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Robert Hutchens

THE EFFECTS OF THE OMNIBUS BUDGET RECONCILIATION ACT OF 1981 ON AFDC RECIPIENTS: A REVIEW OF THE STUDIES

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The Effects of the Omnibus Budget Reconciliation Act of 1981 on AFDC Recipients: A Review of Studies

Robert M. Hutchens
New York State School of Industrial and Labor Relations
Cornell University

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Abstract

The Omnibus Budget Reconciliation Act (OBRA) of 1981 sought to alter the Aid to Families with Dependent Children (AFDC) program in ways that would save public monies and discourage dependence on welfare. This paper reviews six studies that examined the impact of OBRA on the employment status, recipiency status, and economic well-being of AFDC recipients.

With regard to employment and recipiency status after OBRA, the evidence indicates that:

For people who were initially working recipients,

(1) the probability that they remained working recipients decreased.

(2) the probability that they became nonworking recipients did not increase.

(3) the probability that they left the program and became nonrecipients increased.

For people who were initially nonworking recipients,

(4) the probability that they became working recipients decreased.

(5) the probability that they remained nonworking recipients did not increase.

(6) the probability that they left the program and became nonrecipients increased.

These statements are subject to several caveats which are detailed in the text.

Of particular interest are results (2) and (5). The paper argues that they are inconsistent with the hypothesis that OBRA simply raised AFDC marginal tax rates. A possible explanation is that OBRA not only

raised tax rates but also effectively decreased guarantees through its new rules regarding stepparent income, assets, 18-21-year-olds, pregnant women, retrospective budgeting, and monthly reporting.

With regard to economic well-being the six studies yield solid evidence that the average pre-OBRA working recipient suffered a decline in income during the year after OBRA's implementation. Income from earnings, Food Stamps, and other sources did not replace their lost AFDC benefits. Moreover, on average, other medical insurance did not replace the loss of Medicaid, and some families thus confronted higher out-of-pocket costs for medical care. Finally, there is good evidence that after OBRA the working mothers in these families often utilized a lower quality and quantity of day care for their children. All of the evidence thus indicates that the average pre-OBRA working recipient experienced a sharp decline in economic well-being in the year following OBRA.

The paper concludes that OBRA was not a benign policy change. It saved public monies at the cost of decreased economic well-being for many poor families.

INTRODUCTION

Since its inception, the Aid to Families with Dependent Children (AFDC) program has sought to provide adequate financial support to children while encouraging adult recipients to move toward supporting themselves. Indeed, the 1935 Social Security Act--the statutory foundation for the AFDC program--speaks of helping "parents or relatives to attain or retain capability for the maximum self support and personal independence consistent with the maintenance of continuing parental care and protection." As women with children entered the labor force in increasing numbers, this goal came to mean that AFDC mothers should work. To this end there evolved during the 1960s a strategy that used financial rewards to encourage work. Under this strategy a recipient who earned an additional dollar lost less than one dollar in benefits. Work was thus rewarded with increased income; through work an AFDC mother could raise her total (AFDC plus earned) income. Throughout the 1960s and 1970s this strategy influenced government programs for the needy. It shaped the Food Stamp program, President Nixon's Family Assistance Plan, and President Carter's Program for Better Jobs and Income as well as the AFDC program.

At least for the AFDC program, the Omnibus Budget Reconciliation Act (OBRA) of 1981 marked a reversal of that strategy. In seeking to save public monies and discourage welfare dependence, OBRA changed the rules governing calculation of AFDC benefits so that treatment of working recipients became quite similar to that existing before 1960. In particular, after OBRA a recipient who earns a dollar can lose a dollar in benefits and end up with no improvement in total income. This strategy reversal spawned a great deal of controversy. Critics charged that OBRA would force working recipients to quit their jobs, become fully dependent on welfare, and thereby increase taxpayer costs. To help resolve the controversy, several studies were launched that collected and analyzed data on the effects of OBRA.[1] The purpose of this paper is to review six of these studies with an eye to assessing what we learn from them.

At the outset it is important to identify what we wish to know. This helps to circumscribe the task; it facilitates the process of separating relevant evidence from the irrelevant. Four broad questions are of central importance.

1. What effect did OBRA have on AFDC participation?
2. What effect did OBRA have on labor supply behavior (e.g., employment status, hours, earnings).
3. What effect did OBRA have on economic well-being?
4. What effect did OBRA have on AFDC costs?

Of course, narrower versions of these questions can be posed with regard to specific population groups (e.g., AFDC recipients who were working just before implementation of OBRA) or specific time periods (e.g., one year after implementation of OBRA).

The studies reviewed here were the only studies that addressed these four questions with micro-level data. All were available by July 1984; all examined OBRA's effects by collecting "before" and "after" data on the employment status, welfare status, and economic well-being of a cohort of AFDC recipients; and all include personal interviews with people who are no longer AFDC recipients.

A 1984 study by Robert Moffitt is not included in this review because it relies on aggregate rather than micro-level data. My debt to Moffitt must, however, be acknowledged. Moffitt's paper includes a review of some of the papers covered here. Although my notes indicate a few salient points of agreement and disagreement, they cannot provide a full accounting of the frequency with which I have consulted his study.

The structure of the present paper flows out of the above four questions. The next section details the changes wrought by OBRA and develops hypotheses on the effect of these changes. Section II describes the six studies and Section III discusses their answers to the four questions. The final section summarizes and notes areas where further research may be helpful.

I. HOW OBRA CHANGED THE AFDC PROGRAM AND ITS EXPECTED EFFECTS

By design OBRA sought to shift benefits away from AFDC recipients considered less than "truly needy." AFDC families with earnings fell into this category and in consequence lost benefits. Three provisions of OBRA were particularly important in this regard:

- the establishment of a $75 maximum on work expense deductions and a $160 maximum (per child) on child care expense deductions.
- application of the $30 and 1/3 disregard to net rather than gross earnings and its limitation to four consecutive months of employment in any 12-month period.
- restriction of gross income to 150 percent of the state need standard.

As argued by others,[2] these provisions effectively increased the marginal implicit tax rate on earned income for AFDC recipients. The marginal implicit tax rate indicates how a change in earnings affects the sum of AFDC plus earned income. For example, if the tax rate is 100 percent, a one dollar increase in earnings causes a one dollar decrease in AFDC benefits and, in consequence, no change in AFDC plus earned income. At a 50 percent tax rate, a one dollar increase in earnings causes a fifty-cent decrease in AFDC benefits and, in consequence, a fifty-cent increase in the combined income. We know that before OBRA the marginal implicit tax rate on earnings was in the neighborhood of 29 percent.[3] Although the precise magnitude of the post-OBRA tax rate is not known, there undoubtedly exist cases where it is 100 percent or more.[4]

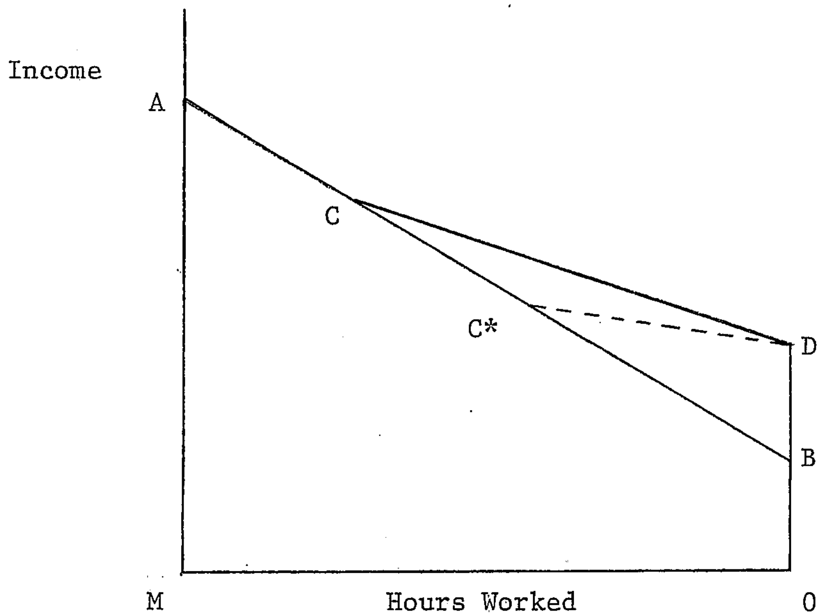
How does an increased tax rate alter the behavior of existing and potential AFDC recipients? The studies reviewed here generally analyzed changes in behavior over time. For example, they collected data on the post-OBRA labor market and AFDC status of pre-OBRA recipients. Before looking at the studies it is useful to ask what we expect to find. This helps to organize our thinking; it makes us more perceptive to what the studies tell us. Would we, for example, expect the increased tax rate to cause working AFDC recipients to stop working? An explicit theory can be quite helpful in trying to answer such questions.

A model of income and leisure choice provides a natural starting point for a theory. As is the nature of theories, this one rather severely abstracts from reality. In particular, it is assumed that people are rational, that they derive utility from nothing other than current income and leisure, that they ignore future wages and prices, that they have perfect information, and that they exercise complete freedom of choice over hours worked per unit of time. Despite this, the theory has the virtue of yielding several hypotheses that tend to be supported by empirical analysis.

The theory and its hypotheses are perhaps most easily exposited with the help of an income-leisure diagram such as that in Figure 1. If there were no AFDC program, families would choose their utility-maximizing hours worked and income combination along the constraint AB. Should they choose to work zero hours, they obtain nonwage income OB. As indicated by the negative slope of AB, increased hours of work lead to increased income. Each point on AB (each feasible combination of income and hours worked) is associated with a level of utility. In line with the rationality assumption, assume that the decision-maker chooses the point on AB that yields maximum utility. Call that point U_M.

