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1981 FEDERAL AFDC LEGISLATION
ON THE WORK EFFORT OF WOMEN
HEADING HOUSEHOLDS: A FRAMEWORK
FOR ANALYSIS AND THE EVIDENCE
TO DATE

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Assessing the Effects of the 1981 Federal AFDC Legislation on the Work Effort of Women Heading Households: A Framework for Analysis and the Evidence to Date

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ABSTRACT

The 1981 federal Omnibus Budget Reconciliation Act (OBRA) increased the benefit-reduction rate in the AFDC program, thereby generating possible work disincentives for welfare recipients. This paper reviews the theoretical basis for the existence of such disincentives, provides a statistical framework for their empirical measurement, reviews critically some of the studies that have been completed to date, and provides some new evidence. It is found that (1) standard economic theory implies that the effect of OBRA on labor supply is ambiguous, not unequivocally negative; (2) statistically measuring the effects of OBRA requires careful allowance for macroeconomic effects within a correctly designed study using either cross-sectional or panel data; (3) the studies that have been completed to date, although indicating little effect of OBRA on labor supply, are incomplete and hence unable to provide a definitive answer on the work effort effects of OBRA; and (4) new evidence presented here--intended to correct the problems with these past studies--does, however, also indicate no effect (if not a positive effect) of OBRA on labor supply and earnings.

Assessing the Effects of the 1981 Federal AFDC Legislation on the Work Effort of Women Heading Households: A Framework for Analysis and the Evidence to Date

In the summer of 1981 Congress passed and the President signed the Omnibus Budget Reconciliation Act (OBRA), a piece of legislation that significantly altered many transfer programs. Major changes were made in the Aid to Families with Dependent Children (AFDC) program, one of the better-known welfare programs in the United States. Among its many changes, perhaps the most important are those that eliminated or reduced the benefits of working AFDC recipients through the institution of a gross income eligibility limit, restricted the amount of available earnings deductions, and put a cap on permitted work-related deductions. Although the intent of these provisions was to direct a greater proportion of federal AFDC expenditures toward those in greatest need (i.e., those with no earned income), a possible by-product of the provisions may be a reduced incentive to work.

This paper analyzes the effects on work incentives of the 1981 Act and reports the evidence gathered to date on measuring those effects. First, the standard economic theory of work effort is used to show the expected effects of the OBRA changes in AFDC. Perhaps surprisingly, the analysis shows that the elimination or reduction of benefits for workers does not necessarily discourage work effort as a whole. Although some working recipients will presumably reduce their work effort in order to stay on the rolls or to increase their benefits, others will choose not to stay on the rolls but instead to work additional hours to obtain more earnings to make up for the loss in welfare income. The net effect of these two responses is ambiguous and cannot be predicted a priori.

Moreover, past econometric studies of the AFDC program are inadequate to provide a reliable prediction of the effects.

Since the direct measurement of OBRA effects is therefore important, the paper also provides a discussion of the statistical issues that will arise in any OBRA study. It points out that comparing pre-OBRA work-effort levels to post-OBRA work-effort levels may incorrectly measure OBRA effects, for, as in any before-and-after study, other events may have taken place which also induce changes in the level of work effort. The most widely noted event of this type is the onset of the 1981-1982 recession, which occurred just as OBRA was implemented. More generally, there can be "macro effects" that arise either for cyclical reasons or for trend reasons. Both imply that multiple periods of data are required to deduce how much of the observed change from pre-OBRA to post-OBRA is in fact a result of the legislation. Another statistical issue discussed at some length is the relationship between two methods for measuring OBRA effects, one in which a series of independent cross-sections of data is examined, and one in which a panel of individuals (e.g., a set of AFDC recipients) is followed over time. A major point of the discussion is based upon the argument that the use of independent cross-sections is sufficient to answer all questions of primary interest regarding OBRA, and in addition that the use of panel data involves a number of pitfalls which may yield incorrect OBRA estimates.

The paper then reviews a few of the studies of OBRA that are currently available. Their results indicate suprisingly little labor supply response to the program changes. However, the studies are found to be incomplete and not definitive: most use panel data and do not measure all effects, and most also do not adequately account for macro effects in the economic environment. An additional analysis using a series of independent cross-sections is then provided, showing again little effect (if not a positive effect) of OBRA on labor supply and earnings.

THE EFFECT OF THE OBRA LEGISLATION ON WORK EFFORT: THEORETICAL CONSIDERATIONS

The OBRA provisions mandated many important changes in the AFDC program. Those most important for work incentives are the following:

1. Elimination of $30-and-one-third deductions after four months. After four consecutive months on the program, deductions which allow workers to keep the first $30 and subsequent one-third of monthly earnings (instituted in the 1967 amendments) end. Since those deductions constitute the primary means by which the benefit-reduction rate (tax on earnings) in the program is kept below 100 percent, their elimination effectively raises the benefit-reduction rate to 100 percent after four months.

2. Reduction in the amount of the $30-and-one-third deductions during the first four months. During the first four months in which the deductions are available, they are deducted, when calculating the benefit amount, from net income instead of gross income, where net income equals gross income minus allowable work-related deductions. Consequently the work-related deductions are effectively reduced by a third. This makes the benefit-reduction rate higher than before in even the first four months.

3. New ceilings on work-related deductions. Work-related deductions are capped (i.e., maximums on deductions are instituted), and lower caps are provided for part-time workers. Assuming these caps are binding in some states for some recipients, the benefit-reduction rate is again effectively increased.

4. New eligibility income limit. Families with income above 150 percent of the state's standard of need are made ineligible for benefits. This provision creates a notch in the benefit schedule at some upper income level, effectively making the benefit-reduction rate greater than 100 percent at that point.

5. New assets eligibility limit. Families with assets greater than newly specified limits are ineligible for benefits. This provides families with an incentive to draw down assets, possibly by reducing earnings and temporarily financing consumption out of existing assets.

All five of these provisions appear to have the same direction of effect--to reduce work incentives. However, the provisions reduce work incentives only for those recipients who respond by remaining on the welfare rolls--those who do not stay on the rolls may increase work effort to make up for the loss in income.

These and other effects of the OBRA provisions on work effort can be seen by an analysis using the standard economic theory of labor supply. The well-known labor-leisure diagram of work effort is shown in Figure 1. The budget constraint AB represents the set of hours-of-work and income combinations available to a family not on welfare; such a family will locate at a utility-maximizing point where the desired combination of work and income is attained. Families on AFDC prior to OBRA are on the pre-OBRA segment shown in the figure. By definition, their income and hours of work must be below those obtaining at the pre-OBRA break-even point (the point at which they are no longer eligible to receive benefits) shown in the figure. Since the benefit-reduction rate was less than 100 percent prior to OBRA, the pre-OBRA segment has a positive slope--increases in hours of work increase take-home income, albeit at a lower rate than for nonrecipients.

The post-OBRA segment is also shown in the figure. This segment is a simplified representation showing only the elimination of the \$30-and-one-third deduction, which increases the benefit-reduction rate to 100 percent. Not shown (for simplicity) are the initial \$30 provision, the effect of deductions changes, or the constraint in the first four months--showing these changes would complicate the graph but would add little, for their effects are in the same direction as those about to

