Selection and the Marriage Premium for Infant Health

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Abstract

Previous research has found a positive relationship between marriage and infant health. However, it is unclear whether this relationship is causal or a reflection of positive selection into marriage. In this paper, we use multiple empirical approaches to address this issue. First, we use the rich set of information available in the Natality Detail Files to control for selection into marriage along observable characteristics. We use a technique developed by Gelbach (2009) to determine the relative importance of different covariates, and show how selection into marriage has changed over time. Second, we construct a matched sample of children born to the same mother and exploit individual-level variation in marital status at birth. We apply fixed-effects and first-differences techniques to this matched sample to account for time-invariant unobserved characteristics. We find evidence of a sizable marriage premium. However, the premium fell by over 40% between 1989 and 2004, largely as a result of declining selection into marriage by race. Accounting for selection reduces OLS estimates of the marriage premiums for birth weight, prematurity, and infant mortality by at least half.

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I. Introduction

Research has consistently found that marriage is associated with a number of positive health outcomes. Married people live longer, have fewer alcohol-related problems, and engage in fewer risky behaviors (Waite 1995). Studies also show that infants born to married parents are less likely to suffer from prematurity, low birth weight, and mortality than infants born to unmarried mothers (Bennett 1992; Bennett, et al. 1994; Bird, et al. 2000; Peacock, Bland, and Anderson 1995). These differences can be large and vary with maternal characteristics such as race, age, or education (Jacknowitz and Schmidt 2008). Disparities in infant health are of particular concern because of the potential for large impacts on long-term outcomes, including chronic illness, educational attainment, income, and the likelihood of physical disability.[1]

Despite the wealth of evidence of a positive relationship between marriage and infant health, it remains unclear whether there is a *causal* effect of marriage. A major challenge with interpreting these results as such is the possibility of selection into marriage. The observable characteristics of married and unmarried mothers are very different; they are likely different in unobservable ways as well. For example, a common concern is that healthier women may be more likely to marry and may also have healthier babies. And as Ribar (2004) notes, plausibly exogenous sources of variation in marriage have been difficult to find.

We contribute to the literature on the infant health marriage premium by using several novel approaches to address the issue of selection into marriage. We begin by using birth certificate data from the Center for Disease Control to estimate a raw marriage premium of around 177 grams and 0.28 weeks gestation; these magnitudes are similar to estimates of the

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[1] See Barker (1995); Behrman and Rosenzweig (2004); Almond, Chay, and Lee (2005); Almond (2006); Oreopoulous et al. (2008); Smith et al. (2008); Chay, Guryan, and Mazumder (2011).

effect of maternal smoking (Ward, Lewis, and Coleman 2007; Bardy et al. 1993). We then take advantage of the rich set of demographic and health controls that are available in the birth certificate data to account for selection on observable characteristics, and use a strategy proposed by Gelbach (2009) to determine the relative importance of the included covariates. We also use the Gelbach (2009) procedure to determine how selection into marriage has changed in recent years.[2]

Next, to account for selection based on time-invariant unobserved characteristics, we exploit individual-level variation in marital status across births. We construct a unique matched sample of siblings from the 1980-1988 Natality Detail Files and use fixed-effects and first-differences methods to estimate the effects of transitioning into or out of marriage. Previous research on the effects of marriage using these techniques has been limited by small sample sizes (Geronimus and Korenman 1993; Aaronson 1998). In contrast, our matched sample has over 620,000 sibling pairs. As both a check on our data and as a demonstration of the value of our matched sample, we supplement this analysis with data from the 1979 National Longitudinal

[2] Throughout the paper, we use the term selection to refer to the marriage decision. There may also be selection into the *sample* if the likelihood of having a live birth conditional on a pregnancy is correlated with the mother's characteristics. Women may select out of the sample through abortion; rates of abortion fell for both single and married women between 1980 and 2004 (Jones et al. 2009), and between 1994 and 2008 the fraction of women receiving abortions that were married declined from 18.4 to 14.8 (Jones et al. 2002; Jones et al. 2010). It is unclear, however, how selection into abortion varies by marital status, and we are therefore unable to determine how this type of selection would affect our results.

Survey of Youth and the 1995, 2002, and 2006-2010 waves of the National Surveys of Family Growth.

Across specifications, we find that selection into marriage can account for over 50% of the observed infant health marriage premium. We also show that demographic characteristics are particularly important—selection on race alone can account for about one-third of the gap in birth weights between infants born to married and unmarried mothers. However, selection on race fell between 1989 and 2004, contributing to a 40% reduction in the overall premium over this period. Accounting for unobserved heterogeneity with our panel data strategies further reduces the estimates, but for this earlier sample there does still appear to be a marriage premium—infant health improves for women transitioning into marriage, while there is a decline of similar magnitude for women who transition out of marriage. But our evidence on the importance of selection in explaining the marriage infant health premium has important implications for policy efforts to improve child outcomes by promoting marriage.[3]

II. Background

A. Marriage and Infant Health: Theory

Most theories of marriage suggest that marriage should have a positive impact on the health of both the individuals who are married and their children. Duncan, Wilkerson, and England (2006) document a number of these reasons. First, marriage facilitates easier monitoring

[3] For example, the Administration for Children and Family's Healthy Marriage Initiative is motivated by the 1996 Congressional finding that "marriage is an essential institution of a successful society which promotes the interests of children."

of each other's behavior. They note that "people behave better when someone with power to reward or sanction is watching" and marriage provides a situation in which there is someone watching you a lot of the time. Second, the institution of marriage itself may include the notion of "cleaning up your act," and may come with expectations, obligations, and social sanctions against certain behaviors that are harmful to one's health (or the health of a child). Third, marriage facilitates a wide net of social bonds involving the extended families and friends of both individuals in the marriage. Finally, marriage provides legal access to each other's resources and a system in which each individual in the marriage can take advantage of economies of scale.

The Weiss and Willis (1985) model also provides insight into why marriage would be particularly beneficial for children. In their model, both the mother and father have a utility function that includes both their own consumption and the quality of their children. Thus children are treated as a collective good by both parents. Marriage allows the couple to monitor and enforce each other's investment in the collective good through proximity and trust. This allows the couple to overcome the free-rider problem inherent with all collective goods.

There are a few reasons that marriage may lead to worse outcomes for children. For example, marriage (and the economic interdependence that it creates) may tie women and children to an abusive relationship (Gelles 1976; Strube and Barbour 1983) or limit geographic mobility (Bartel 1979; Bielby and Bielby 1992). In cases where the husband has little income, the laws that ensure sharing of resources may draw resources away from the children (Edin 2000).

This theoretical work provides some intuition for how marriage might impact infant health specifically. Known inputs into infant health include the quality and timing of prenatal care (Currie and Gruber 1996; Joyce 1999; Evans and Lien 2005; Abrevaya and Dahl 2008), nutrition during pregnancy (Almond and Mazumder 2011), abstaining from smoking during pregnancy (Evans and Ringel 1999), and the stress level of the mother (de Weerth et al. 2003, Ponirakis et al. 1997). Consistent with the theoretical channels discussed above, marriage might affect access to care by providing legal access to the health insurance benefits and income of the spouse (Hahn 1993). It may also reduce smoking or improve nutrition through the increased ability of a spouse to encourage and monitor good behavior (Umberson 1992; Laub, Nagin, and Sampson 1998). The economies of scale associated with marriage might also lead to better nutrition, and the emotional support that accompanies a good marriage might lead to lower levels of stress. Some of these channels could lead to worse health outcomes, however, if the spouse encourages harmful behaviors or if the marriage increases stress.

Of course, some of the channels for an effect of marriage on infant health might also exist in non-marital relationships—in particular, in cohabiting relationships. If, for example, cohabiting but unmarried partners are equally able to monitor behaviors like smoking and nutrition, we will be less likely to find an effect of marriage on infant health. However, other channels such as access to health insurance and income are usually only available to legally married partners. Likewise, the stress-reducing benefits of marriage may be greater when the relationship is legally recognized (Stack and Eshleman 1998).