A welfare program confronts the decision-maker with a different constraint. For present purposes it is sufficient to discuss this in terms of a simple negative income tax (NIT). Although the same theoretical analysis can be carried out with the AFDC program, the complexity of the AFDC payment formula with all of its state variants leads to cluttered graphs and an unduly long exposition. The hours-leisure constraint under a simple negative income tax is depicted as ACD in Figure 1. Families with zero hours of work receive a welfare benefit (the guarantee) equal to BD. Point C is the breakeven point. Families that choose hours-leisure combinations to the right of this point receive a positive welfare benefit, while those who choose combinations to the left of this point are no longer eligible to receive benefits.

Figure 1



OM = Maximum possible hours worked in a period of time

OB = Nonwage income that is not from the NIT

BD = Guarantee

C = Initial Breakeven

C* = New Breakeven

Consider the level of utility attained by nonworking and working NIT recipients.[5] Once again each combination of income and hours worked on the constraint ACD is associated with a level of utility. Nonworking recipients are located at point D on the diagram. They receive BD in benefits and work zero hours. Let U_{NWR} be their level of utility. Working recipients are located at some point on CD. Let U_{WR} be the maximum level of utility attained on CD. Since people are assumed to be rational, nonworking recipients are presumably at a point of maximal utility at a point D. Thus, for them $U_{NWR} > U_{WR}$ and $U_{NWR} > U_M$. Similarly, for working recipients $U_{WR} > U_{NWR}$ and $U_{WR} > U_M$. Finally, for people who choose not to be recipients, $U_M > U_{WR}$ and $U_M > U_{NWR}$.

Given this, what effect does a tax increase such as that instituted by OBRA have on U_M, U_{NWR}, and U_{WR}? As illustrated by AC*D, a higher tax rate on earnings changes the constraint. Although the guarantee is not affected (it is still BD), the breakeven falls to C*. Since C*D is nearly horizontal, increases in hours worked bring recipients smaller increases in total income than under the lower tax rate program (ACD). Given that the guarantee does not change, the utility attainable by nonworkers (U_{NWR}) is not affected by the higher tax rate. Similarly, since AB does not change, the utility attainable by nonrecipients (U_M) is unaffected by the higher tax rate. The utility attainable by working recipients (U_{WR}) does, however, change. Since at any given level of hours worked, the income on C*D is smaller than the income on CD, the higher tax rate decreases the utility attainable by working recipients. Thus, although the higher tax rate does not affect nonworking recipients or nonrecipients, it makes working recipients worse off.

And that leads to hypotheses on how a tax rate increase affects the kinds of transitions examined by the six studies. The studies essentially analyzed transition probabilities. For example, they analyzed the probability that a pre-OBRA working recipient became a nonworking recipient after OBRA. To derive hypotheses on transition probabilities from the foregoing theory, it is necessary to assume that utility embodies a stochastic component that can change over time. That component may be attributable to changing tastes or family circumstances. Since the theoretical details on this are somewhat complicated, they are relegated to the Appendix. The intuition is, however, quite straightforward: since an increased tax rate reduces U_{WR} and does not affect U_{NWR} and U_M, it should reduce that propensity for people to become (or remain) working recipients and increase their propensity to become (or remain) nonworking recipients or nonrecipients. An increase in the tax rate should then have the following effects, ceteris paribus:

For people who were initially in equilibrium as working recipients,

(1) the probability that they remain working recipients will <u>decrease</u>.

(2) the probability that they become nonworking recipients will <u>increase</u>.

(3) the probability that they leave the program and become nonrecipients will <u>increase</u>.

For people who were initially in equilibrium as nonworking recipients,

(4) the probability that they become working recipients will <u>decrease</u>.

(5) the probability that they remain nonworking recipients will <ins>increase</ins>.

(6) the probability that they leave the program and become nonrecipients will <ins>increase</ins>.

For people who were initially in equilibrium off the program as nonrecipients,

(7) the probability that they become working recipients will <ins>decrease</ins>.

(8) the probability that they become nonworking recipients will <ins>increase</ins>.

(9) the probability that they remain off the program as nonrecipients will <ins>increase</ins>.

One can also work out hypotheses on how the increased tax rate will affect average hours, earnings, AFDC benefits, and total income. In most cases the theory yields ambiguous hypotheses for these variables. For example, since the tax rate increase leads some working recipients to leave the program and become nonrecipients (perhaps with increased hours and earnings) while others become nonworking recipients (with decreased hours and earnings), one cannot predict what will happen to average hours and earnings. Thus, the principal hypotheses focus on welfare and employment status.

Before looking at how data from the studies compare with these hypotheses, it is important to recognize that OBRA may have affected not only tax rates but also guarantees and eligibility requirements. In this regard the following rule changes appear to be particularly salient:

-- establishment of a $1000 asset limit.

-- ineligibility of pregnant women (with no other children) until their sixth month of pregnancy.

-- restriction of benefits to children under 18 (making 18-21-year-old dependents ineligible).

-- inclusion of stepparent incomes in benefit calculations.

-- retrospective budgeting and monthly reporting.

Partial evidence for the importance of these provisions comes from a survey of welfare agencies done by the General Accounting Office (GAO). The survey sought views on which OBRA provisions had the greater impact on caseloads and outlays. Several states ranked one or more of these provisions ahead of the previously discussed earnings-related provisions.[6] Another piece of evidence comes from the study by the Center for the Study of Social Policy.

> In the first 15 months of the new rules, Georgia dropped 7,110 families from the AFDC program because of the new federal policies. ...Approximately 75 percent of these families, or 5,240 families, had their grants eliminated because of a new provision that counted a stepfather's income when calculating money available to needy children. The remaining 25 percent, or 1,870 families, were the working mothers and children from whom our sample was drawn.[7]

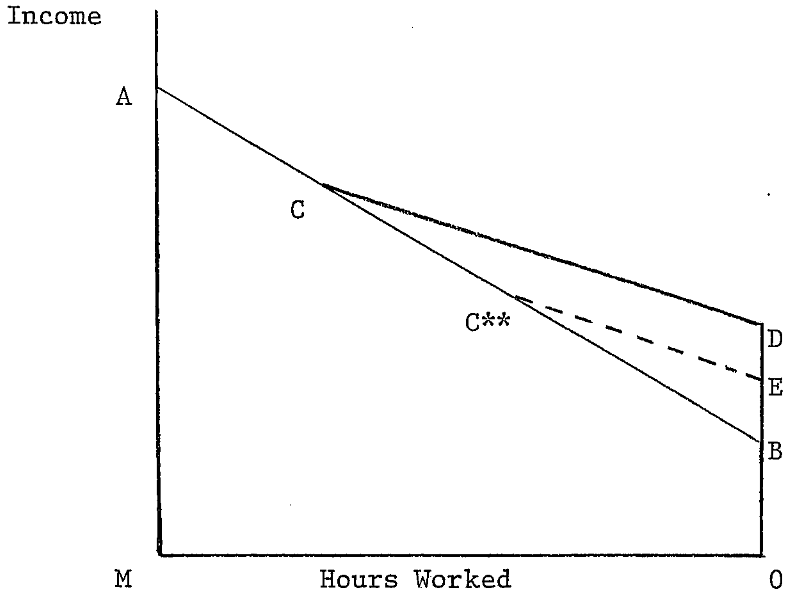
Although incomplete, the evidence makes it difficult to ignore the OBRA provisions that affect nonworking recipients. At least for some recipients these provisions have altered the program in ways not captured by Figure 1.

The effect of most of these provisions can be analyzed in the same way that one analyzes the effect of a change in the guarantee. Consider the new treatment of stepparent income. Stepparent income in excess of certain disregards can now be counted as available to the dependent child. As such this provision can reduce benefits to workers and nonworkers alike. Figure 2 illustrates. In conjunction with the OBRA provisions that increase the tax rate, this provision would shift the income/hours-worked constraint from ACD to AC**E. The guarantee thus changes from BD to BE. Note what that implies about U_M, U_{NWR}, and U_{WR}. Although the utility attainable off the program as a nonrecipient (U_M) is not affected, the utility attainable by working <u>and</u> nonworking recipients (U_{WR} and U_{NWR}) declines.

Note that Figure 2 also applies to OBRA's other provisions. A pre-OBRA recipient family with a 20-year-old in college may experience a reduced guarantee because of OBRA's provision regarding 18-21-year-old children. If this is their only child, the family's guarantee would drop to zero. Thus, although the treatment of 18-21-year-old children is not usually thought of as a determinant of the guarantee, its effects can be analyzed as such. Similarly, OBRA could cause the guarantee to drop to zero for families with countable assets in excess of $1000, or for expectant mothers who are less than six months pregnant. In all of these cases both U_{NWR} and U_{WR} decline.

A slightly different analysis applies to monthly reporting and retrospective budgeting. Monthly reporting means that recipients must fill out income and eligibility reporting forms on a monthly basis. Retrospective budgeting means that agencies compute benefits based on income and circumstances that actually existed in a previous month (rather than use a best estimate of income and circumstances in the current month). Both provisions have the potential to increase the recipient's perceived cost of dealing with the program. As such both provi-

Figure 2



BD = Initial guarantee

BE = New guarantee

C = Initial Breakeven

C**= New Breakeven

sions may reduce utility attainable for working and nonworking recipients.

If OBRA both increased the tax rate and reduced the guarantee, thereby reducing the utility attainable by both working and nonworking recipients, then that significantly alters hypotheses about how OBRA affects behavior. In particular:'

For people who were initially in equilibrium as working recipients,

(1) the probability that they remain working recipients may increase or decrease (the theory yields <u>no prediction</u>).

(2) the probability that they become nonworking recipients may increase or decrease (the theory yields <u>no prediction</u>).

(3) the probability that they leave the program and become nonrecipients will <u>increase</u>.

For people who were initially in equilibrium as nonworking recipients,

(4) the probability that they become working recipients may increase or decrease (the theory yields <u>no prediction</u>).

(5) the probability that they remain nonworking recipients may increase or decrease (the theory yields <u>no prediction</u>).

(6) the probability that they leave the program and become nonrecipients will <u>increase</u>.

For people who were initially in equilibrium off the program as nonrecipients,

(7) the probability that they become working recipients may increase or decrease (the theory yields <u>no prediction</u>).

