# Is Marriage Always Good for Children?

## Evidence from Families Affected by Incarceration

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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 instrumental variables in estimating the effect of never-married motherhood on the high school dropout rate of black and Hispanic children. We find that unobserved factors drive the negative relationship between never-married motherhood and child education, at least for children of women whose marriage decisions are affected by incarceration of men. For Hispanics we find evidence that these children actually may be better off living with a never-married mother.*

## I. 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 receive treatment for an emotional problem than are children living with both biological parents (Dawson 1991). In light of such findings, marriage promotion policies are touted as a strategy for

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improving outcomes for children of poor, single mothers (Rector and Pardue 2004). The most significant recent federal marriage-promotion policies were the 1996 welfare reforms and the Healthy Marriage Initiative included in the 2006 TANF reauthorization, which target low-income, unmarried mothers; two of the stated goals of welfare reform were to prevent out-of-wedlock childbearing and to encourage the formation of two-parent families.[1] 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 poor urban women (Lichter 2001).

Marriage promotion policies presume that marriage itself will directly improve outcomes. But the causal effects of marriage may differ substantially from what is revealed by simple 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 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, resources devoted to encouraging marriage might be better directed toward increasing the human capital of parents or improving the environments faced by poor families.

This paper provides causal evidence on the effects of never-married motherhood on whether children drop out of high school, using changes in male incarceration rates as a source of exogenous variation in marriage market conditions. The analysis relies on data from the U.S. Censuses of Population for 1970–2000. The Census data are central to our analysis because they cover a period in which there were massive increases in incarceration rates in many states, especially for minorities, and it is these changes that provide our identifying information.

At the same time, the Census data dictate the outcomes we can study and how we can characterize family structure. The Census does not contain information on other child outcomes that might be of interest. Nonetheless, high school dropout is a very important outcome, as it is associated with lower wages (Cameron and Heckman 1993), higher probabilities of criminal activity, arrest, and incarceration (Lochner and Moretti 2004), and worse health behaviors and outcomes (Kenkel, Lillard, and Mathios 2006). Similarly, although the Census does not have longitudinal or more-detailed cross-sectional information with which to distinguish among different types of family structures or changes in family structure over time, a focus on never-married motherhood is informative for two reasons. First, never-married motherhood is prevalent and becoming increasingly so, especially for minority women. And second, the prevalence of never-married motherhood can potentially be influenced by marriage-promotion policies.

We account for the endogeneity of family structure by instrumenting 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, specific to the state in which the mother lives

1. See Joyce, Kaestner, and Korenman (2003) for evidence on the effects of the 1996 welfare reform on out-of-wedlock childbearing.

and the ages of men she is likely to marry. 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-age variation by race and ethnicity in incarceration rates has a direct effect on the "supply" of potential husbands in the marriage market. The instrumental variables estimator has a local average treatment effect interpretation, estimating the effect of never-married motherhood on children of mothers whose marital behavior is affected by variation in race-specific or ethnicity-specific incarceration rates. 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.

The evidence suggests that unobservable factors drive the observed adverse relationship between never-married motherhood and educational outcomes, for children whose mothers are most affected by changes in incarceration rates. Moreover, for Hispanics we find evidence that these children may be better off living with a never-married mother. Our findings are robust to a number of approaches that assess or account for threats to the validity of our identification strategy. Overall, the results suggest that simply encouraging marriage for poor, unmarried mothers may not improve outcomes for their children, and could even worsen them depending on which marriages form as a result of such policies.[2]

## II. Related literature on family structure and child outcomes

Our study contributes to a large literature on family structure and child outcomes, which focuses more generally on differences between children raised in single-parent and two-parent households. Because these differences may overstate the causal impact of family structure on child outcomes, existing studies take a number of approaches to account for unobservable factors correlated with family structure. These include the use of longitudinal research designs exploiting changes over time in family structure (for example, Cherlin et al. 1991; Painter and Levine 2000), the use of sibling data to identify the effects of family structure from within-family differences in exposure of children to particular family structures (for example, Ermisch and Francesconi 2001; Ginther and Pollak 2004), "natural experiments" such as a parent's death (Lang and Zagorsky 2001), and the joint modeling of the family structure decisions of parents (Manski et al. 1992; McLanahan and Sandefur 1994). In general, this research finds that children who grow up outside of married two-parent households have worse outcomes than children who grow up in them, but the cross-sectional associations overstate the direct effect of family structure on child outcomes, and these effects sometimes fall to zero. However,

2. Marriage promotion policies focus on marriage per se, without reference to whether the marriages are among biological parents. Nonetheless, at least some of the underlying motivation for these policies was to promote children living with married *and* biological parents (for example, Ooms, Bouchet, and Parke 2004). Thus, our results should be viewed as more informative about the policies actually in place than hypothetical policies that would promote marriage among biological parents.

identification strategies that attempt to account for 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.[3]

The existing research also provides evidence that may help in interpreting our instrumental variables estimates. We argue that our identification strategy can succeed because incarceration rates have a direct effect on marriage markets, but affect children's outcomes only through their effects on marriage markets. At the same time, existing research points to heterogeneity in the effects of family structure across the socioeconomic spectrum, and we identify these effects for families of low socioeconomic status whose decisions are affected by variation in incarceration rates.

Using a small sample of long-term welfare recipients in California, Ehrle, Kortenkamp, and Stagner (2003) find that children living in nonintact families (including single-parent homes) had outcomes no worse than children living with two biological parents. Although the authors caution against generalizing from their small sample, they find evidence that family environment can help to account for their results. In particular, 60 percent of never-married mothers had family environments that they classified as "low-risk," about the same as for children living with two biological parents, and much higher than for other family structures, such as single, ever-married (39 percent), and married, living with stepfather (35 percent). 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. Not all studies of at-risk populations find negligible or negative effects of marriage. For example, Liu and Heiland (2010) find positive effects in an urban sample that oversamples individuals of low socioeconomic status.

This evidence emphasizes that we may be less likely to find positive effects of marriage on children in families of lower socioeconomic status. We should not extrapolate our estimates of the effects of never-married motherhood—identified from variation in incarceration—across the socioeconomic spectrum. Nonetheless, 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.

## III. Never-married motherhood

Never-married motherhood is an increasingly important family structure category in the United States. It is the fastest-growing category among children living in female-headed households (DeVanzo and Rahman 1993). For blacks, the cumulative five- and ten-year marriage rates following nonmarital first births declined steadily from the 1960s through the late 1980s (Bumpass and Lu 2000). And nonmarital births contribute to never-married motherhood, as a nonmarital childbirth

3. For more discussion of the limitations of the existing research, see Ribar (2004).

substantially lowers women's likelihood of subsequent first marriage or first union, whether formal or informal (Bennett, Bloom, and Miller 1995).

There are strong race differences in the rates of never-married motherhood, and 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 (about 26 percent versus 9.5 percent) to be married 30 months later (Harknett and McLanahan 2004). Moreover, while 80 percent 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 that show rising rates of never-married motherhood through 2000, especially for minorities.

One criticism of focusing on never-married motherhood is that some nonmarital births are to cohabiting parents whose family lives resemble those of married parents. However, the vast majority of nonmarital births are to noncohabiting mothers, and children born out of wedlock spend much of their childhood in noncohabiting households, especially children born to never-married mothers. For black nonmarital births between 1970 and 1984, only 18 percent were to cohabiting parents (Bumpass and Sweet 1989), compared to 40 percent and 29 percent, respectively, for Mexican Americans and whites. 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 percent of black children, 15.4 percent of white children, and 17.6 percent of Mexican-American children living with unmarried parents lived with cohabiting parents (Manning and Lichter 1996). Based on data from the 1980s and 1990s, children born to never-married, noncohabiting mothers spent only about 15 percent of their years from ages 0–16 in cohabiting households, versus about 36 percent 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 percent) with cohabiting and noncohabiting or nonmarried mothers (Bumpass and Lu 2000). Together, these figures suggest that never-married motherhood most commonly reflects living in a single-parent household for a good part of one's childhood, especially for black and Hispanic children.

While our emphasis on never-married motherhood ignores some dimensions of family structure, it does present some potential advantages. First, never-married motherhood is easy to measure in cross-sectional data such as the Census data we use, because longitudinal information is not required. Second, using never-married motherhood bypasses complexities in family structure such as those studied by Ginther and Pollak (2004), and the more general "window problem" highlighted by Wolfe et al. (1996), which refers to the problem of characterizing the variables that determine a child's attainments based on data covering only a limited period (that is, a window) of their life (such as at age 14). Wolfe et al. argue that estimates of the effects of a child's environment "are likely to be less biased for variables measuring continuous or persistent variables than for those measuring rare or unique events" (p. 977). Thus, at least relative to "window variables" that capture family structure at a point in time, estimates of the effects of never-married motherhood are less likely to be problematic because never-married motherhood *is* a persistent state.

## IV. Empirical framework, estimation, and identification

### *A. Empirical framework and estimation*

For children aged 15–17, we estimate models relating high school dropout ($Y$) to never-married status of the child's mother ($NM$), and other observable and unobservable factors:

$$(1)\quad Y_{iast} = \beta_0 + \beta_1 NM_{iast} + X_{iast}\beta_2 + S_{st}\beta_3 + D_s\delta_1 + D_t\delta_2 + D_a\delta_3 + D_{at}\delta_4 + \varepsilon_{iast},$$

for child $i$, aged $a$ years, living in state $s$ in year $t$. $X$ is a vector of individual controls and $S$ a vector of state controls.[4] The model includes state ($D_s$), year ($D_t$), and single-year age dummy variables ($D_a$), plus interactions between the year and age dummy variables ($D_{at}$) to allow for different aggregate changes by age. Because childbearing and marriage decisions vary substantially by race and ethnicity, we estimate separate models for each group.

The estimated effect of never-married motherhood ($\hat{\beta}_1$) is biased (inconsistent) if there is a correlation between never-married motherhood, $NM$, and the unobservable determinants of child outcomes in $\varepsilon$, which could arise from endogenous selection into never-married motherhood. The likely direction of bias is to overstate the negative effects of never-married motherhood on children. Our strategy for identifying the causal effect of never-married motherhood is to instrument for it with the male incarceration rate specific to each mother's age ($a'$), each child $i$'s race or ethnicity, state of residence ($s$), and year ($t$), $IR_{ia'st}$.

Although the dependent variable is binary, we estimate the effects of never-married motherhood on high school dropout using a linear probability formulation, because this enables a local average treatment effect interpretation of our instrumental variables estimator, 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 and use them as the instrumental variable for $NM$ in the equation for $Y$. We refer to this as two-stage instrumental variables. This estimator is robust to misspecification of the equation for never-married motherhood as a probit.

