NBER WORKING PAPER SERIES

MIGRATION CONSEQUENCES OF WELFARE REFORM

Robert Kaestner
Neeraj Kaushal
Gregg Van Ryzin

Working Paper 8560
http://www.nber.org/papers/w8560

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2001

Migration Consequences of Welfare Reform
Robert Kaestner, Neeraj Kaushal and Gregg Van Ryzin
NBER Working Paper No. 8560
October 2001
JEL No. J6

ABSTRACT

In this paper, we investigate whether or not recent state and federal changes in welfare policy -- the imposition of time-limited benefits, the use of financial sanctions for non-compliance, and the setting of strict work eligibility rules -- affect the migration of low-educated unmarried women. Estimates of welfare's effect on migration reveal that welfare policy does indeed affect migration. Recent changes in policy that have made public assistance a less attractive alternative are associated with greater migration among low-educated unmarried women. Welfare reform has motivated low-educated women to move greater distances more frequently, and to combine such moves with employment. Estimates also indicate that welfare reform is associated with more local (i.e., within county) changes in residential location that are associated with employment, although estimates of this effect were not robust to estimation method. The close link between residential moves and employment in the post-reform period is consistent with the idea that welfare reform has motivated people to move for economic reasons such as better employment opportunities. This evidence suggests that the traditional way of thinking about the effect of welfare on migration -- as a strategic move to obtain higher benefits -- is inadequate.

Robert Kaestner
Institute of Government and Public Affairs
University of Illinois at Chicago
815 West Van Buren Street, Suite 525
Chicago, IL 60607
and NBER
Tel: 312.996.8227
Email: kaestner@uic.edu

Neeraj Kaushal
The Graduate Center
City University of New York

Gregg Van Ryzin
Baruch College/CUNY

Introduction

Individuals respond to financial incentives. This basic tenet of economic theory underlies the fundamental program design problem associated with many government assistance programs. The problem government officials face is how to structure assistance programs to help a specific group of persons achieve some desired outcome without inducing unintended behavioral changes that undermine the policy's original intentions. Aid to Families with Dependent Children (AFDC) was one such program that received much attention because of perceived unintended and adverse consequences it may have engendered. Scholars from several disciplines have investigated whether or not the AFDC program (welfare) caused women to reduce their labor market attachment, increase their fertility, alter their family structure and change their residential location.

The effect of welfare or cash assistance on residential choice has been labeled the "welfare magnet effect." The label arises from the hypothesis that current and potential welfare recipients are attracted to states with more generous benefits—i.e., magnet states. Current and potential welfare recipients may migrate to states with generous benefits or a state's generous benefits may dampen migration out of the state that would otherwise occur. Such migration may affect the level of welfare benefits that states offer because it may increase the number of program participants and thus the total cost of providing a given level of benefits. Consequently, states may be reluctant to offer generous benefits and as a result, welfare benefits may be lower than the socially desirable level (Brueckner 2000). More generally, the availability of public assistance programs may reduce migration among low-income families, when such a move might increase their ability to become economically independent—for example, because of improved job prospects or greater family support at the new location.

Recent state and federal changes in the AFDC program provide an excellent opportunity to re-examine the effect of welfare policy on migration. State waivers to the AFDC program beginning in the early 1990s and the passage of the Personal Responsibility and Work Opportunities Reconciliation Act (PRWORA) in 1996 resulted in profound changes in welfare policy that made public assistance a less attractive alternative for low-income families. Therefore, we would expect low-income families to increase

their investment in migration; just as we would expect them to increase their investments in all types of human capital, and in other activities (e.g., job search) that enhance economic independence. Accordingly, in this paper, we investigate whether or not recent state and federal changes in welfare policy—the imposition of time-limited benefits, the use of financial sanctions for non-compliance, and the setting of strict work eligibility rules—affect the migration of low-educated unmarried women. We focus on low-educated women because this is the demographic group most likely at risk of welfare receipt and therefore most likely to be affected by changes in welfare policy. If welfare policy does affect migration, it should be easier to identify such effects during a period of rapid and significant change in policy such as that which has recently occurred. Earlier studies of the welfare magnet effect had to rely on a much smaller amount of variation in state welfare policy making it more difficult to detect an effect. This difficulty may partly explain the inconsistent nature of the findings from that earlier empirical literature (Brueckner 2000). Such lack of consensus underscores the importance of additional study of the welfare magnet effect.

Previous Literature

Brueckner (2000) and Meyer (2000) provide recent reviews of the empirical literature related to the migration effects of welfare policy. Both authors note that the evidence is mixed. At one extreme is the study by Levine and Zimmerman (1995), who found that the generosity of welfare benefits had no effect on the migration decisions of poor single women. In contrast, Enchautegui (1997) found very large effects; a $25 (one standard deviation) increase in the difference in weekly AFDC benefits between states results in a 44% increase in the probability that a single woman will migrate out-of-state.

Meyer (2000) suggests that differences in methods may explain much of the inconsistency in reported estimates, and his discussion of this problem provides important guidance for our empirical analysis. The first statistical issue Meyer (2000) discusses is that of sample selection. Several previous studies (e.g., Gramlich and Laren, and Echautegui 1997) have examined the recent migration history of a sample of current welfare recipients. This sampling strategy tends to overstate the effect of welfare policy on migration. Some current welfare recipients may have migrated from low- to high-benefit states for reasons unrelated to the generosity of welfare benefits, but because they are more likely to be eligible for and thus

receive benefits in high-benefit states, it appears that their migration was welfare-induced. In other words, program participation is endogenous, determined by state welfare policy. The research implication of this criticism is clear: analyses of the effect of welfare policy on migration should not be limited to current (or past) welfare recipients.[1] It is preferable to examine a group at risk of welfare receipt, but not necessarily receiving benefits.