(8) the probability that they become nonworking recipients may increase or decrease (the theory yields no prediction).

(9) the probability that they remain nonrecipients will increase.

Note how these hypotheses differ from those for a simple increase in tax rates. Consider people who were initially working recipients. If OBRA only increased the tax rate on earnings, then there is good reason to anticipate an increased tendency for working recipients to drop out of the labor force and become nonworking recipients. If, however, OBRA not only raised tax rates but also effectively reduced the guarantee, that prediction is in doubt. At the same time that the tax rate increase discourages work, the decreased guarantee encourages work. The theory thus yields an ambiguous prediction in this case. Similarly, consider people who were initially nonworking recipients. If OBRA only increased the tax rate, we would expect an increased tendency for them to remain nonworking recipients. If, however, OBRA altered both guarantee and tax rate, then the prediction becomes ambiguous. To conclude, one's prediction about OBRA's effects depends crucially upon one's assessment of what OBRA actually did to AFDC guarantees.

Which set of predictions is correct? We know that OBRA raised tax rates and have reason to think that at least for some people it reduced the guarantee. Indeed, we have reason to think that the guarantee dropped to zero for some people, implying that because of OBRA they were no longer eligible for the program. We do not, however, know how pervasive such guarantee changes were. Perhaps so few people were affected that the simple tax rate predictions are appropriate. It is interesting to keep both sets of predictions in mind when looking at results from the six studies.

II. THE STUDIES

There are marked similarities in the design of the six studies. They share a goal as well as data collection strategy. All drew samples of pre-OBRA AFDC recipients and followed them over several months. Using case records and personal interviews, they collected data on employment, welfare status, and income before and after OBRA's implementation. Although there are subtle differences between the studies, especially with regard to demographic groups excluded from the samples, this overall consistency of design greatly simplifies comparison of their results.

In part due to this consistent design, however, the studies have certain problems in common. Perhaps most important, it is difficult to distinguish OBRA's effect from other factors that may alter recipient behavior. We need to compare what actually happened to a counterfactual. For present purposes the ideal counterfactual is the world that would have been if welfare programs retained their pre-OBRA structure. To illustrate, suppose we find that 60 percent of the recipients in the OBRA cohort did not receive benefits a year later. That does not tell us very much about OBRA's effect; even in the absence of OBRA, during a year some recipients would become nonrecipients. Such "before" and "after" comparisons require information on what would have happened in the absence of OBRA. Of course, an ideal design would randomly select a control group of people from the OBRA cohort and shield them from the OBRA treatment. But that was not feasible.

Four of the studies address this problem by tracking a comparison cohort--a sample of people who were AFDC recipients a year prior to OBRA.

The idea is to isolate OBRA's effects by comparing the OBRA cohort's behavior with that of the earlier cohort. The difficulty with this approach is that the behavior of the two cohorts may differ for reasons unrelated to OBRA. Of particular concern are changes in macro-economic conditions. The OBRA cohorts were usually tracked between the fall of 1981 and the fall of 1982, a period in which unemployment rates rose from 7.5 to 10.1 percent. The corresponding tracking period for the comparison cohort ran from fall 1980 to fall 1981, a period in which unemployment rates remained stable at around 7.5 percent. This creates a problem. Due to this change in macro-economic conditions one would expect behavior to differ between the two cohorts even in the absence of OBRA. Additional factors such as long-run trends in the AFDC recipient behavior could exacerbate the problem. Thus, although these earlier comparison cohorts are helpful in providing a sense for what is "normal," they are not ideal.

Another problem common to the six studies concerns data. The studies are based on two kinds of data: caseload records and personal interviews. The principal advantage to caseload records is that they are reasonably accurate and it is relatively easy to draw a complete sample (there is no problem with nonresponse). The disadvantage is that they lack information on what happened to closed cases. That means they provide no information on income or employment of people who have left the rolls. Moreover, when case records are maintained at the county or city level, a closing may simply mean that the family is no longer receiving AFDC <u>in that jurisdiction</u>. The family may have moved to some other county or city where it receives AFDC. This is less of a problem when states maintain case records on computer files. It is not clear, however, whether any study other than the Institute for Research on Poverty study (see below) was able to tap that kind of data source.

Interview data can ideally address such problems by providing data on what happened to all former AFDC recipients including nonrecipients. Here, however, there is a problem of nonresponse. Response rates were between 73 percent and 88 percent in the General Accounting Office (GAO) study, 70 percent in the Center for Study of Social Policy (CSSP) study, and at or below 50 percent in the other four studies. The resulting samples are probably not representative.[8] It is, for example, conceivable that some people who were especially hard hit by OBRA changed their place of residence and thereby became less likely to be survey respondents. The point is not, of course, that these data should be dismissed. A perfect data set does not exist. Rather the point is that a cautious interpretation is often requisite.

A final problem shared by the six studies is that they obtained no information on pre-OBRA nonrecipients. As noted above, OBRA may have affected the behavior of nonrecipients. Moffitt (1984) argues that because they contain no information on pre-OBRA nonrecipients, these studies cannot be used to address some types of questions. For example, although they can be used to analyze OBRA's effect on transitions from AFDC recipiency to nonrecipiency, they cannot be used to assess OBRA's effect on total labor supply.

Such problems should not obscure the positive aspects of the studies. They provide timely data on an important government policy. Through their consistent design they establish a solid foundation for answering a specific set of questions about that policy. A detailed description of each study helps to indicate their many positive features.

The Research Triangle Institute Study

The Research Triangle Institute (RTI) performed the only study of OBRA that is based on a national sample. As documented in RTI (1983), the study included both a survey of caseload records and telephone interviews. The researchers drew two caseload record samples from the September 1980 and September 1981 national caseload and tracked both over 12 months. The 1981 sample was thus the OBRA cohort and the 1980 sample the comparison cohort. Sample sizes were 610 in 1980 and 1013 in 1981. The only groups excluded from their samples were AFDC-UP (for two-parent families in which the breadwinner is unemployed) and Foster Care cases. Their samples thus include both workers and nonworkers.

The telephone survey was evidently undertaken in February-March 1982 and only covered September 1981 earners whose cases had been discontinued by OBRA and were not reopened a few months later. Survey questions focused on the group's income and employment status. Out of the 360 targeted cases, a sample of 152 were obtained, yielding a response rate of 42.2 percent.

The General Accounting Office Study

There are two major differences between the General Accounting Office's study (GAO, 1984) and the RTI study. First, the GAO did not attempt a national survey but rather collected data from five large cities: Boston, Dallas, Memphis, Milwaukee, and Syracuse. Second, the GAO study not only includes a case record study and telephone interviews, but also a time series analysis of national caseloads and a survey of opinions of employees in state welfare agencies regarding OBRA's impact.

Like RTI, the GAO collected caseload record data on both an OBRA cohort and a comparison cohort. The OBRA cohort was drawn from caseload records one month prior to OBRA implementation (the actual data differs across cities); the comparison cohort comes from caseload records 13 months prior to OBRA. Sample sizes were quite adequate, ranging between 600 and 1600 observations (workers plus nonworkers) in each city. Because these sample sizes are equivalent to those in the other studies reviewed here, the GAO study will frequently be treated as five distinct city studies. Again like RTI, the samples included workers and nonworkers and excluded AFDC-UP and Foster Care cases.

The GAO conducted a telephone interview in August-December 1983. The sample was restricted to people who had left AFDC during OBRA implementation. Unlike RTI, people who returned to the rolls were included in the sample. Sample sizes ranged from 127 to 147 with response rates between 73 percent and 88 percent.

The Institute for Research on Poverty Study

The study by the Institute for Research on Poverty (IRP) was carried out in Wisconsin. It is documented in two preliminary reports: Cole, Danziger, Danziger, and Piliavin (1983) and Feaster, Gottschalk, and Jakubson (1984). Like the RTI and GAO studies, the IRP data take the form of both caseload records and personal interviews. ALso like RTI and GAO, the caseload record data include both an OBRA and a comparison cohort of workers and nonworkers. Both cohorts contain slightly over 1000 observations. It is not clear what restrictions were placed on these data beyond exclusion of AFDC-UP and Foster Care cases.

The IRP telephone interview survey focused on a sample of women who had been receiving AFDC and working in December 1981 and whose benefits were terminated because of OBRA. Most of the data pertain to income, employment, and welfare status in February 1983--about one year after implementation of OBRA. Like the GAO study, the sample includes people who remained on or returned to the rolls. With 1175 completed interviews, the survey response rate was 33 percent.

The Minnesota Study

The Minnesota study is documented in Moscovice and Craig (1983, 1984). It is based on a cohort of 587 AFDC recipients drawn fromthe case records of Hennepin County, Minnesota. (Minneapolis is the county seat of Hennepin County.) No data were collected on either a comparison cohort or on nonworkers. Although some of the data come from caseload records, most are from telephone interviews taken one month before and five months and eleven months after OBRA implementation. Because many potential recipients did not return a consent form allowing the county to give their name to the study team, the survey response rate was 28 percent.

The Center for the Study of Social Policy Study

A study of OBRA's effects in Georgia is presented by the Center for the Study of Social Policy (CSSP) (1984). The data came from a random sample of families with working mothers prior to OBRA who were on a list of cases that were closed by OBRA in 27 Georgia counties. The data were collected through face-to-face interviews conducted one to two years after termination. Data were not then obtained on either a comparison cohort or on nonworkers. With 207 completed interviews, the survey response rate was 70 percent.

The New York City Studies

Two studies analyze OBRA's effect in New York City. The first was performed by the New York City Human Resource Administration (HRC) and is documented in Krauskopf and Taylor (1983). It is based on an OBRA cohort and a comparison cohort drawn from caseload records. Only caseload record data were used; no personal interviews were conducted. The OBRA cohort consists of two samples, one drawn from the recipient population with benefits terminated by the 150 percent rule, and one drawn from the population with benefits either reduced or terminated by elimination of the $30 and one-third rule. The comparison cohort was drawn from the population of recipients with earnings on the New York City rolls nine months prior to OBRA. The OBRA sample contained 1100 families and the comparison sample 500 families. All samples were stratified so as to cover sample families with children under 6; all were tracked over one year. Although nonworkers are excluded from the sample, AFDC-UP families are apparently included.

