Is Marriage Always Good for Children?
Evidence from Families Affected by Incarceration

Keith Finlay and David Neumark*

July 2008

* Finlay is Assistant Professor of Economics at Tulane University. Neumark is Professor of Economics at UCI, research associate of the NBER, and research fellow at IZA. We are grateful to Marianne Bitler, Richard Blundell, Paul Devereaux, Daiji Kawaguchi, Francesca Mazzolari, Jim Raymo, Giuseppe Ragusa, Jeffrey Wooldridge, and seminar participants at Hebrew University, Hitotsubashi University, Osaka University, Tel Aviv University, Tulane University, UCI, University College London, University College Dublin, and the 2008 PAA Economic Demography Workshop for helpful comments.

Is Marriage Always Good for Children?
Evidence from Families Affected by Incarceration

Abstract

Never-married motherhood is associated with worse educational outcomes for children. But this association may reflect other factors that also determine family structure, rather than causal effects. We use incarceration rates for men as an instrumental variable in estimating the effect of never-married motherhood on high school dropout of black and Hispanic children. We find that unobserved factors drive the negative relationship between never-married motherhood and child outcomes, at least for children of women whose marriage decisions are affected by incarceration of men. For Hispanics we find evidence that these children may actually be better off living with a never-married mother.

1. Introduction

A growing proportion of children live with mothers who have never married. Children raised by never-married mothers are more likely to repeat a grade in school, be expelled or suspended from school, and be treated for an emotional problem than children living with both biological parents (Dawson 1991). Given the strong cross-sectional correlations between traditional, two-parent family structures and positive outcomes for children, marriage promotion policies have been touted as a strategy for improving the socioeconomic outcomes of poor, single mothers and their children.[1] These policies provide incentives or support to begin or maintain marriages. While some recent community-based programs have been directed at middle-class, white families (Macomber et al. 2005), arguably the largest recent federal marriage-promotion policies have been the 1996 welfare reform legislation and the Healthy Marriage Initiative included in the 2006 TANF reauthorization, which target low-income, unmarried mothers. For example, two of the stated goals of the 1996 welfare reforms were to prevent out-of-wedlock childbearing and to encourage the formation of two-parent families. There are also pro-marriage policies at the state and local level (Edin and Reed 2005), and a push to extend community-based programs to focus on poor women in urban settings (Lichter 2001). Policymakers argue that marriage is one of the most effective ways of improving outcomes for poor mothers and their children.[2]

Marriage promotion policies are built upon the conjecture that marriage itself will directly improve outcomes for single mothers and their children. But the effects of policy may differ substantially from what is revealed by cross-sectional relationships because of endogenous selection on unobservables at both the individual and environmental level. For example, perhaps the worst prospective female parents do not get married. Alternatively, the potential spouses available to those on the margin of getting married may be of sufficiently low quality that it is in the interest of their children for some women to forego

---

[1] See, e.g., Rector and Pardue (2004).

[2] This argument has arisen frequently, in one form or another, over the past 40 plus years. Significant or at least high-profile milestones include: Senator Daniel Patrick Moynihan's (1965) report on black families; Vice President Daniel Quayle's criticism of the out-of-wedlock childrearing of television character Murphy Brown as mocking "the importance of fathers" (http://www.mfc.org/pfn/95-12/quayle.html, accessed October 8, 2007); and the aforementioned efforts to include marriage-promotion policies in welfare-reform legislation under both Presidents Clinton and Bush.

marriage. And finally, marriage may be less common among adults facing worse economic (and other) environments, and these environments may influence child outcomes. In such cases, fiscal resources devoted to encouraging marriage may be misguided or at least ineffective and might be better directed toward increasing the human capital of parents, improving the environments faced by poor families, or investing in family planning.

A critical question, therefore, is whether the relationship between family structure and child outcomes is causal. Despite the overwhelming evidence that children living in non-intact families have worse outcomes on average, there is little consensus about the causal effects of family structure. Clearly researchers and policymakers should be wary of drawing conclusions about the causal effects of family structure on child outcomes from cross-sectional statistical associations.

This paper estimates the causal effects of never-married motherhood on whether children drop out of high school. Our research design is based on an instrumental variables (IV) approach. In particular, in order to account for the endogeneity of family structure, we instrument for whether a child's mother has ever been married using the incarceration rate for men of the same race or ethnicity of the mother (defined for the state of residence of the mother). For blacks, almost all marriages are between same-race spouses, and the same is true by ethnicity for less-educated Hispanics, so for these groups state-year variation by race and ethnicity in incarceration rates has a direct effect on the "supply" of potential husbands in the marriage market.[3] The IV estimator has a local average treatment effect interpretation, estimating a causal effect for families whose structure would be changed by variation in race-specific or ethnicity-specific incarceration rates if those incarceration rates were randomly assigned. Given that incarcerated men tend to have less education and lower earnings and that there is positive assortative mating, this causal effect is particularly interesting in the context of policies to encourage marriage among poor families. Differences between the estimates with and without accounting for endogenous selection suggest that unobservable factors drive the observed adverse relationship between never-married

[3] Charles and Luoh (2005) and Mechoulan (2007) use similar variables to study the effect of incarceration rates on female fertility, education, and marriage market outcomes. Unlike our paper, their studies do not address effects on children.

motherhood and educational outcomes for children whose mothers are most affected by changes in race-specific or ethnicity-specific incarceration rates. Moreover, particularly for Hispanics, we find evidence that these children may actually be better off living with a never-married mother. These results suggest that simply encouraging marriage for poor, unmarried mothers may not improve the welfare of their children, and could even worsen it depending on which marriages might be formed as a result of such policies.

We explore in considerable detail the possibility that incarceration has a direct effect on child outcomes, violating the central assumption underlying the validity of the IV estimation. This could arise in a number of ways. Spending on incarceration may substitute for spending on education. Alternatively, higher incarceration rates could be associated with greater criminal activity, or perhaps instead with reductions in criminal activity, either one of which may affect whether youths drop out of high school. Higher incarceration may help communities by removing adults who draw youths into crime, or conversely may harm them by removing male role models. Higher incarceration could also have direct positive effects by reducing teen childbearing and increasing education of mothers (Kamdar 2007; Mechoulan 2007), leading to better outcomes for children. We address these possibilities in a number of ways. First, we argue that our results are unlikely to be explained by a direct negative effect of incarceration on child outcomes, since our IV estimates show nonnegative effects of never-married motherhood; thus, the more likely problem is that there is a direct positive effect of incarceration on youths. Second, we supplement the model with controls for educational expenditures and crime rates as well as mother's education and her age at the child's birth, to account for these separate influences on dropout behavior that may be correlated with incarceration. Third, we experiment with using long lags of incarceration rates for younger men as IVs—as these should be less correlated with the current incarceration rates that may have more direct effects—as well as related specifications that address this issue in different ways. And fourth, we compare results for boys and girls, positing that the direct effects of incarceration from, for example, removing from the community those inclined toward criminal behavior are stronger for boys than for girls. In general, our analyses suggest that the results are not driven by violations of the central assumption underlying our identification strategy.[4]

2. Family structure and child outcomes

Much of the literature on family structure and child outcomes focuses on children raised in single-parent households, and establishes that such children are worse off, on average, than children who grow up with two parents (McLanahan 1985; McLanahan and Sandefur 1994).[5] However, these findings may overstate the causal impact of family structure on child outcomes because of unobservable factors that affect both, such as the quality of actual or potential spouses or the environment in which the family lives.[6]

Some studies try to account for unobservable factors associated with family structure by using longitudinal research designs, exploiting changes over time in a family's structure. Using a sample of British and American children of divorced parents from the British National Child Development Study and the U.S. National Survey of Children, Cherlin et al. (1991) find that pre-divorce differences in test scores and behavioral problems explain half of the post-divorce difference in outcomes for boys and a smaller part of the difference for girls. Moreover, the estimated effects of divorce are insignificantly different from zero when the pre-divorce controls are included. In conflicting results, Morrison and Cherlin (1995) find that controlling for pre-divorce conditions does not attenuate the estimated relationship between family structure and the behavioral problems of boys from the Children of the National Longitudinal Survey of Youth 1979 (NLSY79); with or without pre-divorce controls, they do not find significant effects for girls. Based on an analysis of numerous outcomes using the 1988 National

---

[4] Research investigating the link between incarceration and crime has, quite naturally, been concerned with the endogeneity of incarceration with respect to crime. Levitt (1996) uses information on overcrowding litigation as an instrument for incarceration in a regression for crime. Subsequent work has extended Levitt's approach to address the endogeneity of crime in regressions for demographic outcomes like teen childbearing (Kamdar 2007) as well as education and employment outcomes (Mechoulan 2007). These studies find relatively little evidence of substantial bias from the endogeneity of incarceration rates in regressions for these outcomes, and hence we do not concern ourselves with the endogeneity of incarceration rates in the regression for never-married motherhood.
[5] This relationship holds for different levels of parental education and whether or not parents were ever married. The increase in single-parent households has been concentrated among women with less education, leading to a socioeconomic divergence in family structure and the associated negative outcomes of children residing in single-parent households (McLanahan 2004).
[6] See Ribar (2004) for a comprehensive review of the methods and studies used to assess the causal relationship between family structure and child outcomes. Our review in this section is selective, in part emphasizing research that contextualizes our own.

Education Longitudinal Study, Painter and Levine (2000) conclude that "the correlations between family structure and youth outcomes appear to be largely causal" (p. 3). However, we read the evidence as a bit more mixed, although in some cases the results do suggest that prior characteristics of families and children do not fully account for the effects of family structure.[7] One potential problem with exploiting changes in family structure to identify the causal effects of family structure is that data sets typically yield only a small number of family structure switches.

Researchers have also used information on siblings to estimate the effects of family structure on child outcomes, identifying the effects from within-family differences in exposure of children to particular family structures. Using a longer sample from the NLSY79, Ginther and Pollak (2004) find that living with both parents in families in which all children are the joint children of both parents ("traditional nuclear families") increases education (and reading and math assessments) relative to blended two-parent households (whether stepchildren or the joint children of the parents) or single-parent households.[8] But the estimated differences become substantially smaller and often statistically insignificant when controls for family income, parent's education, and other family characteristics are added. Using data from the British Household Panel Survey, Ermisch and Francesconi (2001) find that, cross-sectionally, children with longer exposure to single parenting have more negative education and health outcomes. When the authors look within families they find similar point estimates of the effects of single-parent family structure on A-level completion, early childbearing, and smoking patterns, but each effect is less precisely measured. Ermisch and Francesconi show that sibling effects can only be used to identify the causal effects of family structure if family structure itself is not a function of the idiosyncratic endowments of children. A related argument is that family structure may affect older and younger siblings differently (Lang and Zagorsky 2001)—for example, changes in family structure may affect the division of family resources between children—in which case the first-difference estimator may not net out unobserved

[7] Ribar (2004) shares this view of the evidence, summarizing their study as indication that "Controls for children's initial characteristics reduced and in some cases eliminated the associations with family structure" (p. 28).
[8] One of the contributions of their paper is to use data on respondents' siblings to characterize family structure, and therefore to look beyond, for example, the simple one- vs. two-parent distinction.

across-family heterogeneity.[9]

Another approach is to explicitly model the family structure decisions of parents. Manski et al. (1992) attempt to account for selection with endogenous switching regressions. They find little evidence that selection drives the relationship between non-intact family structure (i.e., a family with one parent, a parent and a stepparent, or no parents) and high school (or GED) completion, as the estimates in univariate probit and trivariate probit models are very similar in indicating that an intact family structure is associated with a higher likelihood of high school graduation. While these models can be identified by the distributional assumptions, Manski et al. (1992) exclude Census region-of-birth dummies, region-of-residence dummies, and an indicator for asymmetry in parents' completed education from the child outcome equation, although it is difficult to argue that these variables affect child outcomes only through their effect on family structure. McLanahan and Sandefur (1994) use the same method to examine the effects of single parenthood; their results also indicate that selection does not drive the relationship between single headship and schooling outcomes of children. While these studies take seriously the endogeneity of family structure, in our view neither provides a compelling identification strategy.[10]

Quasi-experimental techniques are a popular way of trying to estimate causal effects. Using data from the NLSY79, Lang and Zagorsky (2001) use parent's death as a "natural experiment" and find little evidence that parental absence affects test scores, educational completion, or future labor market outcomes. While this experiment likely estimates the average treatment effect of parental death, it is unclear if it measures the effects of divorce or never-married motherhood, since other factors associated with the death of a parent may differ. Gruber (2004) studies the effects of changes in divorce laws on outcomes for children. He finds that unilateral divorce laws increase divorce, and that children who live in states with no-fault divorce finish less schooling, have lower incomes, but are more likely to marry. However, this is a reduced-form analysis that does not identify the effect of family structure per se. For

[9] Other sibling-difference studies include Grogger and Ronan (1995) and Sandefur and Wells (1999). In both studies, the estimated effects of family structure are considerably smaller when the common family factors are removed.
[10] Manski et al. (1992) argue only that their exclusion restrictions "were suggested by … exploration of alternative

example, it does not tell us whether the impact comes from higher divorce induced by unilateral divorce laws, or a direct effect of the laws on bargaining between women and men within married households that in turn could impact investments in children. Nonetheless, work like this highlights the effects of legal institutions on family structure choices. Our research design exploits other sources of institutional variation that affect marriage behavior, in a context where we are arguably better able to identify the causal effect of family structure.

An important issue identified in this literature is that the relationship between family structure and child outcomes may vary depending on other family characteristics. A prominent example concerns the effects of divorce. A few studies have found that divorce is associated with improved outcomes for children in households in which, pre-divorce, their parents fight or argue often, with the opposite conclusion for "low-conflict" households (Amato et al. 1995; Jekielek 1998; Morrison and Coiro 1999).

An example more closely related to our work is research that finds heterogeneous effects of family structure across the socioeconomic spectrum. Using a small sample of long-term welfare recipients in California, Ehrle et al. (2003) find that children living in non-intact family structures (including single-parent homes) had outcomes that were no worse than children living with two biological parents. Although Ehrle et al. caution against generalizing from their small sample, they find evidence that family environment can help to account for their results. In particular, they find that 60% of never-married mothers offered family environments that they classified as "low-risk," about the same as for children living with two biological parents, and considerably higher than for other family structures, such as single, ever-married (39%), and married, living with stepfather (35%). Moreover, the children of never-married mothers have fewer family structure transitions, which the authors find are also harmful for children. Along similar lines, Grogger and Ronan (1995) find that fatherlessness does not appear to lead to lower education among blacks, and may even increase it.[11] Together, this evidence emphasizes that there may be a range of effects of family structure, and marriage may be a less effective or even ineffective means for improving child outcomes in some contexts, with some evidence suggesting that we may be less likely

specifications" (p. 29).

to find positive effects of marriage on children in families of lower socioeconomic status.[12] In empirical research on the effects of family structure, therefore, we have to be cognizant of how both the sample and the estimator might influence the answer. With regard to the second point, Gruber (2004) emphasizes the fact that in different empirical studies using alternative empirical approaches, the effects of family structure are identified for those whose behavior is shifted by the variation in the data exploited by each approach—the marginal decision makers.

In summary, it is clear that, on average, children who grow up outside of two-parent married households have worse outcomes than children who grow up in them. Yet there are important reasons to believe that these estimates overstate the direct effect of family structure on child outcomes. Many studies that attempt to correct for endogenous selection into marriage find diminished effects of family structure, but there is less agreement on whether the effects fall to zero or persist, with more literature pointing to the latter conclusion. The fact that in many studies the key finding is that the associations fall considerably after controlling for observable differences between families suggests that unobservable differences correlated with family structure may help drive the remaining associations. But identification strategies that grapple more seriously with selection on unobservables are not always convincing. In particular, there appear to be few opportunities to exploit exogenous variation in family structure to identify its effect on children. As stated by Gruber (2004) in his summary of the effects of divorce, "What is required to appropriately identify the impacts of divorce is an exogenous instrument that causes some families to divorce and others not, based on a factor independent of the determinants of their children's outcomes. No previous study has been able to uncover such an instrument …" (pp. 806-7). We would argue that the same statement applies to the larger literature on the effects of family structure on child outcomes.