Specifically, we first estimate a probit model for never-married motherhood that includes the incarceration rate instrument and the exogenous controls in Equation 1:

4. The control variables are discussed below, and also listed in Table 7.

$$(2)\quad P[NM_{iaa'st}=1]=\Phi[\alpha_0+\alpha_1 IR_{ia'st}+X_{iast}\alpha_2 + S_{st}\alpha_3+D_s\varphi_1+D_t\varphi_2+D_a\varphi_3+D_{at}\varphi_4],$$

where $\Phi$ is the cumulative normal distribution.[5] After estimating Equation 2, predicted values of never-married motherhood are generated as

$$(3)\quad \hat{\Phi}_{iaa'st}=\Phi[\hat{\alpha}_0+\hat{\alpha}_1 IR_{ia'st}+X_{iast}\hat{\alpha}_2+S_{st}\hat{\alpha}_3+D_s\hat{\varphi}_1+D_t\hat{\varphi}_2+D_a\hat{\varphi}_3+D_{at}\hat{\varphi}_4].$$

The $\hat{\Phi}$'s then serve as an instrument for never-married motherhood in a two-stage least squares model consisting of Equation 1 and

$$(4)\quad NM_{iaa'st}=\theta_0+\theta_1\hat{\Phi}_{iaa'st}+X_{iast}\theta_2+S_{st}\theta_3 + D_s\lambda_1+D_t\lambda_2+D_a\lambda_3+D_{at}\lambda_4+\eta_{iaa'st}.$$

If we begin with the potential outcomes framework where the effect of never-married motherhood can vary over the support of the instrumental variable, then under assumptions specified in Imbens and Angrist (1994), the standard instrumental variables estimator is a weighted average of local average treatment effects with the weight concentrated on parts of the support of the instrumental variable for which variation in the instrumental variable 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 women whose marriage behavior is affected by variation in the incarceration rate of men in their marriage market.[6] These are likely to be families with women who have low skills and poor labor market prospects, and who face a less desirable pool of potential marriage partners.[7]

### *B. Identification*

The causal connection of our incarceration rate instrumental variable to women's marital behavior is clear. When more men are in jail or prison, there likely will be fewer marriages, both because fewer men are available for marriage, and because fewer men are good marriage partners.[8] Our strategy of exploiting how incarceration rates affect marriage decisions is related to other work examining how sex ratios affect marriage decisions. 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. And closer

5. The results are robust to other specifications for this "zeroth" stage, such as a logit regression.
6. Other responses of marital and fertility behavior to variation in the marriage market could differ by socioeconomic status. For example, higher-income women might be more willing (and able) to raise children on their own, or less willing to choose a less suitable partner.
7. 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).
8. 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 (and the possibility that prior incarceration makes them more prone to future criminal activity).

to our approach, Charles and Luoh (2010) find that higher male incarceration rates (and lower male-to-female ratios) were associated with fewer married women.[9]

In the empirical analysis, we use two different incarceration rates: the current rate in the woman's state of residence, based on the woman's current age; and the rate from ten years earlier based on the state in which the child was born. The current rate is more relevant if the never-married decision is importantly influenced by current decisions to remain unmarried, or if incarceration is relatively long-term or occurs repeatedly for the same men (so that contemporaneous incarceration is associated with past incarceration). The lagged rate is more relevant if the incarceration rate in a period closer to when the child was born (or at least was very young) is a more important determinant of the never-married decision. As it turns out, the results are generally similar for the two incarceration rates. The current rate has the advantage of using the last decade's variation in incarceration rates for identification. The lagged rate, however, offers some other advantages with regard to validity of the identification strategy. We therefore prefer the lagged incarceration rate as an instrument, but report both sets of estimates.

With any instrumental variables design there is a concern about weak instruments, which can lead to large confidence intervals and poor asymptotic approximations for them. In linear models with iid errors, Staiger and Stock (1997) and Stock and Yogo (2005) propose rule-of-thumb thresholds for $F$-statistics for the first stage. However, in the non-iid case (in our context, there may be intrastate dependence) less is known about the relationship between the $F$-statistic and the properties of instrumental variables estimates. We nonetheless report this $F$-statistic for each specification. We are also unaware of any such rules of thumb for the case of a generated instrument like in the two-stage instrumental variables estimator we use, although as reported below there is no question that the generated instrument is a very strong predictor of never-married status. Most usefully, perhaps, we report Anderson-Rubin test statistics for the null hypothesis that the coefficient on the endogenous variable (never-married motherhood) is equal to zero in the child outcome equation. We use a cluster-robust version of the Anderson-Rubin test that has the correct size even under weak identification (Chernozhukov and Hansen 2008; Finlay and Magnusson 2009). In all specifications, inference based on the Anderson-Rubin test is consistent with inference based on the Wald test of the same null hypothesis. This suggests that we are not drawing spurious conclusions based on weak instruments.

In addition to predicting variation in the probability that women are never-married, the instrumental variable must also be uncorrelated with the child-outcome error term ($\varepsilon$), so same-race or same-ethnicity and state- and age-specific incarceration rates must not be correlated with child outcomes other than through their effect on

9. They also find that higher incarceration rates increase the proportion of marriages in which the wife's education was greater than the husband's. Charles and Luoh (2010) argue that this indicates that women find lower quality marriage partners when more men are incarcerated. However, spousal quality may be characterized not only by education, but also by criminal records of men in marriage markets, which will vary with incarceration.

Mechoulan (2010) reports OLS estimates pointing to a negative effect of incarceration of black males on marriage probabilities for black females; but in instrumental variables estimates instrumenting for incarceration with changes in sentencing and prison capacity, the evidence is ambiguous.

family structure. This restriction may be plausible because recent increases in incarceration rates have not been caused primarily by rising criminal behavior, but rather by states adopting harsher punishments for drug and repeat offenses (Blumstein and Beck 1999; Mauer 1999; Raphael and Stoll 2007).

There are, of course, other reasons this restriction could be violated. 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 have led 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 also can have a direct relationship with child outcomes, can pose a similar problem, as can rising crime from deinstitutionalization. To address these issues, we include measures of crime rates among the control variables in $S$. We also include indicators of labor market conditions, which can influence crime. 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 outcome equation may be negatively correlated with incarceration rates. We therefore control for state-level educational expenditures.[10]

The instrument also can be invalid if incarceration itself affects child well-being. If incarceration has negative effects on the home communities of prisoners and therefore on child outcomes, this would lead to bias in the instrumental variables estimate in the direction of stronger negative effects of never-married motherhood. However, our instrumental variables estimates indicate *less* adverse effects of never-married motherhood than do 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 case, we might find that the instrumental variables estimates point to weaker adverse effects of never-married motherhood, or even positive effects, compared to the OLS estimates (and compared to the true effect). Given that we find such evidence, 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, although we present some results that attempt to do so by including contemporaneous incarceration rates as controls and instrumenting with the ten-year lagged incarceration rates that may better measure marriage market conditions.

The same arguments apply if incarceration directly affects children, rather than communities. If the presence of adult males would have been "good" for children, this generates a bias toward adverse effects of never-married motherhood in the instrumental variables estimates, which is not what we find. Alternatively, if the presence of the adult males who are removed from households because of incarceration is "bad" for children, then the bias is in the opposite direction, which, as for

10. To the extent that we do not fully capture tradeoffs between expenditures on incarceration and expenditures that might increase the likelihood teenagers stay in high school, the bias in the instrumental variables estimate is toward finding that never-married motherhood increases the likelihood of dropping out. Given that our estimates go in the other direction, this cannot explain our findings.

the effect on communities, could explain our instrumental variables estimates. We again cannot rule out this source of bias, but the evidence would still support the conclusion that children benefit (or at least are not hurt) as a result of this group of at-risk men being incarcerated.

Finally, incarceration may affect child outcomes through the fertility and labor market decisions of mothers. There is evidence that incarceration has reduced teen childbearing (Kamdar 2007). And if incarceration of men improves labor market prospects for women, women may seek more education (Mechoulan 2010). These are likely to be more problematic when we instrument with the lagged incarceration rate from a period closer to the child's birth. To avoid correlation between the instrument and the error term via this channel, we also include controls for mother's education and her age at the child's birth. In addition, if women with worse marriage markets (as reflected in the incarceration rate) have fewer children,[11] and the number of siblings affects child outcomes, then failure to control for number of siblings could lead to correlation between the error term and the instrument. Thus, we also control for the number of siblings in the household.

Finally, past incarceration rates could have affected women's decisions about where to live. For example, employment opportunities for women may be better when incarceration rates of men are high, so the most resourceful never-married mothers—whose children also do better—may have moved to high incarceration states, generating a bias against finding an adverse effect of incarceration on youths. To account for this possibility, when we use the lagged incarceration rate instrumental variable, we base it on the child's state of birth, rather than the state in which the mother resides when we observe the teenager.[12]

## V. Data and descriptive statistics

Our primary data come from the Integrated Public-Use Microdata Series (IPUMS) of the 1970–2000 Censuses (King, Ruggles, and Sobek 2003). The IPUMS data have limited information on child outcomes, but they are suitable for this study because of large samples and consistent variable definitions over a long period. The IPUMS also allows us to estimate incarceration rates by age, race, ethnicity, state, and year. 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[13] (a 1 percent sample

11. This is suggested by the reduction in teen childbearing associated with incarceration (Kamdar 2007), along with earlier demography literature (for example, Morgan and Rindfuss 1999) showing that teen childbearing is associated with higher subsequent fertility (although this latter result may not follow as a consequence of exogenous sources of variation in teen childbearing).

12. Descriptive statistics for mobility do not point to an obvious concern, even looking back further to compare current states of residence to birth states of *mothers*. For blacks, 13.4 percent of never-married mothers live in states different from those in which they were born, versus 20.4 percent for ever-married mothers. For Hispanics, the numbers are 36.7 percent for never-married mothers and 38.5 percent for ever-married mothers. These descriptive statistics could mask selection on unobservables, which our empirical analysis should address.

13. The 1970 Form 1 sample does not have information about school attendance.

of the population) and the 5 percent state samples from the 1980, 1990, and 2000 Censuses. We use IPUMS person weights to account for the smaller sample in 1970. We restrict these samples to children whose race and ethnicity is identified as either Hispanic, non-Hispanic white, or non-Hispanic black.[14] Before 1980, Hispanic ethnicity was imputed from country of birth or whether a respondent's last name was Spanish (for example, Petersen 2001). To have a consistent definition of Hispanic ethnicity, we do not use data on Hispanics from 1970.[15]

We restrict the sample to children living with their mothers.[16] Among these, we drop children who are identified in the IPUMS as probably living with a nonbiological mother (usually a stepmother), because such children may have previously spent time in a household with a biological mother whose marital history is unknown. We exclude children residing in group quarters (institutional or otherwise) because it is impossible to determine their family structure. We categorize children by whether their mothers report having ever married. There are some children in the sample identified as living with married 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. 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 account for the finding that the instrumental variables estimate of the effect of never-married motherhood is typically the opposite sign of the OLS estimate.

Table 1 reports information on intermarriage. Panel A indicates that, for blacks and whites, about 98 percent of married women aged 18–40 are married to men of the same race. Intermarriage has become only slightly more common during the sample period; within-race marriage rates in 2000 were about 96 percent for whites and blacks. On the other hand, Hispanic-white intermarriage is more common, with about 17 percent of Hispanic married women married to white men. This difference between black and Hispanic marriage patterns might suggest that our instrumental variables procedure would be most powerful for black women, as for them variation in incarceration 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, Hispanic-white intermarriage is much less common among the least-educated Hispanics who are most likely to be affected by variation in incarceration rates; Hispanic-white intermarriage rate is only about 4 percent for women with fewer than 12 years of completed education.

