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 observable characteristics 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 premium by at least half. These results have important implications for policy efforts to improve child outcomes by promoting marriage.

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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

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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).

around 177 grams and 0.28 weeks gestation; these magnitudes are similar to estimates of the 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.

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 700,000 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 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.[2]

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 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

[2] 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."

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.

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.

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 700,000 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 birth certificate data, accounting for over 99% of all births.[3] 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.

In 1980, nine states imputed marital status, where a mother was considered unmarried if the father's surname was missing or did not match that of the child. In 1989 six states still imputed marital status; as of 2004 only Michigan and New York did so. 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 "shot-gun" 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

[3] 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.[4] 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.

Jacknowitz and Schmidt (2008) show that the effect of marriage on infant health is heterogeneous, and so we also conduct the analysis for three subsamples of mothers: mothers age 19-24, mothers without a high school degree, and black mothers. We choose young mothers because they may be more likely to be on the margin of the marriage decision, which makes them a relevant group for policy. We also consider those without a high school degree because we are especially interested in the effects of marriage on infant health for women with low socio-economic stats (SES). Babies born to low-SES women tend to be less healthy, with lower birth weights and higher rates of prematurity (Goldenberg and Culhane 2007). Thus, any impact of marriage on infant health for these women would be particularly important. Finally, we also studied the effect of marriage on infant health for blacks, for whom rates of non-marital childbearing and low birth weight are especially high.

Figure 1 depicts the change in non-marital birth rates from 1980-2004, for mothers over 18 and for our three subsamples. The overall non-marital birth rate for mothers over 18 has increased from about 14% in 1980 to about 32% in 2004. Rates are higher for all three subsamples. By 2004, over 55% of births to young and less educated women were out of wedlock, with the rate for young mothers increasing 173% from its 1980 level. For blacks, two-thirds of births in 2004 were non-marital.

[4] 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.

Summary statistics by marital status are presented in Table 1. Here, the sample is limited to births after the 1989 birth certificate revision. The three primary outcome measures that we use to measure the infant's health at birth are birth weight in grams, weeks of gestation, and an indicator for low birth weight (<2500 grams). 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. Our estimated premiums are generally smaller for the subsamples, which we would expect if selection into marriage is responsible for some of the full-sample premium. Rates of low birth weight are much higher for unmarried mothers across samples.

Table 1 also shows demographic characteristics of mothers by marital status. Married mothers are older and more likely to be white on average than unmarried mothers. They are also more highly educated among young and black mothers, though not for those without a high school degree. 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%).

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, where infant health is measured by birth weight and gestation. 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:

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

where *i* indexes the birth, *s* indexes the mother's state of residence, and *t* indexes the year of the birth. The dependent variable y_{ist} is either the infant's birth weight in grams, gestation in weeks, or an indicator for low birth weight. We focus on these outcome measures because they are widely-used measures of infant health that are readily available and reliable in our birth certificate data. Also, as we reference above, each of these measures is associated with later-life outcomes. Our primary independent variable of interest in Equation (1) is $married_{ist}$, a dummy variable equal to one if the mother is married at birth and zero otherwise.

OLS estimates of Equation (1) without the terms X_{ist} , $risk_{ist}$, 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: a quadratic in mother's age; race (indicators for black and other race); mother's education in years; the infant's birth order; and an indicator for whether the child is a girl. The next term, $risk_{ist}$, is a dummy variable indicating whether the mother exhibits a health-related risk factor (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). To make computation possible, the data are collapsed to cells according to demographic characteristics, and regressions are weighted by cell size.

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.[5] We implement this "Gelbach

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[5]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

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 about 4% of the sample which is over 700,000 sibling pairs.[6]

Although the Natality data allows us to create a very large sample of sibling pairs, the matched sample will not be nationally representative since it will include an oversample of

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the full omitted variable bias, and Gelbach shows that under reasonable conditions asymptotic estimation of the covariance matrix for the terms in the decomposition is obtainable.

[6] We are more likely to find a unique match for women born in a smaller state, women who are neither black nor white, and mothers who were older at the time of birth.

mothers with unique characteristics. Therefore, we supplement this analysis with two nationally representative datasets that are much smaller but in which we can match siblings perfectly: the National Longitudinal Survey of Youth 1979 and from the National Surveys of Family Growth (1995, 2002, and 2006-2010). In both datasets, we construct a panel of children born to the same mother with information about each birth including the child's birth weight and gender and the marital status of the mother at the time of birth.

