Did Welfare Cause the Increase

In Non-marital Childbearing in The United States?

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1. **INTRODUCTION**

In 1940, there were 7.1 births for every 1000 unmarried women aged 15 to 44. By 1996, there were over six times as many – 44.8 births per 1000 unmarried women in this age group. Many observers claimed that the main reason for the increase was the perverse incentives in the welfare program, *Aid to Families with Dependent Children* (AFDC), which paid benefits only to unmarried parents.\(^1\) By providing support in this way, it was asserted, informally and in formal theoretical models, that welfare encouraged, or allowed, women to bear children out-of-wedlock.

Although many experts suggested that the evidence (available at the time) did not strongly support the assertion that welfare was the main cause of the increase, policymakers apparently found the arguments to the contrary quite convincing.\(^2\) Consequently, Congress included several provisions in the *Personal Responsibility and Work Opportunity Reconciliation Act of 1996* (i.e., the 1996 Welfare Reform Act) to discourage nonmarital childbearing. First, under the *Temporary Assistance to Needy Families* (TANF) program, which replaced AFDC, states were no longer required to restrict benefits to families with only one parent. Second, states were allowed to impose ‘family caps’, which eliminated or reduced additional benefits for children conceived while the mother was enrolled in the TANF program. Third, to discourage young women from using welfare to escape parental supervision, states were prohibited from using federal funds to make payments to women under 18 who did not live at home or in another adult-supervised setting.

Since the 1996 Act was passed, new evidence has been produced linking welfare to out-of-wedlock childbearing. This paper reviews recent evidence to assess whether, and by how much, welfare benefits affect nonmarital childbearing. We conclude that recent evidence

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\(^1\) See, for example, Murray, 1984. Although a related program, *Aids to Families with Dependent Children – Unemployed Parent* (AFDC-UP), provided payments to two-parent families where the second parent was unemployed or incapacitated, this program had far more restrictions than the AFDC program. See Committee on Ways and Means (1998) for a description of the AFDC, AFDC-UP and TANF programs.
suggests that higher welfare payments do encourage women to have children out-of-wedlock, but that other cofactors have played a significant role in the post-war increase in nonmarital births. We then look at three possible cofactors – falling wages, decreases in the number of ‘marriageable’ men and changing norms of behavior – and assess the empirical evidence on their importance and the interaction between the cofactors and welfare. We also present new results on how ‘peer group’ effects and changing norms might influence nonmarital childbearing.

2. EFFECT OF WELFARE ON NONMARITAL CHILDBEARING

2.1 Cross-sectional and time-series analyses.

One commonly noted stylized fact about nonmarital childbearing is that the time-series evidence does not strongly support the hypothesis that a generous welfare system has caused the post-war increase in nonmarital births (for example, see Moffitt 1998, p. 60). The problem is that nonmarital childbearing has increased significantly since the 1970s, despite a decline in AFDC benefits. Although Medicaid expenses increased significantly in the late 1980s and early 1990s, broader measures of welfare benefits that include Medicaid and Food Stamps also declined from the mid-1970s through the late 1980s (Moffitt, 1999). Consequently, it is difficult to directly link changes in benefits to changes in nonmarital childbearing, without including other cofactors to explain the divergent trends.

Although few studies have examined the effect of welfare on nonmarital childbearing using time-series data, the time-series evidence remains mixed even after additional variables are included in the analysis. One recent literature survey, Moffitt (1995, p. 174), lists three studies (Cutright, 1970; Winegarden, 1988; and Murray, 1993) that compare trends in nonmarital childbearing with trends in welfare benefits. Of these studies, only one (Winegarden, 1988) finds a statistically significant positive relation between the two. Furthermore, Moffitt (1995)

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2 For example, in the Department of Health and Human Services Report to Congress on Out-of-Wedlock Childbearing, Moffitt (1995) wrote “a reasonable reading of the evidence to data is that the welfare system may increase nonmarital childbearing, but the magnitude of the effect may not be large relative to other factors...”

3 Since Food Stamps are available to poor two-parent families as well as to single parent families, it is not clear that it is appropriate to include them in the benefit package available to single parent families.
notes that a problem with Winegarden’s (1988) analysis makes it difficult to assess the results from this study.\footnote{Moffitt (1995, p. 170) notes that the procedure used by Winegarden (1988) to combine several possible measures of welfare generosity into a single measure essentially meant that an instrumented measure of AFDC participation rates was used as the measure of welfare benefits in this study. Since the participation rate did not move in the same direction as benefits over this period, this might lead to incorrect conclusions.} A more recent time-series analysis, Winegarden and Bracy (1997) finds that welfare benefits are positively correlated with the number of nonmarital births per single woman for both white and black women. In contrast to previous studies, they include lagged nonmarital births in their regressions to proxy for changing behavioral norms.

Although the time series evidence does not strongly support a link between welfare and nonmarital childbearing, the decentralized nature of the U.S. welfare system provides an additional way to analyze the relationship between the two. Since welfare benefits are set, within the limits of federal law, at the state level, they differ greatly between states. In 1999, benefits for a family of three under the TANF (Temporary Assistance for Needy Families) program varied from $120 in Mississippi to $923 in Alaska.\footnote{Data are from the Office of Family Assistance Webpage (http://www.acf.dhhs.gov/programs/ofa/).} As Murray (1993, p. 225) notes, this variation appears to “...provide a natural experiment for testing the proposition that welfare is linked to family breakup.” If higher welfare benefits cause increases in nonmarital childbearing, we would expect, all else being equal, states with higher benefits to have higher rates of nonmarital childbearing.

Despite the large variation in benefit levels, the early evidence was mixed. A recent literature survey (Moffitt, 1998) finds few statistically significant positive results in studies from the 1970s and 1980s. For white women, two cross-sectional studies found that higher welfare benefits were correlated with higher rates of nonmarital childbearing while two other studies found statistically insignificant correlations (Moffitt, 1998, Table 4A). For black women, one cross-sectional study found a positive correlation, while three found statistically insignificant or negative correlations.

Recent cross-sectional analyses, however, have generally found stronger results than the earlier studies, at least for white women. Moffitt (1998, Table 4A) lists six cross-sectional studies from the 1990s that found that higher welfare benefits were generally correlated with
higher rates of nonmarital childbearing among white women and two studies that found mixed results.\(^6\) In contrast, only one cross-sectional analysis found higher welfare benefits were correlated with increased nonmarital childbearing among black women, while five found mixed, insignificant or negative results (Moffitt, 1998). Moffitt (1998, p. 68) suggests that the stronger results in more recent studies might be because the effect of welfare on family formation decisions has increased over time.\(^7\)

The inconclusive evidence from early time-series and cross-sectional studies led to an extended discussion on what could be producing the contradictory results. One potential cause discussed extensively in the literature was whether omitted variables might be biasing parameter estimates.\(^8\) This is discussed in detail in the next section of the paper. A second concern, which is related to, but distinct from, concerns about omitted variables, is the possible endogeneity of welfare benefits. AFDC benefit levels are the result of decisions made directly by politicians, and indirectly by voters, in each state. Because of this, benefits might be endogenous if nonmarital birth rates affect voter’s views on welfare. For example, if some voters, concerned about nonmarital births to teens, believe teens have nonmarital births to receive welfare payments (even if they don’t), they might support benefit cuts to discourage teen childbearing. It seems plausible that some voters and politicians do believe this, given the tenor of the debate surrounding the Personal Responsibility and Work Opportunity Reconciliation Act of 1996. In addition to this concern, the public choice literature on state welfare policies shows, theoretically, that the size of the welfare population (relative to the number of taxpayers) is the fiscal price of per recipient benefits. As the number of recipients increases, it becomes more costly to pay a given per recipient benefit (Orr, 1976). Since increases in nonmarital childbearing might increase the size of the welfare population, increases in nonmarital childbearing will increase the ‘price’ of per recipient benefits to voters. Consequently, assuming altruism is a

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\(^6\) Note that this includes results from panel data studies that present cross-sectional analysis for comparison with the panel results.

\(^7\) Other evidence is also consistent with this. Using data from the PSID, Butler (1996) finds that welfare has no effect on nonmarital childbearing among low-income whites and blacks in the period 1968-79, but a significant effect in the period 1980-92.

