

# DOES MARRIAGE REDUCE CRIME? A COUNTERFACTUAL APPROACH TO WITHIN-INDIVIDUAL CAUSAL EFFECTS\*

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*Although marriage is associated with a plethora of adult outcomes, its causal status remains controversial in the absence of experimental evidence. We address this problem by introducing a counterfactual life-course approach that applies inverse probability of treatment weighting (IPTW) to yearly longitudinal data on marriage, crime, and shared covariates in a sample of 500 high-risk boys followed prospectively from adolescence to age 32. The data consist of criminal histories and death records for all 500 men plus personal interviews, using a life-history calendar, with a stratified subsample of 52 men followed to age 70. These data are linked to an extensive battery of individual and family background measures gathered from childhood to age 17—before entry into marriage. Applying IPTW to multiple specifications that also incorporate extensive time-varying covariates in adulthood, being married is associated with an average reduction of approximately 35 percent in the odds of crime compared to nonmarried states for the same man. These results are robust, supporting the inference that states of marriage causally inhibit crime over the life course.*

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*Your friends or me.*

—Spouse of an adult offender who desisted from crime

The association of marriage with a wide range of adult outcomes is well accepted but controversial. Whether crime, mortality, binge drinking, drug use, depression, employment status, or wages, the literature is replete with findings suggesting that marriage is linked to well being. The meaning of these associations is another matter altogether. Questions of selection and confounding are paramount. For example, we may observe that married men are less likely to commit crime or be unemployed than unmarried men, but problems with differential selection into marriage hamper causal conclusions. Yet unlike in some social experiments with housing vouchers or job training, we cannot randomly assign marriage partners. Research must thus rely on observational data that yield ambiguous results subject to alternative interpretations.

This paper addresses the challenge of causality in a long-term study of marriage and crime over the life course. Our approach is to extend “counterfactual” methods for time-varying covariates to a within-individual analysis of the role of marriage in the lives of 500 men who entered the transition to adulthood at high risk for continued involvement in crime. Committed to reform schools in Massachusetts during their adolescence in the 1940s, these men were the original subjects of a classic study of juvenile delinquency and its aftermath (Glueck and Glueck, 1950, 1968). Followed to age 32 by the Gluecks, the early and young-adult lives of these men were later investigated by Sampson and Laub (1993). The analysis in this paper is based on three sets of additional data. As described below, we first launched a 35-year follow-up study to age 70 in which we conducted state and national searches of both crime and death records for the original 500 delinquent men. Second, we tracked and conducted in-depth interviews with a targeted subsample of 52 of the men who varied in patterns of criminality in adulthood. During these interviews we administered a life-history calendar to assess yearly changes in key life events (for example, marriage, crime, and incarceration). Finally, we coded yearly data on key time-varying covariates for the full sample of 500 over the ages 17 to 32 from the original study’s data archives.

Unlike research that contrasts the outcomes of married with unmarried individuals, our strategy is to capitalize on variations within individuals over time, separating the effects of stable characteristics from change. We specifically capitalize on recent advances in counterfactual analysis for longitudinal data, proposing the basic idea of comparing the average causal effect of being married to being unmarried for the same person. By weighting for time-varying propensities to marriage over each year of the life course, our counterfactual strategy “thinks” like an experiment and provides an alternative to the static between-individual comparisons that

dominate the marriage and adult outcome literature. Of course, one can never definitively identify the causal influence of social arrangements on behavior, even in an experiment. Yet by modeling within-individual changes in the propensity to be married, we can at least come closer to the goal of explaining consequences for crime by bringing what is typically viewed as a nuisance—selection into and out of marriage—explicitly into the investigation.

## MARRIAGE MECHANISMS AND DESISTANCE FROM CRIME

*It is not how many beers you have, it's who you drink with that matters.*

—Wife of a man who desisted from crime after she insisted he switch drinking venues

The association of marriage with lower crime among men has been widely reported in both quantitative and qualitative studies (Blokland and Nieuwebeerta, 2005; Farrington and West, 1995; Horney, Osgood, and Marshall, 1995; Irwin, 1970; Maume, Ousey, and Beaver, 2005; Sampson and Laub, 1993; Shover, 1996; Warr, 1998; for a review, see Laub and Sampson, 2001). The idea of marriage as an inhibitor of male crime was illustrated by a former delinquent who had been married for 49 years when we interviewed him at age 70: “If I hadn’t met my wife at the time I did, I’d probably be dead. It just changed my whole life... that’s my turning point.” What is it about marriage that fosters desistance from crime? Consistent with themes articulated by offenders themselves, we highlight four processes.

First, a change in criminal behavior may occur in response to the attachment or social bond that forms as a result of marriage. This notion reflects a classical social control or “social bonding” perspective (Hirschi, 1969), wherein the social tie of marriage is important because it creates interdependent systems of obligation, mutual support, and restraint that impose significant costs for translating criminal propensities into action (Sampson and Laub, 1993).

A second reason marriage might influence desistance is because it leads to significant changes in everyday routines and patterns of association with others. It is well established that lifestyles and routine activities are a major source of variation in exposure to crime and victimization (Hindelang, Gottfredson, and Garofalo, 1978). Consistent with this theme, Osgood and colleagues (1996) showed that unstructured socializing activities with peers increased the frequency of deviant behaviors among those ages 18 to 26. Marriage has the potential to change such routine activities, especially with regard to deviant peer groups (Warr, 1998). As

Osgood and Lee (1993) argued, marriage entails numerous obligations that tend to reduce leisure activities outside of the family. Importantly, we do not assume a miraculous transformation, only that it is reasonable to assume that the same person, when married, will spend less time with same-sex peers than when not married (or before marriage). There is supporting empirical evidence for this hypothesis in the finding that the transition to marriage is followed by a decline in time spent with friends and exposure to delinquent peer groups, controlling for age (Warr, 1998: 183). Parenting responsibilities can also lead to changes in routine activities because more time is spent in family-centered activities than in unstructured time with peers.

Third, and perhaps more intriguing theoretically, marriage may lead to gendered desistance because of the direct social control exerted by female spouses. This seems particularly true of marriages in the 1950s and 1960s, when it was common for wives to limit the number of nights men could “hang with the guys,” thus affecting their associations with peers. Along with providing a base of social support, many wives in this era also took control of the planning and management of household activities and acted as informal guardians of their husbands’ social lives. Implicit was an obligation to family by the male partner, especially concerning economic support. Spouses provided additional support by exercising direct supervision. Umberson (1992), for example, hypothesizes that marriage is beneficial to health because spouses monitor and attempt to control their spouse’s behavior. She finds that women “nag” about health more than men and that men engage in more risky behaviors than women. In a similar vein, Waite and Gallagher (2000: 24) argue: “Marriage makes people better off in part because it constrains them from certain kinds of behavior, which, while perhaps immediately attractive (i.e., staying up all night drinking beer, or cheating on your partner) do not pay off in the long run.” From this viewpoint, marriage has the capacity to generate direct social controls, mainly in the form of supervision.

Fourth, marriage comes in a stylized “package” typically involving a number of identities, some of which can change one’s sense of self through cognitive transformation (Giordano, Cernkovich, and Rudolph, 2002). For some, getting married connotes getting “serious;” in other words, becoming an adult. Although it may now seem a bit retrograde, the men we study came of age when getting married meant “taking responsibility,” at least in theory. Patriarchal marriages meant having someone to care for and having someone to take care of you. This traditional view became even more evident once children entered the family. Cognitive mechanisms, then, have been hypothesized to account for the effect of getting married on desistance from crime (see also Hill, 1971).

An unanswered question is whether the hypothesized crime suppression benefits of marriage extend to those involved in cohabitation or other arrangements. Waite (1995) makes the case that married couples exhibit a greater sense of long-term responsibility and commitment toward each other than is evident in cohabitation. Another key difference involves legal obligations that extend over longer time horizons than typically seen in cohabitations. The data are conflicting on whether marriage yields different empirical results than cohabitation with respect to crime and deviance. Horney, Osgood, and Marshall (1995) showed that monthly within-individual variations in crime for a sample of high-rate convicted felons were negatively associated with marriage but positively associated with cohabitation, though the pattern varied by crime type. It also appears that women are at greater risk for physical abuse from men when they are in shifting cohabitating relationships as opposed to marital relationships (Stets, 1991).

Yet, examining a wider range of licit and illicit activities using National Longitudinal Survey of Youth data, Duncan, Wilkerson, and England (2003) found that both marriage and cohabitation were associated with decreases in binge drinking and marijuana use. The reductions, however, were greater in marriage compared with cohabitation for men and women. They conclude that “the social control provided by ‘social integration’ of marriage apparently works mostly through the normative expectations about how married persons behave” (10).

In short, our review provides theoretical motivation to suggest that marriage influences criminal behavior among men, especially those with damaged or high-risk backgrounds. We set aside the identification of what specific mechanism (for example, monitoring, social support, or norms) is at work, and focus instead on what we consider a prior, first-order issue: is the effect of marriage causal? If it is not, the question of mechanisms becomes moot. Moreover, our perspective extends Sampson and Laub (1993) by conceiving of marriage in dynamic terms rather than as a single turning point. The reality is that people enter and exit (and often re-enter) marriage through time, leading us to conceptualize the potential causal effect on crime of being married (which hypothetically could be randomly or exogenously induced) compared with being unmarried for the same person. Furthermore, we test the hypothesis that marriage has an effect even if marital attachment is low and men tend to partner with criminally inclined wives. Our focus is on the straightforward but powerful question of whether being married is linked to lower crime by men compared to periods of being unmarried. Whether among married men attachment is associated with crime is a separate question; indeed, Sampson and Laub (1993) restricted their analysis of attachment to the sample of married men.