A similar empirical problem exists if samples are limited on the basis of poverty status (e.g., Walker 1994 and Levine and Zimmerman 1995). Limiting the sample to currently-poor families will bias the effect of welfare induced migration. The same reasoning used in the case of welfare participation applies here (Meyer 2000). Some currently-poor families may have migrated from low- to high-benefit states for reasons unrelated to the generosity of welfare benefits, but because poverty status is correlated with benefit levels, it appears that their migration is welfare induced. The direction of the bias depends on the correlation between benefit levels and poverty status. Meyer (2000) argues that poverty will be lower in high-benefit states because wages are higher in high-benefit states. This reasoning suggests that using poverty status will understate the effect of welfare-induced migration. Again, the empirical lesson to be gleaned from this criticism is to select a sample of at-risk families using characteristics other than program participation and poverty status, for example, education.

Meyer (2000) also discusses the empirical problems associated with studies that use a comparison-group approach. In these studies, the migration of a group likely to be affected by welfare policy, the target group, is compared to the migration of an otherwise similar group unaffected by welfare policy, the comparison group. The advantage of this approach is that it has the potential to control for omitted determinants of migration that are difficult to measure and which may be correlated with variation in state welfare policy. The disadvantage of the comparison-group approach is finding suitable target and comparison groups. We have already discussed the problems associated with identifying an appropriate target group to study. Past studies, only some of which have used a comparison-group approach, have used

[1] Meyer (2000) shows that selecting the sample on the basis of prior period welfare receipt produces biased estimates, but the bias is in the opposite direction and is likely to be smaller than if current program participation is used to select the sample.

single mothers on AFDC (Gramlich and Laren 1984; Enchautegui 1997), all single mothers (Blank 1988; Enchautegui 1997; Meyer 2000), poor single mothers (Levine and Zimmerman 1995), low-educated women (Enchautegui 1997; Meyer 2000), and low-educated mothers (Meyer 2000) as target groups.[2] Perhaps the more serious difficulty, however, is to identify an appropriate comparison group. There is rarely a perfect choice. Ultimately, the choice involves a tradeoff between a comparison group whose behavior is similar to the target group, but likely to be affected by the policy of interest, and a comparison group unlikely to be affected by policy, but whose behavior is not similar to the target group. Departures from either of these two selection criteria will result in biased estimates, although dissimilarity of the comparison group can result in a bias in any direction; whereas choosing a comparison group that is similar to the target group but affected by welfare policy will result in a bias toward zero.[3] In light of this statistical problem, previous studies have tended to use more than one comparison group. Among those groups that have been used are poor married women (Levine and Zimmerman 1995), poor single women without children (Levine and Zimmerman 1995; Meyer 2000), poor married women (Levine and Zimmerman 1995), all married women (Meyer 2000), and poor single men (Levine and Zimmerman 1995). The quality of the comparison group is critical to the success of this approach, and different choices may lead to different inferences.

Virtually all of the previous studies of the effect of welfare on migration have focused on interstate, or inter-region, migration. This focus is explained by the fact that historically, most of the variation in welfare policy has been between states. Thus, it was natural for previous researchers to focus on interstate moves and interstate differences in welfare benefits. However, welfare may also affect within-state moves. The availability of cash assistance may discourage current and potential welfare recipients from migrating to locations within their state that have better employment opportunities, more affordable housing, and greater family support. For example, welfare may discourage central city residents from moving to suburban locations where jobs are often more plentiful. Recent changes in welfare law, which have created significant within-state variation in policy, allow us to examine these within-state moves. Current and potential welfare

[2] The use of marital status and presence of children may also be problematic because these outcomes are influenced by welfare policy.

recipients may be more likely to invest in migration activities now that public assistance is temporary and the burdens of participation greater. The reduced attractiveness of public assistance may have induced migration related to improved job prospects and greater family support in the new location. These moves may occur within and between states. All else equal, migration among residents of high-benefit states, regardless of destination, is expected to be lower than migration among residents of low-benefit states; generous benefits dampen the economic incentive to invest in migration. Thus, even if there were no strategic migration behavior, in terms of seeking out higher welfare benefits, we would still expect to find positive net migration between low- and high-benefit states. Thus, previous studies' focus on interstate moves may have masked significant effects of welfare on residential location.

This brief literature review has drawn attention to three points. Additional study of the migration effects of welfare is warranted because of the policy importance of the issue and because there is no consensus as to the existence or magnitude of the welfare magnet effect. Second, methodological issues matter. We believe a good argument can be made for the use of a comparison-group approach; and when using this approach, it is arguably better to use demographic characteristics relatively unaffected by welfare policy to define target and comparison groups than to use program participation status or poverty status.[4] Finally, no previous study has examined the effect of welfare on within-state migration. Indeed, welfare may have a much greater effect on within-state moves than on interstate moves. Identifying only the latter understates the total effect of welfare on migration.

Research Strategy

To obtain estimates of the effect of welfare reform on migration we rely mainly on a comparison-group approach, which is also known as difference-in-differences (DD). The DD procedure is a quasi-experimental research design that compares the change before and after welfare reform in the migration of a group affected by welfare reform—the target group—to the change before and after welfare reform in the

[3] This statement is true only if the target group contains a higher proportion of affected members than the comparison group.

[4] We also use what we will refer to as a statistical comparison group. Specifically, we limit the sample to the target group, and include state-specific time trends in the regression model to account for changes over time in a state that may correlated with changes in welfare policy.

migration of a similar group unaffected by welfare reform—the comparison group. The logic of the DD procedure is revealed by Table 1.