\(^8\) Omitted variable bias and fixed effects estimation is discussed in a variety of papers including Jackson and Klerman (1994), Moffitt (1994) and Hoynes (1997).
normal good, more nonmarital births will lead to lower benefits and a classic simultaneity problem.

It is important to note that including fixed state and time effects in regressions using state-level data does not solve the endogeneity problem. Further, endogeneity could be a concern in studies using individual-level data, even if fixed effects are included in the regressions. If the error term (which may include omitted variables) is independently distributed across teens within the state, then endogeneity caused by the aggregate nonmarital childbearing rate affecting benefit levels should not be a significant concern when using individual data. Any individual teen's decision would have only a tiny net effect on aggregate birth rates and, therefore, the benefit level would not be affected by the teen’s decision. However, if there are omitted variables that are correlated across teens in the state (violating the assumption of independently distributed errors) the effect on aggregate illegitimacy rates might not be trivial. This makes endogeneity a concern whether or not the omitted variables affect benefits directly. If the omitted variables are constant over time in each state, or change by the same amount in all states, fixed state and time effects will effectively solve the endogeneity problem. However, if these omitted variables are not constant for each state over the period being studied, or change by different amounts across states, then including state and time fixed effects will not solve the endogeneity problem when using individual data.

Only one study using state-level data has attempted to correct for potential endogeneity. Using various combinations of instruments, and including state and time fixed effects, Clarke and Strauss (1998) find that welfare benefits are strongly and robustly related to nonmarital childbearing among both white and black teens. Before correcting for endogeneity, they find that welfare has a modest effect on nonmarital childbearing among white teens (elasticity of +0.2) and does not have a statistically significantly effect on nonmarital childbearing among black teens. After correcting for endogeneity, the statistically significant point estimates of the elasticities are +1.3 for white teens and +2.1 for black teens.

2.2 Omitted Variable Bias and Fixed States Effects

The other problem with cross-sectional analyses – omitted variable bias – has been discussed far more than the potential bias due to endogeneity. In the first paper to address this
issue, Ellwood and Bane (1985) suggested that unobserved state-level social and political characteristics might affect both behavioral norms regarding nonmarital birth decisions and the AFDC benefit level. In addition to difficult to measure differences in attitudes, other omitted variables (potentially measurable and potentially unmeasurable) might also be correlated with both benefit levels and the prevalence of out-of-wedlock births.

The solution to the omitted variable problem proposed by Ellwood and Bane (1985) was to include fixed state and time effects in panel data regressions (i.e., to include dummy variables for state of residence and for year or birth cohort). If the omitted variables are constant over time in each state, or change by same amount across states, then time and state dummies remove the omitted variables and allow unbiased estimation of other parameters, including the coefficient on welfare benefits. Since the omitted variables causing the most concern were behavioral norms, which might change slowly over time and might be affected by similar factors across states (e.g., portrayals of nonmarital childbearing in the media), including fixed effects appeared to be an attractive solution to the problem.

As the number of studies using fixed effects estimation grew, several papers noted potential problems with this method of analysis. First, some authors questioned whether the samples in studies using individual level data were large enough to include fixed state effects in regressions. Second, including fixed state and time effects does not correct for omitted variable bias due to variables that are not constant across states or that change at different rates. Finally, several recent studies have suggested that including state and time fixed effects in regressions might remove too much of the relevant variation in benefit levels and, in so doing, worsens bias due to measurement error.

Several authors (e.g., Jackson and Klerman, 1994 and Clarke and Strauss, 1998) have suggested it might be difficult to include fixed effects in individual level studies because sample sizes are often small. In the National Longitudinal Survey of Youth (NLSY79), data are available on family income, mother’s education, family structure and birth and marriage

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9 For example, they suggest (p. 146) that Minnesota’s Scandinavian tradition might encourage both strong family ties and generous welfare benefits.
decisions through age 19 for only 561 white teens and 281 black teens aged under 17 in 1979. Because the NLSY79 contains state of residence information only at birth, at 14 and for sample years (1979 and on), this is the entire sample for whom state of residence is observed during most of the woman’s fertile teen years. Since nonmarital births to teens are relatively rare, only 44 births are observed for white teens and only 94 births for black teens in this sample. Hence, it would be difficult to include fifty state dummies and expect to find significant results. In addition, since states without any nonmarital births are dropped from the regression, the variance of (changes in) welfare benefits could be severely reduced. In the NLSY79 sample discussed above, nonmarital births are observed in only 22 and 23 states for white and black teens respectively. At the very least, this suggests that it is important to keep sample size considerations in mind.

A second problem is that if the omitted variables change at different rates in different states, then parameter estimates will not be consistent even if fixed state and time effects are included in the regression. Moffitt (1995, p.169) argues “the results [from fixed effects estimation] could still be biased if other characteristics changed in different ways across states at the same time that benefits changed – for example, the states that lowered benefits may have experienced more economic stress than the other states.” Similarly, both benefits and the prevalence of nonmarital births might change most radically in those states where attitudes towards nonmarital childbearing change most. One possible solution to this problem might be to restrict the sample to relatively few periods. It seems plausible that problems due to omitted variables that change at different rates across states might become more severe as the period being studied lengthens. For example, even if public opinion about nonmarital childbearing and welfare did not change at substantially different rates across states between 1983 and 1984, it might have changed at radically different rates between 1960 and 1999. This would suggest that

10 For a more detailed discussion see Clarke and Strauss, 1998, Appendix B.

11 Note that state of residence is needed to assign welfare benefits correctly. Typically, studies that use data from the Panel Study of Income Dynamics (PSID) have samples that are similar in size or smaller to those that use data from the NLSY79.

12 Rosenzweig (1998) avoids sample size problems in several ways. First, he assumes that the woman’s state of residence in 1979 (when she was aged between 15 and 21) is her state of residence for all years between age 14 and 1979. Foster and Hoffman (1999), using data from the PSID, estimate than Rosenzweig (1998) misclassifies only 10% of observations using this assumption. Second, he looks at nonmarital births through age 22 rather than through age 19, increasing the number of births substantially. Finally, by using a relatively small set of control variables, he reduces sample attrition due to missing data.
there might be some advantages to keeping the period under study relatively short. However, although this might reduce problems due to omitted variables, it might worsen other problems. For example, restricting the length of the period under study will increase problems due to sample size and will reduce the variation in changes in benefit levels.

The third, and probably most discussed, problem is that including state and time fixed effects in the analysis worsens problems related to measurement error and, in so doing, might bias results downwards. Although measurement error is always a concern, it might be particularly troublesome in this case because current benefits, which most studies use to measure welfare generosity, might be a poor proxy for the true variable of interest. Since having a baby is a long-term decision, a forward-looking rational woman would be more interested in future benefits over the years that she expects to be on welfare than she is in current benefits. Since including fixed effects in the analysis removes all cross-state variation in benefit levels, the effect of welfare on nonmarital childbearing is essentially identified through the effect of changes in current benefit levels on changes in birth decisions. Hoffman and Foster (1999, p. 25) write “including cohort and state fixed effects in the model means that the effect of welfare is identified by cross-sectional variation in benefit time trends. Such blips in payment levels are transitory and one might argue that individuals ignore such variations in making decisions.” Although it is still an open question whether changes in benefits tend to be permanent or transitory, it is fair to say that measurement error is an increasing concern in the literature.

