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ABSTRACT

A study examined the feasibility of using a "nonexperimental" technique to evaluate Job Training Partnership Act (JTPA) programs for economically disadvantaged adults. New statistical techniques were applied to data about a sample of Utah JTPA participants and data about Employment Security registrants linked with their individual Unemployment Insurance earnings records programs in order to derive bias-free "quasi-net impact" estimates. The analysis revealed that JTPA completers who were either placed in unsubsidized jobs or retained by their employer in the case of on-the-job training were significantly (10 percent) more likely to be employed 2 years after their original enrollment. The earnings of program completers placed into unsubsidized employment were also significantly higher (by nearly $1,000 for women and by more than $1,300 for men) than those of non-JTPA participants. The study confirmed the possibility of developing statistically reliable program evaluations for JTPA through the use of nonexperimental techniques that rely on earnings records readily available to states. (Appendices contain the following: experimental evaluations of programs for economically disadvantaged adults, estimates of the selection bias of nonexperimental models, preentry earnings models and selection bias tests, quasi-net impact employment and earnings models, gross versus net impacts of JTPA program activities, and 71 references.) (MN)

EVALUATING JTPA PROGRAMS FOR ECONOMICALLY DISADVANTAGED ADULTS: A Case Study of Utah and General Findings

RESEARCH REPORT

National Commission for Employment Policy

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EVALUATING JTPA PROGRAMS FOR ECONOMICALLY DISADVANTAGED ADULTS: A Case Study of Utah and General Findings

by:
William R. Bowman
Annapolis Economic Research

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and
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PREFACE

Since the Job Training Partnership Act (JTPA) was first enacted a decade ago, relatively few states have evaluated the effectiveness of the training activities in improving adult participants' likelihood of employment, and in increasing their earnings if they are employed. State-level evaluations are particularly important since states have major responsibilities for managing the JTPA programs and for establishing policies which affect the operations and performance of Service Delivery Areas.

A few states have undertaken non-experimental evaluations of their programs: a compariso of the employment and earnings of JTPA participants after they left the program with the employment and earnings of a comparison group who did not enroll in JTPA. Data on earnings are obtained from Unemployment Insurance (UI) wage records. For the most part however, states have relied on descriptive data on post-program outcomes to assess the effectiveness of their programs. This approacn can produce misleading results, as this report indicates.

To encourage state-level evaluations of JTPA, the Commission and the State of Utah co-sponsored this report on the effectiveness of JTPA Title II programs for adults in Utah. The technique used is non-experimental and the comparison group was selected from registrants with Utah's Employment Security. In a step-by-step approach, the report documents how non-experimental techniques can be applied and several specific technical issues can be addressed.

The report has produced two major findings. **First, states need not rely on descriptive information to assess JTPA's effectiveness.** It is feasible to use UI wage-record data and non-experimental techniques to evaluate JTPA programs for economically disadvantaged adults. Smaller states may wish to pool their data to achieve greater statistical precision and to conserve on resources. Also, these evaluations require that state JTPA offices work closely with state UI offices to ensure that the necessary data are maintained. Long-term evaluations of program impacts need UI wage record data spanning many years, from 2 or more years prior to participants' enrollment in JTPA training to 2 or more years following enrollment.

The second major finding from this report is that non-experimental techniques produce results on post-program outcomes that are highly relevant to policy makers. This particular finding comes at a time when the major alternative source of information on JTPA's effectiveness is based on a national experiment, funded by the U.S. Department of Labor. The recently released results from the "National JTPA Study" have become the benchmark for assessments of JTPA's effectiveness.

However, due to difficulties encountered when the experiment was being implemented, results from the National JTPA Study ought not to be considered definitive evidence of the impact of receipt of JTPA training. While the National Study was designed to be a scientifically rigorous social experiment, complete with random assignment of individuals into either a "treatment" or a "control" group," it did not estimate the "net impact" of receipt of JTPA training. That is, the study did not fully answer the question: Did adults who were in JTPA training programs fare better than those who did not receive training?

Instead, the treatment group consisted of persons assigned to a JTPA training activity; some of them subsequently enrolled in another JTPA activity rather than the one to which they were

assigned, while others did not enroll in any activity. The control group consisted of persons assigned to a JTPA activity, but not allowed to enroll in that activity; some subsequently received JTPA-funded job search assistance while others received training through a non-JTPA funded source.

In contrast, the results from this study of Utah, although they are from only one state, shed light on the effectiveness of receipt of JTPA training. For men, participation in Utah's JTPA occupational classroom training and on-the-job training programs was associated with an improved likelihood of employment more than 1 year after participants left the training. For women, participation in on-the-job training was associated with a greater likelihood of being employed 1 year later. For those who were employed, the strongest earnings payoffs were associated with on-the-job training (for women) and occupational classroom training (for men). The results are broadly consistent with those of the National JTPA Study. In particular, on-the-job training appears to be a key training activity.

In addition, participants in Utah who had completed their training and had been placed by the program, experienced earnings gains 2 years later. This last finding highlights the potential importance of JTPA's job placement activities.

The Commission believes that this report provides strong encouragement to states desiring to evaluate their JTPA programs. It also provides important information on the effectiveness of JTPA programs for improving the labor market position of economically disadvantaged adults.

JOHN C. GARTLAND
CHAIRMAN

Table of Contents

ACKNOWLEDGEMENTS i

CHAPTER ONE: Introduction 1

Objective Of Study 1
Study Outline 3

CHAPTER TWO: Prior Evaluations Of Federal Training Programs For Economically Disadvantaged Adults 5

Program Outcomes Versus Program Net Impacts 5
Pre-JTPA Non-Experimental Evaluation Studies 6
Experimental Evaluation Studies 9
Reconciliation Of Findings Based On Alternative Methodologies 11

CHAPTER THREE: Utah JTPA Title II-A Adult Participant Characteristics . . . 17

Utah's JTPA Programs During PY 1987 17
Utah JTPA II-A Adult Participants 19

PRIOR EMPLOYMENT AND EARNINGS EXPERIENCES OF TITLE II-A ADULTS . . 23

CHAPTER FOUR: JTPA II-A Adult Comparison Group Selection 29

PART I: Identifying Comparison Groups For Non-Experimental Evaluations of JTPA 30

DATA BASE FOR IDENTIFYING COMPARISON GROUP 30
METHODOLOGIES USED FOR CHOOSING COMPARISON GROUPS 32
ALTERNATIVE COMPARISON GROUPS FOR UTAH STUDY. 34

PART II: Tests For Selection Bias In Non-Experimental Models 39

MEASURE OF PRE-INTERVENTION EARNINGS FOR SELECTION TESTS 41
SELECTION BIAS TESTS DEVELOPED FOR UTAH STUDY 41

PART III: Results Of Selection Bias Tests For Utah Study 42

TESTS FOR SECONDARY SELECTION BIAS AMONG JTPA PARTICIPANTS 44

CHAPTER FIVE: Utah JTPA II-A Adult Program Impacts 53

PART I: Unadjusted Versus Adjusted Program Impacts 53

POINT ESTIMATES OF PROGRAM IMPACTS 54
CONFIDENCE LEVELS OF ADJUSTED PROGRAM IMPACT ESTIMATES 58
THE "HARD-TO-SERVE" ADULTS IN UTAH 60

PART II: Sensitivity Of Program Impact Estimates 61

THE IMPORTANCE OF A WELL CHOSEN COMPARISON GROUP 62
IMPROPER SPECIFICATION OF PRE-ENTRY WORK EXPERIENCE 62
OBSERVED VERSUS UNOBSERVED ADJUSTMENTS 64

CHAPTER SIX: Conclusions 69

Major Findings 70
Policy Implications 71

APPENDIX A: Experimental Evaluations Of Economically Disadvantaged Adult Programs 75

The National Supported Work Demonstration 75
The National JTPA Experiment 76
Summary 83

APPENDIX B: Estimating Selection Bias Of Non-Experimental Models 89

The Lalonde Study 89
The Fraker And Maynard Study 91
The Heckman And Hotz Study 92
Summary 93

APPENDIX C: Pre-Entry Earnings Models And Selection Bias Tests 97

APPENDIX D: Quasi-Net Impact Employment And Earnings Models 101

APPENDIX E: Gross Versus Net Impacts Of JTPA Program Activities 111

REFERENCES 115

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BARBARA C. McQUOWN
DIRECTOR

CHAPTER ONE: Introduction

Objective Of Study

Since the 1960s, the federal government has sponsored employment and training programs to assist economically disadvantaged youth and adults. Their goal is to improve participants' prospects in the labor market -- to increase their likelihood of employment and if employed, to increase their earnings. During this period, over 50 federal training programs have spent roughly $100 billion under five legislative Acts -- the Area Redevelopment Act of 1961 (ARA), the Manpower Development and Training Act of 1962 (MDTA), the Economic Opportunity Act of 1964 (EOA), the Comprehensive Employment and Training Act of 1973 (CETA), and the Job Training Partnership Act of 1982 (JTPA).

Unlike its predecessors, JTPA has had an evaluation component tied explicitly to its legislative foundation. The 1982 Act states that federally sponsored employment and training efforts are "investments in human capital"; in Section 106(a) it further states:

> ... it is essential that criteria for measuring the return on the investment be developed; and the basic return ... is to be measured by the increased employment and earnings of participants and the reduction in welfare dependency.

For the first decade under JTPA, states have only been able to measure the effectiveness of Title II-A programs for the economically disadvantaged against a set of national standards, which may be adjusted to reflect varying client characteristics and local economic conditions in the 650 plus local Service Delivery Areas (SDAs) across the nation. These measures provide only relative outcome information across the SDAs -- not measures of return on investments, however. The latter can only be determined by comparing the outcomes of participants against what would have occurred had enrollees not participated in JTPA -- the "net impact" of training.

The major objective of this study is to provide states with a methodology for evaluation that meets the guidelines of the Act. The methodology proposed is carried out using as a prototype the state of Utah's JTPA II-A adult program for participants entering during PY 1987. This approach is **non-experimental** in nature; it permits evaluation of on-going programs without disrupting normal operational behavior. Unlike social experiments where eligible clients are randomly assigned to a treatment group or to a control group that is denied services, no one eligible for JTPA assistance need be turned away when using this approach. It is an evaluation methodology that utilizes currently available administrative data bases -- JTPA data file supplemented with state Employment Security registrant files and Unemployment Insurance (UI) quarterly earnings records. As such, the methodology shortens the evaluation cycle and is economical.

Finally, it is a methodology that captures the behavior of clients and program operators within its statistical design so that administrators cannot only learn if a program "works," but **how** the program works, and for **whom** the program works. The information available to program administrators in this non-experimental evaluation is encompassing. The more years of individuals' post-training UI earnings records that are archived, the longer the experience can be measured. The approach can be used for all JTPA programs as well as other state-supported

programs such as higher education, welfare, or even for evaluating the employment and earnings effects of incarceration in a state's penal system.

As discussed in detail throughout the study, this non-experimental approach is only now coming into its own. Previous work utilizing this methodology was plagued by a fundamental problem, termed **selection bias**, which seriously diminished the merit of earlier evaluations using the non-experimental approach. In particular, the non-experimental approach compares the outcomes of those receiving assistance (the treatment group) with similar individuals who did not receive the assistance (the comparison group, analogous to an experimental control group).

Two problems, heretofore, have plagued such studies: the comparison group identified was often dissimilar from the treatment group, and the process by which individuals sorted themselves into either the treatment group or comparison group was not a random process, as is characteristic of social experiments. If, for example, individuals who self-select into JTPA are less motivated, have fewer job contacts, or have greater not-easily-identified employment barriers than those in the comparison group, the evaluation will yield "downward biased" estimates of the net impact of JTPA. In this case, JTPA enrollees may experience lower earnings than the comparison group -- not because the program made them worse off -- but because the individuals who selected into JTPA are "different" from those who chose not to enroll.

Over the last 10 years, improvements in statistical modeling have reduced the suspected selection bias of the non-experimental approach. New methods have been developed to capture better unobserved factors that give rise to selection bias. Also, new tests have been developed to yield better comparison groups which are less susceptible to selection bias. These improvements now make it possible for states to carry out their own evaluation of employment and training programs initially envisioned in the JTPA Act.

A secondary objective of this study is to raise the status of this non-experimental approach relative to social experiments that have been in vogue since the 1970s. A major experimental evaluation of JTPA -- the National JTPA Experiment (NJE) -- has been undertaken since 1986, and its 18-month interim report is just now becoming available to the public.[1] The common perception of this experiment is that its results must reflect the "true" net impact of JTPA since, after all, the social experiment randomly assigned individuals to either the treatment or control group to avoid selection bias.

The results of NJE are not very flattering of JTPA. Adult net impacts are perceived to be "marginal," while those of youth are "disturbing," especially for young males who supposedly are made **worse off** by participating in JTPA.[2]

The problem is that the JTPA experiment was not able to be carried out as originally planned -- resulting in selection bias problems of its own. (Appendix A details these problems.) The seriousness of these selection biases is unknown and may, in fact, never be able to be quantified. Such a state of affairs suggests that major policy changes should not be based upon the NJE results, which may be seriously misleading, or at best uncertain. In truth, after a decade of JTPA we still know very little regarding the effectiveness of our country's major employment and training program, as called for in the original legislation.

This report envisions that state JTPA administrators will begin serious evaluations of their own programs. At best, social experiments can indicate **if** JTPA works. Careful non-experimental evaluations of state JTPA programs can provide service providers with information to improve program effectiveness: the **why**, **how**, and for **whom** JTPA "works." This, after all, was what Congress envisioned in Section 106 of the 1982 JTPA legislation.

Study Outline

Chapter Two below, sets the stage for the non-experimental empirical analysis. It discusses the reasons why this study was undertaken, including the issues raised in the evaluations of employment and training programs undertaken in the early 1980s that led to the Department of Labor's NJE. It also introduces the "non-experimental" evaluation technique used in this study, and describes how better comparison groups can be identified and new statistical techniques can be applied to derive "quasi-net impact" estimates free of selection bias.

Chapters Three and Four delve into issues involved in selecting a comparison group. Chapter Three describes key demographic and labor market characteristics of JTPA Title II adult participants in Utah, who are the focus of the analysis – and whose characteristics are used to match similar individuals from the Employment Security file. Chapter Four discusses the process of selecting this comparison group. By its very nature, this process is technical and the discussion is lengthy. This chapter is intended primarily for evaluators who would be implementing a study similar to the one in this report.

Chapter Five presents the results of the Utah non-experimental evaluation. First, simple descriptive statistics on outcomes are given and are compared with outcomes derived from a rigorous statistical analysis. This discussion indicates the extent to which simple descriptive data can be misleading. Two topics are highlighted in the results of the statistical analysis: the probablistic nature of estimates of the post-program employment and earnings impacts, and the usefulness of the analysis for identifying "hard to serve" participants.

Chapter Five then turns to a comparison of alternative estimates of program impacts. The most reliable ones -- based on the best comparison group, and using the most extensive data and the most sophisticated statistical techniques -- are systematically compared with others. The other results use, respectively, another comparison group, less extensive data, and less rigorous techniques. These comparisons demonstrate that results can differ substantially depending upon the approach taken. While it is necessary to be careful in using a non-experimental approach in program evaluation, the technique can be usefully applied with carefully chosen comparison groups and properly specified program impact models.

The concluding Chapter Six highlights findings and discusses their implications for policies to improve both the operation of employment and training programs and states' evaluation of them.

Endnotes

1. Bloom, et al.(1992). (An Executive Summary, pre-released in May of 1992, has been available to the public. Copies of the Interim Report, Bloom, et al. (1993), began to become available in January 1993.

2. The popular press has interpreted the findings of the 1992 Executive Summary in quite misleading ways - further adding fuel to the fire regarding faulty perceptions of JTPA program effectiveness. See for example, James Bovard, "Clinton's Summer Jobs Sham," The Wall Street Journal, March 5, 1993; and Spencer Rich, "Runaway Training: Why Educating Poor, Unskilled Workers Won't Land Them Jobs," Washington Post, February 7, 1993.

CHAPTER TWO: Prior Evaluations Of Federal Training Programs For Economically Disadvantaged Adults

As indicated above, JTPA was the first federal employment and training program to be viewed as an investment in human capital and one that was to be evaluated according to a rate of return measure of performance. This meant program performance was to be evaluated on economic efficiency grounds, in addition to its effectiveness in improving opportunities of the economically disadvantaged in the labor market.

Section 106(a) of the Job Training Partnership Act (JTPA) of 1982 called for evaluation analysis of program performance so Congress and employment and training officials could better ascertain the effectiveness of JTPA in comparison with other government employment services and training alternatives. The U.S. Department of Labor (USDOL) was instructed to analyze JTPA program performance according to: (1) increased participant employment and earnings, (2) decreased income support costs (i.e., welfare and unemployment), and (3) increased tax revenues.

