HOW COST-BENEFICIAL ARE US WELFARE-TO-WORK PROGRAMS?

David Greenberg

And

Andreas Cebulla*

February 2006

David Greenberg is the corresponding author. Address: 5531 High Tor Hill, Columbia MD 21045. Email: dhgreenb@umbc.edu

ABSTRACT

This article uses the tools of meta-analysis to assess cost-benefit studies of 49 mandatory welfare-to-work programs that were targeted at AFDC recipients and evaluated by random assignment in the USA. The findings suggest that although the benefits from a typical evaluated welfare-to-work program probably exceeded its costs, these differences are so slight that neither those assigned to the program nor the rest of society are made appreciably better off. Further analysis suggests that the programs might be made more cost-beneficial by dropping vocational training and basic education as program components, leaving only mandated job search, sanctions, and (possibly) financial incentives.

HOW COST-BENEFICIAL ARE US WELFARE-TO-WORK PROGRAMS?

INTRODUCTION

There have been numerous evaluations of mandatory "welfare-to-work programs" in the United States. In these mandatory programs, welfare recipients are required to participate in job search, vocational training and remedial education, or "work experience" in which they are assigned to government or non-profit organizations such as charities. If they refuse, they can face sanctions that result in the reduction or loss of transfer benefits. The programs also sometimes incorporate financial incentives intended to encourage employment. The evaluations of the programs, most of which have been based on random assignment, have tended to indicate that mandatory welfare-to-work programs result in modest gains in earning for those assigned to them and modest reductions in the welfare rolls. Partially as a result of these findings, mandatory welfare-to-work initiatives are now a component of most U. S. programs that provide transfer benefits to welfare recipients, the unemployed, the disabled, and other disadvantaged persons. But should they be?

Cost-benefit analysis provides an appropriate means of addressing this question. Because the benefits from welfare-to-work programs are typically modest and the government incurs costs in operating them, it is natural to ask whether their benefits exceed their costs; and this is exactly what cost-benefit analysis attempts to do. In fact, many, but far from all, evaluations of welfare-to-work programs have included cost-benefit analyses.

In this paper, we examine cost-benefit analyses of mandatory welfare-to-work programs that were targeted at AFDC recipients. These analyses constitute by far the largest set of cost-benefit studies of welfare-to-work programs that are based on random assignment. The paper has two major objectives: The first is to determine whether welfare-to-work

programs for AFDC recipients are cost-beneficial. The second is to see whether there are any
programmatic changes that might make these programs more cost-beneficial.

THE CBA FRAMEWORK

An illustration of the framework that is typically used in conducting cost-benefit
studies of welfare-to-work programs appears in Table 1.[1] Only benefits and costs that are
typically estimated are listed in this table.[2] Dollar amounts are indicated as resulting from
program effects on output produced while participating in work experience, earnings, tax
payments, AFDC payments and payments from other government transfer programs, and net
operating costs. The plus and minus signs indicate whether each amount is expected to be a
benefit (+) or cost (-) from the perspectives of four groups: persons assigned to the welfare-
to-work program, non-assignees (i.e., all persons outside the program group, including both
AFDC recipients who were not assigned to the program and taxpayers who do not receive
AFDC but do pay the cost of operating the program), the government, and the whole of
society (the program group plus non- assignees).

As indicated, it is usually assumed that benefits and costs to non-assignees and the
government are identical except that the former benefits from any output members of the
program group produce while participating assigned to work experience programs and the
government does not. As can be seen, benefits and costs to society are simply the algebraic
sum of benefits and costs to those assigned and those not assigned to a program. Hence, the
framework implies that if a welfare-to-work program causes a decline in transfer payments
received by program group members (for example, in AFDC or food stamps receipts), then
this decline should be regarded as a cost to program group members (albeit one that may be
offset by earnings increases), as a savings or benefit to taxpayers, and as neither a benefit nor
a cost to society but simply as a transfer of income from one segment of society to another.

Similarly, increases in tax payments are a cost to the program group, a benefit to non-assignees, and a transfer when viewed from the jperspectiv3e of society as a whole. Increases in earnings from regular employment, in contrast, accrue to members of the program group but are neither benefits nor costs to non-assignees, who presumably benefit from increased output represented by the earnings increase but must pay for this output. Because some members of society benefit from the increase in earnings without offsetting costs occurring elsewhere, society as a whole also benefits.

The goal of cost-benefit analyses of welfare-to-work programs, as the last row of Table 1 suggests, is to determine whether the program being evaluated has positive or negative *net* benefits (i.e., benefits less costs) from each of the four perspectives represented by the four columns. The societal perspective is usually viewed by economists as the appropriate one for assessing the efficiency of social programs. Policymakers, however, often focus on the government perspective, that is, on whether the program increases or decreases government budgetary requirements. Because the value of in-program output produced in most mandatory welfare-to-work interventions is small or non-existent, net benefits from the non-assignee and the government perspectives are usually very similar.

DATA

This study relies on data from 27 random assignment evaluations of mandatory U.S. welfare-to-work programs for AFDC recipients. These evaluations, which are listed in Table 2, were initiated in various localities (often counties) at different points in time between 1982 and 1996.[3] While the potential for generalizing the results of any given individual study is limited, the variation in program content and population characteristics arising from combining the studies into an integrative review enormously increases the scope for generalization (Hall et al., 1994).

To ensure comparability across the evaluations, inclusion criteria were established relating both to the kind of program being evaluated and the evaluation strategy. First, all the evaluated welfare-to-work programs had to include an active intervention (e.g. job search, work experience, education, or training) that was intended to assist welfare recipients to increase their earnings and move off AFDC. Second, all the programs were mandatory in the sense that recipients who did not participate in job-search, vocational training, remedial education, or 'work experience' as required were potentially liable to sanction through the reduction or removal of their welfare benefit. Third, all the programs were directed at persons in receipt of AFDC (Aid for Families with Dependent Children), which during the period the programs were evaluated was the major cash public assistance program for families in the United States.[4]

Finally, the evaluations were restricted to those that assigned AFDC recipients to treatment and control groups on a random basis. AFDC applicants and recipients assigned to the program group were required to participate in the welfare-to-work program being evaluated, while those assigned to the control group were eligible to receive any services that existed prior to the introduction of the welfare-to-work program. By comparing such outcomes as earnings and transfer payments for the two groups, program effects (often called "impacts") can be measured. Not only is randomized assignment considered by many to be the model or 'gold standard' of evaluation procedure, providing unbiased estimates of program effects, this restriction effectively standardized methodological procedures. Moreover, all but four of the 27 evaluations were conducted by just three research organizations. Each of these three organizations has over three decades of experience in implementing and monitoring random assignment procedures and each has a strong reputation for performing random assignment evaluations efficiently and effectively.

The 27 evaluations provide information about 79 welfare-to-work programs that operated in over 50 sites (i.e., separate counties or metropolitan areas). The multi-site evaluations assessed programs that varied to a greater or lesser degree. One reason the number of programs exceeds the number of sites is because, in some sites, two experimental programs were run simultaneously so that outcomes for participants in each could be compared to one another, as well as to a control group. In addition, some of the evaluations conducted separate analyses of one- and two-parent families. Because programs in which these two family-types were enrolled often differed from those for one-parent-families in some of their features, we treat them as separate programs. Of the 79 welfare-to-work programs for which we have data, 49 were subject to cost-benefit analyses. These 49 programs are the focus of this study.