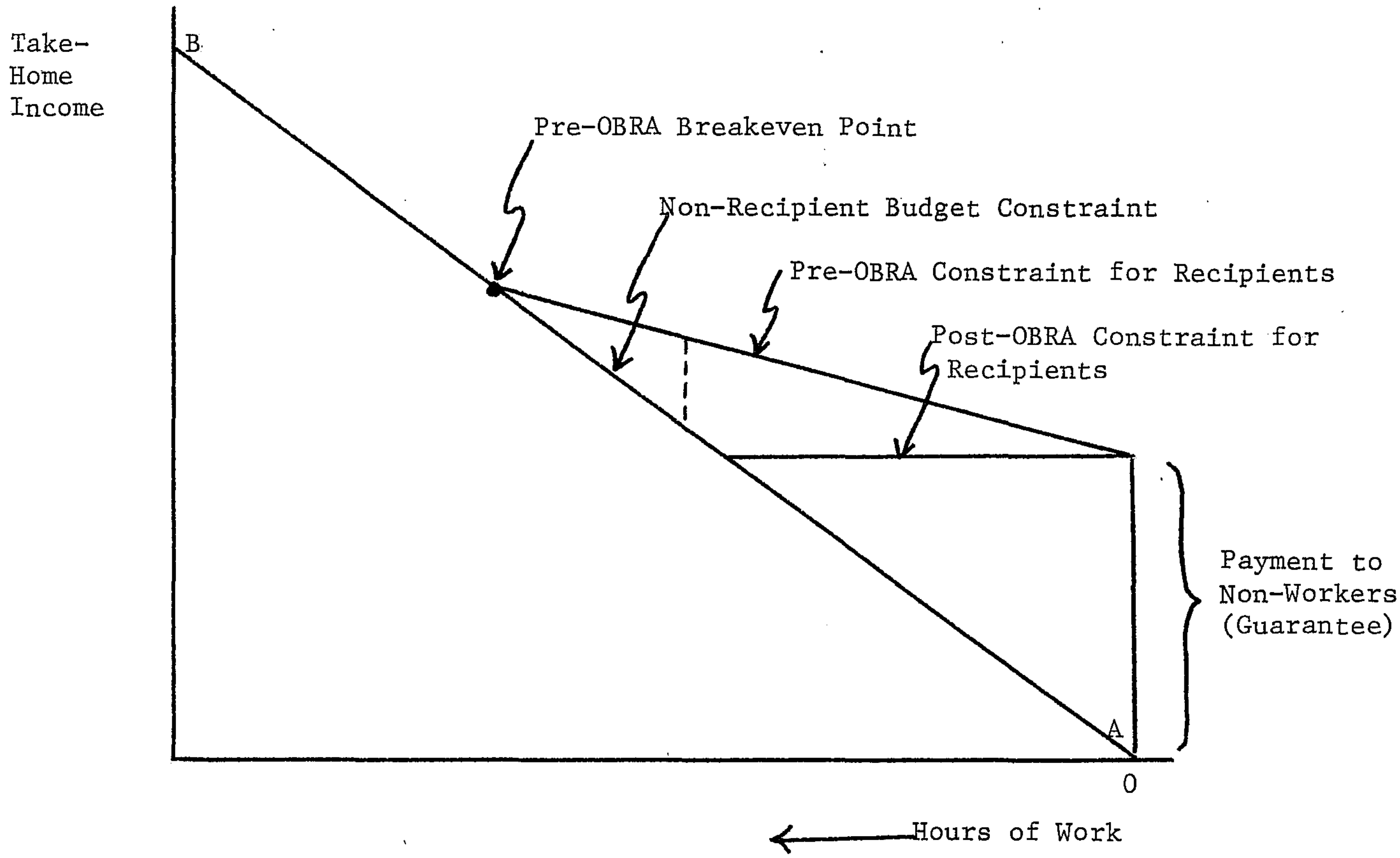

FIGURE 1

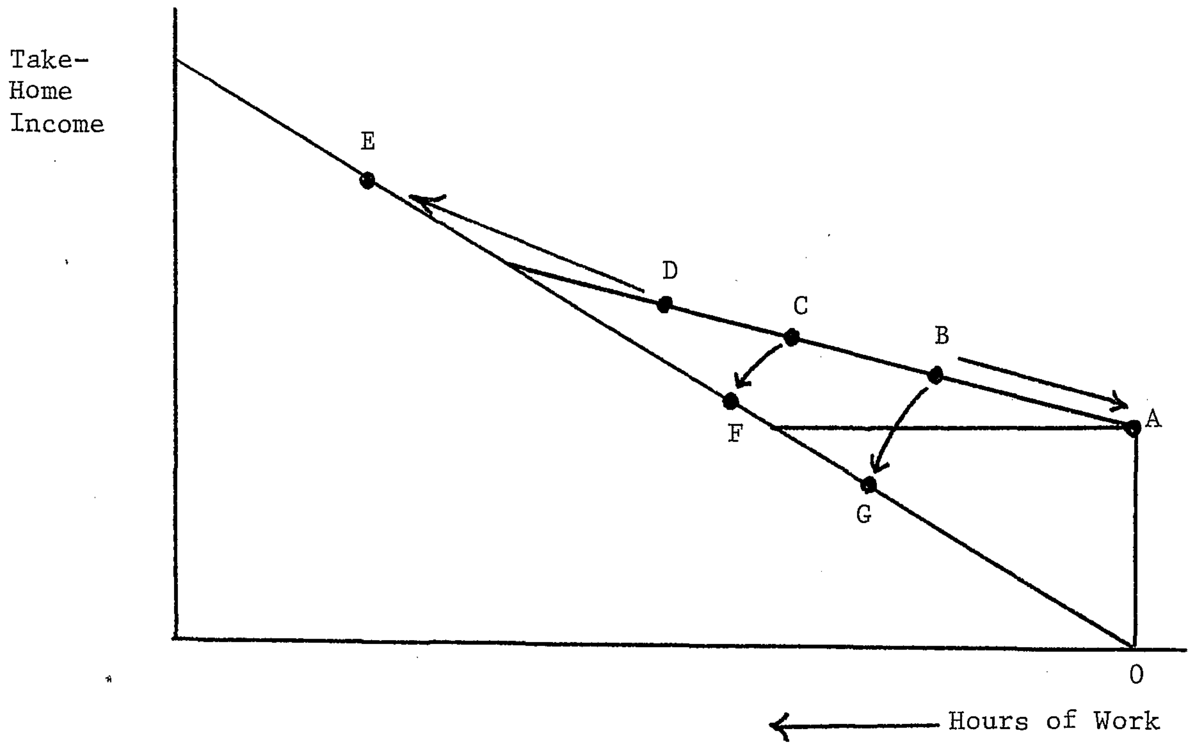

FIGURE 2

be discussed. The new upper income limit is shown by a dotted line in the figure, representing a notch in the schedule. This limit is binding only in the first four months, for as soon as the benefit-reduction rate increases to 100 percent, it is no longer relevant. The upper income limit could occur above the pre-OBRA break-even point, in which case it is not relevant in the first four months as well.

Figure 2 shows the possible responses to the OBRA budget-constraint alteration. The figure shows hypothetical individuals initially located at different points, with arrows indicating the directions of their responses. The responses are based upon the analysis of indifference curves, but to avoid clutter in the diagram these are not drawn in. Individual A, who is not working prior to OBRA, is unaffected by the program. Since over 80 percent of the caseload is at this point, the overall response to OBRA will be dominated by individuals of this type. Individual B, who has some earnings prior to OBRA, responds in one of two ways. First, if she chooses to stay on the program, she moves to point A and reduces earnings to zero. This is the form of response to the benefit-reduction-rate increase most frequently discussed. Second, however, she may choose to move to a point such as G, where she is not receiving benefits at all but is working longer hours. Although take-home income could be increased by moving to point A, the individual will not do so if the stigma of AFDC or the costs of dealing with the program outweigh the attraction of the benefit to be received. Whereas prior to OBRA the individual's benefit and take-home income on the program were sufficiently high to outweigh these factors, after OBRA the reduction in the benefit and in take-home income makes participation no longer sufficiently desirable.

Individual C, who has higher earnings than B prior to OBRA, and who presumably has better job opportunities or a greater commitment to work than B, responds to OBRA by dropping off the rolls and by increasing earnings but not take-home income (moving to point F). For this person, reducing earnings to A would entail too large a reduction in take-home income; individual C thus increases earnings and makes up for the loss of benefits partly, but not totally.[1] The difference between individuals at C and those at B who move to G is that the former group will, post-OBRA, show up as ineligibles, whereas the latter group will show up as eligibles.

Individual D responds to OBRA by increasing work effort by a significant, nonmarginal amount, with the consequence that take-home income actually increases. For example, whereas individual C might work a few hours more to compensate in part for the loss of benefits, individual D might move from part-time work to a steady full-time job, a qualitatively different type of response.[2]

Finally, it should be noted that many individuals are initially located at points E, F, G, and other points along the nonrecipient constraint. Individuals initially at E--that is, with income above the pre-OBRA break-even point--constitute the bulk of the U.S. population but a minority of the U.S. population of women heading households (approximately 75 percent of female household heads are eligible for AFDC). Individuals at E will not be affected by OBRA. Individuals at F and G are eligible for the program but do not participate, either because of stigma, prohibitive costs of receiving benefits, or some other reason. OBRA does not affect them.

The implications of this analysis can be summed up as follows:

1. The effect of OBRA on labor supply is ambiguous in sign. Since some individuals increase work effort and some reduce work effort, the net effect will depend upon the relative numbers of individuals in the two groups and the sizes of their responses.[3]

2. The effect of OBRA on take-home income is ambiguous in sign. Again, the net effect will depend upon the relative numbers and magnitudes of response of individuals of different types.

3. The effect of OBRA on the participation rate (i.e., the caseload) in AFDC is unambiguously negative. Some individuals will stay on the rolls, but some will move off. No individuals will move onto the rolls. Therefore the caseload and participation rate must drop.

4. The effect of OBRA on the participation rate of eligibles is ambiguous in sign. Both the eligible population and the caseload decline, resulting in an ambiguous change in their ratio. However, at any given level of earnings, participation rates will be lower.

5. The effect of OBRA on program costs is ambiguous. Although benefits are no longer paid to nonrecipients, greater benefits may be paid to those who reduce their earnings in response to the program.

As can be seen in points 1-5, most of the effects of OBRA are ambiguous. Note, however, that the changes in the labor supply, income, and benefits of the recipient population will all change unambiguously. After OBRA is instituted, the recipient population will have fewer hours of work, lower employment rates, lower take-home income, and higher average AFDC benefits, all because of the reduction in the break-even point. These effects occur more or less mechanically from the nature of the change in the benefit formula.[4]

Of the five points above, four are ambiguous and can be answered only by direct empirical observation. Unfortunately, our present state of knowledge of the AFDC program is inadequate to make reliable predictions from past studies and past estimates of these effects. There have been a large number of studies of labor supply in general, and there have been a large number of studies of a negative income tax as well, but neither literature bears directly upon the AFDC program as it exists today (which is very different from a negative income tax as tested in the experiments).