We believe our paper makes an important contribution in this respect because we argue that incarceration rates have a direct effect on marriage markets, but in our specifications affect children's

---

[11] There are not many studies estimating the effects of family structure in race-stratified samples.

[12] This is not to say that all studies of at-risk populations find negligible or negative effects of marriage. For example, Liu and Heiland (2007) find positive effects in an urban sample that oversamples individuals of low socioeconomic

outcomes only through their effects on marriage markets, allowing us to identify causal effects of family structure using an IV strategy. At the same time, we are sensitive to the possibility that there are heterogeneous effects of family structure, and that we identify these effects for families of low socioeconomic status whose decisions are affected by variation in incarceration rates. There is no compelling reason to believe that the effects identified from this source of variation generalize across the socioeconomic spectrum. On the other hand, the effects of marriage on children of mothers of low socioeconomic status, and among these women those whose social milieu is likely to be affected by the incarceration of males, is an important policy question, as emphasized by the focus on marriage in the TANF legislation and its subsequent reauthorization.

3. Marriage and nonmarital childbearing

A rising proportion of births in the United States occur outside of marriage. In 1970, only 12% of new mothers were unmarried (DeVanzo and Rahman 1993); this number rose to 38% by 2005. But these statistics differ starkly by race and ethnicity: for black children, the proportion born to unmarried mothers was 70%; for white children, 25%; and for Hispanic children, 48% (Hamilton et al. 2006). There is also a substantial racial differential in the likelihood that parents marry after a nonmarital birth. Of unmarried parents who were romantically involved at the child's birth, white and Hispanic parents were 2.5 times as likely as black parents to be married 30 months after birth. Using data from an urban sample of recent births, Harknett and McLanahan (2004) find that the 30-month marriage rates for nonmarital parents were 9.5% for black parents, 26.7% for white parents, 26.1% for Mexican-American parents, and 23.3% for other Hispanic parents.[13] Because fertility and marriage decisions vary substantially by race and ethnicity, we estimate our models for specific racial or ethnic groups.

In this study, we examine the effect of never-married motherhood on child outcomes. Never-married motherhood is an understudied family structure category, but it is an increasingly

status.

[13] The propensity for prospective parents to marry after a nonmarital conception has also declined rapidly. Using data from the Current Population Survey's Fertility Supplements, Akerlof et al. (1996) find that the decline in the rate of "shotgun marriage" (marriage after conception but before birth) between the late 1960s and the late 1980s accounted for 75% of the increase in nonmarital births for whites and 60% of the increase in nonmarital births for

important one. Among all children living in female-headed households, DeVanzo and Rahman (1993) reported in earlier work that households with never-married mothers were the fastest growing category. Bumpass and Lu (2000) report that for blacks the cumulative 5- and 10-year marriage rates following nonmarital first births declined steadily from the 1960s through the late 1980s. There is also a strong racial differential in the rate of never-married motherhood. While 80% of white children who end up in female-headed households do so as a result of their mothers' separations, divorces, or widowhood, this is the route for less than half of all black children. In 1991, the majority of black children living in female-headed households lived with a never-married mother (DeVanzo and Rahman 1993). Below, we report statistics based on Census data showing rising rates of never-married motherhood through 2000, especially for minorities.

One criticism of examining never-married motherhood as the family structure of interest is that some nonmarital births are to cohabiting parents whose family lives resemble those of married parents.[14] However, few of the nonmarital births to black women occur during parental cohabitation. For black nonmarital births between 1970 and 1984, only 18% were to cohabiting parents (Bumpass and Sweet 1989). The equivalent rates for Mexican Americans and whites were 40% and 29%, respectively. After birth, a small proportion of children living with unmarried parents live with cohabiting ones (as opposed to living with single mothers or fathers). In 1990, 8.6% of black children living with unmarried parents lived with cohabiting parents (Manning and Lichter 1996). For whites and Mexican-American children living with unmarried parents, 15.4% and 17.6% lived with cohabiting parents. Moreover, based on data from the 1980s and 1990s, Bumpass and Lu (2000) suggest that children born to never-married, non-cohabiting mothers spent only about 15% of their years from ages 0-16 in cohabiting households, versus about 36% of years with married mothers, and the rest in households headed by single females; those born to cohabiting mothers spend roughly equal amounts of time (about 25%) with cohabiting and non-cohabiting or non-married mothers. Together, these figures suggest that never-married motherhood is

blacks.

[14] A second criticism,highlighted by DeLeire and Kalil (2002) and Ginther and Pollak (2004) is that simple classifications of family structure may mask additional heterogeneity. Nonetheless, to the extent that policy debate

an important category to study, and probably most commonly reflects living in a single-parent household for a good part of one's childhood, especially for black and Hispanic children.

A number of demographic and institutional factors may have contributed to the rise of nonmarital family structures. Akerlof et al. (1996) posit that the increasing availability of birth control changed the expectations of potential fathers with respect to responsibility for children, and that low-income mothers, who are less likely to be able to afford birth control (and especially oral contraception), were more likely to be left by themselves to care for any unexpected offspring. In a theoretical framework of fertility and marriage decisions, Willis (1999) finds that if women are in excess supply and have relatively high incomes (perhaps because of public assistance programs), a marriage market equilibrium may exist in which there are marital births to high-income parents, while low-income men have children with multiple female partners outside of marriage. Rosenzweig (1999) assesses the incentive effects of welfare benefits on nonmarital birth decisions and finds that higher benefit levels for Aid to Families with Dependent Children were associated with higher rates of nonmarital childbearing by women from the NLSY79.[15]

There is some evidence that local labor market conditions affect the probability of marriage after a nonmarital birth. This is an application of the hypothesis that the decline of marriage, for blacks in particular, is a function of the declining economic success of less-educated men (Wilson 1987). An empirical test of what is known as the Wilson hypothesis by Wood (1995) finds limited supporting evidence, but Harknett and McLanahan (2004) find that the employment rate of black men explains a large portion of the difference in marriage probabilities after childbirth. Neal (2004) shows that conflicting results from regressions of single-motherhood rates on marriage market prospects can arise once we realize that remaining single without children is also a viable option, and that women's preferences for this status versus single motherhood can vary across marriage markets.[16]

Other work has examined how sex ratios affect marriage decisions. All else equal, a more

---

focuses on marriage per se, estimation of the effects of marital status on child outcomes is of interest.

[15] A recent study of the effects of TANF on children's living arrangements finds mixed and imprecise evidence (Bitler et al. 2006).

[16] Neal also argues that the expansion of government aid to single mothers may have been the catalyst for the increase in single-motherhood rates, especially among the less skilled who otherwise would not have had the

asymmetric sex ratio should result in fewer marriages.[17] Using immigration waves as a shock to sex ratios, Angrist (2002) finds that higher male-to-female ratios had a large positive effect on marriage probabilities for women, even for the second generation of immigrants. His results also suggest that higher sex ratios (males per female) were associated with higher male earnings and incomes of parents with young children. Using variation in male incarceration rates by age, race, state, and year, Charles and Luoh (2005) find that higher incarceration rates (and lower male-to-female ratios) were associated with fewer married women.[18]

In summary, the evidence on the growth of nonmarital family structures points to a number of important factors. First, there may have been a decline in the stigma associated with raising children outside of marriage. Second, there is some evidence that the structure of welfare programs gave a disincentive for poor women to marry after childbearing. And third, for low-income women there has been a decline in the marriageability of men because of reduced economic opportunities or increases in incarceration that have removed men from the marriage market. Our research complements this work on how sex ratios affect marriage markets and it also focuses on the low socioeconomic status, unmarried mothers that are the central subjects of much of this research and the focus of marriage policies.

4. Empirical framework, identification, and estimation

We assume that child outcomes (*Y*) are a function of family structure and a multitude of other factors, only some of which are observable (*X*). We estimate a model relating Y to the never-married status of the child's mother (*NM*), and *X*:

$$Y_i = \beta_0 + \beta_1 NM_i + X_i\beta_2 + \varepsilon_i, \qquad (1)$$

where *i* indexes children. However, the estimated effect of never-married motherhood $(\hat{\beta}_1)$ is biased if

---

economic resources to raise children.

[17] Sex ratios may also affect women's bargaining position within the household, and this may benefit children if the utility functions of women and men weight outcomes of their children differently (Chiappori et al. 2002).

[18] They also find that higher incarceration rates lead to a greater proportion of marriages in which the wife's education was greater than the husband's education. Charles and Luoh argue that this indicates that women find lower quality marriage partners when more men are incarcerated. However, it is difficult to characterize spousal quality only by education. For example, this ignores variation in the criminal records of men in marriage markets.

Using incarceration data from the Bureau of Justice Statistics, Mechoulan (2007) reports some evidence pointing to a negative effect of incarceration of black males on marriage probabilities for black females, although the evidence is not robust; OLS estimates produce this effect, whereas IV estimates instrumenting for incarceration with changes in sentencing and prison capacity sometimes do and sometimes do not.

there is a correlation between never-married motherhood, *NM*, and the unobservable determinants of child outcomes in ε. As discussed earlier, it is easy to construct examples that can give rise to this correlation, pertaining either to the women themselves or their environments. Thus, never-married motherhood may be associated with worse child outcomes even if it has no causal impact on outcomes. In such a case there will be negative bias in $\hat{\beta}_1$, and regression estimates of Equation (1) will overstate the negative effects of never-married motherhood.

Our strategy for identifying the causal effect of never-married motherhood on child outcomes is to use an instrumental variable that is correlated with never-married motherhood, but not correlated with the error term in the child outcome equation.[19] We propose to instrument for never-married motherhood with the male incarceration rate specific to each child *i*'s race or ethnicity (indexed by *r*) and state of residence (indexed by *s*), IR_{rs}, so that we have in mind the two-equation model:

$$\begin{aligned} Y_{irs} &= \beta_0 + \beta_1 NM_{irs} + X_{irs}\beta_2 + \varepsilon_{irs} \\ NM_{irs} &= \alpha_0 + \alpha_1 IR_{rs} + X_{irs}\alpha_2 + \mu_{irs}, \end{aligned} \qquad (2)$$

where μ is an error term. For the linear models written above, the parameter β_1 is identified if *IR* is correlated with *NM*, but *IR* is uncorrelated with *Y* other than through its effect on *NM*. As discussed later, we offer an interpretation of our IV estimator as reflecting the behavior of those mothers (and their children's outcomes) whose decisions to remain never-married are affected by variation in incarceration rates, based on local average treatment effects. While this interpretation implies, in contrast to Equation (2), that there is not a single treatment effect but instead that effects that can vary over the support of the instrument, Equation (2) is still a useful heuristic for thinking about the underlying structure.[20]

The causal connection of our instrumental variable, the race/ethnic- and state-specific incarceration rate, to women's marital behavior is obvious. When more men are in jail or prison, there

[19] In this discussion, we are presuming that the identification problem is not one of contemporaneous endogeneity between child outcomes and whether a child's mother has married, although such endogeneity is possible. Instead, we have in mind selection of mothers into marriage (or never-married status) based on fixed characteristics that will also influence child outcomes. However, the instrumental variables strategy we implement addresses contemporaneous endogeneity as well.

[20] The formal treatment of causal effects in terms of potential outcomes or counterfactuals and the local average treatment effect interpretation is presented in Imbens and Angrist (1994).

will likely be fewer marriages, both because fewer men are available for marriage, and because fewer men are good marriage partners.[21] The instrument must also be uncorrelated with the child-outcome error term (ε), so same-race or same-ethnicity incarceration levels must not be correlated with child outcomes other than through their effect on family structure. Incarceration rates are plausibly excludable from the outcome equation because recent increases in incarceration rates have not been caused primarily by corresponding changes in criminal behavior. Rather, some states have adopted harsher punishments for drug and repeat offenses, while the general level of reported crime has not increased much (Blumstein and Beck 1999; Mauer 1999; Raphael and Stoll 2007).[22]

There are, of course, some potential threats to the validity of our instrumental variable. First, changes in criminal behavior cannot be ruled out, and it is possible that these directly affect child outcomes and are also reflected in incarceration rates. For example, geographic variation in the severity of the crack epidemic in the 1980s may lead to more crime and therefore higher incarceration rates, as well as adverse effects on children. Changes in criminal behavior because of worsened labor market prospects for low-skilled men, which can also have a direct relationship with child outcomes, can pose a similar problem, as can rising crime from de-institutionalization. To address these issues, we include measures of crime rates among the control variables in X, making it even more likely that the remaining variation in incarceration reflects policy rather than changes in criminal behavior that may affect child outcomes. We also include indicators of labor market conditions. In addition, year fixed effects control for aggregate changes in criminal behavior that are constant across states and not captured by the other included variables.

Second, public expenditures on incarceration may be a substitute for expenditures on education. In this case, if we do not include controls for expenditures on education the error term in the child

[21] As Western and McLanahan (2000) point out, high incarceration rates may make men worse marriage partners both because of reduced economic opportunities and because of stigma attached to unmarried men with a history of incarceration (including the possibility that prior incarceration makes them more prone to future criminal activity).
[22] For example, Raphael and Stoll (2007) attribute most of the aggregate increase in incarceration to longer sentences and a greater likelihood of being incarcerated conditional on committing a crime (in particular, for less serious offenses); they attribute only one-fifth of the aggregate increase to increased criminality. Among the sources of increased criminality, Raphael and Stoll identify the de-institutionalization of the mentally ill population,

outcomes equation may be correlated with incarceration rates because the latter reflect (inversely) spending on education. We therefore control for state-level educational expenditures.

The instrument can also be invalid if incarceration has direct effects on child well-being. This is more likely if incarcerated populations tend to come from neighborhoods with concentrated populations of blacks or Hispanics, in which case changes in incarceration rates could have an impact on the community other than through family structure. Suppose that incarceration has negative effects on the home communities of prisoners and therefore on child outcomes. This would lead to bias in the IV estimate pointing to stronger negative effects of never-married motherhood than do the OLS estimates, even if the true effect is weaker than suggested by OLS. It turns out, however, that in all of the IV estimations we report, the IV estimates are less indicative of adverse effects of never-married motherhood than are the OLS estimates, so eliminating this type of bias would only strengthen our conclusions.

Suppose, conversely, that incarceration has a positive effect on the home communities of prisoners, perhaps by removing criminals from those communities who, for example, draw teenagers into crime and hence out of school. In this scenario, we might find that the IV estimates point to weaker adverse effects of never-married motherhood, or even positive effects, compared to the OLS estimate (and compared to the true effect). Given that this latter scenario does characterize the differences we find between OLS and IV estimates, our results could be explained by a direct positive effect of incarceration on child outcomes. This alternative explanation of some of our results is difficult to disentangle from the effects of incarceration via marriage. We come back to this point later.

Finally, incarceration can have a direct effect on child outcomes by reducing teen childbearing and increasing education of mothers (Kamdar 2007; Mechoulan 2007). To avoid correlation between the instrument and the error term in the child outcomes equation arising via this channel, we also include controls for mother's education and her age at the child's birth.