METHODOLOGY

In examining the cost-benefit findings from evaluations of welfare-to-work programs, it is important to take account of the fact that the impact estimates for the individual program are based on different sample sizes and, hence, have different levels of statistical precision. The reason for taking account of different levels of statistical precision is suggested by the following formal statistical model, which explains variation in a specific program impacts, such as those for earnings or AFDC receipts:

$$E_i = E_i^* + e_i, \text{ where } i = 1, 2, 3, \ldots, n$$

where E_i is the estimated effect of a welfare-to-work intervention, E_i^* is the "true" effect (obtained if the entire target population had been evaluated), n is the number of interventions for which impact estimates are available, and e_i is the error due to estimation on a sample smaller than the population. It is assumed that e_i has a mean of zero and a variance of v_i.

To provide an estimate of the mean effect that takes account of the fact that v_i varies across intervention effect estimates, a weighted mean can be calculated. In analyses of this sort, the weight that is usually used is the inverse of v_i, $1/v_i$ (for example, see Hedges 1994, Lipsey and Wilson 2001, and Shadish and Haddock 1994). The reason for weighting by the inverse of the variance of the estimates of program impacts is intuitive. In evaluations, estimates of impacts from policy interventions are usually obtained by using samples from the intervention's target population. As a result of sampling from the target population, the impact estimates are subject to sampling error. The variance of an estimated effect, which usually becomes smaller as the size of the underlying sample increases, indicates the size of the sampling error. In general, a smaller variance implies a smaller sampling error and, hence, that an impact estimate is statistically more reliable. Because all estimates of intervention impacts are not equally reliable, they should not be treated the same. By using the inverse of the variance of the effect estimates as a weight, estimates that are obtained from larger samples and, therefore, are more reliable, contribute more to various statistical analyses than estimates that are less reliable.

Unfortunately, no measures of statistical significance are available for the cost-benefit estimates of program effectiveness because, as Table 1 indicates, such estimates are a composite of separate program impact estimates, including those for earnings, AFDC receipts, and costs. Yet, the estimates of these cost-benefit components are each subject to sampling error. Thus, weighting is appropriate. Hence, in our analysis of these measures, we weight by the size of the total sample used in the evaluation, rather than by $1/v_i$. However, both $1/v_i$ and the sample size are available for impact estimates of earnings and AFDC receipts, and in these instances, the simple correlation between them is above .95. To weight by sample size instead of the more appropriate inverse of the variance is, therefore, unlikely to affect our results very much.

HOW LARGE ARE THE NET BENEFITS?

Basic Findings

Table 3 provides summary information on net benefits from each of the four perspectives. Estimates of the government's net operating costs are also presented. All the figures have been converted to year 2000 dollars using the Consumer Price Index. The estimates are measured in terms of costs and benefits per individual assigned to the evaluated interventions. Both unweighted and weighted means are provided in Table 3. As discussed above, the weights we use are the number of observation in the total evaluation sample.

The estimates in Table 3 are for the 49 cost-benefit studies included in our database. One set of estimates is used from each study. Because most evaluations of welfare-to-work programs conduct only one cost-benefit study, summing annual or quarterly estimates and projections of benefits and costs over several years (most often five), only one set of estimates is usually available. If more than one set of estimates were available, however, we use the most recent.

The table indicates that net benefits received by non-assignees are somewhat larger than those received by the government, although not by very much. This is unsurprising because, as mentioned earlier, the computations of net benefits from these two perspectives are identical, except that non-assignees are credited with the value of output program group members produced in work experience programs—typically, by providing a service of some sort (e.g., as a teacher's aide) without pay[5]—and the government is not. In most welfare-to-work programs, the value of such output is small, and in many it does not exist.

With one exception (the unweighted values for the program group), the median net benefit estimates in Table 3 are larger than their corresponding means, indicating that some under-performing interventions are pulling down the average. In addition, the weighted

values are usually considerably larger than their unweighted counterparts, implying that

evaluations with larger samples tend to produce larger net benefit estimates than those with

smaller samples. This finding is mainly, but not entirely, due to an evaluation of one

program: the Indiana Welfare Reform Program. The sample used in this evaluation was over

three times greater than the next largest sample and the program was found to have had

exceptionally large benefits. Although there is no justification for excluding the Indiana

Welfare Reform Program evaluation from the analysis, in discussing the results below, we

also present evidence of how the inclusion of this study influences some of our findings.

Keeping in mind that the mean and median net benefit estimates in Table 3 are

intended to capture the total net benefits of welfare-to-work interventions over several years

(most often five), they are perhaps surprisingly small from all four perspectives. The

weighted medians, which are the largest values, indicate that the welfare of program group

members are improved by around $700; net benefits to non-assignees and net savings to the

government are a bit under $1,000; and society receives net benefits of around $1,700 from

mandatory welfare-to-work programs. However, in assessing the performance of welfare-to-

work programs, it is probably more appropriate to examine a typical or mean program and

thereby allow for the influence of poor performing programs. The weighted means in Table 3

imply that a typical program produced net benefits for the program group of under $300, net

benefits for non-assignees and the government of under $600, and net benefits for society of

around $800. Both the weighted medians and means fall further if the evaluation of the

Indiana Welfare Reform Program is excluded from the calculations. For example, the

weighted means net benefits for the program group, non-assignees, and the government are

all under $100 and social mean net benefits are only $127.

Thus, it seems apparent that the net benefits from a typical welfare-to-work

intervention are modest even when the evaluation of the Indian Welfare Reform Program is

included in the analysis and even more modest when it is excluded. However, net program operating costs[6] are also modest. For example, the weighted mean operating cost is only about $1,400. If the net social benefits of about $800 were spread evenly over 5 years, this would represent a rate of return of around 16 percent on the government's $1,400 investment.

By emphasizing mean and median net benefits and operating costs, we focus on more or less typical mandatory welfare-to-work programs. As the standard deviations and the minimum and maximum values reported in Table 3 make clear, however, the variation in findings across the 49 cost-benefit analyses in our sample is enormous. Thus, according to the cost-benefit findings, there were some impressively successful welfare-to-work programs and a few spectacular failures. We investigate the sources of the variation among the net benefit estimates later.

The variation in cost-benefit findings is also illustrated by the fact that 26 (53 percent) of the net social benefit estimates are positive and 23 (47 percent) are negative. Equally importantly, 24 (49 percent) welfare-to-work interventions were found to have benefited participants and 28 (57 percent) were found to have benefited non-assignees, but only 14 (29 percent) of our sample of 49 programs were estimated to have resulted in positive net benefits for both groups, while 11 (22 percent) were found to have negatively effected both groups.

These results appear more promising if weighted by the total sample used in each cost-benefit analysis, again demonstrating the importance of weighting. For example, of the total of 259,256 observations used in the 49 cost-benefit analyses, 67.1 percent were used in studies in which net social benefits were found to be positive, 59.6 percent in studies in which participants had positive estimated benefits, and 75.6 percent in studies in which non-assignees were found to have benefited. Just under half (49.3 percent) were used in studies in which both groups were found to have positive benefits and only 14.1 percent were used in studies in which negative net benefits were found for both groups.

