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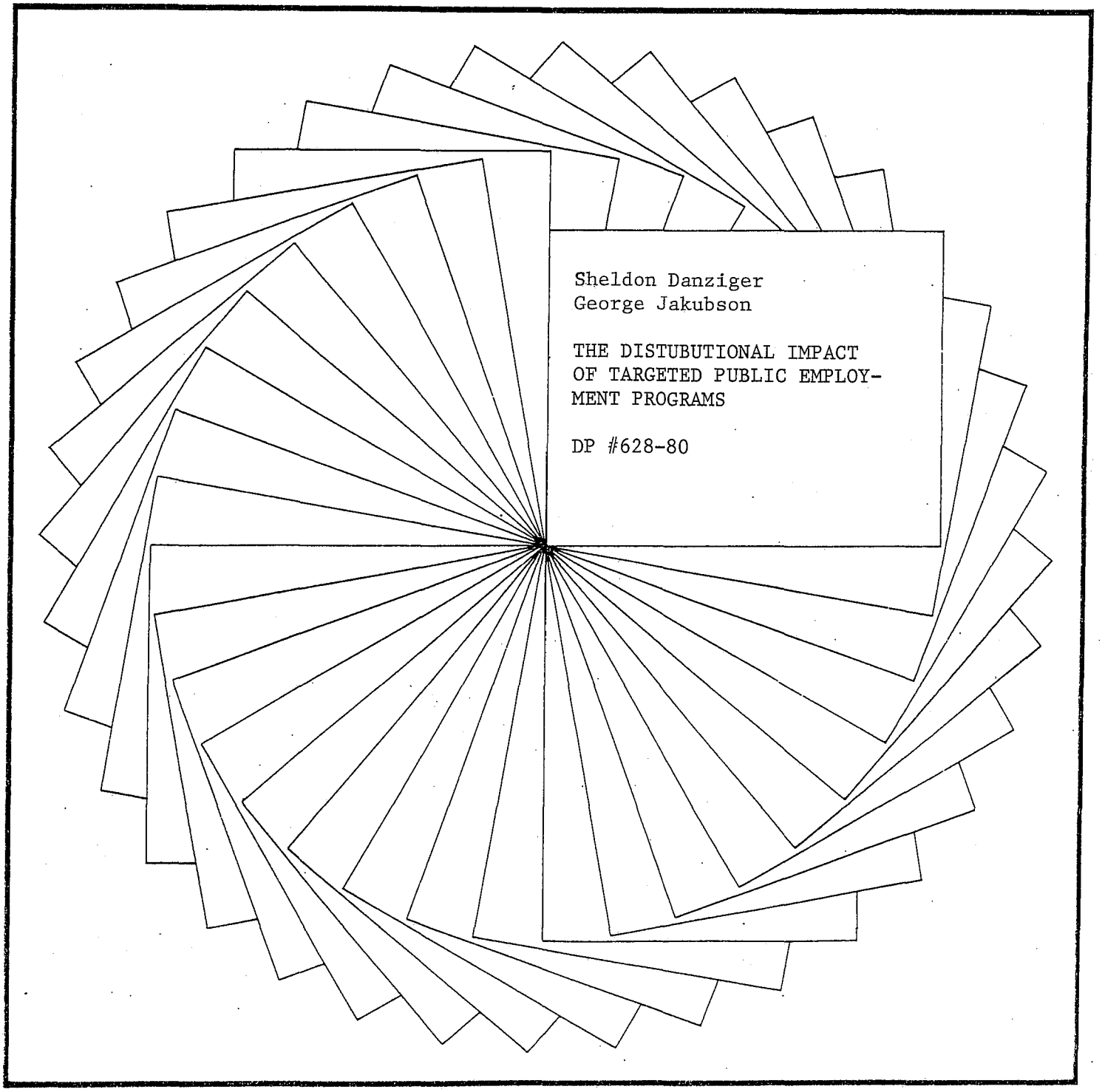

The Distributional Impact of Targeted Public Employment Programs

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ABSTRACT

Policymakers and the public in the U.S. espouse a preference for an antipoverty strategy which increases the earned income of the poor. Despite this preference, most of the reduction in poverty in the last 15 years has been due to increased government income transfers, and not to increased employment and earnings. As a result, several recent policy initiatives have emphasized the provision of public service jobs targeted on the poor.

This paper discusses the evolution of employment and training as a component of antipoverty policy. It then analyzes the potential for integrating targeted public employment programs with existing income maintenance programs for those among the poor who are able to work. Our empirical results are based on a recent demonstration project--Supported Work--which gathered detailed information on both efficiency and distributional effects. We conclude that increased expenditures on targeted public employment would achieve similar anti-poverty objectives but have more desirable effects on work effort than would an equivalent expansion of current transfer programs. However, these employment programs are best viewed as a complement to, and not a substitute for, existing income maintenance programs.

The Distributional Impact of Targeted Public Employment Programs

THE EVOLUTION OF TARGETED PUBLIC EMPLOYMENT AS AN ANTIPOVERTY POLICY

The War on Poverty was declared by President Johnson in January 1964. The strategy proposed included a broad range of policy instruments, but did not include either a public jobs program for the poor or an increase in cash assistance (except for the aged and disabled), although these were advocated by several policymakers at the time. Such approaches were viewed as not consistent with prevailing theories of the causes of poverty, which were based on the assumptions that (1) sufficient jobs existed in the private economy or could be generated through Keynesian policies (like the 1964 tax cut), (2) inadequate levels of education and training of poor individuals were at the core of the poverty problem ,and (3) antipoverty strategy had to be consistent with the American work ethic. Given these assumptions, an antipoverty strategy that stressed the delivery of services to build the human capital of the poor, rather than one that altered private labor market processes or guaranteed an annual income, was in order. The view that the poverty problem could be solved by "adapting and enriching" the poor was consistent with both the "culture of poverty" perspective and the human capital model, and thus was widely held. Moreover, the American work ethic--"if you work hard, you will get ahead"--called for programs which provided an opportunity to escape poverty by one's own efforts in the regular labor market, rather than programs which provided cash assistance or special government jobs.

