USING RANDOM ASSIGNMENT TO EVALUATE AN ONGOING PROGRAM: THE NATIONAL JTPA EVALUATION

Larry L. Orr, Abt Associates Inc.
Howard S. Bloom, New York University
Michael E. Borus, Manpower Demonstration Research Corporation
Larry L. Orr, Suite 500, 4250 Connecticut Avenue, D.C. 20008

This paper describes the National JTPA Evaluation, which will use random assignment of approximately 30,000 individuals in 15 to 20 sites across the country to measure the impacts of an ongoing national program, the Job Training Partnership Act of 1982 (JTPA). The first section briefly describes the background of the evaluation and summarizes its basic design. Following sections discuss key design issues that arose due to the fact that, unlike past randomized experiments to evaluate special demonstration projects, the current experiment must be implemented in the context of an ongoing program and must be designed to evaluate specific components of that program. Specific issues considered include: definition of treatments; placement of random assignment in the program intake process; and what questions can and cannot be addressed directly by the experiment.

Background and Overview of the Evaluation

Over the past decade, classical experiments involving random assignment to treatment and control status have been used to evaluate a number of employment and training programs.[1] In almost all cases, however, these have been special demonstration programs, in which the evaluators have had at least some control--and, in some instances, complete control--over the nature of the program. Demonstration programs can, in effect, be tailored to the needs of the evaluation. In most cases, evaluation activities were built into the demonstration from the outset. Moreover, it was clear that, since the purpose of the demonstration was explicitly to generate information, program funding was dependent on participation in the evaluation.[2]

The National JTPA Evaluation will, for the first time, use a random assignment design to evaluate an ongoing national program. This evaluation, funded by the U.S. Department of Labor in 1986, will examine training programs provided under the Job Training Partnership Act of 1982 (JTPA) in 15 to 20 localities across the country. The design of an experimental evaluation of an ongoing program involves a number of difficult problems, especially if--as in the case of JTPA--it is important to distinguish the impacts of different program services. This paper discusses the issues faced in the design of the JTPA evaluation. While those issues, and their resolution, are obviously particular to the JTPA context, this experience may be useful to future evaluation efforts in other program environments.

JTPA is the federal government's primary vehicle for providing low income persons with preparation for employment, through services which include job search assistance, remedial education, and both classroom and on-the-job occupational skills training. The program is funded by the U.S. Department of Labor and administered by over 600 local Service Delivery Areas (SDAs).

JTPA legislation specifically requires evaluation of the program and further specifies that it should be evaluated in terms of its primary goals: to increase participants' earnings, and to reduce their receipt of public assistance. The program was originally to be evaluated using comparison groups constructed from the Current Population Survey or the Survey of Income and Program Participation. In 1985, however, a U.S. Department of Labor advisory panel (Stromsdorfer, 1985) concluded that such an approach would be of little value; similar techniques used to evaluate predecessor programs yielded widely varying technique-specific results, and there was no clear way to choose among the alternative statistical methods that were used. The panel therefore recommended that a random assignment experiment be adopted to evaluate JTPA. The Department of Labor accepted this recommendation and issued Requests for Proposals to design and implement such an experiment. Contracts were subsequently awarded to Abt Associates Inc. and its subcontractors, New York University, MDRC, NORC, and ICF, to design the experiment and conduct the analysis, and to the Manpower Demonstration Research Corporation to implement the experiment.

The experimental design calls for a sample of 30,000 treatment and control group members from 15 to 20 JTPA local Service Delivery Areas (SDAs) that are as broadly representative as possible of the types of programs, participants, and labor market environments in the national program. The core of this design is random assignment of eligible JTPA applicants to treatment and control groups. To attain the target sample size, random assignment will be conducted in each participating SDA for up to 18 months. Outcome data for each sample member will then be collected through surveys and administrative data sources (welfare records,

unemployment insurance records, and Social Security earnings records) for a period of 30 months after random assignment. From these data, we will estimate program impacts on outcomes such as earnings, employment, and welfare receipt by comparing the postrandom assignment experiences of treatment and control group members.