B. Marriage and Infant Health: Evidence

The preponderance of evidence across a number of studies indicates that infants born to married parents are less likely to suffer from prematurity, low birth weight, and mortality than infants born to unmarried mothers (Bennett 1992; Bennett, et al. 1994; Bird, et al. 2000; Peacock, Bland, and Anderson 1995; Raatikainen et al. 2005). In a 2011 meta-analysis, Shah and Ali conclude that unmarried women have higher unadjusted odds of low birth weight (1.46), prematurity (1.22), and small-for-gestational age (1.45). When only adjusted odds estimates were included in the analysis, the ratios were attenuated but still large. A few studies fail to find a protective effect of marriage for some demographic groups. For example, Jacknowitz and Schmidt (2008) note that children born to unmarried mothers who have at least a college degree do not suffer from negative birth outcomes. Bennett (1992) finds the relationship between marital status and birth outcomes varies by maternal race and age, and suggests that high infant mortality rates for married teen mothers challenge the notion that childbirth is protected by marriage *per se.*

A serious limitation of the research on the infant health marriage premium is the difficulty in accounting for selection into marriage. In his review of empirical studies of the relationships between marriage and outcomes like health or children's well-being, Ribar (2004) states that "selectivity appears to be more than a hypothetical concern" and that "studies in this area generally do not address issues associated with selection and omitted variables bias." When researchers do acknowledge potential selection bias, the usual approach is to control for common cofounders like race, education, and age. Ribar asserts that this approach is "sensible but is only successful if the researcher knows which variables are missing and can find the corresponding measures." Ribar suggests that panel data methods could be provide some insight, but a lack of large panel data sets has precluded their use.[4] He also suggests the use of instrumental variables, but good instruments for marriage are difficult to find.[5]

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[4] While not the focus of their papers, Royer (2004) and Abrevaya and Dahl (2008) do provide fixed-effects estimates of the effect of marriage on infant health using state-level data linking

In this paper, we address these issues in several ways. First, we take advantage of the rich set of observable characteristics available in the Natality Detail Files from 1989-2004 to account for selection on observables. Notably, we are able to include a measure of maternal health, a potentially important omitted variable in previous work. Second, we use an innovative statistical technique developed by Gelbach (2009) to determine *which* covariates are important in explaining the infant health marriage premium, and how much they matter. We apply this technique year-by-year to show how selection into marriage changed over this period. Third, we create a unique matched sample of over 620,000 sibling pairs from the 1980-1988 Natality Detail Files, allowing us to implement fixed effects and first-differences methods that account for time-invariant unobserved characteristics.

III. Data

Our primary analysis uses data from US birth certificates for the years 1980-2004, from the Center for Disease Control's Natality Detail Files. As of 1985, all states report 100% of their

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birth certificates. Royer finds insignificant effects, while Abrevaya and Dahl find small positive effects (though they condition on other endogenous variables like smoking behavior and prenatal care).

[5] Two notable papers that have used an IV for marriage are Finlay and Neumark (2010) and Dahl (2010). Finlay and Neumark use incarceration rates as an instrument for marriage, while Dahl uses state variation in minimum age requirements for marriage. Interestingly, both papers find that for women whose decision to marry is affected by these instruments (and who are generally low socioeconomic status), marriage has negative effects on outcomes.

birth certificate data, accounting for over 99% of all births.[6] This data includes information on characteristics of the mother (age, race, education, state of residence, and marital status) and infant (gender, birth order), and infant outcomes (birth weight and gestation). A major revision to the birth certificate data in 1989 added indicators for maternal risk factors such as anemia and diabetes. There are approximately four million records each year. We restrict our sample to mothers over age 18 since they are eligible to be married in all states and years.

For most states, the birth certificate includes information about the actual marital status of the mother, though some states impute that a mother is unmarried if the surname of the father is missing or does not match that of the mother. The number of states that impute the marital status has dropped from 9 states in 1980 to only 2 states in 2004. One potential limitation is that the birth certificate data does not indicate *when* the mother was married. In the years just before the start of our sample (1975-1979), 49% of white women and 11% of black women who had a premarital conception experienced a "shotgun" wedding (O'Connell and Rogers 1984). If marriage has a causal effect on infant health, having mothers in our sample who get married shortly before the birth will bias our estimates toward zero, since the likely channels for the effect might not have been in place during the prenatal period. To assess the potential size of this bias, we use data on the timing of first marriages and first births from the 1979 National Longitudinal Survey of Youth. About 80% of women in the data with a first birth over age 18

[6] Prior to 1985, a few states reported only 50% of the birth certificate data.

between 1980 and 2004 had a first marriage before a first birth.[7] Of this group, 15.3% married during the 9 months prior to the birth and 9.5% married during the 6 months prior to the birth.

Another limitation of this data is that we only observe whether or not the mother is married, and not whether single women are cohabiting at the time of birth. Cohabitation rates have been increasing over the period that we include in our analysis; if cohabitation confers many of the same benefits as marriage our estimates of the marriage premium in infant health should decrease over time. We discuss this issue in more detail in the results section.

Jacknowitz and Schmidt (2008) show that the effect of marriage on infant health is heterogeneous, and so we also conduct the analysis separately for white and black women.[8] Black mothers are an important population, as their rates of non-marital childbearing and adverse outcomes like low birth weight are especially high. Moreover, our results below suggest that there is significant selection into marriage by race; estimating results separately for blacks and whites allows us to examine the degree of selection along other characteristics within these groups.

Figure 1 depicts the change in non-marital birth rates from 1980-2004, for all mothers over 18 and for blacks and whites. The overall non-marital birth rate for mothers over 18 has increased from about 14% in 1980 to about 32% in 2004. Rates are much higher for blacks, for

[7] This number is higher than the 70.5% of first births that are observed to occur to married mothers in the Natality data because women who have a first marriage before a first birth may have divorced before the birth.

[8] We have also produced results that stratify the sample by education and maternal age. Results are qualitatively similar to those for the full sample, and so we omit them here for brevity.

whom nearly two-thirds of births in 2004 were non-marital. However, non-marital childbearing appears to have stabilized for blacks since 1994, while rates for whites have continued to rise.

Summary statistics by marital status are presented in Table 1. Here, the sample is limited to births after the 1989 birth certificate revision. We use six different measures of infant health: birth weight in grams, an indicator for low birth weight (<2500 grams), weeks of gestation, an indicator for prematurity (<37 weeks), an indicator for low Apgar score (<7), and an indicator for infant mortality within the first year.[9] We choose these outcomes because they are widely-used measures of infant health, and each measure has strengths and weaknesses. Birth weight in grams and gestation in weeks are appealing because they measure health across the entire distribution. Low birth weight, prematurity, and low Apgar scores measure adverse infant health outcomes, but are defined by arbitrary (though widely-accepted) cutoffs. Apgar scores are correlated with neonatal death but may not be good predictors of longer-term outcomes (Oh et al. 1996). Finally, infant mortality is an important but extreme outcome—there are fewer than 5 infant deaths per 1,000 births for this sample. But while no measure is perfect, our use of the six in combination should give a comprehensive picture of the relationship between marriage and infant health.

[9] We use the 5-minute APGAR score, which is an assessment of the infant's overall health five minutes after birth, using a 10-point scale. All data are from the 1989-2004 Natality Detail Files, with the exception of infant mortality, where we use the Vital Statistics linked infant death/birth certificate data from 1989-1991 and 1995-2002. The NCHS did not produce these data for 1992-1994.

We see that for all samples, married mothers have babies with higher birth weight and greater gestation. The gap in birth weight is 177 grams for the full sample, and the gap in gestation is 0.28 weeks. For comparison, these gaps are comparable to estimates of the effects of maternal smoking—Ward, Lewis, and Coleman (2007) find that smoking is associated with a gap of 168 grams, while Bardy et al. (1993) estimate a gap of 0.24 weeks gestation for infants exposed to tobacco smoke. Infants born to married mothers are less likely to be low birth weight (4.1 percentage points), premature (4.7 percentage points), or to have a low Apgar score (0.7 percentage points). Infant mortality rates are 59% higher for single women versus married women. The estimated premiums are generally smaller for black and white subsamples, which we would expect if selection into marriage is responsible for some of the full-sample premium.

Table 1 also shows demographic characteristics of mothers by marital status. Married mothers are older, more highly educated, and more likely to be white on average than unmarried mothers. Married mothers are less likely to experience a medical risk factor (26% vs. 30%). For the full sample, pregnancy-related hypertension is the most common condition (3.6%), followed by diabetes (3.0%) and anemia (2.3%). For each measure, it appears that married women are positively selected on characteristics that are associated with improved infant health outcomes (though the relationship between age and infant health is non-linear, as we discuss below).