Table 2 shows the percentage of children aged 15–17 living with never-married mothers. There has been a secular increase in never-married motherhood for all groups. The relative increases are similar for whites, blacks, and Hispanics, but the

14. We refer to non-Hispanic whites as whites and non-Hispanic blacks as blacks.

15. But we verified that our results for Hispanics are robust to the inclusion of the 1970 data.

16. This definition excludes children living with neither biological parent. Bitler, Gelbach, and Hoynes (2006) show that this is a nontrivial proportion of black children, especially for households with less-educated heads. In 1989, 9 percent of black children living with a household head with at most a high school education lived with neither biological parent; the corresponding number was 15 percent for those living with a household head with fewer than 12 years of education.

**Table 1**
*Percentage of Wives Marrying Husbands of Particular Races/Ethnicities, All Wives and High School Dropouts, Aged 18–40 Years*

| | | Race/ethnicity of husband | | |
|---|---|---|---|---|
| Year | Race/ethnicity of wife | White | Black | Hispanic |
| A: All wives | | | | |
| All years | White | 98.38 | 0.38 | 1.24 |
| | Black | 1.41 | 97.99 | 0.60 |
| | Hispanic | 15.21 | 1.30 | 83.49 |
| 1970 | White | 99.85 | 0.15 | |
| | Black | 0.46 | 99.54 | |
| 1980 | White | 98.15 | 0.34 | 1.51 |
| | Black | 0.91 | 98.50 | 0.59 |
| | Hispanic | 18.52 | 1.26 | 80.22 |
| 1990 | White | 97.46 | 0.51 | 2.03 |
| | Black | 2.06 | 96.88 | 1.06 |
| | Hispanic | 19.01 | 1.34 | 79.64 |
| 2000 | White | 96.36 | 0.88 | 2.76 |
| | Black | 3.06 | 95.52 | 1.42 |
| | Hispanic | 14.38 | 1.56 | 84.05 |
| B: Wives with fewer than 12 years of schooling | | | | |
| All years | White | 98.90 | 0.29 | 0.82 |
| | Black | 0.63 | 99.16 | 0.22 |
| | Hispanic | 3.85 | 0.43 | 95.72 |
| 1970 | White | 99.80 | 0.20 | |
| | Black | 0.24 | 99.76 | |
| 1980 | White | 97.62 | 0.38 | 2.00 |
| | Black | 0.64 | 98.76 | 0.60 |
| | Hispanic | 7.34 | 0.72 | 91.95 |
| 1990 | White | 96.70 | 0.66 | 2.65 |
| | Black | 1.35 | 97.85 | 0.80 |
| | Hispanic | 4.74 | 0.42 | 94.84 |
| 2000 | White | 94.38 | 1.23 | 4.38 |
| | Black | 2.08 | 96.23 | 1.69 |
| | Hispanic | 2.69 | 0.44 | 96.87 |

**Table 2**
*Percentage of Children Aged 15–17 Years Living with a Never-Married Mother, by Race/Ethnicity*

| | White | Black | Hispanic |
|---|---|---|---|
| 1970 | 0.1 | 2.7 | |
| 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*

| | 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 | | |
| 1980 | 9.7 | 14.9 |
| 1990 | 7.1 | 11.9 |
| 2000 | 4.4 | 7.2 |

absolute increase is by far the largest for black children. Fewer than 3 percent of black children aged 15–17 years lived with never-married mothers in 1970, while more than 21 percent 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. 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, which is captured in the year effects in our models.

We create a number of control variables from the IPUMS data. Using information from the mother's record, we construct dummy variables for: mother 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 nine percentage points less likely to have completed four years of college, about as likely to have some college education, three percentage points less likely to have completed only high school, and 12 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 the child's birth. Never-married mothers have their children at an average age of 22.8, while other mothers have their children at an average age of 26.5. Never-married mothers have on average fewer children than those who have married—2.1 versus 2.8.

State-varying controls are in some cases taken from other sources. First, we include per-pupil elementary- and secondary-school expenditures by state for the fiscal years 1969–70, 1979–80, 1989–90, and 1999–2000.[17] Second, we use three-year moving averages of the crime rates from the Federal Bureau of Investigation's Uniform Crime Reports, for 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.[18] Third, we estimate 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 state institutionalization rates as a proxy for state incarceration rates, following other work in this and related areas (for example, Butcher and Piehl 2007; Charles and Luoh 2010). 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 the 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 noninstitutionalized populations. In addition, 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.

The institutionalization rate is defined as the proportion of respondents residing in institutional group quarters, including correctional facilities, mental institutions, and retirement facilities. Noninstitutional group quarters include military housing and college dormitories, and these individuals are excluded from the calculation of institutionalization rates. Based on 1970 and 1980 data, in which institutional categories are broken down so that incarceration in jails or prisons could be separately identified, for younger men large shares of the population institutionalized were

17. These data come from the Digest of Education Statistics 2005 (http://nces.ed.gov/programs/digest/d05/tables/dt05_167.asp, accessed on March 17, 2007).

18. The raw data come from the website of the Bureau of Justice Statistics (http://bjsdata.ojp.usdoj.gov/dataonline/Search/Crime/State/statebystatelist.cfm, accessed on May 20, 2007).

**Table 4**
*Selected Descriptive Statistics, Children Aged 15–17 Years Living with Their Mothers, by Race, Ethnicity, and Family Structure*

| 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 | 0.02 | | | 0.12 | | | 0.04 | | | 0.00 | | |
| Black | 0.14 | 0.70 | 0.12 | | | | | | | | | |
| Hispanic | 0.08 | 0.15 | 0.08 | | | | | | | | | |
| Child has dropped out of high school | 0.05 | 0.07 | 0.05 | 0.06 | 0.07 | 0.06 | 0.07 | 0.09 | 0.07 | 0.04 | 0.07 | 0.04 |
| Mother did not finish high school | 0.26 | 0.38 | 0.26 | 0.40 | 0.36 | 0.41 | 0.52 | 0.57 | 0.52 | 0.21 | 0.23 | 0.21 |
| Mother finished just high school | 0.40 | 0.37 | 0.40 | 0.34 | 0.39 | 0.33 | 0.27 | 0.26 | 0.27 | 0.43 | 0.40 | 0.43 |
| Mother finished some college | 0.21 | 0.21 | 0.21 | 0.19 | 0.21 | 0.19 | 0.16 | 0.14 | 0.16 | 0.22 | 0.27 | 0.22 |
| Mother finished college | 0.13 | 0.04 | 0.13 | 0.07 | 0.04 | 0.07 | 0.06 | 0.03 | 0.06 | 0.14 | 0.09 | 0.14 |
| Mother's age at birth of child | 26.40 | 22.82 | 26.48 | 25.33 | 22.43 | 25.71 | 25.75 | 23.81 | 25.83 | 26.65 | 23.67 | 26.67 |
| | (5.90) | (5.66) | (5.88) | (6.56) | (5.56) | (6.58) | (6.17) | (5.98) | (6.16) | (5.72) | (5.60) | (5.71) |
| Number of siblings in household | 2.73 | 2.07 | 2.75 | 3.07 | 2.21 | 3.18 | 2.99 | 2.20 | 3.02 | 2.65 | 1.26 | 2.65 |
| | (2.14) | (1.82) | (2.14) | (2.38) | (1.85) | (2.42) | (2.16) | (1.79) | (2.17) | (2.09) | (1.48) | (2.09) |
| Institutionalization rate for same-race men (state of residence × race × year × age group) | 0.010 | 0.045 | 0.010 | 0.035 | 0.057 | 0.033 | 0.019 | 0.025 | 0.019 | 0.005 | 0.010 | 0.005 |
| | (0.017) | (0.035) | (0.015) | (0.031) | (0.034) | (0.029) | (0.014) | (0.015) | (0.014) | (0.005) | (0.005) | (0.005) |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Per-pupil school expenditures ($10,000) (state × year) | 0.64 (0.23) | 0.78 (0.23) | 0.64 (0.22) | 0.63 (0.24) | 0.76 (0.24) | 0.62 (0.23) | 0.73 (0.19) | 0.86 (0.23) | 0.72 (0.19) | 0.63 (0.22) | 0.81 (0.21) | 0.63 (0.22) |
| Violent crime rate (1,000 crimes per 100,000 population) (state × year) | 0.53 (0.26) | 0.63 (0.28) | 0.52 (0.26) | 0.60 (0.29) | 0.64 (0.29) | 0.59 (0.29) | 0.70 (0.22) | 0.69 (0.24) | 0.70 (0.22) | 0.50 (0.25) | 0.52 (0.23) | 0.50 (0.25) |
| Property crime rate (1,000 crimes per 100,000 population) (state × year) | 4.33 (1.33) | 4.31 (1.24) | 4.33 (1.33) | 4.40 (1.35) | 4.39 (1.21) | 4.40 (1.36) | 4.97 (1.41) | 4.32 (1.36) | 5.00 (1.40) | 4.26 (1.29) | 3.91 (1.14) | 4.26 (1.30) |
| Larceny rate (1,000 crimes per 100,000 population (state × year) | 2.68 (0.81) | 2.74 (0.71) | 2.67 (0.81) | 2.70 (0.81) | 2.79 (0.70) | 2.69 (0.83) | 3.07 (0.78) | 2.69 (0.74) | 3.09 (0.78) | 2.63 (0.80) | 2.56 (0.68) | 2.63 (0.80) |
| Employment rate for white men aged 18–40 years (state × year) | 0.84 (0.03) | 0.83 (0.03) | 0.84 (0.03) | 0.84 (0.03) | 0.84 (0.03) | 0.84 (0.03) | 0.84 (0.02) | 0.83 (0.02) | 0.84 (0.02) | 0.84 (0.03) | 0.83 (0.02) | 0.84 (0.03) |
| Mean annual earnings for white men aged 18–40 years ($1,000s) (state × year) | 16.80 (9.38) | 23.71 (8.64) | 16.64 (9.34) | 17.06 (9.31) | 22.63 (8.57) | 16.33 (9.15) | 24.32 (8.62) | 27.77 (8.05) | 24.17 (8.61) | 15.98 (9.12) | 24.72 (8.19) | 15.94 (9.11) |
| N (unweighted) | 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).

clearly incarcerated (Butcher and Piehl 2007; Charles and Luoh 2010).[19] Aggregate data by race on incarceration based on the Census institutionalization definition is consistent with information from the Bureau of Justice Statistics (Raphael 2006). Each child observation is assigned an institutionalization rate based on the mother's age, child's race or ethnicity, state of residence, and year. Mothers aged $t$ years are assigned the institutionalization rate for the appropriate sample of men aged $t$ to $t+5$ years. This age range was chosen based on the observed relationship between spousal ages in the United States (husbands are about two years older than wives) combined with the evidence that women in poor, urban communities are more likely to marry older men (Vera, Berardo, and Berardo 1985).

