Cohort Size and the Marriage Market:
Explaining Nearly a Century of Changes in U.S. Marriage Rates∗

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Abstract
We propose an explanation for almost a century of changes in U.S. marriage rates, in three stages. First, we show that changes in cohort size alone can account for around 50 to 70% of the variation in marriage rates since the 1930s for both black and white populations. Specifically, increases in cohort size reduce marriage rates, whereas declines in cohort size have the opposite effect. We provide the most convincing evidence on this relationship by using variation in cohort size due to differences across states in sale bans on oral contraceptives. Using this exogenous variation in access to oral contraceptives, and consequently the number of births, we provide evidence that the relationship between changes in cohort size and changes in marriage rates is causal. Next, we develop and test a model of the marriage market that has the potential of generating the patterns observed in the data. An important implication of our empirical findings is that a standard matching model cannot produce some of the patterns observed in the data. We therefore build and test a dynamic search model of the marriage market and show that a version of it can explain all our empirical results.

1 Introduction
What causes variation in marriage rates over time? For both economists and policy-makers, this is a question of significant interest, as a large body of evidence suggests that marriage rates have important implications for other economic variables. Such variables include fertility rates, children’s welfare, children’s education, labor force participation, hours of work, income inequality, the fraction of individuals on government aid, population growth, and workers’ productivity.1 In spite of this, according to the existing literature, no prevailing

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explanation can account for the variation in marriage formation over time and across geographies. Many existing theories about changes in the marriage rate, which will be reviewed in the next section, have either been empirically rejected, or have explanatory power that is limited to specific periods or specific groups of individuals.

The main contribution of this paper is to provide an explanation for changes in U.S. marriage rates that holds empirically over nearly a century. We show that one variable, changes in cohort size, explains the majority of the variation in U.S. marriage rates since the early twentieth century, both over time and across states. The paper consists of three parts.

In the first part, we present reduced-form evidence which indicates that an increase in cohort size generates a decline in marriage rates and that a reduction in cohort size has the opposite effect. We provide reduced-form evidence in two steps. First, using both time-series variation and cross-sectional state-level variation in cohort size and marriage rates we find that there is a strong and negative relationship between cohort size and marriage rates for both women and men. On average, a 10 percent increase in cohort size is associated with a 0.5 to 1 percent decrease in the share ever married by 40. These are sizable effects that account for around 50 to 70 percent of the variation in marriage rates since the early twentieth century. Our results indicate that changes in cohort size account well for medium-and long-term changes in marriage rates, but cannot explain the short-run year-on-year variation.

In the second step, we provide what is arguably the most convincing evidence on the hypothesis that there is a causal relationship between changes in cohort size and variation in marriage rates. Using an idea based on Bailey (2010), we employ the interaction between the 1957 introduction of Enovid, later known as the birth-control pill, and cross-state variation in anti-obscenity laws, which limited the use of contraception in some states until the mid-1960s, to generate exogenous variation in the number of births and therefore cohort size. Our results indicate that, in states that limited contraceptives, cohort size increased relative to states that did not have such limits, and that this change generated a decline in marriage rates for women as well as for men. The exogenous variation, therefore, gives results that are consistent with the time-series and cross-state variation.

In the second part of the paper, we propose a model that can potentially generate the relationship between changes in cohort size and changes in marriage rates observed in the data. An important implication of our empirical results is that the model commonly used to study the marriage market, a standard matching model, is rejected. A testable implication of the standard matching model is that an increase in cohort size reduces the marriage rate of women, but increases the marriage rate of men, which contradicts our findings. We therefore develop and test as an alternative model of the marriage market a dynamic search model.
Using the search model, we show three main results. First, we show that a simple dynamic search model of the marriage market is rejected for the same reason the matching model is rejected: in the search model an increase in cohort size always increases the marriage rate of men. We then show that there are two variations of the search model that are able to generate all the empirical results documented in this paper. In the first version, the dynamic search model is modified to allow the value of being single to increase with cohort size. The economic idea behind this new feature of the model is that it is more enjoyable to be single when cohort size is large because there are more individuals of the same age with whom to perform leisure activities. In the second version, a man can undertake an investment that increases his probability of meeting a potential spouse and, if they marry, their marital surplus. In this alternative formulation of the search model, men are more likely to invest when cohort size is low because, in this case, the probability with which they meet a potential spouse is generally below the optimal level for a larger fraction of them.

The two versions of the search model have one common feature. In both of them, an increase in cohort size has an additional negative effect on the marriage rate of women and men. We show that, if this additional effect is sufficiently strong, the search model can generate the observed pattern according to which a rise in cohort size generates a decline in the marriage rate of men. But, remarkably, the two alternative models generate this additional effect in different ways. The first version does this by increasing the value of being single when cohort size rises, whereas the second version achieves this by increasing the value of marriage when cohort size declines. This difference generates a testable implication based on the relationship between cohort size and divorce rates. The first version of the search model predicts that an increase in cohort size should generate a decline in divorce rates. The second version has the opposite prediction and generates a positive relationship between those two variables. As a final result, we use this implication to test one model against the other. We document that, in the data, there is a strong and positive relationship between cohort size and divorce rates. We can therefore reject the model in which the value of being single increases with cohort size in favor of the investment model.

Our findings have an important policy implication. In recent years, politicians and policy makers have started to discuss and implement policies that attempt to improve the well-being of low income families by increasing the fraction of married individuals. For instance, between 2004 and 2014 the federal government alone spent more than $800 million on implementing and evaluating policies aimed at promoting marriage (Reyes (2014)). Our results suggest that these policies may prove to be largely ineffective since a significant part of changes in marriage rates is generated by forces that are mostly outside the control of policy makers.

We conclude this section with one remark. The variable cohort size is not exogenous. It is affected by variables such as new fertility technologies, technological progress, child...
care availability, supply of housing, and changes in labor supply decisions of women. This fact does not diminish the importance of our results for social scientists and policy makers. They indicate that to understand the dynamics of the marriage market one has to study the evolution of cohort size and of the variables that have an effect on it.

The paper proceeds as follows. In Section 2, we discuss related papers. In section 3, we describe the data sets used to derive the empirical results. Section 4 documents our reduced-form findings. In section 5, we develop and test the dynamic search model. Section 6 concludes.

2 Existing Explanations

In this section, we describe some of the existing explanations for the variation in U.S. marriage rates for which some empirical evidence is provided.

One set of explanations for historical changes in marriage rates focuses on changes in income. Cherlin (1981) among others notes the correspondence between rising incomes after World War II and the associated marriage boom during this period. A related common insight is that low income during the Great Depression was the main factor behind the reduction in marriage rates during this period. A positive relationship between income and marriage rates, however, has not been successfully tested over different periods. Hill (2011) rejects the hypothesis that there is a positive correlation between income and marriage rates after 1960. Wolfers (2010) looks at the relationship between marriage rates and recessions for the past 150 years and rejects any pattern between marriage and periods of economic decline. These results suggest that income may generate part of the variation in marriage rates. But they also suggest that variation in income cannot be the general explanation that underlies the changes in marriage rates observed in the past century. In addition, there is a potential reverse causality problem with this theory that is not addressed: men have higher labor hours and earn more following marriage.\(^2\) It is therefore difficult to determine whether an increase in income causes marriage rates to rise or whether an increase in marriage rates generates higher income levels.

An alternative set of explanations focuses on technological innovations. Akerlof, Yellen, and Katz (1996) consider the adoption of new fertility technologies such as the pill and abortion in the sixties and seventies. The authors argue that a decline in the cost of abortion and the increased availability of contraception decreased the incentives of women to obtain a promise of marriage if premarital sexual activity results in pregnancy, with a consequent rise in out-of-wedlock births and decline in marriage rates. This explanation may provide important insight into changes during and around the 1970s, but it would not be able to explain intervals of increasing marriage rates after the 1970s, and it cannot address the historical variation prior to this period.

\(^2\)A number of studies have documented this relationship. See Mazzocco, Ruiz, and Yamaguchi (2014).
Greenwood and co-authors also focus on technological innovations and suggest that the decline in the price of household appliances explains patterns for several household outcomes, including marriage. Greenwood and Guner (2009) argue that labor-saving technological progress in the household sector made it easier for singles to maintain their own home, increasing the value of being single and reducing the marriage rate. Their theory addresses in particular the period of rapidly falling marriage rates starting around the mid-sixties. However, the theory would have trouble explaining much of the remaining historical variation in marriage rates. In as far as technological progress has constantly improved household appliances in the past 100 years, we might predict that marriage rates either decline systematically throughout this period or decline and then remain constant. However, this contradicts what we observe in the data.

A different well-known set of theories considers men’s marriageability and policies such as welfare aid, both of which may affect women’s desire to marry. These theories have been widely empirically tested, with mixed results. Ellwood and Crane (1990) review papers that have tested the hypothesis proposed by Wilson (1987) that limited labor market opportunities reduce the number of marriageable men, and thus the marriage rate. While some papers provide evidence in support of this theory, others reject this hypothesis (e.g., Plotnick (1990) and Lerman (1989)). The degree to which men’s employment opportunities affect marriage rates is largely still an open question. A related issue affecting especially black men’s marriageability in the last three decades is the rise in incarceration. Charles and Luoh (2010) study the relationship between incarceration and marriage rates and find that higher incarceration rates decrease the fraction of married individuals both in black and white populations. Quantitatively, however, such an explanation is primarily relevant for black populations and only for the period after 1980, when drug-related policies significantly increased incarceration rates. Ellwood and Crane (1990) have also evaluated the link between welfare aid and marriage. They conclude that there is very little empirical support for the proposition that welfare benefits played a major role in marriage trends for black or white women. Finally, rising income and employment opportunities for women may also affect their desire to marry. However, we do not know of papers that formally test this hypothesis over time or across geographies. One reason may be that potential reverse causality complicates such an empirical analysis: women who face poorer marriage prospects may both invest more in human capital and work more.

Finally, there are two explanations that have commonality with the one we propose. The first explanation is Easterlin’s hypothesis. Easterlin (1987) argues that the relative size of a cohort can explain many of the variables that determine the economic and social outcomes of a birth cohort: earnings and unemployment rates, college enrollment rates, divorce, fertility, crime, suicide rates, and marriage. Easterlin’s explanation is that when income is above the aspiration level for a given cohort, the individuals in that cohort will be
optimistic and therefore will have better economic and social outcomes. If the distribution of income of a cohort is affected by its size, then the size will affect its economic outcomes. Easterlin, however, has provided only indirect evidence in support of his hypothesis and researchers that have attempted to test the general idea behind it have found mixed results (Pampel and Peters (1995)).

The second explanation that is related to ours uses changes in the sex-ratio to understand changes in marriage rates. Most of the papers in the marriage literature define the sex ratio as the number of men in the marriage market divided by the number of women in the marriage market. In this paper, we define the sex-ratio as the number of women divided by the number of men because it better fits the model we will develop later in the paper. The existence of a relationship between the sex ratio and marriage rates was originally postulated, using a two-sided matching model, in Becker (1973)’s seminal work. Since then, almost all papers studying the marriage market have relied on a matching model. An important contribution of Becker (1973) is to derive the following testable implication for the matching model: an increase in sex ratio should reduce women’s marriage rates and increase men’s marriage rates. Several papers have attempted to test the relationship between sex ratios and marriage rates. Akers (1967), Schoen (1983), Bergstrom and Lam (1989), Angrist (2002), and Abramitzky, Delavande, and Vasconcelos (2011) are examples of papers in this literature.

In our paper, we do not explicitly analyze the link between sex ratios and marriage rates. There is, however, a relationship between changes in cohort size and changes in sex ratio under the standard and realistic assumption that women marry on average older men. Under this assumption, if men and women have the same cohort size at birth, the marriage market is composed of the same number of younger women and men, but of a larger number of older men. In this context, an increase in cohort size increases the number of younger men and women without changing the number of older individuals. Since there are more older men than women, the rise in cohort size will have the effect of increasing the ratio between the number of women and the number of men in the marriage market, the sex ratio. The described relationship between cohort size and sex ratio generates the following testable implication for the matching model: an increase in cohort size should reduce the marriage rate of women, but increase the marriage rate of men. An important contribution of our paper is to provide evidence that this testable implication and, hence, the standard matching model is rejected if one considers data on cohort size and marriage rates over the past century.

This finding does not imply that a change in sex ratio can never generate a marriage market outcome that is consistent with the matching model. For instance, Abramitzky,3

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3 Several papers have studied the relationship between the sex ratio and other economic variables, including rates of single motherhood (Neal (2004)), labor force participation of women (Grossbard-Shechtman (1984)), and birth rates (Bitler and Schmidt (2011)).
Delavande, and Vasconcelos (2011) consider variation in the sex ratio due to World War I casualties in France and find that a higher sex ratio is associated with a lower marriage rate for women and with a larger marriage rate for men, in line with Becker’s prediction. Our finding simply indicates that, if the change in sex ratio is generated by a change in cohort size, the standard matching model is not consistent with marriage rate data from the past 100 years.

There is one paper in the sex ratio literature whose results are consistent with ours. Angrist (2002) uses variation in immigration rates from different European countries to the U.S. at the beginning of the twentieth century to study the relationship between sex ratios and marriage rates. He exploits the fact that the majority of migrants were men and that marriages were often formed between individuals belonging to the same ethnicity. Consistently with the patterns document here, he finds that ethnicities with higher sex ratios experienced lower marriage rates for women as well as men. In our paper, we show that the negative relationship between sex ratio and marriage rates of women and men applies to a century of data and not only to the specific period considered in Angrist (2002), if the changes in sex ratios are produced by variation in cohort size. In addition, we propose two models that are consistent with our findings and show that they have opposite implications for the relationship between cohort size and divorce rates. Lastly, we use the derived implications to test one model against the other, therefore formally providing a mechanism that can explain the variation in marriage rates observed in the data.

3 Data

In this section we describe the data used in the analysis. Throughout the paper we rely on the following datasets: National Vital Statistics (1909-1980), Census total population counts (1910-1980), Survey of Epidemiology and End Results (SEER) population estimates (1969-2000), IPUMS CPS (1962-2011) and IPUMS USA (1940-2000). In Data Appendix B we provide a detailed description of how the datasets are used to construct the main variables employed in the empirical analysis. In this section, we give a brief summary of that description.

In the empirical analysis we employ two main variables: cohort size and the share ever married by a given age for a given cohort. We construct two different measures of cohort size: cohort size at birth and cohort size at marriageable age. The first variable is used with longitudinal variation, whereas the second one is employed with cross-state variation. In this paper we are interested in the evolution of the variable cohort size at the ages in which individuals choose whether and whom to marry. With longitudinal variation, however, we use cohort size at birth as the main independent variable for two reasons. First, as shown in Figure B1, when cohort size is computed for the U.S. population there is little difference between cohort size at birth and cohort size at marriageable age, since net migration to
the U.S. was limited during the time period we consider. Second, we can construct the variable cohort size at birth for cohorts born in 1909 and after. The variable cohort size at marriageable age can only be constructed for cohorts born in 1940 and after. By using cohort size at birth we can therefore consider a larger number of cohorts without significant effect on the analysis. When we use cross-state variation, we have to use the variable cohort size at marriageable age because of large migration flows across states during the period of investigation.

