THE CONTEXT OF MARRIAGE AND CRIME: GENDER, THE PROPENSITY TO MARRY, AND OFFENDING IN EARLY ADULTHOOD*

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Marriage is central to theoretical debates over stability and change in criminal offending over the life course. Yet, unlike other social ties such as employment, marriage is distinct in that it cannot be randomly assigned in survey research to more definitively assess causal effects of marriage on offending. As a result, key questions remain as to whether different individual propensities toward marriage shape its salience as a deterrent institution. Building on these issues, the current research has three objectives. First, we use a propensity score matching approach to estimate causal effects of marriage on crime in early adulthood. Second, we assess sex differences in the effects of marriage on offending. Although both marriage and offending are highly gendered phenomena, prior work typically focuses on males. Third, we examine whether

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one's propensity to marry conditions the deterrent capacity of marriage. Results show that marriage suppresses offending for males, even when accounting for their likelihood to marry. Furthermore, males who are least likely to marry seem to benefit most from this institution. The influence of marriage on crime is less robust for females, where marriage reduces crime only for those with moderate propensities to marry. We discuss these findings in the context of recent debates concerning gender, criminal offending, and the life course.

Marriage is a central consideration in contemporary theories of crime and desistance (Giordano, Cernkovich, and Rudolph, 2002; Laub and Sampson, 2003; Sampson and Laub, 1993). From a life-course perspective, marriage suppresses offending by establishing informal social control (Sampson and Laub, 1990), undermining deviant ties (Warr, 1998), and limiting criminal opportunities through integration into normative environments (Laub and Sampson, 2003). In earlier theoretical work, before the articulation of a life-course perspective on crime, marriage was also observed as a central mechanism in the "aging out" of crime (Greenberg, 1977, 1983). Yet, even with several powerful statements on the importance of marriage in the etiology of crime, its causal status has been contested (Gottfredson and Hirschi, 1990; Wilson and Herrnstein, 1985) and extant research is equivocal. Some studies suggest that marriages inhibit criminal offending (Farrington and West, 1995; Laub, Nagin, and Sampson, 1998; Laub and Sampson, 2003; Sampson and Laub, 1993; Sampson, Laub, and Wimer, 2006), whereas others indicate no association (Giordano, Cernkovich, and Rudolph, 2002; Knight, Osborn, and West, 1977; Thornberry, Moore, and Christenson, 1985). Moreover, the study of marriage and crime is potentially more complicated than assessments of crime and other life-course experiences, such as work, as causal effects cannot be assessed through random assignment and experimental design. As such, identifying the true "treatment effect" of marriage is difficult.

Motivated by these issues, we pursue three objectives in this work. First, we further examine the association between marriage and crime using a propensity score matching approach. This approach is well suited to the study of causal effects in observational data because it allows for an examination of the marriage–crime relationship for married and unmarried respondents with equal likelihood of experiencing marriage in early adulthood.

Second, we investigate gender contingencies in the marriage–crime nexus. There are theoretical reasons to suspect that marriage influences offending differently for males and females. Selection into marriage is a highly gendered process (Oppenheimer, 1988), as is involvement in crime. Men are typically more involved in crime than women, thus increasing the

probability that men will marry less-deviant partners and be subjected to greater social control by female spouses (see Laub and Sampson, 2003: 45–46, and Sampson, Laub, and Wimer, 2006, for related arguments). Research also suggests that marriage is a particularly salient institution with particular benefits for men (Nock, 1998). If such relationships are a locus of social control, one implication is that marriage could be a turning point for men more so than for women. That proposition remains largely untested, as few analyses of marriage and crime have focused on females (save Giordano, Cernkovich, and Rudolph, 2002).

Third, we examine whether the association between marriage and crime is contingent on an individual's likelihood to marry. For instance, individuals with low propensities to marry, such as those with few adolescent relationships and higher rates of offending, may not benefit from marriages to the same extent as individuals with higher propensities to marry because they are less capable of forming the necessary attachments that render marriages effective social controls. On the other hand, marriage might matter the most for those who are least likely to marry. If those who have low propensities to marry also have elevated rates of delinquency, then that baseline allows for greater decline after marriage. A third possibility is that marriage has ubiquitous effects regardless of propensity to marry, meaning that marriage is a robust institution that is not contingent on earlier biographic experience. Such contingencies, or the lack thereof, bear on broader theoretical and empirical debates concerning stability and change in crime over the life course (Blumstein et al., 1986; Giordano, Cernkovich, and Rudolph, 2002; Gottfredson and Hirschi, 1990, 1995; Massoglia, 2006; Reiss and Roth, 1993; Sampson and Laub, 1993; Wilson and Herrnstein, 1985).

We proceed by reviewing extant research on marriage and crime before suggesting how the propensity to marry might be modeled in empirical research. We then turn to National Youth Survey (NYS) data to analyze 1) the extent to which marriage influences crime for samples of matched males and females, respectively, and 2) whether propensity to marry conditions the effect of marriage on crime. Finally, we discuss our results in the context of research on informal social control, gender, and crime in early adulthood.

MARRIAGE AND ADULT CRIME

Three general theses characterize extant research on marriage and crime. First, marriage may have no effect on offending. Knight, Osborn, and West's (1977) analysis of the Cambridge Study in Delinquent Development concluded that marriage has no demonstrable effect on criminal offending, although it may curtail other antisocial behavior (see also West,

1982). More recent analyses of data from the United States also indicate no significant relationship between marriage and crime (Giordano, Cernkovich, and Rudolph, 2002: 1012; Thornberry, Moore, and Christenson, 1985). These findings echo Wright and Wright's (1982: 54) earlier conclusion that "no clearly confirming set of findings has emerged from research to date that demonstrates that getting married. . .reduces the likelihood of criminal offenses."

A second body of research investigating intimate relationships more broadly suggests variable effects on offending. Osborn and West (1979), for example, found that some marriages exacerbate offending, particularly when individuals marry delinquent wives (see also Ouimet and LeBlanc, 1996; Shavit and Rattner, 1988). More recently, Simons et al. (2002) found that having an antisocial romantic partner exacerbates criminal behavior, both directly and through its effects on the quality of the romantic relationship and deviant friends. Such work suggests that the effect of marriage may depend on the quality of the relationship.

A third body of research suggests that marriage can reduce crime. Sampson, Laub, and Wimer's (2006) longitudinal analysis of 500 high-risk male adolescents shows that marriage reduces the odds of crime by 35 percent. Related work based on a matched sample of delinquent and nondelinquent Boston males (Glueck and Glueck, 1968) emphasizes marital quality. For example, Sampson and Laub (1990, 1993; Laub, Nagin, and Sampson, 1998) concluded that although marriage has no significant effect on crime, high levels of marital attachment are associated with lower rates of offending. Likewise, Farrington and West (1995) studied the effects of marital disruption on offending and concluded that it is the ability to sustain a marriage that fosters desistance from crime. From this perspective, the deterrent capacities of "good" and stable marriages are large and can constitute turning points in the criminal life course. That perspective is arguably the dominant paradigm in the study of marriage and crime (Laub, 2006).