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.[20] 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.

If 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, the lagged incarceration rate instrumental variable that we use is more appropriate. On the other hand, 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 2002 data, the percentage of black men ever married rises from 46 percent at age 30 to 74 percent at age 40 (Lichter and Graefe 2007).[21] For Hispanics and nonblack 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 indicates that there is a norm of childbearing first followed by a desire for marriage later. The delay arises both because women want men to have established themselves financially and women want to have established *themselves* financially so that they can legitimately threaten

19. In our own calculations from the 1980 Census, we estimate the proportion of institutionalized men who reside in a correctional facility by race, ethnicity, and single-year age group. For blacks and Hispanics, this proportion is higher than 80 percent at age 19 and above 75 percent at ages 30 and 40. By age 50, this proportion falls to 45 percent for blacks and 60 percent for Hispanics. This provides yet another reason to prefer the lagged incarceration rate instrumental variable, as it is based on the more accurate institutionalization rates measured at younger ages. Nonetheless, as long as the across-state-and-year variation in institutionalization rates is driven by variation in incarceration rates, the rates for older men will still provide valid identifying information.

20. There is a question as to whether the effective marriage market should be defined at the state level. Charles and Luoh (2010) use a state-level definition like we do. Brien (1997) explores the explanatory power of marriage markets defined at the state versus local level, and finds that state-level definitions work better.

21. Similarly, Lichter, Graefe, and Brown (2003) study 24–45 year-olds in the 1995 National Survey of Family Growth, and find that of those who have an out-of-wedlock child, 41 percent subsequently marry.

to leave marriages that, for this subpopulation, are often to men with drug-, crime-, or abuse-related problems with relatively low economic security (Edin 2000; Edin and Reed 2005). 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 contemporaneous incarceration rate for older males may be appropriate given that women who give birth out of wedlock are more likely to marry older men if they do marry (Qian, Lichter, and Mellott 2005).

Figure 1 shows histograms for incarceration rates for men aged 18–40 years across states in 1980, 1990, and 2000. Incarceration rates for whites are low in all states as of 2000. In contrast, incarceration rates in most states are much higher for Hispanics, and more so for blacks. Moreover, for both minority groups incarceration rates clearly increased over these decades. Figure 2 shows the histograms of changes in incarceration rates across states over the periods 1980–1990, 1990–2000, and 1980–2000; the vertical axes show the number of states with changes in incarceration rates in the indicated ranges.[22] These histograms show dramatic increases in incarceration rates for minorities, especially in some states, with the greatest increases between 1990 and 2000. Figures 1 and 2 indicate that the shares of blacks and Hispanics potentially affected by changes in incarceration rates are substantial.[23] This variation is central to our identification strategy. Because of the absence of substantial changes for whites, coupled with low incarceration rates for them in general as well as low never-married rates, we focus on blacks and Hispanics in our analysis.

There might be some concern that increases in incarceration have been concentrated in particular geographic regions of the country. Figure 3 maps the changes in incarceration. 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. The figure shows that states with small, medium, and large increases in black incarceration are represented in all major regions of the country.

Table 5 presents descriptive statistics on the percentage of children aged 15–17 living with a never-married mother. The columns are broken down by whether the incarceration rate for men aged 18–40 years and the same race or ethnicity as the child is less than the 25th percentile, between the 25th and 75th percentiles, or greater than the 75th percentile. The 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 whole sample. 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

22. There are some extreme values generated by small cells, but since we use individual-level data, these observations have an inconsequential influence on the results.

23. Any given increase in incarceration rates over a decade implies a considerably larger increase in the probability that an individual was incarcerated at some point over that decade.

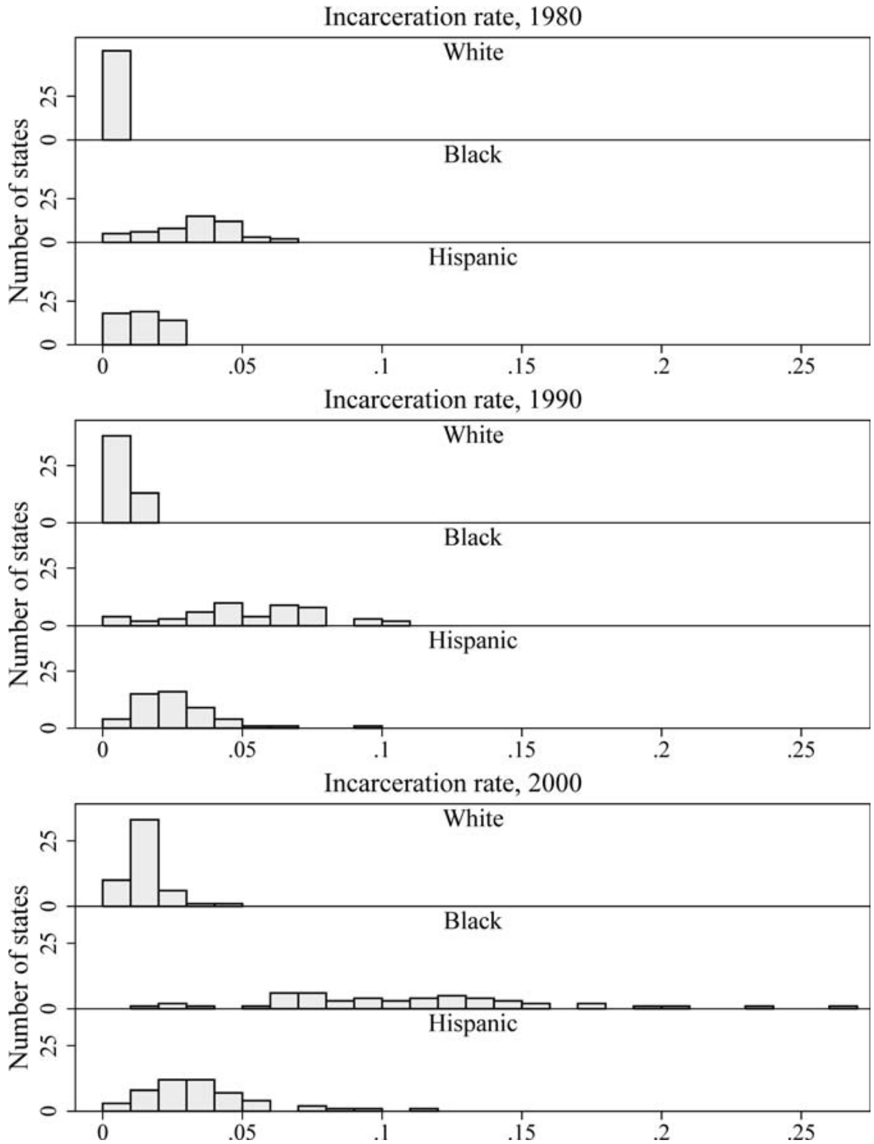


**Figure 1**

*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.

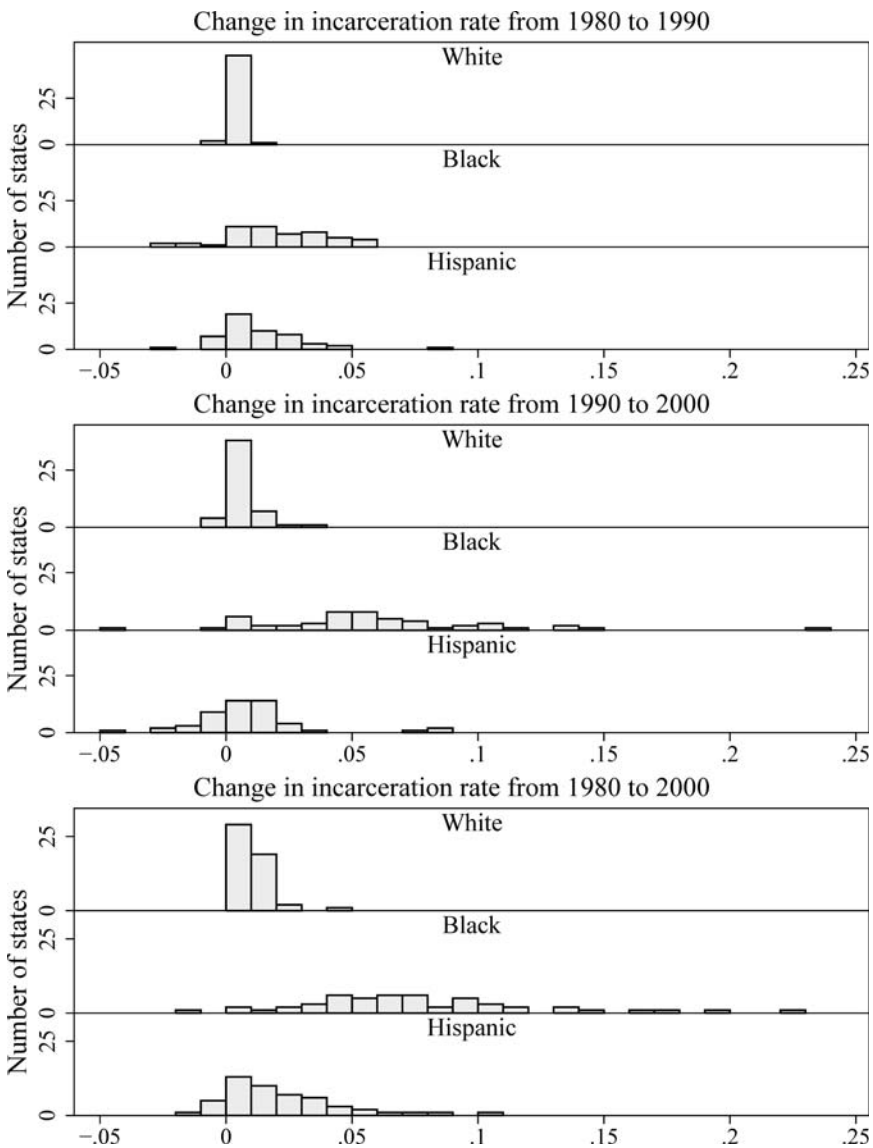


**Figure 2**

*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.