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

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

Variables are defined as above, but j indexes the mother. X_{ij} is a vector containing indicators for child gender and controls for the mother's age at birth, and θ_j represents the mother-specific fixed effect.

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), or "switch out" (married for the first and unmarried for the latter). The base case is mothers who do not change marital status between births. The specification for these results is:

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

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, and probability of low birth weight. First, we see that marriage is associated with an increase in birth weight of 177 grams and 0.28 weeks gestation. These gaps represent increases of about 0.86 and 0.34 standard deviations, respectively. The incidence of low birth weight is also 4.1 percentage points lower for married mothers.

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 birth weight in ounces and gestation in weeks, 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 low birth weight, 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. Particularly for birth weight, the reduction in the premium comes almost entirely from the demographic controls, which alone account for 47% of the raw premium (82.55/176.70). Maternal health and state and year fixed effects combined, on the other hand, account for less than 6%. For the other measures of infant health, demographic characteristics are still the most important contributors to the raw gap, though mother's health can explain about a quarter of the low birth weight marriage premium and 10% of the gestation premium. Thus the results of the Gelbach decomposition show that selection into marriage along demographic characteristics (age, education, and race) drives the infant health marriage premium much more than selection along other observable characteristics.

Each panel in Table 3 provides these same specifications for our three subsamples of the population. These within-group estimates of the marriage premium are generally smaller than those for the full sample, but in most cases adding the controls still reduces the estimate significantly. One exception is worth noting. For blacks, there appears to be much less selection on observables, and for low birth weight and gestation, adding the controls actually *increases* estimates of the marriage premium. This is driven by the addition of the demographic controls, suggesting that for blacks, the characteristics that are associated with marriage are correlated with *worse* infant health.

In Figure 2, we explore how the marriage premium for birth weight is changing over time. 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 115 to 62 grams (a 46% 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 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 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, nearly 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 40 grams and 29%, respectively). Mother's education is the second most important factor for most of the period, while mother's health, child characteristics, and state fixed effects contribute fewer than ten grams to the premium in any given year. Mother's age, on the other hand, contributes almost nothing to the gap in the beginning of the period, but by 2004 contributes about 14 grams, or 10% of the premium in that year.

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.[7] 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.

B. The Marriage Premium and Unobservable Characteristics

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 significant premium remains after these controls are added. We now turn to methods that allow us to address issues of *unobserved* heterogeneity. First, in Table 4 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 (98.5 grams) is above half of a standard deviation of the birth weight across the full sample. This fixed effects marriage gap in birth weight is similarly large for each of the subgroups, ranging from 83.6 to 95.7 grams. We find

[7] The relationship between mother's age and birth weights is actually non-linear, 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.

equally large marriage gaps for each of the other measures of infant health. Among the full sample of mothers, being married is associated with a gestation that is 0.23 weeks longer (or about 25% of a standard deviation). Being married is also associated with a 26% decrease in the likelihood of having a low birth weight baby. Again, we find similar magnitudes for each of these outcomes among the three subgroups that we also examine.

The results in Table 5 use data from the same 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 (particularly for the subsamples) are quite close to those from the fixed effects specification. Moreover, in most cases the effect of moving *into* the 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 108 grams between adjacent births while women who exit marriage experience a 98 gram decrease. For nine of the 12 estimates in Table 5, we fail to reject the hypothesis that the absolute values of the coefficients are different.

One concern about the fixed effects estimates is that there are unobserved time-varying characteristics that affect both marital transitions and infant health. Also, we might be concerned that the transition itself has a direct effect on infant health.[8] 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 estimating is the effect of marriage.

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

As discussed above, we were only able to match a small fraction of the Natality sample and there may be errors in the matching process. As a check on these results, we show estimates from two data sets where we observe the birth outcomes of multiple children born to the same mother—the NLSY and the NSFG. These results are in Tables 6 and 7. In both cases, we show only the results for the full sample, and for birth weight. The OLS estimates of the premium are very similar in magnitude to those from the matched sample, and again fall significantly with the fixed effects specification. In fact, the fixed effects results show a small and statistically insignificant marriage premium when siblings are perfectly observed, though the standard errors are large. We also obtain a smaller estimate of the marriage premium using these data and our "switching" specification, though results are imprecise here as well. That the estimates from the NLSY and NSFG are similar to those in Tables 4 and 5 but are much less precise highlights the value of our large matched Natality sample.

