The Benefits and Costs of JTPA Title II-A Programs: Key Findings from the National Job Training Partnership Act Study

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ABSTRACT

This paper examines the benefits and costs of Job Training Partnership Act (JTPA) Title II-A programs for economically disadvantaged adults and out-of-school youths. It is based on a 21,000-person randomized experiment conducted within ongoing Title II-A programs at 16 local JTPA Service Delivery Areas (SDAs) from around the country. In the paper, we present the background and design of our study, describe the methodology used to estimate program impacts, present estimates of program impacts on earnings and educational attainment, and assess the overall success of the programs studied through a benefit-cost analysis.

I. The National Job Training Partnership Act Study

The National JTPA Study was commissioned by the U.S. Department of Labor in 1986 to measure the benefits and costs of selected employment

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and training programs for economically disadvantaged adults and out-of-school youths funded under Title II-A of the Job Training Partnership Act of 1982 (JTPA).[1] The study grew out of the recommendations of the Job Training Longitudinal Survey Advisory Panel formed to advise the Department of Labor on how to evaluate JTPA (Stromsdorfer et al. 1985). The panel unanimously recommended that estimates of the impacts of JTPA be obtained from a randomized experiment, in which eligible program applicants were randomly assigned either to a treatment group, which was allowed access to the program, or to a control group, which was not. Random assignment assures that the treatment group and control group do not differ systematically in any way except access to the program. Thus, subject only to the uncertainties of sampling error, subsequent differences in outcomes between the two groups can be attributed to the program. These differences represent valid estimates of program impacts in relation to alternative employment and training services available in the community outside of JTPA.

A growing body of research (Betsey, Hollister, and Papageorgiou 1985; Ashenfelter and Card 1985; LaLonde 1986; Fraker and Maynard 1987) indicates the importance of randomized experiments to overcome the selection bias that plagued previous quasi-experimental studies of employment and training programs. Selection bias arises when program impacts are measured by comparing labor market outcomes of program participants with those of nonparticipants who differ in systematic ways.

One of the primary factors that motivated the use of a randomized experiment for the National JTPA Study was the ambiguous findings produced by the quasi-experimental studies commissioned by the U.S. Department of Labor to measure the impacts of programs funded under the Comprehensive Employment and Training Act (CETA), the predecessor to JTPA (Bassi 1983; Bloom 1987; Dickinson, Johnson, and West 1987; Bryant and Rupp 1987; for a review of these studies see Barnow 1987).[2]

The main goals of the National JTPA Study were to produce valid and reliable estimates of program impacts on the earnings, employment, educational attainment and welfare receipt of adults and out-of-school youths that reflect:

- the incremental employment and training services received by persons allowed access to JTPA, beyond what they would have received without the program;[3]

- the effects of JTPA Title II-A programs operating under conditions that were as close to normal as possible;[4] and

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1. Since the study began, JTPA has been amended in several ways. The terminology used in this article reflects the status of JTPA at the time random assignment occurred.
2. The published versions of these studies are cited as a convenience to the reader. Only the earlier unpublished versions were available to the Job Training Longitudinal Study Advisory Panel.
3. As explained later, this means that study findings should not be interpreted as the impact of JTPA services versus *no* employment and training services.
4. Every attempt possible was made to minimize the effect of our study on the ongoing operations of local JTPA programs and on who participated in these programs. We and others (for example, Bloom 1990 and Hotz 1992) recognize, however, that some such effects might have occurred. Nevertheless,

- the experience of a diverse group of local JTPA programs from across the country.

The study was designed to provide impact estimates for several key target groups: adult women, adult men, female youths, and male youths. For reasons discussed below in the analysis, male youths were separated into two groups: (1) those who had been arrested between their sixteenth birthday and their application to JTPA (referred to as male youth arrestees) and (2) those who had not been arrested between their sixteenth birthday and their application to JTPA (referred to as male youth nonarrestees). Impact estimates also were obtained for numerous subgroups defined in terms of factors such as JTPA services recommended by program intake staff, ethnicity and prior labor market experience. In addition, the study was designed to provide estimates of incremental service costs to compare with the estimates of incremental program impacts.

II. Design of the Experiment

To attain these goals, Title II-A applicants at 16 local JTPA programs (referred to as Service Delivery Areas, or SDAs) from across the country were randomly assigned to either a treatment group, which was allowed to enroll in a Title II-A program, or a control group, which was not allowed to enroll for 18 months.[5] Two-thirds of the eligible Title II-A applicants were assigned to the treatment group and one third were assigned to the control group.[6] Random assignment lasted about 15 months in each SDA, on average, beginning in November 1987 and ending in September 1989.

The 16 SDAs in the study were volunteers and therefore do not represent the nation in a statistical sense.[7] Nevertheless, they include a broad range of different geographic locations and programs, with widely varying participant backgrounds and local economic conditions, and average characteristics that are, with few exceptions, similar to those of SDAs nationwide.

Table 1 describes the JTPA program size, duration, cost and adult placement rates at the study sites and compares these program characteristics with those for all SDAs nationwide.[8] As can be seen, the study sites vary widely with respect

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we believe our findings provide a credible answer to the question "did the programs we studied improve the labor market prospects of the persons who participated in them?"

5. Youths were not included in the experimental design for one SDA.

6. The statistically optimal assignment ratio would be half treatment group members and half control group members. A two-thirds/one-third ratio was chosen instead to reduce the number of eligible applicants that local problem operators had to turn away. All program slots were filled, however, because far more persons are eligible for JTPA than it can serve. In several sites, for a brief period, the assignment ratio was increased to 3/1 or 6/1 to respond to a temporary shortage of program applicants. The final analysis sample randomly eliminated a portion of these "extra" treatment group members to simplify the analysis by providing a 2/1-ratio for all sample subgroups.

7. An attempt was made to recruit a probability sample of sites, but this was not possible given the constraints under which the study was conducted (Doolittle and Traeger 1990).

8. For a more detailed comparison of the 16 study sites and SDAs nationwide see Bloom et al. (1993) and Doolittle and Traeger (1990).

Table 1
*Size, Duration, Cost, and Performance of JTPA Title II-A Programs at the 16
Study Sites and all SDAs Nationwide*

	Mean for Program Years 1987–89			
Site	Total Adult and Youth Terminees[a]	Weeks Enrolled Per Adult Terminee	Federal Cost Per Adult Terminee	Adult Entered Employment Rate PY88
16 study sites				
Range[b]	354–1,793	7–34	$1,561–3,637	55.5–89.0%
Average	899	20	2,377	74.5
National average, all SDAS	1,177	20	2,241	74.2

Source: Unweighted annual averages calculated from JTPA Annual Status Report computer files pro-
duced by the U.S. Department of Labor.
Notes: The "entered employment rate" is the percentage of all adult terminees who found a job be-
fore terminating their enrollment in JTPA.
a. The number of terminees equals the number of persons enrolled in the program who terminated
from it during a program year. The only national data available on this measure includes adults and
both out-of-school and in-school youths ages 14 to 21. The National JTPA Study did not include in-
school youths or youths under age 16, however.
b. The corresponding range for all SDAs nationwide is not shown because it is improper to compare
the range of a small sample with that of a large sample.

to each program characteristic. In terms of average program duration, cost, and
performance, however, the study sites look much like the typical SDA nationally.

The average program size at the study sites is slightly smaller than the average
nationally. This reflects a balance between two factors. First, the very smallest
SDAs at the time the study began were excluded because their samples would
have been too small to contribute appreciably to the analysis. Second, we could
not include the very largest SDAs in the study because it was not feasible, given
available resources, to implement a randomized experiment within ongoing JTPA
programs in these complex, decentralized SDAs.