There were recognized flaws that impeded the smooth functioning of labor markets--e.g., discrimination and inadequate labor market

information--but it was believed that these could be remedied by government actions, such as antidiscrimination legislation and the Employment Service, that did not require the public provision of jobs. Policies to foster high employment and economic growth could increase the demand for the labor of the poor while manpower, education, and training programs could increase the quantity and quality of the labor the poor supplied. The poor could then escape poverty in the same manner as the nonpoor--through the regular labor market.

There was, of course, the recognition that this approach would only work for those who could participate in the labor force, especially those who were able-bodied and nonaged. Nonetheless, this emphasis on the provision of opportunity rather than on the direct provision of jobs or income maintenance is reflected in Johnson's remarks of August 20, 1964 when he signed the Economic Opportunity Act.

> We are not content to accept the endless growth of relief rolls or welfare rolls. We want to offer the forgotten fifth of our people opportunity and not doles.
>
> That is what this measure does for our times.
>
> Our American answer to poverty is not to make the poor more secure in their poverty, but to reach down and to help them lift themselves out of the ruts of poverty and move with the large majority along the high road of hope and prosperity.
>
> The days of the dole in this country are numbered.

Perhaps more important than the "antidole" philosophy was the expectation that either a jobs program or an income maintenance program (or both) would be very expensive, and could not be financed without a tax increase.

In the years following the declaration of the War on Poverty, a number of training programs were established or expanded. Although

diverse in their goals and the groups which they served, they shared a common focus--the enhancement of individual skills through classroom or on-the-job education and training. Program graduates were given job search assistance and then launched into the labor market to compete for employment positions. There was little concern with the public provision of postprogram jobs because unemployment rates in the late 1960s had fallen to historically low levels.

Despite the subsequent proliferation of programs, total expenditures on employment and training have represented less than 5 percent of government funds directed toward the poor for the entire post-War on Poverty period.[1] Table 1 shows 1978 expenditures on major income maintenance and employment programs, and reveals the current magnitude of the differential between the two types of programs.

While a guaranteed jobs program continued to be viewed as unnecessary, a guaranteed income in the form of a negative income tax gained in popularity and was endorsed by a Presidential Commission in 1969. However, it was never adopted. But the income maintenance system did rapidly expand--both in terms of new programs and increased benefit levels in existing programs. Although a universal negative income tax was not legislated, many income maintenance programs adopted negative income tax principles. Because each program had its own marginal tax rate (usually 50 percent), and because many individuals participated in several programs, the typical transfer recipient found him/herself facing marginal tax rates that were higher than those facing most taxpayers. Thus, as greater transfer benefits became available to greater numbers of poor people, the work disincentives and the high budgetary costs of the

Table 1

Income Maintenance and Employment Programs, 1978

		Total Government Expenditures (in $ billion)
Income Maintenance Programs		$204
Social Insurance Programs		
Old Age and Survivors Insurance	OASDHI	81.2
Disability Insurance		12.7
Medicare		25.2
Unemployment Insurance		11.8
Workers Compensation		10.0
Veterans Disability Compensation		6.2
Railroad Retirement		4.0
Black Lung		1.0
Income Assistance Programs (Welfare)		
Medicaid		18.9
Aid to Families with Dependent Children (AFDC)		11.9
Supplemental Security Income (SSI)		7.4
Food Stamps		5.5
Veterans Pensions		3.3
General Assistance		1.2
Housing Assistance		3.7
Employment and Training Programs		$ 10.8
General Training and Employment		2.4
Public Service Employment		5.8
Youth Programs		1.5
Other		1.1

Source: Office of Management and Budget. Executive Office of the President. The Budget of the United States Government, Fiscal Year, 1980, Appendix. (Washington, D.C.: U.S. Government Printing Office, 1980).

NOTE: Social insurance programs condition benefits on contributions based on previous employment; income assistance programs condition benefits on current income and assets (means-tested).

transfer system were called into question. The public viewed increasing transfer recipiency as evidence that large numbers were choosing dependency and avoiding work.[2]

The incidence of income poverty declined significantly between 1965 and the present, primarily because of the expansion of the income maintenance system. The employment and training programs seem to have had little discernible effect on increasing the earnings of the disadvantaged, partly because they served only a small percentage of the poor.[3] The successes achieved by the income transfers were overshadowed by growing dissatisfaction with work disincentives and the increasing welfare rolls. A concern for both the antipoverty outcome and the process by which poverty was reduced resulted in a renewed search for an antipoverty strategy that could increase the work effort of the poor. The only method for increasing work incentives within existing income maintenance programs was a reduction in marginal tax rates.[4] Lowered tax rates, however, holding income guarantees constant, would greatly increase welfare costs and extend benefits to a larger number of households.

Because income maintenance policy could not both increase work effort and reduce the number of people receiving welfare, renewed attention was focused on employment and training policies. The labor supply orientation of the early employment and training programs had been called into question by the high unemployment rates--particularly those of youth, women, and minorities--of the 1970s. Concern shifted from merely augmenting the skills of the disadvantaged to increasing the number of employment opportunities. The first public service employment (PSE)

program since the Great Depression was enacted in 1971, primarily as a countercyclical device. PSE slots were increased by the Comprehensive Employment and Training Act of 1979 and have become the largest component of the employment and training budget. Amendments to this act in 1976 targeted a greater percentage of the PSE jobs on the disadvantaged, particularly the long term unemployed and welfare recipients.[5]

The negative income tax developments in income maintenance policy and the PSE jobs emphasis in employment and training policy were integrated in 1977 by President Carter's proposal to reform welfare, the Program for Better Jobs and Income (PBJI). PBJI proposed a universal negative income tax with one income guarantee for those not expected to work, and a lower income guarantee for those expected to work. The latter group was also eligible for a targeted PSE job. PBJI represents the first attempt by the federal government to guarantee jobs to the poor, and thus to remove the onus of poverty and unemployment from the individual.[6]

THE SUPPORTED WORK DEMONSTRATION[7]

Supported Work is a demonstration project, initiated in 1975 to provide jobs to disadvantaged workers, that offers an opportunity for examining the antipoverty potential of a program that links existing income maintenance programs with a guaranteed job strategy. A targeted public employment program in which persons with severe labor market disadvantages were employed in special settings for about one year, Supported Work

was designed to provide participants with work skills and attitudes that would increase their post-program employment and earnings. Participants worked in small groups with others with similar disadvantages under close and supportive supervision. Some of the placements were operated by the local Supported Work agency whereas others were contracted to private firms or government agencies. Demands for punctuality, attendance, and productivity were initially low, and slowly increased until normal labor market standards were reached. Wage rates began at low levels, and gradually increased as the participants advanced through the program. Participants were required to leave Supported Work after a specified period (usually 12 months) whether or not they had found another job. Job placement efforts were provided, and in some instances, a worker moved from a Supported Work job to a regular job in the same firm or agency.

