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EFFECTS OF FAMILY STRUCTURE AND INCOME ON THE RISK OF A PREMARITAL BIRTH

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EFFECTS OF FAMILY INSTABILITY, INCOME, AND INCOME INSTABILITY ON THE RISK OF A PREMARITAL BIRTH

ABSTRACT

In previous work, my colleagues and I reported (1) a strong and statistically significant association between frequent change in family situation and the risk that a young woman bears her first child out of wedlock, and (2) weak and statistically insignificant associations between measures of a young woman's exposure to a mother-only family during childhood and adolescence and premarital birth risks. A serious limitation of these findings is that they lacked controls for income due to limitations in the retrospective data analyzed. Not controlling for income is especially problematic since the association between frequent change in family situation and premarital birth risks could be an artifact of change in economic circumstance that accompanies change in family situation. This paper uses prospective income histories and retrospective parent histories in the National Longitudinal Survey of Youth to determine if effects of family instability are an artifact of poor, unstable, or uncertain economic circumstances of families. The findings indicate significant effects of family instability, income level, and change in income on premarital birth risks. Estimated effects of family instability and income appear to be independent of one another.

Despite widespread agreement that poverty is a critical component of out-of-wedlock childbearing, strikingly little empirical research exists on the effects of income on the risk that a woman bears a child out of wedlock. This gap in the empirical literature is particularly surprising when juxtaposed against the large body of empirical research documenting the effects of family structure on premarital birth risks.

In previous work using data from the National Survey of Families and Households, my colleagues and I found (1) a strong and statistically significant association between frequent change in family situation and the risk that a woman bears her first child out of wedlock, and (2) weak and statistically insignificant associations between premarital birth risks and measures of a woman's family situation during adolescence or exposure to a mother-only family during childhood and adolescence (Wu and Martinson 1993). This work provided tests of competing hypotheses on the effects of family structure but did not control for income due to limitations in the retrospective data. Not controlling for income is especially troubling given our findings since the association between frequent change in family situation and premarital birth risks could be an artifact of changes in economic conditions that accompany change in family situation.

This paper uses prospective income and retrospective parent histories in the National Longitudinal Survey of Youth to determine if the effect of family instability in our earlier work is an artifact of not controlling for low, unstable, or changing income. I find that family instability, income, and income instability have strong and significant effects on the risk of a premarital birth. Moreover, estimates of income and family instability are unaffected by controls for one another, which suggests that economic and family processes have essentially independent effects on premarital birth risks.

THEORY

A large body of empirical literature has documented effects of family structure on the risk of a premarital birth (see, e.g., Garfinkel and McLanahan 1986). Researchers within this literature typically identify three hypotheses on the effects of family structure: (1) a *socialization hypothesis*—that women who grow up in a mother-only family are socialized in ways that result in higher premarital birth risks (Hetherington 1972; Kellam, Ensminger, and Turner 1977; McLanahan 1988; McLanahan and Bumpass 1988); (2) an *adolescent social control hypothesis*—that the supervision of adolescents is more difficult in single-parent families than in two-parent families (Dornbusch et al. 1985; Hogan and Kitagawa 1985; McLanahan and Bumpass 1988; Matsueda and Heimer 1987; Thomson, McLanahan, and Curtin 1992); and (3) an *instability and change hypothesis*—that a premarital birth is a response to the stresses accompanying instability in a woman's family situation (Cherlin et al. 1991; Hetherington 1987; Hetherington, Camara, and Featherman 1983; McLanahan 1985; Rutter 1983; Wallerstein and Blakeslee 1989).

The three hypotheses on family structure imply quite different outcomes (Wu and Martinson 1993). For example, consider two young women at risk of a premarital birth, where one has lived all her life with both biological parents and the other has lived all her life in a mother-only family, but who are otherwise raised in similar socioeconomic environments. The instability and change hypothesis will imply, *ceteris paribus,* that the young woman who lives all her life in a supportive mother-only family will not have higher risks of a premarital birth than the young woman who lives in an intact family all her life; by contrast, socialization and social control hypotheses will imply, *ceteris paribus,* higher risks for the young woman raised in the single-mother family. Similarly, consider two different young women born into intact families and suppose that the first woman has experienced a recent marital disruption and is currently living in a single-mother family while the second has experienced two disruptions, the divorce of her biological parents and a subsequent remarriage of her biological mother. *Ceteris paribus,* the socialization hypothesis implies low risks for both women; by contrast, the social control hypothesis implies higher risks for the woman currently living in a single-mother family than for the woman living in the stepfamily, while the change and instability hypothesis implies the opposite.

Although the effect of family structure on premarital birth risks has been the subject of intensive empirical study, researchers have paid far less attention to arguments regarding *economic deprivation*—that young women from disadvantaged economic backgrounds are more likely to bear a child out of wedlock than young women from more advantaged economic backgrounds. I review three such hypotheses: a *low income* hypothesis, a *permanent and transitory income* hypothesis, and an *income level and change* hypothesis.

The first hypothesis reflects standard concerns about the effect of poverty on premarital birth risks. Some researchers have argued that parents with limited economic means may be less able to invest or may make less intensive investments in the welfare of their children than parents with greater economic means (Becker 1991; Waite and Spitze 1981). Similarly, adolescents and young adults raised in poverty face different structures of economic opportunity than those from more economically advantaged backgrounds; these differences may in turn explain marked differences by social background in marriage and fertility (Jencks 1992; Wilson 1987). Alternatively, low family income may proxy social conditions in low income neighborhoods or in poor labor markets. In such areas, poor job prospects or depressed economic sectors may lower marital prospects for women and hence increase the duration of exposure to outcomes such as a premarital birth (Anderson 1990; Wilson 1987). Finally, the empirical data suggest that unwed mothers, particularly those with limited educational attainment, face substantially higher risks of falling into poverty (Bane and Ellwood 1986); moreover, single mothers and their children have fared particularly poorly in economic terms during the recent decades (Duncan and Hoffman 1991). Taken together, the increasing incidence of poverty among unwed mothers and their children has led many researchers and policymakers to identify poverty as a key factor driving the reproduction of single motherhood across generations.

