

**An Empirical Analysis of the Welfare Magnet Debate  
Using the NLSY**

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## **Abstract**

This paper examines the extent to which differences in welfare generosity across states lead to interstate migration. Using microdata from the National Longitudinal Survey of Youth between 1979 and 1992, we employ a quasi-experimental design that utilizes the categorical eligibility of the welfare system. The “treatment” group consists of all those in the survey who appear eligible to participate in Aid to Families with Dependent Children. The “control” group contains those who are poor but ineligible for other reasons. The pattern of cross-state moves among poor single women with children who are likely to be eligible for benefits (treatment-group members) is compared to the pattern among other poor households. We find little evidence indicating that welfare-induced migration is a widespread phenomenon.

## **An Empirical Analysis of the Welfare Magnet Debate Using the NLSY**

Recent discussions surrounding welfare reform have stridently asserted the failure of Great Society welfare programs. Much of the hostility has been directed at the joint state-federal program, Aid to Families with Dependent Children (AFDC). Proposals for a “new federalism” currently being advanced would radically alter the balance of power between the federal and state governments by moving much of the authority concerning welfare programs to the state level. States would receive block grants from the federal government and would likely exercise wide discretion in using these funds. This, it is argued, would enhance innovation and ultimately reduce poverty—both by program innovation and by eliminating perverse incentives associated with existing federal policy.

The debate over which level of government should design and administer welfare policy has a long history, a crucial issue being the potential for development of interstate inequality in program benefits under a state-dominated system. This inequality could stimulate migration—allegedly generating a variety of potentially negative consequences.<sup>1</sup> Indeed, it is often asserted that traditionally “generous” states such as Wisconsin, New York, and California act as “welfare magnets,” as low-income individuals move to these states to secure higher benefits.

The sensitivity of poor people’s migration decisions to secure welfare benefits is crucial to appraising certain public policies, and is also an important factor in establishing the robustness of a great deal of academic research investigating the incentive effects of the welfare system. This literature

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<sup>1</sup>A number of different perspectives exist on the desirability of interstate variation in benefits and its potential consequences. One is that the resulting “variety” of benefit packages could allow people to find locations better matching their preferences (Tiebout, 1956). Alternatively, state governments could react to a perceived immigration of poor individuals by reducing their welfare benefits. Thus, the threat of “hordes at the gate” could result in a “downward competition,” and the final outcome would be a less generous welfare system. Indeed, the belief by several state governments that they are welfare magnets has led to calls for a variety of policy changes, including benefit reductions, residency requirements, or an increased federal role in establishing uniform benefits. Finally, if individuals do not migrate in response to benefit differentials (perhaps due to financial constraints) the system will result in uneven treatment of the poverty population.

has utilized the perceived “natural experiment” afforded by the interstate variation in benefits.<sup>2</sup> Such variation is thought to provide a set of “miniature laboratories” in which the behavior of poor individuals in more and less generous states can be contrasted to assess the effects of welfare benefits on a variety of behaviors (e.g., labor supply, family formation). This exercise, however, often employs the important maintained hypothesis that state of residence is exogenous—an assumption violated if welfare-induced migration occurs. Previous research on welfare-induced migration has been hampered by crude definitions of the relevant populations, highly aggregated data, and limited controls for other factors influencing migration.<sup>3</sup>

In this paper we use data from the National Longitudinal Survey of Youth (NLSY) to estimate the magnitude of welfare-induced migration. We employ a conceptually straightforward, quasi-experimental approach, contrasting the migration decision of a “treatment group” of individuals eligible for AFDC and “control groups” that are categorically ineligible. Based on our empirical findings, we find little evidence indicating that welfare benefits have an effect on migration decisions.

## II. TRENDS IN THE LEVEL AND DISPERSION IN WELFARE BENEFITS

The greater the variation in benefits across states, the more the incentive for potential welfare recipients to move. We therefore begin by documenting trends in the dispersion of welfare benefits across states.

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<sup>2</sup>This variation results from the fact that AFDC is a joint state-federal program, and states have considerable discretion in setting benefits. Examples of this approach include Zimmerman and Levine (1993) and Currie et al. (1993) among others. Moffitt’s (1992) survey of the impact of the welfare system contains many studies using this approach. See Besley and Case (1994) for a discussion of policy endogeneity.

<sup>3</sup>Notable exceptions are studies by Walker (1994) and Blank (1986). Other studies include Frey et al. (1995), Gramlich and Laren (1984), Hanson and Hartman (1994), and Peterson and Rom (1989). A summary of the relevant research prior to 1979 is provided in Cebula (1979), and the more recent research is summarized and critiqued in Moffitt (1992). Corbett (1991) provides a nice summary of many of the key issues.

Figures 1 and 2 show the distribution of AFDC benefits in 1979 and 1992, respectively, in 1992 dollars.<sup>4</sup> States shaded darker offer higher benefits. Two things can be noted from these figures. First, average real AFDC benefits have fallen over the period.<sup>5</sup> For instance, the real AFDC benefit in Pennsylvania fell from \$494 to \$376 in 1992 dollars between the two years. Second, the dispersion in real benefits has also fallen as traditionally higher-benefit states have cut their real benefit levels to a greater extent than have traditionally lower-benefit states.

The reduced dispersion in welfare benefits over time is affected by the increasing importance of food stamp benefits.<sup>6</sup> Because food stamp benefits are uniform across states, an increase in the proportion of food stamps in the benefit package would reduce the dispersion in combined benefits.<sup>7</sup> As shown in Figure 3, in 1973 food stamps composed about a third of the combined AFDC/food stamp benefit; by 1992 the ratio had risen to about a half.

The trend in benefit variation in AFDC and in combined AFDC plus food stamps across states over time is shown in Figures 4-6. These figures represent trends in the ratio of the highest-benefit state to the lowest-benefit state, the ratio of the 75th to 25th percentile in the benefit distribution, and in the coefficient of variation, respectively. All show a significant reduction in benefit dispersion, particularly when food stamp benefits are included. For instance, in 1974 welfare

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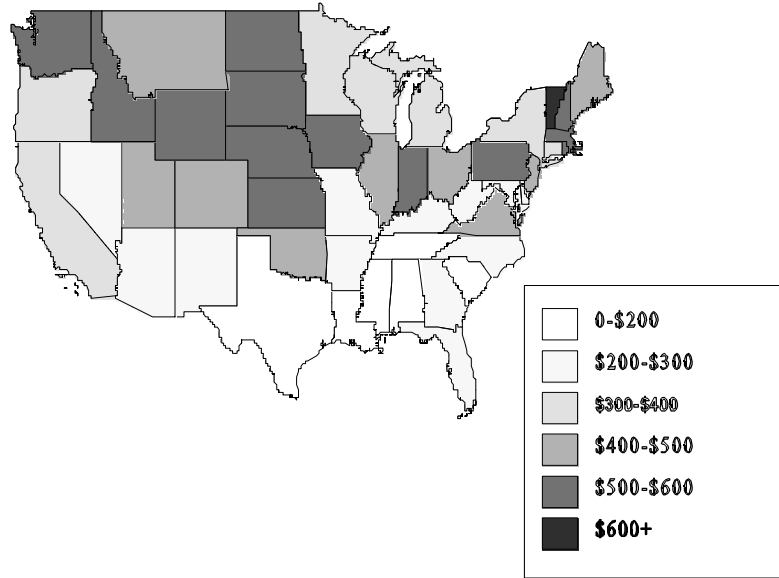
<sup>4</sup>A state-level price index was provided by Steven Craig. Alabama in 1989 is indexed to equal 100. The index is a population weighted average of city and regional nonmetropolitan budgets from the Bureau of Labor Statistics.

<sup>5</sup>This pattern is quite well-known and has been extensively documented by Moffitt (1990).

<sup>6</sup>Other state-run programs in the social safety net, such as public housing, may affect the dispersion in welfare generosity. Typically, the generosity of these other programs is highly correlated with AFDC benefits. One interesting example is the Medicaid system, which expanded significantly during the 1980s, some states increasing their eligibility a year or two before others (Currie and Gruber, 1994). These discrepancies across states, however, were short-term, and more generous welfare states were typically the first to expand eligibility. Incorporating these other programs into the analysis would be unlikely to affect the qualitative findings presented here or the remainder of the paper.

