

## Marriage, Cohabitation, and Sexual Exclusivity

# Marriage, Cohabitation, and Sexual Exclusivity: Unpacking the Effect of Marriage

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**S**exual concurrency, or having temporally overlapping sexual partnerships, has important consequences for relationship quality and individual health, as well as the health and well-being of others embedded in larger sexual networks. Although married and cohabiting couples have similar, almost universal expectations of sexual exclusivity, the former report significantly lower rates of engaging in sexual concurrency than the latter. Given that this difference in behavior occurs despite similar expectations of sexual fidelity, sexual exclusivity can provide an important test of whether marriage has a causal effect on relationship behavior. Using data from the National Longitudinal Study of Adolescent to Adult Health, I estimate an instrumental variable model testing whether observed differences in sexual concurrency between marital and cohabiting relationships are attributable to marriage itself via a recent implementation of the special regressor method, an estimator for binary choice models with endogenous regressors. I find evidence that, relative to cohabitation, marriage reduces the likelihood that an individual will engage in concurrent sexual relationships. Finding an effect of marriage in a recent cohort of young adults suggests that, despite changes in marriage and cohabitation, marriage still influences individual behavior.

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## Introduction

The literature surrounding marriage is commonly centered on the question of whether marriage confers benefits to individuals or merely reflects the advantages that lead to marriage. Changes in the American family over the last half century have sustained ongoing inquiry into whether, and to what extent, outcomes of married individuals are the result of selection or causation. To test whether marriage itself changes behavior, this paper explores the effect of marriage on sexual concurrency, defined as having temporally overlapping sexual partnerships. Marital status is related to sexual exclusivity; compared to those in cohabiting relationships, married individuals are less likely to report concurrent sexual relationships (Forste and Tanfer 1996; Treas and Giesen 2000), even though they report similar expectations of sexual exclusivity (Treas and Giesen 2000). Of course, given the highly selective nature of marriage, any causal interpretation of observed differences remains highly suspect. Nevertheless, focusing on an outcome to which both married and cohabiting couples aspire—sexual exclusivity—reduces the chances that behavioral differences are due to variations in preferences that sort couples into marriage and cohabitation.

In addition to better understanding the effects of marriage, there are important health and relationship implications of sexual concurrency. Having concurrent sexual relationships increases the risk of contracting sexually transmitted infections and spreading them to partners and others in the sexual network. Those with concurrent sexual relationships have higher rates of human papillomavirus (HPV) (Javanbakht et al. 2010), bacterial-based sexually transmitted infections (e.g., gonorrhea, chlamydia, or syphilis) (Kraut-Becher and Aral 2003), and HIV/AIDS (Kretzschmar and Morris 1996; Morris and Kretzschmar 1997). In addition to higher infection risk for the individual and his or her partners, concurrent sexual partnerships increase the risk of sexual infections in the general population by extending the maximum network reach for connected individuals (Moody 2002) who may act as “bridges,” increasing the STI risks for all individuals within the sexual network (Bearman, Moody, and Stovel 2004). Concurrent sexual relationships also have implications for the primary romantic relationships. Violating sexual exclusivity where expected (i.e., sexual infidelity) is among the most frequent disruptors of relationships. Therapists and counselors have traditionally viewed infidelity as one of the most damaging and difficult issues to treat (Whisman, Dixon, and Johnson 1997). Couples who have experienced infidelity are more distressed than those seeking therapy for other reasons and have slower recovery paths in marital therapy (Atkins et al. 2005). Infidelity is also associated with lower relationship quality (Previti and Amato 2004) and strongly predicts marital dissolution (Amato and Rogers 1997).

After reviewing threats to causal inference in the case of marriage, I provide a conservative test of the effect of marriage on sexual concurrency. Using data from Wave IV of the National Longitudinal Study of Adolescent to Adult Health (Add Health), I address selection into marriage with a newly described special regressor approach to instrumental variable estimation. The special

regressor approach overcomes a key limitation in instrumental variable analysis and, unlike other instrumental variable estimators, is suited for use in dichotomous outcome and endogenous regressor systems (Dong and Lewbel 2015). Using this novel methodology, I find that marriage increases sexual exclusivity, with married individuals being eight percentage points less likely to engage in concurrent sexual partnerships than their cohabiting counterparts. I conclude by discussing how this paper contributes not only to the study of concurrent sexual relationships and their impacts, but also to the broader literature on the benefits of marriage.

### ***Effects of Marriage***

Married individuals have better outcomes than their non-married counterparts (for an early review, see Waite and Gallagher [2000]) in areas such as socioeconomic status, health (for a review, see Umberson and Montez [2010]), criminality (e.g., Sampson, Laub, and Wimer 2006), child well-being (for a review, see Ribar [2015]), and even happiness (e.g., Soons and Kalmijn 2009). Recent litigation concerning same-sex marriage has even rested on the belief that married individuals enjoy a variety of benefits, with one recent decision stating that “to exclude a couple from marriage is thus to deny it a coveted status” (Baskin v. Bogan 2014).

Recognizing these associations between marriage and socially beneficial outcomes, many policymakers have identified marriage as a potential mechanism to ameliorate existing social concerns. Marriage promotion exists as a policy goal on multiple levels within the United States. At the federal level, marriage promotion has occurred both indirectly, through reduction of the “marriage penalty” in taxation (Carasso and Steuerle 2005), and directly, through programs like the Healthy Marriage Initiative which, since 2003, has sought to teach skills necessary for sustaining healthy marriages. Many individual states have followed suit and sought to promote marriage through public proclamations recognizing the importance of marriage, modifications to divorce laws, incentives for marriage preparation education, and easier eligibility for state programs for married couples (Gardiner et al. 2002).

However, the use of marriage to ameliorate poverty, improve child well-being, or reduce inequality relies on a causal claim that marriage is responsible for these observed benefits. But scholars have called this assertion into question. For example, married men earn more than their unmarried counterparts (e.g., Cheng 2016; Killewald and Gough 2013). However, this observed “marital wage premium” could result from the selection into marriage by men who anticipate or are already experiencing upward wage trajectories (Killewald and Lundberg 2017; Krashinsky 2004) or who have selected into fields or firms with high potentials for wage growth (Petersen, Penner, and Hogsnes 2011). Considering findings like these, which support marital selection arguments for observed marital benefits, it is essential that researchers evaluate whether and to what extent marriage impacts individual outcomes. If marriage does not create individual change, social policies other than marriage promotion may be more appropriate for addressing societal concerns.