The principal reason that local SDAs refused to participate in the study was
unwillingness to randomly exclude eligible applicants from JTPA. It is difficult,
however, to say whether such unwillingness would be positively or negatively
correlated with program impacts. It might be argued that SDAs which agreed to
participate couldn't self-select themselves on the basis of impact, because their
true impact was unknown. Nevertheless, the decision to participate could still be
based on factors that were correlated with impact by chance. However, it is
noteworthy that the one measure of performance known to SDAs—their JTPA
performance indicators—was virtually identical, on average, for the study sites
and SDAs nationally.[9]

9. To examine the effect of site selection on our findings, Bloom et al. (1993) tried to identify local
factors that were correlated with estimated 18-month impacts on earnings, but no clear patterns emerged

The sample intake and assignment process at each site was designed to fit within that site's ongoing JTPA program. First, applicants were recruited by site staff and screened to determine their JTPA eligibility. Eligible applicants were assessed to determine their employment and training needs and a mix of services was recommended accordingly. Based on these recommended services, each applicant fit into one of three "service strategies": (1) classroom training, (2) a mix of on-the-job training (OJT) and/or job-search assistance (JSA) and (3) other services.[10] At this point, local site staff called the study staff to submit the applicant for random assignment to the treatment group or to the control group. Because service strategy designations were determined before random assignment, each service strategy had a treatment group and a control group, which facilitated separate experimental impact estimates for each.[11]

A total of 20,601 sample members were randomly assigned to the treatment group or the control group. The analysis of program impacts on earnings reported below was based on data for 15,981 sample members. This analysis sample was obtained from the full experimental sample in a way that had virtually no effect on the random determination of treatment or control status within the analysis sample.[12]

Data for the analysis were obtained from:

- a Background Information Form (BIF) completed by sample members when they applied to JTPA;

- JTPA enrollment, tracking, and expenditure records from the 16 SDAs that served as study sites;

- two waves of follow-up surveys conducted by telephone, with personal interviews where necessary;

- state Unemployment Insurance (UI) wage records for 12 of the study sites;

- state AFDC and food stamps records for four of the study sites;

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from the analysis. In addition, Heckman and Smith (1993) and Heckman, Smith, and Clements (1993) examined the sensitivity of our 18-month impact findings for adult men and male youths (including arrestees), respectively, to site selection by dropping one site at a time and reestimating program impacts. The variation in findings for male youths was substantial, but in no case were positive impacts on earnings obtained. The variation for adult males was much less pronounced. Taken together, these findings suggest that site effects probably were not large enough to call into question the basic findings that we report in this paper.

10. As discussed by Kemple, Doolittle, and Wallace (1992), under normal JTPA procedures not all sample members received the primary service for which they were recommended; some received other services and some received no services. Hence, the impacts reported for the three service strategies represent the effect of a *mix* of services, not a single service.

11. Because the study was commissioned to estimate the impacts of JTPA programs as they normally operated, it was clear from the outset that we could not randomly assign sample members to specific JTPA services. To do so would materially affect local SDA decisions about what services to provide to whom. Thus it was not possible to obtain experimental estimates of the impacts of specific services.

12. All but 323 of the 4,620 experimental sample members who were not included in the analysis were deleted in ways that were purely random with respect to treatment or control status (see Orr et al. 1994). Even the 323 sample members who were not deleted on a purely random basis probably were omitted in ways that were mainly random.

- a telephone survey of vocational/technical schools in the study sites to determine the costs of their programs; and

- published data on the instructional costs of high schools and colleges.

III. Estimating Program Impacts

As noted above, the National JTPA Study was designed to measure the impacts of the incremental services provided by JTPA, beyond those available outside of the program. Because JTPA is not the only employment and training service provider in most communities, it was expected that a number of control group members would receive employment and training services from non-JTPA sources. Therefore the impact estimates we present below do not represent the total effect of JTPA relative to no services, but rather the *incremental* effect relative to the non-JTPA services received by the control group.

A. Estimating Incremental Impacts Per Assignee

JTPA impacts were measured in two conceptually different but related ways: impacts per experimental assignee and impacts per JTPA enrollee. The first measure was obtained by taking the difference between mean outcomes for the treatment and control groups. This is a strictly experimental impact measure. We refer to the measure as the *impact per assignee* because it reflects the average difference in an outcome produced by randomly assigning a sample member to the treatment group instead of the control group. Because treatment group members have access to JTPA program services, but control group members do not, the impact per assignee represents the average impact of providing a sample member with access to JTPA services.

To reduce the standard errors of our estimates of impacts per assignee, the treatment-control group outcome differences were regression-adjusted for random variation in individual background characteristics.[13] Controlling for background characteristics generally did not affect the point estimates of program impacts appreciably because the background characteristics of the treatment and control groups were virtually identical, on average, because of random assignment and the large samples involved.[14]

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13. We used ordinary least squares to regress the outcome on background characteristics and a dummy variable representing treatment or control status. The estimated impact per assignee was the coefficient on the treatment/control dummy. This method provides consistent estimates of impacts, even for discrete outcomes, because random assignment ensures that as the sample size increases, the correlations between the treatment/control dummy and the background regressors converge to zero, and the regression-adjusted impact estimate converges to the treatment-control difference in means. For discrete outcomes, we used the White (1980) standard error estimator, which is robust to misspecification of functional form.

14. The estimation approach used here is, in our judgment, the most straightforward, reliable way to analyze the available data. To assess the basic approach, Heckman and Smith (1993) and Heckman, Smith, and Clements (1993) examined the sensitivity of our 18-month impact estimates for adult men and male youths (including arrestees), respectively, by using (1) a different way to compute standard

B. Estimating Incremental Impacts Per Enrollee

Because most, but not all, treatment group members actually enrolled in JTPA (about two-thirds did so) and a few control group members (less than 2 percent) actually enrolled, a second type of measure was constructed to focus on impacts *per JTPA enrollee*. This measure was obtained by adjusting the estimates of impacts per assignee to account for the difference in the proportions of treatment and control group members who enrolled in JTPA.

The method used to do so was developed by Bloom (1984) and is related to recent work by Angrist, Imbens, and Rubin (1995) on instrumental variables. The key assumption made is that assignment to the treatment group had no effect on average outcome levels for sample members who did not enroll in JTPA. The impact per assignee, I_A, which is the average impact on all treatment group members, can be expressed as the weighted average.

$$I_A = rI_E + (1 - r)I_{NE},$$

where I_E is the average program impact on treatment group members who enrolled in JTPA (the impact per enrollee), I_{NE} is the average program impact on treatment group members who did not enroll in JTPA, and r is the enrollment rate of the treatment group. Because we assume that $I_{NE} = 0$, the impact per enrollee is just $I_E = I_A/r$.

For example, if the average impact per assignee were $500, and 60 percent of the treatment group were enrolled in JTPA after random assignment, the estimated impact per JTPA enrollee would be $500/0.6, or $833. Thus, estimated impacts per enrollee are proportional to estimated impacts per assignee. In this example, the 60 percent enrollment rate implies an estimated impact per enrollee that is 1/0.6 or 1.67 times the estimated impact per assignee.

A small adjustment also was made to reflect the fact that a few control group members enrolled in JTPA and thus, the control group's average outcome levels may have been affected by the program to a small extent. The adjustment relies on the additional assumptions that: (1) enrollees in the control group (crossovers) also would have enrolled in JTPA if they had been assigned to the treatment group, and (2) average outcome levels for the crossovers were the same as if they had been assigned to the treatment group. Under these assumptions, plus the assumption of no JTPA impact on nonenrolled treatment group members, the average impact on enrolled treatment group members *who would not have enrolled if assigned to the control group* is $I_A/(r - c)$, where c is the control group enrollment rate (Bloom et al. 1993; Imbens and Angrist 1994). Therefore, we estimated impacts per enrollee by dividing the estimated impacts per assignee by the difference between the treatment and control group enrollment rates. Because the JTPA enrollment rate for the control group was so low, alternative approaches to the crossover problem would not have changed our estimates appreciably.