The incarceration rate we use for most of our analysis is defined for men aged 18-40 in the current year. It is not immediately obvious for what age ranges and years the incarceration rate should be

---

declining labor market prospects for low-skilled men, and the crack cocaine epidemic.

defined. If, for example, marriage decisions are primarily made at young ages (like the late teens or early 20s), then given that we are studying *children* aged 15-17, it would be more appropriate to use an incarceration rate for younger men from a decade prior to the observation on the child. Although we report some estimates using this strategy as part of our robustness analysis, research indicates that contemporaneous incarceration rates of older men are likely to be important as well. Evidence shows that many first marriages are experienced by men in their 30s. For example, based on data from the 2002 National Survey of Family Growth, Lichter and Roempke Graefe (2007) report that the percentage of black men ever married rises from 46 percent at age 30 to 74 percent at age 40. For Hispanics and non-black, non-Hispanics, the increase is smaller, by 18 to 20 percentage points over this age range (from 60 to 78 percent and 62 to 82 percent, respectively). Furthermore, for the lower-income population of single women with children, qualitative evidence reported by Edin (2000) and Edin and Reed (2005) indicates that there is a norm of childbearing first followed by a desire for marriage later, with the delay arising both because women want men to have established themselves financially, and because women want to have established *themselves* financially so that they can legitimately threaten to leave marriages that, for this sub-population, are often to men with drug-, crime-, or abuse-related problems with relatively low economic security. Indeed, many women reported that the ideal age for childbearing was in a woman's early 20s, while the ideal age for marriage was in the late 20s or early 30s. Finally, the incarceration rate for a broad range of ages for males is appropriate given that women who give birth out of wedlock are more likely to marry older men if they do marry (Qian et al. 2005.

Thus, incarceration rates of potential marriage partners in their 30s can impact the probability that a teenager's mother is never married. Of course incarceration rates for currently older men are also informative about the incarceration rates these same men (and their potential spouses) faced when they were younger, to the extent that incarceration is long-term or there is extensive recidivism, which is another reason our incarceration rate IV may help predict never-married status of the mothers of current 15-17 year-olds. Finally, although we base on instrument on the incarceration rate for the broad 18-40 age range, we explore the sensitivity of the results to excluding younger age ranges (18-24); on the other hand, given that our incarceration rate is an estimate and incarceration is highest among 18-24 year-olds, we may get a more accurate measure of incarceration rates when we including this age range.

The child outcome we study is a discrete indicator for whether children drop out of high school. Other child outcomes are of interest, of course, but our estimation strategy requires that we use Census data, in which information on child outcomes is extremely limited. Although this outcome is binary, we estimate the effects of never-married motherhood on high school dropout using the linear probability formulation, because this enables a local average treatment effect interpretation of our IV estimator (as discussed below) and consistency of the estimates does not hinge on a correct assumption about the distribution of the error terms. In addition, the family structure variable capturing never-married motherhood is also a discrete indicator. We follow Wooldridge (2002, Chapter 18) and proceed by first estimating a probit for never-married motherhood, normalizing the variance of the error term to equal one. We then form the estimated probabilities $\Phi(\hat{\alpha}_0 + \hat{\alpha}_1 IR_{rs} + X_i \hat{\alpha}_2)$ and use them as the instrumental variable for *NM* in the equation for *Y*. We refer to this as two-stage IV (2SIV). This estimator is robust to misspecification of the equation for never-married motherhood as a probit.

We also report two-stage least squares (2SLS) estimates treating both equations as linear; these estimates turn out to be much less precise, although almost always of the same sign. The never-married motherhood rate is quite low, especially early in the sample period. As a result, linear probability estimates of the first stage lead to many negative fitted values. This appeared to result in a much weaker first stage, which makes intuitive sense as the variation near and below zero in the estimates of the first stage estimated as a linear probability model are not associated with actual variation in never-married status.[23] However, the difference between the precision of the 2SIV and 2SLS procedures raises the possibility that the non-linearities in the exogenous control variables introduced by using a first-stage probit contributed to stronger identification of the model, which would be less reassuring because we

[23] Using evidence from a Monte Carlo study, Angrist (1991) suggests that estimating a structure like ours with two linear probability models work as well as discrete choice models, but more recent work by Bhattacharya et al. (2006) reaches the opposite conclusion and, in particular, points to problems with predicted values outside the zero to one range in the first-stage linear probability model.

cannot be confident that non-linear functions of the control variables do not actually belong in the model. Later, we assess identification of our model in light of this concern.

It is important to clarify what we identify with this model. In particular, if we begin with the potential outcomes framework where the effect of never-married motherhood can vary over the support of the IV, then under assumptions specified in Imbens and Angrist (1994), the standard IV estimator is a weighted average of local average treatment effects with the weight concentrated on parts of the support of the IV for which variation in the IV has a greater impact on the endogenous variable. In our context, this implies that we are estimating the effects of never-married motherhood for the children of those women whose marriage behavior is affected by variation in the incarceration rate of men of the same race or ethnicity. These are likely to be families with women who have low skills and poor labor market prospects, and who likely face a less desirable pool of potential marriage partners.[24]

With any instrumental variables design there is a concern about the weak predictive power of the instruments, which can lead to large confidence intervals (and poor asymptotic approximations for them). In linear models with iid errors, Staiger and Stock (1997) show that $1/F$ is an approximate estimate of the finite sample bias of IV towards the OLS estimate. This leads to a rule-of-thumb threshold for F-statistics of at least 10 for the first stage.[25] However, in the non-iid case less is known about the relationship between the correct F-statistic (in our case, clustering the data at the state level) and the properties of IV estimates. We nonetheless report this F-statistic for each specification.[26] We are also unaware of any such rules of thumb for the case of a generated instrument like in the 2SIV estimator we use, although as reported below there is no question that the generated instrument is a very strong predictor of never-married status. However, since we are in uncharted territory regarding test statistics that might

[24] Incarcerated men tend to have less education and worse labor market prospects (Pastore and Maguire 2006). Positive assortative mating on education in marriage markets is pervasive, and assortative mating on schooling and work behavior if anything strengthened during the sample period we study (Mare 1991; Pencavel 1998).

[25] Stock and Yogo (2005) suggest a critical value of 16.38 for the F-statistic for a single endogenous regressor and one instrumental variable.

[26] The rules of thumb for the F-statistic are easily met for estimates of the two linear equations if we cluster standard errors by state and year, which is the level at which incarceration rates vary in the models we estimate, but not if we cluster by state only, which is what we show in the tables. The latter F-statistic is robust to more deviations from standard iid assumptions on the error term, and hence is more likely to be valid as a test of restrictions on the

mitigate concerns about weak instruments, we rely more on estimating a number of specifications intended to assess the robustness of the estimates to problems relating to weak instruments.

5. Data and Descriptive Statistics

Our primary data come from the Integrated Public-Use Microdata Series (IPUMS) of the 1970-2000 Censuses (King et al. 2003). The Census data are not longitudinal and have limited information on child outcomes. But the IPUMS is suitable for this study because it has large samples and a set of variables with consistent definitions over a long period. The IPUMS is also ideal for calculating institutionalization rates over race/ethnicity-state-year cells. We use the 1970-2000 surveys because the greatest increase in incarceration occurred within this period (Pastore and Maguire 2006). The specific Census files used are the 1970 Form 2 state sample (a 1% sample of the population) and the 5% state samples from the 1980, 1990, and 2000 Censuses.[27] We restrict these samples to children whose race and ethnicity is identified as either Hispanic, non-Hispanic white, or non-Hispanic black.[28] Regression samples are further restricted to blacks and Hispanics for reasons discussed below.

We want to study the effects on a child of the child's mother not marrying after the child's birth. Thus, we first restrict the sample to children living with their mothers.[29] We exclude children who are coded as residing in group quarters (institutional or otherwise) because it is impossible to determine their family structure. We are primarily interested in decisions of biological mothers, so we drop children who are identified in the IPUMS as probably living with a non-biological mother (usually a stepmother). We do this in part because it is unclear how incarceration rates would affect the remarriage decisions of non-incarcerated biological fathers.

With this sample, we categorize children by whether their mothers report having ever married versus having never married. There are some children in the sample identified as living with married

---

instruments.

[27] The Form 2 sample is used for 1970 because the Form 1 sample does not have information about school attendance.

[28] Throughout the text, we refer to non-Hispanic whites as whites and non-Hispanic blacks as blacks.

[29] This definition excludes children living with neither biological parent. Bitler et al. (2006) shows that this is a nontrivial proportion of black children, especially those of less-educated household heads. In 1989, 9% of children living with a household head with at most a high school education lived with neither biological parent, while 15% of

mothers who might have spent a substantial period of their childhood with mothers who were not married at the child's birth and for part of the rearing of the child, but married later. If these children exhibit any of the effects experienced by children identified as living with never-married mothers at the time of the Census, then estimates of the effect of never-married motherhood would likely be biased toward zero. But this latter type of measurement error cannot by itself account for the finding that the IV estimate of the effects of never-married motherhood is the opposite sign of the OLS estimate, as the this type of measurement error would likely only introduce attenuation of the OLS estimate.[30]

We begin in Table 1 by showing some results on intermarriage. As shown in Panel A, for blacks and whites about 97-98% of married women are married to men of the same race, for the Census data in our sample period. Intermarriage has become only slightly more common during the sample period, so that within-race marriage rates in 2000 for whites were about 97% for whites and 95% for blacks. On the other hand, Hispanic-white intermarriage is more common, with about 16% of Hispanic married women married to white men. This difference between black and Hispanic marriage patterns might suggest that our IV procedure would be most powerful for black women, as for them variation in incarceration rates of men of the same race/ethnicity is likely to be most directly linked to the availability of marriage partners. However, as shown in Panel B of the table, Hispanic-white intermarriage is much less common among the least-educated Hispanics who are most likely to be affected by variation in incarceration rates; the marriage patterns indicate a Hispanic-white intermarriage rate of only about 4% for women with fewer than 12 years of completed education. Thus, variation in incarceration rates may provide as good an "experiment" for Hispanics as for blacks.

Table 2 shows the percentage of children aged 15-17 years living with never-married mothers. For all racial and ethnic groups, there has been a secular increase in never-married motherhood. The relative increases are similar for whites, blacks, and Hispanics, but the absolute increase is by far the

---

children living with a household head with fewer than 12 years of education lived with neither biological parent.

[30] At least, in a standard IV setting with a continuous right-hand side endogenous variable, the IV estimation corrects for measurement error in that variable as well. If we think instead in terms of the Wald estimator of the local average treatment effect, then the bias may be to accentuate the estimate of this effect, as a higher incarceration rate leading to mothers spending more time unmarried even if they eventually marry implies that the predicted shift in

largest for black children. Fewer than 3% of black children aged 15-17 years lived with never-married mothers in 1970, while more than 21% lived with never-married mothers in 2000.

We use cross-sectional data to examine the effect of family structure on child outcomes, so we must focus on educational outcomes that are observable while children still reside with their parents. We define a high school dropout variable that is equal to one if the child is not currently enrolled in school and has not completed 12th grade. Whether young people drop out of high school is a very important outcome to consider. By age 25, workers who graduate from high school have wages at least 20% higher than workers who do not complete high school or complete an equivalency diploma (Cameron and Heckman 1993). High school completion is also a strong negative predictor of criminal activity, arrest, and incarceration (Lochner and Moretti 2004) and a positive predictor of healthy behaviors and health (Kenkel et al. 2006).

Table 3 shows the percentages of children in the sample who have dropped out of high school. For all race/ethnicity-year cells except one, the children of never-married mothers are more likely than the children of ever-married mothers to drop out of high school. However, these differences are smaller for blacks and Hispanics. In general, there has been a secular decline (since 1980) in the proportion of teens dropping out of high school. To control for other factors that may be driving these trends, such as educational policies, all of our models include year fixed effects.

We also create a number of control variables from the IPUMS data. Using information from the mother's record, we construct the following dummy variables for mother's completed education (at the time of the Census): has not finished high school, has finished high school only, has finished only some college, and has finished at least four years of college. Table 4 shows that, compared with all other mothers, never-married mothers are 10 percentage points less likely to have completed four years of college, 2-3 percentage points less likely to have had some college education (but fewer than four years) or to have completed only high school, and 15 percentage points *more* likely to have dropped out of high school. In addition to controlling for the mother's education, we calculate the age of the mother at time of

never-married motherhood as a result of higher incarceration may be understated.

the child's birth. Table 4 shows that never-married mothers have their children at an average age of 22.8 years, while other mothers have their children at an average age of 26.3 years.

We also include some state-varying controls in our analysis. These controls account for consequences of incarceration policy that may affect children in ways other than through changes in family structure. First, since fiscal resources devoted to incarceration may be substitutes for public expenditures on education, we include per-pupil elementary- and secondary-school expenditures by state for the fiscal years 1969-1970, 1979-1980, 1989-1990, and 1999-2000.[31] Second, since incarceration may be related to the level of crime, another set of controls takes into account state-year criminal activity. We use 3-year moving averages of the crime rates from the Federal Bureau of Investigation's Uniform Crime Reports. We control for the rates of violent crime and property crime (the two broadest crime categories) as well as larceny, which is a subset of property crime involving neither violence nor fraud.[32] Third, since levels of crime and incarceration may be a function of state labor market conditions that may also affect child outcomes, we control for the employment rate and mean annual earnings of men aged 18-40 by state and year from the IPUMS. To avoid endogenous effects of incarceration, we construct these statistics for white men.

We use institutionalization rates as a proxy for incarceration rates. Ideally, our incarceration rates would come from administrative records from the Bureau of Justice Statistics (BJS). Unfortunately, the BJS does not publish data by state and race or ethnicity, and data they can make available with estimates by state and race or ethnicity are not considered reliable. Data from the decennial Censuses provide a suitable proxy, since they cover both the institutionalized and non-institutionalized populations and do an excellent job of sampling the institutionalized segment of the population. Census employees use administrative records if institutionalized respondents are unable to fill out the Census forms, so the institutionalized population is well accounted for in the IPUMS.

[31] These data come from the Digest of Education Statistics 2005, which we accessed at http://nces.ed.gov/programs/digest/d05/tables/dt05_167.asp on March 17, 2007.
[32] These data come from the website of the Bureau of Justice Statistics, which we accessed at http://bjsdata.ojp.usdoj.gov/dataonline/Search/Crime/State/statebystatelist.cfm on May 20, 2007. The moving averages are our calculations. These three types of crime often enter significantly into the regression models.

The institutionalization rate is defined as the proportion of respondents residing in institutional group quarters, as identified by the group quarters question. Institutionalization rates are calculated from the full samples. The definition of institutional group quarters includes correctional facilities, mental institutions, and retirement facilities. Non-institutional group quarters includes military housing and college dormitories, and these individuals are excluded from the calculation of institutionalization rates. Butcher and Piehl (2007), based on 1980 Census data in which institutional categories were broken down, show that institutionalization is a very good proxy for incarceration when the sample is limited to adults no more than 40 years old, because older individuals are more likely to be in mental or retirement institutions.[33] After calculating the rates, each child observation is assigned an institutionalization rate based on the child's race or ethnicity and state of residence at the time of the Census.[34]

Despite institutionalization capturing incarceration well, there are other sources of error in measuring incarceration rates. Sampling error is more likely for minorities in small states because of small sample sizes, and sampling error is also more likely in 1970 than in the other years because the sample is one-fifth the size of the 1980-2000 samples. In addition, there is a potential aggregation problem because incarceration rates are calculated at the state level (the level at which the analysis is done), but they may have more local effects. However, since incarceration is not measured at the household level, there is no way to use Census data to construct more geographically disaggregated measures of incarceration.

Figure 1 shows histograms for incarceration rates for men aged 18-40 years across states in 1980, 1990, and 2000. In Figure 1, two things are apparent. First, incarceration rates for whites are low in all states, with all the observations clustered in the lower end of the distribution. In contrast, incarceration rates in most states are much higher for Hispanics, and more strikingly so for blacks. Moreover, for both minority groups incarceration rates clearly increased over these decades, again particularly for blacks.

---

[33] Raphael (2006) shows that the rates generated from the IPUMS are nationally comparable by race to the rates published by the Bureau of Justice Statistics, which are generated by administrative records.

[34] We also constructed incarceration rates using the child's state of birth from the Census 10 years before the child observation. Later, we describe the motivation for using these lagged incarceration rates and the corresponding results.