There have been, however, a few studies of AFDC. Hausman (1981) estimated the effects of AFDC on work effort, but did not simulate the type of change induced by OBRA (i.e., the introduction of a 100 percent tax rate). Moreover, Hausman's estimates were made on the 1975 AFDC program and in the 1975 economic environment. A similar study by Moffitt (1983) estimated the effect of AFDC on work effort allowing for stigma-related effects, but again used 1975 data and did not include 100 percent tax rates (although it was found that a marginal change in the tax rate from its 1975 level would have, on net, no effect on labor supply). Levy (1979) used a simpler model than that of Hausman or Moffitt, but estimated more directly the imposition of a 100 percent tax rate, finding that on net work effort increases. However, Levy's results were based upon the 1967 AFDC program, which was quite different from that today, as was the 1967 economic environment. Finally, Moffitt (1984) performed a set of simulations of the effect of 100 percent tax rates for female household heads, using labor supply elasticities drawn from the existing economic literature, and found that over most tax-rate and guarantee ranges there was very little effect on labor supply resulting from a change in the tax rate.

In any case, these studies are too few and too indirectly related to OBRA to be reliable guides to what we should expect to be its effects.

Consequently, those results will have to be obtained from direct empirical examination. The next section of the paper outlines a framework for direct statistical measurement of OBRA effects.

A STATISTICAL FRAMEWORK FOR THE MEASUREMENT OF OBRA EFFECTS

The statistical measurement of OBRA effects is a difficult task. The primary reason for the difficulty is that, as with all historical events, OBRA occurred nonexperimentally. The nonexperimental nature of the environment can be quite important if there are movements over time in the variables of interest--hours of work, income, participation rates, benefit levels--that are independent of the event (OBRA) and which would have occurred in any case. Such temporal movements are here termed "macro" effects, and can be roughly classified into trend movements and cyclical movements. In the case of OBRA, the latter is particularly important because the passage of OBRA coincided with the onset of a major U.S. recession. However, there may very well have been long-run trends in the above-noted variables of interest which would have continued even in the absence of the recession.

The difference between the actual OBRA experience and that which would have occurred had a controlled experiment been undertaken is worth discussing a bit more, for it provides a perspective for nonexperimental analysis. It furnishes that perspective because most nonexperiments can be viewed as failed experiments. If OBRA effects had been measured in a controlled experiment, one would have selected a random sample of the U.S. population or some well-defined subpopulation (female household heads, low-income individuals, etc.), randomized the sample into an experimental and a control group, and administered OBRA to the experimental group. The effect of OBRA on any variable "Y"--mean hours of work, participation rates, and so on--would be measurable as the difference in values between experimentals and controls at some point in time after the treatment had been administered to the experimentals. A comparison of hours of work, participation rates (of the total population or of eligibles), benefit levels, income, and costs between experimentals and controls would be sufficient. In short, all the effects in points 1-5 above could be measured. Note that these measurements could be made even if no data had been collected on the sample prior to the experiment.

If the randomization had been classically performed, the levels of Y of the two groups would be equal before the experiment. Hence the experimental-control differences in any variable Y after the experiment would be equivalent to the experimental-control difference in the growth rate of Y from before to after the experiment. The presence of the control group "controls" for any macro effects that might have occurred, but these do not have to be known explicitly for the correct measurement to be made by a single cross-section examination at a single point in time during or after the experiment. However, the initial values of Y are necessary if one wishes to know the effect of OBRA on any subpopulation defined as of the pre-OBRA situation. For example, if the sample population had been all AFDC recipients, then to determine the effect of OBRA on the subpopulation of pre-OBRA <u>working</u> recipients would obviously require knowledge of labor supply prior to the experiment. Or if the sampled population were the entire U.S. population of female household heads, then to determine the effect of OBRA on the subpopulation of those who were AFDC recipients prior to OBRA would obviously require knowledge of initial welfare recipiency.[5] To repeat, such initial data are not required to determine the effect of OBRA on the total population sampled in the experiment, whatever it may be; the later single cross-section is sufficient for that. In fact, it will be argued below that the effects of OBRA on the subpopulations just noted are not necessarily of great interest in any case, for such effects are only subcomponents of the total effect of OBRA, and not necessarily the most important ones.[6]

In a nonexperimental population the situation is obviously much different, for no control group is present. In this case the growth rate of any variable Y from the pre-OBRA to the post-OBRA period coincides with the true experimental-control effect only if there are no macro effects. The control group is not available to measure the counterfactual (i.e., what would have happened in the absence of OBRA). In this case it is essential not only to have data at an initial pre-OBRA point but at several previous points. For if macro effects are suspected to be present, as they are in the case of OBRA, they must be estimated from prior historical data. The actual changes from pre-OBRA to post-OBRA must then be adjusted appropriately.[7]

If there are no macro effects present and therefore data from only two points in time are required for the measurement of OBRA effects, a further distinction can be drawn according to whether the data at the two points represent independent cross-sections of the population or panel data. In the former case, for example, one may have a Census-based population sample pre-OBRA and post-OBRA but the individuals in the two samples are not (necessarily) the same. In the latter case, one has a sample of individuals whom one observes at both points in time. It is important to determine whether the panel sample is drawn from a population defined at only one of the points in time (e.g., recipients pre-OBRA) or whether it is drawn jointly from the combined population at both points in time. If drawn from the combined population, then the data at the two points in time are equally usable as two random cross-sections of the total population; therefore any analysis that can be performed for each cross-section can be performed with the panel data set as well. But if the panel is drawn from a population defined at only one point in time, then it may not be representative of the population at the other point in time. In this case the independent cross-sections could be used to perform analyses which could not be performed with the panel data set. One of the questions to be addressed is whether the set of two independent cross-sections is sufficient to answer all the OBRA questions of major interest. A panel data set defined appropriately could answer additional questions, but it may not be necessary.

This distinction between independent cross-sections and panel data is sufficiently important to be used as the basis for distinguishing different types of analyses. I shall therefore consider the measurement of OBRA effects separately for the two data sets.

Independent Cross-Sections

For simplicity, assume that there are no macro effects present and therefore that only two cross-sections are needed. If there are macro effects, more than two cross-sections are needed to estimate trend and cycle effects with which to adjust the simple pre-post comparison. This problem applies equally to panel data sets; in this respect such sets and independent cross-sections do not differ. The macro effects are ignored in the following discussion not because they are unimportant--indeed, the bias they cause may swamp the other types that will be discussed--but because there is little more to say about them than that they generate the need for multiple periods of data. I shall also assume that the two independent cross-sections are cross-sections of the entire U.S. population of female household heads with children, both recipients and nonrecipients, workers and nonworkers.

Several different types of OBRA effects can be estimated with two independent cross-sections, one before and one after OBRA (most are so estimated in the last section of this paper). These are shown in Table 1. First, several participation effects can be identified: the effect on the participation rate of the total population (i.e., the caseload), on the participation rate of eligibles, and on the participation rate of those who would have been eligible if OBRA had not been enacted. The three are not independent of one other and are definitionally related through the OBRA-induced change in the size of the eligible population.

Several labor supply effects can also be estimated. First, changes in the labor supply of recipient groups can be calculated: the change in hours, earnings, and employment rates of recipients as a whole, and the change in hours and earnings of those who are workers. These effects are not of great interest because, as noted above, they will reflect the more-or-less mechanical adjustment in the characteristics of the population induced by the reduction in the AFDC break-even point. The effect of OBRA on the labor supply of the total population can also be estimated. Although this may be small, it should be recalled that 75 percent

Table 1

OBRA Effects Measurable with Independent Cross-Sections

Prose Description	Statistical Description in Each Cross-Section of Variable Measured
Participation Rates and Caseloads	
1. Effect on participation rate of population (i.e., caseload)	$\text{Prob}(P = 1)$
2. Effect on participation rate of eligibles	$\text{Prob}(P = 1 \mid H < H_{BE})$
3. Effect on participation rate of those who would be eligible pre-OBRA	$\text{Prob}(P = 1 \mid H < H_{BE}^{PRE})$
Labor Supply	
4. Effect on labor supply (earnings, hours) of recipients	$E(H \mid P = 1)$
5. Effect on employment rate of recipients	$\text{Prob}(H > 0 \mid P = 1)$
6. Effect on labor supply (earnings, hours) of working recipients	$E(H \mid P = 1, H > 0)$[a]
7. Effect on labor supply of total population	$E(H)$[b]
8. Effect on labor supply of defined subpopulations:	
Those with earnings or hours below pre-OBRA levels	$E(H \mid H < H_{BE}^{PRE})$
Those with income below the poverty line	$E(H \mid Y < Y_{POV})$
Those with income below arbitrary income level	$E(H \mid Y < Y^*)$

(table continues)

Table 1 (cont.)

<u>Note:</u> Definitions of variables are as follows:

$P = 1$ if on AFDC
$\quad = 0$ if not.

H = earnings or hours.

H_{BE} = break-even level of H.

H_{BE}^{PRE} = level of H_{BE} pre-OBRA.

Y = income.

Y^{POV} = poverty-level income.

Y^* = arbitrary income level.

[a] $E(H \mid P = 1) = \text{prob}\,(H > 0 \mid P = 1)\, E(H \mid P = 1, H > 0)$ (i.e., = expected H for AFDC participants).