Programs With and Without Financial Incentive

Financial incentives were tested in 11 of the 49 programs for which cost-benefit analyses were conducted. Adoption of these incentives usually involved reducing the amount by which AFDC benefits fell as earnings rose, thereby increasing the amount of transfer payments that AFDC recipients who were employed could keep.

Table 4 presents weighted mean and median net benefits separately for welfare-to-work programs that do and do not provide financial incentives. As is evident, the findings differ substantially for these two types of programs. Most strikingly, the net benefits received by the program group tend to be much higher for programs with financial incentives, while the mean and median net benefits received by non-assignees are only positive in the absence of financial incentives. This occurs because, almost by definition, financial incentives result in income being transferred from taxpayers to working AFDC recipients. The median weighted net benefit received by society as whole is also somewhat lower for programs with financial incentives.

Distributional Implications

In the cost-benefit results presented in Tables 3 and 4, dollars gained or lost by the program group are valued identically to dollars gained or lost by the non-assignee group. Because the former has much lower incomes, on average, than the latter—which includes income earning taxpayers, as well as non-participating AFDC recipients—it is not clear that they should be. For example, the marginal utility of income is likely to be higher for low income persons than for higher income persons.[7] This issue is relevant whenever a welfare-to-work program makes enrollees better off, but negative net benefits among non-assignees more than offset these gains and net social benefits are therefore negative. It is also germane

when the program group is made worst off, but positive net benefits among non-assignees more than offset these losses and net social benefits are therefore positive. The first situation occurs in 7 and the second in 9 of our 49 cost-benefit analyses, accounting respectively for 5.1 percent and 12.6 percent of the 259,256 observations used in the evaluations.

A considerable literature exists concerning the possibility of treating both sets of circumstances by giving each dollar of the gains or losses of relatively low-income persons greater weight in cost-benefit analyses than each dollar of the gains or losses of higher income persons (for a summary, see Chapter 18 of Boardman et al., 2006). The problem is that the weights that are appropriate for doing this are unknown. However, if we arbitrarily treat each dollar gained or lost by program group members as having twice the value of each dollar gained or lost by non-assignees, which seems high, only two of the 23 negative net social benefit estimates become positive and only three of the 26 positive net social benefit estimates become negative. We conclude that, overall, cost-benefit analyses of welfare-to-work programs are fairly robust to the standard procedure of not using weights, but instead treating dollars gained or lost by the program and non-assignee groups as identical in value.

ARE THE TRUE NET BENEFITS LIKELY TO BE HIGHER OR LOWER THAN THE ESTIMATES?

There are several factors that cause the means and medians presented in Tables 3 and 4 to overstate the net benefits of welfare-to-work programs and other factors that cause them to be understated. This section examines possible biases to estimates of the net benefits of welfare-to-work programs.

Selectivity Bias

It is likely that the net benefits of a typical welfare-to-work program are actually smaller than implied by the estimates in Tables 3 and 4 because cost-benefit analyses are less

likely to be conducted for those programs with especially small impacts. For example, during the 3rd and 7th quarters after random assignment, the weighted means of the estimated program impacts on earnings and AFDC payment amounts are over twice as large for the 49 interventions for which cost-benefit analyses were conducted as for the 30 programs in our sample of studies for which they were not conducted, with the exception of the 3rd quarter impact on AFDC payments, which is a bit over one and a half times as large.[8] The differences are less dramatic for the unweighted averages but still substantial.

It is obviously difficult to know what the net benefits were for those programs for which cost-benefit analyses were not conducted. However, the fact that their impacts on earnings and AFDC payments were much smaller than impacts for those programs for which cost-benefit analyses were conducted suggests that they are likely to be quite small. Indeed, because the answer to the question of whether the program is cost-beneficial is already apparent, cost-benefit analyses are often not conducted in those instances in which estimated program impacts are very small or even negative.

To illustrate the implications of having to exclude programs for which cost-benefit analyses were not conducted from the findings presented in Tables 3 and 4, we simply assume that their net benefits were zero. This is probably a conservative assumption because, as previously mentioned, nearly half of the programs for which cost-benefit analyses *were* conducted were found to have negative net social benefits. If the weighted means shown in Table 3 are re-computed on the basis of this assumption, net benefits for the program group fall from $258 to $158, those for non-assignees decrease from $561 to $349, and those for society diminish from $818 to $501. Hence, society's return on the government's $1,400 investment in a typical welfare-to-work program would fall from around 16 percent to about 11 percent. Thus, there is some evidence that welfare-to-work programs were less effective than Table 3 implies.

Extrapolation of Impacts

Welfare-to-work evaluations have typically followed sampled individuals for one to three years after they are randomly assigned and almost never for more than five years. In conducting cost-benefit analysis, analysts must therefore make assumptions about how program effects on earning and transfer benefits change over time once the follow-up period ends and how long they continue. Although assumptions vary among studies, it is often assumed that program impacts decline after the follow-up period ends, falling to zero around five years after random assignment. If they do not decline and they continue beyond five years, net program benefits would be understated. However, recent evidence indicates that the impacts of a typical welfare-to-work program on earnings and AFDC payments begin to decline two or three years after random assignment, falling to zero after five or six years (Greenberg et. al 2004; and Greenberg, Cebulla, and Bouchet 2005). This finding is fairly consistent with the assumptions that cost-benefit analysts typically make and suggests their assumptions do not seriously bias estimates of net benefits.

Omitted Benefits and Costs

Several benefits and costs are omitted from Table 1 because they are rarely or never included in cost-benefit analyses of welfare-to-work programs as a result of being inherently difficult to measure in terms of dollars. Omitted benefits will, of course, result in net benefits being understated and omitted costs will cause them to be overstated. We next consider the potentially more important omissions.

Effects on Non Market Time

If a welfare-to-work program causes welfare recipients to work more, they then lose non-market time. This lost non-market time may have been used in productive ways (for child care or performing household chores, for example), but even if it not, it presumably has value to those who lose it. The few attempts that have been made to measure the value of lost market time suggest that ignoring it can result in appreciably overstating the net benefits of welfare-to-work programs for the program group and, hence, society as a whole (Bell and Orr 1994; Greenberg 1997; and Greenberg and Robins 2005).

Effects on Children

Welfare-to-work programs may have either positive or negative effects on children. On the one hand, family income may increase and so may parental employment, which may mean that children have more positive role models. On the other hand, if employment increases, it may mean that parents devote less time to their children. This is likely to be especially important in single-parent households, which constitute the majority of families on AFDC.

A number of recent random assignment evaluations of welfare-to-work programs have estimated program effects on various child outcomes, although none have attempted to incorporate these results into cost-benefit analyses. Although the estimated effects varied among the programs for which they were estimated, overall, they were small (Greenberg, Cebulla, and Bouchet 2005). However, one recent review has concluded that there is fairly strong evidence that recent mandatory work-related activities have reduced student achievement among adolescents (Grogger, Karoly, and Klerman 2002).

Effects on Marriage and Fertility

It is not apparent how the effects of welfare-to-work programs on marriage and fertility would be incorporated into a cost-benefit analysis. However, a recent review of what is known about these topics concludes that there is considerable evidence that marriage and fertility are unaffected by mandatory work-related activities (Grogger, Karoly, and Klerman 2002).