IV. Empirical Strategy

A. The Marriage Premium and Observable Characteristics

Our first approach is to document the infant-health marriage premium in the birth certificate data. We begin by estimating the raw premium, and then add controls for a rich set of observable characteristics of the mother. The basic specification is:

$$y_{ist} = \beta_0 + married_{ist}\beta_1 + X_{ist}\beta_2 + risk_{ist}\beta_3 + \alpha_s + \delta_t + \varepsilon_{ist} \quad (1)$$

where *i* indexes the birth, *s* indexes the mother's state of residence, and *t* indexes the year of the birth. To make computation possible, the data are collapsed to cells according to demographic characteristics, and regressions are weighted by cell size.[10] The dependent variable y_{ist} is therefore either the average infant birth weight in grams, the fraction low birth weight, the average gestation in weeks, the fraction premature, the fraction with a low Apgar score, or the one-year infant mortality rate for the cell. Our primary independent variable of interest in Equation (1) is $married_{\text{ist}}$, which is the fraction married at birth in the cell.

OLS estimates of Equation (1) without the terms X_{ist} , $risk_{\text{ist}}$, α_s, and δ_t will yield our baseline (unadjusted) estimates of the marriage premium. We will then add these controls for observable maternal and infant characteristics to account for some types of selection into marriage. The vector X_{ist} includes the following maternal and infant demographic characteristics: single year-of-age dummies, single year-of-education dummies, birth order dummies, indicators for black and other race, and the fraction female in the cell. The next term, $risk_{\text{ist}}$, is the fraction with a health-related risk factor in the cell (defined above), to address the issue of the selection of healthier women into marriage. Finally α_s is a vector of state-of-residence fixed effects, and δ_t is a vector of year dummies that control non-parametrically for national trends in infant health outcomes. Because maternal risk factors are only available after 1989, we restrict our sample to the 1989-2004 period for this analysis. We further limit our sample to women for whom education is observed (over 95% of the full sample).

[10] Cells are defined by single year-of-age, single year-of-education, birth order, race, state, birth year, and marital status.

A comparison of the estimated marriage premium in the basic and full specifications indicates how much of the unadjusted premium is due to selection along observable characteristics. However, we are interested in not only how much the covariates change the estimate, but also *which* covariates are responsible for the change. Is selection along demographic characteristics more or less important than selection on health, for example? One common strategy would be to see how the premium changes as covariates are added sequentially; however, the results from this approach can be very sensitive to the order of their addition (Gelbach 2009). Gelbach provides a method for estimating the contribution of various sets of covariates to the change in the coefficient that is conditional on all covariates and invariant to the order in which they are added. Intuitively, the mean differences between married and unmarried mothers in (for example) demographic characteristics or health are scaled by their infant-health impact conditional on all other covariates.[11] We implement this "Gelbach

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[11]More specifically, consider a regression $y = X_1\beta_1 + X_2\beta_2 + \varepsilon$ that omits the matrix of regressors X_2; the omitted variables bias for β_1 is then $(X_1'X_1)^{-1}X_1'X_2\beta_2$. (Here, X_1 is an indicator for marriage and X_2 includes controls for demographics and maternal risk factors as well as state and year fixed effects.) Gelbach decomposes the contribution to this bias from covariate *k* in X_2 as $(X_1'X_1)^{-1}X_1'X_{2k}\beta_{2k}$, where X_{2k} is column *k* in X_2 and β_{2k} is the associated coefficient for X_{2k} in the regression on *y*. This decomposition is conditioned on all other covariates and thus is invariant to the order in which covariates are considered. The decomposition sums up over *k* to the full omitted variable bias, and Gelbach shows that under

decomposition" to identify the important dimensions along which there is selection into marriage. We then extend this work by conducting the analysis year-by-year, to explore how selection into marriage is changing over time.

B. The Marriage Premium and Unobservable Characteristics

The methods in the previous section allow us to gauge the extent to which marriage is related to infant health through measurable channels. To deal with the issue of selection into marriage based on unobservable characteristics, we use an estimation strategy that exploits individual-level variation in a woman's marital status across births. We use data from the 1980-1988 Natality Detail Files and exploit information on the month and year of the previous birth. This allows us to match each mother who is having her second or higher birth with the Natality record from the previous birth. We match both on the month and year of the previous birth as well as characteristics of the mother including which state she was born in, state of residence, race, and the year that she was born. Because some mothers with these characteristics could still potentially match with many other previous records, we keep only the births that generate a unique match. We are able to identify a unique match for 2.5% of the sample, or over 620,000 sibling pairs.[12]

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reasonable conditions asymptotic estimation of the covariance matrix for the terms in the decomposition is obtainable (Buckles and Hungerman, *forthcoming*).

[12] Two previous papers have used similar approaches to create longitudinal data set using the Natality Detail Files. Currie and Moretti (2002) use first and second births from the 1970-1999 files to estimate the effect of education on infant health. Abrevaya (2006) matches mothers for a

The Natality data allow us to create a very large sample of sibling pairs, but the matched sample is not likely to be nationally representative given the way matches are identified. In Appendix Table 7, we show the results of a probit regression, where the dependent variable is equal to one if we can uniquely identify a younger sibling in the data. We see that we are more likely to find a unique match for infants who are part of a small group, like a minority race or a small state. Also, we find more matches for infants who are more likely to have a younger sibling by 1988—those who are born earlier in the decade, or who are lower birth order. Infants with mothers in their thirties are more likely to be matched (as part of a smaller group), but infants with mothers in their forties are less likely to be matched (due to a lower probability of a subsequent birth by 1988). While these results do show that our sample has some distinctive characteristics, there does not appear to be clear positive or negative selection on characteristics that affect infant health.[13]

The first specification using our data for women with multiple observations is a fixed effects approach:

$$y_{ijt} = \beta_0 + married_{ijt}\beta_1 + X_{ijt}\beta_2 + \theta_j + \delta_t + \varepsilon_{ijt} \qquad (2)$$

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restricted subset of state pairs (using smaller states) for 1990 to 1998 to estimate the effects of maternal smoking.

[13] For example, matched infants are more likely to be black, which is associated with worse infant health, but are more likely to be in their 20s and 30s, which is associated with better infant health. Furthermore, we have no reason to think that being from a smaller state would be systematically related to infant health.

Variables are defined as above, but j indexes the mother, and θ_j represents the mother-specific fixed effect. The coefficient of interest is β_1, which shows the relationship between marriage and infant health for women who change marital status between births.

The specification in (2) constrains the effect of getting married or separating to be the same; for this reason we also estimate a first differences specification in which women's marital status is defined as "switch in" (unmarried for the first birth and married for the latter), "switch out" (married for the first and unmarried for the latter), or "stay unmarried" (unmarried for both births). The base case is mothers who are married for both births. The specification for these results is:

$$\Delta y_j = \beta_0 + switchin_j \beta_1 + switchout_j \beta_2 + stayunmarr_j \beta_3 + X_{ijt} \beta_3 + \delta_t + \varepsilon_i \qquad (3)$$

where Δy_j is equal to $y_{child2} - y_{child1}$. We also perform a test for whether the absolute value the "switch in" and "switch out" coefficients are of the same magnitude. This test examines whether the gains to entering marriage are similar in magnitude to the losses that occur when a mother exits marriage.

V. Results

A. The Marriage Premium and Observable Characteristics

Table 2 provides our estimates of the relationship between marital status and infant health for the full sample. The first column provides the baseline marriage premium in birth weight, gestational age, infant mortality, and probability of low birth weight, prematurity, and low Apgar score. For all six measures, marriage is associated with better infant health. The magnitudes are identical to the differences in means for the two groups described above.

In the next column, we show the estimate of the marriage premium obtained after including the full set of controls in Equation (1). The third column gives the difference between the baseline (unadjusted) and full (adjusted) estimates. For all measures except the Apgar score, the marriage premium falls by more than half when the additional covariates are included, indicating that selection along observables contributes significantly to the estimate of the unadjusted premium. For Apgar scores, the premium declines by 46%.

The results of the Gelbach decomposition are presented in the remaining columns, and indicate which sets of covariates are important in accounting for the marriage premium. In all cases, most of the reduction in the premium comes from the demographic controls. For birth weight, demographic controls account for 52% of the raw marriage premium (92.03/176.70). Maternal health and state and year fixed effects combined, on the other hand, account for less than 6%. Demographic characteristics account for 63% of the marriage premium in infant mortality (-1.52/-2.42), while maternal health has very little impact. For the other measures of infant health, demographic characteristics account for between one-third and one-half of the raw premium. Thus the results of the Gelbach decomposition show that selection into marriage along demographic characteristics (age, education, race, birth order, and gender) drives the infant health marriage premium much more than selection along other observable characteristics.[14]

[14] We have also produced results for birth weight that control for gestation. The results are not surprising—adding this control further reduces the coefficient on marriage by about 22%. This suggests that some, though not nearly all, of the marriage/birth weight relationship is coming from the fact that married women have longer gestations on average.