The purpose of this chapter is to trace the historical background of evaluations of employment and training programs for economically disadvantaged adults, so that the methodological and statistical grounds of the approach used in this study will be understood in the broader context of evaluation research. First, the chapter distinguishes between program-effectiveness measures related to performance standards and those measures related to net impacts, which are at the heart of program evaluation. Next, the non-experimental evaluations of JTPA's predecessor programs are discussed from the perspective of their weaknesses, which led to their abandonment by USDOL for the more theoretically appealing experimental approach. With this as background, the current National JTPA Experiment (NJE) is described, along with suspected selection biases which resulted from implementation problems the experiment encountered.

Finally, advances in the non-experimental approach are briefly outlined; they provide the rationale for resurrecting non-experimental approaches to evaluating employment and training programs. These advances should be especially appealing to states that either cannot afford the more costly experimental evaluations and/or choose not to subject on-going programs to the disruptions caused by an experimental approach.

Program Outcomes Versus Program Net Impacts

Unlike the observed program outcomes, which can be directly compared to JTPA (adjusted) performance standards, program net impacts must be estimated using statistical modeling techniques. For example, the program's net impact on post-training earnings is calculated as the differential between actual post-program earnings of participants and that estimated with statistical models of projected earnings had participants not enrolled in JTPA. (The latter cannot

be observed, rather it is "counterfactual" based upon what enrollees would have experienced in the absence of training.)

Both measures give different information as to program effectiveness. Program outcomes measured against performance standards can be used, for example, to describe the distribution of Service Delivery Area (SDA) performance across a state. In contrast, program net impacts can be used to determine if the state JTPA system as a whole improves the economic welfare of enrollees. Clearly both are of importance to employment and training administrators. The objective of this study is to examine the feasibility of deriving state-based net impacts of program performance.

The difference in the two measures of program performance has recently come to light with the 18-month interim results of NJE, first made public in May of 1992. In the same year the random assignment process of the NJE was being implemented (Program Year 1987), information on outcomes of JTPA's adult programs appeared to be quite favorable. For example, the actual Adult Entered Employment Rate (the percent of adult terminees who are in unsubsidized jobs) for the system as a whole was 72%, compared to the national standard of 62%. Also, the actual Average Wage at Placement (the average wage at termination of those employed) was $5.11, compared to the national standard of $4.91.[1]

Following 10 years of states' using such information as this as a guide to program effectiveness, the NJE gave JTPA its first "score card" on how the system appeared to be operating. As noted in Chapter One, the news was quite sobering. Adult women assigned to JTPA were estimated to increase their chances of being employed by 2% and to increase annual earnings by $1 a day over the control group not assigned to JTPA. Adult men, while more likely to be employed, did not experience significant earnings gains during the 18-month period.[2] Clearly the two measures of program effectiveness tell us very different things.

The fact that evaluations of relative SDA performance are just now being supplemented with net impact evaluations is not a result of neglect or oversight. Rather it is the result of a long-standing and heated debate within the technical evaluation community of how best to derive net impact estimates of federal training programs.

Pre-JTPA Non-Experimental Evaluation Studies

Originally, USDOL administrators envisioned that it would be best to sponsor non-experimental evaluations of the new JTPA program. Social experiments, which had come into vogue during the 1970s, were used mainly to evaluate expected outcomes of demonstrations which used yet-to-be implemented policies.[3] At that time it was thought best to evaluate JTPA as it normally operated rather than under experimental conditions of never-tried policies.

USDOL thought that consistent and reliable estimates of JTPA program impacts could be derived through non-experimental studies that used improved enrollee and comparison group surveys. The Continuous Longitudinal Manpower Survey (CLMS) was to be replaced with the Job Training Longitudinal Survey (JTLS) and the Current Population Surveys (CPS) was to be supplemented with the Survey of History of Work (SHOW). SHOW would provide researchers with a national survey of non-participants, which could be used to develop improved comparison groups for analysis of program impacts.

This non-experimental approach had its roots in the late 1970s when USDOL had contracted with Westat, Inc. to develop the CLMS for use in analyzing CETA. In these non-experimental

studies, program participants were combined with a sample of low income individuals selected from the March survey of the Current Population Surveys (CPS). The latter was chosen so that "similar" comparison groups could be derived for use in the net impact evaluation studies. Early analysis of CLMS by Westat using cohorts enrolled in the mid-1970s resulted in a range of estimated program impacts too wide to be considered reliable, however.

From 1980 - 1982, the USDOL decided to fund outside studies of CETA using alternative modeling specifications and different choices of comparison groups for adult and youth groups separately. The intent of these and related studies, was to determine if non-experimental research could be elevated to yield stable and reliable estimates of program impacts for use by USDOL.

The results of six program impact studies for economically disadvantaged adults are highlighted in Table 2.1.[4] These studies evaluated the earnings experience of CETA participants who enrolled between 1975 and 1976 and are based upon Social Security Administration (SSA) earnings.[5] They sampled the national population of CETA participants and chose various comparison groups from the national CPS data.[6]

TABLE 2.1

PROGRAM IMPACTS FOR SELECTED STUDIES OF CETA ADULTS

STUDY CHARACTERISTIC	BLOOM AND MC LAUGHLIN	BASSI*		DICKINSON, WEST, AND JOHNSON		GERACI
	(1982)	(1984)	(1983)	(1984)	(1986)	(1984)
STUDY DESIGN:						
TRAINING DATES	1975-1976	1975-1976	1975-1976	1975	1976	1975-1976
POST-ENROLLMENT DATES	1976-1977	1978	1979	1977	1978	1979
EARNINGS DATA SOURCE	SSA	SSA	SSA	CETA SURVEY	SSA	SSA
SAMPLE SIZE:						
Treatment Group		8217	8217			3896
Comparison Group		4139	4139	5890	11582	3384
PROGRAM IMPACTS:						
TOTAL PROGRAM:						
Women	800 TO 1300	1621/988	987/1096	905	-26*	NA
Men	200	509/132	414/343*	-817	-1123	NA
CLASSROOM TRAINING:						
Women	800 TO 1400	NA	910/1030	1320	NA	1201
Men	300*	NA	346*/292*	-470*	NA	372*
ON-THE-JOB TRAINING:						
Women	700 TO 1100	NA	214/546*	1107	NA	882
Men	300*	NA	762/887	-282*	NA	-588

* Impacts shown for white/minority ethnic groups .espectively.

Since the studies evaluated programs over 15 years ago, including the highly inflationary period of the late 1970s, all findings should be adjusted for inflation before placing boundaries on the range of estimated program impacts. These adjusted figures are shown below, stated in 1987 dollars.[7]

CETA PROGRAM IMPACT RANGES STATED IN 1987 DOLLARS*

	WOMEN	MEN
TOTAL PROGRAM	+$1,000 TO +$2,200	-$2,000 TO +$900
CLASSROOM TRAINING	+$1,400 TO +$2,600	Not Significant
ON-THE-JOB TRAINING	+$300 TO +$2,000	-$1,000 TO +$1,200

* Figures derived from program impacts stated in current year prices of Table 2.1.

As noted, a wide range of estimated program and activity impacts was found. Two major findings are important, however. First, adult women appear to have benefitted more than men on average, as well as in the more intensive classroom and on-the-job training activities. For example, the estimated program impact for women varied between +$1,000 and +$2,200 per year; that for men varied between -$2,000 and +$900 per year.[8] Secondly, adult women experienced greater earnings impacts through classroom training compared with on-the-job training programs; the opposite was found for adult men. In short, it became clear from these early evaluations that sole reliance on enrollee-comparison group research methodologies could produce a wide range of program impact estimates.

Accordingly, after JTPA was enacted in 1982, a panel of experts was convened by USDOL to review plans for examining JTPA program impacts and to recommend ways to deal with the unresolved methodological issue of selection bias. The panel made their official recommendations to USDOL, and in 1985 under strong pressure from some of the most respected labor economic researchers, the Department abandoned the JTLS/SHOW surveys and re-allocated their funds to classical field experiments.[9] Based upon the panel's recommendations, it issued Request For Proposals (RFPs) for a research design, accommodating both non-experimental and experimental approaches, to derive estimates of JTPA program impacts. In a departure from past methodology, the National JTPA Experiment (NJE) would center on the random assignment of JTPA Title II-A adults and out-of school youth applicants to either a treatment or a control group.

As part of the NJE, efforts were also made to construct an improved comparison group from an eligible non-participant sample so estimates of the selection bias could be derived. The logic for developing both experimental and non-experimental evaluation designs was based on both research methodology and cost considerations. It was believed that the best estimates of program impacts could be found by undertaking an expensive classical experiment involving randomly assigned treatment and control groups. The results from this study could then be used as a bench mark against which to compare various non-experimental impact estimates derived from the same treatment group and a specially designed eligible non-participant (ENP) comparison group. As such, the experimental control group could be used as a standard for choosing the best ENP comparison group from the non-participant sample. Knowledge of how to choose an appropriate comparison group, along with an understanding of the sensitivity of various comparison groups to selection bias, could guide future non-experimental studies.

They would indicate the degree and direction of bias remaining, if any, in the non-experimental modeling designs.

From a practical point of view, USDOL understood that the vast majority of future JTPA program impact analyses, if ever undertaken by states, would of necessity follow the less costly non-experimental designs. The program impact estimates of experimental design would act as beacon in the dark at which all other non-experimental studies could aim.

Over the years, many program evaluation researchers have remained rather skeptical of non-experimental studies, because of their likelihood of generating net impact estimates that reflected severe selection bias. Prior to this study of Utah's economically disadvantaged adult program, eleven states had analyzed Title II-A adult programs, but only two states used non-experimental net impact techniques.[10] Both of these studies chose random samples of state Employment Service applicants for comparison groups and utilized relatively simplistic analytical models to address the selection bias issue.

Indiana evaluated 922 trainees enrolled during Program Year 1983 against 1,146 individuals from the Employment Service (ES) comparison group stratified by race-gender target groups. For the second year following entry, annual earnings impacts of $1,000 for white women and $1,200 for AFDC recipients were found; program impacts for males were reported to be $1,400.

Similar findings were reported by Nevada for their Title II-A programs during Program Year 1985. That state's study evaluated the earnings performance of 817 trainees against 5,300 individuals randomly chosen from the ES applicant file.[11] During calendar year 1987, overall program impacts from $1,400 to $1,700 were reported for adult men in three alternative analytical models, and from $630 to $930 for adult women.

Contrary to prior CETA studies, adult men experienced greater program impacts than women in both state-based studies. Since these findings have not been subjected to more rigorous selection bias tests as used in this study, the net impact estimates should be interpreted with caution.

Experimental Evaluation Studies

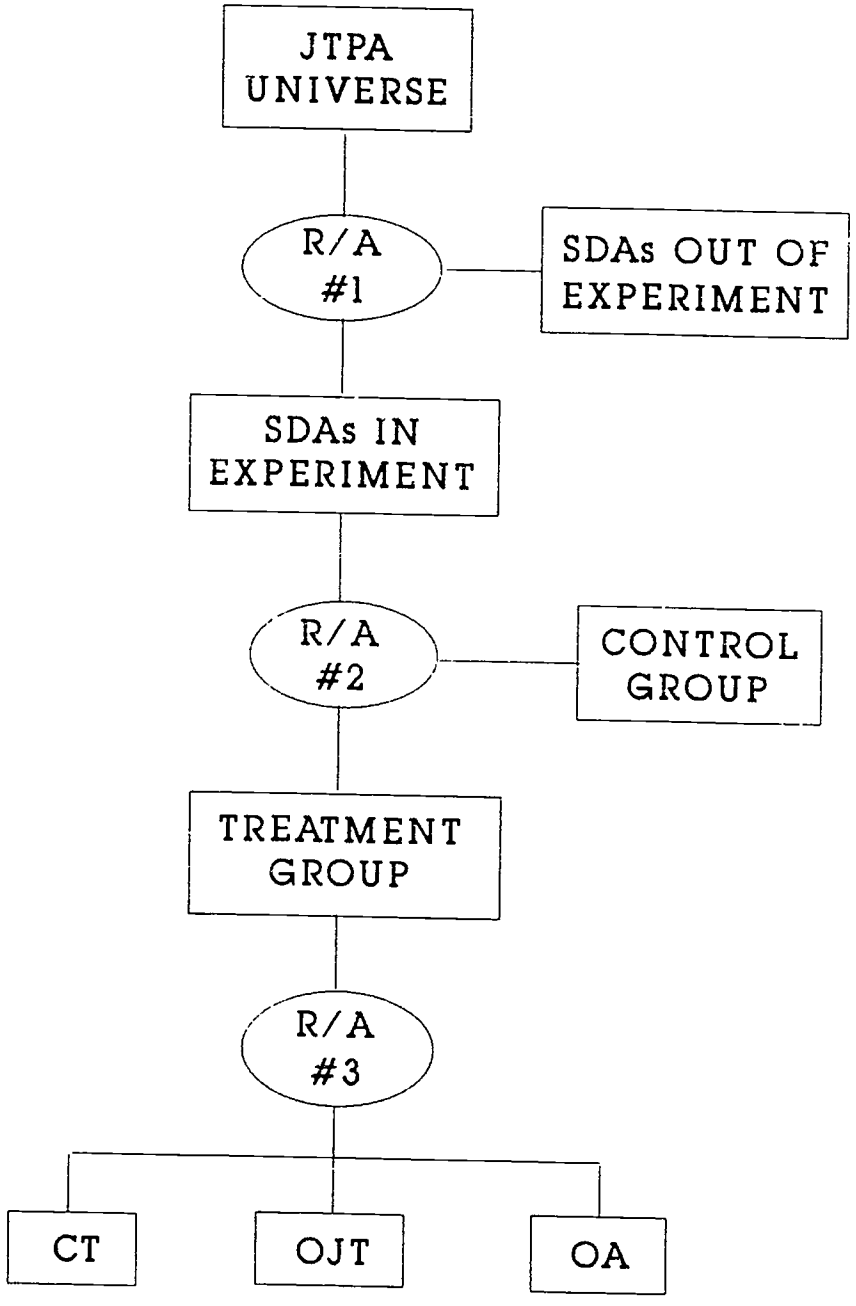
While historically the majority of evaluations of employment and training programs have used non-experimental methodologies, a number of social experiments have also been undertaken to evaluate a host of federally funded education, welfare, and training programs. One study estimates that nearly $1 billion has been spent on over 100 such experiments in the past 25 years.[12]

For the purposes of this study, however, only the National JTPA Experiment (NJE) -- which evaluated the post-training employment and earnings of 16 selected SDAs during the late 1980s -- will be described. This multi-million dollar experiment was patterned after earlier classical random assignment models of social program evaluation.[13]

A hypothetical, and ideal, design structure of such an experiment, with random assignment at multiple stages, is given below in Figure 2.1. In this model, there are three stages of random assignment; in combination, they would yield unbiased JTPA program impacts in total and also by major activities for the national JTPA system.[14]

FIGURE 2.1

HYPOTHETICAL MULTI-STAGED JTPA RANDOM ASSIGNMENT EXPERIMENT



R/A = Random Assignment

The first random assignment occurs across the universe of SDAs; it is designed to ensure the results are not biased by non-random selection of SDAs that participate in the experiment. The second random assignment occurs across eligible participants; it is designed to ensure that the results are not biased by the choices of the eligible participants to enroll or not to enroll in the program (or to be selected, or not selected, by the JTPA staff). The final random assignment is across enrollees and into major activities; it is designed to ensure the results are not biased by the choice of enrollees to select (or be placed) into one of the three major categories of JTPA activities. In this figure, there are three major activities: occupational classroom training (OCT), on-the-job training (OJT), or other activities (OA).[15]

Non-random selection -- or "choice" -- is not permitted in this hypothetical construct so that "true" program impacts, i.e. free of selection bias, can be estimated. According to the figure, the total JTPA program impact is measured as the difference between the mean post-program outcomes of those assigned to the treatment (T) group less the mean outcomes of those in the control (C) group (T minus C). Similarly, JTPA program activity impacts are measured as the difference between the mean outcomes of those assigned to each activity group less the mean outcomes of control group members (OCT minus C; OJT minus C; and OA minus C).[16]

In reality, implementation of the NJE did not follow such a laboratory-like design. (A detailed description of the NJE and its underlying design problems are given in Appendix A.) Breakdowns occurred at all three stages of randomization. First, so few of the SDAs identified on a random basis agreed to participate in the experiment that the first stage random assignment was eliminated during implementation. Instead, USDOL agreed to offer increasingly higher "inducement fees" for participation and still was only able to contract with 16 out of the originally planned 20 SDA sites.[17] From the beginning, it became evident that the experimental results should not -- and could not -- be interpreted as reflecting "the" JTPA system.