The arguments given above identify various mechanisms by which low income might be associated with higher risks of a premarital birth. However, the emphasis on income *levels* common to these arguments neglects dynamic aspects of family income, in particular, unanticipated changes in income or trends in family income over time. Instability in income and downward shifts in income are especially interesting since the association between frequent change in family situation and premarital birth risks could be an artifact of changing economic circumstances that occur contemporaneously with shifts in family structure.

Arguments about anticipated versus unanticipated income fluctuations derive from the economic literature on permanent and transitory income effects (Friedman 1957; Deaton 1992). In this literature, an unmarried pregnant woman is assumed to weigh the advantages of an out-of-wedlock birth against the opportunity costs of such a birth. For many women, opportunity costs outweigh the benefits and some other alternative (marriage or abortion) is chosen. For other women, highly unstable economic circumstances in the family of origin may introduce considerable uncertainty into assessments of opportunity costs. If under conditions of high uncertainty, rational economic actors prefer known outcomes over the short-term (advantages of parenthood) to uncertain outcomes over the long-run (schooling, marital, job, or career opportunities), then the greater the variability of income in the family of origin, the greater the risk of a premarital birth.

Empirically, researchers have estimated permanent income and transitory income effects by specifying models in which individual- or aggregate-level economic outcomes are regressed on measures of income level and income variability. Because measures of variability weigh positive and negative income deviations equally, an implicit assumption is that positive income deviations have identical effects to negative deviations of the same magnitude. Such an assumption is plausible when the economic decision-maker is the wage-earner and hence exercises control over, and can thus reasonably forecast, his or her "permanent" income. It is less plausible when examining the effects of income in the adolescent woman's family of origin on the risk that the woman bears her first child out of wedlock. Because of this, the direction of change in income (e.g., first derivative of the local time-series of family income) may be a more important predictor of premarital birth risks than income uncertainty if *declining*, rather than *uncertain*, economic circumstances in the family of origin increase the likelihood that young women will prefer known short-term outcomes (bearing a child out of wedlock) to increasingly unattainable alternatives (attractive schooling, job, or career opportunities).

Distinguishing between these three income hypotheses is important on theoretical, empirical, and policy grounds. Theoretically, distinguishing between alternative income mechanisms may yield greater theoretical insight into economic dimensions of the premarital birth process. Empirically, large shifts in income often accompany change in family structure; hence, disentangling the effects of income shifts from those of family change is essential to an empirical characterization of the premarital birth process. Finally, policies intended to influence marital or fertility decisions of young adults differ greatly from policies intended to raise or stabilize family incomes; hence, disentangling the effects of income shifts from those of family change has important policy as well as empirical ramifications.

DATA AND METHODS

I use data from the 1979–89 panels of the National Longitudinal Survey of Youth (NLSY), a prospective survey consisting of a nationally representative random sample of young adults aged 14–21 in 1979. The NLSY contains important longitudinal data on family structure and income, which are crucial to testing hypotheses on family structure and economic deprivation.

The NLSY contains 12,686 respondents initially interviewed in 1979, consisting of a main sample of 6,111 individuals, an oversample of 5,295 minorities and poor whites, and a military sample of 1,280 Armed Forces personnel. Since 1979, yearly data on income and household composition have been gathered, and event history data on a respondent's parental, fertility, marital, and homeleaving histories were obtained. Sample attrition has been low, with 84% of the sample in 1979 successfully reinterviewed in 1989, corresponding to an average annual retention rate of 98%.

Models. Data on the outcome—age (in months) at a first premarital birth—were constructed from fertility and marriage histories. Proportional hazard models were used to analyze the age-specific premarital birth rate. I censored women who married without (or before) giving birth at the age of first marriage and women who had not married or who had not given birth at the age of last interview. I lagged all time-varying covariates by 12 months to better capture the social and economic circumstances of a young woman at the time of conception rather than at the time of first birth.[1]

Family structure measures. To construct measures of family situation, I used data from a retrospective parent history gathered in the 1987 NLSY interview. The parent history provides data from birth to age 18+ on whether the respondent lived with biological parent(s), stepparent(s), adoptive parent(s), or in some other situation (e.g., with grandparents or other relatives). I merged these data with a homeleaving history constructed from an item in the parent history and from the annual household rosters to determine the family situations of respondents before they left home. Table 1 lists the 23 types of family situation observed in these data.

[Table 1 about here.]

[1]The choice of a 12-month lag is clearly arbitrary; however, specifying alternative lags of 9, 16, or 20 months does not affect results substantially. Results obtained from alternative lags are available upon request.

Income and Family Effects on Premarital Births

The theoretical discussion given above identifies three broad dimensions of family experience: the duration of exposure to a mother-only family, family situation during adolescence, and frequent change in family situation. For example, the adolescent social control hypothesis contends that a woman's risk of a premarital birth during the adolescent years is (1) influenced by her current, not prior, family situation, and (2) varies with the number and types of adults currently present in the adolescent woman's household. I operationalized these ideas by creating four age-varying dummy variables during the adolescent years for respondents in intact families, mother-only families, families with one biological parent and one stepparent, and all other types of families. Note that these variables are identical to those used by other researchers except that I use age-varying measures instead of a static snapshot measure and that these measures are allowed to affect the risk of a premarital birth only between ages 14 and 18 (i.e., ages 168–227 months).