The variable share ever married at age 30, 35, and 40 is constructed using either the decennial Censuses or the SEER population estimates. Appendix B describes the exact procedure used to construct this variable.

4 Empirical Results

This section is divided into five parts. We first describe the two measures generally used to study the evolution of marriage rates and compare them to an alternative measure we propose, which we believe is better suited to the examination of marriage choices over time. We then provide empirical evidence on the relationship between cohort size and marriage rates using longitudinal variation. In the third subsection, we describe findings obtained using cross-state variation. We then discuss endogeneity issues that may affect the longitudinal and cross-state variation. Finally, we provide evidence that changes in cohort size generate changes in marriage rates by using variation in early access to oral contraceptives across states as a plausible source of exogenous variation in cohort size.

4.1 Different Measures of Changes in the Marriage Rate

When analyzing changes in marriage rates over time, existing studies typically employ one of the following two variables: the number of new marriages per population; and the share of individuals currently or ever married within some age range, e.g. between the ages of 18 and 30. We use a different measure, the share of individuals in a given cohort ever married by a given age. In this section we compare the behavior of these three measures. As a first contribution, we show that, when used to study changes over time in the marriage market, the commonly used measures are problematic because they confound different effects and can potentially lead to incorrect inference about overall marriage behaviors.

Figure 1 graphs the three measures described above: the number of marriages per 1,000 individuals; the share ever married in a cross-section of women between the ages of 18 to 30; and the share of women in a given cohort ever married by 30. We refer to these measures as the population-based measure, the cross-sectional measure, and the cohort-based measure, respectively. The first two measures are based on the calendar-year, whereas our measure is based on the cohort’s year of birth. To make them comparable, we add 25 years to the
cohort’s birth year and plot our measure together with the other two. It is immediately evident from Figure 1 that the three measures give very different pictures of how marriage rates in the U.S. have evolved over time. It is worthwhile to discuss why they behave differently and the main problems with each measure.

The first measure, the number of marriages per 1,000 individuals, is based on the number of new marriages that take place in a given year. It is available from the U.S. Vital Statistics. This measure can be useful to detect years that experience an unusual growth or decline in marriage rates. For example, it captures well the rapid rise in the number of marriages in the immediate post-war years. However, this measure is not as well suited to study the evolution of the share ever married for two main reasons. First, using the population-based measure it is impossible to distinguish between first, second, or later marriages. This distinction is important if the researcher is interested in evaluating the fraction of people who choose to marry, since the second and later marriages should not be included in a measure designed to describe the evolution of the share ever married. The second and more serious problem for evaluating marriage trends is that this measure conflates changes in the numerator, new marriages, with changes in the denominator, population. As a result, if the population undergoes any substantive growth or decline due, for instance, to changes in fertility or migration patterns, one may draw the wrong inference. For example, starting from 1946 this variable drops steeply for about fifteen years. One might infer that marriage rates were falling steadily in the forties and fifties, but both the cross-sectional measure and the cohort-based measure show that marriage rates were flat or rising during this time. Part of the explanation for the large decline in the population-based measure is that during those years the U.S. experienced a sharp increase in population with the baby boom. Similarly, during the sixties and the first half of the seventies, the population-based variable displays rapid growth which can been interpreted as a big increase in marriage rates. The cross-sectional and cohort-based measure, however, show that this interpretation is misleading. During this period, marriage rates experienced a slight decline. A potential explanation for the rise in the population-based measure is that the U.S. population declined during the baby bust that characterized the U.S. in the sixties.4

The second measure illustrated in Figure 1 is the share ever married in a cross-section of women between the ages of 18 to 30, which was constructed using the CPS and Census. It follows closely the cohort-based measure for most of the period. It is only during the second half of the eighties and the nineties that the two variables diverge. The cross-sectional measure would suggest a sustained drop in marriage rates during this period, whereas the cohort-based measure documents a mild increase in the share ever married. The reason for the divergence is that the cross-sectional measure is strongly affected by changes in the age

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4 A second potential explanation for the increase in the population-based measure in the 1970s is that the large baby boom cohorts were coming of age. Even though these individuals were marrying at lower rates, their large overall number will generate an increase in the number of registered marriages, the numerator.
at first marriage: when examining the share of people ever married within an age range, one cannot determine whether individuals are simply delaying marriage or whether they choose not to marry. Starting from the second half of the eighties, the age at first marriage experienced a significant increase, which explains why the cross-sectional measure declines during this period whereas the cohort-based measure increased.

It is important to note that the cohort-based measure may also be affected by changes in the age at first marriage if the age cut-off is too low. It is therefore important that one chooses properly the age cut-off: if one believes that an age cut-off of 30 is too low, an age cut-off of 35 or 40 should be used. In the next subsections, we will show that the trends captured by the cohort-based measure look similar at different age cut-offs. In Figure 1, we choose an age cut-off of 30 mostly for expositional purposes, so that we can include as many recent cohorts as possible.

Because of the advantages the cohort-based measure has over the other two variables, throughout the rest of the paper we will only use that measure to study the evolution of marriage rates.

4.2 Change in Marriage Rates Over Time

In this subsection we provide evidence on the relationship between changes in cohort size and changes in marriage rates using longitudinal variation. The evidence is presented in two steps. We first provide evidence on the general nature of this relationship. We then try to understand whether cohort size can explain the short-run, medium-run, or long-run changes in marriage rates.

In Figure 2, we plot cohort size and the share never married by age 30, separately for women and men, for all cohorts born between 1914 and 1981. The first panel describes these variables for the white population whereas the second panel plots them for the black population. We plot the share never married because visually it is easier to detect a positive correlation between the two variables. Figure 2 contains one main finding. To describe it, we initially focus on cohorts born before 1960. For those cohorts, there is a strong positive correlation between cohort size and the share never married. The decline in size for cohorts born in the 1920s and 1930s is associated with a similar drop in the share never married. This decline corresponds to the well-documented “marriage boom” that starts in the mid-1940s and lasts through the early 1960s, the period in which the cohorts born in the twenties and thirties were active in the marriage market. The sharp increase in the size of cohorts born between 1946 and 1959, which correspond to the post-war baby boom generations, is associated with a share never married that nearly tripled during this period. Births and the share never married for the black population follow similar patterns.

It is left to explain why we lose the positive correlation between our two main variables for cohorts born in the 1960s and 1970s. Note that those cohorts were active in the marriage
market starting from the 1980s, which is the period in which cohabitation began to become a popular form of household formation and potentially a close substitute for marriage. To understand whether cohabitation can resolve the inconsistency between the early and later cohorts, in Figure 3 we plot the variables reported in the previous figure, with the exception that now cohabiting individuals are treated as married individuals instead of being treated as never-married individuals. Remarkably, once cohabiting households are accounted for, the relationship between cohort size and household formation for cohorts born in the 1960s and 1970s resembles that of the earlier cohorts. Falling cohort sizes in the 1960s and early 1970s correspond to a decline in the share never married and not currently cohabiting by 30. Increasing cohort sizes in the second part of the 1970s are associated with a rise in the share never married and not cohabiting.

Figure 3 contains a second noteworthy finding. The strong positive correlation between cohort size and share never married characterizes both the white and the black populations. We emphasize this similarity between the white and black marriage markets because it challenges the common perception that the two markets are governed by different rules and exhibit different marriage behaviors. Figure 3 suggests that the two marriage markets are similar in at least one respect: they respond to changes in cohort size in a similar way.

In Figures 2 and 3, we use an age cutoff of 30. The results may therefore be affected by changes in age at first marriage. To address this concern, in Figure 4 we plot cohort size and shares never married and not cohabiting by age 40. Using this new cutoff age, we find patterns that are similar to the ones observed in the first two figures: there is a positive and strong correlation between cohort size and share never married and not cohabiting.

To show these patterns more formally, in Table 1 we record the average response of marriage rates to changes in cohort size for age cutoffs of 30, 35, and 40. For ease of exposition, for the rest of the paper we will consider the effect of cohort size on the share ever married instead of the share never married. In the table, each coefficient is the outcome of a separate regression of the log share ever married or currently cohabiting on log cohort size. There are three results that are worth discussing. First, elasticities recorded in Table 1 are highest at 30, and gradually decrease with age, for both sexes and both races. This finding suggests that changes in cohort size are associated with two effects: (i) a change in the eventual share ever married or cohabiting; (ii) a change in the age at first marriage, where an increase in cohort size is associated with a higher age at first marriage. This finding also indicates that the coefficient that is better able to isolate the relationship between variation in cohort size and variation in marriage rates from changes in age at first marriage is the one that uses 40 as a cutoff age. The second result is that the effect of cohort size is quantitatively large. An increase of 10% in cohort size reduces the share ever married or cohabiting by 40 by a percentage that ranges from 0.66% for white women to 4.53% for black women. In percentage points, this amounts to a decline that is between 0.6 and 3.8
points in the share of individuals ever married or cohabiting by 40, a large effect. The last finding we wish to emphasize is that the cohort size variable explains a large fraction of the variation observed in marriage rates. For instance, when we use 40 as the cutoff age, the R-squared is between 0.58 and 0.85.\footnote{Note that we are working with non-stationary time series, and must therefore verify that the series are cointegrated to eliminate worries about spurious regression. A Johansen cointegration test rejects the null hypothesis that the series are not cointegrated at the one-percent level. Therefore, our OLS results are consistent and estimate a meaningful (non-spurious) relationship.}

In the rest of the paper, we will focus on the white population only for one main reason. White women and men have similar cohort size at the time of marriage, whereas black men have a significantly lower cohort size than black women because of higher mortality and incarceration rates. As a consequence, the investigation of the marriage market for blacks requires a different type of analysis which we undertake in a separate paper.

In the remaining part of the subsection we will try to understand whether cohort size can explain the short-run, medium-run, or long run changes in marriage rates. Observe that the regressions in log-levels documented in Table 1 capture at the same time the short-run, medium-run, and the long-run effects of changes in cohort size on changes in marriage rates. To see this, note that the regressions in levels measure the correlation between changes in our two main variables independently of whether the changes are over a period of one year or ten years. To measure the short-run, medium-run, and long-run effects of changes in cohort size on changes in marriage rates, we regress $n$-year differences in marriage rates on $n$-year differences in cohort size where $n$ is set equal to 1, 2, 3, 4, 5, 7, and 10. To capture the effect of adjacent cohorts, for $n > 1$ we use differences in cumulative cohort size as our independent variable, where cumulative size for the cohort born in period $t$ for the $n$-year difference is constructed by adding up cohort size from $t - n + 1$ to $t$. We interpret the coefficient estimates on the 1-year and 2-year differences as the short-run effect of cohort size on marriage rates, the coefficients on the 3-year, 4-year, and 5-year differences as the medium-term effect, and the coefficients on the 7-year and 10-year differences as the long-term effect. This choice is somewhat arbitrary, but it helps us focus the discussion.

The results are presented in Table 2. The first two columns report the short-run effect. Clearly, in the short run cohort size has at best a weak effect on marriage rates. The estimates for the 1-year differences indicate that a 1-year change in cohort size is not sufficient to trigger a change in marriage rates. With the exception of the coefficient for women by age 30 and men by age 40, all the coefficients for the 1-year difference are statistically equal to zero. The estimated coefficient for men by age 40 is the only one in all of the estimations we have performed that is positive and statistically significant. This result is generated by the two spikes in births that occurred in 1942 just at the start of World War II and in 1946-1947 after World War II ended. If one drops the observations that characterize the...
period around World War II, the coefficient becomes zero. The effects are slightly larger when we employ 2-year differences. Now the coefficient for women by age 35 is also negative and statistically significant. But even for 2-year differences, the effect of cohort size is very weak. The effect of cohort size on marriage rates is much stronger when we study the medium-term effect. With 3-year differences all the coefficients become large in size, negative, and statistically significant. The only exceptions are the coefficients by age 40 which are negative but statistically not significant. When we increase the differences to four and five years, the coefficients become larger in size and are now all statistically significant. The long-term effect of cohort size is even stronger. The coefficient estimates for the 7-year and 10-year differences suggest that, for an age cutoff of 40, an increase in cohort size of 10% generates a drop in marriage rates of 0.6-0.8%. This is a significant decline since it implies that a standard deviation increase in cohort size decreases the share ever married by 40 by a third to a half of a standard deviation. These findings indicate that cohort size can explain the medium and long term variation in marriage rates, but not the short term variation. Changes have to cumulate for longer than one or two years to generate significant fluctuations in marriage rates.

To summarize, our results indicate that in the time-series data there is a strong and negative relationship between marriage rates and cohort size. The results also indicate that changes in cohort size account for a large fraction of the medium-run and long-run time series variation in marriage rates. In the following subsections, we further explore the empirical link between cohort size and marriage rates using cross-state variation. Because cohabitation has become a close substitute for marriage since the early eighties, in the rest of the paper we will continue to use the same adjusted measure of household formation: the share ever married or cohabiting by a given age. Unless we specifically note otherwise, when we use the shorthand “ever married” we refer to those ever married or currently cohabiting.

4.3 Change in Marriage Rates Across States

In this subsection we provide additional evidence on the relationship between cohort size and household formation rates by using variation across states. The idea is that if changes in cohort size generate variation in marriage rates, we should observe such an effect not just across time but also across geography. We should observe that states with larger increases in cohort sizes experience larger drops in marriage rates.

To use cross-state variation, there is one issue we need to address that is not present in the longitudinal analysis. Changes in the size of a cohort at the time individuals are of marriageable age are endogenous because they are partially driven by migration decisions. These decisions are generally related to differences across states in economic and social conditions which also affect marriage rates. To exacerbate the problem, migration can be sex-biased. It can therefore skew sex-ratios and affect marriage rates directly. To address
this potential source of endogeneity, we use total births in a given year and state, which are arguably unaffected by the endogeneity issues discussed above, as an instrument for the size of the marriage market.

To perform the analysis at the state level, we rely on the decennial Census over the entire period of interest since it is the only dataset with sufficiently large sample sizes for all states. Appendix B provides details about how we construct the three main variables needed for the analysis: cohort size at birth, cohort size at marriageable age, and share ever married for each state and cohort. Using the Census data we can only use the decennial cohorts born between 1910 and 1970, since the share ever married can only be computed for them. We therefore perform the empirical analysis by first constructing ten-year differences for the log of share ever married, the log of cohort size at birth, and the log of the size of the marriage market for each cohort and state. We then use these variables in our regressions. Because of the small number of observations, we pool all cross-sections and regress the differences in log share ever married on the differences in log cohort size where log cohort size is instrumented using log cohort size at birth. We add year fixed effects to the regression to control for general time trends.