Table 1
Residential Moves Before and After Welfare Reform

Period / Group	Before Welfare Reform (e.g., 1995)	After Welfare Reform (e.g., 1999)	After-Before
Target Group (Affected by Reform)	A	B	B-A
Comparison Group (Unaffected by Reform)	C	D	D-C
Difference-in-Differences			(B-A)-(D-C)

In Table 1, the difference B-A measures the change in migration, defined for example as any change in residence between year (t-1) and year (t), of the target group, the group affected by welfare reform, before and after the policy change. This difference may be due to the effect of welfare reform and other factors that change over time. The difference D-C measures the change in migration before and after welfare reform of the comparison group, a group whose behavior is similar to the target group, but a group unaffected by welfare reform. Changes in the migration of the comparison group are due to factors unrelated to welfare reform because this group is by definition unaffected by welfare reform. Thus, the difference-in-differences, (B-A)-(D-C), measures the effect of welfare reform on the migration of the target group. The effect of other factors is eliminated by subtracting the before-and-after change of the comparison group from the before-and-after change of the target group. Obviously, a critical assumption of the DD procedure is that changes in migration caused by factors unrelated to welfare reform are the same for the target and comparison groups. We discuss this assumption in more detail below in the Data section.

The DD analysis can also be cast in a regression framework. For example, the regression specification that corresponds to Table 1 is as follows:

$$(1)\ \text{Move}_{ik(t-1)} = \alpha_0 + \alpha_1 \text{Treat}_k + \alpha_2\ \text{Reform}_{j(t-1)} + \alpha_3\ (\text{Treat}_k\ \text{x}\ \text{Reform}_{j(t-1)}) + e_{ijkt}$$

where $Move_{ik(t-1)}$ is an indicator of whether or not woman 'i' in group 'k' migrated, or more generally moved, between year (t-1) and year (t). The variable $Treat_k$ in equation (1) denotes membership in our target group. $Reform_{j(t-1)}$ is an indicator equal to one if it is an observation from a period after welfare reform. The key parameter in equation (1) is α_3, which is the DD estimate corresponding to (B-A)-(D-C). Note that policy variables are measured as of year (t-1) prior to the move, and they refer to the policy in place in the state of residence in the year (t-1).

As written, the DD estimate obtained from a regression using the specification in equation (1) would equal the estimate obtained by the subtractions in Table 1. This simple specification of the regression model generates no advantage over the differences in means in Table 1. An expanded specification of the regression model that includes controls for personal characteristics, unmeasured state effects, and unmeasured time effects does have some advantage. Notably, DD estimates obtained from such a model will be more precise. Perhaps even more important, however, is the inclusion of personal characteristics, which may strengthen the validity of the underlying identification assumption of the DD analysis. The DD procedure assumes that unmeasured time-varying factors affect the migration decisions of the target and comparison groups equally. Controlling for observed differences between the target and comparison groups improves the chances that this assumption will be correct.

In practice, we estimate a model similar to equation (2) below.

$$(2)\quad \begin{aligned} Move_{ijk(t-1)} = {} & \beta_j + (\beta_j \text{ x } Treat_k) + \delta_t + X_{ijkt}\Gamma + Z_{j(t-1)}\lambda + \alpha_1 Treat_k + \\ & \alpha_2 \, Reform_{j(t-1)} + \alpha_3 (Treat_k \text{ x } Reform_{j(t-1)}) + e_{ijkt} \end{aligned}$$

Equation (2) includes controls for state effects (β_j), year effects (δ_t), personal characteristics (X_{ijkt}) such as age, race and number of children measured at year (t), and state level variables ($Z_{j(t-1)}$) such as the unemployment rate measured at year (t-1) and referring to state of residence in the prior year.

A strength of the difference-in-differences analysis is that it controls, in a parsimonious way, for time variation in outcomes unrelated to welfare reform—for example, due to macroeconomic changes. Clearly, this statement is correct only if we have chosen the "right" comparison group. Assuming we have chosen correctly, then, it is not as crucial in a DD analysis of the effect of welfare reform to control for macroeconomic activity as it is in other approaches (see Ziliak et al. 2000, and Figlio and Ziliak 1999 for an example in the context of welfare reform and caseload declines). To be cautious, however, our regression model includes controls for macroeconomic activity; specifically, we include the unemployment rate in the state of residence at the time of the survey (i.e., March) in the prior year, and the annual rate of unemployment in the calendar year two years prior to the survey (e.g., 1993 for the 1995 survey).

An alternative to the difference-in-differences approach is to limit the sample to members of the target group, and to control for unmeasured time variation using a purely statistical procedure. The advantage of this approach is that we do not need to identify an appropriate comparison group, which may be difficult in practice. Instead, we include controls for state-specific time trends in the model. The regression model specification for this alternative is:

$$(3)\ \text{Move}_{ij(t-1)} = \beta_j + \delta(\beta_j \, x Trend_t) + X_{ijkt}\Gamma + Z_{j(t-1)}\lambda + \alpha_1 \text{ Reform}_{j(t-1)} + e_{ijkt}\,.$$

There are two disadvantages with this approach. First, there may be non-linear time effects that will remain unaccounted for by the linear trend specified in equation (3). Second, there will be a significant amount of collinearity between the state-specific trends and the indicators of welfare reform. This collinearity can make it difficult to identify the effect of the policy, and also prohibits the inclusion in the regression model of controls for non-linear time effects, since this will only exacerbate the collinearity problem. In the current case, however, there is enough independent variation in welfare reform policy to obtain relatively precise estimates; a regression of an indicator of welfare policy (time-limited benefits) on controls for state and state-specific trends yielded an R-square statistic of 0.69.

Data

The data we use come from the Census Bureau's March series of Current Population Surveys (CPS) that are based on a nationwide sample of approximately 62,500 housing units interviewed each year. We use data from the years 1992 to 2000. We chose these years because they cover virtually all of the period of recent welfare reform. The March CPS contains information on important individual characteristics such as age, education level, ethnicity, race, personal earnings, and place of residence; and family characteristics such as family size, number of children under age six, number of children under age 18, family income, and its composition. We use these variables as controls in our regression analyses.