By applying causal reasoning to the case of within-individual variations in crime by men, we necessarily set aside the question of whether marriage has an analogous effect on crime by women. Because males commit by far the lion's share of crime, on average men marry "up" and women "down" when it comes to exposure to crime and violence by a spouse in heterosexual unions. It thus follows that marriage may reduce women's well being even as at the same time it benefits their male partners. Feminist critics of marriage are justified in questioning generic arguments about "good marriage" effects (Stacey, 1998). Good for whom? one must ask. We look to other scholars to uncover the causal role of marriage, if any, in criminal offending by women.<sup>1</sup>

## A COUNTERFACTUAL APPROACH TO MARRIAGE

The biggest threat to the validity of any analysis claiming causal effects of a social state like marriage is to account for the nonrandom selection of individuals into that state. Marriage is not a random event and homophily in partner characteristics is well established, even though it is simultaneously true that fortuitous events influence mating patterns (Blau, 1977). To the extent that marriage is influenced by individual self-selection, the marriage-crime relationship is potentially spurious. Selection is thus the main critique put forth by those suspicious of the idea that adult social forces influence crime (for example, Gottfredson and Hirschi, 1990: 137–41, 163–67, 236–39).

The bottom line seems to be that whereas causal claims are frequent, albeit often ambiguously stated or rendered implicit, the strategies that are used to support them often fall well short (see the recent review in Moffitt, 2005). Take the ubiquitous "control variable" approach. Because marriage cannot be randomized in practice, the canonical solution to date has been to control a host of potentially confounding factors, most notably lagged states of crime itself and other factors that may cause both crime and later marriage, such as prior drinking or unemployment. But controlling past values of the treatment or outcome can easily lead to null or biased estimates because they control for the very developmental pathways that are hypothesized to lead to crime (compare Robins, 1999). A related strategy, used primarily in the area of marriage's effects on earnings, has been to use fixed-effects specifications, whereby unobservable individual (time-invariant) characteristics of those who do and do not marry are differenced out of equations estimating the marriage effect on earnings (Cornwell and Rupert, 1997; Gray, 1997; Korenman and Neumark, 1991;

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1. Evidence is strong, however, that marriage reduces violent victimization against women, especially intimate partner violence by men (Lauritsen and Schaum, 2004).

Light, 2004; Stratton, 2002). However, these studies do not eliminate potential threats to validity based on unobservable time-varying confounding. Controlling for endogenous time-varying confounders can also induce a correlation between the treatment and response even when no causal association exists (Robins, 1999). Moreover, studies using fixed effects and endogenous controls have yielded widely conflicting estimates of whether there are causal effects of marriage.

Another strategy has been to compare married and unmarried men within families, either by comparing brothers (Loh, 1996) or monozygotic twins (Antonovics and Town, 2004; Krashinsky, 2004). These studies have the benefit of eliminating unobserved bias common to brothers or twins (for example, bias due to genetics or family background), but they do not eliminate unobserved selection bias occurring within families (for example, if two brothers differ in personality characteristics that lead one to marry and earn more and one to not marry and earn less). Conflicting results are again common, with one study showing strong and robust effects of marriage (Antonovics and Town, 2004), and two others showing weak and inconsistent effects (Krashinsky, 2004; Loh, 1996). These strategies also focus on the impact of marriage on wages, with the result that we know little about the potential causal effect of marriage on crime or other correlated behaviors, such as drug use and high-risk sex behaviors.

Yet another strategy common in economics is to use instrumental variables to identify the causal effect of a social variable on some outcome (Moffitt, 2005: 95). In practice, however, finding plausible instruments that can pass the necessary identifying restrictions has proven very difficult. One person's instrument is often another person's hypothesized cause of the outcome in question—unfortunately, the data under study cannot be used to adjudicate in these sorts of debates. More practically, however, we are not aware of any plausible instrument for identifying the causal effect of marriage on crime in the literature to date.

We address this conundrum through a multipronged approach that combines a hierarchical longitudinal analysis of changes in marriage and crime over the life course with recently pioneered methods for identifying causal effects using observational data—what are typically called “counterfactual methods” of causal inference. Drawing from the language of randomized experiments, counterfactual methods conceptualize causality in terms of the effect of a definable treatment (for example, marriage) on some outcome (for example, likelihood of committing a crime). In this case, one divides the sample population into a treatment group (those who marry), and a control group (those who do not marry). When examining the causal effect of the treatment, counterfactual methods assume that each individual has two potential outcomes, at least

theoretically. The first is the outcome that the individual demonstrates under the treatment condition, which we will call  $Y_i^t$ . The second is the outcome that the individual demonstrates under the control condition, which we will call  $Y_i^c$ . For each individual, however, only one of these outcomes can be observed at the same time. We can thus recast questions of causality as a “missing data problem” of the unobserved counterfactual (Winship and Morgan, 1999), one that is solved in experimentation through randomization. Assuming equivalence of controls and treatments, in other words, permits the estimation of the causal effect,  $\bar{Y}_t - \bar{Y}_c$ .

When dealing with a treatment at one point in time, a statistical approach is propensity score matching (for a formal discussion, see Rosenbaum and Rubin, 1983a; for empirical examples in the social sciences, see Harding, 2003; Morgan, 2001). With this technique, one can model the propensity that each individual receives the treatment, and then create two groups by matching those who did or did not receive the treatment on this propensity score. This strategy has been shown to yield consistent and unbiased estimates of causal effects, as long as all potential confounding factors are included in the model used to create the propensity score. In essence, the surprising outcome is that matching on the propensity score fully balances the treatment and control groups on all of the covariates used in modeling the propensity of receiving the treatment, allowing the researcher to identify the causal effect by estimating  $\bar{Y}_t - \bar{Y}_c$ .

To deal with unmeasured confounding, researchers can also perform a sensitivity analysis whereby one examines how the estimates of the causal effect would change under different assumptions about unmeasured factors,  $U$ , and their association with the likelihood of receiving the treatment and demonstrating the outcome (Rosenbaum and Rubin, 1983b; Harding, 2003). Haviland and Nagin (2005) have integrated the Rosenbaum and Rubin approach with a group-based modeling of trajectories (Nagin, 1999, 2005) to estimate the impact of a first-time treatment (gang membership) on later violence.

## INVERSE PROBABILITY-OF-TREATMENT WEIGHTING (IPTW)

We build on these advances in the case where the treatment, confounding covariates, and outcome all vary over time. In this situation, Robins and various colleagues (Hernán, Brumback, and Robins, 2000, 2002; Robins, 1986, 1987, 1999; Robins, Hernán, and Brumback, 2000) have shown that estimates of causal effects may be biased when: time-dependent covariates predict both the outcome of interest and subsequent exposure to the treatment, and past exposure history of the treatment



predicts the time-varying confounder. Consider the simple example with one time-varying treatment (such as marriage) over 2 years,  $X_1$ , and  $X_2$ , one time-varying outcome (such as crime),  $Y_1$ , and  $Y_2$ , and one time-varying confounder (such as wages),  $Z_1$  and  $Z_2$ . Assume we want to identify the causal effect of  $X_2$  on  $Y_2$ . If we do not control for  $Z_1$ ,  $Y_1$ , and  $Z_2$  as covariates, using either traditional regression techniques or propensity score methods, we will bias our treatment effect estimates because  $Z_1$ ,  $Y_1$ , and  $Z_2$  are all likely to predict  $X_2$  (getting the treatment in year 2) and  $Y_2$  (committing a crime in year 2). Yet we also have a problem if we control for  $Z_2$  and  $Y_1$  because they are both outcomes of the original treatment  $X_1$ . It can thus be seen that the typical panel models that control for lagged  $X$ s and  $Y$ s are inherently flawed.

Robins and colleagues (see Hernán, Brumback, and Robins, 2000; Robins, Hernán, and Brumback, 2000) have pioneered an innovative method for dealing with this problem—inverse probability-of-treatment weighting (IPTW). They show that bias and the inducement of artificial correlations between treatment and outcome can be appropriately dealt with by fitting a model that weights each subject  $i$  at time  $k$  by a weight consisting of the inverse of the predicted probability that the subject received the treatment they actually received at time  $k$  given prior treatment history, time-varying covariate history, and baseline (time-invariant) covariates. More formally,

$$W_i(t) = \frac{1}{\prod_{k=0}^t f[A_i(k) | \bar{A}_i(k-1), \bar{L}_i(k)]}$$

where  $A_i(k)$  is subject  $i$ 's treatment status at time  $k$ ,  $\bar{A}_i(k-1)$  is subject  $i$ 's treatment history up to time  $k-1$ , and  $\bar{L}_i(k)$  is a vector of both time-varying and time-invariant covariates, which, depending on the nature of the treatment and the outcome, can also include subject  $i$ 's outcome history up to time  $k-1$ . In using IPTW weights we borrow more information from cases with smaller probabilities of receiving the treatment at any given period given selection factors such as treatment and covariate history. Barber, Murphy, and Verbitsky (2004) provide an excellent and more detailed description of the IPTW logic for time-varying confounders in survival analysis.

Simply using the weights defined thus far, however, can lead to highly inefficient and unstable estimates when some subjects have very low probabilities of receiving the treatment that they received (that is, they would be assigned exceedingly high IPTW weights and these small numbers of observations would dominate the analysis). Robins, Hernán,

and Brumback. (2000) propose the use of an alternative set of “stabilized weights,” which (in words) consist of a denominator—the probability that the subject received his own observed treatment at time  $k$ , given past treatment and covariate history—and a numerator, the probability that the subject received his own observed treatment, given past treatment history but not further adjusting for covariate history. More formally

$$SW_i(t) = \prod_{k=0}^t \frac{f[A_i(k) | \bar{A}_i(k-1)]}{f[A_i(k) | \bar{A}_i(k-1), \bar{L}_i(k)]}$$

In short, IPTW models address fundamental problems associated with estimating causal effects of time-varying treatments on outcomes. Rather than creating potential biases by including endogenous confounders, either as control variables or as part of a model creating a propensity score, IPTW methods weight each person-period by the inverse of the predicted probability of receiving the treatment status that they actually received in that period. Analogous to survey weights, IPTW models create a “pseudo-population” of weighted replicates, allowing one to compare times when one does and does not experience the “treatment” of marriage without making distributional assumptions about counterfactuals. IPTW models thus also allow a strategy to properly deal with potentially complex parametric causal pathways between time-varying treatments, time-varying covariates, and time-varying responses (Ko, Hogan, and Mayer, 2003). In the following section we describe our data and measures, followed by a tailored implementation of the IPTW model.

