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First-birth Timing, Marital History, and Women's Health at Midlife

Kristi Williams[1], **Sharon Sassler**[2], **Fenaba Addo**[3], and **Adrianne Frech**[4]

[1]The Ohio State University, Columbus, OH, USA

[2]Cornell University, Ithaca, NY, USA

[3]University of Wisconsin-Madison, Madison, WI, USA

[4]University of Akron, Akron, OH, USA

Abstract

Despite evidence that first-birth timing influences women's health, the role of marital status in shaping this association has received scant attention. Using multivariate propensity score matching, we analyze data from the National Longitudinal Survey of Youth 1979 to estimate the effect of having a first birth in adolescence (prior to age 20), young adulthood (ages 20–24), or later ages (ages 25–35) on women's midlife self-assessed health. Findings suggest that adolescent childbearing is associated with worse midlife health compared to later births for black women but not for white women. Yet, we find no evidence of health advantages of delaying first births from adolescence to young adulthood for either group. Births in young adulthood are linked to worse health than later births among both black and white women. Our results also indicate that marriage following a nonmarital adolescent or young adult first birth is associated with modestly worse self-assessed health compared to remaining unmarried.

Keywords

adolescent childbearing; birth timing; marriage; nonmarital fertility; women's health

The timing of first birth is a central event in the life course that has been linked to women's health through both biological and biosocial processes (Mirowsky 2005). Early childbearing can curtail educational and occupational attainment, resulting in stress and disadvantage that take a cumulative toll on health throughout the life course. Yet, because adolescent childbearing has long been viewed as a social problem, most research on birth timing and women's health has been limited to identifying negative consequences of teen childbearing and has ignored the importance of childbearing during the early adult years (Bonell 2004; Furstenberg 2007; Lawlor and Shaw 2002). As rates of college attendance have grown, especially among women, young adulthood has become an increasingly important life course stage for the acquisition of human capital necessary for later socioeconomic attainment—a fundamental determinant of health throughout the life course (Link and

Corresponding Author: Kristi Williams, Department of Sociology, The Ohio State University, 238 Townshend Hall, Columbus, OH 43210, USA. williams.2339@osu.edu.

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Phelan 1995). Thus, childbearing during the early adult years may also have negative consequences for women's health.

Understanding the impact of birth timing on women's health requires attention to the important role of marriage, both at the time of birth and after. Attempts to determine whether births during adolescence or young adulthood causally impact women's health requires separating the effect of birth timing from the effect of having a nonmarital birth, as the latter is negatively associated with women's health decades later (Williams et al. 2011). Moreover, beginning with the welfare reorganization that created Aid to Families with Dependent Children (AFDC) in 1962 and culminating in the 1996 welfare reform legislation that created Temporary Assistance for Needy Families (TANF), encouraging marriage among young mothers has been a central focus of U.S. welfare policy (Geva 2011; Heath 2012). This pro-marriage policy orientation has flourished despite the absence of empirical evidence that it can improve the long-term well-being of young mothers or their children. Given high rates of instability and conflict in the unions that single mothers form (Lichter, Graefe, and Brown 2003; Timmer and Orbuch 2001; Williams, Sassler, and Nicholson 2008) subsequent marriage may even pose long-term risks to the health of young mothers, but no prior research has directly tested this hypothesis.

Our analysis of 29 years of panel data on a nationally representative sample of youth born between 1957 and 1965 (National Longitudinal Survey of Youth 1979 [NLSY79]) first examines the long-term consequences of childbearing in adolescence and early adulthood for midlife self-assessed health. We focus on a cohort of women who came of age in the late 1970s—a period of unprecedented growth in women's educational and occupational opportunities. These demographic processes continue to strongly shape the life course trajectories of today's women. Next, we differentiate women who gave birth during adolescence or young adulthood by their marital status at birth and later marital history to estimate the effect of marriage on the midlife self-assessed health of young mothers, using multivariate propensity score matching (PSM) to partially address selection bias. Given substantial racial-ethnic differences in the timing and context of childbearing, where possible, we conducted separate analyses for non-Hispanic non-black (hereafter described as white), black, and Hispanic women.

BACKGROUND

Early Childbearing and Women's Health

The timing of first birth is a central event in the adult life course with long-term consequences for women's health. A strictly biodevelopmental perspective suggests that childbearing early in the life course, when the organism is young and biologically resilient, should produce better health outcomes (Gosden and Rutherford 1995) than later childbearing. In contrast, a sociological perspective suggests negative health consequences of early, especially adolescent, childbearing. For example, adolescent births have been linked to lower levels of educational and socioeconomic attainment, higher rates of subsequent marital and family instability, and increased stress throughout the life course (Ermisch and Francesconi 2001; Hoffman 2008), all of which can take a cumulative toll on health and well-being.

Adolescent childbearing, long viewed as a social problem and a threat to public health (Bonell 2004; Furstenberg 2007; Lawlor and Shaw 2002), has been the central focus of research on birth timing and women's health. Such research has generally taken a relatively short view, for example, identifying higher risks of pregnancy complications, low birth weight, and infant mortality among adolescent compared to older mothers (Chen et al. 2007). Yet, the purported biosocial processes through which adolescent childbearing undermines women's health—interrupted or foregone educational, occupational, and socioeconomic attainment processes that contribute to stress over the life course—are cumulative processes, with consequences that may take decades to fully emerge (Ben-Shlomo and Kuh 2002). Two studies of older adults that take a longer view suggest that adolescent childbearing is, in fact, associated with increased mortality risk (Henretta 2007) as well as deficits in self-rated health and more objective health indicators later in life (Taylor 2009).

This near-exclusive focus on the consequences of adolescent childbearing for women's health and well-being obscures the importance of childbearing that occurs in the young adult years to women's health. Since the late 1960s, women's rates of college attendance and completion have outpaced men's, and the gender gap has steadily widened (DiPrete and Buchmann 2013). Women's labor force participation experienced unprecedented growth in the 1970s, exceeding 50% for the first time in 1980 (Fullerton 1999). Thus, beginning with the late baby boomer cohort (for whom births in the early 20s were normative) and continuing through the present, young adulthood has become an increasingly important stage in women's lives for the acquisition of educational and employment experiences that shape human capital and, consequently, health throughout the life course (Mirowsky and Ross 1998). Given that young adults are more likely to live independently than adolescents, childbearing during this period may represent an even greater barrier to educational attainment and investments in employment than adolescent childbearing, with enduring negative consequences for women's health and well-being.

The only two U.S. studies to date to examine this question support a hypothesis of negative health consequences of early adult childbearing, even among women for whom births at this age were normative.[1] In two separate studies, Mirowsky (2002, 2005) finds a linear health and mortality advantage of later ages of birth between the ages of 18 to 20 and 30 years or older, after which increases in age at birth are linked to worse health and greater mortality. These findings applied to both baby boomer and older cohorts of women, suggesting it is not simply an artifact of a single cohort. However, one sample (Mirowsky 2002) excluded teen births, and the other did not explicitly model the health consequences of teen compared to young adult first births, an important consideration given that successful policy efforts to reduce teen pregnancy may shift many births to the early adult years, with unknown consequences for women's health.