Most significantly, however, the CPS provides information about year-to-year changes in residential location. In each year, respondents to the survey are asked whether they moved residences from the prior year, and whether this move involved a change of county, change of urban/rural status or change of state. State of prior residence is provided, as is the urban/rural designation of the prior residence. Using these data we are able to construct a variety of dependent variables:

- an indicator of whether or not a person has changed residence in the prior year,
- an indicator of whether or not a person has moved to a new county,
- an indicator of whether or not a person has moved out-of-state,
- an indicator of whether or not a person moved from a central city to a non-central city,
- and an indicator of whether or not a person moved from a non-central city to a central city.

These dependent variables allow us to investigate a variety of hypotheses. Of particular importance to us is whether or not welfare policy induced moves were for what we will refer to as economic or strategic reasons. The broadest definition, any move, consists primarily of intrastate moves, and corresponds most closely to the concept of migration for economic reasons—better employment, more affordable housing, and greater family support. On the other hand, migration for strategic reasons—higher welfare benefits in the destination state—by definition requires an interstate move. Interstate moves may also reflect migration for economic reasons because of the high correlation of economic activity in small geographic areas (e.g., county and state). One way to separate out the two types of migration is to combine migration outcomes with

employment outcomes. Clearly, migration for economic reasons will be associated with employment since employment is one of the primary reasons people move. Migration for strategic reasons is not likely to be associated with employment since the motivation for this type of move is to take advantage of the more generous benefits in other states. Thus, we redefine all the dependent variables listed above by conditioning on employment status. For example, the first dependent variable listed above is redefined to equal one if a person changed residence and is currently working and zero otherwise.

Our key independent variable is welfare policy. We focus on three aspects of welfare reform: time limits on benefits, financial sanctions for failure to comply with work requirements, and exemptions from work requirements. The federal law imposed a maximum lifetime benefit limit of five years for each family, but 19 states have lifetime limits shorter than five years. In addition, all but two states (Michigan and Vermont use state money to replace federal money) have had a time limit since implementation of TANF in 1997, but eight states imposed these limits before the federal law was passed as part of a waiver from AFDC rules. The other two aspects of reform, financial sanctions and work exemptions are almost always associated with the federal law and are thus highly correlated with time limits. Because of this fact, we do not obtain separate estimates of their effect. Instead, we investigate whether or not migration is differentially affected if a state has only a time limit, or a time limit and one of these other two policies. We define a state to have a sanction policy if the state reduces benefits at least partially for initial failure to comply with work requirements and reduces benefits completely for repeated failure. We consider a state to have strict exemption policy if it requires all recipients with children over six months of age to work. While there are other aspects of welfare reform that may be analyzed, we limit our attention to these three because they are prominent components of welfare reform and are relatively easy to define and measure. Thus, to the extent that other aspects of welfare reform are correlated with these two items, we need to be cautious in our interpretation of the results. Both policy variables are measured as of the date they became effective and are merged to the March CPS files in the appropriate year.[5]

[5] We used a variety of data sources to define the policy variables including the 1999 CEA report, information from the National Governors Association and summaries of legislation maintained by the Agency for Children and Families of the Department of Health and Human Sevices.

We include a variety of other independent variables in the analysis:

- Age - five dummy variables indicating the following age ranges, 18-22, 23-27, 28-32, 33-38, and 39-44,
- Race/ethnicity – three dummy variables indicating black, non-Hispanic, white, non-Hispanic and Hispanic,
- Number of children – two dummy variables indicating the presence of children age 18 or younger and the presence of children age 5 or younger,
- Other family income – defined as total family income less respondent earnings,
- Education – a dummy variable indicating respondent has high school degree,
- State – 51 state dummy variables,
- Urban/rural – three dummy variables indicating that respondent's residence in prior year was in central city, non-central city but in MSA, and non-MSA,
- Year – nine year dummy variables,
- Unemployment – unemployment rate in the state of residence at the time of the survey in the in the prior year (e.g., March 1994 for the 1995 survey) and the annual rate of unemployment two years prior (e.g., 1993 for the 1995 survey).

Sample Selection

As noted above, we rely primarily on a comparison-group approach, also known as difference-in-differences (DD), to obtain estimates of the effect of welfare reform on migration. The DD method compares the changes in migration before and after welfare reform for two groups of people, those most and least likely to be affected by the welfare reform law. The group most affected by reform is the target group. We use low-educated (≤12 years of education), unmarried women between the ages of 18 and 44 as our target group. This is an appropriate target group because many women in this group are at risk of welfare receipt. Indeed, in 1994, when welfare rolls were at their peak nationally, approximately 23% of this group received welfare. In some cases, we further restrict the sample to women with children. Among this latter

group, 43% received welfare in 1994. Importantly, we do not use welfare receipt or income to define our target group, and thus we avoid the most serious biases noted by Meyer (2000).

The selection of the comparison group is difficult. We need to select a sample of persons who have similar migration patterns as our target group, but who were unaffected by welfare reform laws. We use two comparison groups:

- married women with 12 or fewer years of education,
- and unmarried women with 13 to 15 years of education.

We chose the first comparison group, married women with 12 or fewer years of education, because they have a similar level of education as our target group, and because their marital status makes it unlikely that they will be affected by welfare reform. However, migration decisions of married persons are complicated by spousal considerations; married persons move less often than do unmarried persons. Partly to control for this difference, our empirical analysis includes controls for family income and the number of children in the family. Thus, we are assuming that, conditional on observed characteristics, the migration of married and unmarried women with 12 or fewer years of education would be the same in the absence of welfare reform. We chose the second comparison group because women in this group have the same marital status, and their education levels are not too dissimilar to those of our target group. One potential problem with this group, however, is that a significant portion of it is at risk of welfare receipt. In 1994, 7% of women in this group received welfare. While this is significantly less than the 23% who receive welfare among the unmarried women with 12 or fewer years of education, it is still relatively high. This "contamination" of the comparison group will yield estimates of the effect of welfare reform that are biased toward zero.