## STUDY DESIGN

Our main source of data comes from a long-term follow-up of the original subjects studied by Glueck and Glueck in *Unraveling Juvenile Delinquency* (1950). The Gluecks’ study of juvenile and adult criminal behavior involved a sample of 500 male delinquents ages 10 to 17 and 500 male nondelinquents ages 10 to 17 matched on age, ethnicity, IQ, and low-income residence. Over a 25-year period from 1940 to 1965, a wealth of information was collected in childhood, adolescence, and adulthood (Glueck and Glueck, 1950, 1968). Subjects were originally interviewed at an average age of 14, at age 25, and again at age 32 with only 8 percent attrition. Data reconstruction and an analysis of continuity and change in crime for the Glueck men up to age 32 were described in *Crime in the Making: Pathways and Turning Points Through Life* (Sampson and Laub, 1993).<sup>2</sup>

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2. This paper seeks to estimate the causal effects of marriage on desistance from crime in adulthood. We therefore start with the group of high-rate offenders

The men were born between 1924 and 1932 and grew up in central Boston. When we launched a follow-up study in 1994, the oldest subject was nearing 70 and the youngest was 61. We collected three sets of data on the men: criminal records, both at the state and national levels; death records, also at the state and national levels; and interviews with a targeted subset of original delinquent subjects. We briefly describe each in turn; additional details on all aspects of the research design and data collection can be found in Laub and Sampson (2003).

Criminal records were manually searched at the Massachusetts Office of the Commissioner of Probation for 475 of the original 500 delinquents.<sup>3</sup> Operating since 1926, the Office of the Commissioner of Probation is the central repository of criminal record data for the state of Massachusetts. These data allowed us to update the official criminal history for the delinquents in the Glueck study after age 32, but do not provide information for those subjects who moved out of state or for those who reside in the state, but may have committed crimes out of state. We thus collected criminal histories from the FBI and coded all arrests after age 32 that did not appear in the Massachusetts criminal histories, consisting mostly of arrests that occurred out of state (Laub and Sampson, 2003: 63–65). Because of the rarity of crime at older ages, we focus on total crime counts from these combined state and national records.

By definition, official criminal records pertain only to offenses that came to the attention of the criminal justice system. Although limited in this way, official data capture serious offenses (such as robbery) fairly well (Blumstein et al., 1986). In Massachusetts, criminal histories contain a surprising amount of “nonserious” crime as well.<sup>4</sup> The wide range of offenses captured is important given the lack of specialization in criminal careers (Blumstein et al., 1986). Our strategy is to analyze within-individual trajectories of propensity to crime and not the comparison of different groups or cohorts of men with different characteristics often thought to influence official processing (for example, race-ethnicity and social class). For example, it is hard to imagine why a 45-year-old man,

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defined by the delinquent group sample in the Gluecks’ original design, a group that generated almost 10,000 offenses over the life course (Laub and Sampson, 2003: 86). Although the question of marriage and crime is of interest in the non-delinquent group as well (Sampson and Laub, 1993: chapter 8), we do not pursue counterfactual estimates in this group because crime and delinquency were so rare.

3. Twenty-five subjects died during the original follow-up to age 32 and were not included in our records search. Although the Gluecks collected data for 438 subjects at all three waves, we used as our base for the criminal record searches all subjects alive at age 32 (N = 475).
4. To illustrate, there were 1,802 arrests for alcohol and drug offenses and more than 3,000 arrests for offenses such as disorderly conduct, gambling, and failure to pay child support.

compared to the same man at age 40, would be any more or less likely to be arrested given an offense. The criminal records from the Massachusetts Office of the Commissioner of Probation have also been validated in prior research (Sampson and Laub, 1993; Vaillant, 1983) and FBI rap sheets have for a long time been considered the gold standard in criminological research on criminal careers (Blumstein et al., 1986). Perhaps most important, age-specific self-reports of crime recalling back over 35 years are no less limited.<sup>5</sup>

We searched the Massachusetts Registry of Vital Records and Statistics for all 475 subjects from their 32nd birthday onwards, unless an arrest date showed that a later search date was appropriate. (We already knew the dates and the cause of death for the 25 subjects who died during the Gluecks' study). Once a record of death was found, we purchased the death certificate from the Registry. Next, we conducted a search for the remaining living men using the National Death Index (NDI) maintained by the National Center for Health. We searched this index and uncovered additional deaths, both in Massachusetts and out of state. From these sources we coded all dates of death and integrated them into the longitudinal data on criminal histories.

## TRACING AND FINDING SUBJECTS

A key part of our follow-up study involved tracking and conducting detailed interviews with a targeted subset of the original delinquent subjects. After setting aside those men who had died ( $N = 245$ ), phone books (paper and electronic), Web-based search engines like [www.switchboard.com](http://www.switchboard.com), criminal records, death records, motor vehicle records, and voter lists were used to locate the vast majority of men. In addition, records from the Massachusetts Department of Corrections and the Massachusetts Parole Board were used in our search. The Cold Case Squad of the Boston Police Department helped us find the most difficult to locate cases. Of the 230 members of the study who were alive and thus eligible for an interview at the time of the follow-up study, we located reliable information on 181 men, yielding a location rate of 79 percent.

We sought to yield maximum variability in trajectories of adult crime and so using the criminal history records we classified eligible men into strata that reflected persistence in crime, desistance, and “zigzag”

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5. We did ask about self-reported crime during the interviews, but the retrospective placing of events within specific years going back 30 to 40 years proved too difficult for the men. Wherever possible, however, we compared life-history narratives with major official events to discover anomalies. For major crime episodes, the interviews with the men yielded a generally consistent picture (Laub and Sampson, 2003: 251–52).

offending patterns, including late onset and late desistance of violence (Laub and Sampson, 2003: 66–67). Our initial goal was to complete about 40 in-person interviews but with the 35-year gap we anticipated less than a 50 percent completion rate. Given limited resources, 40 of the located men were reserved as possible replicates for future study if funds permitted—but no attempt was ever made on our part to contact them. Of the pool of 141 men selected, we interviewed 52. Twenty-seven refused (this included those who did not respond to messages left on answering machines); nine were willing but seriously ill and therefore declined; 53 had an unlisted phone number and never responded to our mailings. Because IRB restrictions prevented us from contacting these men in person, their ultimate eligibility status remains unknown.

Therefore, of those men we contacted about the study (N = 88), 52 (59 percent) were interviewed and 36 (41 percent) refused or were unable for health reasons to be interviewed. Eliminating refusals due to illness, our rate of interview participation was 66 percent of known eligibles. Both participation figures were beyond what we expected and compare favorably with other long-term follow-up studies with high risk samples (for example, McCord and Ensminger, 1997).

For the 52 interviews, we developed a modified life-history calendar (Freedman et al., 1988) to help subjects place major life events (such as marriages, divorces, births of children) in time.<sup>6</sup> For the narrative portion of the study we used an open-ended interview schedule that covered a variety of life-course domains and retrospective views of one's life, including self-perceived turning points. Although life-history calendars with a long retrospective window should be used with caution (Henry et al., 1994), a "catch-up study" such as ours really has no alternative.

Analyses not presented in tabular form demonstrated that our stratified sampling strategy in the follow-up captured variability in crime while maintaining representativeness relative to the larger group. Namely, when we compared the 52 men to the rest of the delinquent group on a wide range of variables including risk factors in childhood, measures of delinquency in adolescence, and numerous adult outcomes, the differences were surprisingly nil. Of 23 comparisons and formal tests, there was only one significant difference, almost exactly what one would expect by chance at the .05 level (results available on request). These results allow us

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6. Because of the long retrospective window in our life-history calendar, we developed a series of "memory markers" to help the subjects place events in time. Some of these markers were universal (for example, the year of the assassination of President John F. Kennedy) and others were specific to locale of the study (for example, the digging of the tunnel connecting Boston to Logan Airport; perhaps most important, World Series appearances by the Boston Red Sox, which occurred in nearly each decade of our follow-up study—1967, 1975, and 1986).

to conclude that neither our sampling stratification scheme nor interview-based attrition (including death) served to create a sample of interviewed men distinct from the pool from which they were drawn.

## PREDICTING MARRIAGE

For the IPTW estimation techniques to properly eliminate confounding and provide a robust estimate of the causal effect of marriage, we must specify a proper model of nonrandom selection into states of marriage. We thus cast a wide net and selected time-varying and time-invariant covariates that cover factors from early childhood to each year of adult life.

One broad set of predictors cover baseline or “pretreatment” covariates measured prior to entry into first marriage. These include individual differences such as measured intelligence, competence, self control, and temperament that social scientists have largely ignored as relevant to the marriage selection process (see Clausen, 1993; Gottfredson and Hirschi, 1990). Family background factors such as childhood poverty and parental mental health status have also been largely neglected even though on theoretical grounds we expect individual propensities formed early in life to have long-lasting consequences for the later formation of social bonds in marriage and other walks of life. We show below that correcting these oversights is empirically warranted; childhood individual differences such as IQ and competence predict marriage many years later.

We also cover the more traditional factors identified in the literature as important predictors of men’s entry into marriage. The most consistent and robust predictors center around employment and economic potential (Avery, Goldscheider, and Speare, 1992; Clarkberg, 1999; Goldscheider and Waite, 1986; Lloyd and South, 1996; MacDonald and Rindfuss, 1981; Oppenheimer, Kalmijn, and Lim, 1997; Smock and Manning, 1997; Sweeney, 2002; Xie et al., 2003; Yamaguchi and Kandel, 1985). Those who are employed and who demonstrate greater economic potential are more likely to enter into marriage across many samples and time periods, as such men are likely to be both more mature and more attractive as potential marriage partners (for a discussion of theories of marriage timing, see Oppenheimer, 1988; Xie et al., 2003). Educational attainment also positively predicts entry into marriage (Avery, Goldscheider, and Speare; Clarkberg, 1999; Goldscheider and Waite, 1986; MacDonald and Rindfuss, 1981; Mare and Winship, 1991), though in our data education is very truncated—most of the men were high school dropouts (Laub and Sampson, 1998).