## ***Estimating the Effects of Marriage***

To disentangle relationship benefits from selection effects, researchers have employed a number of approaches, including controlling for potential confounding factors, jointly modeling both marriage and the outcome of interest (e.g., [Lillard, Brien, and Waite 1995](#)), individual fixed-effects models (e.g., [Brown 2000](#); [Musick and Bumpass 2012](#)), propensity score matching or counterfactual modeling (e.g., [Kim 2011](#); [Williams et al. 2011](#)), and instrumental variables (for an overview of employed instruments, see [Ribar \[2004\]](#)). But an important first step in any such analysis is determining a reference group with which to compare married individuals. To determine whether the marital status is itself the cause of behavioral change and therefore beneficial outcomes, it makes sense to compare marriage to cohabitation. The similarities between these contexts start at the most basic level; as with marriage,<sup>1</sup> cohabitation entails sharing a living space with a romantic partner. Shared residence means that cohabiting couples benefit from the efficiencies in joint production and consumption that create economic surpluses similar to those seen in marriages ([Becker 1981](#)), such as economies of scale, shared leisure and household goods, and, to a lesser degree ([Moreau and Lahga 2011](#)), specialization and division of labor. In addition to these analogous benefits, recent changes in norms and behaviors suggest increasing similarities between these contexts. For example, cohabitation is an increasingly common context of childbearing ([Martinez, Daniels, and Chandra 2012](#)).

## ***Differences in Cohabitation and Marriage***

Though some similarities between marriage and cohabitation exist, studies also document differences between these two union types. These differences may arise because relationship context creates different expectations and experiences or because of differences in who chooses to cohabit or marry. While these two axes of differentiation are not completely distinct (i.e., people seeking stable relationships may choose to marry because of an understanding that marital unions tend to be more stable than cohabiting unions), they serve as a useful guide for understanding both how marriage may influence behavior and the challenges in isolating such a causal relationship.

Expectations and purposes vary between marriage and cohabitation, including future goals and levels of commitment. Even in the face of increasing marital deinstitutionalization ([Cherlin 2004](#)), most individuals still view marriage as a long-term contract for the purposes of family building and lifelong commitment. In contrast, cohabitation is an “incompletely institutionalized institution” ([Waite 1995](#)) whose meanings vary from a substitute for marriage to marital precursor to co-residing daters ([Casper and Sayer 2000](#); [Heuveline and Timberlake 2004](#); [Stanley, Rhoades, and Fincham 2011](#)). Though this heterogeneity of cohabitation precludes a simple generalization of cohabiting unions, existing research has identified differences between cohabitation and marriage. These differences manifest from the outset; while marriage is defined by an event (e.g., a wedding or a visit to the courthouse), the transition to cohabitation takes

many forms and, though varied, is frequently characterized as a process of sliding or drifting through a sequence of incremental steps and decisions (Manning and Smock 2005). Marriage and cohabitation are also distinguished by their difficulty of dissolution, as the legal, social, economic, and emotional commitments of marriage imply higher direct costs for dissolving a marital tie than a cohabiting one. Though the costs of relationship dissolution for marriage and cohabitation have grown similar over time in some ways (Tach and Eads 2015), the requirements of dissolving legal bonds (i.e., formal divorce proceedings) or being more likely to disentangle conjoined finances (Kenney 2004) could increase the costs of marital dissolution relative to ending a cohabiting union. These structural differences could suggest potential mechanisms by which marriage creates individual benefits.

### ***Challenges to Causal Reasoning***

However, the second axis of differentiation, that individuals choosing to marry are different from those choosing to cohabit, threatens the use of cohabitation as a marital counterfactual. As associations between the characteristics of who marries and outcomes of interest can bias estimates of marital effects, the nature of who selects into marriage is a central causal challenge. Sociodemographic differences between individuals who marry and cohabit have long been noted, and these differences persist. Relative to married individuals, those currently in cohabiting unions are significantly younger, have lower educational attainment, are more likely to be Hispanic or Black, are more likely to have grown up outside a two-parent household, and have lower incomes (Copen et al. 2012; Goodwin, Mosher, and Chandra 2010). Furthermore, early researchers documented differences in attitudes toward structured relationships, family orientation, and desired autonomy between cohabiters and married individuals (Axinn and Thornton 1992; Clarkberg, Stolzenberg, and Waite 1995), though it is unclear to what extent these differences remain, as cohabitation has become an expected relationship experience (Manning, Longmore, and Giordano 2007).

Compositional differences between individuals in cohabiting and married relationships have been frequently attributed to the selective entry of individuals into cohabitation and marriage. That different individuals or relationships are more likely to transition to marriage is a threat to estimating marital effects, and the bias from selection into marriage is possibly increasing as marriage has transitioned to a “capstone” for adult status following educational and career attainment. Selection into marriage can also mislead researchers as to the effect of cohabitation. For example, cohabitations appear more violent than marriages in part because violent cohabitations are less likely to transition to marriage, resulting in a concentration of intimate partner violence within cohabiting relationships (Kenney and McLanahan 2006).

An additional challenge to cohabitation as a marital counterfactual is the heterogeneity of cohabitation itself. That cohabitation varies in meaning and purpose means that comparing marriage and cohabitation may conflate the heterogeneous composition of cohabiting relationships with actual marital

benefits. There is evidence that observed differences between marriage and cohabitation might be, in some cases, attributable to heterogeneity within cohabitation rather than reflecting an effect of marriage. For example, married individuals are more likely to combine finances than are cohabiters, but this difference is driven by variation within cohabiting couples rather than by differences between marriage and cohabitation, as cohabiting couples hoping to marry have similar financial decisions to married couples (Lyngstad, Noack, and Tufte 2011). Similarly, there is no difference in relationship quality between married individuals who cohabited before marriage and cohabiting individuals who plan to marry, though cohabiting relationships without marriage plans overall have lower relationship quality than do marriages (Brown, Manning, and Payne 2017).