The selection of target and comparison groups using marital status and presence of children is not innocuous. The advantage of using these selection criteria is that we identify a target group in which a large majority of women are at risk of public assistance receipt and a comparison group that has similar education level and child rearing responsibilities—two important correlates of migration. The disadvantage is that welfare reform may affect fertility and marriage, and thus we could confound estimates of the effect of welfare reform with changes in sample composition. For example, if welfare reform decreases fertility and

increases marriage, then the before-and-after difference we calculate is the sum of three effects: welfare reform, other time-varying factors that affect outcomes, and changes in sample composition. We try to account for the effect of other factors by subtracting the before-and-after difference of the comparison group. Thus, our estimate of the effect of welfare reform may be biased, assuming the validity of the comparison group, by changes in sample composition. However, there is little existing evidence that welfare reform caused significant changes in fertility or marital status.[6]

We partly address this concern by modifying our choice of target and comparison groups. Specifically, we use the same sample of low-educated women, but use only marital status to define target and comparison groups: unmarried women are the target group and married women are the comparison group. This strategy eliminates the potential bias from changes in composition due to welfare reform induced changes in fertility and allows us to assess the importance of this issue. In addition, we estimate alternative models that do not rely on the presence of a comparison group. Specifically, we limit the sample to members of the target group, and control for state-specific variation in migration that may be correlated with welfare reform by including state-specific time trends.

Results

Table 2 provides sample means of the dependent variables for the target and comparison groups used in the analysis. As shown in Table 2, approximately one quarter of target group members move each year, but most of those moves are within county. Only eight percent of target group members move to a different county, and only three percent move to a new state. Rates of migration similar to those for the two target groups are observed for members of the comparison group consisting of unmarried women. Among married women, however, rates of migration are lower.

In Table 3, we present OLS regression estimates of the effect of time-limited benefits on migration. Estimates in this table were obtained using a comparison-group approach. The top panel of the table shows

[6] See Hoynes (1997) for a review of earlier research on this topic. More recently, Schoeni and Blank (2000) and Kaushal and Kaestner (2001) find that welfare reform (e.g., time limits and family cap) have no effect on marital status.

estimates of the effect of welfare reform on the migration of low-educated unmarried women; the bottom panel shows similar estimates for low-educated, unmarried women with children. Each row of the table shows estimates obtained using the comparison group named in the first column. In the top panel, we show estimates related to two different comparison groups: low-educated married women and unmarried women with 13 to 15 years of education. In the bottom panel, there is only one comparison group: low-educated married women with children. Each column of Table 3 shows regression estimates pertaining to a different dependent variable.

Estimates of the effect of time-limited benefits on the probability of changing residences are small in magnitude and not statistically different from zero. The next dependent variable we consider is whether or not a woman changed county of residence. In this case, estimates in Table 3 indicate that time-limited benefits are associated with an increase in the probability of moving to a different county. All three estimates are positive and two are statistically significant. All of the estimates are of the same approximate magnitude suggesting an increase of approximately 0.8 percentage points, or an 11 percent increase, in the probability of moving to a new county. The estimated effects of time-limited benefits on the probability of changing state of residence are quite similar to those related to the probability of changing county. All three estimates of the effects of time limits on the probability of changing state of residence are positive and two are statistically significant. The magnitudes of these positive effects range from 0.3 to 0.7 percentage points, which represent relative effects of between 9 and 21 percent. Taken together, these estimates indicate that welfare reform induced low-educated women to migrate relatively long distances—across county or state boundaries. In contrast, there is no evidence that welfare reform led to an increase in residence changes involving smaller distances (e.g., within county). In fact, the increase in the number of between county and between state moves while the number of total moves stayed constant implies that short distance changes in residence declined.

We next divide the sample in two parts: those that lived in a central city in the prior year and those who did not. The rationale for dividing the sample in this way is related to the distinction between migration for economic reasons and migration for strategic reasons. Central city areas usually have worse economic

opportunities than non-central city areas and thus, welfare-reform related movements from a central city area to a non-central city area are most likely related to migration for economic reasons—for example, better employment opportunities. Moreover, most moves from central city to non-central city occur within state. Similarly, if economic reasons are the driving force behind welfare-reform related migration decisions, we would expect to see fewer moves from non-central city areas to central city areas. The estimates of the effect of time limits on moves from central city areas to non-central city areas show a now recurring pattern. All three estimates are positive and significant with magnitudes suggesting effects of between 16 and 24 percent. There are no significant effects of time-limited benefits on the probability of moving from a non-central city to a central city. In sum, most of the estimates in Table 3 indicate that welfare reform, as measured by the imposition of time-limited benefits, is associated with a significant increase in migration of low-educated unmarried women.

The use of time-limited benefits to measure welfare reform is crude, although it represents perhaps the most profound change in policy. Many states implemented a variety of other policy changes and this is reflected in the analysis presented in Table 4. In this table we include two measures of welfare reform as an attempt to measure differences in the intensity of reform. We construct two dummy variables: one that indicates that a state has implemented a time limit, but which does not have a strict sanction or work exemption policy, and the other that indicates that the state has a time limit and either a strict sanction or work exemption policy. The estimates in Table 4 are consistent with those in Table 3; welfare reform tends to be positively associated with an increase in migration of low-educated unmarried women. There is mixed evidence, however, as to whether there is a dose-response effect. It is not always the case that migration is affected more in states with time limits and other policies, as compared to states with only time limits. In fact, the estimates are about evenly split in showing evidence for and against a dose-response effect. Moreover, in nearly every case, statistical tests of whether the estimates associated with the two policy measures differ could not reject the null hypothesis of equality. These results suggest that it is primarily the imposition of time-limited benefits that is associated with migration, although other aspects of welfare reform not explicitly measured may still assert an influence. This is an important finding because few

women in the sample actually reach the five-year limit. Low-educated women are responding to the policy before exhausting their benefits.