Although a lack of consistent findings is significant in and of itself, three other issues suggest the need for further investigation of marriage and crime. First, much of the evidence showing an effect of marriage on offending comes from studies of respondents growing up and marrying in the 1950s and 1960s (for example, Farrington and West, 1995; Laub and Sampson, 2003; Sampson and Laub, 1990, 1993; Shannon, 1991; Shavit and Rattner, 1988). This period preceded the "divorce revolution" of the 1970s (Peterson, 1996; Weitzman, 1985) that reshaped the nature of marriage, perhaps increasing the importance of selection and decreasing the significance of postmarital socialization (Oppenheimer, 1988). Such change may alter the deterrent effects of marriages, "good" or "bad." Second, much of the research on the effects of marriage is based on non-random samples

(see, for example, Farrington and West, 1995; Giordano, Cernkovich, and Rudolph, 2002; Laub and Sampson, 2003; Sampson and Laub, 1990, 1993). Although such samples have the advantage of including more serious and frequent offenders, the population to which these samples generalize is not well understood. Finally, most studies focus solely on males (exceptions are Giordano, Cernkovich, and Rudolph, 2002; Simons et al., 2002), even though gender is an important component of the etiology of offending and a crucial comparative element in assessing theories of crime (Kruttschnitt, 1996; Simpson and Ellis, 1995; Uggen and Kruttschnitt, 1998). In addition, gender is a central organizing concept in the study of marriage (Oppenheimer, 1988; Waite and Gallagher, 2000). We thus see theoretical utility in examining the deterrent capacity of marriage for males and females with recent data that include identical measures of offending and background characteristics for both sexes.

EXPLAINING MARRIAGE AND CRIME

Research on marriage and crime is often interpreted through the lens of age-graded variation in social controls (Sampson and Laub, 1993). Building on control theory (Hirschi, 1969), crime results when an individual's bond to society is weak or broken. One key issue is the interconnection of individuals and social institutions over the life span. Early childhood and adolescent deviance likely influences later offending by weakening the formation of social bonds to school, work, and family in later life. Social bonds to adult institutions such as work and marriage, when they do occur, are important mechanisms of social control capable of altering a delinquent trajectory (Sampson and Laub, 1990). This age-graded theory of social control thus proposes continuity in deviant behavior from childhood to adulthood and yet leaves open the possibility of meaningful change in later life.

Life-course theories that emphasize the deterrent capacity of marriage disagree with assumptions implicit in other control theories (Gottfredson and Hirschi, 1990). Control theories in general maintain that individuals who lack social bonds are more apt to act in their own self-interest (Gottfredson and Hirschi, 1990; Hirschi, 1969), remain immune to the psychological and social "costs" of crime (Toby, 1957), and be less restricted by the controlling influences of normative institutions (Hirschi, 1969). Thus, for control theorists, a key question is why individuals with such characteristics would become bonded to normative institutions.

In response to that question, two answers have been proposed. First, some argue that the deterrent effect of adult institutions like marriage are spurious, the outcome of self-selection into good relationships on the part

of those who were never prone to criminal behavior. Gottfredson and Hirschi (1995; see also Gottfredson and Hirschi, 1990), for example, argue that apparent institutional effects of marriage or work are the outcomes of an underlying individual characteristic, low self-control, where those who are unmotivated to change are less likely to enter normative institutions and unlikely to be influenced by them.

Life-course criminologists, confronted with the clear continuity of deviant behavior over time, maintain that "turning points [like good marriages] are 'triggering events' that are in part exogenous—that is, they are chance events" (Laub, Nagin, and Sampson, 1998: 225). If good marriages were solely the product of conscious decision making or the outcome of prolonged patterns of behavior (either normative or deviant), then there would be little grounds for arguing that such bonds have independent effects or are important in the etiology of offending over the life span. Summarizing this position, Laub et al. (1998: 237) state:

> . . .*some* of the time, *some* high-rate offenders enter into circumstances like marriage that provide the potential for informal social control. When they do, and in our case when marital unions are cohesive, the investment has a significant preventative effect on offending. . . . "Good" things sometimes happen to "bad" actors, and when they do, desistance has a chance.

In the end, the crux of this debate concerning marriage and crime is whether marriages are partly exogenous events in the life course or whether those with greater propensities to marry are also apt to curtail their criminal behavior independently of marriage. We cast new light on this debate by employing Rosenbaum and Rubin's (1983) propensity score matching approach, which allows us to model the propensity to marry and then assess the effect of marriage on crime for respondents with equal propensities toward marriage.

ACCOUNTING FOR THE PROPENSITY TO MARRY

Rarely does criminological research on marriage and crime explicitly conceptualize and model the propensity to marry. A notable exception is the recent work of Sampson, Laub, and Wimer (2006), who investigated marriage and crime using a counterfactual approach that accounts for the likelihood of marriage. Prior research more frequently controls for prior offending and then examines the net effect of marriage on later offending. We depart from that line of work by employing propensity score matching to the study of marriage and crime. Propensity score matching is an econometric technique pioneered by Rosenbaum and Rubin (1983) that

uses observed covariates of a treatment variable to estimate a respondent's propensity to receive the treatment. The method can effectively balance treated and untreated respondents on background characteristics, thus ruling them out as confounders and allowing for a comparison of treated and untreated cases with similar propensities to receive the treatment (we discuss the method in more detail below). In this case, we seek to estimate an individual's propensity to marry in early adulthood and then assess the effect of marriage on crime for respondents with equal likelihoods of marriage. As Smith (1997) suggests, this approach is well suited for estimating the counterfactual—what would have happened to married individuals had they not been married?

Against this backdrop, we revisit the association between marriage and crime using data from a (U.S.) national sample of young adults. Our goals are to estimate the treatment effect of marriage on crime for males and females, respectively, and to assess differences across gender and propensity to marry.

RESEARCH OBJECTIVES AND ANALYTIC STRATEGY

DATA

Our research uses data from the NYS. Begun in 1976, the NYS is a longitudinal study of delinquent behavior, alcohol and drug use, and both psychological and physical well-being during adolescence and the transition to adulthood in the United States. The NYS comprised a national probability sample of households based on a multistage, cluster sampling design (Elliott, Huizinga, and Menard, 1989). The original sample consisted of 1,725 youths between the ages of 11 and 17 with an overall response rate of 73 percent. Comparisons with census data show the sample to be representative of the youth population in the United States during the late 1970s (Elliott, Huizinga, and Ageton, 1985). Data were collected at annual intervals through 1981 (waves 1 through 5) with further data collections in 1983 (wave 6) and 1987 (wave 7). During the last data collection, respondents were 21 to 27 years of age and the majority had made the transition to adulthood through the exiting of formal schooling and movement into full-time work, marriage, or parenthood. Respondent attrition over the panels was relatively low, with 80 percent of the sample retained through wave 7.

This sample is useful for investigating our research questions for several reasons. First, the NYS includes a sizeable proportion of respondents that have entered into marriage; yet respondents are unlikely to have aged out of crime irrespective of marriage. Second, there is ample variation on our

key outcome variable, criminal offending, and our focal independent variable, marriage. Third, the NYS is one of only a few longitudinal surveys available that measure crime for males and females, which allows for comparisons across sexes. Fourth, the survey includes adolescent and young adulthood indicators of dating, educational orientations, delinquency, and other background information that enables us to model respondents' propensities to marry.

To construct our key variables, we effectively divide the data into two parts. The first involves data measuring background characteristics, such as attitudes and behaviors, during adolescence. We measure these variables, compiled from the first six waves of data, when respondents were 17 years of age unless otherwise noted. The second part entails measurement of marriage and offending in early adulthood (ages 21 to 27) taken from wave 7 of the data.

