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Marriage Advantages in Perinatal Health: Evidence of Marriage Selection or Marriage Protection?

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Abstract

Marriage is a social tie associated with health advantages for adults and their children, as lower rates of preterm birth and low birth weight are observed among married women. This study tests two competing hypotheses explaining these marriage advantages—marriage protection versus marriage selection—using a sample of recent births to single, cohabiting, and married women from the National Survey of Family Growth, 2006–10. Propensity score matching and fixed effects regression results demonstrate support for marriage selection, as a rich set of early life selection factors account for all of the cohabiting-married disparity and part of the single-married disparity. Subsequent analyses demonstrate prenatal smoking mediates the adjusted single-married disparity in birth weight, lending some support for the marriage protection perspective. Study findings sharpen our understanding of why and how marriage matters for child well-being, and provide insight into preconception and prenatal factors describing intergenerational transmissions of inequality via birth weight.

Keywords

Child Well-Being; Cohabitation; Health Disparities; Infants; Marriage; Pregnancy

Marriage is a social tie associated with health advantages for adults and their children (Repetti, Taylor, & Seeman, 2002; Umberson & Montez, 2010; Waite, 1995), provided the union is generally supportive and not extremely stressful (Kiecolt-Glaser & Newton, 2001; Umberson, Williams, Powers, Liu, & Needham, 2006). Health advantages are also widely documented for infants of married mothers: lower rates of problematic health outcomes such as low birth weight (weighing less than 5 ½ pounds at birth) and preterm birth (delivery before 37 weeks of gestational length) are reported among married (versus unmarried) mothers (Buckles & Price, 2013; Martin, Hamilton, Osterman, Curtin, & Mathews, 2013).

At debate is whether marriage advantages in health are confounded by selection factors—a *marriage selection* hypothesis—or if marriage causes women to adopt healthier attitudes or behaviors that translate to better health—a *marriage protection* hypothesis. In the case of perinatal health, the answer is unclear, in part because nearly all studies to date have not considered both explanations.

We also know very little about advantages in perinatal health for infants born to married women relative to infants born to cohabiting and single women. Past studies have largely contrasted birth outcomes between married women and all unmarried women, despite clear differences between cohabitation and singlehood. Among the studies that have distinguished between cohabitation and singlehood (Bird, Chandra, Bennett, & Harvey, 2000; Fomby, 2011; Sullivan, Raley, Hummer, & Schiefelbein, 2012), most examined pre-1990 births and/or did not directly measure cohabitation. The widespread, dramatic increase in cohabitation and nonmarital childbearing, along with marked compositional change among subgroups of women married, cohabiting, and single at birth, since the 1990s (Cherlin, 2010; Seltzer, 2000; Smock & Greenland, 2010) establish the importance of understanding population-level perinatal health disparities across all three groups, and within recent years. This new knowledge will not only clarify how pervasive these marriage advantages are, but will also enrich our understanding of the broader implications of cohabitation for child well-being—a topic of substantial interest among marriage and family scholars.

Furthermore, nearly all marriage and perinatal health studies have measured marital status at the time of birth. This is likely due to data limitations, however, I will argue below that, in pursuit of capturing the true effect of marriage on perinatal health, it is preferable to measure marital status at conception, given that (1) at least some of marriage's protective effect, as it has been described in past work, is assumed to be exerted during pregnancy, and (2) changes in union status between the time when couples learn of a pregnancy and the time of birth are not uncommon (Bachu, 1999; Rackin & Gibson-Davis, 2012). Establishing the ideal stage at which to capture union status has important implications for how we conceptualize and model exposure to marriage.

Lastly, research in this area has yet to identify a rich set of selection factors that confound the association between marriage and perinatal health. It is well-known that the early life environment influences women's risk of marital childbearing (see, for example, Amato et al. (2008)), but whether and how the early life environment influences birth weight is not well-established. Health scholars have recently articulated the importance of identifying preconception factors affecting perinatal health, yet most studies continue to focus on the prenatal period, or on the twelve months leading up to conception—to the exclusion of prior life events (Johnson et al., 2006; van Dyck, 2010). This is partly due to data limitations, as birth certificate data are commonly used and do not contain this information. But, this is also due to statistical limitations, as fixed effects models are commonly used to account for selection and this method differences out, rather than reveals, early life selection factors. However, new data now exist that include a rich set of preconception factors, in addition to marital status and birth weight. This study also uses a different statistical approach, propensity score matching, that can reveal key preconception factors. This rich description is critical for expanding our knowledge of preconception risk factors. This information can also inform a growing literature in the social sciences that pinpoints birth weight as one specific mechanism that can transmit inequality across generations, from mothers to children (Case, Lubotsky, & Paxson, 2002; Case & Paxson, 2006; Kane, Forthcoming). The rich description of early life selection factors would add new knowledge clarifying the first portion of this mechanism—specific ways in which maternal disadvantage contributes to adverse birth outcomes.

This study goes beyond past research by rigorously assessing the extent to which marriage protection *and* marriage selection explain marriage advantages in perinatal health, using contemporary, population-level data (National Survey of Family Growth, 2006–10). Marriage advantages are contrasted across three relationship statuses (married, cohabiting, single) at the time of conception, and assessed using two statistical strategies that account for unobserved heterogeneity (propensity score matching and fixed effects models). Furthermore, this study seeks to identify a rich set of preconception factors confounding the marriage advantage (see Figure 1). Study findings will reveal if and how marriage matters for a critical indicator of child well-being, and provide insight into early life and prenatal factors that describe how inequality is transmitted across generations via birth weight.

BACKGROUND

Marriage Selection versus Marriage Protection

Two competing hypotheses have been developed to explain marriage advantages in health. The first, marriage selection, posits that selection factors, such as greater socioeconomic status or better health, place individuals at higher risk of being married and of exhibiting health advantages (Goldman, 1993; Haas, 2006, 2008). Accounting for the endogeneity of marriage is therefore necessary to produce an unbiased estimate of the effect of marriage on health. This explanation is plausible in the case of perinatal health as mothers with lower human and financial capital are less likely to be married at childbirth (Seltzer, 2000) and exhibit higher risk of adverse birth outcomes (Martin et al., 2013).