[b] $E(H) = \text{Prob}(P = 1)\, E(H \mid P = 1) + \text{Prob}(P = 0)\, E(H \mid P = 0)$ (i.e., = expected H for entire population of female household heads).

of the population of women who are household heads are eligible for AFDC and that about 35 percent of the total population of female household heads (eligibles plus ineligibles) participates in AFDC. Thus the effect may not be small. Moreover, note that if no macro effects are present, the change in the labor supply of the total population will provide an unbiased measure of the sign of the effect of OBRA on labor supply--that is, whether labor supply has increased or decreased. Nevertheless, since some part of the population is of sufficiently high income to be unaffected by OBRA and by the AFDC program in general, it may be desirable to estimate the change in the labor supply of various subpopulations: those with incomes below the poverty line or below some "AFDC-relevant" income line, those with hours of work or earnings below the pre-OBRA break-even point, or some other subpopulation (see table).

The issue raised by these considerations is how to properly define the population of individuals who might conceivably be affected by OBRA. Examining only those individuals with earnings or hours below the pre-OBRA break-even point, for example, would be too restrictive, for as Figure 2 indicates, some of the OBRA response will be manifested by movements above the eligibility point. Therefore some higher cutoff point should be chosen; perhaps the total population of female household heads is, after all, the most desirable one. In any case, of course, there would be no barrier to calculating hours effects in gradually larger sections of the income distribution and, in so doing, to empirically determine the point at which OBRA effects disappear.[8]

If the two independent cross-sections are not cross-sections of the total population, OBRA effects may not be calculable, depending upon the nature of the subpopulation. If, say, the two cross-sections are two poverty populations, clearly only the participation and labor supply effects within the poverty population can be measured. If the two cross-sections are of the AFDC recipient population, with no data on future or past values of participation and labor supply, only the recipient labor supply effects in 4-6 on the table can be calculated (as well as the caseload effect, of course). This limits the range of the study to such an extent that little can be said about overall OBRA effects.

Panel Data

The obvious disadvantage of the independent cross-sections is that individual transitions cannot be identified, as they can be with panel data. The availability of panel data may, therefore, appear to aid the estimation of OBRA effects greatly. This view is in error, however, for the availability of panel data per se can at best only improve statistical efficiency. At worst, if the data are not drawn from the combined population at both points in time, their use can result in bias.[9]

The notion that panel data and the study of transitions are necessary to estimate the effect of OBRA (or of any event) is based upon the simple economic model discussed above and illustrated in Figures 1 and 2. In that model the effects of OBRA are illustrated by conceiving of a set of pre-OBRA recipients who respond to OBRA and a set of pre-OBRA non-recipients who do not change their participation or labor supply status. In such a world (again ignoring macro effects) the estimation of OBRA effects would only involve following the pre-OBRA recipients to their post-OBRA situations. Unfortunately, things are more complicated than this because large numbers of individuals in the population make participation and labor supply transitions in other ways as well, and these should be affected by OBRA. In Figure 2, one will observe in actuality that individuals will in time move from every point A to G to every other point A to G for random reasons arising outside the model. Students of labor and welfare turnover are quite familiar with this phenomenon, and know that an entire transition matrix is required to estimate the movement in (say) average labor supply between two points in time.

To be concrete, assume that individuals above point E are "full-time" workers and that workers below point E are "part-time" workers. This characterization is only partly accurate, for the AFDC break-even level falls at a very low hours point for some individuals and at a very high hours point for others. But assuming this to be the case for illustration, the population will then distribute itself among five labor-supply-participation states: (1) full-time nonrecipients, (2) part-time recipients, (3) part-time nonrecipients, (4) nonworking recipients, and (5) nonworking nonrecipients. With the introduction of OBRA, we will observe an increase in the transition rate from category (2) to the other categories--these effects were discussed previously and are illustrated by the arrows in Figure 2. But we should also expect reductions in the transition rates from categories (1), (3), (4), and (5) into (2). Fewer full-time workers will come onto the rolls as part-time workers (previously this may have occurred because of a forced reduction in the work week, or because the individual was laid off and then found a part-time job); fewer part-time workers not on the rolls will choose to come onto the rolls (previously this may have occurred because of a reduction in some other source of support, or because of a reduction in stigma);

fewer nonworking nonrecipients will come onto the rolls as part-time workers (intuitively this category would seem to be slight); and fewer recipients who are initially not working will go out to work part-time and stay on the rolls. The OBRA legislation makes one particular state--the recipient, part-time state--less desirable, resulting in both smaller flows into that state as well as greater flows out of it--that is, both "drop-outs" and "drop-ins" will be affected.

With a panel data set drawn from the joint population--that is, a data set that is a representative sample of the entire population at both points in time, both recipients and nonrecipients--all these changes in transition rates can be measured (actually, three points in time are required to measure the changes in the transition rates themselves). To then estimate the effect of OBRA on labor supply, one must calculate the changes in labor supply associated with each type of transition, and simply aggregate them up to a total, giving the net change in labor supply resulting from OBRA. But note that this is precisely what the independent cross-sections already provide. The change in, say, mean labor supply in the total population between the two independent cross-sections represents the net effect of all the transitions made between the two points and their associated changes in labor supply. The actual estimation of those transitions from the panel data merely provides a detailed decomposition of the overall estimate provided by the cross-sections.

As just noted parenthetically, estimating the effect of OBRA on transition rates requires a three-point rather than a two-point panel. In addition, if there are macro effects present, more periods will be required (as with the cross-sections). In the context of turnover analysis, the presence of macro effects implies that the system is not in equilibrium--outflows and inflows across the states do not balance. This imbalance creates a net change in the participation rate or the labor supply of the population even in the absence of OBRA, a net change that shows up in the cross-sections in its net form only.

The number of transition rates (i.e., the size of the transition matrix) to be estimated with panel data depends upon the number of states assumed. Specifically, the number of transition rates is equal to the square of the number of states. This implies that even a simple "state space" (i.e., the number of states) will require the estimation of a significant number of transitions. For example, suppose that individuals are classified by recipiency status and by employment status. In this case there are four states: working recipients, nonworking recipients, working nonrecipients, and nonworking nonrecipients. Consequently there are sixteen transition rates to be estimated, one for a movement from each of the four states to each of the others. OBRA will affect these transition rates as well as the mean hours of work within the two working states. Thus a full analysis of the panel data requires many estimates. Any further disaggregation of the "space" (e.g., into part-time and full-time) require more estimates.

The formal relationship between the changes in the variables shown in Table 1 and the transition rates from panel data can be easily derived. They are shown in mathematical form in Appendix A.

EXISTING OBRA STUDIES

There have been few studies of OBRA to date, for the program was implemented in the fall of 1981 and spring of 1982, too recently for a great many studies to have been done. Five are reviewed here. A number of others, mainly state analyses of caseloads, are not discussed because they can provide little analytic evidence on OBRA labor-supply effects.

The five studies reviewed here are those by conducted by the Research Triangle Institute, the Institute for Research on Poverty at the University of Wisconsin, the U.S. Government Accounting Office, the New York City Human Resources Administration, and the University of Minnesota. All five studies have two major deficiencies. First, and most important, all follow an initial set of AFDC recipients through time in order to observe whether they are or are not on the rolls several months after OBRA and, if they are on, whether or not they work. As emphasized in the previous section, a sample of that nature does not represent the joint population at both points in time (before and after OBRA), but only that at a single point in time (i.e., the population of AFDC recipients at the initial point). Specifically, nonrecipients at both points in time are excluded and, more important, individuals who are recipients at the second point in time and not at the first are excluded. The full matrix of transitions resulting from OBRA cannot be obtained, for only a subset is available--the effect of OBRA on transitions from recipiency to nonrecipiency can be estimated, but not the effect of OBRA on transitions from nonrecipiency to recipiency (which should also be affected, as argued above). Hence the net effect of the transitions, which is the net effect of OBRA on labor supply, cannot be calculated. It is in this sense that the studies are all incomplete.

The second major difficulty with the studies is that none was designed to control for macroeconomic effects. In three of the studies, a comparison group of AFDC recipients from a pre-OBRA period was selected, and the rates of movement from recipiency to nonrecipiency were measured and compared to those of AFDC recipients after OBRA. The difference in exit rates is a correct measure of OBRA effects only if there are no macro effects present. However, as the results below will indicate, the expected direction of bias from this problem (that the recession should have caused more individuals to move to zero hours of work in the post-OBRA period) did not appear to occur.

The RTI Study

The study performed by the Research Triangle Institute (1983), here denoted RTI, drew two national probability samples of the AFDC caseload, one in September 1980 (660 cases) and one in September 1981 (1100 cases). Each sample was followed for twelve months by keeping track of AFDC case records to determine whether the sample members remained on the AFDC rolls and whether they were workers or nonworkers. Since OBRA was implemented during the twelve months of the second cohort's experience, the first cohort provides a baseline by which to judge the OBRA effects on the second cohort. The initial samples contained both workers and nonworkers, although the former were oversampled to ensure adequate sample sizes. In addition, a small telephone interview (only 152 cases) was conducted for those recipients in the second (September 1981) sample who left the rolls altogether. The interview was designed to ascertain the work levels and earnings levels of those who did not return to the rolls.

As a whole the RTI study was carefully designed and performed, and the analysis was well done. The use of the two cohorts, one before OBRA and one during OBRA, has an advantage over many other studies. The provision of a baseline cohort enormously strengthens the inferences about OBRA that can be made. Sample sizes were, on the whole, adequate for the analysis, although some categories turned out to be too small for reliable inferences.