The two panels in Table 3 provide these same specifications for whites and blacks separately. We omit results for weeks of gestation and low Apgar score for brevity, but results are consistent with those for the four measures shown. For whites, estimates of the marriage premium are generally smaller than those for the full sample, but adding the controls still reduces the estimate significantly. For blacks, however, there appears to be much less selection on observables. For low birth weight and prematurity, adding the controls reduces the estimate of the premium by only 4 percent. This is driven by the addition of the demographic controls, which actually serve to increase the estimate of the premium—suggesting that for blacks, the characteristics that are associated with marriage are correlated with *higher* rates of low birth weight and prematurity.

In Figure 2, we explore how the marriage premium for birth weight is changing over time.[15] First, we see that both the raw and adjusted premiums fell substantially between 1989 and 2004. The raw premium went from 226 to 135 grams (a 40% drop), and the adjusted premium went from 105 to 55 grams (a 48% drop). Thus the fraction of the gap explained by observable characteristics increased slightly over this period. In results not shown here, we confirm that this dramatic drop in the premium was a result of both an increase in birth weights for unmarried mothers and a decrease in birth weights for married mothers.

In the Natality data, we are unable to distinguish between unmarried mothers who are single and unmarried mothers who are cohabiting. Since cohabiting relationships may provide

[15] We use birth weight as our measure of infant health in all figures, since it is a widely used measure that captures trends in the entire distribution of infant health. Results are similar using the alternative measures.

many of the same protective effects as marriage, it is likely that the infant health of children born to cohabiting mothers will be more similar to children born to married mothers than are the outcomes of children born to non-cohabiting single mothers.[16] Over most of the period that we examine in our analysis, the fraction of children born to cohabiting couples has followed an increasing trend (Raley 2001). This rise in the cohabiting rates among the single mothers likely contributes to the decrease in the marriage premium that we document in Figure 2.

In Figure 3, we further investigate this fall in the premium by again using the Gelbach decomposition to determine how selection along observables contributes to the marriage premium. But here, we do the decomposition year-by-year, so that we can see how this selection changes over time. We also disaggregate the demographic covariates, so that we can see the extent of selection along child characteristics (birth order and gender) and the mother's age, race, and education separately. The figure shows the contribution of the indicated characteristic to the raw birth weight premium, in grams.

First, note that selection into marriage by race is the most important factor in explaining the marriage premium. In 1989, 80 of the 226 gram difference between birth weights for married and unmarried mothers (or 35% of the gap) is accounted for by race. By 2004, it is still the most significant contributor, but both the contribution in grams and the percent of the premium accounted for by race have fallen significantly (to 39 grams and 29%, respectively). Mother's education is the second most important factor for most of the period, while mother's health, age, child characteristics, and state fixed effects contribute fewer than ten grams to the premium in

[16] Studies that confirm this observation for other health outcomes and behaviors include Duncan, Wilkerson and England (2006) and Wu et al (2003).

any given year. Mother's age actually increases the gap in the beginning of the period, but attenuates it in later years.

Figures 3 provides some insight into how and why the infant health marriage premium changed between 1989 and 2004. Rates of non-marital childbearing stabilized for blacks over the late 1990s and early 2000s, while rates for whites increased (rates for other racial groups were relatively stable). This meant that selection into marital childbearing according to race declined, contributing to a reduction in the marriage premium (since birth weights are higher for white women on average). At the same time, married women were having babies much later—there was a nearly 2-year increase in both the mean and median age at birth for married mothers over this period, while for unmarried mothers there was little change. As older women have higher birth weight babies on average, this served to increase the marriage premium and the importance of age in explaining it.[17] But on net, the declining selection into marital childbearing according to race dominated, so that the raw marriage premium fell dramatically over this short period.

Before turning to our panel data results, we conduct one additional exercise to explore the possible role of selection on unobservable characteristics in explaining the marriage premium. Using the method developed by Altonji, Elder, and Taber (2005), we estimate the ratio of selection on unobservables to selection on observables that would be required in order to

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[17] The relationship between mother's age and birth weights is quadratic, with a peak occurring at around age 32. The average mother's age for married mothers increased from 27.7 in 1989 to 29.4 in 2004, still in the range where increasing maternal age is associated with higher birth weights.

attribute the entire infant health marriage premium to selection bias. The included observables are the same as those used to create the adjusted estimate in Table 2, with the exception of race and state and year fixed effects.[18] The Altonji, Elder, and Taber method is only valid under the assumption that no single characteristic dominates the distribution of the endogenous or dependent variable; because the results in Figure 3 suggest that this assumption may be violated for the full sample, we conduct this analysis separately for blacks and whites.

For birth weight, the implied ratio for whites is 0.49 and for blacks is 0.18, suggesting that the marriage premium would be completely explained by selection bias if the amount of selection on unobservables was at least 49% and 18% percent as large as the amount of selection on observables, respectively. For rates of prematurity, the ratio is 0.19 for whites and 0.25 for blacks; for low birth weight the ratios are 0.22 and 0.28. These implied ratios suggest that estimates of the marriage premium for 1989-2004 could be entirely due to selection if there is even a moderate amount of selection on unobservables.[19]

B. Potential Mechanisms

[18] We omit the fixed effects because the Altonji, Elder, and Taber method requires that the observables are drawn randomly from the full set of characteristics that determine the outcome. Conceptually we think that state and year fixed effects might violate this assumption; practically it makes almost no difference since these covariates explain very little of the variation in infant health.

[19] By comparison, Altonji, Elder, and Taber find that the ratio of selection on unobservables to selection on observables would have to be 3.55 in order to explain the entire difference in high school graduation rates between Catholic and non-Catholic schools.

The above results show that much of the marriage premium for infant health is accounted for by selection, but there is still scope for a small causal impact. To investigate the importance of some of the mechanisms for a causal effect discussed in Section II, we now add controls for smoking, prenatal care, household income, and insurance coverage. We view these variables as endogenous to marriage, though there may be selection along these characteristics as well.[20] We continue to use the 1989-2004 Natality Detail Files for this analysis, since the data include information on both maternal smoking and prenatal care. Our measure of prenatal care is an indicator for receiving care in the first trimester.

Household income and insurance coverage are not available in the Natality data. To include them as controls in our model, we use data on women with children under six from the 1989 to 2004 Current Population Survey (CPS). We collapse the data to cells according to state, year, age, education, race, and marital status, and then merge income and insurance characteristics to corresponding cells in the Natality data.[21] Seventy-six percent of the observations in the Natality data are matched to CPS data—cells with older women or women who are neither white nor black are less likely to be matched. While we expect there to be some

[20] In the analysis above, we treat mother's health as exogenous, since many of the health conditions identified are chronic (such as chronic hypertension, renal and cardiac disease, and some diabetes). But the mother's health is also a mechanism through which marriage could affect infant health. Since the above results consider mother's health separately, one could easily approximate the effect of treating health as a mechanism rather than as a channel for selection.

[21] To increase the number of cells in the Natality data with a match in the CPS data, we use five-year of age cells and education cells defined by degree status.

measurement error with this approach, it will allow us to investigate whether these channels are likely to be important. There is reason to believe that insurance in particular could contribute to the infant health marriage premium, as the decline in the premium over the 1990s coincides with rising rates of insurance coverage for single women as a result of Medicaid expansion (Aizer and Grogger 2003). On the other hand, there is less evidence that the expansion of coverage led to improved birth weight outcomes (Dubay et al. 2001).

Results of this exercise are in Table 4. We include all controls from equation (1), and then successively add controls for the fraction in the cell that smokes, that received prenatal care in the first trimester, the average household income for the cell, and the fraction covered by insurance in the cell.[22] For both birth weight and low birth weight, controlling for smoking reduces the coefficient on the indicator for married by 32%. Thus smoking behavior plays an important role in explaining the remaining marriage premium, though we cannot distinguish between a selection mechanism (smokers are less likely to marry) and a causal mechanism (marriage makes people less likely to smoke). Controlling for prenatal care also reduces the marriage coefficient, by about 10% in each case. Our controls for household income and insurance coverage, on the other hand, have very little effect—likely because the variables themselves have little relationship to our measures of infant health. When all controls are included, marriage is associated with a birth weight premium of about 40 grams, and a one percentage point decrease in the incidence of low birth weight.

C. The Marriage Premium and Unobservable Characteristics

[22] We do not do the Gelbach decomposition here because we are interested in both the total effect of the mechanism controls, and on the unconditional effects of the controls individually.