Evidence of marriage selection in the case of perinatal health has been mounted by studies that have indirectly (Albrecht, Miller, & Clarke, 1994; Bennett, 1992; Bennett, Braveman, Egerter, & Kiely, 1994; Bird et al., 2000) or directly (Buckles & Price, 2013; Sheehan, 1998) tested this hypothesis. The most up-to-date evidence suggests selection accounts for half of the married-unmarried disparity in birth weight; also, race-ethnicity and nativity play a stronger role than maternal health risk in selecting women into being married at the time of a birth (Buckles & Price, 2013). Yet, beyond these factors, we know nothing about what else may comprise the selection mechanism.

Past work in sociology and public health point to the early life environment as one key source of selection factors. Early life poverty, exposure to negative social environments (such as high levels of family instability or child maltreatment), and young women's accumulated human capital influence both family formation decisions (Amato & Kane, 2011; Amato et al., 2008; Cherlin, Kiernan, & Chase-Lansdale, 1995; Goldscheider & Goldscheider, 1998) and birth outcomes (Gavin, Thompson, Rue, & Guo, 2012; Harville, Boynton-Jarrett, Power, & Hypponen, 2010; Kane, Forthcoming). Childhood religiosity may protect against the risk of nonmarital childbearing (Trent, 1994) and foster better general health (Koenig, 1997), but no studies have yet examined its association with perinatal health.

The second hypothesis, marriage protection, posits that marriage causes individuals to adopt healthier attitudes or behaviors that translate to better health outcomes (Hughes & Waite, 2009; Waite & Gallagher, 2002). There are many reasons to presume this may be true in the

case of perinatal health. For example, spouses may monitor and promote healthy behavior by encouraging pregnant partners to quit smoking or begin prenatal care early on in the pregnancy; they may also contribute greater economic resources to the marital union, and provide the highest levels of social support which can influence women's perceptions of pregnancy wantedness. (These prenatal factors are also inter-related (Reichman, Corman, Noonan, & Dave, 2006), suggesting that multiple mediators should be modeled simultaneously to avoid problems stemming from omitted variable bias.) In turn, each of these is a risk or protective factor for adverse birth outcomes (Chomitz, Cheung, & Lieberman, 1995; Goldenberg, Culhane, Iams, & Romero, 2008). In sum, the marriage protection hypothesis would be supported if (a) early life experiences do not entirely confound marriage advantages, and (b) prenatal protective/risk factors mediate the marriage advantage. I now expound on each of these mechanisms by establishing how marriage may affect these prenatal protective/risk factors.

Prenatal Health Behaviors

Social ties, including spousal ties, can influence one's health in many ways. Spouses can monitor and promote healthy behaviors (Umberson, Crosnoe, & Reczek, 2010; Waite, 1995), or convey information about social norms that instill a sense of responsibility in their partners to adopt protective health behaviors (Umberson & Montez, 2010). In the case of prenatal health behaviors such as smoking, spouses may effectively promote cessation among their pregnant partners, perhaps by conveying widely-held social norms discouraging prenatal smoking or by closely monitoring cessation and potential for relapse throughout the pregnancy. This would be consistent with broader evidence suggesting that higher levels of spousal support to quit smoking can protect against the risk of relapse (Gulliver, Hughes, Solomon, & Dey, 1995). The same logic may hold for prenatal care: spouses may promote early initiation to care, and monitor access to prenatal care throughout the pregnancy. Indeed, past studies show co-residence with a marital spouse (Albrecht et al., 1994) and/or cohabiting partner (Landale & Oropesa, 2001) is associated with early and adequate prenatal care.

However, it is also likely that married women are at relatively low risk of prenatal smoking at the outset of pregnancy, as they are less likely to smoke in the three months leading up to pregnancy (Colman & Joyce, 2003). Sharp increases in smoking cessation rates at, or just before, the time of a wedding are also evident (Bachman, Wadsworth, O'Malley, Schulenberg, & Johnston, 1999; Weden & Kimbro, 2007). This trend may reflect role incompatibility—or, conflict between the new role and responsibilities of marriage and the nonconforming role of substance use and abuse (Bachman et al., 1999). Importantly, this suggests that the potentially protective effects of marriage may operate during, but are not limited to, the prenatal period. More will be said on this below.

The act of monitoring and promoting healthy behavior may be made easier by virtue of sharing a residence. To the extent that cohabitation constitutes a stable, residential union (see Manning and Smock (2005) for a discussion of the diversity of arrangements defined as cohabiting unions), pregnant cohabiters may be exposed to some of these same protective mechanisms. However, marriage and cohabitation likely portend distinct health advantages:

marriage is still unique as a recognized social institution, and holds a symbolic meaning that may promote a greater sense of responsibility to engage in healthier behaviors (Duncan, Wilkerson, & England, 2006; Nock, 1998; Umberson & Montez, 2010; Waite, 1995). Indeed, past studies show rates of smoking and late prenatal care initiation are lowest among married women, higher among cohabiters, and highest among single women (Bird et al., 2000).

Whether prenatal smoking or late prenatal care receipt are causally associated with adverse birth outcomes is a source of debate. Research using instrumental variables to account for the endogeneity of prenatal behaviors tends to show causal effects of prenatal smoking on birth weight (see, for example, Reichman et al. (2006)), but mixed evidence for prenatal care. If this study finds prenatal smoking to be a significant mediator after accounting for selection factors, then, together with past research, this would suggest prenatal smoking may be a logical target for future policy and prevention efforts aimed at eliminating marital disparities in birth outcomes.

Pregnancy Wantedness

Social ties can also influence health via social support, which encompasses emotional support (a sense of feeling loved and cared for) as well as instrumental assistance (Thoits, 1995; Uchino, 2004; Umberson & Montez, 2010). Married women tend to report higher levels of social support than cohabiting women, and both groups fare better than single women (Smock, 2000; Waite, 1995). In the context of pregnancy, social support from the baby's father may be reflected in his level of emotional support for the pregnancy or instrumental assistance to the mother during the pregnancy, and may be highest among married couples. Emotional support in particular may be influential in buffering the impact of a stressful life event (Cohen, 2004), such as an unplanned pregnancy. This would be consistent with evidence showing that mother's perceived level of emotional support during pregnancy is associated with lower levels of maternal stress (Landale & Oropesa, 2001). Thus, emotional support may affect a woman's perception of pregnancy wantedness. More broadly, information conveyed via social ties, stemming from social norms regarding nonmarital childbearing or the timing of childbearing for example, may also influence the extent to which a woman perceives a pregnancy as wanted or mistimed. Thus, social ties and social support may not only be stratified by relationship status, but may also influence women's perception of pregnancy wantedness. Indeed, married women report the highest levels of pregnancy wantedness, followed by cohabiting and then single women (Goldscheider & Kaufman, 1996; Santelli et al., 2003). In turn, wantedness is linked with birth outcomes either directly or indirectly via other protective behaviors (Kallan, 1993; Kroelinger & Oths, 2001; Shah et al., 2011; Weller, Eberstein, & Bailey, 1987).