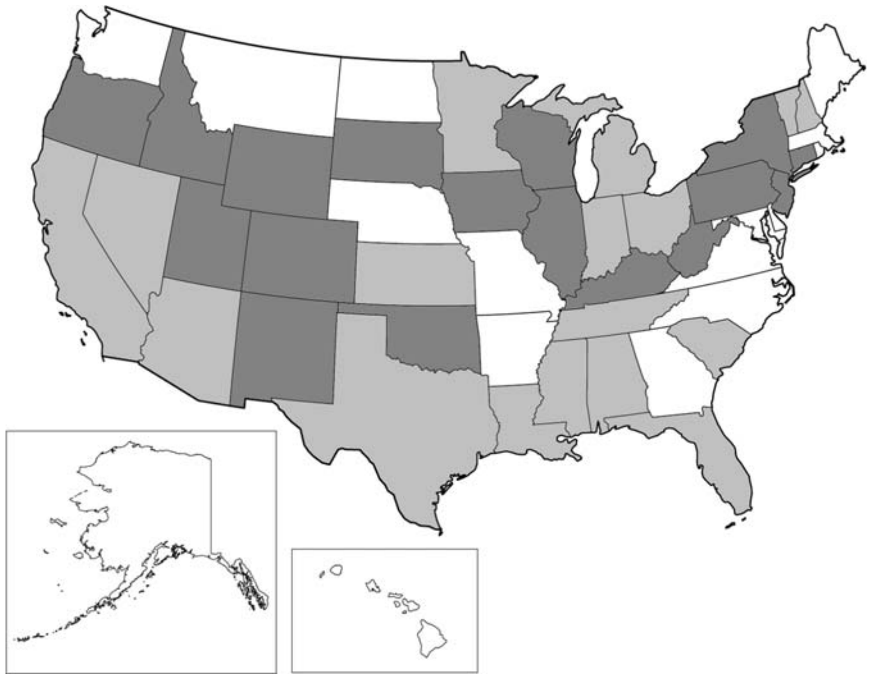

**Figure 3**
*Changes in Incarceration Rates for Black Men Aged 18–40 Years from 1980 to 2000, by State*

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). These cutoffs are approximately tritiles of the distribution of changes by state. Rates from 1980 are used as a baseline for the differences because the 1970 data are relatively noisy.

motherhood; for this reason, as well, our analysis focuses on black and Hispanic children.[24]

## VI. Results

### *A. Main results*

We begin with estimates of the equations for never-married motherhood. Table 6 reports estimates from probit regressions, in Columns 2 and 5 using the contem-

24. The regression analysis, based on age-specific incarceration rates, leads to a similar conclusion that the incarceration "experiment" does not work for whites. In the first-stage equation for never-married motherhood, the effect of incarceration was much weaker for whites than for blacks or Hispanics, using linear probability or probit. For the probit estimation, the estimated effect of the preferred lagged incarceration rate was near zero and statistically insignificant.

**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 Aged 18–40 Years*

| | 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 | 0.14 | 0.31 | 0.88 |
| 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 | 5.73 | 9.77 | 20.54 |
| C: Hispanic children | | | |
| 1980 | 1.05 | 1.45 | 4.69 |
| 1990 | 3.70 | 2.02 | 6.53 |
| 2000 | 4.97 | 4.02 | 9.10 |
| Pooled years | 1.06 | 3.74 | 6.59 |

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.

poraneous incarceration rate, and in Columns 3 and 6 using the lagged incarceration rate. For blacks, the estimated marginal effect of the current incarceration rate is 0.528 (Column 2), and 0.184 (Column 3) for the lagged incarceration rate. The current rate is statistically significantly different from zero at the 1 percent level, and the lagged rate at the 10 percent level. For Hispanics, the estimated marginal effect of the current rate is 0.221 (Column 5), and 0.167 (Column 6) for the lagged rate. The current rate is statistically significant at the 1 percent level and the lagged rate is statistically significant at the 5 percent level. To put these estimates in context, the approximate modal increases in incarceration rates over the 1980–2000 period were 0.07 for black men and 0.02 for Hispanic men. Thus, for blacks, for example, the probit estimates imply that the modal increase in contemporaneous incarceration would lead to a 3.7 percentage point increase in never-married motherhood, or a 52

**Table 6**

*Probit Estimates of Models for Never-Married Motherhood, Children Aged 15–17 Years Living with Their Mothers, by Race and Ethnicity*

| Independent variables | Black (1) | Black (2) | Black (3) | Hispanic (4) | Hispanic (5) | Hispanic (6) |
|---|---|---|---|---|---|---|
| Incarceration rate | | 0.528 | | | 0.221 | |
| (state of residence, year *t*) | | (0.106) | | | (0.085) | |
| Incarceration rate | | | 0.184 | | | 0.167 |
| (state of birth, year *t*-10) | | | (0.105) | | | (0.073) |
| Female (child) | 0.0007 | 0.0006 | 0.0016 | 0.0020 | 0.0020 | 0.0016 |
| | (0.0011) | (0.0011) | (0.0016) | (0.0008) | (0.0008) | (0.0007) |
| Per-pupil educational expenses ($1,000s) | 0.074 | 0.070 | 0.019 | 0.001 | −0.004 | −0.000 |
| | (0.027) | (0.027) | (0.036) | (0.014) | (0.015) | (0.017) |
| Violent crime rate (1,000 crimes | 0.032 | 0.012 | 0.017 | 0.020 | 0.015 | 0.022 |
| per 100,000 population) | (0.023) | (0.023) | (0.021) | (0.007) | (0.008) | (0.008) |
| Property crime rate (1,000 crimes | 0.002 | 0.007 | 0.017 | 0.008 | 0.012 | 0.010 |
| per 100,000 population) | (0.009) | (0.008) | (0.013) | (0.005) | (0.006) | (0.006) |
| Larceny rate (1,000 crimes per | −0.023 | −0.022 | −0.036 | −0.020 | −0.025 | −0.023 |
| 100,000 population) | (0.013) | (0.013) | (0.020) | (0.007) | (0.008) | (0.009) |
| Employment rate for white men | 0.122 | 0.184 | 0.442 | 0.026 | 0.006 | −0.014 |
| aged 18–40 years | (0.126) | (0.117) | (0.140) | (0.051) | (0.052) | (0.064) |
| Mean earnings for white men aged | −0.0006 | 0.0004 | 0.0019 | −0.0003 | 0.0000 | 0.0003 |
| 18–40 years ($1,000s) | (0.0012) | (0.0012) | (0.0011) | (0.0008) | (0.0008) | (0.0007) |
| Mother is high school graduate | −0.043 | −0.042 | −0.059 | −0.012 | −0.012 | −0.014 |
| | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.002) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Mother has some college | −0.063 | −0.063 | −0.089 | −0.016 | −0.016 | −0.017 |
| | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.001) |
| Mother has four years of college | −0.073 | −0.073 | −0.104 | −0.022 | −0.022 | −0.022 |
| | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) | (0.001) |
| Age of mother at birth of child | −0.0072 | −0.0060 | −0.0097 | −0.0018 | −0.0016 | −0.0019 |
| | (0.0002) | (0.0003) | (0.0003) | (0.0001) | (0.0001) | (0.0001) |
| Number of siblings in house | −0.013 | −0.013 | −0.016 | −0.007 | −0.007 | −0.007 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Observations | 197,085 | 196,763 | 178,679 | 151,859 | 151,736 | 111,299 |
| Pseudo $R^2$ | 0.15 | 0.15 | 0.12 | 0.10 | 0.10 | 0.12 |
| Mean of dependent variable | 0.12 | 0.12 | 0.14 | 0.04 | 0.04 | 0.04 |

Notes: The incarceration rate variable is defined based on the woman's age, race/ethnicity, state, and year. For women of age $a$ in year $t$, the incarceration rate is defined for men aged $a$ to $a + 5$ (and age $a - 10$ to age $a - 5$ for women of age $a - 10$ in year $t - 10$). Each specification includes child age effects, state effects, year effects, and child age-year effects. Estimates come from probit specification; table reports marginal effects that are evaluated at the means of each specification's respective sample. Heteroscedasticity-robust standard errors, clustered at the state level, are in parentheses. All estimates are weighted by IPUMS person weights. Data from 1970 are not used in the estimation for Hispanics.

percent increase over the rate of 7.1 percent in 1980 (Table 2); the corresponding numbers for Hispanics are 0.4 percentage point and 22 percent.[25]

Table 7 reports our first set of estimates of the models for whether a child has dropped out of high school. OLS estimates that do not account for endogenous selection (Columns 1 and 4) indicate that black and Hispanic children are more likely, on average, to have dropped out of high school if they live with a never-married mother. In particular, blacks and Hispanics living with never-married mothers are 1.5 and 3.2 percentage points more likely to have dropped out of high school, respectively. Given mean dropout rates at these ages of 6 percent for blacks and 7 percent for Hispanics, these are large effects.

The two-stage instrumental variables estimates that account for nonrandom selection into never-married motherhood are reported in Columns 2, 3, 5, and 6. For blacks, the estimated effects of never-married motherhood on whether a child has dropped out of high school become negative, indicating a lower likelihood of dropout associated with never-married motherhood (0.3 to 0.9 percentage point), although the estimates are fairly small and statistically insignificant. For Hispanics, as well, the estimated effects of never-married motherhood on whether a child has dropped out of high school become negative, but in this case the estimates are larger. Hispanic children of mothers who are affected by variation in the incarceration of men are estimated to be between three and 8.2 percentage points less likely to drop out of high school. The latter estimate, which is based on the lagged incarceration rate instrumental variable, is statistically significant. The Anderson-Rubin test of the null hypothesis that the coefficient on never-married motherhood is equal to zero provides very similar inference to an analogous Wald test (for example, $p$-values of 0.485 and 0.031 for Hispanics), which indicates that the statistical inferences for the two-stage instrumental variables estimates are valid.

Table 8 relaxes the specification of the effects of incarceration by adding polynomials of incarceration rates into the model for never-married motherhood. The estimated probit marginal effects (not shown) indicate that the effects of incarceration rates on never-married motherhood are stronger at higher incarceration rates. Moreover, for both blacks and Hispanics the $\chi^2$-statistic for the joint significance of the incarceration rate in the never-married motherhood probit is greater in the nonlinear models than in the corresponding specification in Table 7. However, the second-stage results when nonlinear effects of the incarceration rate are allowed in Table 8 are very similar to those in Table 7.

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, for those women whose marriage decisions are affected by variation in incarceration rates. And sec-

25. We also estimated these models for three separate education groups: high school dropouts, high school graduates, and any college. For blacks and Hispanics the estimated effects of incarceration decline sharply as education increases, and are never significant for the two higher education groups. This bolsters the local average treatment effect interpretation of the estimates as identifying the effects for less-educated women whose marriage prospects are more strongly affected by variation in incarceration rates.

ond, they suggest that, for Hispanic children, never-married motherhood may actually reduce the likelihood of dropping out.

### *B. Identification and robustness checks*

A potential concern regarding the two-stage instrumental variables estimation is that the nonlinear functions of the control variables in the fitted probability of never-married motherhood, rather than the variation in incarceration rates, serve to identify the effect of never-married motherhood. To avoid relying on this type of identifying information, we can estimate the model using linear probability specifications and two-stage least squares. This approach may yield relatively imprecise estimates in our case, given that 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, which may result in a much weaker first stage 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.[26] Table 9 reports two-stage least squares results using polynomials of the incarceration rate instruments. There is a loss of precision relative to the two-stage instrumental variables models. For blacks and Hispanics, the standard errors of the estimated effects of never-married motherhood are roughly two to four times as large. The resulting estimates are never positive and significant, in contrast to the OLS estimates that indicate an adverse effect of never-married motherhood. However, the sign of the instrumental variables estimate of the effect of never-married motherhood is in this case sensitive to using current versus lagged incarceration rates as instruments. In particular, the estimated effect is consistently negative (for blacks and Hispanics) when the lagged incarceration rates are used as instruments. But the results highlight that drawing stronger conclusions from the data hinges on using the two-stage instrumental variables estimation strategy.