Four target groups will be examined by the evaluation: adult men, adult women, out-of-school white youth, and out-of-school minority youth. Separate analyses for adult men and adult women reflect longstanding differences in evaluation findings for these two groups (e.g., see Ashenfelter [1978], Bloom and McLaughlin [1982], Bassi [1983], and Westat Inc. [1984]). Separate analyses for white and minority youth reflect well documented differences in their labor market experiences (e.g., see Freeman and Wise [1985]). Out-of-school youth, as opposed to students, will be targeted for the analysis because the problems, programs, and relevant outcomes for these two groups are too different to analyze them as one population, and because sample sizes for in-school youth are expected to be too small to evaluate outcomes for this group separately. The distribution of the national JTPA participant population among these four target groups and for in-school youth is: adult men, 28 percent; adult women, 29 percent; white and minority out-of-school youth, 11 and 16 percent, respectively; and in-school youth, 16 percent.[3]

Two major constraints were placed on the evaluation: the necessity to evaluate JTPA as it exists, and the need to secure program operators' agreement to implement the evaluation design (since SDA participation is voluntary). These requirements meant that, unlike evaluations of demonstration projects, our ability to manipulate program operations for purposes of the experiment would be limited. As described below, this limitation influenced key experimental design decisions and consequently influenced the types of experimental tests that could be conducted.

Defining Treatments To Be Tested

Decisions about treatment definitions involve subtle distinctions and difficult tradeoffs. Two key issues are central to these decisions:

° Whether to define treatments in terms of assignment to an activity or receipt of that activity; and

° Whether to define treatments as individual activities or as sequences and combinations of activities.

These issues, in turn, involve a tradeoff between the goal of evaluating JTPA as it currently operates versus the goal of evaluating specific employment and training activities.

There is, of course, strong policy interest in the effects of JTPA services on the people who actually receive these services. Thus all prior nonexperimental evaluations have focused on service receipt, measured ex post, rather than service assignment. In an experimental evaluation, however, treatments must be defined ex ante, at the time of random assignment. Only by doing so can one avoid the selection bias inherent in the determination of which individuals receive which services—the objective that prompted the experiment in the first place.

If treatments must be defined ex ante, then the estimated impact of an experimental treatment—i.e., assignment to a service—will be a good measure of the impact of actual receipt of that service only to the extent that assignment and receipt are highly correlated. If that is not the case in the program as it normally operates, then one is faced with a choice between changing the way the program normally operates, to ensure that most of those assigned to a particular service actually receive it, or explicitly estimating only the impact of assignment to particular services. Thus, the choice between defining treatments in terms of assignment and defining them in terms of receipt may involve a tradeoff between evaluating the program as it actually operates in the field and evaluating specific program services.

The issue of whether to define treatments as single services or as sequences and combinations of services involves a similar tradeoff. If sequences and combinations of services are the norm in the program as it actually operates, the effects of individual services can be estimated experimentally only by changing the way the program operates, so that individuals are assigned to specific services ex ante.

Both the rhetoric and the reality of JTPA suggest that it is as much a process by which services are delivered as it is a set of specific services. SDA staff test, counsel, and assess prospective participants and assign them accordingly to program activities. But many factors can intervene between assignment and receipt of services. For example, assignees may choose not to participate because they find a job, because they do not find what they want from JTPA, or because they are not motivated sufficiently to continue in the program. In addition, service providers may reject some individuals assigned to them. For these rejects, SDA staff may either try to find an alternative source of similar services, or try a different type of service. Thus, service

assignment can be a sequential trial and error process.

Furthermore, services are often intentionally provided in combinations and sequences. For example, basic education is often combined with classroom occupational skills training and job search assistance. Indeed, SDA staff counselors tend to view their job as helping to choose an appropriate mix of activities for their clients.

The essence of experimental estimation of the impact of a particular treatment is the comparison of a treatment group with a control group that matches the treatment group in every respect except assignment to that treatment. Thus, to isolate the impact of a specific service requires identifying a group that is destined to receive only that service and randomly assigning members of that group to treatment and control status. Regular JTPA operations make that difficult, in that persons assigned to a particular service do not necessarily get it and when they do get it they do not necessarily get only that service. Thus, to isolate individual services for the purpose of determining their impacts would require some modification of the JTPA program.

Likewise, to ensure that individuals receive the services for which they are recommended and assigned, in order to determine experimentally their impacts, would require a departure from current practices. Indeed, the more one leans toward experimental estimates of the impacts of receiving individual services, the closer one comes to running a special demonstration, instead of evaluating JTPA as it currently operates.