The socialization hypothesis is operationalized using three measures of exposure to a mother-only family. The first measure is a dummy variable coded 1 if the respondent was born into a mother-only family and 0 otherwise. The second measure is a dummy variable coded 1 if the respondent spent at least 75 percent of the first six years of life in a mother-only family and 0 otherwise. The third measure examines exposure to a mother-only family at all ages using an age-varying dummy variable coded 1 at age t if the respondent spent 75 percent or more of life through age t in a mother-only family and 0 otherwise. Because the parent calendar gathers data to the nearest year, the second measure contrasts those who spent 5 or 6 years of early life in a mother-only family with those who spent 4 years or less in a mother-only family.

To examine the instability and change hypothesis, I coded an age-varying variable for the number of changes in family situation experienced by age t. This variable will capture certain changes (e.g., the change from living with a biological mother and stepfather to living with a biological father and stepmother) that will not be reflected in other age-varying family variables.

Some measures of family situation will vary with age while others will not. The first and second measures of exposure to a mother-only family refer to experiences during early childhood and are thus fixed by the ages examined in these analyses. The third measure of exposure and the measure of change in family situation may change value while the respondent is in the parental home, but become fixed once the respondent first leaves the parental home. Finally, because the scope of social control hypothesis refers to the period of adolescence, the four dummy variables for adolescent family situation are allowed to vary between ages 11 and 18 (ages 132–227 months), but are constrained to have no effect after age 19 (228+ months).

Income measures. The income measures are derived from two parallel income histories: one giving total family income adjusted for family size (hereafter "adjusted income") between 1979–88 and another giving the natural logarithm of adjusted income (ln(1 + adjusted income)) (hereafter "log adjusted income") between 1979–88. I adjusted income for family size by dividing reported total family income in constant 1979 dollars from all sources by the square root of family size (Buhmann et al. 1988; Sandefur, McLanahan, and Wojtkiewicz 1992). A slight complication is that the income data gathered in the NLSY questionnaire refer to income earned in the previous calendar year. Hence, income reported in 1980 is adjusted by family size in 1979, as determined from the 1979 household roster. Given data for the 1979–89 period, this yields annual data on adjusted income for the period 1979–88.

For many respondents, data on adjusted income are available for some years but missing for others. When this occurred, I imputed missing data by linear interpolation when nonmissing values of adjusted income were available at an earlier and later year. For example, if adjusted income equaled $10,000 in 1982 and $13,000 in 1985, but was missing in 1983 and 1984, I imputed values of $11,000 in 1983 and $12,000 in 1984. When data are missing at an end-point of the time-series, for example, at 1979 or 1988, I imputed values by regressing adjusted income on calendar time. If this last imputation method yielded negative value of adjusted income, I treated adjusted income in such years as missing by setting adjusted income to zero and setting a time-varying dummy variable for missing income equal to one.[2]

Tests of the three hypotheses on income effects require different income measures. For each hypothesis I created two measures, one based on adjusted income and the other on log adjusted income. To examine the low income hypothesis, I simply used the time-varying covariates of adjusted income or log adjusted income in year t for the years 1979–88.

I constructed measures of permanent and transitory income using a backward-looking three-year running mean and running standard deviation. For the years 1981–88, I defined permanent (log) income in year t as the average of (log) adjusted income in years t, $t-1$, and $t-2$; similarly, I defined transitory (log) income as the standard deviation of (log) adjusted income in those years. Under this definition, computing permanent and transitory income for 1979 and 1980 requires income data for 1978 and 1977, which are not available. For these years, I simply used the mean and standard deviation for the income data in 1979–81 as measures of permanent and transitory income, respectively. Note that this implies that the measure of permanent or transitory income will be the same for 1979–81.

I constructed measures of income level and change in a manner similar to the measures of permanent and transitory income, but using a backward-looking running line instead of a backward-looking running mean. For the years 1981–88, I regressed (log) adjusted income on the times t, $t-1$, and $t-2$. I then computed measures of income level for 1981–88 using predicted values from the backward-looking OLS

[2]The base samples of white and black women (see below) contain 3,716 cases and 10,438 person-years of data on adjusted income for 1979–88. Of the 10,438 person-years of income data, 272 person-years of income data (2.6%) are missing and 2,208 person-years (21.2%) are imputed. The univariate distributions of imputed and non-missing income data resemble one another closely and are available upon request.

running line and measures of change in income for 1981–88 using the estimated OLS slopes. For the years 1979 and 1980, I simply used the predicted values and slopes from regression of income in 1979–81 on time.

I also created time-varying dummy variables that indicate if the income measures were missing or imputed. For measures for the permanent and transitory income hypothesis and the income level and change hypothesis, I defined data as missing (imputed) in year t if data were missing (imputed) in any of the three years of data used to compute the measure. In all models reported below, when estimating effects of the income measures, I control for a time-varying dummy variable equal to one if the income data were subject to imputation. I also model missing data on income by either entering dummy variables for missing income data or deleting records for an individual in which missing data occur. (For additional details, see Appendix.)

Sample restrictions. Of the 6,283 women in the NLSY, I excluded from all analyses: (1) women in the military oversample (n = 456); (2) women in racial and ethnic groups other than whites and blacks (n = 1,666); (3) women with missing fertility, marriage, or parent histories (n = 382); and (4) women who did not know their mothers or who had missing data on two variables used to proxy the home environment (number of siblings and a variable defined by the sum of dummy variables for the presence of magazines, newspapers, or library cards while growing up, (n = 44).

I also imposed restriction on the sample to address two limitations of the income data. The first, most serious, concern is that the NLSY income items may measure different things over time, even after adjusting for family size. In particular, "total family income" will typically refer to the earnings of very different economic agents when a young woman resides in the parental household and after she has left the parental household. To deal with this difficulty, I restricted the sample to the period between age 11 and age at first homeleaving. This restriction excludes an additional 19 women and yields a base sample of 2,441 white women and 1,275 black women.