### ***Sexual Concurrency and Marriage***

Another difference between cohabiting and married individuals is the likelihood of remaining sexually exclusive. Despite similar expectations of sexual exclusivity (Treas and Giesen 2000), individuals in cohabiting relationships are more likely than their married counterparts to have concurrent sexual relationships (Forste and Tanfer 1996; Treas and Giesen 2000). This study will determine if there is still a lower rate of sexual concurrency in married individuals and, if so, whether the marital state causes this difference. The current study adds to the literature both an updated estimate of the effect of marriage on sexual exclusivity and the most rigorous attempt at a causal estimate of marital benefits to date, with implications both for sexual exclusivity and the broader enterprise of disentangling marital selection and causation.

The first contribution of this study is updating current sexual concurrency differences between marriage and cohabitation. Previous estimates of the effect of marriage on sexual concurrency (e.g., Forste and Tanfer 1996; Treas and Giesen 2000) are derived from data collected in the 1980s and early 1990s. In the decades since these data were collected, changes in American family structure—primarily the normalization of cohabitation—have continued. These changes challenge preexisting comparisons and make it unclear whether marriage remains a more sexually exclusive relationship context.

The second contribution of the current study is providing a test of the effect of marriage on sexual concurrency. Concerns over selection into marriage or cohabitation threaten a causal interpretation of previous sexual concurrency findings. In both Treas and Giesen (2000) and Forste and Tanfer (1996), causation has been understood via “robust dependence” (Goldthorpe 2001) and the selectivity of marriage has been addressed only through the inclusion of a limited set of controls. To better capture an effect of marriage on sexual concurrency, the current study incorporates various measurement, sampling, and design decisions to mitigate threats to causal inference. In addition to better understanding sexual concurrency, determining if marriage increases sexual exclusivity has broader implications; sexual concurrency provides a unique vantage point to determine if marriage influences individual behavior. By focusing on sexual

concurrency as the outcome of interest, I minimize threats to isolating an effect of marriage on individual behavior that have challenged prior work. Thus, by estimating the effect of marriage on sexual concurrency, I aim to test whether marriage creates behavioral change.

## Method

### *Approach*

I seek a conservative test of whether marriage “matters” by examining the effect of marriage on an individual’s likelihood to engage in concurrent sexual relationships. I use data on sexual concurrency reported by individuals from their ongoing or most recent (i.e., concluded) relationships. This study is uniquely positioned to address the threats to identifying marital effects: the heterogeneity of cohabitation, expectation differences between contexts, differences in relationship dissolution, and selective entry into cohabitation and marriage.

That cohabitation is a heterogeneous relationship context suggests that observed average differences between cohabitation and marriage could result from variation within cohabitation rather than an effect of marriage. Focusing on sexual exclusivity addresses this potential source of bias, as the almost universal expectation of sexual exclusivity in cohabitation (Treas and Giesen 2000) means that variation in exclusivity expectations across cohabiting unions is minimal.<sup>2</sup> The second threat to identifying marital effects by comparing marriages and cohabitations is that this comparison may conflate the effect of relationship context with differences in expectations that accompany these relationship forms. Focusing on sexual concurrency also mitigates this threat; as sexual exclusivity is expected in both marriage and cohabitation (Treas and Giesen 2000),<sup>3</sup> expectation differences between marriage and cohabitation cannot explain observed differences in sexual concurrency. The third threat to causal identification is that observed behavioral differences between cohabitation and marriage could be caused by differential responses in relationship dissolution. For example, if marriages end more frequently following an affair than do cohabitations, then examining current relationships would uncover a protective marital effect. To address this threat, I include individuals who report on a concluded relationship so that context differences in dissolution do not bias estimates of marital effects.

The final threats relate to the non-random exposure of individuals to cohabitation or marriage. As individuals select into relationships, relationship status is likely endogenous to the model of relationship behaviors, biasing estimates of marital effects. There are two possible sources of selective entry into relationship context: cohabitation and marriage. To address the former, I limit the sample to unions that experienced non-marital cohabitation. As everyone in the sample is reporting on a relationship that involved cohabitation, selection into cohabitation based on characteristics also associated with concurrency is not a threat. To address the non-random exposure of individuals to marriage, I use an instrumental variable to estimate a binary choice model that predicts the effect of

marriage on the likelihood of engaging in concurrent sexual relationships. The proposed design elements, taken together, provide a conservative test of the effect of marriage on individual behavior.

## **Data**

For the current study, I use data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health is a nationally representative sample that has followed more than 20,000 individuals, initially sampled as adolescents in grades 7–12 in 1994–1995 in the United States, into adulthood (Harris et al. 2009). The respondents were followed in three additional in-home interviews in 1996 (Wave II), 2001–2002 (Wave III), and 2008–2009 (Wave IV), when 15,701 of the original respondents were most recently interviewed.

Add Health is an ideal dataset for assessing the effects of marriage on sexual exclusivity for two reasons. First, it contains a rich set of individual and contextual data across the early life course, allowing me to control for many previously identified predictors of sexual concurrency. Second, using a sample with such a narrow age range (age 24–32 in Wave IV) limits potential confounding due to the historic shifts in cohabitation norms and practices (Guzzo 2014). These data allow me to provide a contemporary assessment of marriage effects for a recent cohort of young adults, a benefit given the age of previous research concerning marital effects on sexual concurrency. Furthermore, the age range of the Add Health sample is ideal for studying relationship dynamics, as it covers a “demographically dense” life period (Rindfuss 1991).

As part of the Wave IV survey, respondents are asked information about their current or most recent romantic relationship. I construct an analytic sample from all individuals with a current or most recent reported relationship that was either a cohabitation or marriage, including relationships that have ended. This sample construction allows me to overcome survivorship bias at the level of relationships, a significant limitation of many studies examining differences between marriage and cohabitation. I also exclude individuals reporting on a marital relationship who did not cohabit with their partner prior to marriage. This choice ensures that observed differences between married and cohabiting individuals are not from selection into cohabitation. To determine whether this decision may limit the generalizability of my results, I also tested the same models using a sample that also included marital relationships without premarital cohabitation, which yielded the same findings as presented below (appendix table 2). I present findings from the more restricted sample for the sake of analytic clarity. The final analytic sample consists of 7,739 individuals with complete information on all variables: 3,502 individuals reporting on cohabiting relationships and 4,237 individuals reporting on marriages.