Financial Capital

Social ties can also affect health via the sharing of financial resources. Financial capital is often highest among married women, in part because marriage is associated with greater economies of scale (Waite, 1995). Indeed, married pregnant women report lower rates of participation in government-subsidized health insurance programs—a proxy for prenatal financial capital—than their unmarried counterparts (Bird et al., 2000). Financial assistance

is one type of instrumental support that not only protects individuals against material deprivation, but can also reduce the impact of stressful life events (Cohen, 2004). Believing that one has access to sufficient financial resources may buffer any stress associated with a pregnancy or stress stemming from the anticipated financial strain of raising a child. This is consistent with past work showing that, among poor, urban women, marriage advantages in birth weight operate partly via financial resources (Sheehan, 1998).

In sum, both marriage selection and marriage protection are plausible explanations of marriage advantages in perinatal health. Yet no studies have rigorously examined both hypotheses using contemporary, population-level data that distinguish between married, cohabiting, and single mothers.

When is the Best Time to Measure Relationship Status: At Birth or Conception?

Nearly all studies examining marriage advantages in perinatal health measure union status at the time of birth, which necessarily assumes union status does not change throughout pregnancy. This assumption is not consistent with the broader literature in this area (Gibson-Davis & Rackin, 2014). Shotgun marriage has been commonplace in the US: from 1930 to 1970, more than half of unwed couples married upon learning of a pregnancy; by the 1990s, these rates hovered around a quarter (Bachu, 1999). Shotgun marriages are less common among more recent births (4% of all births in the NLSY97), but post-conception cohabitations, or, transitions from non-residential unions at conception to cohabiting unions at birth, are more common (10.5%) (Rackin & Gibson-Davis, 2012). Moreover, cohabiting couples who become pregnant are more likely to marry than their counterparts who do not conceive (Manning, 1995). Union formalization in light of a pregnancy is also consistent with the notion that the time surrounding birth is a “magic moment” in which many unmarried couples report higher levels of relationship commitment (McLanahan, Garfinkel, Mincy, & Donahuefragile, 2010).

Thus, known risk of relationship change during pregnancy poses two problems for measuring union status at birth. First, by assuming the absence of change, this approach necessarily equates newly formed unions with longer-term unions, thereby misestimating length of exposure to marriage. Accurately measuring the protective effects of marriage—at least those exerted during pregnancy—hinges on an accurate assessment of exposure to marriage throughout the pregnancy. Second, factors selecting women into marital status at birth may differ from those selecting women into marital status at conception. The latter reflects factors that are, by definition, unaffected by the knowledge of a pregnancy; the same cannot be said of the former. Thus, factors selecting women into marital status at birth may also inadvertently capture characteristics of the relationship, such as relationship quality which may be systematically higher among couples who formalize their union during pregnancy. Therefore, whenever data allow, it appears preferable to measure marital status at conception.

METHOD

Data and Sample

This study uses data from the 2006–2010 NSFG, a nationally representative sample of households containing adults ages 15–44. Analyses are based on a sample of females reporting a live singleton birth (6,629) in the five years prior to interview (3,692) (as some prenatal behaviors were only ascertained within this time frame), who were aged 20 or older at birth (given notable distinctions in teen versus non-teen fertility and birth outcomes) ($N = 3,330$). For mothers reporting more than one singleton birth, one birth was randomly selected for all analyses except mother fixed effects.

Measures

This study's measure of perinatal health is birth weight (range = 227 – 6095 grams). (All analyses were replicated adjusting birth weight for gestational length to approximate fetal growth restriction; results were substantively and statistically similar to those presented here.) Relationship status at conception is indicated by married (reference), cohabiting, or non-romantic non-residential (hereafter, "single"). Several indicators from the preconception environment are considered: if the mother was born out-of-wedlock (1=yes), mother's family structure in adolescence (0 = *living with two biological parents at age 14*, 1 = *single or step-parents*), grandmother's and grandfather's education (1 = *less than high school*, 4 = *Bachelor's degree or higher*), mother's childhood religion (1 = *none*, 0 = *Protestant, Catholic, or other*), and mother's human capital in adolescence (1 = *mother completed high school prior to her first birth*, 0 = *otherwise*).

Multiple prenatal protective/risk mechanisms are considered. Mother's financial capital during pregnancy is approximated by a binary variable indicating what party paid for delivery (1 = *private insurance or personal income*, 0 = *Medicaid, government, or other public assistance*). Wantedness draws on two retrospective measures: if the respondent wanted a pregnancy at any time in the future and if she wanted a pregnancy at that time. Consistent with past work (Santelli et al., 2003), two binary indicators were created: whether the respondent (1) wanted the pregnancy (0 = *wanted*, 1 = *unwanted*), and (2) felt the pregnancy occurred too soon or was mistimed (0 = *not mistimed*, 1 = *mistimed/too soon*). (Supplementary analyses combined these with other indicators; results revealed the same two distinct factors which is consistent with prior literature (Gipson, Koenig, & Hindin, 2008; Hummer, Hack, & Raley, 2004).) Father social support is indicated by two questions: if she felt the baby's father wanted a pregnancy at any time in future, and if he wanted a pregnancy at that time. Principal Components Analysis was used to create a single factor score; high values indicate high levels of unwantedness, or low levels of social support (Cronbach's $\alpha = 0.92$). Lack of smoking indicates if the respondent never smoked after learning she was pregnant (1 = *never*, 0 = *ever*)—a dichotomy consistent with past studies showing any prenatal smoking can be harmful (Marsiglio & Mott, 1988). Prenatal care initiation in the first trimester (1 = *yes*) is the current clinical recommendation. Exploratory analyses also included prenatal heavy drinking and drug use, but both were excluded from final analyses as these are typically underreported behaviors, and neither was a statistically significant predictor of birth weight in the final model. (Prenatal smoking may also be

underreported, but if this is the case here, study findings provide conservative, lower-bound estimates of these associations.)