Figure 2 shows the histograms of changes in incarceration rates across states over the periods 1980-1990, 1990-2000, and 1980-2000. These histograms show that there have been dramatic changes in incarceration rates for minorities, especially in some states, with the greatest changes between 1990 and 2000. It is this variation that is central to our identification strategy.[35] The lack of substantial changes for whites, coupled with low incarceration rates for them in general as well as low never-married rates, helps explain why we focus on minorities in our analysis.

There might be some concern that increases in incarceration have been concentrated in particular geographic regions of the country, but Figure 3 shows that the states with the largest increases are geographically dispersed. States with no shading had the smallest increases in the incarceration rate for black men aged 18-40 years (or even slight decreases). States with the darkest shading had the greatest increases in these rates. Note that states with small, medium, and large increases in black incarceration are represented in all major regions of the country.

Table 5 is a descriptive presentation of the first stage of the research design. Its cells show the percentage of children aged 15-17 living with a never-married mother. The columns are broken down by whether the child's assigned incarceration rate is less than the 25th percentile, between the 25th and 75th percentiles, or greater than the 75th percentile. The incarceration-rate percentiles are calculated for each year of the sample and also for the pooled sample—to reveal how variation in incarceration rates is associated with the rate of never-married motherhood across states within each year, and for the sample as a whole. Looking across the columns, the table provides relatively clear evidence that in states and years with higher incarceration rates the rates of never-married motherhood are higher for blacks and Hispanics, although there are some exceptions, especially in the early years in the sample for blacks. For white women, however, this pattern is not apparent, and within years white women appear to respond quite differently to higher male incarceration, as living in a state with less incarceration is associated with lower rates of never-married motherhood; for this reason and those discussed above, our analysis from this point on focuses on black and Hispanic children.

---

[35] There are some extreme values generated by small cells, but since we use individual-level data, these observations

6. Results

Main Results

Our full model relating educational outcomes of children to family structure is:

$$Y_{iast} = \beta_0 + \beta_1 NM_{iast} + X_{iast}\beta_2 + S_{st}\beta_3 + D_s\delta_1 + D_t\delta_2 + D_a\delta_3 + D_{at}\delta_4 + \varepsilon_{iast}, \qquad (3)$$

where Y_{iast} is an indicator for high school dropout for child *i*, aged *a* years, living in state *s* in year *t*. *NM* is an indicator for whether the child's mother has never married. *X* is a vector of individual-varying controls, and *S* is a vector of state- and time-varying controls. We estimate all of the models discussed in this section separately for blacks and Hispanics, so there is no longer a subscript indicating race or ethnicity. The model also includes state dummy variables (D_s), year dummy variables (D_t), single-year age dummy variables (D_a), and interactions between the year and age dummy variables (D_{at}); the latter allow for different aggregate changes by age. β_1 is the parameter of interest that we expect to be positive in the single-equation model for high school dropout that does not account for endogenous selection, corroborating the evidence discussed earlier that the children of never-married mothers have worse outcomes.

For the two-step instrumental variables estimator, a probit model of never-married motherhood is first estimated that includes the incarceration rate instrument and the exogenous controls and fixed effects used in Equation (3):

$$P[NM_{iast} = 1] = \Phi[\alpha_0 + \alpha_1 IR_{st} + X_i\alpha_2 + S_{st}\alpha_3 + D_s\varphi_1 + D_t\varphi_2 + D_a\varphi_3 + D_{at}\varphi_4], \qquad (4)$$

where Φ is the cumulative normal distribution and *IR* is the incarceration rate for men of the same race or ethnicity as the children in the sample.[36] After estimating Equation (4), predicted values of never-married motherhood are generated as $\hat{\Phi}_{iast} = \Phi[\hat{\alpha}_0 + \hat{\alpha}_1 IR_{st} + X_i\hat{\alpha}_2 + S_{st}\hat{\alpha}_3 + D_s\hat{\varphi}_1 + D_t\hat{\varphi}_2 + D_a\hat{\varphi}_3 + D_{at}\hat{\varphi}_4]$. Then, these predicted values of never-married motherhood serve as an instrument for never-married motherhood in a two-stage least squares model:

have an inconsequential influence on the results.

[36] The results we present below are robust to other specifications for this "zeroth" stage such as a logit regression.

$$
\begin{aligned}
Y_{iast} &= \beta_0 + \beta_1 NM_{iast} + X_i\beta_2 + S_{st}\beta_3 + D_s\delta_1 + D_t\delta_2 + D_a\delta_3 + D_{at}\delta_4 + \varepsilon_{iast} \\
NM_{iast} &= \theta_0 + \theta_1\hat{\Phi}_{iast} + X_i\theta_2 + S_{st}\theta_3 + D_s\lambda_1 + D_t\lambda_2 + D_a\lambda_3 + D_{at}\lambda_4 + \nu_{iast}.
\end{aligned} \tag{5}
$$

In this two-step instrumental variables model, the estimated coefficient on never-married motherhood in the child outcome equation, $\hat{\beta}_1$, is can be interpreted as the treatment effect of never-married motherhood on the relevant child outcome, identified for (and averaging over) those children whose mothers' marriage behavior is affected by variation in incarceration rates (hence the local average treatment effect interpretation).

Before discussing our analysis of child outcomes, we present the estimates of the equations for never-married motherhood. Table 6 reports estimates from OLS and probit specifications, in all cases including the exogenous controls, and in the even-numbered columns including the incarceration rate as well. In all of the columns, we see that children of more highly-educated and older mothers are less likely to have never-married mothers. Turning to the even-numbered columns, for blacks the estimated coefficient on the incarceration rate in the linear probability model is 0.261 (Column 2), statistically significantly different from zero at the ten-percent level (p = .06). The analogous estimated marginal effect from the probit regression for blacks is 0.114 (Column 4), not statistically significant (p = .11).[37] For Hispanics, the OLS estimate of the coefficient on the incarceration rate is 0.671 (Column 6) and statistically significantly different from zero at the five-percent level. The analogous estimated marginal effect from the probit regression for Hispanics is 0.228 (Column 8), also statistically significant at the five-percent level. To put these estimates in context, suppose we chose approximate modal increases in incarceration rates over the 1980-2000 period—increases of 0.07 for black men and 0.02 for Hispanic men. These increases in incarceration would correspond to a 1.8 percentage point increase (0.261 × 0.07 × 100 for OLS) in never-married motherhood for blacks and a 1.3 percentage point increase (0.671 × 0.02 × 100 for OLS) in never-married motherhood for Hispanics; the implied effects based on the probit estimates would be smaller.

[37] Note that this is a case where the marginal effects from the probit are somewhat different from the estimated coefficients of the linear probability model. This likely reflects the fact that the incidence of never-married motherhood is relatively low; this is the type of case where a linear probability model is less appropriate, which is

Table 7 shows estimates of the models for whether a child has dropped out of high school. For both blacks and Hispanics, children are more likely, on average, to have dropped out of high school if they live with a never-married mother. In the OLS specifications (Columns 1 and 4), blacks and Hispanics living with never-married mothers are 1.7 and 3.2 percentage points more likely to be have dropped out of high school, respectively. Given mean dropout rates at these ages of 6% for blacks and 7% for Hispanics, these are economically significant effects.

However, the OLS results may provide biased estimates of the effects of never-married motherhood if there is nonrandom selection into family structure. The 2SIV estimates accounting for this nonrandom selection using the two-step instrumental variables estimator are reported in Columns 3 and 6; we show the 2SLS estimates in Columns 2 and 5. For both blacks and Hispanics, the estimated effects of never-married motherhood on whether a child has dropped out of high school become negative, and are significantly different from zero in the two-step estimator (Columns 3 and 6). For black and Hispanic children whose mothers' marriage decisions are affected by variation in incarceration rates, never-married motherhood is estimated to reduce the likelihood that children drop out of high school, once we account for the endogeneity of their mothers' marriage decisions. For Hispanics, the estimated effect of never-married motherhood might be viewed as quite large—certainly the 2SLS estimate appears suspect, but it is also very imprecise.

Thus far the effect of incarceration on never-married motherhood was restricted to be linear. But the effects of incarceration on marriage markets may be nonlinear, becoming stronger when incarceration rates are high. To explore nonlinear effects of the instrument, Table 8 includes models with polynomials of incarceration rates in the first stage. The point estimates indicate that the effects of incarceration rates on never-married motherhood are in fact stronger at higher incarceration rates.[38] Moreover, for blacks the estimated coefficient of the incarceration rate in the first-stage of the 2SLS estimation was only marginally significant for the simple linear specification in Table 6 (the F-statistics are reported in Table

---

why we focus on estimators that use probit specifications for never-married motherhood.

[38] Although the estimated coefficients of the higher-order terms appear quite large, they are multiplied by numbers in the zero to 0.2 range, and yield reasonable predictions of the probabilities of never-married motherhood in the

7), whereas in this non-linear specification in Table 8 the instruments are jointly significant for both blacks and Hispanics. However, for the most part the second-stage results when non-linear effects of the incarceration rate are allowed in Table 8 are quite similar to those in Table 7. The one difference is that, corresponding to the stronger first-stage, for the 2SLS estimates the standard errors are somewhat smaller and the estimated effects for Hispanics fall somewhat; the greater precision suggests that it is useful to introduce the non-linearities in the first-stage equation. Regardless, the qualitative conclusions are robust to this alternative specification.

The estimates of the effect of never-married motherhood on whether children have dropped out of high school have two implications. First, they suggest that unobservable characteristics drive the selection into never-married motherhood and the negative school outcomes of the children of never-married mothers. And second, they suggest that the children of the women who choose to remain unmarried actually do better in terms of avoiding dropping out, for those women whose marriage decisions are affected by variation in incarceration rates.

Identification and Robustness Checks

There are a few issues regarding identification that merit further consideration. First, it is apparent from comparing the 2SLS and 2SIV estimates in Tables 7 and 8 is that even when 2SLS is uninformative, the 2SIV estimator yields significant and robust results. A potential concern is that in the 2SIV estimation the inclusion of non-linear functions of the control variables in the fitted probability of never-married motherhood serves to identify the effect of never-married motherhood, rather than the variation in incarceration rates. We would not want to rely on this type of identifying information, however, because there is no reason to be highly confident that non-linear functions of the control variables should not themselves appear in the model for high school dropout.

To examine this possibility, we augment the set of control variables to include a large number of interactions and non-linear terms (all cross-products of dummy variables for the mother's or child's characteristics, and quadratics in all the continuous variables). When we did this, as reported in Table 9,

---

range of the data.

the resulting 2SIV estimates were qualitatively similar to the baseline estimates in Table 7 in that the IV estimates of the effect of never-married motherhood on children dropping out of high school are negative rather than positive.[39] However, for blacks the estimate falls by about half and the standard error becomes larger, so that the estimated effect is statistically insignificant. For Hispanics, in contrast, although the estimate also falls by about half, to a more plausible magnitude of −.086, it remains nearly significant at the five-percent level ($p = .052$). Thus, the results for this approach point to some uncertainty regarding whether never-married motherhood actually reduces the likelihood of high school dropout for blacks, but the conclusion that it reduces this likelihood for Hispanics is robust. Moreover, the qualitative similarity of the estimates when the model is loaded up with nonlinear terms in the control variables leads us to conclude that the 2SIV estimator provides credible evidence that estimates that do not take account of selection into marriage may erroneously imply that never-married motherhood is bad for the educational outcomes of all children.

Second, if incarceration rates have direct effects on child outcomes, so that the exclusion restriction underlying the IV estimation is invalid, then our estimates may instead simply be picking up the direct effect of incarceration, albeit still suggesting that higher incarceration leads to better outcomes for children. Although we account for environmental and policy differences across states by controlling for crime rates and education expenditures, we cannot rule out direct effects associated with incarceration even conditioning on these controls—in particular, effects of incarceration policy.

We took a number of approaches to address this concern. One was to use 10-year-lagged incarceration rates for younger men (aged 18-24) from the birth state of the child as the instrument for family structure, since these lagged incarceration rates are unlikely to directly affect dropout behavior.[40] For blacks, lagged incarceration rates did not predict never-married motherhood significantly, whether in the linear or non-linear (quadratic or cubic) specifications. For Hispanics, however, the instrument was stronger, and the results reported in Panel A of Table 10 are quite similar to the earlier findings, indicating

[39] We do not report such estimates for the 2SLS estimator because in that case the identification comes solely from the variation in incarceration rates.
[40] This contrasts with the preceding results, which use the contemporaneous incarceration rate for 18-40 year-old

strong effects of incarceration on never-married motherhood (as reflected in the F-statistics), and estimates of the effect of never-married motherhood on the likelihood that children drop out of high school that are similar to the previous table, although a little weaker statistically (two significant at the ten-percent level, and the third, in Column 6, with a p-value of .11).[41] The slightly weaker statistical evidence relative to Table 9 is attributable to larger standard errors, which is perhaps not entirely surprising given that using lagged incarceration rates means that we discard information on the large changes in incarceration rates that occurred between 1990 and 2000 (shown earlier in Figure 2). Regardless, our confidence in the results for Hispanics obtained from the preceding estimates using contemporaneous incarceration rates is bolstered directly by these findings. For blacks, in contrast, the evidence is at best indirect, as we simply cannot tell whether the indication for Hispanics of the validity of the contemporaneous instrument used in the preceding tables necessarily carries over to blacks; it is not immediately obvious, however, why this would differ between blacks and Hispanics.

A second approach is to revert to using the contemporaneous incarceration rate as an IV, but to use a narrower age range that excludes 18-24 year-olds whose criminal behavior might have a more direct effect on teenagers. Thus, Panel B reports estimates using the contemporaneous incarceration rate for 25-40 year-olds. In the models with linear, quadratic, and cubic IV terms, we find results that are qualitatively similar to our baseline estimates in Tables 7 and 8. Although not shown in the table, the results for blacks were also very similar to the earlier estimates.

Finally, a more demanding approach is to continue to use the incarceration rate for men aged 25-40 as an instrument, and to include the incarceration rate for men aged 18-24 as a control, since the incarceration of this younger cohort should not affect the marriage decisions of mothers with teenage children but, as noted above, might more likely affect teen outcomes.[42] As expected given what we found

---

men. When we use an incarceration rate lagged 10 years, it is less likely that the rate for older men will matter.

[41] We report the 2SLS as well as the 2SIV estimates, but given the earlier findings from this point on we focus only on the latter. The weaker predictability of never-married motherhood for blacks than for Hispanics when using the 10-year lagged incarceration rate for young men may reflect the findings reported in Lichter and Roempke Graefe (2007), discussed in Section 4, that a larger share of marriage among black men than among Hispanic men occurs between ages 30 and 40.

[42] In this case, we stick with linear specifications of the effects of incarceration rates in the equation for

simply using lagged incarceration rates as IV's, this approach was not informative for blacks. However, as reported in Panel C of Table 10, the results for Hispanics are again similar.

Table 11 presents a series of additional analyses and robustness checks. First, evidence discussed earlier finds differential effects of family structure for boy and girls, so we would like to know if our approach points to differences. This analysis may also help to address the identification issue. In particular, we might expect that any potential direct effects of incarceration on children may be more acute for boys than girls. Thus, if our conclusion that never-married motherhood reduces the likelihood of dropping out of high school holds only for boys, we might suspect that the evidence is driven by direct beneficial effects of incarceration on teenage boys.[43] The estimates stratified by sex of the child are reported in Panels B and C (Panel A repeats the baseline estimates for comparison). The 2SIV estimates for blacks are very similar for boys and girls. For Hispanics, we also find qualitatively similar evidence. Indeed, if anything for Hispanics, the effect of never-married motherhood in reducing high school dropout is stronger for girls than for boys, which, based on the reasoning above, implies that the results are not driven by a direct positive effect of incarceration of slightly older males on teenage boys. Thus, these findings lend further support to our argument that direct effects of incarceration do not confound our identification strategy.