A second difficulty is that missing income data may not occur at random but instead may vary systematically with particular respondent characteristics. For example, since income data are missing prior to 1979 for all respondents and since the initial 1979 sample is composed of respondents aged 14–21, data on income during the period of adolescence is missing for many respondents, with missing data more prevalent for older respondents. But even after 1979, there may be systematic variation in income nonresponse; for example, some respondents may find it difficult to answer questions about family income following a recent change in family situation. To address these concerns, I report analyses of a second sample that deletes missing income observations.[3] This restriction reduces the overall sample substantially by 1,438 women, yielding 1,471 white women and 766 black women.

Background Variables. All estimated models control for a common set of background variables: whether the respondent was raised in the Catholic faith; years of schooling completed by the respondent's mother; socioeconomic index (SEI) of the respondent's father (or adult male figure) when the respondent was age 14; dummy variables equal to one if the respondent's father (or adult male figure) was not working or was not present in the household when the respondent was age 14; an index defined by adding dummy variables equal to one if magazines, newspapers or library cards were available when the respondent was age 14; number of siblings; age of the respondent's mother at the time of her first birth; and the respondent's summary score on the Armed Forces Qualifying Test (AFQT) standardized for age. Mother's education, father's SEI, reading materials, and number of siblings control for the socioeconomic status of the respondent's family during adolescence. Mother's age at first birth provides a

[3] In these analyses, I only utilize spells for which income data are available. Multiple spells of nonmissing income data will yield left-truncated and right-censored spells for a respondent that are easily handled (see Appendix) under the assumption that the process generating spells of missing income is not informative to the process governing the outcome. Violations of this assumption would occur, for example, if an omitted third variable governed both missing income and premarital births.

crude control for the association between early childbearing by the respondent and her mother. Finally, all analyses include dummy variables for missing mother's education, missing father's SEI, missing mother's age at first birth, and missing AFQT. Table 2 presents descriptive statistics on the background variables for the two samples.

[Table 2 about here.]

RESULTS

Figure 1 presents nonparametric estimates of the logarithm of the premarital birth rate and smoothed curves passed through these estimates using a procedure described in Wu (1989). The dotted curve for black women is substantially higher than the solid curve for white women, indicating that at all ages black women have substantially higher premarital birth rates than white women.

[Figure 1 about here.]

Income and family instability. Table 3 contrasts the estimated effects of change in family situation with those of various income measures. Estimates obtained from the base sample of 2,441 white women and 1,275 black women are given in the rows labeled "1"; estimates obtained from the restricted sample of 1,471 white women and 766 black women in which missing income observations for a woman have been deleted are given in the rows labeled "2." Rows labeled "1" give estimates for the base sample while those labeled "2" give estimates for the restricted sample.

[Table 3 about here.]

The column labeled "0" presents zero-order effects, which give the estimated effect of a single variable (or effects of sets of related variables) after controlling for homeleaving and the background variables. Zero-order estimates for change in family situation are reported in the first four rows and first column of Table 3. Subsequent rows of estimates in the first column present zero-order estimates for logged and unlogged income measures for the low income, permanent and transitory income, and income level and change hypotheses. The remaining columns of Table 3 present estimates from models in which both effects of change in family situation and the income measures are estimated.

Is the association between change in family situation and the risk of a premarital birth an artifact of not controlling for income? Comparisons of the zero-order estimate for change in family situation with the corresponding estimates in Models 1–6 suggests that the answer is no. In the base sample, the estimated magnitude and significance of zero-order effects of change in family situation are nearly identical to estimates in Models 1–6 for both white and black women. Similarly, in the restricted sample, zero-order estimates are again close to those in Models 1–6, despite the greater sampling variability generated by the smaller samples.

Is the association between income and premarital birth risks an artifact of not controlling for change in family situation? The answer again is no. The estimated magnitude and significance of zero-order income effects closely resemble estimates in Models 1–6 for both white and black women in both the base and restricted samples.

These results show that estimated effects of the income measures and family instability change little when controlling for one another. Hence, *both* income and family instability appear to have largely independent effects on premarital birth risks.

Examining the effects of adjusted income and log adjusted income in more detail shows that, for white women, effects of both logged and unlogged measures are significant, with higher levels of (log) income adjusted for family size decreasing premarital birth risks, as hypothesized, in both the base and restricted samples. For black women, adjusted income and adjusted log income have negative effects on premarital birth risks, but effects are larger and more significant in the restricted sample in which missing income observations have been deleted.[4]

[4]Recall that all income measures are coded zero when data are missing. I chose such a coding

Income and Family Effects on Premarital Births

Estimates in Table 3 are consistent with predictions of the permanent income hypothesis, but provide mixed support for the transitory income hypothesis. As expected, higher permanent incomes are associated with lower premarital birth risks, with effects significant for logged and unlogged permanent income measures for both white and black women in both the base and restricted samples. For white women, greater variability in family income is associated with higher premarital birth risks, as expected, with effects again significant for both logged and unlogged measures in both the base and restricted samples. However, for black women, greater variability in family income is associated with *lower* premarital birth risks in all but one estimated equation; moreover, effects are large and significant in the restricted sample.

Recall that the transitory income hypothesis places emphasis on the effects of uncertainty caused by unanticipated fluctuations in income; hence, the greater the variability of actual income about expected "permanent" levels of income, the higher the risk of the risk of a premarital birth. An implicit assumption of this argument is that positive income deviations from expected permanent income levels will have identical effects as negative deviations of the same magnitude.

Aspects of this assumption are examined with the next set of income measures, which specify effects of the level of income and change in income. In particular, the level and change hypothesis posits that, net of expected permanent levels of income, upward trends in income lower premarital birth risks while downward trends increase risks. Estimates are generally consistent with predictions. Higher levels of income are associated with lower premarital birth risks for both white and black women in both the base and restricted samples, although effects for the log income measures only approach significance for seven of the eight equations estimated. Upward trends in income, net

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rather than recoding missing values to a specific subsample mean because of the different subsamples used in these analyses. Because of this coding, determining if premarital birth risks are identical for missing income and nonmissing income data requires testing if the difference between mean (log) income times the estimate for (log) income and the estimate for missing (log) income is significantly different from zero.

of income level, are associated with lower premarital birth risks, with effects significant for white and black women across all measures and samples examined.