Before turning to a discussion of our findings, 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 attribute the entire infant health marriage premium to selection bias. For this exercise we return to the data from 1989-2004, which allows for a richer set of observable characteristics. The included observables are the same as those used to create the adjusted estimate in Table 2, with the exception of state and year fixed effects.[9]

[9] We omit the fixed effects because one assumption of the Altonji, Elder, and Taber (2005) approach is that the observables are drawn randomly from the full set of characteristics that

For birth weight, the implied ratio is 0.39, suggesting that the marriage premium would be completely explained by selection bias if the amount of selection on unobservables was at least 39% as large as the amount of selection on observables. For gestation and low birth rate, the ratios are even lower at 0.20 and 0.16, respectively. 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.[10] However, we interpret these results with caution, as it is not clear that the assumptions required for the Altonji, Elder, and Taber (2005) technique are satisfied here. Specifically, we have one observable element (race) that may dominate the distribution of infant health.

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 gap is as large as the gap between mothers who do and do not smoke. 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

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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.

[10] 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.

according to observable characteristics, and how that selection varies over time and across groups.[11] Adding a rich set of demographic and maternal health controls reduced the estimated premium by over half for the full sample—selection by race alone could account for about one-third of the birth weight gap. We also find some evidence of negative selection into marriage 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.

We add to this analysis by constructing a unique matched sample of over 700,000 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. However, 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.

Taken together, our results show that the relationship between marital status and infant health is much weaker once we carefully account for selection bias—particularly for recent years. This finding is relevant to a number of important public policy reforms that have been