There were four groups of disadvantaged participants in the Supported Work demonstration--former drug addicts, former law offenders, unemployed youth, and long-term welfare recipients who were female heads of household--and four control groups. Because of our concern with analyzing the integration of income maintenance and employment policy, we focus here only on the welfare group. All of the other participants had only limited access to existing income maintenance programs.[8]

To be eligible for Supported Work, a woman had to have limited employment experience (unemployed at time of program entrance, no more than 10 hours per week of employment in each of the last 4 weeks, and no regular job in the last 6 months), no child under the age of 6 years, and

significant welfare experience (currently a recipient, and on welfare for 30 of the past 36 months). Participation in Supported Work was voluntary.

Several aspects of the demonstration allow us to simulate the effects of a national program that combines an income maintenance with a targeted public employment strategy. First, detailed data on program costs and benefits, including the value of output produced by the participants, are available. Second, detailed background and longitudinal data gathered on the randomly chosen Supported Work participants, and on those randomly assigned to the control group who had full access to existing welfare programs, allow us to analyze the impact of the addition of a targeted public employment program to the existing welfare system. We used data gathered when the women were Supported Work participants to evaluate economic outcomes (employment, transfer recipiency, poverty, etc.) that would result from the implementation of a targeted public employment program which provided continuous access to a job--that is, a program that did not require participants to leave a targeted job after a specified period. Similarly, we used post-program data to evaluate economic outcomes that would exist if the program were transitional and provided access only for a one-year period. The data on controls enable us to gauge the effects of the existing welfare system.

METHODOLOGY

We chose a nationally representative sample from the March 1975 Current Population Survey (CPS) that closely approximated the population

of welfare recipients who were eligible for the Supported Work demonstration. Using the Supported Work data, we estimated a set of regressions for experimentals and a separate set for controls (described below). We then applied the regression coefficients from each of these samples to the characteristics of the women in the CPS, and imputed, to each woman in the CPS, values which predict her income and work effort in two situations--if she had access to (1) a Supported Work job, or (2) the current welfare system that was available to the controls. A maximization procedure in which each woman chooses the situation which yields maximum income was simulated; from it we derived the aggregate costs, benefits, and distributional effects that would result if a Supported Work type of program were to be implemented on a national scale.

We began with a sample of nonwhite female heads of household from the CPS. We restricted the sample to nonwhites since only 5 percent of the actual Supported Work participants were white. We further restricted the sample to women who seemed to satisfy the Supported Work eligibility criteria--i.e., women who were between the ages of 25 and 54, who had children between the ages of 6 and 18, and who lived in a household currently receiving welfare income.[9] This yields an eligible (weighted) sample of 459,037 nonwhite female household heads, which represents 25 percent of the 1,785,369 nonwhite female household heads in 1975, and 44 percent of those receiving welfare.

Regression Model and Antipoverty Effects

We used data gathered during the 9th month of the Supported Work demonstration to proxy a world in which any eligible woman could choose

to work at a targeted public job. At this point in the demonstration, the experimental group could claim a job. We used the data gathered during the 27th month of the demonstration to proxy the situation of a woman who had been eligible for a transitional job, but was not currently eligible. The 9-month data are used to simulate a state in which the women have continuous access to a targeted job. The 27-month data represent a state where the women have had only a one-time access to a job, and reveal the longer-run results of such transitional access.

For each time period (months 1-9 and months 19-27) and for each group of women (experimentals and controls) we estimated reduced form regressions for earnings, monthly hours of work, and other income (which was primarily income from government transfers). The explanatory variables in each of the regressions are personal characteristics that are available in both the Supported Work and the CPS data. The four sets of regression equations estimated with the Supported Work data appear in Appendix tables A1-A4.

Because many in the control sample were not working in either time period, and because many experimentals were not working after they had left Supported Work, we estimated many of the regressions as Tobits. The remaining regressions where the dependent variable rarely was zero were estimated using ordinary least squares.[10] For computational convenience, the two-step Tobit estimation suggested by Heckman (1980) was used. In the first step we estimated the probability that a woman will have positive earnings (or other income) as a function of her personal characteristics. A probit estimator was used for this first stage and the sample includes all women. In the second step we estimated, using

ordinary least squares, the amount of these earnings (or other income) for only those women with positive earnings (or positive other income). The independent variables include all the personal characteristics from the first step, plus a variable λ, which is a function of the probability that the woman has positive earnings (or positive other income).[11]

The estimated coefficients from the Supported Work experimentals were applied to the appropriate characteristics of the women in the CPS sample. This yields an estimate of what earnings, hours and other income would have been if the women had continuous access to a public employment program, and then if they had transitional access. The coefficients from the controls were used to produce estimates of the economic outcomes of the existing income maintenance programs.

Table 2 shows the mean value of the imputations for the CPS sample. Each woman has an imputation for each entry in the table. For example, contrast the first two columns. Column 1 shows that if all women had continuous access to a targeted public job, their mean annual income would be $6251, and they would work 134 hours per month. The jobs program increases work effort, earned income, and total income, and reduces other (transfer) income. If the women had not been in the program (column 2) they would have had a total income of only $3981, worked only 22 hours per month, and relied more heavily on income maintenance transfers (other income).