The Wisconsin Study

A second study was conducted on data from the state of Wisconsin by the Institute for Research on Poverty (Cole et al., 1983).[10] Only a preliminary report is available at this time. The study drew a sample from the population of AFDC recipients who were working in December 1981, immediately before the OBRA provisions were implemented in the state, and whose benefits were then terminated or reduced as a result of the rule changes concerning earned income. A telephone interview with women in this sample was conducted from February to May, 1983. The sample size appears to be adequate for the main analysis (about 1200 cases).

Since the Wisconsin study is similar to the RTI study in that it followed a panel of AFDC recipients identified at a single point in time, it also can only provide estimates of a subset of the transition matrix. And since the Wisconsin study does not include a prior cohort, it is difficult to judge what the Wisconsin transition rates would have been, either in the presence or absence of macro effects. The study includes only base-month earners in the sample; nonearners are excluded. The Wisconsin sample does, however, exclude those who were terminated from the rolls solely because of the OBRA assets test, which should mitigate a problem, to be discussed below, of estimating those effects in the other studies.[11]

The GAO Study

A recent study by the U.S. General Accounting Office (1984), referred to here as the GAO study, was similar in design to the RTI study except that it included only five localities (RTI drew a national probability sample). In those five localities (Boston, Dallas, Memphis, Milwaukee, and Syracuse) both an OBRA cohort and a pre-OBRA, or comparison, cohort of recipients were drawn from the AFDC rolls. The OBRA cohort was drawn from the rolls just one month prior to the implementation date of OBRA (which varied a bit across the sites) and the pre-OBRA cohort was drawn from the caseload 13 months prior to the OBRA implementation date. Both samples included earners and nonearners, like the RTI study but unlike the Wisconsin study and unlike the two subsequent studies noted below. However, earners were oversampled to obtain adequate sample sizes for separate analyses of earners. In total, 4724 cases were drawn for the pre-OBRA cohort and 4972 were drawn for the OBRA cohort, samples more than adequate for the analyses of interest. The GAO study included several other components that will not be discussed here, including an additional cohort drawn 11 months after OBRA (this cohort was only followed one month); a personal interview with a subsample of those who had lost eligibility because of OBRA; and a separate analysis of national AFDC caseloads and costs from 1973 to 1983.

The main disadvantage of the GAO study relative to the RTI study is its inclusion of only five localities. The five chosen were intended to represent different types of welfare programs, but it is unclear whether they together present a good picture of the national caseload. Perhaps in future analyses of the data GAO will compare the characteristics of its sample with those of the nation as a whole to determine whether there are any observed differences.

The Study by the New York City Human Resources Administration

As part of an evaluation of the effect of OBRA on the New York City caseload, a study was conducted by Krauskopf and Taylor (1984) in a manner similar to those discussed previously. Two samples were drawn, one a comparison group drawn from the caseload nine months prior to the first implementation of OBRA in New York City and one from the post-OBRA period. The latter was composed of two subsamples, one drawn from the sample affected by the 150-percent-of-need rule (see above) and one drawn from the sample affected by the elimination of the 30-and-one-third rule. For purposes of this paper, the two post-OBRA subsamples are combined into one. Both samples included only earners. Approximately 8454 cases were included in the OBRA cohort and 500 in the comparison cohort. All cohorts were followed for a year.

The Minnesota Study

A small evaluation of the effect of OBRA in Minnesota was conducted by Moscovice and Craig (1983, 1984). Drawing a sample of about one thousand AFDC earners from the rolls in Hennepin County (which includes Minneapolis) one month prior to the implementation of OBRA, the analysts conducted personal interviews of the sample six months and one year later. In the end the sample size of completed interviews generated 542 observations.

The main disadvantage of the Minnesota study is its lack of a comparison group, unlike three of the studies already discussed. Also, the use of data from personal interviews rather than caseload records, while having the advantage of permitting more information to be obtained (e.g., earnings status of those not on the rolls), has the disadvantage of obtaining employment status and recipiency data that may be misreported.

The Results

The results of the five studies are shown in Tables 2 and 3. First consider the results of the RTI study, shown in the first part of both tables. They indicate that OBRA had no effect on either the probability that a working recipient would move to being a nonworking recipient (Table 2) or on the probability that a nonworking recipient would become a working recipient (Table 3). Recall that the predicted effects would be a higher probability in the first case and a lower probability in the second case. The finding in Table 2 that about 18 percent of both pre-OBRA and post-OBRA cohorts of working recipients in the base month (September) were on the rolls but not working one year later is even more

Table 2

Results of Studies of Effect of OBRA on AFDC Recipients with Earnings

	Status of Cases One Year After Initial (Base) Month[a] (Percentage Distribution)		
	On AFDC		Not on AFDC
	Not Working	Working	
RTI			
OBRA cohort	18%	27%	55%
Pre-OBRA cohort	18	54	28
Wisconsin			
OBRA cohort	15	25	60
GAO			
OBRA cohort	15-27	7-21	57-75
Pre-OBRA cohort	17-28	32-58	20-38
NYC/HRA			
OBRA cohort	16	25	59
Pre-OBRA cohort	18	45	37
Minnesota			
OBRA cohort	16	12	72

Sources: RTI (1983), Table 31, pp. 3-8; Cole et al. (1983), pp. 7-9; U.S. GAO (1984), Table 12, p. 31; Krauskopf and Taylor (1983); Table 12, p. 23 (150% and 30-1/3 groups weighted 25%-75%); Moscovice and Craig (1983), Figure 2, p. 12 (64%-32% weighted average of terminees and reductees).

[a]Time interval varies by study: RTI, 12 months; Wisconsin, 14 months; GAO, 12 months; NYC/HRA, 12 months; Minnesota, 12 months.

Table 3

Results of Studies of Effect of OBRA on
AFDC Recipients without Earnings

	Status of Cases One Year After Initial (Base) Month (Percentage Distribution)		
	On AFDC		Not on AFDC
	Not Working	Working	
RTI			
OBRA cohort	73%	3%	24%
Pre-OBRA cohort	80	4	16
GAO			
OBRA cohort	61-76	0-3	22-39
Pre-OBRA cohort	65-77	2-6	17-33

Sources: RTI (1983), Table 3.1, pp. 3-8; GAO (1984), Table 12, p. 31.

surprising in light of the expected macro bias, for unemployment rates for the entire U.S. labor force (though perhaps not for female household heads) increased more rapidly from 1981 to 1982 than from 1980 to 1981. The results in Table 3 also show that for both cohorts less than 5 percent of those who were not working in the base month were on the rolls and working a year later; there was no significant difference between the two.

The other RTI results in the tables are in line with expectations: many more of the OBRA earners were off the rolls a year later than were those in the comparison cohort, and the employment rate of recipients dropped precipitously after OBRA. Both of these findings follow from the nature of the OBRA reduction of the break-even point.

Although the result that shows no effect on the probability of becoming a nonearner is fairly surprising, equally so is the fact that the other studies in Tables 2 and 3 lead to the same conclusion. The Wisconsin study indicates that 15 percent of earners were on the rolls and not working about a year later, very close to the RTI figure of 18 percent, despite the differences in the samples (Table 2). Of course, since no comparison group was included in the Wisconsin study, it cannot be determined how these results would compare to AFDC recipients in Wisconsin prior to OBRA. The Wisconsin study also shows a similar proportion of earners who did not return to the rolls at all--60 percent, close to the 55 percent figure in the RTI study.

The GAO study, which did include a comparison group, gives similar results. Its findings are presented in the form of ranges, because separate analyses were performed for each of the five sites mentioned above. Between 15 and 27 percent of the OBRA earners were on the rolls but not working one year later, compared to 17 to 28 percent of pre-OBRA earners (Table 2). These ranges are a bit higher than those of RTI and Wisconsin, but the more important point is that the ranges for the two cohorts are quite close. Likewise, from zero to 3 percent of OBRA nonearners were still not working one year later (Table 3), compared to 2 to 6 percent of pre-OBRA nonearners. As in the RTI case, these results contain a slight suggestion of work disincentives in OBRA (a bit stronger than the RTI results, in fact), but the indication is quite small in magnitude and of doubtful statistical significance.

The NYC/HRA study found that 16 percent and 18 percent of the OBRA and pre-OBRA earners, respectively, were on the rolls and not working several months later (Table 2). These figures are quite close to one another and to the RTI and Wisconsin figures.[12] The Minnesota study, which did not include a comparison group, found that 16 percent of its OBRA earners were on the rolls and not working one year later. This percentage is similar to the comparable figures in the other studies.

Recall that the designs of these studies do not allow the estimation of other potentially important transitions. As noted earlier, OBRA should reduce the probability of coming onto the rolls as a (perhaps part-time) worker and increase the probability of coming onto the rolls as a nonworker. In the specific context of the recession, for example, it could be that individuals who lose their jobs and come onto the rolls as nonworkers fail to look for or to accept job possibilities for part-time work because of OBRA. However, OBRA may also induce some (perhaps full-time) workers who are not AFDC recipients to continue working and remain off the rolls instead of coming onto the rolls at a lower level of work effort.