<sup>7</sup>The reduction in dispersion is somewhat muted by the fact that food stamp benefits are reduced by 30 cents for each dollar of AFDC receipt.

**Figure 1**  
**Maximum AFDC Benefits, 1979 (1992 dollars)**



**Figure 2**  
**Maximum Benefits, 1992 (1992 dollars)**

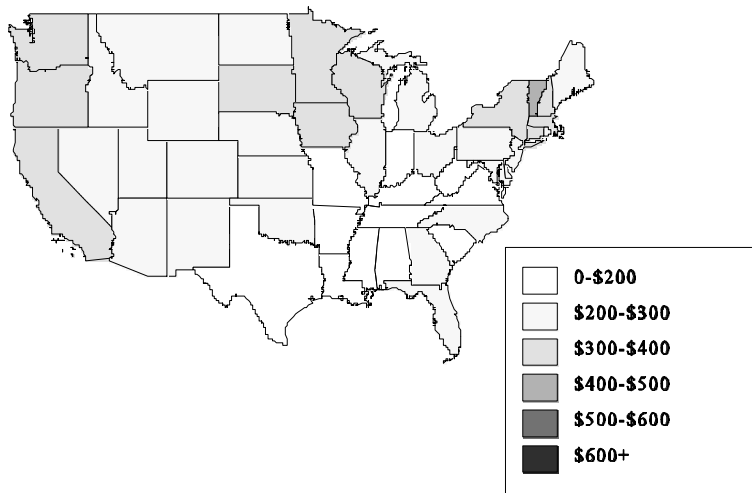
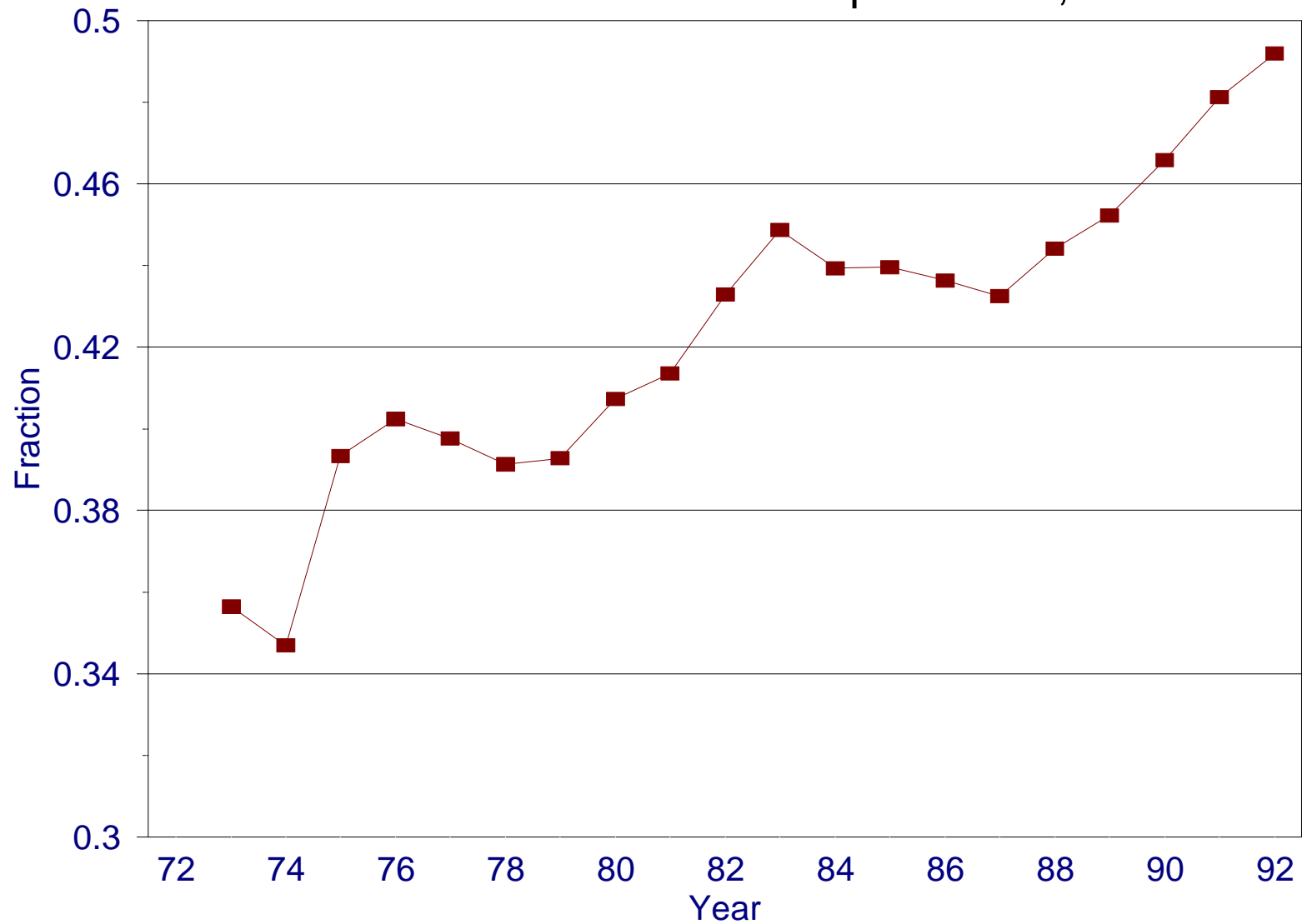
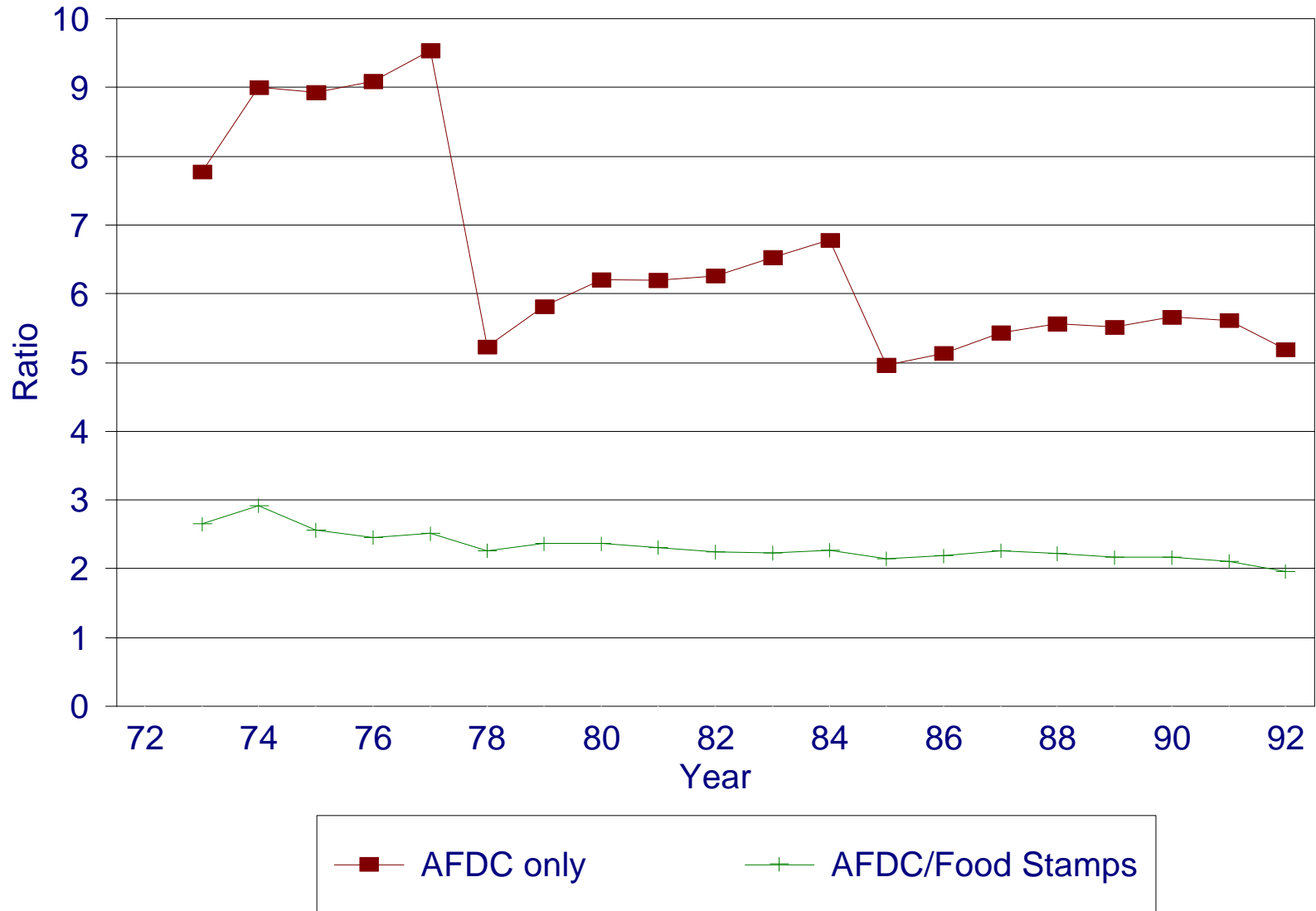