Second, all eligible participants were given some "minimal" amount of JTPA assistance -- in the form of testing, assessment, and varying degrees of job search and referral assistance. In essence, the control group did receive at least some level of JTPA treatment. Also, some members of the control group received training and other assistance from non-JTPA sources. Thus, the program impacts were downward biased. That is, the impact of those included in the treatment group was compared to a control group that <u>was not</u> denied services provided by JTPA and other sources. The extent of this bias varied by individuals and types of services made available through Title II-A and other sources.

Third, a small -- but significant -- proportion of the treatment group participants enrolled in activities different from those to which they were initially assigned. Either the individuals self-selected out of their initial assignment, or they were selected out of the initially assigned activity due to changes in program funding/availability or for other reasons. In either case, non-random selection partially characterized activity enrollment, which resulted in biased estimates of activity enrollment impacts.[18]

Reconciliation Of Findings Based On Alternative Methodologies

The justification for using the non-experimental evaluation approach is based upon a reconciliation of findings using experimental and non-experimental methodologies. In the mid-1980s, evaluation experts tried to duplicate estimates of experimental net impacts using non-experimental statistical techniques. They were seeking to determine if results would be seriously biased when non-experimental models were used to simulate experimental net

impacts. If such a result were found, this would demonstrate that program net impacts should only be derived through randomized social experimentation.

Selection Bias of Non-Experimental Studies. To test the usefulness of non-experimental models, two independent researchers tried to simulate the findings of an earlier demonstration experiment which served adult women who were long-term AFDC recipients. The experiment, the National Supported Work (NSW) Demonstration, had been run in the mid- to late 1970s and had previously been evaluated under a USDOL research contract. (More detailed discussions of these studies are given in Appendix B.)

These two follow-on studies found that non-experimental models, when applied to the experimental NSW design, produced a wide range of quasi-net impact estimates. The results supported the view that non-experimental results are seriously biased.[19]

In the first study, LaLonde derived two sets of comparison groups from the Panel Study on Income Dynamics (PSID) and the Current Population Survey (CPS), which were used as surrogates for the experimental control group. Social Security Administration (SSA) earnings data were appended to both treatment and comparison groups for measures of post-program outcomes. Results using the PSID comparison group yielded quasi-net earnings impacts from two to three times as high as the experimental control group; those using the CPS varied from smaller estimated quasi-net earnings impacts to impacts over twice as high as the experimental results.

In the second study, Fraker and Maynard derived six comparison groups from another CPS file and also appended SSA earnings data to the treatment and comparison groups. Once again, the non-experimental approach yielded a wide range of quasi-net impact estimates, from "not significant" to estimates twice as high as that from the experimental net impacts.

Bias-Free Non-Experimental Models. These findings temporarily sealed the fate of non-experimental evaluation methodologies. However, there were two important weaknesses in the studies that affected their results.

First, the comparison groups selected differed greatly from the treatment group in the NSW demonstrations. In the LaLonde study, only 30% of the PSID comparison group were AFDC recipients; as were 2% of the CPS sample. The pre-intervention earnings of these comparison groups were sometimes 10 times as high as the treatment group.

More importantly, the non-experimental models were later shown to be misspecified, which produced a selection bias. In a key study in evaluation literature, Heckman and Hotz later developed a series of tests on pre-training and post-training earnings to identify selection bias.[20] By comparing the earnings processes of the non-experimental comparison groups with the experimental control group in the NSW demonstration, the authors were able to identify which non-experimental models were subject to selection bias. With the misspecified models removed from analysis, the authors then estimated the quasi-net impacts using only those comparison groups that passed the tests. The use of more complex statistical models, which could capture the impact of unobserved factors on program selection, along with improved comparison groups resulted in quasi-net impact estimates similar to those of experimental NSW study.

For the first time, use of non-experimental evaluation techniques had simulated results of a randomized social experiment. The authors were careful to point out, however, that before non-experimental quasi-net impact estimates can be free of selection bias, much care must be taken in deriving similar comparison groups and in utilizing appropriate selection-bias tests.[21]

Ideally, states would need to archive three or more years of individuals' Unemployment Insurance (UI) earnings data -- both prior to and following intervention -- in order to use the more stringent selection bias tests for non-experimental evaluation models. Nevertheless, a non-experimental approach to program evaluation can produce results that are technically sound, and thus useful to policy makers. It is this approach that is taken in the remainder of this report.

Endnotes

1. U.S. Department of Labor, Division of Performance Management and Evaluation, Office of Strategic Planning and Policy Development, "Summary of JTQS Data for JTPA Title IIA and III Enrollments and Terminations During PY 1987 (July 1987-June 1988), Washington, D.C., December 1988, p. 16.

2. These figures are derived from Bloom, et al. (1993).

3. Perhaps the most widely known experiments of this type were those that estimated the expected outcomes of income maintenance programs for the poor. These experiments were set up in four areas throughout the country to determine the labor force participation responses of men and women who were given varying amounts of income maintenance. These "negative income tax" evaluations were analyzed in: Kershaw and Fair (1976); Hannon, Tuma and Groevenveld (1977); and later in Cain and Wissoker (1990).

4. Two studies were completed by Westat, Inc., but did not estimate program impacts separately for adults, as is the focus of this study; see: Westat (1981) and Westat (1984). As such, the results of the studies are not included in the table. A survey of the major CETA evaluation statistical methodologies and their findings are summarized in Barnow (1987).

5. At this time, longitudinal earnings files for individuals had to be constructed from individual Social Security Administration (SSA) records, which resulted in lengthy delays. For example, the USDOL-sponsored evaluations of CETA, completed in 1984, evaluated CETA programs which operated between 1975 and 1976.

6. A major problem faced by these early evaluations was choosing comparison groups which were similar to the CETA participants in both observed and "non-observed" factors. In addition, the selection of the comparison groups based upon pre-enrollment earnings, when successful, resulted in sampling contamination as CETA participants were included in the CPS-based comparison groups. One study [Dickinson, West, and Johnson (1984)] estimated that up to one-fourth of the comparison group was composed of CETA treatment groups.

7. The year 1987 is chosen because this is the program year analyzed in the Utah Title II-A adult program in this study.

8. The negative findings for adult men may be attributed to the program diverting participants from productive job search or employer discrimination against hiring and/or paying CETA participants. In spite of lower post-training earnings observed for the average participant, individual participants may still enroll in such programs due to their uncertainty of finding jobs outside of CETA as opposed to their certainty of receiving training and/or stipends in CETA.

9. See: Stromdorfer, et al. (1985).

10. See Reed (1986) and Hanna and Turney (1988). A Title III Dislocated Worker program net impact evaluation by Bowman (1988) was also completed for Utah. A national survey of state evaluation projects for Title II-A is found in Figueroa and Misch (1990).

11. See: Hanna and Turney (1988).

12. Greenberg and Shroder (1991).

13. The use of random assignment models to evaluate social programs was an outgrowth of early agricultural experiments proposed by Fisher (1935). Needless to say, the extension of such experiments to evaluate social programs, characterized by human behavior, has resulted in strong proponents as well as opponents to such applications. The debate in this heated controversy is covered, among other places, in: Campbell (1970), Rivlin (1971), Cook and Campbell (1979), Lucas (1981), Burtless and Haveman (1984), Hausman and Wise (1985), Burtless and Orr (1986), Greenberg and Robins (1986), Sawhill (1988), Heckman (1992), and Levitan (1992).

14. A fourth stage of random assignment could be added to this hypothetical model; it would be at the final stage, for JTPA placement services. It is excluded here for ease of exposition.

15. These categories are the same as those specified in the NJE, which allowed for multiple activity sequences, where individuals were classified according to their major activity if more than one was received.

16. It may be noted that the NJE design altered this hypothetical design by instituting randomization between treatment and control groups after individuals were assessed, tested, and assigned to an activity. More importantly, the assignment to an activity was not done on a random basis, since this procedure would have assigned individuals to an activity which might not have been appropriate or optimal. As such, the NJE design precluded unbiased estimates of inter-activity impact comparisons (i.e., OCT-OJT; OCT-OA; or OJT-OA), since the assignment across activity groups was based upon a non-random process.

17. Initially SDAs were offered $40,000 to participate in the experiment. This figure was raised to an average of $170,000, with one site being paid $300,000 to participate. These figures are taken from Hotz (1992), p.95.

18. As a matter of feasibility, the NJE was constrained to only a single level of random assignment -- which was placed after systematic assignment to one of the three major activity sequences. Had the non-random selection across activities not occurred, the experiment could have produced unbiased estimates of activity enrollment impacts. Since this was not possible, unbiased estimates (ignoring the previous two sources of bias) of only the assignment to an activity -- as distinct from actual enrollment in an activity -- was possible in the experiment.

19. See LaLonde (1986) and Fraker and Maynard (1987).

20. See Heckman and Hotz (1989).

21. A recent study by Friedlander and Robins (1992) used only the most simplistic selection bias tests involving comparison groups that were computed from various experimental control groups of mostly single female AFDC recipients. The binary outcomes of being employed and receiving welfare during the post-training period were analyzed. These tests resulted in uncertain outcomes, which were limited by the nature of the simplified selection processes tested and the discrete nature of the program outcome measures.

CHAPTER THREE: Utah JTPA Title II-A Adult Participant Characteristics

This chapter begins the non-experimental approach to evaluation by describing the demographic characteristics and prior work experiences of the economically disadvantaged adults enrolled in Utah's JTPA Title II-A program during Program Year (PY) 1987 (1,864 individuals). The study includes all applicants aged 22 to 54 who had never previously enrolled in one of three major activities during that program year (classroom training in occupational skills; on-the-job training; and other activities, composed mainly of job search assistance).[1] This number includes participants who completed and were placed into unsubsidized jobs in addition to those who completed but were not placed into jobs, and those who dropped out.

The description of the participants and their prior work experiences is important because the observed participant characteristics will later be used to identify similar comparison groups from the files of Utah Employment Security (ES) participants. These characteristics are used to account for observed differences between JTPA participants and non-participants in the net impact statistical models of the study.

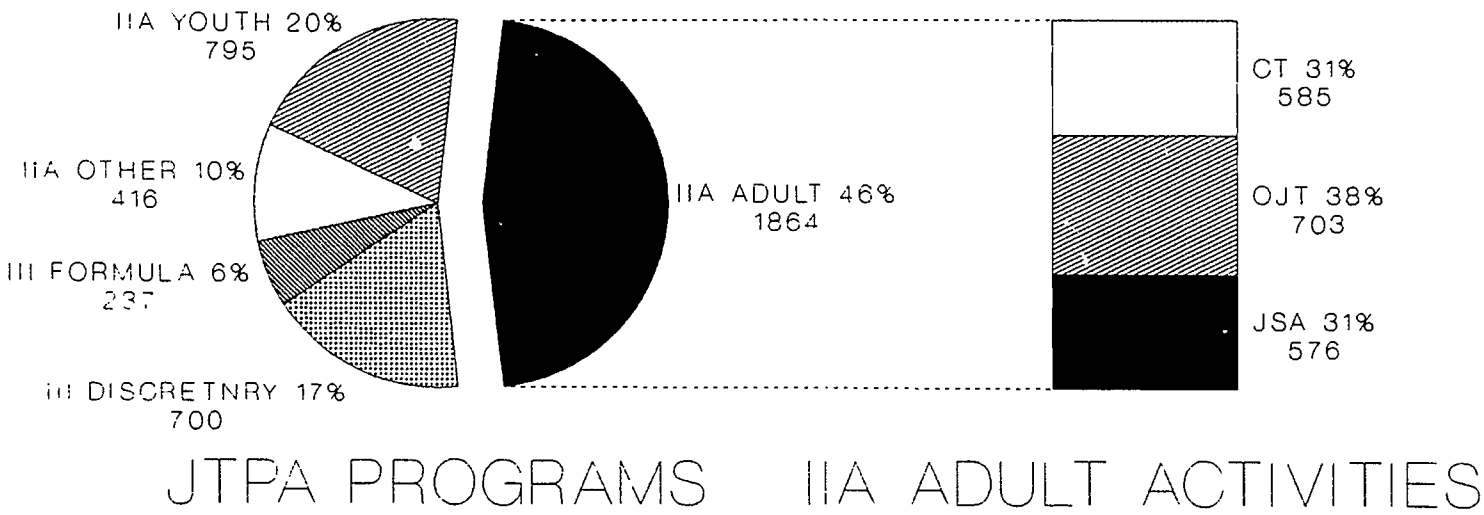
In the first section, a general description of Utah's JTPA programs is given to emphasize the important role the Title II-A program for economically disadvantaged adults plays in the state. In the next section, the PY 1987 Title II-A adult participants are described. In the last section, the pre-entry work history of Utah's Title II-A adult participants is described in more detail because this work history is fundamental to the identification of experimental comparison groups used to derive program impact estimates in this study.

Utah's JTPA Programs During PY 1987

The PY 1987 adult participants selected for analysis were a group whose employment and earnings history can be tracked with Utah's Unemployment Insurance (UI) wage records over a 4-year pre- to post-entry period. That is, historical UI wage files in Utah, available when this study began, include all quarters continuously from the third quarter of 1984 through the second quarter of 1990. The files allow for 8 full quarters of pre- and post-entry work experience to be identified for all participants prior to entry into JTPA.[2]

During this period, over 5,000 individuals entered all Utah JTPA programs for the first time.[3] Nearly one-half (46%) were eligible for the Title II-A adult program (which is composed of individuals aged 22 and older who were certified as economically disadvantaged). Title II-A adults meeting the federal/state guidelines are assessed and placed into one of three employment and training programs -- occupational classroom training (OCT), on-the-job training in the private sector (OJT), and other activities (OA), which as noted earlier, in Utah consists predominantly of job search assistance (JSA) for those with readily marketable skills. As shown in Figure 3.1, Title II-A adult programs constitute by far the largest federal-state employment and training activity; the second largest is for younger disadvantaged individuals (Title II-A programs for persons between 16 and 21 years of age).

Figure 3.1
Utah JTPA in PY1987



Total First Time Enrollees = 4012

Differences between the Title II-A adult program and the other JTPA programs in Utah are shown in Figure 3.2. The roughly proportional assignment of adult Title II-A participants across the three program activities stands in contrast to other JTPA programs, whose participants received a disproportionate share of one type of employment and training assistance or another. For example, Title II-A youth and Title III (dislocated worker) formula programs, emphasized classroom training. "Other Title II" programs (such as those funded under the state set-asides: 3% Older Workers and 8% Vocational Education programs) emphasized on-the-job training, while Title III discretionary programs, targeting specific manufacturing plants and industries, emphasized job search assistance.

In addition, Title II-A adult programs served a disproportionate share of individuals residing in rural Service Delivery Areas (SDAs) compared with other JTPA programs. As seen in Figures 3.3 and 3.4, Utah is divided into 9 SDAs, with the population served reflecting the population distribution of the state at large. Four SDAs account for nearly 70% of those served -- all clustered together in the urbanized area around the state capital of Salt Lake City. In fact, 2 SDAs -- Salt Lake and Mountainland-- comprise over one-half of all eligible program participants in the state. The remaining 30% are scattered throughout the northern Bear River SDA and the four large, contiguous SDAs of Uintah Basin, Central, Southwest, and Southeast SDAs.

Finally, men comprised over 80% of all participants in Title III programs. In all Title II programs, women were more equally represented, accounting for 50% of all participants.

Utah JTPA II-A Adult Participants

As shown in Table 3.1, adult men and women in the Title II-A programs differed little by race/ethnicity. Most were white (just over 80%); about 2% were black. Hispanics and "others" (mainly American Indians) were the largest minority groups (about 7% and 10% respectively).

Differences between men and women appeared in their other characteristics. The Title II-A men were slightly younger and less educated than the women. More of the men were between the ages of 22 and 29 (47% compared to 41% among women) and more of the men were school drop-outs (22% compared to 17% among women).

The men showed a greater attachment to the workforce than the women. Seventy-four percent had been employed in the previous year, compared with two-thirds of the women. Among those who had been employed in this pre-entry period, the men had earned over $5,700, while the women had earned considerably less ($3,251). Also, 1 in 4 of the men, but only 1 in 10 of the women, were UI claimants at the time of entry into JTPA. Finally, a greater percentage of women than of men were welfare recipients (38% versus 9%).

The enrollment distribution across the three major activities differed between men and women as well. A smaller proportion of men were enrolled in classroom training (23% versus 40% for women), while more men were enrolled in on-the-job training (40% versus 35% for women) and also in "other activities" composed mainly of job search assistance (37% versus 25%).