[1]Mirowsky's (2002) analysis of women born between 1900 and 1977 reported a mean age at first birth of 23. In his analysis of women born between 1891 and 1961, mean age at first birth was 22 (Mirowsky 2005). In comparison, mean age at first birth in our analytic sample was 24.

Another limitation of past research on birth timing and women's health is that it has focused nearly exclusively on white married populations (Taylor 2009) or combined race-ethnicity groups into a single analysis (Henretta 2007; Mirowsky 2002, 2005). This is an important consideration in the United States, where rates of early and nonmarital childbearing are higher for black, compared to white, women (Martin et al. 2015). Moreover, some evidence casts doubt on the dominant cultural belief that early childbearing has widespread individual and social costs for black women (Geronimus 2003). Rather, early fertility may be an adaptive strategy for low-income urban African American women vulnerable to "weathering"—accelerated declines in health that pose challenges to bearing and raising children at older ages (Geronimus 1996).

Racial-ethnic differences in fertility and family context may also shape the relative advantage or risks of adolescent and early adult versus later parenthood in different ways. For example, family support moderates some negative consequences of early parenthood (Mollborn 2010), but the availability of family support in adolescence versus young adulthood may vary by race-ethnicity. Compared to their young adult counterparts who are more likely to live independently, black teen mothers may have greater access to family support, perhaps suggesting some advantages of adolescent versus young adult childbearing for this group. Family support, although also important to white young mothers, may be less stratified by age at birth because white families' greater socioeconomic resources can be used to assist both teen and young adult mothers. Further, Hispanic women appear to be particularly resilient to negative socioeconomic consequences of teen childbearing (Lee 2010) and to the health consequences of nonmarital childbearing (Williams et al. 2011). Although our aim is not to test hypotheses about the direction or magnitude of racial-ethnic differences in the effect of age at first birth on health, the social contexts against which any causal effects play out are likely so fundamentally varied as to warrant separate analyses by race-ethnicity.

Finally, efforts to understand whether early childbearing exerts a causal effect on women's health are complicated by what appear to be very sizable selection processes into early birth, which have received little attention in prior research on health outcomes associated with birth timing. This question has substantial relevance for public policy, as efforts aimed at reducing early childbearing are premised on the view that it is an important cause of a range of negative individual and societal outcomes. However, women who begin childbearing in their adolescent years differ substantially on a range of background characteristics that are themselves strongly associated with health. As such, negative associations of adolescent childbearing with health later in life may reflect the health risk factors that predispose women to adolescent or early childbearing in the first place, rather than a causal effect of fertility timing itself, and these selection processes may differ by race-ethnicity.

A growing body of research using instrumental variables, PSM, and sibling models to minimize selection bias suggests that teen childbearing has very little causal effect on educational and socioeconomic attainment (Geronimus and Korenman 1993; Hotz, McElroy, and Sanders 2005; Levine and Painter 2003; Ribar 1994) and psychological distress (Mollborn and Morningstar 2009). Kearney and Levine (2012:142) conclude, "Our reading of the totality of evidence leads us to conclude that... teen childbearing is explained by the low economic trajectory [that precedes teen childbearing] but is not an additional cause of later difficulties in life." Whether selection bias plays a similar role in the association of adolescent or young adult childbearing with self-assessed health is unclear.

The NLSY data allow us to control for a range of background characteristics predictive of entry into early childbearing, including socioeconomic and family background. We employ multivariate PSM to determine whether significant observed associations of early childbearing with midlife health persist when women who had an early first birth are matched with those who have a similar estimated propensity of having an early first birth, based on observed background characteristics. Prior research indicates that women who have adolescent (and likely young adult births) differ from those who have later births on multiple background characteristics (see Kearney and Levine 2012) that are also clearly linked to health. In this context, nonparametric matching approaches that do not assume a linear relationship of the covariates with the dependent variable have advantages over regression-based methods in minimizing bias due to selection on observed characteristics (DiPrete and Gangl 2004).

Early Childbearing, Marriage, and Women's Health

Understanding the consequences of adolescent and early adult fertility with later life health requires unraveling the separate effects of age and marital status at birth. Prior studies have been limited in this respect, either by controlling only for number of marriages (Mirowsky 2002) or by using a sample composed almost entirely of marital first births (Taylor 2009). Early first births are more likely than later births to occur outside of a marital union, and nonmarital fertility has been linked to poor health outcomes among U.S. women (Henretta 2007; Williams et al. 2002, 2011), likely through many of the same mechanisms (disadvantage, instability, and chronic stress) theorized to be relevant in linking early childbearing to women's health (Mirowsky 2005). Our analyses consider whether any negative health consequences of early childbearing are confounded by the fact that such births are disproportionately nonmarital—a question that can inform both family and public health policy designed to improve women's health.

Also relevant to both policy and theory is an understanding of the health consequences of marriage following an early nonmarital birth. Encouraging marriage among single parents has been a key focus of U.S. welfare policy since the creation of AFDC in 1962. Such efforts picked up momentum following the 1996 welfare reform legislation that created TANF and authorized the use of welfare funds to promote so-called healthy marriages among low-income parents (Geva 2011; Heath 2012). Yet, there are several reasons to expect that marriage may not be beneficial for young single mothers. On average, single mothers' relationships are characterized by relatively low levels of marital quality and high levels of conflict (Williams et al. 2008) and instability: In one nationally representative study, approximately 64% of single mothers who later married were divorced by ages 35 to 44 (Graefe and Lichter 2007). Others have found that any health benefits of later marriage following nonmarital fertility are limited to white women who enter an enduring marriage with the biological father of their child (Williams et al. 2011). However, because this study excluded adolescent births, it is unclear whether later marriage offers greater benefits or perhaps introduces health risks for women who have had an early nonmarital birth compared to remaining unmarried. In the second part of this study, we consider how the midlife health of women who later marry following a non-marital birth that occurred in adolescence or young adulthood compares to that of their counterparts who never marry.

Self-assessed Health in Midlife

Estimating consequences of fertility timing and union status for women's health requires taking a long view of how these processes play out over the adult life course. Both life course epidemiology and social stress research suggest that many chronic illnesses have long latency periods (Lynch and Smith 2005) and the physiological and psychological toll of chronic stressors accumulates over time (Pearlin et al. 2005). As a result, health consequences of life course stressors may take decades to emerge (Ben-Shlomo and Kuh 2002; Hayward and Gorman 2004; Palloni et al. 2009). Moreover, from a public health perspective, identifying life course trajectories that have enduring consequences for health is particularly valuable. We use data from a cohort of women born between 1957 and 1965 who have recently entered midlife, a time when chronic health problems begin to emerge.

We focus on a global measure of self-assessed health, which has several advantages in population-based research. It is associated with morbidity and mortality over and above objective diagnoses of existing health conditions (Idler, Russell, and Davis 2000), yet is not subject to the bias associated with lack of access to health care that may affect studies drawing on physician-diagnosed conditions. Selfrated health also allows an individual to subjectively report his or her daily functioning while taking into account conditions that are unobservable or difficult to measure (e.g., energy, pain, or latent or undiagnosed conditions). For middle-aged and older adults, self-rated health is independently predictive of many of the chronic conditions that contribute most to midlife health disparities, including arthritis, coronary heart disease, lung disease, and stroke (Latham and Peek 2012). However, self-rated health is not a clinical indicator, and some qualitative research suggests modest differences in how some subgroups report their daily functioning through this measure (see Krause and Jay 1994).