MEASURES

Marriage and Offending

As we are interested in a straightforward "marriage effect," we measure *marital status* as whether the respondent reported being married one year prior to the interview at wave 7 (January 1986). We use that measure instead of marital status at the time of that interview because the offending questions, asked in early 1987, pertain to the previous year. It is thus reasonable, if not prudent, to measure marriage prior to the period of self-reported offending. As our interest is in the effect of intact marriages, we do not differentiate between different types of nonmarriage and we treat separated couples as unmarried. About 43 percent of female respondents were married at that point in the life course, compared with 29 percent of males.

We measure *offending* in early adulthood using data from wave 7, and we include a measure of prior offending from wave 6 in the marital matching model. Both measures of offending are weighted rates of self-reported involvement in crime and deviance for 17 offenses. Described in detail in table 1, these include several measures of violent offending (e.g., attacking someone or using force), property offending (e.g., breaking and entering or vandalism), drug offending (e.g., selling marijuana or hard drugs), and status offenses (e.g., being drunk or hitchhiking where illegal). Each item was weighted by a corresponding Sellin–Wolfgang crime seriousness score (Wolfgang et al., 1985) to ensure that the index was not dominated by trivial offenses.[1] The outcome variable indexes some low-rate and some

1. A reviewer expressed concern that our outcome variable, which gives greater weight to violent crimes, could bias our results for females because serious and violent offending is less likely among females. Additional analyses used a

rather high-rate offending. Also, gender differences in crime are apparent in the NYS sample, as males report higher weighted rates of offending than females. Still, the sample includes a nontrivial number of females that engaged in significant offending. Over 20 percent of females in the sample report weighted rates of adolescent offending that exceed the median level for males, and this includes some rather high-rate female offenders.

Measuring the Propensity to Marry

Prior work on "marriage markets" and the timing of marriage (e.g., Becker, Landes, and Michael, 1977; Lloyd and South, 1996; Oppenheimer, 1988) provides some guidance concerning background factors that influence respondents' propensities to marry. For our purposes, that body of work is useful because it suggests that the propensity to marry is partly a function of individual biographical experiences and demographic factors.

We first consider *educational expectations*, in line with research showing that education typically slows movement into marriage (Marini, 1978; Modell, Furstenberg, and Strong, 1978; Sweet, 1977). Women, in particular, are likely to delay marriage due to educational aspirations (for a related argument, see Goldscheider and Waite, 1986). We thus measure orientations toward education and schooling as a standardized scale comprising the *importance of school work*; the *importance of going to college*; the *expectations of going to college*; the *frequency of studying on afternoons, evenings, and weekends*; and *grade point average*. The scale has a standardized alpha reliability of .76.

Prior work also suggests that *work and employment* influence marriage. Life-course research includes competing ideas concerning work and marriage, but there is general agreement that employment is a significant factor in predicting the timing of marriage. Some suggest that paid employment lays a foundation that allows individuals the financial resources to marry and raise a family (Hogan, 1978; Lichter, LeClere, and McLaughlin, 1991; Wilson, 1987). Others argue for greater complexity in the employment relationship, where a "career orientation" may delay movement into marriage (Goldscheider and Waite, 1986; Mortimer, 2003), and they argue that the work–marriage nexus may be gendered

weighted index of property and drug offending, which included selling marijuana, stealing goods of more than $50, breaking and entering, buying stolen goods, stealing from family/employer, stealing goods worth less than $5, stealing goods worth $5–50, and being loud and rowdy (see table 1 for weights). That index yielded the same substantive results (available on request). In additional analyses not shown here but available from the first author on request, we also examined whether the unweighted property and drug crime index was associated with marriage for females, and that coefficient was also consistent with the results reported in the text.

(Clarkberg, Stolzenberg, and Waite, 1995; Sassler and Schoen, 1999). Despite these competing arguments, research clearly demonstrates the need to account for employment history when estimating the propensity to marry in early adulthood. In line with that body of work, we include a measure of *work history* at wave 6 when predicting marriage. This variable measures how many of the past 3 years that the respondent was employed.

Research also indicates that adolescent *relationship orientations and experiences* are associated with marriage (Sassler and Schoen, 1999; Whyte, 1990). Several measures in the NYS indicate orientations toward interpersonal and family roles. *Frequency of dating* is measured by the number of evenings in a week spent dating or on social activities. *Frequency of sexual activity* is the respondent's self-reported annual incidence of sexual intercourse. *Dating importance* is measured by two items indexing the importance of having a boyfriend or girlfriend and the importance of having lots of dates ($\alpha = .62$). More long-term *family orientations* are measured by combining two items asking about the importance of getting married and the importance of having children in the future ($\alpha = .74$). We expect that such measures index both an orientation toward marriage as well as social and psychological preparation for such roles.

We further consider one's *criminal history and delinquent environment* when predicting marriage. Although not often the focus of research on marriage markets, some individuals, by virtue of their criminal past and present, may position themselves out of the market (Gottfredson and Hirschi, 1990). In addition, criminal involvement and contact with the criminal justice system undermines socioeconomic viability (Hagan, 1993; Pager, 2003; Western, 2002; Wilson, 1987). To that end, we include a measure of weighted self-reported offending (defined above and in table 1) and an indicator of arrest history that records the number of times the respondent was arrested (ranging from "0" to "10 or more"). We also include a measure of *peer delinquency* during adolescence measured by the proclivity for the respondent's peers to destroy property, hit people, and sell hard drugs.[2]

Finally, *sociodemographic factors* are also important when considering one's propensity to marry, as they can serve as both assets and liabilities in a marriage market. Age, for instance, may signal maturity, stability, and readiness to invest in a marriage. Other factors, such as family background (Goldscheider and Waite, 1986), race (Lichter, LeClere, and McLaughlin, 1991; Wilson, 1987), and rural residence may influence "marriageability."

2. We use these three measures because they represent moderate-to-serious forms of peer offending and cover a range of offenses (drug, violent, and property); yet the metric limits extreme skew that might accrue if all possible indicators of delinquent peers were included.

We thus include measures of *age* (in years at wave 7), *race* (black = 1, else = 0), respondent's *family structure* (coded 1 if parents were married when respondent was an adolescent), *family educational background* as indicated by the highest education achieved by a parent, *number of children* in the household during childhood, *neighborhood conditions* such as the presence of disorders, and *rural residence*.

Unless constant or otherwise noted, we attempted to measure background variables at approximately the same point in the life course for all respondents as opposed to garnering all background information from wave 1 of the survey. Nearly all information on behaviors and expectations that might predict marriage was measured when the respondent was 17 years old.[3] Exceptions include those respondents who were 11 or 12 years of age at the first wave of data. The sampling design did not allow information on these two cohorts at age 17 because of a 3-year gap between the fifth and sixth waves of data. We use information on these respondents at ages 18 and 19, respectively (from NYS wave 6).

PROPENSITY SCORE MODELS AND TREATMENT EFFECTS

Prior research examining the impact of marriage on crime often employed covariate adjustment models (Giordano, Cernkovich, and Rudolph, 2002; Sampson and Laub, 1990, 1993; Warr, 1998). As typically used, such models give mean offending differences between those who are married and those who are not married, which are adjusted for any number of control variables. Under very specific conditions, such as when respondents are randomly assigned to a particular status, the results generated from regression models may represent the actual treatment effect of marriage on crime. As marriage is not a randomly occurring event, we use propensity score matching estimators to assess the treatment effect of marriage on adult offending. Matching estimators use background variables to model a respondent's propensity to marry and then assesses the effect of marriage on crime for respondents who are matched based on those propensity scores. As discussed by Rosenbaum and Rubin (1983), the key notion of such models is to replicate conditions of an experiment such that the treatment variable, in this case marriage, can be treated as though it occurred at random and that the individuals under analysis are homogenous on all other factors except the treatment variable (for accessible discussions and illustrations of matching models, see Harding, 2003; Morgan, 2001; Smith, 1997; Winship and Morgan, 1999). The matching thus rules

3. When data were missing at this age, we assigned the value on that variable at the previous available age. In most cases, this was age 16.