Note also that no estimates of the effect of OBRA on the earnings of those who did not return to AFDC were obtained in the studies. Although some interviews were conducted with this group, they concerned only the OBRA cohort. As a consequence there were no comparison groups which could yield an estimate of this effect of OBRA.[13] For this reason, as well as that discussed in the previous paragraph, the total, or net, effect of OBRA was not obtained.

It should also be noted that these results bear on employment status rather than earnings. None of the studies examined the earnings levels of those who stopped working in the OBRA and pre-OBRA cohorts. One would expect, for example, that those in the OBRA cohort who moved from being working recipients to nonworking recipients might have reduced their earnings more than those in the pre-OBRA cohort who made the same transition (since presumably more high earners in the OBRA cohort reduced their earnings). If so, the earnings (and hours) reduction in the OBRA cohort may have been greater than that in the pre-OBRA cohort.

Another potential problem with these studies lies in the fact that by their nature they cannot determine which of the provisions of OBRA caused the observed effects. Consider, for example, the results in Table 3 indicating that more nonearners in the OBRA cohort were off AFDC a year later than nonearners in the pre-OBRA cohort. This could be a result of women taking part-time or full-time jobs for which they would have been eligible prior to OBRA but were not after OBRA, although again the recession makes this seem unlikely. An alternative explanation is that the higher exit rate in the OBRA cohort is a result of the increased restrictiveness of the assets test (described above). The increased severity of this test was widely thought to have the potential of greatly reducing the rolls, and this would certainly cause higher rates of exit in the OBRA cohort. But note that it also implies that, if the assets test alone had been imposed, the reentry rate would also be lower (since some could certainly not reestablish eligibility). Hence many OBRA earners may not have returned to the rolls (as nonworkers) because of their assets, and the fact that the return rates to nonwork were equal in the OBRA and pre-OBRA cohorts may in fact be a sign of work disincentives, after all. Although this hypothesis is purely speculative and must remain so until an examination of the effectiveness of the assets test is performed, the more general point is that various changes in eligibility rules in the OBRA legislation other than those directly related to the benefit-reduction rate may confound the simple comparison-group methodology used here.

NEW EVIDENCE FROM INDEPENDENT CROSS-SECTIONS

The main argument of this paper has been that an analysis of several independent cross-sections would give a more complete picture of the effects of interventions such as OBRA than the previously described analyses, which use recipient panel data. To illustrate this point, a simple examination of eight annual U.S. population cross-sections was undertaken. The data for each cross-section were drawn from the public use micro files of the March Current Population Survey (CPS), a random sample of the U.S. population. Using the 1977-1983 CPS files available at the Institute for Research on Poverty at the University of Wisconsin, I computed several measures for all female households heads with at least one child under the age of 18 in each year: (a) the number of weeks worked in the previous year; (b) employment status in the week of the survey (employed or not); (c) real earnings over the previous year; and (d) participation in AFDC (whether any AFDC income had been received over the previous year). For the three variables covering the previous year's activity, the data thus give a time series from 1976 to 1982; for the one variable pertaining to the survey week, the data give a time series from 1977 to 1983. The years 1976-1981 are pre-OBRA years; 1982 and 1983 are post-OBRA. The question that can be addressed with these data is whether any of the measures of labor supply show a deviation from trend after 1981.

Table 4 shows the mean values of labor supply over the available years. The main object of interest in the table is the change from 1981 to 1982. Over these two years, weeks worked by female household heads dropped from 27.9 to 26.3, a large drop by historical standards. But the unemployment rate rose at the same time, from 7.6 percent to 9.7 percent, also a larger change than any other in the table. The obvious question is how much of the drop in weeks worked can be attributed to OBRA. The percentage of those employed also dropped, from 57.2 percent to 55.5 percent of the population of female household heads. Participation in the AFDC program dropped as well, as should be expected. However, rather surprisingly given the severity of the recession, real earnings of female household heads rose from 1981 to 1982, from $2344 to $2351.[14] (Note

Table 4

Trends in Mean Labor Supply

	All Women Heading Households with Children under 18					AFDC Recipients			National Unempl. Rate	CPI	Real AFDC Guarantee[a]
	Weeks Worked	Nominal Earnings	Real Earnings	AFDC Part.	Empl. Status	Weeks Worked	Nominal Earnings	Real Earnings			
1976	25.6	$3653	$2143	.392	N.A.	10.9	$1063	$623	7.7	1.71	$170
1977	26.0	4065	2240	.375	.527	10.6	1061	585	7.1	1.82	166
1978	27.2	4572	2340	.376	.557	12.3	1310	670	6.1	1.95	162
1979	28.5	5518	2538	.344	.558	12.0	1636	753	5.8	2.17	154
1980	28.1	5917	2397	.335	.575	10.2	1375	557	7.1	2.47	142
1981	27.9	6385	2344	.341	.572	10.2	1667	612	7.6	2.72	130
1982	26.3	6798	2351	.337	.555	7.3	1302	450	9.7	2.89	131
1983	N.A.	N.A.	N.A.	N.A.	.516	N.A.	N.A.	N.A.	9.6	2.98	N.A.

Source: Computations by the author from March Current Population Survey, 1977-1983.

Note: Employment status refers to whether employed or not in the week of the survey; AFDC participation is defined by receipt of any AFDC income in previous year.

[a]For a family of four, per month.

that all earnings figures in the table include nonworkers, who have an earnings value of zero.) The table also shows the trends for AFDC recipients alone; these are less interesting because the reduction in the AFDC break-even level induced by OBRA will automatically lower the number of weeks worked and the earnings of those remaining recipients--as confirmed by the table.

To disentangle the macro influences from the effect of OBRA, a simple set of regressions were estimated. With the six observations from 1976 to 1981 (only five for employment status), each of the labor supply measures was regressed upon a time trend and the unemployment rate shown in Table 4. A value for 1982 was then predicted from the estimated coefficients, and this was compared to the actual 1982 value. The difference is an indication of whether there was any deviation from trend after 1981, controlling for the business cycle. The results are shown in Model I of Table 5. As expected, the unemployment rate had a negative effect on labor supply and a positive effect on AFDC participation rates (column 1). There was also a positive time trend in labor supply and a negative trend in participation rates (column 2; these are discernible in Table 4). The next two columns show that the actual 1982 values of weeks worked and employment status were lower than the predicted values; this implies that OBRA had, on net, a negative effect on labor supply. However, neither predicted value is significantly different from the actual value, even at the 20 percent level. Hence the results should be interpreted as indicating no significant effect of OBRA.[15] On the other hand, the predicted value of real earnings is significantly below the

Table 5

Effects on Labor Supply of Unemployment Rate
and Real AFDC Guarantee: Regression Results

Dependent Variable	Coefficients in 1976-1981 Regressions[a] Unempl. Rate	Time Trend	Real Guarantee	Intercept	Predicted 1982 Value[b]	Actual 1982 Value
A. All Female Household Heads						
Model I. Regression Using Time Trend and Unempl. Rate						
Weeks worked	-0.676*	0.530*	—	-9.75*	27.2	26.3
Employment status	-0.007	0.012*	—	-0.36*	.573	.555
Real earnings[c]	-0.119*	0.045*	—	-0.40*	2159*	2351
AFDC part.	0.003	-0.012*	—	1.24*	.330	.337
Model II. Regression Using Real Guarantee and Unempl. Rate						
Weeks worked	-0.958*	—	-0.066*	44.01*	26.1	26.3
Employment status	-0.012	—	-0.001*	0.86*	.549	.555
Real earnings[c]	-0.143*	—	-0.006*	4.20*	2064**	2351
AFDC part.	0.010	—	0.001*	0.07*	.354	.337

(table continues)

Table 5, continued

Dependent Variable	Coefficients in 1976-1981 Regressions[a] Unempl. Rate	Time Trend	Real Guarantee	Intercept	Predicted 1982 Value[b]	Actual 1982 Value
B. All Female Households Heads with Income Below 125% of Break-even Level[d]						
Model I.						
Weeks worked	-0.756*	0.120*	—	18.237*	20.7**	23.8
Real earnings[c]	-0.095*	-0.017*	—	3.534*	1216**	1878
AFDC part.	0.002	-0.006*	—	0.861*	.416**	.373
Model II.						
Weeks worked	-0.820*	—	-0.015*	30.424*	20.5**	23.8
Real earnings[c]	-0.086*	—	0.002*	1.813*	1252**	1878
AFDC part.	0.005	—	0.001*	0.285*	.428**	.373

Source: Computations by the author from March Current Population Survey, 1976-1982.

[a]Starred coefficients significant at 20 percent level.

[b]* = significantly different from actual 1982 value at 20 percent level.
** = significantly different from actual 1982 value at 10 percent level.

[c]Variable divided by 1000.

[d]Employment status equations not available.

actual 1982 value, implying that OBRA had a positive effect on real earnings. This almost has to be the case in light of the increase in real earnings despite the recession. Together with the previous results, this indicates that OBRA may have had a positive effect on hours of work or on the hourly wage rate. (Hours of work last year is a question not asked on the CPS.) The fourth row and last two columns of the table indicate, also rather surprisingly, no significant deviation from the (cyclically adjusted) trend in AFDC participation rates.