DATA AND METHODS

Data

The NLSY79 includes a nationally representative sample of 9,763 young men and women (4,926 of whom are women) ages 14 to 22 in 1979. Respondents were interviewed annually through 1994 and continue to be interviewed biennially since 1994.[2] We used data through 2008, when all NLSY79 mothers had reached age 40, the age at which our dependent variable was first measured.

We first limited our analytic sample to the 4,021 women who gave birth prior to age 40 and excluded 18 women whose marital status at birth could not be determined and 113 women whose first birth occurred while divorced. Of the remaining 3,890 women, 3,479 (89.4%)

[2]The National Longitudinal Survey of Youth 1979 (NLSY79) originally included supplementary oversamples of military and economically disadvantaged white respondents (n = 2,923), but these were dropped prior to 1991.

completed the age 40 health assessment, and we excluded the 411 women missing data on self-assessed health. We further excluded 23 women who had births prior to age 15 because several of these reports were inconsistent and because many of our control variables were measured at age 14. We excluded 20 women who had births after age 35 to allow a minimum 5-year lag between the time of birth and the age 40 measurement of health, important for the analysis that examines the consequences of union transitions after birth. Thus, our final analytic sample comprised 3,348 women who had a first birth between the ages of 15 and 35 while married or never married and who were not missing data on age 40 selfassessed health. We used multiple imputation (mi impute chained in Stata with five implicates) to impute missing data on seven covariates and list the number of missing cases imputed for each in the measures section below.

Weighted statistics on key variables were consistent with population data. The mean age at first birth in our analytic sample was 23.7, identical to the population mean of 23.7 in 1985, the median year of first birth in our data (Mathews and Hamilton 2002). Approximately 22% of first births in our analytic sample were to never-married women. Population data are not available on births to never-married women, but 27.7% of first births in 1985 were to never-married or divorced women (National Center for Health Statistics 1988).

In some analyses, we estimated separate models for each of the three categories of race-ethnicity: (1) non-Hispanic, non-black (n = 1,633), (2) black (n = 1,029) and (3) Hispanic (n = 686). Although we follow the convention of describing the non-His-panic non-black subgroup as “white,” a small number may have a different ethnic identification.[3]

Measures

Self-assessed Health.—Self-assessed health was measured at age 40 with a single question: “In general, would you say your health is excellent, very good, good, fair, or poor?” Responses were coded 1 to 5 with higher values indicating better health.

Age at First Birth.—Dummy variables distinguished women whose first birth occurred (1) during adolescence (ages 15–19), (b) in early adulthood (ages 20–24) and (c) ages 25–35 (reference). The adolescent age range corresponded to that typically examined in research on adolescent fertility. The age 20-to-24 category represented approximately one third of all births and the age 24 cut point represents the median age at first birth in our analytic sample (23.7). Results were robust to minor variations in cut points for the early adulthood category.

Marital History.—Dummy variables differentiated women by their marital status at the birth of the first child and their subsequent marital history: (1) never married at first birth and remained never married through age 40, (2) never married at birth and ever married by age 40, and (c) married at first birth (reference category).

[3]In our analytic sample, 78% of those in the non-Hispanic non-black (“white”) category listed a European, “American,” or no ethnic identification, and an additional 10% chose *other* from a list of 28 ethnic categories. Approximately 1% listed one of seven ethnic categories commonly labeled “Asian or Pacific Islander.” Although an additional 9% are coded as “Native-American” or “American-Indian,” the NLSY cautions that comparisons with Census data suggest this percentage is inflated by approximately a factor of 9, likely due to misunderstanding of the meaning of the term *Native American* to mean “native-born American.”

Covariates.—Covariates were measured at or prior to age at first birth and include dichotomous indicators of (1) health problems that would limit ability to work that began prior to first birth, (2) residence with both biological parents at age 14, (3) urban residence at age 14, (4) residence in the U.S. South at age 14 (imputed $n = 15$), (5) contraceptive use prior to first pregnancy (imputed $n = 54$), and (6) dummy variables indicating religious affiliation in childhood (Baptist [reference], Catholic, liberal Protestant, other religion, no religion; imputed $n = 7$). Models also control for (7) whether the respondent's mother had an adolescent first birth (1 = yes) and for the following variables as proxies for the socioeconomic status of the respondent's family of origin: (8) years of education of the respondent's mother (imputed $n = 200$), (9) whether reading material (books, magazines, etc.) was available in the respondent's childhood home (imputed $n = 15$), (10) whether the respondent lived in a household with an employed adult female (including a mother or mother figure) at age 14 (imputed $n = 23$), and (11) whether the respondent lived in a household with an employed adult male (including a father or father figure) at age 14 (imputed $n = 42$). Dichotomous variables are coded as '1' for yes and '0' for no. These covariates were used to predict propensity scores in both sets of PSM analyses.

We did not adjust models for mechanisms measured after the age at first birth that may link birth timing and health or use such variables in estimating the propensity to have an early birth or to later marry. For example, although the number of biological children a woman has and her marital status at age 40 influence health, they are consequences rather than causes of fertility timing and union history. Controlling for the downstream consequences of nonmarital or early childbearing may underestimate the gross effect of early/single motherhood or subsequent union history on midlife health. However, including these variables in supplementary analyses did not change the overall pattern of findings.

Analysis

Our analyses addressed two central questions. First, was adolescent or young adult (vs. older) age at first birth associated with women's midlife health, and to what extent is this explained by the association of adolescent/early parenthood with nonmarital childbearing? Second, was the midlife health of women who had an adolescent or early first birth, affected by her marital status at her child's birth and her subsequent marital history? We used ordinary least squares (OLS) regression and multivariate PSM to address these questions.[4]

PSM has several advantages over OLS regression. Parametric approaches, including OLS, estimate an average treatment effect, which is unbiased only if treatment is randomly assigned. If treatment is nonrandom, as is empirically established in the case of childbearing and subsequent union formation, conditioning linearly on the covariates as in OLS cannot sufficiently eliminate selection bias. Matching estimators, such as PSM, minimize bias resulting from misspecification of the functional form of a linear model by constructing distributions of covariates between the treatment and control groups to be as similar as

[4]Supplementary models using ordered probit regression, ordered logit regression, and logistic regression on a dichotomized version of the dependent variable (1 = poor or fair health) were nearly identical in relative magnitude and significance to the ordinary least squares results, with one minor exception: In the logistic regression model predicting fair or poor self-assessed health, the estimated effect of a young adult first birth on the midlife health of white women was significant at only the $p \leq .09$ level, likely due in part to reduced statistical power. Results of the all supplementary models are available upon request.

possible and matching on the probability of treatment, the propensity score. Rosenbaum and Rubin (1983) showed that any differences between treatment and control groups with similar propensity scores balance during estimation, eliminating any potential bias from these variables. Matching methods further ensures common support on observables, particularly important when large differences exist between the treatment and control groups.