Given the primary goal of evaluating JTPA as it currently operates, we have focused the experimental design on impact estimates for the assignment of individuals to JTPA activity streams for which they were recommended by SDA staff. Because service recommendation decisions are the key point of control for SDA staff, and because activity streams appear to be common practice, this more fluid view of treatments is probably the most relevant way to represent JTPA programs. We refer to these experimental impact estimates, hereafter, as the impacts of service assignments.

At the same time, we recognize that there is considerable interest in the impacts of receiving specific employment and training activities. Fortunately, a relatively simple adjustment is available to take account of those assigned to the treatment group who receive no treatment at all (see Bloom, 1984). We cannot, however, correct for those who receive less service, or a different service, than intended. In defining the specific JTPA services or combinations of services that will constitute the experimental treatments, then, it

is important to attempt to ensure that individuals are likely to receive the treatment to which they were assigned. We turn now to the specific definition of treatments.

The initial design of the experiment called for random assignment to treatment or control status for individuals recommended to receive each of the three major JTPA services as their primary service: on-the-job training (OJT), classroom training in occupational skills (CT), and job search assistance (JSA). Since program staff recommendations would be known prior to random assignment, this design would provide well matched treatment-specific control groups. In effect, a separate experimental evaluation would be conducted for each major recommended service. Applicants recommended for other services (basic education, work experience, counseling, etc.) were to be excluded from the research sample.

This evaluation strategy was based on a widely shared policy interest in the three major JTPA services. Virtually all evaluations of JTPA's predecessor program, the Comprehensive Employment and Training Act (CETA), had focused on these services. Moreover, program data indicate that approximately 75 percent of JTPA participants receive one or more of these services. As we learned more about the specific features of SDAs that were contacted for our research sample, however, and as we considered more deeply the implications of a decision to emphasize evaluating JTPA as it actually operates, two issues caused us to reconsider our initial treatment definitions:

° The relatively infrequent occurrence of planned "stand-alone" JSA--i.e., JSA as the only service an applicant is expected to receive; and

° The potential importance of including all JTPA service streams in the research sample, for estimating overall JTPA impacts.

The first problem became clear when we computed expected sample sizes for each of our initial treatment categories in SDAs that were most likely to participate in the evaluation. Expected JSA samples were found to be relatively small--one-half the size of the expected OJT sample and two-thirds the size of the expected CT sample.

Perhaps more important is the fact that stand-alone JSA is often provided by default rather than by design. Some persons recommended by SDA staff for CT or OJT end up getting JSA only instead. In addition, JSA is frequently provided as a screening device. If participants find unsubsidized employment, they go on the books as a successful JSA recipient. If they do not become employed, they may be enrolled in a

different JTPA service or they may never become enrolled in JTPA.

Thus the number of participants who <u>receive</u> JSA almost certainly overstates the number initially <u>recommended</u> for JSA as their primary activity. And it is this latter number that is relevant for this evaluation, since treatment groups must be defined <u>ex ante</u>, at the point of random assignment, rather than <u>ex post</u>, on the basis of services actually received. To increase the sample of participants recommended for stand-alone JSA substantially would require markedly changing current SDA practices.

The second problem with our initial treatment definitions was that they excluded participants recommended for services other than CT, OJT, or JSA. Because these "other JTPA activities" represent between 10 and 20 percent of JTPA participants in SDAs that are most likely to participate in the evaluation, excluding them may exclude an important piece of JTPA.

Given the likelihood of a small stand-alone JSA assignee sample and the desirability of including all JTPA services in the overall JTPA impact estimates, we decided to combine JSA with the remaining JTPA services into one "other services" (OS) treatment category. For purely descriptive purposes it will be possible to estimate separate OS impacts. However, because of the diversity of treatments within this category, it will be difficult to interpret such findings. Nevertheless, combining OS impact estimates with those for the CT and OJT streams, weighted appropriately, will provide the best available estimates of overall JTPA impacts.

Exhibit 1 summarizes our treatment definitions.[4] Each column in the exhibit represents an experimental treatment or control group category. Each row represents a JTPA service. A "yes" in a cell designates the primary service for that experimental treatment. Thus OJT is the primary service for the OJT treatment, and CT is the primary service for the CT treatment. A "+" in a cell represents an allowable secondary service for an experimental treatment. Thus, for example, JSA can be provided as a secondary service, in addition to the OJT primary service, for persons assigned to the OJT treatment. A "No" in a cell represents a nonallowable service for a particular treatment. For example, OJT may not be provided to persons assigned to the CT treatment and CT may not be provided to persons assigned to the OJT treatment. Control group members are excluded from all JTPA services. This is required in order for their subsequent experience to reflect what the experience of JTPA treatment group members would have been if they had been excluded from JTPA.