Table 3 presents the results of the cross-state regressions separately by gender. The first column presents the estimates obtained using a standard OLS regression. Similarly to the findings obtained using longitudinal variation, the estimated coefficient is negative and statistically significant for the two age cutoffs we consider, for both men and women. As argued above, the OLS estimates may be biased because of migration decisions. In particular, if migration is partially driven by the desire to find a spouse, the estimates will be downward biased. A similar result applies if migration is partially driven by job-related reasons. In this case, on average one would expect individuals to move to states with higher earning opportunities. If individuals with higher income are also more likely to marry, this would generate a positive correlation between cohort size growth and marriage rates, biasing the results away from the strongly negative relationship we predict.

We now describe the estimates obtained when we control for endogeneity by instrumenting the size of the marriage market using cohort size at birth. At the bottom of the table we present the first stage results. Two findings are worth discussing. First, the first stage suggests that cohort size at birth explains a large fraction of the marriage market size. Second, the coefficient on log cohort size at birth is estimated to be 0.44, which is well below 1. This indicates that cross-state migration plays an important role in changes in cohort size at marriage age. In column two we present our IV estimates. They are all negative, statistically significant and, as expected, larger in size than the OLS estimates. For the share ever married by 30, the coefficient is estimated to be -0.104 for women and -0.123 for men. When we use the age cutoff of 40, the coefficient drops to -0.058 for women and -0.047 for men. The size of the IV estimates are therefore similar to the ones obtained using
the longitudinal data. They also similarly decline when we increase the age cutoff. In the third column, we add time-region fixed effects to the IV specification to make sure that our findings are not driven by systematic differences in trends across regions. The coefficient estimates are similar to the ones presented in column 2, but we lose significance for men when we use an age cutoff of 40 because of an increase in the standard errors.

### 4.4 Potential Endogeneity Concerns

The findings obtained using cross-state regressions strongly corroborate the negative relationship between changes in cohort size and changes in marriage rates observed when longitudinal variation was used. However, there are reasons that prevent a causal interpretation of the relationship. While using number of births as our independent variable allows us to reasonably avoid reverse causality problems as well as important endogeneity concerns due to migration, one may nevertheless worry about omitted variables: state-level characteristics that drive changes in birth rates for a particular cohort as well as changes in marriage decisions of individuals that belong to that cohort 20 to 30 years later. Such omitted variables would have to be highly persistent shocks that affect growth in births in a given year as well as growth in subsequent marriage rates about 20 to 30 years later.

It is not easy to think of variables that fit this description. Potential examples include highly persistent productivity shocks which cause wages to grow more rapidly in some states over time, affecting both birth rates at the time of the initial shock and marriage rates two or three decades later. A positive trend in men’s earnings in some states fits this description. If children are a normal good, states with such positive trends may see increased births in 1950 relative to 1940 as well as a greater number of marriageable men in 1980 relative to 1970. Alternatively, improving fertility technologies may have had a differential effect in states that are strongly religious or have stronger preferences for forming a family compared to states that do not. In states with weaker preferences for family, one might expect depressed birth rates as well as lower marriage rates in the future.

Note that in these and most credible cases we would typically expect an increase in both births and subsequent marriage rates or a decline in both variables. This bias would work against our favor and would result in a positive coefficient on cohort size, which is not what we find. Nevertheless, without exogenous variation in cohort size we cannot entirely eliminate the possibility that some biases could work in our favor.

### 4.5 Instrumental Variables Strategy

To address the potential endogeneity issues outlined in the previous subsection, we construct an instrument which is based on an idea first proposed by Bailey (2010). The idea is to use the interaction between the introduction of Enovid in 1957, later known as the birth control pill, and cross-state variation in anti-obscenity laws, which limited the use of
contraception, to generate exogenous variation in number of births and therefore cohort size.

In 1873, the U.S. Congress enacted the Comstock Act which had two main goals. The direct objective was to ban the interstate mailing, shipping, and importation of products and printed materials that were considered to be “obscenities”. Since the Act considered anything employed for the prevention of conception an obscenity, it outlawed any interstate transaction involving contraceptives. The indirect objective was to “incite every State Legislature to enact similar laws” as stated by U.S. Representative John Merriman during an interview with the New York Times on March 15, 1873. The Comstock Act was highly successful in achieving this goal. By 1900, 42 states had approved anti-obscenity laws and by 1943 the number of states had increased to 48.

There is considerable variation across states in the type of anti-obscenity statutes that were enacted. As a consequence, these laws had different effects on the introduction of the pill in different states. As suggested by Bailey (2010), the states can be grouped into four categories depending on the type of law they enacted. The first group includes states that explicitly banned the sale, advertisement, and distribution of information of any product for the prevention of conception. This category includes seventeen states. The second group consists of all states that had the same ban on sales, advertisement, and distribution of information as the first group of states, but added an exception for physicians and pharmacists who were allowed to sell, advertise, and distribute information on products and materials related to birth-control methods. Seven states belong to this category. The third category includes states that explicitly only banned the advertisement and distribution of information of products and materials for the prevention of contraception, but did not outlaw their sale. Six states enacted this type of statute. The final group is composed of states that approved a law that banned the sale, advertisement, and dissemination of information of obscene products and materials, without explicitly classifying the prevention of conception as obscene. This category includes eighteen states. In our analysis, we refer to states in the first two groups as having a sales ban. We control explicitly for whether or not a state had a physician exception.

An important question is whether some states enacted stricter anti-obscenity laws because they had more conservative constituencies or because of other observable cross-state differences. Bailey (2010) provides evidence that this is not the case. For instance, among the states that adopted sales bans of contraceptives one can find both typically conservative and typically liberal states. California and Washington, two of the states that repealed anti-abortion laws before the Roe v. Wade decision, enacted the strictest version of the bans whereas Alabama, a generally conservative state, adopted a statute that did not explicitly categorize the prevention of conception as obscene.

These anti-obscenity state laws lasted until the sixties when they were repealed or struck down by the individual states or by the 1965 U.S. Supreme Court’s decision in Griswold
v. Connecticut. Specifically, two states repealed their anti-obscenity statutes in 1961, one state in 1962, four states in 1963, and Connecticut in 1965 after the U.S. Supreme Court’s decision. Griswold v. Connecticut expedited the repeal of anti-obscenity statutes in all the remaining states between 1965 and 1971. In the empirical part, we follow Bailey (2010) and use the period between 1957 and 1965, the year of the U.S. Supreme Court decision, as the period in which the introduction of Enovid interacted with the anti-obscenity laws generated what is arguably exogenous variation in cohort size.

We will start by giving some descriptive and graphical evidence on the effect of the source of variation described above on our variables of interest. To do this, we divide the states in two groups: states that enacted sales bans of anti-conception methods and the remaining states. Before presenting the evidence, it is important to emphasize a difference between this paper and Bailey (2010). Bailey is interested in the relationship between the anti-obscenity laws and birth rates of married women after the pill was introduced. She finds that states with the sales ban experienced a marital birth rate that was 8% higher than the remaining states during the period 1957-1965. In this paper we are interested in the link between the anti-obscenity laws and the following two variables: cohort size at birth and cohort size at marriage age. Differences in the growth of cohort size at birth between the two groups of states provide the first evidence of the effect of the Comstock laws on cohort size between 1957 and 1965. We find that in states with the sales ban, the growth in cohort size at birth was about three percent higher than in states with no ban.

We will now show graphically the relationship between the anti-obscenity laws and cohort size, starting with cohort size at birth followed by cohort size at marriage age. In Figure 5, Panel A, we report the difference in growth of cohort size at birth between states with the sales ban and the remaining states from 1950 to 1970. We follow Bailey (2010) and present the graphical results separately for the four Census regions. There are two features that are worth discussing. First, after the introduction of the pill, in all regions, states with the ban experienced larger growth in cohort size at birth. The figure suggests that in the South the states with the ban were reducing the gap in number of births with the states without ban before the introduction of the pill. But it also suggests that the process was expedited by the introduction of the pill. The second noteworthy feature is that in all regions, when states started to outlaw the sales bans on contraceptives, the growth in cohort size at birth in states with the ban started to converge to the growth in states with no ban. The convergence continues until 1965 when the Griswold v. Connecticut decision took place, at which point the two groups of states have similar rates of growth in cohort size. These two features taken together generate the hump shape in the difference in growth of number of births that characterizes all census regions.

In Figure 5, Panel B, we replace cohort size at birth with cohort size at age 25, which represents a measure of cohort size at marriageable age. The figure displays patterns that
are similar to the ones observed for cohort size at birth. Between 1957 and 1965, in all regions the difference in growth in cohort size at 25 between the two groups has the familiar hump shape that was observed in the previous graph. This indicates that cohort size at the time of marriage was affected in the expected way by the interaction of the Comstock laws with the introduction of the pill: it increased the growth in size of cohorts born in states with the ban in the period considered until the anti-obscenity laws started to be repealed. The growth in cohort size in these states then started to converge to the growth experienced by states with no ban.

Figure 5, Panel B, describes graphically the first stage of an IV regression where the introduction of the pill interacted with the sales bans is used as an instrument. We will now formally use that variation in a standard IV setting. To do that, we construct two dummy variables. The first one, \( \text{ban}_s \), is equal to one for all cohorts from state \( s \) that adopted a sales ban on contraceptives and zero otherwise. The second dummy variable, \( \text{ban}_s \times \text{pill}_{c,s} \), takes a value of one if a cohort \( c \) was born between 1957 and 1965 in a state \( s \) that enacted a contraceptive ban and zero otherwise. Then in the first stage we regress the \( n \)-year difference in log cohort size at the time of marriage on the two dummy variables and a set of controls, i.e.

\[
\log \frac{y_{c,s}}{y_{c-n,s}} = \alpha + \beta_1 \text{ban}_s + \beta_2 \text{ban}_s \times \text{pill}_{c,s} + \sum_{c,r} \pi_{c,r} + X' \gamma + \varepsilon_{c,s},
\]

where \( y_{c,s} \) is the size of cohort \( c \) in state \( s \), \( y_{c-n,s} \) is the same variable for cohort \( c-n \), \( \pi_{c,r} \) are cohort-region fixed effects, and \( X' \) is a set of control variables that includes an indicator equal to 1 if the state had a physician exception, the physician indicator interacted with \( \text{pill}_{c,s} \), and an indicator equal to 1 if the state enacted an advertising ban on contraception.

The results of the first stage are presented in Tables 4, where we report the effect of the anti-obscenity laws on 1-year, 3-year, 5-year, and 7-year differences in log cohort size. Consistent with the graphical evidence provided in Figure 5, Panel B, we find that after the introduction of the pill and before the repeal of the Comstock laws, the sales ban on contraceptives had a positive and statistically significant effect on cohort size at marriage age in all cases. Consistently with the graphical evidence, the effect increases when we go from a 1-year difference to a 5-year difference. For the 1-year difference, a sales ban increases cohort size by 0.012%, whereas the 5-year differences raises cohort size by 0.041%. The coefficient on the 7-year difference is similar in size. The F-tests to evaluate the strength of the instruments are between 10.11 and 19.22 in our four specifications. An additional result is that the coefficient on \( \text{ban}_s \) is always small and statistically insignificant suggesting that the sales ban had no effect on cohort size before the introduction of the pill. This finding is consistent with Bailey’s results which indicate that the Comstock laws had no effect on other forms of contraception.

In the second stage, we use a specification similar to the one employed with the cross-
state variation, i.e.,

\[ \log \frac{\text{mar}_{c,s}}{\text{mar}_{c-n,s}} = \beta_0 + \beta_1 \log \frac{\text{size}_{c,s}}{\text{size}_{c-n,s}} + \sum_{c,r} \pi_{c,r} + X'\gamma + \varepsilon_{c,s}, \]

except that now we instrument cohort size growth with \( \text{ban}_s \) and \( \text{ban} \times \text{pill}_{c,s} \). Before presenting the results, it is important to remark that to construct the share ever married we must use the decennial Censuses since the CPS does not have enough state-level observations. The decennial Censuses have two limitations. First, in principle the share ever married can be computed for each cohort born in a particular state if one observes in the Census data a recall variable measuring the age at first marriage. Unfortunately, after 1980 this variable is not available in the Censuses. The second limitation is that in each decennial Census we only observe a particular cohort at a particular age. We therefore cannot directly compute the share ever married for each cohort. Instead, we rely on the following strategy. In each Census, we first consider all individuals between the ages of 25 and 45. We then compute the share ever married for each cohort born in a particular state. Notice that we cannot use this variable directly in our regressions because it is affected by the age at which we observe a particular cohort in a particular Census. To address this issue, we regress the computed share ever married on age, state, cohort, and cohort-region dummies. We then remove the effect of age by subtracting the estimated coefficient on the age dummy multiplied by the dummy itself. Finally, we use the constructed variable in our regressions.

The second stage results are reported in Table 5 for men and women separately. The coefficient estimates have the expected negative sign, are statistically different from zero, and large in magnitude. They indicate that during the period considered a 1% increase in cohort size at marriageable age generated a reduction in marriage rates between 0.24% and 0.44%. The point estimates in the IV regressions are somewhat larger in size than the corresponding estimates using the longitudinal or cross-sectional variation. Note, however, that we would expect the IV coefficients to be negative and larger in magnitude given the discussion in section 4.4 on potential endogeneity concerns, since the most plausible omitted variables would bias the coefficients positively toward zero. We conclude that the IV findings are consistent with the results obtained using longitudinal and cross-state variation and they suggest that there is a causal relationship between changes in cohort size and changes in marriage rates.