The second New York City study is described in Ginsberg and Mesnikoff (1984). These researchers essentially conducted face-to-face interviews with a sample drawn from the above-noted HRC sample. Their sample differs from the HRC sample in that it excludes AFDC-UP and other two-parent families. Moreover, no interviews were conducted with a comparison cohort. They completed 290 interviews approximately one year after OBRA implementation, yielding a response rate of about 50 percent. Unfortunately, most of their results are presented in unweighted form. Given the stratification of the HRC sample, results may be biased by the overrepresentation of families with children under six and of families affected by the 150 percent rule.

III. WHAT HAVE WE LEARNED?

What Effects Did OBRA Have on AFDC Participation?

There is not much question that OBRA succeeded in reducing the number of AFDC recipients. Intriguing issues remain, however, with regard to the kind of recipients who were affected, the impact of various OBRA provisions, and OBRA's long-term impact. This section focuses on such issues.

AFDC caseload data provide a rough indication of OBRA's initial impact. OBRA was passed by Congress in August 1981 and implementation began in October. Although the majority of states had introduced most of OBRA's provisions by May 1982, some provisions were not fully implemented until 1983.[9] As indicated by the following data, AFDC caseloads dropped significantly during this period.[10]

	# Cases	# Recipients
September 1981	3,880,253	11,141,287
January 1982	3,574,361	10,477,355
June 1982	3,488,422	10,182,551
September 1982	3,535,815	10,338,149
January 1983	3,637,386	10,636,585

Of course, not all of this change can be attributed to OBRA. Other factors like the recession undoubtedly also played a role. The GAO researchers sought to address this by estimating a time series model with data on national caseloads for January 1973 through June 1983. They compared predicted and actual caseloads for the period after implementation and concluded that "in the short-term OBRA decreased the caseload by 493,000 cases, compared to what the caseload would have been in the absence of OBRA."[11] (The short-term evidently refers to the period September 1981 through June 1982.) The GAO goes on to caution that their model suggests a smaller long-term effect.

Given OBRA's design, working AFDC recipients were likely to be affected by and participate in these caseload reductions. All of the studies reviewed here provide detailed information on how the AFDC status of these working recipients changed after OBRA, and Table 1 summarizes their results. Before launching into a comparison of the numbers, a few caveats are in order. As indicated in the table notes, the types of people included in the samples differ across studies. For example, the RTI sample is drawn from pre-OBRA cases with earnings while the New York City sample is drawn from pre-OBRA cases with earnings that were explicitly affected by OBRA. The two may not be the same. Some pre-OBRA

Table 1

Percentage of OBRA-Cohort Working AFDC Recipients Who Were on or off AFDC at a Point in Time Approximately One Year after Implementation of OBRA

Study	Percentage on AFDC	Percentage off AFDC
RTI[a]	44.8	55.2
GAO[b]		
Boston	25.2	74.8
Dallas	29.4	70.6
Memphis	34.4	65.6
Milwaukee	43.3	56.7
Syracuse	40.0	60.0
IRP		
Cole et al.[c]	37.4	62.6
Sample 2[d]	55.2	44.8
Minnesota[e]	26.2	73.8
New York City		
K and T[f]	38.2	61.8
G and M[g]	36.6	63.4

[a]From RTI (1983), Table 3.1, p. 3-8. Based on a national sample of caseload records for AFDC recipients with earnings in September 1981.

[b]From GAO (1984), Table 12, p. 31. Based on five city samples of caseload records for AFDC recipients with earnings one month prior to OBRA implementation.

[c]Derived from Cole et al. (1983), Table 3. Based on telephone interviews with a sample of Wisconsin adult female AFDC recipients with earnings one month prior to OBRA, who had benefits reduced or terminated by OBRA.

[d]From telephone conversation with Daniel Feaster, September 11, 1984. Based on a sample of Wisconsin caseload records for AFDC recipients with earnings one month prior to OBRA.

[e]From Moscovice and Craig (1983), Figure 1, p. 10. Based on telephone interviews with a sample of Hennepin County AFDC recipients with earnings one month before implementation of OBRA.

[f]Derived from Krauskopf and Taylor (1983), Table 12, p. 23. Based on a sample of caseload records for AFDC recipients with benefits either reduced or terminated by the 150 percent rule or by elimination of the $30 and 1/3 disregard.

[g]Derived from Ginsberg and Mesnikoff (1984), Table 17, p. 44. Based on telephone interviews with a sample of New York City single-parent families drawn from the caseload records described in (f).

recipients with earnings may have left the program before OBRA's changes (e.g., the change in the $30 and 1/3 rule) took effect. Another aspect of this difference in samples concerns "tracking periods." To illustrate, in the Minnesota study the pre-OBRA data come from January 1982, OBRA implementation began in February 1982, and the post-OBRA data come from Januay 1983. In the RTI study the pre-OBRA data come from September 1981, OBRA implementation began sometime between October 1981 and February 1982 (depending on state of residence), and the post-OBRA data come from September 1982. Thus, the data come from slightly different points in time. Finally, the different studies were done in different locations. That further complicates comparisons because the characteristics of the AFDC program and its recipients vary across states and cities. Indeed, even when the study design is held constant, as it is in the GAO study, there remains significant variation in results across sites.

In light of these caveats, the different studies yield remarkably similar results. With the exception of IRP's second sample, between 55 and 75 percent of the pre-OBRA working recipients were no longer AFDC recipients about one year after OBRA began changing the AFDC program.[12]

Another way to assess OBRA's effect on working recipients is to compare the Table 1 data to data on a cohort from a period prior to OBRA. The GAO, RTI, IRP, and the New York City study by Krauskopf and Taylor used 1980 caseload data for this purpose, and the left side of Table 2 presents the comparison. All four studies found that working recipients in the OBRA cohort were less likely than working recipients in the pre-OBRA cohort to be on AFDC after a year. In the RTI study, for example, while 72.4 percent of the workers in the 1980 cohort were still recipients after a year, the corresponding number for the 1981 OBRA cohort is 44.8 percent--a statistically significant difference of 27.6 percentage points. There is then solid evidence that part of the 1981-1982 decline in AFDC caseloads was due to OBRA's effect on working recipients.

Whereas all of the studies analyze OBRA's effect on working recipients, only the GAO, IRP, and RTI studies consider nonworking recipients. The right side of Table 2 presents these results. Note the difference between the OBRA cohort and the comparison cohort. In all but one case (GAO, Syracuse) the nonworkers in the OBRA cohort were less likely to be AFDC recipients after a year. Since the recession should have had the opposite effect, the Table 2 results are consistent with the hypothesis that nonworkers left AFDC because of OBRA.

This interpretation raises a statistical issue. Only in the case of the GAO's Milwaukee study can one reject the null hypothesis of no difference between the OBRA and comparison cohorts at a .05 level. That is, however, a test of whether one observation looked at in isolation is consistent with the null. We do not have a test of whether a null hypothesis of no difference can be rejected for all seven observations taken jointly.[13] Since in six out of seven cases, OBRA cohort nonworkers were less likely to remain recipients than comparison cohort nonworkers, it is probable that the latter joint test would reject the null. To conclude, at least in the opinion of this writer, Table 2 provides evidence (albeit not strong evidence) that part of the 1981-1982 decline in AFDC caseloads was due to OBRA's effect on nonworking recipients.

It is interesting to speculate about the quantitative importance of OBRA's effect on nonworkers. Suppose that prior to OBRA the AFDC system

Table 2

Percentage of Working and Nonworking AFDC Recipients in the OBRA and Comparison Cohorts Who Were on or off AFDC at a Point in Time Approximately One Year after the Cohort Starting Date

	Percentage of Working Recipients Who Were, at Year's End,		Percentage of Nonworking Recipients Who Were, at Year's End,	
Study	On AFDC	Off AFDC	On AFDC	Off AFDC
RTI[a]				
OBRA	44.8	55.2	76.3	23.7
COMP	72.4	27.6	83.4	16.6
GAO[b]				
Boston				
OBRA	25.2	74.8	78.5	21.5
COMP	75.9	24.1	82.6	17.4
Dallas				
OBRA	29.4	70.6	61.3	38.7
COMP	59.4	40.6	67.1	32.9
Memphis				
OBRA	34.4	65.6	72.6	27.4
COMP	62.1	37.9	78.1	21.9
Milwaukee				
OBRA	43.3	56.7	78.0	22.0
COMP	80.2	19.8	83.0	17.0
Syracuse				
OBRA	40.0	60.0	77.2	22.8
COMP	67.7	32.3	73.8	26.2
IRP[c]				
OBRA	55.2	44.8	83.9	16.1
COMP	86.3	13.7	88.2	11.8
New York City[d]				
OBRA	38.2	61.8	--	--
COMP	62.9	37.1	--	--

[a]RTI (1983) Table 3.1, p. 3-8.
[b]GAO (1984) Table 12, p. 31.
[c]Telephone conversation with Daniel Feaster, Sept. 11, 1984.
[d]Krauskopf and Taylor (1983) Table 12, p. 23.

was in a steady state with numbers of entries equal to numbers of exits. Assume that OBRA then increased exit rates of nonworkers such that the percentage not on AFDC after a year increased by five percentage points--a number that is roughly consistent with the Table 2 data. Since there were approximately 3.43 million nonworker cases on AFDC in September 1981,[14] that would reduce the caseload by 171,000 cases (3.46 x .05 = .171) after one year. Of course a similar exercise can be performed for worker cases. If OBRA had a thirty percentage point effect on workers--again a number that is roughly consistent with the data--the caseload would fall by 135,000 cases (.45 x .3 = .135). This suggests that in terms of numbers of human beings affected, OBRA's impact on pre-OBRA nonworkers may have been greater than its impact on workers.