Figure 3: Food Stamp Benefits as a Fraction of Combined AFDC and Food Stamp Benefits, 1973-92



Source: Authors' calculations based on data from the Green Book (various years).

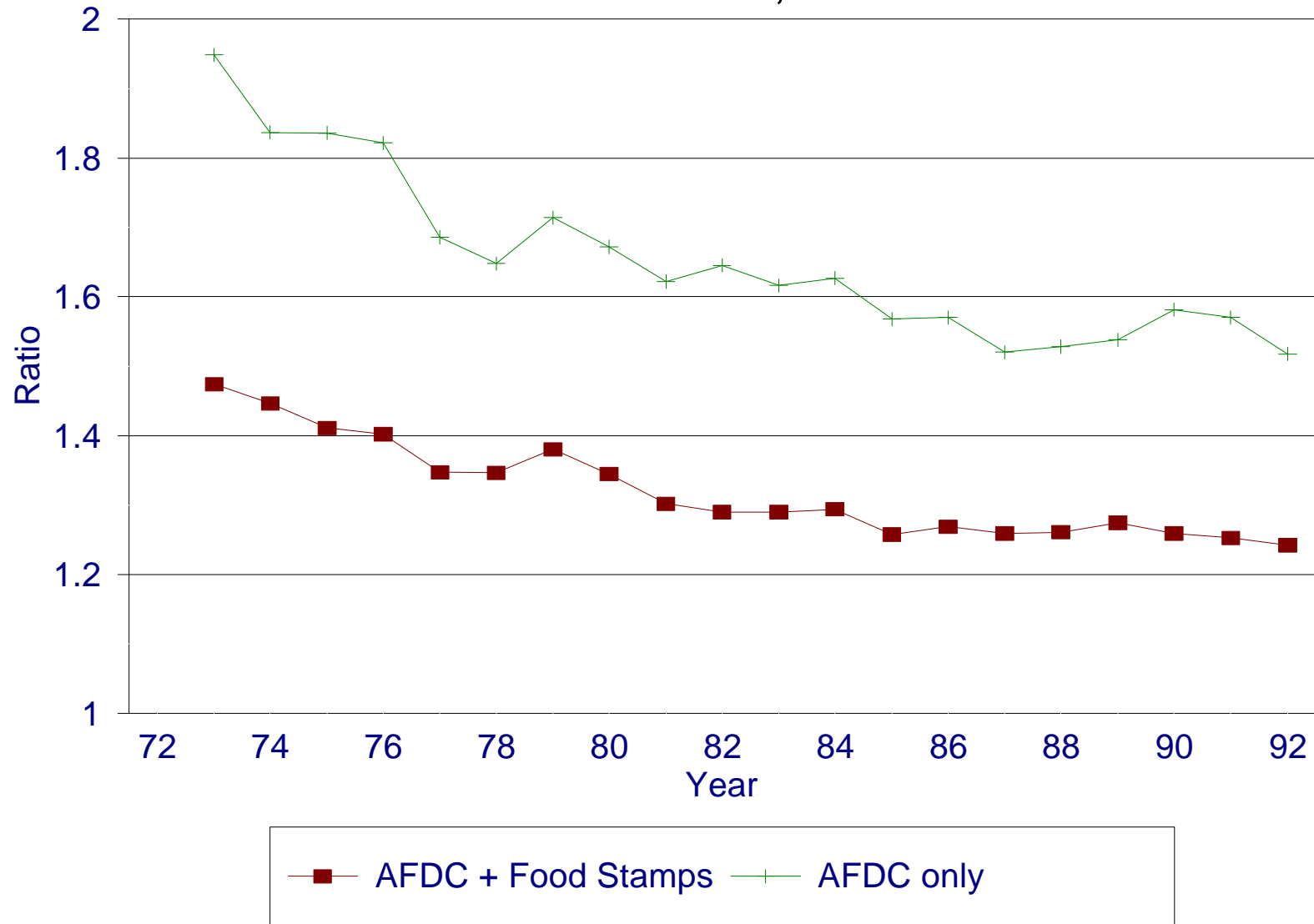
Figure 4: Ratio of Maximum to Minimum Welfare Benefits, 1973-1992



Source: Authors' calculations based on data from the Green Book (various years).

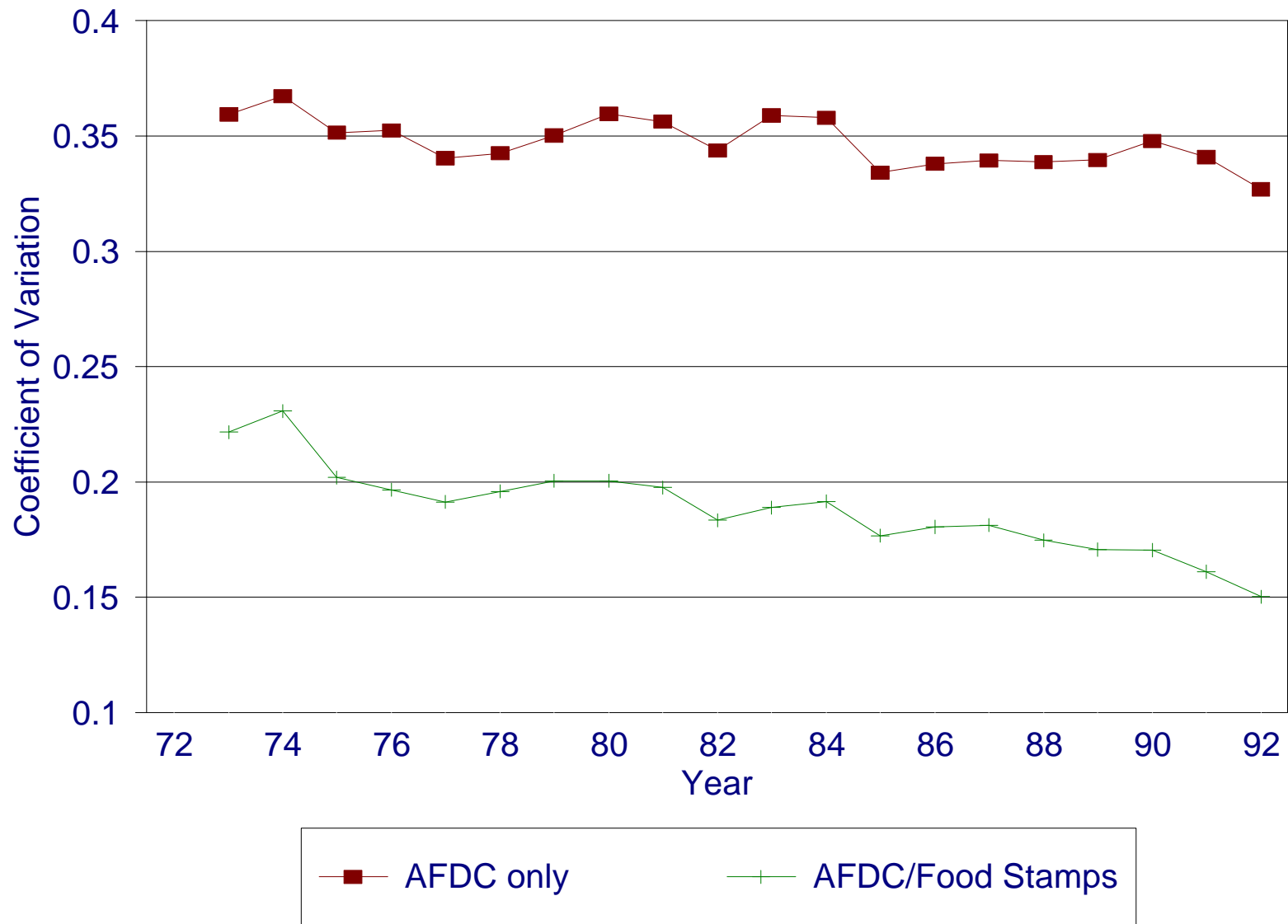


Figure 5: Ratio of 75th Percentile to 25th Percentile in Welfare Benefits, 1973-92



Source: Authors' calculations based on data from the Green Book (various years).