We conclude this section with a discussion of a potential threat to our IV strategy. It is possible that the negative relationship between cohort size and share ever married we find in our IV regressions is generated by some type of selection process governing who becomes a mother in states without the ban after the introduction of the pill. The most serious hypothesis that could confound the interpretation of our results is that, after the introduction of the pill, mothers in states without the ban give birth to fewer children who
are positively selected along some dimension. If those children are more likely to marry, as
the literature suggests, our IV regressions will estimate a negative relationship between our
two main variables. To evaluate this hypothesis, we follow Ananat and Hungeman (2012)
and test whether children born in states where the pill was banned are more or less likely
to have low birth weight. We employ the same specification used in the first stage of the
IV estimation except that, to make our results comparable with Ananat and Hungeman
(2012)’s findings, we use levels instead of differences for the following two new dependent
variables: the share of children born with extremely low birth weight, which is defined as a
birth weight below 1500 grams, and the share of children with low birth weight, which is a
birth weight below 2500 grams. The estimation results are reported in Table 6. Using both
dependent variables, the estimated coefficient on the interaction between the ban and the
introduction of the pill is small and statistically insignificant. We find therefore no evidence
that the initial access to the pill had an effect on the fraction of children born with low
weight. Our result is different from the one obtained in Ananat and Hungeman (2012),
where the authors find that initially access to the pill increased the share of children born
with low weight. But the sample used is also different. Here, we consider the sample of
married women, whereas Ananat and Hungeman (2012) study the behavior of single women
younger than 21. The different results can therefore be rationalized by a more widespread
early use of the pill by married women relative to young single women.7

Ananat and Hungeman (2012) also find weak evidence that early access to the pill had
the effect of increasing the share of children born in poor families, suggesting that the young
women in their sample period who reduced their unwanted pregnancies using the Pill may
have been positively selected. Bailey (2013) tests for such selection during our relevant
sample period in the 1960s using data from the Integrated Fertility Survey Series (IFSS).
The IFSS includes socioeconomic measures such as race and education, as well information
about the wantedness and timing of pregnancies and births of female respondents. Bailey
documents that Pill usage in the early 1960s was concentrated among women in married
households, as expected. However, Bailey does not find evidence that reductions in unwanted
births were higher for highly-educated women. Finally, we want to note that an increase
in the share of children born in poor families would be a threat to our IV estimates only
if children born in low income families are more likely to marry. In this case, the negative
selection would generate the negative relationship between cohort size and marriage rates
we observe in the data. But the literature on household formation appears to rule out this
alternative.8

7 A second difference between our paper and Ananat and Hungeman (2012)’s paper is that we use the
interaction between bans on contraception and the introduction of the pill as our main source of exogenous
variation, whereas Ananat and Hungeman (2012) use restrictions on access to the pill for minors. Using
the same exogenous variation as we do, but a slightly different specification, Bailey (2013) similarly finds no
evidence that the birthweight of infants born in the 1960s changed differentially in states where selling the
Pill was legal and in states where it was not.

8 For instance, the handbook chapter by Black and Devereux (2010) indicates that there is a positive
In this section, we have provided evidence on two main results. First, there is a causal negative relationship between cohort size and marriage rates of women. Second, there is a similar causal negative link between cohort size and marriage rates of men. Our findings differ from the results presented in Akers (1967), Schoen (1983), and Bergstrom and Lam (1989). Akers (1967) and Schoen (1983) use simulated data to suggest that there is either a positive relationship between cohort size and marriage rates of men or no relationship between those two variables. Bergstrom and Lam (1989) argue that changes in cohort size affect the marriage market primarily by affecting the age difference between spouses, and construct a model that can match the relationship observed in the data between those two variables. Their model, however, holds fixed the share that ever marry in every cohort. As a result, they only provide an intuition that the proportion of men ever marrying should increase when cohort size increases, which is counter to what we observe in the data.

The rest of the paper proposes and tests potential mechanisms that can explain our empirical results.

5 A Dynamic Search Model of the Marriage Market

In this section, we develop and test a model that has the potential of generating the two empirical patterns observed in the data. The theoretical and empirical literature that has studied the marriage market has used for the most part matching models to analyze marital patterns. Some examples of papers in that literature are Gale and Shapley (1962), Becker (1973), Becker (1974), Mortensen (1988), Bergstrom and Lam (1989), Bergstrom and Bagnoli (1993), Angrist (2002), Peters and Siow (2002), Choo and Siow (2006), Chiappori, Iyigun, and Weiss (2009), Hitsch (2010), Iyigun and Walsh (2007), and Abramitzky, Delavande, and Vasconcelos (2011). The standard matching model, however, has one major limitation. It is rejected by the data since, as argued in Section 2, it predicts that an increase in cohort size should raise the marriage rate of men, which contradicts one of our findings.

For this reason, in this section we construct and test a dynamic search model of the marriage market. A priori, this model has the potential of producing the patterns observed in the data for reasons that will be discussed in the next subsection. In addition, this type of model enables us to easily capture the dynamic nature of the marriage market which, we believe, is an essential part of the mechanism behind the empirical results documented in this paper.
5.1 Characterization of the Model

We start by developing the simplest possible version of a dynamic search model. We then evaluate whether it can generate the empirical patterns observed in the data.

The model we propose considers an economy populated by \(T+1\) overlapping generations of men and women. In each period \(t = \{0, \ldots, T\}\), a new generation is born and lives for \(T+1\) periods. We will denote with \(N^w_{0,t}\) and \(N^m_{0,t}\) the size of the new generation of women and men. Throughout the section we will assume that women and men have the same cohort size \(N_{0,t}\), which is a good assumption for the white population. In the Appendix we consider the more general case in which \(N^w_{0,t} \neq N^m_{0,t}\). Men and women can be either single or married. If an individual is married she or he makes no choice. If in period \(t\) an individual of gender \(i\) and age \(a\) is single, she or he meets a potential spouse with probability \(\theta^i_{a,t}\).

The within-period utility of being single is denoted by \(\delta\), whereas the within-period utility of being married for the couple as a whole is denoted by \(\eta\). The value of being married is drawn from a distribution \(F(\eta)\) which does not depend on the age of the couple or time. The utility from future periods is discounted at the discount factor \(\beta \leq 1\). We will assume that the value of being single is constant across individuals and over time. If two potential partners decide to marry, the within-period utility they have drawn is also the utility they will experience in each period for the rest of their life or until they divorce. Divorce can occur in each period with a probability \(q\) which does not depend on the within-period utility, time, or age. At the end of the subsection, we will discuss the consequences of allowing the probability of divorce to depend on the quality of the marriage. We will assume that the couple can freely divide the gains from marriage and that its lifetime utility is split between the two spouses using a Nash-bargaining solution. We allow the Nash-bargaining solution to be asymmetric and denote with \(\gamma \in [0,1]\) the parameter that determines how the marriage surplus is divided.

We can now introduce the main assumption of the model. We assume that women meet men with a positive probability only in their first period of life, while men meet a potential spouse with a positive probability in their first two periods of life. Two ideas form the basis for this assumption. First, women’s fertile lifespan is shorter than men’s since women are fertile only in the first part of their adult life, whereas men are fertile for most of their adult life. Second, one important benefit of marriage is that it is an effective arrangement for having and raising children. These two ideas imply that, with age, the value of getting married for a woman declines faster than the value for a man. Our assumption that this value for a woman is zero in the second part of her adult life is a special case of an economy in which the value of marriage for women and men follow this pattern. Our main assumption has two implications. First, the marriage market is populated by women of age 0 and by
men of age 0 and 1. We will refer to women and men of age 0 as younger women and men and to men of age 1 as older men. Second, women cannot change their marital status after the first period and men cannot change it after the second period. Allowing women to marry for more than one period with a declining value of marriage and men to marry for more than two periods makes the model more complicated without changing the qualitative nature of the results.

The solution of search models is generally provided in terms of reservation values. In our model, the relevant reservation value is the match quality $\eta$ at which a pair of potential spouses is indifferent between marrying or staying single. In our context, two types of couples can form in the marriage market: couples in which the woman is younger and the man is older and couples in which the woman and the man are both younger. As a consequence, our model is characterized by two reservation utilities. To derive them we will denote the probability that an older man meets a woman with $\theta_{1,t}$ and the corresponding probability for a younger man with $\theta_{0,t}$.

In the Appendix, we show that a couple with an older men has the following reservation match quality:

$$\eta_{1,t} = 2\delta.$$  

The intuition behind this result is straightforward. A couple composed of a woman and an older men are indifferent between marrying and remaining single if the match quality they can enjoy if married is equal to the sum of their values of staying single and they choose to marry if the drawn match quality $\eta$ is greater than $2\delta$. The reservation value of a couple in which the man is younger is slightly more complicated to derive because a younger man has the option value of waiting until next period and drawing a new potential spouse. In the Appendix, we show that the existence of this option value generates a reservation utility for this type of couple of the following form:

$$\eta_{0,t} = A + B\theta_{1,t+1},$$  

where $A$ and $B$ only depend on the parameters of the model. The option value is included in the term $B\theta_{1,t+1}$, which measures the probability that a younger man will meet a woman when older multiplied by the share of the expected marital surplus he will receive times the probability he will choose to marry her.

We will now use the derived reservation utilities to solve for the steady state equilibrium in the marriage market. We will then evaluate the impact on marriage rates of a change in cohort size from its steady state level. To solve for the steady state equilibrium, we have to derive the probability that a younger man meets a woman $\theta_{0,t}$ and the corresponding probability for an older man $\theta_{1,t}$. Let $N_{i,t}^{a}$ be the number of individuals of gender $i$, age $a$, in period $t$ who are present in the marriage market. Then, under our assumption that men
and women have identical cohort size, $\theta_{0,t}^m$ and $\theta_{1,t}^m$ can be derived by noting that

\[ \theta_{0,t}^m = \theta_{1,t}^m = \frac{N_{0,t}^w}{N_{0,t}^m + N_{1,t}^m} = \frac{N_{0,t}}{N_{0,t} + N_{1,t}^m}. \]  

The probability $\theta_{0,t}^m$ is the correct measure in our model of the variable sex-ratio. Equation (3) shows that an increase in cohort size $N_{0,t}$ increases the sex-ratio, as argued earlier in the paper.

The number of individuals of age 0 is exogenously given by the cohort size of a generation. However, the number of old men in the marriage market $N_{1,t}^m$ is endogenously determined by the decisions of younger men. As a consequence, to derive $\theta_{0,t}^m$ and $\theta_{1,t}^m$ we need to solve for $N_{1,t}^m$. This variable can be computed as the number of younger men who did not meet a woman at $t-1$ plus the number of younger men who met a woman at $t-1$ but drew a match quality $\eta$ lower than the reservation value, i.e.

\[ N_{1,t}^m = N_{0,t-1} \left( 1 - \theta_{0,t-1}^m \right) + N_{0,t-1} \theta_{0,t-1}^m F (\eta_{0,t-1}). \]  

In the Appendix we show that, in steady state, equation (4) simplifies to

\[ N_{1,t}^m = N_0 F (\eta_0)^{\frac{1}{2}}. \]

Substituting for $N_{1,t}^m$ in the equation defining the meeting probabilities $\theta_{a,t}^m$, we obtain

\[ \theta_{0,t}^m = \theta_{1,t}^m = \frac{N_0}{N_0 + N_0 F (\eta_0)^{\frac{1}{2}}} = \frac{1}{1 + F (\eta_0)^{\frac{1}{2}}}. \]

To determine the reservation value of younger men in steady state, we can substitute for $\theta_{1,t}^m$ in the equation that determines the reservation value (2) and obtain

\[ \eta_{ss} = A + B \frac{1}{1 + F (\eta_{ss})^{\frac{1}{2}}}. \]

Note that $F (\eta)$ is monotonically increasing in $\eta$. As a consequence, there is a unique solution for $\eta_{ss}$ and hence a unique steady state equilibrium. Moreover, in the steady state equilibrium the reservation value is independent of cohort size $N_0$. The following Proposition summarizes the result.

**Proposition 1** In steady state, there is a unique reservation value for marriage $\eta_{ss}$ and hence a unique equilibrium. Moreover, the reservation utility does not depend on cohort size.

Now that we have characterized the steady state equilibrium we can study the effect of a change in cohort size first on the marriage rate of women and then on the marriage rate of men. We will focus on the case in which the shock is unexpected. Similar results apply if the shock is known with certainty.
Suppose the steady state economy is hit by an unexpected shock in period \( t = \tau \) that changes permanently the cohort size from \( N_0 \) to \( N_0 + \Delta \). We consider the case of a permanent shock because in the data changes in cohort size tend to be persistent and even reinforcing. The change in cohort size affects the marriage rates by changing the probability that an individual meets a potential spouse and, as a consequence, the reservation match quality of younger men. The following Proposition establishes that an increase in cohort size raises the reservation utility of younger men.

**Proposition 2** A positive and permanent shock to cohort size in period \( \tau \) increases the reservation value \( \eta_{0,\tau} \). A negative shock has the opposite effect.

**Proof.** In the appendix. ■

The intuition behind this result is straightforward. With a permanent increase in cohort size, men are more likely to meet a woman when old. As a consequence, the option value for younger men of waiting until next period goes up and with it their reservation match quality.

Using Proposition 2 we can now study the effect of a cohort shock on the marriage rate of women and men. The following Proposition determines the effect for women.

**Proposition 3** A positive and permanent shock to cohort size in period \( \tau \) reduces the fraction of cohort \( \tau \) women who get married. A negative shock in period \( \tau \) has the opposite effect.

**Proof.** In the appendix. ■

To provide the insight behind this result, consider an increase in cohort size. After this event, older men become a scarce resource. This change has two effects. First, the fraction of women who marry mechanically declines because women are now less likely to meet older men. Second, younger men become more selective because they will have a larger group of women to choose from when they are older. As a consequence of this second effect, the fraction of women who marry decreases. The total impact of an increase in cohort size is therefore a reduction in the fraction of women who marry. This result indicates that the search model developed in this paper can explain the negative relationship observed in the data between cohort size and marriage rates.

The following Proposition establishes that the search model is not as good at generating the observed patterns for the marriage rates of men.

**Proposition 4** A positive and permanent shock to cohort size in period \( \tau \) increases the fraction of cohort \( \tau \) men who get married. A negative shock in period \( \tau \) has the opposite effect.

**Proof.** In the appendix. ■
Proposition 4 contains a negative result. Since our simple dynamic search model generates a positive relationship between cohort size and marriage rates of men, it cannot explain the patterns observed in the data. We want to point out, however, that without a formal proof this result is not obvious. To understand why, observe that an increase in cohort size has three different effects on men’s decisions. First, after the shock, in relative terms, there are fewer older men. Second, the probability that a younger man meets a woman increases with cohort size. Lastly, younger men become more selective because they will have more women to choose from when they are old. The first two effects go against our empirical finding since they imply an increase in the marriage rate of men when cohort size increases. But the last effect is in our favor and generates a decline in the marriage rate of men as a consequence of the increase in cohort size. Proposition 4 establishes that the first two effects always dominate, therefore rejecting our simple search model as a framework that can explain the observed patterns.

The previous discussion also suggests that, if the probability of divorce is modeled as a decreasing function of match quality, the negative result presented in Proposition 4 should still hold. In the search model, the only effect that goes in our favor is the increase in reservation utility that follows the positive shock to cohort size. With a probability of divorce that declines with the actual value of marriage, younger men will be more selective because the value of marrying a woman with high match quality is larger. As a consequence, the reservation utility of younger men will increase. But the increase will be similar for all cohort sizes, implying that the marriage rate of men will still rise with cohort size.

Our results clearly reject the simple search model developed in this section. In the next subsection, however, we will show that, with a couple of changes, the search model can explain the observed patterns. Specifically, we will present two modified versions of the search model and show that both models can generate the two main patterns documented in the data. We will then derive a testable implication for the two models based on the relationship between cohort size and divorce rates and provide evidence that only one of the two versions is consistent with the data.