Columns 3 and 4 show that if the women had access to a targeted job for only a transitional period, the mean difference in economic well-being would significantly narrow in the long run (by 27 months). For those in a transitional, rather than a continuous program, earnings and

Table 2

Imputed Mean Values[a]

Dependent Variable	Continuous Access to Job[b]		Transitional Access to Job[c]	
	Targeted Public Employment Program	Existing Income Maintenance Only	Targeted Public Employment Program	Existing Income Maintenance Only
Earnings	$4525	$ 785	$2744	$1570
Other (transfer) income	1726	3196	2356	3070
Total income	6251	3981	5100	4640
Hours worked	134	22	64	45

Source: Regressions were estimated by the authors from Supported Work data tapes (see Appendix Tables) and then applied to the characteristics of eligible women drawn from the 1975 CPS data tape. The sample, when weighted, represents 459,037 nonwhite female heads of household from the CPS who, we predict, would be eligible for a Supported Work program.

[a]All imputed values are expressed in 1977 dollars. Earnings and income data are in annual terms; hours are monthly.

[b]Based on 9-month data.

[c]Based on 27-month data.

hours decline while other income increases; total income drops to $5100. The mean predicted differentials between those in the transitional program and those in the existing income maintenance system are only $460 annual income, and 19 hours.[12]

Table 3 presents the results of our simulation of the program's effects on the demand for targeted public jobs and on measured poverty.[13] Column 1, for the continuous access case, shows that 80.3 percent of the women who met our simulated Supported Work eligibility requirements would be poor if they merely had access to the current income maintenance system, whereas only 34.5 percent would be poor if they had access to the jobs program. If the women were free to choose the situation yielding the maximum income, all 459,037 would choose the job. Column 2 shows that the antipoverty effect of a transitional program is substantially lower. If there were no targeted employment program, 64.3 percent of the eligibles would be poor, whereas if there were a transitional jobs program, 56.3 percent would be poor. However, if the women could choose the situation which maximizes their income, 45.8 percent would be poor, and 295,161 women would choose a targeted job. This represents a substantial increase in the potential demand for public employment, since only about 600,000 slots were available for all persons in 1978.

The maximization procedure used in our simulation produced an upper-bound estimate of the effect of a jobs program on poverty and on the demand for targeted jobs because we did not value foregone leisure or fully account for all the costs and benefits of employment. A woman who finds her total income to be $1 higher with a job than with the

Table 3

Simulation Results: The Demand for Targeted Public Jobs and the Incidence of Poverty

	Access to Targeted Job:	
	Continuous[a]	Transitional[b]
1. Current income maintenance system only:		
Incidence of poverty[c]	80.3%	64.3%
2. Addition of targeted employment program:		
Incidence of poverty	34.5	56.3
3. Maximization:		
Incidence of poverty if women choose situation with highest income	34.5	45.8
Percentage of women choosing job	100.0	64.3
Number of jobs demanded	459,037	295,120

Source: See Table 2.

[a]Based on 9-month data.

[b]Based on 27-month data.

[c]The incidence of poverty is defined as the percentage of women in the group whose yearly income falls below the official government poverty lines.

current system chooses the job regardless of her change in leisure. Similarly, we neglected any costs associated with work (e.g., payroll taxes, child care costs, transportation expenses). The only neglected benefits from work are the social and psychological esteem she or her family may derive from her working and reduced welfare dependency, or the value of the employment experience in future periods.

This upper-bound bias is reinforced by the fact that participation in Supported Work was voluntary. Thus, if the experimentals were more highly motivated than average, their gains from the program might be higher than those that would result from a national program. If this were the case, then the gains attributed to the women in the CPS sample will be overstated.

It should be noted that even though the increased earnings from the targeted public employment program result in reduced income maintenance payments (other income), dependence on income transfers remains high. Also, the most optimistic scenario in Table 3 shows that 34.5 percent of the sample remains poor (the rate for the entire U.S. population in 1975 was about 11 percent). Thus, for the very disadvantaged, targeted public employment should be viewed as a complement to and not a substitute for income maintenance transfers.

If the wage rates of the targeted jobs had been higher, the incidence of poverty would have been lower. There are several reasons why a program for the disadvantaged might pay low wages. First, the lower the wage, the lower the probability that persons who are able to find regular jobs will be attracted to a subsidized jobs program. Second, given a

fixed program budget, the lower wage means more participants can be enrolled. Third, even the low wage probably exceeds the marginal productivity of these workers.

We have shown that a transitional program has the potential for reducing poverty among eligible female welfare recipients by about 30 percent, whereas a continuous program could cut poverty by about 60 percent. However, there would be a demand for an additional 150,000 jobs under a continuous program rather than a transitional one (see the last line in Table 3). After we estimate the additional costs of these jobs in the next section, we can contrast two equal-cost alternatives: the addition of the jobs program and an equivalent expansion of the current transfer system.

Benefit-Cost Analysis and Efficiency Effects

Up to this point, we have analyzed a targeted public employment program from the standpoint of participants. We have examined their increased employment and income and their reduced poverty, but we have not considered the program's efficiency effects. The benefit-cost data gathered during the demonstration value the benefits attributable to the program, compare them to the costs, and allow us to examine the efficiency impact. The program's "net present value," the difference between benefits and costs, is available on a participant basis. The results were calculated from three perspectives: (1) the social perspective, which values benefits and costs to society as a whole, but does not value transfers among particular groups within society; (2) the nonparticipants' (taxpayers') perspective; (3) the participants' perspec-

tive. The social perspective tells us about the efficiency effects of the program, while the other two reveal its distributional effects, the sum of which is equal to the efficiency effects. By examining the transfers and program benefits received by participants and the transfer and program costs borne by the taxpayers, we can see what implications the taxpayers' preference for public employment over income maintenance programs might have upon the public budget. The benefit-cost analysis compares the Supported Work experimentals to the controls, and thus compares the addition of a targeted public employment program to the set of existing income maintenance programs.

The benefit-cost results reported here are based on Peter Kemper, David Long, and Craig Thornton's analysis (1980) but have been modified to fit our extrapolation to the CPS data and a national program.[14] Table 4 shows that the targeted public employment program had relatively small social costs--$1907 per participant per year for a continuous access program, and $683 for a transitional one. These costs are smaller than those found for other jobs programs because the women produced valuable outputs while on the job and reduced their reliance on existing income transfers. The same program would have had much greater social costs for participants not already receiving income transfers.[15] The net benefits to participants were $2229 and $1716 for the two types of program.