Control variables include race-ethnicity (non-Hispanic black, Hispanic, and non-Hispanic other, relative to non-Hispanic white), nativity status (1 = *foreign born*), and mother's age at conception (20–34 (reference), 35+ years); each of these factors may affect both the risk of marriage and birth weight. Other controls related to the outcome include parity (1 = *first born*, 9 = *ninth born*) and infant sex (1 = *male*). Select analyses also control for relationship change during pregnancy (relationship formation and dissolution, relative to stability) and conception year fixed effects.

Methods

This study uses two statistical strategies that account for unobserved heterogeneity to test the marriage selection hypothesis, propensity score matching (PSM) and mother fixed effects (FE) models, and then a variant of ordinary least squares (OLS) regression to test the marriage protection hypothesis.

PSM is a nonparametric matching technique that indirectly addresses potential endogeneity by comparing the outcome (birth weight) across matched cases that exhibit similar propensities of the treatment (being single or cohabiting, versus married, at conception). Logistic regression is first used to estimate the propensity score, or the probability of being treated given a vector of observed characteristics, $\mathbf{X}$: $p(\mathbf{X}) = \Pr(D = 1 \mid \mathbf{X} = \mathbf{x})$ (Rosenbaum & Rubin, 1983). Next, the propensity scores of treated and non-treated cases are matched, according to a given matching algorithm. Matched cases are compared to ensure that groups are balanced on observed covariates. Matches are then used to calculate outcome differences, thereby producing an average treatment effect on the treated (ATT).

PSM models are unique and powerful in that a relatively large subsample is retained (all cases containing a match and within the region of common support) and by using a nonparametric approach to model the marriage advantage. However, PSM rests on several assumptions—i.e., that assignment into control and treatment groups is random, meaning that all pertinent factors are observed or, at the very least, that relevant unobserved characteristics are correlated with observables (Rosenbaum & Rubin, 1983). (For a more thorough discussion of strengths/limitations see DiPrete and Gangl (2004).)

As a supplementary analysis, mother FE models are performed which implicitly account for all mother-level (preconception) factors. Conceptually, these models regress the difference in outcome (birth weight) across sibling pairs, on the difference in exposure (mother's relationship status at conception), controlling for the difference in all infant-level and time-varying mother-level covariates in the model. Implicit in this model structure is that all time-invariant mother-level covariates, observed or unobserved, difference out of the model. Thus, by definition, relevant preconception selection factors cannot be directly identified. The general equation is: $Y_{ij} = \alpha_j + \Gamma X_j + \sum \beta X_{ij} + \varepsilon_{ij}$, where the first term (α_j) indicates the unobserved family effect, the second term (ΓX_j) indicates mother-level factors that do not differ between births, the third term ($\sum \beta X_{ij}$) indicates the sum of all characteristics unique to infants in the same family, and epsilon (ε_{ij}) indicates the error term capturing unmeasured

factors (Allison 2009). Although useful, these analyses remain supplementary given that preconception factors cannot be identified, and the important limitation that only women who had more than one birth *and* changed relationship status in the last 5 years are retained in the sample.

Finally, the marriage protection hypothesis is examined using an OLS approach, where birth weight is regressed on the treatment (singlehood or cohabitation), the estimated propensity score of singlehood (or cohabitation) $\hat{p}(X)$, mediators, and post-treatment controls. The OLS equation is statistically identified by the nonlinearity of the propensity score. This approach is an extension of covariate adjustment and is discussed elsewhere in detail (Potosky et al., 2000; Wooldridge, 2002), but in short, as the estimated propensity score is a univariate summary of all observed pre-treatment covariates, it can be included as a control (that approximates selection) in a subsequent model. The coefficient (b) on the treatment indicator is an estimate of the treatment effect, but is not constrained by the assumption of strong ignorability and therefore may differ from that produced by PSM. If the treatment and estimated propensity score are collinear, the estimate of the treatment effect could be less precise. Also, in some cases, it has been shown that the estimate of the treatment effect appears to be biased towards the null, relative to PSM (Austin, Grootendorst, Normand, & Anderson, 2007; Austin & Mamdani, 2006). As a result, a supplementary analysis using a treatment effects model (via the *treatreg* command in Stata) based on two-stage least squares regression is performed (G. K. Brown & Mergoupis, 2011). The equations are detailed elsewhere (Kane, Morgan, Harris, & Guilkey, 2013).

Missing data ranged from 1–2% on most variables; thus, a single imputation in Stata was performed. Supplementary analyses (available upon request) suggest that findings were substantively and statistically similar to estimates from complete case analysis. Survey weights are used in the OLS and treatment effects models to account for clustering and stratification in the survey design.

Analytic Strategy

First, the ATT of singlehood or cohabitation (versus marriage) on birth weight is estimated using PSM. Based on statistical advice guiding variable selection to balance precision and bias, relevant confounders (demographic characteristics, early life environment) and key pretreatment covariates of the outcome (maternal age at conception, parity) are included (Brookhart et al., 2006; Brooks & Ohsfeldt, 2013; Garrido et al., 2014), but variables affected by the treatment, such as mediators, are excluded (Ho, Imai, King, & Stuart, 2007; Imbens, 2004). (Exploratory analyses included different subsets of covariates; results were robust to those presented here.) The ATT is contrasted across four matching strategies to examine robustness of study results: caliper radius, k -nearest neighbor, kernel matching, and stratification. (See Garrido et al. (2014) for more detail on matching strategies.) Caliper width is identified as twenty percent of the standard deviation of the logit of the propensity score (Austin, 2011). A preferred estimate of the ATT is selected by identifying the matching strategy with the lowest average value of the standardized absolute mean difference (ASAMD). Adjusted standard errors are calculated using the Abadie-Imbens (AI)

method (Abadie & Imbens, 2008, 2012) on the basis that matched estimates may produce incorrect standard errors.