To help maintain the distinctions between the OJT and CT treatments and to strengthen the linkage between services assigned and services received, we will attempt to gain SDA cooperation in maximizing the number of persons who receive their recommended primary service. Thus we will work with SDAs to minimize the number of treatment group members who do not receive services. In addition we will try to get SDAs to work toward ensuring that primary services are received before secondary services are provided, in order to reduce the number of persons who receive secondary services <u>instead</u> of primary services.

The question marks associated with CT and OJT in the Other Services (OS) treatment column of Exhibit 1 reflect an unavoidable ambiguity in the definition of this treatment. Ideally, the OS treatment stream should represent individuals who SDA staff have decided not to recommend for CT or OJT. In other words, assignment to this treatment category should reflect anticipated exclusion from the first two categories. Exclusion of OS assignees from CT and OJT would help distinguish this category from the others. In addition, it would help remove incentives for SDAs to "game the experiment" by overassigning individuals to OS, where they would have the most discretion if all JTPA services were allowable.

Unfortunately, this clear distinction runs counter to the way JTPA operates in the field. For a significant number of applicants initially assigned to services other than OJT and CT, SDA staff are undecided as to what, if any, services should follow these initial assignments. Thus some portion of these participants end up in OJT or CT following receipt of their initial services. To prohibit these sequences would materially change the way JTPA treats these individuals. Therefore, we have decided not to exclude OS assignees from CT or OJT. We will, however, work closely with SDA staff during their training for the evaluation, to help ensure that CT or OJT is clearly not <u>anticipated</u> for persons who are assigned to OS. Moreover, we will place a limit on the number of applicants who can be assigned to the OS category, to prevent SDAs from gaming the experiment.

It should be noted that, although we have defined the OS treatment as a single, heterogeneous category that potentially includes all JTPA services, we will retain the ability to distinguish individuals within this category in terms of their initial service assignment. At the time of random assignment, we will ask SDAs to indicate the JTPA services for which their clients are being recommended in terms of seven categories (CT, OJT, JSA, basic education, work experience, combined CT-OJT, and miscellaneous services). CT and OJT treatment constraints, as shown in Exhibit 1, will apply to persons assigned to these two treatments and OS treatment constraints will apply to all others.

These seven categories will provide matched treatment and control groups for seven treatment streams. Thus, for example, we could produce

Exhibit 1

Experimental Treatment Definitions

Allowable JTPA Service	Treatment Category			
	CT	OJT	Other Services	Control
Classroom skills training	Yes	No	?	No
On-the-job training	No	Yes	?	No
Job search assistance	+	+	+	No
Basic education	+	+	+	No
Work experience	+	+	+	No
Miscellaneous services	+	+	+	No

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separate JSA impact estimates if the stand-alone JSA assignment sample were large enough to warrant doing so. This would apply to the other OS subcategories as well, although their sample sizes probably would be much smaller. Regardless of sample size, however, identifying seven treatment categories at the time of random assignment will maintain the future option of combining groups in different ways. For example, the basic education assignment sample might later be combined with the CT assignment sample to define a general classroom training treatment, for some analytic purposes.

If, in fact, our three basic treatment streams (CT, OJT, and OS) turn out to represent very different service packages, and these service packages can be summarized in a meaningful generic way, then our impact estimates will tell us a great deal about the relative effectiveness of different service paths, as they exist currently under JTPA. Alternatively, if service assignments turn out to bear little relationship to services that are received, and all three service streams tend to look alike, they will lose their analytic and operational meaning. In particular, to the extent that primary services are not received and secondary services are received instead, distinctions between the CT and OJT streams will become less meaningful. Of special concern are CT and OJT assignees who receive only JSA. This may occur because persons who first receive JSA are then assigned to CT or OJT but receive no further service. In addition, this may occur because persons who are assigned to CT or OJT but cannot find a slot are given JSA as a consolation.