Figure 3.2
JTPA Enrollee Characteristics By Program

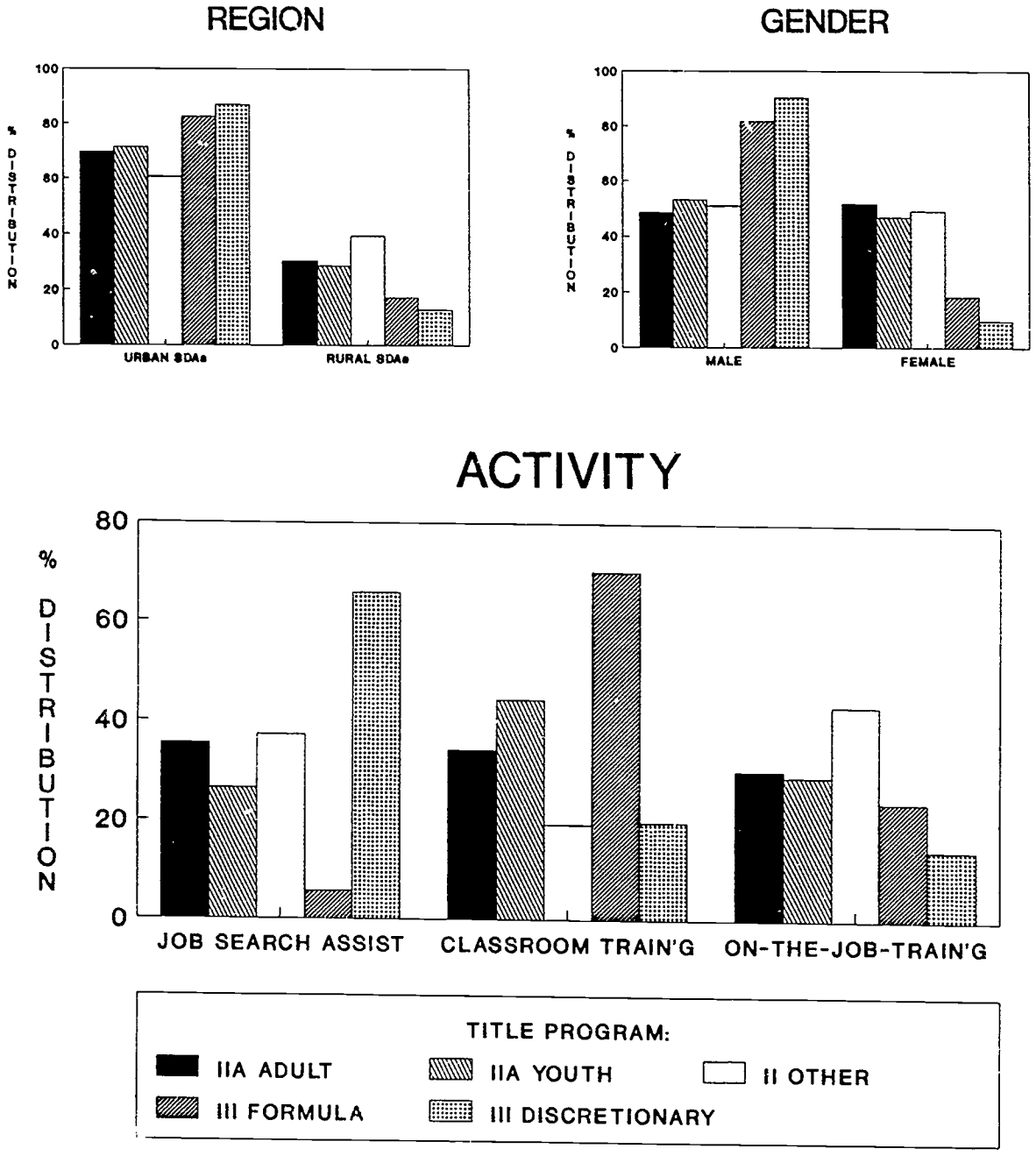


Figure 3.3
Utah Service Delivery Areas

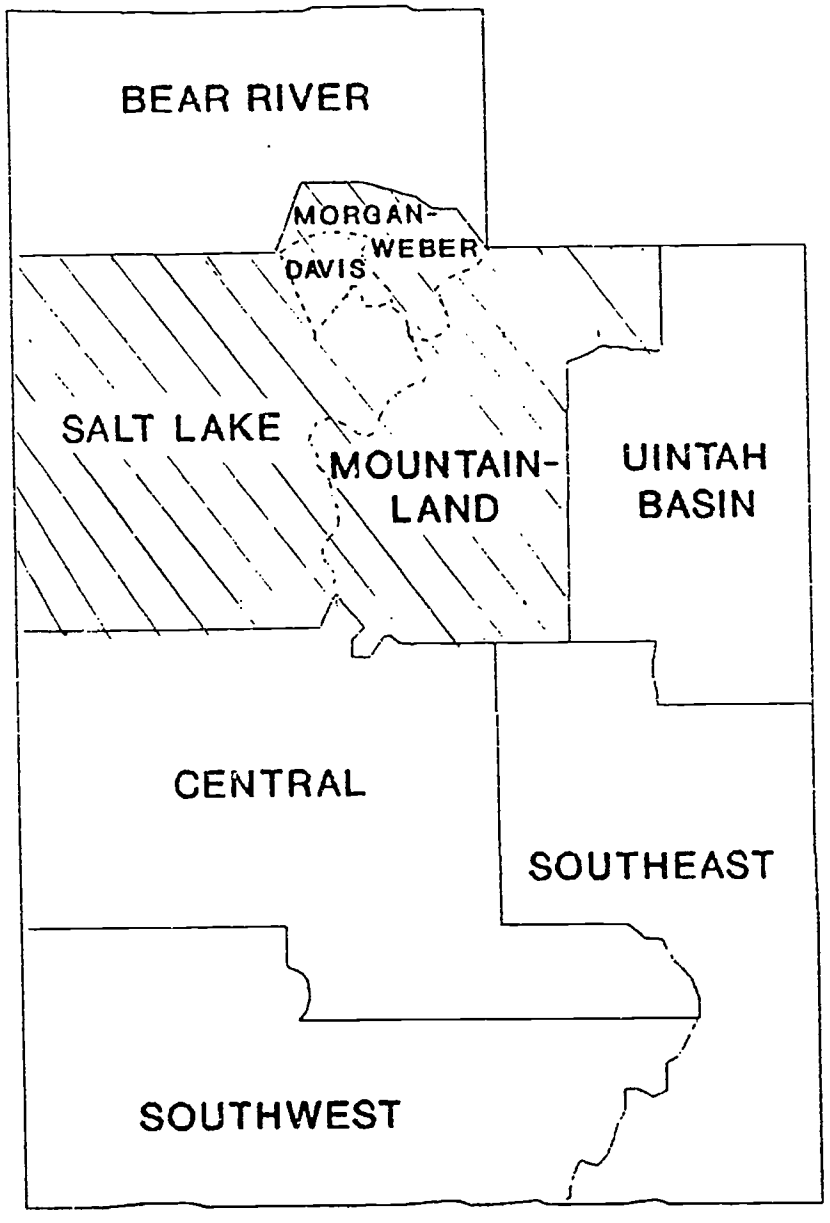
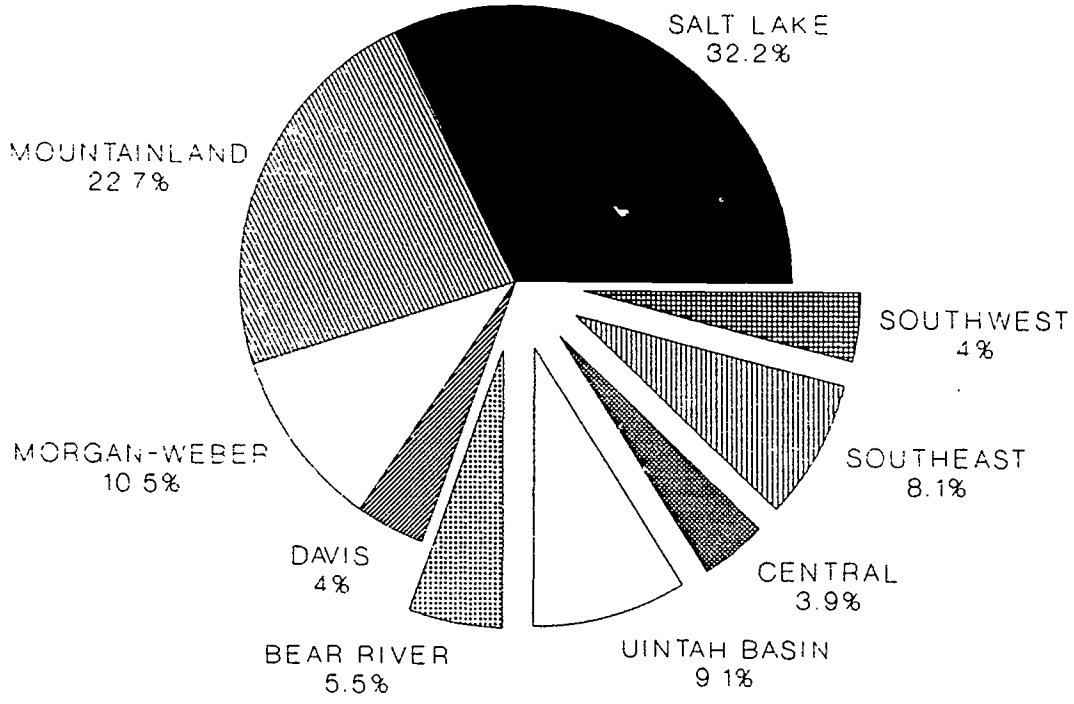


Figure 3.4
Title IIA Adult Population by SDA
(PY1987)

URBAN SDAs



RURAL SDAs

Table 3.1

PARTICIPANT CHARACTERISTICS OF UTAH'S
TITLE II-A ADULTS DURING PY 1987
(Percent Distribution)

CHARACTERISTIC	UTAH	
	WOMEN	MEN
[NUMBER]	[936]	[928]
ETHNICITY:		
WHITE (Non-Hispanic)	82.6	80.3
BLACK (Non-Hispanic)	1.6	2.3
HISPANIC	5.9	7.9
OTHER	9.9	9.6
SCHOOLING :		
HIGH SCHOOL DROP-OUT	16.5	22.0
HIGH SCHOOL GRAD/GED	51.0	44.9
POST-HIGH SCHOOL	32.6	33.1
AGE:		
22 - 29	41.3	46.7
30 - 44	48.2	45.6
45 - 54	10.5	7.8
WELFARE RECIPIENT	35.7	8.9
UI CLAIMANT	9.1	24.5
PRIOR YEAR:		
PERCENT EMPLOYED	66.8	74.1
EARNINGS IF EMPLOYED	3251	5751
NUMBER OF FIRMS	1.77	1.97
ACTIVITY (PREDOMINANT):		
CLASSROOM TRAINING	39.6	23.1
ON-THE-JOB TRAINING	35.3	40.2
OTHER ACTIVITIES	25.1	36.7

PRIOR EMPLOYMENT AND EARNINGS EXPERIENCES OF TITLE II-A ADULTS

The theory of "human capital" provides the conceptual basis for the earnings models developed in the study: the idea is that an individual's current employment status and earnings are reflections first and foremost of prior investments of human capital -- including schooling, formal occupational training, and work experience.[4] In addition, the theory is broadened to allow for unobserved factors -- including motivations and attitudes related to work, language barriers in the work place among others -- to affect individuals' employment and earnings.

The identification of "similar" comparison groups, which are used as proxies for the expected labor market outcomes of program participants had they not enrolled in JTPA, is critically dependent upon the specification of pre-entry employment and earnings. Knowledge of the actual work history of enrollees is key.

As seen in Figures 3.5 and 3.6 below, these experiences differed significantly between women and men enrolled in Utah's PY 1987 Title II-A adult programs. As shown in Figure 3.5, both the levels and trends of employment rates leading up to enrollment differ significantly between the two gender groups. The quarterly employment rates of adult men averaged nearly 10 percentage points higher than adult women, with the differential eroding closer to the enrollment date. The proportion of adult men who were employed declined over time, while adult women were more likely to be employed closer to the enrollment date. In addition, those enrolled in the more intensive training programs (i.e., classroom training and OJT) had higher employment rates than those assigned to other activities.

As seen in Figure 3.6, of those employed, the quarterly earnings differences between the two gender groups differed as well. Adult men's quarterly earnings were significantly higher than the women's in the earlier pre-entry period and were subject to a greater temporary decline the closer to the JTPA enrollment period. Specifically, employed adult men earned over $3,000 per quarter 8 quarters prior to entry and experienced over a 50% decline in earnings over the two year pre-entry period.[5] The decline in earnings for adult women over this period was less dramatic, falling roughly one-third from an initial level of $1,500 per quarter.

The patterns of pre-entry earnings over this period for those enrolled in OJT also differed between women and men. The women enrolled in OJT experienced the greatest decline in quarterly earnings, whereas adult men enrolled in OJT experienced the smallest decline. This difference is important since significantly different program impacts are found for this program activity between the two gender groups.

This description of personal demographics, schooling, and prior work experiences of Utah's Title II-A adult participants is fundamental to the derivation of "similar" comparison groups and the estimation of JTPA program impacts in the chapters that follow. In the next chapter these participant characteristics will be used to identify alternative comparison groups, which will be used to derive estimates for JTPA program impacts for Utah's adult Title II-A programs.