It is also interesting to return at this point to a question raised in Section I: Did OBRA simply increase tax rates or did it also decrease guarantees? Because they are consistent with either case, the data in Table 2 shed no light on this issue. If OBRA simply raised tax rates, then as discussed in Section I we would expect both working and nonworking recipients to move off the program. If OBRA not only raised tax rates but also reduced guarantees, we get the same prediction. Since the data indicate that there was in fact an increased probability of moving off the program for both working and nonworking recipients, the data support either case. Only with additional data--such as the subsequent data on labor force participation--can this issue be addressed.

The discussion has thus far focused on one year of data. What about the long-term? Do these near-term reductions underestimate or overestimate OBRA's long-term effects? Although such questions are of fundamental importance, they cannot be resolved without additional data. There is a logical basis for predicting either an under- or overestimate. On the one hand, the near-term reduction may exceed long-term reductions because some OBRA terminees do not attain equilibrium during the tracking period. For example, a woman with a good job match (she likes the job and the employer likes her work), may initially respond to OBRA termination by keeping the job and receiving no AFDC. In the event of a layoff, however, she may return to AFDC and decide against a prolonged search for an equivalent job. Although her equilibrium state in the post-OBRA system is "on AFDC," it may take her more than one year to reach that state. By this argument the immediate caseload reductions caused by OBRA will be more dramatic than the ultimate reductions. On the other hand, the near-term reduction may underestimate the long-term reduction because OBRA deters entry. OBRA may reduce the number of families that are potentially eligible for AFDC because it tends to exclude families with stepparent income, 18-21 year-old children, and assets above $1000. Other things equal, that will reduce numbers of new entrants per unit of time. Moreover, some people who would be willing to enter a program with low tax rates may not enter a program with higher tax rates. If OBRA does permanently reduce numbers of AFDC entrants per unit of time, ceteris paribus, then one year of data could underestimate OBRA's ultimate impact on caseloads.

To conclude, the six OBRA studies indicate that OBRA significantly reduced AFDC participation and caseloads. Although the evidence on this is strongest for recipients who were working shortly before implementation of OBRA, nonworkers were apparently also affected. This conclusion is limited to the near-term. The long-term effect of OBRA on participation and caseloads may be larger than or smaller than the near-term effect.

What Effect Did OBRA Have on Labor Supply Behavior?

All six studies collected data on aspects of labor supply. Most of the data concern changes in employment status for working recipients, and that topic will be discussed first. This is followed by an assessment of similar data for nonworking recipients and a second look at whether OBRA changed guarantees as well as tax rates. The discussion concludes with a summary of information on earnings and hours.

Table 3 draws together data on the post-OBRA employment status of pre-OBRA working recipients. The table is similar to Table 1. The two tables differ in that Table 3 breaks the "on AFDC" and "off AFDC" categories into "working" and "not working" subcategories. There are also more gaps in Table 3. This is because the data on the employment status of nonrecipients must come from interviews, and some studies either did not collect or chose not to present such data. In these cases the numbers in parentheses are the percentage of total "off AFDC" observations. Since Tables 1 and 3 are based on the same data, all the caveats for Table 1 apply.

Once again, despite differences in data, the studies yield remarkably similar results. There is no evidence of a mass exodus from the labor market. Although a fraction of the pre-OBRA workers were on AFDC and not working a year later, that fraction never exceeds 27 percent. Indeed,

Table 3

Percentage of OBRA-Cohort Working AFDC Recipients Who Were in Four Employment and Recipiency Categories at a Point in Time Approximately One Year after Implementation of OBRA

	On AFDC		Off AFDC		
Study	Not Working	Working	Not Working	Working	Total
RTI[a]	17.8	27.0	-- (55.2) --		100.0
GAO[b]					
Boston	14.7	10.5	-- (74.8) --		100.0
Dallas	22.8	6.6	-- (70.6) --		100.0
Memphis	21.9	12.5	-- (65.6) --		100.0
Milwaukee	26.7	16.6	-- (56.7) --		100.0
Syracuse	18.6	21.4	-- (60.0) --		100.0
IRP					
Cole et al.[c]	17.1	20.3	5.4	57.2	100.0
Sample 2[d]	24.1	31.0	-- (44.8) --		100.0
Minnesota[e]	12.3	13.9	5.0	68.8	100.0
New York City					
K and T[f]	15.8	22.4	-- (61.8) --		100.0
G and M[g]	13.0	23.6	6.3	57.1	100.0

[a]RTI (1983) Table 3.1, p. 3-8.
[b]GAO (1984) Table 12, p. 31.
[c]Cole et al. (1983) Table 3.
[d]Telephone conversation with Daniel Feaster, Sept. 11, 1984.
[e]Moscovice and Craig (1983) Figure 1, p. 10.
[f]Krauskopf and Taylor (1983) Table 12, p. 23.
[g]Ginsberg and Mesnikoff (1984) Table 17, p. 44.

despite a severe economic downturn, the three studies with information on employment status of post-OBRA recipients indicate that around 80 percent of the pre-OBRA workers were working a year later. One has the impression that these pre-OBRA workers had a strong attachment to the labor force.

This impression is reinforced by four studies that focus on the employment status of "terminees"--pre-OBRA workers whose cases were terminated by OBRA.[15] Their results indicate that terminees were even more likely to work and remain off AFDC than the pre-OBRA workers in Table 3. For example, in the data for Hennepin County, Minnesota,[16] only 3.2 percent of the terminees were on AFDC and not working a year after OBRA. That compares with 12.3 percent for the full Hennepin County OBRA cohort sample. Moreover, whereas 82.7 percent of this OBRA cohort worked a year later, fully 91.9 percent of the terminees worked.

Although Table 3 indicates that OBRA did not precipitate a mass exodus from the labor force, it leaves unanswered the question of whether labor supply behavior was somehow altered by OBRA. Once again, the RTI, GAO, IRP, and the Krauskopf and Taylor New York study give a perspective on this by presenting data on a 1980 comparison cohort. Table 4 presents these data for working recipients. There are two important pieces of information here. First, OBRA-cohort working recipients were much less likely to remain working recipients than their counterparts in the comparison cohort. In the RTI data, for example, 27 percent of the OBRA-cohort working recipients were still working recipients after a year versus 54 percent in the comparison cohort. The evidence thus indicates that OBRA reduced the probability that a working recipient would remain a

Table 4

Percentage of Working AFDC Recipients in the OBRA and Comparison Cohorts Who Were in Three Employment and Recipiency Categories at a Point in Time Approximately One Year after the Cohort Starting Date

Study	On AFDC and: Not Working	On AFDC and: Working	Off AFDC	Total
RTI[a]				
OBRA	17.8	27.0	55.2	100.0
COMP	18.2	54.2	27.6	100.0
GAO[b]				
Boston				
OBRA	14.7	10.5	74.8	100.0
COMP	18.4	57.6	24.1	100.0
Dallas				
OBRA	22.8	6.6	70.6	100.0
COMP	27.6	31.8	40.6	100.0
Memphis				
OBRA	21.9	12.5	65.6	100.0
COMP	21.4	40.7	37.9	100.0
Milwaukee				
OBRA	26.7	16.6	56.7	100.0
COMP	26.0	54.2	19.8	100.0
Syracuse				
OBRA	18.6	21.4	60.0	100.0
COMP	17.1	50.6	32.3	100.0
IRP[c]				
OBRA	24.1	31.0	44.8	100.0
COMP	24.8	61.5	13.7	100.0
New York City[d]				
OBRA	15.8	22.4	61.8	100.0
COMP	17.9	45.0	37.1	100.0

[a]RTI (1983) Table 3.1, p. 3-8.
[b]GAO (1984) Table 12, p. 31.
[c]Telephone conversation with Daniel Feaster, Sept. 11, 1984.
[d]Krauskopf and Taylor (1983), Table 12, p. 23.

working recipient. Second, the evidence does not indicate a tendency for OBRA-cohort working recipients to become nonworking recipients. In some cases the fraction of OBRA-cohort nonworking recipients exceeds the fraction of comparison-cohort nonworking recipients, and in some cases it does not. And when it does not, the difference is often quite large (see Boston or Dallas). Such numbers are particularly surprising given that even in the absence of OBRA, the 1981 recession should have increased numbers of nonworking recipients in the OBRA cohort. Table 4 thus indicates that for people who were initially working recipients, OBRA decreased the propensity to remain working recipients without increasing the propensity to become nonworking recipients.

Before trying to make sense of this, it is important to look at Table 5, which presents parallel figures for nonworking recipients. Again, the table yields two important pieces of information. First, OBRA-cohort nonworking recipients were less likely to become working recipients. For example, the RTI data indicate that 3.2 percent of the OBRA cohort and 3.7 percent of the comparison cohort were working recipients after a year. That is not a large difference, but all of the other studies say the same thing: the OBRA-cohort fraction is smaller than the comparison-cohort fraction. This may not, however, be due to OBRA but rather to the 1981 recession. Second, the OBRA-cohort nonworking recipients appear to be somewhat less likely to remain nonworking recipients. In six out of seven cases, the "on AFDC and not working" fraction is smaller for the OBRA cohort. Cautiously interpreted, the propensity for people to remain nonworking recipients did not increase under OBRA. To conclude, Table 5 suggests that for people who were ini-

Table 5

Percentage of <u>Nonworking</u> AFDC Recipients in the OBRA and Comparison Cohorts Who Were in Three Employment and Recipiency Categories at a Point in Time Approximately One Year after the Cohort Starting Date

Study	On AFDC and: Not Working	On AFDC and: Working	Off AFDC	Total
RTI[a]				
OBRA	73.1	3.2	23.7	100.0
COMP	79.8	3.7	16.6	100.0
GAO[b]				
Boston				
OBRA	75.5	3.0	21.5	100.0
COMP	76.9	5.7	17.4	100.0
Dallas				
OBRA	60.9	.4	38.7	100.0
COMP	65.1	2.0	32.9	100.0
Memphis				
OBRA	72.2	.4	27.4	100.0
COMP	73.8	4.2	21.9	100.0
Milwaukee				
OBRA	76.2	1.8	22.0	100.0
COMP	77.4	5.6	17.0	100.0
Syracuse				
OBRA	74.5	2.6	22.8	100.0
COMP	69.6	4.2	26.2	100.0
IRP[c]				
OBRA	77.1	6.8	16.1	100.0
COMP	77.7	10.5	11.8	100.0

[a]RTI (1983) Table 3.1, p. 3-8.
[b]GAO (1984) Table 12, p. 31.
[c]Telephone conversation with Daniel Feaster, Sept. 11, 1984.

tially nonworking recipients, OBRA reduced the propensity to become working recipients without increasing the propensity to remain nonworking recipients.

