WHAT DETERMINES FAMILY STRUCTURE?

DAVID M. BLAU and WILBERT VAN DER KLAAUW*

We use data from the 1979 cohort of the National Longitudinal Survey of Youth to estimate the effects of policy and labor market variables on the demographic behaviors that determine children's family structure experiences: union formation and dissolution, and fertility. Male and female wages have substantial effects on family structure for children of black and Hispanic mothers. The tax treatment of children also affects family structure. Welfare reform, welfare benefits, and unilateral divorce had much smaller effects on family structure for the children of this cohort of women. Trends in wages and tax rates explain only a small share of the observed changes in family structure in recent decades. (*JEL* J12)

I. INTRODUCTION

The most prevalent type of family structure in which children in the United States are raised today is the traditional one, in which both biological parents are present in the home and married. But in the past 30–40 years, it has become increasingly common for children to experience alternative family structures, such as living with the mother with no father present, the mother and a stepfather, and cohabiting parents. Children who grow up in a family with married biological parents have better education, employment, marriage, childbearing, and psychological outcomes on average than their counterparts who spend substantial parts of childhood living in alternative family structures.[1] These differences are generally quite large and dwarf the effects of income and maternal employment. The evidence suggests that at least part of the association between family structure and child outcomes is causal. There is much still to be learned about the consequences of growing up in alternative family structures, but there is a consensus that family structure matters for child development.

In contrast, there is much less known about the determinants of family structure. The proximate determinants of family structure are well-studied demographic behaviors: union formation and dissolution, transition from cohabitation to marriage, and fertility, both in and outside of unions. But the implications of adult demographic behaviors for the family structure experiences of children depend crucially on *interactions* among these behaviors. For example, the impact on a child of being born out of wedlock is likely to depend on whether the mother and biological father subsequently marry

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Blau: Department of Economics, Ohio State University, Columbus, OH 43210-1172. Phone 614-292-2009, Fax 614-292-3906, E-mail blau.12@osu.edu

van der Klaauw: Microeconomic and Regional Studies Function, Federal Reserve Bank of New York, New York, NY 10045. Phone 212-720-5916, Fax 212-720-1844, E-mail wilbert.vanderklaauw@ny.frb.org

1. See, for example, Aughinbaugh, Pierret, and Rothstein (2005), Chase-Lansdale, Cherlin, and Kiernan (1995), Gennetian (2005), Ginther and Pollak (2004), Hetherington and Stanley-Hagan (1999), Hofferth (2006), Lang and Zagorsky (2001), McLanahan and Sandefur (1994), and Sigle-Rushton, Hobcroft, and Kiernan (2005).

ABBREVIATIONS

AFDC: Aid to Families with Dependent Children
AFQT: Armed Forces Qualification Test
CPS: Current Population Survey
EITC: Earned Income Tax Credit
NBER: National Bureau of Economic Research
NLSY79: 1979 Cohort of the National Longitudinal Survey of Youth
PCED: Personal Consumption Expenditure Deflator
TANF: Temporary Assistance for Needy Families

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or cohabit, and if so, how soon after the birth of the child. The impact on a child of the dissolution of a union may depend on whether the man in the union was the child's biological father or a stepfather and on the duration of the union.

Economic theories of family formation and dissolution suggest a number of observable factors that affect the demographic behaviors that determine family structure.[2] These include the wage rates available to men and women; the tax and transfer incentives to cohabit, marry, and bear children; the legal environment governing divorce and child support provided by absent parents; and the state of the marriage market. Many studies have examined the effects of these factors on the family structure experiences of children, but most have taken a narrow approach. For example, a typical study examines the impact of changes over time in one or two determinants of family structure, without considering the implications of simultaneous changes in other factors. Most studies examine only one or two of the key demographic behaviors that determine the family structure experienced by children. For example, one study might focus only on entry to cohabitation and marriage, whereas another study examines childbearing while single, and a third study analyzes marital dissolution.

In this article, we propose a new approach to analyze the determinants of the family structure experiences of children. Our approach has four distinguishing features. First, we jointly model union formation, union dissolution, and childbearing decisions. Previous analyses have integrated some of these behaviors in a single model, but none has integrated the full range of behaviors needed for a thorough analysis of family structure. A major feature of change in recent years has been de-linking of marriage and childbearing decisions. Hence, it is crucial to recognize, as emphasized by Ellwood and Jencks (2004), that marriage and childbearing are in fact distinct decisions and that treating "single parenthood" as one decision rather than the consequence of related but distinct union and childbearing decisions misses key elements of changes in behavior. Furthermore, Bumpass and Lu (2000) point out that a substantial part of the increase in single parenthood in the last three decades can be accounted for by a rise in the presence of children with cohabiting parents, but child outcomes are worse in cohabitation than marriage, other things equal.[3] Thus, for the purpose of analyzing family structure, it is important to allow for a three-way classification of unions.

Second, we analyze the major hypothesized driving forces behind family structure changes jointly, including changes in public assistance policy, divorce law, tax law, and wage rates. By considering the main driving forces jointly rather than focusing on one or two in isolation from others, as in much of the literature, we provide a more robust accounting of the factors driving family structure changes.

Third, the analysis is dynamic and distinguishes between the short-run timing effects and the long-run "avoidance" effects of key driving forces. In some cases, the major changes have been in the timing of childbearing and marriage, whereas for others, the most important aspect of change has been more radical, namely avoiding marriage or childbearing altogether (Ellwood and Jencks 2004). Most empirical analyses do not come to grips with this issue: they are either explicitly focused on outcomes at certain ages (e.g., marriage by age 24 or nonmarital childbearing by age 19) or they look at marital and childbearing transitions over short periods. Exceptions to this generalization include studies by Keane and Wolpin (2010), Seitz (2009), Swann (2005), Tartari (2006), and van der Klaauw (1996). These studies structurally estimate dynamic economic models of marriage and employment (and in some cases fertility and welfare participation). With the exception of Tartari, these studies do not focus on family structure from the perspective of children, so they do not model cohabitation or the identity of male partners, which are important features of our model.

Fourth, and perhaps most important, we model the behavior of the adults who make union and childbearing decisions, but we derive from the model the consequences of these decisions for the family structure experienced by children. Thus, we model choices that determine the *identity* of men who are in the mother's household from the perspective of children: step or biological father. This approach is unique in the literature on family structure changes. This is important because there is considerable evidence

2. See Akerlof, Yellin, and Katz (1996), Becker (1981), Neal (2004), and Willis (1999). Other theories emphasize less easily observed factors: see Ellwood and Jencks (2004).

3. See DeLeire and Kalil (2002), Hofferth (2006), and Thomson et al. (1994).

that living with the biological father is associated with better child outcomes compared to living with a stepfather, other things equal (e.g., Hofferth 2006; McLanahan and Sandefur 1994).

We use data from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY79) to analyze the fertility, union formation, union dissolution, type of union (cohabiting vs. married), and father identity (biological vs. step) choices of women born from 1957 to 1964. We follow these women from the 1970s, as they enter adolescence, through 2004, when they are in their 40s. We analyze the effects of state-year-specific policy and labor market variables over a three-decade period, allowing the effects of these variables to differ for whites, blacks, and Hispanics, in recognition of the important differences in levels and trends for these groups. We exploit both cross-state and within-state variation over time to identify the effects of these contextual variables, and we examine the sensitivity of the results to the source of variation. A limitation of using a narrow range of birth cohorts is that we do not have independent variation in age and calendar time. For example, welfare reform occurred in the 1990s, when the NLSY79 cohort was well past the teenage years, so our approach cannot provide a credible estimate of the impact of welfare reform on the behavior of teenagers. But the richness and long duration of the NLSY79 data provide information that is not available from other sources.

The econometric model we specify can be interpreted as an approximation to the decision rules implied by a dynamic economic model that fully specifies preferences, the budget constraint, and the expectation formation process. Although computationally less demanding, the nonstructural approach used here does not provide a precise interpretation of the parameters: they are combinations of parameters describing preferences, budget constraints, and expectations. In our analysis, we do not condition on other potentially jointly chosen determinants of family structure, such as education, employment, child support, and welfare enrollment, that may be endogenous. Structural estimation of a fully specified economic model of family structure that would also include these additional determinants as choice variables is an important task for future research.[4]

The results indicate that the wage rates available to men and women have substantial effects on family structure for children of black and Hispanic mothers but not for whites. A higher female wage rate increases the proportion of childhood spent living with no father and reduces time spent living with the married biological father. A higher male wage rate decreases the proportion of childhood spent living with no father. For Hispanics, this is accompanied by an increase in time spent with the married biological father. For blacks, there is an increase in cohabitation but not in marriage, and time spent in cohabitation increases by about the same proportion for the biological and stepfathers. These effects are all consistent with standard economic models of the family. Changes in tax rates also affected family structure, while welfare benefits, welfare reform, and unilateral divorce laws are estimated to have had small effects. We use longitudinal data on a narrow range of birth cohorts, so it is difficult to make credible inferences from our estimates about the causes of cohort trends in family structure. Nevertheless, we use our model to simulate the effects of observed changes in the contextual variables from the 1970s to the 2000s compared to the counterfactual of no change in these variables since the 1970s. The results indicate that the observed changes in policy and labor market variables over this period should have caused an increase in the proportion of childhood lived with the biological father and a decline in time spent with no father. Because the observed trends in family structure were in the opposite direction, we conclude that trends in wages and the policy variables cannot explain the trend away from traditional family structure.

We provide background and a brief review of previous findings in Section II. Section III specifies the model and econometric approach. Section IV describes the data. Section V presents the main results. Alternative specifications are discussed in Section VI, and Section VII concludes the study.

II. BACKGROUND AND PREVIOUS FINDINGS

The changes in family structure that are of interest here have been the result of a

4. This nonstructural approach is in the tradition of the "heterogeneity versus state dependence" literature (Heckman, 1981), in which the cause of state dependence and other sources of dynamics are not explicitly modeled, but a rich dynamic specification can be estimated. Our specification, described below, includes measures of union duration, duration single, ages of the oldest and youngest children, the cumulative number of cohabitations, and other variables determined by previous choices.

decline in marriage, increases in divorce and cohabitation, and an increase in childbearing outside of marriage. These changes are well known and have been discussed extensively by Bumpass and Lu (2000), Bumpass, Sweet, and Cherlin (1991), Cherlin (1999), Fields and Casper (2001), Martin et al. (2002), and Stevenson and Wolfers (2007), among others. Here, we discuss their consequences for the family structure experiences of children and briefly summarize previous findings on the causes of the changes.

Kreider (2008) summarizes recent family structure patterns of children using data from the Survey of Income and Program Participation. In 2004, 58% of children under the age of 18 were living with their married biological parents. Another 3% were living with their cohabiting biological parents. Eight percent of children were living with one biological parent and one step or adoptive parent (in 80% of these cases, the biological parent was the mother). Twenty-six percent were living with one parent only (in 88% of these cases, the parent was the mother). Finally, 4% were living with neither parent. For most of the twentieth century up to 1970, the percentage of children living in a two-parent family remained stable at 83%–85%. Between 1970 and 1990, the percentage in two-parent families fell from 85% to 73% and the percentage in one parent families rose from 13% to 25%, with little further change since 1990. Family structure patterns and their changes vary substantially by race and, to a lesser extent, by ethnicity. In 2004, 67% of non-Hispanic white children lived with both biological parents compared with 31% of non-Hispanic black children and 61% of Hispanic children.[5]

Economic theories of the determinants of union formation, union dissolution, and childbearing behavior emphasize the role of the wage rates available to men and women; the tax treatment of marriage and children; the generosity and terms of public assistance to low-income families with children; and the legal environment governing divorce.[6] We briefly discuss findings from the literature on each of these factors.