5.2 Two Augmented Dynamic Search Models of the Marriage Market

There are two possible ways of reconciling the proposed dynamic search model with our empirical findings. The first possibility is to introduce a positive correlation between the value of being single and cohort size. The economic idea behind the positive correlation is straightforward: when cohort size increases is more enjoyable to be single because there are more people of the same age with whom to perform different types of leisure activities. We will refer to this model as the $\delta$-model. The alternative way of reconciling the search model with the data is to allow men to undertake a costly investment that increases their probability of meeting a woman and, in case they choose to marry, the corresponding marital
surplus. The economic insight underlying this model is that men can work longer hours or choose higher-paid jobs with fewer amenities to increase the probability of meeting a potential spouse and their marital surplus. In this model, the fraction of men who select to invest is higher when cohort size is relatively low, since in this case the probability of meeting a woman will generally be lower than optimal for a higher fraction of them. We will denote this model with the term \( \eta \)-model.

These two models have one common feature. In both of them, changes in cohort size generate an additional negative effect on marriage rates. But they achieve this in two different ways. The \( \delta \)-model does this by increasing the value of the outside option when cohort size is high, whereas the \( \eta \)-model achieves this by increasing the value of marriage when cohort size is low. This common feature has three main implications. First, both models have the potential of explaining the two main empirical findings of our paper. The reason for this is that the new feature strengthens the negative relationship between cohort size and marriage rates generated by the simple search model for women. It also introduces a new effect for men which, if strong enough, can outweigh the effects that in the search model produce the positive relationship between cohort size and marriage rates of men.

The second implication of introducing the new feature is that the models can generate the observed patterns only for some parameter choices. If the additional negative effect is not strong enough, the two models will produce the same positive relationship between cohort size and marriage rates of men that characterizes the simple search model for women. It is therefore not productive to search for a general proof showing that our models can produce the observed patterns for men. Instead, we will calibrate the models and evaluate whether they can match our empirical findings for a realistic set of parameters.

The third implication of the new feature is that, if the probability of divorce declines with the match quality of the marriage, our two models generate relationships between cohort size and divorce rates of opposite sign. The \( \delta \)-model predicts a negative relationship between cohort size and divorce rates. When cohort size declines, the value of being single becomes lower. As a consequence, the average match quality of formed marriages drops, which increases the probability that a marriage will end in divorce. The \( \eta \)-model has the opposite prediction. When cohort size decreases, men invest more and, by doing so, they increase the match quality of their marriage. The probability of divorce will therefore decline, generating a positive relationship between cohort size and divorce rates. We will use this result to test the \( \delta \)-model against the \( \eta \)-model. To implement this test, we need to slightly modify the way divorce is modeled and allow its probability to depend on match quality. As argued at the end of the previous subsection, this change should have no effect on the rejection of the simple search model based on Proposition 4. To confirm this, in the Appendix we report the results obtained by simulating the original search model when the probability of divorce is an decreasing function of match quality.
We will now describe how we modify the search model to obtain the \(\delta\)-model and \(\eta\)-model. To derive the \(\eta\)-model we implement one simple change. We allow the value of being single to be an increasing function of cohort size, where the function takes the following form:

\[
\delta = \alpha_0 + \alpha_1 N_0.
\]

The linear functional form has been chosen for simplicity. We have also experimented with alternative functional forms that are increasing in cohort size with similar results. The new parameters \(\alpha_0\) and \(\alpha_1\) affect the share of individuals who choose not to marry. Everything else equal, a higher value of \(\alpha_0\) increases the share of people who choose to remain single independently of cohort size, whereas a higher value of \(\alpha_1\) raises proportionally more the share of never married individuals born in larger cohorts. We therefore calibrate these two parameters using the following two moments: the minimum share of individuals never married in a cohort, where the minimum is computed over our time period, and the average share never married, where the average is computed across cohorts.

The derivation of the \(\eta\)-model is slightly more complicated. In the \(\eta\)-model, men make one additional decision. When young, before meeting a potential spouse, they choose whether to undertake the investment that increases their chances of getting married. If they choose to invest, they pay a cost \(c_i\) which is individual specific and takes the following functional form:

\[
c_i = \mu_0 + \mu_1 (1 + x_i)^{\mu_2},
\]

where \(x_i\) is drawn from a uniform defined on the interval \([0, 1]\). The power coefficient \(\mu_2\) is useful to match how the share of men who invest changes with cohort size. Without this parameter, or if this parameters is too low, the model generates overly large swings in investment due to the following amplification effect. When cohort size declines and the probability of meeting a woman falls, more men choose to invest. The investment further lowers the probability of meeting a woman for the remaining men who did not invest, increasing their incentives to undertake the investment.

The investment has two benefits. First, men who invest meet a woman with probability \(\theta_h\), whereas men who do not undertake the investment meet a woman with probability \(\theta_l\), with \(\theta_h > \theta_l\). In the model, \(\theta_h\) is set equal to a constant, while \(\theta_l\) is endogenous and equal to the number of younger women who did not meet a man who invested divided by the number of men who chose not to invest. The second benefit of the investment is that a man who invests experiences a higher level of match quality in case he marries his potential spouse. Specifically, if he and his potential spouse draw a match quality \(\eta'\), during their marriage they enjoy a within-period value of marriage \(\eta = \eta' + K\), where \(K\) is a positive constant. The idea behind this modeling choice is that men who invest have higher income and wealth and can therefore afford to buy more public goods when married.
The new parameters of the $\eta$-model are calibrated as follows. The investment parameter $K$ affects the share of younger men who choose to marry since, for a larger $K$, they would forgo in the current period a larger increase in their marital utility. We therefore calibrate this parameter by matching the share of men who marry when younger, which we assume to be by age 30. The cost parameters $\mu_0$, $\mu_1$, and $\mu_2$ influence the distribution across cohorts of the share of men who invest. While we cannot observe such a measure directly, we use as a proxy the share of men working full-time and full-year or studying at age 25 and match its distribution over cohorts. Lastly, in the data we do not observe the probability with which a man who invests meets a woman. But we know that, to match the marriage patterns in the data, $\theta_h$ must be larger than $\theta_l$. If not, men would choose either not to invest or to invest in periods with high cohort size, since those are the periods in which men have a higher probability of meeting a woman and, hence, the periods in which they are more likely to enjoy the higher match quality. For this reason, we have experimented with values of $\theta_h$ that are between 0.7 and 1, obtaining similar results. The simulations presented in this paper have been generated using $\theta_h$ equal to 0.85.

In both the $\eta$-model and the $\delta$-model, we allow the probability of divorce to decline with match quality using the following linear functional form:

$$q(\eta) = \gamma_0 + \gamma_1 \eta,$$

where $\gamma_1 \leq 0$. The parameter $\gamma_0$ affects the share of households who choose to divorce independently of match quality. The parameter $\gamma_1$, instead, influences how the share of divorces changes with match quality. To calibrate those two parameters, we therefore restrict $\gamma_0$ and $\gamma_1$ so that $0 \leq q(\eta) \leq 1$ for all values of $\eta$ and calibrate them by matching the following two moments: the minimum share of households in a cohort ever divorced over our time period and the average share of individuals ever divorced. To construct the share ever divorced, we need one additional variable, the number of times an individual was married, which is available in the American Community Survey (ACS) but only for the years 2008-2013. In those years, we consider all individuals between the ages of 50 and 75, i.e. individuals who are sufficiently old to have had a chance to experience a divorce. We then construct the share ever divorced for all cohorts born in years that are present in our ACS sample, which are the cohorts born between 1933 and 1963.

A couple of additional parameters are common to the $\delta$-model and $\eta$-model. In both frameworks, we choose the parameter governing how the marital surplus is allocated between spouses by assuming that Nash-bargaining is symmetric and, hence, by setting $\gamma$ equal to 0.5. Moreover, we assume that the distribution of match quality $F(\eta)$ is uniform in the interval $[0, 1]$.

In Figure 6, Panel A, we document the ability of the $\delta$-model to replicate the patterns observed in the data. The model can generate the main empirical results of the paper.
Similarly to the simple search model, the relationship between cohort size and marriage rates of women is negative. But differently from the search model, the same relationship for men is now negative, which matches the data. In Figure 6, Panel B, we report the simulation results for the $\eta$-model. This alternative specification of the search model does also an excellent job in matching our two main empirical findings.

Since both proposed models can match well our empirical results, we will now use the testable implication based on the relationship between cohort size and divorce rates to evaluate which framework is a better characterization of the data. We argued at the beginning of this subsection that the $\delta$-model and $\eta$-model have opposite predictions for the relationship between cohort size and divorce rates: the $\delta$-model predicts a negative relationship, whereas the $\eta$-model predicts a positive relationship. Figure 7, Panel A, documents the evolution in the data of the share of households ever divorced for cohorts born between 1933 and 1963. The figure shows that there is a strong and positive relationship between cohort size and the share ever divorced for both women and men. Individuals born between 1933 and the first half of the fifties experienced increasing cohort size and rising divorce rates, whereas people born in the second half of the fifties and the first half of the sixties are characterized by declining cohort size and falling divorce rates. The evidence provided in Figure 7 and the previous discussion clearly implies that the $\delta$-model is rejected in favor of the $\eta$-model.

Figures 7B and 7C, which report the simulated share ever divorced for the $\delta$-model and $\eta$-model, confirm this conclusion. The $\eta$-model is the only framework able to generate the positive relationship between cohort size and share ever divorced. In that model, the share of women and men ever divorced peaks a bit early relative to the data, but otherwise it does a good job at matching the observed patterns.

An interesting feature of the data displayed in Figure 7 is that, when the time-series of cohort size starts to flatten, the share of women ever divorced crosses from below the share of men ever divorced. Remarkably, the $\eta$-model can generate the same type of crossing. Two features of the $\eta$-model produce the crossing: women marry on average older men and the share of men in a cohort who invest declines with cohort size. Recall that, all else equal, the marriages with the lowest probability of divorce are those in which the man has made an investment. In the first half of the thirties, cohort sizes, after falling for over a decade, began to increase. As a result, the fraction of men who invested declined and their divorce rates started to increase. During the same period, however, the rise in the share of women ever divorced increased at a slower rate. To understand why, observe that a fraction of those women married older men who made the investment decision when cohort size was still low. As a consequence, a significant share of the women born in the thirties are in a marriage with an investment and, hence, with a probability of divorce that is lower than for the men born in the same cohorts. During the thirties and beginning of the forties, the fraction of those marriages dropped steadily. Accordingly, in Figure 7, Panel C, the difference between the
probability of divorce of women and men gradually shrinks until the two variables become approximately equal for cohorts born around 1950. Cohorts born after 1950 display the opposite pattern. Men began investing again at increasing rates as they experienced first a flattening in cohort size growth and then a significant drop. Correspondingly, divorce rates of men began to fall. The effect of the evolution of cohort size on the divorce rate of women is not as strong. A share of women born in this period married older men who undertook the investment when cohort size was still increasing at a faster rate. As a consequence, the share of women ever divorced stays above the share of men for the rest of the sample period.

We end by noting that divorce rates start to fall earlier in the simulations than in the data. One possible explanation for the difference is that there were significant changes in divorce laws in the 1970s, when cohorts born in the 1950s were entering the marriage market. Since the model abstracts from this aspect of the data, it is to be expected that our simulations generate lower divorce rates, especially for cohorts born in the fifties and later years. But it is remarkable that we can match the general pattern of the observed divorce rates using only cohort size without considering the legal changes to the marriage contract that took place during our sample period.

The results presented in this section indicate that a simple dynamic search model with investment can generate the marriage and divorce patterns observed in the data. This is not an easy task, since some of the patterns, such as the crossing in the divorce data, are not a priori easy to explain.

In principle, it should be possible to explain the same patterns by adding investment to the standard matching model. We have used the search model instead, because it is better suited to capture how changes in cohort size affect the sex-ratio and, hence, marriage and divorce decisions. Our results should, therefore, not be interpreted as a rejection of the matching model in favor of the search model. They should be understood as indicating that the effects of cohort size on marriage decisions are sufficiently complicated that a standard search or matching model cannot explain them. To rationalize the data an additional force must be added that increases the value of marriage when cohort size declines.

6 Conclusions

In this paper we provide an explanation for the variation in U.S. marriage rates over the past century. Using time-series variation, cross-state variation, and cross-state variation in the adoption of the pill we provide evidence in support of the following two results. First, cohort size can explain on its own more than 50% of the variation in U.S. marriage rates. Second, an increase in cohort size reduces marriage rates for both women and men and a decrease has the opposite effect.

We then develop and test a model that can generate the empirical patterns observed in the data. An important implication of our results is that the standard matching model is
not consistent with our empirical findings, since it predicts that an increase in cohort size should reduce the marriage rate of women but increase the marriage rate of men, which contradicts our results. For this reason, we propose and test a dynamic search model of the marriage market. Using this model we document three results. First, a standard dynamic search model is rejected by the data for the same reason the standard matching model is rejected: it predicts that a rise in cohort size should always increase the marriage rate of men. We then show that two variations of the standard search model can generate all the patterns observed in the data. In the first variation, the search model is modified by allowing the value of being single to be an increasing function of cohort size. In the second variation, men can choose to undertake an investment that increases their probability of meeting a potential spouse and, if they marry, their marital surplus. An interesting feature of the two variations of the search model is that they imply a relationship between cohort size and divorce rates of opposite sign. Using this testable implication, we reject the first model in favor of the investment model.

Our results have important implications for policy analysis. In recent years, politicians and policy makers have begun to consider and implement policies to increase the fraction of married individuals with the intent of reducing the poverty rate. Several examples of such policies exist. For instance, the program Temporary Assistance to Needy Families (TANF) allows states to use a fraction of its funds to implement policies aimed at increasing the share of married individuals. In West Virginia, households receiving TANF receive an additional 100 dollars a month if they are headed by a legally married household. States have also undertaken other policies to promote marriage. “First Things First” in Tennessee and “Healthy Marriages Grand Rapids” are examples of such programs. Our findings indicate that these types of policies may at best only have short term effects, since in the medium and long run marriage rates are for the most part outside the control of policy makers and politicians.