## **Measures**

The reliability and accuracy of sexual behavior measures has long concerned researchers. Though studies rely almost exclusively on retrospective self-reports of

sexual behavior and sexual activity, these measures struggle due to retrospective recall and self-report bias of sensitive topics (for review, see [Schroder, Carey, and Venable \[2003\]](#)). Evaluations of existing methods have identified strategies to improve the accuracy of measuring sexual behavior, in particular respondent anonymity and self-administration of questionnaires ([Durant, Carey, and Schroder 2002](#); [Schroder, Carey, and Venable 2003](#)). Specific to the measurement of sexual exclusivity, computer assisted self-interview (CASI), where the respondent confidentially replies to questions directly through the computer, is demonstrably more effective than direct response to an interviewer ([Whisman, Gordon, and Chatav 2007](#)).

Measurement of sexual concurrency in the Add Health study uses many of the known “best practices” for evaluating sexual behavior in surveys. Add Health uses CASI for assessing sexual behavior; both confidential self-administration of the relationship details section and computer assistance improve the accuracy of measurement. Additionally, participants have a long-standing relationship with the Add Health study. Measures from the survey are consistent with participant trust in the survey; only 7 percent of all contacted individuals declined to participate in Wave IV for any reason, and less than 1 percent of all respondents refused to answer questions on sexual concurrency.

I operationalize sexual concurrency through a dichotomous item that asks the individual about their current or most recent relationship: “During the time you and [partner] have had a sexual relationship, have you ever had any other sexual partners?” As Add Health includes no information about the specific expectations of sexual exclusivity within the respondent’s relationship, I refer to this reported behavior as sexual concurrency rather than sexual infidelity. Measuring sexual concurrency as an incidence rather than frequency measure increases the measure reliability (for review, see [Catania et al. \[1990\]](#)) and is preferable to calculating concurrency from a calendar of all previous sexual relationships ([Nelson et al. 2007](#)). Though the incidence measure does have attractive features, it limits the ability to temporally situate the behavior within the relationship. An analysis of this limitation and robustness checks of model outputs to measurement imprecision are presented in the Results section.

In all analyses, I control for previously identified correlates of sexual concurrency (for a review, see [Blow and Hartnett \[2005\]](#); for additional controls, see [Adimora et al. \[2002\]](#); [Adimora, Schoenbach, and Doherty \[2007\]](#); and [Lalasz and Weigel \[2011\]](#)). These controls include respondents’ demographic characteristics: sex, age at the start of the romantic relationship, race, and educational attainment (measured as dummy variables for less than high school, high school, some education past high school, or at least a bachelor’s degree). I control for religious participation (attend church services at least once a week), religious beliefs (faith is very important for daily life), drug usage (ever used intravenous drugs), impulsivity (frequently distracted), perceived attractiveness (interviewer’s rating of the respondent’s physical appearance as “Very Attractive”), and employment status (respondent is currently working over ten hours a week for pay). I also include controls for the family environment of the respondent’s early life residence (married biological parents, married biological and stepparent, or

other family form). To account for the time an individual was exposed to the risk of sexual concurrency, models control for relationship duration, measured as years since the initiation of the relationship to either the interview date (current relationships) or date of relationship dissolution (ended relationships).

While relationship satisfaction or goals are relevant to the sexual exclusivity decision, I do not include them in the current model. For obvious reasons, a general measure of relationship satisfaction is available only in ongoing relationships. Recall that to address non-random relationship exit, I include respondents who are not in a relationship at the time of the interview. However, sensitivity checks (not shown) that incorporate overall relationship satisfaction find the same substantive results as presented below.

## **Model**

To estimate the effect of marriage on the likelihood of engaging in sexual concurrency, I fit two models. The first is a probit regression predicting an individual's reported concurrency as a function of relationship type and controls. Estimates from this model may be threatened by omitted variables (e.g., a characteristic of individuals or relationships makes them simultaneously more likely to transition to marriage and remain sexually exclusive) or reverse causation (e.g., sexual concurrency inhibiting the transition from cohabitation to marriage). In either case, the association between marital status and the model's error term will result in inconsistent estimates of the association between sexual concurrency and marriage.<sup>4</sup>

To address this concern, I use an instrumental variable to assess the impact of marital status on sexual exclusivity. A number of instrumental variables have been previously proposed for studying marriage, such as state variation in divorce laws (Dee 2003; Gruber 2004) or regional indicators and educational heterogamy (Manski et al. 1992). As Ribar (2004) notes in his discussion of marital effects, the difficulty lies not so much in identifying possible instruments, but rather in "coming up with suitable instruments." Suitable, in the context of instrumental variables, means that the instrument is both strongly associated with the independent variable and otherwise theoretically uncorrelated with the dependent variable.

I employ as an instrument for relationship type, married or cohabiting, the proportion of adults of the respondent's sex who were married and living with a spouse in the respondent's adolescent (Wave I) census block of residence. My instrument corresponds to normative neighborhood modeling of marital behavior during adolescence, which sociologists have long considered relevant to an individual's subsequent relationship and family experiences (Crane 1991; Hogan and Kitagawa 1985; Wilson 1996). Individuals raised in an environment where most adults are married are more likely to view marriage as a desirable social norm. As such, it is relevant to the individual's subsequent likelihood of marrying. Analysis of the first-stage model of a two-stage least-squares model ( $F = 19.13$ ) suggests that the instrument is strong by both conventional standards ( $F > 10$ ) and more formal critical values (Stock and Yogo 2002).