A. *Wage Rates*

Becker's (1981) theory of marriage implies that the difference in potential wage rates between men and women affects the gains from specialization within marriage. The higher a woman's wage rate, the greater is the opportunity cost of staying home and raising children. The higher a potential husband's wage rate relative to the woman's wage rate, the greater is the incentive to marry in order to realize gains from specialization within marriage. A number of studies have found a negative effect of male wages and a positive effect of female wages on the prevalence of female headship. However, trends in wages do not contribute much to explaining the trend in single headship during the 1970–1990s.[7] The effect of wage rates on fertility has also been studied; see Francesconi (2002) and references cited therein.

B. *Taxes*

It has been argued by Hotz and Scholz (2003) that expansion of the earned income tax credit (EITC) in the 1980s and 1990s caused an increase in the marriage tax penalty. However, there is little empirical evidence that the EITC has influenced marriage decisions.[8]

C. *Welfare*

Moffitt (1998) reviewed the large literature on the effect of welfare benefits on family

5. Non-Hispanic will be implicit henceforth when referring to whites and blacks.

6. Many studies have examined the impact of abortion legalization and the availability of oral contraceptives on demographic behavior. We do not focus on these factors because both the legalization of abortion and the diffusion of easy access to oral contraceptives were completed by the early 1970s, before the women in our sample began childbearing and union formation. Other studies have analyzed the effects of marriage markets (i.e., the sex ratio) and the legal environment governing enforcement of child support obligations. In an earlier version of the article, we reported estimates of a specification that included measures of the sex ratio and child support enforcement. The effects of these variables were generally small and insignificantly different from zero. We dropped them from the model in order to focus on the contextual variables that appear to be more important.

7. See, for example, Blau, Kahn, and Waldfogel (2000), Fitzgerald and Ribar (2004), Bitler et al. (2004), and Moffitt (2001). Other features of the labor market in addition to wages may affect demographic behavior as well. We investigated the effects of the unemployment rate, but dropped it from the model after finding no evidence of any effects on the behaviors of interest.

8. See Dickert-Conlin and Houser (1998) and Ellwood (2000). There is no evidence on whether the EITC has influenced fertility. Other features of the tax code that affect marriage and childbearing incentives have also been analyzed, with results generally suggesting small effects in the expected direction (see Alm and Whittington 2003).

behavior and concluded that there is evidence of a positive association between welfare benefits and female headship. However, the magnitude and precision of the estimated effect are rather sensitive to specification. Furthermore, the trend in real welfare benefits in the 1980s and 1990s was downward, which should have led to a decline in female headship rather than the increase that was observed. Some recent studies, such as those by Rosenzweig (1999), Foster and Hoffman (2001), and Hoffman and Foster (2000), have found more consistent evidence of a positive association between welfare benefits and female headship among disadvantaged young women, for whom welfare is likely to be a relevant option. Blau, Kahn, and Waldfogel (2000) find no evidence that welfare benefits affect the likelihood that a young woman is a single mother. Light and Omori (2006) find that an increase in welfare benefits causes a reduction in transitions into marriage and an increase in transitions to cohabitation. They also report that an increase in the welfare benefit increases divorce for black women but not for other groups.

A recent literature examines the impact of welfare reform in the late 1980s to the mid-1990s on family structure. The majority of studies find that welfare reform caused an increase in marriage and a decrease in divorce.[9] However, social experiments undertaken as part of welfare reform show no consistent impact on union formation in the welfare population (Harknett and Gennetian 2003), and there is evidence from the studies by Bitler et al. (2004) and Kaestner et al. (2003) that welfare reform actually caused a decrease in marriage. Fitzgerald and Ribar (2004) find no significant impact of welfare reform on female headship.

D. Divorce Laws

Many studies have analyzed the impact of enactment of unilateral divorce laws on the divorce rate and related outcomes. Peters (1986) finds no impact, but Friedberg (1998), Gruber (2004), and others find a positive association between unilateral divorce law and the frequency of divorce. Wolfers (2006) reconciles these differences by showing that there is a positive short-run impact of enactment of unilateral divorce but apparently no long-run impact. This finding suggests the importance of dynamic considerations. Alesina and Giuliano (2005) find evidence that unilateral divorce reduces out of wedlock fertility, with no impact on marital fertility. They interpret this as indicating that when it is easier to escape marriage, women who plan to have a child are more willing to have the child within marriage.

III. MODEL

Our goal is to understand the family structure experiences of children who reside with their biological mother.[10] The family structures of interest are living with the biological mother and (1) the married biological father, (2) the cohabiting biological father, (3) a married stepfather, (4) a cohabiting stepfather, and (5) no man. We assume that women become at risk of entering a union and conceiving a child at age 12. A "union" refers to a coresidential romantic relationship, which may be a marriage or a cohabitation. We use a discrete-time framework in which the unit of time is a month. In a given month (t), woman i's situation is characterized by the following state variables: (a) a set of fixed characteristics X_i such as her race, ethnicity, and year of birth; (b) the outcomes of previous childbearing and union formation and dissolution decisions, Y_{it}, such as the number of children born and their ages, current marital and cohabitation status, and marital and cohabitation history; and (c) a set of policy, labor market, and other aggregate variables Z_{ijt}, some of which may be choice specific (j is the indicator for choices, defined below). We do not model schooling and employment decisions, and, as noted in the introduction, we do not condition on education and employment status. We also do not model migration behavior, but we do condition on the woman's state of residence.

Each period, a woman faces a set of childbearing and union options, from which she can choose one. We assume that at most one alternative can be selected from the choice set in a given month. The set of alternatives available to a woman in a given period depends on her previous choices. For example, if she is currently

9. See Acs and Nelson (2004), Bitler et al. (2006), and Gennetian and Miller (2004).

10. The NLSY has little information on children who do not live with the biological mother. Also, we do not distinguish among living arrangements by the presence of grandparents or other nonparental adults because the model would have to be much more complex in order to do so. See Bitler et al. (2006) and DeLeire and Kalil (2002) for analyses of the presence of grandparents.

married, then the option of entering a marriage or cohabitation is not available. If she is currently pregnant, then conceiving a child is not an option.[11] We assume that if she is in a cohabitation, then the only man she can marry in the current month is her partner. We also assume that if she is currently in a union, the only man with whom she can conceive a child is her current spouse or partner. Let $A(Y_{it})$ denote the set of alternatives available to a woman in period t, given her current state Y_{it}. The alternatives are specified below. The value to a woman of choosing alternative j is specified as

$$(1) \qquad V^*_{ijt} = \beta_{1j}X_i + \beta_{2j}Y_{it} + \beta_{3j}Z_{ijt} + \beta_{4j}X_iZ_{ijt} + \beta_{5j}\mu_i + \varepsilon_{ijt,}\ j \in A(Y_{it})$$

where μ_i is a permanent unobserved woman-specific effect, β_{5j} is an alternative-specific factor loading, and ε_{ijt} is an *iid* shock. The inclusion of μ_i captures persistence in unobserved factors, such as preferences, partner characteristics, and the state of the marriage market. The interaction between X_i and Z_{it} allows policy and labor variables effects to differ by race/ethnicity.

We do not specify an explicit theory of choice behavior, but Equation (1) is consistent with choice-theoretic approaches proposed by Becker (1981) and others. It is useful to think of (1) as an approximation to the value function associated with a given alternative.[12] But the parameters do not have explicit choice-theoretic interpretations, as they capture both the response to current incentives and expectations about the future evolution of the key driving forces. If the policy and labor market contextual variables of interest are exogenous, the parameters can be interpreted as causal effects.

If woman i chooses the alternative with the highest value in month t, and if ε_{ijt} follows the Type I extreme value distribution, then conditional on μ the probability that she makes choice j, P_{ijt}, has the multinomial logit form:

$$(2) \qquad P_{ijt} = \exp\{V_{ijt}\} / \sum_{k \in A(Y_{it})} \exp\{V_{ikt}\}$$

where $V_{ijt} = V^*_{ijt} - \varepsilon_{ijt}$. The conditional likelihood function contribution for woman i is formed as the product, over the months for which she is observed, of probabilities for her observed choices, conditional on μ. The unconditional likelihood contribution is the integral of the conditional likelihood over the distribution of μ. The latter is treated as a discrete random factor with a two-point distribution. The model is thus a discrete-time multistate competing risks model of childbearing, union formation and dissolution, and "father identity." The model does not suffer from the usual Independence of Irrelevant Alternatives property of the multinomial logit model because the β_{5j} parameters allow the disturbances to be correlated, although in a restricted manner (eight parameters determine the covariances among the disturbances).[13] The model is estimated by maximum likelihood.

The full set of alternatives, not all of which are available in a given month, is

0. Do nothing
1. Conceive a child with the current man
2. Conceive a child with a new man
3. End the current union and become single
4. Enter a cohabiting union with the current man
5. Enter a cohabiting union with a new man
6. Marry the current man
7. Marry a new man

A *new* man is defined as a man who is not the father of any of a woman's children and with whom she has never lived. The *current* man is her partner or spouse if she is currently in a union. If she is not in a union, the current man is the father of her most recent child conceived since the end of her last union, if any, or since she began conceiving children if she has never been in a union. If she is not in a union and has not given birth to any children since the end of the previous union (or ever, if she has never been in a union), then there is no current man and alternatives 1, 3, 4, and 6 are not available. If she is currently in a union or pregnant, then as indicated above, we assume that only the

11. We consider only conceptions that lead to a live birth. Conception is treated as a choice, but the birth is treated as a censoring event that ends the current pregnancy. Thus, the duration of pregnancy and the decision to terminate a pregnancy are not treated as choices. Twin births are treated as an exogenous random event.

12. It is not a reduced form, as it contains variables (Y_{it}) determined by past choices.

13. As discussed in the next section, the richness of the NLSY79 data allows us to construct event histories that begin at age 12 for most women, so there is no initial condition problem. The only exception is for cohabitations that began and ended before the first interview, which were not recorded. The average age at the first interview was 17 and the maximum age was 22, so it is unlikely that many cohabitations were missed. Marriages, divorces, and births that occurred before the first interview were recorded at the first interview.

current man is relevant: she can conceive a child or enter a union only with the current man, so alternatives 2, 5, and 7 are not available.

Distinguishing between a new man and the current man is important because the choice between the two determines which of a woman's children will reside with, or be at risk of residing with, the biological father and which with a stepfather. This important distinction has rarely been made in analyses of family *formation* behavior (see Graefe and Lichter 1999 for an exception). We impose one key assumption in order to make it feasible to model the choice between a new man and the current man. If a woman ends a union with the current man or if she has a child with a new man, then she is not at risk of conceiving a child or entering a union again with the former current man. With this assumption, there is at most one current man.

The model is quite rich and flexible. It allows for observed and unobserved heterogeneity, state dependence, duration dependence, and other forms of history dependence. The effects of policy and labor market conditions are allowed to vary by race and ethnicity. Geographic and time effects are included in order to allow for unobserved heterogeneity across states and over time. In practice, the specification is restricted in various ways described below, in order to avoid an excessive number of parameters. But even after imposing restrictions, the model allows substantial flexibility in the effects of contextual variables on the family structure experiences of children. These effects are derived from simulations of the model, as described below.

IV. DATA

A. *The National Longitudinal Survey of Youth, 1979 Cohort (NLSY79)*

The NLSY79 began in 1979 with a sample of young men and women who were born between 1957 and 1964. They were interviewed annually from 1979 to 1994 and biennially since 1994. We use prospective data on female respondents through the 2004 interview, along with retrospective reports from the first interview about pre-1979 marriage and fertility behavior. We use the representative cross-sectional sample and the supplementary over-samples of blacks and Hispanics. Here, we briefly describe measurement of the key variables; more details are available in Blau and van der Klaauw (2008).