Other family effects, white women. The analyses in Table 3 estimated effects of income and family instability on premarital birth risks but ignored the effects of other family variables. The analyses in Table 4 contrast effects of all family variables with effects of level and change effects in income for the base and restricted sample of white women. As before, the rows marked "1" and "2" report estimates obtained from the base and restricted samples, respectively.

[Table 4 about here.]

Zero-order effects for the three exposure measures are estimated from three equations, one for each measure, with controls for the background variables and homeleaving but no controls for the income measures or other family measures. The zero-order effects of all exposure measures have positive effects on premarital birth risks, as hypothesized; however, none of the effects is significant in either the base or restricted samples of white women.

When controls for the other family measures and income (Models 1–9) are added to these models, the effect of being born into a mother-only family and of prolonged exposure to a mother-only family between ages 0 and 5 remains positive but not significant in both the base and restricted samples of white women. The effect of prolonged exposure defined over all ages has a positive effect on premarital birth risks and is significant in the base sample but not in the restricted sample of white women.

Zero-order estimates for the effects of family situation during adolescence are estimated using a single equation that lets premarital birth risks at age t differ for women living in an intact, mother-only, step, or other family at age $t - 12$ months during ages 11–18 (132–227 months). The zero-order estimates suggest that living in a mother-only, step, or other family during adolescence increases premarital risks relative to living in an intact family; however, none of these effects is significant, although the zero-order effect of living in a stepfamily approach significance in both the base and restricted samples of white women.

When controls are added for income and the other family measures, the effects of living in a mother-only and other type of family change sign but remain not significant in both the base and restricted samples of white women. By contrast, the effect of living in a stepfamily is positive in Models 1–9 in the base sample but negative in the restricted sample. Although estimated effects of living in a stepfamily are not significant in either sample, differences in sign in the base and restricted samples could point to a source of nonresponse bias in income data, particularly if some respondents in stepfamilies do not know the earnings of a stepparent.

Estimated effects of change in family situation and the income measures are quite stable across models. The zero-order estimates and estimates in Models 1–9 of change in family situation are highly significant and in the expected direction in both the base and restricted samples of white women. Estimates in Models 1–9 are slightly larger than the zero-order estimates in both the base and restricted samples of white women. Estimates of the effect of income level are highly significant for the unlogged income measure, but only approach significance in seven of eight estimated equations for the logged income measure. Estimates of the effect of change in income are significant in all equations.

Other family effects, black women. Table 5 presents estimates for black women, with results qualitatively similar to those for white women. Estimates of the effect of frequent change in family situation are in the expected direction in all models, with estimates somewhat more significant in the base sample than in the restricted sample. Estimates of the effect of income level are in the expected direction for the unlogged income measure and are highly significant in both the base and restricted samples of black women; they remain in the expected direction for the logged income measure but are not significant in both the base and restricted samples of black women.

[Table 5 about here.]

The effects of the exposure variables for black women contrast with those for white women. Exposure to a mother-only family at birth and during ages 0–5 has positive and significant effects in Models 0–9 in both the base and restricted samples of black women, while exposure to a mother-only family defined over all ages has positive but not significant effects in Models 0–9 in both the base and restricted samples. These results differ qualitatively from those for white women: exposure to a mother-only family during early ages significantly increases the risk of a premarital birth for black but not white women, while exposure to a mother-only family defined over all ages significantly increases premarital birth risks for white but not black women.

Estimates of the effect of family situation during adolescence are not significant in most models. Estimates in Models 1–2, 4–5, and 7–8 suggest that net of the other variables, premarital birth risks do not differ significantly for women living in intact and the three non-intact types of families during adolescence in both the base and resticted samples of black women. Estimates in Models 3, 6, and 9 suggest higher premarital birth risks for women living in other types of family during adolescence relative to risks for women living in an intact family during adolescence; however, these models omit controls for exposure to a mother-only family during childhood.

DISCUSSION

The primary goal of this paper is to answer a simple question: Is the effect of frequent change in family situation on premarital birth risks found in previous research an artifact of not controlling for various measures of income. The answer to this question is no. Effects of frequent family change do not appear to be an artifact of not controlling for low income, changes in income, or unstable income in a woman's family of origin;

moreover, the magnitude of estimated effects does not change when controlling for income measures. These findings suggest that *both* income and family instability have strong, and largely independent, effects on the risk that a young woman bears her first child out of wedlock.

A secondary goal of this paper is to examine competing hypotheses concerning the effects of family structure and income on the risk of a premarital birth: (1) a *socialization hypothesis*—that women who grow up in a mother-only family during early childhood or adolescence are socialized in ways different from women who grow up in an intact family; (2) an *adolescent social control hypothesis*—that the supervision of adolescents is more difficult in single-parent families than in two-parent families; (3) an *instability and change hypothesis*—that a premarital birth is a response to the stresses accompanying frequent change in a woman's family situation; (4) a *low income hypothesis*—that premarital birth risks are higher because women from disadvantaged economic backgrounds possess fewer or less attractive economic opportunities; (5) a *permanent and transitory income hypothesis*—that uncertainties generated by unexpected fluctuations in family income raise premarital birth risks net of absolute income levels; and (6) an *income level and change hypothesis*—that downward trends in family income reflect worsening economic and social opportunities that increase premarital birth risks net of absolute income levels. Results for both white and black women are consistent with the family instability, low income, and income level and change hypotheses, provide qualified support for the socialization and permanent and transitory income hypotheses, and suggest at best limited support for the social control hypothesis.

