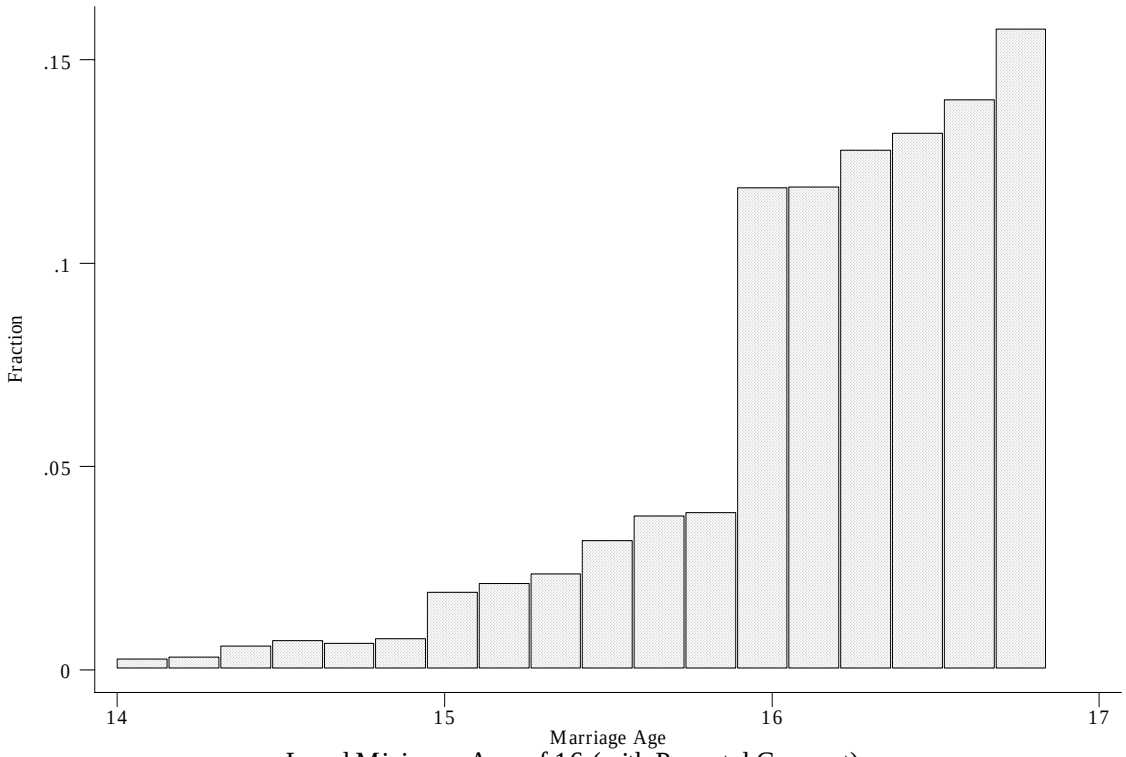


Legal Minimum Age of 14 (with Parental Consent)



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.