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ASSESSING THE EFFECTS OF THE
1981 FEDERAL AFDC LEGISLATION
ON THE WORK EFFORT OF WELFARE
RECIPIENTS: 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 Welfare Recipients:

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, and reviews critically the studies that have been completed to date. 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; and (3) the studies that have been completed to date, although indicating little effect of OBRA on labor supply, are incomplete and therefore unable to provide any definitive answer as yet on the labor supply effects of OBRA.

Assessing the Effects of the 1981 Federal AFDC
Legislation on the Work Effort of Welfare Recipients:
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 propor-
tion 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, existing 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 an 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 two studies of OBRA that are currently available. Their results indicate suprisingly little labor supply response to the program changes. However, the studies to date 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. Further study is therefore required before any definitive conclusions can be drawn.

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
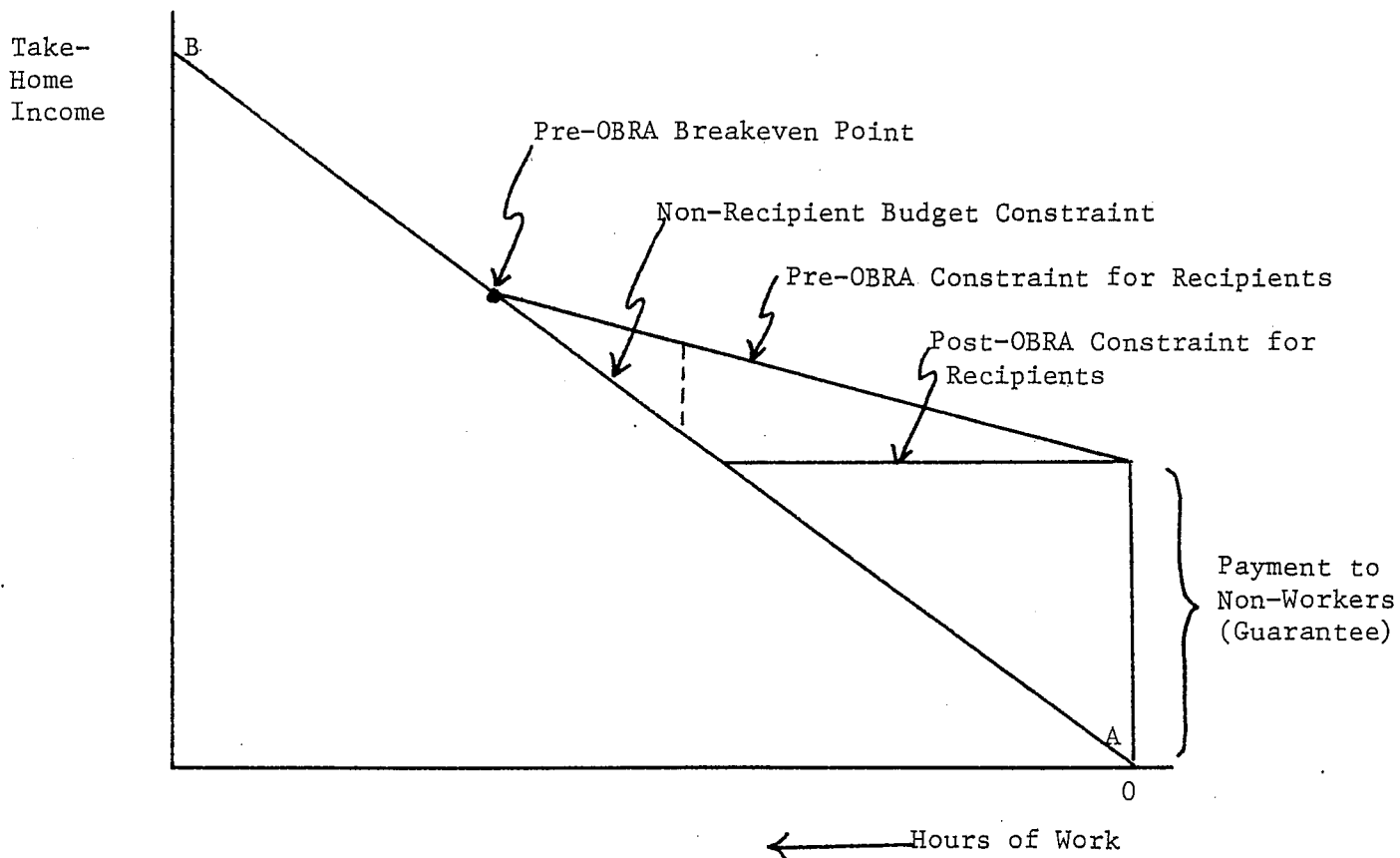
Take-
Home
Income