We matched women in our sample based on the propensity score of the predicted probability of early first birth or particular union history as a non-parametric function of characteristics observed prior to the first birth or measurement of union status (Dehejia and Wahba 2002). We used one-to-one nearest-neighbor matching with replacement (Morgan and Harding 2006; Rosenbaum and Rubin 1983) in our first set of PSM analyses and Mahalanobis matching in our second set of PSM analyses. Nearest-neighbor matching is preferred when there is substantial overlap in propensity scores between treatment and control groups (Black and Smith 2004; Dehejia and Wahba 2002). Mahalanobis matching outperformed nearest-neighbor matching in achieving covariate balance in our second PSM analysis.

Matched observations from the treatment and control groups were used to estimate differences in health at age 40, and the average difference was computed across all matches. We used the psmatch2 suite in Stata 13 with Abadie and Imbens standard errors (Leuven and Sianesi 2014) and ensured that the range of propensity scores shares “common support” or “overlap” across the treatment and control groups. Those with a very low propensity to occupy either the treatment or the control were trimmed from the analysis. We used the pstest suite to ensure that all covariates were adequately balanced across treatment and control groups. Across all models, *t* tests indicated that the mean value of each individual covariate was not significantly different across treatment and control groups, and total median bias in each model did not exceed 7%. Individual covariate bias was less than 11% across all models.

We also conducted Rosenbaum bounds sensitivity analysis to estimate the influence of bias from unobserved variables that could confound the relationship between the treatment(s) and outcome of interest (Rosenbaum 2002). The Rosenbaum bounds method (implemented using rbounds in Stata) is a conservative test that estimates how large an influential unobserved confounder would need to be to render the estimated treatment effect nonsignificant (Rosenbaum 2002). The statistic gamma estimates the critical levels for which the hypothetical unobserved variable would cause the odds ratio of treatment assignment to differ between treatment and control groups. Higher gamma values are associated with a reduction in sensitivity to hidden bias (Rosenbaum 2002).

Descriptives

Table 1 shows means or percentages for all variables by race-ethnicity and age at first birth. Consistent with prior research, both union status at birth and age 40 health, as well as several demographic background characteristics, differ substantially by both age at first birth and race-ethnicity. These patterns underscore both the value of race-ethnicity-stratified models and of PSM in modeling differential selection into birth timing.

RESULTS

Is Age at First Birth Associated with Women's Midlife Health?

Table 2 presents OLS regression models comparing the health at age 40 of women who had an adolescent or young adult first birth to that of their counterparts who had a first birth between the ages of 25 and 35, separately by race-ethnicity. For each group, the first model shows the estimated effect of age at first birth conditioning on background characteristics. In the second model, we controlled for marital status at birth to consider whether observed associations of age at first birth with midlife health are partly explained by the fact that births to younger mothers are more likely to be nonmarital.

Prior to entering controls, the first model for each racial-ethnic group (Models 1, 3, and 5) indicated that first births in adolescence (ages 15–19) and young adulthood (ages 20–24) are associated with poorer midlife health than later first births for black and white women but not for Hispanic women. Additional tests (not shown) find no significant differences in self-reported health between women who experienced their birth in adolescence versus young adulthood.

The second set of models (Models 2, 4) strongly supports the hypothesis that the observed association of adolescent childbearing among white women is partly explained by the fact that such births are disproportionately nonmarital. Net of controls, the estimated effect of adolescent childbearing among white women is reduced by approximately 47% to nonsignificance (Model 4) after adjusting for marital status at birth. Marital status at birth explains less of the estimated effect of adolescent childbearing on the midlife health of black women (23.5%, Model 2) and of the estimated effect of a first birth in young adulthood on the health of white (12.5%) and black (14.83%) women; each of these coefficients remain statistically significant. Also, consistent with prior research on adult births (Williams et al. 2011), non-marital childbearing is associated with worse health at midlife for black and white but not Hispanic women.

It is important to note that our study does not hypothesize that the average estimated effect of age at first birth on self-assessed health differs significantly by race-ethnicity, and the results in Table 2 do not allow such a conclusion.[5] Rather, the stratified models in Table 2 show that tests of our core hypothesis of a (null) effect of age at first birth on age 40 self-assessed health lead to different conclusions for blacks, whites, and Hispanics when the varying influence of each group's background characteristics on health are appropriately modeled.

We next employ multivariate PSM to determine whether the associations presented in Table 2 are robust to an approach that better accounts for the differential selection of women into adolescent or young adult first births and employs a more appropriate counterfactual comparison. This analysis estimates the likelihood of experiencing an adolescent or early first birth for the total sample of women who became mothers, by conditioning on

[5]We do not present a pooled analysis with race-ethnicity interactions. The stratified models we present in Table 2 indicate substantial racial-ethnic heterogeneity in the residual variances, which biases tests of interaction effects in pooled models.

pretreatment observable characteristics (Rosenbaum and Rubin 1983). Because our matching procedure excludes unmatched observations with propensity scores that fall beyond the region of common support (those that violate the overlap assumption), sample sizes in some PSM models do not exactly match that of the corresponding OLS models. All control variables used in the OLS models were used to predict the propensity score, but interaction terms and functional forms vary as a result of an iterative model-building procedure that maximized covariate balance in each PSM model (results available upon request).

Table 3 presents the average treatment effects for the treated, estimated from the PSM models, separately by race-ethnicity. In Panel A, the treatment refers to experiencing an adolescent first birth (ages 15–19) compared to having the first birth at ages 25–35 (control). The PSM results are consistent with the OLS results and indicate that among those with similar predicted propensities to have an adolescent first birth, black women (but not Hispanic or white women) who have adolescent births report worse midlife health than those whose first births occur between ages 25 and 35. In Panel B, the treatment refers to experiencing a first birth between the ages of 20 to 24 compared to ages 25 to 35. Results from the PSM models suggest that, as in the OLS models, first births occurring between the ages of 20 and 24 (compared to ages 25 to 35) are negatively associated with midlife health for black and white women.

Results of Rosenbaum bounds sensitivity analyses indicated that the estimated effects of a young adult birth on the self-assessed health of black women are reasonably robust to the presence of hidden bias. The gamma level of 1.5 indicates that in order to render the estimated treatment effect nonsignificant, an unobserved confounder would have to cause the treatment assignment to differ between treatment and control cases by a factor of 1.5 in addition to very strongly predicting health at age 40. Gamma levels for the other two estimated treatment effects are smaller, suggesting that the treatment effects may be somewhat more vulnerable to unobserved variable bias.

In sum, our results suggest that young adult first births are associated with worse midlife health than later first births for black and white women, but adolescent births are linked to worse health only among black women. In Panel C, we examine whether young adult compared to adolescent first births are associated with better (or worse) midlife self-assessed health. The PSM models indicate no significant differences. Taken together, these results suggest that delaying a first birth from adolescence to early adulthood has no measurable positive or negative consequences for the midlife health of white, black, or Hispanic women.

Does Marital Status at Birth or Later Influence the Health of Women Who Had an Early First Birth?