Next, we consider a couple of measurement issues. First, some prisoners are incarcerated outside of the state in which they previously resided. In that case, the measured incarceration rate in a state may inaccurately capture the extent to which men have been removed from the marriage market. To more accurately capture how incarceration might affect the sex ratio, the results in Panel D are based on incarceration rates calculated only for men who currently reside in the same state they did five years before the Census. The 2SIV estimates are qualitatively similar to our baseline estimates.[44]

Second, given the massive increase in adult incarceration, it is not surprising that there has been

---

never-married motherhood, even though the specifications with polynomials generally gave more precise IV estimates. We did not want to favor the incarceration rate IV for older men over the (linear) control simply via the inclusion of higher-order terms.

[43] We are grateful to Andrew Noymer for suggesting this test.

[44] The 2SLS estimates are much closer to zero, and one is positive.

some increase in youth institutionalization.[45] Institutionalized youths are not in our sample because their family structures cannot be identified from the Census data. If teen institutionalization is positive correlated with being raised by a never-married mother (owing in part to higher incarceration rates of men) and with dropping out of high school, both of which seem plausible, then our IV strategy may put more weight on the best performing children of never-married mothers. To attempt to account for this, we estimated models for a sample including institutionalized children. We classified all of these children as having never-married mothers, imputing to their "mothers" the associated maternal controls for never-married mothers in the same state, year, and race/ethnic group. As reported in Panel E, in all cases the estimates become more positive, consistent with the possibility that our estimates are biased toward finding that never-married motherhood reduces high school dropout. However, the signs of the 2SIV estimates remain the same, and the estimate remains statistically significant for Hispanics. Since this approach in a sense assumes the worst—that all institutionalized children have never-married mothers—it no doubt overstates the extent to which our estimates might be biased by the exclusion of institutionalized children. The findings therefore establish that youth institutionalization is not mechanically driving our results.

A final potential concern is that changes in other policies that affect schooling decisions may be correlated with changes in incarceration rates, biasing the IV estimates. Two policies of particular concern are compulsory schooling and minimum wage laws; minimum wages have been shown to reduce high school attendance among teenagers (e.g., Neumark and Wascher 2003) and compulsory schooling laws to increase it (e.g., Acemoglu and Angrist 2000). In Panel F of Table 11 we present estimates of models in which we control for the state minimum wage and for whether a child was covered by a compulsory schooling law.[46] The estimates for both blacks and Hispanics are almost identical to those from the

[45] For black children aged 15-17 years, the institutionalization rate was 1.3% over the sample period, increasing from 0.3% in 1970 to 2.2% in 2000. For Hispanic children of the same ages, 0.8% were institutionalized over the sample period, increasing from 0.01% in 1970 to 1.1% in 2000.
[46] Compulsory schooling laws come from various editions of the Digest of Education Statistics. Because a record of laws is not available for every year, we use the closest available listing of compulsory schooling laws: 1972 laws for 1970, 1978 laws for 1980, 1989 laws for 1990, and 2000 laws for 2000. A child is coded as being covered by a compulsory schooling law if the child's age is less than the maximum required age of compulsory schooling in a

baseline specifications.[47] Thus, changes in compulsory schooling requirements and minimum wages are not confounding our IV estimates.

Summing up, our estimation and assessment of the robustness of the results to identification- and measurement-related issues leads to a few conclusions. First and foremost, in *none* of the 2SIV estimations do we obtain an estimate of the effect of never-married motherhood that is the same sign as the OLS estimate that does not account for endogenous selection into never-married motherhood. For both blacks and Hispanics, the sign of the IV estimate always indicates that never-married motherhood leads to a *lower* likelihood that children drop out of high school.[48] Second, the conclusions are stronger and more robust for Hispanics than for blacks. Although the 2SIV estimates are always negative for blacks, they are often insignificant, and the estimates are less robust to alternative specifications varying exactly how incarceration rates identify the effects of never-married motherhood, perhaps raising concerns about how well the identification strategy works for them.[49] For Hispanics, in contrast, the results are quite robust, and for a battery of specifications using our preferred (2SIV) estimator that are intended to gauge the strength of the evidence and the validity of the identification strategy, we always find significant or marginally significant evidence pointing to beneficial effects of never-married motherhood on children's educational outcomes.

Interpretation

The finding that children of never-married mothers have better educational outcomes is likely to

---

particular state and year. The minimum wage variable is the maximum of the state and federal minimum wages in 1970, 1980, 1990, and 2000, adjusted to 1983 dollars using the All-Urban series of the Consumer Price Index.

[47] Although not reported in the table, the point estimates confirm earlier research, with higher minimum wages increasing the likelihood of dropping out (significant for blacks) and higher compulsory schooling ages lowering it (significant for Hispanics).

[48] Regarding concerns about weak instruments, the evidence that our 2SIV estimates are quite different from our OLS estimates and of opposite sign, which suggests that problems of finite-sample bias of the IV estimates toward the OLS estimates owing to weak instruments cannot account for our results. But if the true confidence intervals for the 2SIV estimates are very large, then we would not want to embrace this conclusion with much confidence. The robustness analyses suggest, however, that these estimates are quite robust to a variety of specification choices, especially for Hispanics, which ought to help mitigate concerns about potential imprecision of the IV estimates.

[49] The weaker identification for blacks implies that it is more difficult to sort out direct effects of incarceration on teenage dropout from indirect effects acting through never-married motherhood. However, the direction of the effect of incarceration is the same regardless, which is also borne out by reduced-form estimates indicating a negative relationship between incarceration rates and teen dropout for blacks and Hispanics, using OLS or probit estimation. (These estimated reduced-form coefficients are insignificant, but that is not informative about the statistical

be regarded as surprising. However, the conclusion that partial correlations between never-married motherhood and child outcomes overstate the adverse effects of never-married motherhood is not surprising. Existing research, as explained earlier, often shows that adverse effects of non-traditional family structures are greatly diminished or even disappear once account is taken of possibly unobserved differences between families with different structures. One explanation for our particular results is that men likely to be incarcerated are from the left tail of the distribution of quality of potential spouses. When mothers who would have married these men had the men not been incarcerated decide not to marry, their children may grow up in better home environments on average. This is consistent with evidence from Ehrle et al. (2003) suggesting that, for long-term welfare recipients, never-married mothers offered low-risk family environments for their children.

Indeed other evidence on low-income women backs up the notion that they often face poor options regarding the pool of marriageable males. Waller and Swisher (2006) note that low-income women are more likely than other women to experience physical abuse within their relationships with men—abuse that is likely to extend to children as well (see also Edin 2000). Edin and Reed (2005) discuss other evidence pointing to a poor pool of potential spouses in low-income communities. Aside from physical and substance abuse, they note that many potential fathers have other children, and therefore that the benefits of marriage may be less likely to accrue to the woman's children. Edin (2000) summarizes findings from her research as indicating that "though most low-income single mothers aspire to marriage, they believe that, in the short term, marriage usually entails more risks than potential rewards" (p. 113). She also documents low-income womens' concerns over the ability of men in their communities to bring in a regular paycheck and avoid becoming a financial drain on the household, as well as concerns regarding men relying on criminal activity for their income. Moreover, findings in the studies just cited suggest a fair amount of overlap between women who are long-term welfare recipients and women whose potential marriage partners are relatively likely to come from the population of criminal offenders and ex-offenders. For example, Waller and Swisher's analysis of data from the Fragile

significance of the effect of never-married motherhood.)

Families and Child Wellbeing Study points to an 11.7% rate of incarceration of fathers within 18 months of a child's birth, and a 30.2% rate of incarceration prior to the birth.

There is also evidence to suggest that incarcerated fathers have characteristics that may make them low-quality fathers. More than half of prisoners in the United States have children under age 18, and almost 1.4 million children under age 18 had a father in state or federal prison at the end of 1999.[50] Of fathers in prison, 45% lived with their children at the time of their admission to prison. But traditional family structure was rare; almost half of the parents incarcerated in 1999 had never been married, and only 21% of incarcerated fathers lived in a two-parent household before their prison admission. Many incarcerated fathers were admitted because of violent offenses (42%) or drug trafficking offenses (16%), and nearly half the fathers in prison had a violent offense before their current admission, indicating a history of such offenses. Incarcerated fathers also report high levels of drug use prior to admission to prison; more than half (57%) reported illicit drug use in the month prior to their admission to prison, and 85% reported ever using illicit drugs (52% for cocaine or crack).[51] Incarcerated fathers reported relatively good employment levels before incarceration, but this statistic disguises a dependence on illegal activity for some of their income. Of fathers in prison, 73% report being employed in the month before their admission, but 27% relied upon illegal sources for at least part of their income. These statistics support the hypothesis that higher incarceration removes from the marriage market men who are less than ideal candidates for marriage or childrearing.

It is also possible that the OLS results indicating adverse effects of never-married motherhood are driven by environmental factors, with women who forego marriage, on average, living in environments where children do worse. This could explain IV estimates that indicate no effect of never-married motherhood (i.e., estimates that are diminished relative to OLS). But it is less plausible as an explanation of positive effects of never-married motherhood from the IV estimation. Since many of our estimates indicate such positive effects, we are more inclined to the interpretation based on selection on spousal

[50] All of the statistics in this paragraph come from Mumola's (2000) report for the Bureau of Justice Statistics on incarcerated parents.
[51] Waller and Swisher (2006) discuss research linking substance abuse by parents to poor parenting and worse

quality.

If we have identified the causal effect of never-married motherhood for the children of women whose decisions are affected by variation in incarceration rates, then one conclusion might be that never-married motherhood is not irrational for these women from the perspective of achieving positive outcomes for children. This is consistent with evidence that women with nonmarital births have worse marriage partners if they do get married. Qian et al. (2005) find that women with nonmarital births are more likely to have less-educated and older spouses than women without nonmarital births. On the other hand, this interpretation of our findings does raise the question of why these women marry when incarceration rates are *not* high, leading to worse outcomes for children. One answer, of course, is that marriage may bring other benefits that also enter into their decision making.

Our evidence is also consistent with other findings that the effects of out-of-wedlock childbearing on the outcomes of both children and mothers exhibit some heterogeneity as a function of the relative disadvantage of the mother. For example, Levine and Painter (2003) study the effect of teenage out-of-wedlock childbearing on the educational completion of young mothers. They find that teenage childbearing has less deleterious effects for the least disadvantaged girls (as measured by the estimated probability of becoming a teenage mother). This research and ours indicates that women of different socioeconomic status might respond differently to policies aimed at promoting marriage.

7. Discussion and Conclusions

A rapid increase in the proportion of children living with never-married mothers and the negative child outcomes associated with living with a never-married mother have led to public policies that provide incentives or support for traditional, two-parent marriages. These policies rest upon the conjecture that the relationship between family structure and child outcomes is causal. In this paper, we identify the causal effect of family structure by instrumenting for never-married motherhood with the incarceration rate specific to the mother's marriage market. For the sample of women for which this is a salient instrument, we find no evidence that never-married motherhood has a negative, causal effect on

---

outcomes for children.

whether children drop out of high school. This result implies that, for some children, unobservable factors drive the relationship between family structure and educational outcomes. Indeed the evidence is more consistent with the conclusion that some children may be better off living with a never-married mother, and for Hispanic women and children this latter conclusion is rather strongly supported by the data and empirical analysis.

Our instrumental variables approach has a policy-relevant interpretation. Changes in incarceration rates for men are most likely to affect the marriage market decisions of women of lower socioeconomic status. Therefore, our estimates reflect the outcomes of the children of these women, and likely the heterogeneity in the relationship between family structure and child outcomes. These children are particularly vulnerable to a host of negative outcomes in their education, labor market experiences, criminal behavior, and family lives. Proponents of marriage-promotion policies view marriage as a crucial step in reducing these negative outcomes. But our results demonstrate that marriage, in itself, does not necessarily make children better off, and suggests that efforts focused on the broader set of environmental factors that influence both child outcomes and family structure among those of low socioeconomic status may prove more productive, and conversely that marriage-promotion policies that ignore the background of potential husbands and wives could have adverse effects. This result is not completely contrary to the existing literature, which typically finds that cross-sectional associations overstate the strength of the relationship between family structure and child outcomes, but still often find beneficial effects of two-parent families, disadvantages of divorce, etc. As our review of the literature explained, however, for very low socioeconomic status populations, such as long-term welfare recipients, there is some evidence that the findings are reversed.

It is also important to delineate the limitations of this evidence. First, none of our evidence addresses efforts to increase the quality of existing marriages or new marriages, which is also emphasized with respect to the Healthy Marriage Initiative.[52] If marriage-promotion policies create a set of marriages that on average are like those whose effects we identify, then our estimates provide valid information

[52] See, e.g., http://www.acf.hhs.gov/healthymarriage, accessed on September 28, 2007.

about the effects of marriage-promotion policy on children. But if marriage-promotion policies lead to higher-quality, longer-lasting marriages, then the effects on children could be different. A second limitation of our evidence is that it has no implications for the effects of marriage on children in households that are not affected by variation in incarceration rates, since our results identify the effects of marriage for those women (and their children) whose behavior is affected by variation in incarceration rates.

References

Acemoglu, Daron and Joshua Angrist. 2000. How Large are Human-Capital Externalities? Evidence from Compulsory-Schooling Laws. *NBER Macroeconomics Annual* 15: 9-59.

Akerlof, George A., Janet L. Yellen, and Michael L. Katz. 1996. An Analysis of Out-of-Wedlock Childbearing in the United States. *Quarterly Journal of Economics* 111(2): 277-317.

Amato, Paul R., Laura Spencer Loomis, and Alan Booth. 1995. Parental Divorce, Marital Conflict, and Offspring Well-Being during Early Adulthood. *Social Forces* 73(3): 895-915.

Angrist, Joshua D. 1991. Instrumental Variables Estimation of Average Treatment Effects in Econometrics and Epidemiology. National Bureau of Economic Research Technical Working Paper 115.

———. 2002. How Do Sex Ratios Affect Marriage and Labor Markets? Evidence from America's Second Generation. *Quarterly Journal of Economics* 117(3): 997-1038.

Bhattacharya, Jay, Dana Goldman, and Daniel McCaffrey. 2006. Estimating Probit Models with Self-Selected Treatments. *Statistics in Medicine* 25(3): 389-413.

Bitler, Marianne P., Jonah B. Gelbach, and Hilary W. Hoynes. 2006. Welfare Reform and Children's Living Arrangements. *Journal of Human Resources* 41(1): 1-27.

Blumstein, Alfred and Allen J. Beck. 1999. Population Growth in U.S. Prisons, 1980-1996. In Michael Tonry and Joan Petersilia, editors, *Crime and Justice: Prisons*, Volume 26. Chicago: University of Chicago Press.

Bumpass, Larry and Hsien-Hen Lu. 2000. Trends in Cohabitation and Implications for Children's Family Contexts in the United States. *Population Studies* 54(1): 29-41.

Bumpass, Larry L. and James A. Sweet. 1989. Children's Experience in Single-Parent Families: Implications of Cohabitation and Marital Transitions. *Family Planning Perspectives* 21(6): 256-60.

Butcher, Kristin F. and Anne Morrison Piehl. 2007. Why Are Immigrants' Incarceration Rates So Low? Evidence on Selective Immigration, Deterrence, and Deportation. National Bureau of Economic Research Working Paper 13229.

Cameron, Stephen V. and James J. Heckman. 1993. The Nonequivalence of High School Equivalents. *Journal of Labor Economics* 11(1): 1-47.

Charles, Kerwin Kofi and Ming Ching Luoh. 2005. Male Incarceration, the Marriage Market, and Female Outcomes. Unpublished manuscript. University of Michigan.

Chiappori, Pierre-Andre, Bernard Fortin, and Guy Lacroix. 2002. Marriage Market, Divorce Legislation, and Household Labor Supply. *Journal of Political Economy* 110(1): 37-72.