Figure 6: Coefficient of Variation in Welfare Benefits, 1973-1992



Source: Authors' calculations based on data from the Green Book (various years).

recipients in the highest-benefit state (Wisconsin) could obtain AFDC benefits that were nine times greater than the lowest-benefit state (Mississippi) and combined AFDC and food stamp benefits that were almost three times greater. By 1992, these ratios had fallen to about five and two, respectively (between Vermont and Mississippi). Similar comparisons can be made in the other measures of benefit dispersion.

Two principal conclusions can be drawn from these figures. First, benefits vary significantly across states. Second, the dispersion—although still substantial—has been falling through time. This suggests that there is a plausible case to be made for the *possibility* of welfare-motivated migration, although such incentives have fallen over the last decade or so.

## EXPERIMENTAL FRAMEWORK

### Hypothetical Randomized Experiment

A hypothetical experimental study examining the extent of welfare-induced migration could proceed by randomly assigning individuals into a treatment group, consisting of those who are eligible to receive welfare benefits, and an otherwise identical control group, which are not. Subjects would then be distributed across states which differ only by the generosity of their welfare systems (assumed here to be “high” and “low” benefit states, for simplicity). We could then observe whether (a) the probability that a person moves is associated with their treatment/control status, and (b) that moves are consistent with a hypothesized welfare motivation. To facilitate the discussion, consider the following transition matrix showing the locational choices of the treatment (T) and control groups (C):

Residence at time t:	Proportion Moving:	Residence at time t+1:	
		High	Low
High	$M_H^T, M_H^C$	$M_{HH}^T, M_{HH}^C$	$M_{HL}^T, M_{HL}^C$
Low	$M_L^T, M_L^C$	$M_{LH}^T, M_{LH}^C$	$M_{LL}^T, M_{LL}^C$

where  $M_H^T, M_H^C$  are the fraction of treatments and controls originally in high-benefit states who move and  $M_{LH}^T, M_{LH}^C$  are the fraction of the treatment and control *populations* (i.e., nonmovers are included in the denominator), respectively, moving from low-benefit states to high-benefit states. The notation is analogous for other entries.

### Implications for the Probability of Moving

Differential welfare benefits may affect whether a person moves out of state. We would expect the following to hold true given the experiment outlined above (*ceteris paribus*):

$$M_L^T - M_L^C > 0 \quad (1)$$

$$M_H^T - M_H^C < 0 \quad (2)$$

$$M_L^T - M_H^T > 0 \quad (3)$$

$$M_L^C - M_H^C = 0 \quad (4)$$

The logic of these implications draws on the fact that treatments and controls are assumed to be the same, on average, except that relative welfare benefits could influence the treatment group's migration decision among states that are otherwise identical. Thus, for example, equation (1) states that treatment-group members in low-benefit states should be more likely to move than should controls. This is because all else is assumed equal between treatment and controls except the fact that treatments have an added incentive to obtain higher benefits. Equation (2) similarly suggests that treatments should

be less likely to move out of high-benefit states. Indeed, in both equations (1) and (2) the control group can be thought of as providing a baseline estimate of the fraction of treatments that would move if there were no incentive to migrate for higher benefits (e.g., if benefits were uniform). Random assignment would cause other factors (e.g., proximity to family and friends) influencing migration to be similar between the two groups. Equation (3) states that treatments should, *ceteris paribus*, be more likely to leave low-benefit states than high-benefit states. In this case we are contrasting treatments across states. Such a comparison is valid so long as these states are identical in all other dimensions, which has been assumed in this hypothetical experiment. This assumption is also implicit in equation (4), which indicates that control-group members are equally likely to move out of high-benefit and low-benefit states.

#### Implications Concerning the State Chosen

In addition to differences between treatment and control groups in the likelihood of moving, we can also place conditions on the relative likelihood of treatment and control groups moving to higher- or lower-benefit states. Again *ceteris paribus*, the experiment outlined above suggests:

$$M_{LH}^T - M_{LH}^C > 0 \quad (5)$$

$$M_{LH}^T - M_{HL}^C < 0 \quad (6)$$

$$M_{HL}^T - M_{HL}^C > 0 \quad (7)$$

$$M_{LH}^C - M_{HL}^C = 0 \quad (8)$$

Equation (5) states that a higher proportion of treatment members than controls should move to higher-benefit states.<sup>8</sup> That is, higher benefits should attract the treatment members, but not controls (other things equal). Indeed, if other factors are equal, and if the treatment and control groups are otherwise identical, then the difference shown in (5) would provide an estimate of the fraction of treatment moves that are caused by interstate variation in benefits: i.e.,  $M_{LH}^C$  provides a baseline estimate of the fraction of treatments who would move to higher-benefit states in the absence of benefit considerations.

Equation (6) is analogous, though opposite in direction. Equation (7) contrasts the movements of treatments from low- to high-benefit states and high- to low-benefit states. Other things equal, treatments should be more likely to move from low-benefit to high-benefit states than the reverse, if other state characteristics are identical. For the same reason, equation (8) suggests that the fraction of the controls who move to higher-benefit states should equal the fraction moving to lower-benefit states.

### The Natural Experiment

Although the experimental design outlined above clearly depicts the hypotheses we are interested in testing, many complications are involved in implementing it. Foremost among them is that there is no randomized assignment to treatment and control groups. The categorical nature of welfare eligibility, however, allows us to set up a natural experiment similar in nature to the approach just described.

The requirement for membership in the treatment group is the possibility of benefiting from higher welfare benefits. Providing such a classification is, however, not without its pitfalls. Clearly,

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<sup>8</sup>Note that this is not the probability of moving from a low-benefit state to a high-benefit state *conditional on moving*. Since the fraction moving could depend on benefit differences, comparing conditional differences could yield misleading results. For example, suppose there are 100 people in both the treatment and control populations. Suppose 10 control members and 15 treatment members move, and of these, 5 controls and 10 treatments move to a higher-benefit state. Then, conditional on moving, the treatments are a third more likely to move to a higher-benefit state. This estimate is, however, biased downward, as shown by the fact that twice as many people from the control versus the treatment population move from a low- to a high-benefit state.

current welfare recipients should be included in this group. Recent recipients may also be considered potential beneficiaries, since the likelihood of subsequent welfare receipt is high among this group. Other low-income single women with children may meet the eligibility requirements or be close to meeting these requirements and may face an incentive to move to higher-benefit states. In this analysis, we classify women with these characteristics as members of the treatment group, “low income” being defined as income below the federal poverty standard.

This classification system may introduce some biases into estimated treatment effects. For example, poor married women with children or poor single women without children may benefit by moving to more generous states if they subsequently get divorced or have a child, respectively. In addition, some women may be poor but are still not categorically eligible for welfare because state AFDC need standards are lower than federal poverty standards. Either of these circumstances would introduce a downward bias on the estimated extent of welfare-induced migration. We have tested the sensitivity of our results to alternative definitions of the treatment group and obtained results similar to those presented below.<sup>9</sup>

Our control group includes respondents who are poor but not categorically eligible for AFDC: they include poor single women without children, poor single men, poor married men, and poor married women. No one in the control group has received welfare within (at least) two years. Men, regardless of their economic status, have historically been largely excluded from receiving welfare benefits. Married men and women are largely ineligible because the Unemployed Parent component of AFDC, which provides benefits to couples, is limited. Selecting only poor individuals increases the similarity between the control and treatment groups.