To the extent that SDAs help enforce treatment distinctions, by trying hard to find a suitable slot for each person randomly assigned to treatment, we will ultimately obtain meaningful distinctions. From SDA responses so far, there appears to be reason for guarded optimism on this issue. However, even if treatment streams ultimately tend to blur, we can still pool findings across streams to provide meaningful average JTPA impact estimates, for the program overall, and by target group. Thus our analytic fallback position, after the fact, is to aggregate findings.

Implementing Random Assignment

Exhibit 2 presents a schematic summary of the experimental design that implements random assignment to the treatments described above. This random assignment model can be integrated into the normal SDA intake process as follows. Individuals first apply to JTPA and are screened for eligibility; those who are eligible are assessed to determine their training needs and capacities, and are recommended for CT, OJT, or OS. During this process, applicants will be

informed about the evaluation and the fact that they will be randomly assigned to a treatment or a control group. They also will be asked to sign an informed consent statement that indicates they have been properly informed and that authorizes the release of information from their welfare, unemployment insurance, and Social Security earnings records for use by the evaluation.
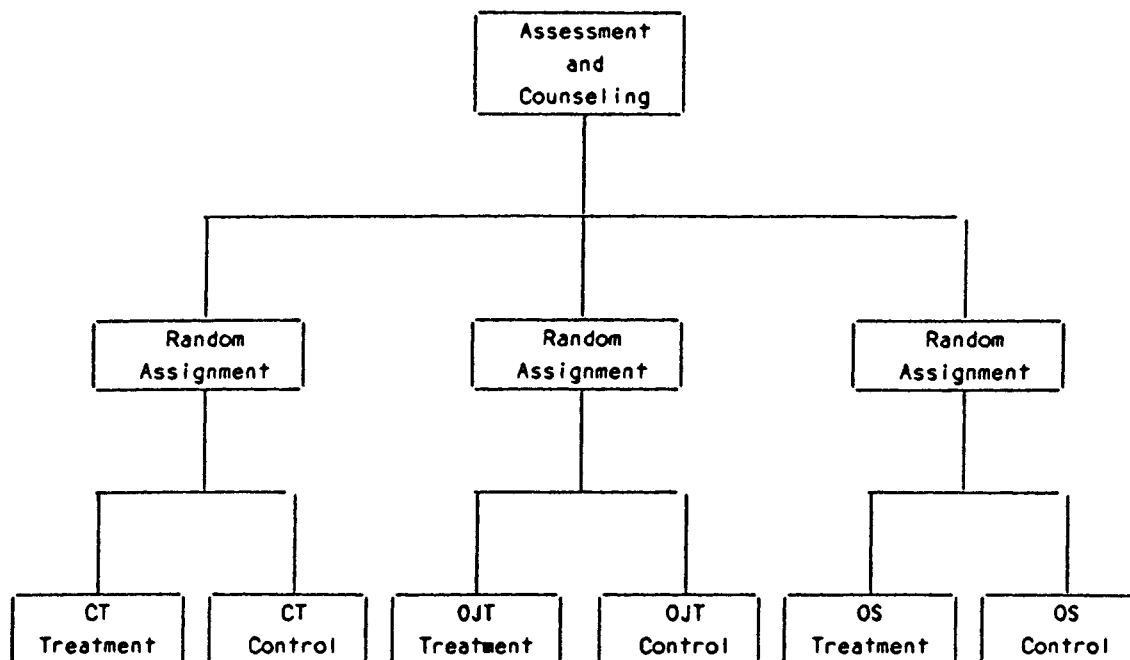
After an eligible JTPA applicant is assessed and recommended for a specific treatment, SDA staff will call the evaluation contractor using a special dedicated long-distance phone line. The evaluation staff will record identifying information for the individual and, using a computer algorithm, will randomly assign him/her to treatment or control status. Treatment group members will be contacted personally by SDA staff, as soon as possible thereafter, to help ensure their participation in the program. Control group members will be informed of their status by mail in most sites. The random assignment algorithm will assign two-thirds of the research sample to the treatment for which they were recommended and one-third to a treatment-specific control group.

Placement of random assignment just prior to the point where individuals enter a specific treatment stream is extremely important. Relative to designs that place random assignment earlier in the program intake process, this ensures that a higher proportion of persons assigned to JTPA treatments will actually receive them. This feature serves two related purposes. First, it increases the service contrast between treatment and control group members. Thus, we will have a greater chance of observing impacts, if they exist. Second, since a greater proportion of persons assigned to treatment will receive it, the link between service assignment and service receipt is strengthened, which thereby improves our chances of estimating meaningful impacts for services that are received.