In 1979, when the sample women were between the ages of 14 and 22, the survey collected information on the beginning and ending dates (to the nearest month) of up to two marriages. In subsequent waves, information has been collected on up to three changes in marital status since the previous interview. We treat the date of separation as the date of the end of a marriage, because the issue of interest is the presence of a man in the mother's household. However, there are many temporary separations that are followed by reuniting. Modeling the process that determines whether a couple reunites after a separation would make an already rich analysis excessively complicated. Thus, we ignore temporary separations if the duration of the separation was less than or equal to 2 years. Cases in which a temporary separation lasted more than 2 years are censored at the date of separation and no information beyond the separation date is used in the analysis.[14]

The survey has collected information on cohabitation in several different ways, including snapshots of cohabitations in progress at each interview date; the starting date of cohabitations that were in progress at the interview date, beginning with the 1990 interview; the starting date of cohabitations that turned into marriages that were in progress at the interview date, also beginning with the 1990 interview; and both the beginning *and* ending date of cohabitations that did not turn into marriages, beginning with the 2002 interview. Cohabitations that began and ended before the 1979 interview or that began and ended between interviews before 1990 are missed.[15] We combined information from the

14. There is one exception to this rule: if a woman never had any children prior to the end of a temporary separation that exceeded 2 years, her record is not censored, because there are no children affected by the separation. Nineteen percent of the approximately 1,700 separations were temporary. The median duration of a temporary separation was 17 months, and 60% were shorter than 2 years.

15. Sixty percent of observed cohabitations that did not turn into marriages had a beginning date that was not known to the nearest month, and 95% had an ending date that was not known to the nearest month. Forty percent of cohabitations that turned into marriages had a beginning date that was not known to the nearest month. Bumpass and Lu (2000) use retrospective data and report that almost 50% of women in the NLSY79 cohort had ever cohabited by the time they were in their 30s. Our estimate from the NLSY79 is 40%, so clearly we are undercounting cohabitations. The cohabitations most likely to be missed in the NLSY79 are short, and children are unlikely to be born during a short cohabitation. So missed cohabitations are less important for purposes of studying family structure experienced by children than for studying the incidence of cohabitation.

various reports to form as complete a cohabitation history as possible. The cohabitation and marriage histories were combined to form a complete union history. We performed extensive consistency checks on the union history and examined and corrected many anomalous cases by hand (the resulting code is available on request). Cases in which exact starting or ending dates of unions are uncertain are retained, and the likelihood function is modified to integrate over all feasible dates. See Appendix A for details. However, we dropped 401 cases with either unresolvable inconsistencies in the timing of unions or patterns that violate the assumptions of the model.[16]

The month and year of birth is reported for each child, and beginning in 1984, women were asked the month in which each pregnancy began. We use this information to identify the month of conception. If the month of conception is missing, we assume the conception occurred 9 months prior to the birth.

Beginning with the 1984 interview, the mother is asked for each of her coresident biological children whether the child's biological father is present in the household. Thus, when a woman lives with a man before or during the conception and birth, identifying fathers is straightforward. The more difficult cases are those in which a woman who has given birth to a child since the end of her previous union (or since she began bearing children, if she has never been in a union) conceives and bears another child while single. In such cases, we need to determine whether the father of the new child was the same man who fathered her previous child, but we can do this only if she subsequently enters a union (and is interviewed while the union is still in progress). If she never enters a union following the birth of a child, we cannot determine whether the father of that child was the current man or a new man. Of the 1,086 cases in which a child was conceived and born to a single woman who had given birth to a child since the end of her previous union, we are able to identify whether the father is the current man or a new man in 35% of the cases. Rather than discard the remaining cases, we modify the likelihood function to account for both the possibilities, weighted by the probability (from Equation [2]) that the father was the current man or a new man. This modification is described in Appendix A.[17] This approach will produce consistent parameter estimates if the data are missing at random conditional on the observables and the permanent unobserved factor (μ).

At each interview date, we can determine from the household roster whether a given child is present in the mother's household. Modeling whether a child lives with the biological mother would be interesting but is beyond the scope of this article. The processes that determine this are thus treated as exogenous censoring processes, and the number of children present in the mother's household is adjusted when a child moves in or out. Cases in which a child is away at school or living part-time with the mother are treated as if the child is living with the mother. The death of a child is treated as a censoring event, and children's records are censored at age 18.

After dropping cases with incomplete data or unresolved inconsistencies, we are left with a sample of 4,476 women of 4,926 eligible for inclusion.[18] Descriptive statistics on the analysis sample are displayed in Table 1, separately for whites, blacks, and Hispanics. The first panel

16. These include 114 cases in which a woman dissolved a union with a man and subsequently reentered a union with the man, 68 cases in which a woman had a child with one man, then had a child with a second man, and finally had another child with the first man, and 65 cases in which two or more demographic events occurred in the same month. Many of these cases may be a result of errors in identifying men. We were able to correct such errors in some cases but not in these cases.

17. In some cases, the sequence in which events occurred is uncertain, as a result of lack of exact information on start or end dates of unions. For example, if a cohabitation begins between interviews and a child was born between the same interviews, we cannot always determine whether the man moved in before or after the child was born. We modified the likelihood function to account for the alternative feasible sequences in which the events occurred. This is also described in Appendix A. Missing information on the identity of men and uncertainty about the sequence of events occurred for 12% of children of white mothers, 48% of children of black mothers, and 25% of children of Hispanic mothers. This pattern reflects the high incidence of births while single among black women and cohabitations among Hispanic women. We compared sample means of the variables reported below in Table 1 for the full sample, weighting by the inverse of the number of sequences, with corresponding statistics on the subsample with no missing information. The two samples are very similar for whites, with the largest difference in means of binary variables equal to 0.03. For Hispanics, the largest difference is 0.06, and most are equal to 0.01 or 0.02. For blacks, the largest difference is 0.09, with most of the differences in the range of 0.05–0.06.

18. The omitted cases include the 401 cases mentioned above with inconsistent marriage and cohabitation histories and another 32 cases with problematic data on children and fathers. Another 17 cases are lost as a result of missing or inadequate data on contextual variables.

TABLE 1
Descriptive Statistics on Women and Children as of the Last Interview

	White	Black	Hispanic
Demographic outcomes of women			
Number of children born	1.71	1.89	1.99
No children born	0.21	0.18	0.17
Ever married	0.89	0.62	0.82
Ever cohabited	0.43	0.36	0.39
Age at last observation	40.5	39.9	39.6
Number of women	2,292	1,338	846
Family structure outcomes of children			
Ever lived with no father	0.31	0.76	0.45
Ever lived with married father	0.95	0.61	0.87
Biological	0.92	0.47	0.79
Step	0.14	0.20	0.17
Ever lived with cohabiting father	0.14	0.27	0.23
Biological	0.04	0.10	0.09
Step	0.11	0.18	0.16
Ever lived with biological father	0.94	0.52	0.85
Ever lived with stepfather	0.18	0.28	0.24
Age of child at last observation	12.9	14.0	13.3
Number of children	3,864	2,496	1,667

Notes: The last interview was in 2004 for 72% of women. The child outcomes are censored at age 18. Observations are weighted by the inverse of the number of event histories per woman. As described in the text, a woman may have multiple event histories if there is ambiguity about the timing or sequence in which demographic events occurred. In these cases, an event history is generated for each of the feasible timing or sequencing alternatives. See the text and Appendix A for further discussion.

summarizes demographic outcomes as of the last interview. The sample women were aged about 40 on average as of their last interview.[19] white women had given birth to an average of 1.71 children and 21% had not given birth to any children. Black and Hispanic women had about 0.2–0.3 more births on average than whites. Eighty-nine percent of white women had ever been married compared with 62% of black women and 82% of Hispanic women. White women were also somewhat more likely to have ever cohabited.

The lower panel of Table 1 summarizes the incidence of the family structure outcomes experienced by the 8,027 children born to the sample women as of their last interview. The children were aged 13–14 on average at the time of the last observation (after truncating at age 18; without truncating, they were 14–16). Thirty-one percent of children of white mothers had ever lived without a father figure present compared with 76% of the children of black mothers and 45% of the children of Hispanic mothers. Most children of white and Hispanic mothers lived with both biological parents at some point in their childhood (94% and 85%, respectively) compared with 52% of the children of black mothers. Children of black mothers were more likely to live with a stepfather and/or a cohabiting father compared with children of white and Hispanic mothers, but these differences are smaller.

A concern with using a long panel for a study of family structure is that attrition and immigration could make the sample increasingly unrepresentative over time. Most studies on family structure use a time series of cross sections and do not face this problem, although they cannot study individual-level dynamics with such data. To examine this issue, we compared summary statistics for the NLSY79 cohort in the NLSY79 data and the March Current Population Survey (CPS), for 2 years, 1995 and 2004. 1995 was the first CPS survey year in which cohabitation was well measured, and 2004 was the last year of data in our NLSY sample. The results (available in the working paper version, Blau and van der Klaauw 2009) indicate close agreement on family structure between the two data sources. With a few exceptions, the NLSY79 sample has not been compromised by attrition for whites and blacks but is increasingly unrepresentative of Hispanics.[20]

B. Contextual Data

The geo-coded version of the NLSY79 provides the state of residence at each survey date,

19. Women who attrited from the sample are included in the analysis, with attrition treated as an exogenous censoring event. The last interview was in 2004 for 72% of women. Women who were interviewed in 2004 were between the ages of 39 and 47.

20. See MaCurdy et al. (1998) for an extensive analysis of attrition in the NLSY79.

at the time of the woman's birth, and when she was age 14. We collected data from a variety of sources on welfare benefits, welfare reform, divorce laws, tax rates, and labor market conditions and merged them with the NLSY79 data by state and year. Here, we briefly describe the key measures; Appendix B provides details and describes how state of residence was assigned for nonsurvey years.

The real (year 2000 dollars) Aid to Families with Dependent Children (AFDC) or Temporary Assistance for Needy Families (TANF) plus Food Stamp benefit for a family of four (single mother with three children under 18) with no other income is used as a measure of the welfare benefit. The average welfare benefit declined in real terms over much of the sample period, with a couple of episodes of relative stability. The month and year of implementation of major welfare waivers and the TANF program for each state are used to characterize welfare reform. The welfare reform variable indicates the presence of any major change in welfare rules authorized by a waiver or TANF.[21]

The month and year of enactment of unilateral divorce laws were taken from Gruber (2004), which is an update of Friedberg's (1998) data. Unilateral divorce means that mutual consent for a divorce is not required. Most such laws were enacted in the 1970s, but there were occasional later cases in which states passed a unilateral divorce law.

The TAXSIM program provided by the National Bureau of Economic Research (NBER) was used to compute the average tax rate for alternative filing statuses and numbers of children. The program accounts for all major features of the tax code, including the EITC and (beginning in 1977) state taxes. Rather than conditioning on the woman's observed income, we specify an arbitrary real income level that is used for all women in all years. This ensures that the only variation in the tax rate used in the model is due to tax code variation over time and across states. In the results reported here, we used the real equivalent of the year 2000 poverty line for a family of three. We estimated an alternative specification using the real equivalent of year 2000 median family income and found similar results. The average tax rate is a better characterization than the marginal tax rate for the implications of alternative discrete marriage and childbearing choices.

The tax rate is treated as a choice-specific variable that depends on the marital status and number of children associated with each alternative a woman faces. For example, the alternatives available to a married woman with one child are to remain in this state, conceive a second child, or end the union and become single. The tax rate is different for each alternative: married filing jointly with one child, married filing jointly with two children, and head of household with one child, respectively. Marital status and number of children are outcomes of the choice processes and therefore endogenous if there is serially correlated unobserved heterogeneity. Conditioning on the permanent woman-specific effect (μ_i) and integrating it out of the likelihood function accounts for this source of endogeneity if the heterogeneity is permanent. Thus, in our analysis, the tax rate varies over time, across states, and by fertility and marital status.[22] There was rapid growth in the tax subsidy to children for low-income women beginning in the 1980s. Much of this growth is a result of large expansions of the EITC, which provides benefits almost exclusively to low-income families with children (and is refundable, hence the possibility of a negative average tax rate).