A number of studies have looked at men’s engagement in risky and antisocial behaviors, such as heavy drinking, illicit drug use, and crime, finding that engagement in such behaviors negatively predicts entry into

marriage (Sampson and Laub, 1993, chapter 6; Yamaguchi and Kandel, 1985). Interestingly, though, one study finds that early engagement in substance use may accelerate entry into very early marriages (Martino, Collins, and Ellickson, 2004). Findings for other factors have been more inconsistent. Men's military status inconsistently predicts marriage, with some studies showing positive relationships and others negative (Goldscheider and Waite, 1986; MacDonald and Rindfuss, 1981; Oppenheimer, Kalmijn, and Lim, 1997) depending on when military status is measured and how it is operationalized. Childhood family structure also shows mixed results (Avery, Goldscheider, and Speare, 1992; Clarkberg, 1999; Jiang and Wojtkiewicz, 1995; Lloyd and South, 1996; see review by Wolfinger, 2003), with discrepant findings potentially related to changes over time in the relationship between childhood family structure and adult marital decision making in different historical periods (Wolfinger, 2003). Finally, men's mental health largely seems unrelated to marital entry (Brown, 2000; Horwitz, White, and Howell-White, 1996; Lamb, Lee, and DeMaris, 2003; Simon and Marcussen, 1999).

## MEASURES

At the person level we examine 10 individual-specific differences and 10 family and parental background factors exogenous to age 17. Individual differences are comprised of five measures:

1. intelligence assessed using the Wechsler-Bellevue Full-Score IQ test (mean IQ = 92)
2. a composite scale (ranging from 1 to 26) based on self-, parent-, and teacher-reports of delinquent behavior (such as stealing, vandalism) and other misconduct (such as truancy, running away) not necessarily known to the police (mean number of offenses = 14.21)
3. age at first arrest (mean = 11.92)
4. total number of days incarcerated up to age 17 (mean = 553 or 1.52 years)
5. a multi-item scale of adolescent competence (ranging from 0 to 6) that includes ambitions, planfulness, conscientiousness, attitudes towards school, and actual school grades (mean = 1.24)

From a detailed and independent psychiatric assessment of each boy, five dichotomous indicators tapping personality are examined:

1. extroversion defined as "uninhibited in regard to motor responses to stimuli" (56 percent of the delinquent sample)
2. adventurousness defined as "desirous of change, excitement, or risk" (55 percent of the delinquent sample)

3. egocentricity defined as “self-centered, inclined not to make allowances for others, selfishly narrow in viewpoint” (14 percent of the delinquent sample)
4. aggressiveness defined as “inclined to impose one’s will on others” (15 percent of the delinquent sample)
5. stubbornness defined as “resistive or persistent” (41 percent of the delinquent sample)

For more details on all of these measures, see Glueck and Glueck (1950: 245–47), Laub and Sampson (1998), and Sampson and Laub (1993).

The 10 indicators of family and parental background are derived from home interviews and supplemental records and include the following measures.

1. Family poverty status is a standardized scale drawing on information on the family’s average weekly income and the family’s reliance on public assistance (mean = .00).
2. Parental education is coded 1 if either of the boy's parents attended or graduated from high school (27 percent of the parents of delinquents attended or completed high school).
3. Residential mobility is the number of times the boy’s family moved during his childhood (ranges from 1 to 16 with a mean of 8.67).
4. Mother’s supervision is defined as suitable, fair, or unsuitable depending upon whether the mother monitored the boy’s activities in the home and in the neighborhood (mean = 1.43).
5. Immigrant status indexes whether one or both parents were born outside the United States (58 percent of the parents of delinquents were born outside of the United States).
6. Family size is defined as the number of children in the boy’s family (mean = 5.44 children).
7. Erratic–threatening discipline measures the degree to which parents used inconsistent disciplinary measures in conjunction with harsh, physical punishment and/or threatening or scolding behavior (mean of standardized scale = -.08).
8. Family disruption is coded 1 when the boy was reared in a home with one or both parents absent because of divorce, separation, desertion, or death (61 percent of the delinquents).
9. Criminality-alcoholism of parent or parents (ranges from 0 to 4) is determined by official records of arrest or conviction along with reports from interviews of frequent, regular, or chronic addiction to alcohol (mean = 1.97).
10. Mental disorder of parent or parents (ranges from 0 to 2) draws on medical reports to capture whether the boy’s parents were diagnosed with “severe mental disease or distortion” including



“marked emotional instability,” “pronounced temperamental deviation,” or “extreme impulsiveness” (mean = .87).

For more details on all of these measures, see Glueck and Glueck, (1950), Sampson and Laub (1993: 71–77), and Sampson and Laub (1994). Overall, the 20 measures cover a comprehensive multimethod and multiscore inventory of between-individual predictors of adult development and marriage, including child and adolescent behavior, ability, personality, social background, parental personality-criminality, and family processes.

Finally, we exploit the prospective longitudinal nature of the yearly data and coded the time-varying treatment, confounders, and crime. Because of our focus on the life course, we chose age-years as the unit of within-individual change. We coded person-year observations for each of the men beginning at age 17, marking the transition to young adulthood and potentially a first marriage. Based on the combination of Massachusetts criminal histories, national FBI search, and death records, we coded the number of criminal events for each year ages 17 to 32 in the full delinquent group, and ages 17 to 70 in the interviewed follow-up sample, in both cases adjusting for mortality. From official records and interviews we coded the number of days free from incarceration each year for both samples. Using marriage dates from the Glueck archives up to age 32 and from the life-history calendars from age 32 onward, we coded whether each man was married (1 = yes, 0 = no) during each year from 17 to 70. For the full delinquent group, we coded yearly episodes of military service, number of subject’s biological children in the household, and stable employment.<sup>7</sup> Each of these factors has been posited as a predictor of marriage and hence a confounder for estimating the effect of marriage on crime.<sup>8</sup> For the long-term follow-up (ages 32 to 70), we were unable to reliably measure age-specific employment.<sup>9</sup> We were, however, able to

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7. Employment at each age to 32 takes on the value 1 if the person had one job and was employed for at least 8 months during the year, and 0 otherwise. Military service was coded as 1 if the person was in the military for any portion of the yearly period, 0 otherwise. If a child was born during the age year, children in the household was coded a 1, and 0 otherwise.

8. The percentage of men that produced at least one change in status over time was large: 90 percent for marriage, 87 percent for steady employment, and 64 percent for military service. Crime also changes significantly within persons over time, such that all 52 men in the interview follow-up contributed at least one change in criminal status over the adult yearly observations. The total variation in the event rate of criminal offending while free that lies within individuals (over time) is greater than 60 percent for men ages 17 to 70.

9. Because of the long recall (35 years) we were unable to collect reliable employment data by year past age 32. Unlike for marriage or divorce events, it was common to get very general answers or for the men to forget sequences of employment over this long period.

determine stable cohabitation independent from marriage, which allows us to specify cohabitation as potential confounder in modeling age-specific probabilities of marriage.

## MARRIAGE PREDICTION

The above covariates correspond with those factors identified in the literature as predicting men's selection into marriage, in addition to a broad battery of predictors that have received little attention in the literature, such as IQ, personality characteristics, and childhood residential mobility. Table 1 supports our selection model with the findings for our vector of predictors of marriage in both the 17 to 32 and the 17 to 70 samples. Interestingly, out of 20 possible relationships for the 10 individual-difference constructs with later married person-years, 17 are significant. The only individual difference that fails to predict later marriage in both groups is aggressiveness. Indicators such as competence and personality characteristics are strongly predictive of marriage probabilities. The 10 family-background measures emerge as somewhat less important predicting marriage during ages 17 to 32, yet for the long-term prediction among the 52 men from ages 17 to 70, only the mental disorder of parents fails to predict later marriage. Considering the long time frame, individual differences and family background are surprisingly consistent in predicting selection into marriage from 17 to 70.<sup>10</sup>

The time-varying covariates are the strongest and most consistent predictors. In fact, out of 25 possible predictions, all are significant. Age, age<sup>2</sup>, marriage history, criminal-deviance histories, military service, children in the household, and steady employment (cumulative and most recent) are particularly sturdy predictors, consistent with expectations. Cohabitation among the 52 men is also negatively related to later marriage probability. Finally, the cumulative IPTW models are highly significant. For the 17 to 32 sample, with 31 degrees of freedom among predictors, the model  $\chi^2$  is over 1,000 ( $p < .000$ ). Again, in somewhat of a surprise, our selection model does a better job in the 52 men sample ( $\chi^2$  over 2,000,  $p < .000$ ). The longer the life course is observed, the better we do in predicting marital history. These results are not only interesting in and of themselves; they provide further confidence in the IPTW strategy.

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10. Many of the predictors in table 1 are highly correlated, of course, but the purpose of the weight construction is not to estimate their independent "effects." The goal is to form a well-specified overall model with as many pre-treatment covariates as possible. The multivariate model fit shows that our set of predictors reasonably accounts for later selection into marriage.