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<sup>9</sup>In addition to experimenting with different measures of poverty status, we have also used alternative specifications for our control and treatment groups. In particular, we have experimented with a treatment group of women who dropped out of high school and a control group who did not. Similar results are obtained using this alternative stratification of the sample.

### Difficulties in Implementation

There are two main problems in using these defined treatment and control groups to examine the implications provided in equations (1)–(8) regarding the influence of welfare benefit differentials on migration decisions. First is the possibility that the treatments and controls are different along dimensions other than their welfare eligibility, a likely situation in the absence of true random assignment. It may, for example, be more difficult for poor single women with children to move than for a poor man with no family obligations, owing to differential costs of mobility. This condition would yield biased estimates of welfare-induced migration obtained from equations (1), (2), (5), and (6). Second is the fact that states differ in their welfare generosity *and* a myriad of other characteristics. For example, if low-benefit states have a more salubrious climate, then the flow of treatments out of the state might be smaller than the flow of treatments out of higher-benefit states with a worse climate. This set of circumstances would indicate that estimates of welfare-induced migration provided by equations (3) and (7) would be biased towards zero.

A potential correction to these problems is possible by applying a difference-in-difference estimator. Instead of estimating equations (1)–(3) or (5)–(7), this approach would estimate

$$(M_L^T - M_H^T) - (M_L^C - M_H^C) > 0 \quad (9)$$

$$(M_{LH}^T - M_{HL}^T) - (M_{LH}^C - M_{HL}^C) > 0 \quad (10)$$

or, equivalently

$$(M_L^T - M_L^C) - (M_H^T - M_H^C) > 0 \quad (11)$$



$$(M_{LH}^T - M_{LH}^C) - (M_{HL}^T - M_{HL}^C) > 0 \quad (12)$$

This approach is facilitated by two assumptions: (1) control and treatment groups value other state characteristics equally,<sup>10</sup> and (2) differences in the propensity to move among control and treatment groups are constant in the absence of welfare benefit differentials. The first assumption allows us to control for differences in other state characteristics by comparing migration patterns among control-group members in different states. If other differences between states exist, then  $M_L^C - M_H^C$  and  $M_{LH}^C - M_{HL}^C$  would provide an estimate of the effect of these differences on migration patterns between the states (i.e., the difference in migration we would expect to observe for treatments between the low- and high-benefit states if welfare differentials did not exist). Applying this assumption, equations (9) and (10) will provide an estimate of the extent of welfare-induced migration controlling for differences in state characteristics.

The second assumption allows us to correct for differences in the propensity to move. If  $M_L^T - M_L^C = M_H^T - M_H^C = \alpha$ , then estimating equations (11) or (12) differences out  $\alpha$ , which represents the constant difference in the propensity to move. Therefore, this assumption indicates that equations (11) and (12) can provide an estimate of welfare migration unbiased by constant differences

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<sup>10</sup>To illustrate the effect of the failure of this assumption, suppose that high-benefit states are also high-tax and low-crime states (relative to the low-benefit states). Then, if treatments care more about crime, and controls more about taxes, then the fraction of treatments moving out of the low-benefit state versus high-benefit state would be larger, but the fraction of controls making the comparable move would be lower. Thus, the control group would not provide a good baseline for the treatments. The more homogeneous the treatment and control groups, the less likely this is to be a problem.

in the propensity to move. Thus, the difference-in-difference equations control for both constant differences in the propensities of the treatments and controls to migrate and for observable and unobservable differences between the high- and low-benefit states.

## EMPIRICAL SPECIFICATION

Although this experimental framework provides useful intuition regarding the analysis we plan to conduct, other shortcomings in this approach require additional statistical solutions. For instance, a significant problem in implementing the experimental design is that there are 51 state AFDC systems with different levels of benefits, not just high-benefit and low-benefit states. Estimating migration rates among all 51 states is too strenuous a task given the data available for this project. As an alternative, we initially approximate the tests described earlier in the following ways. First, we examine whether the extent of outmigration among treatment-group members is higher in states with lower benefits,  $dM^T/dB < 0$ . By definition, the treatment effect on control-group members must be zero. Therefore, to the extent that  $dM^C/dB$  does not equal zero, this must be the result of a correlation between benefit levels and some other amenities within states. As in the experimental design suggested earlier, we can difference between control and treatment groups to control for these otherwise unobservable differences in amenities. The statistic  $dM^T/dB - dM^C/dB$  is analogous to the difference-in-difference statistic represented in equation (9).

One problem with this approach is that it treats the effects of benefit differentials in a linear manner. The estimated effect on outmigration rates in states with, say, a \$20 higher benefit are constrained to be twice as great as the effect in states with a \$10 higher benefit relative to a third state. In fact, it may be reasonable to expect discontinuous effects if a welfare-eligible person is only willing to undertake the costs of moving if he or she can obtain a large increase in benefits. To examine this alternative hypothesis, we create two dummy variables indicating whether or not an individual living in

a particular state could receive a \$100 or \$200 benefit increase if the individual moves. Denote these variables  $B(100)$  and  $B(200)$ . We then examine whether  $dM/dB(z) > 0$  for  $z = 100$ , or 200 to determine whether the assumed linear effect on migration in the last method is, in fact, valid. Again, estimating the same statistics for members of the control group and differencing across groups will reduce biases introduced by a correlation between benefits and other state amenities. It also controls for differences in the propensities of the groups to move that are independent of welfare differentials.

Refining this approach further, we note that people may be more willing to move to, say, a state paying \$100 more if that state is closer to their current state of residence. Therefore, we can also include an interaction between the discontinuous benefit increase variables and the distance to the closest state that pays benefits this much higher than the respondent's home state.<sup>11</sup> This allows the treatment effect to vary with the distance needed to secure the higher benefit. Yet again, differencing estimates between control and treatment groups can alleviate potential biases introduced by the correlation between benefits and amenities across states.

The analysis described so far examines the decision to move as reflected in equation (9). An extension of this analysis would examine whether or not an individual moves to a higher-benefit state as reflected in equation (10). The issues involved in implementing this approach are identical to those just discussed. To generate simple estimates of equation (10) we estimate models where the dependent variable is a dummy variable indicating whether a respondent relocated to a state that provides more generous AFDC benefits. Again, taking differences in parameter estimates between control and

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<sup>11</sup>Given the latitude and longitude of two locations  $\{(lat_1, long_2), (lat_1 - long_2)\}$  the distance is calculated using  $D=69.16ARCCOS[SIN(lat_1)SIN(long_2) + COS(lat_1)COS(long_2)COS(lat_1 - long_2)]$ . Interacting distance and benefits in this fashion assumes the cost of the move to be linear in distance, a tenuous assumption that has nevertheless been utilized in alternative applications. See, for example, Kane and Rouse (1995) and Kane and Staiger (1995). We would like to thank Doug Staiger for providing these data.

treatment groups controls for unobservable differences in states that may be correlated with welfare benefits and in constant differences in the propensity to move between groups.<sup>12</sup>

A final problem that may bias the results of these analyses is that some of the assumptions made in the experimental framework discussed above may not be valid. For example, it is assumed that amenities differing across states are valued similarly by the control and treatment groups. Second, it is assumed that there is a constant difference between the treatment and control groups in their propensity to move. Violation of either of these assumptions would lead to biased results. To partially address these issues we include covariates representing personal and state characteristics in our estimation. To the extent that differences between groups and across states remain after controlling for these factors, estimates may still be biased.

Incorporating these modifications to the difference-in-difference approach described above, we estimate several probit equations. The first set of models considers the probability of moving out of one's home state. Using a continuous measure of benefits we estimate a model of the form:

$$P(M_{ijt} = 1) = F(\alpha_0 + \alpha_1 T_{ijt} + \alpha_2 B_{ijt} + \alpha_3 B_{ijt} \times T_{ijt} + \gamma_1 X_{ijt} + \gamma_2 S_{jt}) \quad (13)$$

where  $M_{ijt}$  is a dummy variable equaling one if the individual moves and zero otherwise,  $B_{ijt}$  is the combined real AFDC and food stamps benefit level for a family of two,  $T_{ijt}$  is a dummy variable indicating whether the individual is in the treatment group,  $X_{ijt}$  is a vector of individual characteristics,  $S_{jt}$  is a vector of state characteristics,  $i, j,$  and  $t$  index individuals, states, and years, respectively, and  $F$  is the standard normal distribution function. The coefficient  $\alpha_3$  in equation (13) represents the negative of the difference-in-difference statistic in equation (9).