The female wage rate is measured by the mean real full-time average hourly earnings of women aged 16–47. The state-year-specific mean wage rate is constructed separately for whites, blacks, and Hispanics using data from the CPS by dividing weekly earnings in the survey week by hours of work per week. The age group 16–47 spans the (employment-eligible) age range of the NLSY sample in the years for which we have data. In order to avoid introducing composition effects into the wage trends, we regression-adjust wages for education and age. The wage measures used here are standardized to a constant level of education (high school graduate) and age (26–30). The male wage rate is constructed in the same way, for a sample of men aged 18–49. Note that the wage rate is *not* choice specific: it is not conditioned on marital status or fertility. It is also not conditioned on the education or other human

21. TANF was implemented by all states, while not all states requested a welfare waiver. TANF incorporated many of the rule changes implemented by various states as part of their waivers, including time limits and welfare-to-work (workfare and learnfare) programs. TANF was implemented by states between 1996 and 1998.

22. Other explanatory variables such as the male wage rate could also be treated as choice-specific attributes. We do not adopt this approach because it requires additional assumptions about income sharing in cohabitation.

capital characteristics of the women in our sample.[23] The male–female wage gap narrowed for all three groups through the mid-1990s, especially for Hispanics, but has been constant more recently. In absolute terms, only for white and Hispanic women are mean real wages higher in 2004 than in the 1970s.

V. RESULTS

A. Specification

The parameter estimates and standard errors on the policy and labor market variables are reported in Table A1.[24] The parameter estimates are not particularly informative, so we do not discuss them.[25] The specification reported here includes the six contextual variables described above, each interacted with indicators for black and Hispanic, thus allowing the effects to differ freely by race/ethnicity. The specification also includes dummies for nine census regions and the 22 largest states, a quadratic in calendar time, dummies for 5-year (or in some cases 10-year) periods, and dummies for several individual years in the mid-1990s, around the time of welfare reform. The model is nonlinear and has a large number of parameters. Given the small numbers of women from the less-populated states, as well as the low frequency with which some alternatives were chosen in some of the calendar years, it was not feasible to incorporate full sets of state fixed effects and calendar year fixed effects, leading us to group some of them together instead. The geographic and time controls are included in order to avoid attributing the effects of unobserved differences across states and over time to the contextual variables of interest. However, the geographic controls also absorb the true effects of permanent cross-state differences in the contextual variables, as well as other permanent differences across states, thus leaving only variation over time around state-specific averages to identify the effects of the contextual variables (Keane and Wolpin 2002). Below, we discuss the sensitivity of the results to specifications with alternative sets of geographic controls.

B. Model Fit

We use the parameter estimates to simulate the life history of each woman in the sample. The simulations condition only on the woman's race/ethnicity, age, and the state of residence in each year in which she is observed. A woman is assigned a heterogeneity type (a value of μ) based on a draw from the estimated heterogeneity distribution. Each woman starts out single and with no children at age 12. The estimates are used to compute the probability of each of the three events in the choice set in this case (enter a cohabitation, enter a marriage, and conceive a child), given her type (μ), race/ethnicity, and state of residence at age 12. A random number generator determines which, if any, event occurs. If the event is conceiving a child, a pregnancy duration is randomly assigned by drawing from the observed distribution of pregnancy durations in the sample. The Y_{it} variables are updated according to which event, if any, occurred, and the process is repeated for the next month. If pregnant, the birth occurs at the assigned duration. The simulation continues through the last month in which

23. Conditioning the predicted wage on the woman's education would generate more variation in wages but could result in endogeneity of the wage if education is jointly determined with demographic behaviors. This approach is not feasible for male wages, because we do not observe education for potential mates.

24. In addition to the contextual variables, the specification includes black and Hispanic indicators, a quadratic in the woman's age, the number of children fathered by the current man, the cumulative number of cohabitations, whether a single woman was in a cohabitation or a marriage in her previous spell, quadratics in the ages of her youngest and oldest children, and quadratics in the duration of cohabitation and single spells. See Table A2 for the parameter estimates on these variables. In the interests of empirical tractability, we imposed a substantial number of exclusion restrictions in cases in which a given variable consistently had small and statistically insignificant effects. In alternative specifications, we found that the mother's date of birth, number of marriages, total number of children, duration of marriage, and duration of pregnancy could be excluded with little impact on the predictions of the model. The estimates of the factor loadings and probability weight shown in Table A2 are jointly highly significant and imply a plausible correlation structure among the disturbance. For example, the disturbance in the union dissolution alternative (Choice 3) is negatively correlated with the other disturbances, indicating that unobserved factors that increase the likelihood of ending a union are negatively correlated with unobserved factors that increase the propensity to enter a union and bear children. The correlation between the disturbances in the conceive-a-child-with-a-new-man and marry-a-new-man alternatives is 0.46.

25. Of the 126 parameter estimates reported in Table A1, 19 are significantly different from zero at the 10% level. This is more than would be expected if the contextual variables had no impact, but it does suggest some weakness in the model. In addition to these 126 parameters, there are many others on state, region, and time variables, many of which are highly significant. When the state and region variables are omitted, more of the 126 parameters of interest are significantly different from zero. Simulations based on the latter specification are discussed below.

the woman is observed in the data.[26] This procedure is repeated 100 times for each woman. To generate standard errors for the simulations, we took 200 random draws from the joint distribution of the parameter estimates and repeated the entire simulation procedure for each draw. We report the mean and standard deviation of the resulting simulations.

In the baseline simulation, the contextual variables take on their observed values. Table A3 in the Appendix compares simulation results for selected variables characterizing choice behavior to the observed values in the data.[27] The model reproduces most aspects of the data reasonably well, but underpredicts the proportion of childhood living without any man present. Table A4 illustrates the fit of the model to transitions of children among the five family structure categories of interest. This is a demanding measure of fit, because these transition rates are not directly estimated but rather are derived from the underlying transition probabilities of women among states. The upper panel compares simulated and actual transition probabilities averaged over all ages from 0 through 17. The model fits the transition probabilities very well in some cases, such as transitions involving a man entering the household (Rows 1–4) and breakup of cohabitations and marriages with stepfathers (Rows 7 and 10). The simulations underestimate the rate of dissolution of marriage to the biological father (Row 9) and overestimate the rate at which cohabitations are converted to marriages (Rows 6 and 8) and the rate at which cohabitations with the biological father break up (Row 5). The fit averaged over ages 0–5 shown in the lower part of the table is similar to the fit averaged over all ages.

C. *Counterfactual Simulations*

To illustrate the effects of wage rates and the welfare benefit, we compare two scenarios (separately for each variable): one in which the variable is held constant for all women and all periods at its overall sample mean and another in which it is held constant at the mean plus one standard deviation. For the tax rate, we compare one scenario in which the tax rate for each combination of marital status and number of children is held constant at its sample mean to three alternative counterfactuals: one in which the tax gain from marriage is eliminated; a second in which the tax gain from having children conditional on marriage is eliminated; and a third in which the tax gain from having children conditional on being unmarried is eliminated (see the notes to Table 2 for details). For welfare reform and unilateral divorce, the two scenarios hold the variable constant at zero and at one. The values of the contextual variables used in the simulations are shown in Table 2.

Table 3 shows simulated effects of the contextual variables on the proportion of childhood spent in the five family structures of interest. The results show that an increase in the average female wage rate causes an increase in the proportion of childhood spent living with no father. The effect is very small for children of white mothers but is large for children of black and Hispanic mothers. The implied wage elasticities of the proportion of childhood spent with no father are 1.36 for children of black mothers and 3.64 for children of Hispanic mothers.[28] Another way to illustrate the magnitude of these effects is to note that the mean real wage rate of black women *fell* by more than one standard deviation, from over \$10 to \$8.50, from the mid-1970s to the early 1990s. The results in Table 3 imply that this decline would have reduced the proportion of childhood spent with no father by more than 0.06, from a baseline of 0.33. Most of this decrease in the proportion of childhood spent living with no man would be associated with an increase in time spent with the married biological father.

The effects of an increase in the male wage rate are almost all opposite in sign to the effects of an increase in the female wage rate. This is consistent with the prediction of Becker's

26. The simulated data for children are truncated at age 18. As in the data, some children are not observed for their entire childhood in the simulations, because they have not reached age 18 in the last period in which the mother is observed. In the simulations, there are no deaths, no twin births, and no cases in which children move in or out of the mother's household.

27. Tables A3 and A4 use the actual parameter values rather than drawing from the parameter distribution, so no standard deviations are reported.

28. From Table 2, a one standard deviation increase in the wage rate is a 13.4% change. The baseline-simulated proportion of childhood spent living with no father is 0.33 for blacks and 0.12 for Hispanics (see Table A3). The simulated change of 0.060 for blacks in Table 3 is an 18.2% increase over the baseline value, yielding an elasticity of 1.36 = 18.2/13.4. For Hispanics, the corresponding elasticity estimate is 3.64 = 47.5/13.4.

TABLE 2
Summary Statistics for Contextual Variables

A. Means and standard deviations

	Mean	SD
Monthly welfare benefit for a family of four with no income	1,013	265
Unilateral divorce law in effect	0.552	
Welfare reform in effect	0.210	
Male hourly wage rate	12.12	1.80
Female hourly wage rate	9.70	1.30

B. Mean tax rate by marital status and number of children

Number of children	0	1	2	3	4	5	6	7	8	9
Married	0.170	0.061	0.030	0.028	0.027	0.027	0.026	0.026	0.025	0.025
Single	0.228	0.077	0.035	0.031	0.030	0.028	0.028	0.027	0.027	0.026

Notes: Unit of observation is a state-year-race/ethnicity cell. Observations are weighted by the cell sample size in the NLSY sample. Dollar amounts are in year 2000 dollars, using the PCED. The simulations reported in Table 3 use the means reported here as the baseline. The welfare benefit and wage counterfactual simulations add one standard deviation to the mean. The counterfactual for the "eliminate the gain to marriage" simulation replaces the tax rates for singles with the tax rates for married couples. The counterfactual for the "eliminate the gain to having children if married" simulation replaces the tax rates for married couples with children with the tax rate for married couples without children (0.170). The counterfactual for the "eliminate the gain to having children if single" simulation replaces the tax rates for single women with children with the tax rate for single women without children (0.228). The unilateral divorce and welfare reform simulations compare values of zero to one.

model of specialization in marriage. The simulated effects are small for children of white and black mothers. For children of Hispanic mothers, an increase in the male wage causes a decline in time spent with no father and with a married stepfather, accompanied by an increase in time spent living with the married biological father. The decline of 0.037 in the proportion of childhood spent with no father is quite large relative to the baseline of 0.122 for children of Hispanic mothers.

These hypothetical exogenous changes in average market wage rates affect behavior presumably because the wage offers available to individuals in our sample are drawn from the corresponding market wage distributions. The estimates can be interpreted as reduced form effects, showing how changes in average market wages affect family structure without identifying the underlying mechanisms of the effects. Thus, we cannot identify how an increase in the mean wage offer affects the distribution of wage offers by skill or ability nor the effect of wage offers on employment decisions. The advantage of the approach is that it is feasible to estimate the net impact of wages on all the demographic behaviors that determine family structure without modeling additional choice variables such as employment.

An increase in the welfare benefit is estimated to cause a decrease in the proportion of childhood spent living with the married biological father for all three groups, but the estimates are not significantly different from zero. The negative effect is consistent with the predictions of economic models of the family such as Neal (2004) and Willis (1999). The decrease is accompanied by an increase in time spent living with no man (except for blacks) and cohabiting men, but the proportion of childhood spent living with a married stepfather also increases.