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

viewed as opportunities for increasing marriage rates, including welfare reform, reducing requirements of marriage, or changes in the way that taxes penalize marriage. Our results suggest that simply increasing the marriage rate as a means to improve child outcomes 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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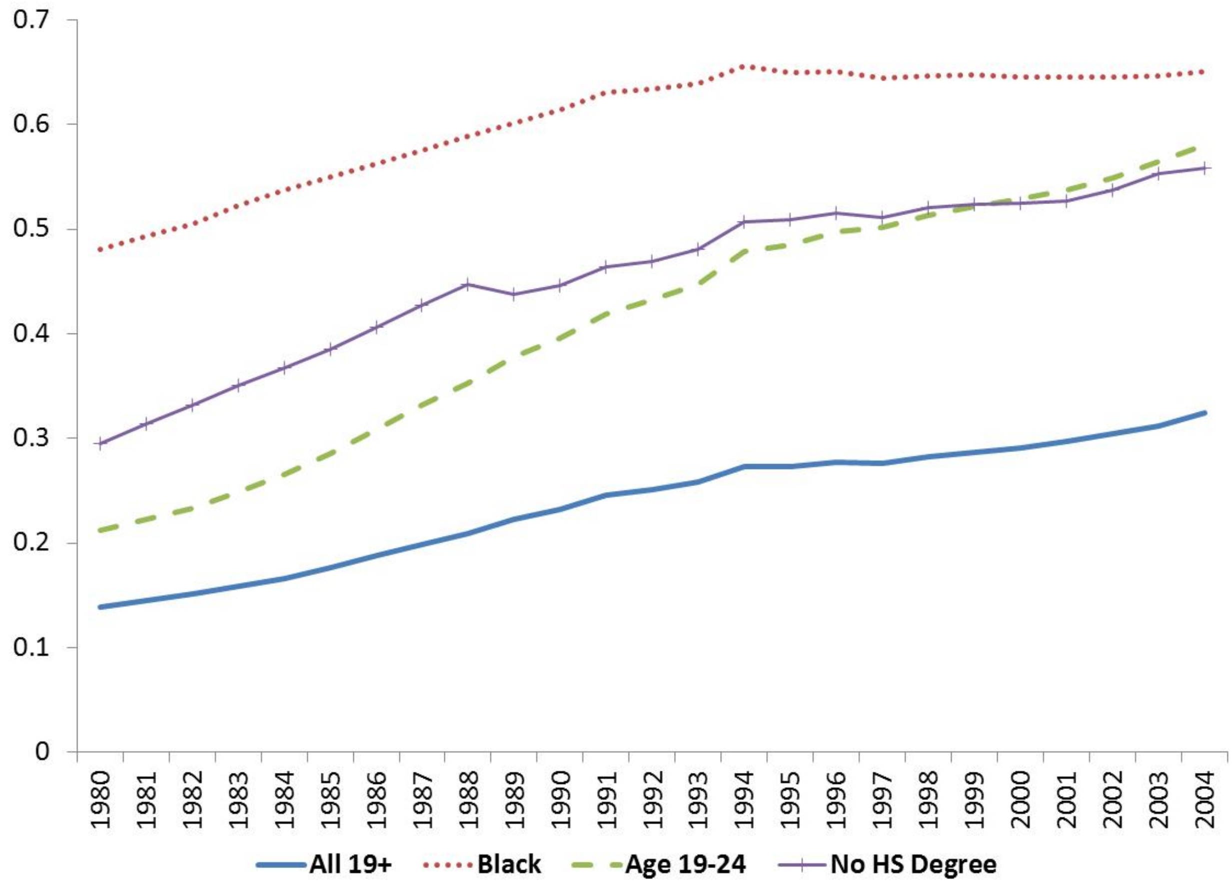
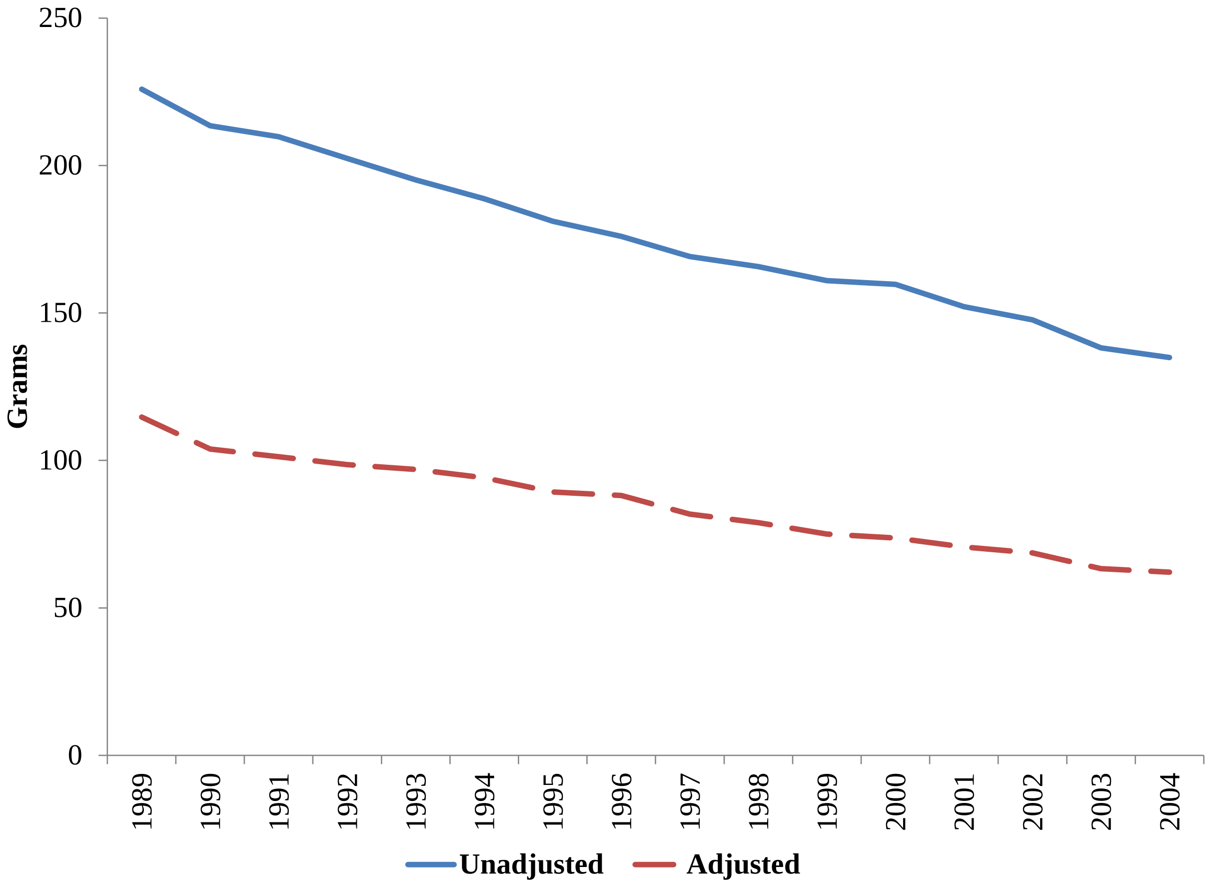
Figure 1: Fraction of Births to Unmarried Mothers, 1980-2004