General Equilibrium Effects

Mandatory welfare-to-work programs may have important effects on the well-being of non-assignees that influence their net benefits in ways that do not get measured in cost-benefit analyses. Three such effects are deterrent effects, equilibrium wage effects, and displacement effects.

Deterrent effects will occur if some individuals who would have otherwise applied for AFDC benefits decide not to do so because of the requirement that they participate in a welfare-to-work program. Such individuals therefore become part of the non-assignee group. Although these individuals will presumably be worst off because they will not receive AFDC payments to which they are entitled, taxpayers who also belong to the non-assignee group will be better off because they will not have to pay for these benefits. These effects on the two subgroups of non-assignees should be more or less offsetting and, thus, have little overall effect on the net benefits resulting from welfare-to-work programs. However, some persons who are deterred may seek jobs and receive increased earnings as a result. If so, this would increase the net benefits of the non-assignee group. Unless deterrent effects are large, however, any increases in earnings that they generate are likely to be small.

If participants in a welfare-to-work program search harder for jobs or work more weeks or hours than they otherwise would, the resulting increase in labor supply will tend to reduce the equilibrium wage in the labor markets in which they find jobs. Thus, workers who are employed in the same labor markets could receive lower wages than they otherwise would. However, in practice, this effect is unlikely to be very large because the number of welfare recipients who find jobs as a result of welfare-to-work programs tends to be fairly small, and, consequently, they account for only a small share of all the workers in the labor markets in which they are employed. For example, welfare-to-work programs that have been evaluated increased employment rates among program group members by considerably less than five percentage points, on average (Greenberg, Cebulla, and Bouchet 2005).

Finally, displacement effects occur if welfare recipients who find jobs as a result of a welfare-to-work program hold jobs that individuals who are in the non-assignee group would otherwise have held. If these non-assignees suffer periods of unemployment or accept lower-wage jobs as a result, then their earnings fall, and the net benefits of the non-assignee group is lower than they otherwise would be. Not much is known about the magnitude of displacement effects, although one recent British study suggests they could be small (Blundell et al. 2002). They are especially likely to be small if the local labor markets located near the site of a welfare-to-work program are tight and, as a consequence, alternative job opportunities are likely to be available for non-assignees. If local labor markets are slack, however, then the size of displacement effects could be substantial, and program net benefits could be overstated.

Value Taxpayers Place on Reductions in the Welfare Rolls

If non-assignees, especially taxpayers, positively value AFDC recipients leaving the welfare rolls and finding employment in and of itself—that is, beyond any tax savings they

may receive—then, to the extent taxpayers are willing to pay for such an outcome, this is a program benefit. Given practical difficulties, however, this potential benefit has never been measured.[9] Nonetheless, the benefit may not be very large because welfare-to-work programs have typically reduced the rate of receipt of AFDC among program group members by only two or three percentage points (Greenberg, Cebulla, and Bouchet 2005) and, as mentioned above, have increased employment rates by less than five percentage points.

To summarize, there are at least three reasons to believe that the estimates that appear in Tables 3 and 4, which are already modest in magnitude, appreciably overstate the net benefits of welfare-to-work programs: cost-benefit analyses tended not to be conducted for welfare-to-work programs with relatively small impacts on earnings and AFDC benefits, the value of the non-market time that members of the program group lost are not counted; and losses to non-assignees that resulted from displacement effects are omitted. There is some evidence that the first two issues are important, while there is considerable uncertainty about whether the third factor is of much consequence. There is also at least one reason to think that the net benefit estimates in Tables 3 and 4 may be appreciably understated: taxpayers receive intangible benefits as a result of program group members leaving the welfare rolls and going to work. Although it is possible that this last factor could dominate the other three, given the rather small impacts of welfare-to-work programs on the size of the welfare rolls and on employment rates, this seems unlikely. Thus, we conclude that the net benefits of welfare-to-work programs are probably even more modest than cost-benefit studies of them have suggested.

CAN WELFARE-TO-WORK PROGRAMS BE MADE MORE EFFECTIVE?

Although welfare-to-work programs typically do seem to have positive net benefits regardless of perspective, these benefits are typically quite modest. In this section, we explore whether these net benefits can be improved.

Regressions on Net Benefits

To determine the factors that might influence the size of the net benefits from welfare-to-work, we conducted a regression analysis with the net benefit estimates from the program group, non-assignee, and the societal perspectives as dependent variables. We do not report the government perspective because the results are virtually identical to those for the non-assignee perspective. The explanatory variables include measures of each program's effects on sanctions and the receipt of services (i.e., the difference between the program group and the control group in sanction rates and rates of participation in services), dummy variables for whether the program tested financial incentive and time limits, measures of the characteristics of the participants in each program, and site characteristics (i.e., the characteristics of the socio-economic environment in which each of the programs operated). Because the primary goal of the regression analysis is to see if there are ways in which the net benefits of welfare-to-work programs can be increased, the effects of program characteristics are of greatest interest because they are at least partially under the control of those designing and operating the programs.

The considerable variation among the net benefit estimates for mandatory welfare-to-work programs was discussed earlier. It is this variation that the regressions attempt to explain. The regressions can never succeed in explaining all of the variation in the net benefit estimates, however, because it is due both to true differences among programs and to

sampling error. It also results because cost-benefit analyses of welfare-to-work programs require various assumptions (see Boardman et al., 2006, Chapter 11 for a detailed discussion)—for example, concerning how long program impacts on earning and transfer benefits persist—and different evaluators make somewhat different assumptions. Both sampling error and the required assumptions suggests that the individual net benefit estimates are subject to errors and these errors may differ in unsystematic ways across the studies.

There is a possibility that the measures of program participation rates that are used as explanatory variables in the regressions are endogenously determined. This could occur, for example, if programs that have a client population of individuals who are mostly job ready (e.g., as a result of previous work experience) tend to stress job search, while programs with large fractions of clients who are not job ready tend to emphasize basic education or vocational training. Similarly, programs that are located at sites with available jobs might tend to emphasize job search and those at locations with few jobs might make more use of work experience. Under these circumstances, program characteristic measures would, in part, reflect client and site characteristics, causing estimates of the relation between these measures and program impacts to be biased. It should be borne in mind, however, that the regressions control directly for client and site characteristics. Moreover, the program characteristic variables that we actually use in the regressions are measured in terms of the degree to which each program *changes* the pre-program regime—that is, the difference between service participation rates and sanction rates for the program group and the control group. Although programs may reflect the characteristics of the available client population and the surrounding environment, it is not apparent that *changes* in how programs are run would be affected by client and site characteristics, assuming that these characteristics remain fairly stable.

For reasons discussed earlier, the regressions are weighted by the total sample size used in each study. However, we also computed unweighted regression estimates (see appendix Table A1), which turned out to be similar. The regression estimates are presented in Table 5.[10] Because of inaccuracies and inconsistencies in the estimates of net benefits, because cost-benefit analyses are conducted for an unrepresentative subset of all evaluations, and because it was not possible to use the weighting scheme recommended in the meta-analysis literature, findings from these regressions should be considered as exploratory and viewed with caution.