One way to make sense of these results is to again ask whether OBRA not only raised tax rates but also reduced guarantees. It is useful to begin by organizing the results in the same way that the hypotheses were organized in Section I. Thus the above evidence indicates that:

For people who were initially working recipients,

(1) the probability that they remained working recipients decreased.

(2) the probability that they became nonworking recipients did not increase.

(3) the probability that they left the program and became nonrecipients increased.

For people who were initially nonworking recipients,

(4) the probability that they became working recipients decreased.

(5) the probability that they remained nonworking recipients did not increase.

(6) the probability that they left the program and became nonrecipients increased.

If OBRA simply increased tax rates, then these are surprising results. Although results (1), (3), (4), and (6) accord with expectations, (2) and (5) do not. If OBRA simply raised tax rates, there should have been evidence of an increased probability that working recipients became nonworking recipients as well as an increased probability that nonworking recipients remained nonworking recipients. This is because, as argued in Section I, an increased tax rate reduces the utility attainable by working recipients relative to nonworking recipients. On the other hand, if OBRA both increased tax rates and reduced guarantees, the results accord with expectations. By this interpretation, although the higher tax rate may induce people to become or remain nonworking recipients, the reduced guarantee discourages such behavior. Results (2) and (5) simply indicate that guarantee effects were large enough to cancel out any tax rate effects. Since results (1), (3), (4), and (6) also accord with this interpretation, the data are most consistent with the view that OBRA both increased effective tax rates and reduced effective guarantees. There is then some reason to believe that OBRA's new rules regarding stepparent income, assets, 18-21-year-olds, pregnant women, retrospective budgeting, and monthly reporting had an important impact.[17]

This is a good time to sound a note of caution. First, the above interpretation is based on a comparison between an OBRA cohort and an earlier cohort. As noted in Section II, the earlier cohort is not an ideal control group and may introduce bias. Second, the discussion of results does not address the issue of statistical significance. The appropriate significance test would be a joint test on all of the studies taken together, and none of the authors (including this one) have attempted that. Finally, other interpretations of the results are conceivable. This interpretation is simply the one that the author finds most plausible.

Another way to analyze labor supply issues is in terms of hours worked and earnings. Here, however, the data are problematic. Although most of the studies collected data on hours and earnings in the OBRA cohort, they were not able to obtain similar data for a comparison cohort. Moreover, the studies present their data in different ways, thereby complicating across-study comparisons. Table 6 presents the available data. The table is split into two panels. The top panel pertains to people in the OBRA cohort who were working recipients before OBRA; the bottom panel pertains to the subset of those in the top panel whose cases were terminated by OBRA. Note the asterisks in the table. Entries with an asterisk indicate average hours or earnings for people who worked both before and one year after OBRA; entries without asterisks indicate averages for people who worked before but not necessarily one year after OBRA. In the latter case averages were computed after assigning zero hours and earnings to the nonworkers. Such differences in measurement obviously complicate an assessment of what the studies tell us about hours worked and earnings.

Although Table 6 does not yield firm conclusions, two tentative conclusions are at least defensible. First, average hours worked per week did not change significantly for pre-OBRA working recipients who worked one year after OBRA. When people who did not work after OBRA are included in the averages (with zero hours), the evidence indicates a slight decline in average hours. Second, average earnings increased for pre-OBRA working recipients who worked one year after OBRA. (Since hours did not change appreciably for this group, the increased earnings are primarily due to increased wages.)[18] The evidence is mixed when people who did not work after OBRA are included in the averages. Some studies indicate a slight increase (e.g., IRP) and others a slight decrease (e.g., the New York City study by Ginsberg and Mesnikoff).

Although intriguing, these results do not speak to the issue of OBRA's effect on hours or earnings. This is because there are no data on

Table 6

Hours and Earnings a Few Weeks before and about One Year after Implementation of OBRA
(hours are hours per week; earnings are current dollar earnings per month)

	Hours		Earnings	
Study	One Month before OBRA	About One Year after OBRA	One Month before OBRA	About One Year after OBRA
1. All Pre-OBRA Working Recipients				
IRP[a]	32.2	32.2*	$522	$559
Minnesota[b]	29.0	27.4	643	650
New York City[c]	33.9*	34.1*	620	553
2. Pre-OBRA Working Recipients with Cases Terminated by OBRA				
IRP[d]	37.3*	37.0*	702	738
New York City[e]	36.1*	36.1*	682	622
GAO[f]				
Boston	35.3*	37.5*	700*	933*
Dallas	34.4*	34.4*	563*	650*
Memphis	27.9*	29.3*	458*	529*
Milwaukee	37.8*	38.9*	789*	1000*
Syracuse	37.2*	38.4*	678*	846*

*Average computed for people who worked both before and one year after implementation of OBRA.

[a]Hours derived from Cole et al. (1983), Tables 4 and 5. Earnings from same source, Table 8.

[b]Hours derived from Moscovice and Craig (1983), p. 17. Earnings from same source, p. 21.

[c]Hours derived from Ginsberg and Mesnikoff (1984), Table 22, p. 51. Earnings from same source, Table 42, p. 82.

[d]Hours derived from Cole et al. (1984), Table 4. Earnings from same source, Table 8.

[e]Same as c.

[f]Hours from GAO (1984), Table 19, p. 47. Earnings from same source, Appendix III.

a comparison cohort. There are no data with which to assess whether in the absence of OBRA, these samples would have experienced a similar change in hours and earnings. Thus, data on work status, such as in Tables 4 and 5, are the principal source of information on how OBRA affected labor supply of pre-OBRA AFDC recipients.

To conclude, there exist reasonably good data on the employment effects of OBRA. These data indicate that OBRA neither increased the propensity for working recipients to become nonworking recipients nor increased the propensity for nonworking recipients to remain nonworking recipients. Such results are consistent with the view that OBRA not ony raised effective tax rates but also reduced effective guarantees.

What Effect did OBRA Have on Economic Well-Being?

The studies reviewed here take an appropriately broad view of the determinants of economic well-being. They present data on money and in-kind income as well as aspects of consumption behavior. The most complete data were obtained for AFDC, Food Stamps, and earned income, and they are discussed first. This is followed by a summary of results on child care and health care, two areas in which particularly worrisome trends emerge.

Most of the results on economic well-being pertain to people who were working AFDC recipients a few weeks before implementation of OBRA. There is almost no information on nonworkers. Moreover, the data usually come from interviews taken about a year after OBRA implementation.[19] When compared with similar data from before OBRA--data that usually come from caseload records or from the same post-OBRA interview--it is possible to assess changes in variables that affect well-being over time. Of course, we cannot be sure that the whole difference between these before and after snapshots is due to OBRA; part of it may be due to the recession and part may have occurred even in the absence of OBRA or the recession. Moreover, since, with one unimportant exception, parallel data were not collected on a comparison cohort, our ignorance is particularly acute. Caution is then required in interpreting these data.

Income. Table 7 summarizes data on changes in AFDC, Food Stamp, earned, other, and "total" income. (It is necessary to put "total" in quotation marks because, with the exception of the Minnesota study, the total figure is simply the sum of AFDC, Food Stamp, and earned income.) The table is split into two panels, one for data on all pre-OBRA workers and the other for data on those pre-OBRA workers whose grants were terminated by OBRA. Entries for each type of income depict what happened to the average worker (or terminee) in a sample. Thus, even people who did not receive that type of income are included as zeros when computing the averages. Despite its complexity, the table tells a simple story: in the year following implementation of OBRA, incomes declined. Any increase in income from earnings or other non-AFDC sources was overwhelmed by the large decline in AFDC benefits.

The best-documented and least-surprising result is that AFDC benefits declined. Since a large fraction of these working recipients left AFDC after OBRA, a decline in average benefits is to be expected. And that is what all studies found. For example, in the RTI national sample of

Table 7

Changes in Components of Income During the Year Following Implementation of OBRA (all figures are current dollars per month)

Study	AFDC	Food Stamps	Earnings	Other	Total	Level of Pre-OBRA Total Income
			1. Pre-OBRA Workers			
RTI[a]						
OBRA Cohort	-105					
COMP Cohort	-44					
IRP[b]	-193	+19	+37		-140	841
Minnesota[c]	-155		+7	+93	-55	1002
New York City[d]	-91	-1	-67		-159	858
			2. Terminees			
GAO[e]						
Boston	-138	-25	+100		-63	924
Dallas	-45	-50	-90		-185	745
Memphis	-45	-6	-107		-158	653
Milwaukee	-98	-7	-12		-117	1008
Syracuse	-151	-19	+63		-107	874
IRP[f]	-208	+11	+36		-165	951
New York City[g]	-101	-7	-60		-168	878

[a]RTI (1983), Table 3.3, p. 3-19, top panel. Based on a national sample of caseload records for AFDC recipients with earnings in September 1980 and September 1981.

[b]Cole et al. (1983). AFDC from Table 6, Food Stamps derived from Table 8, total from Table 9A. Data came from telephone interview. Sample restricted to women who were not married both before and after OBRA and who had grants either reduced or terminated by OBRA.

[c]Moscovice and Craig (1983). Derived from data on pp. 21-22. Other income is calculated by subtracting gross earnings and AFDC from gross income. Thus other income includes Food Stamps. Data came from telephone interviews.