In addressing the welfare of families and children in poverty, policymakers have historically looked to policies that might alleviate economic hardships for low income households; more recently, policymakers have increasingly considered initiatives intended to influence the marital and fertility behaviors of individuals in such households. The results of this paper provide good and bad news for both types of policy initiatives. For example, an unintended consequence of proposed "bridefare" initiatives (Wisconsin Department of Health and Social Services 1992; Wiseman 1993) might be to increase the likelihood of divorce since such initiatives provide marriage incentives to couples who would not ordinarily marry. Since the results of this paper suggest that *both* family instability and prolonged exposure to a mother-only family are associated with higher risks of a premarital birth, such policies could *increase* the reproduction of out-of-wedlock births across generations if the effects of instability dominated the effects of exposure, or *decrease* reproduction across generations if the effects of exposure dominated the effects of instability.

Because the findings in this paper suggest that low income and downward trends in incomes in the family of origin are associated with higher risks of a premarital birth, these results might appear to have clear implications for policies designed to increase incomes of families in poverty. Still, the nonexperimental nature of these data makes it difficult to conclude that such policies will necessarily lead to reductions in the incidence of out-of-wedlock births. Moreover, in practice there is substantial covariation between low income, family instability, and prolonged exposure to a mother-only family, which will complicate assessments of the effects of income policies on the reproduction of out-of-wedlock births across generations.

Appendix: Specification of parametric models.

The hazard rate used in all analyses is given by:

$$r(t) = q(t)\exp(b_1 x_{1i}(t) + \cdots),$$

where $\mathbf{x}_i(t)$ denotes a vector of covariates (possibly time-varying) for respondent i, and

$$\log q(t) = \begin{cases} \beta_1 + \delta_1 t & \text{ages 11–17.5 (132–209 months),} \\ \beta_2 + \delta_2 t, & \text{ages 17.5–20 (210–239 months,} \\ \beta_3 + \delta_3 t, & \text{ages 20+ (240+ months),} \end{cases}$$

with $\log q(t)$ subject to the following two spline constraints:

$$\beta_1 + \delta_1 \times 209.5 = \beta_2 + \delta_2 \times 209.5$$

$$\beta_2 + \delta_2 \times 239.5 = \beta_3 + \delta_3 \times 239.5$$

When data for case i are observed from the start of the process, case i contributes in the usual way to the log likelihood function

$$\ln L_i = -\int_0^t r_i(s)ds + (1 - c_i)\ln r_i(t),$$

where c_i is a censoring indicator equal to one if data for i are right-censored. When data for case i are observed sporadically, for example, if there are spells of missing data for a particular covariate, then the contribution of case i to the log likelihood is:

$$\ln L_i = -\sum_{k=1}^{K_i} \int_{\tau_k}^{\tau'_k} r_i(s)ds + (1 - c_i)\ln r_i(t)$$

where $(\tau_1, \tau'_1], (\tau_2, \tau'_2], \ldots, (\tau_{K_i}, \tau'_{K_i}]$ denote the spells in which data for case i are observed. Consistency of maximum-likelihood estimates follows under the assumption that the process generating observed spells is noninformative to the event process (Fleming and Harrington 1991).

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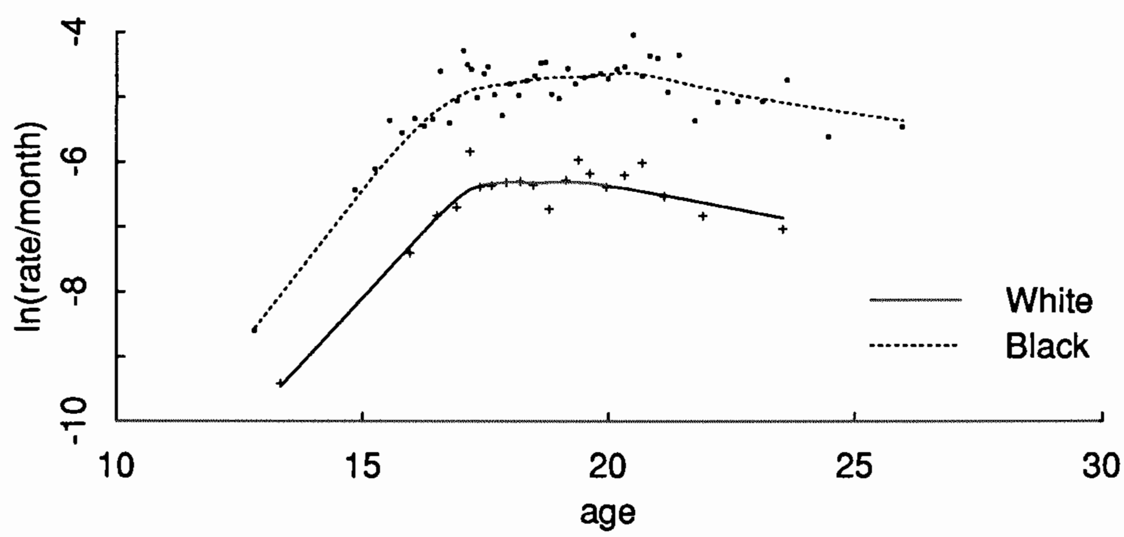

Figure 1: Smoothed nonparametric estimates of age dependence in the logarithm of the premarital birth rate for white women (n = 2,441) and black women (n = 1,275), National Longitudinal Survey of Youth, 1979–89.

Table 1: Parental situations of white and black women, National Longitudinal Survey of Youth, 1979–89.