The budgetary costs to nonparticipants of $4136 per job for the continuous case and $2399 for the transitional case would be $1899 million for a continuous program and $708 million for a transitional one if the program were to be implemented on a national scale, and the demand for jobs were as shown in Table 3.[16] We used these estimates to simulate a

Table 4

Net Present Value (Benefits minus Costs) per Participant per Year[a]

	Accounting Perspective		
	Social	Participant	Nonparticipant
1. Continuous Access to Targeted Public Employment	$-1907	$2229	$-4136
2. Transitional Access to Targeted Public Employment	- 683	1716	-2399

Source: Adapted from data in Kemper et al. (1980). The values of increased post-program earnings and tax payments and reduced transfer payments were adjusted to our sample's differences, but the other entries on costs and benefits are taken directly from them.

[a]Costs and benefits are expressed in 1977 dollars for both the continuous (9-month) and the transitional (27-month) data.

proportionate expansion of the existing transfer system, and produce comparisons between the current system and the targeted jobs program that hold budget costs constant. These simulation results are shown in Tables 5 and 6.

Comparison of Equal Budget Cost Alternatives: Targeted Public Jobs and Augmented Income Maintenance System

Lines 1 and 2 of Table 5 show that the budgetary costs of $1899 million represent a 50.6 percent increase in current income maintenance costs for the 1,785,369 nonwhite female household heads. Whereas the jobs program targets all $1899 million on 459,037 of these women (our estimate of eligibles), an expansion of the current system would benefit all current welfare recipients, and would make additional women eligible. We simulated a proportional expansion of existing income guarantees by increasing each current recipient's welfare benefit by 50.6 percent. Because increased transfers lead to reduced earnings, we reduced our estimates of their earnings using the labor supply elasticities from the Seattle-Denver Income Maintenance Experiments.[17]

The results on the poverty effect for the targeted sample and for all of the nonwhite female-headed families show that access to a job means a larger reduction in poverty than the augmented income maintenance system for the targeted sample--the incidence falls from 80.3 percent to 34.5 percent or 51.7 percent. However, for the entire sample the reduction due to the augmenting of the current system is greater--the incidence falls from 55.2 percent to 43.4 percent or 39.8 percent.

Although the differences in poverty due to the additional $1899 are similar, the differences in work effort are not. The jobs program

Table 5

Simulation Results: Poverty and Work Effort With Program Budget Costs Held Constant, Continuous Access to a Targeted Job

	Current Income Maintenance System	Continuous Access to Targeted Jobs or Current Income Maintenance System	Augmented Income Maintenance System
1. Extra budget costs applied	0	$1899 million	$1899 million
2. Extra budget costs applied, as a percentage of existing costs for female-headed families	0	50.6%	50.6%
3. Incidence of poverty, targeted sample[a]	80.3%	34.5%	51.7%
4. Incidence of poverty, all female-headed families	55.2%	43.4%	39.8%
5. Index of work effort, targeted sample	100	576	79
6. Index of work effort, all female-headed families	100	124	97

[a]Targeted sample contains 459,037 nonwhite female heads of household receiving welfare and meeting eligibility criteria for targeted job.

[b]There are 1,785,369 nonwhite female-headed families.

Table 6

Simulation Results: Poverty and Work Effort With Program Budget Costs Held Constant, Transitional Access to a Targeted Job

	Current Income Maintenance System	Transitional Access to Targeted Jobs or Current Income Maintenance System	Augmented Income Maintenance System
1. Extra budget costs applied	0	$708 million	$708 million
2. Extra budget costs applied as a percentage of existing costs for female-headed families	0	19.2%	19.2%
3. Incidence of poverty, targeted sample[a]	64.3%	45.8%	56.3%
4. Incidence of poverty, all female-headed families[b]	51.1%	46.3%	46.0%
5. Index of work effort, targeted sample	100	174	96
6. Index of work effort, all female-headed families	100	107	99

[a]Targeted sample contains 459,037 nonwhite female heads of household receiving welfare and meeting eligibility criteria for targeted job.

[b]There are 1,785,369 nonwhite female-headed families.

leads to increased work effort, while expanding the current system reduces work effort. Work effort under the current system for the targeted sample (line 5) and for all female heads (line 6) is indexed at 100. The jobs program leads to over five times as much work effort as the current system for the targeted sample, a 24 percent increase for all nonwhite female heads. Expansion of the current system reduces work effort by 21 percent for the targeted sample and by 3 percent for the entire sample.

The results in Table 6 for the transitional program are similar. The additional expenditures, $708 million, or 19.2 percent of current costs, have smaller impacts than those of the continuous program. Again, the anti-poverty impacts are similar--the jobs program increases and the augmented current system decreases work effort.[18]

CONCLUSION

We have analyzed data from the Supported Work demonstration, and estimated the distributional and efficiency effects of a targeted public employment program of national scale for female welfare recipients. We found that the reduction in poverty and the increase in work effort were much larger when continuous rather than transitional access to a job was provided. The benefit-cost analysis, however, reveals a reverse pattern. Although the net present value to society of both types of programs is negative, the social and budgetary costs of the continuous program are higher.[19]

We began by referring to the American preference for aiding the poor through employment rather than income maintenance programs.

Increased expenditures on targeted employment could both increase work effort and reduce poverty. Equivalent increased expenditures on the existing income maintenance system would reduce poverty by about as much, but would also reduce work effort. The choice between an increase in transfers and the implementation of a targeted jobs program must be made on the basis of value judgments. Our own view is that a targeted jobs program would permit a reorientation of antipoverty policy towards the major goal of the War on Poverty--the reduction of poverty through earned incomes.

APPENDIX

Regression Results

This appendix presents four sets of regressions that were estimated with the Supported Work data. The regressions on the 9-month data were estimated separately for a sample of 704 experimentals (Table A1) and 652 controls (Table A2); for the 27-month data there were 292 experimentals (Table A3) and 280 controls (Table A4). The smaller sample sizes for the 27-month data occur primarily because of planned differential follow-up--all participants and controls were not followed for 27 months.