The robust nature of PSM results are then examined using mother FE models. Finally, OLS models examine if protective/risk factors mediate the association between singlehood (or cohabitation) and birth weight, controlling for the estimated propensity score, prenatal union change, post-treatment covariates of the outcome (infant sex), and conception year fixed effects. Each mediator or set of mediators is added to the model, one at a time; a full model is then estimated to reveal which mediators retain a significant effect. Sobel-Goodman tests identify variables performing significant mediation.

RESULTS

The average birth weight in this sample is 3,321 grams or, nearly 7 ½ pounds (see Table 1), which is comparable to births to non-teen mothers nationwide in 2005, the median birth year in this sample (Hamilton et al., 2007). Over half of mothers (59%) were married at conception; a quarter were cohabiting (24%), and nearly one-fifth were single (17%).

Comparisons across relationship status at conception show many statistically significant differences. Fewer married (than cohabiting or single) mothers lived with single or step-parents during the teen years or were born out-of-wedlock. Married mothers report higher levels of parental education, and report growing up with some religion in childhood with greater frequency. Their own mothers delayed childbearing longer than mothers of cohabiting and single women. A greater proportion of married mothers also completed high school before transitioning to motherhood, suggesting higher levels of human capital in adolescence. Married mothers more frequently use private insurance to pay for the birth, indicating greater financial capital during pregnancy. Married mothers less frequently report a birth was unwanted or mistimed and report higher levels of social support from the baby's father. They also more frequently report abstaining from prenatal smoking and initiating early prenatal care. On nearly every indicator, cohabiting mothers are advantaged relative to single mothers but disadvantaged relative to married mothers.

Next, the propensity score estimation results are presented. Table 2, Model 1 shows that black and Hispanic (versus white) race-ethnicity, being native- (versus foreign-) born, lower levels of grandfather's education, living in a single or step-parent (versus two parent) family, being born-out-of-wedlock (versus within a marital union), reporting no (versus some) childhood religion, not completing (versus completing) high school before transitioning to motherhood, maternal age of 20–34 (versus 35+), and lower parities are associated with singlehood (versus marriage). Conception year fixed effects are included but not shown. The estimation of Model 1 produced a propensity score of singlehood ranging from .05 to .97 (mean = .32); the balancing property was satisfied (not shown).

Propensity score matching results for singlehood versus marriage are presented in Table 3, Panel A. Only minor differences in the ATT are observed across matching strategies: the ATT ranges from –73.50 (kernel matching) to –97.18 grams (*k*-nearest neighbor), the standard error ranges from 33.57 (stratification matching) to 44.39 (*k*-nearest neighbor), and

each estimate is statistically significant at the $p < .05$ level or better. T-tests for equality of means in the treated and non-treated groups show that, after matching, none of the model predictors were significantly different across groups at the $p < .05$ level, further indicating sufficient balancing. The ASAMD was less than 3% after matching for each, and was lowest (1.7%) based on caliper radius matching. Therefore, the ATT estimated via caliper radius is the preferred estimate. Supplementary analyses using the Abadie-Imbens method suggest results are robust ($ATT = -82.72$, adjusted $SE = 42.29$, $p < .05$).

Thus far, PSM analyses demonstrate that, after adjusting for observed confounders of the relationship status—birth weight association, there is a statistically significant single-married disparity in birth weight such that infants of single mothers are 80 grams lighter than their counterparts whose mothers who were married at conception. This is also likely to be of substantive import: an 80 gram difference is nearly one-seventh of a standard deviation of birth weight ($80/617 = 0.13$), and more than half of the raw difference in birth weight ($80/144 \text{ grams} = 0.55$) between single and married mothers.

Next, the cohabiting-married disparity is considered. The logistic regression results presented in Table 2, Model 2 are extremely similar to those depicted in Table 2, Model 1, in terms of both statistical significance and the direction of specific covariates. In this model, the estimated propensity score ranges from .04 to .94 (mean = .36); the balancing property was satisfied.

However, propensity score matching results for cohabitation suggest a different pattern than previously observed. Each estimated ATT, presented in Table 3, Panel B, indicates the lack of a statistically significant cohabiting-married disparity in birth weight. Furthermore, the ATT is quite similar across three strategies—ranging between -10.34 and -18.77 grams based on caliper radius, stratification, and kernel matching—and moderately higher among one strategy— k -nearest neighbor (-36.38 grams). These findings also suggest little substantive difference: a difference of 10–36 grams represents 0.02 – 0.06 of a standard deviation of birth weight. Good balancing was supported: t-tests for equality of means showed model predictors were not significantly different across treated and non-treated groups, and the ASAMD was less than 3% for all matching strategies. The corrected standard error based on the AI method was similar to that produced by other matching strategies (37.85).

Supplementary analyses replicated these models using relationship status at the time of birth. Substantively, this tells us what study results would have been under the assumption that union status did not change throughout the pregnancy. PSM estimates based on caliper radius matching show the ATT of singlehood (versus marriage) on birth weight is -19.19 grams ($SE = 36.77$, $p > .10$), and the ATT of cohabiting (versus marriage) on birth weight is -9.49 grams ($SE = 30.22$, $p > .10$) (results not shown). Thus, assuming no changes in union status greatly underestimates the single-married disparity and does not under- or over-estimate the cohabiting-married disparity.

These findings are clarified upon considering the nature of prenatal union change in these data. Over a quarter of women (28%) who changed statuses formalized their preconception

cohabitation by getting married; eight percent had a shotgun marriage (results not shown). Given step-wise advantages of married, cohabiting, and then single mothers, these changes imply the addition of less advantaged women to a more advantaged subgroup. This change in composition is consistent with underestimating the single-married disparity. Similarly, more than a third of women who changed statuses formed a post-conception cohabitation (35%), but, nearly a quarter dissolved their preconception cohabitation (22%). This volatility, moving in both directions, is consistent with the relative similarity in magnitude of the cohabiting-married disparity regardless of when relationship status is measured.

Next, mother FE models are performed within a subsample of 726 mothers. Results (not shown) indicate a single-married disparity of -90 grams; this is within the range of PSM estimates previously reported. The FE standard error (67.10) is wider than that of PSM—likely reflecting smaller sample size and precluding the detection of a statistically significant disparity. Overall, the similarity in point estimates buttresses the finding of a single-married disparity in birth weight.