The simulated effects of the tax gains to marriage are quite small and precisely estimated, in the sense that we can reject large effects with considerable confidence. The simulated effects of the tax gain from having children conditional on being married are also quite small in most cases, but a few of the effects are a bit larger. It is surprising that the tax gain from children conditional on marriage is estimated to cause a *decrease* in the proportion of childhood spent living with a married father, by about 0.025 for all three groups, accompanied by an increase in time spent living with no father. This is a puzzling finding and is robust across the alternative specifications we have estimated. The simulated effects of the tax gain from children conditional on being unmarried are also counterintuitive, resulting in an increase in time spent with the married biological father and a decline in time spent with no father present.[29]

The last two panels in Table 3 show the simulated effects of welfare reform and unilateral divorce. The simulations reveal some moderately large effects in a few cases, but none is significantly different from zero. Welfare reform is estimated to cause an increase of 0.049 in the proportion of childhood lived with the married biological father for children of black mothers. Unilateral divorce also causes a rather large increase in the proportion of childhood living with the married biological father for children of black and Hispanic mothers. The lack of precision of these estimates is probably due to the fact that most of the changes in unilateral divorce laws were in the early 1970s, so these changes affected few individuals in our sample during the prime childbearing years. Welfare reform occurred in a fairly narrow time span from the late 1980s through 1997 when the NLSY79 cohort was in their 30s, past prime childbearing ages.

Family structure changes are thought to have different effects on children in different stages of childhood (e.g., Hill, Yeung, and Duncan 2001; Moore et al. 2001). We examined whether the effects shown in Table 3 were concentrated in particular phases of childhood: early (0–4), middle (5–11), and late (12–17). These are hypothesized by developmental psychologists to be distinct stages in the developmental life course. The results (not shown) do not indicate any systematic tendency for the effects of the contextual variables on family structure to be concentrated in particular stages of childhood. In a few cases, the effects are stronger at younger ages (e.g., welfare reform for blacks), whereas in a few other cases, the effects are stronger at older ages (e.g., tax gain to having children for whites). In the great majority of cases, the effects are quite similar across the age groups.

The outcomes shown in Table 3 are measures of the "stock" of time spent in alternative family structures. It is of considerable interest to investigate the underlying determinants of these stocks, which include both a child's family structure at birth and "flows" of men in and out of a child's household. Consider the effect of a one standard deviation increase in the female wage rate, which was estimated to cause increases of 0.060 and 0.057 in the proportion of childhood living with no father for children of black and Hispanic mothers, respectively. The simulated increase in the probability that the mother was single at the birth of the child is 0.038 (SE 0.035) for blacks and 0.056 (SE 0.031) for Hispanics, accounting for a substantial part of the increase in the proportion of childhood living with no father (not shown). The simulated wage increase causes an increase of 0.02–0.03 in the annualized transition rate out of marriage for children of black mothers, thus contributing to the increase in time spent with no father. Cohabitations break up at a more rapid rate as well.

Another interesting finding in Table 3 is the negative effect of a higher male wage rate for children of Hispanic mothers on the proportion of childhood living with no father (−0.037) and the corresponding positive effect on time spent living with the married biological father (0.059). The simulated effect of an increase in the male wage rate on the probability that the mother was single at the birth of the child is −0.046 (SE 0.023) for children of Hispanic mothers, which can account for all the −0.037 change in the proportion of childhood spent with no father. Changes in transition rates contribute little in this case.

D. Simulated Effects of Observed Changes in Contextual Variables

We now use the estimates to address a different question: how did the observed trends in the contextual variables affect family structure compared to a counterfactual scenario in which the contextual variables remained constant at their state-and-race/ethnicity-specific 1970–1974 means, the values that prevailed when the NLSY79 cohort of women was entering adolescence? The first panel of Table 4 shows the simulated impact of exposure to the observed values of the contextual variables compared to the counterfactual in which they *all* remained at their early 1970s levels. For children of white mothers, the simulations imply that changes in the contextual

29. The simulation of the effects of the tax gain from an additional child within marriage holds constant the tax gain from an additional child outside of marriage and vice versa. Further examination of these effects would require an analysis of income and substitution effects (both within and across periods) of the tax gain on labor supply and earnings. Note that in a household bargaining model with divorce and child support transfers, the impact of an EITC-type tax credit, which is especially beneficial to single mothers, on divorce has an ambiguous sign (Francesconi et al., 2009).

TABLE 3
Simulated Effects of Changes in Contextual Variables on the Proportion of Childhood Spent in Alternative Family Structures

	Change in Proportion of Childhood Lived with the Biological Mother and				
	Married Biological Father	**Cohabiting Biological Father**	**Married Stepfather**	**Cohabiting Stepfather**	**No Father**
Female wage rate					
White	0.001 (.016)	−0.001 (0.002)	−0.002 (0.006)	−0.001 (0.001)	0.002 (.009)
Black	−0.049 (0.032)	**−0.005 (0.003)**	−0.003 (0.008)	−0.003 (0.003)	**0.060 (0.030)**
Hispanic	**−0.077 (0.043)**	−0.002 (0.003)	**0.020 (0.010)**	0.001 (0.004)	**0.057 (0.033)**
Male wage rate					
White	−0.019 (0.018)	0.001 (0.002)	0.008 (0.007)	0.002 (0.002)	0.008 (0.010)
Black	−0.011 (0.035)	0.017 (0.012)	−0.003 (0.012)	0.011 (0.008)	−0.014 (0.028)
Hispanic	**0.059 (0.025)**	−0.002 (0.003)	**−0.016 (0.009)**	−0.003 (0.003)	**−0.037 (0.014)**
Welfare benefit					
White	−0.021 (0.015)	0.000 (0.001)	0.010 (0.006)	0.002 (0.002)	0.009 (0.009)
Black	−0.004 (0.038)	0.002 (0.004)	0.007 (0.012)	0.002 (0.004)	−0.008 (0.032)
Hispanic	−0.037 (0.025)	0.002 (0.003)	**0.016 (0.008)**	0.005 (0.004)	0.014 (0.016)
Tax gain from marriage					
White	−0.001 (0.002)	0.000 (0.001)	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)
Black	−0.009 (0.006)	0.000 (0.001)	0.003 (0.002)	0.000 (0.001)	0.006 (0.004)
Hispanic	−0.007 (0.006)	0.001 (0.001)	0.002 (0.002)	0.000 (0.001)	0.004 (0.003)
Tax gain from a child if married					
White	**−0.025 (0.008)**	−0.000 (0.001)	**0.007 (0.003)**	0.001 (0.001)	**0.017 (0.005)**
Black	−0.025 (0.026)	0.002 (0.002)	−0.003 (0.011)	0.003 (0.003)	0.022 (0.024)
Hispanic	−0.026 (0.021)	0.001 (0.003)	0.001 (0.007)	0.002 (0.003)	0.021 (0.013)
Tax gain from a child if single					
White	**0.058 (0.020)**	0.000 (0.001)	**−0.017 (0.007)**	−0.002 (0.002)	**−0.038 (0.013)**
Black	0.011 (0.036)	−0.005 (0.005)	0.013 (0.015)	−0.005 (0.006)	−0.014 (0.032)
Hispanic	0.041 (0.039)	−0.003 (0.005)	−0.001 (0.010)	−0.005 (0.006)	−0.032 (0.026)
Welfare reform					
White	−0.003 (0.020)	−0.001 (0.002)	0.004 (0.010)	−0.001 (0.002)	0.001 (0.011)
Black	0.049 (0.060)	−0.001 (0.005)	−0.021 (0.019)	−0.004 (0.004)	−0.023 (0.046)
Hispanic	0.008 (0.037)	−0.002 (0.004)	0.010 (0.014)	−0.001 (0.004)	−0.013 (0.021)
Unilateral divorce					
White	−0.004 (0.030)	−0.001 (0.004)	0.002 (0.011)	0.000 (0.003)	0.004 (0.017)
Black	0.066 (0.068)	−0.002 (0.007)	−0.010 (0.021)	−0.005 (0.007)	−0.049 (0.054)
Hispanic	0.102 (0.065)	−0.007 (0.009)	−0.022 (0.018)	−0.009 (0.009)	−0.064 (0.044)

Notes: The five family structures are mutually exclusive and exhaustive, so the entries in each row sum to zero. Standard errors are in parentheses, computed as described in the text. Entries in bold are significantly different from zero at the 10% level. The wage rate and welfare benefit simulations show the effect of a one standard deviation increase, relative to the mean. The tax gain simulations show the effect of the observed mean tax rates relative to the counterfactual of the tax rate for singles set equal to the tax rate for married households (tax gain to marriage), the tax rate for married families with children set equal to the tax rate for married families with no children (tax gain from a child if married), and the tax rate for single mothers with children set equal to the tax rate for single mothers with no children (tax gain from a child if single). The welfare reform and unilateral divorce simulations show the effect of setting the variable equal to one, relative to setting it equal to zero. See Table 2 for the data values used in the simulations. Childhood is defined as birth up to but not including age 18.

variables would have caused an increase in the proportion of childhood spent with the married biological father of 6.1 percentage points and decreases of 2.3 percentage points in time with a married stepfather and 3.2 percentage points in time spent with no father. These are moderately large effects, given the simulated baseline proportions of 87% with a married biological father, 5% with a married stepfather, and 7% with no father (see Table A2). The simulated effects for the children of black mothers are similar in sign and magnitude but are not

TABLE 4
Simulated Effects of Observed Changes in Contextual Variables

	Proportion of Childhood Lived with the Biological Mother and				
	Married Biological Father	**Cohabiting Biological Father**	**Married Stepfather**	**Cohabiting Stepfather**	**No Father**
All					
White	**0.061** (0.026)	−0.002 (0.002)	**−0.023 (0.010)**	−0.006 (0.004)	**−0.032 (0.016)**
Black	0.050 (0.040)	−0.009 (0.006)	0.000 (0.010)	**−0.010 (0.006)**	−0.038 (0.034)
Hispanic	0.001 (0.037)	−0.010 (0.009)	−0.001 (0.011)	−0.005 (0.006)	0.014 (0.022)
Female wage rate					
White	−0.001 (0.004)	0.000 (0.001)	0.001 (0.001)	0.000 (0.001)	0.001 (0.003)
Black	**0.023 (0.009)**	**0.002 (0.001)**	0.004 (0.005)	0.002 (0.001)	**−0.029 (0.011)**
Hispanic	**−0.021 (0.011)**	−0.001 (0.002)	**0.008 (0.004)**	0.000 (0.001)	**0.014 (0.006)**
Male wage rate					
White	0.019 (0.018)	−0.001 (0.002)	−0.008 (0.006)	−0.002 (0.002)	−0.006 (0.010)
Black	−0.017 (0.017)	**−0.009 (0.005)**	0.008 (0.007)	−0.006 (0.004)	0.022 (0.018)
Hispanic	**−0.035 (0.015)**	0.001 (0.002)	0.003 (0.005)	0.002 (0.002)	**0.030 (0.012)**
Welfare benefit					
White	0.017 (0.012)	−0.000 (0.001)	**−0.008 (0.004)**	−0.002 (0.001)	−0.007 (0.007)
Black	0.005 (0.025)	−0.002 (0.004)	−0.007 (0.007)	−0.002 (0.003)	0.004 (0.022)
Hispanic	0.020 (0.013)	−0.002 (0.002)	−0.007 (0.004)	−0.003 (0.002)	−0.009 (0.009)
Tax rate					
White	**0.021 (0.009)**	−0.000 (0.001)	**−0.007 (0.003)**	−0.001 (0.001)	**−0.013 (0.005)**
Black	0.031 (0.028)	−0.002 (0.003)	−0.004 (0.006)	−0.003 (0.002)	−0.023 (0.022)
Hispanic	0.041 (0.027)	−0.005 (0.005)	−0.009 (0.006)	−0.003 (0.003)	−0.024 (0.016)
Welfare reform					
White	0.001 (0.003)	−0.001 (0.001)	−0.001 (0.002)	0.000 (0.001)	0.000 (0.002)
Black	−0.002 (0.005)	−0.001 (0.001)	0.000 (0.003)	−0.001 (0.001)	0.003 (0.005)
Hispanic	0.001 (0.004)	−0.001 (0.001)	0.002 (0.002)	−0.000 (0.001)	−0.002 (0.004)
Unilateral divorce					
White	−0.000 (0.005)	−0.000 (0.001)	0.000 (0.002)	−0.000 (0.001)	0.001 (0.003)
Black	0.007 (0.007)	−0.000 (0.001)	−0.001 (0.002)	−0.001 (0.001)	−0.004 (0.007)
Hispanic	0.008 (0.006)	−0.001 (0.001)	−0.002 (0.002)	−0.001 (0.001)	−0.005 (0.004)

Notes: The five family structures are mutually exclusive and exhaustive, so the entries in each row sum to zero. Standard errors are in parentheses, computed as described in the text. Entries in bold are significantly different from zero at the 10% level. The simulations compare the baseline, in which all contextual variables take on their observed values, to a counterfactual in which a given contextual variable is held constant at its state-specific 1970–1974 mean value. For the tax rate, the 1977–1981 means are used, because state taxes were not included in the 1970–1974 data. In all cases, the state fixed effects, division fixed effects, time trends, and period effects take on their actual values.

as precisely estimated. The effects for Hispanics are substantially smaller. These results imply that if the contextual variables had remained at their early values during the past 30 years, the increase in the proportion of time children lived without their biological father (or without any father) would have been even larger than the observed increases.