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errors (development by White 1980); (2) different ways to handle "outliers" (unusually high values for earnings); and (3) several different ways to combine (weight) impact estimates across sites. Although their impact estimates for male youths varied with the method used, no combination of methods produced estimates of positive impacts on earnings. Findings for adult men varied much less, and were quite similar to those which we obtained (Bloom et al. 1993).

The estimated impact per enrollee has the same significance level as the estimated impact per assignee because I_E is nonzero if and only if I_A is nonzero. Thus, testing whether the impact per assignee is significantly different from zero is the same as testing whether the impact per enrollee is significantly different from zero. We estimated standard errors for impacts per enrollee by treating enrollment rates as fixed and dividing the standard errors per assignee by $r - c$.[15]

The procedure described above to estimate program impacts per enrollee is similar to one developed independently to deal with noncompliance bias in medical clinical trials (Sommer and Zeger 1991). In addition, Angrist (1990) employed a related instrumental variables approach to use the U.S. draft lottery as an experimental basis for estimating the effect of being a Vietnam Veteran on future earnings. Angrist, Imbens, and Rubin (1995) present a comprehensive discussion of this instrumental variables approach to estimating causal effects. When the impact per assignee is estimated by a difference in means, our procedure is equivalent to using assignment to the treatment group as an instrument for enrollment in JTPA.

IV. Incremental Impacts on Earnings, by Target Group

A. The Earnings Experience of Assignees in the Treatment and Control Groups

Figure 1 illustrates the average quarterly earnings during the 30-month (10-quarter) follow-up period of adult women (top panel) and adult men (bottom panel) who were randomly assigned to the treatment group and the control group. As can be seen, the earnings of both women and men assigned to the treatment group increased appreciably over time.

Not all of this increase can be attributed to JTPA, however, because during the same period the earnings of women and men assigned to the control group also increased, as they emerged from the period of unusually low earnings that led them to apply to JTPA (the so-called "preprogram dip"). Thus, even without JTPA, the earnings of treatment group members would have risen substantially.

The incremental impact of JTPA per assignee is the difference between the earnings of treatment group members during the follow-up period and the corresponding earnings of control group members. This difference is slightly positive throughout the follow-up period for both women and men, suggesting a modest positive impact for these target groups.

Figure 2 presents the earnings time paths of female out-of-school youths and the subgroup of male out-of-school youths who had not been arrested before

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15. As a check on this procedure, we also used the delta method (Efron and Tibshirani 1993, pp. 313–15), which takes into account the variability of the enrollment rates, to estimate standard errors of the estimated impacts per enrollee in Table 2. The standard errors estimated using the delta method fell within one dollar of the fixed-enrollment rate standard errors. Similar results were obtained by Heckman, Smith, and Taber (1994).
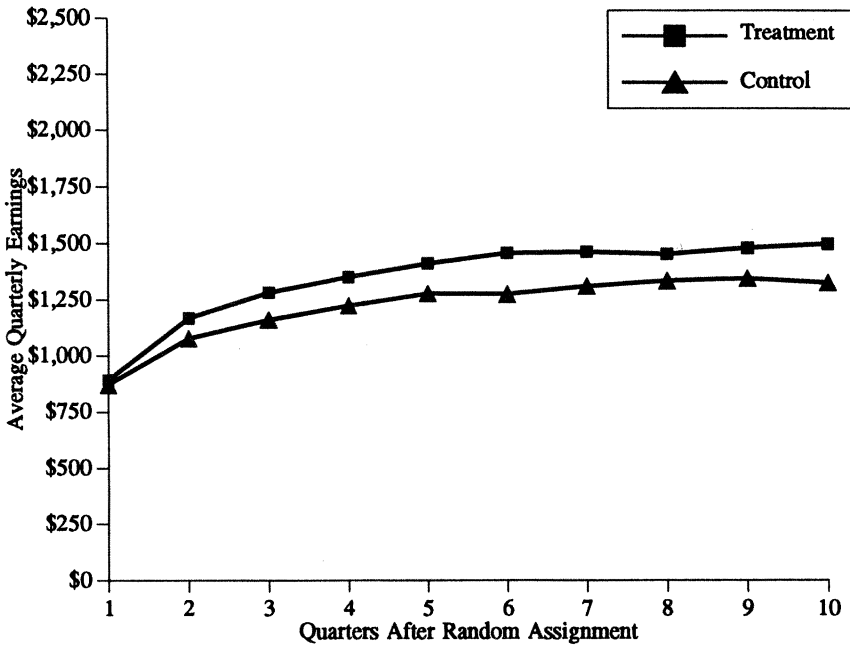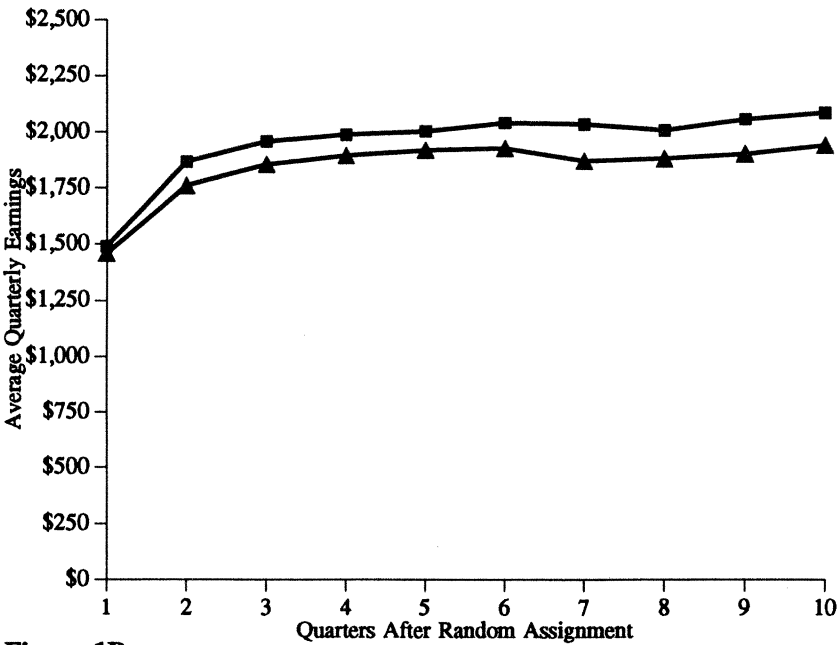
Figure 1A
Mean Earnings, by Quarter: Adult Women
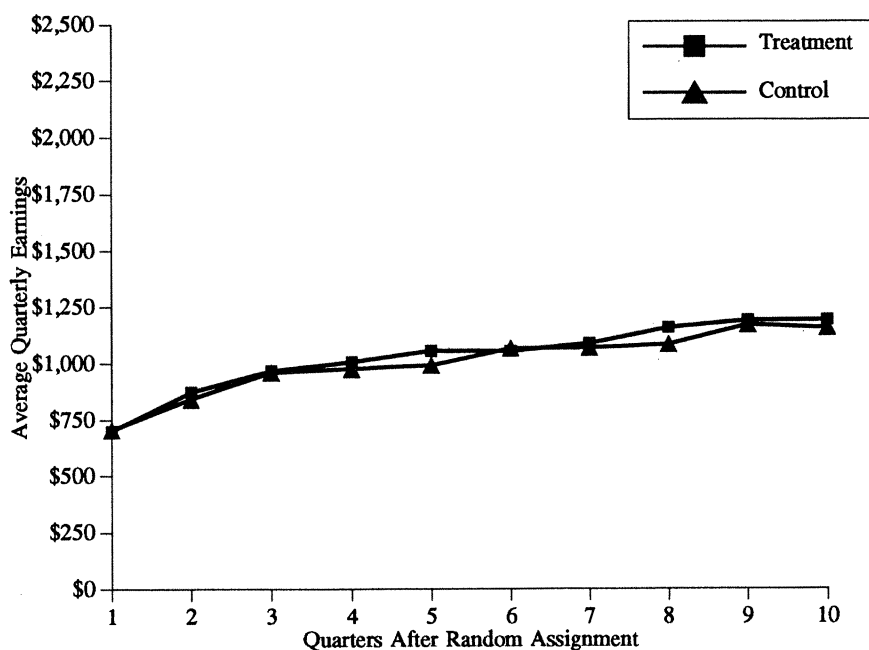