While the instrument is associated with an individual's subsequent likelihood of marrying, it is unlikely to affect differences in the adult likelihood of engaging in concurrent sexual relationships, conditioned on covariates including respondent's religiosity and family structure. Though the instrument has a significant bivariate relationship with sexual concurrency, there is no relationship between the two after including all controls and marital status. The validity of the instrument is supported by three additional pieces of evidence: 1) the timing of the measure and included controls speak to the threat of selection into neighborhoods; 2) the instrument is conditionally unrelated to other sexual behaviors; and 3) the result of an over-identification test is consistent with valid instruments. First, because the instrument is measured when the respondent is a teenager with minimal choice in residential location, the individual is not selecting into the neighborhood based on individual characteristics. Though parents may select into neighborhoods based on their own characteristics, I include controls for family structure and other characteristics to address this threat. Second, I find no evidence that the instrument is associated with other sexual behaviors. If exposure to low-marriage neighborhoods also instilled permissive sexual views, then my instrument may be invalid. Instead, I find that there is no significant association between the instrument and other sexual behaviors (age of first sex, total number of sexual partners, ever exchanging sex for money, and number of "one-night stands"), net of controls. Finally, there are tests often used to support instrument validity that rely on over-identification of model parameters (having more instruments than endogenous variables). Though the model is exactly identified, including a second instrument—the proportion of adults of the opposite sex of the respondent who were married in the respondent's adolescent census block of residence—enables me to estimate this statistic. Results of the Sargan test (Sargan 1958) are consistent with the joint validity of the instruments.

The nature of the model, with dual dichotomization of outcome and treatment, challenges common forms of instrumental variable estimation: linear probability model, control function, and maximum likelihood estimation. The linear probability model (e.g., two-stage least-squares regression) assumes that the regressor and outcome are continuous. Violating this assumption can result in fitted choice probabilities outside the possible range (0–1) and, due to the binary nature of the outcome, an error term that depends on the set of regressors. While some (Angrist and Pischke 2009) have argued that the method is robust to these limitations, recent work (Lewbel, Dong, and Yang 2012) documents cases in which the linear probability model is unable to recover even the correct direction of the average treatment effect. Furthermore, simulation studies suggest that the linear probability model in the presence of binary outcome/binary endogenous regressor models may have "standard errors too large for meaningful hypothesis testing" (Chiburis, Das, and Lokshin 2012). The control function, while respecting the functional form of the outcome, is similar to the linear probability model in that it assumes a continuous endogenous regressor and provides inconsistent estimation in the face of binary, discrete, or categorical endogenous regressors (Lewbel, Dong, and Yang 2012). While potentially more efficient than the linear probability model, maximum likelihood estimation is

sensitive to model misspecification (i.e., violations of joint normality of the error term) (Chiburis, Das, and Lokshin 2012), which appears to be the case in these data.<sup>5</sup>

To overcome these limitations, I turn to a new procedure, the special regressor model. This model, first described by Lewbel (2000) and further elaborated by Dong and Lewbel (2015), provides a possible solution to estimating models that include binary outcomes and endogenous regressors. Prior work on binary choice models has employed the special regressor to, among other things, estimate the impact of conditional cash transfer programs on maternal and sibling childcare (Dubois and Rubio-Codina 2012), the effect of parental income on being held back in elementary school (Maurin 2002), and the effect of PhD funding source on subsequent job type (Blume-Kohout and Adhikari 2016). In addition to a suitable instrumental variable, the researcher identifies an additional “special regressor” that is uncorrelated with the second-stage model error term, appears as an additive term in the second-stage model, has a continuous distribution, and has a large support (which is roughly defined as the set of values for which the distribution of the special regressor is non-zero is wide). For special regressors meeting all these conditions, models can be estimated in a manner that, among other properties, constrains effects to lie within possible bounds.

I use a lagged measure of Body Mass Index (BMI) calculated from self-reported height and weight during the respondent’s adolescence (Wave I) as the special regressor. Adolescent BMI meets all requirements of the special regressor. BMI is measured as a continuous variable with a wide range (11.5 to 54.2). The support of the special regressor is sufficient; the spread of adolescent BMI (standard deviation of 4.5) exceeds that of  $X'\hat{\beta}$  (standard deviation of 0.8). Above-average fat mass is associated with lower perceived attractiveness for both men and women (Tovee et al. 1998; Tovéé et al. 1999),<sup>6</sup> which can increase the difficulty of attracting additional sexual partners (e.g., Eastwick and Finkel 2008; Fisman et al. 2006). In contrast to the theorized curvilinear relationship between BMI and attractiveness, I model BMI as a continuous, additive predictor of attractiveness. Modeling BMI linearly is reasonable given both the rarity of underweight individuals (about 1 percent of Add Health Wave IV respondents are below the “healthy” range) and that sensitivity checks of model specification, both by modeling BMI as a higher-order function (e.g., including BMI<sup>2</sup>) or by excluding individuals with “underweight” BMI, do not indicate an improvement in model fit. Both the measure’s timing and the inclusion of other correlates support BMI as an exogenous predictor of sexual exclusivity. Height and weight values were collected in adolescence, prior to the initiation of the reported relationship. Including a range of relevant controls, such as education, family background, and impulsivity, also limits the potential correlation between adolescent BMI and adult concurrency. Consistent with common practices, I center and reverse the special regressor so that increasing values correspond to greater likelihood of sexual concurrency. While not a formal condition, previous work has suggested that special regressors with kurtotic distributions are desirable for estimator efficiency (Lewbel, Dong, and Yang 2012); the distribution of BMI in the sample is kurtotic (6.01).

With lagged BMI as the special regressor ( $V$ ), we can write the binary choice model (Eq. (1)) for the dichotomous measure of sexual concurrency ( $D$ ) as a function of both endogenous ( $X^e$ ) and exogenous ( $X^o$ ) covariates.

$$D = I(X^e\beta_e + X^o\beta_o + V + \varepsilon \geq 0) \quad (1)$$

Having defined lagged BMI as a “special regressor,” the estimator proceeds in three rough steps. First, we obtain the residuals ( $U$ ) from regressing lagged BMI on the instrument, neighborhood marital context in adolescence, and all covariates. The second step involves constructing a new variable  $T$  defined as

$$T = \frac{D - I(V \geq 0)}{f(U)} \quad (2)$$

where  $f(U)$  is a probability density function for  $U$ . Finally, as demonstrated by [Dong and Lewbel \(2015\)](#),  $T$  can be used in place of  $D$  to obtain estimates of beta, particularly the estimate for marital state, using a standard two-stage least-squares estimator with  $T$  regressed on marital status and the other exogenous covariates with the adolescent neighborhood marital context as the instrument. See [Dong and Lewbel \(2015\)](#), [Lewbel, Dong, and Yang \(2012\)](#) and [Lewbel \(2014\)](#) for a derivation, additional technical details, and proofs regarding the properties of this estimator and obtaining marginal effects.