We next address our second central research question using OLS regression and PSM to examine how marriage at birth or later shapes the health of women who had an adolescent or young adult first birth (prior to the age of 25). Substantively, this analysis addresses whether, on average, the midlife health of young mothers is better if they were married compared to unmarried at birth and whether young unmarried mothers have better midlife health if they later marry compared to remaining unmarried. Although there are theoretical reasons for expecting racial-ethnic differences across the range of racial-ethnic categories we examined in the first set of analyses, separate models for whites and Hispanics are underpowered due to small cell sizes in union history categories. We therefore present models only for the total sample and for black women, the group with the largest number of first births in the adolescent and young adult age categories considered here.

In the first model for the total sample (Model 1) and for black women (Model 4), the *unmarried* coefficient shows the estimated difference in age 40 self-assessed health of women who had an adolescent or young adult first birth while unmarried compared to women had an early first birth while married. The results indicate that among women who become mothers prior to age 25, nonmarital childbearing is linked to poorer midlife health in the total sample but not for the subsample of black women. However, the coefficients do not differ much in magnitude, and a larger subsample of black women may reveal a statistically significant association. Our subsequent propensity score analysis will further clarify this result.

In Models 2 and 3 for the total sample and Models 5 and 6 for black women, we estimate the consequences of later marriage for the midlife health of young unmarried mothers by disaggregating women who were never married at their first birth into two groups: those who later married and those who remained never married. Models 2 and 5 compare each group to young mothers who had marital first births, and Models 3 and 6 vary the reference category to compare young unmarried mothers who later married (reference) to young unmarried mothers who remained never married.

The results suggest that later marriage may pose modest risks to the midlife health of young unmarried mothers, including black women. In both the total sample (Model 2: −.37**) and the black subsample (Model 5: −.27**), young unmarried mothers who later marry are estimated to have substantially worse midlife health than young mothers who were married at first birth. Yet, this is not the case for young unmarried black mothers who remain unmarried; the estimated midlife health of black continually never-married mothers is very similar to that of young mothers who were married at birth (Model 5: −.04, *ns*). Although continually never-married mothers in the total sample have significantly worse midlife health than their counterparts who were married at birth, the magnitude of this difference is much smaller (−.15**) than the difference between unmarried mothers who later married compared to those married at birth (−.37**). This predicted self-assessed health detriment is most evident in Models 3 and 6, which indicate that young unmarried mothers who never marry have significantly better midlife self-assessed health than young unmarried mothers who later marry in the total sample (.24**) and the black subsample (.24*). In sum, the OLS analyses reveal that in both the total sample and among black women specifically, marriage following a young nonmarital first birth is associated with worse midlife health than remaining continually unmarried.

In the final set of analyses, we employ multivariate propensity score models to determine whether the significant associations of marital status at birth and marital history with the midlife health of young mothers shown in Table 4 are robust to a consideration of differential selection into marriage at birth or later. We first estimate the likelihood of occupying each of the marital history categories, employing three contrasts: (1) unmarried at birth but later married compared to married at birth, (2) unmarried at birth and never married by age 40 compared to married at birth, and (3) unmarried at birth and never married compared to unmarried at birth and later married.

The results from the PSM models in Table 5 generally strengthen the conclusions of the OLS models shown in Table 4. Turning first to the results for the total sample, the PSM models suggest that, regardless of later marital history, women who had an early nonmarital first birth report worse midlife health than their counterparts who were married at birth, although the health disadvantage appears to be smaller for never-married mothers who never marry (Panel B: −.22**) compared to those who later marry (Panel A: −.45**). As shown in Panel C, this difference is statistically significant. Among women in the total sample who have a first birth before age 25, those who never marry report significantly better age 40 health than those who later marry (.30**).

The results for black women are even more striking. Among black women who have an early nonmarital first birth, only those who later marry (Panel A: −.36**) and not those who never marry (Panel B: −.04) report worse midlife health than their counterparts who were married at first birth. Consistent with the results in the total sample and in the OLS models, Panel C indicates that black women who have a nonmarital first birth before age 25 and never marry report significantly better self-assessed health at midlife than their counterparts who later marry. Notably, the Rosenbaum sensitivity analyses for the Panel C models indicate that the estimated treatment effect of never marrying versus marrying following a nonmarital first birth is robust (gamma = 1.4 and 1.5) to the presence of a moderately influential but unobserved pretreatment variable. Taken together, the PSM results suggest that having a nonmarital birth before age 25 is not linked to worse midlife selfassessed health than having a marital first birth at that life course stage unless the nonmarital first birth is followed by a marriage. Moreover, remaining unmarried may have modest benefits for the self-assessed health of women who have an early nonmarital first birth compared to later marriage.

DISCUSSION

For decades, research documenting the association of teen and nonmarital childbearing with a range of negative socioeconomic and well-being outcomes has supported a conclusion that both are important social problems in the United States (Furstenberg 2007). In fact, the preamble to the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (104th Congress 1996) explicitly stated that nonmarital and adolescent childbearing have created a “crisis in our Nation” that welfare reform, including its focus on promoting marriage, was designed specifically to address. Our central findings clarify the scope of the crisis posed by adolescent childbearing, suggest few health benefits of encouraging women to delay first births from adolescence to early adulthood, and challenge the promotion of marriage as solution, at least in terms of its long-term impact on the self-assessed health of this cohort of women.

To the extent that adolescent childbearing in the 1980s to 1990s was negatively associated with midlife health, this pattern appears limited to black women.[6] That we find no evidence that adolescent childbearing undermines the health of white and Hispanic women is consistent with a growing body of evidence that questions a causal effect of teen childbearing on a range of negative socioeconomic and well-being outcomes for women (Geronimus and Korenman 1993; Hotz et al. 2005; Mollborn and Morningstar 2009; Ribar 1994). In fact, we find that the negative association of adolescent childbearing with health among white women is partly due to the fact that these births are disproportionately nonmarital. This is especially concerning in light of current trends: While the adolescent birth rate has reached its nadir, nonmarital fertility is at an all-time high. Our results, along with prior research (Williams et al. 2011) suggest that improving women's health requires attention to the causes and consequences of nonmarital fertility in addition to those of adolescent and young adult births.

Ours is the first U.S. study to show that childbearing in young adulthood is associated with worse self-assessed health decades later for black and white (but not for Hispanic) women. This represents a substantial expansion of the scope of prior research on first-birth timing and women's wellbeing, which has focused primarily on adolescent childbearing. Perhaps most importantly, our findings suggest no long-term midlife self-assessed health advantages or disadvantages of delaying adolescent births to early adulthood. For black women, both adolescent and young adult births are associated with worse self-assessed health in midlife compared to later births. In contrast, for white women, it is only young adult and not adolescent births that appear to undermine midlife selfassessed health, although young adult first births are not linked to significantly worse health outcomes than adolescent births for any group. Notably, this association persists even after controlling for the fact that births in young adulthood are disproportionately nonmarital, suggesting that both nonmarital and young adult fertility independently undermine black and white women's self-assessed health. The importance of our findings is underscored by contemporary demographic trends: In the United States, approximately one third of all first births occur in the 20-to-24 age group, and the majority of these births are nonmarital (Martin et al. 2015).