Pre-OBRA Breakeven Point

Non-Recipient Budget Constraint

Pre-OBRA Constraint for Recipients

Post-OBRA Constraint for
Recipients

Payment to
Non-Workers
(Guarantee)

Hours of Work

FIGURE 1

Take-
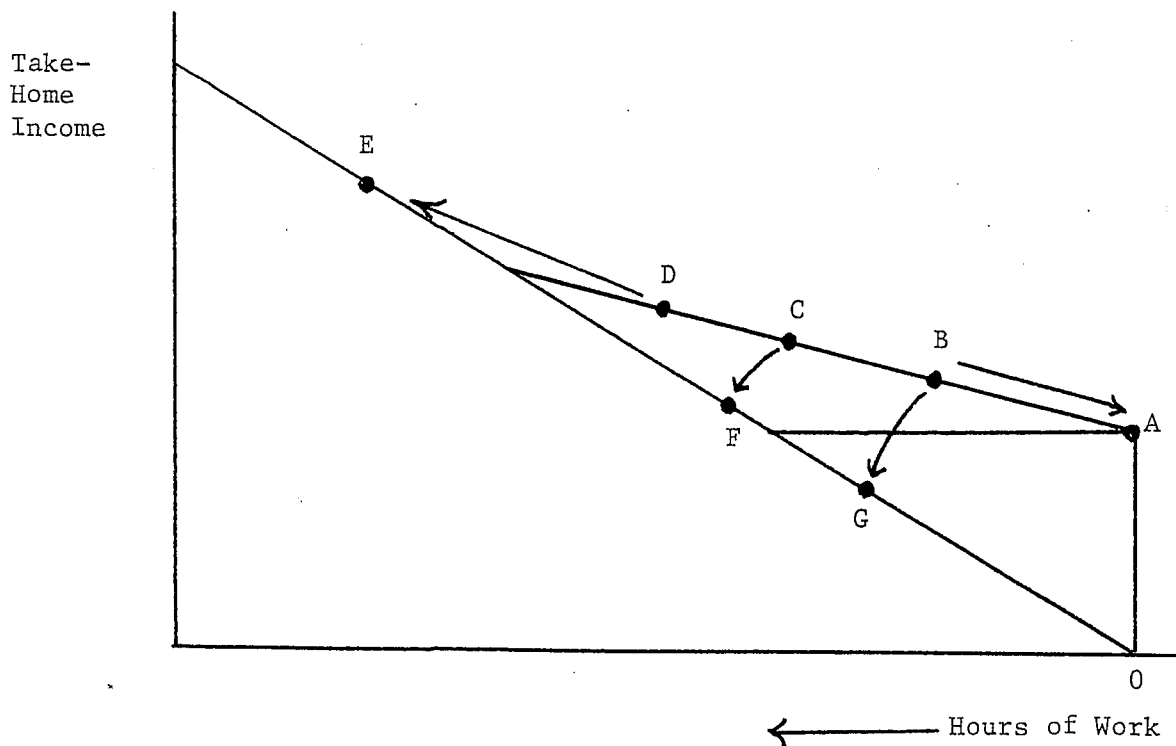Home
Income

Hours of Work

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, non-marginal 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 households 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. <u>The effect of OBRA on labor supply is ambiguous in sign.</u> 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. <u>The effect of OBRA on take-home income is ambiguous in sign.</u> Again, the net effect will depend upon the relative numbers and magnitudes of response of individuals of different types.