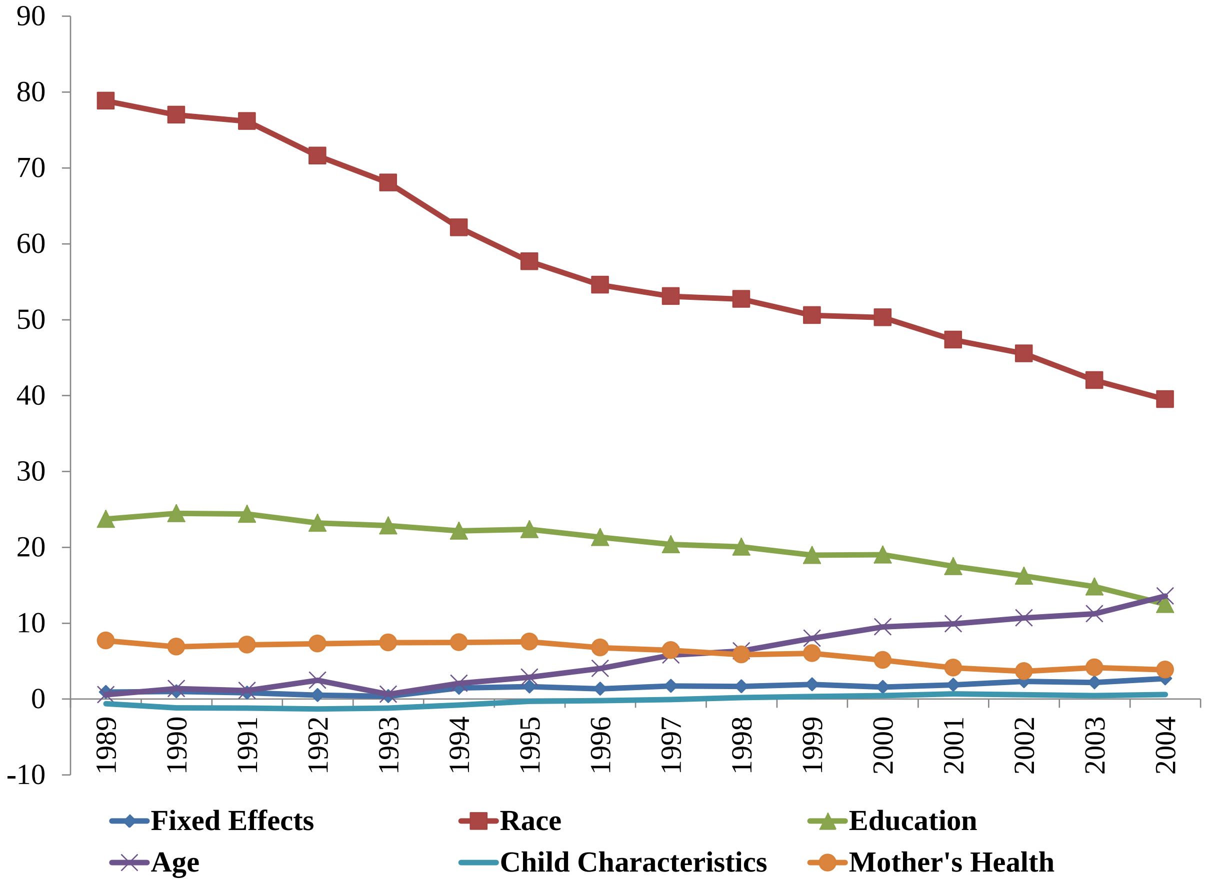
Notes: Data are from the 1980-2004 Natality files. Sample is restricted to mothers age 19+.

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.