**Table 1. Logistic Regression Coefficients and Standard Errors: Bivariate Predictions of Person-Period Marriage**

<i>Individual Differences, &lt; 17</i>	<i>Ages 17 to 32</i>	<i>Ages 17 to 70</i>
Measured IQ	.006** (.002)	.005 (.003)
Competence	.111*** (.023)	.331*** (.043)
Delinquent behavior	-.016** (.006)	-.050*** (.010)
Age at first arrest	-.023+ (.012)	-.056** (.019)
Days incarcerated up to age 17	-.0005*** (.0001)	.0004** (.0001)
Extroversion	.184*** (.052)	-.190* (.094)
Adventurousness	.158** (.051)	.186* (.082)
Egocentricity	-.279*** (.077)	.586*** (.127)
Aggressiveness	-.042 (.072)	.162 (.103)
Stubbornness	.152** (.051)	.576*** (.082)
<i>Family-Parental Background</i>		
Family poverty	-.031+ (.016)	.203*** (.026)
Parental education	.039 (.058)	.635*** (.098)
Residential mobility	-.015** (.005)	.062* (.009)
Mother's supervision	-.026 (.040)	.207** (.067)
Immigrant status	.083 (.052)	.249** (.080)
Family size	-.001 (.012)	-.083*** (.020)
Erratic-threatening discipline	-.033+ (.017)	.060* (.025)
Family disruption	-.243*** (.052)	-.303*** (.081)
Criminality-alcoholism of parents	-.015 (.020)	.148*** (.032)
Mental disorder of parents	.007 (.034)	.068 (.058)

<i>Adult Time-varying Covariates</i>	<i>Ages 17 to 32</i>	<i>Ages 17 to 70</i>
Age	.197*** (.007)	.027*** (.003)
Age <sup>2</sup>	.004*** (.0001)	.0002*** (.00003)
Married in last person-period	4.85*** (.101)	5.93*** (.187)
Cumulative sum of years married up to last person-period	.674*** (.018)	.107*** (.005)
Any arrest in last person-period	-.532*** (.059)	n/a
Cumulative sum of criminal events up to last person-period	-.024*** (.005)	-.041*** (.003)
Days incarcerated in last person-period	-.006*** (.0003)	-.007*** (.00049)
Cumulative sum of days incarcerated up to last person-period	-.0004*** (.00003)	-.0002*** (.00002)
In military in last person-period	-.410*** (.071)	n/a
Cumulative sum of in military person-periods up to last person-period	.213*** (.013)	n/a
Had a child in last person-period	2.45*** (.114)	n/a
Cumulative sum of number of children in household in past person-periods	.145*** (.006)	n/a
Steady employment in last person-period	.854*** (.054)	n/a
Cumulative sum of steady employment in past person-periods	.262*** (.010)	n/a
Lagged pooled violent crime count	n/a	-.639*** (.190)
Lagged pooled property crime count	n/a	-.936*** (.125)
Lagged pooled drug crime count	n/a	-.669*** (.112)
Stable cohabiting relationship in last person-period	n/a	-3.96*** (.590)
Cumulative sum of steady cohab relationship in past person-periods	n/a	-.418*** (.053)
Full multivariate IPTW model fit	$\chi^2 = 1071.96$ 31 d.f., $p < .000$	$\chi^2 = 2109.72$ 34 d.f., $p < .000$

Note: n/a = not applicable, because covariates included in the models generating the IPTW weights differed between the two samples due to data availability.

+  $p < .10$  \*  $p < 0.05$  \*\*  $p < .01$  \*\*\*  $p < .001$

Figure 1 displays the person-period distribution of the probability of crime (dichotomized here for descriptive purposes) and marriage from ages 17 to 70.<sup>11</sup> Crime levels off and begins to decline, on average, during early adulthood—the period of a rapid rise in the probability of marriage. Although the mean age at first marriage is the early 20s, the highest proportion of person-years that men are married is in the 40s and 50s, after which it slowly declines in older age, due primarily to widowhood. These patterns are consistent with prior literature and the hypothesis that marriage is potentially a causal factor in explaining desistance trajectories of crime. Figure 1 and table 1 both demonstrate the strong quadratic pattern to age and marriage, which we explicitly model in the IPTW specification. We now turn to formal estimates of causality.

## IPTW MODELS OF WITHIN-INDIVIDUAL CHANGE

To calculate both the nonstabilized and stabilized IPTW weights, we first arranged the data longitudinally by person-years. We then estimated a series of pooled logistic regression models with time-dependent intercepts to determine the predicted probability of receiving the treatment status that the subject actually received in year  $k$ . To account for the non-independence of observations over time within persons, we used the Huber-White adjustment for standard errors (StataCorp, 2003: 270–75) clustered on the person. For the calculation of the weights' denominator, these pooled logistic models took the form:

$$\begin{aligned} \text{Logit pr } [A_k = 1 | A_{k-1} = a_{k-1}, L_k = l_k] = \\ \alpha_0 + \alpha_1(\text{age}) + \alpha_2(\text{age}^2) + \alpha_3(a_{k-1}) + \bar{\alpha}_j(\bar{l}_{k-1}) \end{aligned}$$

where age is modeled as a quadratic function,  $a_{k-1}$  represents the respondent's treatment history, and  $l_{k-1}$  is the vector of time-varying and time-invariant covariates as specified above. We entered the vector of both lagged (that is, stable employment in the prior year) and cumulative history (that is, number of years prior with stable employment) measures for time-varying covariates, with the result that we generated two predictors for each time-varying confounder.

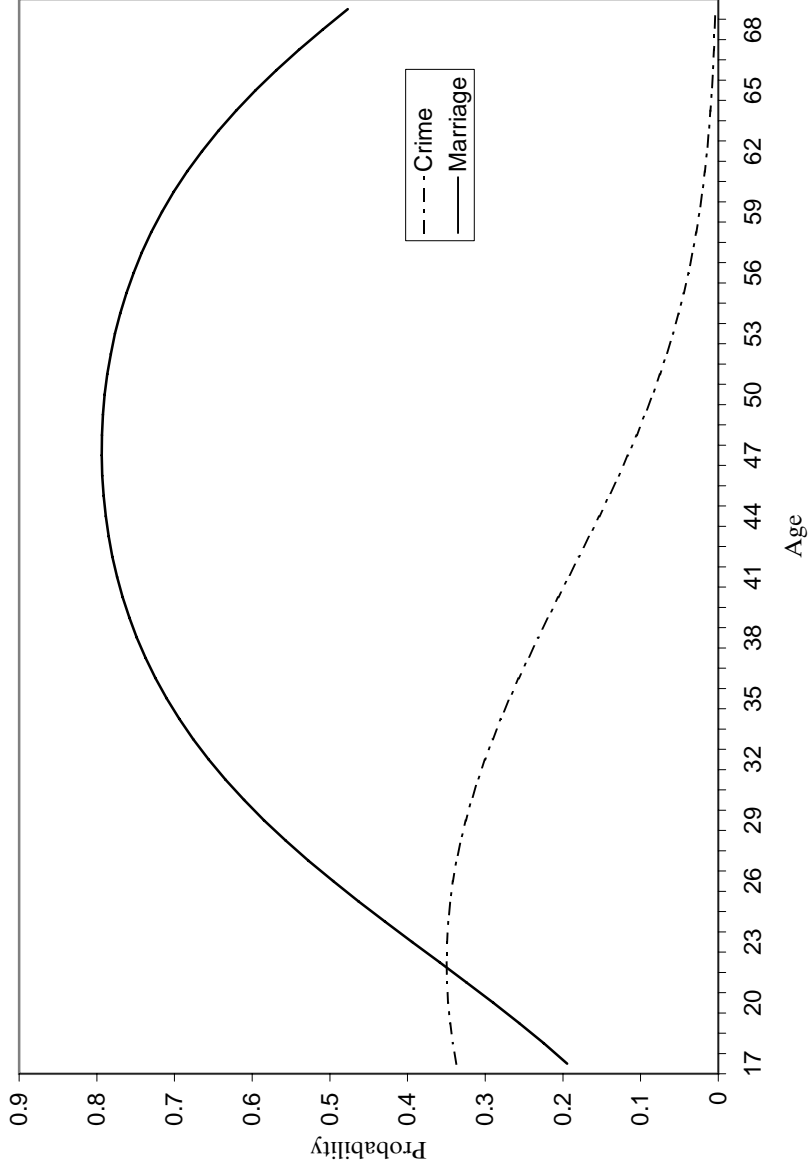
To derive the numerator of the stabilized weights, we estimated a pooled logistic regression model of the form:

$$\begin{aligned} \text{Logit pr } [A_k = 1 | A_{k-1} = a_{k-1}, L_k = l_k] = \\ \alpha_0 + \alpha_1(\text{age}) + \alpha_2(\text{age}^2) + \alpha_3(a_{k-1}) \end{aligned}$$

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11. Figure 1 excludes person-periods when a subject was incarcerated the entire year (N = 171). The pattern in figure 1 was identical when these periods were included.

Figure 1. Predicted Crime and Marriage Probabilities by Age (Quadratic Model, N=2,585 Person-Years)



We modeled the treatment history as a cumulative history of the number of years up to time  $k-1$  that the respondent had been married, along with a “recency” parameter for whether the respondent was married in the previous person-year. The cumulative history and lagged state of marriage independently predicted the present state of marriage in the multivariate estimation of the IPTW weights.

To give an example, married men who have a high probability of being married at any given age based on their marital, criminal, employment, military, and childbearing history would effectively be “downweighted” in the IPTW analysis for that year. Such person-periods reflect a higher degree of “selection” into the observed treatment status given values on confounding covariate histories that make them especially likely to be married (or unmarried). As a result, we do not want them to contribute as much information to the estimation of the causal effect of marriage on crime. On the other hand, married men with low probabilities of being married (but who actually marry) at a given age provide more useful information, and are therefore “upweighted” when estimating the final causal effect. An examination of the calculated IPTW weights confirmed that those person-periods with high degrees of selection on the observed covariates were appropriately downweighted, whereas those with lower degrees of selection present on the observed covariates were appropriately upweighted.<sup>12</sup>

To ensure the robustness of results we created two additional sets of weights. First, we estimated the same model but used a stepwise estimation procedure that eliminated redundant predictors, setting a  $p$ -value level of .2 or higher for a covariate to be excluded from the model. Second, and we believe more important, we split our sample into person-periods where the respondent was married in the prior person-year and where the respondent was unmarried in the prior person-year. We then re-estimated the full pooled logistic regression models on each subsample and recalculated the predicted probabilities for the weight construction

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12. Applying the stable unit treatment value assumption (SUTVA; see Rosenbaum, 2002), we make the reasonable assumption of no interaction across units—one man’s response to marriage is not conceived as dependent on another subject’s response. (The study started with 500 boys spread across the entire city of Boston and, in adulthood, extending well beyond.) Marriage is also defined equivalently across all subjects even though the quality of marriage varies, as discussed later. Within-subject correlation of errors is explicitly accounted for in our hierarchical change model described in the next section. We further assume “sequential ignorability and randomization” or that marriage propensity does not depend on unobservables after accounting for observed covariates, prior treatment history, and outcomes—the thought experiment is that marriage is randomized within levels of prior variables for each person.

from the results of the appropriate model.<sup>13</sup> This latter procedure allows us to treat the covariates in our selection model as fully interactive with the complete prior marital history of respondents. We report the results from all three IPTW weighting procedures below.