At the same time, placement of random assignment after assessment may create problems, because some SDAs provide a limited version of JSA to screen applicants as part of their counseling and assessment (referred to hereafter as "up-front" JSA). Persons who find a job (are placed) during this process are never recommended for a JTPA service stream and consequently would not go through random assignment. Thus they would be excluded from the research sample. Available evidence, although sketchy, suggests that this missing piece of JTPA would be quite small. Some, but not all, SDAs that are likely to participate in the evaluation provide up-front JSA; only a fraction of the participants from these SDAs receive it, however. On balance, it appears that only 25 to 30 percent of all eligible JTPA applicants in our research sample would receive

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Exhibit 2

RANDOM ASSIGNMENT MODEL

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                        │  Assessment  │
                        │     and      │
                        │  Counseling  │
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            ┌──────────────────┼──────────────────┐
      ┌─────┴──────┐    ┌──────┴─────┐    ┌────────┴────┐
      │   Random   │    │   Random   │    │   Random    │
      │ Assignment │    │ Assignment │    │ Assignment  │
      └─────┬──────┘    └──────┬─────┘    └────────┬────┘
       ┌────┴───┐         ┌────┴───┐         ┌─────┴──┐
   ┌───┴──┐ ┌───┴──┐  ┌───┴──┐ ┌───┴──┐  ┌───┴──┐ ┌───┴──┐
   │  CT  │ │  CT  │  │ OJT  │ │ OJT  │  │  OS  │ │  OS  │
   │Treat-│ │Cont- │  │Treat-│ │Cont- │  │Treat-│ │Cont- │
   │ment  │ │rol   │  │ment  │ │rol   │  │ment  │ │rol   │
   └──────┘ └──────┘  └──────┘ └──────┘  └──────┘ └──────┘
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Treatment and Control Status

CT	= Classroom occupational skills training
OJT	= On-the-job training
OS	= Anticipated services other than CT or OJT
Control	= No JTPA services

Experimental Impact Estimates

Impact of assignment to CT
Impact of assignment to OJT
Impact of assignment to OS
Impact of assignment to JTPA

up-front JSA, and only a small fraction of this group would be placed prior to random assignment.

The second problem produced by up-front JSA is that persons who are not placed go through random assignment, and end up in the control group. If up-front JSA is a weak treatment with a negligible impact, then control group exposure to this service is not much of a problem. On the other hand, if up-front JSA has an appreciable impact, its receipt by substantial numbers of control group members would be serious. Discussions with staff from SDAs that are most likely to participate in the evaluation suggest that where provided, most up-front JSA is less than eight hours in duration. Thus it appears to be a minimal service.[5]

A final point to consider when assessing the importance of up-front JSA is the fact that only persons who are not placed by this service continue in JTPA, and thereby become control group candidates. For these up-front JSA "failures," it is difficult to imagine that the service had much effect.[6]

On balance, we thus do not believe that up-front JSA will appreciably bias the experimental estimates, given that:

° It would apply only to a fraction of the control group;

° It is a minimal service; and

° Only recipients who are not placed (and thus are unlikely to be affected) can become control group members.

We shall continue to evaluate this situation, however, as more SDAs are recruited. If at some point the situation changes, three options will be considered:

° Placing random assignment before up-front JSA, and thus before treatment streams are determined (which would eliminate treatment-specific impact estimates, but would preserve overall JTPA impact estimates);

° Excluding SDAs where this practice is sufficiently intensive and extensive to be a problem (which would reduce our potential pool of SDAs); and

° Negotiating with SDAs to eliminate or cut back this practice (which would change the program somewhat and might be difficult to arrange).

What Can Be Learned from the Evaluation?

The design described here will provide internally valid (i.e., unbiased) estimates of the net impact of JTPA--both overall and by treatment stream--on those assigned to JTPA services. Ideally, in addition to the net impact estimates, the evaluation would also calculate differential impacts for different treatments--i.e., it would determine what would happen if one service were substituted for another. To estimate differential treatment effects experimentally, however, one must randomly assign a common pool of individuals to multiple treatments. For example, one could randomly assign all persons recommended for CT to either CT or OJT. This would allow direct comparison of the effects of OJT and CT on the kind of participants who normally receive CT. However, random assignment directly to multiple treatments was judged to be clearly unacceptable to program operators.