A second identification-related issue, discussed earlier, is that incarceration rates may have direct effects on child outcomes. The most plausible scenario is that incarceration rates of older men (or lagged incarceration rates of younger men) are correlated with contemporaneous variation in incarceration rates of younger men. Incarceration of younger men could directly affect the costs and benefits of alternative decisions made by teenagers and hence their decisions to drop out of high school.[27] In this case, the exclusion restriction underlying the instrumental variables estimation is invalid, and our estimates may instead reflect the direct effect of incarceration, albeit still suggesting that higher incarceration leads to better outcomes for children.

To address this concern that the inclusion of crime rates and other controls are not sufficient to prevent the violation of the exclusion restriction, we calculate the incarceration rate for same-race/ethnicity men aged 18–24 years living in the same

26. See Angrist (1991) and Bhattacharya, Goldman, and McCaffrey (2006) for discussion of these and related issues for the two-equation linear probability model.
27. As an example, a referee suggested that higher incarceration of youths may be associated with the break-up of gangs, and consequently higher enrollment of those prone to gang membership, who may be more likely to be the children of never-married mothers.

**Table 7**
*OLS and Two-Stage Instrumental Variables Regression Estimates of Models for Whether Child Has Dropped Out of High School, Children Aged 15–17 Years Living with Their Mothers, by Race and Ethnicity*

| Independent variables | Black OLS (1) | Black 2SIV (2) | Black 2SIV (3) | Hispanic OLS (4) | Hispanic 2SIV (5) | Hispanic 2SIV (6) |
|---|---|---|---|---|---|---|
| Endogenous covariates | | | | | | |
| Mother never married | 0.015 | −0.003 | −0.009 | 0.032 | −0.030 | −0.082 |
| | (0.002) | (0.016) | (0.017) | (0.004) | (0.044) | (0.039) |
| Anderson-Rubin $p$-value | | 0.863 | 0.605 | | 0.485 | 0.031 |
| Other controls | | | | | | |
| Female (child) | −0.0051 | −0.0051 | −0.0057 | −0.0051 | −0.0076 | −0.0052 |
| | (0.0015) | (0.0015) | (0.0015) | (0.0015) | (0.0014) | (0.0015) |
| Per-pupil educ. exp. ($1,000s) | −0.001 | 0.004 | −0.001 | −0.001 | 0.073 | 0.074 |
| | (0.017) | (0.017) | (0.018) | (0.017) | (0.022) | (0.026) |
| Violent crime rate | 0.034 | 0.034 | 0.036 | 0.034 | 0.002 | 0.002 |
| (1,000 crimes per 100,000 population) | (0.012) | (0.012) | (0.013) | (0.012) | (0.012) | (0.013) |
| Property crime rate | −0.001 | −0.001 | 0.001 | −0.001 | 0.009 | 0.008 |
| (1,000 crimes per 100,000 population) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) |
| Larceny rate | −0.010 | −0.010 | −0.013 | −0.010 | −0.011 | −0.016 |
| (1,000 crimes per 100,000 population) | (0.009) | (0.009) | (0.010) | (0.009) | (0.010) | (0.012) |
| Employment rate for white men aged | 0.161 | 0.160 | 0.151 | 0.161 | 0.434 | 0.182 |
| 18–40 years | (0.072) | (0.072) | (0.076) | (0.072) | (0.094) | (0.106) |
| Mean earnings for white men aged 18–40 | −0.0003 | −0.0004 | −0.0003 | −0.0003 | −0.0005 | −0.0000 |
| years ($1,000s) | (0.0006) | (0.0006) | (0.0006) | (0.0006) | (0.0008) | (0.0009) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Mother is high school graduate | −0.041 | −0.042 | −0.043 | −0.041 | −0.047 | −0.039 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Mother has some college | −0.053 | −0.055 | −0.056 | −0.053 | −0.056 | −0.046 |
| | (0.002) | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) |
| Mother has four years of college | −0.063 | −0.066 | −0.068 | −0.063 | −0.067 | −0.059 |
| | (0.002) | (0.003) | (0.003) | (0.002) | (0.003) | (0.003) |
| Age of mother at birth of child | 0.0000 | −0.0001 | −0.0001 | 0.0000 | −0.0004 | −0.0007 |
| | (0.0001) | (0.0002) | (0.0002) | (0.0001) | (0.0002) | (0.0002) |
| Number of siblings in house | 0.0008 | 0.0006 | 0.0005 | 0.0008 | 0.0006 | −0.0006 |
| | (0.0004) | (0.0005) | (0.0005) | (0.0004) | (0.0005) | (0.0005) |
| First stage | | | | | | |
| Instrumental variables: | | | | | | |
| Incarceration rate (state of residence, year $t$) | | X | | | X | |
| Incarceration rate (state of birth, year $t-10$) | | | X | | | X |
| Predicted NM | | 1.139 | 1.208 | | 1.052 | 1.158 |
| | | (0.021) | (0.022) | | (0.054) | (0.060) |
| $F$-statistic for instrumental variable | | 2,888.15 | 3,078.73 | | 374.65 | 377.54 |
| $\chi^2$-statistic for incarceration rate in probit | | 76.23 | 8.12 | | 16.83 | 17.15 |
| Observations | 197,129 | 196,762 | 189,735 | 152,073 | 151,736 | 111,278 |
| $R^2$ | 0.03 | | | 0.04 | | |
| Mean of dependent variable | 0.06 | 0.06 | 0.06 | 0.07 | 0.07 | 0.06 |

Notes: See notes to Table 6 for the definition of the incarceration rates. 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 instrumental variables procedure described in Wooldridge (2002, p. 623). All estimates are weighted by IPUMS person weights. Data from 1970 are not used in the estimation for Hispanics.

**Table 8**
*Two-Stage Instrumental Variables Regression Estimates of Models for Whether Child Has Dropped Out of High School, with Incarceration-rate Polynomials in the First Stage, Children Aged 15–17 Years Living with Their Mothers, by Race and Ethnicity*

| Independent variables | Black (1) | Black (2) | Black (3) | Black (4) | Hispanic (5) | Hispanic (6) | Hispanic (7) | Hispanic (8) |
|---|---|---|---|---|---|---|---|---|
| Endogenous covariates | | | | | | | | |
| Mother never married | −0.002 | −0.001 | −0.009 | −0.008 | −0.029 | −0.019 | −0.083 | −0.083 |
| | (0.016) | (0.016) | (0.017) | (0.017) | (0.044) | (0.043) | (0.038) | (0.037) |
| Anderson-Rubin *p*-value | 0.884 | 0.964 | 0.601 | 0.623 | 0.500 | 0.656 | 0.028 | 0.025 |
| Other controls | | | | | | | | |
| Female (child) | −0.005 | −0.005 | −0.006 | −0.006 | −0.008 | −0.008 | −0.005 | −0.005 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| Per-pupil educational expenses ($1,000s) | 0.004 | 0.004 | −0.001 | −0.001 | 0.073 | 0.073 | 0.074 | 0.074 |
| | (0.017) | (0.017) | (0.018) | (0.018) | (0.022) | (0.022) | (0.026) | (0.026) |
| Violent crime rate | 0.034 | 0.034 | 0.036 | 0.036 | 0.002 | 0.003 | 0.002 | 0.002 |
| (1,000 crimes per 100,000 population) | (0.012) | (0.012) | (0.013) | (0.013) | (0.012) | (0.012) | (0.013) | (0.013) |
| Property crime rate | −0.001 | −0.001 | 0.001 | 0.001 | 0.009 | 0.009 | 0.008 | 0.008 |
| (1,000 crimes per 100,000 population) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) | (0.007) |
| Larceny rate | −0.010 | −0.010 | −0.013 | −0.013 | −0.011 | −0.011 | −0.016 | −0.016 |
| (1,000 crimes per 100,000 population) | (0.009) | (0.009) | (0.010) | (0.010) | (0.010) | (0.010) | (0.012) | (0.012) |
| Employment rate for white men aged | 0.160 | 0.160 | 0.151 | 0.151 | 0.434 | 0.430 | 0.182 | 0.182 |
| 18–40 years | (0.072) | (0.072) | (0.076) | (0.076) | (0.094) | (0.094) | (0.106) | (0.106) |
| Mean earnings for white men aged | −0.0004 | −0.0004 | −0.0003 | −0.0003 | −0.0005 | −0.0006 | −0.000 | −0.0000 |
| 18–40 years ($1,000s) | (0.0006) | (0.0006) | (0.0006) | (0.0006) | (0.0008) | (0.0008) | (0.001) | (0.0009) |
| Mother is high school graduate | −0.042 | −0.042 | −0.043 | −0.043 | −0.047 | −0.046 | −0.039 | −0.039 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Mother has some college | −0.055 | −0.054 | −0.056 | −0.056 | −0.055 | −0.055 | −0.046 | −0.046 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) | (0.002) |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Mother has four years of college | −0.066 | −0.066 | −0.068 | −0.068 | −0.067 | −0.066 | −0.059 | −0.059 |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Age of mother at birth of child | −0.0001 | −0.0001 | −0.0001 | −0.0001 | −0.0003 | −0.0003 | −0.0007 | −0.0007 |
| | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0002) | (0.0002) |
| Number of siblings in house | 0.0006 | 0.0006 | 0.0005 | 0.0005 | 0.0007 | 0.0007 | −0.0006 | −0.0006 |
| | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.0005) | (0.0005) |
| First stage | | | | | | | | |
| Instrumental variables: | | | | | | | | |
| Incarceration rate (state of residence, year *t*) | X | X | | | X | X | | |
| Incarceration rate squared (state of residence, year *t*) | X | X | | | X | X | | |
| Incarceration rate cubed (state of residence, year *t*) | | X | | | | X | | |
| Incarceration rate (state of birth, year $t-10$) | | | X | X | | | X | X |
| Incarceration rate squared (state of birth, year $t-10$) | | | X | X | | | X | X |
| Incarceration rate cubed (state of birth, year $t-10$) | | | | X | | | | X |
| Predicted NM | 1.143 | 1.117 | 1.207 | 1.201 | 1.053 | 1.026 | 1.162 | 1.117 |
| | (0.021) | (0.021) | (0.022) | (0.022) | (0.054) | (0.054) | (0.059) | (0.058) |
| *F*-statistic for instrumental variable | 2911.26 | 2912.93 | 3073.42 | 3068.36 | 377.93 | 367.26 | 383.61 | 376.56 |
| $\chi^2$-statistic for incarceration rate variables in probit | 76.41 | 128.01 | 8.63 | 12.61 | 17.12 | 29.52 | 17.14 | 41.17 |
| Observations | 196,762 | 196,762 | 189,735 | 189,735 | 151,736 | 151,736 | 111,278 | 111,278 |
| Mean of dependent variable | 0.06 | 0.06 | 0.06 | 0.06 | 0.07 | 0.07 | 0.06 | 0.06 |

Notes: See notes to Table 7.