To further explore the distinction between migration for economic reasons and migration for strategic reasons, we constructed variants of our dependent variables that combined migration and employment outcomes. Specifically, for each dependent variable, we redefined the variable to be equal to one (and zero otherwise) only if the respondent moved (residence, county, and state) between year (t) and year (t-1) and was employed at the time of interview in year (t). The rationale for this strategy is that migration for economic reasons should be associated with a higher probability of employment than migration for strategic reasons. The estimates pertaining to this analysis are presented in Table 6 and sample means of the dependent variables are shown in Table 5.

The estimates in Table 6 provide additional insight. Welfare reform is positively associated with an increase in the probability that a low-educated, unmarried woman will move, defined as any move, and have a job. This contrasts with the corresponding estimate in Table 3 that indicates no effect of welfare reform on the probability of moving. This provides evidence that moves are more likely to be associated with employment after welfare reform than prior to welfare reform. The difference in the estimates suggests that welfare reform has increased the employment of low-educated unmarried women, a result consistent with previous studies (Kaushal and Kaestner 2001). Estimates in Table 6 also indicate that welfare reform is associated with an increase in moving to a new county or new state, or from a central city to non-central city, and having a job. The fact that the estimates in Table 6 are of approximately the same magnitudes as those in Table 3 is evidence that more moves in the post-welfare reform period are associated with employment than in the pre-reform period. In a related analysis, we examined whether the effect of welfare reform on employment was larger for movers. Consistent with the evidence in Table 6, welfare reform had a larger effect on employment of movers versus non-movers (results not shown). While not conclusive, this evidence, and the evidence in Table 6 supports the notion that welfare reform has induced considerable migration for economic reasons, in particular for employment.

Interestingly, differences in economic and social conditions between the origin and destination state of movers have not changed pre- and post-welfare reform. In Table 7, we present average unemployment rates, poverty rates, wages, and crime rates in the origin and destination states of persons who changed states before-and-after the imposition of time-limited benefits. As the figures indicate, the post-reform period is characterized by lower rates of unemployment, poverty and crime. This is consistent with the improving economy during this period. However, destination-origin differences in unemployment and crime were greater in the pre-reform period. Interstate movers in the pre-reform period moved to states with better employment opportunities and states with lower crime. This is consistent with the more generous nature of welfare benefits during the pre-reform period. In order to induce a low-educated woman to move, the destination state must have significantly better opportunities.

Up to this point, we have been discussing estimates obtained using a comparison-group approach. As noted, the critical aspect of this approach is the choice of comparison group, and prudence motivated us to use more than one comparison group to obtain such estimates. An alternative, however, is to limit the sample to members of the target group and to include in the regression model controls for state-specific time trends that may confound estimates related to our policy variables. Equation (3), presented above, defines this model, and estimates from it are presented in Table 8. We present only those estimates analogous to Table 3 since these estimates provide sufficient information as to whether or not the estimates of the effect of welfare reform on migration presented earlier are sensitive to the method of estimation. In general, estimates in Table 8 are consistent with those presented earlier: time-limited benefits are associated with a statistically significant increase in the number of inter-county and interstate moves among low-educated unmarried women. The one major difference between Tables 3 and 8 relates to moves from central cities to non-central cities. For this outcome, estimates in Table 8 indicate that time-limited benefits are associated with a decrease in the number of such moves, which is the opposite of that indicated by the estimates in Table 3. The difference in these estimates is attributable to the fact that the positive estimates in Table 3 are the combination of two changes associated with welfare reform: an absolute and relative decline in central city to non-central city moves among the comparison groups, and an absolute increase, but relative decrease, in

central city to non-central city moves among the target group. In Table 8, estimates were obtained without using the comparison group, and the effect of welfare reform (i.e., time limits) is identified from deviations from a linear trend in the number of central city to non-central city moves. Obviously, there was a relative decline in the number of central-city to non-central city moves among low-educated unmarried women in periods after welfare reform. The comparison-group estimate of this effect was positive because the decline was greater, in fact negative in absolute terms, for members of the comparison group. It is not clear which estimate is correct, and the lack of consistency between the estimated effects warrants a more cautious interpretation of these estimates.

Conclusions

In this paper, we have re-examined the effect of welfare on migration. We have taken a more general approach to the problem than previous researchers by examining the effect of welfare on several types of moves—within county, between county, between state, and between central city and non-central city—not just interstate moves. While interstate moves to obtain more generous welfare benefits are important because of the public finance implications of such moves, they may be a small part of the impact of welfare policy on migration. Thus, our broader focus is appropriate; the availability of public assistance affects investments in migration generally, and therefore may affect all types of changes in residential location. Previous research has underestimated the total effect of welfare on migration by limiting the analyses to examining only interstate moves. Moreover, a comparison of the effect of welfare on different types of moves can identify exactly what types of migration decisions are being affected and can provide insight into whether these moves are being motivated by a search for greater welfare benefits or for other reasons such as improved job opportunities.

The results of our analysis reveal that welfare policy does indeed affect migration. Recent changes in welfare policy that have made public assistance a less attractive alternative are associated with greater migration among low-educated unmarried women. Welfare reform has apparently motivated low-educated women to move greater distances more frequently, and to combine such moves with employment. Estimates

also indicate that welfare reform is associated with more local (i.e., within county) changes in residential location that are associated with employment, although estimates of this effect were not robust to estimation method. The close link between residential moves and employment in the post-reform period is consistent with the idea that welfare reform has motivated people to move for economic reasons such as better employment opportunities. Indeed, a comparison of the estimates of the effect of welfare reform on interstate moves and on interstate moves that are associated with employment, suggest that nearly all interstate moves among low-educated unmarried women were associated with employment. For example, estimates indicate that the imposition of time-limited benefits was associated with a 0.007 increase in the proportion of low-educated unmarried women who move across state boundaries. Welfare reform was also associated with a 0.006 increase in the proportion of low-educated women who move across state boundaries and who are working. This implies that six out of every seven interstate moves among this group that were associated with welfare reform involved employment.