Figure 3.5
Pre-Entry Percent Employed

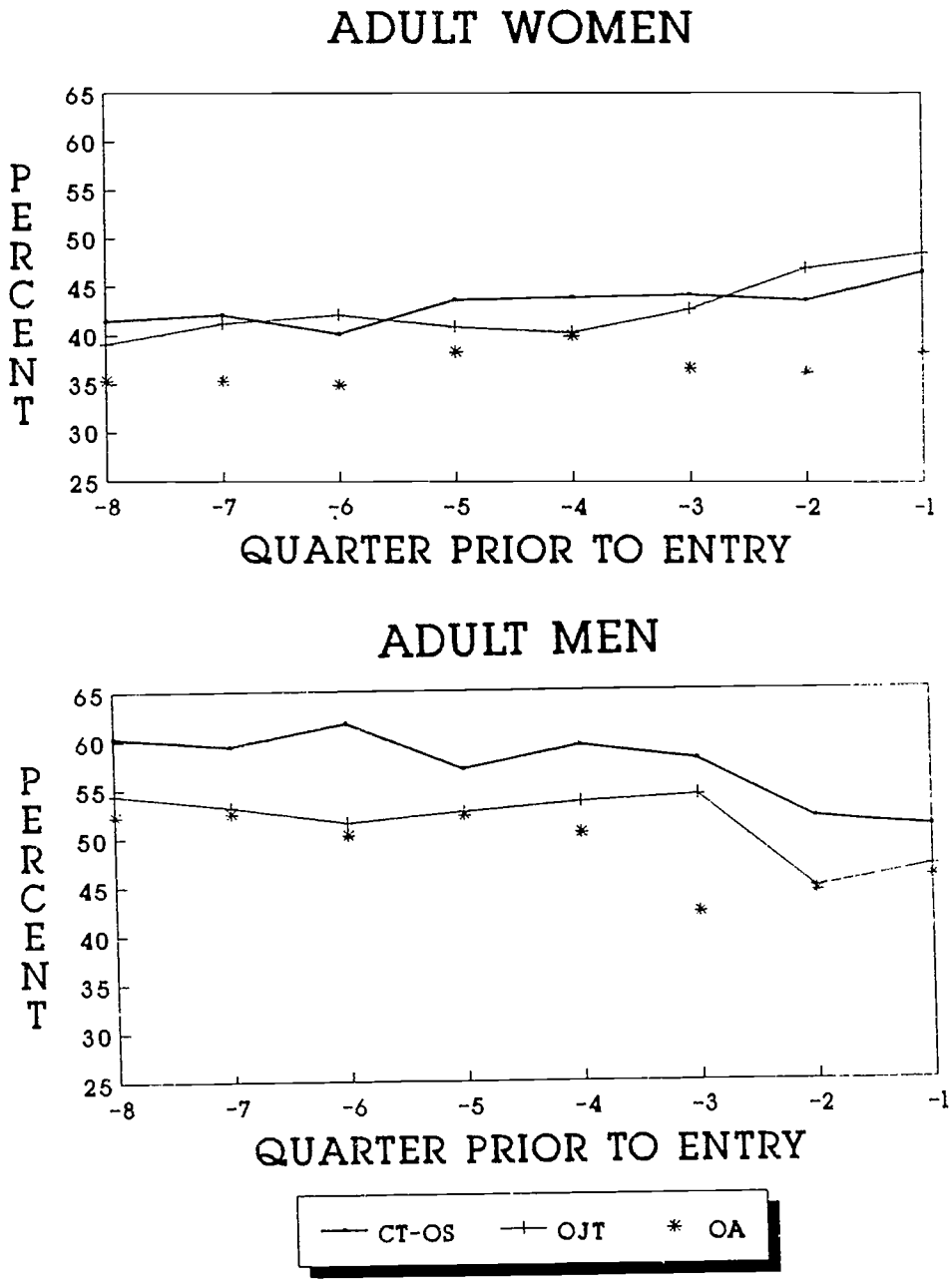
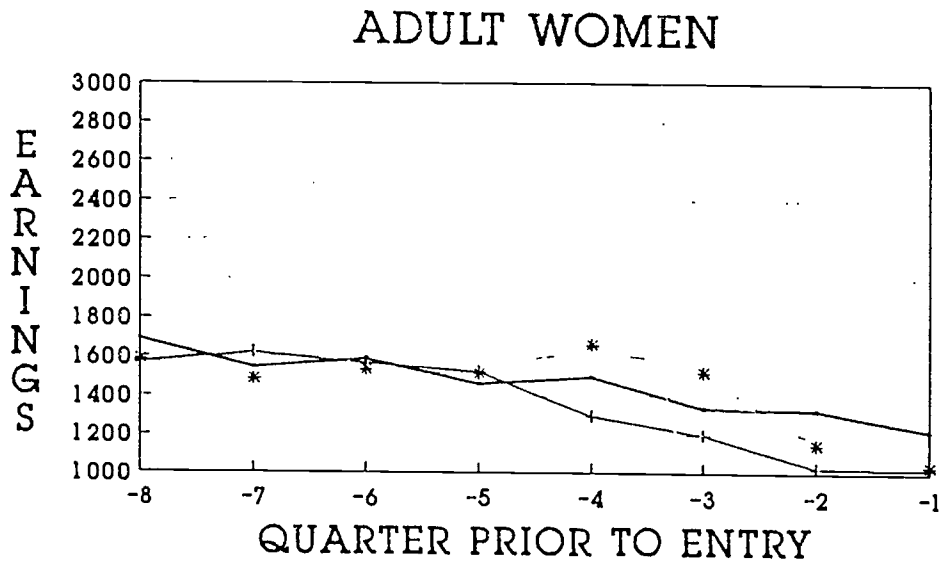
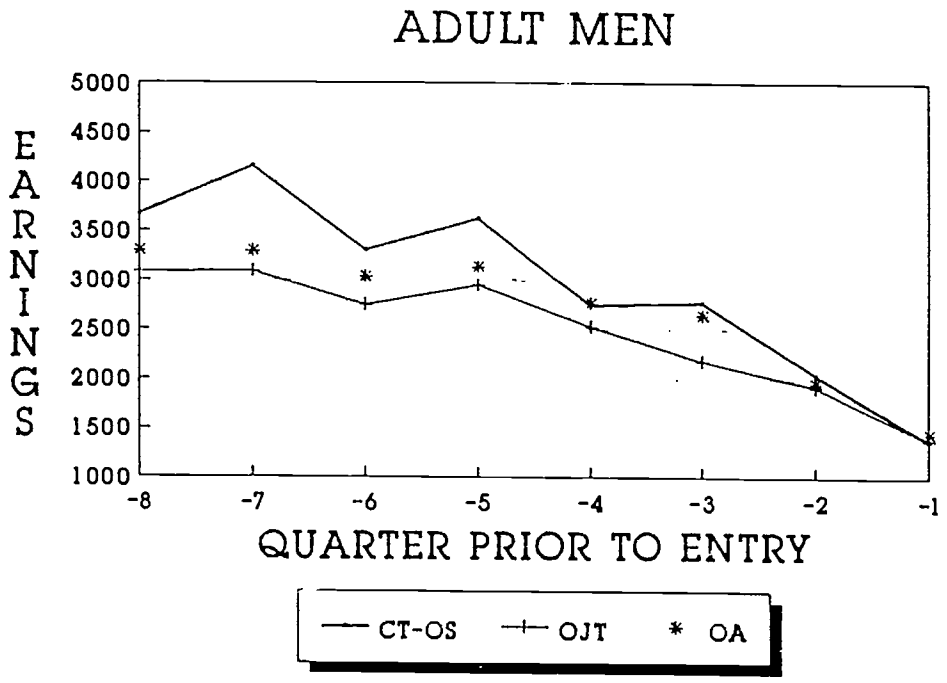


Figure 3.6
Pre-Entry Earnings If Employed





Endnotes

1. Over 5% of all entrants during PY 1987 had initially enrolled in the previous program year and were either still enrolled in their initial activity or had been re-enrolled in a separate activity during PY 1987.

2. It may be noted that the departure point used in the study is the entry date as opposed to the exit date commonly used in JTPA reports. The difference reflects the nature of net impact analysis used in the study. In particular, the treatment group enrolling in JTPA is compared to a "similar" comparison group of individuals who choose not to enroll but are available for full-time employment at the time of interruption in (or entry to) their employment history. As such, an important "cost" to program participants is the foregone earnings they could have realized had they not enrolled. Thus the appropriate employment interruption period begins with program entry not exit.

3. The study omits any individual who was previously enrolled in a Title II-A adult program in an earlier program year to ensure "pre-entry" does not include time a participant spent in the program rather than in the work force. In addition, the study excludes program year "repeaters" (composing less than 3% of all participants) to ensure pre- or post-training quarters are not contaminated with program participation. Utah state JTPA data records alone do not identify such individuals, since program operators utilize the same resources (except perhaps intake resources) regardless of whether one participant undertakes two separate services or two participants undertake one service each.

4. This theory, first popularized by Becker (1964) and later tested empirically by Mincer (1974), has since become standard in most applied labor economics literature.

5. This "dip" in pre-entry earnings was first reported by Ashenfelter (1978) and Kiefer (1979) for economically disadvantaged adults under Manpower Development Training Administration (MDTA) in the 1960s and later documented for similar individuals served under CETA. See, for example: Bassi (1984), Ashenfelter and Card (1985), and LaLonde (1986). It may be noted that this pattern of pre-entry earnings was reported by the National Commission for Employment Policy (1992) for over 60,000 economically disadvantaged adults pooled across 11 states.

CHAPTER FOUR: JTPA II-A Adult Comparison Group Selection

As discussed earlier, the methodology used to evaluate the effectiveness of employment and training programs is a comparison of the actual post-training outcomes of participants against "counterfactual" outcomes -- those outcomes that hypothetically would have occurred had enrollees not participated in JTPA. With the non-experimental approach taken in this study, the counterfactual outcomes are approximated by a select comparison group which should be similar to the JTPA treatment group -- for both observed and unobserved factors -- except for program participation. The objective of this chapter is to identify such a comparison group.

The identification of a comparison group that is truly representative of JTPA enrollees is difficult for two reasons. First, one must find a large enough sample of individuals who are eligible for enrollment and are distributed similarly to JTPA enrollees with respect to observed demographics and employment histories. Second, unobserved factors which affect the decision to enroll in JTPA and are related to subsequent earnings and employment should be distributed similarly between the enrollees and the comparison group. In practice, one attempts to select individuals who are similar to JTPA enrollees with respect to major observed characteristics; unobserved differences must be controlled by statistical modeling.[1]

The discussion explaining how a comparison group is selected in this non-experimental study is divided into three major parts. First, the procedure used to identify alternative comparison groups from a random sample of Employment Security (ES) registrants is given. The initial data base is described; then the methodology for choosing comparison groups is explained, followed by a description of three alternative comparison groups selected for consideration in the non-experimental impact models developed later.

The second part of the chapter describes in non-mathematical terms how one may choose the "most similar" comparison group for use in program impact analysis. This choice is guided by specification tests which are applied to each group to indicate the presence and extent of selection bias. First, a series of tests are proposed to examine if selection bias can be removed by modeling the JTPA selection process with observed factors; next tests are proposed to determine if the bias can be removed by modeling the JTPA selection process with unobserved factors using more complex statistical techniques.

The last part of the chapter subjects the alterative comparison groups of Utah ES clients to the selection bias tests and identifies the "most similar" comparison group. This group is used in the following chapter for estimating unbiased program employment and earnings impacts of JTPA training.

PART I: Identifying Comparison Groups For Non-Experimental Evaluations Of JTPA

DATA BASE FOR IDENTIFYING COMPARISON GROUP

The comparison groups used in this study are drawn from a sample of ES registrants who received basic employment assistance during PY1987. Individuals processed through Utah's Employment Security agencies are eligible for job search assistance -- including assessment, counseling, resume writing preparation, and job placement with listed vacancies. These are common forms of basic employment assistance JTPA enrollees could expect to receive if they had not instead enrolled in more intensive government-subsidized training programs.[2] The sample of ES registrants chosen is a 10% randomly chosen sub-set, which was designed to select approximately 10,000 individuals from whom a comparison group could be identified. This procedure was used as a second best approach to selecting only those individuals who were coded "economically disadvantaged."[3]

From the original total of 9,836 Employment Security applicants in the 10% sample, a sub-set of 5,851 observations was selected who met the age eligibility criterion (22 to 54 years of age at entry to the Title II-A adult program) and whose education, gender, and race were known.[4] These records were then matched with another ES record containing the application date and finally merged with Utah UI wage records. This process produced 5,413 complete records containing the intake information along with 16 quarters of earnings records -- 8 pre-ES and 8 post-ES application.

As can be seen in Table 4.1, the resulting ES sample differs significantly with respect to observed personal characteristics and employment histories from that of the 1,864 JTPA II-A adults who enrolled for the first time during PY 1987. The randomly chosen ES registrants are composed of greater proportions of whites, males, and more highly educated individuals. They are less likely to be in the prime of their work history (aged 31 through 40), and more likely to be older (aged 41 through 54).

Two of the most salient differences between the two groups are residential location and welfare status. ES clients are far less likely to be on welfare (1.4% versus 22.3% of JTPA participants), and are far more likely to be living in urbanized Wasatch Front South and in Weber-Morgan and Davis SDAs. This differential mainly reflects the state JTPA allocation formula, which places great weights on unemployment rates and low wages -- both of which favor the less densely populated rural areas of Utah.

In addition, ES clients register evenly throughout the program year, whereas JTPA enrollment is partially influenced by the timing of program year funding. For example, a greater portion is enrolled in JTPA in the first quarter of the program year when funding is plentiful and fewer are enrolled during the last quarter when the year's budget is low.

The distribution of ES clients by the length of continuous spells of unemployment also differs from that of JTPA enrollees. While JTPA enrollees are more likely to experience intermediate spells of unemployment prior to entry, ES clients are more likely to experience either no unemployment just prior to entry or they tend to be completely out of the labor force during the 2-year period prior to intervention. While the random sample of ES clients' industry distribution (2 years prior to entry) is similar to that of their JTPA counterparts, they are either more likely to be fully attached to the labor force (work all 4 quarters) or out of the labor force entirely (zero quarters of covered employment).[5] These differences exist for both of the pre-enrollment years and are greatest in the year just prior to entry. JTPA II-A adults, on the

TABLE 4.1

BACKGROUND CHARACTERISTICS OF JTPA II-A ADULT PROGRAM AND ES REGISTRANT ADULT SAMPLE (Percent Distribution)

CHARACTERISTIC	JTPA II-A ADULT	EMPLOYMENT SECURITY
[NUMBER]	[1,864]	[5,413]
GENDER: FEMALE	49.9	43.3
MALE	50.1	56.7
ETHNICITY: WHITE (NON-HISPANIC)	81.3	89.1
OTHER MINORITIES	19.4	10.9
SCHOOLING: H.S.DROP-OUT	19.6	13.4
H.S.GRAD/GED	48.4	42.8
POST-H.S.	32.0	43.8
AGE: 22-25	24.2	26.0
26-30	24.5	25.4
31-35	21.5	17.6
36-40	14.2	12.5
41+	15.6	18.6
RESIDENTIAL LOCATION: URBAN	56.5	72.3
RURAL	43.5	27.7
PCT. WELFARE RECIPIENTS	22.3	1.4
ENTRY QUARTER: 87:3	36.6	24.7
87:4	22.6	22.9
88:1	27.7	26.9
88:2	13.1	25.5
QUARTERS OF CONTINUOUS SPELLS OF UNEMPLOYMENT: NONE	45.6	56.6
ONE-TWO	17.7	8.9
THREE-SEVEN	14.3	8.1
EIGHT OR MORE	22.4	26.3
INDUSTRY AFFILIATION IN 2ND YEAR PRIOR TO INTERVENTION:		
CONSTRUCTION	10.2	11.0
DURABLE MANUFACTURING	10.6	11.4
NON-DURABLE MANUFACTURING	7.1	6.7
WHOLESALE/RETAIL TRADE	29.0	28.8
BUSINESS SERVICES	28.4	25.9
OTHER INDUSTRIES	14.7	16.2
WORK FORCE ATTACHMENT 2ND YEAR PRIOR TO INTERVENTION:		
OUT OF LABOR FORCE	36.1	45.5
PARTLY ATTACHED (1-3 QRS)	34.3	21.4
FULLY ATTACHED (4 QRS)	29.6	33.1
WORK FORCE ATTACHMENT 1ST YEAR PRIOR TO INTERVENTION:		
OUT OF LABOR FORCE	29.6	42.9
PARTLY ATTACHED (1-3 QRS)	48.5	26.7
FULLY ATTACHED (4 QRS)	21.9	30.4

other hand, are more likely to be partly attached to the labor force (working from 1 to 3 quarters), especially in the year just prior to enrollment.

These observed differences between the JTPA and ES groups, along with possible unobserved differences, result in considerably different pre-entry employment and earnings patterns. As seen in Figure 4.1, differences between the two groups with respect to their prior employment rates (percent of group employed at any time during a quarter) and employed earnings (earnings of those employed) are quite evident. The randomly chosen ES client sample has higher rates of employment in all quarters prior to intervention, and those employed have higher earnings. These latter characteristics become especially evident in the 4 quarters just prior to intervention due to the decline in employment of JTPA II-A adults in the year prior to entry. While the employment rates differ little between the 2 years for JTPA enrollees, the variability of their employment increases dramatically in the year prior to entry. In comparison, ES clients experience a gradual rising rate of employment over the 2-year pre-entry period.

An even more striking difference is observed with respect to the quarterly earnings pattern of those employed in a given pre-entry quarter. The quarterly earnings of employed JTPA trainees falls nearly 50% -- from $2,500 to $1,250 -- while that of ES clients rises slightly from an initial level of $3,500. This observed dip in quarterly earnings of JTPA enrollees, as compared to ES clients, could result from sudden unfavorable working conditions, personal health problems, or the like.[6]

Methodologies Used For Choosing Comparison Groups

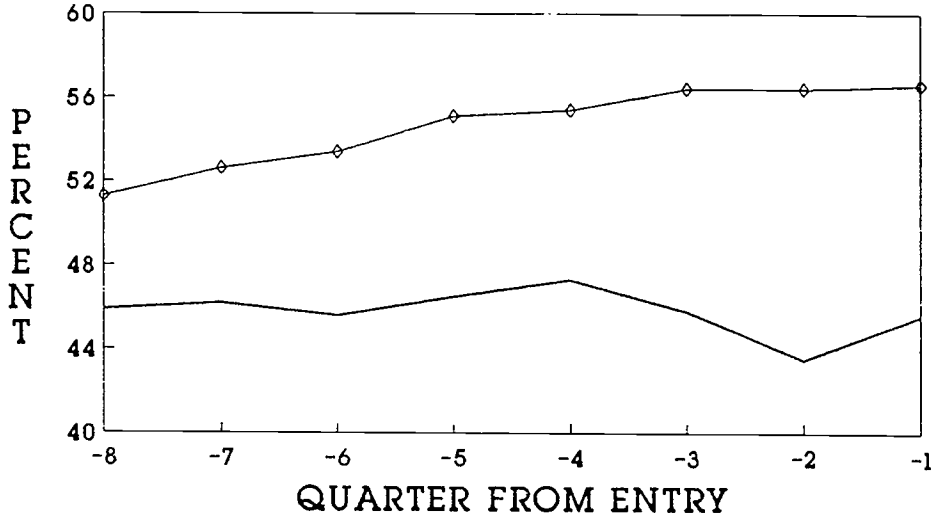
The identification of a "similar" comparison group is, perhaps, the most important and difficult task when applying non-experimental techniques to estimate quasi-net impacts of JTPA training. In the past, most researchers relied on national surveys from which they attempted to identify a sub-set of individuals who were similar to Comprehensive Employment and Training Administration (CETA) and JTPA participants being analyzed. As noted in Appendix B, for example, the Current Population Survey (CPS) March edition was used to derive a comparison group for the National Supported Work (NSW) Demonstration. The wage records of these individuals were then matched against Social Security Administration (SSA) files and, due to confidentiality requirements imposed by SSA, were aggregated into defined cells according to specific observed personal demographic characteristics.[7] As described in Appendix B, this early comparison group approach had two flaws: it was not possible to identify and eliminate those individuals who enrolled in CETA, and the pre-enrollment employment and earnings patterns differed dramatically between the treatment group in NSW and the comparison groups selected from the CPS. In essence, the two groups were so different that neither matching procedures nor statistical techniques to control for observed differences were effective in deriving a "similar" comparison group. In addition, using the CPS-SSA data files necessitated analyzing grouped data, rather than individual data, due to the confidentiality restriction imposed by SSA, as noted above.