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<sup>12</sup>This approach may be problematic because it may obscure differences between states that offer a few dollars more than the respondent's home state and states that offer a few hundred dollars more. As an alternative, we have also estimated Tobit models where the dependent variable is the change in welfare generosity between the respondent's state of residence at time  $t$  and the state of residence at time  $t+1$ . These estimates yield similar conclusions to those of the reported probits and are not reported.

Additional models are estimated that allow for the effects of benefit differentials to be nonlinear and for the effect of benefit differentials to vary by distance. These models are specified as:

$$P(M_{ijt} = 1) = F \left( \begin{array}{l} \beta_0 + \beta_1 T_{ijt} + \beta_2 B(100)_{jt} + \beta_3 B(200)_{jt} + \\ \beta_4 B(100)_{jt} \times T_{ijt} + \beta_5 B(200)_{jt} \times T_{ijt} + \\ \gamma_1 'X_{ijt} + \gamma_2 'S_{jt} \end{array} \right) \quad (14)$$

and

$$P(M_{ijt} = 1) = F \left( \begin{array}{l} \beta_0 + \beta_1 T_{ijt} + \beta_2 B(100)_{jt} + \beta_3 B(200)_{jt} + \\ \beta_4 B(100)_{jt} \times T_{ijt} + \beta_5 B(200)_{jt} \times T_{ijt} + \\ \beta_6 DB(100)_{jt} + \beta_7 DB(200)_{jt} + \\ \beta_8 DB(100)_{jt} \times T_{ijt} + \beta_9 DB(200)_{jt} \times T_{ijt} + \\ \gamma_1 'X_{ijt} + \gamma_2 'S_{jt} \end{array} \right) \quad (15)$$

where  $B(100)_{jt}$  is a dummy variable indicating whether a two-person family could obtain between \$100 and \$199 more in benefits by moving,  $B(200)_{jt}$  is a dummy variable indicating whether a two-person family could obtain \$200 or more in benefits by moving,  $DB(100)_{jt}$  is the distance a person would have to move to obtain between \$100 and \$199 more in benefits by moving interacted with  $B(100)$ ,  $DB(200)_{jt}$  is the distance a person would have to move to obtain \$200 or more in additional benefits by moving interacted with  $B(200)$ , and all other notation is defined above.<sup>13</sup> In these models, nonlinearities in the treatment effect are estimated through the parameters  $\beta_4$  or  $\beta_5$  and differences in treatment effects by distance are estimated through the parameters  $\beta_8$  and  $\beta_9$ .

Models of locational choice are then estimated. These models differ from those just presented in that the dependent variable is equal to unity if the individual moves to a higher-benefit state. In all other dimensions, these models are identical to those just described.

## DATA

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<sup>13</sup>The omitted category is states in which increases of \$100 or more are not possible. While these states vary across years, in 1990 they included CA, CT, MA, MI, MN, NY, and WI. The lowest-benefit states (those for which a \$200+ increase was possible) included AL, AZ, AR, IN, KY, LA, MS, SC, TN, TX, VA, WV.

We use the National Longitudinal Survey of Youth (NLSY) to estimate the models just presented. This survey began collecting data on respondents between the ages of 14 and 21 in 1979 and has been repeated annually since then. The panel employed in this research spans 1979–1992. Blacks, Hispanics, and poor whites are oversampled at twice their rate in the population, but sampling weights are provided to create parameter estimates representative of the population as a whole.<sup>14</sup> We merge data for the maximum two-person welfare benefit (including food stamps and AFDC) for each state/year onto the NLSY sample.<sup>15</sup> All dollar values are deflated using a state-level price deflator.

The NLSY data provide significant advantages for the purposes of this project. First, supplemental geocode files are available that allow us to determine whether or not an individual moved across state lines from one year to the next. Second, the higher propensity of youth to move provides us with a substantial number of cross-state moves over the 14-year period. Third, these data provide a wealth of information about each respondent, including demographic characteristics, family income, AFDC status, and characteristics of an individual's county of residence, which will be quite useful in this analysis.

The format of these data also provide another statistical advantage over using cross-sectional, nationally representative data. One problem in examining the welfare-magnet hypothesis is that states that provide high benefits in one period are quite likely to provide high benefits in subsequent periods. Therefore, if we look at a cross-section of the population, it may consist of welfare migrants who have already moved to the location that will provide them with high benefits. This will lead to a downward bias in estimates of the welfare-magnet effect. The youthfulness of the NLSY respondents lessens this

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<sup>14</sup>Estimates with and without weights yield very similar results. The reported estimates are unweighted.

<sup>15</sup>The results are not sensitive to whether we use AFDC benefits or the combined AFDC and food stamp benefit package.

problem, since most respondents were still living in their parent's household in 1979 and had not yet made their own migration decisions.

These data have significant advantages over other data sets that could be used for studying the issue of welfare-induced migration. Decennial census data, the main alternative source, contain many more observations per state, but have some serious limitations. First, they measure migration at just two points over a five-year window—1975 and 1980, because the 1980 census asks place of residence five years earlier. Movements within this period are not observed. Second, classifying individuals as “treatment-group” members is difficult because the data only provide an income measure *within* the five-year window (i.e., 1979). This income could reflect either pre- or post-migration income and could, therefore be endogenous.

## SAMPLE CHARACTERISTICS

Descriptive statistics for the NLSY sample are presented in Tables 1 and 2. Table 1 reports sample sizes for the treatment and control groups. The element of observation is a person year. Of the 18,782 person years observed for the combined control and treatment sample over the 1979–1992 period in the NLSY, 1,052, or about 5.6 percent, move to a different state. In addition, 519, or 2.7 percent, move to a higher-benefit state. Of the 7,611 observations in the treatment sample, 3.8 percent move and 1.8 percent move to a higher benefit state. Only 0.8 percent of the treatment population move to a higher-benefit state and take up welfare immediately, and 0.9 percent do so within two years. For the control group, of the 11,171 observations, 763, or 6.8 percent, move to a different state. Roughly half, or 3.4 percent, move to a higher-benefit state.

This table illustrates the important tradeoff in this analysis between the power of the hypothesis being tested and the power of the statistical procedures employed. The strongest test of the welfare-magnet hypothesis would involve observing that people move to higher-benefit states and

**TABLE 1**  
**Treatment and Control Sample Sizes**

	Number of Person Years	% (n) Moving	% (n) Moving to a Higher Benefit State	% (n) Moving and Receiving AFDC within One Year of Moving	% (n) Moving and Receiving AFDC within Two Years of Moving	% (n) Moving to a Higher-Benefit State and Receiving AFDC within One Year of Moving	% (n) Moving to a Higher-Benefit State and Receiving AFDC within One Year of Moving
<i><b>Treatment Groups</b></i>							
Single women on AFDC in t or t-1 (Treatment1)	5,690	3.37 (192)	1.5 (86)	2.2 (124)	2.4 (137)	.9 (53)	1.0 (58)
Poor single women with children, not on AFDC in t or t-1 (Treatment2)	1,921	5.05 (97)	2.6 (50)	.8 (16)	1.2 (23)	.4 (9)	.6 (12)
Total treatment members	7,611	3.8 (289)	1.8 (136)	1.8 (140)	2.1 (160)	.8 (62)	.9 (70)
<i><b>Control Groups</b></i>							
Poor single women without children (Control1)	3,297	7.6 (252)	3.6 (120)				
Poor single men (Control2)	5,458	6.9 (381)	3.6 (199)				
Poor married men (Control3)	1,072	6.2 (66)	3.2 (34)				
Poor married women (Control4)	1,344	4.8 (64)	2.2 (30)				
Total control members	11,171	6.8 (763)	3.4 (383)				
Total observations	18,782	5.6 (1,052)	2.7 (519)				



begin receiving welfare after the move. Statistically, however, estimating these joint hypotheses may provide a weak test, because of the imprecision arising from the small cell sizes. Alternatively, we could test whether out-of-state migration among treatment-group members is higher than among control-group members. This test, while testing weaker implications of the theory, is statistically more powerful because of the larger cell sizes available. Our approach in this paper is to examine a range of hypotheses, recognizing the tradeoff between the strength of the theoretical implication and the statistical power of the test.