Family types for NLSY respondents
1. both biological parents
2. biological father only
3. biological mother only
4. biological father and stepmother
5. biological father and adoptive mother
6. stepfather and biological mother
7. adoptive father and biological mother
8. two stepparents
9. stepfather only
10. stepmother only
11. two adoptive parents
12. adoptive father only
13. adoptive mother only
14. adoptive father and stepmother
15. grandparents
16. other relative
17. foster parents
18. friends
19. children's home
20. group home
21. detention center
22. other institution
23. other nonparent

Table 2: Means and standard deviations (in parentheses) for background variables, white and black women, base and restricted samples, National Longitudinal Survey of Youth, 1979–89.

	Whites		Blacks	
	base	restricted	base	restricted
Catholic	.34	.37	.07	.07
Mother's education	11.62 (2.51)	11.80 (2.55)	10.74 (2.63)	10.98 (2.55)
Mother's education missing	.04	.04	.07	.06
Father's SEI, age 14	36.80 (17.05)	37.58 (17.44)	24.94 (10.41)	25.10 (10.76)
Father did not work at age 14	.07	.07	.07	.07
No father present at age 14	.12	.12	.34	.32
Father's SEI missing	.07	.06	.10	.10
Reading materials	2.30 (.88)	2.38 (.83)	1.66 (1.05)	1.77 (1.03)
Number of siblings	3.27 (2.08)	3.13 (2.00)	4.85 (3.09)	4.55 (3.02)
Mother's age at birth of first child	22.15 (4.32)	22.25 (4.33)	20.19 (4.38)	20.32 (4.33)
Mother's age at first birth missing	.07	.06	.11	.10
AFQT, standardized for age	.35 (.84)	.45 (.83)	−.60 (.82)	−.45 (.78)
AFQT missing	.04	.03	.02	.01
sample size	2441	1275	1471	766

Table 3: Effects of change in family situation, income, and income instability on the risk of a premarital birth for white and black women, base and restricted samples, National Longitudinal Survey of Youth, 1979–89.

	0	1	2	3	4	5	6
Change in Family Situation							
1. whites	.33**	.33**	.33**	.32**	.33**	.33**	.33**
2. whites	.34**	.33**	.34**	.31**	.35**	.34**	.35**
1. blacks	.26**	.26**	.26**	.26**	.26**	.26**	.26**
2. blacks	.19*	.17*	.18*	.17*	.18*	.17*	.18*
Adjusted Income							
1. whites	−.76**	−.76**					
2. whites	−.88**	−.86**					
1. blacks	−.41*	−.41*					
2. blacks	−.71**	−.69**					
Log Adjusted Income							
1. whites	−1.75**		−1.81**				
2. whites	−2.02**		−1.99**				
1. blacks	−.97*		−.94*				
2. blacks	−1.57**		−1.56**				

Table 3: (continued)

	0	1	2	3	4	5	6
Permanent and Transitory Adjusted Income							
1. permanent, whites	−1.36**			−1.33**			
2. permanent, whites	−1.51**			−1.47**			
1. permanent, blacks	−.38*			−.38*			
2. permanent, blacks	−.79**			−.76**			
1. transitory, whites	1.81**			1.76**			
2. transitory, whites	1.74**			1.69**			
1. transitory, blacks	.02			−.01			
2. transitory, blacks	−.04			−.06			
Permanent and Transitory Log Adjusted Income							
1. permanent, whites	−3.36*				−3.47**		
2. permanent, whites	−3.85*				−4.01**		
1. permanent, blacks	−1.66*				−1.72*		
2. permanent, blacks	−3.81**				−3.73**		
1. transitory, whites	1.51				1.39		
2. transitory, whites	.94				.75		
1. transitory, blacks	−1.04				−1.14		
2. transitory, blacks	−2.99*				−2.94*		

Table 3: (continued)

	0	1	2	3	4	5	6
Level and Change in Adjusted Income							
1. level, whites	−.72**					−.71**	
2. level, whites	−.78**					−.77**	
1. level, blacks	−.32*					−.32*	
2. level, blacks	−.59**					−.57**	
1. change, whites	−.75*					−.76*	
2. change, whites	−.73*					−.75*	
1. change, blacks	−.88**					−.87**	
2. change, blacks	−.78*					−.78*	
Level and Change in Log Adjusted Income							
1. level, whites	−1.40*						−1.38†
2. level, whites	−1.30†						−1.30†
1. level, blacks	−.32						−.36†
2. level, blacks	−1.05†						−1.03†
1. change, whites	−3.01*						−3.07*
2. change, whites	−3.06*						−3.14*
1. change, blacks	−2.79**						−2.76**
2. change, blacks	−2.33*						−2.33*
Controls for Imputed and Missing Income							
1. imputed, whites		.03	.10	.36†	.34†	.30	.35†
2. imputed, whites		.05	.13	.33	.33	.29	.36†
1. imputed, blacks		−.14	−.12	−.16	−.16	−.14	−.12
2. imputed, blacks		−.06	−.03	−.10	−.11	−.07	−.06
1. missing, whites		−.80**	−1.78**	−.68*	−2.98*	−.63*	−1.26†
1. missing, blacks		−.22†	−.77*	−.24†	−1.49*	−.18	−.29

Note: Rows labeled "1" and "2" report estimates for the base and restricted samples, respectively. All models control for age, homeleaving, and background variables. Income measures are adjusted for family size. See text for additional information.