3. <u>The effect of OBRA on the participation rate (i.e., the caseload) in AFDC is unambiguously negative.</u> 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. <u>The effect of OBRA on the participation rate of eligibles is ambiguous in sign.</u> 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. <u>The effect of OBRA on program costs is ambiguous.</u> Although benefits are no longer paid to non-recipients, 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 <u>recipient</u> 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 (1983a) 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 Hausman's 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 (1983b) 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 households heads, low-income individuals, etc.), randomized the sample into an

experimental and 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 sub-population 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 working 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 sub-population 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 sub-components 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 populations 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, 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). 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

of the population of women who are household heads are eligible for AFDC
and that about 35 percent of that female-headed population 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 effect of OBRA on
labor supply—for example, whether labor supply has increased or
decreased. Nevertheless, since some part of the population is of suf-
ficiently high income that it is 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 breakeven point, for example, would be too restrictive, for as
Figure 2 indicates, some of the OBRA response will be manifested by move-
ments 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

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.)

Note: Definitions of variables are as follows:

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

H = earnings or hours.

H_{BE} = breakeven level of H.

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

Y^{BE} = income.

Y^{POV} = poverty - level income.

Y^* = arbitrary income level.

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

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

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 populations at both points in time, their use can result in bias.[9]

The notion that panel data and the study of transitions is 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-period rather than a two-period 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 the Appendix.

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. Two 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 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.

Nevertheless, there are limits to the study design that follow from the previous discussion in this paper. First and most important, the use of panel data in which only an initial set of recipients is followed from one point in time to another does not represent a sample of the joint population from the two points in time. Nonrecipients at both points are excluded and, perhaps more important, individuals who were recipients at the second point in time and not at the first point are not included in the sample and their behavior is consequently not examined. Therefore the full matrix of transitions resulting from OBRA cannot be obtained from the study; only a subset of those transitions can be examined, and consequently the net effect cannot be calculated. Specifically, the RTI study can only estimate the effect of OBRA on transitions from recipiency status (working and nonworking) to nonrecipiency status, and not vice versa.

Note too that the design does not allow for estimation of macro effects. The use of two cohorts (three points in time) is required to estimate the effect of OBRA on transition rates if there are no macro effects present; if there are, additional periods are needed. Therefore, for example, the comparison of the transitions for the two cohorts may reflect differing economic environments over the two periods as well as the effects of OBRA (although, as will be noted below, the expected direction of bias was not found in the study).

Nevertheless, the RTI findings on the transition rates that were measured are fairly surprising. They are shown in Table 2. The results

Table 2

RTI Findings on Effect of OBRA

	Status of Cases One Year Later (Percentage Distribution)		
	On AFDC		Not on AFDC
	Not Working	Working	
Those with Earnings Initially (in base month)			
OBRA Cohort	18%	27%	55%
Pre-OBRA Cohort	18	54	28
Those without Earnings Initially (in base month)			
OBRA Cohort	73	3	24
Pre-OBRA Cohort	80	4	16

Source: RTI (1973), Table 3.1, p. 3-8.

Note: Base month for OBRA cohort is September 1981; for pre-OBRA cohort, September 1980.

indicate that OBRA had no effect on either the probability that a working recipient would move to being a nonworking recipient (col. 1, rows 1 and 2) or on the probability that a nonworking recipient would become a working recipient (col. 2, rows 3 and 4). Recall that the predicted effects would be a higher probability in the first case and a lower probability in the second case. The finding for both cohorts that about 18 percent of working recipients in the base month (September) were on the rolls but not working one year later is even more surprising in light of the expected macro bias, for unemployment rates for the entire U.S. labor force (though not for female household heads) increased more rapidly from 1981 to 1982 than from 1980 to 1981. The results also show that for both cohorts less than 5 percent (col. 2) 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.

Recall that the design of the study does 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 off the rolls instead of coming onto the rolls at a lower level of work effort.