Figure 1B
Mean Earnings, by Quarter: Adult Men

Figure 2A
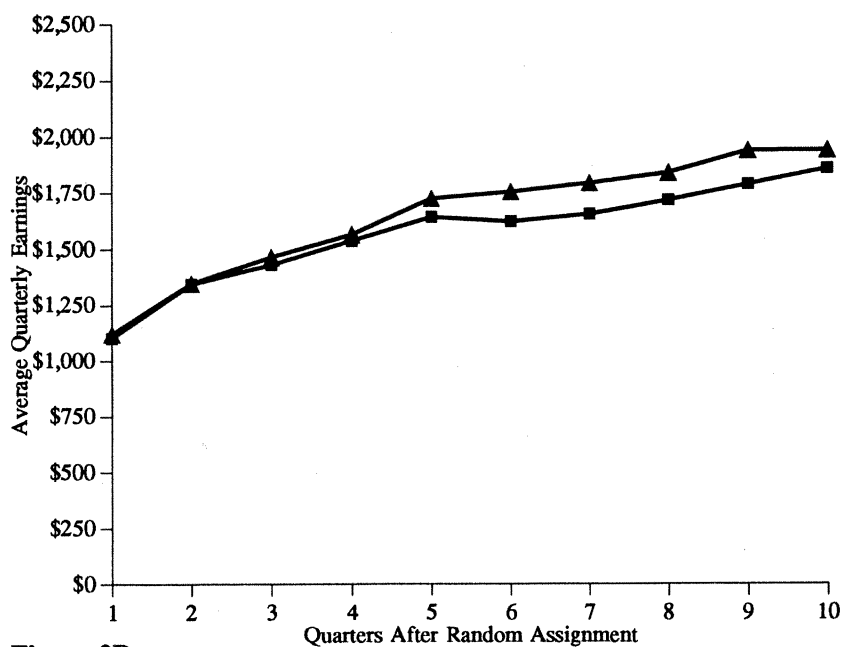*Mean Earnings, by Quarter: Female Youth*

Figure 2B
Mean Earnings, by Quarter: Male Youth Non-arrestees

random assignment.[16] Male youth arrestees (about one quarter of the male youths) were not included, because, as discussed below, our two major sources of earnings data gave contradictory results for this subgroup.

The earnings of youths increased substantially after random assignment, as was the case for adults. But unlike the results for adults, the earnings of youths in the treatment group did not exceed those of youths in the control group. For female youths, the earnings of treatment group members were almost identical to those of control group members throughout the follow-up period. For male youth nonarrestees, the earnings of treatment and control group members were almost identical for the first several follow-up quarters; thereafter, treatment group earnings dropped slightly below control group earnings. Hence, there was no sign in the raw data of a program-induced earnings gain for either target group.

B. Incremental Impacts on Total Earnings During the 30-Month Follow-up Period

Table 2 summarizes the findings illustrated in the preceding graphs by presenting regression-based estimates of JTPA impacts on total earnings during the 30-month follow-up period for each target group.

As can be seen, adult women in the treatment group earned a total of $13,417 ($5,367 per year), on average, during the follow-up period and adult women in the control group earned $12,241 ($4,896 per year). These averages include zero earnings for sample members who were not employed at all during the follow-up period.

The $1,176 difference between the mean earnings of these two groups is the best available estimate of the incremental JTPA impact per treatment group member (assignee). This estimate is statistically significant and represents a 9.6 percent increase over what treatment group members would have earned without access to JTPA. When adjusted for the 65.7 percent JTPA enrollment rate of adult women in the treatment group and the 1.7 percent JTPA enrollment rate of adult women in the control group, the estimated impact per adult female enrollee was $1,837.

Adult men experienced similar program-induced earnings gains. Treatment group members earned $19,474, on average, during the 30-month follow-up period, while control group members earned $18,496. The $978 difference was statistically significant and represents a 5.3 percent incremental impact per assignee. The corresponding impact per enrollee was $1,599.

As suggested by Figures 1 and 2, the experience of youths was quite different from that of adults. Female youths in the treatment group earned $10,241, while their control group counterparts earned $10,106. The difference of only $135, or

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16. Arrest status was determined for youths only, from their responses to the follow-up surveys. The arrestee subgroups include those respondents who reported being arrested between their sixteenth birthday and their date of random assignment. As expected, the prior arrest rates for treatment and control group members were quite similar: 25.0 percent and 21.5 percent, respectively, for male youths, and 6.5 percent and 5.0 percent, respectively, for female youths. Originally, arrest status was not expected to be of central concern to the analysis, so no attempt was made to cross-validate measures of arrest status obtained from the follow-up survey against administrative records.

Table 2
Impacts on Total 30-Month Earnings: Assignees and Enrollees, by Target Group

	Mean Earnings		Impact per Assignee		
	Treatment Group (1)	Control Group (2)	In Dollars (3)	As a Percent (2)	Impact per Enrollee in Dollars
Adult women	$13,417	$12,241	$ 1,176***	9.6%	$ 1,837***
Adult men	19,474	18,496	978*	5.3	1,599*
Female youths	10,241	10,106	135	1.3	210
Male youth nonarrestees	15,786	16,375	− 589	− 3.6	− 868
Male youth arrestees					
Using survey data	14,633	18,842	− 4,209**	− 22.3	− 6,804**
Using scaled UI data	14,148	14,152	− 4	0.0	− 6

Sources: Estimates based on First and Second Follow-up Survey responses and earnings data from state Unemployment Insurance (UI) agencies.
Sample sizes: adult women, 6,102; adult men, 5,102; female youths, 2,657; male youth nonarrestees, 1,704; male youth arrestees, 416.
* Statistically significant at the .10 level, ** at the 0.5 level, *** at the .01 level (two-tailed test).

1.3 percent, was not statistically significant. Hence, there was virtually no sign of a program impact on the earnings of female youths.

The results for male youths are more complex, but the basic conclusion for them is the same as that for female youths—JTPA did not appear to increase earnings. For male youth nonarrestees, treatment group earnings were $589 (3.6 percent) *less* than control group earnings.[17] This difference was not statistically significant, however, and thus might simply represent random sampling error. In any case, there was no evidence of a program-induced earnings gain for male youth nonarrestees.

The findings for male youth arrestees in Table 2 illustrate a fundamental inconsistency in the earnings data for this small but important subgroup of youths. Two sets of findings are presented, one based on earnings data from the follow-up surveys and one based on earnings data from UI wage records. Although both sets of findings are for the same sample, the findings are entirely different.