Cherlin, Andrew J., Jr. Frank F. Furstenberg, P. Lindsay Chase-Lansdale, Kathleen E. Kiernan, Philip K. Robins, Donna Ruane Morrison, and Julien O. Teitler. 1991. Longitudinal Studies of Effects of Divorce on Children in Great Britain and the United States. *Science* 252(5011): 1386-89.

Dawson, Deborah A. 1991. Family Structure and Children's Health and Well-Being: Data from the 1988 National Health Interview Survey on Child Health. *Journal of Marriage and the Family* 53(3): 573-84.

DeLeire, Thomas and Ariel Kalil. 2002. Good Things Come in Threes: Single-Parent Multigenerational Family Structure and Adolescent Adjustment. *Demography* 39(2): 393-413.

DeVanzo, Julie and M. Omar Rahman. 1993. American Families: Trends and Correlates. *Population Index* 59(3): 350-86.

Edin, Kathryn. 2000. What Do Low-Income Single Mothers Say about Marriage? *Social Problems* 47(1): 112-33.

Edin, Kathryn and Joanna M. Reed. 2005. Why Don't They Just Get Married? Barriers to Marriage among the Disadvantaged. *The Future of Children* 15(2): 118-37.

Ehrle, Jennifer, Katherine Kortenkamp, and Matthew Stagner. 2003. Family Structure and Outcomes for Children of Long-Term Welfare Recipients. Unpublished manuscript. Washington, DC: Urban Institute.

Ermisch, John F. and Marco Francesconi. 2001. Family Structure and Children's Achievements. *Journal of Population Economics* 14(2): 249-70.

Ginther, Donna K. and Robert A. Pollak. 2004. Family Structure and Children's Educational Outcomes: Blended Families, Stylized Facts, and Descriptive Regressions. *Demography* 41(4): 671-96.

Grogger, Jeff and Nick Ronan. 1995. The Intergenerational Effects of Fatherlessness on Educational Attainment and Entry-Level Wages. National Longitudinal Surveys Discussion Paper 96-30. U.S. Bureau of Labor Statistics.

Gruber, Jonathan. 2004. Is Making Divorce Easier Bad for Children? The Long Run Implications of Unilateral Divorce. *Journal of Labor Economics* 22(4): 799-833.

Hamilton, Brady E., Joyce A. Martin, and Stephanie J. Ventura. 2006. Births: Preliminary Data for 2005. *National Vital Statistics Reports* 55(11): 1-20.

Harknett, Kristen and Sara S. McLanahan. 2004. Racial and Ethnic Differences in Marriage after the Birth of a Child. *American Sociological Review* 69(6): 790-811.

Imbens, Guido W. and Joshua D. Angrist. 1994. Identification and Estimation of Local Average Treatment Effects. *Econometrica* 62(2): 467-75.

Jekielek, Susan M. 1998. Parental Conflict, Marital Disruption and Children's Emotional Well-Being. *Social Forces* 76(3): 905-36.

Kamdar, Amee. 2007. Male Incarceration and Teen Fertility. Unpublished manuscript, University of Chicago.

Kenkel, Donald, Dean Lillard, and Alan Mathios. 2006. The Roles of High School Completion and GED Receipt in Smoking and Obesity. *Journal of Labor Economics* 24(3): 635-60.

King, Miriam, Steven Ruggles, and Matthew Sobek. 2003. Integrated Public Use Microdata Series, Current Population Survey: Preliminary Version 0.1. Minneapolis: Minnesota Population Center, University of Minnesota.

Lang, Kevin and Jay L. Zagorsky. 2001. Does Growing Up with a Parent Absent Really Hurt? *Journal of Human Resources* 36(2): 253-73.

Levine, David I. and Gary Painter. 2003. The Schooling Costs of Teenage Out-of-Wedlock Childbearing: Analysis with a Within-School Propensity-Score-Matching Estimator. *Review of Economics and Statistics* 85(4): 884-900.

Levitt, Steven D. 1996. The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding. *Quarterly Journal of Economics* 111(2): 319-51.

Lichter, Daniel T. 2001. Marriage as Public Policy. Progressive Policy Institute, Washington, DC.

Lichter, Daniel T. and Deborah Roempke Graefe. 2007. Men and Marriage Promotion: Who Marries Unwed Mothers? *Social Service Review* 81(3): 397-421.

Liu, Shirley H. and Frank Heiland. 2007. Should We Get Married? The Effect of Parents' Marriage on Out-of-Wedlock Children. Unpublished manuscript. University of Miami.

Lochner, Lance and Enrico Moretti. 2004. The Effect of Education on Crime: Evidence from Prison Inmates, Arrests, and Self-Reports. *American Economic Review* 94(1): 155-89.

Macomber, Jennifer Ehrle, Julie Murray, and Matthew Stagner. 2005. Service Delivery and Evaluation Design Options for Strengthening and Promoting Healthy Marriages. Washington, DC: Urban Institute.

Manning, Wendy D. and Daniel T. Lichter. 1996. Parental Cohabitation and Children's Economic Well-Being. *Journal of Marriage and the Family* 58(4): 998-1010.

Manski, Charles F., Gary D. Sandefur, Sara McLanahan, and Daniel Powers. 1992. Alternative Estimates of the Effect of Family Structure during Adolescence on High School Graduation. *Journal of the American Statistical Association* 87(417): 25-37.

Mare. Robert D. 1991. Five Decades of Assortative Mating. *American Sociological Review* 56(1): 15-32.

Mauer, Marc. 1999. *Race to Incarcerate*. New York: New Press.

McLanahan, Sara. 1985. Family Structure and the Reproduction of Poverty. *American Journal of Sociology* 90(4): 873-901.

———. 2004. Diverging Destinies: How Children Are Faring under the Second Demographic Transition. *Demography* 41(4): 607-27.

McLanahan, Sara and Gary D. Sandefur. 1994. *Growing Up with a Single Parent: What Hurts, What Helps*. Cambridge, MA: Harvard University Press.

Mechoulan, Stéphane. 2007. The External Effects of Black-Male Incarceration on Black Females. Unpublished manuscript. University of Toronto.

Morrison, Donna Ruane and Andrew J. Cherlin. 1995. The Divorce Process and Young Children's Well-Being: A Prospective Analysis. *Journal of Marriage and the Family* 57(3): 800-12.

Morrison, Donna Ruane and Mary Jo Coiro. 1999. Parental Conflict and Marital Disruption: Do Children Benefit When High-Conflict Marriages Are Dissolved? *Journal of Marriage and the Family* 61(3): 626-37.

Moynihan, Daniel P. 1965. *The Negro Family: The Case for National Action*. U.S. Department of Labor.

Mumola, Christopher J. 2000. Incarcerated Parents and Their Children. Bureau of Justice Statistics, NCJ 182335.

Neal, Derek. 2004. The Relationship between Marriage Market Prospects and Never-Married Motherhood. *Journal of Human Resources* 39(4): 938-57.

Neumark, David and William L. Wascher. 2003. Minimum Wages and Skill Acquisition: Another Look at Schooling Effects. *Economics of Education Review* 22(1): 1-10.

Painter, Gary and David I. Levine. 2000. Family Structure and Youths' Outcomes: Which Correlations are Causal? *Journal of Human Resources* 35(3): 524-49.

Pastore, Ann L. and Kathleen Maguire, editors. 2006. *Sourcebook of Criminal Justice Statistics* [Online]. http://www.albany.edu/sourcebook (accessed on September 14, 2006).

Pencavel, John. 1998. Assortative Mating by Schooling and the Work Behavior of Wives and Husbands. *American Economic Review* 88(2): 326-29.

Qian, Zhenchao, Daniel T. Lichter, and Leanna M. Mellott. 2005. Out-of-Wedlock Childbearing, Marital Prospects, and Mate Selection. *Social Forces* 84(1): 473-91.

Raphael, Steven. 2006. The Socioeconomic Status of Black Males: The Increasing Importance of Incarceration. In Alan Auerbach, David Card, and John Quigley, editors, *Poverty, the Distribution of Income, and Public Policy*. New York: Russell Sage Foundation.

Raphael, Steven and Michael A. Stoll. 2007. Why Are So Many Americans in Prison? Unpublished manuscript. University of California, Berkeley.

Rector, Robert E. and Melissa G. Pardue. 2004. Understanding the President's Healthy Marriage Initiative. Backgrounder 1741, The Heritage Foundation [Online]. http://www.heritage.org/Research/Family/bg1741.cfm (accessed on September 28, 2007).

Ribar, David C. 2004. What Do Social Scientists Know about the Benefits of Marriage? A Review of Quantitative Methodologies. Institute for the Study of Labor (IZA) Discussion Paper 998.

Rosenzweig, Mark R. 1999. Welfare, Marital Prospects, and Nonmarital Childbearing. *Journal of Political Economy* 107(6, Part 2): S3-S32.

Sandefur, Gary D. and Thomas Wells. 1999. Does Family Structure Really Influence Educational Attainment? *Social Science Research* 28(4): 331-57.

Staiger, Douglas and James H. Stock. 1997. Instrumental Variables Regression with Weak Instruments. *Econometrica* 65(3): 557-86.

Stock, James H. and Motohiro Yogo. 2005. Testing for Weak Instruments in Linear IV Regression. In Donald W.K. Andrews and James H. Stock, editors, *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg*. Cambridge: Cambridge University Press.

Waller, Maureen R. and Raymond Swisher. 2006. Fathers' Risk Factors in Fragile Families: Implications for "Healthy" Relationships and Father Involvement. *Social Problems* 53(3): 392-420.

Western, Bruce and Sara McLanahan. 2000. Fathers Behind Bars: The Impact of Incarceration on Family Formation. Center for Research on Child Wellbeing Working Paper No. 00-08. *Families, Crime and*

Criminal Justice 2: 309-24.

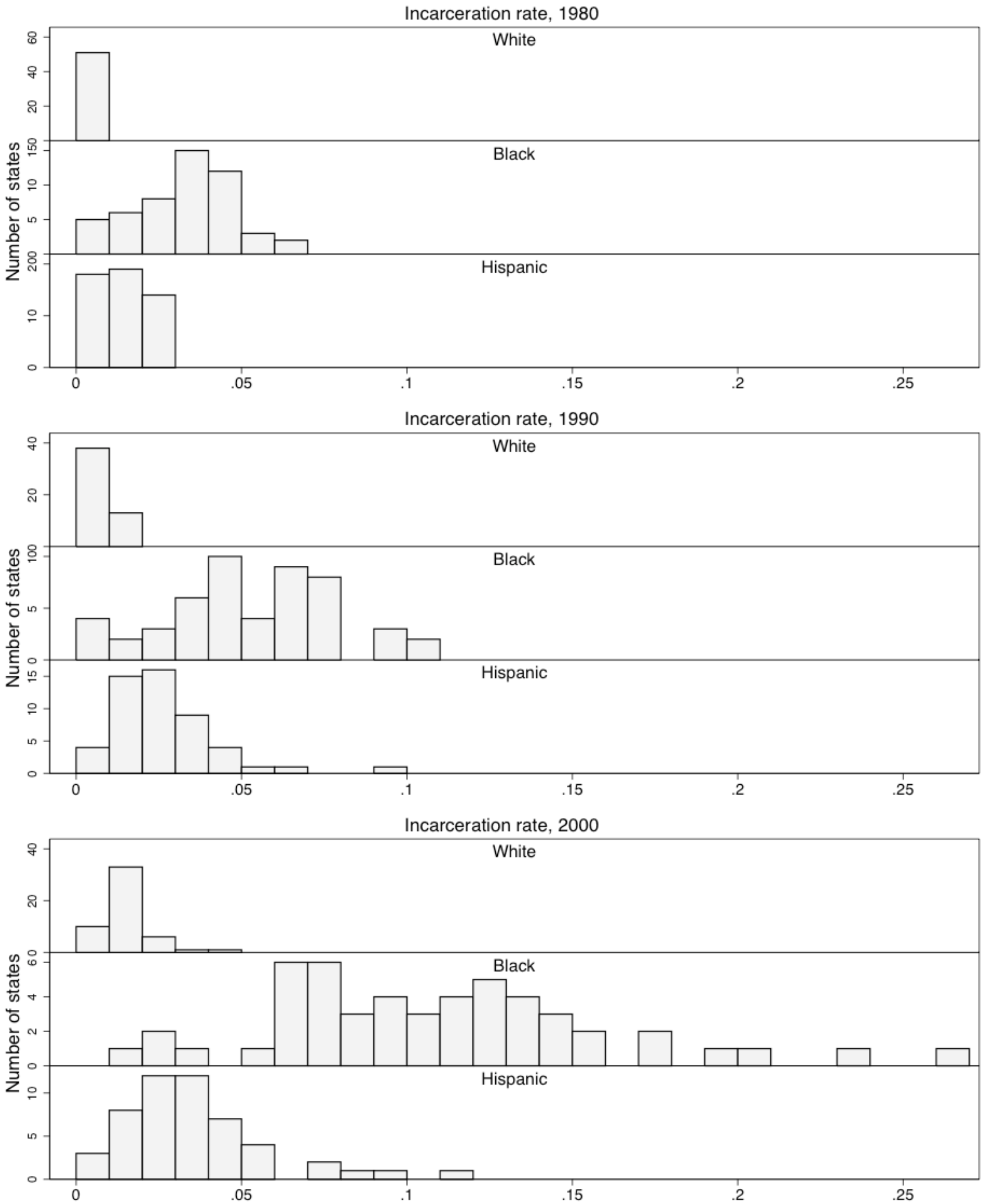
Willis, Robert J. 1999. A Theory of Out-of-Wedlock Childbearing. *Journal of Political Economy* 107(6, Part 2): S33-S64.

Wilson, William Julius. 1987. *The Truly Disadvantaged: The Inner City, the Underclass, and Public Policy*. Chicago: University of Chicago Press.

Wood, Robert G. 1995. Marriage Rates and Marriageable Men: A Test of the Wilson Hypothesis. *Journal of Human Resources* 30(1): 163-93.