The results in the previous section indicate that much of the infant health marriage premium is due to observable characteristics, but that in most cases a meaningful premium remains after these controls are added. We now turn to methods that allow us to address issues of *unobserved* heterogeneity. First, in Table 5 we present estimates of the marriage premium for our matched sample from the 1980-1988 Natality data. Compared to Tables 2 and 3 which used data from 1989-2004, we find even larger estimates of the baseline marriage premiums. This is consistent with the evidence of a declining infant health marriage premium seen above.

Just as before, adding controls for observable characteristics substantially decreases estimates of the premium. When we include the mother fixed effects, we find that the marriage gap drops even further, so that our fixed effects estimates of the premiums for the full sample are about 40% of the estimates of the unadjusted premium. But a statistically significant premium remains; for birth weight the fixed effect estimate (96 grams) is above half of a standard deviation of the birth weight across the full sample. The fixed effects also show that being married is associated with a 2.7 percentage point decrease in the likelihood of low birth weight and a 3.0 percentage point decrease in the likelihood of prematurity. Results are similar for both whites and blacks.[23]

[23] Because the 1980-1988 Natality Detail Files do not include information on smoking, we limit our exploration of mechanisms to the 1989-2004 data (see Table 4). However, we have replicated the fixed effects results in Table 5 including a control for prenatal care, which is available in the earlier data. Including this control attenuates our estimates of the marriage premium by 7% for birth weight and by 6% for low birth weight. This is consistent with the

In the bottom two rows of Table 5, we show the results from a replication of our analysis using data from two panel data sets—the 1979 National Longitudinal Survey of Youth (NLSY79) and the National Surveys of Family Growth (1995, 2002, and 2006-2010). Results using these data are interesting for two reasons. First, NLSY79 and NSFG are much smaller than our matched sample, and a comparison of the precision of the estimates demonstrates the value of our large sample. Second, these datasets are nationally representative and allow for siblings to be matched perfectly. If our point estimates are similar to those from the NLSY79 and NSFG, it suggests that our results are not an artifact of the unique properties of our sample.

Particularly for birth weight and low birth weight, the OLS point estimates are very similar to those for the full sample from the Natality data. Fixed effects estimates of the premium are smaller in the NLSY79 and NSFG. In fact, the NLSY estimate for birth weight is negative, and the NSFG estimates for low birth weight and prematurity are positive. The standard errors are very large, however, so that large beneficial marriage premiums for the fixed effects parameters cannot be rejected. That the estimates from the NLSY and NSFG are similar to those in the matched Natality sample but are much less precise highlights the value of our approach.

The results in Table 6 use data from the same matched Natality sample, but allow the effects of marriage to vary by whether the mother transitioned into or out of a marital relationship. The results are striking—first, estimates of the premium are quite close to those from the fixed effects specification. Moreover, in most cases the effect of moving *into* the

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results in Table 4—differences in prenatal care can account for a small part of the infant health marriage premium.

married state is very similar in magnitude to the effect of moving *out* of it. For example, when using birth weight as the outcome variable, we find that women who enter marriage experience an increase of 98 grams between adjacent births relative to women who are married for both, while women who exit marriage experience a 108 gram relative decrease. For six of the nine estimates in Table 6, we fail to reject the hypothesis that the absolute values of the coefficients are different at the five percent level. One concern about the fixed effects estimates is that there are unobserved time-varying characteristics that affect both marital transitions and infant health. But there is no reason to expect that a spurious relationship due to time-varying characteristics or direct effects would be symmetric; the fact that they are gives us some confidence that what we are capturing is largely the effect of marriage.

We might also be concerned that adverse infant health outcomes could affect marital transitions, as there is ample evidence that prematurity and NICU stays cause stress and anxiety (Miles et al. 2007, Miles et al. 1991). Were that the case, it would appear that divorce and infant health are positively related, since the pre-divorce baby is of especially poor health. In our switching results in Table 6 we find the opposite—parents who transition out of marriage have worse infant health outcomes in the second birth. We therefore feel that our FE results are not driven by a causal effect of infant health on marriage/divorce.

Finally, it is possible that the transition itself has a direct effect on infant health.[24] There is some support for this in the results—where the estimates are statistically different, the effect of switching out of the marriage state is larger in absolute value terms than the effect of switching

[24] Wu and Hart (2002) find that transitions out of marriage are associated with decreases in physical and mental health.

in, which may reflect adverse changes in stress or resources associated with ending a marriage.[25] Thus it is important not to interpret the fixed effects results as identifying the causal effect of marriage. Rather the results show that accounting for selection on observable and time-invariant characteristics significantly reduces estimates of the marriage premium, but the possibility of a meaningful causal effect for the 1980-1988 period remains.

VII. Discussion

Using birth certificate data from the 1989-2004 Natality Detail Files, we find that there are large health disparities between babies who are born to married and unmarried parents. In fact, the marriage gaps for birth weight and gestation are as large as the gaps between mothers who do and do not smoke.[26] We use a decomposition approach developed by Gelbach (2009) to shed light on the extent to which the observed relationship between marriage and infant health can be explained by selection according to observable characteristics, and how that selection

[25] The coefficient on the dummy indicating that the woman was unmarried for both births is small in magnitude and statistically insignificant for whites, but is statistically significant for blacks. Black women who were unmarried for both births saw a greater decrease in infant health than those who were married for both. This may indicate that time-varying factors that affect infant health worsen over time for single black women.

[26] For another comparison, non-marital childbearing is associated with a 67% increase in the rate of low birth weight, while interpregnancy intervals of three rather than 18 months are associated with a 49% increase (Conde-Agudelo et al. 2006).

varies over time.[27] Adding a rich set of demographic and maternal health controls reduced the estimated premiums substantially for the full sample—selection along demographic characteristics alone could account for over half of the birth weight and infant mortality gaps. Race is particularly important, accounting for about one-third of the birth weight gap. We also find some evidence of negative selection into marriage along demographic characteristics for black mothers. And between 1989 and 2004, the raw marriage premium fell by over 40%, largely driven by declining selection into marriage by race. Our results implementing the Altonji, Elder, and Taber (2005) method suggest that the marriage premium in more recent years could be due entirely to selection if there is even a moderate amount of selection on unobservables.

We add to this analysis by constructing a unique matched sample of over 620,000 sibling pairs from the 1980-1988 Natality Detail Files. This allows us to estimate fixed-effects and first-differences specifications to account for heterogeneity in time-invariant unobserved characteristics. Doing so further reduces estimates of the marriage premium, and results are similar using comparable samples of mothers from the NLSY and NSFG. We do find that a meaningful premium remains in these specifications using the 1980-1988 data: for the full sample, transitioning into marriage is associated with an increase in birth weights of about 100 grams, and there is a similar-in-magnitude decline for women who transition out of marriage.

Taken together, our results find little scope for a large causal effect of marriage on infant health—particularly for recent years. This finding is relevant to a number of important public

[27] We note that demographers should find the Gelbach approach useful for accounting for selection bias in a variety of other settings as well.

policy reforms that have been viewed as opportunities for increasing marriage rates, including welfare reform, reducing requirements for a marriage license, or changing the way that taxes penalize marriage. Our results suggest that efforts to improve child outcomes by increasing the marriage rate may have limited impact. An important caveat is that our results assess the importance of a mother's marital status, rather than the *quality* of the relationship. They do not, therefore, speak directly to policies designed to improve the quality of marriages, such as those that promote marital counseling or marriage education.