Survey data indicate that providing male youth arrestees with access to JTPA *reduced* their earnings by a statistically significant − $4,209 per assignee, or − 22 percent. In contrast, UI wage records indicate that JTPA had virtually no effect

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17. Our initial analysis for male youths, conducted on the entire target group, produced large negative impact estimates according to the follow-up survey data. In an effort to explain this finding, we were able to isolate it in the subgroup of male youths with prior arrests. Because of the major discrepancy between survey data and UI data for this subgroup, we report findings separately for it in subsequent analyses.

on earnings—the estimated impact is −$4. Extensive tests of both data sources were conducted to help resolve this inconsistency, but it was not possible to determine conclusively which data source was more accurate for the subgroup (Bloom et al. 1993, Appendix E).

In contrast, the two data sources produced virtually identical estimated percentage impacts for adult women, adult men, female youths and male youth nonarrestees.[18] Therefore, data from the two sources were combined to produce the impact estimates presented for these groups (see Orr et al. 1996).[19] Because of the unresolved inconsistency for male youth arrestees, however, this subgroup was kept separate and was not included in the more detailed analyses.[20]

C. Impacts on Earnings by Subperiod

To help understand how the preceding overall impacts occurred, Table 3 divides the follow-up period into three segments and presents impact estimates for each. Months 1–6 after random assignment represent the period when most JTPA enrollees were in the program. Months 7–18 represent approximately their first post-program year, and months 19–30 represent approximately their second post-program year.[21]

The first column of Table 3 shows the actual earnings of enrollees during each subperiod and for the follow-up period overall. The second and third columns present estimates of the impact per enrollee, in dollars, and as a percentage of what enrollees would have earned without JTPA. What enrollees would have earned without JTPA was inferred by subtracting the estimated impact per enrollee from the mean earnings of enrollees.

18. Survey data reported more earnings, on average, than did UI wage records, although the ratio of average earnings form the survey to average earnings from UI wage records was virtually the same for treatment and control group members in all target groups except male youth arrestees (Bloom et al. 1993, Appendix E). For male youth arrestees, the ratio of survey earnings to UI earnings was much higher for control group members than it was for treatment group members. No clear explanation of this difference could be found. The difference could not be attributed to exaggerated survey reports of control group earnings because the mainly low-skill, low-wage-rate jobs they reported were quite plausible. The difference could not be attributed to outliers (unusually high earnings) in the survey data because dropping the outliers from the data did not eliminate the difference. The difference could not be fully attributed to systematic under-reporting of jobs by UI wage records because the employment rates measured by the survey and those measured using UI wage records were not sufficiently different to explain the discrepancy in earnings reported by the two data sources. Nevertheless, unpublished comparisons of UI earnings data with data on earnings obtained from the Internal Revenue Service, suggest that UI wage records under-report earnings to some degree.
19. Survey data were used for all sample members for whom they were available for the entire 30-month follow-up period. UI earnings records were used for those with less than 30 months of survey data (primarily survey nonrespondents and sample members who were not included in the Second Follow-up Survey sample). UI earnings were multiplied by the ratio of mean survey earnings to mean UI earnings, based on the sample for which both data-sources were available, to adjust for differential reporting error in the two data sources. This adjustment was performed separately for the treatment and control groups within each target group.
20. The small size of this subgroup precluded more detailed analyses.
21. The amount of time spent in JTPA varied, but by month 7 after random assignment only 26 percent of the adult women, 15 percent of the adult men, 26 percent of the female youths, and 18 percent of the male youth nonarrestees in the treatment group were still enrolled in JTPA.

Table 3

Impacts on Enrollee Earnings, by Target Group and Follow-up Period

	Mean Earnings of Enrollees	Impact per Enrollee	
		In Dollars	As a Percent
Adult women			
Months 1–6	$ 2,138	$ 170*	8.6%
Months 7–18	5,794	820***	16.5
Months 19–30	6,292	847***	15.6
Total	14,224	1,837***	14.8
Adult men			
Months 1–6	$ 3,718	$ 204	5.8%
Months 7–18	8,807	538	6.5
Months 19–30	8,996	856**	10.5
Total	21,521	1,599*	8.0
Female youths			
Months 1–6	$ 1,564	$ −5	−0.3%
Months 7–18	4,199	53	1.3
Months 19–30	4,744	162	3.5
Total	10,508	210	2.0
Male youth nonarrestees[a]			
Months 1–6	$ 2,628	$ 61	2.4%
Months 7–18	6,538	− 289	−4.2
Months 19–30	7,252	− 639	−8.1
Total	16,418	− 868	−5.0

Sources: Estimates based on First and Second Follow-up Survey responses and earnings data from state unemployment Insurance (UI) agencies.
Sample sizes: adult women, 6,102; adult men, 5,102; female youths, 2,657; male youth nonarrestees, 1,704.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).
a. Corresponding results for male youth arrestees are not presented here because of the inconsistency between the findings obtained from UI wage records and those obtained from the follow-up survey.

The time paths of impacts on earnings in Table 3 mirror those illustrated by the graphs in Figures 1 and 2. For adults, the estimated impacts were positive, during the in-program period and during both post-program years, although not all of these estimates were statistically significant. There was no sign of a decay in the impact by the end of the 30-month follow-up period.[22]

For youths, there was virtually no sign of a positive impact on earnings, either during the in-program period or during the two post-program years. For male youth nonarrestees the estimated impacts were actually negative during both post-program years, but neither estimate was statistically significant; hence, they may only reflect random sampling error.

D. Impacts on Earnings by Site

When impacts on earnings were estimated separately for each study site, positive estimates were obtained in 11 of the 16 sites for adult women and in 12 sites for adult men. This indicates that the positive overall results for these target groups were widespread; they were not concentrated in a few atypical sites. Impact estimates for female youths and male youth nonarrestees were broadly distributed from positive to negative across sites; no site dominated the overall average impact estimate of near zero for these groups. Thus, the average impact findings for each target group reflect the central tendency of the results in many sites, not extreme results for a few idiosyncratic sites.

Moreover, differences in the estimated impacts across sites were not statistically significant for any of the target groups.[23] Variation among the estimates was well within the range of random sampling error, providing no statistical evidence that true impacts for a target group varied across the sites. In addition, the correlations between the site-specific impact estimates for different target groups were low, indicating no clear pattern of sites that generally were stronger or weaker overall.

Nevertheless, an exploratory analysis was conducted to identify local factors that might have influenced program impacts. Three types of factors were considered: (1) characteristics of the JTPA programs; (2) prevailing labor market conditions; and (3) the types of persons accepted into the programs. No clear patterns emerged from the analysis and almost none of the factors analyzed had a statistically significant influence on earnings impacts. Our ability to detect such effects was limited, however, by the small samples at each site, the small number of sites involved, and the large number of local factors that might affect the impact of the program (Bloom et al. 1993).

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22. The patterns of impacts over time were difficult to assess for all target groups because of the large standard errors associated with the differences in the impact estimates for different time periods. Few of these differences were statistically significant, even those differences that appeared to be large.
23. This finding was based on an F-test of the regression coefficients of a set of interactions between treatment status and the site dummies.

E. Impacts on the Earnings of Sample Subgroups

In addition to estimating impacts on the earnings of the study's main target groups, we also estimated impacts on the earnings of many different sample subgroups.[24] In general, however, because of the small sample sizes of these subgroups, few of their impact estimates were statistically significant, and even fewer differences among the impact estimates for different subgroups were significant.

For example, separate impact estimates were obtained for the members of each target group recommended for each of the three service strategies described earlier: classroom training, OJT/job search assistance, and other services. Table 4 summarizes these findings.

As can be seen, the results for female and male youths in all three service strategy subgroups are the same—no service strategy produced statistically significant impacts. Hence, the lack of statistically significant positive impacts for female youths and male youths *overall* presented earlier in Table 2 represent consistently insignificant impact estimates for youths in all three of the service strategy subgroups.