There has been a sizable shift in the timing of first births among black women, as the proportion experiencing a first birth during the teen years has declined significantly (Wildsmith, Steward-Streng, and Manlove 2011). Nonetheless, 63% of all first births to black women occur to women who are age 24 or younger (Martin et al. 2015). It is especially important that future research and theory specify the mechanisms responsible for the negative health outcomes associated with black women's fertility at this life course stage. Although only suggestive, our pattern of findings indicate that social factors linked to disadvantage and stress, both of which are strongly linked to health over the life course, are likely more relevant than biosocial explanations that would predict health disadvantages of *delaying* fertility, especially for black women (Geronimus 1996; Goisis and Sigle-Rushton 2014). The social stress model (Pearlin et al. 2005), in contrast, draws attention to the importance of timing and social context in shaping stress proliferation—a process in which

[6]See note 5.

specific role transitions, such as the transition to motherhood, precipitate exposure to chronic stressors that, over the life course, can take a cumulative toll on health.

The social context in which the black women in our sample had their first births was arguably conducive to stress proliferation. About half had their babies between the start of the survey and the mid-1980s, a period noted for high rates of unemployment and crime, and low rates of health insurance coverage among the most vulnerable adult populations. In 1985, 15.1% of black women were unemployed, compared with 6.2% of white women. Younger black women experienced much higher rates of unemployment than their white counterparts throughout the 1980s and into the 1990s (DeSilver 2013; U.S. Bureau of Labor Statistics 2012) and were more likely to live in neighborhoods with high rates of crime, which reached record high levels between the 1970s and the 1990s. Furthermore, substantial proportions of young black women lacked health insurance. In the early 1980s, 25.1% of adults between the ages of 19 and 25 lacked health insurance coverage, and blacks were far more likely to be without health insurance coverage than whites (National Center for Health Statistics 2014:Table 125).

The resilience of white women to the negative health consequences of adolescent but not young adult childbearing may reflect in part access to socioeconomic resources and family support. Teen mothers are more likely than those who have first births in young adulthood to live with a parent or other adult (Sigle-Rushton and McLanahan 2002), and these multigenerational resources may minimize the negative impact of childbearing on white adolescent mothers' educational and occupational attainment (Gordon, Lindsay Chase-Lansdale, and Brooks-Gunn 2004). Differential access to family support may also explain Hispanic women's resilience to negative health consequences of early or adolescent fertility. Hispanic single mothers are more likely than those in other racial-ethnic groups to live in multigenerational households (Cohen 2002), which may provide instrumental, economic, and emotional support helpful in navigating the challenges of early childbearing.

Our second key finding challenges assumptions about the benefits of marriage for the health of young single mothers, with possible relevance to family policy aimed at increasing marriage rates for this group. When women with similar propensities to marry or remain unmarried are directly compared, those who never marry following an early nonmarital first birth have better self-rated health than those who later marry. The constrained marriage markets of young single mothers may partly underlie this pattern (Harknett and McLanahan 2004; Wilson 1987). Single mothers are more likely to marry men who are also unwed fathers (Graefe and Lichter 2007), have few economic resources (Graefe and Lichter 2008), lack a high school diploma (Lichter et al. 2003), or have been incarcerated or have substance abuse problems (Lopoo and Carlson 2008). Rather than being a source of emotional and instrumental support that is beneficial for health, subsequent marriage may introduce additional strains into the lives of young single mothers in ways that take a cumulative toll on their health.

It is important to note that although our propensity score analyses offer several advantages, they do not allow us to conclude that the significant associations we observe reflect solely a causal effect of birth timing or later marriage on women's midlife health. First, the number of predictors of the propensity to have an early first birth that we were able to include was limited by the fact that some of the first births occurred prior to the 1979 baseline interview. We found similar results in supplementary models limited to women whose first births occurred after 1979 and including more predictors, but poor covariate balance prohibits drawing strong conclusions. Second, it is unclear to what extent our inability to include a baseline measure of selfassessed health affects our results. As Mirowsky (2002) notes, there is no clear evidence in the literature that adolescent self-assessed health shapes fertility timing. Of course, extreme health problems or disability could cause women to delay or forego childbirth, and this is captured in the measure of health limitations prior to first birth that we include.

Finally, Rosenbaum sensitivity tests suggest that the estimated effect of teen childbearing on black women's midlife health and that of young adult childbearing on white women's health may be especially sensitive to the influence of an unobserved confounder. However, as DiPrete and Gangl (2004) point out, the Rosenbaum bounds test is an exceptionally conservative test of the "worst-case" scenario. It assumes a very strong effect of a hypothetical unobserved confounder on the outcome that, in this case, would almost completely determine the difference in self-assessed health between the treatment and control cases in each pair of matched cases in the data. Unobserved con-founders that have a strong effect on assignment but a weak effect on self-assessed health would not render the estimated treatment effect nonsignificant (DiPrete and Gangl 2004). Nevertheless, future studies using instrumental variables (if appropriate instruments can be identified) or individual fixed effects would be of value especially for identifying short-term health consequences of fertility timing in a more contemporary sample of women.

Finally, the midlife health consequences of early childbearing on which we focus cannot necessarily be generalized to more recent cohorts of U.S. women. Because health detriments associated with particular fertility patterns may accumulate slowly over time, we were interested in estimating longterm consequences for health in midlife, a time when chronic health problems begin to emerge. However, both the prevalence and context of adolescent and young adult first births are markedly different for more recent cohorts. We can only speculate about the likely consequences of early adult fertility among more recent cohorts, but several strands of evidence suggest that they may be even more negative than what we observe in the NLSY79. Indeed, it is notable that our results suggest negative health consequences of young adult first births in a cohort for whom such births were not uncommon: The mean age at first birth in our sample is age 24. By 2013, the mean age at first birth in the United States had risen to an all-time high of age 26. As rates of college attendance and completion have grown, particularly among women, young adulthood has become an increasingly important time in the acquisition of resources necessary for later socioeconomic attainment with likely consequences for health over the life course. This could suggest even more negative consequences of childbearing in the young adult years among more recent cohorts of U.S. women.

Moreover, the 1996 welfare reform legislation shifted support away from low-income single mothers toward married-parent families (Moffitt 2015). Because an increasing share of fertility in the age 20-to-24 age group is nonmarital, this decline in the social safety net may have also increased any negative health consequences of early fertility for more recent cohorts of young single mothers. On the other hand, the expansion of health care coverage through the Affordable Care Act may help to mitigate some negative health outcomes for this group. Speculation aside, it is clear that nonmarital fertility among young adult women has become a demographic reality in the United States. It is therefore essential that future research continue to track the health outcomes of this vulnerable group of women and identify factors that improve their health and well-being over the life course.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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AUTHOR BIOGRAPHIES

Kristi Williams is an associate professor in the Department of Sociology and faculty affiliate at the Institute for Population Research at The Ohio State University and a senior scholar at the Council on Contemporary Families. Her research focuses on demographic changes in marriage and parenthood and their consequences for health and well-being over the life course and across generations, with attention to inequality in these processes. Her recent work has been published in *American Sociological Review, Demography, Journal of Marriage and Family, Journal of Health and Social Behavior*, and *Social Forces*.

Sharon Sassler is a professor in the Department of Policy Analysis and Management at Cornell University, Development Core Director of the Cornell Population Center, and a field member in the Department of Sociology. Her research examines the factors shaping young adults' transitions into relationships, school and work, and parenthood, with an emphasis on how these behaviors vary by gender, social class, and race-ethnicity. Her recently published articles have appeared in *Family Relations, Demographic Research, Social Forces*, and *Social Science Research*.