The remaining panels of Table 4 show the effects of changing one variable at a time. An important source of the total effects for whites and blacks is the change in the average tax rate, which declined substantially for families with children beginning in the mid-1980s. This was reinforced by a declining female wage rate for blacks. For whites, the decline in the welfare benefit also contributed to the observed changes. For Hispanics, female and male wage trends that caused both less time spent in marriage and with the biological father were offset by the effects of trends in tax rates and welfare benefits. Because female and male wage rates tend to move in the same direction and have opposite effects on most behaviors, the large effects of both male and female wages for blacks and Hispanics shown in Table 3 tend to cancel each other out. The effects of welfare reform and unilateral divorce are negligible for all three

TABLE 5
Comparison of Actual and Simulated Trends in Living Arrangements of Children

	Actual Trend			
	2000–2004	**1970–1974**	**Difference**	**Simulation**
Children living with mother only, as a proportion of children living with mother only and two-parent families				
White	0.191	0.095	0.096	−0.028
Black	0.572	0.388	0.184	0.013
Hispanic	0.278	0.253	0.025	0.036
Children living with mother only: proportion of mothers never married	0.417	0.086	0.331	0.037

Source: Actual trends: http://www.census.gov/population/www/docdemo/hh-fam.html# ht, Tables CH1–CH5.
Note: For Hispanics, the data series begins in 1980, so the simulation uses 1980–1984 instead of 1970–1974.

groups, not surprising given the small estimates in Table 3.

E. Explaining Trends

The final issue considered here is whether the model can explain the large changes in family structure in recent decades in the United States. There are no consistent time series available from the 1970s onward on children living in cohabiting arrangements and on biological versus stepfathers, so the only trend we can analyze is the proportion of children living with the mother only. We compare results from two counterfactual simulations in order to determine how much of the observed trend in the proportion of children living with no father from the early 1970s through the early 2000s can be explained by our model. In one case, we hold all the contextual variables constant at their 1970–1974 state-race/ethnicity-specific means, and in the other case, we hold them all constant at the corresponding 2000–2004 values. Comparing the two simulations gives an estimate of the effect of the observed changes in contextual variables on the proportion of childhood spent living with no father, which can be compared to the actual trend.

Table 5 uses CPS data to show that the proportion of children living with no father increased from 0.095 to 0.191 for whites from 1970–1974 to 2000–2004 and from 0.388 to 0.572 for blacks. For Hispanics, the time series begins in 1980, and there was a small increase from 0.253 to 0.278 from 1980–1984 to 2000–2004. The table shows that our simulations cannot explain any of the observed change for whites and only a very small proportion for blacks. The simulations overpredict the magnitude of the increase for Hispanics, but it is not clear how much weight to put on this, given that the composition of the Hispanic population is changing over time, while the NLSY79 Hispanic sample is representative only as of 1979. Another series available on a consistent basis is the fraction of those children living with no father whose mother has never been married. Table 5 shows that this increased from 0.086 to 0.417 since the early 1970s, a change of 0.331. The observed changes in the contextual variables predict an increase of 0.037, only about one tenth of the observed change. Thus, as in much previous research, our estimates indicate that the economic and policy variables we considered contributed little to the observed changes in family structure, despite their explanatory power in the panel. There has been considerable speculation about the role of changes in attitudes toward cohabitation, out-of-wedlock childbearing, and single motherhood in explaining the trend away from traditional family structure (see, e.g., Akerlof, Yellin, and Katz, 1996; Ellwood and Jencks, 2004). Changes in attitudes may well be an important part of the explanation, but they are obviously difficult to measure and are probably themselves affected by evolving trends in family structure. Measuring and disentangling these factors is a difficult challenge.

VI. ALTERNATIVE SPECIFICATIONS

The results discussed above are based on a specification with a rich set of controls for

TABLE 6
Simulated Effects of Contextual Variables on the Proportion of Childhood Spent with No Father for Alternative Specifications

	Baseline	No State Fixed Effects	No State or Division Fixed Effects	1 + Mother's Family Structure at Age 14	4 + Mother's Education	5 + Mother's AFQT Score
Female wage rate						
White	0.003	0.001	0.002	0.003	0.006	0.025
Black	0.059	0.045	0.048	0.062	0.063	0.014
Hispanic	0.052	0.058	0.058	0.049	0.067	0.022
Male wage rate						
White	0.007	0.006	0.004	0.007	−0.000	−0.012
Black	−0.013	−0.016	−0.017	−0.014	−0.020	0.013
Hispanic	−0.035	−0.042	−0.042	−0.035	−0.056	−0.025
Welfare benefit						
White	−0.007	0.004	0.005	−0.007	0.001	0.005
Black	0.005	0.031	0.025	0.005	0.014	0.009
Hispanic	−0.011	0.006	−0.003	−0.015	0.000	0.008
Tax gain from marriage						
White	0.001	0.000	0.002	0.000	0.001	−0.001
Black	0.007	0.005	0.006	0.006	0.001	0.002
Hispanic	0.004	0.003	0.005	−0.001	0.002	0.001
Tax gain from a child if married						
White	0.015	0.016	0.016	0.015	0.005	−0.001
Black	0.023	0.022	0.023	0.024	0.032	0.023
Hispanic	0.021	0.021	0.021	0.018	0.015	0.006
Tax gain from a child if single						
White	−0.035	−0.036	−0.035	−0.032	−0.014	−0.001
Black	−0.016	−0.013	−0.011	−0.021	−0.040	−0.029
Hispanic	−0.030	−0.031	−0.031	−0.032	−0.036	−0.012
Welfare reform						
White	0.000	−0.002	−0.001	−0.001	−0.021	−0.021
Black	−0.022	−0.001	−0.001	−0.015	−0.004	−0.008
Hispanic	−0.014	−0.016	−0.012	−0.010	−0.027	−0.019
Unilateral divorce						
White	0.005	0.016	0.017	0.004	0.006	0.002
Black	−0.049	0.009	0.007	−0.045	−0.021	−0.017
Hispanic	−0.061	−0.026	−0.025	−0.040	−0.034	−0.005

Notes: See the notes to Table 3 for a description of the simulations. The baseline simulations shown in the first column are from the same specification as those reported in the last column of Table 3. However, the simulated values differ slightly from the corresponding entries in Table 3 because the results reported here use the actual parameter estimates rather than 200 draws from the parameter distribution, as in Table 3. All specifications are the same as the baseline except for the difference noted in the column headers.

fixed geographic effects: 22 state dummies and 9 census division dummies. As discussed above, this specification has the advantage of controlling for unobserved differences across states and census divisions that could be correlated with the contextual variables. The source of identification is variation in state-specific trends in the contextual variables around the state-specific means. In order to determine whether the results are sensitive to the source of identification, we re-estimated the model with two alternative specifications: one that drops the state fixed effects and another that drops the nine division dummies as well. Table 6 shows the simulated effects of the contextual variables on the proportion of childhood spent living with no father, for alternative model specifications. Column 1 reproduces the results for the main specification, from the last column of Table 3.[30] Columns 2 and 3 report results from the new specifications. Most of the simulated effects are very similar, suggesting that responses to permanent differences across states are similar to responses to variation over time around means within states. There are a few notable differences, however: the effect of the welfare benefit for blacks changes from 0.005 to 0.031; the effect of welfare reform for blacks changes from −0.022 to −0.001; and the effect of unilateral divorce changes from −0.049 to 0.009 for blacks and from −0.061 to −0.026 for Hispanics.

Another important feature of the specification is the absence of controls for the characteristics of women, other than race/ethnicity. The effects that we attribute to the contextual variables could be due in part to differences across states in characteristics of women, which are not included in the specification. To investigate this possibility, we estimated three alternative specifications of the model, adding controls for the woman's own family structure at age 14, her completed years of schooling, and her cognitive achievement, as measured by the Armed Forces Qualification Test (AFQT) score.[31] Columns 4–6 in Table 6 show the results for these specifications. Controlling for the woman's family structure at age 14 has very little impact on the simulated effects (compare Columns 1 and 4). Adding completed years of schooling has little impact as well (compare Columns 4 and 5), with a couple of exceptions. But adding the AFQT score in Column 6 changes the results substantially, yielding much smaller effects of several of the contextual variables. This is somewhat surprising, but it turns out that the AFQT score is positively correlated with all the contextual variables, with a correlation as high as 0.27 with the male wage rate. And the correlation between education and both the male wage and the average tax rate is 0.10. This is presumably due to cross-state correlations between AFQT, education, and state-specific time variation in the contextual variables. Thus, the effects that we attribute to the contextual variables may be due in part to education and cognitive ability. However, both education and cognitive ability are malleable, and the contextual variables may affect family structure in part via effects on education and cognitive ability of mothers. This is an interesting possibility to examine in future research.

30. The entries in Column 1 of Table 6 differ slightly from the corresponding entries in Table 3 because the results in Table 6 use the actual point estimates of the parameters rather than 200 draws from the parameter distribution as in Table 3.

31. Education and AFQT are treated as exogenous, and we do not attach any specific interpretation to their effects.

VII. CONCLUSIONS

The evidence presented here indicates that family structure experiences of the children of women born from 1957 to 1964 were affected by male and female wage rates and tax rates. Welfare benefits, welfare reform, and unilateral divorce are estimated to have little impact on family structure. The results show that both the magnitudes of the effects and the channels through which they operate are often quite different for whites, blacks, and Hispanics. The methods used to produce this evidence are rather new, and the consistency between our findings and those of previous studies, for the outcomes that can be compared, is encouraging. But we readily acknowledge several limitations that suggest caution in drawing any strong conclusions based on the results.

First, our results apply to a narrow range of birth cohorts, and it is difficult to see how to generalize them in the absence of comparable data for other cohorts. Second, a limitation of the NLSY79 data is that we have little information on children who do not live with the biological mother. Thus, while our model is rich, it omits this potentially important channel through which the contextual variables could affect family structure. Third, we do not model the processes that determine other potentially important aspects of family structure, such as the presence in the household of stepsiblings and grandparents. Temporary separations are ignored as well. And a potentially important channel through which several of the contextual variables may operate is the labor market, suggesting the need to model employment choices. All these channels are worth exploring in future work.

An important motivation for our analysis was the challenge posed by Ellwood and Jencks (2004) to develop new approaches to analyze the determinants of family structure change. We believe that our analysis has been successful in responding to several of their suggestions for new directions in this field. But the results of our analysis imply that trends in the contextual variables considered here cannot account for the trend away from traditional family structure in the last 30 years. Explaining this trend remains an important challenge. Another challenge that we are pursuing in ongoing research is to develop a theoretical model that can provide an explanation and interpretation of the main results, including the large differences across racial and ethnic groups.