† $p < .10$ * $p < .05$ ** $p < .005$ (two-tailed test)

Table 4: Effects of family situation and income on the risk of a premarital birth for white women, base and restricted samples, National Longitudinal Survey of Youth, 1979–1989.

		no income			adjusted income			log adjusted income		
	0	1	2	3	4	5	6	7	8	9
Exposure to a mother-only family										
1. at birth	.04	.17	—	—	.14	—	—	.19	—	—
2. at birth	.12	.25	—	—	.16	—	—	.27	—	—
1. at least 75 percent of life, ages 0–5	.12	—	.29	—	—	.21	—	—	.25	—
2. at least 75 percent of life, ages 0–5	.18	—	.41	—	—	.36	—	—	.46	—
1. at least 75 percent of life, all ages	.34	—	—	.68*	—	—	.66*	—	—	.66*
2. at least 75 percent of life, all ages	.16	—	—	.51	—	—	.54	—	—	.56
Family situation during adolescence										
1. mother-only family	.12	−.34	−.35	−.42	−.37	−.37	−.45†	−.30	−.30	−.39
2. mother-only family	.15	−.14	−.15	−.17	−.23	−.24	−.27	−.05	−.06	−.10
1. stepfamily	.40†	−.24	−.25	−.26	−.24	−.24	−.26	−.22	−.22	−.23
2. stepfamily	.59†	.11	.09	.13	.11	.09	.12	.15	.14	.17
1. other type of family	.33	−.25	−.25	−.25	−.27	−.27	−.28	−.24	−.24	−.24
2. other type of family	−.07	−.58	−.60	−.57	−.66	−.69	−.67	−.58	−.62	−.59
Instability and change in family situation										
1. number of changes	.33**	.39**	.39**	.41**	.39**	.39**	.41**	.39**	.39**	.40**
2. number of changes	.34**	.37**	.38**	.39**	.39**	.40**	.40**	.39**	.40**	.40**

Table 4: (continued)

		no income			adjusted income			log adjusted income		
	0	1	2	3	4	5	6	7	8	9
Level and Change in Income										
1. level of adjusted income	−.72**	—	—	—	−.72**	−.72**	−.71**	—	—	—
2. level of adjusted income	−.78**	—	—	—	−.79**	−.80**	−.79**	—	—	—
1. change in adjusted income	−.75*	—	—	—	−.77*	−.77*	−.77*	—	—	—
2. change in adjusted income	−.73*	—	—	—	−.77*	−.77*	−.78*	—	—	—
1. level of log adjusted income	−1.40*	—	—	—	—	—	—	−1.38†	−1.38†	−1.41*
2. level of log adjusted income	−1.30*	—	—	—	—	—	—	−1.38†	−1.40†	−1.42†
1. change in log adjusted income	−3.01*	—	—	—	—	—	—	−3.00*	−2.99*	−2.93*
2. change in log adjusted income	−3.06*	—	—	—	—	—	—	−3.09*	−3.10*	−3.05*
1. imputed income	—	—	—	—	.28	.28	.29	.34†	.33†	.35†
2. imputed income	—	—	—	—	.28	.28	.29	.35†	.34†	.36†
1. missing income	—	—	—	—	−.65*	−.65*	−.64*	−1.28*	−1.28*	−1.30*

Note: See notes to Table 3.

Table 5: Effects of family situation and income on the risk of a premarital birth for black women, base and restricted samples, National Longitudinal Survey of Youth, 1979–1989.

		no income			adjusted income			log adjusted income		
	0	1	2	3	4	5	6	7	8	9
Exposure to a mother-only family										
1. at birth	.27*	.27*	—	—	.27*	—	—	.29*	—	—
2. at birth	.40*	.44*	—	—	.41*	—	—	.44*	—	—
1. at least 75 percent of life, ages 0–5	.25*	—	.31*	—	—	.31*	—	—	.32*	—
2. at least 75 percent of life, ages 0–5	.40*	—	.49**	—	—	.46*	—	—	.48**	—
1. at least 75 percent of life, all ages	−.09	—	—	.01	—	—	−.01	—	—	−.01
2. at least 75 percent of life, all ages	.13	—	—	.17	—	—	.11	—	—	.10
Family situation during adolescence										
1. mother-only family	.16	−.06	−.08	.01	−.08	−.10	.00	−.10	−.11	−.01
2. mother-only family	.23	.04	.02	.08	−.01	−.03	.04	−.01	−.03	.04
1. stepfamily	.19	−.22	−.24	−.11	−.23	−.25	−.12	−.23	−.25	−.12
2. stepfamily	−.36	−.74†	−.77†	−.61	−.72†	−.74†	−.58	−.74†	−.76†	−.59
1. other type of family	.70**	.31†	.31†	.41*	.31†	.30†	.41*	.30†	.30†	.40*
2. other type of family	.70*	.35	.34	.45	.35	.34	.46	.31	.30	.41
Instability and change in family situation										
1. number of changes	.26**	.24**	.26**	.22**	.24**	.26**	.22**	.25**	.26**	.23**
2. number of changes	.19*	.20*	.23*	.20*	.19*	.21*	.17†	.20*	.23*	.19*

Table 5: (continued)

		no income			adjusted income			log adjusted income		
	0	1	2	3	4	5	6	7	8	9
Level and Change in Income										
1. level of adjusted income	−.32*	—	—	—	−.30*	−.30*	−.32*	—	—	—
2. level of adjusted income	−.59**	—	—	—	−.51**	−.52**	−.54**	—	—	—
1. change in adjusted income	−.88**	—	—	—	−.89**	−.88**	−.87**	—	—	—
2. change in adjusted income	−.78*	—	—	—	−.82**	−.81**	−.79*	—	—	—
1. level of log adjusted income	−.32	—	—	—	—	—	—	−.18	−.21	−.27
2. level of log adjusted income	−1.05†	—	—	—	—	—	—	−.75	−.80	−.88
1. change in log adjusted income	−2.79**	—	—	—	—	—	—	−3.00**	−2.94**	−2.82**
2. change in log adjusted income	−2.33*	—	—	—	—	—	—	−2.57*	−2.47*	−2.32*
1. imputed income	—	—	—	—	−.15	−.28	−.15	−.13	−.13	−.13
2. imputed income	—	—	—	—	−.08	−.09	−.08	−.07	−.07	−.07
1. missing income	—	—	—	—	−.19	−.19	−.20	−.16	−.19	−.24

Note: See notes to Table 3.

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