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# The Effect of Welfare on Marriage and Fertility: What Do We Know and What Do We Need to Know?

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## Abstract

The recent literature on the effects of welfare on marriage and fertility includes studies employing a wide variety of methodologies and data sets and covering different time periods. A majority of the studies show that welfare has a significantly negative effect on marriage or positive effect on fertility rather than none at all, and thus the current consensus is that the welfare system probably has some effect on these demographic outcomes. Considerable uncertainty surrounds this consensus because a sizable minority of the studies find no effect at all, because the magnitudes of the estimated effects vary widely, and because puzzling and unexplained differences exist across the studies by race and methodological approach. At present, and with the information provided in the studies, the source of these disparities cannot be determined. While a neutral weighing of the evidence still leads to the conclusion that the welfare system affects marriage and fertility, research needs to be conducted to resolve the conflicting findings.

# The Effect of Welfare on Marriage and Fertility: What Do We Know and What Do We Need to Know?

The research literature over the last 30 years on the effects of welfare on marriage and fertility includes studies employing a variety of methodologies and data sets and covering different time periods. Several studies were conducted in the 1970s and early 1980s, but a second wave of studies began in the mid-1980s and is still under way. Based on the early work, a consensus among researchers developed a decade or so ago that the welfare system had no effect on marriage and fertility. However, a majority of the newer studies show that welfare has a significantly negative effect on marriage or positive effect on fertility rather than none at all. Because of this shift in findings, the current consensus is that the welfare system probably has some effect on these demographic outcomes.

However, considerable uncertainty surrounds this consensus because a significant minority of the studies find no effect at all, because the magnitudes of the estimated effects vary widely, and because puzzling and unexplained differences exist across the studies by race and methodological approach. For example, the findings show considerably stronger effects for white women than for black or nonwhite women, despite the greater participation rates of the latter group in the welfare system. Also, the findings often differ when demographic outcomes are correlated with welfare generosity in different ways—variation in welfare benefits across states in a particular year, for example, versus variation in welfare benefits over time. Whether the differences in study findings result from inherent differences in different data sets or from differences in the way the data are analyzed—for example, in estimating techniques, definitions of variables, characteristics of the individuals examined, and other influences controlled for—is difficult to determine because most authors do not systematically attempt to determine why their findings differ from those of other studies.

This paper summarizes the relevant literature and discusses the differences across studies. Because of the diversity of findings, methodological considerations necessarily must be a major focus of the discussion. Section I provides background on the U.S. welfare system and those aspects of its

structure relevant to marriage and fertility and discusses the context of social science theories of marriage and fertility in which the welfare system plays a role. Section II outlines the different questions of interest and discusses those questions that have been addressed in the research literature. Section III discusses the methodological approach taken in the research literature toward the question and contrasts the method of experimentation with the nonexperimental method of using natural program variation. Broad trends in the U.S. on demographic outcomes and the welfare system are presented in Section IV; these trends establish a set of basic patterns in the data. Section V reviews the multivariate research studies on the question, compares and contrasts their approaches, and discusses possible reasons for the diversity of findings. Finally, suggestions for future research are outlined in Section VI.

## I. BACKGROUND

The U.S. welfare system is currently undergoing major changes as a result of 1996 legislation, the Personal Responsibility and Work Opportunity Reconciliation Act. However, because the research reviewed in this paper entirely concerns the welfare system prior to this legislation, only the old system will be described here. The relevance of this research to the future welfare system will be discussed in the last section.

Only the features of the system specific to marriage and fertility will be outlined. The most well-known aspect of the welfare system bearing on marriage and fertility is the set of eligibility rules in the Aid to Families with Dependent Children (AFDC) program resulting in a high concentration of single mothers among recipients, a relatively tiny fraction of married couples on the rolls, and no families or individuals without children (single mothers are defined as women with children under 18 in the household but no spouse or cohabiting partner present). As specified in the 1935 Social Security Act creating the program, AFDC is intended to provide cash support only to children living without at least one of their biological parents. Thus children for whom one parent has died are eligible, but so are

children whose parents never married but are living apart, or whose parents are divorced or separated. The mother, or other caretaker relative, is also supported by the grant. Children living with both parents are eligible, along with their parents, only for the AFDC-UP program (UP for "unemployed parent"), but eligibility for those benefits has additional conditions requiring that at least one parent be unemployed, that that parent have a significant history of employment, and that the family meet the same stringent income and asset requirements as a single-parent family. As a result, AFDC-UP families constitute only a small fraction of the AFDC caseload.[1]

The Food Stamp program provides food coupons to low-income families regardless of family structure and hence does not have the same "bias" toward single-parent families as does AFDC. Eligibility and benefits for the program are based on the income and resources of a group of people who eat together, regardless of their relationship to each other. Thus two-parent as well as single-parent families are eligible, although the fixed upper income and asset limits knock more two-parent families than single-parent families out of eligibility.[2] Single individuals and childless families are also eligible.

The Medicaid program provides subsidized medical care assistance to poor families. Historically it has been available primarily to AFDC recipients and therefore has the same bias toward single-parent families. However, in the last decade eligibility for Medicaid benefits has been greatly broadened to include children in poor families even if both parents are present and the family is off AFDC. However, despite the growth of Medicaid recipients under these new eligibility rules, the program is still disproportionately composed of single-parent families.

Housing programs come in several different forms—public as well as subsidized private housing, for example—and provide housing at below-market rents to families with low income and assets.

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[1]The eligibility rules have many other important facets not discussed here, especially rules governing eligibility of children living with cohabiting adults and whose caretaker parent has remarried. For details on these rules see Moffitt, Reville, and Winkler (1997).

[2]AFDC recipient families are automatically eligible for Food Stamp benefits, so this also results in a disproportionate number of single-parent families actually on the Food Stamp rolls.

However, these programs are distinguished from the other programs discussed above by their non-entitlement status. Expenditure allocations to local public housing authorities limit the amount of funds available and therefore restrict the number of recipients who can be served. Eligible families that apply and are accepted but cannot be supported are put on waiting lists that can be as long as several years. To choose from among the pool of eligibles, local housing authorities are required to give certain groups priority over others (called "preferences"). One of the preferred groups is AFDC recipients. This, along with the fact that family income (per family member) is lower among the single-parent population than the two-parent population, results in a high fraction of single-parent families receiving housing benefits. However, the preference is not absolute, and there have been times in the history of the program when middle-income families were preferred, so there is a sizable representation of two-parent families in the housing program.

In summary, therefore, the conventional perception of the U.S. welfare system as largely favoring single-parent families over two-parent families, childless couples, and individuals is essentially correct.[3] This favored treatment affects incentives to marry as well as incentives to have children. An additional fertility incentive arises, however, because benefits are based on the number of children present in the family unit. Hence the monetary cost of having an additional child is smaller in the presence of these welfare programs than it would be in their absence.

That these marriage and fertility incentives may have an effect on behavior can be understood both with common sense and from a variety of theoretical perspectives. The most natural modern conceptual framework is the economic theory of marriage and fertility as developed by Becker (1981)

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[3]It is worth noting, however, that any program which provides benefits on the basis of the income of a family unit rather than the income of individuals will necessarily, and inherently, have at least a minimal amount of bias toward single-parent families. If bias is defined as occurring when the income gain to marrying, for example, is less in the presence of a government program than in its complete absence, then a welfare program will be nonbiasing only if benefits are completely unaffected if a single parent marries. But this violates the definition of a targeted transfer program, namely, one that concentrates its benefits on those with lower income.

because of that model's emphasis on the economic gains to marriage and on the economic benefits and costs of having children. However, one could easily understand incentives induced by the welfare system without the formalization of the Beckerian theory, since almost any framework in which economic factors play a role will predict that, holding all else fixed, a welfare system biased against marriage and toward childbearing will change behavior in that direction (although the magnitude of the effect can, of course, be large or small).

Although more complex theories can give different predictions, the only simple economic theory that does so is that which conceptualizes single-parenthood as an unlucky outcome of an attempt at marriage (or union formation in general) and in which benefits play the role of insurance against that outcome. Standard economic theories imply that government provision of such insurance—welfare benefits—would induce more individuals to attempt marriage in the same way that providing insurance protecting checking accounts against bank failure encourages individuals to put their money in banks. The difficulty with this way of viewing the problem is that it ignores what is called the "moral hazard" problem in insurance terminology—the simple fact that individuals who are given insurance have an incentive to put themselves more at risk or even to cause the insured-against event to happen. In the case of welfare and family structure, this simply means that individuals have an incentive to take actions that lead, directly or indirectly, to single motherhood as an outcome.

Welfare effects on marital and fertility behavior occur necessarily through one of a fixed set of routes. An unmarried childless woman entering adulthood may have a child out of wedlock, and welfare may affect the probability of this outcome. She may later marry and possibly have additional children within marriage, but then separate or divorce, returning to a state of single motherhood; welfare may also affect the likelihood of this outcome. Alternatively, she may marry and begin childbearing within marriage but then divorce or separate, which is a different path to the same eventual outcome. Once

divorced or separated, she may have additional children out of wedlock, and she may or may not remarry. Both of these behaviors may be affected by the presence of welfare and the level of benefits.

Whether welfare is more likely to influence some of these behaviors than others is an empirical matter, but it is often argued on intuitive grounds that some "routes" to single motherhood are more likely to be affected than others. For example, it has been suggested that an unmarried woman's second and subsequent out-of-wedlock births may be more influenced by welfare benefits, especially if the woman is already on welfare, than the first birth because the first birth is more likely to be unintended and because awareness of welfare is less acute before a woman has been on welfare. It is also often proposed that divorce and separation are likely to be less affected by welfare than are remarriage probabilities, because divorce and separation are heavily influenced by other factors—most notably, whether the marital "match" is a good one—while remarriage (so it is argued) is more subject to rational calculation. These notions are helpful as a starting point in thinking about differential motivations for women in different positions, but they should be regarded initially only as hypotheses to be tested.

When other determinants of marriage and fertility are considered, a rich set of conceptual models developed over decades of research becomes available. Some of the more important factors posited to affect marriage propensities and fertility rates are economic opportunities for women, economic opportunities for men (often hypothesized to have the opposite effects of those of women), sex and sex-employment ratios in the population, neighborhood effects, and the influence of education, family background, and other factors on social norms and values. We will not elaborate on these factors, but it is important to emphasize that many influences other than welfare benefits affect marriage and fertility decisions, a point often de-emphasized in studies whose sole focus is a single-minded search for welfare effects. Moreover, even if these other factors are not examined in detail when testing for the effects of the welfare system, it is always necessary either implicitly or explicitly to parcel out their influence relative to that of welfare, which means in most cases controlling for these other factors statistically, a

point to be discussed further in the next section. Since a single mother does, after all, have alternatives to welfare, it is only the influence of the welfare benefit relative to the alternatives that should affect her choices. Unfortunately, the large number and diversity of these alternative factors make it difficult empirically to control for them all, leaving the door open to doubts as to whether it is welfare that is affecting behavior or some other omitted factor, as will be discussed below in the review of the empirical research literature.

## II. DIFFERENT QUESTIONS OF INTEREST

Turning from theories of welfare effects to the more specific issue of what empirical questions are of interest, an important distinction must be made at the outset between what may properly be called a "time-series" question and a "cross-sectional" question. An important time-series question is why marriage rates have declined and nonmarital childbearing rates have increased in the U.S. The corresponding welfare-related question is whether the welfare system has contributed to these trends. An important cross-sectional question, on the other hand, is whether eliminating or reducing the generosity of welfare would raise marriage rates and lower nonmarital fertility rates, *holding all else fixed*.

The answers to these questions need not be the same. One may simultaneously conclude, for example, that welfare is not a major contributor to the time-series trends in marriage and fertility but also that welfare, if reduced in generosity, would have the effects mentioned above, holding all else fixed. Differing answers to these two questions are not necessarily inconsistent because all else is not "held fixed" in time series; many other factors are changing at the same time, most notably changes in the economic and social environment and in social norms. These other factors could have been primarily responsible for the marriage and fertility trends and could have outweighed any welfare effect. However, if it is concluded that welfare would have had an effect if nothing else had changed, one must also conclude that the time-series trend would have been different if welfare had not trended the way it did.