## HIERARCHICAL IPTW MODELS OF CRIMINAL PROPENSITY

We merge the IPTW model with an extended generalized hierarchical model for longitudinal data (see also Raudenbush, Hong, and Rowan, forthcoming). Time periods are nested within individuals, so that level 1 of the hierarchical model becomes the change analysis and level 2 yields between-individual parameters. The interdependence of observations over time is explicitly modeled and the IPTW weights are applied to the level 1 analysis of within-individual change.

We extend the hierarchical model proposed by Laub and Sampson (2003: chapter 9) to incorporate three important features of the data. The first is a conception of crime as a rare event in any given year, especially in the older ages. The second is unexplained variation (heterogeneity) between individuals in the underlying or latent propensity to offend (Bushway et al., 2001). The third is that there is variation across time and individuals in incarceration, yielding a varying “street time” during which one has the opportunity to commit crime (Blumstein et al., 1986). To accommodate these three features, our model views the count of crime  $Y_{ij}$  for a given person  $i$  at time  $j$  as sampled from a Poisson distribution with mean  $n_{ij}\lambda_{ij}$ , where  $n_{ij}$  is the number of days free on the street for person  $i$  at time  $j$  and  $\lambda_{ij}$  is the latent or “true” offending rate for person  $i$  per day free in year  $j$ . We view the resulting log-event rates of crime as normally distributed across persons; using a hierarchical generalized linear model (Raudenbush and Bryk, 2002), we set the natural log link  $\eta_{ij} = \log(\lambda_{ij})$  equal to a mixed linear model that includes relevant covariates, a random effect for each person to account for heterogeneity, and an overdispersion parameter.<sup>14</sup> The individual offending rate thus conforms to a Poisson

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13. James Robins, 2005, personal communications.

14. In all initial models we allowed for overdispersion in the Poisson distribution, similar to the negative binomial model with unobserved heterogeneity often used in the criminology literature. The difference is that we do not impose any distributional assumptions (for example, gamma distributed) on the extra-Poisson variance parameter. We used the HLM 6.0 software to estimate population-average model parameters with robust standard errors. Consistent with the logic and technical requirements of the Poisson model with variable exposure (Raudenbush et al., 2000: 148–50), we examine observations where men were free on the street at least one day (that is greater than 0) in the year of observation. The overwhelming majority of observations (96 percent) met this requirement and thus contributed to



distribution, allowing for overdispersion, that incorporates the skewed nature of crime with its many values of 0 in any given year, while at the same time creating a metric to define meaningful effect sizes. Our approach also incorporates unique unobserved differences between persons via random effects (Horney, Osgood, and Marshall, 1995: 661). If individuals have stable features that affect crime, random effects can be important in accounting for variation not explainable by the structural model.

We specify the log of a person’s total crime rate per day free<sup>15</sup> as a function of our causal “treatment” of marriage, weighted via IPTW as a quadratic function of age and all covariates. Using the above notation the final elements of interest in the within-person model thus become:

$$E(\text{Crime}_{ij} \mid \beta_i) = n_{ij} * \lambda_{ij}$$

$$\text{Log}(\lambda_{ij}) = \beta_{0,i} + \beta_{1,i} \text{Marriage}_{ij} + r_{ij}$$

where  $i$  is the index for individuals,  $j$  for longitudinal observations, and  $n_{ij}$  is the number of days free on the street for person  $i$  at time  $j$ . Marriage is a time-varying covariate that can take on values of 0 (not married) or 1 (married) during each year from 17 up to 70. The intercept,  $\beta_{0,i}$ , is the estimated log event-rate of crime while free during unmarried person-years. Each observation at level 1 is weighted via the IPTW methodology, yielding the causal effect interpretation for the marriage parameter.

The initial between-person model takes the following general form:

$$\beta_{0,i} = \gamma_{0,0} + u_{0,i}$$

$$\beta_{1,i} = \gamma_{1,0} + u_{1,i}$$

As indicated by the presence of an error term, persistent heterogeneity is modeled by allowing the latent rate of offending to vary randomly across persons. We also estimated models in which the marriage effect was allowed to vary but the random effects were largely insignificant. In these models the marriage slope was constrained to 0. The parameter of major interest is the average within-individual causal effect of marriage derived from the between-person model. That is, the average time-varying effect of marriage in the level 1 model,  $\beta_{1,i}$ , is captured in the between-person model by the term,  $\gamma_{1,0}$  (Horney, Osgood, and Marshall, 1995: 661; Laub and Sampson, 2003: 310). All results refer to population-average effects with robust standard errors.

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the analysis. We experimented with models where these observations were included and exposure time was ignored. The results were very similar.

15. Based on our theory, the rarity of many specific crimes (for example, violence), and a vast research literature showing versatility in offending and lack of offense specialization (Blumstein et al., 1986; Sampson and Laub, 1993), our analysis focuses on total crime propensity.

## CAUSAL ESTIMATES

Table 2 presents the hierarchical Poisson count models that account for exposure time (days not incarcerated) and persistent heterogeneity in unobserved causes of offending. We begin with the larger sample of delinquent boys followed for a shorter period: ages 17 to 32 ( $N = 5,391$  person-years). As in the logic of an experiment, we focus on the single estimate of the average causal effect of marriage on crime. However, to assess robustness we display the results from eight models reflecting variations in IPTW model specification for the weighting procedures. Redundant results are not automatic; for example, the raw interactive weights are correlated only .20 with the noninteractive weights for the full entry specification. Thus we focus on bounded causal estimates across the full range of weight specifications, especially event rate ratios and associated 95 percent confidence intervals.<sup>16</sup>

The first four models in table 2 are based on the more common IPTW specification used in the literature (for example Robins, Hernan, and Brumback, 2000). Whether or not the marriage effect is allowed to vary across individuals, and whether or not stepwise elimination is used, the estimates are highly stable and tightly clustered around -.52 to -.56. In our preferred model, the full entry estimation with fixed marriage effect, the causal estimate is -.56 ( $p < .000$ ). The exponentiated event rate is .57 ( $p < .000$ ; 95 percent confidence interval = .51 to .64), meaning that being married is estimated to reduce the latent rate of offending by about 43 percent. Even if we take the most conservative estimate, namely the highest value of the 95 percent confidence interval, the data indicate the causal effect of marriage is approximately a 36 percent reduction in crime. When the marriage effect is allowed to vary randomly across individuals, the effect estimate is near equivalent (event rate ratio = .59); the stepwise estimation is also very similar (rows 3–4). Application of IPTW to account for selection into marriage thus produces a large and consistent estimate of crime reduction during the years former delinquents are married.

The second set of four estimates is based on a fully interactive model of treatment history, with weights conditioned on the prior year's marital status as described. Although in some sense this provides a more rigorous specification of treatment history, the causal effect estimates are only slightly lower, with event ratios ranging from .58 to .60. Again the estimates are tightly bounded and the confidence intervals suggest that the

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16. The overdispersed event rate Poisson model was not estimable in the 17 to 32 sample. We did successfully estimate an overdispersed model that did not control for exposure time, with near equivalent results. The estimate of  $\sigma^2$  in these models was about 1.3, suggesting only slight overdispersion. We prefer the models in table 2, given the wide variability in exposure time.

average reduction in crime association with marriage is between 40 and 42 percent. For the main specification that fixes the marriage slope in the full entry model, the reduction is 42 percent.

**Table 2. Hierarchical IPTW Variable-Exposure Poisson Models of Marriage and Total Crime, Ages 17 to 32 (N = 440 Men, 5,364 Observations)**

<i>IPTW Specification of Marriage Effect</i>	<i>Coefficient (SE)</i>	<i>p-value</i>	<i>Event Rate Ratio</i>	<i>Confidence Interval</i>
Noninteractive, full entry, fixed slope	-.559 (.057)	0.000	.572	(.511, .640)
Noninteractive, full entry, freed slope	-.523 (.066)	0.000	.593	(.521, .675)
Noninteractive, stepwise entry, fixed slope	-.558 (.058)	0.000	.573	(.511, .641)
Noninteractive, stepwise entry, freed slope	-.526 (.065)	0.000	.591	(.520, .672)
Fully interactive, full entry, fixed slope	-.537 (.064)	0.000	.584	(.516, .662)
Fully interactive, full entry, freed slope	-.515 (.069)	0.000	.598	(.522, .685)
Fully interactive, stepwise entry, fixed slope	-.550 (.059)	0.000	.577	(.514, .647)
Fully interactive, stepwise entry, freed slope	-.541 (.065)	0.000	.582	(.513, .661)

Table 3 exploits the life-course data we collected up to age 70 (N = 2,137 person-years). The question here is what happens when we extend the period of observation to older ages, during the peak person-years of marriage and its later decline? Although our statistical power at the between-individual level is more limited compared to the previous models, the age range of estimation is now much wider.<sup>17</sup> The results show that being married over the full life course is significantly related to lower crime in three of four of the noninteractive IPTW specifications. There is more variability in results than in the 17 to 32 sample, especially with randomly varying marriage effects that are highly sensitive to model

17. We included lagged violence, drug and alcohol, and property crimes as predictors in addition to total criminal history each year up to 70 in calculating the IPTW weights (see also table 1). We do this because violence and drug-alcohol use peaks later in life—in the mid 30s for drug and alcohol offenses (Laub and Sampson, 2003). Research on this age cohort of men also shows the detrimental effects of alcoholism on marriage (for similar findings on the nondelinquent cohort, see Sampson and Laub, 1993: 192–94, 234–41; Vaillant, 1983: 97). The results were virtually equivalent, however, when the highly correlated total crime measure was used.

assumptions. Although not surprising given the smaller sample, the results are still consistent with respect to the main picture. For example, the estimates range from -.72 to -1.18 and in the main noninteractive IPTW specification with a fixed marriage effect, the exponentiated event rate of .32 translates to a 68 percent average reduction in crime. The conservative estimate (upper 95 percent CI) yields a substantial 36 percent reduction estimate (CI = .16, .64), exactly the same as the corresponding specification in table 2.