Interpreting the Net Benefit Regressions

Perhaps, at least in part, because of the shortcomings just discussed, a number of the coefficients in Table 5 are relatively small in absolute magnitude and are statistically insignificant. However, there may be other reasons as well, and these can have important policy implications. For example, factors that cause a welfare-to-work program to successfully increase the earnings of the program group may also reduce their transfer benefits and, consequently, result in little net gain for program participants. Similarly, a program component that successfully reduces expenditures on transfer payments also costs something to provide. Thus, the net benefits received by taxpaying non-assignees from paying for the component may be small or even negative.

To interpret Table 5, therefore, it is necessary to see how the explanatory variables listed in the table affect the operating costs of welfare-to-work programs and program impacts on earnings and transfer benefits. To aid in doing this, Table 6 reports estimates from weighted regressions in which program impacts on earnings and AFDC payments during the 3rd and 7th quarters after random assignment and program operating costs are used as dependent variable and the explanatory variables are the same as those used in Table 5.

Total sample size is used for weighting purposes in the regression on costs, but the inverse of the variance of the impact estimates is used in the remaining regressions because they are available and the meta-analysis literature recommends weighting in this way. Because the regressions in Table 6 are intended to help in interpreting the regressions in Table 5, with one minor exception, the same set of programs are used as observations.[11] The exception occurs because estimates of AFDC payment impacts were not available in the 3rd and 7th quarters for three programs.

The findings for sanction rates provide a good illustration of how the results in Table 6 can help in interpreting Table 5. The coefficients on this variable are positive in the earnings regressions, although not very precisely estimated, but negative in the AFDC payments regressions and highly statistically significant at conventional levels. For example, the point estimates for the 7th quarter implies that a one percentage point increase in the sanction rate increased 7th quarter earnings by $4.70, but decreased 7th quarter AFDC payments by $4.12. Thus, it is not surprising that the weighted regressions in Table 5 imply that an increase in sanction rates has a negligible effect on the net benefits received by the program group.

The reduction in AFDC payments resulting from increasing sanction rates should increase the net benefits received by non-assignees. To a lesser extent, the improvement in earning should also work in this direction because tax revenues will tend to increase. However, the first column of Table 6 indicates that a one percentage point increase in the sanction rate cost about $90 for each program group member, presumably because of agency expenditures required for administering and enforcing sanctions. Given these off-setting effects, Table 5 indicates that an increase in sanction rates has a negligible effect on the net benefits received by non-assignees.

The implications of program impacts on the receipt of various program services can be traced through in a similar fashion. For example, there is some suggestion in Table 6 that greater use of job search increases the earnings of participants (the coefficient is significant at the 10-percent level in the 3rd quarter and barely misses statistical significance in the 7th quarter), and there is a strong indication that it also reduces their transfer benefits. Thus, as implied by the first column of Table 5, the net effect of job search on benefits received by those assigned to welfare-to-work programs is small and statistically insignificant. However, Table 5 also suggests that increases in participation in job search do not increase net benefits received by non-assignees and may even decrease them a bit. The latter finding is counter-intuitive because, as suggested by Table 6, participation in job search results in very small increases in the costs of programs, while also causing an appreciable reduction in transfer payments. The apparently small cost of job search, although imprecisely estimated, is consistent with the usual view on this topic, and helps explain why job search is a major component of most welfare-to-work programs.

Table 6 indicates that program net costs increase by over $80 per program group member for every percentage point increase in participation in basic education. The table also suggests that basic education has a more or less negligible net effect on the earnings and transfer receipts of program group members. Thus, there are few benefits to offset these costs. Consequently, it is not surprising that increased participation in basic education significantly reduces the net benefits to non-assignees, but has little effect on the welfare of the program group.

Table 6 also implies that there is a negative relationship between program effects on participation in vocational training and both earnings and AFDC payment amounts.[12] These negative relationships presumably account for the fairly large negative and statistically significant effect of increases in participation in vocational training on net benefits received

by program group members. Table 6 also indicates that program net costs increase by over $100 per program group member for every percentage point increase in participation in vocational training. The middle column in Table 5 suggests that these increases in costs are at least sufficient to offset any gains to non-assignees that result from the reductions in AFDC payments.

According to Table 5, increases in participation in work experience do not increase costs. This may be because work experience participants are often assigned to agencies other than those operating welfare-to-work programs and whatever operating costs are involved may not get incorporated into the estimates of net operating costs. Increases in participation in work experience also seem to have negligible effects on program impacts on earnings and AFDC payments. Consequently, Table 5 indicates that the relation between work experience and net benefits is small and statistically insignificant from each perspective.

Unsurprisingly, and as suggested both earlier and by the last two columns in Table 6, net benefits are larger for program group members assigned to programs that offer financial incentives than for persons assigned to programs that do not provide these incentives, and smaller for non-assignees. These effects are large and highly statistically significant. The first two columns in Table 5 indicate that the increases in net benefits to the program group are offset or nearly so by the reductions in the net benefits of non-assignees. Thus, the net social cost of financial incentives appears to be small or negligible, although, as shown in the first column of Table 6, they are fairly costly to administer. Because, as suggested by Table 6, they do little to increase earnings but result in small social costs, financial incentives that are provided through welfare-to-work programs are perhaps best viewed as simply transferring income from the government to low wage welfare recipients who find jobs in a relatively efficient manner. In contrast, some research has suggested that it typically costs taxpayers about $1.50 to $2.00 to transfer one dollar to low income persons via conventional

transfer programs (for example, see Gramlich, 1990, pp. 123-127 and Browning and Johnson, 1984). However, the results shown in Table 5 do not take account of potential social costs resulting from distortions in the labor supply and investment behavior of taxpayers, which may result from funding transfer programs.

The estimate in Table 5 that appears most illogical concerns time limits, which restrict the length of time AFDC recipients are allowed to receive payments. According to Table 5, time limits substantially increase the net benefits of the program group and society as a whole. Although time limits could, in principle, result in increases in the earnings of the program group, there is no evidence in Table 6 that they actually did so. However, the table does indicate that time limits cause AFDC payments to unexpectedly increase in the 3rd quarter after random assignment.

These perplexing findings are entirely attributable to the evaluation of the Indiana Welfare Reform Program. As previously mentioned, this evaluation used an extremely big sample and produced exceptionally favorable cost-benefit findings. In addition, of the 49 programs for which cost-benefit analyses were conducted, it was one of only five that tested time limits. If this observation is dropped from the analysis,[13] then the coefficient on time limits for the program group changes from $2,563 to -$801 and that for society falls from $2005 to $596. Moreover, neither of the new estimates come close to approaching statistical significance. In addition, 3rd quarter AFDC payments are no longer found to increase unexpectedly as a result of time limits.[14]

The remaining variables in Table 5 are contextual in nature and are included for mainly control purposes. In general, the coefficient estimates for them are straight-forward to interpret. For example, the number of years since 1982, the year prior to when random assignment for the first evaluation took place, was included in the cost-benefit regressions to

see if welfare-to-work programs have become more effective over time. There is no evidence that they have.

Welfare-to-work programs appear to reduce the transfer payments received by two-parent families by more than they diminish those received by one-parent families and are possibly less effective in increasing the earnings of such families. As a consequence, even though more has been spent in providing welfare-to-work services to two-parent families, these families appear to benefit substantially less than one-parent families from being assigned to welfare-to-work programs, and this translate into lower net benefits to society from requiring two-parent families to participate in these programs.