**Table 9**
*Two-Stage Least Squares Regression Estimates of Models for Whether Child Has Dropped Out of High School, Children Aged 15–17 Years Living with Their Mothers, by Race and Ethnicity*

| Independent variables | Black (1) | Black (2) | Black (3) | Black (4) | Hispanic (5) | Hispanic (6) | Hispanic (7) | Hispanic (8) |
|---|---|---|---|---|---|---|---|---|
| Endogenous covariates | | | | | | | | |
| Mother never married | 0.011 | 0.011 | −0.089 | −0.048 | −0.162 | 0.052 | −0.026 | −0.057 |
| | (0.034) | (0.031) | (0.055) | (0.048) | (0.169) | (0.114) | (0.142) | (0.133) |
| Anderson-Rubin *p*-value | 0.944 | 0.140 | 0.241 | 0.241 | 0.556 | 0.100 | 0.570 | 0.753 |
| Other controls | | | | | | | | |
| Female (child) | −0.005 | −0.005 | −0.006 | −0.006 | −0.007 | −0.008 | −0.005 | −0.005 |
| | (0.001) | (0.001) | (0.002) | (0.002) | (0.001) | (0.001) | (0.002) | (0.002) |
| Per-pupil educational expenses | 0.001 | 0.001 | 0.014 | 0.006 | 0.080 | 0.071 | 0.076 | 0.077 |
| ($1,000s) | (0.018) | (0.018) | (0.021) | (0.020) | (0.023) | (0.022) | (0.026) | (0.026) |
| Violent crime rate | 0.034 | 0.034 | 0.037 | 0.037 | 0.001 | 0.004 | 0.005 | 0.003 |
| (1,000 crimes per 100,000 population) | (0.012) | (0.012) | (0.013) | (0.013) | (0.012) | (0.012) | (0.014) | (0.014) |
| Property crime rate | −0.001 | −0.001 | 0.001 | 0.001 | 0.011 | 0.009 | 0.008 | 0.008 |
| (1,000 crimes per 100,000 population) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) | (0.007) |
| Larceny rate | −0.010 | −0.010 | −0.015 | −0.014 | −0.013 | −0.010 | −0.016 | −0.016 |
| (1,000 crimes per 100,000 population) | (0.009) | (0.009) | (0.010) | (0.010) | (0.011) | (0.010) | (0.012) | (0.012) |
| Employment rate for white men | 0.158 | 0.158 | 0.173 | 0.163 | 0.473 | 0.403 | 0.143 | 0.157 |
| aged 18–40 years | (0.072) | (0.072) | (0.076) | (0.076) | (0.110) | (0.100) | (0.124) | (0.123) |
| Mean earnings for white men aged | −0.0004 | −0.0004 | −0.0005 | −0.0004 | −0.0003 | −0.0007 | −0.0003 | −0.0002 |
| 18–40 years ($1,000s) | (0.0006) | (0.0006) | (0.0006) | (0.0006) | (0.0008) | (0.0008) | (0.0010) | (0.0010) |
| Mother is high school graduate | −0.041 | −0.041 | −0.047 | −0.045 | −0.049 | −0.045 | −0.038 | −0.038 |
| | (0.003) | (0.002) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Mother has some college | −0.053 | −0.053 | −0.065 | −0.060 | −0.059 | −0.054 | −0.045 | −0.045 |
| | (0.004) | (0.004) | (0.006) | (0.005) | (0.004) | (0.003) | (0.004) | (0.004) |
| Mother has four years of college | −0.064 | −0.064 | −0.079 | −0.073 | −0.072 | −0.063 | −0.057 | −0.058 |
| | (0.005) | (0.005) | (0.008) | (0.007) | (0.007) | (0.005) | (0.007) | (0.006) |
| Age of mother at birth of child | 0.0000 | 0.0000 | −0.0008 | −0.0004 | −0.0006 | −0.0002 | −0.0005 | −0.0006 |
| | (0.0003) | (0.0003) | (0.0005) | (0.0004) | (0.0004) | (0.0003) | (0.0004) | (0.0004) |
| Number of siblings in house | 0.0007 | 0.0007 | −0.0005 | 0.0000 | −0.0003 | 0.0013 | −0.0002 | −0.0004 |
| | (0.0006) | (0.0006) | (0.0008) | (0.0007) | (0.0013) | (0.0009) | (0.0010) | (0.0010) |
| First stage | | | | | | | | |
| Incarceration rate | 1.785 | 1.199 | | | 0.730 | −0.728 | | |
| (state of residence, year $t$) | (0.093) | (0.175) | | | (0.132) | (0.260) | | |
| Incarceration rate squared | −1.188 | 3.401 | | | −1.559 | 32.122 | | |
| (state of residence, year $t$) | (0.404) | (1.325) | | | (1.118) | (5.648) | | |
| Incarceration rate cubed | | −4.832 | | | | −169.284 | | |
| (state of residence, year $t$) | | (1.253) | | | | (27.117) | | |
| Incarceration rate | | | −0.013 | −0.895 | | | 0.814 | 0.357 |
| (state of birth, year $t-10$) | | | (0.147) | (0.185) | | | (0.113) | (0.176) |
| Incarceration rate squared | | | 5.237 | 20.116 | | | −2.423 | 4.475 |
| (state of birth, year $t-10$) | | | (1.223) | (3.005) | | | (0.475) | (2.230) |
| Incarceration rate cubed | | | | −63.583 | | | | −19.210 |
| (state of birth, year $t-10$) | | | | (14.084) | | | | (5.904) |
| $F$-statistic for instrumental variable | 296.93 | 195.85 | 59.38 | 59.25 | 30.95 | 26.00 | 27.85 | 19.93 |
| Observations | 196,805 | 196,805 | 189,773 | 189,773 | 151,948 | 151,948 | 111,482 | 111,482 |
| Mean of dependent variable | 0.06 | 0.06 | 0.06 | 0.06 | 0.07 | 0.07 | 0.06 | 0.06 |

Notes: See notes to Table 7.

state as the child and add this as a control. These results are reported in Table 10. In Columns 1 and 6, we show OLS estimates comparable to the baseline estimations in Table 7 but with the incarceration rate for men aged 18–24 years used as an additional control. For blacks and Hispanics, there is essentially no change in the estimate of the coefficient on never-married motherhood. We use the contemporaneous incarceration rate as an instrument in the two-stage instrumental variables model in Columns 2 and 7, and the lagged rate in Columns 3 and 8. In Columns 5 and 10, we use both. (Columns 4 and 9 use both rates as instruments and leave out the younger incarceration rate control, for comparison.) The estimates from these models, which should be more robust to the exclusion restriction, all parallel the estimates from the baseline models. For blacks, the rates are all very close to zero and statistically insignificant; all point estimates are negative. For Hispanics, as long as the lagged incarceration rate is used as an instrumental variable, never-married motherhood is associated with a seven to eight percentage point reduction in the probability of high school dropout, significant at the 5 or 10 percent level. Given that these specifications all require variation in incarceration rates for younger men that is independent of variation in incarceration rates for potential spouses of the mother, the evidence in Table 10 bolsters our conclusions by showing that our results are not confounded by a violation of the exclusion restriction stemming from a direct effect of the incarceration of younger men on teen outcomes.[28]

Finally, Table 11 presents a series of additional analyses and robustness checks. We begin by considering 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 estimates in Panel B are based on incarceration rates calculated only for men who currently reside in the same state they did five years before the Census. The two-stage instrumental variables estimates are similar to our baseline estimates.

Second, given the massive increase in adult incarceration, it is not surprising that there has been some increase in youth institutionalization.[29] 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 instrumental variables strategy may put more weight on the best performing children of never-

28. We also experimented with adding the instrumental variable based on the current incarceration rate as a control variable, instrumenting with the lagged incarceration rate only, to address the related but different question about the direct effects of removing fathers to prison. For blacks, the predictive power of the lagged instrument in the first-stage probit using this specification was very low (the $\chi^2$ statistic was only 0.02), whereas for Hispanics it was higher (4.33, significant at the 5 percent level); this is reflected also in Table 6, where, for blacks, the current incarceration rate is a stronger predictor of never-married motherhood. Thus, only the instrumental variables estimation for Hispanics is relevant, and yielded evidence similar to that reported above, with an estimated coefficient (standard error) of never-married motherhood of $-0.078$ (0.044).

29. For black children aged 15–17, the institutionalization rate increased from 0.3 percent in 1970 to 2.2 percent in 2000. For Hispanics, the rate increased from 0.08 percent in 1980 to 1.1 percent in 2000.

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, and imputed to their "mothers" the associated maternal controls for never-married mothers in the same state, year, and race/ethnic group. As reported in Panel C, in most 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, all of the estimates remain negative, and the estimate for Hispanics remains statistically significant when the lagged incarceration rate is used as an instrument. 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 driving our results.

Finally, changes in other policies that affect schooling decisions may be correlated with changes in incarceration rates, biasing the instrumental variables 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 (for example, Neumark and Wascher 2003) and compulsory schooling laws to increase it (for example, Acemoglu and Angrist 2000). More generally, there may be changes in nonschool opportunities that are contemporaneous with changes in incarceration rates. In Panel D of Table 11 we present estimates of models in which we control for the state minimum wage, whether a child was covered by a compulsory schooling law, and the employment rate for white 15–17 year-olds.[30] The estimates for both blacks and Hispanics are almost identical to those from the baseline specifications.[31]

Summing up, *none* of the two-stage instrumental variables estimates point to evidence of a beneficial effect of never-married motherhood in reducing teen dropout, in contrast to OLS estimates that do not account for endogenous selection into never-married motherhood. For blacks, the sign of the instrumental variables estimate changes across specifications, but is generally negative, always very small, and never statistically significant. For Hispanics, the sign of the instrumental variables estimate always indicates that never-married motherhood *reduces* the likelihood that children drop out of high school, and the estimates are often statistically significant. These conclusions are robust to a battery of specifications intended to gauge the strength of the evidence and the validity of the identification strategy.

30. Compulsory schooling laws come from various editions of the Digest of Education Statistics. Records are not available for every year, so 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 covered by a compulsory schooling law if the child's age is less than the maximum required age of compulsory schooling in a 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.

31. 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).