Thus far, the marriage selection hypothesis is supported for both single-married and cohabiting-married disparities in birth weight. As the latter disparity is fully explained by preconception selection factors, it is not examined further. Marriage protection is now evaluated as a potential explanation of the residual single-married disparity in birth weight, using a subsample of single or married mothers with an estimated propensity score within the region of common support, or 2,343 mothers.

Table 4 presents the results. Model 1 shows that single mothers exhibit lower birth weight relative to married mothers ($b = -63.54$), and that mothers with a higher propensity score of singlehood exhibit lower birth weight relative to mothers with a lower propensity score of singlehood. Relationship formation (versus stability) is positively associated with birth weight whereas dissolution is inversely associated, though neither coefficient is statistically significant. The estimate of the treatment effect in Model 1 (-64 grams) is slightly lower than that estimated by PSM (decrease of $74 - 97$ grams), which is consistent with past studies showing covariate adjustment can produce estimates of the treatment effect that are biased towards the null. Also, the standard error (55.86) is notably higher than the range from PSM estimation ($34 - 44$). Exploratory analyses showed the treatment to be collinear with the estimated propensity score of singlehood, suggesting that precision is likely affected. Thus, I continue on with an examination of potential mediation despite the lack of statistical significance as these are likely to be lower bound, or conservative estimates.

Model 2 adds the first mediator: type of insurance. Using private insurance to pay for the birth, relative to public insurance or having no insurance, is associated with a 43 gram increase in birth weight, although this coefficient is not statistically significant. Model 3 shows both unwantedness and mistiming are inversely associated with birth weight, but neither is statistically significant. The same holds for Model 4: lower levels of social support from the baby's father are associated with a reduction in birth weight, but this coefficient is not statistically significant. Abstaining from smoking during pregnancy is protective and marginally statistically significant: it is associated with nearly a 116 gram increase in birth weight. Early initiation into prenatal care is not significantly associated with birth weight

(see Model 5). The full set of mediators in Model 6 explain 69% of the total effect of singlehood on birth weight (total effect (63.54) – direct effect (19.43)/total effect = .69), lending some support to the marriage protection hypothesis. Only one protective/risk factor retains a significant association with birth weight in the full model: abstaining from smoking during the pregnancy ($b = 106.60$). Sobel-Goodman mediation tests suggest that lack of smoking performs significant mediation (Sobel test statistic = -13.28 , $SE = 4.66$, $p < .01$), and mediates 8% of the total effect.

Supplementary analyses using 2SLS suggest similar results. Singlehood (versus marriage) is associated with lower birth weight; this coefficient is statistically significant. Private insurance usage, unwanted birth, low social support, and abstaining from smoking are significantly associated with birth weight in intermediate models; only abstaining from smoking remains significant in the full model ($b = 122.14$, $p < .10$), and mediates 9% of the total effect (Sobel test statistic = -12.31 , $SE = 4.34$, $p < .01$).

DISCUSSION

Marriage is a social tie often associated with health benefits for adults, as well as for children and infants of married parents (Buckles & Price, 2013; Martin et al., 2013; Repetti et al., 2002; Umberson & Montez, 2010; Waite, 1995). This study examined two explanations of marriage advantages in perinatal health, marriage selection and marriage protection, by first examining a rich set of preconception factors selecting women into three different relationship statuses (married, cohabiting, single) at conception; and then by examining whether prenatal protective/risk mechanisms mediated the residual relationship status—birth weight association.

Study findings contribute to the literature in four ways. First, results demonstrated a salient disparity in birth weight between mothers who were single versus married at conception. This pattern was robust across four propensity score matching strategies; the preferred PSM estimate revealed a disparity of 80 grams. This finding was supported by supplemental FE analyses showing a similar disparity: 90 grams. Interestingly, PSM results consistently showed cohabiting mothers fared no worse than married mothers. The salience of a single-married disparity net of selection is consistent with studies contrasting birth outcomes across married and unmarried women (Albrecht et al., 1994; Bird et al., 2000; Buckles & Price, 2013), but goes beyond past research by providing evidence that birth weight disparities are concentrated between single and married women, and do not hold, net of selection, for cohabiting versus married women. The latter is consistent with evidence showing cohabiting-married disparities in low birth weight have diminished over time (Fomby, 2011).

The lack of a salient cohabiting-married disparity in birth weight suggests exposure to a cohabiting union does not disadvantage infants. This stands in contrast to a broader literature documenting poorer well-being of children growing up with cohabiting parents (S. L. Brown, 2004; Manning & Lichter, 1996). However, scholars have argued that these detriments are often associated with the experience of living with cohabiting parents—via either diminished economic and parenting resources (S. L. Brown, 2004) or instability that is

the primary driver of family structure effects on child well-being (Bumpass & Lu, 2000; Sandefur & Mosley, 1997; Wu, 1996; Wu & Martinson, 1993). If we accept that pathways linking *in utero* exposure to parental cohabitation and birth weight are distinct from those linking *in vivo* exposure and child well-being, then study findings are not in conflict with past research. Alternatively, this finding could indicate growing convergence in child well-being across family types as family diversity grows, but more research is needed to evaluate this possibility.

Second, PSM analyses revealed key preconception factors that appear to confound marriage advantages in perinatal health. Consistent with past research (Buckles & Price, 2013), race-ethnicity, nativity status, and birth year are influential. But study results go further by richly describing other preconception factors that also play a role, including childhood socioeconomic status (grandfather's education), early life family instability (if the mother was born out-of-wedlock and family structure in adolescence), childhood religion, mother's human capital in adolescence, and maternal age. These results contribute to a growing literature on preconception health by providing indirect evidence that these early life factors shape perinatal health. (The evidence is indirect in that the PSM included factors that affect both the treatment (relationship status) and outcome (birth weight), and did not distinguish between the two.) However, that these preconception factors appear to affect birth weight is consistent with past work citing direct effects of childhood poverty (Harville et al., 2010) and mother's preconception education (Kane, Forthcoming) on adverse birth outcomes. That early life family instability and childhood religion may also influence birth weight are new contributions. This finding also contributes to the literature describing birth weight as one mechanism through which inequalities are transmitted across generations (Case et al., 2002; Case & Paxson, 2006; Kane, Forthcoming), by providing new knowledge describing the first portion of this mechanism—how maternal disadvantage affects birth weight.