Table 6 suggests that members of the program group with recent employment experience (which was usually measured as having worked during the year prior to random assignment) lost less in transfer payments than those without recent jobs as a result of having been assigned to welfare-to-work programs. Yet, their earnings may have increased by somewhat more. As a consequence, their net benefits also appear to be larger.

The final statistically significant result in Table 5 implies that net benefits for the program group are larger in high poverty areas. Table 6 suggests that this is due to welfare-to-work programs reducing AFDC payments by considerably less in these areas.

CONCLUSIONS

The meta-analysis described in the article suggests that a typical welfare-to-work program, which has been evaluated by random assignment, *probably* has positive net benefits and, therefore, passes the cost-benefit test. However, these net benefits are likely so small that neither those who are assigned to the program nor the rest of the citizenry (i.e., non-assignees) are appreciably better off. Thus, it would appear that society gains little from

25

requiring welfare recipients to participate in welfare-to-work programs. Indeed, there may be alternative investments with a larger payoff.

This leads naturally to the question of whether these programs can be made more cost-beneficial. The regression analysis suggests several possibilities. There is some evidence that net benefits would increase if the programs were targeted mainly at heads of one-parent AFDC families with recent work experience. Indeed, two-parent AFDC families appear to be substantially worst off as a result of being required to participate in welfare-to-work programs, with little off-setting gain to non-assignees.

Vocational training seems to reduce the net benefits of both program assignees and non-assignees, while basic education has a negligible effect on the net benefits of assignees and a negative effect on the net benefits of non-assignees. This suggests two possibilities. The most obvious is that vocational training and basic education simply be dropped as a component of welfare-to-work programs. The less obvious is that vocational training and basic education be made more intensive for those who participate in it. After all, the increase in funds provided for most of the program interventions that we examined was quite small, averaging less than $1,500, and only part of this amount increased the resources available for vocational training and basic education. Thus, it is unlikely that the human capital of many participants in these program components was very much augmented. However, there is no real evidence that "throwing money" at vocational training and basic education will make welfare-to-work programs more cost-beneficial and some evidence from the regressions to the contrary.

If vocational training and basic education were dropped as components of welfare-to-work programs, the programs would then consist mainly of mandated job search, sanctions for not complying with the mandate, and financial incentives. Sanctions appear to have negligible effects on the net benefits received by both program assignees and non-assignees.

Job search also appears that have little effect on the net benefits of assignees; although there is some reason to think that job search has a positive effect on the net benefits of non-assignees, this effect does not show up in our findings.

The usefulness of financial incentives is more difficult to assess. As expected, they increase the incomes of incomes of assignees, while decreasing those of non-assignees. Moreover, the amounts involved are more or less offsetting, suggesting that if a dollar of benefits received by assignees are valued equally to a dollar of costs incurred by non-assignees, then financial incentives are a relatively efficient means of transferring income. If increases in the incomes of assignees, a group that consists mainly of the disadvantaged, are valued by more than an equal decrease in the incomes of non-assignees, who are relatively well-off, then financial incentives would be viewed even more positively.

REFERENCES

Bell, S. H. and Orr, L. L. (1994), "Is Subsidized Employment Cost Effective for Welfare
Recipients? Experimental Evidence from Seven State Demonstrations," *Journal of
Human Resources*, Vol. 19, No. 1, pp. 42-61.

Blundell, R. Costa Dias, M., Meghir C., and Van Reenen, J., (2002), *Evaluating the
Employment Impact of a Mandatory Job Search Program: The New Deal for Young
People in the UK*," unpublished manuscript, London: University College and Institute
for Fiscal Studies.

Boardman, A., Greenberg, D., Vining, A., and Weimer, D. (2006). *Cost-benefit Analysis:
Concepts and Practice, 3rd edition.* Upper Saddle River, NJ: Prentice Hall.

Browning, E. and Johnson, W. (1984). "The Trade-Off between Equality and Efficiency,"
Journey of Political Economy, Vol. 92, No.2, pp175-203.

Gramlich, E. M. (1990). *A Guide to Cost-Benefit Analysis*. Englewood Cliffs, NJ: Prentice
Hall.

Greenberg, D. (1997). "The Leisure Bias in Cost-Benefit Analyses of Employment and
Training Programs," *Journal of Human Resources*, Vol. 32, No. 2, pp. 413-439.

Greenberg, D., Ashworth, K., Cebulla, A., Walker, R. (2004). "Do Welfare-to-Work
Programmes Work for Long?" *Fiscal Studies*, Vol. 25, No. 1, pp. 27-53.

Greenberg, D., Cebulla, A., and Bouchet, S. (2005). *Report on a Meta-Analysis of Welfare-
to-Work Programs*, Discussion Paper no. 1312-05, Madison, WI: Institute for
Research on Poverty.

Greenberg, D. H. and Robins, P. K. (2005). *Incorporating Nonmarket Time Into Benefit-Cost
Analyses of Social Programs: An Application to the Self-Sufficiency Project*,
unpublished manuscript.

Grogger, J., Karoly, L. A., and Klerman, J. A. (2002). *Consequences of Welfare Reform: A Research Synthesis*, DRU-2676-DHHS, Santa Monica, CA: The Rand Corporation.

Hall, J. A., Tickle-Degnen, L., Rosenthal, R., and Mosteller, F. (1994). "Hypotheses and Problems in Research Synthesis," in H. Cooper and L.V. Hedges (eds.), *The Handbook of Research Synthesis*, New York: Russell Sage Foundation.

Hedges, L. V. (1994). "Fixed Effects Models," in H. Cooper and L. V. Hedges (eds.), *The Handbook of Research Synthesis*, New York: Russell Sage Foundation.

Lipsey, M. and Wilson, D. B. (2001). *Practical Meta-Analysis*. Thousand Oaks, CA: Sage.

Shadish, W. R. and Haddock, C. K. (1994). "Combining Estimates of Effect Size," in H. Cooper and L.V. Hedges (eds.), *The Handbook of Research Synthesis*, New York: Russell Sage Foundation.

Table 1
STYLIZED COST-BENEFIT FRAMEWORK OF WELFARE-TO-WORK PROGRAMS

	Program Group (A)	Non-Assignees (B)	Government (C)	Society (A + B) (D)
In-Program Output	0	+	0	+
Earnings	+	0	0	+
Tax Payments by Program Group	-	+	+	0
AFDC Payments	-	+	+	0
Other Transfer Payments	-	+	+	0
Net Program Operating Costs	0	-	-	-
Net Benefits of Program (column sum)	?	?	?	?