References


### 7 Tables and Figures

#### Table 1: Time Series Regression of Log Share Ever Married on Log Cohort Size

<table>
<thead>
<tr>
<th></th>
<th>White Men</th>
<th>Black Men</th>
<th>White Women</th>
<th>Black Women</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ever married by age 30</td>
<td>-0.294***</td>
<td>-0.592***</td>
<td>-0.193***</td>
<td>-0.870***</td>
</tr>
<tr>
<td>R²</td>
<td>0.50</td>
<td>0.70</td>
<td>0.46</td>
<td>0.74</td>
</tr>
<tr>
<td>Ever married by age 35</td>
<td>-0.182***</td>
<td>-0.440***</td>
<td>-0.111***</td>
<td>-0.560***</td>
</tr>
<tr>
<td>R²</td>
<td>0.71</td>
<td>0.77</td>
<td>0.65</td>
<td>0.74</td>
</tr>
<tr>
<td>Ever married by age 40</td>
<td>-0.107***</td>
<td>-0.322***</td>
<td>-0.066***</td>
<td>-0.453***</td>
</tr>
<tr>
<td>R²</td>
<td>0.77</td>
<td>0.84</td>
<td>0.58</td>
<td>0.85</td>
</tr>
</tbody>
</table>

*** Significant at 1%. Newey-West standard errors in parentheses. Each coefficient is the outcome of a separate regression. Regressions include cohorts born after 1914 until the most recent cohort observed at a given age in 2011. The number of observations in each regression is equal to 68 for the share ever married by 30, 63 for the share ever married by 35, and 58 for the share ever married by 40. Sources: IPUMS CPS 1962-2011, IPUMS USA 1960-1970.

#### Table 2: Regression of Change in Log Share Ever Married on Change in Log Cumulative Cohort Size

<table>
<thead>
<tr>
<th></th>
<th>1-Yr.</th>
<th>2-Yr.</th>
<th>3-Yr</th>
<th>4-Yr</th>
<th>5-Yr.</th>
<th>7-Yr.</th>
<th>10-Yr.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Men</td>
<td>0.049</td>
<td>0.017</td>
<td>-0.157***</td>
<td>-0.220***</td>
<td>-0.233***</td>
<td>-0.238***</td>
<td>-0.277***</td>
</tr>
<tr>
<td>By Age 30</td>
<td>(0.037)</td>
<td>(0.062)</td>
<td>(0.061)</td>
<td>(0.049)</td>
<td>(0.045)</td>
<td>(0.040)</td>
<td>(0.047)</td>
</tr>
<tr>
<td>Men</td>
<td>-0.054</td>
<td>-0.057</td>
<td>-0.090**</td>
<td>-0.101***</td>
<td>-0.098**</td>
<td>-0.115***</td>
<td>-0.163***</td>
</tr>
<tr>
<td>By Age 35</td>
<td>(0.054)</td>
<td>(0.044)</td>
<td>(0.040)</td>
<td>(0.034)</td>
<td>(0.026)</td>
<td>(0.023)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Men</td>
<td>0.073***</td>
<td>0.037</td>
<td>-0.018</td>
<td>-0.048**</td>
<td>-0.054**</td>
<td>-0.052**</td>
<td>-0.085***</td>
</tr>
<tr>
<td>By Age 40</td>
<td>(0.016)</td>
<td>(0.038)</td>
<td>(0.029)</td>
<td>(0.022)</td>
<td>(0.023)</td>
<td>(0.023)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Women</td>
<td>-0.064***</td>
<td>-0.077***</td>
<td>-0.101***</td>
<td>-0.095***</td>
<td>-0.103***</td>
<td>-0.125***</td>
<td>-0.156***</td>
</tr>
<tr>
<td>By Age 30</td>
<td>(0.024)</td>
<td>(0.037)</td>
<td>(0.014)</td>
<td>(0.019)</td>
<td>(0.021)</td>
<td>(0.024)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Women</td>
<td>-0.005</td>
<td>-0.053**</td>
<td>-0.070***</td>
<td>-0.084***</td>
<td>-0.081***</td>
<td>-0.090***</td>
<td>-0.113***</td>
</tr>
<tr>
<td>By Age 35</td>
<td>(0.056)</td>
<td>(0.021)</td>
<td>(0.011)</td>
<td>(0.012)</td>
<td>(0.015)</td>
<td>(0.016)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Women</td>
<td>-0.011</td>
<td>-0.005</td>
<td>-0.008</td>
<td>-0.022**</td>
<td>-0.036**</td>
<td>-0.056***</td>
<td>-0.073***</td>
</tr>
<tr>
<td>By Age 40</td>
<td>(0.021)</td>
<td>(0.021)</td>
<td>(0.012)</td>
<td>(0.013)</td>
<td>(0.014)</td>
<td>(0.010)</td>
<td>(0.006)</td>
</tr>
</tbody>
</table>

* Significant at 10%. ** 5%. *** 1%. See notes in Table 1. Newey-West standard errors in parentheses.
Table 3: Cross-Sectional Regression of Log Share Ever Married by 30 or 40

<table>
<thead>
<tr>
<th>Dependent Variable: 10-Yr. Difference in Log Share Ever Married</th>
<th>OLS</th>
<th>IV (1)</th>
<th>IV (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>10-Yr. Difference in Log Cohort Size</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Men</td>
<td>-0.041**</td>
<td>-0.123***</td>
<td>-0.168***</td>
</tr>
<tr>
<td>By Age 30</td>
<td>(0.019)</td>
<td>(0.041)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>Men</td>
<td>-0.029***</td>
<td>-0.047**</td>
<td>-0.040</td>
</tr>
<tr>
<td>By Age 40</td>
<td>(0.010)</td>
<td>(0.018)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Women</td>
<td>-0.041***</td>
<td>-0.104***</td>
<td>-0.115***</td>
</tr>
<tr>
<td>By Age 30</td>
<td>(0.010)</td>
<td>(0.023)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>Women</td>
<td>-0.032***</td>
<td>-0.058***</td>
<td>-0.048***</td>
</tr>
<tr>
<td>By Age 40</td>
<td>(0.010)</td>
<td>(0.017)</td>
<td>(0.023)</td>
</tr>
<tr>
<td>R^2</td>
<td>0.807</td>
<td>0.791</td>
<td>0.819</td>
</tr>
<tr>
<td>Men</td>
<td>0.555</td>
<td>0.551</td>
<td>0.595</td>
</tr>
<tr>
<td>By Age 30</td>
<td>(0.010)</td>
<td>(0.023)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>Women</td>
<td>0.539</td>
<td>0.528</td>
<td>0.605</td>
</tr>
</tbody>
</table>

First Stage Results

<table>
<thead>
<tr>
<th>Dependent Variable: 10-Yr. Difference in Log Share Ever Married</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Cohort Size at Birth</td>
</tr>
<tr>
<td>F-test</td>
</tr>
<tr>
<td>R^2</td>
</tr>
<tr>
<td>0.440*** (0.073)</td>
</tr>
<tr>
<td>36.44</td>
</tr>
<tr>
<td>0.831</td>
</tr>
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</table>

** Significant at 5%. *** 1%. Robust std. errors in parentheses. N=288. Each coefficient is the outcome of a separate, population-weighted regression. We control for cohort fixed effects, and for cohort-region fixed effects in IV-(2). Includes all decennial cohorts born between 1910 and 1970, in all states except HI and AK. Sources: US Population Counts, 1910-1990; IPUMS USA, 1940-2010.

Table 4: Comstock Laws and N-Year Differences in Log Cohort Size

<table>
<thead>
<tr>
<th>Dependent Variable: N-Yr. Difference in Log Cohort Size</th>
</tr>
</thead>
<tbody>
<tr>
<td>1-yr</td>
</tr>
<tr>
<td>Ban* Pillc,s</td>
</tr>
<tr>
<td>(0.005)</td>
</tr>
<tr>
<td>Ban,s</td>
</tr>
<tr>
<td>(0.005)</td>
</tr>
<tr>
<td>N</td>
</tr>
</tbody>
</table>

* Significant at 10%. ** 5%. *** 1%. Robust standard errors in parentheses. Regressions are weighted by population and include controls for physician exception, physician exception interacted with “pill,” advertising bans, and cohort-region fixed effects. Sources: IPUMS USA, 1980-2000, NIH SEER Population Counts.

Table 5: N-Year Differences in Log Share Ever Married and N-Year Differences in Log Cohort Size

<table>
<thead>
<tr>
<th>Dependent Variable: N-Yr. Difference in Log Share Ever Married (Men)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1-yr</td>
</tr>
<tr>
<td>N-yr Difference in Log Cohort Size</td>
</tr>
<tr>
<td>(0.206)</td>
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<table>
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<tr>
<th>Dependent Variable: N-Yr. Difference in Log Share Ever Married (Women)</th>
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<tr>
<td>1-yr</td>
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<tr>
<td>N-yr Difference in Log Cohort Size</td>
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<tr>
<td>(0.161)</td>
</tr>
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<td>N</td>
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* Significant at 10%. ** 5%. *** 1%. See note in Table 4.
Table 6: Comstock Laws and Birth Weight

<table>
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<th>Share with Birth Weight &lt; 1500</th>
<th>Share with Birth Weight &lt; 2500</th>
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<tr>
<td>Ban * Pill&lt;sub&gt;c,s&lt;/sub&gt;</td>
<td>0.0001025</td>
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<td>(-0.0002037)</td>
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<tr>
<td>Ban&lt;sub&gt;s&lt;/sub&gt;</td>
<td>-0.000462</td>
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<td></td>
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<td>N</td>
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See note in Table 4.

Table 7: Calibrated Parameters

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<tr>
<td>α&lt;sub&gt;0&lt;/sub&gt;</td>
<td>Value of being single</td>
<td>-0.07</td>
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<td>γ&lt;sub&gt;1&lt;/sub&gt;</td>
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<td>Probability of divorce</td>
<td>0.47</td>
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</table>

Figure 1: Different Measures of Marriage Rates

The left vertical axis marks the percentage of individuals ever married for the cohort-based and cross-sectional measures; the right axis corresponds to marriages per thousand. Sources: Vital Statistics of the United States; IPUMS CPS, 1962-2005; IPUMS USA, 1940-1960.
Note: The vertical axis represents both the percentage of individuals ever married as well as normalized cohort size. In Panel A we normalize cohort size by dividing by 10,000,000; in Panel B by 1,000,000. For share ever married, we graph three-year moving averages. Sources: Vital Statistics of the U.S.; IPUMS CPS, 1962-2011; IPUMS USA, 1960-1970.
Figure 4: Share Never Married and Not Cohabiting By 40
A. White
B. Black

* See note in Figure 2

Figure 5: Growth by Region: States with Sales Bans - States without Sales Bans
A. Growth of Total Births
B. Growth of Total Adult Population, at Age 25

Figure 6: Share Never Married

A. $\delta$-Model

B. $\eta$-Model

Figure 7: Share Ever Divorced

A. Data

B. $\delta$-Model

C. $\eta$-Model
A Appendix: Proofs and Derivations

A.1 Reservation Values

We begin by characterizing the decisions of a man of age 1 in period $t$. If an old man chooses to be single in the second period, his lifetime utility takes the following form:

$$v_{1,t}^m = \sum_{t=0}^{T-1} \beta^t \delta = \frac{1 - \beta^T}{1 - \beta}.$$ 

Similarly, if a woman decides to stay single in her first period of life, her lifetime welfare can be computed as follows:

$$v_{0,t}^w = \sum_{t=0}^{T} \beta^t \delta = \frac{1 - \beta^{T+1}}{1 - \beta} = \frac{1 - \beta^T}{1 - \beta} + \beta^T \delta.$$ 

If two potential partners decide to marry, the within-period utility they have drawn is also the utility they will experience in each period for the rest of their life or until they divorce. Divorce can occur each period with a probability $q = 1 - p$. If a couple divorces, each individual receives the value of being single for the remainder of their lifetime. The lifetime utility of a couple of individuals who are both of age 0 and have drawn a value $\eta$ in period $t$ can therefore be written as follows:

$$v_{0,0,t} = \eta \sum_{t=0}^{T} \beta^t p^t + 2\delta \sum_{t=1}^{T} \beta^t (1 - p^t) = \eta \sum_{t=0}^{T} \beta^t p^t + 2\delta \sum_{t=1}^{T} \beta^t - 2\delta \sum_{t=1}^{T} \beta^t p^t =$$

$$\eta \sum_{t=0}^{T} \beta^t p^t + 2\delta \sum_{t=1}^{T} \beta^t + 2\delta - 2\delta \sum_{t=1}^{T} \beta^t p^t - 2\delta = \eta \sum_{t=0}^{T} \beta^t p^t + 2\delta \sum_{t=0}^{T} \beta^t - 2\delta \sum_{t=0}^{T} \beta^t p^t =$$

$$(\eta - 2\delta) \sum_{t=0}^{T} \beta^t p^t + 2\delta \sum_{t=0}^{T} \beta^t = 1 - \frac{(p\beta)^{T+1}}{1 - p\beta} (\eta - 2\delta) + \frac{1 - \beta^{T+1}}{1 - \beta - 2\delta},$$

where the last equality follows from the following geometric series formula:

$$\sum_{t=0}^{T} ab^t = a \frac{1 - b^{T+1}}{1 - b}.$$ 

If the couple is composed of an older man and a woman, the man will die one period earlier. As a consequence, following the same steps as in the derivation of $v_{0,0,t}$, their lifetime utility...
takes the following form:

\[ v_{0,1,t} = \eta \sum_{t=0}^{T-1} \beta^t p^t + 2\delta \sum_{t=1}^{T-1} \beta^t (1 - p^t) + \beta^T \delta = \frac{1-(p\beta)^T}{1-p\beta} (\eta - 2\delta) + \frac{1-\beta^T}{1-\beta} 2\delta + \beta^T \delta. \]

We will assume that the couple can freely divide the gains from marriage and that its lifetime utility is split between the two spouses using a Nash bargaining solution. For a couple composed of a woman of age 0 and a man of age 1, the share received by the man in period \( t \) is, therefore,

\[ w_{1,t}^m(\eta) = v_{1,t}^m + \gamma \left[ v_{0,1,t} - v_{1,t}^m - v_{0,t}^w \right] \]

\[ = v_{1,t}^m + \gamma \left[ \frac{1-(p\beta)^T}{1-p\beta} (\eta - 2\delta) + \frac{1-\beta^T}{1-\beta} 2\delta + \beta^T \delta - v_{1,t}^m - v_{0,t}^w \right], \]

where the parameter \( \gamma \in [0,1] \) allows for possible asymmetries in the way the marriage surplus is divided and \( v_{1,t}^m \) and \( v_{0,t}^w \) are the value of being single in this and future periods that were computed above. A similar equation can be derived for the woman.

We can solve the model starting with the decisions of a man of age 1 in period \( t \). With probability \( \theta_{1,t}^m \), he meets a woman and they marry if their joint lifetime utility from marrying \( v_{0,1,t} \) is greater than the sum of their lifetime utilities if they choose to stay single \( v_{1,t}^m + v_{0,t}^w \).