Some analysts argue that only the time-series question is important, and it does receive much of the public's attention. However, the cross-sectional question is also important because it bears on what would happen in the future if the welfare system were altered, regardless of what might have caused marriage and fertility trends in the past. If welfare has had undesirable effects, for example, it could nonetheless be used as a tool to increase marriage rates and reduce nonmarital fertility rates in the future. In any case, as the review below will show, virtually the entire research literature on the effect of welfare on demographic outcomes has focused on the cross-sectional question, not the time-series question. The majority of analyses have attempted to "hold everything else fixed" in a cross-sectional sense. Indeed, those studies that have utilized data over multiple time periods, which could conceivably examine time-series questions, have, by and large, deliberately eliminated the influence of time trends in the data and instead have based their welfare results on the cross-sectional variation in the data.[4]

## III. METHODOLOGIES USED IN ESTIMATING WELFARE EFFECTS

### Experimental versus Nonexperimental Analysis

Although nonexperimental analysis is the norm in the social science research literature, experimental analysis is more familiar today to policy analysts involved in evaluations of welfare reforms. The most well-known experimental evaluations have examined the effects of various interventions on the employment, earnings, and welfare participation outcomes of welfare recipients (e.g., see the studies reviewed in Gueron and Pauly, 1991). However, experimental methods have not been widely applied to the study of welfare effects on fertility and marriage.[5] Because much of the

[4]In a regression framework, "eliminating the influence of time trends in the data" is meant to imply, for example, entering dummies for year or other time interval into the equation.

[5]Exceptions exist, and more experimental evaluations examining demographic outcomes are now under way. See the study by Maynard et al. (forthcoming) for a discussion of state-level experiments on demographic outcomes. Also, experiments in the 1980s examined the effect of a negative income tax on marital stability (Hannan and Tuma, 1990; Cain and Wissoker, 1990), but the results cannot be generalized to the AFDC program.

discussion of reasons for differences in study findings will turn on differences in nonexperimental methodologies—or, in the language of evaluation, the use of different nonexperimental comparison groups—a brief discussion of the reason that experimental methodologies have not been applied in this area is warranted.

The method of experimentation, wherein a randomly chosen experimental group of individuals is given a "treatment" and a randomly chosen control group is not, is a general methodology for inferring causal effects of a program or an alteration in a program. One can imagine experimenting with the level of welfare benefits, for example, giving the treatment group a higher level than the control group (or possibly giving the control group none, if the total effect of welfare is of interest). Clearly the methodology cannot be applied in time-series because the rest of society cannot be frozen in place and held "fixed" when the welfare system is altered. However, experimental methods are not always easily applied in cross-section either, for a number of reasons. One problem is that the outcomes of interest here—marriage and fertility—do not respond quickly to changes in the welfare and socioeconomic environment, so any experiment to measure welfare effects might have to continue several years for a credible estimate to be obtained. A second problem is that many welfare reforms are intended to have "community" effects—that is, effects that percolate through the community and affect general norms. Experiments cannot capture such outcomes unless the experiments are "saturation-site" in nature—that is, unless entire communities are made the unit of observation and either all individuals within that community are given the treatment or all are not. Saturation-site experiments are rare and have never been very successful when tried. A third problem is that experiments can at best determine the effects of only one "bundle" of welfare reforms at a time, making it difficult to isolate the effects of any one piece of a welfare reform program from others that are part of the same reform package. This problem afflicts many of the welfare experiments undertaken in the last decade or so in the U.S. Fourth, and related, it is often difficult to extrapolate and generalize from experimental results, since experiments by and large

test only one reform, or one bundle of welfare reforms, at a time. Fifth, for ethical reasons experiments are limited in the types of reforms that can be tested (e.g., eliminating benefits entirely for the experimental group has, thus far, not been deemed ethical).[6]

For these reasons, almost all research studies on the effects of welfare on marriage and fertility have utilized nonexperimental methods. Nonexperimental methods identify the effects of welfare by using natural variation in the welfare system, variation that generally arises through the political process, and by determining the existence and magnitude of correlations of such variation with variation in fertility and marital outcomes. Variations in benefits across states, across individuals within states, and over time across states have all been used for this purpose. Unfortunately, different sources of welfare variation may have different empirical associations with marriage and fertility behavior—even though they should not "in theory"—and it is possible that this will lead to conflicting results across methods. Reconciling those differences requires determining why they yield different results and what confounding factors might be present in each.

Most of the research in this area has examined the effects and correlates of variation in the level of welfare benefits rather than of variation in other features of welfare programs, such as earnings disregards, training programs, and child support reform. While this may seem limiting from the point of view of a policymaker, for whom more specific programmatic reforms are generally of greater interest, much can be learned from the basic issue of whether welfare-eligible women alter their behavior in response to benefit levels. If they do so, it is not unreasonable to assume that they will respond as well to changes in other characteristics of the program which have, either directly or indirectly, monetary implications.

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[6]Even the 1996 welfare legislation does not eliminate welfare entirely for anyone, since a minimum number of years of receipt is guaranteed.

Types of Natural Variation Used in the Research Literature

Aside from time-series variation, three types of natural variation in the welfare system have been utilized in most studies: cross-state comparisons of levels, cross-state comparisons of changes over time, and within-state comparisons. The differences are important because welfare-effect estimates often differ depending on which method is used.

A cross-state comparison of levels is the most common method in the literature and involves a determination of whether levels of welfare benefits are correlated with marriage and fertility behavior across states. Such comparisons need not literally be conducted at the state level but rather can be conducted at the individual level as long as the data include individuals in multiple states. The widespread use of this technique is based on the recognition that AFDC benefits are set at the state level and hence are generally the same within states, at least for families of the same size and with the same income and other characteristics. Consequently, holding these family characteristics constant, benefits vary only across states. Using individual-level data, one can control for other confounding factors at the individual level (age, education, and the "other factors" referred to previously) and therefore get closer to determining the effect of welfare "holding all else fixed."

Cross-state comparisons of changes are less common but have recently gained popularity in the research literature, where they are often called "state fixed effects" models. In this case, changes over time in benefit levels across states are compared with changes over time in outcome variables such as marriage and fertility. A case can be made that such comparisons are superior to those using cross-state comparisons of levels, inasmuch as the levels of benefits and levels of marriage and fertility behavior may covary across states not only because of some true relationship but also for some other, spurious, reason. For example, the low AFDC benefit levels and high marriage rates in most Southern states may not reflect a true welfare effect but may instead reflect the fact that the South is socially a relatively conservative region where social and cultural norms encourage marriage and is also politically a

relatively conservative region where elected representatives do not legislate generous welfare benefits.[7] In this latter interpretation, a positive correlation between benefit levels and marriage would arise because a third variable—social, cultural, and political norms—causes them both, not because benefits affect marriage. In the method of cross-state comparisons of changes, changes in benefits over time are inspected instead of differences in levels. For example, as it turns out, benefit levels have been falling in the South more slowly than in the Midwest over the last two decades; if there is a true effect of welfare on marriage, then marriage rates should fall less (or rise more) in the South than in the Midwest, even if the two regions started off at very different levels—that is, even if marriage levels were higher in the South to begin with for other reasons.

The method of cross-state comparisons of changes has its own complications, however. One important problem is the difficulty of measuring long-term responses to changes in welfare benefits. Because marriage and fertility behaviors do not respond quickly to alterations in benefit levels, a fairly long time interval must be examined to measure changes in behavior.[8] If one attempts to examine long time intervals, an additional problem arises because significant state in- and out-migration may occur, which may change state-level average outcomes merely because the composition of the population has changed, not because a fixed set of individuals have changed their behavior. More generally, it has to be assumed that over long time intervals the "omitted" influences—for example, the social and cultural norms referred to previously—do not change, and do not change differentially across states. In addition, a comparison of cross-state changes in welfare merely throws the bias problem back one stage, for it then needs to be determined why some states increase their benefits faster, or lower them less rapidly, than

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[7]This notion appears to have first been explicitly discussed and emphasized by Ellwood and Bane (1985).

[8]A related possibility is that the comparison-of-changes method measures a "short-term" response while the comparison-of-levels method measures a "long-term" response if it shows where marriage and fertility levels have ended up after several years of adjustment. Thus it may be that the two methods are simply not measuring the same thing.

other states, and whether omitted state-specific time-varying influences might confound the welfare effect by being responsible for both benefit trends and marriage and fertility trends.

Within-state comparisons are the most difficult and the least used, for they rely on comparisons of outcomes for women within a state who are offered different benefit levels, or on comparisons between women who are eligible and women who are not eligible for welfare. The problem with this method is that, because the eligibility and benefit determination rules are generally the same statewide, benefit-level differences between women within a state are almost always associated with a demographic characteristic (e.g., having children) which by itself could have an impact on the outcomes of interest. A comparison of eligibles with ineligibles is an extreme version of this method.

Time-series analysis is a fourth method, which is fraught with the difficulty already mentioned of controlling for alternative factors that are also changing over time.

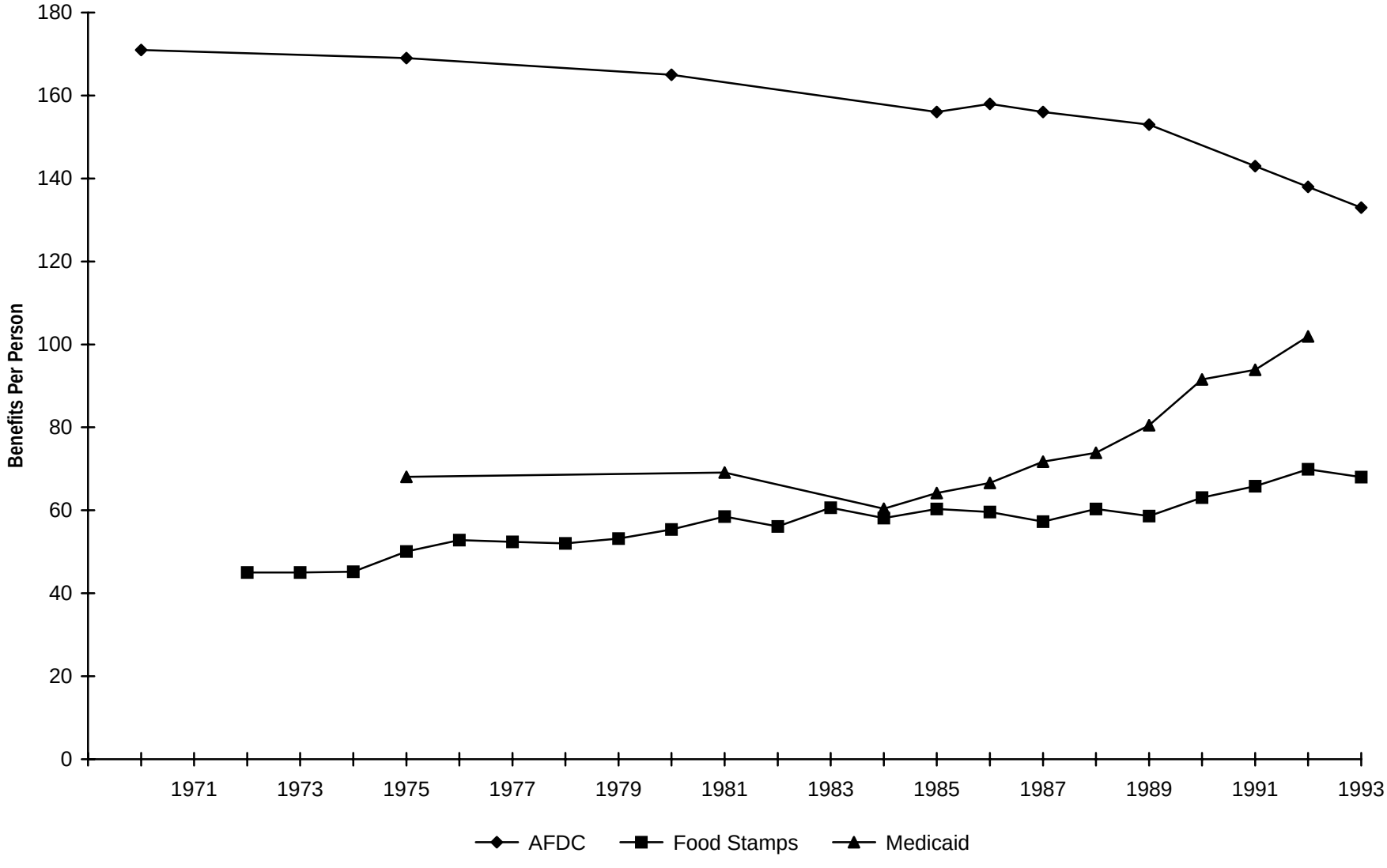
## IV. BASIC TIME-SERIES PATTERNS IN WELFARE AND DEMOGRAPHIC OUTCOMES

Three of the methodologies—cross-state comparison of levels, cross-state comparison of changes, and time-series analysis—can be studied by examining trends over time in unadjusted state-level or national-level aggregates of demographic outcomes on the one hand and measures of welfare generosity on the other. It is instructive to present the basic patterns of these correlations with unadjusted aggregates before reviewing the multivariate analyses in the econometric literature. As will be seen, the patterns that emerge in this analysis capture, in large degree, the patterns revealed by the multivariate analyses. Consequently, much of the basic story is understandable in relatively simple terms and does not need recourse to controlling for additional variables or use of specialized statistical methods.