### 2.3 Results from studies including state fixed effects

#### 2.3.1 Studies using state-level data

Ellwood and Bane (1985) were the first researchers to include fixed state effects in their analysis of the effect of welfare on nonmarital childbearing. Using Census data from 1960, 1970 and 1975, they found no evidence that welfare benefits affected state-level nonmarital

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13 This paragraph draws heavily on McKinnish (1999) and Black, McKinnish and Sanders (1998), which present a far more complete discussion of the effect of measurement error on parameter estimates in fixed effects regressions. Although recent studies have focussed on measurement error problems caused by using current benefit levels as a proxy for expectations about future benefit levels, this was been noted as a problem in the earliest fixed effects studies. For example, Ellwood and Bane (1985, p. 191) write “it seems reasonable to argue that current benefit levels are an adequate instrument for expectations, but changes can be misleading.”
childbearing rates (see Table 1). A series of later studies, however, have found statistically significant results (at least for white women) for women of different ages and for different periods (see Table 1). These studies have used larger data sets from later periods than Ellwood and Bane (1985) did, with between 11 and 20 years of data generally from the mid-1970s through the early 1990s. The increased number of periods might affect statistical significance by increasing the variation in changes in benefit levels. Further, the use of data from a later period might also affect results, since several authors have noted that the effect of welfare appears to be increasing over time.\footnote{See, for example, Plotnick (1990, p. 743), Moffitt (1998, p. 68) and Butler (1996).}

The evidence from the recent studies using state-level data appears relatively consistent in terms of statistical significance and the magnitude of the estimated effect. Most notably, when state and time fixed effects are included in the analysis, increases in welfare benefits appear to be correlated with increases in nonmarital childbearing for white women (and for all women). For black women, the results are less consistent and welfare benefits are not correlated with nonmarital childbearing in two of three studies (see Table 1). In addition, even among white women, welfare appears to have only a modest effect on nonmarital childbearing once fixed effects are included in the analysis. Jackson and Klerman (1994) report elasticities of between 0.1 and 0.2 for white women. Mathew, Ribar and Wilhelm (1996) report an elasticity of 0.12 for all women and McKinnish (1999) reports elasticities of between 0.06 and 0.23 for white women and between 0.07 and 0.31 for black women. When they include fixed state and time effects, but before they correct for endogeneity, Clarke and Strauss (1998) report an elasticity of 0.18 for white teens.

As McKinnish (1999) notes, these modest results might be consistent with the presence of measurement error. She compares results from first difference, long difference and fixed effects models and concludes that the results for white women are consistent with this.\footnote{She notes that the results for black women are not consistent with the presence of measurement error in standard econometric models.} Two of the studies, Black, McKinnish and Sanders (1998) and Clarke and Strauss (1998) include results from estimation techniques that might be robust to the presence of measurement error. Black,
McKinnish and Sanders (1998) take a moving average of AFDC benefits, which they claim will smooth out transitory fluctuations in benefit levels and, therefore, reduce problems due to measurement error. Once they do this, they find slightly larger elasticity estimates (0.16 and 0.18 for white women aged 15-19 and 20-24 respectively). Clarke and Strauss (1998) use several different sets of instruments to try to correct for the endogeneity of benefit levels. However, instrumental variable techniques are also a standard way of correcting for measurement error (see, for example, Greene, 1998, p. 435-444). Once they do this, they find that welfare has a statistically significant effect on nonmarital childbearing for both white and black teens. Consistent with the presence of measurement error, parameter estimates are far larger once they use IV techniques (elasticities of 1.3 and 2.1 for white and black teens respectively). These larger results from IV estimation are also consistent with Clarke and Strauss’s (1998) interpretation of their results – that high rates of nonmarital childbearing increase the per recipient cost of benefits to taxpayers and, therefore, lead to reductions in benefits.

One concern, noted in both Jackson and Klerman (1994) and McKinnish (1999), is that state-level regressions appear to indicate that welfare benefits affect the childbearing decisions of married young women as much as, or more than, unmarried young women. This is somewhat strange, since most married women would not be eligible to receive basic AFDC benefits. Although, given the relatively high probability of marital failure for young mothers, married women might be concerned about the insurance value of welfare, it seems unlikely that they would be affected as much as unmarried women are.

2.3.2 Studies using individual-level data

Several recent studies have included fixed effects in regressions using individual level, rather than state-level, data. In general, the results from these studies are less consistent with each other than the results from the studies using state-level data. Robins and Fronstin (1995) use data from the Current Population Survey (CPS) and find that higher AFDC benefits increase

16 However, they note that empirical estimates that take into account the endogeneity of recipiency rates suggest that the elasticity of AFDC benefits with respect to the ratio of recipients to taxpayers is probably not very large. See Ribar and Wilhelm (1996) and Shroder (1995).
nonmarital childbearing among black and Hispanic women, but not among white women (see Table 2). Although they find some statistically significant results once fixed effects are included, they find stronger results when they are excluded.\textsuperscript{17} This is a sharp contrast to the findings in studies using state-level data.

In a second study, using data from the NLSY79, Rosenzweig (1998) finds that AFDC benefits have a large and statistically significant effect on nonmarital childbearing when fixed effects are included in the analysis. He finds that a 37\% reduction in benefits would result in a 7\% percentage point (43.5\%) decrease in the proportion of low-income women who have nonmarital births. His study has several notable differences from earlier studies. First, unlike previous studies that used NLSY data (i.e., Plotnick, 1990; Lundberg and Plotnick, 1990; Acs 1993), he includes fixed state (and cohort) effects in his analysis. Second, he uses a different set of independent variables, including the woman’s percentile score on the Armed Forces Qualification Test (AFQT) and estimated parental income based upon the woman’s parents’ occupations.\textsuperscript{18} Third, he shows that welfare appears to have a greater effect when he restricts the sample to individuals from low-income families. Fourth, he includes births up to age 22, rather than only teen births. Finally, he uses average benefits in the states where the woman was resident between ages 12-20, rather than rather than contemporaneous AFDC benefits, as his measure of welfare generosity. If a woman moved between states then the benefit was averaged over the states where she lived. For example, for a woman aged 14 at the beginning of 1979 who moved from Mississippi to California at the beginning of 1983, benefits would be averaged using benefits in Mississippi between 1977 and 1982 and benefits in California between 1983 and 1987.\textsuperscript{19}

Several papers have tried to reconcile the results in Rosenzweig (1998) with cross-sectional studies that have used NLSY79 data. The differences do not appear to be simply due to

\textsuperscript{17} When they omit state fixed effects, they include regional fixed effects instead.

\textsuperscript{18} He calculates this using mean earnings by occupation from the 1970 Census.

\textsuperscript{19} As noted above, in practice, Rosenzweig did not observe state of residence for many women in his sample for years between when they were 14 and 1979. For example, for women born in 1958 (the first cohort in the NLSY), state of residence is not observed for ages 1 to 13 and 15 to 20. To include these women, he assumes that state of residence in 1979 was state of residence for all missing years. For the woman born in
inclusion of fixed effects. Using a discrete hazard model, data from the NLSY79, and similar, but not identical, control variables, Foster and Hoffman (1999), find that including fixed effects reduces the size and statistical significance of results. When they omit fixed effects from the analysis, they find a statistically significant positive relationship between welfare benefits and nonmarital births for white women and all women, but not for black women (see Table 1). Once they include fixed effects in their analysis, they do not find statistically significant results for any subsample. They note (p.23) that this is consistent with Hoynes (1997) and Moffitt (1995), who find that including fixed effects reduces the size and statistical significance of the effect of welfare on female headship. The main differences between their study and Rosenzweig (1998) are that they do not average welfare benefits and that they allow the state fixed effect included in the regression to change whenever the woman moves to a different state.

Rosenzweig (1998) notes that the difference between his results and other papers does not appear to be due to the set of control variables that he uses. He notes (p. 25) that his results are similar when he drops either the AFQT score or occupational income variable. Similarly, Hoffman and Foster (1999) find similar results when they try to replicate Rosenzweig’s (1998) results using data from the PSID. Although they use an identical estimation technique, they include a slightly different set of control variables.20

In Hoffman and Foster’s (1999) replication of Rosenzweig (1998) using data from the PSID, they find that the coefficient on benefits is statistically insignificant for their low-income sample, but significant for the sample of all women. This suggests that the difference between Rosenzweig’s (1998) results and earlier results is not due to the sample restriction. However, the difference in results might be because the sample in Hoffman and Foster becomes quite small once they restrict it to women from low-income families (880 women). Robins and Fronstin (1995) also find that welfare appears to affect the childbearing decision of women with no high school diploma, but not women with a high school diploma. Overall, although not conclusive, these results suggest that welfare benefits might affect the childbearing decisions of women from

1958, her state of residence in 1979 (when she turned 21) was assumed her state of residence for ages 15 to 20. Based upon PSID data, Foster and Hoffman (1999) estimate than Rosenzweig (1998) misclassifies only 10% of observations.

20 In general, their results were somewhat weaker, but this not be surprising given that the sample was considerably smaller.
disadvantaged backgrounds more than the decisions of other women. However, this does not appear to explain the difference in results in Rosenzweig (1998) and other studies.

Hoffman and Foster (1999), and Foster and Hoffman (1999) note that it is possible that the inclusion of older women leads to stronger results. Both papers find that welfare appears to affect the childbearing decisions of women in their early 20s more than it affects the decisions of younger women. However, this does not appear to be the complete explanation, since the results in Hoffman and Foster (1999) are not statistically significant for older women (or younger women) once they include fixed effects in the analysis.