[d]Ginsberg and Mesnikoff (1984), Table 42, p. 82. Derived using raw data counts at top of table. Based on personal interviews with a sample of New York City single-parent families with benefits either reduced or terminated by the 150 percent rule or by elimination of the $30 and 1/3 disregard.

[e]GAO (1984). AFDC and Food Stamps derived from Tables 15 and 16. Total income from Appendix III. Data came from interviews with people who were working before OBRA and lost AFDC eligibility because of OBRA.

[f]See note b. Derived from terminee panels in tables.

[g]See note d. Derived from terminee panels in tables.

pre-OBRA earners, average AFDC income fell from $217 in September 1981 to $112 in September 1982, a $105 decline. Interestingly, average AFDC income also fell for the September 1980 comparison cohort, but not as much. That suggests that not all of the changes in AFDC income for the OBRA cohort should be attributed to OBRA.

Whereas the change in AFDC income is predictable, the change in Food Stamp income is not. On the one hand, if the Food Stamp program remained constant, the decline in AFDC income might have triggered an increase in Food Stamp benefits. On the other hand, OBRA not only changed the AFDC program, but also changed the Food Stamp program. In particular, the earned income disregard was reduced from 20 to 18 percent and in most cases people with gross income above 130 percent of the poverty level were excluded from the program. Such changes would tend to reduce Food Stamps for pre-OBRA workers. Thus, Food Stamp income could either increase or decrease. Table 7 does not clarify matters. Food Stamp income decreased in the New York and GAO studies and increased in the IRP study. About all that can be concluded from the table is that whatever happened to Food Stamp income, it was minor in comparison to what happened to AFDC income.

The Table 7 data on earnings are dealt with in the discussion of Table 6, and will not be discussed again here. As with Food Stamps, some studies indicate increased average earnings and others indicate decreased average earnings.

Only the Minnesota study presents data on income other than AFDC, Food Stamps, and earnings. Included in the "other" income for Minnesota is financial assistance from friends/relatives, child support, interest, rental income, earnings from work of other household members, Food Stamps, rent subsidies, and fuel assistance. As indicated in Table 7, this component of income increased rather dramatically in the year after OBRA. Given the Minnesota study's 28 percent response rate, it is difficult to attribute too much significance to this result. It does, however, raise concern that in focusing on AFDC, Food Stamps, and earnings, the other studies may have left out sources of income that helped to cushion OBRA's blow.

The data are unanimous in indicating a decline in the sum of AFDC, Food Stamp, and earned income. Note that this decline had little to do with initial levels of income. In Memphis the pre-OBRA average income for terminees was $653 and over a year that dropped by $158. In Boston the pre-OBRA average income was $924 and that only dropped by $63. The decline in well-being that followed implementation of OBRA may have thus fallen as severely on those below the poverty line as on the near poor.

Data on financial emergencies following OBRA reinforce the view that the decline in income meant a decline in economic well-being. For OBRA terminees the GAO study found evidence of an increased incidence of both borrowing $50 or more from a friend, and of running out of food and having no money to buy more.[20] The Ginsberg and Mesnikoff New York study includes the following quote:

> I was evicted because I couldn't pay the rent. Right now I live in an illegal apartment; its not listed. I've lived now for 9 months without a kitchen. I bought a kerosene heater and only heat one room where my son and I sleep.[21]

It is clear that many of the recipients who were affected by OBRA suffered substantial economic hardship in the months following its implementation.

Health Insurance. This is another aspect of economic well-being that was quite possibly affected by OBRA. Medicaid health insurance is provided to all AFDC recipients. Families that left AFDC because of OBRA lost automatic eligibility for Medicaid, though they may receive partial assistance through "spend down" provisions in some states. Thus, unless they had insurance coverage through their employer, these families suffered a decline in health insurance coverage. Three of the studies (GAO, Minnesota, and the New York study by Ginsberg and Mesnikoff) examined this issue and found evidence of a precipitate decline in coverage. For example, with regard to their sample of pre-OBRA recipient earners (some of whom remained AFDC recipients), the Minnesota researchers write that "eighteen percent of the respondents and twenty-eight percent of their children had no (public or private) health insurance one year after the cutbacks..."[22] For those who were working but no longer recipients, fully 23 percent of the respondents and 37 percent of the children were without coverage.

Not only do the GAO findings corroborate this, they suggest that Minnesota's recipients may have been more fortunate than recipients in other regions of the country. In interviews with pre-OBRA earners who had lost AFDC eligibility because of OBRA, the GAO found that 64 percent of the Dallas respondents and 60 percent of the Memphis respondents had no health insurance coverage a year after OBRA implementation.

The studies go on to document the behavioral consequences of this decline in coverage. The GAO's Boston results are typical. Twenty-one percent of the GAO interviewees did not seek treatment for a medical problem because of expense, 48 percent did not seek dental care for the same reason, and 13 percent were refused medical or dental care because they were unable to pay or had no insurance.[23] In addition, Ginsberg and Mesnikoff in New York and the Minnesota study both indicate that nonrecipient families are more likely to delay seeking help from doctors and dentists that recipient families with Medicaid coverage.[24] In the CSSP study, 21 percent of the Georgia respondents had overdue medical bills, and "several mothers had medical bills for $2000, $3000, and $5000."[25] One woman told her interviewer, "The only thing I really miss is Medicaid. They encourage you to work, but now I can't afford to take my kids to the doctor."[26] Given the above statistics, she evidently speaks for a broad class of people.

Child Care. Yet another way in which OBRA may have affected economic well-being is through its impact on child care. Since OBRA not only placed a maximum on AFDC child care deductions but also tended to reduce total incomes, working mothers may have sought to economize on child care arrangements. The CSSP, the New York study of Ginsberg and Mesnikoff, and the Minnesota study examine this issue. All reveal a tendency for working mothers to spend less on child care.[27] Even mothers who left AFDC and became working nonrecipients reduced child care expenditures. The Minnesota researchers provide a succinct summary of their particularly detailed results. Pre-OBRA workers who were working a year after OBRA implementation had slightly decreased their use of day care for children under 13,

> but found many other ways to lower their monthly dollar outlays. These ways included: a 47 percent increase in their use of Hennepin County/Title XX funds from 1/82 to 1/83, switching types of day care, leaving the child alone for part of the time,

> or switching to less expensive day care centers. These changes had not been made without a price being paid. More than twice as many people were dissatisfied with the day care their children were getting one year later. There was also a one-third increase in the number of children needing, but not getting, day care.[28]

Another Minnesota result deserves special emphasis: fully 23.6 percent of the working nonrecipients left their child (or children) alone at home part of the day. A year before, when they were AFDC working recipients, only 15.6 percent had left their children alone. And once again the Minnesota sample may have been more fortunate than similar groups of pre-OBRA recipients living elsewhere. Moscovice and Craig write that Hennepin County uses its own resources along with Title XX funds to provide day care vouchers to families with incomes below 60 percent of the state median. Since that is probably not the case throughout the country, the Minnesota data may paint too rosy a picture of what happened to the OBRA cohort's child care arrangements.

In conclusion, there is solid evidence that many AFDC recipient families suffered a decline in economic well-being in the months that followed implementation of OBRA. Mothers who had meager incomes before OBRA often raised their children on smaller incomes after OBRA. With the loss of Medicaid eligibility, thousands of families went without health insurance. With the loss of AFDC deductions for work-related expenses, working mothers often sought to economize on child care costs. The period following OBRA must have been a particularly difficult time for thousands of America's dependent children.

What Effect Did OBRA Have on AFDC Costs?

There are good reasons to claim that OBRA reduced AFDC expenditures. Perhaps most dramatic, between October 1981 and June 1982 average monthly AFDC outlays declined by $75.7 million.[29] Also consistent with the claim is the Section I evidence that OBRA reduced caseloads and the Section III evidence of post-OBRA declines in level of AFDC income received by pre-OBRA workers. Researchers at RTI sought to measure OBRA's impact by analyzing the change in outlays between their comparison and OBRA cohorts. They conclude that OBRA reduced expenditures on earners by between $20.3 million and $26.6 million per month over the period January-September 1982.[30] Of course, this estimate ignores OBRA's possible effects on nonearners and new entrants. Given that OBRA probably reduced numbers of recipients in these groups, the RTI $20-$27 million estimate should be viewed as a lower-bound estimate of OBRA's effect on AFDC costs.

The GAO study took a different tack in measuring OBRA's cost impact. As in their work on caseloads, the GAO researchers fit a time series model to data on program outlays over the period January 1973 through June 1983. Comparing predicted and actual outlays for the period after OBRA, they estimate that OBRA's short-term effect was to reduce outlays by $92.8 million per month.[31] This is an estimate of OBRA's effect on all groups (earners, nonearners, new entrants) combined, and is thus consistent with the foregoing RTI lower-bound figure.

It is important to emphasize two points about the RTI and GAO estimates. First, they are short-term estimates. As with the caseload effects in Section I, these short-term estimates may be larger than or smaller than OBRA's long-term effect. Second, these estimates are sample statistics, and we have no information on their variance. Since actual costs declined by $75 million per month at a time when, ceteris paribus, caseloads were probably rising because of the recession, the GAO estimate of $93 million appears reasonable.

IV. CONCLUSION

The studies reviewed here provide partial answers to the four questions posed in the Introduction. There remain, however, broad areas in which our knowledge about OBRA's effects is incomplete. This conclusion summarizes what we know and notes areas where further research may be desirable.

On the basis of the studies reviewed here, we know the following:

-- OBRA increased the propensity for AFDC recipient families to leave the program. Although the evidence on this is strongest for working recipients, it also applies to nonworking recipients. In part because exits increased, caseloads and program costs fell during the year following OBRA's implementation.

-- Although OBRA did not increase the propensity for AFDC recipient families to become or remain nonworking recipients, it apparently did reduce their propensity to become or remain working recipients. Again, the evidence on this is strongest for pre-OBRA working recipients.