Means of the dependent variables are shown at the end of each table. The major change in the period, the sharp reduction in earnings and hours by the experimental group, was caused by the sharp fall in employment after they left the program: from 97 percent at the beginning of the 9th month to 48 percent at 27 months. Note, however, that mean monthly earnings for those who did work rose close to 23 percent from $406 to $498. The income data in the text are expressed in annual terms by multiplying the monthly data by 12.

Note 11 explains the details of our estimation procedure. Notice that while the OLS coefficients are consistent estimators of the relevant population parameters, the standard errors in the OLS steps are incorrect. We chose the computationally convenient two-step estimation procedure rather than a maximum likelihood technique, since the equations were to be used solely to predict out of sample and not to test hypotheses about the magnitudes of these reduced form coefficients.

The major weakness with the estimation-simulation procedure is that we were restricted to that set of variables which can be found both in the Supported Work data and in the CPS. But the probit equations predict significantly (in terms of the likelihood ratio test) even though the sample is fairly homogenous due to the program's eligibility criteria. In general, the signs of the coefficients match our theoretical expectations. The explanatory power of the OLS step for earnings and hours is relatively low but that of the other income equation is relatively high.

We used estimates for the economic variables for both the targeted public employment program and the existing income maintenance system. We did not use the actual economic variables in the CPS or compare actual and imputed values, however. There may be some unobserved characteristic, such as work ability, that caused a woman's actual earnings to exceed the value predicted by our equation, using the data on the control sample. When we predict her earnings in the jobs program, our equation will not account for this unobserved factor. This means that if we compare a predicted value with an actual value the difference between the two would be distorted. For our purposes, it is more important to accurately predict differences between two situations than to predict well the level in any situation. Using two predicted values more accurately preserves the likely differences between them than would the use of a mixture of actual and predicted values.

Table A1

Regression Results: Experimentals, 9-Month

Independent Variable	Monthly Earned Income OLS	Monthly Other Income OLS	Monthly Hours OLS
Constant	-153. (231.)	196. (188.)	-58.0 (71.7)
Age	20.2 (8.65)	-8.03 (7.04)	6.23 (2.68)
Age^2	-.194 (.094)	.085 (.077)	-.054 (.029)
Education	27.7 (24.2)	4.39 (19.7)	11.4 (7.52)
$Education^2$	-.609 (.870)	-.147 (.708)	-.274 (.270)
Age x Education	-.401 (.402)	.102 (.327)	-.160 (.125)
Disability	-123. (18.0)	38.2 (14.7)	-36.5 (5.60)
# of Children	-.736 (4.74)	58.5 (3.85)	-1.04 (1.47)
Northeast	46.5 (20.2)	22.7 (16.5)	23.1 (6.28)
Northcentral	74.2 (23.5)	-28.8 (19.1)	33.0 (7.28)
South	5.00 (25.5)	-120. (20.7)	28.9 (7.90)
Never Married	5.33 (12.8)	5.27 (10.4)	2.39 (3.96)
R^2	.108	.343	.101
# Observations	704	704	704
Standard Error of Regression	148.4	120.7	46.0
Mean of Dependent Variable	406.4	222.1	136.3

NOTE: Standard errors appear in parentheses below the regression coefficients.

Table A2

Regression Results: Controls, 9-Month

Independent Variable	Monthly Earned Income Probit Step	Monthly Earned Income OLS Step	Monthly Other Income OLS	Monthly Hours Probit Step	Monthly Hours OLS Step
Constant	.612 (2.23)	-46.9 (692.)	28.7 (170.)	*	43.2 (209.)
Age	-.071 (.082)	-74.7 (70.1)	3.65 (6.21)		-30.1 (21.1)
Age^2	.001 (.001)	.882 (856.)	-.020 (.067)		.348 (.258)
Education	-.061 (.222)	-122. (74.5)	22.9 (16.7)		-40.1 (22.5)
Education[a]	.008 (.007)	10.9 (6.84)	-.561 (.537)		3.32 (2.06)
Age x Education	-.001 (.004)	-.650 (.922)	-.186 (.304)		-.073 (.278)
Disability	-.461 (.151)	-582. (442.)	27.3 (10.7)		-204. (133.)
# of Children	.085 (.043)	81.4 (78.7)	61.7 (3.27)		29.9 (23.7)
Northeast	.162 (.188)	206. (155.)	6.03 (14.0)		77.5 (46.7)
Northcentral	.411 (.214)	492. (381.)	-59.5 (16.0)		181. (115.)

Table A2--Continued

Independent Variable	Monthly Earned Income Probit Step	Monthly Earned Income OLS Step	Monthly Other Income OLS	Monthly Hours Probit Step	Monthly Hours OLS Step
South	.605 (.228)	639. (545.)	-169. (17.3)		227. (164.)
Never Married	-.068 (.116)	-112. (66.5)	-8.89 (8.70)		-33.9 (20.0)
Lambda		1589. (1340)			545 (404)
R^2	$\chi^2_{(11)}=34.4$	.065	.526		.076
# Observations	652	236	652		236
Standard Error of Regression		171.1	98.7		51.6
Mean of Dependent Variable	.362	219.6	355.7		74.0

Note: Standard errors appear in parentheses below the regression coefficients.

*Probit Coefficients are identical in Earned Income and Hours equations.

Table A3

Regression Results: Experimentals, 27-Month

Independent Variable	Monthly Earned Income		Monthly Other Income		Monthly Hours	
	Probit Step	OLS Step	Probit Step	OLS Step	Probit Step	OLS Step
Constant	-3.55 (3.00)	5472. (3047.)	-.618 (.332)	528. (491.)	*	1102. (565.)
Age	.245 (.116)	-239. (143.)	.012 (.129)	-12.6 (15.9)		-46.68 (26.51)
Age^2	-.003 (.001)	3.47 (1.92)	-.0002 (.0015)	.162 (.182)		.661 (.356)
Education	-.008 (.307)	-27.7 (96.3)	.099 (.329)	26.2 (53.3)		-19.4 (17.9)
Education[a]	.005 (.011)	-.060 (3.87)	-.008 (.011)	-1.38 (1.90)		.247 (.717)
Age x Education	.0002 (.0055)	-1.14 (2.13)	.0003 (.0061)	-.016 (.731)		.038 (.395)
Disability	-.709 (.227)	525. (412.)	.882 (.316)	62.1 (64.7)		87.0 (76.4)
# of Children	-.108 (.067)	110. (61.8)	.327 (.086)	58.6 (24.5)		22.1 (11.5)
Northeast	-.878 (.404)	617. (420.)	.290 (.407)	-165. (56.9)		155. (77.8)
Northcentral	-1.09 (.424)	940. (541.)	.357 (.438)	-210. (60.7)		217. (100.)