Another plausible reason for the difference between results in Foster and Hoffman (1999) and results in Hoffman and Foster (1999) and Rosenzweig (1998) is that the studies use different measures of welfare benefits. Hoffman and Foster (1999) and Rosenzweig (1998) average benefits over the states that the woman lives in between the ages 12 and 20. In contrast, Foster and Hoffman (1999) use contemporaneous benefits in the woman’s actual state of residence for each year that she is at risk of having a nonmarital birth. McKinnish (1999) notes that the procedure used by Rosenzweig (1998) and Hoffman and Foster (1999) might reduce the downward bias due to measurement error and, consequently, lead to stronger results (see the discussion of measurement error in Section 2.2).

Rather than reducing measurement error, the difference in results might be because the fixed effects remove less of the variation in AFDC benefits in Rosenzweig (1998) and Hoffman and Foster (1999) than they do in Foster and Hoffman (1999). For example, consider a 14-year girl in 1979 who moves from Mississippi to California in 1983. The welfare benefit used by Rosenzweig (1998) and Hoffman and Foster (1999) is the average of benefits in Mississippi (from 1977 to 1982) and California (from 1983 to 1987). However, the state fixed effect included for this woman will be for Mississippi (not for California). Since the coefficient on the ‘fixed effect’ for Mississippi will primarily be determined by women who do not move states, the state fixed effect will remove less of variation from the measure of welfare benefits.

One problem with this procedure is that the measure of welfare benefits might not be appropriate. It seems unlikely that a 21-year old woman will be influenced by the welfare benefits in the state where she lived when she was 12, unless she plans to return to that state. In
particular, a rational individual who moved from Mississippi to California (and did not intend to move back) would not allow the benefit level in Mississippi when she was 12 to affect her predictions of future benefit levels in California. In addition, because of this procedure, it is unclear whether the fixed effects are good controls for omitted variables. On the one hand, if the woman’s values concerning family formation decisions are set at an early age, then the fixed effect for state of residence in 1979 might be an appropriate control. For example, a woman who lives in Mississippi for most of her teenaged life might retain the values she learns in that state even if she moves to California. Further, her parents and other family members are also likely to retain these values and this will likely influence her choice. However, if the fixed effect is controlling for marriage or work opportunities, or if the woman’s values change when she moves, then residence in 1979 might not be as good as state of residence at time of birth. Further, Hoffman and Foster (1999, p. 19) note that this procedure might bias results upwards if young women move to states with generous welfare benefits when they are planning or anticipating a nonmarital birth.

Results presented in this paper (see Table 3) are more consistent with the possibility that women who move states, rather than the process of averaging benefits to reduce measurement error, drive the results. We find that when we include state fixed effects for the woman’s state of residence at age 14 and a control for state-level benefits in the woman’s state of residence at age 14 that the significance level tends to fall. This is despite the fact that the benefit level is averaged (for the state of residence at age 14) to get a better measure of the expected future value of welfare benefits (see Table 3). Since this evens out transitory fluctuations in benefit levels (see Black, McKinnish and Sanders, 1998), but does not correct for women who move states, it suggests that women who move states might be driving the results in Rosenzweig (1998).

In summary, recent studies using individual and state-level data that include fixed state (and time or cohort) effects have often found that welfare has a statistically significant effect on

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21 One additional problem with including state fixed effects for state of residence in 1979 (see, for example, Table 3) is that it treats women born in different years differently. For example, a woman aged 14 in 1979 who lived in Mississippi until she was 20 and then moved to California would have a dummy variable for Mississippi included. In contrast, a woman aged 21 in 1979 who lived in Mississippi until she was 20 and then moved to California would have a dummy variable for California included.
nonmarital childbearing. However, the results from studies using individual level data tend to be weaker than the results from studies using state-level data. In fact, two of the studies that use individual data find that including fixed state effects reduces the significance level of their results (see Table 2), while only one of the studies finds stronger results.\footnote{Rosenzweig does not present results without fixed state effects.} This is consistent with studies using individual data looking at the effect of welfare on other family formation decisions, which have often found that including fixed effects reduces the significance of results.\footnote{Moffitt (1994) and Hoynes (1997) both find that including fixed effects in female headship regressions reduces the significance of results. Similarly, Argys et al. (1999) finds that including state fixed effects in a regression looking at the effect of incremental welfare benefits on decisions of women on welfare to have additional children makes their results become statistically insignificant.} In contrast, both studies using state-level data that present results with and without fixed state effects find stronger results when fixed state effects are included (see Table 1).

Another observation is that although several studies that have included fixed effects have found significant effects on the behavior of black women, results for white women are generally stronger and more consistent across studies (See Table 1 and Table 2). This is consistent with the evidence from the cross-sectional analyses presented in the previous section. Although these results are not conclusive and some issues need to be resolved in both individual and state-level analyses, they appear to broadly support the hypothesis that welfare affects nonmarital childbearing decisions, at least among white women.

3. CHANGES IN NORMS AND INTERDEPENDENT PREFERENCES

Although recent research suggests that the welfare system has encouraged nonmarital childbearing in the United States, it is difficult to attribute the post-war increase in nonmarital childbearing to welfare alone. First, as noted in Nechyba (1999), although nonmarital childbearing is more common among the low-income population, it is not confined to this group. In 1992, 5% of births to women with 16 or more years of education and 20% of births to women with between 13 and 15 years of education were nonmarital (U.S. Department of Health and

\footnote{In fact, since Rosenzweig (1999) includes welfare benefits averaged over ages 12-20, women who move to states with higher welfare benefits after a nonmarital birth, but before the age of 20, might also bias results.}
Although this is far lower than for less educated women – nearly 55% of births to women with between zero and eleven years of education are nonmarital – it is a significant number. The literature on marriage and childbearing among middle class women has tended to focus on the increase in female wages relative to male wages, which has reduced the gains to marriage, and on ‘marriage taxes’. However, several of explanations discussed in this section, including changes in norms and the technology shock due to improved contraception and increased access to abortion, might also apply to middle-class women.

Second, as discussed above, between the early 1970s and late 1980s, welfare benefits declined while nonmarital childbearing increased. The divergent trends in nonmarital childbearing and welfare benefits suggests that some other factor, whose influence dominated the effect of decreasing welfare benefits, caused the increase in nonmarital childbearing. This interpretation is consistent with evidence from panel data studies, which have often found (especially for white women) an increasing trend in nonmarital childbearing even after controlling for changes in welfare benefits and other variables. In the next section, we discuss three possible reasons suggested in the sociology and economics literatures for the increase in nonmarital childbearing. These are:

- Declining wages at the bottom of the income distribution.
- Declining numbers of ‘marriageable men.’
- Changes in norms regarding nonmarital childbearing.

3.1 Reconciling results from recent studies with time-series evidence.

Although welfare benefits have been declining since the mid-1970s, wages at the bottom of the income distribution have also declined in the United States. For example, average earnings for men aged between 25 and 34 with a ninth grade education or less fell in real terms

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25 In comparison, less than 4% of total births were nonmarital in 1940.
26 See, for example, Alm and Whittington (1995a,b), Sjoquist and Walker (1995) and Wood (1995).
27 For example, Butler (1996), using data from 1969-1992, found “when year of observation was included as a continuous variable, it showed an increase in the likelihood of a nonmarital birth over time for white women, but not for black women.” (p. 18). Similarly, Robins and Fronstin, who use data from 1980-1988, found “the year dummies indicate a mild upward trend from 1980 through 1987 in the probability of having additional children.” Finally, Clarke and Strauss (1998, p. 850) note “for white teens, the coefficients on time dummies generally trend upward over time.”
from $20,577 in 1974 to $15,841 in 1997. Noting that it is the relative value of welfare benefits, not the absolute value, that is important, Becker (1991, p.16) observed:

... my analysis of the marriage market indicates that the incentive to remain single depends upon income while single relative to income expected if married. The real wage of young high school male dropouts and the lowest quartile of graduates has dropped by more that 25% over the past 15 years and these young men may have become less attractive marriage partners for other reasons as well.