-- There is solid evidence that the average pre-OBRA working recipient suffered a decline in income during the year after OBRA's implementation. Income from earnings, Food Stamps, and other sources did not replace their lost AFDC benefits. Moreover, on average other medical insurance did not replace the loss of Medicaid, and some families thus confronted higher out-of-pocket costs for medical care. Finally, there is good evidence that after OBRA the working mothers in these families often utilized a lower quality and quantity of day care for their children. All of the evidence thus indicates that the average pre-OBRA working recipient experienced a sharp decline in economic well-being in the year following OBRA.

One of the principal themes of this paper is the appropriate interpretation of these results. It is conceivable that the lack of dramatic labor supply effects could be read as indicating that OBRA was a somewhat benign policy change. Since former working recipients tended to continue working after OBRA, since they did not return in droves as nonworking recipients, one might conclude that many of these families did not suffer and were able to get along quite well without AFDC. In the author's view, that is the wrong interpretation.

The lack of dramatic labor supply effects may indicate that OBRA was anything but a benign policy. It is possible that for many recipients OBRA both raised effective tax rates and reduced effective guarantees. By this interpretation, some families did not return to the program as nonworking recipients because given cuts in the effective guarantee, they were better off working. Other families did not return because even as nonworkers they would be ineligible. By this interpretation, a mere tax rate increase would have been a more benign policy than OBRA. Although it would have had more dramatic labor supply effects, a simple tax rate increase would not have reduced well-being as much as OBRA did. All of the evidence is consistent with the claim that OBRA was an important policy with significant negative effects on economic well-being.

Another goal of this paper has been to assess where further research may be needed. Four areas seem particularly important:

-- We know nothing about what happened to the economic well-being of nonworking pre-OBRA recipients.

-- We know nothing about OBRA's effect on potential entrants.

-- We have but limited knowledge of the extent to which pre-OBRA AFDC recipients were able to increase their income from sources other than earnings and Food Stamps.

-- We have but limited knowledge of OBRA's effects on children.

These points are developed below.

There is good reason to believe that OBRA caused many nonworkers to leave the AFDC program. Indeed, as noted in Section III, the number of pre-OBRA nonworkers who left the rolls because of OBRA may have been as large as the number of pre-OBRA workers. None of the studies reviewed here examined what happened to the income of these nonworking recipients during the year after OBRA. Clearly, a full assessment of the effects of OBRA requires information on the extent to which such recipients and their families were able to obtain alternative sources of support.

A full assessment of OBRA's effects also requires information on how OBRA altered the behavior of potential AFDC entrants. As argued in

Moffitt (1984), OBRA may have altered the labor supply behavior of potential entrants; because of OBRA some people may have not entered the program and reduced their labor supply. A complete picture of OBRA's labor supply effects thus requires information on potential entrants. In addition, the behavior of entrants is central to our understanding of OBRA's long-run caseload and cost effects. As argued in Section III, if OBRA reduced entry rates, then OBRA's long-term effect on caseloads and costs may be larger than the near-term effects measured by the studies reviewed here.

Another gap in our knowledge concerns changes in total income in the year following OBRA. Most of the studies analyze changes in AFDC plus Food Stamps plus earned income. There is some indication in the Minnesota data that other forms of income (such as child support payments or earnings of other family members) increased during the year following OBRA and thus served to cushion OBRA's impact. The facts on that should be pinned down.

Finally, although we know that for many pre-OBRA recipient families economic hardhip increased after OBRA, we know relatively little about what happened to the children in these families. Was nutrition affected? Did health levels decline? It is important to determine this because children lie at the heart of the rationales for the AFDC program. AFDC families receive government support because they contain dependent children. As such an evaluation of how a decrease (or increase) in AFDC benefits affects economic well-being would ideally focus on how it affects the well-being of children.

To conclude, the studies reviewed here have provided much valuable information on the effects of OBRA. Some rather important questions, however, remain to be answered.

APPENDIX

I. In line with the discussion in the text let

$$U_M = U_M(Y,W)$$

$$U_{NWR} = U_{NWR}(Y + G)$$

$$U_{WR} = U_{WR}(Y + G,\ S \cdot W),$$

where

$U_M(\quad)$, $U_{NWR}(\quad)$, and $U_{WR}(\quad)$ are indirect utility functions indicating utility attainable by people who are not receiving benefits, by nonworking recipients and by working recipients.

Y is nonwage income

W is the wage rate

G is the NIT guarantee

S is the NIT tax rate.

It is straightforward to show that

$$\partial U_M/\partial G = 0 \qquad \partial U_M/\partial S = 0$$

$$\partial U_{NWR}/\partial G > 0,\ \partial U_{NWR}/\partial S = 0$$

$$\partial U_{WR}/\partial G > 0,\ \partial U_{WR}/\partial S < 0.$$

Assume that in period t,

$$U_M^t = U_M(\quad) + \varepsilon_M^t$$

$$U_{MWR}^t = U_{NWR}(\quad) + \varepsilon_{NWR}^t$$

$$U_{WR}^t = U_{WR}(\quad) + \varepsilon_{WR}^t$$

where the epsilons are independently distributed random variables with finite variances. In addition assume that decision-makers are utility maximizers. Thus, for example, they become working recipients in period t if and only if $U_{WR}^t = \mathrm{Max}[U_M^t, U_{NWR}^t, U_{WR}^t]$.

Consider then a working recipient in period t - 1. The probability that that person occupies a given "state" in period t is

Pr[Remains a working recipient] =

$$\Pr[U_{WR}^t > U_m^t \text{ and } U_{WR}^t > U_{NWR}^t] = \Pr[U_{WR}^t > U_m^t] \cdot \Pr[U_{WR}^t > U_{NWR}^t].$$

Pr[Becomes a nonworking recipient] =

$$\Pr[U_{NWR}^t > U_{WR}^t \text{ and } U_{NWR}^t > U_M] = \Pr[U_{NWR}^t > U_{WR}^t] \cdot \Pr[U_{NWR}^t > U_M^t].$$

Pr[Becomes a nonrecipient] =

$$\Pr[U_M^t > U_{WR}^t \text{ and } U_M^t > U_{NWR}^t] = \Pr[U_M^t > U_{WR}^t] \cdot \Pr[U_M^t > U_{NWR}^t].$$

An increase in the tax rate decreases U_{WR} without affecting either U_M or U_{NWR}. As such,

$$\partial\Pr[\text{Remains a working recipient}]/\partial S = \partial\Pr[U_{WR}^t > U_M^t]/\partial S \cdot \Pr[U_{WR}^t > U_{NWR}^t].$$
$$+ \Pr[U_{WR}^t > U_M^t] \cdot \partial\Pr[U_{WR}^t > U_{NWR}^t]/\partial S < 0.$$

$$\partial\Pr[\text{Becomes a nonworking}]/\partial S = \partial\Pr[U_{NWR}^t > U_{WR}^t]/\partial S \cdot P[U_{NWR}^t > U_M^t] > 0.$$

$$\partial\Pr[\text{Becomes a nonrecipient}]/\partial S = \partial\Pr[U_M^t > U_{WR}^t]/\partial S \cdot P[U_M^t > U_{NWR}^t] > 0.$$

Using the same method for nonworking recipients and people not receiving benefits in t - 1, one can derive the results in the text.

NOTES

[1]See The Center for the Study of Social Policy (1983) for a list of these studies.

[2]See, for example, Moffitt (1984).

[3]This is a state average. See Fraker and Moffitt (1984).

[4]It can be 100 percent for families who have worked more than four consecutive months and who are at the maximum work-related expense and child-care-expense deduction. It can exceed 100 percent at the point where the provision takes hold that restricts gross income to less than 150 percent of the standard of need.

[5]This treatment borrows from Levy (1979).

[6]GAO (1984), p. 51.

[7]The Center for the Study of Social Policy (1984), p. 68.

[8]A preliminary Ph.D. dissertation chapter draft by Steven Cole supports this.

[9]GAO (1984), Table 4, p. 14.

[10]U.S. Department of Health and Human Services, Social Security Bulletin, (January 1984) Table M-30, p. 53; (March 1984), Table M-30, p. 39; (May 1984), Table M-29, p. 49; (July 1984), Table M-29, p. 52; (September 1984), Table M-29, p. 49.

[11]GAO (1983), p. 18.

[12]This IRP result may be due to sampling variance. There were 290 workers in the sample.

[13]The former is a test of whether a sample with mean μ_1 could have been drawn from a population with mean zero. The latter is a test of whether seven sample means ($\mu_1,\ldots,\mu_7$) could have been drawn from a population with mean zero.

[14]According to Committee on Ways and Means (1984), p. 325, 11.5 percent of the May 1981 AFDC families had earnings. Since the September 1981 caseload was 3.88 million, approximately 3.88 X .895 were nonworkers and 3.88 X .115 were workers.

[15]IRP, the New York studies, and the Minnesota study provide rather complete information on terminees. The GAO report, Table 10, indicates employment status of terminees who returned.

[16]From Moscovice and Craig (1983), Figure 1, p. 10.

[17]The discussion of the asset test in Moffitt (1984) gets close to this position.

[18]In this regard see GAO (1984), Table 19, p. 47, and Cole et al. (1983), Table 4.

[19]Some studies present data on income the month before and the month after OBRA implementation. Such data are not presented here because they are not comparable with data from other studies dealing with income one year after OBRA. Also, the one-year data give a better indication of how people adjusted to OBRA.

[20]GAO (1984), Table 18, pp. 44 and 45.

[21]Ginsberg and Mesnikoff (1984), p. 110.

[22]Muscovice and Craig (1983), p. 26.

[23]GAO (1984), Table 17, p. 42.

[24]Ginsberg and Mesnikoff (1984), pp. 97-99, Moscovice and Craig (1983), p. 27.

[25]The Center for the Study of Social Policy (1984), p. 51.

[26]Ibid., p. 31.

[27]The Center for the Study of Social Policy (1984), p. 42; Ginsberg and Mesnikoff (1984), pp. 73-77; Muscovice and Craig (1983) pp. 30-32.

[28]Muscovice and Craig (1983), p. 30.

[29]GAO (1984), p. 18.

[30]RTI (1983), p. 3-55.

[31]GAO (1984), p. 18.

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