It should also be noted that these results bear on employment status rather than earnings. The RTI study did not extensively examine the

effect of OBRA on earnings, relying instead on a relatively brief analysis. One would expect, for example, that those who moved from being working recipients to nonworking recipients might have reduced their earnings more in the second (OBRA) cohort than in the first cohort, given the nature of OBRA. If so, the hours reduction in the OBRA cohort may have been greater than that in the first cohort. This was not examined in the report. And since an estimate of the OBRA effect on the employment rate of those who left the rolls after a year was not estimated, a figure for the net, or aggregate, labor supply effect of OBRA cannot be obtained.[10]

It should also be noted that by its nature the RTI study cannot determine which of the provisions of OBRA caused the observed effects, for all provisions were implemented more or less simultaneously. A particular concern in this regard is the effect of the increased severity of the assets test, which should be expected to induce a greater rate of exit from the program than otherwise. For example, as a result of the assets provision alone, one should expect fewer individuals to remain on the rolls, including those who move from working to nonworking status. The fact that the RTI results show equal rates of such movement in the two cohorts may therefore indicate that the work-incentive provisions of OBRA may after all have increased movements from working to nonworking status.

Another piece of evidence suggesting that this may have been the case is the finding that the rates of movement from recipiency to nonrecipiency status were higher in the OBRA cohort than in the first cohort not only for those who worked in the base month, but also for those who

did not work in the base month (Table 2, col. 3). It is difficult to see how the work-incentive provisions of OBRA could have induced an increase in the rate at which nonworkers leave the rolls, especially in the face of a growing recession. Finally, note that these arguments apply equally for any of the other OBRA provisions relating to eligibility, not just the assets test.

Despite the many caveats associated with this review of the RTI report, the central findings should still be termed unexpected and surprising. They deserve more examination on their own, and more studies need to be performed to determine whether the RTI findings are robust.

The Wisconsin Study

A second study was conducted on caseload data from the state of Wisconsin by the Institute for Research on Poverty (Cole et al., 1983).[11] Only a preliminary report is available at this time. The study drew a sample from the population of working AFDC recipients in December 1981, shortly before the OBRA provisions were implemented in the state. The case records were reexamined in the spring of 1983 to determine which recipients were still on the rolls, and a telephone interview of the sample was conducted from February to May. Sample sizes appear 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 chosen 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
the problem of estimating those effects in the RTI study.[12]

The most unusual aspect of the findings is their similarity to the
RTI results, at least where they can be compared. RTI found that in both
cohorts 18 percent of base-month earners were still on the rolls but not
working one year later, and the Wisconsin study found that 15 percent of
those who were initial earners were likewise nonearner recipients about
14 months after the base month. Also, RTI found that 45 percent of
earners in the OBRA cohort were still on the rolls a year later, and the
Wisconsin analysts found a comparable figure of 40 percent, again 14
months later. Given that the Wisconsin sample is only from one state and
the RTI sample is a national probability sample, these results are quite
close. Of course, since no comparison cohort was used in the Wisconsin
study, there is no guarantee that the transition rates observed are the
same as those that would have occurred in the absence of OBRA.

The Wisconsin telephone survey gathered information on hours of work
as well. The results indicated large hours reductions for those
remaining on the rolls, but also hours reductions for almost all those
not on the rolls as well. This would seem to suggest that effects of the
recession were present in the sample.[13]

SUMMARY

The effect of the 1981 Omnibus Budget Reconciliation Act on the labor supply of AFDC recipients and female households heads is theoretically ambiguous. Existing empirical studies of AFDC are inadequate to predict OBRA's impact. The major studies to date are incomplete, for they examine only a subset of all OBRA effects. However, their findings suggest that thus far OBRA has had little effect on labor supply along the dimensions examined. These studies are too few and too tentative to warrant definitive conclusions about OBRA to date. More work needs to be done, using the framework of statistical design and measurement of effects given in this paper.

APPENDIX

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:

$$F_P(t) = \text{Probability that } P = 1 \text{ at time } t$$

$$F_N(t) = \text{Probability that } P = 0 \text{ at time } t$$

$$= 1 - F_P(t)$$

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

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

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

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 $i = 1, \ldots, n$, 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.

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 by an 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 sub-population 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 identical 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]The telephone interview, although it suffered from high nonresponse rates, indicated that approximately 87 percent of such individuals who responded were working and had fairly high earnings. The lack of similar data for the first cohort means that we cannot determine whether these same figures would apply in the absence of OBRA.

[11]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.

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

[13]The employment rate of those off the rolls as of the interview date is in the range of .79 to .95, which includes the RTI figures (see footnote 9).

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