Table 2 presents sample means for the NLSY data. The treatment and control groups differ somewhat, treatments being more likely to be black and to live in higher-benefit states. Importantly, estimates indicate that members of the different control groups are somewhat more likely to move than members of the treatment group. Roughly 5–7.5 percent of control-group members are observed to move across state lines from one year and the next as compared to just under 4 percent of treatment-group members. These statistics underscore the value of our difference-in-difference approach.

## EMPIRICAL RESULTS

Table 3 presents the probit estimates for equations (13)–(15) in specifications that exclude individual and state characteristics. Models were estimated separately for each of the four control groups defined above and were then estimated for one pooled control group. The first row of the table, labeled “Continuous Treatment Effect,” represents estimates of  $\alpha_3$  in equation (13). These coefficients would be expected to be negative, because higher benefits reduce the outmigration of treatments relative to controls. Here we can see that a \$100 increase in benefits actually increases the probability that treatments will move by .252 percentage points relative to control group 1. The effect is smaller, but still positive, when compared with control group 2. Estimates are negative for control

**TABLE 2**  
**Descriptive Statistics**

Variable	Treatment	Control 1	Control 2	Control 3	Control 4
Sex	Female	Female	Male	Male	Female
Poverty status	Poor	Poor	Poor	Poor	Poor
Have children? (fraction if yes)	Yes	No	.072 (.259)	.817 (.387)	.855 (.351)
Married	No	No	No	Yes	Yes
% received AFDC in t or t-1	75	0	0	0	0
Aage (1979)	17.8 (2.19)	17.4 (2.14)	17.39 (2.12)	18.15 (2.29)	18.11 (2.21)
Highest grade attained	10.98 (1.93)	12.38 (2.21)	11.35 (2.32)	10.61 (2.59)	10.43 (2.56)
Black	.528 (.499)	.347 (.476)	.455 (.498)	.211 (.408)	.217 (.412)
Maximum benefit (avg.)	364 (85)	363 (79)	362 (77)	339 (70)	341 (80)
Moved across county within state	.115 (.320)	.210 (.407)	.218 (.413)	.186 (.389)	.149 (.357)
Moved to different state	.034 (.181)	.076 (.265)	.069 (.254)	.062 (.240)	.048 (.213)
Live in B(100) state?	.400 (.490)	.377 (.484)	.405 (.491)	.446 (.497)	.358 (.479)
Live in B(200) state?	.280 (.449)	.322 (.467)	.306 (.461)	.371 (.483)	.443 (.497)
Distance to B(100) state?	187 (346)	179 (339)	191 (343)	207 (340)	184 (350)
Distance to B(200) state?	242 (436)	271 (448)	272 (470)	368 (568)	437 (587)
Crimes per 100,000 pop	5,789 (1,204)	5,880 (1,363)	5,817 (1,299)	5,673 (1,497)	5,856 (1,490)
State per capita income	13,952 (3,343)	12,532 (2,932)	13,079 (3,114)	13,468 (2,898)	12,837 (2,809)
Number of person years	5,690	3,297	5,458	1,072	1,344

**TABLE 3**  
**Probit: Dependent Variable Is Probability of Moving to a Different State**

	Control 1	Control 2	Control 3	Control 4	Pooled Controls
<b>I. Continuous Treatment Effect*</b>	.252 (.548)	.056 (.509)	-.545 (.836)	-.512 (.757)	-.094 (.473)
<b>II. Discrete Treatment Effect</b>					
<i>A. Basic Model</i>					
Potential increase in benefits \$100–\$200**	-1.34 (1.00)	-.941 (.917)	-.673 (1.63)	.369 (1.50)	-.943 (.851)
Potential increase in benefits \$200+**	-.856 (1.07)	-.295 (1.01)	.027 (1.74)	2.33 (1.70)	-.057 (.954)
<i>B. Distance Model</i>					
Potential increase in benefits \$100–\$200**	-1.18 (1.24)	-.011 (1.16)	.929 (2.00)	-.483 (1.82)	-.296 (1.07)
Potential increase in benefits \$200+**	-3.11 (1.68)	-1.18 (1.72)	-.520 (2.61)	-1.73 (2.27)	-2.11 (1.50)
Potential increase in benefits \$100–\$200*Distance*	-.029 (.152)	-.187 (.133)	-.303 (.191)	.173 (.222)	-.130 (.126)
Potential increase in benefits \$200+*Distance*	.328 (.196)	.110 (.169)	.078 (.205)	.437 (.225)	.266 (.160)

\* Derivative multiplied by 10,000.

\*\* Derivative multiplied by 100.

Standard errors in parentheses.

**Note:** Treatment group = poor single women on AFDC in past two years or not. Control group 1 = poor single women without children. Control group 2 = poor single men. Control group 3 = poor married men. Control group 4 = poor married women.

groups 3 and 4 and for the pooled control group. Importantly, none of the estimates are statistically different from zero.

The following block of the table, labeled “Discrete Treatment Effect” provides parameter estimates for equations (14) and (15). The “Basic Model” (equation 14) reports estimates of  $\beta_4$  and  $\beta_5$  from equation 14 and the “Distance Model” (equation 15) reports estimates of these parameters plus the distance interaction terms,  $\beta_8$  and  $\beta_9$ . In these models, the welfare-magnet hypothesis would be supported if the potential for a large benefit increase led to a greater probability of moving. That probability should decrease if the distance necessary to obtain the higher benefits is greater. In both models, point estimates often have signs opposite from those hypothesized and the estimated parameters are uniformly insignificantly different from zero.

Table 4 repeats the estimation of equations (13)–(15), but now includes controls for individual’s age, education, the state per capita income, the state crime rate, and the local unemployment rate. Again, there is no strong evidence that the treatments are more likely to move from low- versus high-benefit states than the controls. The estimated coefficients are all statistically insignificant. Distance also has no significant effect on the magnitude of the treatment effect here.

Tables 5 and 6 are analogous to the two previous tables except that dependent variable is now a dummy variable indicating whether a person moved to a higher-benefit state. The results are comparable to those for the probability of moving. Contrary to the welfare-magnet hypothesis, treatments are typically less likely to move to a higher-benefit state, though parameter estimates are imprecise. Again, the effect does not vary with distance.

Taken as a whole, these results provide little evidence that those women most likely to be candidates for AFDC are moving to take advantage of benefit differentials across states. Parameter estimates of interest are uniformly insignificant and often have the opposite sign to that hypothesized.

**TABLE 4**  
**Probit: Dependent Variable Is Probability of Moving to a Different State**  
**(Includes additional control variables)\*\*\***

	Control 1	Control 2	Control 3	Control 4	Pooled Controls
<b>I. Continuous Treatment Effect*</b>	.069 (.54)	-.065 (.501)	-.666 (.830)	-.554 (.747)	-.171 (.463)
<b>II. Discrete Treatment Effect</b>					
<i>A. Basic Model</i>					
Potential increase in benefits \$100–\$200**	-1.23 (1.00)	-.657 (.918)	-.952 (1.62)	.557 (1.49)	-.736 (.847)
Potential increase in benefits \$200+**	-.596 (1.07)	-.097 (1.00)	-.315 (1.72)	2.31 (1.68)	.035 (.942)
<i>B. Distance Model</i>					
Potential increase in benefits \$100–\$200**	-1.23 (1.22)	.347 (1.16)	.714 (1.98)	-.204 (1.82)	-.127 (1.06)
Potential increase in benefits \$200+**	-2.43 (1.70)	-.838 (1.69)	-.698 (2.54)	-1.61 (2.25)	-1.60 (1.51)
Potential increase in benefits \$100–\$200*Distance*	.014 (.147)	-.168 (.127)	-.297 (.188)	.154 (.219)	-.098 (.121)
Potential increase in benefits \$200+*Distance*	.247 (.190)	.091 (.162)	.067 (.198)	.421 (.221)	.208 (.154)

\* Derivative multiplied by 10,000.