Table 1. Descriptive Statistics for Dependent and Independent Variables

Variable	Description	Females Mean (SD) [Valid *N*]	Males Mean (SD) [Valid *N*]
Dependent Variable			
Adult crime	Sum of weighted rates of self-reported crime and deviance. Items were coded as: 0 = never; 1 = once or twice a year; 2 = once or twice every 2 or 3 months; 3 = once a month; 4 = once every 2–3 weeks; 5 = once per week; 6 = 2–3 times per week; 7 = once per day; 8 = 2–3 times per day. Offenses include (*Sellin–Wolfgang weight in [brackets]*): (1) sold hard drugs [21.6]; (2) attacked someone [13.5]; (3) break & enter [7.5]; (4) used force/strong-arm [11.7]; (5) sold marijuana [9.5]; (6) stole something worth more than $50 [8.9]; (7) carried a hidden weapon [4.5]; (8) bought stolen goods [6]; (9) took a vehicle [5.4]; (10) stole from family/employer [5.1]; (11) stole something worth $5-$50 [3.3]; (12) been drunk [2.3]; (13) been rowdy or loud [2.1]; (14) stole something worth less than $5 [1.7]; (15) vandalized property [3.1]; (16) avoided paying for things [2.2]; (17) hitchhiked where illegal [1].	5.09 (16.83) [*N* = 683]	14.01 (29.19) [*N* = 699]
Independent Variables			
Focal independent variable			
Marriage	Respondent was married as of January 1986 (coded 1; else coded 0). Information taken from the wave 7 interview. Cohabiting and separated individuals are *not* coded as married.	.43 (.50) [*N* = 683]	.29 (.46) [*N* = 701]
Background and prior offending			
Prior offending	See "adult crime" description above. Prior offending measured at wave 6.	4.86 (12.42) [*N* = 726]	15.90 (34.34) [*N* = 766]
Arrest history	Number of prior arrests (0–10+), taken from wave 6 questionnaire.	.08 (.48) [*N* = 726]	.54 (1.48) [*N* = 770]
Work history	Number of last three years steadily employed as of wave 6 interview.	2.33 (.94) [*N* = 726]	2.48 (.82) [*N* = 768]
Black	Respondent self-identifies as black (coded 1; else coded 0).	.14 (.34) [*N* = 807]	.16 (.37) [*N* = 918]

Table 1 (continued)

Variable	Description	Females Mean (SD) [Valid *N*]	Males Mean (SD) [Valid *N*]
Age	Respondent's age at wave 7.	23.78 (1.95) [*N* = 807]	23.96 (1.94) [*N* = 918]
Family structure	Respondent's parents were married (at wave 1 interview).	.80 (.40) [*N* = 781]	.82 (.39) [*N* = 902]
Family educational background	Ordinal scale indicating highest level of education achieved by a parent. Scale ranges from 1 (some grade school) to 7 (postgraduate degree). Measured at wave 1.	3.70 (1.29) [*N* = 781]	3.63 (1.32) [*N* = 902]
Number of children in adolescent household	Number of children in the household measured at wave 1 (parent or guardian report).	3.23 (1.89) [*N* = 807]	2.99 (1.72) [*N* = 918]
Neighborhood conditions	Sum of measures indicating presence of neighborhood problems, such as vandalism, winos or junkies, traffic as a problem, abandoned houses, burglaries and theft, run down and poor buildings, assaults, and muggings. Respondents were asked if the issue is (1) not a problem, (2) somewhat of a problem, or (3) a big problem. Items were measured at wave 1.	8.84 (2.26) [*N* = 779]	8.80 (2.24) [*N* = 899]
Rural residence	Respondent resided in a rural area during adolescence (wave 1).	.26 (.44) [*N* = 807]	.28 (.45) [*N* = 918]
Adolescent orientations[a]			
Orientation towards school	Standardized scale comprising the (1) importance of school work, (2) importance of going to college, (3) expectations of going to college, along with the frequency of studying during (4) afternoons, (5) evenings, and (6) weekends, as well as (7) GPA (Standardized Cronbach's alpha = .76. Note: variables centered before summing).	.03 (4.38) [*N* = 797]	.04 (4.48) [*N* = 904]
Frequency of dating	Number of evenings in a week spent dating or on social activities.	2.50 (1.57) [*N* = 807]	2.17 (1.50) [*N* = 917]
Frequency of sexual activity	Self-reported annual incidence of sexual intercourse.	19.62 (65.07) [*N* = 767]	18.15 (56.43) [*N* = 879]
Dating importance	Combines two items indexing the importance of having a boyfriend or girlfriend and the importance of having lots of dates (α = .62).	3.19 (1.18) [*N* = 805]	3.23 (1.15) [*N* = 918]
Orientation towards family	Combines two items asking about the importance of getting married and the importance of having children in the future (α = .74).	3.67 (1.20) [*N* = 805]	3.25 (1.28) [*N* = 917]

Table 1 (continued)

Variable	Description	Females Mean (SD) [Valid *N*]	Males Mean (SD) [Valid *N*]
Delinquent peers	Degree to which peers engage in property crime (destroyed property), violent crime (hit others), and serious drug crime (sold hard drugs).	3.76 (1.33) [*N* = 786]	4.66 (1.85) [*N* = 889]

[a] Measures taken at age 17 unless otherwise specified in the text.

out confounding effects of observed background variables on the treatment effect, with the assumption that potential confounding variables were not omitted from the propensity score model, which is referred to as the strong ignorability assumption (Rosenbaum and Rubin, 1983).

The propensity score, which represents the conditional probability of marriage, can be written as:

$$p(marriage) = \Pr(T_i = 1 \mid X_i) \quad (1)$$

where $T_i = 1$ if individual i is married and X_i is a vector of covariates for individual i that predict marriage and are potential confounding variables in the association between marriage and crime. In this analysis, the propensity score is estimated with a logit specification.

This procedure uses observed covariates to generate a propensity score for each respondent that is bound between zero and one, which represents an individual's propensity to marry. Once the propensity score for each individual is calculated, a matching algorithm pairs married individuals (treated) with like or identical nonmarried (control) individuals based on their likelihood to experience marriage. Analysis of the effect of marriage on crime is then conducted on the matched sample.

The effectiveness of propensity models is dependent on the quality of the matched sample, and various matching procedures match treatment and control cases differently. We thus employ two different matching algorithms, one-to-one nearest-neighbor matching without replacement and kernel matching, to minimize the chance that our findings are biased by the choice of matching estimator.

The nearest-neighbor matching algorithm identifies the propensity score of a married individual and then selects a nonmarried individual whose propensity score is closest to each married respondent. Once matched, the nonreplacement option guarantees that an unmarried individual cannot be matched to more than one married respondent. After the neighbor matching procedure, the resulting matched sample is homogeneous with respect to key factors that predict marriage; yet it differs only with respect to whether respondents were actually married. Marriage can thus be treated

as though it occurred at random given the assumptions underlying the model. Moreover, because these individuals are not significantly different on other measured predictors of offending and, subject to the matching procedure, differ only in whether they actually experienced marriage, any difference in rates of offending can be viewed as the treatment effect of marriage.