In contrast, for adult women, the OJT/job search assistance strategy and the other services strategy produced significant positive impacts. Because the impact estimates were large and significantly positive for two of the three subgroups for adult women, their overall impact estimate in Table 2, which is a weighted average of their subgroup estimates in Table 4, is positive and statistically significant.

Because the service strategy subgroup impact estimates in Table 4 for adult men are moderate and consistently positive (although not statistically significant) their overall impact estimate in Table 2 was significantly positive.[25]

Impact estimates were derived for many other subgroups defined in terms of such characteristics as lack of a high school credential, household composition, prior work history, prior welfare experience, and age. Almost none of these categorizations produced subgroups whose impact estimates were statistically significantly different from each other, however.

Nevertheless, two subgroup findings were particularly noteworthy. First was the large and statistically significant estimated impact for adult women who were receiving AFDC when they applied to JTPA (welfare mothers), a group that is central to the current welfare reform debate. The estimated 30-month earnings

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24. The subgroup findings discussed in this section are presented in Orr et al. 1996.

25. Two points about the findings in Table 4 are important to note. First is the fact that different types of persons were recommended by program staff for each service strategy subgroup. Hence, the impact findings for each service strategy represent estimates of its impact for the type of person recommended for it. Therefore, one cannot use these findings to compare the impacts of different service strategies for the same type of person. This limitation of interpretation holds for all comparisons of the impacts for different service strategy subgroups and is a feature of the design of the experiment that was produced by the mandate of the study to maintain the local decision-making process by which eligible JTPA applicants were chosen for different services (Doolittle and Traeger 1990).

A second point to note is that not all persons in a subgroup received the same program services. Indeed, a broad mix of different services were received by members of each service strategy subgroup (Bloom et al. 1993). Hence, one must interpret the findings in Table 4 as estimates of the impacts of the mix of services received by the type of person in each service strategy subgroup.

Table 4
Impacts on Total Earnings During the 30-Month Follow-up
Period by Target Group and Service Strategy Subgroup

Service Strategy Subgroup	Mean Earnings of Enrollees	Inferred Impact per Enrollee	
		In Dollars	As a Percent
Adult women			
Classroom training	$12,008	$ 630	5.5%
OJT/JSA	17,319	2,292**	15.3
Other Services	14,191	3,949***	38.6
Adult men			
Classroom training	$19,349	$1,287	7.1%
OJT/JSA	23,621	2,109	9.8
Other services	20,023	941	4.9
Female youths			
Classroom training	$10,279	$ 839	8.9%
OJT/JSA	14,256	− 578	− 3.9
Other Services	8,286	− 33	− 0.4
Male youth nonarrestees[a]			
Classroom training	$16,362	$ 251	1.6%
OJT/JSA	21,101	− 3,012	− 12.5
Other Services	12,819	− 438	− 3.3

Sources: Estimates were based on First and Second Followup Survey responses
and earnings data from state Unemployment Insurance (UI) agencies.
Note: Results were inferred from findings for all treatment and control group mem-
bers for each service strategy subgroup within each target group. Sample sizes by
service strategy subgroup were: 2,343, 2,284 and 1,475 for adult women; 1,034,
2,571 and 1,497 for adult men, 1,150, 614 and 893 for female youths; and 489, 554
and 661 for male youth nonarrestees.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level
(two-tail test).
a. Corresponding results for male youth arrestees are not reported here because
the subgroup sample sizes were too small and the findings obtained from the fol-
low-up survey were not consistent with those obtained from UI wage records.

impact for this subgroup was $2,387 (28 percent) per enrollee. Within the sub-group, welfare mothers who were recommended for the OJT/job search assis-tance strategy appeared to experience the largest impacts by far (a statistically significant $4,833, or 49 percent) and those who were recommended for the class-room training strategy appeared to experience the smallest impacts (a statistically insignificant $1,077, or 13 percent). The difference between these two impact estimates was statistically significant.[26] Hence, a strategy focused on direct place-ment in subsidized or unsubsidized jobs seemed to work better *for welfare moth-ers who were recommended for this approach* than did one emphasizing class-room training *for welfare mothers who were recommended for that approach.*

The second noteworthy subgroup finding was that discussed earlier for male youth arrestees. As indicated, the survey data suggest that this subgroup experi-enced a very large negative impact on earnings that was statistically significant and significantly different from the estimated impact for male youth nonarrestees. UI wage data indicate no impact for this subgroup, however.

V. The Increase in Employment and Training Services Due to JTPA: Measuring the Service Increment

To help interpret the estimates of incremental JTPA impacts on earnings obtained from the randomized experiment, it is useful to examine the service increments that produced these impacts. Table 5 presents three measures of this service increment for each target group.[27] The top panel of the table shows the percentage of sample members who received any employment and training service after random assignment. The middle panel presents the average number of hours of service received by all sample members, including zero hours for those receiving no service. The bottom panel shows the average cost of services received, including a cost of zero for sample members who received no service.

Access to JTPA approximately doubled the incidence of service receipt for adult assignees—from 33.1 percent for women in the control group to 59.5 percent for women in the treatment group and from 23.4 percent for men in the control group to 49.6 percent for men in the treatment group. The average number of hours of service received by the treatment group was also about twice that re-ceived by controls for both women and men. This implies that the average number

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26. At the 0.10 level (two-tail test).
27. Receipt rates and hours of OJT and work experience were estimated for treatment group members using data from the JTPA management information system at each site. Control group members were assumed not to receive these services because they usually are provided only by JTPA. Receipt rates and hours of classroom training in occupational skills, basic education, job search assistance, and other services were measured for both treatment and control group members using data from the follow-up surveys, which may understate the receipt of these services, especially job search assistance, due to respondent error. In the benefit-cost analysis, we conducted sensitivity tests to determine whether under-reporting of services could affect the conclusions of the study. We concluded that this potential problem was not likely to have affected the study's conclusions (see Orr et al. 1996).

Table 5

Employment and Training Services Received by Treatment Group and Control Group Members, by Target Group

	Treatment Group	Control Group	Difference per	
			Assignee	Enrollee
Percentage receiving a service				
Adult women	59.5%	33.1%	26.4%***	40.7%***
Adult men	49.6	23.4	26.2***	42.1***
Female youths	66.1	44.3	21.8***	33.9***
Male youth nonarrestees	62.7	34.6	28.1***	41.2***
Male youth arrestees	54.9	27.4	27.5***	42.4***
Mean hours of services received				
Adult women	359	190	169***	260***
Adult men	267	131	136***	219***
Female youths	438	256	182***	283***
Male youth nonarrestees	406	231	175***	257***
Male youth arrestees	320	193	127**	195**
Mean cost of services received				
Adult women	$2,147	$1,286	$ 861***	$1,324***
Adult men	1,571	902	669***	1,076***
Female youths	2,717	1,824	893***	1,390***
Male youth nonarrestees	2,896	1,496	1,401***	2,055***
Male youth arrestees	2,315	1,173	1,142**	1,759**

Sources: Estimates based on First Follow-up Survey responses, published school expenditure data, SDA enrollment and expenditure records, and a telephone survey of vocational/technical schools.
Sample sizes: adult women, 5,253; adult men, 4,026; female youths, 2,283; male youth nonarrestees, 1,338; male youth arrestees, 383.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

of hours of service received by *service recipients* in the treatment group was about the same as that received by service recipients in the control group.

Although large in relative terms, the service increments experienced by adult enrollees were not large in absolute terms. Adult women received 260 additional hours of service per enrollee and adult men received 219 additional hours. These service increments are roughly equivalent to between five and seven additional weeks of full-time training and are consistent in magnitude with the modest impacts on earnings observed for adults in the study sample.