Third, results buttressed the importance of measuring relationship status at conception, not birth, whenever possible. Relying on relationship status at birth under-estimated the single-married disparity in birth weight by more than three-quarters (–17 grams, compared with –80 grams). This finding likely reflects the fact that many women in these data formalized their union upon learning of a pregnancy—a trend consistent with studies documenting that prenatal union change is not uncommon (Rackin and Gibson-Davis (2012)). Future work measuring relationship status at conception will continue to sharpen our understanding of why and how marriage benefits women and their children.

Fourth, study findings demonstrated that abstaining from prenatal smoking mediated the association between singlehood and birth weight, net of controls and observed early life covariates selecting young women into singlehood (versus marriage) at conception. Thus, the marriage protection hypothesis found some support in the case of single-married disparities in birth weight. In contrast with past research (Sheehan, 1998), financial capital was not a significant mediator in the final model. However, Sheehan's study did not include the same set of multiple mediators examined here, many of which are correlated with financial capital. This underscores the importance of including multiple protective/risk factors simultaneously to avoid problems stemming from omitted variable bias.

It is well-established that prenatal smoking is the leading preventable cause of perinatal morbidity and mortality (Chomitz et al., 1995; Cnattingius, 2004), but findings presented here suggest smoking is also key to understanding how and why marriage matters for perinatal health. The mediation of prenatal smoking, above and beyond selection effects, may reflect the notion that spouses can monitor and promote smoking cessation, perhaps via communicating social norms discouraging prenatal smoking, or may reflect the unique symbolic meaning of marriage that can promote a greater sense of responsibility within individuals to engage in healthier behaviors. (The extent of mediation documented here may also underestimate the full protective effect of marriage on birth weight, as some of marriage's protective effect likely operates before a birth—by inducing smoking cessation at the time of a wedding or immediately before conception, for example.) Future research should use richer data to examine prenatal smoking in conjunction with preconception smoking to test the salience of this finding. If this result holds, then this would suggest, together with past research demonstrating causality of prenatal smoking for birth weight (e.g., Reichman et al. (2006)), that future policy efforts could target prenatal smoking cessation in an effort to minimize marital disparities in birth weight.

Study limitations should be noted. First, some protective/risk factors examined here reflect, at least in part, some measure of selection as well (e.g., prenatal financial capital is likely related to preconception financial capital, and prenatal smokers likely take up smoking prior to pregnancy). The approach demonstrated here is a clear step beyond past research, but nonetheless, future research could model the endogeneity of each mediator separately to test this possibility. Second, retrospective measures of pregnancy wantedness were used which may be subject to a revisionist history (Miller & Jones, 2009). Pre-pregnancy measures, if available in the future, would be optimal. Third, general health may mediate marriage advantages in health (Umberson & Montez, 2010) and may select women into marriage (Buckles & Price, 2013). Data on general health were not available here, although, to the extent that prenatal health behaviors matter more for birth weight than general health (as they are more proximate to the time of birth), this may be inconsequential. Lastly, this study used an indirect measure of father's social support. Although instrumental or emotional support do not appear to directly affect birth weight (Lanale & Oropesa, 2001), future research may benefit from exploring richer measures.

In conclusion, this study adds to our understanding of how and why marriage matters for one important indicator of child well-being. In doing so, findings lend more insight into how inequality is transmitted across generations via birth weight. Future work should continue examining this and other ways maternal disadvantage can contribute to lower birth weight to increase our understanding of population-level trends of social inequality. Research on the role of relationship quality and prenatal union change may be particularly fruitful to further increase our understanding of how marriage advantages in perinatal health operate.

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FIGURE 1.
CONCEPTUAL FRAMEWORK LINKING PRECONCEPTION ENVIRONMENT,
RELATIONSHIP STATUS AT CONCEPTION, PRENATAL PROTECTIVE/RISK
MECHANISMS, AND BIRTH WEIGHT

Note: Dashed lines denote hypothesized confounding pathways; double lines denote hypothesized mediating pathway.

Table 1
Weighted Descriptive Statistics for Analytic Sample, and Weighted Means (or Percentages) by Mother's Relationship Status

	Mean or Percent	Standard Deviation	Mean or Percent by Relationship Status			Significant Difference
			Married	Cohabiting	Single	
Birth weight (range, 227–6095 grams)	3,321.36	617.37	3,363.31	3,288.65	3,219.16	M > C > S
Relationship status at conception						
Cohabiting	24.42%		–	–	–	–
Single	16.45%		–	–	–	–
Married (<i>ref</i>)	59.13%		–	–	–	–
<i>Preconception Environment</i>						
Childhood Family Structure and Socioeconomic Status						
Respondent born out-of-wedlock (1 = <i>yes</i>)	17.87%		10.22%	23.13%	37.55%	M < C < S
Single or step-parent family (1 = <i>yes</i>)	35.43%		26.36%	45.64%	52.89%	M < C < S
Grandmother's education (range, 1–4)	2.28	1.03	2.38	2.12	2.13	M > C, S
Grandfather's education (range, 1–4)	2.31	1.06	2.47	2.04	2.15	M > C, S; C < S
Childhood religion (<i>ref</i> = Protestant/Catholic/other)						
None	8.54%		6.15%	13.54%	9.72%	M < C, S
Human Capital in Adolescence						
Finished HS before having first child (1 = <i>yes</i>)	76.33%		84.88%	63.43%	64.75%	M > C, S
<i>Prenatal Protective (or Risk) Mechanisms</i>						
Financial capital during pregnancy						
Private insurance paid for birth (1 = <i>yes</i>)	58.13%		76.21%	33.53%	29.64%	M > C > S
Birth was unwanted (1 = <i>yes</i>)	13.20%		7.90%	18.33%	24.63%	M < C < S
Birth was mistimed (1 = <i>yes</i>)	17.42%		10.70%	24.07%	31.68%	M < C < S
Low social support (range, –0.86–3.77)	–0.18	1.39	–0.50	0.00	0.72	M < C < S
Never smoked during pregnancy (1 = <i>yes</i>)	88.19%		93.88%	81.21%	78.13%	M > C, S
Rec'd prenatal care in 1st trimester (1 = <i>yes</i>)	90.43%		93.77%	86.58%	84.14%	M > C, S
Controls						
Demographic characteristics						
Mother's race-ethnicity (<i>ref</i> = non-Hispanic white)						
non-Hispanic black	14.97%		7.49%	17.38%	38.30%	M < C < S