Still, nearest-neighbor matching is not without potential shortcomings. Among them, the nearest neighbor, or best match, of a treated individual may in fact have a different likelihood of marriage. Assuming no additional restrictions, the matching algorithm picks the closest nonmarried match, as measured by propensity scores, to a married individual without consideration of the actual distance between treatment and control cases. Simply being the nearest match to a treated individual does not ensure that the individuals have identical likelihood of experiencing marriage.

Careful specification of the matching equation minimizes this concern. We invoke a caliper restriction of .01 when performing the nearest-neighbor match, thus ensuring that treated individuals and their nonmarried match will differ on their estimated marriage propensity score by no more than .01 units.[4]

We also employ a second matching estimator, kernel matching, to further assess the robustness of our findings. The key aspect of the kernel match is the weighting procedure that accounts for the distance, measured through the difference in propensity scores, between treated cases and their matched controls. In contrast to neighbor matching, where the treatment effect of marriage on crime is estimated using a sample that has only one matched individual for each treated individual, kernel matching uses information from a larger number of respondents. However, in kernel matching, the contribution of individuals in the control group to the overall estimation of the treatment effect of marriage is dependent on their distance, measured through differences in propensity scores, from their matched treated case. The estimate of the marriage effect is thus weighted so that control group members (nonmarried) who are closest to treated individuals (married) contribute more to the estimation of the overall treatment effect than those with greater differences on the propensity

4. Restricting the caliper to a smaller value assures even closer matches but with the cost of restricting the size of the matched sample. We suggest that a caliper of .01 effectively balances closely matched pairs while allowing an adequate sample size for subsequent analysis within matched samples (see below). We note that additional analyses using a caliper of .001 yielded the same substantive results with respect to the estimated treatment effect of marriage on crime for samples of males and females, respectively.

score. In other words, better matches have greater impact on the parameter estimate. As the match decreases in quality, so too does the contribution of the control individual in calculating the treatment effect (Becker and Ichino, 2002).

Our canvassing of the literature on propensity score estimators finds no firm consensus on the appropriate way to specify propensity models. This issue is even less resolved as it relates to the inclusion of variables in the model that generate the propensity scores. One line of scholarship suggests that matching equations should include a large number of variables, including covariates that are only weakly or insignificantly associated with the treatment outcome (Rubin and Thomas, 1996: 253). Others stress the use of theory and prior research to guide parsimonious model building (Dehejia and Wahba, 2002: 161). We endeavor to strike a balance between the two positions, including a comprehensive number of predictor variables and then testing the robustness of our findings by including various combinations of interaction terms.[5] All analyses are restricted to entirely male or female samples, and thus, there are no cross-gender matches.

MATCHING RESULTS

The propensity model logit coefficients are reported in the appendix.[6] The success of the matching procedure is based, in part, on the extent to which pretreatment variables are balanced on means and variances. If covariate balance is not achieved after matching on propensity score, then sample heterogeneity may bias estimates of the marriage effect. Becker and Ichino's (2002) program specifically tests for covariate balance, and this "balancing property" was satisfied when generating propensity scores for the male and female samples. In addition to that diagnostic test, we also report *t* test results to compare the full (unmatched) samples of males

5. See Frisco, Muller, and Frank (2007) for additional discussion on this debate and related issues concerning matching equations. Also, given the variables in our matching equation, hundreds of possible two- and three-way interactions could be included in the propensity score models. We report the results that included only main effects parameters because the two-way interactions added little explanatory power to the propensity model. We then replicated the results using an alternative specification of the propensity score that include multiple two-way interactions (available from the first author on request). That model also replaced prior delinquency at wave 6 with a measure from adolescence (approximately age 17 or the nearest age with available data). We measure delinquency at wave 6 here because it was a better predictor of marriage in the propensity model.

6. The statistical significance of a given coefficient in the propensity score model is not necessarily of great concern because the high number of predictor variables in the model likely poses collinearity problems that can influence singular coefficients without biasing the overall propensity score model.

Table 2. *t* Test Results Comparing Married and Unmarried Respondents on Background Factors for Full and Matched Samples of Male and Female Respondents

	Full Sample			Matched Sample		
	Not Married	Married	*t* Value	Not Married	Married	*t* Value
Age (wave 7)						
Male	23.5 (1.9)	24.9 (1.8)	–8.7*	24.5 (1.7)	24.4 (1.7)	.43
Female	23.4 (1.9)	24.3 (1.9)	–6.5*	24.1 (1.8)	23.9 (1.8)	1.2
Black						
Male	.18 (.38)	.12 (.32)	1.9*	.15 (.36)	.11 (.31)	1.1
Female	.17 (.35)	.08 (.26)	2.9*	.11 (.31)	.10 (.31)	.16
Intact Family						
Male	.81 (.39)	.83 (.38)	–.57	.83 (.37)	.86 (.35)	–.51
Female	.78 (.42)	.85 (.36)	–2.3*	.79 (.41)	.81 (.39)	–.51
Parent Education						
Male	3.8 (1.4)	3.6 (1.2)	2.0*	3.6 (1.3)	3.6 (1.1)	–.10
Female	3.8 (1.3)	3.7 (1.2)	1.4	3.8 (1.4)	3.7 (1.1)	.56
Sibling Size						
Male	3.0 (1.6)	2.8 (1.5)	1.1	2.9 (1.5)	2.8 (1.3)	.73
Female	3.2 (1.7)	3.1 (1.8)	.63	2.9 (1.5)	3.1 (1.6)	–.74
Neighborhood Problems						
Male	8.8 (2.3)	8.6 (1.9)	1.3	8.6 (2.1)	8.6 (1.8)	.00
Female	9.0 (2.3)	8.5 (1.8)	3.1*	8.6 (2.0)	8.6 (1.9)	.03
Rural						
Male	.28 (.45)	.36 (.48)	–2.2*	.39 (.49)	.33 (.47)	.90
Female	.23 (.42)	.33 (.47)	–2.8*	.29 (.46)	.28 (.45)	.23
Educational Orientations						
Male	.64 (4.6)	–.57 (4.4)	3.2*	–.45 (4.6)	–.32 (4.2)	–.24
Female	.43 (4.3)	–.56 (4.4)	2.9*	.04 (4.4)	–.04 (4.4)	.19
Frequency of Dating						
Male	2.2 (1.5)	2.4 (1.5)	–1.4	2.3 (1.4)	2.2 (1.4)	.13
Female	2.4 (1.6)	2.7 (1.5)	–2.3*	2.7 (1.6)	2.6 (1.5)	.67
Importance of Having Dates						
Male	3.0 (1.3)	3.2 (1.3)	–1.4	3.4 (1.2)	3.2 (1.1)	1.3
Female	2.8 (1.3)	2.9 (1.4)	–1.2	2.9 (1.4)	2.8 (1.3)	.62
Orientation toward Family						
Male	3.2 (1.3)	3.4 (1.3)	–2.2*	3.4 (1.2)	3.2 (1.2)	.94
Female	3.5 (1.2)	3.9 (1.1)	–3.4*	3.8 (1.2)	3.7 (1.2)	.13
Prior Offending						
Male	18.9 (39.6)	9.1 (16.1)	3.3*	13.2 (25.5)	11.5 (18.6)	.64
Female	6.2 (12.4)	3.5 (13.0)	2.7*	4.5 (7.7)	4.0 (15.5)	.40