The pure time-series method involves a simple comparison of trends in welfare benefits and in demographic outcomes. Figure 1 shows the time trend in welfare benefits of different types in the U.S. over the period 1970–1993. It has been noted repeatedly that the time-series evidence for a welfare effect

## FIGURE 1

### Trends in Real Monthly Welfare Benefits per Person




**Source**: U.S. House of Representatives, 1994 (pp. 378, 782, 806).

on marriage and fertility is weak because welfare benefits have declined in real terms over the 1970s and 1980s while marriage rates have declined and nonmarital childbearing has increased. This is confirmed in Figure 1, which indicates that real AFDC benefits have fallen continuously since the early 1970s. Real Food Stamp benefits have remained roughly constant, primarily because they are indexed to inflation, while real Medicaid benefits were roughly fixed until the mid-1980s, when they began to rise. The sum of benefits therefore declined up to the late 1980s. It did begin to rise at that time, but this increase is too late to explain the secular trends in marriage and fertility. In addition, Medicaid benefits began to be available to many poor families off AFDC in the late 1980s, thereby weakening the link between welfare and the availability of medical care.

The inconsistency between benefit and demographic trends could mask the presence of long lags (Murray, 1993). The generosity of the transfer system increased significantly in the late 1960s and early 1970s, as Food Stamps were mandated nationally and the Medicaid system was expanded. It is possible that this expansion of benefits resulted in increases in (say) nonmarital childbearing 10 years later, if the effect of the expansion took time to occur because social norms were slow to adjust. This is a difficult hypothesis to prove or disprove because the trends have been so universal. It is not possible to isolate specific communities where benefits increased much more than other communities, for example, and where the population was fixed for 10 years so that lagged effects could be measured. Consequently, the importance of this argument at the present time must rest largely on whether one believes that low-income families react quickly or slowly to the monetary opportunities facing them.

As noted previously, the inconsistency between time trends in benefits and demographic outcomes may only mean that other factors changing over time have masked the effect of welfare benefits; this is the major weakness of the method. There may have been changes in the other factors affecting marriage and divorce—economic opportunities for women and men, the availability of partners in the marriage market, and changes in social norms. More persuasive evidence on the effect of welfare

per se might therefore be gained from cross-state comparisons of levels because these comparisons are made at a single point in time, across states, and hence are not complicated by such major time trends. Figure 2, drawn from Murray (1993), shows illegitimacy rates and welfare benefit levels among white women in different states in 1988.[9] A positive relationship between benefits and illegitimacy is clear from the figure. Much of the relationship comes from the concentration of Southern states with low benefits and low rates of illegitimacy, although the relationship would still be positive (but weaker) if the South were omitted. Thus some evidence for a positive effect of welfare on out-of-wedlock childbearing is yielded by these data.

To insure that this pattern is not special to the particular data set, time period, and variables used by Murray, Current Population Survey (CPS) data for 1993 were obtained for this study and tabulations of welfare benefits and rates of single motherhood by state were computed. Rates of single motherhood rather than illegitimacy are examined because single motherhood is a broader and more inclusive measure of the demographic outcome of interest.[10] Figure 3 shows the cross-state result for white women.[11] Interestingly, very little relationship between headship and benefits appears in this figure, contrary to the results of Murray. A least-squares regression line, also shown in the figure, confirms this visual impression of only a slight positive relationship between the two variables. However, when women 20–44 and without a high-school diploma are examined instead—a subpopulation with relatively high welfare participation rates—the positive correlation reappears with a greater magnitude (Figure 4).

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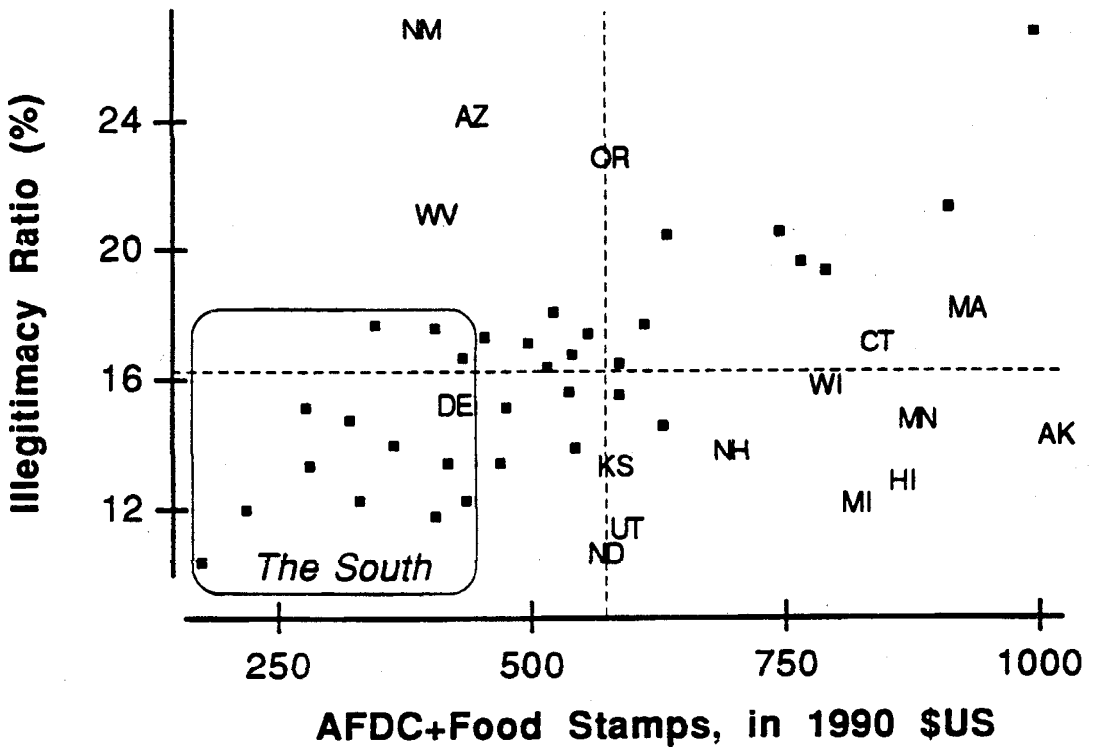
[9]The illegitimacy ratios are taken from Vital Statistics reports.

[10]This is because single motherhood is an overall category that can be reached by any of the routes discussed earlier—from nonmarital childbearing, divorce or separation, and failure to remarry. Thus it is a summary measure of all these routes taken together.

[11]The March CPS is used. Single mothers are defined as women without a spouse in the household but who have children under 18. Family and subfamily heads are included as separate observations. The rates are computed as a fraction of all women 18–64. The AFDC benefit variables are those for a family of four with no other income.