	Mean or Percent by Relationship Status					Significant Difference
	Mean or Percent	Standard Deviation	Married	Cohabiting	Single	
Hispanic	20.94%		18.25%	27.45%	20.95%	C > M, S
non-Hispanic other	6.65%		7.49%	6.43%	3.97%	M > C > S
Nativity status (1 = <i>foreign-born</i>)	19.29%		20.67%	19.88%	13.46%	M > C > S
Pregnancy characteristics						
Mother's age at conception (<i>ref</i> = 20–34 years)						
35+ years old	12.34%		16.59%	5.49%	7.24%	M > C, S
Parity (range, 1–10)	2.12	1.18	2.18	2.02	2.07	M > C, S
Infant sex (1 = <i>male</i>)	49.67%		52.07%	48.72%	49.32%	ns
Birth year (range, 2001 – 2010)	2005	1.90	2005	2005	2005	
Sample sizes (unweighted)	3,330		1,640	914	776	

Note: Figures are adjusted for weighting. Significant differences indicate differences in the level of covariates by relationship status at conception at the $p < .05$ level or better; *ns* = no significant difference.

Table 2

Logit Coefficients from the Logistic Regression Model Predicting Singlehood versus Marriage (Model 1, n = 2,416), and Cohabitation versus Marriage (Model 2; n = 2,554)

	Model 1: Singlehood	Model 2: Cohabitation
Race-ethnicity (ref = non-Hispanic White)		
non-Hispanic black	1.99*** (0.14)	1.37*** (0.13)
Hispanic	0.80*** (0.16)	0.71*** (0.14)
non-Hispanic other	0.31 (0.28)	0.52* (0.21)
Foreign-born (ref = native-born)	-1.05*** (0.16)	-0.80*** (0.14)
Childhood Socioeconomic Status and Family Structure		
Grandmother's education	-0.07 (0.06)	-0.01 (0.06)
Grandfather's education	-0.14* (0.06)	-0.27*** (0.06)
Single or step-parent family (ref = two biological parents)	0.68*** (0.11)	0.61*** (0.10)
Mother born out-of-wedlock (ref = marital birth)	0.49*** (0.13)	—
No religion in childhood (ref = Protestant, Catholic, other)	0.50** (0.19)	0.71*** (0.16)
Human Capital in Early Adulthood		
Finished high school before having first child	-0.87*** (0.13)	-0.92*** (0.11)
Pre-Treatment Covariates of Outcome (Birth Weight)		
Maternal age at conception (ref = age 20 – 34)		
Age 35+	-0.51** (0.18)	-1.07*** (0.19)
Parity	-0.24*** (0.05)	-0.21*** (0.04)
Intercept	-0.38 (0.31)	-0.04 (0.39)
-2 log pseudolikelihood	2,362.16	2,837.06
Wald chi-square	671.21***	494.29***

Note:

 $p < .001$

**
 $p < .01$

*
 $p < .05$

†
 $p < .10$ (two-tailed). Models include conception year fixed effects.

Table 3

Summary of Propensity Score Matching Results, Showing the Average Treatment Effect (on the Treated) of Singlehood versus Marriage (Panel A), and Cohabitation versus Marriage (Panel B), on Birth Weight

	Caliper Radius	<i>k</i> -Nearest Neighbor (<i>k</i> = 3)	Kernal Matching	Stratification
Panel A:				
β_{single} (SE)	-79.57* (37.18)	-97.18* (44.39)	-73.50* (36.22)	-89.98** (33.57)
Panel B:				
$\beta_{\text{cohabitation}}$ (SE)	-18.77 (30.34)	-36.38 (34.29)	-17.23 (30.21)	-10.34 (28.81)

Note: Caliper width is .05 in Panel A, and .04 in Panel B.

OLS Regression Coefficients from Models Predicting Birth Weight from the Singlehood (versus Marriage) at Conception, the Estimated Propensity Score of Singlehood, and Potential Mediators

Table 4

	1	2	3	4	5	6
Single (ref = Married)	−63.54 (55.86)	−46.56 (56.06)	−48.17 (61.75)	−46.19 (63.48)	−46.94 (56.16)	−19.43 (64.34)
<i>Protective (or Risk) Mechanisms</i>						
Financial Capital during Pregnancy						
Private insurance paid for birth		43.21 (35.88)				28.96 (39.70)
Birth was unwanted			−47.43 (56.70)			−32.44 (55.85)
Birth was mistimed			−38.76 (51.46)			−27.20 (52.18)
Low Social Support from Partner				−11.71 (14.70)		−4.99 (13.75)
Prenatal Behaviors						
Never smoked during pregnancy					115.89 [†] (64.17)	106.60 [†] (64.72)
Received prenatal care in first trimester					−10.64 (57.68)	−16.86 (58.89)
Estimated propensity score of singlehood	−399.18 ^{***} (105.38)	−377.93 ^{***} (109.20)	−394.76 ^{***} (104.68)	−403.05 ^{***} (105.40)	−400.74 ^{***} (106.60)	−386.01 ^{***} (110.21)
Relationship change (ref = stable)						
Relationship formed	56.29 (77.27)	49.12 (77.03)	53.12 (77.53)	48.73 (78.45)	54.46 (74.58)	44.89 (75.16)
Relationship dissolved	−99.18 (80.87)	−83.05 (84.36)	−83.72 (85.49)	−86.99 (83.81)	−87.71 (84.67)	−62.70 (91.05)
Intercept	3,366.71 ^{***}	3,329.15 ^{***}	3,373.14 ^{***}	3,361.09 ^{***}	3,270.00 ^{***}	3,261.89 ^{***}
F statistic	4.50 ^{***}	4.31 ^{***}	4.05 ^{***}	4.28 ^{***}	4.40 ^{***}	3.56 ^{***}

Note:

 $p < .001$

**
 $p < .01$

*
 $p < .05$

$p_i < .10$ (two-tailed), $N = 2,343$ (759 single women and 1,584 married women in the region of common support). All models control for infant sex and conception year fixed effects.

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