Models are estimated using Stata 15 ([StataCorp 2017](#)). The special regressor model is estimated with the “`sspecialreg`” command developed by [Baum \(2012\)](#). As the current special regressor estimating program does not account for complex survey design, the results reported below do not account for the complex survey design of the Add Health study. Probit models incorporating the Add Health survey design found similar results to those presented here (shown in appendix table 3).

## Results

A description of the sample is available in table 1. The analytic sample is relatively evenly divided between cohabiting relationships (45.3 percent) and marriages (54.7 percent). While the duration of these relationships is quite long, on average about six years, it is also quite variable. The sample is relatively diverse on racial and educational background. The second and third panels of table 1 break down the sample by relationship type to explore raw differences between married and cohabiting individuals. As expected, the subset of individuals in cohabiting unions is more racially diverse, less educated, and less religious than those in marriages.

Approximately 20 percent of respondents have had an additional sexual partner during their current or most recent relationship. Stratifying by marital status, I find that cohabiters are more likely to report sexual concurrency; 24 percent of individuals in cohabiting unions report having such a sexual relationship, while 19 percent of married respondents do so. The estimated prevalence of marital

**Table 1. Sexual Concurrence and Predictive Factors: Descriptive Statistics (*N* = 7,739)**

	Full sample		Marital relationships		Cohabiting relationships	
	Mean or proportion	S.D.	Mean or proportion	S.D.	Mean or proportion	S.D.
<b>Relationship characteristics</b>						
Sexual concurrency	0.21		0.19		0.24	
Relationship context						
Marriage	0.55					
Cohabitation	0.45					
Relationship duration (years)	5.97	3.72	7.42	3.44	4.21	3.26
Current relationship	0.87		0.97		0.75	
<b>Respondent characteristics</b>						
Age at relationship start	28.31	2.08	28.80	1.75	27.72	2.28
Female	0.54		0.56		0.52	
Race/Ethnicity						
Non-Hispanic White	0.58		0.64		0.51	
Non-Hispanic Black	0.19		0.14		0.26	
Hispanic	0.14		0.15		0.14	
Asian	0.05		0.05		0.06	
Other race	0.03		0.03		0.03	
Educational attainment						
College or more	0.28		0.29		0.25	
Some college	0.46		0.47		0.46	
High school	0.18		0.17		0.19	
Less than high school	0.08		0.07		0.10	
Very impulsive	0.16		0.15		0.16	
Respondent is attractive	0.08		0.08		0.08	
Currently employed	0.66		0.65		0.68	
Ever used intravenous drugs	0.01		0.00		0.01	
Ever incarcerated	0.08		0.05		0.11	
Faith is important	0.08		0.09		0.07	
Weekly church attendance	0.13		0.16		0.09	
Residential family structure (Wave I)						
Two biological parents	0.50		0.52		0.46	
Parent/Stepparent	0.20		0.21		0.20	
Other family structure	0.30		0.27		0.34	
Body Mass Index (Wave I)	22.64	4.45	22.72	4.43	22.55	4.48
<i>N</i>	7739		4237		3502	

sexual concurrency is in line with both contemporaneous estimates (2008: 17 percent) and the relatively stable historical trends (1991–2016 range: 14.6 percent to 19.2 percent) from the General Social Survey (Smith et al. 2016).<sup>7</sup> Though married Add Health respondents in this sample are slightly more likely to report sexual concurrency than married GSS respondents born in the same approximate years (13 percent, 95 percent confidence interval: 11–15 percent), some of this difference is likely attributable to features of my analytic sample (i.e., inclusion of concluded relationships and exclusion of relationships without premarital cohabitation). The estimated prevalence of sexual concurrency in cohabiting unions (24 percent, 95 percent confidence interval: 23–25 percent) is like that in other studies, specifically Forste and Tanfer (20 percent, 95 percent confidence interval: 14–26 percent) or, more recently, Mark et al. (17 percent, 95 percent confidence interval: 10–23 percent). Some 15.5 percent of individuals reporting sexual concurrency are in concluded rather than ongoing relationships, but there is no significant difference in sexual concurrency within relationship type by dissolution status. Results are robust to excluding respondents who report on concluded rather than current relationships (appendix table 4).

Panel A of table 2 provides results of the probit model predicting sexual concurrency. For ease of interpretation and comparability between models, the results are presented as marginal effects estimated at the mean of all other values.<sup>8</sup> As such, for factor variables, the presented value corresponds to a difference in the predicted probability of the outcome between the binary states (e.g., the differences in the predicted probability of sexual concurrency for those married and unmarried). I find that younger, male, Black, impulsive, and previously incarcerated individuals are more likely to have concurrent sexual relationships. The respondent's adolescent family home environment is also associated with sexual concurrency; respondents who lived in stable, married-parent families are less likely to engage in sexual concurrency than those raised in other types of families. Married individuals are significantly more sexually exclusive than are cohabiters—marriage is associated with a six-percentage-point decrease in the probability of reporting sexual concurrency.

To test whether these naïve results provide evidence of a marital effect or merely reflect selection into marriage, I next estimate the instrumental variables model using the special regressor. The results of this model are shown in panel B of table 2. As this model addresses selective entry into marriage, a significant coefficient provides strong support for a potential marital effect. All else equal, being married rather than cohabiting reduces the absolute likelihood of engaging in sexual concurrency by eight percentage points. For context, the marital effect is roughly two-thirds the coefficient for incarceration observed in the probit model and incarceration is a strong predictor of sexual concurrency (Adimora, Schoenbach, and Doherty 2007).