Other approaches to identify comparison groups in experimental studies using individual data have used control groups from different demonstration sites, or control groups from an earlier time period in a given demonstration site for separate non-experimental analysis.[8] Because these approaches are based upon control groups from random assignment experiments, they can not be used for non-experimental studies which, by their nature, do not have randomly assigned control groups.

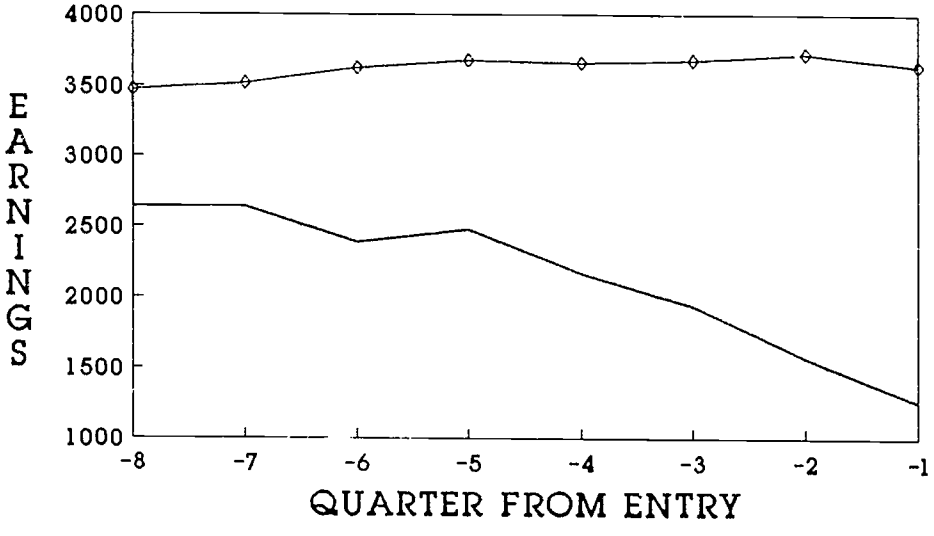
FIGURE 4.1

PRE-ENTRY WORK HISTORY: JTPA & EMPLOYMENT SECURITY SAMPLE

EMPLOYMENT RATES



QUARTERLY EARNINGS



JTPA — EMPLOYMENT SECURITY

For the vast majority of JTPA programs operated by the approximately 650 SDAs across the country, researchers wishing to evaluate JTPA as it works on a day-to-day basis must find proxies for the "counterfactuals" of JTPA participants. That is, they must find individuals who are similar to JTPA participants with respect to all observed and unobserved factors except one -- participation in JTPA. The comparison group chosen should approximate the post-training experience of JTPA participants had they not enrolled in JTPA (the hypothetical counterfactual that by definition does not exist).

Clearly, the 10% random sample of ES registrants differs significantly with respect to observed demographic factors and pre-entry employment and earnings patterns from that of JTPA II-A adult trainees. As such, use of the 10% sample as a comparison group would yield biased estimators in the quasi-net impact models of JTPA training.[9]

Two approaches, not mutually exclusive, have been proposed to address the selection problem in non-experimental studies. First, cell matching techniques can be used to screen out possible comparison group members who do not share a combination of observed factors similar to the treatment JTPA group. One technique, the "nearest-neighbor" approach, was used to evaluate selected CETA programs.[10] Another approach is to use regression analysis, combined with various assumptions about the nature of the JTPA selection process, in quasi-experimental studies to "control for" both observed and unobserved differences between comparison groups and JTPA treatment groups. These two approaches can be used together, with the comparison group first refined using cell matching techniques and then regression analysis used to control for the observed and unobserved differences between the comparison group members who passed through the cell matching screens and the treatment group. This latter approach is taken in this study.

ALTERNATIVE COMPARISON GROUPS FOR UTAH STUDY.

The first step in developing similar comparison groups is to apply cell matching techniques which will result in a sub-set of the 10% sample that more closely resembles the JTPA treatment group in its observed demographic characteristics and pre-enrollment employment and earnings histories. As shown in Table 4.2 below, three such comparison groups are considered.

In each case, the 10% ES sample was first stratified according to four gender-race groups: white males, non-white males, white females, and non-white females.[11] Next, alternative cell matching strategies were applied as a series of screens to each stratified gender-race group to derive the three comparison groups. In the first group, the sub-set is reduced in half by an initial cell matching of the age-schooling distribution and the distribution of the change in earnings (Group A).

The next two are reduced in half by matching either the change in employment distribution (Group B) or the change in earnings distribution (Group C) over the 2-year pre-registration period for the ES 10% sample. In both of these groups, cell matching according to observed personal characteristics is purposely avoided. A third and final cell matching technique is applied to the last two comparison groups with regard to the distribution of pre-entry annual earnings.

For all three comparison groups, the final matching reduces the sample by an additional 15%, resulting in comparison groups nearly 70% smaller than the original sample and roughly equal in number to the JTPA II-A adult participant group.

TABLE 4.2

REDUCTION IN ORIGINAL SAMPLE SIZE DUE TO SELECTIVE COMPARISON GROUP SCREENS

SCREEN	COMPARISON GROUP CELL SIZE		
	GROUP A	GROUP B	GROUP C
ORIGINAL SAMPLE	5413	5413	5413
SCREEN # ONE: Age-schooling Groups	3708 (-31.5%)		
SCREEN # TWO: Change In Employment		2467 (-54.4%)	
Change In Earnings	2557 (-21.3%)		2527 (-53.3%)
SCREEN # THREE: Earnings (2nd Year)	1716 (-15.5%)		1635 (-16.5%)
Earnings (1st Year)		1659 (-15.0%)	
(TOTAL PERCENT REDUCTION)	-68.3%	-69.0%	-69.8%

The first cell matching described above results in a comparison group (Group A) selected to replicate the age and schooling distribution of JTPA enrollees as closely as possible. To the extent possible, additional cell matching is applied to this sub-set in order to replicate the level of earnings in the second year prior to entry, as well as the fall in pre-entry earnings in the year prior to entry.[13] This comparison group may be viewed as one that assumes the observed forms of human capital are the important characteristics of a comparison group and should therefore be matched as closely as possible to the levels found in the JTPA enrollee group. Two major forms of human capital investments -- schooling and general work experience, as approximated by age -- are used in this matching stage. The second year employment and earnings focus of this sub-set is based upon the assumption that the best measure of a worker's stock of human capital and earnings potential is that existing before the year just prior to enrollment. Measures of work experience in the year just prior to JTPA enrollment may understate the trainee's true stock of human capital due to the pre-enrollment dip in earnings that characterize employment and training participants; a similar understatement would result if the focus on quarterly employment and earnings of ES registrants were on the year just prior to entry.

Two additional comparison groups are chosen which ignore the distributions of age and schooling as measures of human capital, and emphasize instead the unobserved factors that give rise to the dip in pre-entry employment (Group B) and earnings (Group C) not explained by human capital factors. The cell matching strategy in the second comparison group (Group B) first replicates the JTPA distribution of employment in the second year prior to entry according to a worker being out of labor force, partly attached, or fully attached to the labor force. Next, the distribution of the change in employment status from the second to the first year prior to entry of JTPA trainees is replicated for each of the three employment categories. To the extent possible, final cell matching is applied to the distribution of quarterly earnings in the year prior to entry so as to approximate the observed dip in earnings of JTPA enrollees.[14]

A final round of matching is applied to the third comparison group (Group C), which emphasizes the change in quarterly earnings over the 2-year pre-entry period. Like the second comparison group above, the cell matching technique is applied to the JTPA participant distribution of employment in the second year prior to entry -- whether a worker is out of labor force, partly attached, or fully attached to the labor force.[15] Next, cell matching is used to duplicate the JTPA distribution of the change in quarterly earnings from the second to first year prior to entry. A final cell matching is applied to the distribution of quarterly earnings in the year prior to entry to approximate the observed dip in earnings of JTPA enrollees.[16]

In all cases, the number of observations in each comparison group is constrained by attempting to maintain a one-to-one ratio between the comparison and treatment groups for the four gender-race categories. As shown in Table 4.3, the varying cell matching techniques applied to the 10% ES sample for each comparison group result in differing distributions of demographic characteristics, pre-entry employment and earnings patterns, and industry affiliation relative to the JTPA treatment group.

Group A has similar distributions of demographic characteristics, whereas the other two groups have a greater proportion of males and are more highly educated and younger (i.e., more are 25 years of age or younger) compared with JTPA participants. In addition, it appears the cell matching techniques used to derive the three comparison groups result in small but important differences with regard to prior work histories. In the year prior to intervention, the ES comparison groups chosen on the basis of earnings changes (Groups A and C) were 50% more likely to be employed all 4 quarters (35% and 32% versus 20%) than the comparison group (Group B) chosen to simulate the employment status change of the JTPA group.

The pre-intervention pattern of quarterly employment and earnings (if employed) of the comparison groups relative to JTPA enrollees are shown in Figure 4.2. While the two comparison groups matched on pre-entry quarterly earnings (Groups A and C) display rising employment rates prior to ES intervention, the second comparison group -- which simulated the change in employment of JTPA enrollees (Group B) -- displays an employment rate pattern similar to the JTPA group. All three comparison groups display the fall in quarterly earnings of those employed. The two groups which simulated the earnings change patterns (Groups A and C) differ from the second comparison group (Group B) by having relatively high earnings in the second year prior to entry. In addition, the first comparison group (Group A), which replicates the observed demographic and schooling distributions of JTPA enrollees, has a flatter earnings decline from its relatively high original level of quarterly earnings.

In summary, the search for a comparison group similar to JTPA II-A adult program enrollees during PY 1987 using the 10% sample of ES participants with cell matching techniques results in differing types of comparison groups. On the one hand, comparison Group A seems to be more similar to JTPA enrollees with respect to observed personal demographic factors, but relatively different from JTPA enrollees with respect to pre-entry quarterly earnings and employment. On the other hand, comparison Groups B and C show a greater dip in pre-entry earnings and employment patterns (especially Group B) -- which may be related to unobserved factors causing a temporarily worsening economic condition -- but have a different distribution of observed demographic factors than Group A.

It appears that cell matching that more closely replicates observed human capital variables is not related to unobserved factors which could produce the dip in pre-entry employment and earnings of JTPA enrollees. Similarly, cell matching that more closely replicates the dip in pre-entry earnings is less similar to JTPA enrollees with respect to observed human capital variables. There do not appear to be cell matching techniques, using these data, which incorporate both observed and unobserved factors in a way to generate human capital distributions and pre-entry employment and wage patterns similar to that of JTPA enrollees.

TABLE 4.3

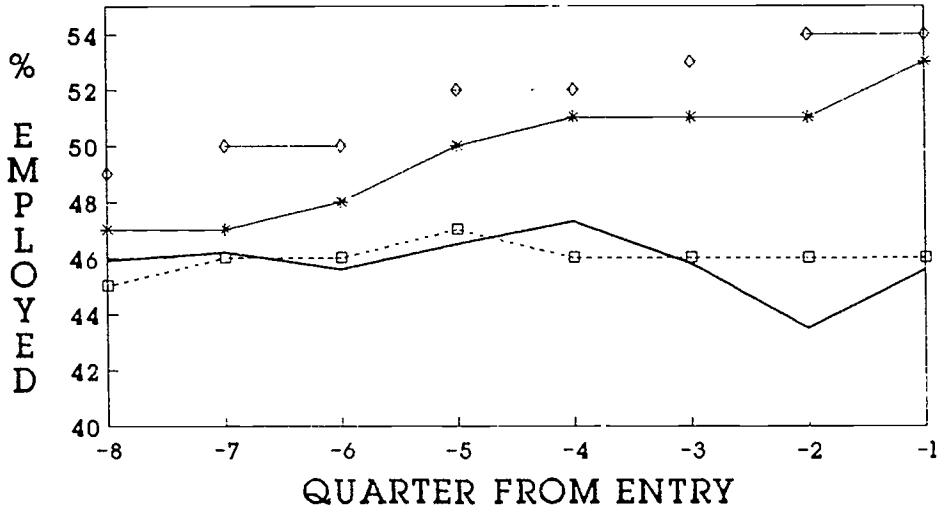
BACKGROUND CHARACTERISTICS OF JTPA II-A ADULT PROGRAM AND ES REGISTRANT ADULT SAMPLE (Percent Distribution)

CHARACTERISTIC	JTPA	GROUP A	GROUP B	GROUP C
[NUMBER]	[1,864]	[1,610]	[1,573]	[1,530]
GENDER: FEMALE	49.9	48.8	47.9	47.1
MALE	50.1	51.2	47.9	47.1
ETHNICITY: WHITE (NON-HISPANIC)	81.3	81.1	81.1	81.8
OTHER MINORITIES	19.4	18.9	18.9	18.2
SCHOOLING: H.S.DROP-OUT	19.6	18.4	16.3	15.4
H.S.GRAD/GED	48.4	50.1	41.6	41.9
POST-H.S.	32.0	31.5	42.0	42.5
AGE: 22-25	24.2	25.3	29.4	30.5
26-30	24.2	7.72	7.6	25.9
31-35	21.5	18.2	16.8	16.1
36-40	14.2	13.0	10.6	11.8
41+	15.6	15.8	15.6	15.8
RESIDENTIAL LOCATION: URBAN	56.5	39.8	42.3	42.4
RURAL	43.5	60.2	57.7	57.6
PCT. WELFARE RECIPIENTS	22.3	1.0	1.4	1.2
ENTRY QUARTER: 87:3	36.6	25.7	25.3	24.2
87:4	22.6	23.4	22.3	23.2
88:1	27.7	27.1	26.9	27.3
88:2	13.1	23.9	25.5	25.2
QUARTERS OF CONTINUOUS SPELLS OF UNEMPLOYMENT: NONE	45.6	54.2	46.3	52.7
ONE-TWO	17.7	12.5	20.0	14.6
THREE-SEVEN	14.3	14.5	13.9	12.9
EIGHT OR MORE	22.4	18.8	19.8	19.8
INDUSTRY AFFILIATION IN 2ND YEAR PRIOR TO INTERVENTION:				
CONSTRUCTION	10.2	9.7	9.9	10.3
DURABLE MANUFACTURING	10.6	6.3	5.3	5.7
NON-DURABLE MANUFACTURING	7.1	4.8	4.6	4.9
WHOLESALE/RETAIL TRADE	29.0	11.4	10.0	10.8
BUSINESS SERVICES	28.4	35.5	33.2	32.6
OTHER INDUSTRIES	14.7	32.3	37.0	35.7
WORK FORCE ATTACHMENT 2ND YEAR PRIOR TO INTERVENTION:				
OUT OF LABOR FORCE	36.1	32.3	37.0	35.7
PARTLY ATTACHED (1-3 QRS)	34.3	35.8	33.6	33.7
FULLY ATTACHED (4 QRS)	29.6	31.9	29.4	30.6
WORK FORCE ATTACHMENT 1ST YEAR PRIOR TO INTERVENTION:				
OUT OF LABOR FORCE	29.6	30.4	28.2	28.8
PARTLY ATTACHED (1-3 QRS)	48.5	34.2	51.9	39.7
FULLY ATTACHED (4 QRS)	21.9	35.3	19.9	31.6

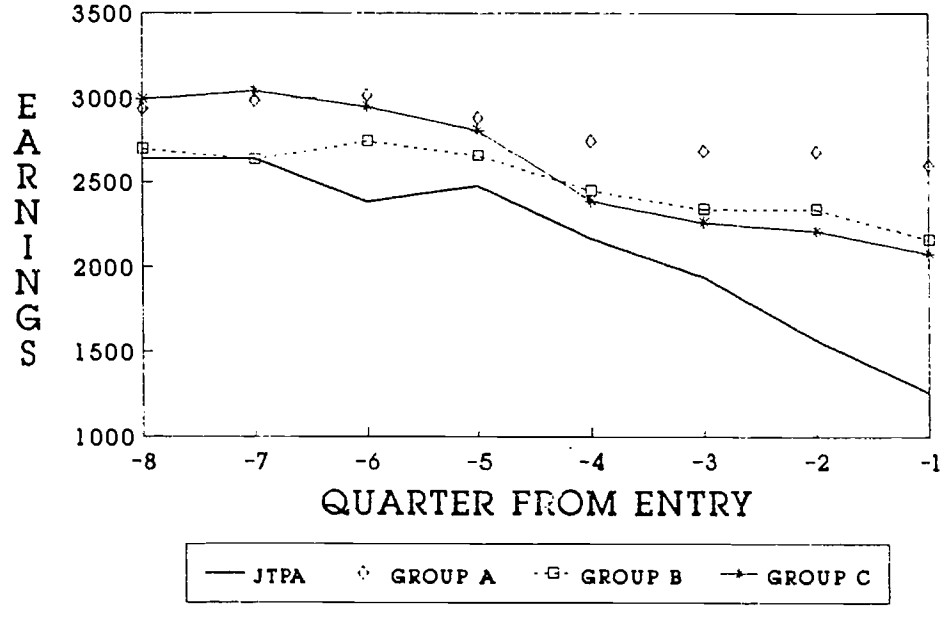
FIGURE 4.2

PRE-ENTRY WORK HISTORY: JTPA AND COMPARISON GROUPS

EMPLOYMENT RATES



QUARTERLY EARNINGS



Given this situation, it is interesting to note the distributions of pre-entry employment and earnings of the comparison group members who classified themselves "economically disadvantaged" on the self-reporting portion of the ES intake form. Figure 4.3 shows the distribution of the sub-sets of economically disadvantaged within each of the three comparison groups (they comprise roughly 13%, or nearly 225 observations, of each group). The economically disadvantaged have significantly lower employment rates and quarterly earnings, especially in the second year prior to entry into the ES system. From these observed patterns of employment and earnings, it appears that those declaring themselves as being "economically disadvantaged" are truly disadvantaged. In the future, it may prove worthwhile to choose a comparison group for JTPA from the population of those who report themselves as being disadvantaged.