This evidence suggests that the traditional way of thinking about the effect of welfare on migration—as a strategic move to obtain higher benefits—is inadequate. Welfare appears to have a much larger effect on residential location because of its relationship to employment. This suggests that states should adopt welfare policies that facilitate changes in residential location such as transitional housing benefits and relocation expenses. For example, preliminary data from Tulare County, California, which provides relocation expenses to welfare recipients, suggests that such a policy could be effective. In the past three years, Tulare County has provided moving expenses for 750 welfare recipients, and 85% of these people had jobs six months after moving (Horst 2001). This suggests that states need to pay attention to housing policies related to welfare just as they do to child care and health insurance.

References

Brueckner, Jan K. 2000. "Welfare Reform and the Race to the Bottom: Theory and Evidence." *Southern Economic Journal* 66: 505-525.

Blank, Rebecca M. 1988. "The Effect of Welfare and Wage Levels on the Location Decisions of Female-headed Households. *Journal of Urban Economics* 24:186-211.

Council of Economic Advisers. 1999. Technical Report: The Effects of Welfare Policy and Economic Expansion on Welfare Caseloads: An Update. Washington, DC: Executive Office of the President of the United States.

Ennchautegui, Maria E. 1997. "Welfare Payments and Other Economic Determinants of Female Migration." *Journal of Labor Economics* 15:529-554.

Figlio, David N., and James P. Ziliak (1999). Welfare Reform, the Business Cycle, and the Decline in AFDC Caseloads. In Sheldon H. Danziger, (Ed.). *Economic Conditions and Welfare Reform* (pp 17-48). Kalamazoo, MI: W.E. Upjohn Institute for Employment Research.

Gramlich, Edward M. and Deborah S. Laren. 1984. "Migration and Income Redistribution Responsibilities. *Journal of Human Resources* 19:489-511.

Horst, Toni. 2001. "Move It or Lose It." www.dismal.com July 5, 2001.

Hoynes, Hilary Williamson. 1997. "Work and Marriage Incentives in Welfare Programs: What Have We Learned?" in *Fiscal Policy: Lessons from Economic Research* ed. By Alan Aurerbach. Cambridge, MA: MIT Press.

Kaushal, Neeraj and Robert Kaestner. 2001. "Welfare to Work: Has Welfare Reform Worked?" *Journal of Policy Analysis and Management* (forthcoming).

Levine, Phillip B. and David J. Zimmerman. 1995. "An Empirical Analysis of the Welfare Debate Using the NLSY. *National Bureau of Economic Research Working Paper No. 5264*, Cambridge, MA: NBER.

Meyer, BruceD. 2000. "Do the Poor Move to Receive Higher Welfare Benefits?" unpublished manuscript Northwestern University.

Schoeni, Robert F. and Rebecca M. Blank (2000). What has Welfare Reform Accomplished? Impacts on Welfare Participation, Employment, Income, Poverty and Family Structure. *National Bureau of Economic Research, Working Paper No. 7627*, Cambridge, MA: NBER.

National Governors Association. 1997. Summary of Selected Elements of State Plans for Temporary Assistance for Needy Families as of November 20, 1997, National Governors' Association Center for Best Practices. http://www.nga.org/CBP/Activities/WelfareReform.asp.

Walker, James R. 1994. "Migration Among Low-income Households: Helping the Witch Doctors Reach Consensus." unpublished manuscript University of Wisconsin.

Ziliak, James P., David N. Figlio, Elizabeth E. Davis, and Laura S. Connolly (2000). Accounting for the Decline in AFDC Caseloads: Welfare Reform or the Economy. *Journal of Human Resources*, 35:570-586.

Table 2
Description of the Sample: 1992-2000

Comparison and Target Groups	Movers		Inter-county Movers[1]		Inter-state Movers		Central City to Non-central City Movers[1]		Non-central City to Central City Movers[1]	
	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean
Target Group:										
Unmarried Women Education: 12 years or fewer	56901	0.270	50212	0.079	56901	0.034	17309	0.102	32903	0.031
Comparison Groups:										
Married Women Education: 12 years or fewer	65639	0.176	57917	0.057	65639	0.028	13259	0.109	44658	0.012
Unmarried Women Education: 13 to 15 years	57506	0.257	50730	0.097	57506	0.044	16538	0.124	34192	0.035
Target Group:										
Unmarried Women with Children Education: 12 years or fewer	24733	0.293	21813	0.075	24733	0.033	8331	0.093	13482	0.034
Comparison Group:										
Married Women with Children Education: 12 years or fewer	52281	0.167	46182	0.053	52281	0.026	10586	0.104	35596	0.011

Note: 1. Sample does not include observations from 1995.

Table 3
OLS Regression Estimates of the Effect of Time Limit on Migration of Unmarried Women
(Difference-in-differences analysis using CPS data for years 1992-2000)

Dependent Variable Target Group/ Comparison Group	Movers	Inter-county Movers[1]	Inter-state Movers	Central City to Non-central City Movers[1]	Non-Central City to Central City Movers[1]
Unmarried Women Education: 12 years or fewer					
Married Women Education : 12 years or fewer	-0.000 (0.005)	0.008*** (0.003)	0.007*** (0.002)	0.024*** (0.007)	0.002 (0.002)
Unmarried Women Education : 13 to 15 years	-0.004 (0.005)	0.006 (0.004)	0.003 (0.002)	0.016** (0.007)	0.001 (0.003)
Unmarried Women with Children Education: 12 years or fewer					
Married Women with Children Education: 12 years or fewer	0.002 (0.007)	0.008* (0.004)	0.005* (0.003)	0.021** (0.009)	0.001 (0.003)

Notes: 1. Analysis excludes data for the year 1995.
2. Each cell is a separate regression that includes the target group and the corresponding comparison group labeled in each row.
3. Heteroscedasticity adjusted standard errors in parentheses.
4. * .05<p=<.1, ** .01<p=<.05, *** p=<.01.