The coefficients for control variables support the validity of this model. Except for attractiveness, whose interpretation is unclear because adolescent BMI is the special regressor, I re-create associations between sexual concurrency and characteristics previously described in the literature, notably relationship duration

**Table 2. Marginal Effects Estimated from Models Predicting Reported Sexual Concurrency within Relationships ( $N = 7,739$ )**

	Panel A: Naïve probit model			Panel B: Special regressor model		
	$dy/dx$	SE	$p$	$dy/dx$	SE	$p$
Marriage	-0.064	0.011	***	-0.081	0.026	**
Relationship duration	0.008	0.003	**	0.014	0.001	***
Age at start of relationship	-0.009	0.002	***	0.007	0.001	***
Female	-0.059	0.001	***	-0.022	0.005	***
Race/Ethnicity						
Non-Hispanic White (ref)						
Black	0.110	0.015	***	0.027	0.021	
Hispanic	0.020	0.014		0.011	0.006	+
Asian	0.003	0.022		-0.012	0.009	
Other race	0.023	0.030		0.004	0.014	
Educational attainment						
College or more (ref)						
Past high school	0.030	0.012	*	0.012	0.009	
High school	-0.004	0.015		0.005	0.010	
Less than high school	0.017	0.020		0.017	0.011	
Very impulsive	0.066	0.014	***	0.021	0.006	**
Currently employed	0.007	0.010		0.001	0.004	
Intravenous drug use	0.020	0.061		-0.006	0.025	
Ever incarcerated	0.135	0.020	***	0.022	0.016	
Adolescent residential family structure						
Two biological parents (ref)						
Stepparent family	0.020	0.013		0.009	0.005	+
Other family structure	0.033	0.012	**	0.008	0.007	
Very attractive	0.009	0.017		-0.020	0.006	**
Faith is important	-0.013	0.017		0.001	0.007	
Attend church weekly	-0.009	0.015		0.022	0.006	
BMI (Wave I—Reverse coded, mean centered)				0.019	0.004	***

+  $p < 0.1$ ; \*  $p < 0.05$ ; \*\*  $p < 0.01$ ; \*\*\*  $p < 0.001$

(Adamopoulou 2013), impulsivity (Lalasz and Weigel 2011), and respondent demographics (Atkins, Baucom, and Jacobson 2001). Taken together, these associations help validate the presented model for sexual concurrency.

## ***Sensitivity to Measurement Precision***

One concern with this study is the sexual concurrency measure. Asking respondents if they had sexual intercourse with any other individual during their current or recent cohabiting or marital relationship limits my ability to situate the behavior within the relationship. For example, married respondents could have engaged in concurrent sexual behavior exclusively during dating or cohabitation. This inability poses three distinct challenges to this study: that estimated marital effects capture the effect of sexual concurrency on relationship context rather than the effect of marriage on sexual exclusivity (i.e., reverse causation), that the estimated prevalence of cohabiting or marital concurrency is incorrect, and that the estimated marital effect is incorrect.

Two features of the current study protect against reverse causation. First, I include concluded relationships. This means differences in rates of relationship dissolution following sexual concurrency in cohabitation or marriage do not bias the comparison of married and cohabiting individuals. Second, the use of an exogenous instrumental variable is a well-documented method to address potential endogeneity caused by reverse causation.

Because an affirmative response to the sexual concurrency question could indicate concurrency either during the context of interest or outside of it, the estimated prevalence of sexual concurrency should be considered an upper bound on the prevalence of concurrent sexual behavior. Despite this overestimate, estimates of sexual concurrency are consistent with prior work, as described above. This suggests any overestimate of the population value is likely minor.

Finally, measurement imprecision could result in incorrect estimates of the marital benefit. Though overestimating the prevalence of sexual concurrency in marriage necessarily implies underestimating the effect of marriage on sexual concurrency, I provide two additional tests of model robustness. First, I estimate a model comparing cohabiting to married individuals who did not have a premarital cohabitation with their spouse. Without the premarital cohabitation, these individuals have one fewer relationship context in which concurrent sexual behavior may have occurred. In this analysis (results shown in appendix table 5), I again find that marriage decreases sexual concurrency relative to cohabitation. Second, I tested the implications of measurement imprecision on my model estimates in a series of simulations using the analytic sample and methods described above (for more information on simulation procedures and results, see the online supplemental material). I find that representing all concurrent sexual behavior reported by married individuals as occurring during the marriage, as presented models do, underestimates the effect of marriage on sexual concurrency (appendix table 6). My findings are also robust to measurement imprecision in both relationship contexts (appendix table 7). Though uncertainty in the timing of sexual concurrency may affect the point estimate I present above, these simulations suggest that it does not appear to alter the substantive finding of marital protection. As such, these simulation results support this study's claim to be a conservative test of the effect of marriage on individual behavior.

## Discussion

Twenty years ago, in her presidential address to the Population Association of America, Linda Waite (1995) asked researchers, “Does marriage matter?” Revisiting this question with regard to sexual concurrency, this study sought to determine if married individuals remain less likely to engage in sexual concurrency than cohabiters and, if so, whether this difference reflected an effect of marriage.

First, this paper updated the literature on the concurrency difference between cohabitation and marriage. Though family context has continued to change, my results are in the same direction as prior work, albeit of lower magnitude. I find that cohabitation increases the odds of sexual concurrency by 57 percent relative to marriage,<sup>9</sup> in comparison to 400 percent (Forste and Tanfer 1996) or 100 percent (Treas and Giesen 2000) in prior work. This difference could reflect the very low estimates of marital concurrency in these prior studies (4 percent and 8 percent, respectively) or the erosion of differences between marriage and cohabitation in the United States resulting from continuing changes in the acceptability, desirability, and experience of both cohabitation and marriage over this period. Differences from existing estimates could also reflect the consequences of the paper’s second aim, testing whether marriage reduces concurrent sexual behavior. Addressing the endogeneity of relationship status in the current study means my estimates are less likely to embed either unmeasured correlates or reverse causation than those of prior work. For example, selection of better (i.e., less likely to “cheat”) relationships into marriage would have resulted in positively biased estimates of marital effects in prior work, but this selection has been mitigated by the current study’s methodological design. Taken together, my findings suggest that married individuals are less likely to engage in sexual concurrency than are cohabiters and that this benefit is likely due in part to a marital benefit.