Fenaba Addo is an assistant professor of Consumer Science at the University of Wisconsin—Madison. Her current research explores how household financial decisions influence the cohabitation and marriage decisions of young adults and how wealth inequality impacts racial health disparities. Her research has been published in *Demography, Journal of Marriage and Family*, and other journals.

Adrianne Frech is an assistant professor in the Department of Sociology at the University of Akron. Her research focuses on longitudinal relationships between family formation, workforce and schooling, and mental and physical aspects of health. Her previous research has appeared in *American Sociological Review, Demography, Journal of Health and Social Behavior, Journal of Marriage and Family*, and *Advances in Life Course Research*.

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Table 1.

Descriptive Statistics on Imputed Analytic Sample: U.S. Women Ages 14 to 22 in 1979 with a First Birth between Ages 15 and 36 (National Longitudinal Survey of Youth, 1979).

	Age at First Birth								
	Black Women			White Women			Hispanic Women		
	15–19	20–24	25–35	15–19	20–24	25–35	15–19	20–24	25–35
Never married at first birth	85.54%	62.84%	39.61%	31.68%	12.11%	5.15%	42.91%	26.34%	13.74%
Health limitations before first birth	1.18%	7.25%	3.38%	13.50%	7.62%	4.88%	8.81%	4.12%	2.75%
Self-assessed health (age 40)	3.27	3.38	3.67	3.52	3.65	3.90	3.38	3.39	3.64
R's mother's education (years)	9.98	1.96	11.11	1.59	11.53	12.42	7.16	7.18	8.62
R's mother had adolescent first birth	12.63%	1.27%	5.31%	8.26%	7.03%	2.37%	1.34%	7.41%	6.59%
R used contraception before birth	31.36%	46.22%	45.02%	36.09%	59.69%	45.83%	22.61%	37.37%	4.44%
Home environment at age 14									
Lived with both parents	41.14%	49.55%	59.42%	68.87%	74.80%	84.70%	57.09%	67.08%	74.73%
South	63.75%	61.03%	57.20%	39.45%	24.96%	26.04%	27.66%	22.72%	22.53%
Urban	79.84%	78.55%	8.19%	7.80%	7.54%	78.23%	9.42%	87.24%	86.26%
Reading material in home	74.87%	86.10%	87.83%	91.46%	95.70%	97.89%	76.55%	74.49%	82.64%
Adult female in home worked	56.13%	58.55%	62.13%	51.63%	54.77%	52.01%	42.07%	37.86%	59.01%
Adult male in home worked	52.10%	6.12%	67.15%	8.94%	84.80%	87.57%	66.13%	7.53%	76.92%
Religious affiliation in childhood									
Baptist (reference)	66.35%	68.28%	59.90%	37.19%	2.66%	13.72%	6.51%	2.47%	3.85%
Catholic	7.13%	7.85%	11.11%	22.92%	28.71%	39.97%	84.67%	9.53%	86.26%
Liberal Protestant	1.26%	1.27%	15.46%	24.35%	32.66%	31.13%	1.92%	1.23%	3.85%
Other religion	1.71%	1.27%	11.11%	9.37%	13.09%	1.16%	4.98%	3.70%	4.95%
No religion	5.54%	3.32%	2.42%	6.17%	4.88%	5.01%	1.92%	2.06%	1.10%
% of births within race-ethnic group	47.71%	32.17%	2.12%	22.23%	31.35%	46.42%	38.19%	35.42%	26.53%
n	491	331	207	363	512	758	261	243	182

Note: R = respondent.

Table 2.

Self-assessed Health at Age 40 Regressed on Age at First Birth and Covariates by Race-ethnicity: U.S Women Who Had a First Birth between the Ages of 15 and 35 (National Longitudinal Survey of Youth, 1979).

	Self-assessed Health at Age 40					
	Black Women		**White Women**		**Hispanic Women**	
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Age at first birth (reference = 25–35)						
15–19	−.35***	−.26**	−.19**	−.10	−.18	−.15
	(.09)	(.09)	(.07)	(.07)	(.11)	(.11)
20–24	−.28**	−.24*	−.17**	−.15*	−.20	−.19
	(.09)	(.09)	(.06)	(.06)	(.11)	(.11)
Unmarried at first birth (reference = married)	—	−.19**	—	−.39***	—	−.10
		(.08)		(.08)		(.09)
R's mother's education	.04*	.04*	.05***	.05***	.02	.01
	(.02)	(.02)	(.01)	(.01)	(.01)	(.01)
R's mother had teen first birth	.07	.08	.06	.08	−.02	−.01
	(.10)	(.10)	(.11)	(.11)	(.15)	(.15)
R used contraception before first pregnancy	.02	.02	.01	.00	.08	.08
	(.07)	(.07)	(.05)	(.05)	(.09)	(.09)
Home environment age 14[a]						
Lived with both parents	−.13	−.15	.11	.08	−.14	−.14
	(.09)	(.09)	(.07)	(.07)	(.11)	(.11)
Urban	−.00	−.01	−.04	−.06	−.08	−.10
	(.07)	(.07)	(.06)	(.06)	(.10)	(.10)
South	−.01	−.00	.09	.10	.10	.11
	(.08)	(.08)	(.06)	(.06)	(.13)	(.13)
Reading material in home	−.03	−.03	.23	.22	.00	.01
	(.09)	(.09)	(.12)	(.12)	(.10)	(.10)
Adult female in home employed	.06	.05	.04	.03	.01	.00
	(.07)	(.07)	(.05)	(.05)	(.09)	(.09)
Adult male in home employed	.03	.04	.07	.05	.27*	.26*
	(.09)	(.09)	(.08)	(.08)	(.12)	(.12)
Religion in childhood (reference = Baptist)						
Catholic	.01	−.03	.13	.12	−.14	−.15
	(.12)	(.12)	(.08)	(.08)	(.21)	(.21)
Liberal Protestant	.07	.05	.00	−.00	.31	.30
	(.10)	(.10)	(.08)	(.07)	(.34)	(.34)
Other religion	−.03	−.03	−.05	−.04	−.14	−.15
	(.11)	(.11)	(.09)	(.09)	(.28)	(.28)
No religion	.22	.26	−.04	−.02	−.07	−.09
	(.16)	(.16)	(.12)	(.12)	(.37)	(.37)

Variable	Self-assessed Health at Age 40					
	Black Women		White Women		Hispanic Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Health limitations before Ist birth (reference = no)	−.37**	−.38**	−.34***	−.35***	−.60***	−.59**
	(.12)	(.12)	(.09)	(.09)	(.18)	(.18)
Constant	3.27***	3.37***	2.76***	2.87***	3.44***	3.47***
	(.20)	(.21)	(.19)	(.19)	(.28)	(.28)
n	1,029	1,029	1,633	1,633	686	686

Note: Standard errors in parentheses. R = respondent.

[a]Six dichotomous measures of home environment at age 14 coded so that 0 = absence of the characteristic.

*p <.05

**p <.01

***p <.001 (two-tailed tests).

Table 3.