APPENDIX A

Here, we describe how the likelihood function was modified to deal with missing data, uncertain dates of events, and uncertain sequences of events. To do this, it is convenient to rewrite the likelihood function in terms of spells of time spent in a given state. *States* are defined by Y_{it}. For example, when a woman first is at risk of experiencing demographic events at age 12, she is single, not pregnant, and there is no current man. This defines a particular state. If a woman is married and not pregnant, this defines a second state. There are a total of eight states. A woman who is in a particular state remains in that state until she experiences one of the events in her set of alternatives. See Blau and van der Klaauw (2008) for a full description of the state space.

Consider a spell in state s that began in month t and ended in month n with the occurrence of event j, one of the relevant alternatives available in state s. Use the convention that the month in which the transition occurred is the last month in which state s was occupied and the following month is the first month in which the new state $s^*(s,j)$ is occupied. Note that the state s^* occupied in the subsequent spell depends on both the event j that occurred to end the spell and the state s previously occupied. Modifying the notation in the text by dropping the individual subscript (i) and adding a state subscript (s), the probability that event j occurs in month t of a spell in state s is

$$P_{jst} = \exp\{V_{jst}\} / \sum_{k \in A(Y_t)} \exp\{V_{kst}\}$$

where

$$V_{jst} = \beta_{1j} X + \beta_{2j} Y_t + \beta_{3j} Z_{jt} + \beta_{4j} X Z_{jt} + \beta_{5j} \mu, \quad j \in A(Y_t)$$

and the probability that no event occurs in month t is

$$P_{0st} = \exp\{V_{0st}\} / \sum_{k \in A(Y_t)} \exp\{V_{kst}\}.$$

If the spell began in period t^* and ended in period n, the likelihood function contribution for the spell is (conditional on μ):

$$L_{js}(t, n, \mu) = \left(\prod_{\ell = t^*}^{n-1} P_{0s\ell} \right) P_{jsn}.$$

If the spell is censored at month n, the last term is dropped and the upper limit of the product is n.

Consider a woman who experiences a total of M spells. The mth spell begins in calendar month $t(m)$ and ends in calendar month $n(m)$. The state occupied in spell m is $s(m)$, and the event causing the mth spell to end is $j(m)$. For simplicity, assume that none of the spells is

TABLE A1

Coefficient and Standard Error Estimates on Contextual Variables

	Conceive			Enter Cohabitation		Marry	
	Current Man	**New Man**	**Become Single**	**Current Man**	**New Man**	**Current Man**	**New Man**
Welfare benefit	−0.015 (0.027)	0.025 (0.048)	0.069 (0.045)	0.039 (0.173)	**0.112** (0.050)	0.032 (0.045)	−0.069 (0.041)
Black[a]	−0.035 (0.030)	0.000 (0.043)	0.022 (0.042)	**0.258** (0.131)	−0.046 (0.052)	0.043 (0.045)	**0.086** (0.047)
Hispanic[a]	0.013 (0.024)	0.027 (0.052)	0.022 (0.041)	0.163 (0.139)	0.007 (0.047)	−0.048 (0.044)	**0.090** (0.040)
Unilateral divorce	0.107 (0.164)	−0.122 (0.248)	0.190 (0.272)	−0.036 (0.822)	0.089 (0.258)	0.135 (0.271)	−0.046 (0.274)
Black[a]	−0.078 (0.082)	−0.041 (0.140)	−**0.261** (0.115)	−0.033 (0.431)	−0.221 (0.137)	−0.010 (0.133)	**0.299** (0.140)
Hispanic[a]	0.100 (0.111)	0.149 (0.229)	−**0.543** (0.181)	−0.043 (0.640)	0.035 (0.193)	**0.457** (0.235)	−0.030 (0.210)
Welfare reform	0.090 (0.093)	0.409 (0.345)	0.002 (0.134)	0.392 (0.622)	−0.100 (0.171)	0.242 (0.168)	0.036 (0.205)
Black[a]	−**0.261** (0.125)	−**0.605** (0.332)	0.094 (0.148)	0.079 (0.587)	−0.056 (0.177)	0.111 (0.188)	−0.251 (0.205)
Hispanic[a]	−0.016 (0.115)	−0.136 (0.399)	0.051 (0.154)	−0.004 (0.724)	0.261 (0.212)	0.092 (0.219)	0.291 (0.238)
Male wage	−**0.136** (0.051)	0.044 (0.103)	0.032 (0.086)	0.287 (0.281)	0.029 (0.089)	−0.050 (0.091)	0.079 (0.080)
Black[a]	**0.200** (0.061)	0.022 (0.101)	−0.074 (0.100)	0.033 (0.290)	0.008 (0.104)	−0.110 (0.106)	−0.078 (0.104)
Hispanic[a]	**0.187** (0.054)	−0.136 (0.119)	−0.160 (0.107)	−0.050 (0.308)	0.100 (0.116)	**0.218** (0.107)	−0.016 (0.090)
Female wage	**0.127** (0.065)	−0.060 (0.153)	0.045 (0.108)	−0.459 (0.374)	−0.112 (0.115)	−0.008 (0.110)	0.002 (0.101)
Black[a]	−**0.200** (0.086)	0.020 (0.165)	0.126 (0.139)	−0.146 (0.449)	−0.037 (0.149)	0.064 (0.143)	−0.091 (0.150)
Hispanic[a]	−**0.188** (0.095)	0.278 (0.199)	0.181 (0.163)	0.044 (0.474)	−0.029 (0.193)	0.073 (0.183)	−0.091 (0.150)
Tax rate	−0.322 (0.492)	0.664 (1.115)	**2.846** (0.884)	0.000 (0.000)	0.000 (0.000)	−0.641 (1.036)	1.134 (1.241)
Black[a]	**1.238** (0.735)	−2.000 (1.225)	−**2.952** (1.491)	0.000 (0.000)	0.000 (0.000)	2.189 (1.401)	0.028 (1.513)
Hispanic[a]	−0.337 (0.864)	−1.482 (1.678)	−1.183 (1.500)	0.000 (0.000)	0.000 (0.000)	1.490 (1.791)	1.583 (1.694)

Notes: Welfare benefit is in units of 100 dollars/mo. Standard errors are in parentheses. Coefficient estimates that are significantly different from zero at the 10% level are in bold.

[a]Variable is multiplied.

TABLE A2
Coefficient and Standard Error Estimates on Individual Variables

	Conceive			Enter Cohabitation		Marry	
	Current Man	New Man	Become Single	Current Man	New Man	Current Man	New Man
Intercept	−5.186 (0.854)	−14.616 (1.272)	−5.489 (1.205)	−10.700 (3.817)	−19.049 (1.345)	−5.740 (1.248)	−19.530 (1.135)
Numfath	0.000 (0.000)	0.000 (0.000)	−0.158 (0.037)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Num cohab	0.000 (0.000)	0.165 (0.171)	0.357 (0.100)	0.000 (0.000)	−0.048 (0.095)	0.000 (0.000)	−0.231 (0.140)
Prev marr	0.000 (0.000)	0.292 (0.226)	0.000 (0.000)	0.000 (0.000)	0.567 (0.159)	0.000 (0.000)	−0.357 (0.205)
Prev cohab	0.000 (0.000)	0.380 (0.276)	0.000 (0.000)	0.000 (0.000)	0.875 (0.171)	0.000 (0.000)	−0.378 (0.259)
Age youngest	1.508 (0.138)	0.000 (0.000)	0.816 (0.163)	−3.638 (0.648)	0.000 (0.000)	−0.307 (0.215)	0.000 (0.000)
(Age youngest)^2	−1.491 (0.107)	0.000 (0.000)	−0.200 (0.068)	1.050 (0.264)	0.000 (0.000)	0.080 (0.092)	0.000 (0.000)
Age oldest	−0.789 (0.403)	0.190 (0.287)	0.000 (0.000)	−1.145 (0.510)	−0.768 (0.150)	−2.711 (0.422)	−1.416 (0.235)
(Age oldest)^2	0.178 (0.192)	−0.326 (0.141)	0.000 (0.000)	0.321 (0.181)	0.239 (0.062)	0.687 (0.173)	0.386 (0.076)
Age mother	0.891 (0.346)	4.391 (0.624)	−1.276 (0.264)	0.087 (0.559)	4.772 (0.525)	0.084 (0.380)	7.398 (0.513)
(Age mother)^2	−0.193 (0.051)	−0.728 (0.104)	0.085 (0.035)	0.069 (0.079)	−0.691 (0.074)	0.036 (0.056)	−0.973 (0.074)
Dur cohab	0.000 (0.000)	0.000 (0.000)	2.041 (0.405)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
(Dur cohab)^2	0.000 (0.000)	0.000 (0.000)	−1.348 (0.281)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Dur single	0.000 (0.000)	0.964 (0.278)	0.000 (0.000)	0.000 (0.000)	0.677 (0.168)	0.000 (0.000)	1.441 (0.210)
(Dur single)^2	0.000 (0.000)	−0.243 (0.087)	0.000 (0.000)	0.000 (0.000)	−0.207 (0.047)	0.000 (0.000)	−0.425 (0.056)
Black	0.425 (0.425)	0.793 (0.735)	0.045 (0.484)	−2.028 (1.758)	−0.044 (0.602)	−0.933 (0.602)	0.132 (0.601)
Hispanic	−0.144 (0.696)	−0.900 (1.342)	0.416 (0.972)	−1.669 (2.409)	−1.617 (1.203)	−3.988 (1.308)	0.537 (1.211)
Prev marr[a]	0.000 (0.000)	0.179 (0.239)	0.000 (0.000)	0.000 (0.000)	0.196 (0.179)	0.000 (0.000)	−0.276 (0.215)
Factor load	0.384 (0.343)	1.857 (0.166)	−0.687 (0.153)	0.690 (0.550)	0.914 (0.150)	1.287 (0.552)	1.995 (0.120)
Prob weight	0.303 (0.102)						

Notes: Numfath = number of the mother's children fathered by the current man; Num cohab = number of previous cohabitations; Prev marr = married in previous spell (currently single); Prev cohab = cohabited in previous spell (currently single); Age youngest = age of youngest child in mo/100; Age oldest = age of oldest child in mos/100; Age mother = age of mother in mo/100; Dur cohab = duration of current cohabitation in mo/100; Dur single = duration of current single spell in mo/100; Factor load = coefficient on the random effect; Prob weight = logit of estimated probability weight (log[pw/(1 − pw)]). Coefficient estimates on state dummies, division dummies, period dummies, year dummies, and time trends are not shown.

[a]Variable is multiplied.

censored except the last. The likelihood contribution for the M spells observed for a given woman, conditional on μ, is

$$\text{(A.1)} \qquad L(\mu) = \left[\prod_{m=1}^{M-1} L_{j(m)s(m)}(t(m), n(m), \mu)\right] \times L_{s(M)}(t(M), n(M), \mu).$$

We now describe how the likelihood function is modified to deal with uncertain dates and sequences of events.