However, it is unlikely that this explains the entire increase in nonmarital childbearing. First, several observers have noted that wages have fallen more slowly than benefits. Figure 1 shows a graph of the ratio of welfare benefits to the mean earnings of male high school dropouts. While nonmarital childbearing has been increasing, especially for white women, the ratio of benefits to wages has been falling or flat. In addition, several of the panel data studies have included measures of wages in their regressions and have still found upward trends in nonmarital childbearing. For example, Clarke and Strauss (1998) find an upward trend in nonmarital births remains for white women after controlling for welfare benefits and median male and female wages for individuals aged 20-35 with a high school education or less.

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28 Data is from the Census Bureau webpage (http://www.census.gov/)

29 See, for example, Moffitt (1999a), which notes “some have argued that wages for less-skilled workers have also been falling, this time-series evidence against a welfare effect is vitiated. That is not the case, however, for benefits have been falling faster than aggregate wages.” (p. 1)

30 Further, Clarke and Strauss (1998) uses a measure of benefits that includes Medicaid and Food Stamps.
Another related hypothesis is that the increase in nonmarital childbearing is the result of a decline in the number of ‘marriageable men.’ According to this hypothesis, which was offered by Wilson (1987) to explain the increase in inner-city poverty, the declining number of relatively high-paying manufacturing jobs has isolated the poor residents of these areas. This has led to men leaving the labor force and has resulted in a breakdown of the family and an increase in criminal activity in these communities. In his book, *The Code of the Street*, Anderson describes how the decline in job opportunities for men might have led to the increase in nonmarital childbearing in black, inner-city neighborhoods (p. 149):

“Each sexual encounter generally has a winner and a loser. The girls have a dream, the boys a desire. The girls dream of being carried off by a Prince Charming who will love them, provide for them, and give them a family. The boys often desire either sex without commitment or babies without responsibility for them. It becomes extremely difficult for the boys, in view of their employment prospects, to see themselves taking on the responsibilities of conventional fathers and husbands. Yet they know what the girls want and play that role to get sex. In accepting a boy’s advances, a girl may think she is maneuvering him toward a commitment or that her getting pregnant is the nudge he needs to marry her and give her the life she wants. What she does not see is that the boy, despite his claims, is often incapable of giving her that life, for in reality he has little money, few prospects for earning much, and no wish to be tied to a woman who will have a say in what he does.”

Akerlof et al (1996, p. 305) suggest that the empirical evidence supports this hypothesis only weakly. Similarly, in a survey on the effect of male labor market conditions on nonmarital childbearing, Duncan (1995) also notes that although male labor markets might affect nonmarital childbearing, the results are somewhat mixed. For example, using SMSA-level data, Wood (1995) estimates that the decline in the number of high earning, young black men explained only 3-4% of the decline in the marriage rate for black women in the 1970s (p. 188). Further, Wood (1995) finds that there is no evidence that a lack of ‘marriageable men’ affected marriage among white women over this period.\footnote{The independent variable used by Wood (1995) is the ratio of high-income black men aged 25-34 to black women. Wood (1995) also concludes that welfare benefit levels had little effect, since they were falling for much of the 1970s.} Since the recent increase in nonmarital childbearing has been more pronounced among white women, it is unclear whether this hypothesis can fully explain the upward trend in nonmarital childbearing.
Studies looking at the effect of welfare on nonmarital childbearing have tried to control for the decline in the number of marriageable men in a variety of ways. Control variables include the unemployment rate (e.g., Ac, 1993; Ellwood and Bane, 1985; Plotnick, 1990), the ratio of males to females (Winegarden and Bracy, 1997) and the incarceration rate (Clarke and Strauss, 1998). However, most of these controls are only indirect measures of the process described by Wilson (1987) and Anderson (1999). For example, cyclical changes in unemployment, which are likely to dominate other changes in fixed effects regressions, are unlikely to have a large effect on long-term decisions such as childbearing. In addition, many of the studies include measures only at an aggregate level. Ratios of men to women at the state or national level might fail to properly measure the shortage of ‘marriageable men’ at the local level. Since the controls have been relatively weak, it is possible that time dummies and time trends in panel data and time-series regressions are partially capturing this effect.

Another explanation for the change in nonmarital childbearing is that changing norms regarding nonmarital births have made women more willing to bear children outside of marriage. Evidence from surveys suggests that people are becoming more accepting of nonmarital childbearing. This explanation is especially attractive because, in contrast to the two previous hypotheses, it would appear to be consistent with the observation that nonmarital childbearing has increased among the non-welfare population.

Two recent papers, Akerlof et al (1996) and Nechyba (1999), have presented theoretical models that explain the change in norms and link this change to the increase in nonmarital childbearing. In the first paper, Akerlof et al (1996) suggest that improvements in birth control and the legalization of abortion in the 1960s might have changed attitudes towards premarital sex. They argue that the legalization of abortion and introduction of the birth control pill made some women more willing to engage in premarital sex without an explicit, or implicit, promise of marriage if they were to get pregnant. In turn, this put pressure on women who rejected the new technologies or who wanted children by decreasing their ability to bargain for a marriage guarantee. Further, they suggest that these changes might also decrease men’s willingness to
marry after the fact if the woman rejected abortion. They note (p. 308), however, this theory would suggest a sudden increase in nonmarital childbearing at the time of innovation, rather than a slow and continuing increase over the past 30 years. They propose, therefore, that the technology shock might have been caused a slow endogenous change in norms, which resulted in the slow increase. They write (p. 309):\footnote{They also suggest (p. 308-9) that it might have taken some time for men to realize that an implicit or explicit promise of marriage if the woman became pregnant was too high a price to pay for sexual relations.}

“[One] reason why the decline in the shotgun marriage ratio occurred gradually, rather than abruptly, relates to stigma. Declining stigma of out-of-wedlock childbirth was a natural, endogenous consequence of the technology shock. A decline in stigma... further reinforced the technology-driven causes for the decline in shot-gun marriage and increased retention of out-of-wedlock children.”

Although Nechyba (1999) also suggests that the increase in nonmarital childbearing was the result of changing norms, he attributes the change in norms to the increase in welfare payments in the 1950s, 1960s and early 1970s. In his theoretical model, stigma is a function of the number of women in past generations who have had nonmarital births. He argues that under reasonable assumptions about how the number of women with nonmarital births affects stigma and about the joint distribution of ability and utility from children, the economy can have two stable steady state equilibria. In the absence of welfare, the economy is stuck in an equilibrium with little or no out-of-wedlock childbearing. However, the introduction of welfare can move the economy from the low equilibrium to a high equilibrium, where nonmarital childbearing is more common. He then shows that if the economy has moved sufficiently far along the path towards the high equilibrium, nonmarital childbearing can continue to increase even if welfare payments are reduced or eliminated.\footnote{Nechyba (1999) notes that his model is also consistent with the increase in nonmarital childbearing among women who do not participate in the welfare system.}

He argues (p. 35):

“More specifically, the model presented in this paper suggests that the introduction of financial incentives for out-of-wedlock births through AFDC can result in gradual changes in how illegitimacy is viewed. This in turn can lead to gradually increasing levels of illegitimacy and single motherhood among both AFDC populations as well as those not choosing to accept AFDC. Furthermore,

\footnote{Between 1974 and 1985, the percentage of people who agreed that there was no reason why single women should not have children increased from 30.7% to 45.3%. Similarly, the number of people who said it was acceptable for their own daughter to have a child outside of marriage increased from 7.7% to 13.9% (Pagnini and Rindfuss, 1993).}
after a certain time, cultural changes (in terms of how illegitimacy is viewed) may progress to a point past which elimination of AFDC does little in the way of reducing the problem of illegitimacy.” © 1999 by Thomas J. Nechyba.

Based upon their theoretical models both papers conclude that eliminating welfare payments to single mothers might not have a large effect on nonmarital childbearing, despite increasing poverty among this group. Although this might not be surprising in Akerlof et al’s (1996) model, since technology not welfare drove the change in norms, in the model presented by Nechyba (1999), it holds even though welfare caused the original change in norms.