The service increments for youths were roughly comparable to those for adults both in terms of rates of service receipt and in terms of the average number of hours of services received. Hence, differences between the magnitudes of the

service increments for youths and adults do not explain the differences in their estimated JTPA impacts.

Much the same patterns emerge when the service increment is expressed in dollar costs. All target groups experienced a modest increase in the average costs of the employment and training services they received. The incremental costs of services per enrollee were $1,324 for adult women, $1,076 for adult men, $1,390 for female youths, $2,055 for male youth nonarrestees, and $1,759 for male youth arrestees. In the benefit-cost analysis presented later, these incremental service costs are compared with their corresponding incremental program impacts on earnings.

VI. Incremental Impacts on the Receipt of a High School Diploma or GED Certificate by School Dropouts

Estimates of the incremental impacts of JTPA on the educational attainment of school dropouts in each target group are shown in Table 6. Educational attainment was measured as the percentage of school dropouts at program entry who obtained a high school diploma or GED certificate within 30 months after random assignment. The findings indicate that JTPA had an appreciable positive impact on the educational attainment of adult women and female youths who were school dropouts and also may have had an appreciable impact on adult male dropouts. In contrast, JTPA had no discernible effect on the educational attainment of male youth dropouts.

The findings for adult women indicate that 32.0 percent of the treatment group members who were school dropouts attained a high school diploma or GED certificate during the 30-month follow-up period, whereas only 20.4 percent of the control group dropouts did so. The 11.6 percentage point difference is an estimate of the impact of JTPA per assignee. When adjusted for the JTPA enrollment rate of dropouts in the target group, this finding implies an impact of 18.8 percentage points per enrollee. The estimated impact per enrollee for female youth dropouts was 10.6 percentage points and that for adult male dropouts was 14.4 percentage points. The estimates for adult women and female youths were statistically significant; the estimate for adult men was almost statistically significant at conventional levels.

Although JTPA appreciably increased the proportion of dropouts who achieved a high school credential in three target groups, only a fraction of the target group members were school dropouts.[28] Accordingly, JTPA did not have a large effect on the overall educational level of any target group. For example, because only about half of all female youths were dropouts, the 10.6 percentage point impact for enrollees who were dropouts translates into about a five percentage point increase in educational attainment for female youth enrollees overall.

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28. The percentage of treatment group members who were school dropouts when they applied to JTPA was 24 percent for adult women, 32 percent for adult men, 47 percent for female youths, 58 percent for male youth nonarrestees, and 65 percent for male youth arrestees.

Table 6
Impacts on Attainment of a GED or High School Diploma: School Dropouts, by Target Group

| | Percent with GED or High School Diploma 30 Months After Random Assignment | | Impact in Percentage Points per | |
	Treatment Group	Control Group	Assignee	Enrollee
Adult women	32.0%	20.4%	11.6**	18.8**
Adult men	24.2	16.3	7.9	14.4
Female youths	39.4	31.7	7.7*	10.6*
Male youth nonarrestees	36.8	36.3	0.5	0.7*
Male youth arrestees	29.9	28.9	1.0	1.7*

Source: Estimates based on Second Follow-up Survey responses.
Sample sizes: adult women, 301; adult men, 314; female youths, 605; male youth nonarrestees, 413; male youth arrestees, 118.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

VII. Incremental Impacts on Welfare Receipt

Reducing welfare dependence is a central goal of JTPA. Therefore, we estimated the extent to which JTPA reduced the average AFDC benefits and food stamp benefits received by each target group. Data for this analysis were obtained from a combination of follow-up surveys and administrative records obtained from state welfare offices. Usable data on AFDC benefits were obtained for a subsample of 6,206 persons from six sites. Usable data on food stamp benefits were obtained for a subsample of 5,141 persons from five sites. Although the usable data for these estimates were obtained only for a subset of sites, the estimated earnings impacts for these sites yielded approximately the same conclusions as those for all 16 sites. Thus, there is no obvious reason to expect the estimates of impacts on AFDC benefits or food stamp benefits for these sites to differ appreciably from those that would have been obtained if welfare data had been available for all 16 sites.

The program had no statistically significant impacts on the average number of months that either AFDC or food stamps benefits were received by any target group. Nor was there any statistically significant reduction in the dollar amount of AFDC or food stamps benefits for any target group. Even when the analysis was restricted to women or female youths who were on AFDC or food stamps

when they applied to JTPA, there were still no significant impacts on the benefits received from these programs.[29]

VIII. The Incremental Benefits and Costs of JTPA

This section compares the incremental benefits of JTPA with its incremental costs from three perspectives: (1) program enrollees; (2) all other persons (referred to as the "rest of society"), and (3) society as a whole.

The principal expected benefit of JTPA is increased enrollee earnings. We distinguish between increased earnings net of OJT wage subsidies, which represent added output (and therefore are not a cost to others) and OJT wage subsidies, which are a benefit to enrollees but a cost to taxpayers.

Earnings gains (net of OJT wage subsidies) from private employment were computed by taking program impacts per enrollee on total 30-month earnings minus total OJT wage subsidies per enrollee. The program may, of course, have impacts on earnings beyond the follow-up period, but we did not attempt to estimate these impacts for two reasons. First, we had little information with which to project impacts beyond the period we directly observed. Second, it was unnecessary to do so, because the earnings gains observed for adults were sufficient to offset their incremental JTPA costs (from the perspectives of participants and society as a whole) and the earnings gains observed for youths were negligible or even negative; hence, for youths, no reasonable extrapolation would produce sufficient benefits to offset JTPA costs. In both cases then, extrapolation would not change our main conclusions.

The principal expected cost of JTPA is the cost of the incremental employment and training services received by enrollees. This cost was measured by the treatment-control difference in the cost per enrollee of both JTPA and non-JTPA services received (the service cost increment per enrollee in Table 5).

In some cases (especially for non-JTPA training) part of the cost of training is borne by participants. One effect of enrollment in JTPA is to reduce these out-of-pocket costs. We count this reduction as a benefit to enrollees and a cost to the rest of society.

Reductions in welfare benefits as a result of enrollment in JTPA represent a cost (loss of income) to enrollees and a benefit (reduction in taxes) to the rest of society.[30] While it is important to measure this redistributional effect, the cost to enrollees and benefit to the rest of society are offsetting from the perspective of society as a whole; they represent a transfer of resources from one group to another. The impact on welfare benefits was measured by the estimated program impact on total AFDC and food stamp benefits per enrollee over the 30-month

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29. Sample sizes for male adults or male youths receiving AFDC or food stamps when they applied to JTPA were too small to provide useful impact estimates.

30. Reduced welfare benefits may also produce reduced administrative costs for AFDC and food stamps, which are a true resource savings for society. Because we found no significant impacts on months of receipt of AFDC and food stamps, however, we did not attempt to measure impacts on the administrative costs of these programs.

follow-up period.[31] Finally, enrollees' earnings gains are subject to taxation. Increases in the taxes paid by enrollees are a cost to the enrollee and a benefit to the rest of society.[32]

Table 7 presents the estimated values of the benefits and costs for each target group. Positive values indicate benefits and negative values indicate costs. The first column presents the benefits and costs for JTPA enrollees, the second for the rest of society, and the third for society as a whole (the sum of benefits and costs for enrollees and taxpayers). The net benefit for each target group is the sum of the benefits and costs it incurred.

Adult enrollees experienced positive net benefits from JTPA equal to $1,422 for women and $1,822 for men. This reflected earnings gains of $1,683 for women and $1,355 for men, plus small OJT wage subsidies, increases in taxes, reductions in out-of-pocket training costs, and changes in welfare benefits.