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

	Full Sample		Age 19-24		No HS Degree		Black	
	Unmarried	Married	Unmarried	Married	Unmarried	Married	Unmarried	Married
Birth Weight (g)	3204.02	3380.72	3206.87	3338.39	3174.29	3305.42	3075.05	3186.32
	(236.71)	(171.62)	(171.10)	(148.23)	(283.85)	(274.70)	(229.36)	(275.66)
Low Birth Weight	0.1023	0.0612	0.0938	0.0615	0.1084	0.0724	0.1417	0.1097
	(0.1101)	(0.0654)	(0.0736)	(0.0592)	(0.1402)	(0.1216)	(0.1230)	(0.1319)
Weeks Gestation	38.70	38.98	38.81	39.13	38.6890	38.98	38.24	38.43
	(1.09)	(0.69)	(0.79)	(0.65)	1.3502	(1.22)	(1.16)	(1.28)
Age	24.67	28.70	21.18	21.96	23.87	25.84	24.73	28.69
	(5.14)	(5.20)	(1.66)	(1.63)	(4.84)	(5.36)	(5.10)	(5.34)
Education in Years	11.81	13.52	11.62	12.07	9.49	8.81	12.13	13.39
	(2.12)	(2.54)	(1.84)	(2.11)	(1.98)	(2.31)	(1.71)	(2.19)
Black	0.3526	0.0721	0.3485	0.0758	0.3006	0.0618		
	(0.4778)	(0.2586)	(0.4765)	(0.2646)	(0.4585)	(0.2408)		
Other	0.0352	0.0457	0.0320	0.0331	0.0340	0.0440		
	(0.1844)	(0.2089)	(0.1761)	(0.1790)	(0.1812)	(0.2052)		
Maternal Risk Factor	0.3004	0.2594	0.2812	0.2351	0.3066	0.2582	0.3185	0.2988
	(0.1754)	(0.1311)	(0.1320)	(0.1132)	(0.2141)	(0.2109)	(0.1778)	(0.2032)
Observations	13,548,132	37,101,735	8,024,297	8,682,962	3,925,274	3,893,545	4,777,474	2,674,789

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	84.35	92.34	3.28	82.55	6.52
	(0.07)	(0.09)	(0.06)	(0.01)	(0.05)	(0.02)
=1 if Low Birth Weight	-0.0411	-0.0223	-0.0189	-0.0006	-0.0148	-0.0034
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Gestation in Weeks	0.2782	0.1345	0.1437	0.0274	0.0895	0.0268
	(0.0003)	(0.0004)	(0.0002)	(0.0001)	(0.0002)	(0.0001)

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, Selected Subsamples

	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: Age 19-24						
Birth Weight	131.52	59.17	72.35	0.85	65.12	6.38
in Grams	(0.08)	(0.09)	(0.07)	(0.02)	(0.05)	(0.04)
=1 if Low	-0.0323	-0.0140	-0.0183	0.0001	-0.0148	-0.0036
Birth Weight	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Gestation	0.3197	0.1094	0.2103	0.0224	0.1648	0.0232
in Weeks	(0.0004)	(0.0004)	(0.0003)	(0.0001)	(0.0002)	(0.0002)
Panel B: No HS Degree						
Birth Weight	131.13	71.57	59.56	-0.40	53.15	6.80
in Grams	(0.20)	(0.23)	(0.12)	(0.05)	(0.11)	(0.05)
=1 if Low	-0.0361	-0.0198	-0.0163	0.0001	-0.0125	-0.0039
Birth Weight	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)
Gestation	0.2879	0.1413	0.1466	0.0029	0.1138	0.0299
in Weeks	(0.0009)	(0.0011)	(0.0005)	(0.0002)	(0.0005)	(0.0002)
Panel C: Black						
Birth Weight	111.27	94.22	17.05	1.46	11.87	3.73
in Grams	(0.20)	(0.24)	(0.12)	(0.04)	(0.11)	(0.04)
=1 if Low	-0.0321	-0.0338	0.0018	-0.0006	0.0044	-0.0020
Birth Weight	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
Gestation	0.1884	0.2569	-0.0685	0.0039	-0.0882	0.0157
in Weeks	(0.0009)	(0.0012)	(0.0006)	(0.0002)	(0.0005)	(0.0002)

See Table 2 notes for details.