As with any simple before-and-after study such as this one, it is important to determine whether the results are sensitive to the trend and cycle effects implied by the regressions. The most important method by which to do so is to extend the data further back in time in order to better estimate the trend and cycle effects, and to continue to track the variables into the future so that more post-OBRA osbervations can be obtained. With only one post-OBRA observation for most of the data, the conclusions that can be drawn about permanent deviations from trend obviously have to be very tentative.[16] Another question is whether the 1981-1982 recession was cyclically different from past recessions, a question almost impossible to answer with these data. One test that can be performed, however, concerns sensitivity to the unemployment rate used. The variable used in Table 5 is the unemployment rate for the entire civilian population, not that for female household heads. The unemployment rate for the latter group is considerably higher than for the total population and is a bit less cyclically sensitive. However, when the analysis reported in Table 5 was repeated using the unemployment rate for female household heads, the results were the same in all major respects. The results are shown in Appendix B for the reader interested in the details of that analysis.

Another question that arises concerns the source of the time trend. One explanation is that labor supply and AFDC participation trends have been responding to reductions in the real AFDC guarantee. The real AFDC guarantee for a family of four is shown in Table 4.[17] Because states have not been raising nominal AFDC benefit levels to match inflation over the 1970s, real guarantees have fallen. Unfortunately for present purposes, the correlation coefficient between the real guarantee and the time trend over the 1976-81 period is .97. Six observations are too few to separate time trends from real guarantee effects. One can, however, redo the above regression analysis by replacing the time trend with the real guarantee, giving the results shown for Model II in part A of Table 5. The regression coefficient on the guarantee is negative on labor supply and positive on participation, as expected. The predictions for 1982 again show positive effects on real earnings, but essentially no effects on labor supply. As Table 4 indicates, the real guarantee was essentially constant from 1981 to 1982; hence no further increase in labor supply from this source is contained in the prediction. In the time-trend results, in contrast, a further increase in labor supply was contained in the prediction. Thus we must be left here with some uncertainty regarding the magnitude of the labor supply effect. But note that in both cases the predicted values are insignificantly different from the actuals; consequently, the finding of "no significant effect" still holds. (The remainder of Table 5 is discussed in conjunction with Table 6, below).

A final question that can be addressed with these data is the extent to which the 1981-1982 shifts are coming from the low-income section of the income distribution. It has been argued in this paper that, in principle, all income strata can be affected by OBRA. But it was pointed out in the discussion of Table 1 that an examination of low-income strata alone would certainly be useful, inasmuch as most of the effects of OBRA should occur there. As argued in the theoretical section, there should be some discernible effect not only below the break-even level but above it as well, for OBRA should induce movements around the break-even level. Just how far above the break-even level OBRA effects will occur cannot be determined a priori. In any case, to examine these effects the mean labor supply and participation rates over the years were recalculated for those female household heads whose incomes were not too far above their AFDC break-even levels. A break-even level of income was calculated for each woman, based upon the effective guarantees and tax rates in her state for each year (these have been estimated by Fraker et al., 1984).[18]

The results are shown in Table 6. The first three columns repeat the trends for the total population of women heading households. The second three columns show the trends for those whose income was below twice the break-even level. Although real earnings again rose and participation rates fell for this group, weeks worked actually rose from 1981 to 1982, in marked contrast to those for all female household heads in the first column. The remaining columns in the table show the trends for successively lower strata of the income distribution, and show the same effects. As one moves down the income distribution, the 1981-1982 increases in weeks worked and in real earnings, and the decreases in participation rates, become ever larger. For the lowest stratum, those with

Table 6

Labor Supply and Participation Trends among Female Household Heads of Different Income Strata[a]

	All			Income Below 200% of Break-even Level			Income Below 150% of Break-even Level			Income Below 125% of Break-even Level			Income Below Break-even Level		
	Weeks Worked	Real Earnings	AFDC Part.	Weeks Worked	Real Earnings	AFDC Part.	Weeks Worked	Real Earnings	AFDC Part.	Weeks Worked	Real Earnings	AFDC Part.	Weeks Worked	Real Earnings	AFDC Part.
1976	25.6	$2143	.392	23.9	$1828	.417	22.7	$1656	.433	21.7	$1501	.447	20.2	$1312	.470
1977	26.0	2240	.375	24.1	1868	.401	23.0	1707	.417	21.9	1561	.432	20.1	1319	.457
1978	27.2	2340	.376	24.1	1777	.424	24.1	1777	.424	22.8	1590	.445	20.8	1334	.472
1979	28.5	2538	.344	26.4	2104	.375	24.9	1854	.397	23.5	1664	.417	21.3	1365	.449
1980	28.1	2397	.335	25.7	1942	.369	24.1	1703	.391	22.6	1510	.411	20.4	1227	.441
1981	27.9	2344	.341	25.1	1829	.382	23.6	1607	.406	22.1	1420	.425	19.5	1167	.454
1982	26.3	2351	.337	25.2	2094	.354	23.9	1879	.373	23.8	1879	.373	22.7	1744	.387

Source: Computations by the author from March Current Population Survey, 1976-1982.

[a]In 1982, break-even level calculated using 1981 effective tax rates.

income below the break-even level, the total number of weeks worked rose by almost three weeks from 1981 to 1982. (Note that the break-even level was calculated using the 1981 tax rate.) The data seem to be indicating stronger positive effects of OBRA at the lower income levels.

To confirm this statistically, the regression analysis was repeated for the group with income below 125 percent of the break-even level. The results are shown in part B (lower half) of Table 5, with both the time trend (Model I) and the real guarantee (Model II) used in the equation. The 1982 predicted values for labor supply are in this case significantly below the actuals, indicating positive effects for OBRA, controlling for trend and cycle. Again, the results are insensitive to the use of either the total unemployment rate in the nation or the unemployment rate for female household heads. The results for the latter are shown in Appendix B.

SUMMARY

The effect of the 1981 Omnibus Budget Reconciliation Act on the labor supply of female household heads eligible for the AFDC program is theoretically ambiguous. While an increase in the benefit-reduction rate may discourage work effort for some, others will increase their work effort to compensate at least in part for the loss of benefits. Thus whether labor supply increases or decreases, on net, is an empirical question. Unfortunately, past econometric studies of the effect of AFDC on labor supply are inadequate to predict the net effect of OBRA. Consequently, new analyses are needed. A properly designed study should carefully delineate the population being studied; if not, then not all OBRA effects may be captured by the measurement. In particular, a study examining a series of independent cross-sections should provide a more complete picture of the effects of OBRA than a study examining only the exit and reentry rates of a set of AFDC recipients. Several studies using the latter approach--focusing on recipients who move off and on the rolls--have been completed and are reviewed in this paper. Their results indicate, almost uniformly, that OBRA did not discourage work effort. A simple study of the former type, analyzing independent cross-sections of female household heads, is also provided in this paper; its results again indicate that OBRA exerted no significant effect (if not a positive one) on labor supply and earnings of all female household heads, and a noticeable positive effect on those in the lower income strata. Additional research, particularly over more periods in the past and into the future, is required for more definitive conclusions.

APPENDIX A

Decomposition of Changes in Level Variables by Transition Rates

The decomposition of changes in "level" variables (i.e., variables such as those in Table 1 which are defined at a single point in time) into transition rates involves a conceptually straightforward derivation of a series of accounting identities, each describing the components of change of the level variable. A formal statement of such accounting identities is useful in illustrating the nature of the analysis required with panel data.

First consider the simple case in which no distinction is made between individuals with zero hours and earnings and those with positive amounts. Define the following variables:

$F_P(t)$ = Probability that P = 1 at time t

$F_N(t)$ = Probability that P = 0 at time t

$= 1 - F_P(t)$

$H_P(t) = E(H(t) \mid P = 1)$

$H_N(t) = E(H(t) \mid P = 0)$

R_{ij} = probability of moving from state i to state j, with i,j equal to either P or N (on or off AFDC).

Since there are only two states here--participating and not participating--there are four transition rates. Expected hours is equal to

$$E(H(t)) = F_P(t)H_P(t) + F_N(t)H_N(t).$$

To illustrate changes in level variables, consider just two--the total participation rate in the population, F_P, and the mean hours level in the population, E(H). By working logically with the formulas for these variables, it can be seen that the following two accounting identities describe their decomposition (here Δ signifies the change from t to t+1):

$$\Delta F_P = F_N(t)R_{NP} - F_P(t)R_{PN}$$

$$\Delta E(H) = F_P(t)[H_P(t+1) - H_P(t)] + (\Delta F_P)[H_P(t+1) - H_N(t+1)] + (1 - F_P(t))[H_N(t+1) - H_N(t)].$$

Both of these decompositions have ready intuitive explanations. The change in the participation rate equals the difference between inflows and outflows from participation, each equal to the relevant transition rate weighted by the fraction of the population initially in each state. The change in mean hours equals the weighted average of the change in hours of participants and the change in hours of nonparticipants, plus the change in hours induced by the change in the participation rate itself, ΔF_P. The latter is defined by the previous equation.