Youths did not receive a net benefit from enrolling in JTPA. Female youths experienced a negligible net cost of −$121 per enrollee, reflecting the very small earnings impacts estimated for this group. Male youth nonarrestees experienced a modest net cost of −$530, reflecting the insignificant but negative estimated impact on their earnings. Benefit-cost findings are not reported for male youth arrestees, because of the ambiguity in the estimates of their earnings impacts, discussed earlier. There was no sign of a positive net benefit for this group, however, regardless of which estimate of their earnings impact was used.

The rest of society incurred the costs of providing the incremental services and OJT wage subsidies to JTPA enrollees, plus the offsetting benefits or costs of any changes in welfare payments to enrollees or taxes on their earnings. As shown in the second column of Table 8, the net cost to the rest of society was −$910 per adult female enrollees, −$1,298 per adult male enrollee, −$1,059 per female youth enrollee, and −$2,393 for enrollees who were male youth nonarrestees.

Benefits and costs to society as a whole were derived by summing the benefits and costs to enrollees with those to the rest of society. From a social perspective, the benefits of OJT wage subsidies and the costs of increased taxes and welfare benefit reductions to enrollees are exactly offset by their equal and opposite impacts on the rest of society. Therefore, net social benefits are simply the incremental earnings gains of enrollees less the incremental costs of employment and training services per enrollee.

Net social benefits were positive for adults—a gain of $512 per enrollee for women and $524 for men. Net social benefits were negative for youths—a loss

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31. We used the point estimates of these impacts, even when they were not statistically significant, because they provide the best information available.

32. Increased taxes on earnings are estimated as 12.8 percent of earnings gains, including OJT wages. This percentage is the sum of the effective total federal tax rate (including the Earned Income Tax Credit) in 1988–89 for the bottom quintile of all families (9.3 percent), according to Congressional Budget Office estimates, plus the average state sales and income tax rate for poor two-parent families of four in the 16 study states (3.5 percent), according to General Accounting Office estimates (see Committee on Ways and Means, 1992, pp. 1488–90 and 1510). We used a weighted average of state tax rates in the 16 study states, with weights equal to the proportion of the 30-month study sample in each state.

Table 7
Impacts on the Arrest Rates of Youths

	Percentage Arrested During Follow-Up Period		Impact per	
	Treatment Group	Control Group	Assignee	Enrollee
Female youths	7.0	5.3	1.7	2.7
Male youth nonarrestees	25.8	18.7	7.1**	10.4**
Male youth arrestees	59.2	55.7	3.5	5.5

Sources: Estimates based on First and Second Follow-up Survey responses.
Sample sizes: Female youths, 1,153; male youth non-arrestees, 708; male youth arrestees, 198.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

of −$1,180 for female youths and −$2,923 for male youth nonarrestees. Findings for male youth arrestees also indicate negative net social benefits, regardless of the data source used to estimate program impacts on earnings.

In summary, then, JTPA conveyed positive net benefits for adults, both to enrollees themselves and to society as a whole, but not to the rest of society. For youths, net benefits were negative from all three perspectives.[33]

IX. The Findings in Comparison with those from Previous Experimental Studies

Findings from the National JTPA Study parallel those from the few other studies of employment and training programs that have employed experimental designs. The modest positive impacts on earnings experienced by adult women in the JTPA sample, especially those who were on welfare when they applied to JTPA, are consistent with the impacts observed by previous randomized studies of work-welfare programs (for a summary see Gueron and Pauly 1991; also see Bell and Orr 1994). The modest positive impacts experienced by adult men in the JTPA sample are consistent with the impacts observed for men by the several existing randomized studies of programs for displaced workers (Bloom 1990, Corson et al. 1989). Although the programs and the characteristics of sample members in the present study differ from those of earlier studies, there is a consistent overall pattern of modest program-induced earnings gains for both adult women and adult men.

For out-of-school youths, there is a disturbingly consistent lack of program-induced earnings gains across several major studies. Two other major randomized

33. Net social benefits were positive for five of the six service strategy subgroups for adults but for none of the six service strategy subgroups for youths (see Orr et al. 1996).

Table 8
Benefits and Costs of JTPA per Enrollee

	Enrollees	Rest of Society	Society
		Adult women	
Earnings gain (minus OJT subsidy)	$ 1,683	$ 0	$ 1,683
OJT wage subsidy	154	−154	0
Increased taxes on earnings	−236	236	0
Incremental training cost	56	−1,227	−1,171
Welfare benefit reduction	−235	235	0
Net benefits	1,422	−910	512
		Adult men	
Earnings gain (minus OJT subsidy)	$ 1,355	$ 0	$ 1,355
OJT wage subsidy	244	−244	0
Increased taxes on earnings	−211	211	0
Incremental training cost	100	−931	−831
Welfare benefit reduction	334	−334	0
Net benefits	1,822	−1,298	524
		Female youths	
Earnings gain (minus OJT subsidy)	$ 136	$ 0	$ 136
OJT wage subsidy	74	−74	0
Increased taxes on earnings	−28	28	0
Incremental training cost	76	−1,392	−1,316
Welfare benefit reduction	−379	379	0
Net benefits	−121	−1,059	−1,180
		Male youth nonarrestees	
Earnings gain (minus OJT subsidy)	$ −968	$ 0	$ −968
OJT wage subsidy	100	−100	0
Increased taxes on earnings	109	−109	0
Incremental training cost	110	−2,065	−1,955
Welfare benefit reduction	119	−119	0
Net benefits	−530	−2,393	−2,923

evaluations of employment and training programs for out-of-school youths have been conducted, and both obtained results similar to those of the National JTPA Study.

The first such study, the youth component of the National Supported Work Demonstration, evaluated an intensive work experience program (MDRC 1980, Hollister, Kemper, and Maynard 1984). The second study, JOBSTART, evaluated intensive education, employment, and training services provided through JTPA (Cave et al. 1993). Supported Work produced negligible short-run impacts

on the earnings of youth participants, most of whom were male. Supported Work's long-run impacts, for up to eight years after random assignment, were also negligible (Couch 1992). JOBSTART produced negligible short-term impacts for female youths and large negative short-term impacts for male youths. Estimates based on four years of follow-up data provide more favorable findings for JOBSTART, but most of these findings are not statistically significant and hence, are difficult to interpret.

X. Implications of the Findings

To date, the few existing randomized experimental studies of employment and training programs for disadvantaged persons present a consistent pattern of modest positive impacts on the earnings of adults and no positive impacts on the earnings of out-of-school youths. Moreover, it appears that employment and training programs for adults can be cost-effective from a societal perspective. So where should we go from here in terms of policy?

For adults, we still do not know what works best for whom. Hence, addressing this question with well-designed randomized experiments is a next logical step to take. To do so will require an ability to clearly define specific program strategies (including new interventions) which appear to be appropriate for their intended target group and to randomly assign eligible program applicants to these *different strategies*. This, in turn, will most likely require a series of special demonstration projects, because random assignment to different services is extremely difficult to implement within an ongoing program such as JTPA.

For out-of-school youths, we are at a more primitive stage in our understanding of how to increase labor market success; we have not found *any* way to do so. Supported Work was an intensive work experience program that typically lasted a year or more. JOBSTART was an intensive program of basic education and training that typically lasted a half-year or more. JTPA's Title II-A programs, at the time we studied them, were less intensive and typically lasted only several months. None of these programs were able to increase the earnings of out-of-school youths, however. Are these programs "too little too late"? Have they been targeted on the wrong youths? Were their services poorly administered? Or do we need a totally new approach to improving the labor market skills, behavior and experiences of economically disadvantaged out-of-school youths? The answers to these questions are not at all clear. What is clear, however, is that only by carefully designing and rigorously testing new alternatives for addressing the needs of this important group can we begin to make progress on this issue.

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