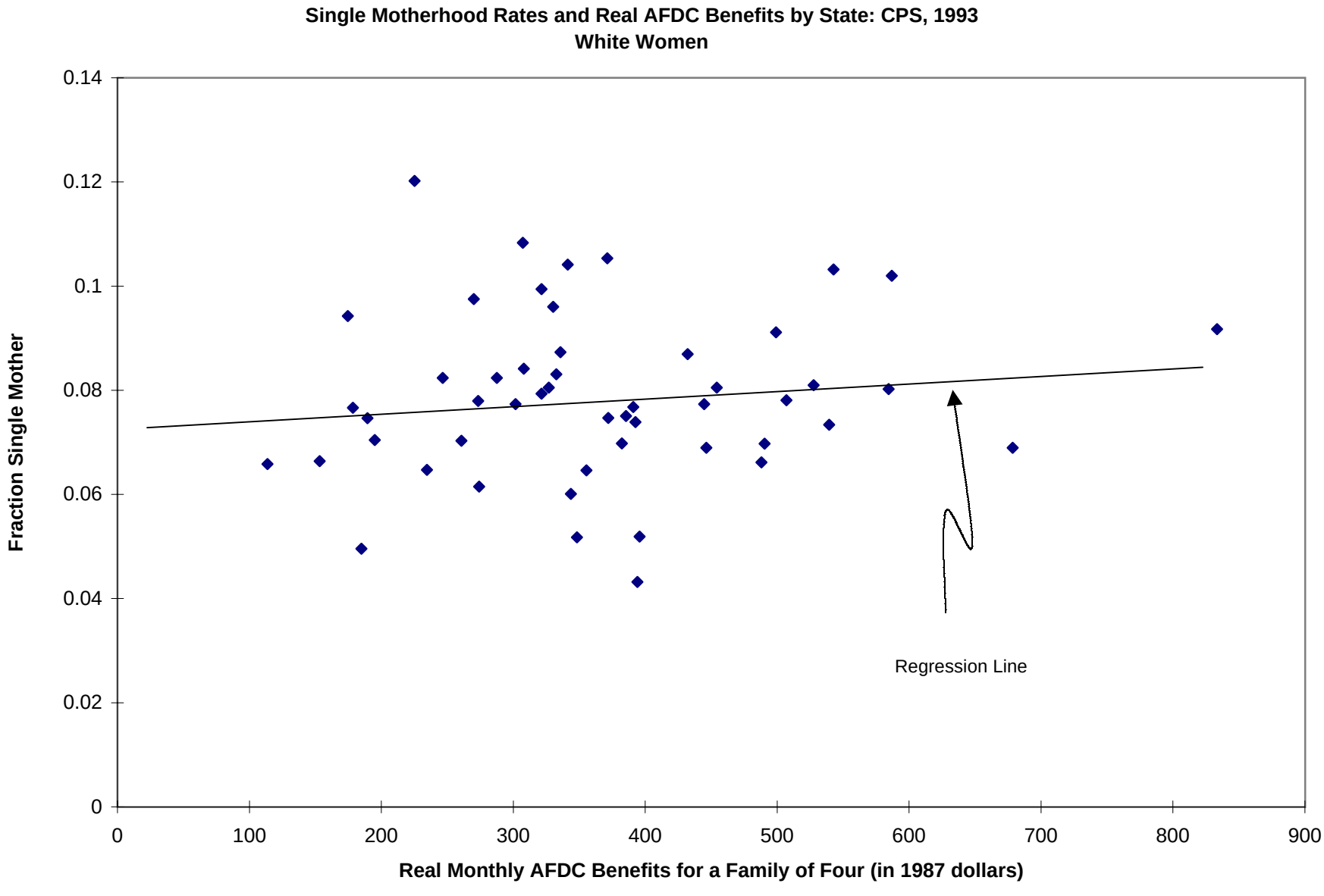
**FIGURE 2**



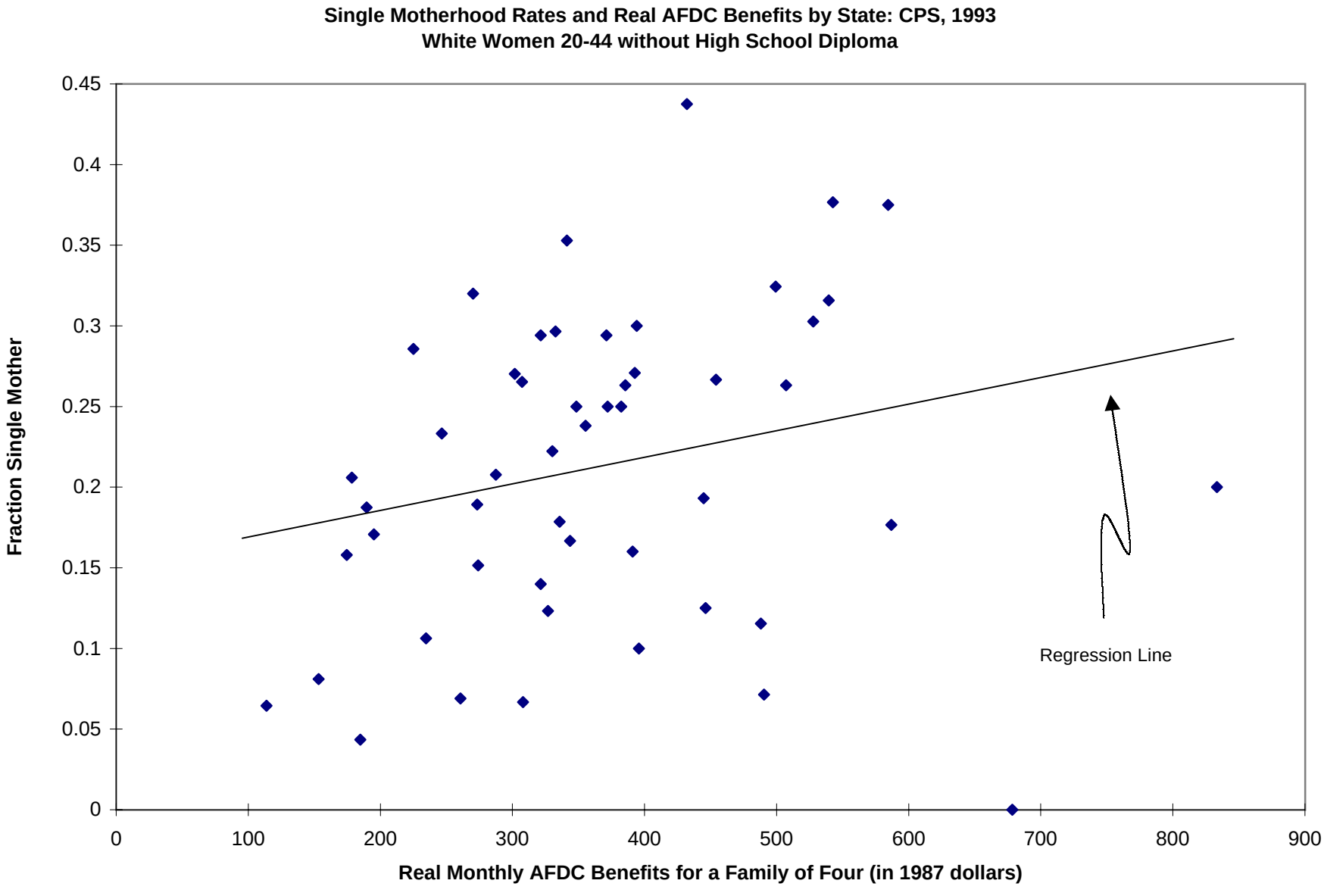

White Women, 1988

**Source**: Murray, 1993.

## FIGURE 3


Single Motherhood Rates and Real AFDC Benefits by State: CPS, 1993
White Women
Fraction Single Mother
0.14
0.12
0.1
0.08
0.06
0.04
0.02
0
0
100
200
300
400
500
600
700
800
900
Real Monthly AFDC Benefits for a Family of Four (in 1987 dollars)
Regression Line

## FIGURE 4


Single Motherhood Rates and Real AFDC Benefits by State: CPS, 1993
White Women 20-44 without High School Diploma
Fraction Single Mother
0.45
0.4
0.35
0.3
0.25
0.2
0.15
0.1
0.05
0
0
100
200
300
400
500
600
700
800
900
Real Monthly AFDC Benefits for a Family of Four (in 1987 dollars)
Regression Line

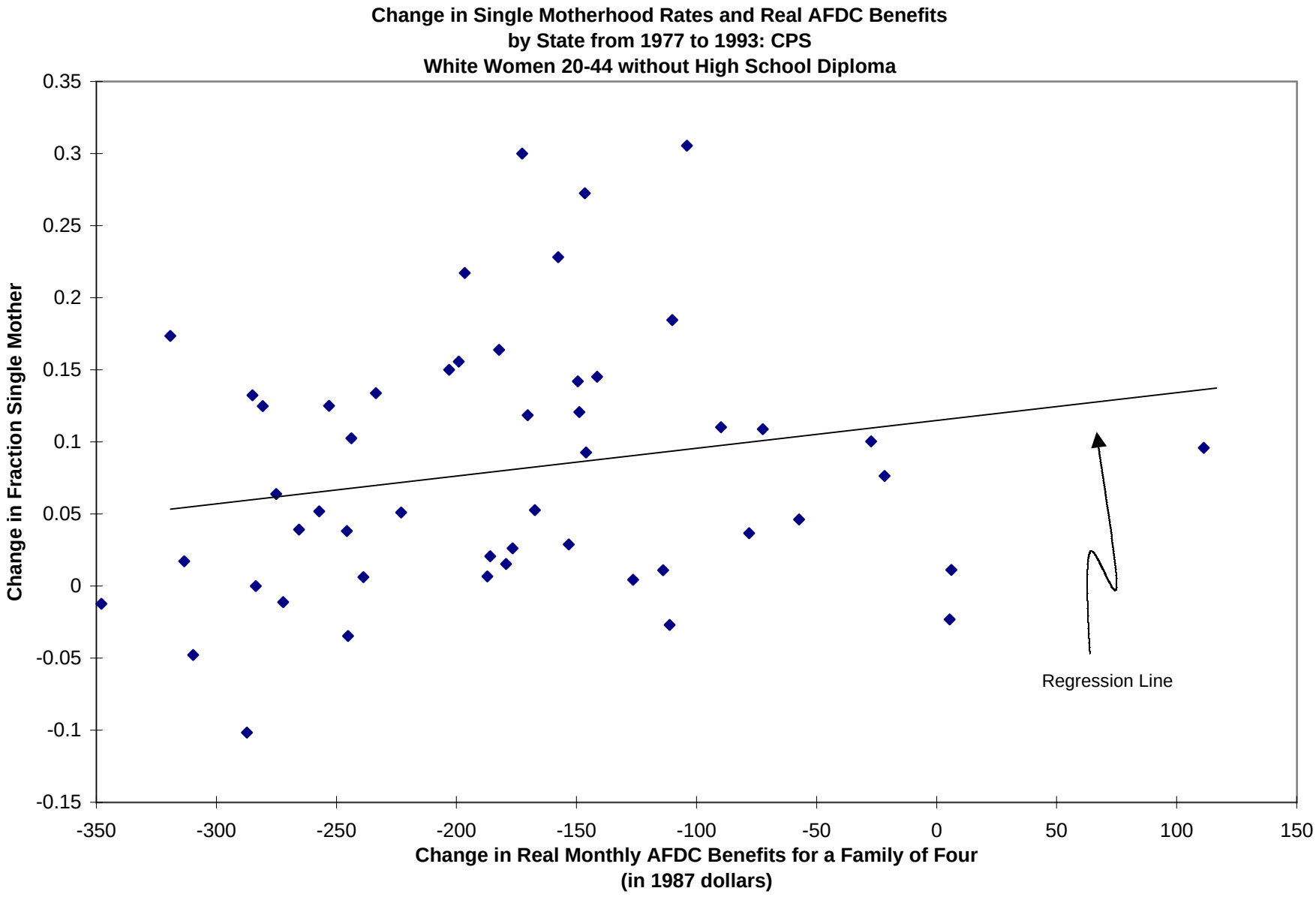
This simple analysis shows that the level of state welfare benefits is substantially correlated with single-motherhood rates. Many of the largest states such as New York, California, and Illinois have relatively generous welfare systems as well as high rates of single motherhood; another large state, Texas, has low benefits and low single-motherhood rates. Clearly, a major question is whether this simple correlation is the result of some other characteristic of the populations of these states or of their socioeconomic environments; however, as will be seen in the next section, this positive covariation persists even when other measurable influences are controlled for and therefore appears to be reasonably robust.

The positive relationship holds for other periods as well—all the way back to the 1960s, when CPS microdata are first available for these computations. It also holds when other measures of the welfare system—including Medicaid—are included. The relationship also appears in simple regional comparisons. For example, the Northeast has high welfare benefits and high rates of single motherhood while the South has the lowest benefits and lowest single-motherhood rates. The Midwest and West have much higher benefits than in the South and slightly higher rates of single motherhood.

To determine whether these comparisons of levels have the same implications as those from a cross-state comparisons of changes, CPS data from a different year can be compared to the 1993 data. The following tables use CPS information from 1977, when benefit levels were quite a bit higher. Rates of single motherhood were lower in 1977 overall, but the issue here is whether those states that lowered their AFDC benefits the least—benefits fell in virtually all states—also had the largest increases in single motherhood; if so, this could be taken as evidence of an effect of welfare consistent with the pure cross-state comparison of levels.

As Figure 5 indicates, however, the relationship between benefit levels and single motherhood is extremely weak for young, less-educated white women when this type of comparison is made. Although different states lowered benefits over this period by different amounts, the increases in single

## FIGURE 5


Change in Single Motherhood Rates and Real AFDC Benefits
by State from 1977 to 1993: CPS
White Women 20-44 without High School Diploma
Change in Fraction Single Mother
0.35
0.3
0.25
0.2
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-350
-300
-250
-200
-150
-100
-50
0
50
100
150
Change in Real Monthly AFDC Benefits for a Family of Four
(in 1987 dollars)
Regression Line

motherhood across the states were fairly uniformly distributed. New Jersey, for example, which reduced its benefit by a large amount over the period ($257 per month reduction) saw its single-motherhood rate increase by about 5 percentage points, whereas Texas, which reduced its benefit by much less ($78 per month) saw a similar increase of 4 percentage points.

Mechanically, the difference in results between the different comparisons arises from two facts. First, over the 1970s and 1980s, states with high average welfare benefits had higher than average rates of single motherhood (as well as nonmarital fertility rates); this relationship held not just for 1993 but also for 1977 and other years. Second, over the course of the 1970s and 1980s, states that raised benefits more than others—or, more accurately, lowered them less than others—did not experience faster than average increases in single motherhood and fertility. Welfare benefits across U.S. regions have converged slightly over the 1970s and 1980s, with the Southern states lowering benefits the least and Northeastern states lowering their benefits the most, but this pattern does not correspond at all to rates of change of single motherhood among less-educated white women (e.g., the Northeast experienced the greatest increase in single motherhood even though it, along with the industrial Midwestern states, lowered benefits the most).

The difference between the results using these two sources of welfare variation may stem from the omission of factors in one or both of the two comparisons. One possibility is that the levels comparison omits key state differences that affect both marriage and fertility behavior as well as benefits. For example, as mentioned earlier in the paper, Southern states have strong pro-marriage social norms and also have low welfare benefits; the correlation between marriage and benefits may therefore arise coincidentally. The fact that the South did not lower its benefits very much over time, and yet did not experience high growth of single motherhood relative to regions that lowered their benefits a great deal,

suggests indeed that the cross-state levels relationship may have been spurious and due to other factors.[12] However, it may also be the case that the changes comparison omits some factor that is causing benefits to change at different rates across states. Differences in rates of change in the economic performance of different states, in unemployment rates, and in related factors may have been responsible for both the change in benefits and the change in single motherhood. For example, the South experienced significant economic growth in the late 1970s and 1980s and closed its economic gap with the rest of the country to some degree; this could have caused both its relative increase in welfare benefits and its relative decline in single motherhood. The way to begin testing these hypotheses, and reconciling the different forms of evidence (levels versus changes), is to attempt to control for some of these omitted variables in a multivariate analysis. This is one of the roles of the econometric research to be described below.

The patterns for black women are roughly similar. As shown in Figure 6, the levels of single motherhood for young, less-educated black women are positively related to welfare benefits. Also, a comparison of changes in single-motherhood rates and benefit levels also shows no relationship between the two, if not a negative relationship, as seen in Figure 7. Overall, single-motherhood rates grew quite a bit faster for black women than for white women over this period, but, as for white women, the rate of growth across states was not closely related to the magnitude of changes in welfare benefits in the state. Single-motherhood rates for less-educated black women grew at about the same rate in the South, the Northeast, and the Midwest despite very different changes in welfare benefits in those regions.

[12]For example, Hoynes (1996) used data from individuals followed over time (i.e., panel data) to determine whether the correlation between changes in single motherhood at the individual level and changes in benefits is the same as that at the state level. She found this to be the case for white women. This supports the interpretation of the cross-state differences as traceable to differences in the types of women in those states.