For the fully interactive models over the wider age range, there is apparently more heterogeneity in the Poisson variance that is now estimable—the results in rows 5–8 are based on the overdispersed random effects Poisson models. The effect estimates are lower than in rows 1–4, but if we take the average of the four estimates the data yield an event rate of .58, translating into a substantial 42 percent average reduction in crime. The averaged event rate applying the highest end of the confidence interval is .88, a 12 percent reduction. In short, though there is more fluctuation in the smaller sample of table 3 than in table 2, all fixed-effect marriage estimates are significant and the average reductions remain substantively large.

**Table 3. Hierarchical IPTW Variable-Exposure Poisson Models of Marriage and Total Crime, Ages 17 to 70 (N = 52 Men, 2,137 Observations)**

<i>IPTW Specification of Marriage Effect</i>	<i>Coefficient (SE)</i>	<i>p-value</i>	<i>Event Rate Ratio</i>	<i>Confidence Interval</i>
Noninteractive, full entry, fixed slope	-1.134 (.349)	0.002	.322	(.162, .637)
Noninteractive, full entry, freed slope	-.847 (.281)	0.004	.429	(.244, .752)
Noninteractive, stepwise entry, fixed slope	-1.185 (.578)	0.040	.306	(.099, .948)
Noninteractive, stepwise entry, freed slope	-.722 (.456)	0.119	.486	(.195, 1.212)
Fully interactive, full entry, fixed slope*	-.482 (.223)	0.031	.617	(.398, .956)
Fully interactive, full entry, freed slope*	-.341 (.241)	0.163	.711	(.439, 1.152)
Fully interactive, stepwise entry, fixed slope*	-.774 (.157)	0.000	.461	(.339, .627)
Fully interactive, stepwise entry, freed slope*	-.650 (.212)	0.004	.522	(.341, .798)

\* Overdispersed Poisson model

## SENSITIVITY AND ROBUSTNESS ANALYSES

What if crime in a particular year leads some men's marriages to dissolve? The IPTW model fully accounts for the criminal history but not necessarily concurrent reciprocal causation. The usual option is to look at the lagged effects of a predictor, but the logic of social control theory does not specify such an approach. In explaining crime, it is the current state of the social bond that matters most by the theory's logic (Hirschi, 1969: 19). Consider the man who is married in one year and divorced the next. It would not make sense to consider him married (as in a lag model) for the purposes of explaining crime when he is in fact divorced. It also seems unlikely that crime in any given year, net of the person's entire criminal history, would account for not being married that year. Nevertheless, it is important to consider this critique, and also the possibility that marriage has durable or spillover properties, even during times of change.

As a further test, we therefore re-estimated the IPTW results for both the 17 to 32 and 17 to 70 samples with two lagged models of marriage that take the scenarios into account. In table 4, model 1 estimates the lagged causal effect of marriage using the lagged IPTW of the propensity to marriage. Model 2 enters the contemporaneous change in marital state (such that divorce = -1, no change = 0, and entered a new marriage = 1), allowing us to estimate the lagged marriage effect controlling for whatever changes occurred in the year crimes were measured. Both models produce a highly significant lagged effect of marriage at ages 17 to 32. Without controls for contemporaneous marital change, model 1 yields a significant ( $p < .001$ ) marriage effect, with an event rate ratio of .74, meaning that being married at a particular age is linked to roughly a 25 percent reduction in crime at the next age. Model 2, however, suggests that contemporaneous change—or moving into marriage—is highly significantly related to lower crime as well. Once we adjust for contemporaneous change, the lagged causal effect of marriage is now even stronger (event rate ratio = .61, CI = .54, .70). Consideration of the underlying event rate ratio for lagged marriage reveals that being married is associated with 39 percent reduction in crime in the next age period, controlling for changes in that period. Lagged marriage and current change are correlated (.33,  $p < .001$ ), such that failure to adjust current change (model 1) appears to underestimate the lagged marriage effect. We emphasize model 1, however, because it does not mix unweighted and weighted predictors; in addition, marital change is measured subsequent to and is thus potentially endogenous to lagged marriage.

Panel 2 extends the analysis to consider lagged marriage and contemporaneous change in marital states up to age 70 in the targeted sample of 52 men. Because of the small sample size the variability of the coefficients and larger standard errors are more noticeable. In model 1,

lagged marriage is negative at  $p < .10$ —if married in any person year, the same man is estimated to have a 32 percent reduction in the probability of crime in the following year (event rate = .68, CI = .44, 1.06). When we simultaneously control for change in marital status, the results are again slightly stronger, because marriage is significantly linked to a 41 percent reduction in crime a year later (event rate ratio = .59, CI = .35, 1.001). The estimate of the effect of entering a state of marriage on concurrent crime is not significant in this model. Overall, then, if we stick purely to the more conservative estimate in which marriage is lagged and lagged IPTW weights are applied, thereby ruling out reciprocal effects and imposing a strict causal order, the estimated causal reduction in crime in the next year is from 25 to 30 percent lower across the two samples.

**Table 4. Hierarchical IPTW Variable-Exposure Poisson Models of Lagged Marriage and Total Crime**

<i>IPTW Specification: Fully Interactive, Full Entry, Fixed Slope</i>	<i>Coefficient (SE)</i>	<i>p-value</i>	<i>Event Rate Ratio</i>	<i>Confidence Interval</i>
<b>Panel 1: Ages 17 to 32 for 440 men (N = 5,364 observations)</b>				
Model 1				
Marital status, lagged	-.296 (.059)	0.000	.744	(.662, .835)
Model 2				
Marital status, lagged	-.489 (.066)	0.000	.613	(.539, .698)
Marital status, contemporaneous change	-.495 (.073)	0.000	.609	(.528, .704)
<b>Panel 2: Ages 17 to 70 for 52 men (N = 2,137 observations)</b>				
Model 1*				
Marital status, lagged	-.379 (.225)	0.092	.684	(.440, 1.064)
Model 2*				
Marital status, lagged	-.524 (.268)	0.050	.592	(.350, 1.001)
Marital status, contemporaneous change	-.429 (.391)	0.273	.651	(.302, 1.402)

\*Overdispersed Poisson model

We considered several additional specifications to assess the robustness of results. Although our focus is total criminal propensity we did explore models disaggregated by type of crime with generally similar results. For predatory offenses (violent and property) estimated with the main IPTW specification (analogous to row 1 of tables 2 and 3), the event rate ratio was .48 ( $t$ -ratio = -11.23, 95 percent confidence interval = .422 to .545) at ages 17 to 32, such that being married is estimated to reduce the latent rate

of predatory offending by over 50 percent; if we take the upper bound of the confidence interval, then the conservative estimate is 45 percent. When further disaggregated, marriage was again significant for both property crime ( $p < .01$ ) and violence ( $p < .10$ ). For alcohol and drug offending, the average causal effect estimate for marriage at ages 17 to 32 was significant ( $t$ -ratio = -2.23) with an event ratio of .80, and thus a 20 percent estimated reduction associated with marriage (CI = .66, .97). For the follow-up sample to age 70, the estimated marriage effect on the event rate for predatory offenses was .25 ( $p < .01$ ). The marriage effect was also significant for violence and property offending ( $p < .05$ ) but not drugs and alcohol.

### MARITAL ATTACHMENT AND DEVIANCE OF SPOUSE

An intriguing question is whether our current focus on the effects of marriage is antithetical to concerns with the quality of marriage or the criminal behavior of spouses. Does a poor quality marriage or a marriage to a “deviant spouse” inhibit crime relative to nonmarriage? In prior work with the Glueck data to age 32, Sampson and Laub (1993) argued for the importance of marital quality, specifically marital attachment. Their analysis, however, did not examine the time-varying effects of marriage as here and the data on marital quality were by definition restricted to the married subsample of men and were not measured on a yearly basis. The question in this paper is thus quite different, but available information on average marital quality and also the criminality of spouses does offer an interesting possibility for further analysis.

We specifically examine two key measures of marital attachment and spousal deviance collected during the age 25 interview that refer to the current marriage, if applicable (some of which were intact for several years prior to age 25; see Sampson and Laub, 1993: 144–45).<sup>18</sup> These measures

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18. We use a composite measure validated in Sampson and Laub (1993: chapters 6–8) derived from the age 25 interview describing the general conjugal relationship between the subject and his spouse during the period, plus the subject’s attitude toward marital responsibility (Glueck and Glueck, 1968: 84–88). Weak attachment was indicated by signs of incompatibility and men neglectful of marital responsibilities, financial as well as emotional. In contrast, subjects who were strongly attached displayed close, warm feelings toward their wives, or were compatible in a generally constructive relationship. The crime-deviance measure of spouse was derived from the social investigation interview with the subject and other knowledgeable informants, supplemented by extensive record checks (see Glueck and Glueck, 1968: 83–84). Not only were criminal records checked, assessments of deviance were obtained from social welfare, mental health, school, and employment records as well (Sampson and Laub, 1993: 190).

permit us to control for between-individual differences in married men's propensity to be in "good" marriages and with criminal or deviant spouses at the age 25 interview. We control for these propensities in assessing within-individual (year by year) effects of marriage on crime. We also control for the proportion of years married from 17 to 32 as a way to control for exposure time to marital quality and crime by spouses. This test is restrictive in that by definition it reduces the variability in marriage, but has the advantage of controlling for the type of marriage men were in during their early 20s. Because of the time frame, we conduct the analysis on the subset of 226 men who were married at the time of the age 25 interview.<sup>19</sup>

Table 5 shows a hierarchical IPTW model of crime that simultaneously estimates variability between individuals in proportion of years married, marital quality, and spousal deviance, and within-individual deviations in marriage year by year. The results show that men with high marital attachment exhibit lower involvement in crime ( $t$ -ratio = -7.42) and men with criminal or deviant wives displayed higher crime rates ( $t$ -ratio = 2.21). The latter may reflect homophily, of course, but for our purposes the main result is that among those men who were married at 25, and controlling for marital quality and spousal criminal record, within-individual variations in marriage from 17 to 32 are significantly negatively related to crime, with an effect estimate of -.45 (-.46 when person-centered) and an event rate ratio of .64. Despite this very different specification and subsample of marrieds, then, the estimated causal reduction in crime associated with being married versus being unmarried, for the same man, is still substantial, in this case just over a 35 percent reduction.