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Table 1: Summary Statistics by Marital Status, Natality Detail Files 1989-2004

	Full Sample		White		Black	
	Unmarried	Married	Unmarried	Married	Unmarried	Married
Birth Weight (g)	3204.02	3380.72	3274.44	3403.16	3075.05	3186.32
	(236.71)	(171.62)	(195.67)	(137.99)	(229.36)	(275.66)
Low Birth Weight	0.1023	0.0612	0.0811	0.0568	0.1417	0.1097
(<2500 Grams)	(0.1101)	(0.0654)	(0.0898)	(0.0500)	(0.1230)	(0.1319)
Weeks Gestation	38.70	38.98	38.95	39.03	38.24	38.43
	(1.09)	(0.69)	(0.89)	(0.55)	(1.16)	(1.28)
Premature	0.1444	0.0977	0.1197	0.0931	0.1886	0.1541
(<37 Weeks)	(0.1264)	(0.0796)	(0.1051)	(0.0623)	(0.1363)	(0.1526)
Low Apgar	0.0198	0.0125	0.0161	0.0119	0.0267	0.0214
(<7)	(0.0631)	(0.0462)	(0.0589)	(0.0425)	(0.0681)	(0.0698)
Infant Mortality	6.53	4.11	6.07	4.06	7.56	5.38
(x 1,000)†	(40.76)	(25.84)	(36.21)	(22.69)	(47.23)	(45.33)
Age	24.67	28.70	24.60	28.65	24.73	28.69
	(5.14)	(5.20)	(5.15)	(5.19)	(5.10)	(5.34)
Education	11.81	13.52	11.62	13.51	12.13	13.39
in Years	(2.12)	(2.54)	(2.29)	(2.54)	(1.71)	(2.19)
Black	0.3526	0.0721				
	(0.4778)	(0.2586)				
Other	0.0352	0.0457				
	(0.1844)	(0.2089)				
Maternal	0.3004	0.2594	0.2876	0.2560	0.3185	0.2988
Risk Factor	(0.1754)	(0.1311)	(0.1619)	(0.1156)	(0.1778)	(0.2032)
Observations	13,548,132	37,101,735	8,303,388	32,731,122	4,777,474	2,674,789

†Infant mortality rates are constructed using the Vital Statistics linked birth/infant death files for 1989 to 1991 and 1995 to 2002. The NCHS did not produce these data for 1992-1994.
Notes: Sample is restricted to women over 18. Standard deviations in parenthesis.

Table 2: Infant Health Marriage Premium in Natality Data, 1989-2004, Full Sample

				Decomposition of Change in Coefficients From Three Added Sets of Controls:		
Dep. Var.:	Unadjusted Estimate	Adjusted Estimate	Change (Unadj-Adj)	State and Year FEs	Demographics	Mother's Health
Birth Weight in Grams	176.70	75.64	101.06	3.39	92.03	5.64
	(0.07)	(0.08)	(0.06)	(0.01)	(0.05)	(0.02)
Fraction Low Birth Weight	-0.0411	-0.0199	-0.0212	-0.0007	-0.0174	-0.0032
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Gestation in Weeks	0.2782	0.1305	0.1476	0.0276	0.0935	0.0266
	(0.0003)	(0.0004)	(0.0002)	(0.0001)	(0.0002)	(0.0001)
Fraction Premature	-0.0467	-0.0225	-0.0241	-0.0014	-0.0194	-0.0033
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Fraction Low Apgar	-0.0072	-0.0040	-0.0033	0.0009	-0.0036	-0.0006
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Infant Mortality Rate	-2.4227	-1.0463	-1.3764	0.1951	-1.5164	-0.0551
	(0.0143)	(0.0164)	(0.0106)	(0.0051)	(0.0091)	(0.0042)

Notes: Data are from Natality Detail Files, 1989-2004. The adjusted estimates include controls for mother's age, race, and education; child gender and birth order; an indicator for maternal risk factor; and dummies for year and state of residence. Columns 4-6 show the results of a Gelbach decomposition of the contribution of the indicated sets of covariates to the reduction in the estimate of the marriage premium between the unadjusted and adjusted specifications. Sample is restricted to women over 18. For tractability, data were collapsed to cells along demographic characteristics and then weighted by cell size. See Table 1 for sample sizes. Robust standard errors in parenthesis. All coefficients are statistically significant at the 1% level.

Table 3: Infant Health Marriage Premium in Natality Data, 1989-2004, by Race

	Unadjusted Estimate	Adjusted Estimate	Change (Unadj-Adj)	Decomposition of Change in Coefficients From Three Added Sets of Controls: State and Year FEs	Demographics	Mother's Health
Panel A: White						
Birth Weight in Grams	131.52	59.17	72.35	0.85	65.12	6.38
	(0.08)	(0.09)	(0.07)	(0.02)	(0.05)	(0.04)
Fraction Low Birth Weight	-0.0242	-0.0177	-0.0065	0.0000	-0.0042	-0.0023
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Fraction Premature	-0.0266	-0.0193	-0.0073	-0.0010	-0.0038	-0.0025
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Infant Mortality Rate	-2.0125	-1.1124	-0.9002	-0.0583	-0.7930	-0.0488
	(0.0160)	(0.0169)	(0.0095)	(0.0063)	(0.0061)	(0.0040)
Panel B: Black						
Birth Weight in Grams	111.27	85.63	25.65	1.35	20.85	3.45
	(0.20)	(0.24)	(0.12)	(0.04)	(0.11)	(0.03)
Fraction Low Birth Weight	-0.0321	-0.0308	-0.0013	-0.0006	0.0012	-0.0019
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
Fraction Premature	-0.0345	-0.0329	-0.0015	-0.0002	0.0005	-0.0018
	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
Infant Mortality Rate	-2.1878	-1.4209	-0.7669	-0.2356	-0.5216	-0.0097
	(0.0422)	(0.0463)	(0.0300)	(0.0217)	(0.0199)	(0.0047)

See Table 2 notes for details.

Table 4: Infant Health Marriage Premium in Natality Data, 1989-2004, with Controls for Potential Mechanisms

	[1]	[2]	[3]	[4]	[5]	[6]
Panel A: Birth Weight						
Married	70.19	47.87	63.54	69.78	70.1832	40.06
	(0.09)	(0.10)	(0.11)	(0.09)	0.0867	(0.12)
Smoking		-199.3014				-198.1742
		(0.4635)				(0.4633)
Care 1st			68.4616			64.5129
Trimester			(0.6986)			(0.6886)
Household				0.0405		0.0403
Income ($1000)				(0.0142)		(0.0143)
Covered by					0.1763	2.5949
Insurance					(0.1584)	(0.1678)
Other controls?	Y	Y	Y	Y	Y	Y
R-squared	0.4332	0.4475	0.4346	0.4332	0.4332	0.4488
Panel B: Low Birth Weight						
Married	-0.0176	-0.0119	-0.0159	-0.0176	-0.0176	-0.0102
	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Smoking		0.0504				0.0501
		(0.0002)				(0.0002)
Care 1st			-0.0167			-0.0158
Trimester			(0.0003)			(0.0003)
Income				0.0000		0.0000
($1000)				(0.0000)		(0.0000)
Covered by	0.0002				0.0002	0.0001
Insurance	(0.0001)				(0.0001)	(0.0001)
Other controls?	Y	Y	Y	Y	Y	Y
R-squared	0.1790	0.1857	0.1796	0.1790	0.1790	0.1862

See Table 2 notes for details of specification and controls. Cell-level rates of smoking and prenatal care are taken from the Natality Detail Files. Household income and insurance coverage are taken from the CPS, were cells are defined by state, year, age, education, race, and marital status and then merged to corresponding cells in the Natality data.

Table 5: OLS and Fixed Effects Estimates of Marriage Premium, Matched Sample from Natality Data, 1980-1988

	Dependent Variable:								
	Birthweight (grams)			Low Birth Weight			Premature		
Sample:	OLS(1)	OLS(2)	FE	OLS(1)	OLS(2)	FE	OLS(1)	OLS(2)	FE
All-Natality	248.22	113.50	96.01	-0.0652	-0.0367	-0.0269	-0.0762	-0.0395	-0.0304
	(1.65)	(2.01)	(3.13)	(0.0008)	(0.0010)	(0.0015)	(0.0009)	(0.0011)	(0.0019)
	1,298,578	1,146,932	1,146,932	1,298,578	1,146,932	1,146,932	1,228,550	1,086,812	1,086,812
White-Natality	189.94	132.85	107.35	-0.0435	-0.0368	-0.0278	-0.0453	-0.0371	-0.0278
	(2.08)	(2.35)	(3.79)	(0.0010)	(0.0011)	(0.0017)	(0.0011)	(0.0013)	(0.0021)
	1,013,454	905,003	905,003	1,013,454	905,003	905,003	961,943	860,114	860,114
Black-Natality	148.72	115.89	81.07	-0.0480	-0.0438	-0.0277	-0.0559	-0.0481	-0.0407
	(2.99)	(3.54)	(6.36)	(0.0016)	(0.0019)	(0.0034)	(0.0018)	(0.0021)	(0.0042)
	195,703	174,726	174,726	195,703	174,726	174,726	183,826	164,301	164,301
All-NLSY	186.70	108.89	-8.57	-0.0517	-0.0336	-0.0023	-0.0333	-0.0246	-0.0133
	(21.22)	(23.54)	(75.26)	(0.0103)	(0.0112)	(0.0377)	(0.0108)	(0.0123)	(0.0432)
	5,503	5,503	5,503	5,503	5,503	5,503	5,410	5,410	5,410
All-NSFG	207.60	105.67	65.56	-0.0610	-0.0399	0.0181	-0.0308	-0.0192	0.0211
	(31.55)	(32.38)	(181.43)	(0.0146)	(0.0150)	(0.1038)	(0.0147)	(0.0161)	(0.0940)
	3,291	3,291	3,291	3,291	3,291	3,291	3,291	3,291	3,291

Notes: Data for the first three rows are from Natality Detail Files, 1980-1988. See text for details of the matching process used to create the matched sample. Data for the last two rows are from the National Longitudinal Survey of Youth, 1979, and from the National Surveys of Family Growth (1995, 2002, 2006-2010); samples are limited to births between 1980 and 1988. Each cell is the coefficient on the marriage dummy for the indicated sample and specification. OLS(1) includes no controls; OLS(2) adds controls for mother's age, education, race, child birth order and gender, and year fixed effects; FE adds mother fixed effects. Robust standard errors in parenthesis.