**FIGURE 6**

**Single Motherhood Rates and Real AFDC Benefits by State:**
**CPS, 1993 - Black Women 20-44 without High School Diploma**

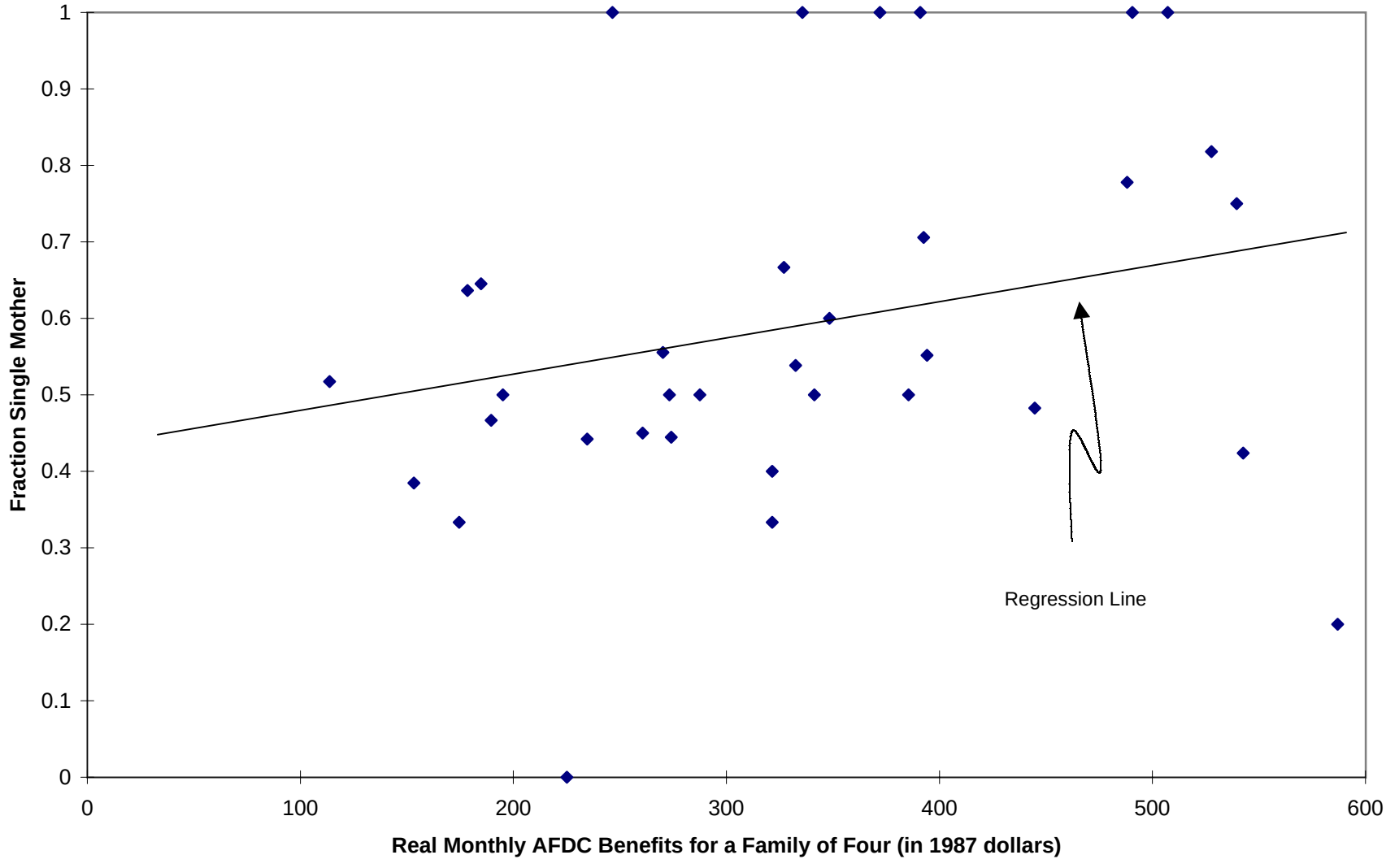
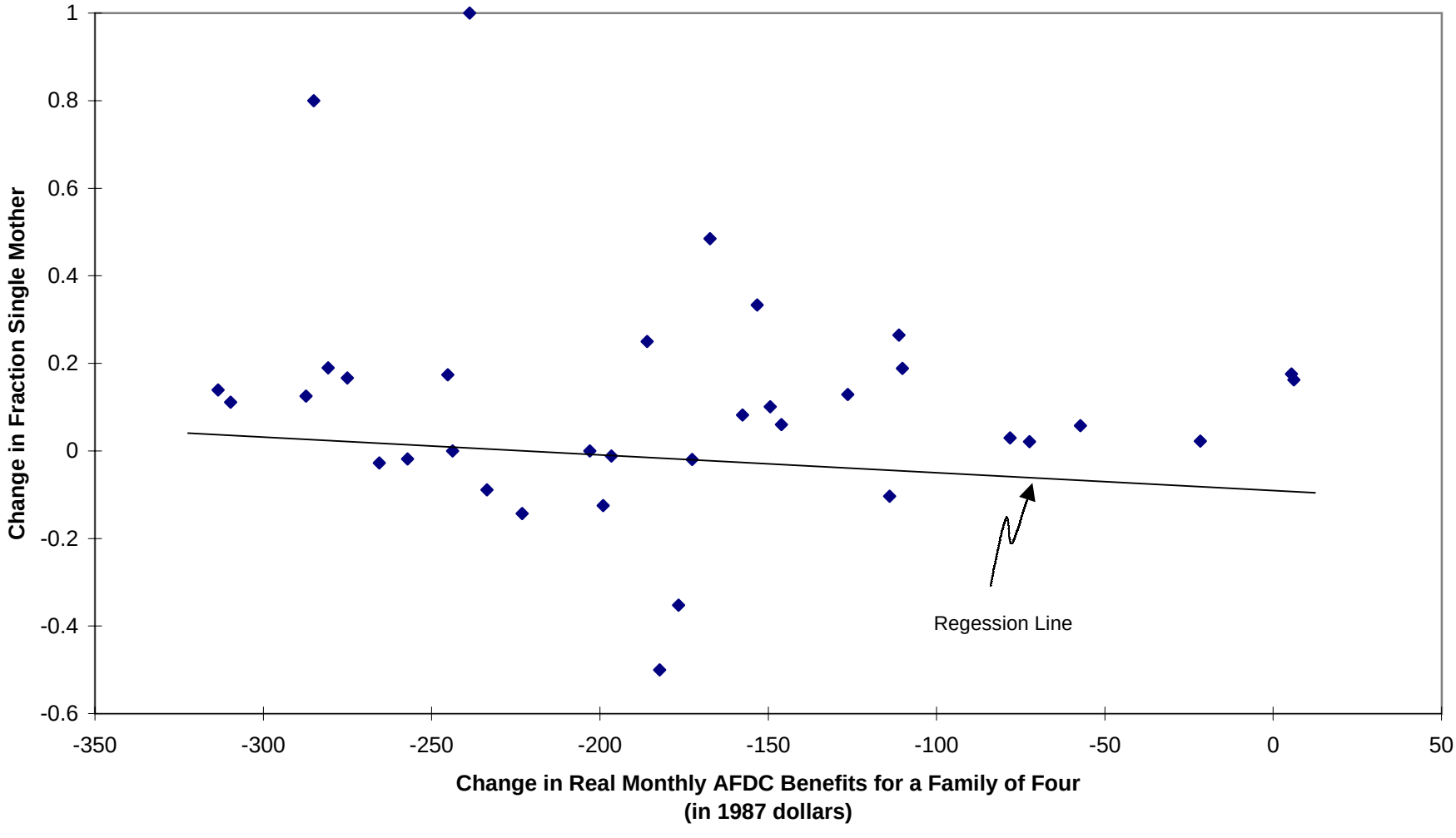


**FIGURE 7**

**Change in Single Motherhood Rates and Real AFDC Benefits by State from 1977 to 1993: CPS Black Women 20-44 without High School Diploma**



## V. RESULTS FROM MULTIVARIATE ECONOMETRIC MODELS

A table listing many of the relevant econometric studies appears in the Appendix. A more detailed summary of each is available in Hudson and Moffitt (1997). These studies all use one of the four methods of obtaining welfare variation described in Section III and (except for within-state variation) discussed in graphical terms in Section IV. Relative to the graphical analysis, a simple question that can be answered here is whether the patterns of effects across states, over time, and for different racial groups are any different in a multivariate analysis where additional covariates are entered into the model and where more sophisticated methods of estimation are employed.

Table 1 summarizes the results of 68 different estimates from these studies, classified by the method by which welfare variation was obtained and by the nature of the result.[13] Over all types of studies, a slight majority find a significantly negative effect on marriage or a positive effect on fertility rather than an insignificant effect (ignoring the mixed estimates, which could be classified in either way). This may seem surprising in light of what was taken to be the conventional wisdom approximately 10 years ago, when it was generally believed that the evidence did not support much of an effect of welfare on marriage and fertility at all. However, that consensus was based on studies from the 1970s, which indeed showed weaker results than the studies that have been conducted since then. Among analysts who work on the topic, there is now a rough consensus that the evidence does support some effect of welfare on marriage and fertility, although the magnitude of the effect remains in question. Whether this change in estimates has been a result of superior analytical methods in the later studies or an increase in the

[13]The studies were located by searching the economics and sociology literatures since 1970 and following references to other articles therein, as well as by a general search for articles on the subject since 1970 in a variety of other sources. All studies located were included which (i) had the estimation of the effect of AFDC on marriage, fertility, or a related demographic outcome as a significant, major focus of the study and (ii) were either published or had been circulated in draft form by May 1996. No study that met these criteria was intentionally excluded. It should also be noted that there are 68 estimates but fewer individual studies than this because most studies provided estimates for both racial categories. Estimates for outcomes other than marriage and fertility—e.g., living arrangements—are excluded from the table but appear in the Appendix.

**TABLE 1**
**Counts of Studies of Effect of Welfare on Marriage and Fertility, by Nature of Findings and Source of Welfare Variation**

| | Race Category | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | All Races | | | White | | | Nonwhite or Black | | |
| | Insig. | Sig. | Mixed | Insig. | Sig. | Mixed | Insig. | Sig. | Mixed |
| All Types | 8 | 5 | 1 | 8 | 13 | 5 | 10 | 12 | 6 |
| By Type: | | | | | | | | | |
| Cross-state levels | 6 | 3 | 1 | 2 | 9 | 4 | 7 | 6 | 3 |
| Cross-state changes | 1 | 2 | - | 4 | 4 | - | 1 | 5 | 2 |
| Within-state | 1 | - | - | 1 | - | - | 1 | - | - |
| Time-series | - | - | - | 1 | - | 1 | 1 | 1 | 1 |

**Notes**: Entries denote number of studies of each type showing no statistically significant effect of welfare (Insig.), a significantly negative effect of welfare on marriage or a positive effect on fertility or both (Sig.), or a mixed pattern of results (Mixed) with some significant and some insignificant results. Studies listed under "All Races" did not report results separately by race. If a study presented more than one estimate or model, the author's preferred estimate is tabulated.

underlying effect of welfare on behavior is difficult to determine with certainty, but some evidence points to the latter (Moffitt, 1990).

However, the overall counts of estimates are misleading because they are disproportionately concentrated among studies using cross-state comparisons of levels—a much smaller number have used cross-state comparisons of changes and only a handful have used either within-state or time-series methods—and because the results differ notably by race. As Table 1 shows, a majority of the estimates from cross-state comparisons of levels show that welfare benefits have an effect of welfare on marriage or fertility—negative on the former, positive on the latter—but when the results are disaggregated by race, the studies show more of an effect for white women than for nonwhite or black women. For white women, nine studies show effects of welfare while only two show no effect. For black and nonwhite women, however, the split is almost exactly fifty-fifty between those which find an effect and those which find none. Thus these multivariate analyses are quite similar to those revealed by the simpler graphical analyses reported in the last section, at least for white women. The implication, perhaps surprising, is that the additional covariates added in these studies—typically variables like age, educational level, and family background, as well as (sometimes) variables for the state unemployment rate, labor market wages, etc.—do not explain away the cross-state differences for white women in the simple unadjusted cross-state comparisons. For black women, however, these variables do appear to explain much of the raw difference; black women of similar characteristics in different states do not have significantly different demographic outcomes, at least in many of the studies in the literature, despite the differences in benefit levels across those states. It is not possible to determine the precise set of measured influences that account for the unadjusted difference across states noted earlier, but differences in urbanization may be one factor.[14]

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[14]For example, see Moffitt, 1994, Table 3. Adding age, education, urban residence, and a few other variables to an equation explaining black female headship rates changed a welfare-benefit coefficient from significance to insignificance. Urban residence, which is less common in the South than in other regions, had a

The weaker effect for black women is unexpected in light of their greater rates of participation in the welfare system as compared to those of white women. In general, it is possible that some omitted factor differs between the races (including possibly cultural differences), but no such factor has yet been identified in the literature. Murray (1993) hypothesizes that the low-income black population is more geographically concentrated than the low-income white population and that neighborhood effects lead to changes in social norms which increase illegitimacy rates (for example) even in the face of low benefits. Thus the South, with its concentration of the black population, has high illegitimacy rates. However, if this argument is correct, it implies that the variation in illegitimacy between black women in different states is indeed a result of something other than the welfare system. Racial differences therefore must still be regarded as an unresolved puzzle.