3.2 Empirical Results on Nonmarital childbearing and changes in norms.

The theoretical models discussed in the previous section link the increase in nonmarital childbearing to shocks – the growth of welfare benefits in Nechyba (1999) and technology shocks in Akerlof et al (1996) – that slowly reduced societal disapproval of nonmarital childbearing. Although this story is consistent with the observation that both nonmarital childbearing and the acceptance of nonmarital childbearing have increased in recent years, it is possible that causality runs primarily in the opposite direction. As nonmarital childbearing increases, individuals are more likely to be a single parent or to be friend (or child, sibling or parent) of a single parent and, therefore, might be less likely to condemn it. Further, there is little evidence on how much stigma affects individuals’ marriage and childbearing decisions or on how much increases in nonmarital childbearing change behavioral norms. Given that the theoretical papers have profound policy implications, assessing the magnitude of the effect seems an important goal.

Although several recent papers looking at the effect of welfare on nonmarital childbearing have tried to relate past changes in nonmarital childbearing to present changes, results have been mixed. Winegarden and Bracy (1997) use annual data for the United States from 1973 through 1992 and conclude that lagged nonmarital childbearing rates are significantly correlated with current rates even after controlling for welfare benefits and other variables. Further, they find that once they control for this, welfare benefits have a statistically significant

positive effect on nonmarital childbearing for both black and white women. However, the time trend that they include in their analysis remains statistically significant and positive, suggesting that this does not fully explain the change in behavior. In contrast, Clarke and Strauss (1998), using state-level data from 1982 to 1990, find no correlation between lagged and current nonmarital childbearing rates for either white or black teenagers. Further, the statistically insignificant point estimate of the parameter on lagged rates is negative for white teens.

This is not, however, the only plausible way to test this hypothesis. Nechyba (1999) suggests that if the effect of stigma and social acceptance is local then it is plausible that changes in nonmarital childbearing in a single community might primarily affect norms within that, and neighboring, communities. This suggests that one way to test the whether changes in norms are important is to look at how much neighborhood characteristics affect the probability that a woman will have a nonmarital birth after controlling for other factors. Although the observation that nonmarital childbearing is higher in some communities than in others is consistent with this hypothesis, it does not imply stigma plays an important role in nonmarital childbearing. First, characteristics of families and individuals (e.g., income and education level) are probably correlated within communities. Consequently, ‘neighborhood’ or ‘peer group effects’ might pick up the effect of omitted individual or family level variables. Further, even after controlling for individual level and economic variables (e.g., local wages and welfare benefits), it is possible that factors other than stigma are the cause. For example, a woman in a high poverty neighborhood might be more likely to have a nonmarital birth than a similar woman in a wealthier neighborhood due to the omission of other community level variables, such as marriage opportunities. The importance of neighborhood-level variables would probably be predicted by other hypotheses such as a lack of ‘marriageable men’.

In a survey looking at the effect of neighborhood conditions, Duncan (1995) concludes:

“Studies of neighborhood effects show that, even after adjusting for differences in the family characteristics of women raised in different kinds of neighborhoods, growing up in a resource-rich neighborhood is associated with a lower incidence of both early sexual intercourse and nonmarital childbearing. What neighborhood

\[^{36}\] He notes that the degree of interaction between communities rather than the physical distance between them is the important ingredient when assessing spillovers.
characteristics matter most varies from study to study. Early intercourse appears most likely in neighborhoods in which monitoring the behavior of adolescents is most difficult. Nonmarital childbearing is least frequent in neighborhoods with greater concentrations of high SES families. Whether the greater resources, higher quality public services, stronger role models or some other feature of more affluent neighborhoods matters the most has yet to be discovered in this line of research.” (p. 182).

None of the studies in Duncan (1995) appears to directly control for past (or current) levels of nonmarital childbearing.

The effect of including neighborhood or peer group-level variables on estimates of the importance of welfare is also not clear. Few studies looking at the effect of welfare on nonmarital childbearing control for local (i.e., less than state-level) characteristics other than unemployment. One notable exception is An et al (1993), which includes a variable indicating whether the teenager lives in a bad neighborhood. However, the variable is not statistically significant at conventional levels. Further, the coefficient indicates that, after controlling for a selection of family background and economic variables, bad neighborhoods reduce rather than increase the likelihood of a nonmarital birth.

3.3 Additional Results on Interdependent Preferences and Changes in Norms.

3.3.1 Model

In this section of the paper, we provide some additional evidence regarding neighborhood/peer effects and see how this affects the estimates of the effect of welfare benefits on nonmarital childbearing. The data that we use is from the National Longitudinal Survey of Youth (NLSY79). So that the results can be compared with other studies that have used this data set, we pick a set of independent variables similar to those used in Lundberg and Plotnick (1995).

37 They define ‘bad neighborhood’ as the sum of positive response to whether the following things were problems in their neighborhood (i) burglaries and robberies, (ii) muggings, rapes, pushers, junkies or too few police, (iii) crowded areas with too many people, too much noise, and bad traffic; (iv) a poor neighborhood for kids, (v) unkempt houses, yards and lawns or infrequent garbage collection. (p. 202).
The dependent variable is the probability that the teenager gives birth out-of-wedlock before age 22 using Logit estimation. The model is, therefore:

\[
\text{Probability (birth before age 22)} = \alpha + \beta_1'X_i + \beta_2'X_s + \beta_3'X_n
\]

where \(X_i\) is a vector of individual control variables used in Lundberg and Plotnick (1995), \(X_s\) is a vector of state-level controls (for state of residence at age 14) and \(X_n\) is the measure of neighborhood/cohort quality. The measure that we use in this study is the percentage of students classified as disadvantaged in the (most recent) high school attended by the woman.

3.3.2 Results

In Table 3, we present results for all women, white women and black women using individual control variables similar to those in Lundberg and Plotnick (1995). When fixed effects are omitted from the regression, AFDC benefits (in the woman’s state of residence at age 14) appear to encourage nonmarital births among all women and among white women. However, benefits do not have a statistically significant effect on nonmarital childbearing among black women (see Table 3). Once fixed effects are added, the point estimates of the coefficients on AFDC benefits increase in magnitude for all samples. However, they become statistically insignificant for both the white and black subsamples and drop to a 10% significance level for the sample of all women. Although the control variables, the measure of welfare benefits, the age groups included in the analysis and the estimation technique used are all different, these results are broadly consistent with Foster and Hoffman (1999).
In Table 4, we include additional state-level controls, a measure of the women’s cognitive ability (her percentile score on the AFQT) and the percentage of the woman’s high school class that was classified as disadvantaged. The inclusion of these variables increases the size of the coefficient on AFDC benefits and makes the coefficient statistically significant for black women as well, when state fixed effects are excluded. When fixed effects are included, the coefficients on welfare benefits remain statistically insignificant.

The changes in results for the AFDC benefit appear to be partially due to the inclusion of the peer effects variable. When the peer effects variable is dropped from the analysis but the other additional controls (e.g., AFQT score) are retained, the coefficient on AFDC benefits for black women becomes statistically insignificant. Further, the point estimates of the coefficients on the AFDC variable decrease for all women and white women, although they remain slightly larger than the results in the previous table.

If peer group behavior affects the woman’s behavior, we might expect women who attended high school in poorer areas to be more likely to have nonmarital births than women who attend high school in wealthier areas. The results are, in general, consistent with this hypothesis. When state fixed effects are omitted from the analysis, we find that women who attended high schools with large numbers of disadvantaged students were more likely to have nonmarital births. The coefficients on this variable are

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41 See Clarke and Strauss (1999) for a discussion of the effect of cognitive ability on nonmarital childbearing.
statistically significant at a 5% level for all women and black women and at a 10% level for white women. Including state fixed effects does not affect the results for all women or the results for black women, but makes the coefficient on the peer group variable become statistically insignificant for white women.

Moving from a high school where many students are disadvantaged to one where few students are has a reasonably large effect on the probability that a woman will have a nonmarital birth by age 22. The probability that a white woman from a mother-only family who attends religious services infrequently has a nonmarital birth by age 22 increases from 15% to 20% when the percentage of disadvantaged students in her high school increases from one standard deviation below the mean to one standard deviation above the mean (see Figure 2). For a similar black woman, the probability increases from 52% to 60%.

In summary, the results in this subsection support the hypothesis that peer group or neighborhood characteristics affect the probability that a woman will have a nonmarital birth by age 22. This result is consistent with Nechyba’s (1999) hypothesis that stigma might affect the likelihood of nonmarital births. However, it is not a direct test of Nechyba’s (1999) hypothesis, since the variable might be proxying for other neighborhood characteristics (e.g., the availability of ‘marriageable men’). Including the proxy for peer group effects – the percentage of the woman’s high school class that is classified as disadvantaged – in the regression seems to increase the size and statistical significance of the coefficient on welfare benefits. In particular, the coefficient on welfare benefits becomes statistically significant for black women once the peer group effects variable is included in the analysis.