PART II: Tests For Selection Bias In Non-Experimental Models

The objective of deriving a comparison group similar to the JTPA trainee group is to reduce the likelihood of obtaining an estimate of JTPA training that is "biased" due to the selection process. A program impact estimate of JTPA participation is said to be biased if omitted factors related to selection into JTPA are related to the post-training employment and earnings outcomes.

For example, consider a situation where individuals who are less motivated to work, who have greater learning disabilities, or who have poorer health are less likely to search for and find employment through the Utah ES. In time, they turn to JTPA for employment assistance in hopes of finding a job. Following JTPA training they find employment in relatively less stable and lower paying jobs that are matched to their skill levels, health, and work habits. If the post-training employment and earnings pattern of these individuals are compared with comparison group members who have stronger motivation to work, fewer learning disabilities, and better health -- the results will be "biased" against the JTPA trainee group.[17] Information regarding the selection process into JTPA is needed to "control for" the impact of these factors on post-training employment. The problem, however, is that both observed and unobserved factors may affect the selection process. If relevant factors are excluded from the earnings models which are systematically related to the selection into JTPA, "selection bias" will result.

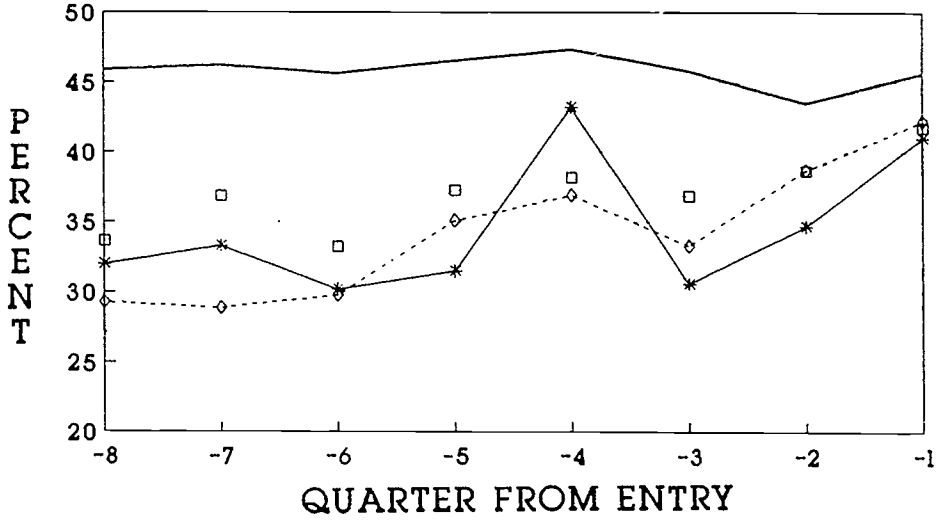
To remove the selection bias it is necessary to derive a "similar" comparison group using as many related factors as possible so as to reduce the number of factors which, if omitted, would bias the program impact estimate. Then particular modeling specifications are tested to control for observed and unobserved factors that may further bias the program impact estimates.

Since an early evaluation of MDTA by Ashenfelter in 1978, it has been evident that non-experimental approaches to estimating training effects need to be subjected to various tests to ensure the validity of the comparison groups as good proxies for the treatment counterfactual, and thus eliminate selection bias. It was not until recently, however, that economists developed more thorough selection bias tests of comparison groups used in the evaluation of training programs.

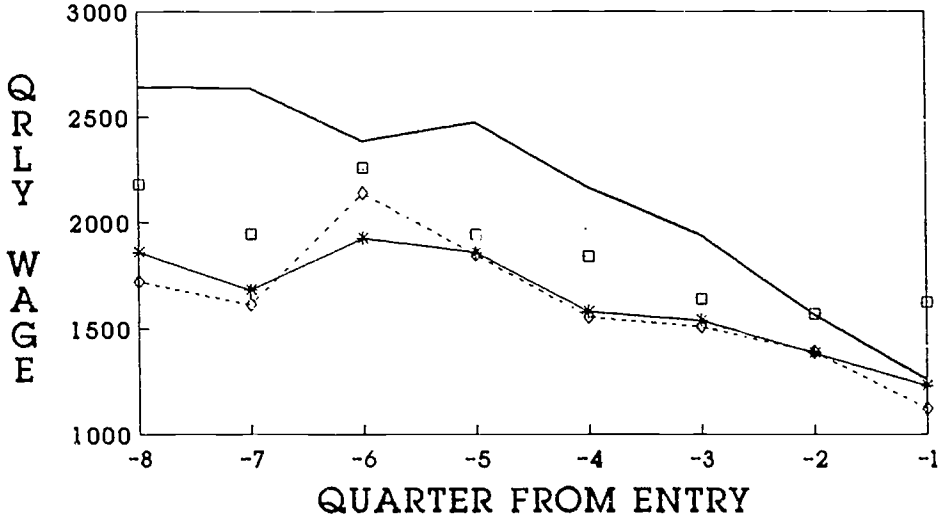
In 1989, Heckman and Hotz developed a series of tests which compared both pre-enrollment and post-enrollment earnings of experimental control groups and selected comparison groups for the National Supported Work (NSW) demonstration. The objective of their study was to determine if the wide range of training estimates produced by non-experimental models -- and attributed to selection bias -- could be reduced by identifying only those comparison groups which could pass more stringent validity tests.[18]

FIGURE 4.3
JTPA II-A Adults & Economically Disadvantaged: Prior Employment and Earnings Pattern

EMPLOYMENT RATES



QUARTERLY EARNINGS



JTPA — GROUP A — GROUP B — GROUP C

The selection bias tests used here rest on the assumption that the earnings generating process existing prior to government intervention is similar to that following intervention, and -- once both observed and unobserved factors of the selection process are controlled -- any remaining difference between the post-training earnings of the treatment and comparison groups must be due to differences in the intervention strategies (i.e., JTPA versus ES).

The selection bias tests for the Utah JTPA study were constructed to test the null hypothesis that selection into JTPA has no statistically significant effect on earnings before JTPA training is received. If the two groups already differ significantly prior to treatment, this suggests that post-training differences are not solely due to the difference in services received. In all tests, a binary training participation variable (1 = yes; 0 = no) was added to pre-entry earnings models along with factors related to the selection process. The expectation was that the training coefficient would not be significantly different from zero. If, for example, the JTPA coefficient in the pre-entry earnings model differs from zero (i.e., the model specification "fails the test"), one can expect the model specification to yield biased estimates of post-training earnings impacts. If, however, the JTPA coefficient in the pre-entry earnings model does not differ from zero significantly (i.e, the model specification "passes the test"), use of the particular earnings specification for the post-training period would yield unbiased estimates of post-training earnings impacts.

Measure Of Pre-Intervention Earnings For Selection Tests

The selection bias tests performed in this study are based on pre-entry earnings measures which, unfortunately, were constrained by available data. Ideally, one would like to use several years of pre-entry earnings so as to fully specify the pre-entry earnings process.[19] In the case of the Utah study, only 2 years of pre-enrollment data were available. The transitory nature of earnings in the year prior to entry into JTPA makes that year's earnings a poor measure of the earnings potential of the treatment group. Furthermore, using the second year of earnings prior to entry in the selection bias tests is problematic as no earnings prior to this period can be specified in the earnings models -- a characteristic critical to the longitudinal nature of earnings models.[20] As such, the measure of pre-entry earnings used in all selection bias tests below is the sum of earnings in seventh through twelfth month (i.e. the third and fourth pre-entry quarter) prior to entry into JTPA or registration for ES services. This is the closest period before the dip in earnings experienced by many JTPA participants prior to enrollment. It is also a 2- quarter period that can be compared with the same 2 calendar quarters in the second year prior to entry (i.e., seventh and eighth pre-entry quarters) available in the data set. In this way, the earnings measured over common calendar quarters will not be affected by the seasonality present in quarterly earnings. In essence, the two 6-month earnings periods are surrogates for two earlier pre-entry annual earnings periods that would have been used had they been available for this study.

Selection Bias Tests Developed For Utah Study

Five selection bias tests are conducted for each of the Utah comparison groups derived above, and are estimated for women and men separately. The first three test-models of the selection process use only observed factors, while the next two test-models incorporate unobserved factors in the JTPA selection process. Each of these tests makes different assumptions about the dependence between the training variable and the error term of pre-enrollment earnings. The

model specifications were chosen to represent various ways to remove the dependency between JTPA selection and the pre-entry earnings, thereby eliminating any suspected selection bias.

Selection Bias Tests On Observed Factors. In the first test, only the cell matching techniques on observed factors were used to define the selection process. Then a univariate regression model which specifies only the JTPA training binary variable was used to test the null hypothesis of no difference ' pre-training earnings. If the test is rejected (i.e., the coefficient is significantly different from zero), one may conclude that use of the cell matching technique alone would likely result in biased estimates of post-training earnings impacts.

The second model uses a simple "linear control function" which adds observed factors relevant to the selection process to the regression model mentioned above in addition to the JTPA training binary variable. If the coefficient on the JTPA variable is still significant, selection bias continues to characterize non-experimental models of post-training earnings even after the available observed factors are included.

The third technique is called the "two stage least squares" (2SLS) method, which was proposed initially by Barnow, Cain and Goldberger (1980) for use in evaluating employment and training programs. In the first stage, the probability of enrollment was modeled and the estimate of the enrollment probability of trainees was then used in the second stage earnings model rather than the actual training binary variable. If all factors affecting both the selection decision and earnings are specified, and if the decision to enroll is a linear function of all explanatory variables, the dependency between the (estimated) training variable and the error term is eliminated. Under these conditions, estimates of post-training earnings impacts estimated with the 2SLS model would yield unbiased estimators.

Selection Bias Tests on Unobserved Factors. A second group of validity tests was developed based on models that incorporate unobserved factors to the JTPA selection process. The first version, as used by Bassi (1983), is the "fixed effects" model which removes the selection bias if unobserved factors which affect the selection process and the level of pre-entry earnings are constant over time. Put another way, by transforming the dependent earnings variable into the change of earnings, selection bias can be eliminated if the impact of unobserved factors on the level of earnings is the same for each year. (If more motivated individuals make $1,000 more each year, then the impact of being more motivated on the change in earnings is zero.) In this form, the dependence between the error term of the newly specified dependent variable and JTPA training variable is removed and fixed effects model estimates of post-training earnings would be unbiased.[21]

The second version removes the selection bias induced by unobserved factors affecting enrollment and post-training earnings, as developed originally by Heckman (1979). This technique models the selection process simultaneously with post-training earnings by using the error structure of the enrollment model as an explanatory variable in the earnings equation. This non-linear simultaneous equation model version will yield unbiased estimates if its assumptions concerning the structure of errors are valid.[22]

PART III: Results Of Selection Bias Tests For Utah Study

The five selection bias tests described above were applied to the 6-month pre-entry earnings of adult women and men who enrolled in JTPA for the first time during PY 1987 along with comparison group members who registered with ES during the same period.[23] As noted in Table 4.4 below, all models which specify the selection process in terms of only observed factors failed

TABLE 4.4

SELECTION BIAS TESTS ON PRE-INTERVENTION EARNINGS USING ALTERNATIVE COMPARISON GROUPS AND PY1987 JTPA II-A ADULTS:

(A) WOMEN

JTPA SELECTION PROCESS	COEFFICIENT AND STANDARD ERROR OF JTPA VARIABLE FOR PRE-ENTRY EARNINGS MODELS: (Indication of Selection Bias)		
	GROUP A	GROUP B	GROUP C
OBSERVED FACTORS:			
Cell Matching Technique	-242*** (53)	-105** (50)	-130** (75)
Linear Control Function Model	-165** (86)	+69** (48)	+82** (37)
UNOBSERVED FACTORS:			
Two-stage-least-squares Model	-212* (125)	-144** (72)	-269* (152)
Fixed Effects Model	-124** (52)	+26 (17)	+67 (40)
Heckman Selection Model	-66* (38)	+43 (45)	+45 (62)

(B) MEN

JTPA SELECTION PROCESS	COEFFICIENT AND STANDARD ERROR OF JTPA VARIABLE FOR PRE-ENTRY EARNINGS MODELS: (Indication of Selection Bias)		
	GROUP A	GROUP B	GROUP C
OBSERVED FACTORS:			
Cell Matching Technique	-642*** (104)	-213** (98)	-278*** (97)
Linear Control Function Model	-261*** (66)	-159*** (70)	-188** (89)
UNOBSERVED FACTORS:			
Two-stage-least-squares Model	-1409** (649)	-361*** (124)	-438** (204)
Fixed Effects Model	-263*** (94)	-184** (86)	-258*** (98)
Heckman Selection Model	-301*** (80)	-140* (77)	-76 (50)

NOTE: The level of confidence for each coefficient is indicated by the asterisks following each coefficient (* = 90%; ** = 95%; *** = 99%).

the selection bias tests for both women and men across all three comparison groups. (That is, the coefficient on the JTPA variable was significantly different from zero.)

The degree of selection bias remaining in models which include only observed factors appears to be greater for men than women. The test results based on cell matching alone indicate that women who enrolled in JTPA earned $242 less over the 6-month pre-intervention period than those in the first ES comparison group (Group A); men who enrolled in JTPA earned $642 less over this period compared with their counterparts in the comparison (Group A).

Significant improvements in the results of the selection bias tests were found when unobserved factors (along with observed factors) were added to the pre-entry earnings models. The fixed effects model passed the selection bias test (i.e., the coefficient on the JTPA variable is not significantly different from zero) for the two comparison groups (Group B and Group C) that emphasized patterns of pre-enrollment employment and earnings in the cell matching phase for women; these same models failed the selection bias tests for men. The Heckman selection model passed the selection bias test for both women and men when using the third comparison group (Group C). Women who enrolled in JTPA earned $45 more than women in the third comparison group during the 6-month pre-intervention period, after accounting for both observed and unobserved factors in the pre-intervention earnings model. This difference, however, is not statistically significant -- indicating increased confidence that estimates of the post-training impact of JTPA enrollment based on the Heckman selection model may be unbiased. Similarly, men who enroll in JTPA earned $76 less than men in the third comparison group during the pre-intervention period, but this is not statistically significant.

In summary, the selection bias test results from the Utah study indicate that selection into JTPA -- if not properly modeled -- will result in biased estimates of program impacts. Quasi-net impact estimates would be expected to vary greatly across earnings models that failed to pass the pre-intervention earnings selection bias test. Use of the Heckman selection model, which utilized information in the structure of errors in the selection process to capture unobserved factors, increases the confidence in post-training impact estimates may be unbiased. For this reason, only the Heckman selection model will be used in the Utah study to estimate the program impacts for Title II-A adult women and men.