One could, of course, approximate the differential impacts by calculating differences in treatment-specific net impacts. Thus, for example, if assignment to CT produced a $700 earnings gain and assignment to OJT produced a $500 earnings gain, then a first approximation to differential impacts would be $700 minus $500, or $200. But persons assigned to CT and OJT represent different groups and thus may respond differently to treatment. One way to reduce this difference is to control statistically for observed individual characteristics (age, sex, race, prior employment, etc.). This can be accomplished by interacting these characteristics with treatment status in deriving impact estimates and then computing expected CT and OJT impacts for the "same" individual (e.g., the typical CT or OJT participant).[7] In doing so, we will be limited to individual characteristics for which data have been collected. Thus we will not be able to control for unobserved differences between CT and OJT recommendees. By examining how impacts for these two treatments vary across types of participants, however, we may be able to provide some insight into likely differential treatment effects for groups with common characteristics.

While we will thus be somewhat limited in our ability to answer questions about the effects of changes in the allocation of JTPA services among participants, we will be able to say a great deal about which services work best for whom under the existing allocation. Because all JTPA services will be included in the evaluation, we will be able to produce comprehensive overall JTPA impact estimates for adults and out-of-school youth. This "bottom line" finding will summarize the impact for the mix of services provided by the program and the mix of participants who receive these services. In addition, our design will provide opportunities for disaggregating findings in ways that will promote a deeper understanding of what services work for whom, and why.

For example, according to the JTPA legislation, the program is intended to serve individuals who are both most in need of assistance and who will benefit most from it. By comparing impact estimates for various subgroups with their corresponding program participation rates (relative to their presence in the JTPA-eligible population), we will be able to measure the extent to which the program is achieving this objective. In addition, we can provide valuable information for future program targeting decisions by estimating average impacts by target group (adult men, adult women, out-of-school white youth, and out-of-school minority youth) and by estimating impacts for other policy-relevant groups (welfare recipients, long-term unemployed persons, individuals with little or no prior employment experience, etc.).

Similarly, since our design will randomly assign individuals to their recommended treatment stream or a treatment-specific control group, we will be able to provide separate impact estimates for assignment to CT, OJT, or OS. Such information will help us to interpret the overall JTPA "bottom line" by indicating which service streams are making what contributions to the overall program impact. In addition, to the extent that we are successful in maintaining clear distinctions among the CT, OJT, and OS streams (by enforcing the constraints for each treatment definition), we will be able to distinguish services that work well from services that do not. Furthermore, by disaggregating each treatment's impacts by type of participant, we will be able to provide information about which groups respond most favorably to each treatment. Thus we can further refine our empirical support for program targeting decisions.

We also plan to examine the extent to which JTPA produces changes in family size and composition, family income (from all sources), fertility patterns (especially for youth), and future educational achievement (again, especially for youth). Correlating these effects with each other, and with the labor market effects outlined above will further improve our knowledge of how programs work and why they may work for some groups but not for others.

FOOTNOTES

[1]See, for example, Auspos, Cave and Goldman (1986); Bell, Burstein and Orr (1987); Bloom and Kulik (1986); Goldman (1981); Gueron (1986); and Kemper, Long and Thornton (1981).

[2]The exception to these statements was the state work/welfare initiatives, which were conducted by regular WIN programs. These projects were, however, intended to be demonstrations of innovative employment and training approaches.

[3]These figures are based on data for Program Year 1984 from the JTPA Annual Status Report (JASR).

[4]There is one relatively minor exception to these definitions. A small number of JTPA participants may receive both OJT and CT, usually as part of a "customized" training program developed to meet the needs of a specific employer. To preserve the interpretability of the OJT and CT treatments, these individuals will be included in the OS category unless it is clear that either OJT or CT is the predominant service received.

[5]These figures are based on qualitative judgments, not hard numbers. Detailed records are not kept by SDAs, because up-front JSA often is provided before individuals are formally enrolled in JTPA.

[6]Theoretically, up-front JSA failures could experience a delayed effect. We find no convincing evidence that this is likely to occur, however.

[7]More precisely, one would interact individual characteristics with the treatment dummy variable separately for the CT and OJT treatment and control group analyses.

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