**Table 10**
*Regression Estimates of Models for Whether Child Has Dropped Out of High School Including Control for Young Male Incarceration Rate, Children Aged 15–17 Years Living with Their Mothers, by Race and Ethnicity*

| Independent variables | Black OLS (1) | Black 2SIV (2) | Black 2SIV (3) | Black 2SIV (4) | Black 2SIV (5) | Hispanics OLS (6) | Hispanics 2SIV (7) | Hispanics 2SIV (8) | Hispanics 2SIV (9) | Hispanics 2SIV (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| Endogenous covariates | | | | | | | | | | |
| Mother never married | 0.015 | −0.002 | −0.008 | −0.005 | −0.004 | 0.033 | −0.002 | −0.072 | −0.090 | −0.078 |
| | (0.002) | (0.015) | (0.017) | (0.016) | (0.017) | (0.004) | (0.015) | (0.039) | (0.038) | (0.038) |
| Anderson-Rubin $p$-value | | 0.887 | 0.623 | 0.750 | 0.790 | | 0.887 | 0.061 | 0.018 | 0.040 |
| Other controls | | | | | | | | | | |
| Incarceration rate | −0.034 | −0.030 | −0.031 | | −0.036 | −0.426 | −0.362 | −0.438 | | −0.434 |
| (state of residence, year $t$, men aged 18–24) | (0.042) | (0.052) | (0.044) | | (0.044) | (0.173) | (0.235) | (0.196) | | (0.196) |
| First stage | | | | | | | | | | |
| Incarceration rate (state of residence, year $t$) | | X | | X | X | | X | | X | X |
| Incarceration rate (state of birth, year $t-10$) | | | X | X | X | | | X | X | X |
| Predicted NM | | 1.143 | 1.202 | 1.137 | 1.140 | | 1.143 | 1.153 | 1.135 | 1.138 |
| | | (0.036) | (0.022) | (0.021) | (0.021) | | (0.036) | (0.060) | (0.059) | (0.059) |
| $F$-statistic for instrumental variable | | 983.41 | 3031.12 | 2892.18 | 2868.16 | | 983.41 | 372.84 | 374.23 | 370.41 |
| $\chi^2$-statistic for incarceration rate variables in probit | | 22.22 | 5.91 | 70.41 | 55.01 | | 22.22 | 12.84 | 27.17 | 19.52 |
| Observations | 197,129 | 196,762 | 189,735 | 189,424 | 189,424 | 152,073 | 196,762 | 111,278 | 111,196 | 111,196 |
| Mean of dependent variable | 0.06 | 0.06 | 0.06 | 0.06 | 0.06 | 0.07 | 0.06 | 0.06 | 0.06 | 0.06 |

Notes: See notes to Table 7. In all specifications, the covariates included are the same as in Table 7.

**Table 11**

*Robustness Checks from Regression Estimates for Whether Child Has Dropped Out of High School, Children Aged 15–17 Years Living with Their Mothers, by Race and Ethnicity*

| Instrument:<br>Independent variables | Black<br>OLS (1) | Black<br>Current IR<br>2SIV (2) | Black<br>Lagged IR<br>2SIV (3) | Hispanic<br>OLS (4) | Hispanic<br>Current IR<br>2SIV (5) | Hispanic<br>Lagged IR<br>2SIV (6) |
|---|---|---|---|---|---|---|
| A. Baseline specification (Table 7) | | | | | | |
| Mother never married | 0.015 | −0.003 | −0.009 | 0.032 | −0.030 | −0.082 |
| | (0.002) | (0.016) | (0.017) | (0.004) | (0.044) | (0.039) |
| Anderson-Rubin $p$-value | | 0.863 | 0.605 | | 0.485 | 0.031 |
| $F$-statistic for instrumental variable | | 2888.15 | 3078.73 | | 374.65 | 377.54 |
| B. Incarceration rate instrument calculated for men who live in the same state they did five years earlier | | | | | | |
| Mother never married | 0.015 | −0.001 | −0.001 | 0.032 | −0.026 | −0.083 |
| | (0.002) | (0.016) | (0.013) | (0.004) | (0.044) | (0.037) |
| Anderson-Rubin $p$-value | | 0.960 | 0.937 | | 0.545 | 0.024 |
| $F$-statistic for instrumental variable | | 3004.73 | 2394.28 | | 374.47 | 371.29 |
| C. Baseline sample plus institutionalized children, classified as having never-married mother | | | | | | |
| Mother never married | 0.042 | −0.003 | −0.009 | 0.062 | −0.030 | −0.082 |
| | (0.003) | (0.015) | (0.017) | (0.005) | (0.047) | (0.044) |
| Anderson-Rubin $p$-value | | 0.855 | 0.606 | | 0.494 | 0.023 |
| $F$-statistic for instrumental variable | | 1003.93 | 1366.18 | | 99.88 | 82.91 |
| D. Specification includes compulsory schooling, minimum wage, and white teen employment rate controls | | | | | | |
| Mother never married | 0.015 | −0.003 | −0.011 | 0.033 | −0.030 | −0.084 |
| | (0.002) | (0.016) | (0.017) | (0.004) | (0.043) | (0.038) |
| Anderson-Rubin $p$-value | | 0.862 | 0.495 | | 0.490 | 0.026 |
| $F$-statistic for instrumental variable | | 2891.49 | 3080.59 | | 374.89 | 376.86 |

Notes: See notes to Table 7. In all specifications, the covariates included are the same as in Table 7. In Panel C, mother-specific covariates are imputed for children not living with a mother, from a sample of children living with a never-married mother; we use means for age at birth, and modes for the other (discrete) variables. “IR” denotes the incarceration rate used as the instrument, with incarceration rate instruments defined in Table 7.

## VII. Discussion

In light of past research, the conclusion that partial correlations between never-married motherhood and child outcomes overstate the adverse effects of never-married motherhood is not surprising. However, evidence that children of never-married mothers may have better educational outcomes is likely to be regarded as surprising. One explanation for the latter finding 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, Kortenkamp, and Stagner (2003) suggesting that, for long-term welfare recipients, never-married mothers offer lower-risk family environments for their children.

Before turning to other evidence regarding this explanation, we consider the possibility that, rather than affecting never-married households, incarceration adversely affects the households of *ever*-married mothers, thus improving the *relative* circumstances faced by teenagers in never-married households. First, incarceration could remove adult males from ever-married households, negatively affecting teenagers in those households. To explore this, we estimated probit models for the effects of the incarceration rate on the probability that the teenage child of an ever-married mother has an adult male in the household (this is based on the Census measure that captures step- and adoptive fathers as well, which is what we want for addressing the issue of adult males in the household). We do this for the current (age-specific) incarceration rate measure, since that is the one that should be relevant to whether there is currently an adult male in the household.[32] The estimated coefficient on the incarceration rate is always negative (indicating a lower probability of an adult male present), but only significant at the 10 percent level for blacks, and about half the size and insignificant (*t*-statistic less than 0.5) for Hispanics. Given that we find stronger evidence of beneficial effects of never-married motherhood for Hispanics, changes in the composition of ever-married households cannot explain the results, as this would require that higher incarceration shifts the composition of ever-married Hispanic households more unfavorably.[33]

Turning back to the question of how incarceration affects never-married households, other evidence on low-income women supports the idea that they often face a low-quality pool of marriageable males. Waller and Swisher (2006) show that low-income women are more likely than other women to experience physical abuse

32. The control variables are the same as in Table 6.

33. Along similar lines, tighter marriage markets (more incarceration) may induce more women to "marry down," again adversely affecting the composition of ever-married households. A referee suggested that we test this by asking whether there is more divorce when incarceration is high, resulting from choosing a less compatible spouse. We therefore also estimated models of the effect of incarceration on the probability that ever-married women are divorced. In this case, it is not entirely clear whether we should prefer the current or lagged incarceration rate. Regardless, the evidence points to some negative effect on divorce among black women, but no effect (estimates near zero and insignificant) for Hispanic women. Once again, then, the pattern does not fit; if this were driving our results, it would be because higher incarceration induced more "marrying down," and hence more divorce, among Hispanics.

within their relationships with men—abuse that is likely to extend to children as well. Aside from physical and substance abuse, Edin and Reed (2005) note that many potential fathers in low-income communities have other children, and therefore that the benefits of marriage may be less likely to accrue to the woman's children. Edin (2000) 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. She concludes 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).

Evidence also suggests that incarcerated fathers have characteristics that 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.[34] Of fathers in prison, 45 percent 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 percent of incarcerated fathers lived in a two-parent household before their prison admission. Many incarcerated fathers were admitted because of violent (42 percent) or drug trafficking offenses (16 percent), and nearly half the fathers in prison had a violent offense before their current admission. Incarcerated fathers also report high levels of drug use prior to admission to prison; more than half (57 percent) reported illicit drug use in the month prior to their admission to prison, and 85 percent reported ever using illicit drugs (52 percent for cocaine or crack). Incarcerated fathers reported relatively good employment levels before incarceration, but also dependence on illegal activity for some of their income; of fathers in prison, 73 percent report being employed in the month before their admission, but 27 percent relied upon illegal sources for 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.[35]

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. Although this could explain instrumental variables estimates that indicate no effect of never-married motherhood (that is, estimates that are diminished relative to OLS), it is less plausible as an explanation of positive effects of never-married motherhood from the instrumental variables estimation. Since many of our estimates indicate such positive effects, we are more inclined to the interpretation based on selection on spousal 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

34. The statistics in this paragraph come from Mumola (2000).
35. A corollary that is suggested by this discussion and our interpretation of the estimates is that higher incarceration should reduce some of the problems experienced by women and children at the hands of men, such as domestic violence. We are not aware of any research on this topic.

consistent with evidence that women with nonmarital births have worse marriage partners if they do get married. Qian, Lichter, and Mellott (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.[36]

## VIII. Conclusions

Public policies that provide incentives or support for traditional, two-parent marriages are based on the view that better outcomes of children of such marriages reflect causal effects of family structure. In this paper, we identify the causal effect of being raised by a never-married mother on whether black and Hispanic teenage children drop out of school, 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 effect on whether children drop out of high school, and for Hispanic women and their children the evidence is more consistent with the conclusion that children may be better off living with a never-married mother.

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 low socioeconomic status.[37] Therefore, our instrumental variables estimates reflect the outcomes of the children of these women. These children are particularly vulnerable to a host of negative influences with regard to education, labor market experiences, criminal behavior, and family lives. Proponents of marriage-promotion policies view marriage as a crucial step in reducing these negative influences. But our results demonstrate that marriage, in itself, does not necessarily improve outcomes for children in households with low socioeconomic status, and even suggest that marriage-promotion policies that ignore the background of potential spouses 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. Al-

36. These benefits might include conformity with norms or with religious beliefs. This could explain the stronger evidence of beneficial effects of never-married motherhood for Hispanics than for blacks (for those for whom we identify the effect). Hispanics are predominantly Catholic, and, more generally, appear to exhibit stronger norms for childbearing within marriage than do blacks. As a consequence, higher incarceration rates may do more to reduce marriage to low-quality spouses among Hispanic women than among black women.

37. Some of the studies discussed in the previous section 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 Families and Child Wellbeing Study points to an 11.7 percent rate of incarceration of fathers within 18 months of a child's birth, and a 30.2 percent rate of incarceration prior to the birth. (See also Edin 2000; Edin and Reed 2005.)

though much of this literature still often finds beneficial effects of two-parent families, there is some evidence that the findings are reversed for very low socioeconomic status populations such as long-term welfare recipients.

It is also important to note 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 in the Healthy Marriage Initiative.[38] 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 about the effects of marriage-promotion policies on children. But if marriage-promotion policies lead to higher-quality 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.

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38. See http://www.acf.hhs.gov/healthymarriage, accessed on September 28, 2007.

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