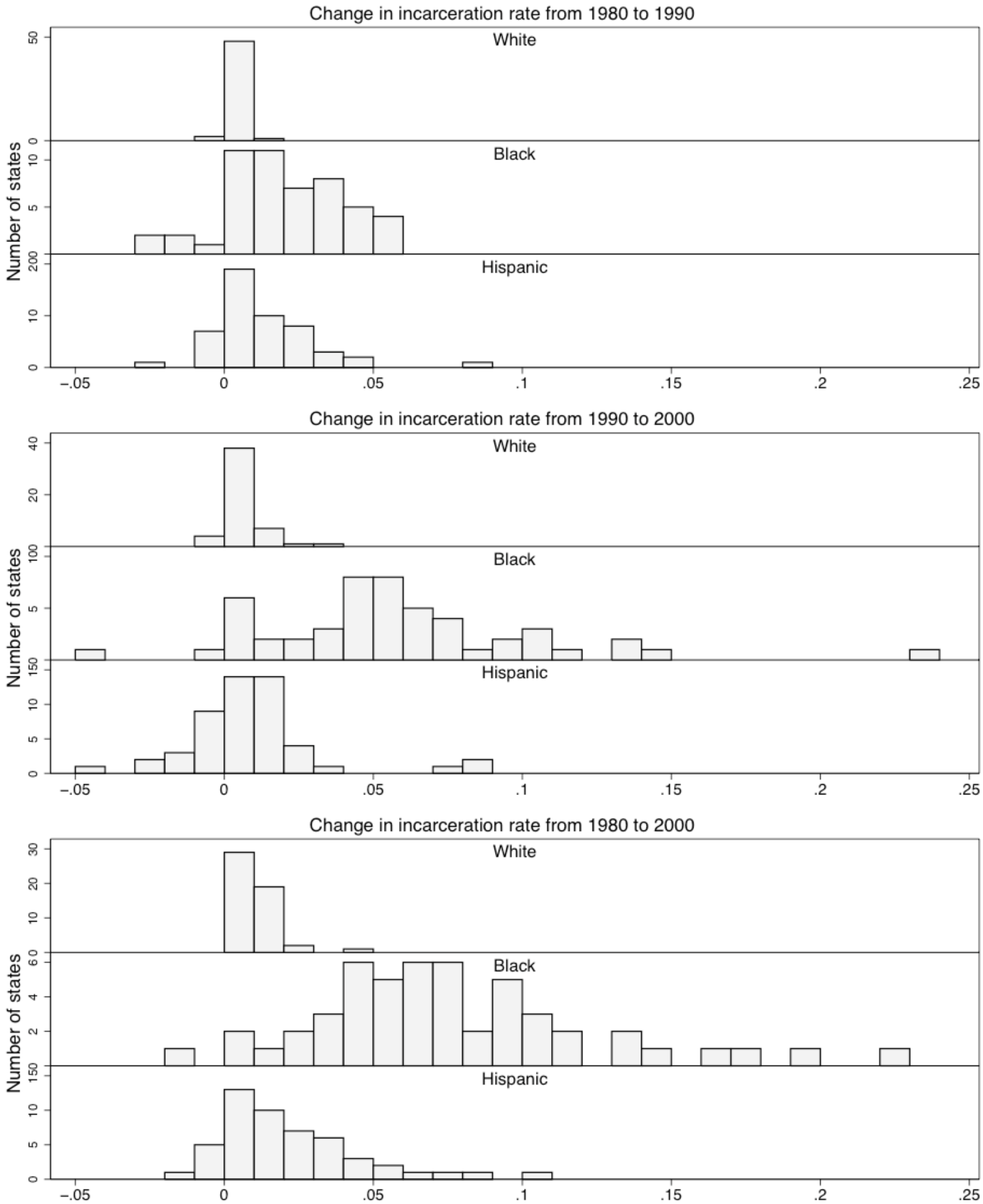
Wooldridge, Jeffrey M. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.

Figure 1: Histogram of incarceration rates for men aged 18-40 years, across states, by race and ethnicity, 1980, 1990, 2000



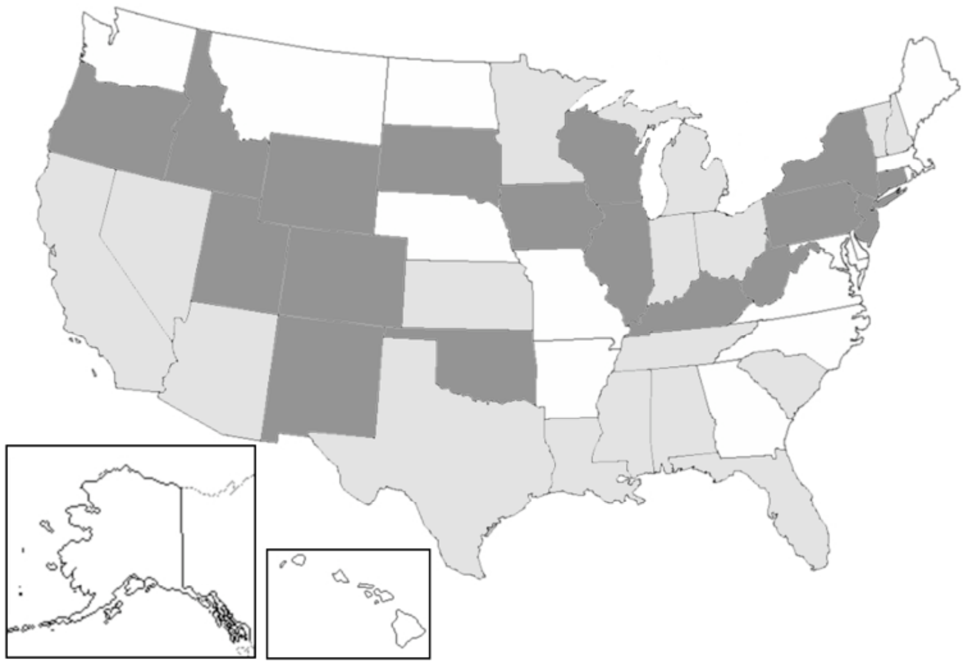
Note: The unit of observation for each histogram is the state.

Figure 2: Histogram of changes in incarceration rates for men aged 18-40 years, across states, by race and ethnicity, from 1980 to 1990, 1990 to 2000, and 1980 to 2000



Note: The unit of observation for each histogram is the state.

Figure 3: Changes in incarceration rates for black men aged 18-40 years from 1980 to 2000, by state



Notes:
- No shading indicates a small change in the incarceration rate for black men (between -1 and +5.4 percentage points).
- Light shading indicates a medium change (between +5.4 and +9 percentage points).
- Dark shading indicates a large change (between +9 and +22 percentage points).
- Rates from 1980 are used as a baseline for the differences because the 1970 data are relatively noisy.

Table 1: Percentage of wives marrying husbands of particular races/ethnicities, all wives and high school dropouts, 1970-2000

		Race/ethnicity of husband		
Year	Race/ethnicity of wife	White	Black	Hispanic
A: All wives				
All years	White	97.81	0.46	1.73
	Black	1.75	97.41	0.84
	Hispanic	15.88	1.26	82.86
1970	White	99.85	0.15	0
	Black	0.46	99.54	0
	Hispanic	0	0	100
1980	White	98.15	0.34	1.51
	Black	0.91	98.5	0.59
	Hispanic	18.52	1.26	80.22
1990	White	97.64	0.46	1.9
	Black	2.04	96.95	1.01
	Hispanic	18.86	1.23	79.91
2000	White	96.66	0.77	2.56
	Black	3.11	95.49	1.4
	Hispanic	14.28	1.43	84.29
B: Wives with fewer than 12 years of schooling				
All years	White	97.83	0.43	1.74
	Black	0.83	98.66	0.51
	Hispanic	4.26	0.46	95.28
1970	White	99.8	0.2	0
	Black	0.24	99.76	0
	Hispanic	0	0	100
1980	White	97.62	0.38	2
	Black	0.64	98.76	0.6
	Hispanic	7.34	0.72	91.95
1990	White	96.95	0.58	2.47
	Black	1.3	97.88	0.82
	Hispanic	4.72	0.4	94.88
2000	White	94.96	1.07	3.97
	Black	2.21	96.13	1.66
	Hispanic	2.75	0.41	96.84

Notes:
- In 1970, Hispanic ethnicity was determined by the surname of the head of household, so there is no intermarriage by construction.

Table 2: Percentage of children aged 15-17 years living with a never-married mother, by race/ethnicity, 1970-2000

	White	Black	Hispanic
1970	0.1	2.7	0.4
1980	0.1	7.1	2.0
1990	0.5	15.8	3.7
2000	1.1	21.2	5.4

Table 3: Percentage of children aged 15-17 years who have dropped out of high school, by race/ethnicity and family structure, 1970-2000

		Ever-married mother	Never-married mother
A: White children			
	1970	4.9	11.1
	1980	5.0	15.9
	1990	4.9	8.5
	2000	2.4	5.3
B: Black children			
	1970	8.4	7.5
	1980	6.4	9.2
	1990	6.1	8.9
	2000	2.7	4.5
C: Hispanic children			
	1970	9.2	10.0
	1980	9.7	14.9
	1990	7.1	11.9
	2000	4.4	7.2

Table 4: Selected descriptive statistics, children aged 15-17 years living with their mothers by race, ethnicity, and family structure, 1970-2000

Variable	All	NM	EM	Black All	Black NM	Black EM	Hispanic All	Hispanic NM	Hispanic EM	White All	White NM	White EM
Never-married mother	0.03			0.13			0.04			0.01		
Black	0.13	0.68	0.12									
Hispanic	0.10	0.16	0.10									
Child has dropped out of high school	0.05	0.07	0.05	0.06	0.07	0.05	0.07	0.09	0.07	0.04	0.07	0.04
Mother did not finish high school	0.23	0.37	0.22	0.34	0.35	0.34	0.53	0.58	0.53	0.17	0.22	0.17
Mother finished just high school	0.40	0.38	0.40	0.37	0.40	0.36	0.26	0.26	0.27	0.43	0.41	0.43
Mother finished some college	0.24	0.21	0.24	0.21	0.21	0.22	0.15	0.14	0.15	0.25	0.28	0.25
Mother finished college	0.14	0.04	0.14	0.08	0.04	0.08	0.06	0.03	0.06	0.16	0.09	0.16
Mother's age at birth of child	26.21	22.83	26.30	25.20	22.44	25.63	25.75	23.90	25.83	26.44	23.43	26.46
	(5.83)	(5.63)	(5.80)	(6.50)	(5.54)	(6.53)	(6.17)	(5.98)	(6.16)	(5.63)	(5.43)	(5.63)
Incarceration rate for same-race/ethnicity men 18-40 (st. of residence × race/ethnicity × year)	0.017	0.059	0.016	0.063	0.077	0.060	0.023	0.029	0.022	0.009	0.012	0.009
	(0.023)	(0.043)	(0.021)	(0.036)	(0.041)	(0.035)	(0.012)	(0.016)	(0.012)	(0.005)	(0.006)	(0.005)
Per-pupil school expenditures ($10,000)	0.70	0.78	0.70	0.68	0.76	0.66	0.72	0.84	0.71	0.70	0.83	0.70
(state × year)	(0.21)	(0.22)	(0.21)	(0.22)	(0.22)	(0.22)	(0.19)	(0.23)	(0.19)	(0.22)	(0.20)	(0.22)
Violent crime rate (1k per 100k population)	0.57	0.64	0.57	0.64	0.65	0.63	0.69	0.69	0.69	0.54	0.53	0.54
(state × year)	(0.26)	(0.27)	(0.26)	(0.28)	(0.28)	(0.28)	(0.22)	(0.24)	(0.22)	(0.25)	(0.23)	(0.25)
Property crime rate (1k per 100k	4.55	4.38	4.56	4.67	4.49	4.70	4.98	4.36	5.00	4.48	3.93	4.48
population) (state × year)	(1.27)	(1.22)	(1.27)	(1.24)	(1.18)	(1.24)	(1.40)	(1.36)	(1.40)	(1.25)	(1.11)	(1.25)
Employment rate for white men 18-40	0.84	0.83	0.84	0.84	0.84	0.84	0.84	0.83	0.84	0.84	0.83	0.84
(state × year)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Mean annual earnings for white men	19.66	24.35	19.54	19.36	23.24	18.76	23.80	27.78	23.64	19.16	25.70	19.12
18-40 (state × year)	(8.62)	(8.16)	(8.59)	(8.51)	(8.06)	(8.42)	(9.04)	(8.23)	(9.04)	(8.42)	(7.30)	(8.41)
Observations	1,513,288	38,674	1,474,614	197,166	26,409	170,757	156,935	6,188	150,747	1,159,187	6,077	1,153,110

Notes:
- Means shown with standard deviations in parentheses.
- Family structure is broken down by whether mothers have never married (NM) or ever married (EM).

Table 5: Percentage of children aged 15-17 years living with a never-married mother, by race/ethnicity, and by percentile of incarceration rate for men, 1970-2000

	Percentile of state-year-race/ethnicity incarceration rate		
	≤25th	25th–75th	≥75th
A: White children			
1970	0.08	0.08	0.07
1980	0.19	0.12	0.11
1990	0.56	0.51	0.33
2000	1.30	1.19	0.88
Pooled years	0.17	0.46	1.02
B: Black children			
1970	2.74	2.67	2.60
1980	7.64	7.06	6.55
1990	16.09	15.47	16.50
2000	19.69	20.60	24.33
Pooled years	7.81	12.47	21.12
C: Hispanic children			
1970	0.34	0.29	0.94
1980	1.05	1.45	4.69
1990	3.70	2.02	6.53
2000	4.97	4.02	9.10
Pooled years	2.07	4.01	6.34

Notes:
- In the first four rows of each panel, percentiles are calculated separately for each race/ethnicity and year.
- In the last row of each panel, percentiles are calculated for each race/ethnicity across all years.

Table 6: Regressions of never-married motherhood on incarceration rates, children aged 15-17 years living with their mothers, by race and ethnicity, 1970-2000

Independent variables	Black OLS (1)	Black OLS (2)	Black Prob (3)	Black Prob (4)	Hispanics OLS (5)	Hispanics OLS (6)	Hispanics Prob (7)	Hispanics Prob (8)
Incarceration rate		0.261		0.114		0.671		0.228
(st. of residence, year t)		(0.138)		(0.071)		(0.180)		(0.108)
Female (child)	0.0014	0.0014	0.0012	0.0012	0.0025	0.0024	0.0022	0.0022
	(0.0014)	(0.0014)	(0.0012)	(0.0012)	(0.0011)	(0.0011)	(0.0008)	(0.0008)
Per-pupil educ. exp. ($1,000s)	0.097	0.077	0.029	0.020	0.062	0.029	0.006	-0.003
	(0.036)	(0.041)	(0.031)	(0.031)	(0.022)	(0.022)	(0.012)	(0.013)
Violent crime rate (1,000 crimes	-0.006	-0.011	0.013	0.011	-0.007	-0.006	0.019	0.019
per 100,000 pop.)	(0.031)	(0.032)	(0.026)	(0.027)	(0.014)	(0.015)	(0.005)	(0.005)
Property crime rate (1,000 crimes	0.016	0.022	0.018	0.021	0.010	0.012	0.011	0.013
per 100,000 pop.)	(0.015)	(0.014)	(0.013)	(0.013)	(0.009)	(0.009)	(0.005)	(0.005)
Larceny rate (1,000 crimes per	-0.037	-0.044	-0.038	-0.041	-0.018	-0.023	-0.024	-0.026
100,000 pop.)	(0.022)	(0.021)	(0.020)	(0.020)	(0.014)	(0.014)	(0.007)	(0.007)
Employment rate for white men	0.397	0.382	0.255	0.239	0.079	0.033	-0.092	-0.124
aged 18-40 years	(0.175)	(0.161)	(0.132)	(0.125)	(0.111)	(0.102)	(0.048)	(0.050)
Mean earnings for white men aged	-0.0012	-0.0006	0.0000	0.0003	0.0019	0.0026	0.0001	0.0002
18-40 years ($1,000)	(0.0014)	(0.0015)	(0.0011)	(0.0012)	(0.0011)	(0.0010)	(0.0007)	(0.0007)
Mother is HS graduate	-0.055	-0.055	-0.047	-0.047	-0.012	-0.012	-0.009	-0.009
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
Mother has some college	-0.103	-0.103	-0.074	-0.074	-0.019	-0.019	-0.013	-0.013
	(0.004)	(0.004)	(0.002)	(0.002)	(0.005)	(0.005)	(0.002)	(0.002)
Mother has 4 years of college	-0.144	-0.143	-0.092	-0.091	-0.032	-0.033	-0.021	-0.021
	(0.005)	(0.005)	(0.002)	(0.002)	(0.007)	(0.007)	(0.001)	(0.001)
Age of mother at birth of child	-0.0084	-0.0084	-0.0086	-0.0086	-0.0017	-0.0017	-0.0015	-0.0015
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0001)	(0.0001)
Observations	197,166	197,166	197,122	197,122	156,935	156,935	156,715	156,715
R^2	0.08	0.08	0.00	0.00	0.03	0.03	0.00	0.00
Mean of dependent variable	0.13	0.13	0.13	0.13	0.04	0.04	0.04	0.04

Notes:

- Each specification includes child age effects, state effects, year effects, and child age-year effects.
- For probits, estimates are marginal effects that are evaluated at the means of each regression's respective sample.
- Heteroscedasticity-robust standard errors, clustered at the state-level, are in parentheses.