(1) The month in which an event occurred is unknown. Suppose the month in which event j occurred during spell m in state s is not observed. We know only that the event occurred between month r and month q. In the standard case in which the month (n) in which the event occurred is known, the likelihood contribution for the pair of spells m and $m+1$ is part of the product in Equation (A1). Assuming for simplicity that spell $m+1$ is the last one and is censored at date $n(m+1)$, this part of the likelihood contribution is given by

$$L(m, m+1, \mu) = L_{j(m)s(m)}(t(m), n(m), \mu) \times L_{s^*(m+1)}(n(m)+1, n(m+1), \mu)$$

where s^* is the state occupied in spell $m+1$. If we know only that the event occurred between month r and month q, then the likelihood contribution for the pair of spells is

$$L(m, m+1, \mu) = \sum_{a=r}^{q} L_{j(m)s(m)}(t(m), a, \mu) \times L_{s^*(m+1)}(a+1, n(m+1), \mu).$$

(2) The sequence in which events occurred is uncertain. To illustrate this case, suppose the exact month in which a cohabitation began is unknown, but it is known to have begun between months r and q. And suppose a child was conceived in month o, where $r < o < q$. Then, we do not know whether the child was conceived before the cohabitation began or after. In this case, there are two events and three spells to consider: spell m (single, not pregnant), spell $m+1$ (either cohabiting and not pregnant or single and pregnant), and spell $m+2$ (cohabiting and pregnant). Let j denote the event of entering a cohabitation and let k represent the event of conception. Let s denote the state occupied in spell m, and $s^*(j(m), s)$ the state occupied in spell $m+1$ if the event that terminates spell m is $j(m)$, and $s^{**}(j(m+1), s^*)$ the state occupied in spell $m+2$ if the event that terminates spell $m+1$ is $j(m+1)$. Suppose for illustration that spell $m+2$ is the last spell, and as before let $n(m+2)$ denote the censoring date for spell $m+2$. Then, the likelihood contribution for the three

TABLE A3

Comparison of Baseline Simulation Outcomes with Actual Outcomes

	White		Black		Hispanic	
	Actual	**Simulated**	**Actual**	**Simulated**	**Actual**	**Simulated**
Mother						
Number of children born	1.71	2.04	1.89	2.42	1.99	2.13
No children	0.21	0.21	0.18	0.21	0.17	0.26
Ever cohabited	0.43	0.32	0.36	0.32	0.39	0.24
Ever married	0.89	0.87	0.62	0.74	0.82	0.78
Marital status at first birth						
Single	0.11	0.05	0.66	0.52	0.26	0.13
Cohabiting	0.03	0.03	0.04	0.04	0.06	0.04
Married	0.85	0.92	0.29	0.45	0.68	0.83
Age at first birth	25.2	24.1	21.8	21.7	23.3	22.7
Child						
Mother was single at conception	0.16	0.11	0.64	0.53	0.27	0.18
Mother was single at birth	0.08	0.03	0.59	0.44	0.20	0.10
Ever lived with no father	0.31	0.19	0.76	0.61	0.45	0.28
Proportion of childhood lived with						
No father	0.12	0.07	0.55	0.33	0.22	0.12
Married biological father	0.77	0.87	0.33	0.48	0.65	0.79
Cohabiting biological father	0.01	0.00	0.02	0.01	0.03	0.01
Married stepfather	0.06	0.05	0.07	0.16	0.07	0.07
Cohabiting stepfather	0.02	0.01	0.03	0.02	0.03	0.01
Ever experienced the following event, conditional on being at risk						
Biological father enters household	0.27	0.08	0.18	0.12	0.24	0.11
Biological father leaves household	0.25	0.16	0.45	0.31	0.31	0.21
Stepfather enters household	0.53	0.59	0.36	0.60	0.51	0.61
Stepfather leaves household	0.38	0.21	0.52	0.33	0.39	0.25

Notes: All entries are means. Actual observations are weighted by the inverse of the number of distinct event histories per woman.

spells is

$$L(m, m+1, m+2, \mu) = \left[\sum_{a=r}^{o-1} L_{js}(t, a, \mu) L_{ks^*(s,j)}(a+1, o)\right] \times L_{s^{**}(s^*,k)}(o+1, n(m+1), \mu)$$

$$+ L_{ks}(t, o, \mu)\left[\sum_{a=o+1}^{q} L_{js^*(s,k)}(o, a, \mu) \times L_{s^{**}(s^*,j)}(a+1, n(m+1), \mu)\right].$$

The first line accounts for the probability that the cohabitation began before the conception occurred ($a < o$). The second line accounts for the probability that the cohabitation began after the conception occurred ($a > o$). Note that only one event can occur in a given month.

(3) A single woman who has given birth to at least one child outside of a union since the end of her previous union (or since age 12 if she has never been in a union) conceives a child, but we cannot determine from the data whether it is with the current man (father of the most recent child) or a new man. In this case, we know that in a given month either Event 1 or 2 occurred, but we do not know which. Suppose the conception occurred in month q of spell z. The likelihood contribution for the woman in this case is

$$L(\mu) = \left[\prod_{m=1}^{z-1} L_{j(m)s(m)}(t(m), n(m), \mu)\right]$$

$$* \left\{ L_{1s(z)}(q, n(z), \mu) \left[\prod_{m=z+1}^{A-1} L_{j(m)s(m)}(t(m),\right.\right.$$

TABLE A4

Comparison of Baseline-Simulated Monthly Child Transition Probabilities (*100) among Different Family Structures with Actual Rates

	White		Black		Hispanic	
	Actual	Simulated	Actual	Simulated	Actual	Simulated
All ages						
No father to						
1. Cohabiting biological father	0.04	0.02	0.05	0.04	0.08	0.04
2. Cohabiting stepfather	0.60	0.59	0.21	0.34	0.48	0.54
3. Married biological father	0.07	0.04	0.06	0.05	0.05	0.04
4. Married stepfather	0.36	0.39	0.13	0.37	0.26	0.39
Cohabiting biological father to						
5. No father	0.75	1.12	1.18	1.24	0.79	1.13
6. Married biological father	1.64	1.98	1.71	3.17	1.24	2.05
Cohabiting stepfather to						
7. No father	1.06	1.09	1.23	1.11	0.90	1.04
8. Married stepfather	2.84	6.33	2.39	6.40	1.96	4.72
Married biological father to						
9. No father	0.19	0.11	0.74	0.23	0.40	0.14
Cohabiting biological father to						
10. No father	0.35	0.16	0.24	0.27	0.23	0.18
Ages 0–5						
No father to						
11. Cohabiting biological father	0.12	0.06	0.11	0.08	0.18	0.10
12. Cohabiting stepfather	0.64	0.67	0.19	0.32	0.44	0.56
13. Married biological father	0.20	0.11	0.14	0.11	0.20	0.10
14. Married stepfather	0.38	0.49	0.11	0.41	0.24	0.43
Cohabiting biological father to						
15. No father	0.81	1.20	1.16	1.27	1.01	1.20
16. Married biological father	1.80	2.30	1.68	3.63	1.39	2.43
Cohabiting stepfather to						
17. No father	1.45	0.97	1.09	1.06	0.81	0.97
18. Married stepfather	3.49	7.88	2.26	7.42	1.83	5.69
Married biological father to						
19. No father	0.42	0.14	1.08	0.28	0.57	0.17
Cohabiting biological father to						
20. No father	0.21	0.19	0.27	0.36	0.25	0.22

$$\times\, n(m), \mu | j = 1)\Bigg] L_{s(A)}(t_A, n(A), \mu | j = 1)$$

$$+ L_{2s(z)}(q, n(z), \mu) \left[\prod_{m=z+1}^{A-1} L_{j(m)s(m)}(t(m),\right.$$

$$\left.\times\, n(m), \mu | j = 2)\right] L_{s(A)}(t_A, n(A), \mu | j = 2) \Bigg\},$$

where $n(z-1) = q - 1$ and the conditioning on $j = 1$ and $j = 2$ indicates that the entire subsequent demographic history may depend on which event occurred.

APPENDIX B

A. Wage Rates

The mean hourly wage rate was computed for men and women aged 16–47 by year, state, and race/ethnicity from the Merged Outgoing Rotation Group files of the CPS for 1979–2004 and from the May CPS files for 1970–1978. The wage rate is computed by dividing weekly earnings by hours of work per week. Cases were included in the computation only if weekly earnings were at least $150 (in year 2000 dollars), hours of work were at least 30, and the resulting hourly wage rate was between $2.00 and $200.00. Weekly wages were topcoded at $999 from 1970 to 1988, $1,923 from 1989 to 1997, and $2,884 from 1998 on. Wages were deflated using the Personal Consumption Expenditure Deflator (PCED, base year 2000) and weighted by the sampling weight provided in the CPS files. Before 1977, some states are not separately identified, so for those years, the mean wage for the group of states (by sex, year, and race/ethnicity) is assigned to each state in the group.

Weekly earnings are given in categorical form before 1973 in the May CPS files. The midpoint of each category is used in this case, with $600 assigned for the highest category in 1972 (when the lower limit is $500), and $300 assigned in 1970–1971 (when the lower limit of the highest category is $200). In the 1973–1978 May CPS files, the continuous weekly earnings variable is missing for some cases, but the categorical version of earnings is also on the file for those years. If the categorical variable is not missing, it is used to compute the wage when the continuous measure is missing (the categories are the same in 1973–1978 as in 1972). Hispanic ethnicity is not identified in the May CPS in 1970–1972. The real 1973 means by state were used for 1970–1972 for Hispanics.

The wage rate is regressed on education dummies (four groups), age dummies (six groups), and state of residence, separately by year, sex, and race/ethnicity. A wage rate is predicted for each employed individual, holding education constant at high school graduate and age constant at 26–30. Wages are then averaged within state-year-sex-race/ethnicity cells. In order to smooth out spurious fluctuations due to small sample size in some cells, we use a 3-year moving average of wage rates, within state-sex-race/ethnicity groups. Cells with fewer than 30 cases (after averaging) are omitted. This resulted in the loss of 5.4% of the potential NLSY person-month observations, with the loss disproportionately larger for blacks and Hispanics.

B. Welfare Benefit

Data for the AFDC/TANF cash benefit for a family of four with no income are from Robert Moffitt's welfare benefits file for the years 1970–1998 (http://www.econ.jhu.edu/People/Moffitt/datasets.html). Data for 1999–2004 are from the 2004 Green Book (http://www.gpoaccess.gov/wmprints/green/index.html), the Congressional Research Service (2005), and the Urban Institute's Assessing the New Federalism Web site (http://www.urban.org/center/anf/index.cfm). In some recent years, data are only available for a family of three. The benefit for a family of four was estimated by applying the state-specific ratio of benefits for households of size three and four, which are both available for 1996–1998 and 2003–2004. The Food Stamp guarantee for a family of four is from Moffitt's database for 1970–1998, updated with data from the Web site of the Food and Nutrition Service.

C. Welfare Reform

The timing of implementation of welfare waivers and TANF are from the Web site of the Office of the Assistant Secretary for Planning and Evaluation, Department of Health and Human Services.

D. Divorce Law

The year of enactment of unilateral divorce is from Gruber (2004), Table 1.

E. Tax Rates

Tax rates are computed using the NBER's TAXSIM program. Tax rates were computed for two income levels: the poverty line for a family of three (one adult and two related children) in 2000 ($13,874) and for median family income in 2000 ($50,372), both adjusted for inflation in other years. All income was assumed to be from earnings. Child care expenditure for a poor family was assumed to be 23% of income and for a median-income family 6% of income (Johnson 2005). All children were assumed to be under 17 for purposes of the child tax credit. Taxes were computed for alternative numbers of children (0–9) and filing statuses (single, head of household, and married filing jointly). State taxes are included from 1977 to 2004 but are not included for 1970 to 1976. In married families, 60% of earnings were allocated to the husband and 40% to the wife.

F. Assigning State of Residence before the First-Survey and Between-Survey Years

Respondents were asked to report their state of residence at age 14 and at birth, but the dates of moves are not recorded. We assign state of residence for years before the first-survey year (1979) based on which reporting year (year of birth, year in which the respondent turned age 14, and 1979) is closest in time to a given calendar year. The state of residence is reported in each interview from 1979 to 1994. The state reported is assumed to apply for the entire calendar year. In 1996 and 1998, state of residence is ascertained at the survey date, but the dates of moves are not recorded. We assign the 1995 and 1997 state of residence according to which interview month in the adjacent survey year is closest in time to 1995 or 1997. In 2000, 2002, and 2004, the dates of moves between interviews were recorded, so we assign state of residence for the between-survey years according to where the respondent lived longest during the between-survey years. The survey-date state of residence is assigned to the entire calendar year for that survey year.

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