Table 2 (continued)

	Full Sample			Matched Sample		
	Not Married	Married	*t* Value	Not Married	Married	*t* Value
Work History						
Male	2.4 (.87)	2.7 (.56)	–5.3*	2.7 (.61)	2.7 (.59)	.21
Female	2.4 (.90)	2.3 (.96)	.68	2.4 (.92)	2.3 (.97)	.43
Arrest History						
Male	.56 (1.5)	.59 (1.6)	–.25	.42 (1.0)	.52 (1.38)	–.61
Female	.11 (.62)	.04 (.20)	1.7*	.06 (.29)	.05 (.21)	.40
Delinquent Peers						
Male	4.6 (1.8)	4.7 (1.8)	–.68	4.7 (1.7)	4.7 (1.7)	–.07
Female	3.7 (1.3)	3.8 (1.4)	–.46	3.7 (1.2)	3.8 (1.4)	–.38
Frequency of Sex						
Male	18.6 (56.5)	15.9 (39.5)	.60	21.8 (70.9)	15.8 (35.3)	.87
Female	15.9 (44.3)	28.0 (90.7)	–2.2	17.7 (50.0)	21.3 (51.9)	–.70*

*$p < .05$, one-tailed test.
NOTE: The matched sample was generated using the nearest-neighbor matching without replacement algorithm. Standard errors are in parentheses.

and females, respectively, with the matched samples.[7] Table 2 shows several differences between married and unmarried respondents in the full (unmatched) samples. Married and unmarried males, for instance, significantly differ on 8 background variables, including likely correlates of offending such as age, race, prior offending, and employment. Married females differ from their unmarried counterparts on 11 background characteristics, again including correlates of offending. The objective of propensity score matching is to limit differences based on the treatment variable, in this case marital status, and successful matching should mitigate differences between married and nonmarried respondents. The last three columns of table 2 (columns 5 through 7) compare matched samples of males and females separately. If the matching procedure were successful, then we would expect no significant differences and convergence on means in our matched sample. This was indeed the case, as no significant differences remain for either males or females after matching on propensity to marry, indicative of successful matching. More importantly, the means are similar on key background variables in the matched sample. For instance, the 1.4-year age difference for males is reduced to .1, and a similar reduction is found for females. In addition, a 9.8 unit difference between married and unmarried respondents on prior offending for the unmatched male sample reduces to a nonsignificant difference of 1.7 units,

7. The matched *t* test comparisons use the sample generated from the nearest-neighbor matching algorithm.

a change that is paralleled for the female sample. Overall, both the *t* tests and the balancing properties in the propensity score procedures are indicative of effective matching.[8]

FINDINGS—MARRIAGE, GENDER, AND CRIME

We first present the ordinary least-squares (OLS) coefficients for weighted offending on marriage for purposes of comparison with the matched sample estimates. The bivariate regression coefficients for males and females in the full (unmatched) sample are reported in table 3. The coefficients in table 3 suggest that marriage is negatively and significantly associated with our weighted measure of offending for males ($b = –9.222$) and females ($b = –2.614$; see table 3), although the size of the marriage coefficient is much greater for males.

Table 3. Ordinary Least-Squares Regression Coefficients: Crime on Marriage for Unmatched Samples of Males and Females

	Crime at Wave 7 Males	Crime at Wave 7 Females
Marriage coefficient	–9.222**	–2.614*
(Standard error)	(2.402)	(1.298)
N	699	683

*$p < .05$; **$p < .001$ (two-tailed test).

We next report estimates from our matching models, beginning with the kernel matching estimates (table 4) followed by the one-to-one nearest-neighbor matching estimates without replacement. The effect of marriage on offending in early adulthood is noticeably smaller in the propensity score matching analyses, particularly when employing the kernel match (table 4), indicating that selection plays a nontrivial role in the marriage–crime relationship. The kernel matching estimates indicate that marriage is a negative and significant predictor of crime for males ($b = –4.364$); yet the size of the marriage effect is approximately half the size of the OLS model. To wit, marriage has a significant but rather small effect on crime once we account for selection into marriage.

8. All analyses are restricted to regions of common support, which refers to the range of propensity score values for which there is a nonzero probability of observing both a treated and an untreated case. Histograms for the propensity scores of matched and unmatched samples, which is one means of assessing common support, are available from the first author on request.

The deterrent capacity of marriage is less efficacious for females. The kernel matching estimate for females is reduced to –1.804, or a decrease of about 30 percent compared with the OLS estimates, and is not statistically significant. Based on both the coefficient size and its statistical significance, marriage seems to be a more salient institution with respect to criminal behavior for men relative to women.[9]

Table 4. Kernel-Based Matching Estimates: Treatment Effect of Marriage on Adult Crime for Males and Females

	Males	Females
Marriage coefficient	–4.364*	–1.804
(Standard error)	(1.810)	(1.309)
t value	–2.579	–1.424
N (treated/controlled)	175/391	266/342
Empirical distribution (95% bounds: lower, upper)	–8.42, –1.06	–4.74, .58

*$p < .05$ (two-tailed test).

NOTE: The number of treated in the matching model corresponds to married respondents, whereas the control group refers to unmarried respondents. Kernel-based matching was specified using the "attk" procedure in Stata 9.2 (see Becker and Ichino, 2002). Our model specified bootstrapped standard errors, 1,000 repetitions, and was restricted to the region of common support.

The kernel matching estimates are consistent with the nearest-neighbor matching results shown in table 5. Nearest-neighbor matching procedures generate average rates of offending for the treatment and control groups, along with the differences, standard errors, and comparison statistics for the unmatched samples. The neighbor matching estimates also illustrate that the effect of marriage on crime is smaller for the sample of matched males, albeit still statistically significant. A difference of –10.62 units on the weighted crime index for the *unmatched* sample compares with a significant difference of –7.54 for the *matched* sample. For females, the marriage effect drops from –2.88 ($p < .05$) for unmatched respondents to a nonsignificant –1.69 for matched females. Akin to the kernel matching estimates, the neighbor matching procedure again suggests no effect of

9. Our results reported in table 4 show the coefficients and standard errors, along with the 95 percent confidence intervals when using nonparametric methods, which are based on the actual empirical distribution of the data.

marriage on crime for females but a small and significant effect for males.[10]

Table 5. Average Effect of Marriage on Crime Using One-to-One Nearest-Neighbor Matching without Replacement

Sample	$\bar{x}$ Rate of Offending - Treated (Married)	$\bar{x}$ Rate of Offending - Control (Unmarried)	Difference	Standard Error	*t* Value
Males					
Unmatched	7.20	17.80	–10.62	2.73	–3.90*
Matched	7.54	15.08	–7.54	2.98	–2.53*
Females					
Unmatched	3.76	6.64	–2.88	1.44	–2.01*
Matched	4.52	6.21	–1.69	1.76	–0.96

*$p < .05$ (two-tailed test).
NOTE: One-to-one nearest-neighbor matching without replacement was estimated using "psmatch2" in Stata 9.2 (Leuven and Sianesi, 2003). We specify a model with a logit specification for the propensity model (predicting marriage) and a caliper of .01. The substantive results are consistent when specifying a caliper of .001, although the sample size is reduced substantially. The balancing property was satisfied in all analyses using the "pscore" procedure in Stata 9.2. The treatment estimate for females includes 191 married and 191 unmarried respondents. The treatment estimate for males includes 132 married and 132 unmarried respondents.