Table 6: First Differences Estimates of Marriage Premium, Matched Sample from Natality Data, 1980-1988

	Dependent Variable:		
	Birth Weight (in Grams)	Low Birth Weight	Premature
Panel A: Full Sample			
Switch In	98.03**	-0.0265**	-0.0292**
	(3.57)	(0.0017)	(0.0021)
Switch Out	-107.55**	0.0309**	0.0376**
	(5.32)	(0.0026)	(0.0031)
Single for Both	-5.06	0.0048*	0.0021
	(3.89)	(0.0021)	(0.0026)
P-value for Test of Equality of Coefficients	0.159	0.178	0.0326
Observations	587,796	587,796	529,823
Panel B: Whites			
Switch In	107.84**	-0.0273**	-0.0271**
	(4.20)	(0.0019)	(0.0023)
Switch Out	-111.67**	0.0301**	0.0321**
	(6.33)	(0.0029)	(0.0036)
Single for Both	-2.20	-0.0021	0.0007
	(5.59)	(0.0030)	(0.0037)
P-value for Test of Equality of Coefficients	0.629	0.445	0.251
Observations	461,364	461,364	418,500
Panel C: Blacks			
Switch In	68.70**	-0.0230**	-0.0318**
	(8.00)	(0.0042)	(0.0052)
Switch Out	-114.48**	0.0383**	0.0561**
	(11.47)	(0.0062)	(0.0073)
Single for Both	-23.75**	0.0102**	0.0129**
	(6.87)	(0.0039)	(0.0047)
P-value for Test of Equality of Coefficients	0.00316	0.0641	0.0148
Observations	92,694	92,694	81,905

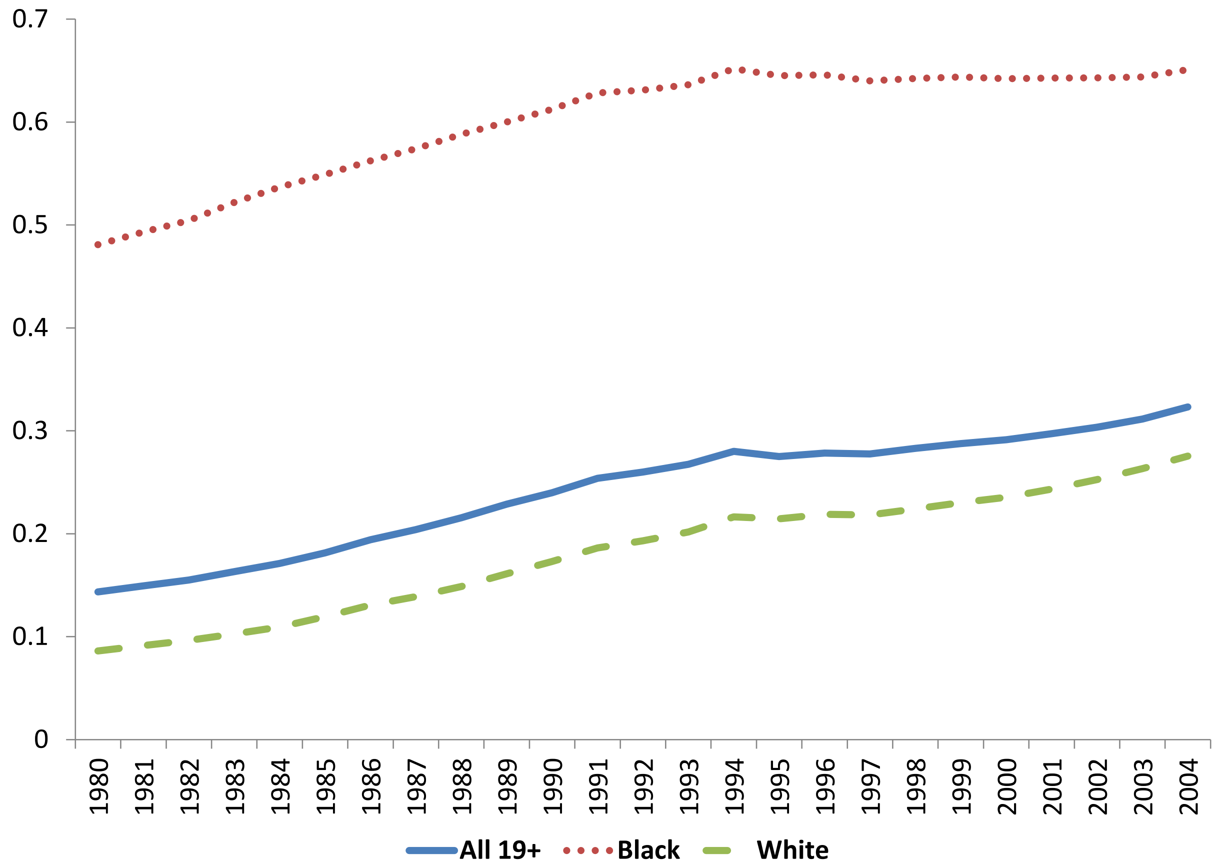
Notes: Data are from Natality Detail Files, 1980-1988. See text for details of the matching process used to create the matched sample. Columns within panels are from a single regression for the indicated sample. The independent variables identify whether the woman moved into or out of a marriage between children (switched in or out), or remained unmarried for both births. The base case is women who remained married for both births. Regressions also include controls for mother's age, race, and education, child gender and birth order, and year fixed effects. Standard errors are clustered by mother and are in parenthesis. **, * Denote statistical significance at the 1% and 5% levels, respectively.

Appendix Table 7: Characteristics Predicting a Match in the 1980-1988 Natality Data

Characteristic		All	White	Black
Age:	25-29	0.0004	0.0003	0.0019
		(0.0001)	(0.0001)	(0.0002)
	30-34	0.0023	0.0026	0.0038
		(0.0001)	(0.0001)	(0.0003)
	35-39	0.0028	0.0037	0.0023
		(0.0001)	(0.0002)	(0.0005)
	40+	-0.0053	-0.0033	-0.0108
		(0.0003)	(0.0004)	(0.0009)
Race:	Black	0.0041		
		(0.0001)		
	Other	0.0196		
		(0.0002)		
Birth Order:	Second	-0.0074	-0.0077	-0.0037
		(0.0001)	(0.0001)	(0.0002)
	Third	-0.0057	-0.0061	-0.0023
		(0.0001)	(0.0001)	(0.0002)
State Population	2nd	-0.0057	-0.0059	-0.0039
Quintile:		(0.0001)	(0.0001)	(0.0002)
	3rd	-0.0083	-0.0086	-0.0047
		(0.0001)	(0.0001)	(0.0002)
	4th	-0.0130	-0.0131	-0.0093
		(0.0001)	(0.0001)	(0.0002)
	5th	-0.0139	-0.0139	-0.0106
		(0.0001)	(0.0001)	(0.0002)
Year of Birth:	1981	-0.0003	-0.0001	-0.0012
		(0.0001)	(0.0001)	(0.0003)
	1982	-0.0007	-0.0006	-0.0016
		(0.0001)	(0.0001)	(0.0003)
	1983	-0.0011	-0.0008	-0.0023
		(0.0001)	(0.0001)	(0.0003)
	1984	-0.0024	-0.0021	-0.0036
		(0.0001)	(0.0001)	(0.0003)
	1985	-0.0050	-0.0044	-0.0067
		(0.0001)	(0.0001)	(0.0002)
	1986	-0.0089	-0.0082	-0.0102
		(0.0001)	(0.0001)	(0.0002)
	1987	-0.0166	-0.0158	-0.0175
		(0.0001)	(0.0001)	(0.0002)
	1988	-0.0242	-0.0227	-0.0277
		(0.0000)	(0.0000)	(0.0001)
Pseudo R-Squared		0.0570	0.0587	0.0383
Fraction Matched		0.0248	0.0236	0.0264
Observations		25,070,761	20,697,395	3,428,488