Table 4: OLS and Fixed Effects Estimates of Marriage Premium, Matched Sample from Natality Data, 1980-1988

	Dependent Variable:								
	Birthweight (grams)			Gestation (Weeks)			Low Birth Weight		
Sample:	OLS(1)	OLS(2)	FE	OLS	OLS(2)	FE	OLS	OLS(2)	FE
All	248.2	128.7	98.5	0.571	0.318	0.23	-0.065	-0.041	-0.027
	[1.510]	[1.818]	[3.118]	[0.008]	[0.010]	[0.018]	[0.001]	[0.001]	[0.002]
N	1,298,578	1,146,932	1,146,932	1,228,550	1,086,812	1,086,812	1,300,053	1,148,194	1,148,194
Age 19-24	203.137	108.799	95.676	0.669	0.271	0.238	-0.055	-0.032	-0.025
	[1.993]	[2.413]	[6.088]	[0.011]	[0.014]	[0.038]	[0.001]	[0.001]	[0.003]
N	479,436	422,092	422,092	447,320	394,505	394,505	479,990	422,560	422,560
No HS Degree	174.216	110.239	90.021	0.569	0.277	0.225	-0.051	-0.035	-0.026
	[2.534]	[2.819]	[6.894]	[0.014]	[0.016]	[0.043]	[0.001]	[0.001]	[0.004]
N	267,605	267,605	267,605	248,675	248,675	248,675	267,962	267,962	267,962
Black	148.721	128.413	83.624	0.394	0.44	0.318	-0.048	-0.049	-0.029
	[2.985]	[3.475]	[6.327]	[0.017]	[0.020]	[0.039]	[0.002]	[0.002]	[0.003]
N	195,703	174,726	174,726	183,826	164,301	164,301	196,015	175,000	175,000

Notes: Data are from Natality Detail Files, 1980-1988. See text for details of the matching process used to create the matched sample. 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 brackets. All coefficients are statistically significant at the 1% level.

Table 5: First Differences Estimates of Marriage Premium, Matched Sample from Natality Data, 1980-1988

	Sample			
	All	Age 19-24	Less Than HS	Black Mothers
Panel A: Birthweight (in Grams)				
Switch In	108.590**	92.595**	85.412**	94.259**
	[4.578]	[6.245]	[7.827]	[7.763]
Switch Out	-98.284**	-100.390**	-85.795**	-90.804**
	[6.032]	[8.636]	[9.271]	[11.193]
P-value for Test of Equality of Coefficients	0.244	0.526	0.978	0.816
Observations	587,796	178,190	112,557	92,694
Panel B: Gestation (in Weeks)				
Switch In	0.204**	0.200**	0.189**	0.262**
	[0.028]	[0.040]	[0.047]	[0.048]
Switch Out	-0.321**	-0.371**	-0.284**	-0.492**
	[0.036]	[0.054]	[0.050]	[0.068]
P-value for Test of Equality of Coefficients	0.0328	0.0312	0.287	0.0132
Observations	529,823	157,008	98,049	81,905
Panel C: Low Birth Weight				
Switch In	-0.032**	-0.028**	-0.027**	-0.034**
	[0.002]	[0.003]	[0.004]	[0.004]
Switch Out	0.025**	0.027**	0.023**	0.028**
	[0.003]	[0.005]	[0.005]	[0.006]
P-value for Test of Equality of Coefficients	0.149	0.944	0.588	0.518
Observations	589,052	178,600	112,844	92,965

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). The base case is women who did not change marital status between 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. All coefficients are statistically significant at the 1% level.

Table 6: OLS and Fixed Effects Estimates of Marriage Premium, NLSY and NSFG, 1980-1988

	NLSY			NSFG		
	OLS(1)	OLS(2)	FE	OLS(1)	OLS(2)	FE
Married	213.76**	112.11**	37.93	207.60**	106.64**	54.98
	(16.82)	(18.63)	(35.18)	(31.55)	(32.26)	(173.29)
Observations		10,222			3,291	

Notes: Data are from the National Longitudinal Survey of Youth, 1979, and from the National Surveys of Family Growth (1995, 2002, 2006-2010). Sample is 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. The dependent variable is birth weight in grams. Standard errors are clustered by mother and are in parenthesis. ** p<.05.

Table 7: First Differences Estimates of Marriage Premium, NLSY and NSFG, 1980-1988

	NLSY	NSFG
Switch In	26.29	39.72
	(33.13)	(49.37)
Switch Out	-65.96	-18.62
	(56.41)	(72.52)
P-value for Test of Equality of Coefficients	0.150	0.499
Observations	5,341	3,262

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). The base case is women who did not change marital status between 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.