Table 4
OLS Regression Estimates of the Effect of Time Limits, Financial Sanctions and Work Exemptions on Migration of Unmarried Women
(Difference-in-differences analysis using CPS data for years 1992-2000)

Dependent Variable	Movers		Inter-county Movers[1]		Inter-state Movers		Central City to Non-central City Movers[1]	
Target Group / Comparison Group	Time Limit	Time Limit, Sanctions, Exemption	Time Limit	Time Limit, Sanctions, Exemption	Time Limit	Time Limit, Sanctions, Exemption	Time Limit	Time Limit, Sanctions, Exemption
Unmarried Women Education: 12 years or fewer								
Married Women Education : 12 years or fewer	-0.000 (0.008)	-0.000 (0.006)	0.008 (0.006)	0.009** (0.004)	0.008** (0.004)	0.006*** (0.002)	0.018* (0.011)	0.026*** (0.008)
Unmarried Women Education : 13 to 15 years	0.011 (0.009)	-0.010 (0.006)	0.010 (0.006)	0.004 (0.004)	0.010** (0.004)	0.000 (0.003)	0.036*** (0.010)	0.005 (0.008)
Unmarried Women with Children Education: 12 years or fewer								
Married Women with Children Education : 12 years or fewer	-0.004 (0.012)	0.004 (0.008)	0.002 (0.007)	0.010* (0.005)	0.004 (0.005)	0.006* (0.003)	0.013 (0.014)	0.023** (0.011)

Notes: 1. Analysis excludes data for the year 1995.
2. Each cell is a separate regression that includes the target group and the corresponding comparison group labeled in each row.
3. Heteroscedasticity adjusted standard errors in parentheses.
4. * .05<p=<.1, ** .01<p=<.05, *** p=<.01.

Table 5
Description of the Sample of Migrants: 1992-2000

Comparison and Target Groups	Movers and Have Job		Inter-county Movers and have Job		Inter-state Movers and have Job		Central City to Non-central City Movers and have Job		Non-central City to Central City Movers and have Job	
	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean
Target Group:										
Unmarried Women Education: 12 years or fewer	56901	0.160	50212	0.047	56901	0.020	17309	0.061	32903	0.018
Comparison Groups:										
Married Women Education: 12 years or fewer	65639	0.096	57917	0.029	65639	0.014	13259	0.059	44658	0.007
Unmarried Women Education: 13 to 15 years	57506	0.204	50730	0.076	57506	0.033	16538	0.096	34192	0.029
Target Group:										
Unmarried Women with Children Education: 12 years or fewer	24733	0.151	21813	0.037	24733	0.016	8331	0.049	13482	0.017
Comparison Groups:										
Married Women with Children Education: 12 years or fewer	52281	0.085	46182	0.025	52281	0.012	10586	0.052	35596	0.005

Note: 1. Sample does not include 1995.

Table 6
OLS Regression Estimates of the Effect of Time Limit on Migration of Unmarried Women
(Difference-in-differences analysis using CPS data for years 1992-2000)

Dependent Variable Target Group/ Comparison Group	Movers and have Job	Inter-county Movers and have Job[1]	Inter-state Movers and have Job	Central City to Non-central City Movers and have Job[1]	Non-Central City to Central City Movers and have Job[1]
Unmarried Women Education: 12 years or fewer					
Married Women Education: 12 years or fewer	0.021*** (0.004)	0.011*** (0.002)	0.006*** (0.002)	0.019*** (0.006)	0.002 (0.002)
Unmarried Women Education: 13 to 15 years	0.013*** (0.005)	0.007** (0.003)	0.003 (0.002)	0.016*** (0.006)	0.001 (0.002)
Unmarried Women with Children Education: 12 years or fewer					
Married Women with Children Education: 12 years or fewer	0.037*** (0.006)	0.012*** (0.003)	0.007*** (0.002)	0.015** (0.007)	0.002 (0.003)

Notes: [1] Analysis excludes data for the year 1995.
2. Each cell is a separate regression that includes the target group and the corresponding comparison group labeled in each row.
3. Heteroscedasticity adjusted standard errors in parentheses.
4. * .05<p=<.1, ** .01<p=<.05, *** p=<.01.

Table 7
Differences in State Characteristics of Movers Before and After Time Limit

Community Characteristics	No Time Limit		Time Limit	
	Move From	Move To	Move From	Move To
Unemployment Rate	6.58	6.31	4.64	4.60
Median Real Wage[1]	4.91	4.83	4.89	4.87
Median Real Wage[1,2]	4.87	4.81	4.90	4.88
Poverty Rate	14.42	14.38	12.74	12.76
Poverty Rate[2]	14.30	14.17	12.80	12.77
Crime Rate	5568	5351	4680	4702
Violent Crime	724	639	556	547
Property Crime	4844	4711	4124	4155

Note: [1]Deflated with CPI base years 1982-84.
[2] Three year moving average.

Table 8
OLS Regression Estimates of the Effect of Time Limit on Migration of Unmarried Women
(With controls for state-specific trends; CPS data for years 1992-2000)

Dependent Variable Sample	Movers	Inter-county Movers [1]	Inter-state Movers	Central City to Non-central City Movers [1]	Non-Central City to Central City Movers [1]
Unmarried Women Education: 12 years or fewer	0.005 (0.007)	0.009** (0.005)	0.004 (0.003)	-0.023*** (0.009)	0.002 (0.004)
Unmarried Women with Children Education: 12 years or fewer	0.014 (0.012)	0.014** (0.007)	0.008* (0.004)	-0.019 (0.013)	-0.003 (0.006)

Notes: [1] Analysis excludes data for the year 1995.
2. Each cell is a separate regression that controls for number of children, number of children less than age 6, family income, race, state unemployment rate, state unemployment rate with one year lag, state effects and state trend effects. Each regression also includes five age dummies and a dummy for high school.
3. Heteroscedasticity adjusted standard errors in parentheses.
4. * .05<p=<.1, ** .01<p=<.05, *** p=<.01.