Table A3--Continued

Independent Variable	Monthly Earned Income		Monthly Other Income		Monthly Hours	
	Probit Step	OLS Step	Probit Step	OLS Step	Probit Step	OLS Step
South	-.416	282.	-.021	-319.		101.
	(.447)	(199.)	(.469)	(59.4)		(36.9)
Never Married	-.080	-11.0	.576	-30.9		-1.59
	(.176)	(70.0)	(220.)	(50.0)		(13.0)
Lambda		-1528.		-79.8		-292.
		(900.)		(209.)		(167.)
R^2	$\chi^2_{(11)}$=35.4	.103	$\chi^2_{(11)}$=33.7	.329		.184
# Observations	292	139	292	233		139
Standard Error of Regression		284.3		156.6		52.7
Mean of Dependent Variable	.476	497.9	.800	318.2		123.4

NOTE: Standard errors appear in parentheses below the regression coefficients.

*Probit coefficients are identical in Earned Income and Hours equations.

Table A4

Regression Results: Controls, 27-Month

Independent Variable	Monthly Earned Income Probit Step	Monthly Earned Income OLS Step	Monthly Other Income OLS Step	Monthly Hours Probit Step	Monthly Hours OLS Step
Constant	4.55 (3.34)	2383. (1990.)	72.5 (388.)	*	226. (432.)
Age	.220 (.123)	-163. (147.)	4.83 (14.4)		-1.38 (31.8)
Age^2	.002 (.001)	1.83 (1.59)	-.041 (1.51)		.056 (.345)
Education	-.367 (.311)	-187. (193.)	17.0 (35.2)		-8.75 (41.8)
Education[a]	.018 (.010)	11.7 (9.57)	-1.36 (1.12)		.674 (2.08)
Age x Education	.004 (.006)	2.30 (2.94)	.006 (.695)		-.187 (.637)
Disability	-.552 (.215)	-672. (412.)	69.4 (24.1)		-12.6 (89.5)
# of Children	.049 (.064)	32.4 (41.4)	55.4 (7.61)		-.006 (8.98)
Northeast	.424 (.355)	159. (304.)	-14.6 (41.6)		-18.3 (65.9)
Northcentral	.046 (.385)	-13.2 (137.)	-33.2 (45.0)		26.4 (29.6)

Table A4--Continued

Independent Variable	Monthly Earned Income Probit Step	Monthly Earned Income OLS Step	Monthly Other Income OLS	Monthly Hours Probit Step	Monthly Hours OLS Step
Never Married	-.217 (.177)	-244. (168.)	16.9 (20.9)		20.3 (36.4)
Lambda		1286. (1061.)			-42.9 (230.)
R^2	$\chi^2_{(11)}=26.6$	.156	.271		.142
# Observations	280	113	280		113
Standard Error of Regression		277.8	154.2		60.3
Mean of Dependent Variable	.404	408.9	310.4		111.9

NOTE: Standard errors appear in parentheses below the regression coefficients.

*Probit Coefficients are identical in Earned Income and Hours equations.

NOTES

[1]See Plotnick and Skidmore (1978) for a detailed description of antipoverty expenditures by type of program, and the essays in Ginzberg for an analysis of manpower policy since the War on Poverty.

[2]The negative income tax experiments suggested that work discentives were relatively small for male heads of household, and somewhat larger for female heads of household and wives. Nonetheless, a call for cutbacks in the income maintenance system appears regularly on the editorial pages of the Wall Street Journal, and in numerous academic articles, as well. For a review of the efficiency effects of the transfer system, see Danziger et al. (1980).

[3]See Borus (1980): ". . . training, at least as it currently exists should not be viewed as the major weapon for fighting poverty. The gains in participants' earnings are measured in hundreds of dollars and are unlikely to move many people out of poverty. Small investments yield small returns" (p. 40).

[4]Work registration was required as a prerequisite for the receipt of benefits for some transfer recipients, but few jobs were directly provided. See Schiller (1978).

[5]PSE is not concerned solely with reducing structural unemployment and poverty, or overcoming the work disincentives of transfer programs. For a discussion of other functions--satisfying needs not met by the private sector or regular public employment, reducing the severity of the business cycle, improving the social or psychological well-being of the worker and his/her family--see the essays in Palmer (1978, 1980).

Although this paper analyzes only the direct provision of public jobs, there are other types of targeted public employment programs--earnings or wage subsidies which are paid to disadvantaged workers, or tax credits to employers who hire them. Both of these kinds of policies are currently in effect in the U.S. (e.g., the Earned Income Tax Credit, the Targeted Jobs Tax Credit). For a discussion of a broad range of direct job creation measures, see Haveman (1980).

[6]For an excellent discussion of recent developments in the U.S., see Orr and Skidmore (1980). Analyses of the Program for Better Jobs and Income, can be found in Danziger et al. (1977), and Danziger and Plotnick (1979).

[7]This section draws from several analyses of the Supported Work program. See Kemper and Moss (1975); Masters and Maynard (1980); Kemper et al. (1980).

[8]We should also point out that the program seems to have been most effective for the women receiving welfare.

[9]A greater percentage of our initial sample was classified as eligible for Supported Work than would have been so classified by the program. The discrepancy arises because the CPS has data only on current welfare recipiency, so many women listed as receiving AFDC will not have received it for 30 of the past 36 months. A similar data problem led us to ignore the unemployment criteria. Thus, our CPS sample is probably not as disadvantaged as the Supported Work demonstration sample.