As shown in the table, many fewer studies have included cross-state comparisons of changes instead of levels. Of those that have used this method, however, the estimated strength of the welfare system is markedly different from the results of the levels method. For white women, the estimated welfare effect is quite weakened, because the changes-comparison studies are evenly spread over those that find an effect and those that find none. For black and nonwhite women, the estimated welfare effect is actually somewhat stronger in the changes comparison than it was in the levels comparison, in terms of the relative numbers of studies finding an effect. These results are, therefore, once again quite consistent with the simpler analysis reported in the previous section for white women but only roughly so for black women, although even for black women there is about an even split between studies finding a significant effect and studies finding no effect. In addition, an implication of this pattern of results is that the differences between the levels and changes comparisons, and between the race differences within each, are not explained away by the typical covariates used in these analyses.

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positive effect on headship rates.

Considerable discussion in the research literature has concerned the different results across methods, but no definitive resolution has been achieved. Although it has been argued that the levels comparison is subject to the biases noted in Section III, the changes comparison also has the defects noted there as well. In the absence of definitive evidence that either methodology is incorrect, an equal weighting of the two still leads to a conclusion that the welfare system has effects on marriage and fertility, even if not as strong as might be thought based on the levels-comparison methods alone.

Even fewer within-state and time-series studies have been conducted, mainly because within-state comparisons must find some characteristic of women that affects their eligibility for benefits and that does not independently affect their marriage and fertility outcomes, while time-series analyses inevitably have difficulty controlling for all alternative influences that are changing over time. For example, one study utilizing within-state variation did not examine benefits at all but found no effect of AFDC participation rates on demographic outcomes across races, a method which implicitly assumes that no racial difference in demographic outcomes would exist in the absence of AFDC. Another study compared the divorce rates of women with and without children in states with high and low welfare benefits, thus implicitly assuming that divorce rates would be identical among women with and without children in the absence of AFDC. The implausibility of these assumptions shows the extreme weakness of the method. As for the time-series studies, most simply estimate a variety of bivariate relationships and find either no effect or mixed effects. The one study that found a negative effect regressed the illegitimacy ratio in a year on the lagged AFDC participation rate rather than the AFDC benefit; yet the AFDC participation rate is an endogenous variable and is as much the product of time-series trends in illegitimacy as its cause. The within-state and time-series methods are sufficiently problematic that they should probably be dropped from any weighing of the evidence on the question.

While the discussion thus far has concentrated on what now appear to be unresolved differences between results using levels and changes comparisons, and between races, a considerable variance of

results also occurs within these types of studies, and quite a few studies in each category differ from the central tendency of the results for each type. Once again, without further analysis, it is difficult to resolve most of these differences. To be sure, a few studies appear to suffer from a significant defect that could explain why their results differ from the central tendency. Many of these defects concern the use of "endogenous" variables for either the welfare benefit or in controlling for nonwelfare factors, where an endogenous variable is, roughly, a variable that is a result of women's marital and fertility choices themselves (rather than a cause of them). Among the levels studies finding a significant cross-sectional welfare effect for black and nonwhite women, for example, one study replaced the welfare benefit ("instrumented" it, using econometric parlance) with such endogenous variables while others included in the regression variables of questionable exogeneity such as the labor force participation rates and earnings levels of men and women. Other defects in the studies arise as well. One study constrained the welfare benefit coefficient to be the same as the coefficient on other income, while another defined the dependent variable as AFDC receipt, which could by itself and separately be expected to respond to benefit levels. However, the number of studies that can be dropped from consideration for these reasons is relatively small, and even for these it cannot be determined conclusively that a correction of the problem would have had a major quantitative effect on the results Thus most studies must be given some positive weight in a balancing of the evidence.[15]

One notable difference between the different studies behind Table 1 is their great diversity in the types of variables held fixed when estimating welfare effects. Duncan and Hoffman (1990), for example, control for differences in women's labor market opportunities and even for differences in the labor market opportunities of potential male marital partners. Schultz (1994) and Lundberg and Plotnick (1995)

[15]These remarks are relevant to a common criticism of the "vote-counting" method used in Table 1—namely, that simply counting studies which have differing results without any adjustment for the quality of the study is misleading. The argument here is that in only rare instances can defects in the methodology in a study be determined to account for any nontrivial amount of the difference in estimates from another study, because too much else differs as well; hence the magnitude of the bias cannot be isolated.

similarly attempt to control for labor market differences. Ellwood and Bane (1985) and Matthews, Ribars, and Wilhelm (1995) go the furthest in this direction, controlling for a large number of state characteristics, even including characteristics of state political systems. On the other hand, Murray (1993), in an intentional effort to keep his analysis simple and easy to understand, does not adjust for any other differences between women or across states besides welfare. Roughly speaking, the more variables controlled for in an analysis, the weaker the estimated effect of welfare—although there is no logical reason why this need be so—which may also be responsible for some of the differences across studies. Determining whether this is the case would require reanalyzing some of the data sets under consideration and estimating similar specifications across data sets.[16]

In addition to these differences, however, the studies with different results vary in the data set used, in the age range of the individuals examined, and in the calendar years covered by the data. A simple way to summarize these differences is by ordinary least squares regression, using as a dependent variable the strength of the estimated effect and as independent variables the characteristics of the study. Taking only the studies in the first two rows of Table 1 (levels and changes studies), and defining a dependent variable (Y) equal to 1 if an effect was found, 0 if not, and .5 if a mixed finding was obtained, the following regression-based summary of the importance of study characteristics results:

$$\begin{aligned} Y = {} & -1.33 + \underset{(.16)}{.15}*\text{CHANGES} - \underset{(.13)}{.07}*\text{BLACK} + \underset{(.015)}{.016}*\text{YEAR} + \underset{(.010)}{.022}*\text{AGE} \\ & + \underset{(.19)}{.12}*\text{VITAL} + \underset{(.20)}{.08}*\text{NLSY} - \underset{(.26)}{.12}*\text{PSID} - \underset{(.20)}{.19}*\text{CPS} - \underset{(.18)}{.04}*\text{SM} - \underset{(.34)}{.024}*\text{REMDIV} \end{aligned}$$

$n=55$, $R^2=.24$

[16]Although in general the goal is to control for as many alternative influences as possible, this does not extend to endogenous variables, discussed previously, which should not be included. However, as important as this distinction is, it is not necessary to discriminate between exogenous and endogenous variables when one is attempting merely to answer the simpler factual question of whether differences in regressor sets across studies account for their differences in estimated welfare impacts. Only after it has been determined which variable sets lead to what results can the question of which is "best" be addressed.

where CHANGES is a dummy equal to 1 if the study used the changes rather than levels method; BLACK is a dummy equal to 1 if the estimate in question is for the black or nonwhite population; YEAR is the median year of the data; AGE is the median age of the individuals in the data; VITAL, NLSY, PSID, and CPS are dummies equal to 1 if the study used Vital Statistics, National Longitudinal Study of Youth, Panel Study of Income Dynamics, or Current Population Survey data, respectively (omitted category is all other data sets); and SM and REMDIV are dummies equal to 1 if the study dependent variable was single motherhood or divorce-remarriage transitions (omitted category is a dependent variable pertaining to fertility, almost always nonmarital). Standard errors, in parentheses, are large because of the small sample size. Interestingly, the results imply that changes studies yield stronger, rather than lesser, effects when the other variables are controlled; that estimated effects are larger in samples of older women (contrary to some of the hypotheses in the literature) and grow over time; and that the effects are stronger when Vital Statistics and NLSY data are used rather than CPS or PSID data.[17] The summary also indicates that welfare effects are weaker in studies that examine single motherhood as a single state, or remarriage or divorce, than studies that examine effects on nonmarital fertility. Taking these results at face value, they imply that the strongest effect of welfare occurs in nonmarital fertility but that these effects eventually disappear, perhaps because a woman eventually marries and her subsequent demographic behavior is unaffected by her having experienced an out-of-wedlock birth previously.[18] This finding warrants further investigation because it implies that the

[17]Klerman (1996) argues that the sample sizes in all data sets except the Vital Statistics are insufficient to detect effects of reasonable magnitudes. This is supported by the estimated coefficient on VITAL but not by the coefficient on CPS, which is the next largest data set.

[18]This conclusion necessarily follows because a young woman who has a premarital birth automatically becomes a single mother, thereby driving up the fraction of the population who are single mothers; but if the overall rate of single mothers is not significantly affected by welfare, it must be the case that these young mothers later marry so that, on average and over all ages, the single-motherhood rate ends up not much different than it would have been if the early premarital childbearing had not occurred. It should be noted that the vast majority of studies are of nonmarital fertility (about three-quarters) and that there is only one study of divorce, which is why it is lumped in with remarriage (for which there are only two studies as well).

implications of early nonmarital childbearing for later family structure may not be as strong as imagined. Of course, many other differences in these studies have not been controlled for. Once again, however, only a reanalysis of the various data sets and models can confirm these differences.

The changes-comparisons studies are thought by many analysts to be more reliable than the levels studies for the reasons noted previously—namely, that the levels studies confound cross-sectional benefit variations and unobserved variations in economic, social, and political factors. If this view is taken, there are sufficiently small numbers of these studies to allow more detailed comparisons between specific individual studies. When the studies are examined at this more detailed level, many possible explanations for differences appear. For example, the stronger effects found by Jackson and Klerman (1995) hold only when effects on nonmarital fertility in isolation are estimated; when effects on marital fertility are examined as well, no effect of welfare on their relative magnitudes is found. This should properly move the study from one reporting a significant effect to an insignificant effect in Table 1. Clarke and Strauss (1997), who also find a significant effect of welfare, obtain strong effects with a two stage least squares procedure using state per capita income (among other variables) as an "instrument" for the benefit, but per capita income probably belongs in the main equation. Rosenzweig (1995) argues that his significant estimated effects of welfare result from separating out the low-income population for analysis, but a similar separation was conducted by Hoynes (1996), Moffitt (1994), and Robins and Fronstin (1996), who all found effects either not affected by this separation or to be insignificant even for the disadvantaged population. This suggests that some other difference between the Rosenzweig study and the other three studies explains their differences in findings.[19] Finally, these studies differ dramatically in the extent to which other state-level influences are controlled in the regressions, and in the types of influences controlled for. Table 2 shows the different area-level controls used in the changes

[19]A notable difference, however, is that Rosenzweig stratified on the income of the family of origin, while the other three studies stratified on the education of the woman in question. Whether this could explain the differing results cannot be determined.

**TABLE 2**
**State-Level Control Variables Used in Cross-State Changes Studies**

Clarke and Strauss (1995)

Median wage of working women; median wage of working men; incarceration rate; unemployment rate; percentage living in metropolitan area

Ellwood and Bane (1985)

Percentage nonwhite; percentage high school graduate; mean wage; fraction of population under 18; unemployment rate; fraction of population living in metropolitan area

Hoynes (1996)

Average manufacturing wage; unemployment rate; per capita income; percentage of population over 65; percentage of population less than 18; percentage black; Republican governor; Republican House; Republican Senate

Jackson and Klerman (1995):

Unemployment rate; mean wage; mean manufacturing wage; mean wage in retail trade

Lichter, McLaughlin, and Ribar (1996):

Sex ratio; male full-time median income; male education; male employment levels; female full-time median income; female education; percentage population 65+; percentage black; percentage Hispanic; percentage rural; population; percentage Catholic; percentage Latter Day Saints; percentage anti-abortion Protestant

Moffitt (1994)

Unemployment rate; percentage employed in manufacturing; percentage employed in retail and wholesale trade; percentage employed in services; percentage employed in government

Robins and Fronstin (1996):

None

Rosenzweig (1995)

None

studies. While some of the variables are potentially endogenous, and therefore perhaps should be excluded, some of the studies control for no area-level variables at all, which could easily explain some of the differences in findings.