Table 2
U.S. WELFARE-TO-WORK EVALUATIONS INCLUDED IN THE DATABASE

Program Title	Short Program Name	Evaluator/Author	Mid-point of Random Assignment	Cost-Benefit Analysis Conducted	Financial Incentive Scheme
Greater Avenues for Independence Program	GAIN (California)	MDRC	1989	√	
Job Search and Work Experience in Cook County	Cook County	MDRC	1985	√	
Community Work Experience Demonstrations	West Virginia	MDRC	1983	√	
WORK Program	Arkansas	MDRC	1983	√	
Employment Initiatives	Baltimore	MDRC	1983	√	
Saturation Work Initiative Model	SWIM (San Diego)	MDRC	1985	√	
Employment Services Program	Virginia	MDRC	1984	√	
Project Independence (Florida's JOBS Program)	Florida	MDRC	1991	√	
Jobs First	Connecticut	MDRC	1996	√	√
The Family Transition Program	FTP (Florida)	MDRC	1994	√	
The Los Angeles Jobs-First GAIN Evaluation	Los Angeles	MDRC	1996	√	
The San Diego Job Search and Work Experience Demonstration	San Diego	MDRC	1983	√	
National Evaluation of Welfare-to-Work Strategies	NEWWS	MDRC	1993	√	
Minnesota Family Investment Program	MFIP	MDRC	1994	√	√
Vermont's Welfare Restructuring Project.	Vermont	MDRC	1995	√	√
Teenage Parent Demonstration	Teenage Parents	Mathematica Policy Research (MPR)	1988		
Wisconsin Welfare Employment Experiment	Wisconsin	University of Wisconsin	1988		
Ohio Transitions to Independence Demonstration	Ohio	Abt Associates	1990	√	
The Indiana Welfare Reform Program	Indiana	Abt Associates	1995	√	
Saturation Work Program.	Philadelphia	PA Department of Public Welfare	1986		
To Strengthen Michigan Families	TSMF (Michigan)	Abt Associates	1993		
A Better Chance	ABC (Delaware)	Abt Associates	1996		
Virginia Independence Program	VIEW	MPR	1996		
Family Investment Program	FIP(Iowa)	MPR	1994	√	
Personal Responsibility and Employment Program	PREP (Colorado)	The Centers of the University of Colorado	1995		
California Work Pays Demonstration Program (financial incentive only)	CWPDP	UCLA School of Public Policy and Social Research	1993		√
Child Assistance Program	CAP (New York)	Abt Associates	1989	√	√

Table 3
SUMMARY STATISTICS FOR COST-BENEFIT OUTCOMES IN YEAR 2000 DOLLARS

	MEAN		STANDARD DEVIATION		MEDIAN		MINIMUM	MAXIMUM	NO OF OBS
	Unweighted	Weighted	Unweighted	Weighted	Unweighted	Weighted			
PERSPECTIVE									
PROGRAM GROUP	121	258	3002	2602	-54	705	-6559	11040	49
NON-ASSIGNEES	-188	561	3133	2660	436	966	-9372	5287	49
GOVERNMENT	-236	544	3088	2629	246	948	-9207	5287	49
SOCIETY	-68	818	2935	2617	85	1668	-5904	5966	49
NET PROGRAM OPERATING COST	1967	1378	1861	1541	1346	1216	-290	8613	49

Table 4
COST-BENEFIT OUTCOMES FOR PROGRAMS WITH AND
WITHOUT FINANCIAL INCENTIVES IN YEAR 2000 DOLLARS

	WEIGHTED MEANS		WEIGHTED MEDIANS	
	FINANCIAL INCENTIVES—			
	Yes	No	Yes	No
PERSPECTIVE				
PROGRAM GROUP	3301	-274	1459	705
NON-ASSIGNEES	-2470	1090	-7	1940
GOVERNMENT	-2384	1056	45	1787
SOCIETY	830	816	1263	1931
NUMBER OF OBSERVATIONS	11	38	11	38

Table 5
WEIGHTED REGRESSION MODELS OF THE IMPACTS OF THE
COST-BENEFIT OUTCOMES FROM THREE ALTERNATIVE PERSPECTIVES
(Standard errors in parentheses)

PERSPECTIVE	PROGRAM GROUP	NON-ASSIGNEES	SOCIETY
Constant	-13231.68 **	2779.18	-10452.50 *
	(6394.2)	(6852.3)	(5385.4)
Intervention impact on	11.64	10.32	21.96
% sanctioned	(83.3)	(89.3)	(70.2)
Intervention impact on	7.20	-13.73	-6.53
% participated in job search	(31.4)	(33.7)	(26.5)
Intervention impact on	36.69	-129.17 **	-92.48 *
% participated in basic education	(56.0)	(60.0)	(47.1)
Intervention impact on	-379.68 ***	-55.45	-435.14 ***
% participated in vocational training	(131.5)	(141.0)	(110.8)
Intervention impact on	59.47	18.09	77.55
% participated in work experience	(95.3)	(102.1)	(80.2)
Intervention included financial	4686.41 ***	-4351.58 ***	334.83
incentive=1	(1046.0)	(1121.0)	(881.0)
Intervention included time limit=1	2563.02 **	-558.41	2004.61 *
	(1223.8)	(1311.5)	(1030.7)
Number of years since 1982	-178.90	-42.94	-221.84
	(184.4)	(197.6)	(155.3)
Two-parent family target group=1	-5164.13 ***	-10.16	-5174.29 ***
	(1649.9)	(1768.1)	(1389.6)
Average age of target group	185.75	11.56	197.31
	(200.9)	(215.3)	(169.2)
% of target group with recent	115.58 ***	6.68	122.26 ***
Employment	(38.4)	(41.1)	(32.3)
Annual % change in local	-31.82	195.18	163.36
manufacturing employment	(129.5)	(138.8)	(109.1)
Poverty rate	258.31 **	-86.40	171.91 *
	(111.4)	(119.4)	(93.9)
Adjusted R-squared	0.359	0.296	0.551
F-test	3.071 ***	2.554 **	5.525 ***
Number of observations	49	49	49

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

TABLE 6
WEIGHTED REGRESSION ESTIMATES OF THE DETERMINANTS OF THE COSTS AND THE IMPACTS OF WELFARE-TO-WORK PROGRAMS ON EARNINGS AND AFDC RECEIPTS DURING THE 3RD AND 7TH QUARTERS 7
(Standard errors in parentheses)

	COSTS	EARNINGS		AMOUNT OF AFDC	
		3rd Quarter	7th Quarter	3rd Quarter	7th Quarter
Constant	-5442.12 **	-309.97	13.87	-71.79	-1.47
	(2613.2)	(203.9)	(206.6)	(87.8)	(90.9)
Intervention impact on	90.81 **	2.31	4.70 *	-2.29 **	-4.12 ***
% sanctioned	(34.0)	(2.3)	(2.7)	(1.0)	(1.1)
Intervention impact on	12.08	1.80 *	1.73	-1.16 **	-1.87 ***
% participated in job search	(12.8)	(1.0)	(1.1)	(0.4)	(0.5)
Intervention impact on	81.94 ***	-1.01	0.02	-0.49	-0.64
% participated in basic education	(22.9)	(1.7)	(1.8)	(0.9)	(0.9)
Intervention impact on	137.18 **	-8.41 **	-3.25	-2.65	-4.06 **
% participated in vocational training	(53.8)	(4.0)	(3.9)	(1.7)	(1.7)
Intervention impact on	-53.25	0.90	0.50	1.05	2.64
% participated in work experience	(38.9)	(3.0)	3.7	(1.4)	(1.6)
Intervention included financial	846.29 *	-23.71	-29.36	178.47 ***	140.81 ***
incentive=1	(427.5)	(30.6)	(30.6)	(13.1)	(11.6)
Intervention included time limit=1	894.89 *	-27.12	-18.13	43.73 ***	-17.05
	(500.2)	(35.0)	(37.6)	(14.3)	(15.0)
Number of years since 1982	0.76	2.61	4.70	-2.54	1.21
	(75.3)	(4.7)	(6.4)	(2.2)	(2.9)
Two-parent family target group=1	1250.26 *	-31.44	-33.88	-115.37 ***	-41.84
	(674.3)	(52.9)	(55.1)	(33.3)	(30.3)
Average age of target group	222.76 ***	6.27	1.69	-2.20	-4.88
	(82.1)	(6.5)	(6.8)	(3.1)	(3.3)
% of target group with recent	-33.45 **	2.22 *	0.00	1.16 **	1.25 **
Employment	(15.7)	(1.2)	(1.1)	(0.5)	(0.5)
Annual % change in local	-71.55	6.25	6.06	-3.54 **	-7.81 ***
manufacturing employment	(52.9)	(4.1)	(4.4)	(1.7)	(2.1)
Poverty rate	-21.37	4.17	-3.44	5.70 ***	5.99 ***
	(45.5)	(3.1)	(3.8)	(1.6)	(1.6)
Adjusted R-squared	0.695 ***	0.467	0.169	0.439	0.403
F-test	9.410	4.235 ***	1.752 *	3.710 ***	3.333 ***
Number of observations	49	49	49	46	46