FURTHER CONTINGENCIES IN MARRIAGE AND CRIME

We further investigate the influence of marriage on crime by stratifying our samples based on propensity score. This set of analyses shows whether the effect of marriage on crime reported in tables 4 and 5 differs by propensity to marry within our matched male and female samples, respectively. As described, each respondent receives a propensity score between 0 and 1 that indicates the respondent's propensity to marry. Within our respective matched samples of males and females, we divide this marriage propensity scale into equal thirds, corresponding to low, medium, and high propensities to marry, so that each group has an equal number of cases.[11]

10. We also examined gender differences in marriage and crime by assessing the change in offending for respondents who were not married between waves 6 and 7 compared with those who became married between those waves of data. Those results, which are available on request from the first author, are consistent with the propensity score models.
11. We performed *t* tests for all background variables within each subgroup, which consisted of 96 comparisons (6 groups × 16 variables). There were a total of 10 significant differences using equally sized subgroups, and 5 significant differences

For males, the bottom third included respondents with propensity scores between .04 and .27, the middle third ranged from approximately .28 to .42, and the top third ranged from .43 to .79. For females, the low-propensity group included scores of .03 to .38, the middle comprised propensity scores from .39 to .53, and the high group ranged from approximately .54 to .79.[12]

We examine whether the relationship between marriage and crime is contingent on one's propensity to marry by comparing *t* test results for married and unmarried respondents within each propensity score stratum (figures 1 and 2). Figure 1 illustrates the differences in crime for married and unmarried respondents within propensity score groups for males. The figure reveals that marriage has a sizeable effect for males who, relative to other males, have a low propensity to marry. The mean rate of offending for this group of low-propensity males that are not married is 23.36, which is noticeably higher than the mean rate of offending for low-propensity males that are, in fact, married (7.65). Much smaller differences are found for the medium- and high-propensity male respondents. The difference of 15.71 units for the low-propensity group is sizeable compared with the 2.14 unit difference for moderate-propensity males (9.93–7.79, not significant) and the 4.76 unit difference for high-propensity males (11.95–7.19, not significant). These results suggest that males who are unlikely to enter into the institution of marriage may be most apt to benefit from it, at least with respect to deviant behavior.[13]

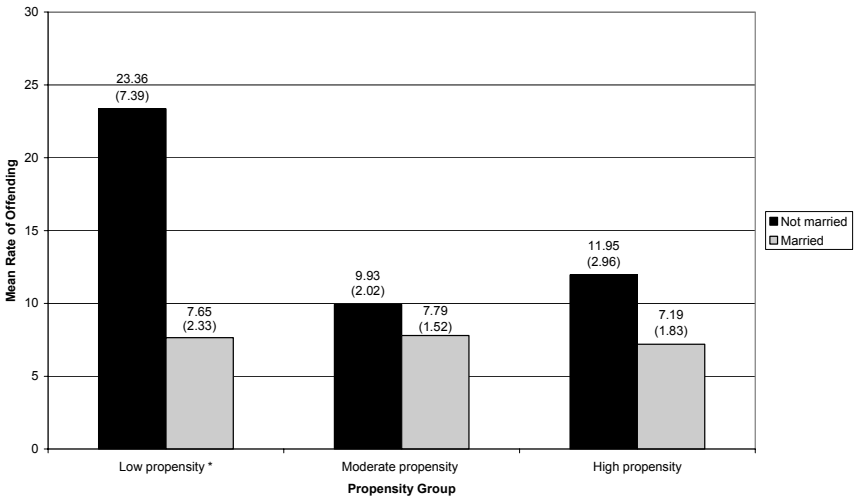
A different pattern emerges for females. As depicted in figure 2, there is no significant association between marriage and crime for low- or high-propensity females. Yet marriage has a significant and negative effect on crime for females with moderate propensities to marry. In this case, the

when using the cut points described in footnote 12. In additional analyses (available on request), we controlled for those background variables that differed by marriage in a regression model, and the results are substantively the same.

12. An alternative means of splitting the samples is to use the same cut points for males and females, thereby standardizing the cut points for low-, moderate-, and high-propensity groups. In that comparison, the three groups were determined not by numeric representation, but by the value of the propensity score alone. For both groups, the propensity scores ranged from just under .05 to about .80, or a range of about .75. We then created the following three groups within the male and female samples: lowest–.30 (low), .31–.55 (moderate), and .56–.80 (high). The results, regardless of cut points, are substantively alike and are available from the first author on request.

13. The low-propensity group of males includes a few very high-rate offenders. Even when removing the most extreme cases and reestimating the models, the effect is marginally smaller but remains significant and larger than that observed in the moderate- and high-propensity groups.

Figure 1. Males: Mean Rates of Weighted Offending in Early Adulthood for Matched Sample by Marriage and Propensity to Marry



*$p < .05$ (two-tailed).

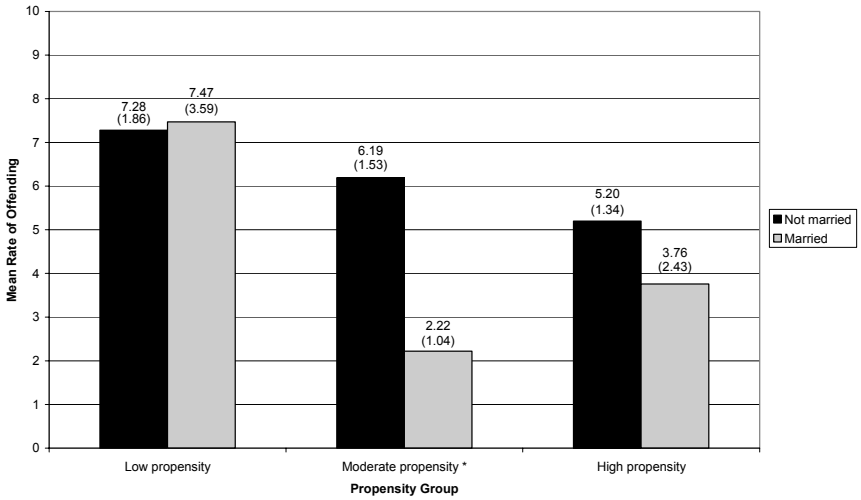
NOTE: Standard errors are in parentheses. For all comparisons, the respective sample sizes are 44 married and 44 unmarried respondents. Respondents were matched using the one-to-one nearest-neighbor match without replacement.

mean rate of offending for nonmarried females with moderate propensities to marry is 6.19, compared with a mean rate of 2.22 for married females in the same propensity group. In almost complete contrast to men, women with moderate propensities to marry seem most strongly influenced by marriage, whereas marriage has no significant influence for those with the low propensities to marry.

DISCUSSION

Our analysis reassessed the marriage–crime relationship using a propensity score matching approach for a nationally representative sample of males and females, and our results reveal both consistencies and inconsistencies with life-course theories (Sampson and Laub, 1993). We suggested that marriages may be more efficacious for males because they are apt to marry partners with less-deviant histories, thus increasing the conventionalizing influence of marriage. As Laub and Sampson (2003: 45–46) state, "it is almost invariably the case that men marry 'up' and women 'down' when it comes to exposure to violence and crime." We find some support for this suggestion, as marriage had a modest effect on crime for male respondents and seems particularly salient for males with low propensities

Figure 2. Females: Mean Rates of Weighted Offending in Early Adulthood for Matched Sample by Marriage and Propensity to Marry



*$p < .05$ (two-tailed).