Propensity Score Matching Results (ATT) Estimating the Effect of Age at First Birth on Age 40 Self-assessed Health among Women with a First Birth between Ages 15 and 19 (National Longitudinal Survey of Youth, 1979).

	Self-assessed Health at Age 40		
	Black Women	White Women	Hispanic Women
Variable	ATT	ATT	ATT
Panel A: First birth ages 15–19 compared to ages 25–35			
First birth ages 15–19	−.35**	−.14	−.05
(0 = first birth ages 25–35)	(.11)	(.09)	(.18)
Treatment observations	431	347	254
Control observations	207	758	182
Total *n*	638	1,105	436
Mean % bias	4.0%	5.3%	4.7%
Gamma (Γ)	1.2	—	—
Panel B: First birth ages 20–24 compared to ages 25–35			
First birth ages 20–24	−.25*	−.18*	−.08
(0 = first birth ages 25–35)	(.11)	(.08)	(.15)
Treatment observations	323	512	242
Control observations	207	758	182
Total *n*	523	1,270	424
Mean % bias	5.2%	4.3%	5.3%
Gamma (Γ)	1.5	1.2	—
Panel C: First birth ages 20–24 compared to ages 15–19			
First birth age 20–24	.10	−.00	.03
(0 = first birth ages 15–19)	(.10)	(.09)	(.13)
Treatment observations	322	502	241
Control observations	491	363	261
Total *n*	813	865	502
Mean % bias	4.1%	4.1%	3.7%

Note: Standard errors in parentheses. ATT = average treatment effect for the treated. Mean % bias is the average bias in covariate balance after matching. Gamma (Γ) is the factor by which an unobserved covariate must cause the odds ratio of treatment assignment to differ between treatment and control cases in order for the estimated treatment effect to no longer be statistically significant.

* p <.05

** p <.01

*** p <.001 (two-tailed tests).

Table 4.

Age 40 Self-assessed Health Regressed on Union Status at Birth and Subsequent Union History among Women Whose First Birth Was between the Ages of 15 and 25 (National Longitudinal Survey of Youth, 1979).

	Self-assessed Health at Age 40					
	Total Sample			**Black Women**		
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Marital status at birth						
Unmarried (reference = married)	−.23***	—	—	−.16	—	—
	(.05)	—	—	(.09)	—	—
Marital status at birth and later						
Nonmarital birth, later married	—	−.37***	—	—	−.27**	—
	—	(.07)	—	—	(.10)	—
Nonmarital birth, never married	—	−.15**	.24**	—	−.04	.24**
	—	(.05)	(.07)	—	(.09)	(.09)
Married at first birth	—	—	.40***	—	—	.32**
	—	—	(.07)	—	—	(.11)
First birth at ages 20–24 (reference = ages 15–19)	−.00	.02	.01	.05	.08	.07
	(.05)	(.05)	(.05)	(.08)	(.08)	(.08)
R's mother's education	.04***	.04***	.04***	.03	.03	.03
	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)
R's mother had adolescent birth	.07	.07	.08	.05	.05	.06
	(.08)	(.08)	(.08)	(.12)	(.11)	(.11)
R used contraception before first pregnancy	.02	.02	.02	.00	−.01	−.01
	(.05)	(.05)	(.05)	(.08)	(.08)	(.08)
Home environment at age 14[a]						
Lived with both parents	−.03	−.03	−.03	−.10	−.09	−.10
	(.06)	(.06)	(.06)	(.10)	(.10)	(.10)
Urban	.05	.05	.05	.01	.00	.00
	(.06)	(.06)	(.06)	(.10)	(.10)	(.10)
South	−.07	−.07	−.07	−.02	−.03	−.04
	(.05)	(.05)	(.05)	(.08)	(.08)	(.08)
Reading material in home	.04	.03	.04	−.03	−.03	−.03
	(.07)	(.07)	(.07)	(.10)	(.10)	(.10)
Adult female in home employed	.02	.02	.02	.09	.08	.09
	(.05)	(.05)	(.05)	(.08)	(.08)	(.08)
Adult male in home employed	.09	.09	.08	−.02	−.03	−.03
	(.06)	(.06)	(.06)	(.10)	(.10)	(.10)
Religion in childhood (reference = Baptist)						
Catholic	.06	.05	.05	−.04	−.05	−.06
	(.06)	(.06)	(.06)	(.14)	(.14)	(.14)
Liberal Protestant	.04	.03	.03	.02	.01	.01

Variable	Self-assessed Health at Age 40					
	Total Sample			Black Women		
	(1)	(2)	(3)	(4)	(5)	(6)
	(.07)	(.07)	(.07)	(.12)	(.12)	(.12)
Other religion	−.02	−.03	−.03	−.01	−.02	−.02
	(.08)	(.08)	(.08)	(.12)	(.12)	(.12)
No religion	.04	.04	.04	.24	.25	.25
	(.11)	(.11)	(.11)	(.11)	(.10)	(.18)
Health limitation prior to first birth (reference = none)	−.35***	−.36***	−.36***	−.36**	−.38**	−.38**
	(.08)	(.08)	(.08)	(.13)	(.13)	(.13)
Constant	3.06***	3.07***	2.68***	3.14***	3.15***	2 87***
	(.12)	(.12)	(.12)	(.22)	(.22)	(.21)
N	2,201	2,201	2,201	822	822	822

Note: Standard errors in parentheses. R = respondent.

[a]Six dichotomous measures of home environment at age 14 coded so that 0 = absence of the characteristic.

* p <.05

** p <.01

*** p <.001 (two-tailed tests).

Table 5.

Propensity Score Matching Estimates (ATT) of the Effect of Marital Status at Birth and Marital History on Self-assessed Health at Age 40 among Women with a First Birth Prior to Age 25 (National Longitudinal Survey of Youth, 1979).

	Self-assessed Health at Age 40	
	All Women	**Black Women**
Variable	**ATT**	**ATT**
Panel A: Unmarried at birth and later married compared to married at birth		
Unmarried at birth and married by age 40	−.45***	−.36**
(0 = married at birth)	(.12)	(.15)
Treatment observations	260	208
Control observations	1,220	194
Total *n*	1,480	402
Mean % bias	3.4%	3.6%
Gamma (Γ)	1.8	1.3
Panel B: Unmarried at birth and never married compared to married at birth		
Unmarried at birth and never married	−.22**	−.04
(0 = married at birth)	(.08)	(.13)
Treatment observations	701	407
Control observations	1,220	194
Total *n*	1,921	601
Mean % bias	2.7%	5.7%
Gamma (Γ)	1.3	—
Panel C: Unmarried at birth and never married compared to unmarried at birth and later married		
Unmarried at birth and never married	.30**	.30**
(0 = unmarried at birth and later married)	(.11)	(.12)
Treatment observations	721	420
Control observations	260	208
Total *n*	981	628
Mean % bias	4.8%	4.3%
Gamma (Γ)	1.5	1.4

Note: Standard errors in parentheses. ATT = average treatment effect for the treated. Mean % bias is the average bias in covariate balance after matching. Gamma (Γ) is the factor by which an unobserved covariate must cause the odds ratio of treatment assignment to differ between treatment and control cases in order for the estimated treatment effect to no longer be statistically significant.

* p <.05

** p <.01

*** p <.001 (two-tailed tests).