4. CONCLUSION

In the title to this paper, we asked whether welfare caused the increase in nonmarital childbearing in the United States. As noted in Moffitt (1998), this question should not be confused with a second question – do increases in welfare payments increase the probability that a given woman will have a nonmarital birth? Although questions concerning appropriate econometric techniques remain – in particular how to control for omitted state-level characteristics without relying on transitory changes in benefits and how important the endogeneity of benefit levels is – recent evidence suggests that the answer to the second question
is ‘yes’. Since most theoretical models with rational behavior suggest that a welfare program that only pays benefits to unmarried mothers will increase this behavior, at least at the margin, this should not come as a surprise to most economists.

Although the weight of recent evidence suggests that economic incentives affect childbearing decisions, some questions remain. Although several studies (e.g., Rosenzweig, 1999 and Clarke and Strauss, 1998) have found that welfare has a large effect on nonmarital childbearing, other studies (e.g., Black, McKinnish and Sanders, 1998 and Jackson and Klerman, 1994) find far smaller effects. Ultimately, the magnitude of the effect is more important than simple statistical significance and, therefore, pinning down more precise parameter estimates and reconciling results from different studies is vital. Another important question is why results for black women tend to be weaker than results for white women. This appears to be true in both cross-sectional and panel data analyses. It is plausible that the main reason is that sample sizes, even in state-level studies, are smaller for blacks, allowing less precise estimates of parameters. However, it is also possible that omitted factors are driving the results for black women. Solving this question might provide insight into the missing factors that are driving time-series results. Finally, recent theoretical models, where changes in norms of behavior that affect nonmarital childbearing, have important policy implications. Consequently, evidence on the importance of the changing norms would be useful. If neighborhood or peer group effects are important determinants of childbearing behavior, then linking these factors to changing norms also appears to be an important goal.

This, however, does not answer whether welfare caused the increase in nonmarital childbearing in the United States. Certainly, the time-series evidence suggests that if welfare is one the factors that led to the increase in nonmarital childbearing, it did it only with the help of other co-determinants. Time dummies and time trends appear to have a large effect on results in panel data and time-series studies, even after controlling for welfare benefits and other factors. Answering the question of what, and how important, these other factors are will allow us to estimate the effect of changes in benefits more precisely and will improve policy advice.
5. References


Table 1: Summary of studies using aggregate data and including state (and year) fixed effects.

<table>
<thead>
<tr>
<th>Study</th>
<th>Sample</th>
<th>Age</th>
<th>Period</th>
<th>Excluding fixed state effects</th>
<th>Including fixed state effects</th>
<th>Comments</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ellwood and Bane (1985)</td>
<td>All</td>
<td>15-44</td>
<td>1960,70,75</td>
<td>----</td>
<td>Insig</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Whites</td>
<td>15-44</td>
<td>1960,70,75</td>
<td>----</td>
<td>Insig</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Blacks</td>
<td>15-44</td>
<td>1960,70,75</td>
<td>----</td>
<td>Insig</td>
<td></td>
</tr>
<tr>
<td>Jackson and Klerman (1994)</td>
<td>Whites</td>
<td>15-24 (single years)</td>
<td>1975-88</td>
<td>----</td>
<td>Pos.</td>
<td>Results for black women are for total, rather than nonmarital, fertility. For total fertility, they find neg. and significant results for white women when state and year fixed effects are excluded.</td>
</tr>
<tr>
<td></td>
<td>Blacks</td>
<td>15-24 (single years)</td>
<td>1975-88</td>
<td>----</td>
<td>Insig. (see comments)</td>
<td></td>
</tr>
<tr>
<td>Mathews, Ribar and Wilhelm (1996)</td>
<td>All</td>
<td>15-44</td>
<td>1978-88</td>
<td>----</td>
<td>Pos.</td>
<td>Results are similar when state-level time trends are included</td>
</tr>
<tr>
<td></td>
<td>Blacks</td>
<td>15-19</td>
<td>1980-90</td>
<td>Neg.</td>
<td>Insig. (see comments)</td>
<td></td>
</tr>
</tbody>
</table>

Note: Neg. means coefficient is statistically significant and negative (i.e., counter to theory, higher benefits make births less likely) Pos. means coefficient is statistically significant and positive (i.e., consistent with theory, higher benefits make births more likely) Insig. Means coefficient is statistically significant. In all studies, we picked the author’s preferred specification if there was more that one
Table 2: Summary of studies using individual-level data and including state (and cohort) fixed effects.

<table>
<thead>
<tr>
<th>Study</th>
<th>Sample</th>
<th>Age</th>
<th>Period</th>
<th>Excluding fixed states effects</th>
<th>Including fixed state effects</th>
<th>Comments</th>
</tr>
</thead>
<tbody>
<tr>
<td>Robins and Fronstin (1994)</td>
<td>All</td>
<td>18-30</td>
<td>1980-88</td>
<td>Pos.</td>
<td>Insig</td>
<td>Model is a bivariate probit model, corrected for selectivity bias, with state and time effects included. AFDC benefits also affect childbearing for women of all races with no HS diploma.</td>
</tr>
<tr>
<td></td>
<td>White</td>
<td>18-30</td>
<td>1980-88</td>
<td>Pos.</td>
<td>Insig</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Model was a bivariate probit model, corrected for selectivity bias, with state and time effects included. AFDC benefits also affect childbearing for women of all races with no HS diploma.</td>
</tr>
<tr>
<td></td>
<td>White (low income)</td>
<td>12-22</td>
<td>1968-86</td>
<td>----</td>
<td>Pos.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Black (low income)</td>
<td>12-22</td>
<td>1968-86</td>
<td>----</td>
<td>Insig.</td>
<td></td>
</tr>
<tr>
<td>Foster and Hoffman (1999)</td>
<td>All</td>
<td>12-22</td>
<td>1968-92</td>
<td>Insig.</td>
<td>Pos.</td>
<td>Used data from PSID to confirm results in Rosenzweig (1999). Results for low-income sub-sample were not statistically significant at conventional levels.</td>
</tr>
<tr>
<td></td>
<td>Whites</td>
<td>14-30</td>
<td>1979-95</td>
<td>Pos</td>
<td>Insig.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Blacks</td>
<td>14-30</td>
<td>1979-95</td>
<td>Insig.</td>
<td>Insig.</td>
<td></td>
</tr>
</tbody>
</table>

Note:  
Neg. means coefficient is statistically significant and negative (i.e., counter to theory, higher benefits make births less likely)  
Pos. means coefficient is statistically significant and positive (i.e., consistent with theory, higher benefits make births more likely)  
Insig. Means coefficient is statistically significant.  
In all studies, we picked the author’s preferred specification if there was more that one
Table 3: Probability that a woman gives birth by age 22 regressed on AFDC benefits and individual controls (Logit).