\*\* Derivative multiplied by 100.

\*\*\*Control variables include age, education, state per capita income, state crime rate, and local unemployment rate.

Standard errors in parentheses.

**Note:** Treatment group = poor single women on AFDC in past two years or not. Control group 1 = poor single women without children. Control group 2 = poor single men. Control group 3 = poor married men. Control group 4 = poor married women.

**TABLE 5**  
**Probit: Dependent Variable Is Probability of Moving to a Higher-Benefit State**

	Control 1	Control 2	Control 3	Control 4	Pooled Controls
<b>I. Continuous Treatment Effect*</b>	.493 (.390)	.143 (.371)	-.325 (.573)	.082 (.587)	.156 (.352)
<b>II. Discrete Treatment Effect</b>					
<i>A. Basic Model</i>					
Potential increase in benefits \$100–\$200**	-1.29 (.782)	-.821 (.722)	-.611 (1.44)	-.942 (1.33)	-1.01 (.660)
Potential increase in benefits \$200+**	-.956 (.767)	-.265 (.766)	-.461 (1.41)	.014 (1.43)	-.457 (.708)
<i>B. Distance Model</i>					
Potential increase in benefits \$100–\$200**	-.714 (.926)	-.517 (.842)	.695 (1.73)	-.609 (1.48)	-.487 (.804)
Potential increase in benefits \$200+**	-1.74 (.886)	-.621 (1.01)	-.076 (1.78)	-1.42 (1.37)	-1.15 (.858)
Potential increase in benefits \$100–\$200*Distance*	-.140 (.088)	-.075 (.087)	-.239 (.106)	-.072 (.117)	-.131 (.084)
Potential increase in benefits \$200+*Distance*	.150 (.097)	.046 (.087)	-.030 (.098)	.202 (.115)	.105 (.082)

\* Derivative multiplied by 10,000.

\*\* Derivative multiplied by 100.

Standard errors in parentheses.

**Note:** Treatment group = poor single women on AFDC in past two years or not. Control group 1 = poor single women without children. Control group 2 = poor single men. Control group 3 = poor married men. Control group 4 = poor married women.

**TABLE 6**  
**Probit: Dependent Variable Is Probability of Moving to a Higher-Benefit State**  
**(Includes additional control variables)<sup>\*\*\*</sup>**

	Control 1	Control 2	Control 3	Control 4	Pooled Controls
<b>I. Continuous Treatment Effect*</b>	.411 (.370)	.159 (.364)	-.447 (.544)	.106 (.565)	.172 (.342)
<b>II. Discrete Treatment Effect</b>					
<i>A. Basic Model</i>					
Potential increase in benefits \$100–\$200**	-1.21 (.733)	-.804 (.708)	-.646 (1.35)	-.739 (1.25)	-1.01 (.638)
Potential increase in benefits \$200+**	-.837 (.727)	-.182 (.758)	-.434 (1.34)	.053 (1.34)	-.425 (.689)
<i>B. Distance Model</i>					
Potential increase in benefits \$100–\$200**	-.723 (.864)	-.500 (.823)	.613 (1.62)	-.241 (1.43)	-.517 (.777)
Potential increase in benefits \$200+**	-1.57 (.840)	-.538 (1.00)	-.157 (1.66)	-1.37 (1.26)	-1.04 (.846)
Potential increase in benefits \$100–\$200*Distance*	-.115 (.082)	-.066 (.084)	-.220 (.101)	-.099 (.111)	-.118 (.081)
Potential increase in benefits \$200+*Distance*	.138 (.092)	.048 (.085)	-.018 (.092)	.201 (.110)	.092 (.079)

\* Derivative multiplied by 10,000.

\*\* Derivative multiplied by 100.

\*\*\* Control Variables include age, education, state per-capita income, state crime rate, and local unemployment rate.

Standard errors in parentheses.

**Note:** Treatment group = poor single women on AFDC in past two years or not. Control group 1 = poor single women without children. Control group 2 = poor single men. Control group 3 = poor married men. Control group 4 = poor married women.

## SUBSTANTIVE CONSIDERATIONS

An alternative interpretation of these findings may be valid, however. The inability to reject the null hypothesis that treatment and controls respond differently to the geographic dispersion in welfare generosity could also reside in the imprecision of our estimates. Significance tests that find no statistical difference between the behavior of treatment and control groups indicate that the parameter estimates are small relative to the standard error. If those standard errors are large, such tests will be very weak.

To examine this argument in more detail, we conduct a simulation exercise designed to provide a conservative upper bound on the extent of welfare-induced migration and its effect on welfare caseloads in generous states. This exercise divides states into three categories: low-benefit states, in which a \$200 or more benefit increase is possible; middle-benefit states, in which a \$100–\$199 benefit increase is possible; and the remainder of high-benefit states. We then simulate the effect on the welfare caseload in the high-benefit states using generous assumptions about the extent of migration from the other two groups of states.

The basis for this simulation is the upper bound of the 95 percent (one-tailed) confidence interval of the point estimates obtained from the “Basic Model” reported in Table 3 using the pooled control group ( $\beta_4 + 1.645\sigma_\beta$  and similarly for  $\beta_5$ ). We assume (counterfactually) that all migrants from lower-benefit states move to high-benefit states. With point estimates of -.057 and -.943 and a standard error of .954 and .851 in low- and middle-benefit states, respectively, plausible upper bounds on the difference in outmigration rates between treatment and control groups in these lower-benefit states are 1.51 and .457, respectively. The product of these estimates and the population of plausibly welfare-eligible women in the low- and middle-benefit states indicates the number of welfare-induced movers



in any given year.<sup>16</sup> We then assume that the typical mover will stay on the welfare rolls in their new locations for three years.<sup>17</sup> In steady state, these movers will cumulate in the high-benefit states to a level of three times greater than the number of annual movers. A generous estimate of the fraction of the welfare caseload in high-benefit states that are out-of-state migrants may be formalized as:

$$\text{Steady State Impact} = \frac{n_{100}(\beta_4 + 1.645\sigma_{\beta_4})D_{100} + n_{200}(\beta_5 + 1.645\sigma_{\beta_5})D_{200}}{C_0}$$

where  $N_{100}$  and  $N_{200}$  are the number of treatment-group members in the middle- and low-benefit states, respectively,  $D$  represents the assumed median duration of benefit receipt, and  $C_0$  is the initial caseload in the generous states.

The results of this simulation suggests that an upper bound of the impact of welfare-induced migration on caseloads in high-benefit states is approximately 5 percent. That is, caseloads in generous states are most likely to be no more than 5 percent higher than they would be in a system with uniform benefits across states. In all likelihood, the effect is actually smaller than that.

## CONCLUSIONS

In this paper we present an empirical evaluation of the welfare-magnet hypothesis using data from the National Longitudinal Survey of Youth. We employ a conceptually straightforward, quasi-

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<sup>16</sup>The number of families on AFDC in the high-benefit states in 1991 was approximately 1.6 million. The number of poor single women with children in the middle- and low-benefit states was approximately 2.3 million and 1.2 million, respectively. These last estimates were obtained by taking the size of the welfare rolls in these groups of states and multiplying by the ratio of women in this demographic group to women on welfare in our NLSY sample.

<sup>17</sup>Three years is longer than the median duration of welfare receipt actually observed, which is only about two years (Hoynes and Macurdy, 1994). We have chosen this higher level because the selection of those recipients who have moved to collect benefits is likely to lead to longer than usual spells.

experimental approach contrasting the migration decision of a “treatment” group of individuals—those eligible for AFDC—with a “control” group—people categorically ineligible.

The results produce little evidence that those women most likely to be candidates for AFDC move in a pattern consistent with the welfare-magnet hypothesis. They are no more likely to move from low versus high benefit states than other poor individuals not eligible for AFDC. Neither are they more likely to move to a higher-benefit state. This suggests that either other factors dominate their decision to move or that the perceived costs associated with moving exceed any potential gain from increased benefits. Conservative estimates suggest that the caseloads in generous states are increased by 5 percent, at most, by welfare-motivated migration.

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