Table 7: Regressions of whether child has dropped out of high school on mother's marital status, children aged 15-17 years living with their mothers, by race and ethnicity, 1970-2000

Independent variables	Black OLS (1)	Black 2SLS (2)	Black 2SIV (3)	Hispanics OLS (4)	Hispanics 2SLS (5)	Hispanics 2SIV (6)
Endogenous covariates						
Mother never married	0.017	-0.145	-0.031	0.032	-0.459	-0.190
	(0.002)	(0.148)	(0.013)	(0.003)	(0.411)	(0.067)
Other controls						
Female (child)	-0.006	-0.006	-0.006	-0.007	-0.006	-0.007
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Per-pupil educ. exp. ($1000s)	-0.003	0.012	0.001	0.058	0.089	0.070
	(0.019)	(0.025)	(0.019)	(0.019)	(0.027)	(0.017)
Violent crime rate	0.033	0.032	0.033	0.024	0.021	0.022
(1,000 crimes per 100,000 pop.)	(0.014)	(0.013)	(0.014)	(0.015)	(0.016)	(0.015)
Property crime rate	-0.006	-0.004	-0.006	0.003	0.007	0.005
(1,000 crimes per 100,000 pop.)	(0.006)	(0.006)	(0.006)	(0.014)	(0.012)	(0.013)
Larceny rate	0.004	-0.002	0.002	-0.006	-0.015	-0.010
(1,000 crimes per 100,000 pop.)	(0.010)	(0.011)	(0.010)	(0.023)	(0.020)	(0.022)
Employment rate for white men aged	0.191	0.255	0.210	0.426	0.464	0.448
18-40 years	(0.080)	(0.109)	(0.077)	(0.085)	(0.100)	(0.083)
Mean earnings for white men aged 18-40	-0.0002	-0.0004	-0.0002	-0.0009	0.0000	-0.0004
years ($1,000)	(0.0007)	(0.0008)	(0.0007)	(0.0009)	(0.0014)	(0.0009)
Mother is HS graduate	-0.040	-0.049	-0.042	-0.046	-0.052	-0.049
	(0.002)	(0.009)	(0.003)	(0.003)	(0.007)	(0.003)
Mother has some college	-0.052	-0.069	-0.057	-0.054	-0.063	-0.058
	(0.002)	(0.016)	(0.003)	(0.003)	(0.009)	(0.004)
Mother has 4 years of college	-0.062	-0.085	-0.069	-0.064	-0.080	-0.071
	(0.003)	(0.023)	(0.003)	(0.005)	(0.015)	(0.005)
Age of mother at birth of child	0.0002	-0.0012	-0.0002	-0.0002	-0.0011	-0.0006
	(0.0001)	(0.0013)	(0.0001)	(0.0002)	(0.0007)	(0.0002)
First stage						
Incarceration rate		0.261			0.671	
(st. of residence, year t)		(0.138)			(0.180)	
Predicted NM			1.268			1.234
			(0.038)			(0.102)
F-statistic for IV with state clustering		3.57	1136.94		13.96	147.19
Observations	197,166	197,166	197,122	156,935	156,935	156,715
R^2	0.03			0.04		
Mean of dependent variable	0.06	0.06	0.06	0.07	0.07	0.07

Notes:

- Each specification includes child age effects, state effects, year effects, and child age-year effects.
- Heteroscedasticity-robust standard errors, clustered at the state-level, are in parentheses.
- 2SIV is the two-step IV procedure described in Wooldridge (2002, p.623).

Table 8: Regressions of whether child has dropped out of high school on mother's marital status with incarceration-rate polynomials in the first stage, children aged 15-17 years living with their mothers, by race and ethnicity, 1970-2000

Independent variables	Black 2SLS (1)	Black 2SLS (2)	Black 2SIV (3)	Black 2SIV (4)	Hispanics 2SLS (5)	Hispanics 2SLS (6)	Hispanics 2SIV (7)	Hispanics 2SIV (8)
Endogenous covariates								
Mother never married	-0.239	-0.052	-0.032	-0.029	-0.197	-0.302	-0.191	-0.185
	(0.120)	(0.124)	(0.012)	(0.012)	(0.329)	(0.331)	(0.067)	(0.068)
Other controls								
Female (child)	-0.005	-0.006	-0.006	-0.006	-0.007	-0.007	-0.007	-0.007
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Per-pupil educ. exp. ($1000s)	0.022	0.003	0.001	0.001	0.072	0.079	0.070	0.070
	(0.024)	(0.026)	(0.019)	(0.019)	(0.025)	(0.024)	(0.017)	(0.017)
Violent crime rate	0.032	0.033	0.033	0.033	0.023	0.022	0.022	0.022
(1,000 crimes per 100,000 pop.)	(0.013)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)
Property crime rate	-0.002	-0.005	-0.006	-0.006	0.005	0.006	0.005	0.005
(1,000 crimes per 100,000 pop.)	(0.006)	(0.006)	(0.006)	(0.006)	(0.012)	(0.012)	(0.013)	(0.013)
Larceny rate	-0.006	0.001	0.002	0.002	-0.011	-0.012	-0.010	-0.010
(1,000 crimes per 100,000 pop.)	(0.012)	(0.012)	(0.010)	(0.010)	(0.020)	(0.019)	(0.022)	(0.022)
Employment rate for white men aged	0.293	0.218	0.210	0.209	0.444	0.452	0.448	0.447
18-40 years	(0.100)	(0.093)	(0.077)	(0.077)	(0.091)	(0.093)	(0.083)	(0.083)
Mean earnings for white men aged	-0.0005	-0.0003	-0.0002	-0.0002	-0.0005	-0.0003	-0.0004	-0.0005
18-40 years ($1,000)	(0.0008)	(0.0008)	(0.0007)	(0.0007)	(0.0011)	(0.0012)	(0.0009)	(0.0009)
Mother is HS graduate	-0.054	-0.044	-0.042	-0.042	-0.049	-0.050	-0.049	-0.049
	(0.007)	(0.008)	(0.003)	(0.003)	(0.006)	(0.006)	(0.003)	(0.003)
Mother has some college	-0.079	-0.060	-0.057	-0.057	-0.058	-0.060	-0.058	-0.058
	(0.013)	(0.013)	(0.003)	(0.003)	(0.008)	(0.008)	(0.004)	(0.004)
Mother has 4 years of college	-0.099	-0.072	-0.069	-0.069	-0.071	-0.075	-0.071	-0.071
	(0.018)	(0.018)	(0.003)	(0.003)	(0.013)	(0.013)	(0.005)	(0.005)
Age of mother at birth of child	-0.0020	-0.0004	-0.0002	-0.0002	-0.0006	-0.0008	-0.0006	-0.0006
	(0.0010)	(0.0010)	(0.0001)	(0.0001)	(0.0006)	(0.0006)	(0.0002)	(0.0002)
First stage								
Incarceration rate	-0.214	0.675			0.008	-0.588		
(st. of residence, year t)	(0.345)	(0.663)			(0.512)	(0.797)		
Incarceration rate squared	1.985	-7.094			7.546	23.442		
(st. of residence, year t)	(1.225)	(6.451)			(5.081)	(15.879)		
Incarceration rate cubed		26.686				-111.278		
(st. of residence, year t)		(19.089)				(106.902)		
Predicted NM			1.267	1.266			1.236	1.255
			(0.038)	(0.039)			(0.102)	(0.112)
F-statistic for IV with state clustering	5.27	3.99	1123.99	1063.67	10.50	12.84	145.51	125.56
χ^2-statistic for incarceration rate variables in probit			6.45	8.71			5.72	10.72
Observations	197,166	197,166	197,122	197,122	156,935	156,935	156,715	156,715
Mean of dependent variable	0.06	0.06	0.06	0.06	0.07	0.07	0.07	0.07

Notes:

- Each specification includes child age effects, state effects, year effects, and child age-year effects.
- Heteroscedasticity-robust standard errors, clustered at the state-level, in parentheses.
- 2SIV is the two-step IV procedure described in Wooldridge (2002, p.623).

Table 9: Regressions of whether child has dropped out of high school on mother's marital status (and with additional interactions of controls), children aged 15-17 years living with their mothers, by race and ethnicity, 1970-2000

Independent variables	Black OLS (1)	Black 2SIV (2)	Hispanics OLS (3)	Hispanics 2SIV (4)
Endogenous covariates				
Mother never married	0.018	-0.016	0.031	-0.086
	(0.002)	(0.027)	(0.003)	(0.044)
Other controls				
Female (child)	0.000	0.000	-0.002	-0.001
	(0.003)	(0.003)	(0.002)	(0.002)
Per-pupil educ. exp. ($1000s)	0.082	0.084	0.140	0.139
	(0.064)	(0.062)	(0.073)	(0.073)
Violent crime rate	0.058	0.061	-0.017	-0.011
(1,000 crimes per 100,000 pop.)	(0.027)	(0.026)	(0.067)	(0.069)
Property crime rate	-0.036	-0.036	0.030	0.023
(1,000 crimes per 100,000 pop.)	(0.017)	(0.016)	(0.029)	(0.029)
Larceny rate	0.029	0.028	-0.069	-0.064
(1,000 crimes per 100,000 pop.)	(0.026)	(0.026)	(0.051)	(0.052)
Employment rate for white men aged	-0.568	-0.795	7.608	8.078
18-40 years	(1.396)	(1.396)	(3.266)	(3.505)
Mean earnings for white men aged 18-40	0.003	0.002	0.001	0.001
years ($1,000)	(0.002)	(0.002)	(0.003)	(0.003)
Mother is HS graduate	0.111	0.108	0.162	0.165
	(0.060)	(0.061)	(0.071)	(0.074)
Mother has some college	0.194	0.188	0.193	0.227
	(0.071)	(0.074)	(0.089)	(0.095)
Mother has 4 years of college	0.159	0.147	0.175	0.185
	(0.075)	(0.076)	(0.121)	(0.126)
Age of mother at birth of child	-0.001	-0.001	0.005	0.004
	(0.004)	(0.004)	(0.007)	(0.007)
First stage				
Predicted NM		1.258		1.113
		(0.059)		(0.093)
F-statistic for IV with state clustering		454.42		142.40
Observations	197,166	197,122	156,935	156,715
R^2	0.03		0.04	
Mean of dependent variable	0.06	0.06	0.07	0.07

Notes:
- Estimates are only shown for main effects of control variables. Additional controls include quadratics of continuous variables and interactions between the gender of child and mother's education, between the gender and age of child, between the mother's age at birth of child and mother's education, and between mother's completed education and age of child.
- Each specification includes child age effects, state effects, year effects, and child age-year effects.
- Heteroscedasticity-robust standard errors, clustered at the state-level, are in parentheses.
- 2SIV is the two-step IV procedure described in Wooldridge (2002, p.623).

Table 10: Alternative estimates of regressions of whether child has dropped out of high school on mother's marital status, children aged 15-17 years living with their mothers, Hispanics, 1970-2000

Independent variables	Hispanics 2SLS (1)	Hispanics 2SLS (2)	Hispanics 2SLS (3)	Hispanics 2SIV (4)	Hispanics 2SIV (5)	Hispanics 2SIV (6)
A. Lagged incarceration for men 18-24, from birth state of child, as instrument with polynomials						
Mother never married	-0.118	-0.140	-0.016	-0.091	-0.092	-0.086
	(0.196)	(0.203)	(0.138)	(0.055)	(0.055)	(0.054)
First stage						
Incarceration rate	0.733	0.448	-0.949			
(state of birth, year t-10, men 18-24)	(0.283)	(0.310)	(0.543)			
Incarceration rate squared		5.022	62.114			
(state of birth, year t-10, men 18-24)		(6.550)	(21.075)			
Incarceration rate cubed			-525.655			
(state of birth, year t-10, men 18-24)			(158.375)			
Predicted NM				1.390	1.392	1.366
				(0.123)	(0.124)	(0.119)
F-statistic for IV	6.71	3.80	4.47	127.22	126.76	132.75
B. Incarceration for men 25-40 as instrument with polynomials						
Mother never married	-0.333	-0.086	-0.240	-0.188	-0.187	-0.184
	(0.446)	(0.404)	(0.311)	(0.067)	(0.067)	(0.067)
First stage						
Incarceration rate	0.504	0.130	-0.746			
(state of birth, year t, men 25-40)	(0.147)	(0.427)	(0.616)			
Incarceration rate squared		3.808	23.771			
(state of birth, year t, men 25-40)		(4.305)	(9.184)			
Incarceration rate cubed			-112.254			
(state of birth, year t, men 25-40)			(40.465)			
Predicted NM				1.241	1.239	1.253
				(0.106)	(0.105)	(0.110)
F-statistic for IV	11.75	5.47	13.73	137.39	140.44	130.98
C. Incarceration rate for older men as an instrument and incarceration rate for younger men as a control						
Mother never married	0.057			-0.168		
	(0.612)			(0.063)		
Incarceration rate	-0.488			-0.338		
(state of residence, year t, men 18-24)	(0.353)			(0.235)		
First stage						
Incarceration rate	0.376					
(state of birth, year t, men 25-40)	(0.155)					
Predicted NM				1.250		
				(0.102)		
F-statistic for IV	5.90			149.58		

Notes:
- Each specification includes child age effects, state effects, year effects, and child age-year effects.
- Heteroscedasticity-robust standard errors, clustered at the state-level, in parentheses.
- 2SIV is the two-step IV procedure described in Wooldridge (2002, p.623).
- In all specifications, the other control variables included are the same as in Table 7.

Table 11: Robustness checks from regressions of whether child has dropped out of high school on mother's marital status, children aged 15-17 years living with at least their mothers, by race/ethnicity, 1970-2000

Independent variables	Black OLS (1)	Black 2SLS (2)	Black 2SIV (3)	Hispanics OLS (4)	Hispanics 2SLS (5)	Hispanics 2SIV (6)
A. Baseline specification						
Mother never married	0.017	-0.145	-0.031	0.032	-0.459	-0.190
	(0.002)	(0.148)	(0.013)	(0.003)	(0.411)	(0.067)
First-stage F-statistic		3.57	1136.94		13.96	147.19
B. Sample consists of boys only						
Mother never married	0.018	-0.200	-0.035	0.029	-0.384	-0.094
	(0.002)	(0.295)	(0.018)	(0.004)	(0.492)	(0.071)
First-stage F-statistic		2.29	702.75		10.73	136.15
C. Sample consists of girls only						
Mother never married	0.017	-0.109	-0.033	0.035	-0.502	-0.263
	(0.003)	(0.110)	(0.014)	(0.005)	(0.476)	(0.099)
First-stage F-statistic		2.77	1367.70		12.33	99.21
D. Incarceration rate instrument calculated for men who live in the same state they did 5 years earlier						
Mother never married	0.017	-0.007	-0.025	0.032	0.011	-0.196
	(0.002)	(0.150)	(0.013)	(0.003)	(0.378)	(0.068)
First-stage F-statistic		9.19	916.87		7.14	113.89
E. Baseline sample plus institutionalized children, classified as having never-married mother						
Mother never married	0.040	0.010	-0.003	0.060	-0.298	-0.151
	(0.002)	(0.128)	(0.012)	(0.007)	(0.281)	(0.061)
First-stage F-statistic		7.49	1088.94		18.54	140.55
F. Specification includes compulsory schooling and minimum wage controls						
Mother never married	0.017	-0.145	-0.031	0.032	-0.441	-0.193
	(0.002)	(0.149)	(0.012)	(0.003)	(0.382)	(0.068)
First-stage F-statistic		3.65	1102.36		16.95	147.55

Notes:
- Each specification includes child age effects, state effects, year effects, and child age-year effects, except Panel G, which includes child age effects, state-year effects, and child age-year effects.
- Heteroscedasticity-robust standard errors, clustered at the state-level, in parentheses.
- 2SIV is the two-step IV procedure described in Wooldridge (2002, p.623).
- In all specifications, the covariates included are the same as in Table 7, except the female indicator is excluded from Panels B and C.
- In Panel E, mother-specific covariates are imputed for children not living with a mother, from a sample of children living with a never-married mother.