As a consequence, they will marry if and only if

\[ 1-(p\beta)^T (\eta - 2\delta) + \frac{1-\beta^T}{1-\beta} 2\delta + \beta^T \delta \geq 1-\beta^T (2\delta + \delta \beta^T). \]

This implies that the reservation value for marriage between a woman and a man of age 1 is

\[ \eta_{1,t} = 2\delta. \]

We can now derive the expected value function for an older man before he enters the marriage market. If in period \( t \) this man meets a woman and draws a match quality \( \eta \), Nash-bargaining implies that he receives the following share of the couple’s lifetime utility:

\[ w_{1,t}^m(\eta) = \delta \frac{1-\beta^T}{1-\beta} + \gamma \left[ \frac{1-(p\beta)^T}{1-p\beta} (\eta - 2\delta) + \frac{1-\beta^T}{1-\beta} 2\delta + \beta^T \delta - \frac{1-\beta^T}{1-\beta} 2\delta - \delta \beta^T \right] \]

\[ = \delta \frac{1-\beta^T}{1-\beta} + \gamma (\eta - 2\delta) \frac{1-(p\beta)^T}{1-p\beta}. \]

As a consequence, the expected value function of an older man can be written in the following
form:

\[ v^m_1, t = \left( \delta \frac{1 - \beta T}{1 - \beta} + \frac{\beta}{1 - p \beta} \right) \theta^m_1, t + \delta \frac{1 - \beta T}{1 - \beta} F(\eta_1, t) \theta^m_1, t + \delta \frac{1 - \beta T}{1 - \beta} (1 - \theta^m_1, t). \]

It is composed of three parts. The first term describes the value for the older man of meeting a woman with a match quality \( \eta \) sufficiently high that the couple will choose to marry multiplied by the corresponding probability. The second term characterizes the value of meeting a woman with a match quality \( \eta \) that is below the reservation value \( \eta_1, t \) times the probability of this event. Finally, the last term captures the value of not meeting a woman in the current period multiplied by the probability. By replacing \( \eta_1, t = 2 \delta \) and simplifying some of the terms, we obtain the following equation for the value function:

\[ v^m_1, t = \delta \frac{1 - \beta T}{1 - \beta} + \frac{\beta}{1 - p \beta} \left( \frac{1 - (p \beta)_T}{1 - p \beta} \right) \left( 1 - F(\eta_1, t) \right) \theta^m_1, t. \]  

(5)

We are now in position to consider the decision of a younger man. He meets a potential spouse with probability \( \theta^m_0, t \) and they marry if their joint lifetime utility is greater than the sum of their lifetime utilities if they choose to be single in this period, i.e. if

\[ 2 \delta \frac{1 - \beta T}{1 - \beta} + (\eta - 2 \delta) \frac{1 - (p \beta) T}{1 - p \beta} \geq 2 \delta + \beta v^m_1, t + \beta \delta \frac{1 - \beta T}{1 - \beta}, \]

where the first term on the right hand side is the joint value of being single in this period, the second term is the man’s discounted expected value function for next period if he chooses to stay single today, and the third term is the woman’s discounted value from next period onward if she chooses to stay single today. With this expression, we can now solve for the reservation value of a man of age 0. Substituting for the expected value function of an older man using equation (5) and simplifying some of the terms, we obtain the following equation for the reservation value of a younger man:

\[ u^m_0, t = 2 \delta + \beta \frac{1 - (p \beta)_T}{1 - (p \beta) T + 1} \gamma \left\{ E \left[ \eta | \eta \geq 2 \delta \right] - 2 \delta \right\} \left( 1 - F(2 \delta) \right) \theta^m_1, t + 1. \]  

(6)

Using \( u^m_0, t \), one can derive the expected value function for a woman and a younger man. They are presented in Appendix A.6.

### A.2 Steady State

In this subsection, we use the reservation values discussed above to solve for the steady state equilibrium in the marriage market. To do this, we have to derive the probability that a younger man meets a woman \( \theta^m_0, t \) and the corresponding probability for an older man \( \theta^m_1, t \).
Let $N_{a,t}$ be the number of individuals of gender $i$, age $a$, and period $t$ who are present in the marriage market. Then $\theta_{0,t}^m$ and $\theta_{1,t}^m$ can be derived by noting that

$$\theta_{0,t}^m = \theta_{1,t}^m = \frac{N_{0,t}^w}{N_{0,t}^m + N_{1,t}^m}. \quad (7)$$

The number of individuals of age 0 is exogenously given by the cohort size of a generation. However, the number of older men in the marriage market $N_{1,t}^m$ is endogenously determined by the decisions of younger men. As a consequence, to derive $\theta_{0,t}^m$ and $\theta_{1,t}^m$ we need to solve for $N_{1,t}^m$. This variable can be computed as the number of younger men who did not meet a woman at $t-1$ plus the number of younger men who met a woman at $t-1$ but draw a match quality $\eta$ lower than the reservation value, i.e.

$$N_{1,t}^m = N_{0,t-1}^m \left( 1 - \theta_{0,t-1}^m \right) + N_{0,t-1}^m \theta_{0,t-1}^m F (\eta_{0,t-1}) = N_{0,t-1}^m \left( 1 - \theta_{0,t-1}^m \left( 1 - F (\eta_{0,t-1}) \right) \right). \quad (8)$$

We can now replace for $\theta_{0,t-1}^m$ using (7) and obtain the following equation for $N_{1,t}^m$:

$$N_{1,t}^m = N_{0,t-1}^m \left( 1 - \frac{N_{0,t-1}^m}{N_{0,t-1}^m + N_{1,t-1}^m} \left( 1 - F (\eta_{0,t-1}) \right) \right)$$

$$= N_{0,t-1}^m \left( \frac{N_{0,t-1}^m + N_{1,t-1}^m - N_{0,t-1}^m \left( 1 - F (\eta_{0,t-1}) \right)}{N_{0,t-1}^m + N_{1,t-1}^m} \right).$$

In a steady state equilibrium, the cohort size $N_{0,t}^w$ and $N_{0,t}^m$ and the number of older men in the marriage market $N_{1,t}^m$ are constant over time. We therefore have that

$$N_{1}^m = N_{0}^m \left( \frac{N_{0}^m + N_{1}^m - N_{0}^m \left( 1 - F (\eta_{0}) \right)}{N_{0}^m + N_{1}^m} \right).$$

We can now solve for $N_{1}^m$ and obtain

$$N_{1}^m = \sqrt{(N_{0}^m)^2 - N_{0}^m N_{0}^w + N_{0}^m N_{0}^w F (\eta_{0})}.$$ 

Generally, men and women have identical cohort size, i.e. $N_{0,t}^m = N_{0,t}^w = N_{0,t}$. In this case the solution for $N_{1}^m$ simplifies to

$$N_{1}^m = N_{0} F (\eta_{0})^{\frac{1}{2}}.$$ 

---

9This is not the case if men or women are more likely not to be in the marriage market for particular reasons. For instance, African-American men are more likely than African-American women to be incarcerated during their marriage years. As a consequence, the relevant cohort size for African-American men is smaller than the corresponding cohort size for women.
If we substitute $N_m^1$ back into $\theta^m_j$, we have

$$\theta^m_0 = \theta^m_1 = \frac{N_0^w}{N_0^m + \sqrt{(N_0^m)^2 - N_0^m N_0^w + N_0^m N_0^w F(\eta_0)}}.$$  

If men and women have identical cohort size, $\theta^m_j$ simplifies to

$$\theta^m_0 = \theta^m_1 = \frac{N_0}{N_0 + N_0 F(\eta_0)^{1/2}} = \frac{1}{1 + F(\eta_0)^{1/2}}.$$  

To determine the reservation value of younger men in steady state, we can substitute for $\theta^m_1$ in the equation that determines the reservation value (6). We can then derive, for the case in which $N_0^m \neq N_0^w$, the following equation for the steady state reservation value:

$$\eta_{ss} = 2\delta + \beta \frac{1 - (p\beta)^T}{1 - (p\beta)^T + 1} \gamma \{E[\eta] | \eta \geq 2\delta\} (1 - F(2\delta)) \frac{N_0^w}{N_0^m + \sqrt{(N_0^m)^2 - N_0^m N_0^w + N_0^m N_0^w F(\eta_{ss})}},$$

If $N_0^m = N_0^w$, the equation simplifies as follows:

$$\eta_{ss} = 2\delta + \beta \frac{1 - (p\beta)^T}{1 - (p\beta)^T + 1} \gamma \{E[\eta] | \eta \geq 2\delta\} (1 - F(2\delta)) \frac{1}{1 + F(\eta_{ss})^{1/2}}.$$  

Note that $F(\eta)$ is monotonically increasing in $\eta$. As a consequence, there is a unique solution for $\eta_{ss}$. Moreover, if men and women have identical cohort sizes, the steady state reservation value is independent of $N_0^m$ and $N_0^w$.

### A.3 Proof of Proposition 2 and The Effect of an Unexpected Shock to Cohort Size

Suppose the economy is in steady state when it is hit by an unexpected shock in period $t = \tau$ that changes permanently the cohort size from $N_0$ to $N_0 + \Delta$. According to equation (7), the probabilities $\theta^m_{j,t}$ take the following form:

$$\theta^m_{0,t} = \theta^m_{1,t} = \frac{N_{0,t}}{N_{0,t} + N_{1,t}^m} \quad \text{if } t < \tau$$

and

$$\theta^m_{0,t} = \theta^m_{1,t} = \frac{N_{0,t} + \Delta}{N_{0,t} + \Delta + N_{1,t}^m} \quad \text{if } t \geq \tau.$$  

Consider the period in which the shock is realized and notice that $N_{1,t}^m$ are the men born in period $\tau - 1$ who did not marry when younger. As a consequence, $N_{1,t}^m$ equals the number of older men in steady state, i.e. $N_{1,t}^m = N_{0,\tau-1} F(\eta_{ss})^{1/2} = N_0 F(\eta_{ss})^{1/2}$. Substituting for $N_{1,t}^m$...
in the probabilities \( \theta_{m,t}^n \), we have that in period \( \tau \)

\[
\theta_{0,\tau}^m = \theta_{1,\tau}^m = \frac{N_0 + \Delta}{N_0 + \Delta + N_0 F(\eta_{ss})^2} = \frac{1}{1 + \frac{N_0}{N_0 + \Delta} F(\eta_{ss})^2}.
\]

The previous equation implies that a positive cohort shock \( \Delta \) increases the probability that a man of any age meets a woman, whereas a negative cohort shock has the opposite effect. In our economy there are always more men than women in the marriage market. As a consequence, the probability that a woman meets a younger man, \( \theta_{w,t} = \frac{N_0}{N_0 + N_0 + \Delta + N_1} \), is equivalent to the probability that a man meets a woman. Therefore, the previous result also implies that a positive cohort shock increases the probability that a woman meets a younger man.

We can now determine the effect of a shock to cohort size on the reservation value of younger men \( \bar{\eta}_{0,\tau} \). Notice that in the determination of \( \bar{\eta}_{0,\tau} \), a younger man compares the value of getting married at \( \tau \) with the value of waiting until next period. The value of waiting depends on the probability he will meet a woman in period \( \tau + 1 \). This probability depends on the number of older men at \( \tau + 1 \), which can be written as follows:

\[
\theta_{m,\tau+1}^0 = \theta_{1,\tau+1}^m = \frac{N_0 + \Delta}{N_0 + \Delta + N_1}.\]

Using equation (8), we can substitute for \( N_1 \) to obtain the following expression:

\[
\theta_{0,\tau+1}^m = \theta_{1,\tau+1}^m = \frac{N_0 + \Delta}{N_0 + \Delta + N_1} \left( 1 - \theta_{m,\tau}^0 \left( 1 - F(\bar{\eta}_{0,\tau}) \right) \right) = \frac{1}{1 + \left( 1 - \theta_{m,\tau}^0 \left( 1 - F(\bar{\eta}_{0,\tau}) \right) \right)}.
\]

We can now substitute for \( \theta_{1,\tau+1}^m \) in the equation that determines \( \bar{\eta}_{0,\tau} \) to obtain

\[
\bar{\eta}_{0,\tau} = 2\delta + \beta \frac{1 - (p\beta)^T}{1 - (p\beta)^T + 1} \gamma \left\{ E [\eta | \eta \geq 2\delta] - 2\delta \right\} (1 - F(2\delta)) \frac{1}{1 + \left( 1 - \theta_{m,\tau}^0 \left( 1 - F(\bar{\eta}_{0,\tau}) \right) \right)}.
\]

The same equation for the reservation value in steady state can be derived as follows:

\[
\bar{\eta}_{0,ss} = 2\delta + \beta \frac{1 - (p\beta)^T}{1 - (p\beta)^T + 1} \gamma \left\{ E [\eta | \eta \geq 2\delta] - 2\delta \right\} (1 - F(2\delta)) \frac{1}{1 + \left( 1 - \theta_{m,ss}^0 \left( 1 - F(\bar{\eta}_{0,ss}) \right) \right)}.
\]

Earlier in this section we have shown that, with a positive shock to cohort size, \( \theta_{0,\tau}^m \) is greater than \( \theta_{0,ss}^m \). As a consequence, a simple comparison of the last two equations implies that an increase in cohort size has the effect of increasing the reservation value of younger men. Specifically, by substituting \( \theta_{0,ss}^m \) with \( \theta_{0,\tau}^m \) and by using the result that \( \theta_{0,\tau}^m > \theta_{0,ss}^m \), we obtain the following
inequality:
\[
\eta_{0,ss} < 2\delta + \beta \frac{1 - (p_0\beta)^T}{1 - (p_0\beta)^T} \gamma \{ E[\eta | \eta \geq 2\delta] - 2\delta \} (1 - F(2\delta)) \left( \frac{1}{1 + \theta_{0,0,T} \gamma \{ (1 - F(\eta_{0,ss})) \}} \right).
\]

Since the left hand side of the inequality is increasing in \(\eta_0\) and the right hand side is decreasing in \(\eta_0\), equation (9) implies that \(\eta_{0,ss} > \eta_{0,0}\).