A final important issue concerns the magnitudes of the estimated effects of welfare for those studies finding significant estimates. Not surprisingly, the estimated magnitudes have a wide dispersion as well. At the upper end are three studies (Fossett and Kiecolt, 1993; Hill and O'Neill, 1993; Rosenzweig, 1995) which imply that a 25 percent reduction in welfare benefits would lessen the probability of a nonmarital birth by approximately .04 or .05.[20] If the mean probability is .16, this suggests a reduction to a level of .11 or .12, or about a 30 percent drop in the rate. In time-series, the welfare benefit has indeed fallen by about 25 percent over the last 20 years (see Figure 1) while the nonmarital childbearing rate for this age group doubled (U.S. DHHS, 1995, Figure II-1). One interpretation of these estimates is therefore that the historical increase in the nonmarital childbearing rate could have been cut by a significant amount if benefits had been reduced by twice the amount that they were. At the other end are studies obtaining statistically significant estimates but quite small in magnitude (Danziger et al., 1982; Duncan and Hoffman, 1990; Lichter, LeClere, and McLaughlin, 1991; Lichter, McLaughlin, and Ribar, 1996). A typical and recent estimate is that of Lichter, McLaughlin, and Ribar (1996), who found that a 25 percent reduction in the welfare benefit would increase the percent of women who are female heads by a mere .007. Clearly, therefore, a resolution of the differences in these magnitudes is also a priority item for future research.

[20]The different studies define their dependent variables slightly differently, and for two of them it is approximately the probability of ever having had a nonmarital birth up to a particular age (which is higher than the annual probability of the event). The 4 percentage point number is scaled from the numbers actually given in the articles.

## VI. WHAT DO WE NEED TO KNOW?

This review of what we know about the effect of welfare on marriage and fertility has demonstrated that much has been learned from research regarding the basic patterns of relationship between welfare and the demographic outcomes, where a significant relationship appears and where it does not, and about the general robustness of the strength of the estimated relationship across different data sets and different methods. Based on this review, it is clear that a simple majority of the studies conducted to date show a significant correlation between welfare benefits and marriage and fertility, suggesting the presence of such behavioral incentive effects. However, in addition to this finding not being able to explain the time-series increase in nonmarital fertility and decline in marriage, the majority finding itself is weakened by the sensitivity of the result to the methodology used and to numerous other differences in specifications across the studies. A neutral weighing of the evidence still leads to the conclusion that welfare has incentive effects on marriage and fertility, but the uncertainty introduced by the disparities in the research findings weakens the strength of that conclusion.

The resolution of the discrepancies between these studies is important for welfare policy because the issue of how demographic outcomes are affected by the overall level of welfare benefits is so basic to all discussions of welfare effects. It is also relevant to many of the reforms tested in the past several years in the states and to many of the changes enacted in the 1996 welfare legislation. Women who lose eligibility because of time limits or failure to comply with new rules, as well as women who do not choose to go onto welfare when they would have otherwise, can be viewed as having suffered benefit reductions similar to those whose effects are studied by the research literature. More generally, the legislation is intended to reduce the welfare caseload and to lower the overall level of welfare benefits

provided to low-income populations, and is explicitly intended to have effects on nonmarital fertility of the type with which the research literature is concerned.[21]

Although much of the analysis of the 1996 legislation will be conducted with program evaluation methodologies using experimental/control or treatment/comparison-group frameworks rather than the econometric approach underlying the studies in the research literature, the latter has a role to play in understanding the former. Ideally, the econometric research should be consistent with demonstration and evaluation evidence, and any differences should be reconciled. If, for example, the New Jersey family cap experiment shows little effect of a family cap on fertility, it would increase the confidence in that finding considerably if it could be concluded from the research literature that incremental benefits in the range tested in New Jersey also appear to have no effect on fertility. Even more important, and continuing to follow the New Jersey case, the research literature should be capable of providing estimates of the effects of benefit changes of greater magnitudes than that in New Jersey and for a greater number of states with differing economic and social environments. Regardless of how many demonstration evaluations are conducted, there will never be a sufficient number of them to provide the same range of alternative programs that occur naturally over time and across states. Econometric research using secondary data can tap into that larger range.

Unfortunately, the diversity in findings in the research literature necessarily reduces the power of that research to play this role. Moreover, studies to resolve the discrepancies have not been conducted. Three different types of studies are needed. First are *replication* studies that reanalyze the same data set used by each study (or the major studies) to determine whether the results were correct as reported. Second are *robustness* studies that conduct sensitivity testing to the model reported in each study to determine if the results are robust to variations in the specification. Third are *reconciliation* studies that

[21]In addition, many of the states have adopted or will adopt family caps on payments to additional children, changes in the AFDC-UP program to encourage married-couple welfare participation, and other rules that directly affect fertility and marriage apart from simply reducing the caseload (see Maynard et al., forthcoming).

estimate common specifications across studies on common samples in an attempt to reconcile why the studies achieved different findings. These types of studies—the "Three R's" of replication, robustness, and reconciliation—have not been applied to this literature. As in many other literatures whose main contributors are academic scholars, the lack of attention to the Three R's is primarily a result of the bias in academic publishing and research toward new findings, new techniques, and original analysis, and against mere replication of other researchers' results. This situation is unlikely to change without government or other funding to give researchers an incentive to conduct such studies.

The most likely cause of the discrepancy across studies is the omission of different alternative influences on marriage and childbearing. Very few studies control for the same factors, and almost never do the studies using the different methodologies outlined here attempt to control for the confounding influences appropriate to the method in question (e.g., alternative influences across states, across states over time, or in time series). Relatively little attention has been paid to nonwelfare influences, particularly those that might be correlated with welfare, in the analyses. This defect could also be addressed with additional research.

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**Appendix:**

**List of Studies**

Source: J. Hudson and R. Moffitt, "Welfare, Nonmarital Childbearing, and Single Motherhood: Literature Results and Summaries," Johns Hopkins University, 1997