[10]The percentage of experimentals and controls with no earnings or hours or other income and the estimation procedure are as follows:

	9-Month Experimentals	9-Month Controls	27-Month Experimentals	27-Month Controls
No Earnings or Hours	3% (OLS)	64% (TOBIT)	52% (TOBIT)	60% (TOBIT)
No Other Income	4 (OLS)	1 (OLS)	20 (TOBIT)	9 (OLS)

[11]Consider a latent variable, $\tilde{y}$, which, conditional on X, is normally distributed around a regression line:

$$\tilde{y} = X\beta + \varepsilon \qquad \text{where } \varepsilon \sim N(o, \sigma^2).$$

Now $\tilde{y}$ is observed only if it is positive for when it is negative, the observed variable, y, takes on the value of zero:

$$y = \begin{cases} 0, & \tilde{y} \leq 0 \\ \tilde{y}, & \tilde{y} > 0 \end{cases}.$$

For example, let $\tilde{y}$ be desired hours of work in the labor market, and y be actual hours. Under competitive conditions, actual and desired hours are equal when desired hours are nonnegative. However, when desired hours are negative, the individual is at the corner solution of zero hours of work.

Our estimation technique is as follows. Let D equal a dummy variable which takes on the value of 1 if a positive value of the dependent variable y is observed.

$$D = \begin{cases} 0, & y = 0 \\ 1, & y > 0 \end{cases}.$$

Then $E(y \mid x) = EE(y \mid x, D)$

$$= \Pr(D = 1 \mid x)E(y \mid x, D = 1) + \Pr(D = 0 \mid x)E(y \mid x, D = 0)$$

$$= \Pr(D = 1 \mid x)E(y \mid x, D = 1)$$

$$= \Pr\left(\frac{\varepsilon}{\sigma} > \frac{-\beta x}{\sigma}\right)E\left(y \mid x, \frac{\varepsilon}{\sigma} > \frac{-\beta x}{\sigma}\right)$$

$$= \left[\Phi\left(\frac{\beta x}{\sigma}\right)\right][\beta x + \sigma\lambda]$$

where $\lambda = \phi\left(\frac{\beta x}{\sigma}\right)/\Phi\left(\frac{\beta x}{\sigma}\right)$

ϕ = density of a standard normal variable

Φ = distribution function for a standard normal variable.

The Heckman (1980) procedure estimates $\Pr(D = 1)$ using probit analysis. The results of the probit analysis are then used to construct an estimate of λ, which becomes an explanatory variable in an OLS regression of y on x and $\hat{\lambda}$ for that part of the sample for which $D = 1$. This procedure yields consistent estimates of β.

The standard errors which we report, however, are those taken directly from the computer printout of the OLS regression. These are not the true standard errors. Because we are merely using these equations for imputations, we did not go back and recover the true standard errors. It is possible to do so, but the process is involved.

Our imputation procedure follows directly from the estimation:

$$\hat{y} = [p(\hat{y} > 0)][\hat{\beta}x + \hat{\sigma}\hat{\lambda}],$$

where $\hat{p}$ is the estimated probability and $\hat{\beta}$, $\hat{\sigma}$ are the estimated coefficients, and $\hat{\lambda}$ is the estimate of λ derived from the probit step.

[12]We have ignored the possibility that the nature of the program induced behavioral changes. Participants in a temporary program like Supported Work may have acted differently during the experiment than they would have if the program were continuous; for example, the experimentals may have increased their in-program work effort and reduced future work effort in response to the program.

[13]We use the official U.S. government measure of poverty, which varies by family size. In 1978 the poverty line for a family of four was $6628, which was about 35 percent of the median family income.

[14]The Kemper, Long, and Thornton (1980) benefit-cost analysis is quite thorough. Detailed data are presented on project input, overhead, and central administrative costs. Participant labor costs and fringes are the costs to the taxpayers. These costs, reduced by the foregone earnings of participants (measured by the earnings of the control sample), represent the net benefit to participants. Benefits are estimated from experimental-control differences and include in-program output, post-program output, tax liabilities of participants, transfer recipiency, and the use of alternative education and employment programs. The most difficult benefit to estimate is the value of in-program output that serves as an offset to

the project costs. The approach taken by Kemper et al. is to use the price an outside supplier in the regular labor market would charge for the output, not the input costs of the program.

[15]The data in Table 4 were derived at a point at which all of the program costs had been incurred, and do not include any future benefits that might have resulted beyond the 27th month. If the increased earnings and reduced transfer dependency were to continue into the future, the net costs to society and to the taxpayer shown in Table 4 could become net benefits. Kemper, Long, and Thornton's estimates (1980) of these future effects yield positive net present values.

[16]We disregarded the difficulties inherent in establishing a national jobs program of this size and type. For a discussion of some of the problems, see Danziger et al. (1977).

[17]Keely et al. (1978) report an estimate of the income effect on hours to be .1011. We assumed that changes in hours and earnings were equal in proportional terms. Our simulation increased incomes but did not change program tax rates, so there is no substitution effect.

Our simulation is overly targeted, however. We raised welfare income, holding tax rates constant, and therefore raising the guarantee. But we ignored the fact that raising the guarantee raises the breakeven levels and results in a larger number of women eligible for welfare. The increased expenditures for the augmented income maintenance system would be more widely distributed than is shown here. Thus, the estimates shown in the last column of Tables 5 and 6 should be viewed as upper bounds.

[18]Tables 5 and 6 do not report our maximization simulation for the comparison of the jobs program with the augmented income maintenance system. If the women chose the situation with the maximum income, then the demand for jobs would be lower than that shown in Table 3: 347,646 jobs from a continuous program and 198,211 from a transitional one.

[19]As we have suggested throughout the paper, the extrapolations from the Supported Work demonstration to a national program should be viewed as illustrative. Besides the prediction errors always associated with regression models, selection bias would be present if the voluntary nature of participation in the demonstration were correlated with some observed characteristics. Also, no experiment of limited duration will be able to anticipate behavioral changes that might be induced by a permanent program. All of these caveats relate to participant behavior. But our method also rests on some assumptions about the labor market. We assumed that whenever an experimental wanted to work post-program, her labor supply would be met by increased labor demand. This would not be the case if labor demand were less than perfectly elastic and the woman either could not find work or displaced others.

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