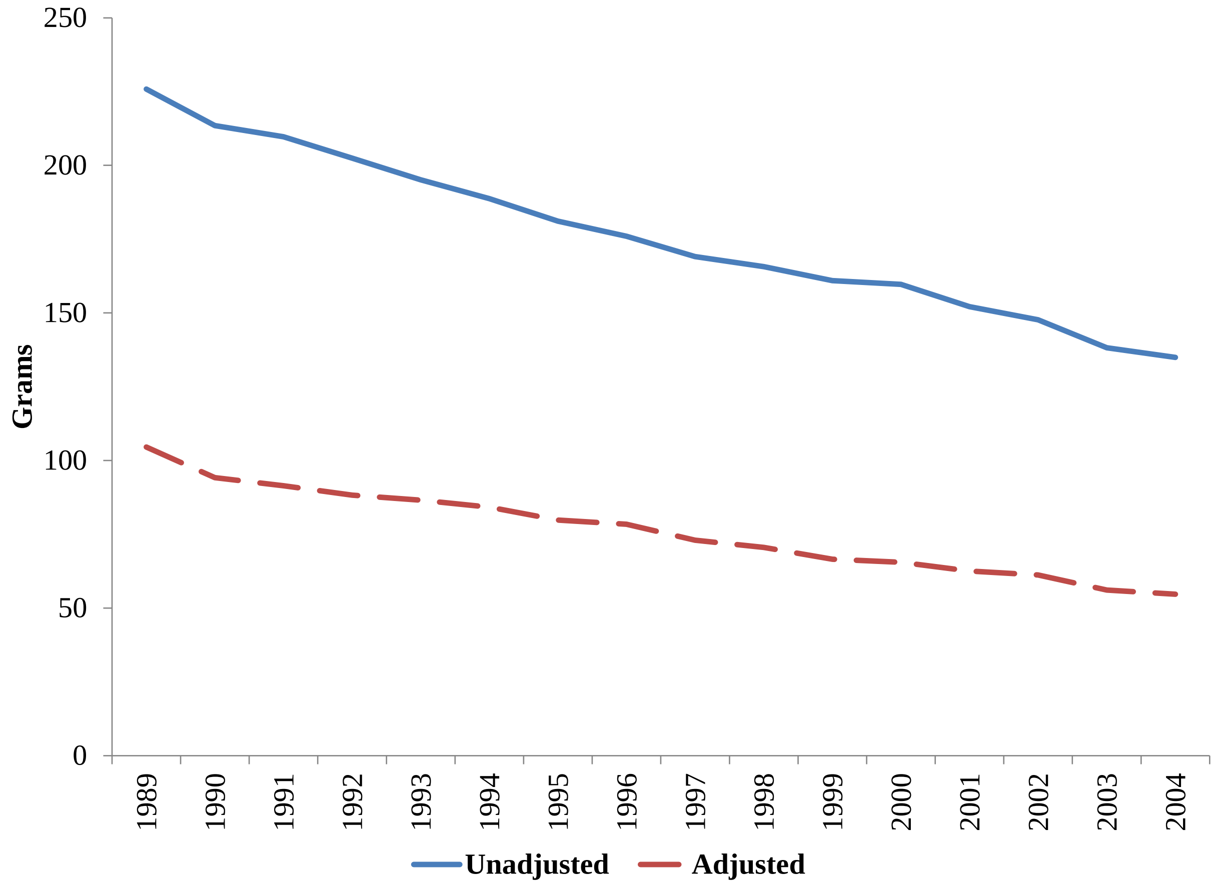
Notes: Coefficients are marginal effects from a probit regression, where the dependent variable is equal to one if the birth was matched to a later birth in the 1980-1988 Natality Detail Files. Sample is limited to births to women over 18 and to first through third births. See text for details of the matching process used to create the matched sample. All coefficients are statistically significant at the 1% level, except for the coefficient on 1981 for whites (p=0.18).

Figure 1: Fraction of Births to Unmarried Mothers, 1980-2004



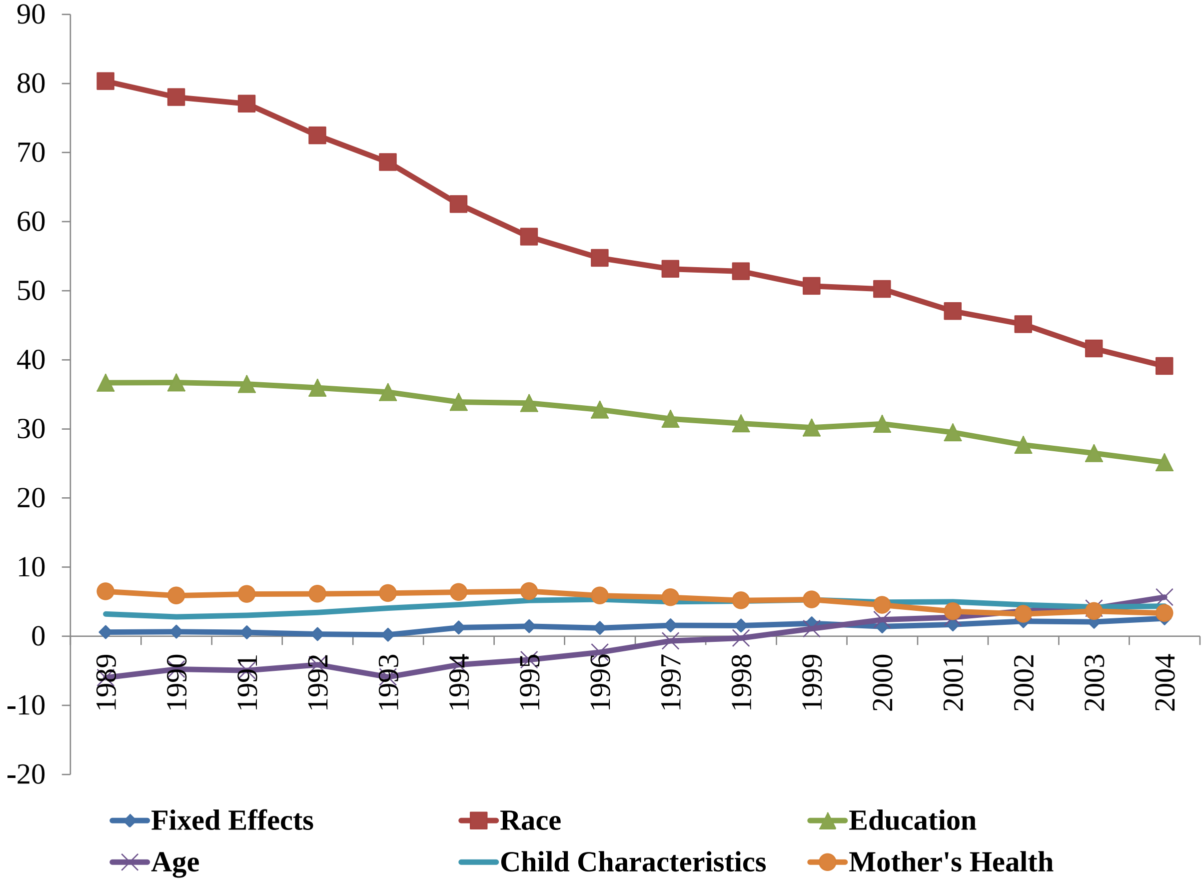
Notes: Data are from the 1980-2004 Natality files. Sample is restricted to mothers age 19+.

Figure 2: Birth Weight Marriage Premium, Natality Detail Files, 1989-2004



Notes: Data are from the 1989-2004 Natality Detail Files. Sample is restricted to mothers over age 18. The adjusted premium includes controls for mother's age, education, and race; infant birth order and gender; an indicator for the presence of a medical risk factor in the mother; and state fixed effects.

Figure 3: Contribution of Covariates to Marriage Premium, 1989-2004
(Birth Weight, in Grams)



Notes: Data are from the 1989-2004 Natality files. Sample is restricted to mothers age 19+. Results are from a Gelbach decomposition of the contribution of the indicated covariates to the observed marriage premium. See Equation (1) for the full specification.