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Women</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Probability of birth by age 22</td>
<td>5317</td>
<td>5290</td>
<td>1341</td>
<td>1321</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Observations</td>
<td></td>
<td></td>
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<tr>
<td>Number of States (inc. District of Columbia)</td>
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<tr>
<td>Cohort Dummies Included</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>State Dummies Included</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Level Controls</td>
<td></td>
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</tr>
<tr>
<td>AFDC payment for a family of 4 ++</td>
<td>0.0818*** (3.56)</td>
<td>0.2641* (1.86)</td>
<td>0.1031*** (2.80)</td>
<td>0.1294 (0.69)</td>
<td>0.0340 (0.97)</td>
<td>0.1200 (0.70)</td>
</tr>
<tr>
<td>Percentage of counties with an abortion provider</td>
<td>-0.3176* (-1.80)</td>
<td>2.8678** (2.37)</td>
<td>-0.3729 (-1.42)</td>
<td>-0.5462 (-0.30)</td>
<td>-0.4111 (-1.50)</td>
<td>5.0838** (2.49)</td>
</tr>
<tr>
<td>Individual and Family Controls</td>
<td></td>
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</tr>
<tr>
<td>Foreign Language Spoken at Home When Growing Up</td>
<td>-0.0953 (-0.59)</td>
<td>-0.0946 (-0.58)</td>
<td>-0.0831 (-0.38)</td>
<td>-0.1062 (-0.48)</td>
<td>-0.4047 (-1.22)</td>
<td>-0.2884 (-0.83)</td>
</tr>
<tr>
<td>Lived with mother only at age 14</td>
<td>0.7831*** (8.05)</td>
<td>0.7754*** (7.85)</td>
<td>1.2717*** (7.62)</td>
<td>1.2132*** (7.01)</td>
<td>0.4925*** (3.68)</td>
<td>0.5005*** (3.62)</td>
</tr>
<tr>
<td>Lived in ‘other’ family type (i.e., not both parents) at age 14</td>
<td>0.7004*** (6.89)</td>
<td>0.7118*** (6.90)</td>
<td>0.7437*** (4.41)</td>
<td>0.7371*** (4.26)</td>
<td>0.6424*** (4.22)</td>
<td>0.6769*** (4.26)</td>
</tr>
<tr>
<td>Mother did not have job when woman was 14</td>
<td>0.1432* (1.77)</td>
<td>0.1499* (1.82)</td>
<td>0.3356*** (2.51)</td>
<td>0.3140** (2.30)</td>
<td>0.0064 (0.05)</td>
<td>0.0240 (0.19)</td>
</tr>
<tr>
<td>Mother’s education</td>
<td>-0.0813*** (-6.72)</td>
<td>-0.0796*** (-6.43)</td>
<td>-0.1099*** (-5.38)</td>
<td>-0.1115*** (-5.22)</td>
<td>-0.0686*** (-3.83)</td>
<td>-0.0627*** (-3.38)</td>
</tr>
<tr>
<td>Number of Siblings</td>
<td>0.0796*** (5.55)</td>
<td>0.0761*** (5.16)</td>
<td>0.1021*** (3.79)</td>
<td>0.0983*** (3.48)</td>
<td>0.0779*** (3.91)</td>
<td>0.0768*** (3.68)</td>
</tr>
<tr>
<td>Brought up Baptist</td>
<td>0.2019* (1.96)</td>
<td>0.2182** (2.07)</td>
<td>0.3096* (1.72)</td>
<td>0.3177* (1.70)</td>
<td>0.0620 (0.46)</td>
<td>0.0596 (0.43)</td>
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<tr>
<td>Brought up Catholic</td>
<td>-0.0580 (-0.48)</td>
<td>-0.0638 (-0.52)</td>
<td>-0.0328 (-0.21)</td>
<td>0.0114 (0.07)</td>
<td>-0.2948 (-1.16)</td>
<td>-0.2822 (-1.05)</td>
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<tr>
<td>Attended religious services infrequently +++</td>
<td>0.3354*** (3.96)</td>
<td>0.3296*** (3.82)</td>
<td>0.4250*** (3.10)</td>
<td>0.4268*** (3.05)</td>
<td>0.3895*** (3.00)</td>
<td>0.3892*** (2.88)</td>
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<tr>
<td>Age of Menarche</td>
<td>0.0052 (0.21)</td>
<td>-0.0017 (-0.07)</td>
<td>-0.0689 (-1.61)</td>
<td>-0.0766* (-1.75)</td>
<td>0.0088 (0.26)</td>
<td>0.0020 (0.06)</td>
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<tr>
<td>Born in South</td>
<td>-0.0397 (-0.35)</td>
<td>0.0612 (0.43)</td>
<td>-0.2620 (-1.26)</td>
<td>-0.1881 (-0.71)</td>
<td>-0.0049 (-0.03)</td>
<td>0.1520 (0.71)</td>
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<tr>
<td>Black</td>
<td>1.8391*** (17.72)</td>
<td>1.8499*** (16.67)</td>
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<tr>
<td>Hispanic</td>
<td>0.4962** (2.71)</td>
<td>0.5553*** (2.89)</td>
<td></td>
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<td></td>
</tr>
</tbody>
</table>

+ Defined for state of residence at age 14
++ Averaged over ages 12-20 as a measure of long-run expectations of AFDC benefits (see Black, McKinnish and Sanders, 1998) for state of residence at age 14.
+++ Measured in 1979 since data was not available at age 14
Table 4: Probability that a woman gives birth by age 22 including additional controls and peer group effects (Logit).

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
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<tbody>
<tr>
<td>All Women</td>
<td>White Women</td>
<td>Black Women</td>
<td>Probability of birth by age 22</td>
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<tr>
<td>Number of Observations</td>
<td>2622</td>
<td>2602</td>
<td>1608</td>
<td>1502</td>
<td>625</td>
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<tr>
<td>Number of States (inc. District of Columbia)</td>
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<td>41</td>
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<tr>
<td>Cohort Dummies Included</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>State-Level Dummies Included</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
</tr>
<tr>
<td>State-Level Controls</td>
<td>AFDC payment for a family of 4 ++</td>
<td>0.1778***</td>
<td>0.1679</td>
<td>0.2167***</td>
<td>0.3452</td>
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<td>Percentage of counties with an abortion provider</td>
<td>-0.6370**</td>
<td>-0.1392</td>
<td>-0.2998</td>
<td>-1.9732</td>
<td>-0.8182</td>
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<tr>
<td>Median female wage for women with high school diploma or less</td>
<td>-0.8161</td>
<td>-0.3950</td>
<td>-2.5498**</td>
<td>-2.6876*</td>
<td>0.1602</td>
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<tr>
<td>Median male wage for men with high school diploma or less</td>
<td>-0.1721</td>
<td>-0.3224</td>
<td>0.3708</td>
<td>0.3884</td>
<td>-0.2031</td>
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<td>Ability</td>
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<tr>
<td>Percentile score for Armed Forces Qualification Test (AFQT)</td>
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<tr>
<td>Percentage of high school classified as disadvantaged</td>
<td>0.0048**</td>
<td>0.0058**</td>
<td>0.0082*</td>
<td>0.0075</td>
<td>0.0066**</td>
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<tr>
<td>Peer-Group Controls</td>
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<tr>
<td>Foreign Language Spoken at Home When Growing Up</td>
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</tr>
<tr>
<td>Lived with mother only at age 14</td>
<td>0.6436***</td>
<td>0.6988***</td>
<td>0.9375***</td>
<td>0.9540**</td>
<td>0.4503**</td>
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<tr>
<td>Lived in ‘other’ family type (i.e., not both parents) at age 14</td>
<td>0.4106**</td>
<td>0.375**</td>
<td>0.3926</td>
<td>0.4534</td>
<td>0.4125**</td>
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<tr>
<td>Mother did not have job when woman was 14</td>
<td>0.0617</td>
<td>0.0530</td>
<td>0.2522</td>
<td>0.1982</td>
<td>-0.1970</td>
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<tr>
<td>Mother’s education</td>
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<tr>
<td>Number of Siblings</td>
<td>0.0656***</td>
<td>0.0602**</td>
<td>0.0812**</td>
<td>0.0788*</td>
<td>0.0676*</td>
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<tr>
<td>Brought up Baptist</td>
<td>-0.0526</td>
<td>-0.0563</td>
<td>-0.0734</td>
<td>0.0964</td>
<td>-0.1231</td>
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<tr>
<td>Brought up Catholic</td>
<td>-0.0486</td>
<td>-0.0761</td>
<td>-0.0573</td>
<td>0.0867</td>
<td>-0.1410</td>
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<tr>
<td>Attended religious services infrequently +++</td>
<td>0.2568***</td>
<td>0.2622**</td>
<td>0.4125</td>
<td>0.3977*</td>
<td>0.2465</td>
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<tr>
<td>Age of Menarche</td>
<td>0.0036</td>
<td>0.0033</td>
<td>-0.0886</td>
<td>-0.0913</td>
<td>0.0134</td>
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<tr>
<td>Born in South</td>
<td>-0.1990</td>
<td>-0.1179</td>
<td>-0.7928**</td>
<td>-0.5415</td>
<td>0.1452</td>
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<tr>
<td>Black</td>
<td>1.5663***</td>
<td>1.5967***</td>
<td>-388.71</td>
<td>-371.49</td>
<td>-392.20</td>
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<td>Hispanic</td>
<td>0.4646</td>
<td>0.5935**</td>
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<tr>
<td>Log Likelihood</td>
<td>-957.84</td>
<td>-938.45</td>
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</tbody>
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