Next consider the case in which a distinction is made between workers and nonworkers, generating four states and sixteen transition rates. Let

$F_N^0(t)$ = fraction of population not participating and not working

$F_P^0(t)$ = fraction of population participating and not working

$F_N^+(t)$ = fraction of population not participating and working (i.e., with positive hours)

$F_P^+(t)$ = fraction of population participating and working

$H_j^i(t)$ = hours or earnings of individuals in employment status i (0 or +) and participation status j (P or N)

R_{kl}^{ij} = probability that an individual in employment status i (0 or +) and participation status k (P or N) will move to employment status j (0 or +) and participation status l (P or N). E.g., R_{PN}^{+0} is the probability that a working participant will become a nonworking nonparticipant.

Given these definitions note that expected hours are:

$$E(H(t)) = F_P^+(t)\ H_P^+(t) + F_N^+(t)\ H_N^+(t).$$

The decomposition of the four participation rates can again be written as the sum of outflows and inflows. Two representative decompositions are the following:

$$\Delta F_P^+ = F_P^0(t)\ R_{PP}^{0+} - F_P^+(t)\ (R_{PP}^{+0} + R_{PN}^{++} + R_{PN}^{+0}) + F_N^0(t)\ R_{NP}^{0+} + F_N^+(t)\ R_{NP}^{++}$$

$$\Delta F_N^+ = F_P^0(t)\ R_{PN}^{0+} + F_P^+(t)\ R_{PN}^{++} + F_N^0(t)\ R_{NN}^{0+} - F_N^+(t)\ (R_{NN}^{+0} + R_{NP}^{+0} + R_{NP}^{++}).$$

The decomposition of mean hours can be written in the following form:

$$\Delta E(H) = F_P^+(t)[H_P^+(t+1) - H_P^+(t)] + F_N^+(t)[H_N^+(t+1) - H_N^+(t)]$$

$$+ (\Delta F_P^+)\ H_P^+(t+1) + (\Delta F_N^+)\ H_N^+(t+1).$$

Here again the change in hours equals a weighted average of changes in hours and changes in participation rates, the latter derivable from the above two equations.

The pattern of the analysis should be clear from these cases. For the general case, assume that there are n states i = 1, ..., n, that the probability of being in each state is $F_i(t)$, and that hours or earnings in each state is $H_i(t)$. Then expected hours is:

$$E(H(t)) = \sum_{i=1}^{n} F_i(t)\ H_i(t).$$

Now let R_{ij} be the probability of moving from state i to state j. Then the decompositions of changes in participation rates and mean hours are the following:

$$\Delta F_i = \sum_{j \neq i} R_{ji} F_j(t) - F_i(t) \sum_{j \neq i} R_{ij} \qquad (i = 1, \ldots, n)$$

$$\Delta E(H) = \sum_{i=1}^{n} [(\Delta F_i)\ H_i(t) + (\Delta H_i)\ F_i(t+1)].$$

Again the change in a participation rate is defined as equaling inflows minus outflows, and the change in mean hours decomposes into a weighted average of changes in hours within states and changes in hours resulting from changes in the probabilities of being in states.

APPENDIX B

Regression Results Using the Unemployment Rate among Women Heading Households

The unemployment rates for female household heads over the period are as follows:

1976	10.0%
1977	9.4
1978	8.5
1979	8.3
1980	9.2
1981	10.4
1982	11.7
1983	12.2

There is some danger in adding this unemployment rate to a regression in which labor supply for all female household heads is the dependent variable, for there is a definitional relationship between the two. An increase in the employment rate of female household heads will by itself probably decrease the unemployment rate. In the present context, this implies that the unemployment rate of single women with children may itself be affected by OBRA. In any case, the results of the regression analysis shown in Table B.1 are quite similar to those reported in Table 5.

Appendix Table B.1

Regression Results with Unemployment Rate for Female Household Heads

Dependent Variable	Coefficients in 1976–1981 Regressions[a]: Unempl. Rate	Time Trend	Real Guarantee	Intercept	Predicted 1982 Value[b]	Actual 1982 Value
A. All Female Household Heads						
Model I.						
Weeks worked	-0.0655*	0.568*	—	-11.293*	27.6**	26.3
Employment status	-0.009	0.013*	—	-0.402*	.575	.555
Real earnings[c]	-0.113*	0.052*	—	-0.687*	2245	2351
AFDC part.	0.005	-0.012*	—	1.242*	.330	.337
Model II.						
Weeks worked	-0.960*	—	-0.073*	47.443*	26.6	26.3
Employment status	-0.145*	—	-0.002*	0.933*	.554	.555
Real earnings[c]	-0.141*	—	-0.007*	4.677*	2151**	2351
AFDC part.	0.011*	—	0.002*	0.022*	.352	.337

(table continues)

Appendix Table B.1, continued

Dependent Variable	Coefficients in 1976-1981 Regressions[a] Unempl. Rate	Time Trend	Real Guarantee	Intercept	Predicted 1982 Value[b]	Actual 1982 Value
B. All Female Household Heads with Income below 125% of Break-even Level						
Model I.						
Weeks worked	-0.716*	0.162*	—	16.402*	21.3**	23.8
Real earnings[c]	-0.091*	-0.012	—	3.306*	1283**	1878
AFDC part.	0.004	-0.006*	—	0.855*	.418**	.373
Model II.						
Weeks worked	-0.804*	—	-0.021*	33.156*	21.0**	23.8
Real earnings[c]	-0.085*	—	0.001	2.101*	1305**	1878
AFDC part.	0.007	—	0.001*	0.244*	.429**	.373

[a]Starred coefficients significant at 20 percent level.

[b]* = Significantly different from actual 1982 value at 20 percent level.
** = Significantly different from actual 1982 value at 10 percent level.

[c]Variable divided by 1000.

[d]Employment status equations not available.

NOTES

[1]In the extreme case in which the individual increases earnings to make up entirely for the loss of benefits, the arrow would indicate a movement horizontally to the left of point C.

[2]Note, however, that the level of utility is nevertheless lower, as it is for all individuals initially on the AFDC program who are working. Regardless of the labor supply response to OBRA, all affected individuals are worse off.

[3]This is a general result, for a change in the tax rate of a welfare program always has ambiguous effects on labor supply. See Moffitt (1983b).

[4]The true OBRA effects are not quite as mechanical as portrayed here, for several reasons: (1) the benefit-reduction rate is not zero during the first four months; (2) some individuals with a sufficient commitment to work will continue to do so and will stay on the rolls even though take-home income would be the same if they did not work; (3) other provisions of OBRA may reduce benefits of recipients; and so on.

[5]These examples are used because the subpopulation is defined on the basis of an endogenous variable--welfare recipiency or hours of work--not an exogenous one. If the subpopulation were exogenously defined--e.g., the subpopulation of high school graduates (assuming OBRA did not affect educational levels)--it could be analyzed with the single cross-section.

[6]Parenthetically it may be noted that even initial data are not sufficient to answer all questions of interest in an experiment. For example, it is not possible to measure experimentally the effect of a

program on "those who respond to it." The subpopulation of responders is defined as an outcome variable--and there is no means by which a subpopulation of the control group can be defined (experimentally, that is) to which experimental responders could be compared.

[7]It may be noted that OBRA must still be treated, in this case, as a "natural" experiment. A natural experiment occurs when an event occurs exogenously in "nature." An event occurs exogenously if its occurrence is independent of the prior level and growth rate of the outcome variable. If, for example, states that pass right-to-work laws are those that have low levels of unionization in the first place, the passage of the law does not represent a natural experiment. In the case of OBRA, an argument for its failure as a natural experiment would have to be based upon an argument that its passage was a response to growing conservatism in welfare policy, and that similar sorts of legislation would have occurred in any case.

[8]The point at which the effects disappear is the point at which the change in the hours effect is exactly equal to the previous hours effects weighted by the change in the proportion of the population covered.

[9]The statistical point here is a simple one. The panel data allow one to estimate the covariance between the two populations because one has two observations on a set of individuals, but this is needed only in the calculation of the variance of the difference in population means (which improves efficiency) and not in the calculation of the difference in means itself (the bias question).

[10]See also Davies (1983) for an earlier simulation study on Wisconsin data. The Davies study also provides a detailed discussion of the effect of OBRA on individual budget constraints.

[11]However, those jointly terminated for both OBRA-related assets reasons and OBRA-related earnings reasons are included.

[12]Since the NYC/HRA cohort was selected nine months prior to OBRA but was followed for twelve months, there is some contamination in the comparison group figures. A sensitivity analysis was performed, following both cohorts for only nine months; the results were very similar to those for twelve months.

[13]RTI found that 87 percent of the group was working, while Wisconsin found an employment rate of 79 to 95 percent for the same group.

[14]To ensure that this was not a computational error in the analysis, the published Current Population Reports, Series P-60, for 1981 and 1982, which are simply published versions from the same tapes used here, were checked. The increase in real earnings also appears there. In fact, real earnings for all women, not just female household heads, rose over the two years. Real earnings for men declined, however.

[15]In a previous paper (Moffitt, 1984), a simulation of an increase in the tax rate to 100 percent also showed relatively small net effects over this range.

[16]The one variable available for 1983--employment status--dropped considerably, suggesting that disincentive effects may have grown. Further analysis will have to await the next CPS.

[17]The guarantees from 1976 to 1981 were taken from an unpublished Appendix to Kasten and Todd (1983) and the 1982 figures were obtained from Solomon (1982).

[18]The break-even level is equal to the guarantee divided by the tax rate. The guarantee was specific not only to the state and year but also varies by family size. See the estimates in Fraker et al. (1984).

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