NOTE: Standard errors are in parentheses. The respective sample sizes are as follows: Low propensity: 62 unmarried and 65 married; Moderate propensity: 64 unmarried and 63 married; and High propensity: 65 unmarried and 63 married. Respondents were matched using the one-to-one nearest-neighbor match without replacement.

to marry. The latter finding aligns with claims that men who seem unlikely to enter the institution of marriage sometimes do, and given their elevated rates of prior delinquency, they may see reductions in crime as a consequence (Laub, Nagin, and Sampson, 1998). At the same time, we see substantial evidence that selection dynamics have notable influence on the magnitude of the "marriage effect" and demonstrates the need for greater attention to such issues and their implications.

Whereas marriage was of some consequence for males, no equivalent relationship surfaced for our sample of matched females. To that end, our work partly challenges recent arguments that marriage is an equally beneficial institution for men and women (Waite and Gallagher, 2000). In addition, the influence of marriage on crime is contingent on female respondents' propensities to marry. Both gender and one's propensity to marry, in conjunction, are significant for understanding the role of normative institutions and crime over the life course. That notion agrees with recent insights concerning social roles, social controls, gender, and crime. Giordano, Cernkovich, and Rudolph (2002) suggest both similarity and peculiar differences in the desistance process for males and females. Our

work largely supports this argument. Given the gender differences in criminal behavior and working from the premise that males are more likely to marry partners with shorter criminal histories, marriage for males can better facilitate a "knifing off" from criminal opportunities (Laub and Sampson, 2003: 145). Marriage is thus a more salient institution for males relative to females in the study of crime over the life course.

Why is marriage significant for females with moderate propensities to marry but not for low- and high-propensity females? High-propensity females may experience little efficacy from marriage because they are less prone to criminal behavior in the first place. In our matched sample of females, the high-propensity group reported a lower level of prior offending (at wave 6) and a lower variance relative to females with moderate and low propensities to marry. Thus, the nonsignificant difference may reflect the low mean and smaller variance for high-propensity females. That group had little room for change because it largely consisted of nonoffenders. Our results also indicate no significant difference between married and nonmarried female respondents with low propensities to marry. To explain this pattern, we draw from the metaphor of Giordano, Cernkovich, and Rudolph (2002: 1055) of "hooks for change." Low-propensity females, like males, may have less "conventionalizing potential." Yet, whereas low-propensity males are still apt to "marry up" with respect to crime and violence, similarly situated females are probabilistically more likely to enter a relationship with an equally or more deviant male partner. Thus, not only are low-propensity females potentially less likely to "set the hook" for change, but they may encounter fewer nondelinquent partners to which they can attach.

In contrast, women in "the middle" are those capable of drifting into crime *or* capitalizing on hooks for change and may be most susceptible to the transformative effects of social institutions (Giordano, Cernkovich, and Rudolph, 2002: 1054). Females in our moderate-propensity group experienced elevated levels of prior delinquency relative to high-propensity females; yet they still have the ability to capitalize on movement into normative institutions such as marriage. Currently, this explanation is suggestive but is consistent with the theory put forth by Giordano, Cernkovich, and Rudolph. We propose that future studies build on the work of Giordano, Cernkovich, and Rudolph and more closely focus on the intersection of gender, disadvantage, and life-course prospects.

Our research is not without limitations. The matched sample estimates we provide are not a complete substitute for the full sample estimates. Matching estimators have several advantages over traditional regression models, such as balancing treatment and control groups on background factors (Dehejia and Wahba, 2002; Morgan, 2001; Rosenbaum and Rubin,

1983; Smith, 1997). Yet, statistical inference from the matched sample estimates is limited because marital matching reduces the random nature of the NYS sample, and accordingly, we must limit our generalizations from the matching models to married and unmarried respondents with similar attributes and cognitions. Also, propensity score modeling is limited by its reliance on observable and measured background variables, and thus we acknowledge the possibility that time-varying factors that influence marriage and crime that are not measured in the NYS could have implications for the results. We thus echo Smith and Todd's (2005) contention that propensity score matching models are not a "magic bullet" for assessing treatment effects in the absence of random assignment. Propensity models represent *a* method, not *the* method, for assessing treatment effects. Given that the econometrics literature on propensity score modeling remains contested, we suggest that future work employ different estimators and give due attention to differences across model specification.

In addition to issues concerning statistical inference, our data also cannot account for an important piece of the marriage–crime puzzle: the criminal behavior of the spouse. That facet of life-course research receives theoretical attention, but data are rarely afforded to thoroughly investigate the influence of delinquent partners. In addition, future work might also build on the current work by using a similar methodology with different data. Our marriages largely pertained to young adults, and thus, they provide both a complement and point of comparison with research using older cohorts (e.g., Sampson, Laub, and Wimer, 2006).

In closing, we position our argument alongside recent literature asking not only *whether* adult institutions curtail offending, but *for whom* involvement in conventional adult institutions inhibits criminal behavior. Although prior work highlights age as a salient factor interacting with crime and normative institutions (Uggen, 2000), we emphasize gender and propensity. Future work might consider other contexts than age and biography. For example, cohort and period effects that inform much life-course research (Shanahan, 2000) may also condition the deterrent capacity of marriage. Most evidence concerning marriage and crime stems from samples that transitioned to adulthood in the 1950s and 1960s, eras that preceded the divorce revolution that has greatly increased the prevalence of marital disruption (Peterson, 1996; Weitzman, 1985). Whereas both culture and law increased the durability of marriages in earlier decades, the current era is characterized by a marital regime that potentially weakens postmarital socialization by increasing the ease of separation and divorce, placing increased importance on premarital selection (Oppenheimer, 1988). But to what extent this larger social change influences marriage and crime is less well understood. Cultural context surrounding social institutions and their concomitant roles is plausibly an important contingency in

the deterrent capacities of adult social roles. Future work might further explore such conditional effects when investigating changes in crime over the life course.

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Appendix. Logistic Regression Estimates for Propensity Score Models

Variable	Males Coefficient (Standard Error)	Females Coefficient (Standard Error)
Prior arrest	.061	–.704
	(.092)	(.411)
Work history	.511*	–.161
	(.167)	(.106)
Delinquent peers	.081	–.006
	(.065)	(.073)
Age (wave 7)	.390*	.295*
	(.061)	(.053)
Black	–.924*	–.511
	(.370)	(.338)
Intact family	.261	.182
	(.301)	(.257)
Family educational background	–.156	–.100
	(.091)	(.078)
Number of siblings	–.061	–.010
	(.074)	(.060)
Neighborhood problems	.035	–.074
	(.059)	(.048)
Rural	.379	.371
	(.223)	(.199)
Educational orientations	–.040	–.011
	(.026)	(.022)
Frequency of dating	.019	.052
	(.074)	(.063)
Importance of dating	–.113	.105
	(.106)	(.084)
Orientation towards family	.179	.200*
	(.093)	(.080)
Prior offending (wave 6)	–.022*	–.018
	(.002)	(.010)
Frequency of sexual intercourse	.005*	.006*
	(.002)	(.002)
Constant	–11.890*	–7.251*
	(1.721)	(1.515)
N	609	611
–2 LL	599.313	748.09

*$p < .05$ (two-tailed).

NOTE: The region of common support for the propensity model for females ranges from a low of .03 to a high of .99. For males, the region of common support ranges from .04 to .83.