**Table 5. Hierarchical ITPW Variable-Exposure Poisson Model of Total Crime, Ages 17 to 32 (N = 226 Married Men at 25; 2,921 Person-Years)**

	<i>Coeff. (SE)</i>	<i>t-ratio</i>
Intercept	-4.736 (.265)	-17.84**
<i>Within-individual</i>		
Marriage, 17 to 32	-.449 (.091)	-4.93**
<i>Between-individual</i>		
Mean years married, 17 to 32	-.322 (.324)	.99
Marital attachment, 17 to 25	-1.12 (.157)	-7.42**
Spouse crime–deviance, 17 to 25	.236 (.107)	2.21*
<i>Variance components</i>		
Between-individual	1.273**	
Marriage slope	.799**	
* $p < .05$ ** $p < .01$		

19. It is important to note that there are significant within-individual variations (including 7 years prospectively) in marriage over the 17 to 32 age periods among those married at 25.



## COHABITATION

Finally, we explored the potential effect of being in a stable relationship even if not married. In our interviews with the 52 men, we collected annual data on cohabitation using the life-history calendar. Each man was coded 1 if he was involved in a stable, long-term cohabitation relationship during any year from age 17 to age 70, and 0 otherwise. Not surprisingly, for this cohort of men born on the cusp of the Great Depression, cohabitation was a very rare occurrence (only 3 percent of person-years). In light of secular declines in marriage and current interest in cohabitation, however, we examine within-individual yearly deviations in cohabitation, controlling for between-individual variations in the proportion of time spent in cohabitation. Because of its rarity we did not pursue an IPTW analysis of cohabitation, using instead a hierarchical fixed-effects framework that examines within-person deviations in cohabitation, controlling for time-invariant person factors and time-varying changes in marriage.

We found that cohabitation had an independent relationship with crime controlling for marriage, with a coefficient of -1.06 and estimated event rate ratio of .37 (95 percent CI = .31 to .39). Similar results were obtained in a model with lagged cohabitation, lagged marriage, and change in marital status. All coefficients were statistically significant and negative, with a lagged cohabitation coefficient of -.68 and an event rate ratio of .50 (95 percent CI = .42 to .61). We emphasize that these results are exploratory and *not* based on IPTW models. Still, they are substantively large and the confidence intervals are relatively small, suggesting that the mechanism for desistance may stem ultimately from being stably partnered—whether in marriage or cohabitation. This hypothesis awaits future testing.

## CONCLUSIONS

*I keep a close watch on this heart of mine  
I keep my eyes wide open all the time  
I keep the ends out for the tie that binds  
Because you're mine, I walk the line  
—Johnny Cash, 1956*

The United States has witnessed extensive normative debates about the role of marriage in modern society. Indeed, the 2004 presidential campaign saw its energy boosted by groups as varied as Christian evangelicals and gay activists concerned about competing visions of the future of marriage. As noted at the outset, there is considerable disagreement in the broader social science literature as well, especially

whether a causal effect of marriage exists on a wide range of adult behaviors, from wages to sex to health to crime.

Given this backdrop, we believe it is important to assess what is known about marriage in as nonideological and rigorous a manner as possible. We attempted to do so by estimating the causal effects of marriage on crime using a unique compilation of data—arguably the longest longitudinal study to date on crime and adult development. We found that being married is associated with a significant reduction in the probability of crime, averaging approximately 35 percent across key models in both the full sample of nearly 500 men examined from ages 17 to 32 and the targeted subsample of 52 men assessed from ages 17 to 70. These basic findings were robust, and thus consistent with the notion that marriage causally inhibits crime over the life course.

Why is marriage important in the process of desistance from crime? Supported by a mix of theory and consistent narrative materials derived from in-depth interviews with the same men studied here (see Laub and Sampson, 2003), we have argued that marriage has the potential to “knife-off” the past from the present in the lives of disadvantaged men and lead to one or more of the following: opportunities for investment in new relationships that offer social support, growth, and new social networks; structured routines that center more on family life and less on unstructured time with peers; forms of direct and indirect supervision and monitoring of behavior; or situations that provide an opportunity for identity transformation and that allow for the emergence of a new self or script, what Hill (1971) described as the “movement from a hell raiser to a family man.”

We wish to be clear that the results in this paper do not confirm the existence of these or any other specific mechanisms. Yet even in true experimental designs, it is usually unclear what the exact mechanism is that produces a given result. In any randomized trial, the causal inference is about the specific treatment—for example, even if marriage could be randomly assigned, any crime outcome differences could still not be apportioned among hypothesized mediating mechanisms. To take another example, housing voucher experiments cannot tell us *why* individuals randomly assigned to low poverty neighborhoods do better (if they do). Or take job training experiments—is the mechanism specific skills one is taught? Personal counseling? Social solidarity and encouragement? Treatments are a package and the relative contributions of the components can’t easily be disentangled.<sup>20</sup> The problem of mechanisms is therefore not unique to our study. In terms of causality, we take the

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20. We thank Stephen Raudenbush for emphasizing this point and for these examples.

position that one must first demonstrate the effect of a treatment before tackling the question of mechanisms.

Because of data constraints, we also could not model yearly changes in the quality of marital attachment or in the criminal or deviant character of the man's spouse. We could and did, however, examine between-individual differences in marital attachment at age 25 and criminal involvement of spouses at age 25 among the subset of approximately 225 men who were married at that age. Controlling for these differences, our data reveal that within-individual variations in crime were still negatively and strongly associated with being married versus being unmarried. This implies that there is something about being married, at least during the young adult years, that inhibits crime regardless of the quality of the marriage and even the criminal involvement of the spouse. Perhaps the latter is not so surprising—because on average men are much more involved in crime than women, it is almost invariably the case that men marry “up” and women “down” when it comes to exposure to crime and violence. For these men in these times, any marriage may have worked as something like a “civilizing” effect.<sup>21</sup>

Probably the biggest limitation of our study is that the IPTW modeling approach we adopted assumes no unmeasured covariates linked to both treatment and outcome. In practice, the criterion of having no unobserved confounding is impossible to verify—the data in any observational study provide no definitive information (Robins, Hernán, and Brumback, 2000). As discussed above, however, we tried to counteract this limitation by exploiting what we believe are rich individual baseline data and time-varying covariates over the full life course in order to model the propensity to marriage. It is hard to imagine what the missing time-stable or time-invariant covariates are that would overcome the magnitude and robustness of results. From IQ to the cumulative history of both the outcome and the treatment, we accounted for 20 baseline covariates and approximately a dozen time-varying confounders measured from widely varying sources—many of which predict the course of marriage as theoretically expected (table 1).

We thus argue that omitted confounders would have to be implausibly large to overturn the basic results obtained under a number of different

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21. Given secular changes in marriage and relationships between spouses, it may be that deterrent effects of marriage on crime have changed over time. Variation in the patterns of family formation and marriage, especially for African Americans, may also affect the potential of marital relationships to foster desistance from crime (see especially Giordano, Cernkovich, and Rudolph, 2002). These are issues that our data cannot address and that remain important for future research.

model specifications and assumptions.<sup>22</sup> It is also not clear what alternative methods are both better and practical. At the very least, a major advantage of the IPTW approach (and counterfactual logic more generally) is that it forces conceptual clarity and transparency in the assumptions and causal claims that are made, unlike much literature that continues to use causal language in informal or disguised terms.

## IMPLICATIONS

More broadly, we see our work as informing debates about how social behavior changes over time as individuals connect or disconnect across a variety of institutional domains (for example, marriage, work, education, and the military). Even among high-risk offenders, as in the general population, most men marry and most that divorce get married again, underscoring the potential role of marriage as a time-varying source of variations in crime. We would note that though cohabitation is rare in our cohort of men, the results for the subsample of 52 men followed to age 70 are intriguing—there are hints in the data that desistance effects may not be limited to marriage or marital relationships as traditionally defined. This is an important area for future research using younger cohorts among which cohabitation is more normative, as is the casual effect of partnership on female crime, which is apparently growing. Is marriage a protective factor for female offending as it appears to be for female victimization (Lauritsen and Schaum, 2004)? Note also that our theoretical approach is logically applicable to gay marriage—for example, we would predict that a gay man in a marital situation is less likely to be criminal or engage in high-risk sexual behavior than when the same gay man is otherwise unattached.

Our results bear on policies for ex-offenders as well. The state cannot (nor should it even if imaginable) force individuals to marry, especially given that ex-offenders do not make the most attractive marriage mates. At the same time, marriage is a potentially transformative institution that may assist in promoting desistance from criminal behavior. It is also the case that some women consciously choose to marry ex-cons, often with the foresight to recognize the heavy burdens that await them (see Laub and Sampson, 2003: 120–21, 137–38, 187). Thus our results suggest more rigorous evaluation of recent policy initiatives that support marriage and stable relationships among ex-offenders (Lyman, 2005). “Re-entry” is a growing concern as hundreds of thousands of ex-convicts, many with

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22. A formal sensitivity analysis (see Robins, 1999: 167–73) is beyond the scope of the current paper. Moreover, such analyses require assumptions about the magnitude, direction, and functional form of potential biases that ultimately raise more questions than they answer.

backgrounds like our Glueck men, are being released each year, a trend projected to continue for the next decade (Bureau of Justice Statistics, 2004; Pettit and Western, 2004; Western, Kling, and Weiman, 2001). It may even be possible to design policy experiments where supports for marriage (for example, tax benefits) are randomly assigned to prisoners upon release as a way to more rigorously assess causal effects on recidivism.

In the meanwhile, we believe that counterfactual methods and IPTW models offer a promising approach to the inherent problem of making causal inferences, whether in criminology (Haviland and Nagin, 2005), demography and life-course dynamics (Barber, Murphy, and Verbitsky, 2004), or the social sciences at large (Winship and Morgan, 1999). Although certainly no panacea, we see the benefit in extending counterfactual life-course models to other hypothesized sources of desistance from crime at the within-individual level for young adult offenders, such as work, schooling, and military service. One can extend the logic of counterfactual models to other time-varying adult outcomes in the general population that have been associated with marriage such as wages, mental health, and physical well being. Given secular declines in marriage, it would seem especially wise to further assess the effects of cohabitation on criminal behavior and other adaptations to life.

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