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Acs 1994 | Cross-state comparison of levels | NLSY 1979–1988 | Women 14–23 | Probability that woman has first birth | Not significant |
| | | | Unmarried women 14–23 | Probability that unmarried woman has first birth | Not significant |
| | | | Women 14–23 | Probability that woman has a birth and goes on AFDC | Positive |
| | | | Women 14–23 who have had a first birth | Probability that woman has second birth | Not significant |
| Acs 1996 | Cross-state comparison of levels | NLSY 1979–1988 | Women 23–25 in 1988 who have had a child | Probability that woman has second birth | Not significant for blacks or for whites |
| | | | Women 23–25 in 1988 who have had a child and were on AFDC | Probability that woman has second birth | Not significant. for blacks or for whites |
| | | | Women 23–25 in 1988 who have had a child and were on AFDC and who grew up in a low income single parent home | Probability that woman has second birth | Not significant. for blacks or for whites |
| Allen 1993 | Cross-province comparison of levels | Census of Canada 1986 micro data | Women less than 45 on or at poverty line | Probability that woman is a single parent | Positive |
| | | | | Probability that woman has an out of wedlock birth | Positive |
| | | | | Probability that woman is divorced | Positive |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| An, Haveman, and Wolfe 1993 | Cross-state comparison of levels | PSID 1968–1987 | Women 19–25 in 1987 | Probability of having an out-of-wedlock birth between ages 13 and 18 | Not significant |
| Blank, George, and London 1994 | Cross-state comparison of changes | Alan Guttmacher Institute data 1974–1988 | All U.S. states | Abortion rate by state of occurrence | **AFDC:** Mixed but usually insignificant<br><br>**Medicaid:** In-state restrictions: negative<br><br>Border-state restrictions: positive |
| | | | | Number of abortion providers in state | **AFDC:** Not significant<br><br>**Medicaid:** In-state restrictions: not significant<br><br>Border-state restrictions: negative |
| | | | | Abortion rates for state residents (occurring inside or outside the state) | Not significant for both AFDC and Medicaid restriction variables |
| | | | | Difference between abortion rates by state of occurrence and by state of residence | **AFDC:** Not significant<br><br>**Medicaid:** In-state and border-state restrictions significant, with larger gap associated with border-state Medicaid restrictions |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Blank, George, and London 1994 | Cross-state comparison of changes | Centers for Disease Control data 1973–1990 | All U.S. states | Abortion rates by age and race<br><br>Four groups: teens and nonteens, whites and nonwhites | **AFDC:** Positive for all groups<br><br>**Medicaid:** In-state restrictions: not significant. Border-state restrictions: positive |
| Clarke and Strauss 1997 | Cross-state comparison of levels | U.S. Vital Statistics 1980–1990 | Unmarried women 15–19 (blacks: 36 states only) | Illegitimacy rate | Whites and blacks: no effect in OLS but positive effect in 2SLS |
| | Cross-state comparison of changes | | | | **AFDC guarantee:**<br>White: positive in OLS and 2SLS<br>Black: negative in OLS and positive in 2SLS<br><br>**Benefit differential**: Not significant for black or white women in any specification |
| Cutright 1970 | Time-series | Vital Statistics 1940–65 | Annual aggregates 1940–1965 | U.S. illegitimacy rates 1940–1965 | Overall positive relationship holds but not for specific time periods, especially for blacks |
| | Cross-state comparison of levels | | 4 U.S. states | State illegitimacy ratio 1950–1964 | States with higher benefit levels or recipiency rates do not have higher illegitimacy ratios |
| | Time-series | | Several countries | International illegitimacy rates | Negative |
| Danziger, Jakubson, Schwartz, and Smolensky 1982 | Cross-state comparison of levels | CPS 1975 | Women 25–54 married or female heads | Female headship | Positive for white and nonwhite women |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Darity and Myers 1983 | Time-series | Not reported 1955–1980 | Annual aggregates | Ratio of black female-headed households to black non-female-headed households | Not significant |
| Duncan and Hoffman 1990 | Cross-state comparison of levels | PSID 1968–1985 | Black women in 1985 who turned 19 between 1973 and 1985 | Probability of an out of wedlock birth followed by AFDC receipt | Positive (weak) on AFDC related births<br><br>Not significant for non-AFDC related births |
| Ellwood and Bane 1985 | Within-state comparison of women with different probabilities of being on AFDC | Survey of Income and Education 1976 | All women 16–44 | Probability that woman is an independent female head | Positive for whites and nonwhites 16–34, with higher significance levels for whites and higher magnitudes for nonwhites |
| | | | | Probability that woman is a single mother | Not significant for nonwhites<br><br>Positive for whites, but age range is sensitive to specification |
| | | | Single mothers 16–44 | Probability that a single mother lives independently | Positive for whites and nonwhites 16–24 |
| | | | Married mothers 16–44 | Probability that woman is newly divorced | Not significant for whites or nonwhites |
| | | | Ever-married mothers 16–44 | Probability that woman is currently divorced | Positive for whites and nonwhites 16–24 and for whites 25–30<br><br>Not significant for older nonwhite women |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Ellwood and Bane 1985 | Within-state comparison of women with different probabilities of being on AFDC | Survey of Income and Education 1976 | Unmarried women without children or child < 1 | Probability that woman has a nonmarital birth | Not significant for whites and nonwhites 16–24<br><br>Positive for older white women 25–34 |
| | | | Never married women 16–44 | Probability that a woman becomes a mother | Not Significant for whites and nonwhites 16–24<br><br>Positive for whites and nonwhites 25–34 |
| | Within-state comparison of different eligibility types | U.S. Vital Statistics and Census 1970 | Selected states, women 14–44 | Percentage of ever-married mothers who are divorced or separated | Negative for whites and not significant for nonwhites |
| | | | | Birthrate for unmarried women | Not significant for whites and nonwhites |
| | | | | Ratio of percentage of ever-married mothers > 14 who are divorced or separated to percentage of ever-married childless women > 14 who are divorced or separated | Not significant for whites and nonwhites |
| | | | | Ratio of birthrate of unmarried women to birthrate of married women | Not significant for whites and nonwhites |
| | Cross-state comparison of changes | U.S. Census 1960, 1970 | All U.S. states | Percentage of women > 14 who are independent female heads | Not significant for whites or blacks |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Ellwood and Bane 1985 | Cross-state comparison of changes | U.S. Census 1960, 1970 | All U.S. states | Number of children living with a female head as a fraction of total children not living with both parents | Positive for 1960 and 1970 benefit levels of white and nonwhite women |
| | | | | Percentage of ever-married women > 14 who are divorced | Positive for whites in 1970<br><br>Not significant for blacks in 1960 and 1970 or for whites in 1960 |
| | | | | Unmarried birthrate | Not significant for whites or blacks |
| Fossett and Kiecolt 1993 | Cross-city comparison of levels | Census 1980 Vital Statistics 1979–81 | 270 SMSAs | Percentage of black men in metropolitan area who are married | Negative |
| | | | | Percentage of black women in metropolitan area who are married | Negative |
| | | | | Percentage of black women with children < 6 in metropolitan area | Negative |
| | | | | Percentage of black women with children < 18 in metropolitan area | Negative |
| | | | | Percentage of families with children < 6 in metropolitan area and who are married | Negative |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Fossett and Kiecolt 1993 | Cross-city comparison of levels | Census 1980 Vital Statistics 1979–81 | 270 SMSAs | Percentage of families with children > 18 in metropolitan area and who are married | Negative |
| | | | | Percentage of children living in husband-and-wife families in metropolitan area | Negative |
| | | | | Percentage of births to black women in metropolitan area who are married | Negative for all four groups, with a higher magnitude for black women 20–29 than for black teens |
| Freshnock and Cutright 1979 | Cross-county comparison of levels | Vital Statistics 1970 | Approx. 1000 counties with usable data | Illegitimate birthrate for unmarried women | Not significant for teens<br><br>Positive for never-married whites 20–44<br><br>Negative for blacks 20–44, with a larger magnitude in absolute value than that of whites |
| Hill and O'Neill 1993 | Cross-state comparison of levels | NLSY 1979–87 | Women 23–30 in 1987 | Probability a woman has had a child but has never been married since 1979 | Positive for white women<br><br>Not significant for black women |
| | | | | Probability a woman had an out-of-wedlock birth in the last year | Positive for white women<br><br>Not significant for black women |
| Hoffman and Duncan 1988 | Cross-state comparison of levels | PSID 1969–1982 | Women who were divorced or separated 1969–82 and were < 45 at time of event | Probability of remarriage | Not significant for blacks or whites |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Hoffman and Duncan 1995 | Cross-state comparison of levels | PSID 1967–85 | Women with children and with a first marriage during 1967–1993 | Probability of divorce | Positive for AFDC 5-year moving average<br><br>Not significant for AFDC guarantee |
| Hoynes 1996 | Cross-state comparison of levels | PSID 1969–89 | Women 16–50 either married or household head in selected states | Probability a woman is a female head | Positive for blacks and whites, with larger magnitude for blacks |
| | Cross-state comparison of changes | | | Probability a woman is a female head | Zero and not significant for whites with state and/or individual fixed effects<br><br>Positive for blacks with state fixed effects but zero and not significant with individual fixed effects |
| Hutchens 1979 | Cross-state comparison of levels | PSID 1968–1972 | Female heads in 1970 in 20 states | Probability of 1970 female head remarrying or cohabiting by 1972 | Negative |
| Hutchens, Jakubson, and Schwartz 1989 | Cross-state comparison of levels | CPS 1984 | Women < 36, with at least one child, no husband present | Probability of being a household head | Positive for difference between household head and subfamily head benefit levels<br><br>Not significant for benefit guarantee level alone |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Hutchens, Jakubson, and Schwartz 1989 | Cross-state comparison of levels | CPS 1984 | Women < 36, with at least one child, no husband present | Probability that a woman is on or off welfare and is a household head or subfamily head | Difference between household head and subfamily head benefits significantly positive only for household head vs. subfamily head on welfare<br><br>All other effects insignificant |
| Jackson and Klerman 1995 | Cross-state comparison of levels | National Center for Health Statistics birth certificate tapes 1975–1990 | All women 15–44 | Birthrate for a state in a year (by age and race) | For both whites and blacks, negative for age < 21 and positive for age > 21 |
| | Cross-state comparison of changes | | | | Positive for whites through age 30, with largest magnitudes in early 20s<br><br>Negative for blacks after age 33 |
| | | | | Birthrate for a state in a year for first births (by age and race) | Positive for whites 15–19 and for blacks 16–26 |
| | | | | Birthrate for a state in a year for higher-order births (by age and race) | Positive for whites > 17 and for blacks 18–21 |
| | | | Selected states | Marital birthrate (marital births per total women) | Positive for blacks and whites |
| | | | | Nonmarital birthrate (nonmarital births per total women) | Positive for blacks and whites |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Janowitz 1976 | Cross-SMSA comparison of levels | Census 1969, 1970, 1973 DHEW 1968, 1970 | 58 SMSAs > 250,000 with illegitimacy data by race | Illegitimate birthrate among unmarried women | Positive for nonwhites 15–29<br><br>Not significant for whites |
| Lichter, LeClere, and McLaughlin 1991 | Cross-labor market area comparison of levels | Census 1980 | 328 labor market areas | Proportion of women<br>1. currently married<br>2. ever married<br>3. recently married (5 yr) | Negative for all three measures for blacks and whites |
| Lichter, McLaughlin, Kephart, and Landry 1992 | Cross-labor market area comparison of levels | NLSY 1979–1986 | Never-married women 18–28 from 1979 to 1986 | Probability a woman will have a transition into marriage | Not significant |
| Lichter, McLaughlin, and Ribar 1996 | Cross-county and -state comparison of changes | Census 1980,1990 | All counties with sufficient sample size | Fraction families with children < 18 headed by never-married or divorced women | Positive for whites and blacks<br><br>No effect for Latinos |
| Lundberg and Plotnick 1990 | Cross-state comparison of levels | NLSY 1979–1986 | Unmarried white women 21–23 in 1986 | Probability that a teen will not marry conditional on having a birth | Positive |
| | | | | Probability that a teen carries pregnancy to term conditional on pregnancy | Negative |
| | | | | Probability that a teen will become pregnant | Negative |
| Lundberg and Plotnick 1995 | Cross-state comparison of levels | NLSY 1979–1986 | Unmarried women 21–23 in 1986 | Probability a teen will not marry conditional on having a birth | Positive for whites<br><br>Not significant for blacks |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Lundberg and Plotnick 1995 | Cross-state comparison of levels | NLSY 1979–1986 | Unmarried women 21–23 in 1986 | Probability that a teen carries pregnancy to term conditional on having a pregnancy | Positive for whites<br><br>Not significant for blacks |
| | | | | Probability that a teen becomes pregnant | Positive but small for whites<br><br>Not significant for blacks |
| Matthews, Ribar, and Wilhelm 1995 | Cross-state comparison of levels | U.S. state data 1978–1987 | All U.S. states | Birthrate | Negative |
| | | | | Abortion rate | Positive |
| | Cross-state comparison of changes | | | Birthrate | Positive |
| | | | | Abortion rate | Insignificant |
| Moffitt 1990 | Cross-state comparison of levels | CPS 1969, 1977, 1985 | Men and women 16–55 | Probability of being married<br><br>Probability of being female head | Insignificant for whites<br><br>Negative for black men and mixed for black women |
| Moffitt 1994 | Cross-state comparison of levels | CPS 1968–1989 | Women 20–44 with < 12 years of education | Probability a woman is a subfamily or household head | Effects for whites are positive and significant<br><br>Effects for blacks are insignificant |
| | Cross-state comparison of changes | | | | No effect for whites<br><br>Negative effects for blacks |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Moore and Caldwell 1977 | Cross-state comparison of levels | Kanter and Zelnik 1971 survey | Women 15–19 | Probability a teen is sexually active | Positive for whites 16–18 with AFDC benefit level but not significant at other ages or for blacks |
| | | Vital Statistics 1974 | Selected states | Probability a teen becomes pregnant | Negative for blacks 12–15 but not sig. at other ages or for whites |
| | | | | Probability a pregnant teen will obtain an abortion | Negative |
| | | | | Probability a pregnant teen will marry before the birth | Not significant |
| | | | | Probability a pregnant teen will have an out-of-wedlock birth | Not significant |
| | | | | Out-of-wedlock birth rate of women 15–44 | Moore, Morrison, and Glei 1995 |
| Moore, Morrison, and Glei 1995 | Cross-state comparison of levels | National Survey of Children 1976, 1981, 1987 | Individuals 11–17 | Probability of first premarital sex | Not significant for girls and boys |
| | | | | Probability of contraceptive use conditional on premarital sex | Not significant for girls or boys |
| Murray 1993 | Time-series | Vital Statistics 1960–1988 | Annual aggregates | Illegitimacy ratio | |
| | Cross-state comparison of levels | | U.S. states | Illegitimacy ratio | Positive for white women starting in the mid-60s<br><br>No relationship for black women |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Ozawa 1989 | Cross-state comparison of levels | Vital Statistics 1984 | All U.S. states | Illegitimacy ratio for women < 19 | Positive for white teens<br><br>Not significant for black teens |
| Plotnick 1990 | Cross-state comparison of levels | NLSY 1979–1984 | Women 19–20 in 1984 | Probability a teen has a nonmarital birth by age 19 | Not significant for Hispanic or black women<br><br>Sometimes positive and sometimes not significant for white women |
| Rank 1989 | Within-state comparison of participants and nonparticipants | Wisconsin welfare records 1980–1983 | Two percent sample of all cases on rolls in September 1980 | Fertility rate | Negative effect |
| Robins and Fronstin 1996 | Cross-state or -region comparison of changes | CPS 1980–1988 | Never-married women 18–30 with no or 1 child | Probability of giving birth to first child | Not significant for whites and positive for blacks |
| | | | Never-married women 18–30 with no or 1 child and with no high school diploma | Probability of giving birth to first child | Not significant |
| | Cross-state comparison of changes in benefit increments | CPS 1980–1988 | Never-married women 18–30 with at least one child | Probability a woman will give birth to another child | Negative for whites and positive for blacks for second birth only<br><br>Higher-order births not significant for both races |
| | | | Never-married women 18–30 with at least one child and with no high school diploma | Probability a woman will give birth to another child | Positive for second birth<br><br>Not significant for higher-order births |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Rosenzweig 1995 | Cross-state comparison of changes | NLSY 1979–1990 | Women aged 22 from 1980 to 1987 | Probability a woman has had a premarital birth vs. only marital births or no births | Premarital birth vs. no birth: positive with a higher magnitude for low income women<br><br>Low income subsample: whites stronger than blacks<br><br>Marital births vs. no births: not significant for blacks or whites in full or low income samples |
| Rosenzweig and Wolpin 1994 | Cross-state comparison of levels | NLSY 1979–1987 | Female siblings 22–29 in 1987 | Probability of coresiding with parents and not being on welfare vs. not coresiding with parents and not being on welfare | AFDC benefit has no effect, but Food Stamp benefit has negative effect |
| Schultz 1994 | Cross-state comparison of levels | U. S. Census 1980 | Women 15–65 | Probability a woman is married | Negative at ages 15–24 for blacks and whites, not at other ages |
| | | | | Number of children ever born | Positive for black women 25–34<br><br>Negative for white women 15–24<br><br>Not significant at other ages |
| Schultz 1995 | Cross-state comparison of levels | U. S. Census 1990 | Women 15–64 | Probability a woman is married | Negative for blacks and whites |
| | | | | Number of children ever born | Negative for blacks and whites |
| Southwick 1978 | Cross-state comparison of levels | AFDC Characteristics Study 1973 | 31 U.S. states | Percentage of AFDC families with absent fathers | Positive |

**SUMMARY OF STUDIES OF EFFECT OF WELFARE ON THE FAMILY**
(see notes at end of table)

| Author, Date | Type of Study | Data Set | Main Sample | Dependent Variable | Welfare Result |
|---|---|---|---|---|---|
| Southwick 1978 | Cross-state comparison of levels | AFDC Characteristics Study 1973 | 31 U.S. states | Proportion of AFDC families in which the father is not married to the mother | Positive for difference and ratio between income available to a two- parent family vs. single-parent family |
| | | | | Proportion of AFDC families with at least 1 illegitimate child | Negative |
| | | | | Percentage of AFDC families with divorce, legal separation, or separation without a court decree | Positive for divorce and legal separation<br><br>Not significant for separation without a court decree |
| Winegarden 1988 | Time-series | Aggregate U.S. data 1947–1983 | Annual aggregates | Illegitimacy ratio | Positive for blacks but not for whites |
| Winkler 1995 | Cross-state comparison of levels | National Survey of Families and Households 1987 | Mothers 19–35 | Probability that a mother lives in an AFDC-UP defined two-parent family | Not significant |
| | | | | Probability that a mother is married | Negative for some specifications |
| Yelowitz 1993 | Cross-state comparison of changes | CPS 1989–1992 | Women 18–55 with at least one child | Probability that a mother is married | Having children in a family that is eligible for Medicaid has a positive effect |

Notes:

1) All Welfare Results are reported by race if separate estimates by race were obtained. If no race is mentioned, the results apply to all women.

2) Data Set refers only to the data source for the dependent variable.

## Appendix References


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