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

Appendix Table A1
UNWEIGHTED REGRESSION MODELS OF THE IMPACTS OF THE
COST-BENEFIT OUTCOMES FROM THREE ALTERNATIVE PERSPECTIVES
(Standard errors in parentheses)

PERSPECTIVE	PROGRAM GROUP	NON-ASSIGNEES	SOCIETY
Constant	-11496.80 *	8764.	-2732.62
	(6640.3)	(7405.4)	(6347.3)
Intervention impact on	-33.06	-3.53	-36.59
% sanctioned	(79.1)	(88.2)	(75.6)
Intervention impact on	34.87	-44.85	-9.98
% participated in job search	(32.8)	(36.6)	(31.3)
Intervention impact on	73.10	-155.10 ***	-81.99 *
% participated in basic education	(49.9)	(55.7)	(47.7)
Intervention impact on	-351.44 **	-7.87	-359.30 **
% participated in vocational training	(142.0)	(158.3)	(135.7)
Intervention impact on	78.55	-27.70	50.84
% participated in work experience	(84.3)	(94.0)	(80.6)
Intervention included financial incentive=1	5096.89 ***	-5467.36 ***	-370.47
	(1360.6)	(1517.3)	(1300.5)
Intervention included time limit=1	1206.90	311.89	1518.80
	(1547.3)	(1725.6)	(1479.1)
Number of years since 1982	-100.56	-115.97	-216.53
	(148.2)	(165.2)	(141.6)
Two-parent family target group=1	-4401.30 ***	749.53	-3651.76 **
	(1464.7)	(1633.5)	(1400.1)
Average age of target group	142.43	-32.27	110.17
	(186.8)	(208.3)	(178.5)
% of target group with recent employment	79.22 **	-13.94	65.28 *
	(38.1)	(42.5)	(36.4)
Annual % change in local	61.40	201.89	263.29 *
manufacturing employment	(143..0)	(159.5)	(136.7)
Poverty rate	234.74 **	-234.21 **	0.53
	(95.7)	(106.7)	(91.5)
Adjusted R-squared	0.356	0.264	0.384
F-test	3.039 ***	2.327 **	3.301 ***
Number of observations	49	49	49

***significant at the 1-percent level; **significant at the 5-percent level; *significant at the 10-percent level.

[1] For a detailed description of issues involved with conducting cost-benefit analyses of welfare-to-work programs, see Chapter 11 of Boardman et al. 2006.

[2] For example, losses in non-market time that result if welfare-to-work programs cause participants to work more hours do not appear in the table. However, this topic is discussed later.

[3] Between 1982 and 1996, changes in state AFDC programs usually could not be made without first obtaining waivers from the U.S. Department of Health and Human Services (DHHS). In most such instances, DHHS required random assignment evaluations of the changes as a condition for receiving waivers. We have checked with DHHS to verify that the 27 studies listed in Table 2 include all the random assignment evaluations of mandatory welfare-to-work programs that began in the U.S. between 1982 and 1996. With the exception of two evaluations that had severe technical problems and consequently are excluded from our analysis, they do.

[4] In 1997, AFDC was replaced by Transitional Aid for Needy Families (TANF).

[5] Cost benefit analysts usually value these services by determining what it would have cost to have hired a worker in the labor market to perform the same duties.

[6] Operating costs are government expenditures on providing program services, but do not include transfers of income, such as AFDC payments.

[7] For a discussion of additional reasons for treating high and low income persons differently in cost-benefit analysis, see Chapter 18 of Boardman et al. (2006.)

[8] The weights used for this analysis are the inverse of the variance of the impact estimates.

[9] One approach that could be used for this purpose is contingent valuation, which utilizes surveys to attempt to measure the willingness to pay for goods not exchanged in markets.

[10] In Tables 5, the mean of the observed values for a variable is assigned to all observations for which the value is missing. Five of the 49 values for two variables (sanction rates and recently employed) are missing. Three other variables have one or two missing values. The remaining variables have no missing values.

[11] As previously indicated, estimates of impacts on earnings and AFDC payments are available from a considerable number of evaluations that did not conduct cost-benefit analyses. Because of the larger number of observations and the fact that cost-benefit analyses tended not to be conducted for programs with small impacts, regressions run on all the available impact estimates differed somewhat from those reported in Table 6. In particular, there were more statistically significant coefficients. For example, increasing participation in work experience had a significant positive effect on impacts on the amount of AFDC received in the 7th quarter after random assignment. Testing time limit in a welfare-to-work program had a negative and statistically significant effect on AFDC payments in both the 3rd and the 7th quarters (in Table 6, in contrast, the coefficient on time limits is positive and significant in the AFDC payment regression in the 3rd quarter). In addition, the annual percentage change in manufacturing employment was significantly related to earning impacts, as well as impacts on AFDC payments. Finally, increasing sanction rates and the use of job search had a more significant positive effect on earnings impacts than is apparent in Table 6 (Greenberg, Cebulla, and Bouchet 2005).

[12] Note that the major effect of vocational training on earnings occurs in the 3rd quarter and the major effect of vocational training on AFDC payments take place in the 7th quarter. This could result if some individuals tend to delay taking jobs for longer than they otherwise would because they are receiving vocational training, while other individuals who were assigned to vocational training failed to participate (perhaps because they were already engaged in undeclared work), but there was a delay before they were dropped from the AFDC rolls.

[13] On the one hand, there is little justification for ignoring the evaluation of the Indiana Welfare Reform Program. On the other hand, reaching a policy conclusion on the basis of a result that is driven solely by one of 49 programs seems contrary to the objectives of meta-analysis.

[14] With one exception, the findings for the other explanatory variables do not appreciably change when the Indiana Welfare Reform Program is excluded from the analysis. The exception is for financial incentives, which were not tested by the Indiana Welfare Reform Program. Specifically, the absolute value of the coefficient on this variable increases from $4,686 to $8,375 for the program group, from -$4,352 to -$6,494 for

the non-assignee group, and from \$335 to \$1,881 for society, although neither estimate of the latter coefficient comes close to statistical significance.