In answering these questions, this study makes two distinct contributions, one methodological and one substantive. Methodologically, the current study represents, to the best of my knowledge, one of the first uses of the special regressor approach in the sociological literature and its first application to marriage. Despite their benefits for causal inference, instrumental variables “are underutilized tools to address common problems in sociological research” (Bollen 2012). One difficulty these methods face is that sociologists often confront dichotomous treatments and outcomes, which some instrumental variable approaches may be ill equipped to handle. In contrast, the special regressor is well suited for models such as these. The successful use of the special regressor suggests a means by which sociologists can leverage instrumental variables for causal estimates in situations with bounded outcomes and treatments. Additionally, the strength of the instrument used in this study supports prior research on the importance of early life exposures for adult outcomes—the “long arm of childhood” in life course research (Hayward and Gorman, 2004)—and highlights the potential of employing previously enumerated early life family exposures to examine causal influences in the family domain.

Substantively, this paper finds strong evidence that marriage reduces the risk of sexual concurrency, relative to cohabitation. A protective marriage effect suggests additional consequences of marrying rather than cohabiting. For example, sexual concurrency increases the risk of relationship dissolution (Frisco, Wenger, and Kreager 2017). If cohabitation increases the risk of sexual concurrency relative to marriage, then cohabitations would be more likely to dissolve. This difference could help explain the relative instability of cohabiting unions (e.g., Lau 2012). And the consequences ripple further; because of higher rates of sexual concurrency, cohabiters are more likely to experience the mental, physical, emotional, or financial consequences of relationship dissolution. Additionally, in finding that marriage reduces sexual concurrency, this study also provides evidence that marriage changes behavior. Though generalizing from sexual concurrency to behaviors of policy interest requires additional research, demonstrating that marriage can induce behavioral change supports a necessary prerequisite for marriage as a policy target for promoting social welfare.

Throughout the text, I have detailed both technical checks and sensitivity analyses where appropriate; however, additional limitations should be noted. First, there is no single “silver bullet” for identifying causation in observational data. I have combined measure selection, data restrictions, and analytic techniques to provide some of the strongest possible evidence with observational data that marriage does change behavior, but we should always be mindful of the limits to understanding causal pathways in non-experimental data. Second, relying on self-reports of sexual behavior introduces potential threats from response bias. Though relative underreporting by cohabiters or similar underreporting by married and cohabiting individuals could result in a less precisely estimated protective effect of marriage, more troubling is that response bias could lead me to overestimate the effect of marriage if married individuals selectively underreport sexual concurrency. Focusing on a behavior with similar expectations in marriage and cohabitation collected using a CASI survey limits, but does not remove, this potential threat. Gender gaps in reported sexual concurrency between marriage and cohabitation, a strong gauge of response biases in sexual data (for a review, see Schroder, Carey, and Vanable [2003]), do not uncover evidence of excess marital underreporting. Furthermore, additional simulations (see appendix figure 3) suggest my results would be robust to even high levels of marital underreporting. Thus, though I cannot rule out the potential of response bias through underreporting by married individuals, there is little evidence it exists in the data and my results are generally robust to this threat. Finally, this study focuses on a single cohort. As such, the results may not be generalizable to other cohorts. For example, the emergent view of marriage as a “capstone” marking a successful transition to adulthood may cause behavioral changes following marriage in recent cohorts to differ from those in prior cohorts. Furthermore, the focus on a single cohort may neglect potential changes in the expectations of sexual exclusivity over time, particularly in cohabitations. Prior estimates found nigh-universal expectations of sexual exclusivity, but these standards may have loosened since. Though the simulation analyses suggest that relaxing of cohabitation expectations would have had to vastly outpace those of

marriages and there is no indication of such seismic shift in expectations, additional work is needed on sexual expectations in current relationships.

What might these findings mean for the future? American young adults are increasingly likely to enter into cohabiting unions (Copen, Daniels, and Mosher 2013) and to delay marital entry (Copen et al. 2012). Social trends that lead individuals to increase time spent in cohabitation at the expense of marriage would suggest increasing risks of sexual concurrency and therefore its downstream consequence (e.g., STI transmission or relationship instability) within the overall population. However, cohabitation remains, in some ways, a stratified experience; for example, race and socioeconomic status are important predictors of cohabitation status. Consequently, the higher rates of sexual concurrency in cohabitation may lead cohabitation to widen existing disparities.

In addition to insight into possible future trends, this work highlights directions for future research. First, as noted previously, there are open questions about how views toward sexual exclusivity may have changed in cohabiting unions that should be answered. Second, this approach to causal estimation could be expanded to other potential marital effects, including outcomes with potential policy implications. Finally, this paper showcases the potential of using early life experiences as potential instruments for estimating causal effects with bounded treatments and outcomes more generally. Using tools like the special regressor, in combination with available longitudinal data, sociologists have the potential to test whether other individual statuses are, like marriage, special.

## Notes

1. Though not universal, recent Census estimates suggest only 3 percent of married individuals live separately.
2. As an additional robustness check to cohabitation variation, I fit my model in a sample limited to “serious” cohabitations (i.e., those that reported the odds of either marrying or staying together permanently were either “pretty good” or “almost certain”). I find the same substantive results as I present below (see appendix table 1).
3. For details on this point, see the online supplemental material.
4. In a bivariate probit model jointly estimating marriage and infidelity, I find evidence that marriage is an endogenous regressor in the model for sexual infidelity, as the correlation between model errors is significantly different from zero ( $\rho = 0.29$ ,  $SE = 0.13$ ).
5. Rejection of the null in the Rao score test developed by Murphy (2007) suggests that observed data is significantly unlikely to have arisen via a joint normal data-generating process.
6. Alternatively, high BMI may be indicative of high muscle mass and, particularly in men, be perceived as attractive by potential partners. However, very few respondents fit this athletic profile. In men who were trying to build muscle mass through exercise or weightlifting in Wave I, the average BMI is 20 and only 8 (0.2 percent of men in the sample) would be classified as obese.
7. For details on these trends, see appendix figures 1 and 2.
8. For the special regressor model, bootstrapped standard errors are computed for the marginal effects with ten replications.

9. With a baseline sexual concurrency probability of 19 percent in married individuals and an 8 percent increase in probability associated with cohabitation rather than marriage, the implied odds ratio for cohabiting rather than marrying for sexual concurrency is 1.57.

## About the Author

Brandon Wagner is an Assistant Professor of Sociology at Texas Tech University. His primary research interest is the intersection of health and family across the life course.

## Supplementary Material

Supplementary material is available at *Social Forces* online.

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