A.4 Proof of Proposition 3

The total number of women that marry in a particular cohort is given by the total number of women in the cohort time the probability that a woman in that cohort marries. As a consequence, the fraction of women in a cohort that marries is simply the probability of marriage for those women. The probability that a woman marries can be written as the probability that she meets a younger man times the probability she marries him plus the probability she meets an older man times the probability she marries him, i.e.

\[
P (\text{woman marries at } \tau) = \theta_{0,0,T}^w (1 - F(\eta_{0,0,T})) + (1 - \theta_{0,0,T}^w) (1 - F(2\delta))
\]

Define \(1 + \lambda_\tau = \frac{F(\eta_{0,0,T})}{F(\eta_{0,ss})}\) and \(1 + \phi_\tau = \frac{\theta_{0,0,T}^w}{\theta_{0,ss}^w}\), where \(\lambda_\tau > 0\) and \(\phi_\tau > 0\) because \(\frac{\partial \eta_{0,0,T}}{\partial N_0} > 0\) and \(\frac{\partial \theta_{0,0,T}^w}{\partial N_0} > 0\). We then have

\[
P (\text{woman marries at } \tau) =
\]

\[
= \theta_{0,0,T}^w (1 - F(\eta_{0,0,T})) + (1 - \theta_{0,0,T}^w) (1 - F(2\delta))
\]

\[
= \theta_{0,ss}^w (1 + \phi_\tau) (1 - F(\eta_{0,ss}) (1 + \lambda_\tau)) + (1 - \theta_{0,ss}^w (1 + \phi_\tau)) (1 - F(2\delta))
\]

\[
= \theta_{0,ss}^w (1 - F(\eta_{0,ss})) + (1 - \theta_{0,ss}^w) (1 - F(2\delta)) - \theta_{0,ss}^w \lambda_\tau F(\eta_{0,ss}) + \theta_{0,ss}^w \phi_\tau (1 - F(\eta_{0,ss}) (1 + \lambda_\tau))
\]

\[
- \theta_{0,ss}^w \phi_\tau (1 - F(2\delta))
\]

\[
= P (\text{woman marries at ss}) - \theta_{0,ss}^w \lambda_\tau F(\eta_{0,ss}) + \theta_{0,ss}^w \phi_\tau (1 - F(\eta_{0,0,T})) - \theta_{0,ss}^w \phi_\tau (1 - F(2\delta))
\]

\[
< P (\text{woman marries at ss}) - \theta_{0,ss}^w \lambda_\tau F(\eta_{0,ss})
\]

\[
< P (\text{woman marries at ss}) .
\]

A.5 Proof of Proposition 4

We prove the Proposition in two steps. We first prove that the probability that a man marries when younger in period \(t\) increases with cohort size. When then prove that a man marries when younger or older increases with cohort size.

First step. Let \(P_{tt}^{mm}\) be the probability that a man marries when younger the period of the shock \(\tau\). Since we consider the case of a permanent shock to cohort size we have
Using equation (8), we can therefore write the number of older men in period \( t + 1 \) as follows:

\[
N_{0,\tau+1}^m = N_{0,\tau}^m \left( 1 - \theta_{0,\tau}^m \left( 1 - F \left( \eta_{0,\tau} \right) \right) \right) = N_{0,\tau}^m \left( 1 - P_{\tau}^{ym} \right). \tag{11}
\]

Using the previous equation and equation (9), the reservation utility of a younger man can be written in the following form:

\[
\eta_{0,\tau} = A + B \frac{N_{0,\tau}}{N_{0,\tau} + N_{0,\tau} \left( 1 - \theta_{0,\tau}^m \left( 1 - F \left( \eta_{0,\tau} \right) \right) \right)} = A + B \frac{1}{1 + (1 - P_{\tau}^{ym})} = A + B \frac{1}{2 - P_{\tau}^{ym}}.
\]

Proposition 2 establishes that \( \eta_{0,ss} < \eta_{0,\tau} \). Hence,

\[
\eta_{0,ss} = A + B \frac{1}{2 - P_{ss}^{ym}} < A + B \frac{1}{2 - P_{\tau}^{ym}} = \eta_{0,\tau}.
\]

The inequality implies that \( P_{\tau}^{ym} > P_{ss}^{ym} \). We can therefore conclude that an increase in cohort size increases the probability that a man marries when younger.

Second step. The probability that a man marries when younger or older \( P_{\tau}^{m} \) can be written as the probability that a man married when younger in period \( \tau \) plus the probability that the same man marries when older in period \( \tau + 1 \), i.e.

\[
P_{\tau}^{m} = \theta_{0,\tau}^m \left( 1 - F \left( \eta_{0,\tau} \right) \right) + \left( 1 - \theta_{0,\tau}^m \left( 1 - F \left( \eta_{0,\tau} \right) \right) \right) \theta_{1,\tau+1}^m \left( 1 - F \left( 2\delta \right) \right). \tag{12}
\]

The first part of the right hand side is the probability that a younger man meets a woman and marries her in period \( \tau \), which we denoted with \( P_{\tau}^{ym} \). The second part is the probability that a younger man does not marry in period \( \tau \), \( 1 - P_{\tau}^{ym} \), meets a woman in period \( \tau + 1 \), and marries her. Using equation (11), the probability that an older men meets a woman can be written as follows:

\[
\theta_{1,\tau+1}^m = \frac{N_{0,\tau+1}^m}{N_{0,\tau+1}^m + N_{1,\tau+1}^m} = \frac{1}{2 - P_{\tau}^{ym}}.
\]

As a consequence, equation (12) can be written as follows:

\[
P_{\tau}^{m} = P_{\tau}^{ym} + \frac{1 - P_{\tau}^{ym}}{2 - P_{\tau}^{ym}} \left( 1 - F \left( 2\delta \right) \right).
\]

Taking the derivative with respect to cohort size \( N \) of both size and rearranging terms, we have,

\[
\frac{\partial P_{\tau}^{m}}{\partial N} = \frac{\partial P_{\tau}^{ym}}{\partial N} \left[ 1 - \frac{1 - F \left( 2\delta \right)}{(2 - P_{\tau}^{ycm})^2} \right] > \frac{\partial P_{\tau}^{ym}}{\partial N} F \left( 2\delta \right) > 0,
\]

where the first inequality follows from \( (2 - P_{\tau}^{ym})^2 > 1 \) and the second from the first step of
the proof. Hence, an increase in cohort size increases the probability that a man marries.

A.6 Expected Value Functions

For completeness, in this appendix we derive the expected values for younger men and women. The expected value of a younger man takes the following form:

\[
v^m_{0,t} = \theta^m_{0,t} (1 - F(\eta_{0,t})) \left\{ 1 - \beta v^m_{1,t} + \gamma \left\{ 2\delta \frac{1 - \beta T + 1}{1 - \beta} + \frac{1 - (p\beta)^{T + 1}}{1 - (p\beta)} E[\eta - 2\delta | \eta \geq \eta_{0,t}] - \left( \delta + \beta v^m_{1,t} \right) \right\} + \theta^m_{0,t} F(\eta_{0,t}) \left( \delta + \beta v^m_{1,t} \right) \right\}
\]

The first term represents the value of meeting a woman with a match quality \( \eta \) higher than the reservation value times the probability of this event. The second term describes the value of meeting a woman characterized by an \( \eta \) lower than the reservation value multiplied by the corresponding probability. The third term measures the value of not meeting a woman when younger times the probability.

To derive the woman’s expected value function we have to take into account that she can meet both younger and older men. As a consequence, it takes the following more complex form:

\[
v^w_{0,t} = \theta^m_{0,t} (1 - F(\eta_{0,t})) \left\{ 1 - \beta v^m_{1,t} + \gamma \left\{ 2\delta \frac{1 - \beta T + 1}{1 - \beta} + \frac{1 - (p\beta)^{T + 1}}{1 - (p\beta)} E[\eta - 2\delta | \eta \geq \eta_{0,t}] - \left( \delta + \beta v^m_{1,t} \right) \right\} + \theta^m_{0,t} F(\eta_{0,t}) \left( \delta + \beta v^m_{1,t} \right) \right\}
\]

The first term measures the value of meeting a younger man with an \( \eta \) higher than the reservation value times the corresponding probability. The second term is the value of meeting a younger man whom it is optimal not to marry times the probability of this event. The third and fourth terms describe the same values of meeting an older man.

A.7 Basic Search Model with Divorce

In Figure A1, we show simulation results for the basic search model, under two different assumptions about the probability with which a divorce may occur. Under the first assumption, the probability that a divorce occurs is constant. Under the second assumption, the probability of divorce is modeled as a decreasing function of match quality. As Figure A1 shows, under both assumptions, the share never married changes in opposite directions for men and women, in contrast to the patterns observed in the data. Additionally, as discussed
in the paper, the share never married is higher for all cohorts when the probability of divorce is decreasing in eta since younger men become more selective and increase their reservation utility.

**Figure A1:** Share of Men and Women Never Married, Basic Search Model
Appendix: Data Description

Table B1 provides a summary of the datasets employed in the construction of the main variables of interest. In the rest of the appendix, we give additional details about how we construct the variables cohort size at birth, cohort size at marriageable age, share ever married, and age differences of spouses.

In the paper we use two different measures of cohort size: cohort size at birth and cohort size at marriageable age. Cohort size at birth is used in three ways: as the main independent variable when we employ longitudinal variation; as an instrument for cohort size at marriage age in the cross-state regressions; and as one of the variables used to determine the effect of the introduction of the pill in states with different anti-obscenity laws. With longitudinal variation we use cohort size at birth as the main independent variable instead of cohort size at marriageable age for two reasons. First, as shown in Figure B1, when cohort size is computed for the U.S. population there is little difference between cohort size at birth and cohort size in adulthood, since migration from and to the U.S. was limited. Second, we can construct the variable cohort size at birth for cohorts born in 1909 and after. The variable cohort size at marriageable age can only be constructed for cohorts born after the 1940s. By using cohort size at birth we can therefore consider a larger number of cohorts without significant effect on the analysis. As indicated in Table B1, in the longitudinal analysis cohort size at birth is constructed using the U.S. Vital Statistics which provide information on this variable by race from 1909 to 1980. For the cross-state regressions, there are two data sets that can be used to measure cohort size at birth: the U.S. Vital Statistics which record births by race and by state from 1940; and the decennial Censuses which provide information on population counts from the beginning of the twentieth century to 2010. In the cross-state regressions, we work with the decennial cohorts 1910-1970. For consistency, rather than combining two different datasets, we use the Censuses over the entire period of interest. One limitation of the decennial Censuses is that population counts are published for 5-year age groups. From each decennial Census, we therefore record the number individuals between the ages 0 to 4 and we use it to construct the cohort size at birth. Our results do not change if we use data from the U.S. Vital Statistics for the 1940 to 1970 cohorts. In the regressions that use the introduction of the pill as an instrumental variable, we consider cohorts born between 1945 and 1970. We can therefore use the U.S. Vital Statistics to compute cohort size at birth for all of them.

Cohort size at marriageable age is used as the main independent variable in the cross-state regressions and in the regressions that use the introduction of the pill as an instrument. There are two datasets that can be used to measure this variable: the decennial Censuses and the SEER population estimates. SEER records cohort sizes at different ages starting from 1969. Hence, using this dataset we can construct cohort size at marriage age only for some of the decennial cohorts born between 1910 and 1970, which are the ones we consider in
the cross-state regressions. For consistency, we therefore construct cohort size at marriage age by recording the number of individuals between the ages 20 and 24 in the decennial Censuses 1930-1990. We also experimented with the 5-year age group 30-34 with similar results. In the regressions that use the pill and the anti-obscenity laws as instruments, we use cohorts born between 1945 and 1970 which are all observed in SEER at age 25 or older. We therefore measure cohort size at marriage using the information in SEER at age 25.

The variable share ever marriage is constructed using a different procedure depending on whether we use longitudinal or cross-state variation. With longitudinal variation, we employ a combination of the CPS, which covers the period 1962-2011, and of the decennial Censuses. In the CPS, we observe the age and the marital status of each respondent. We can therefore easily compute the share ever married by age 30, 35, or 40 for each cohort born after a particular year. For instance, for the variable share ever married by age 30, we can use the CPS for all cohorts born on or after 1932; for the variable share ever married by age 40, we can use the CPS for all cohorts born on or after 1922. For cohorts born before those years, we use the 1960, 1970, and 1980 Censuses, which contain information on the marital status and the age at first marriage, a recall variable. Using these two variables, we construct the share ever married by age 30 and 35 for different cohorts by considering all individuals who in a given Census are between the ages of 30 and 45. We use a maximum cutoff age of 45 to avoid potential measurement errors due to differential mortality rates of married and non-married individuals. For the share ever married by age 40, we use the same procedure with a maximum cutoff age of 50. With cross-state variation, for all cohorts we only use information from the Censuses, as sample sizes in the CPS are too small to provide reliable estimates at the state level. In the longitudinal as well as in the cross-state variation, we cannot construct the share ever married for cohorts born before 1914 because the 1960 Census is the first one that records the age at first marriage.

We use the variable “Relationship to household head” in the Census and CPS to record households in which a cohabiting partner is present. The Census began recording unmarried partners only in 1990, and the CPS only in 1995. As a result, in the longitudinal analysis we may miss cohabitations for cohorts born before 1965 when we use 30 as the age cutoff, or 1955 when we use 40 as the cutoff. In the cross-sectional analysis, we may similarly miss cohabitations for cohorts born before 1960 or 1950, depending on the age cutoff. In the data we observe that cohabitation for early cohorts is limited. For the 1965 cohort, the share of individuals cohabiting at age 30 was 2.8%. For the 1955 cohort, the share cohabiting at age 40 was 0.76%. We examined data in the National Survey of Families and Households (NSFH) to test whether we miss a substantial number of cohabitations for the cohorts for which we do not have the cohabitation variable, especially at the lower age cutoffs. The first wave of the NSFH (1987-1988) is nationally representative and provides retrospective data on marriage and cohabitation. We use the dataset to examine cohabitation patterns at age
30 for cohorts born 1957 or earlier. We found that cohabitation at age 30 is almost non-existent for pre-baby boom cohorts. From 1945 to 1957, the average share of individuals cohabiting is 0.5%. We conclude that we only marginally underestimate the share ever married or cohabiting at age 30 for the early baby boom cohorts.

The variable age difference between spouses is used to test the search model using both longitudinal and cross-state variation. With longitudinal variation, we use cohorts born between 1930 and 1975 so that we can use the CPS to construct this variable. Specifically, for each cohort we consider all women between the ages of 30 and 35 who are married. We then compute the difference between their age and the age of their spouse. Finally, we calculate the average for each cohort. When we employ cross-state variation, the average age difference is computed using the 1940-2000 Censuses, since the CPS does not have enough observations at the state level. In this case, for each decennial cohort born between 1910 and 2000 we consider all women of age 30 to 35 who are married, compute the age difference with their spouse, and calculate the average at the state level.

Table B1: Data Sets Used in the Construction of the Main Variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Variation of Interest</th>
<th>Dataset</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>State Level, Decennial Years</td>
<td>U.S. Decennial Census, 1910-1970</td>
</tr>
<tr>
<td></td>
<td>(Around the Introduction of the Pill)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>State-Level, Decennial Years</td>
<td>U.S. Decennial Census, 1930-1990</td>
</tr>
<tr>
<td></td>
<td>State Level, Decennial Years</td>
<td>U.S. Decennial Census, 1940-2000</td>
</tr>
<tr>
<td></td>
<td>State Level, Decennial Years</td>
<td>U.S. Decennial Census, 1970-2000</td>
</tr>
</tbody>
</table>

Figure B1: Cohort Size at Birth and at Age 30