Tests For Secondary Selection Bias Among JTPA Participants

While the primary focus of the Utah study is to estimate the JTPA impact on post-training employment and earnings for the average enrollee, the study also investigates the differential impacts of participants enrolled in one of three major program activities -- occupational classroom training (OCT), on-the-job training (OJT), and job search assistance (JSA) in the absence of more intensive classroom or work place training. In addition, the study also estimates the differential impacts on participants who complete training (compared with program drop-outs), and those who are placed with private sector firms following training.[24] In this way, the study will go beyond determining if, on average, Utah's JTPA II-A adult program makes enrollees better off; it will address the next level of concern relating to what type of individual or program has higher (or lower) pay offs compared to the average participant.

Given this added level of analysis, it must also be recognized that a second level of selection bias, not previously discussed, may characterize JTPA enrollees. This form of selection bias may occur within the JTPA treatment group once participants have selected into the program. Two types of secondary selection bias are considered in this study -- program activity selection and program completion selection.

The first type of selection occurs when participants are sorted into available activities (or sequence of activities) according to personal preferences and/or assignment by the assessment staff of the SDA or service provider. For example, participants who are placed into -- or self-select into -- the OJT program may be more job ready and more highly motivated than those who enroll in classroom training or job search assistance.[25] These same individuals may be more likely to be employed -- and if employed to earn higher earnings -- than other JTPA enrollees. Failure to account for this secondary form of selection will result in biased estimates of program activity impacts.[26]

A second type of selection occurs when participants choose whether or not to complete their assigned activity. Participants are fully aware of the foregone earnings possible had they not enrolled in JTPA, and at any time participants know they could leave training for available employment. The decision to complete the program, as well as to be placed upon completion, is of critical importance to understanding why the JTPA program benefits some more than others.

In both cases, failure to address the secondary forms of selection within the JTPA program may result in biased estimates of the impacts of JTPA program activities and program completion on the post-training employment and earnings of participants. Like before, various specifications of the earnings process can be tested for the existence of these secondary forms of selection bias. Since only the third ES comparison group (Group C) passed the selection bias tests for the average JTPA participant, only the one comparison group is used in the following secondary forms of selection bias tests.

As can be seen in Figures 4.4 and 4.5, the pre-intervention employment and earnings pattern of JTPA participants by activity and termination status is relatively similar. Both employment rates and employed earnings of those enrolled in the more intensive training programs (OCT and OJT) track each other closely, while those enrolled in JSA-only experienced slightly lower employment rates and higher employed earnings.

The employment and earnings patterns of individuals who either fail to complete the program or fail to be placed upon completion are also very similar, while those placed experienced slightly higher employment rates and employed earnings in the two year period prior to JTPA enrollment.

As shown in Table 4.5, the secondary forms of selection bias were only tested with the first two tests utilizing observed factors as discussed earlier. Cell matching strategies on observed characteristics appear sufficient to capture the selection decisions by both activity type and program termination status for women. In no case was the JTPA binary variable statistically significant for women. This finding was further supported by the linear control function test, which specified observed factors in the pre-enrollment earnings models for women.

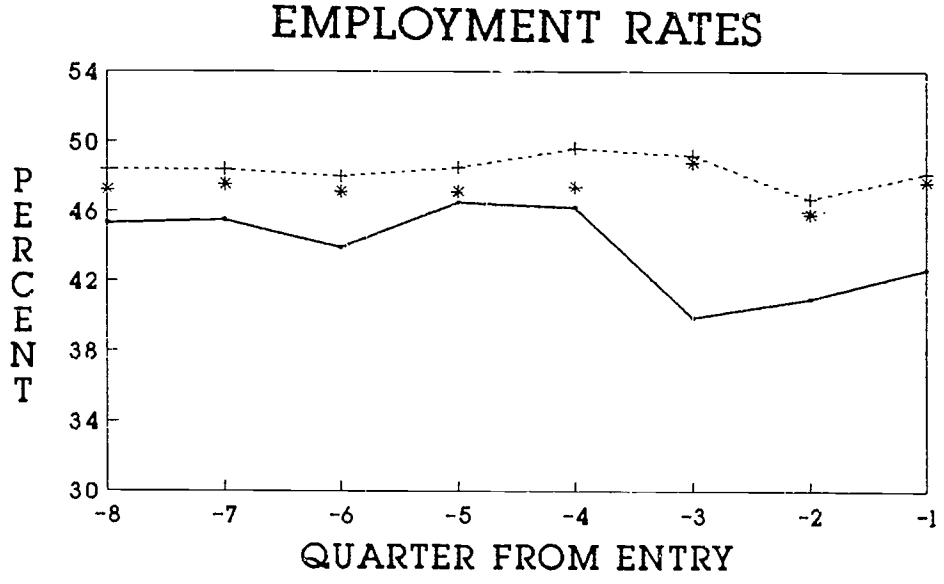
For men, the cell matching strategies would yield biased results of activity program impacts for those enrolling in OCT programs. Failure to control for this selection bias would result in activity impact estimates that would be biased upwards. This selection bias can easily be removed with observed factors (i.e., personal demographics, schooling, and prior employment and earnings patterns) in the post-training employment and earnings impact models.

Similarly, male JTPA enrollees are characterized by secondary selection bias regarding their termination status. Estimates of JTPA training for those placed would be biased upwards and those of enrollees who completed, but who were not placed, would be biased downward. Once again, simple addition of observed factors to the post-training employment and earnings impact models appears all that is necessary to remove this form of secondary selection bias.

In summary, selection bias tests performed on the decision to enroll in JTPA, and once enrolled to undertake and complete an assigned activity, have been used to identify appropriate specifications of models used to derive estimates of JTPA quasi-net impacts that are least likely to be unbiased. These tests showed that the decision to participate in JTPA is affected by both observed and unobserved factors that must be included in post-training employment and earnings models to yield unbiased estimators. In addition, the secondary forms of selection bias regarding the selection of an activity type and of completing the assigned activity can be modeled by using available observed personal demographics and pre-entry work histories used in the primary form of selection bias.

FIGURE 4.4

PRE-ENTRY WORK HISTORY: JTPA ACTIVITY SEQUENCE



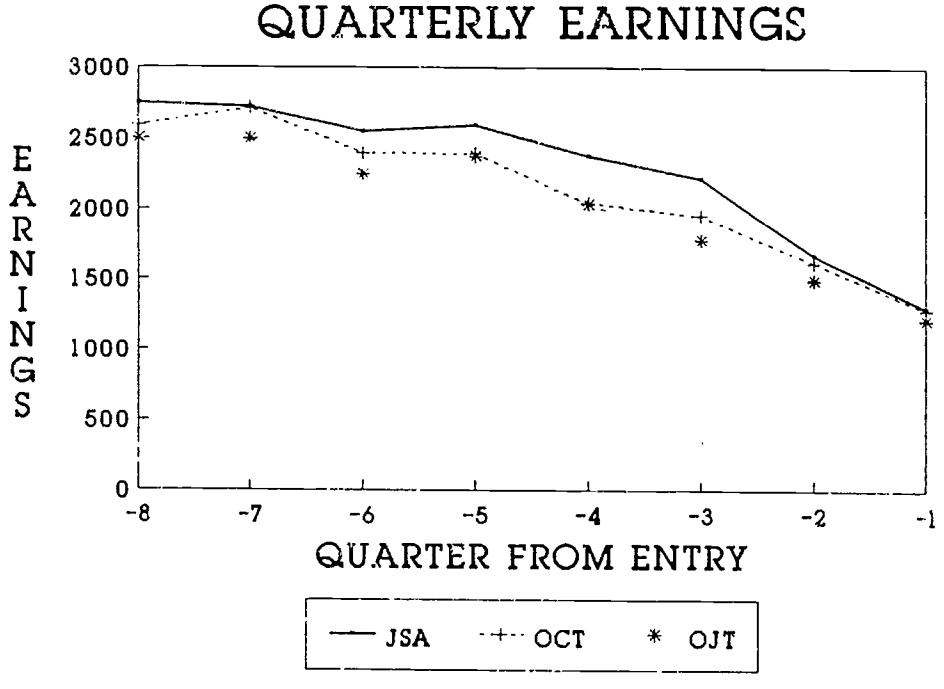
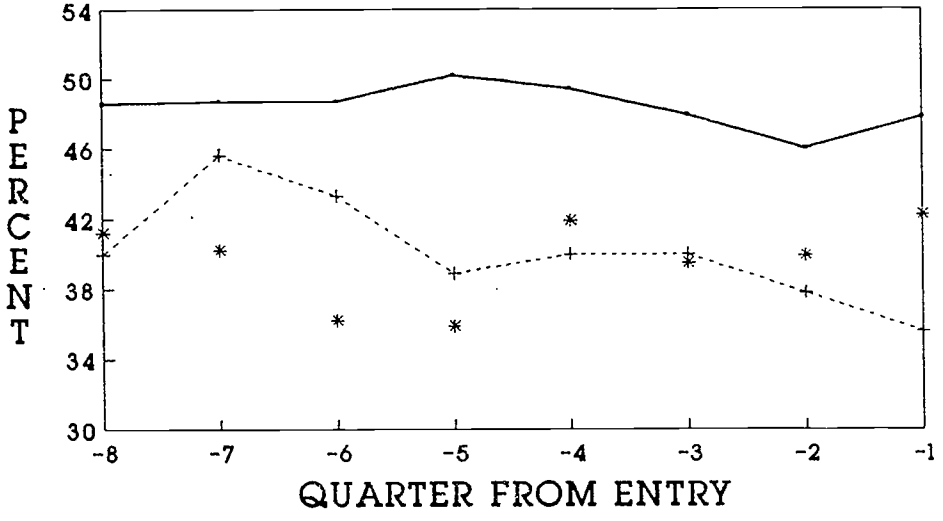
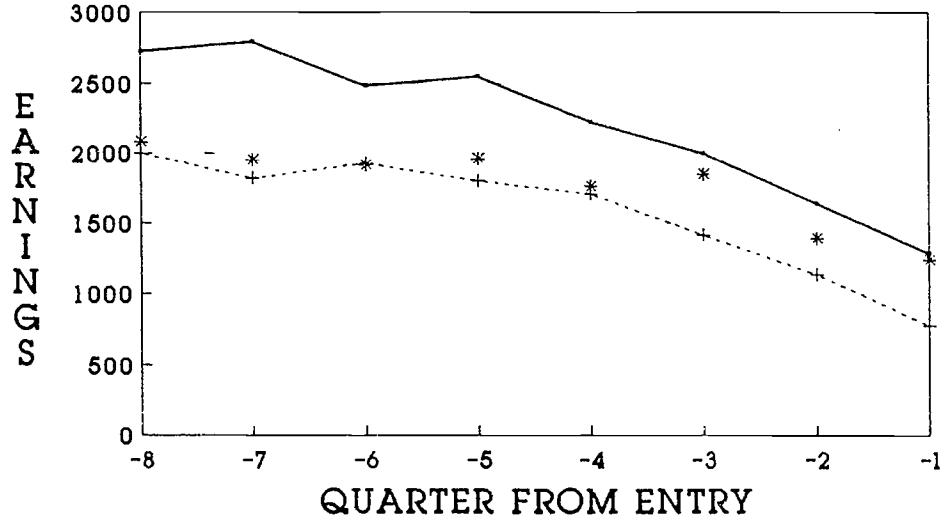


FIGURE 4.5
PRE-ENTRY WORK HISTORY: JTPA TERMINATION STATUS

EMPLOYMENT RATES



QUARTERLY EARNINGS



PLACED — NOT PLACED + NOT COMPLETE *

TABLE 4.5

SELECTION BIAS TESTS ON PRE-INTERVENTION EARNINGS: PY1987 JTPA II-A ADULTS BY ACTIVITY AND TERMINATION STATUS

(A) ACTIVITY TYPE

JTPA SELECTION PROCESS	COEFFICIENT AND STANDARD ERROR OF ACTIVITY VARIABLE FOR PRE-ENTRY EARNINGS MODELS: (Indication of Selection Bias)		
	OCT	OJT	JSA
WOMEN:			
Cell Matching Technique*	+68 (64)	-102 (66)	+37 (73)
Linear Control Function Model	+51 (45)	-127 (78)	+97 (55)
MEN:			
Cell Matching Technique	+356** (150)	-126 (129)	-141 (80)
Linear Control Function Model	+88 (111)	+20 (96)	-91 (98)

(B) TERMINATION STATUS

JTPA SELECTION PROCESS	COEFFICIENT AND STANDARD ERROR OF TERMINATION STATUS VARIABLE FOR PRE-ENTRY EARNINGS MODELS: (Indication of Selection Bias)		
	PLACED	NOT PLACED	NOT COMPLETE
WOMEN:			
Cell Matching Technique*	+108 (72)	-126 (144)	-86 (77)
Linear Control Function Model	-54 (127)	-162 (220)	+55 (53)
MEN:			
Cell Matching Technique	+328* (175)	-569* (301)	-184 (203)
Linear Control Function Model	-25 (49)	-89 (98)	+145 (147)

NOTE: The level of confidence for each coefficient is indicated by the asterisks following each coefficient (* = 90%; ** = 95%; *** = 99%).

In the following chapter, the Heckman selection model is used to derive estimates of average JTPA impacts as well as separate activity and termination status impacts for enrollees. The third comparison group (Group C), identified in this chapter as being the most similar to JTPA enrollees in PY 1987, will be used as a proxy for the JTPA counterfactual. Based upon the selection tests run above, the likelihood that the non-experimental earnings estimates of program activity, and termination status impacts are characterized by selection bias are minimized.

Endnotes

1. An explanation of the statistical procedures used to control for unobserved differences affecting the selection decision and subsequent employment and earnings is beyond the scope of this study. The econometric literature often cited to explain this statistical methodology includes: Heckman (1979), Barnow, et.al. (1980), Bassi (1983), Heckman and Hotz (1989), and Greene (1993).

2. It may be noted that many unemployed individuals who register with ES do so to be placed into full time employment without first having to undergo intensive job training. To the extent this characterizes those in the ES comparison group, the non-experimental net impact estimates of this study approximate the premium realized by JTPA trainees relative to not undertaking any form of government funded training. In contrast, the net impact estimates of the National JTPA Experiment (NJE) measure the premium realized by those assigned to JTPA compared to the control group who may either register for ES or undertake other forms of training self-financed or funded through other government programs. Since not all members of the NJE control group registered with a state's employment service, the net impact estimates are likely to be smaller than those of the Utah study because more of the NJE control/comparison group may have undertaken non-JTPA II-A funded training.

3. A random sample was drawn instead of using only those cases coded as disadvantaged because Utah Employment Security does not verify the accuracy of this data element on the intake form. It is coded according to a self-reporting process whereby the applicant informs the Employment Security intake official of his/her status following the viewing of a thirty minute movie explaining the eligibility for and services available from the department.

4. Less than 2% of all individuals were omitted because of missing demographic variables. As such, the resulting sample should reflect the general demographic composition of the original 10% sample of ES registrants.

5. Observations showing no quarterly wages are composed of (a) unemployed individuals who wish to work but can find no acceptable employment during the quarter, (b) individuals who are not working and who are not looking for work (e.g. students, housewives, the institutionalized, etc.), and (c) individuals employed but not in Utah's covered sector (e.g. self-employed or those employed outside the state).

6. The "dip" in earnings of JTPA participants is characteristic of the pattern of pre-enrollment earnings of those enrolling in most federally funded job training programs. This pattern was first analyzed for Manpower Development and Training Administration (MDTA) enrollees by Ashenfelter (1978) and later with Comprehensive Employment and Training Act (CETA) enrollees by Ashenfelter and Card (1985).

7. Three independent studies using the CPS-SSA data for deriving comparison groups to analyze the NSW are found in: LaLonde (1986), Fracker and Maynard (1987), and Heckman and Hotz (1989).

8. See for example: Long and Wissoker (1992) and Friedlander and Robins (1992).

9. As noted ear r, one way an estimate of JTPA training impact is said to be biased is when factors which affect the decision to enroll in JTPA are omitted from the model and are also related to the post-training outcome measures of net impact studies. When this occurs, the independent JTPA training variable is correlated with the error term in the net impact model -- causing

estimators to be biased. See Heckman and Robb (1985), Heckman and Hotz (1989), and Moffit (1992) for a detailed statistical explanation of the selection bias problem. Other biases, such as measurement error bias, may also characterize the non-experimental models, but are not considered to be significant in this study.

10. These techniques are described in Rubin (1980) and Rosenbaum and Rubin (1985) and later used in a CETA evaluation study for USDOL by Westat, Inc. and later published by Dickinson, Johnson and West (1986).

11. Since less than 20% of JTPA II-A adult participants in Utah are non-white, it is not possible to classify racial ethnic minorities into smaller sub-sets.

12. A fifteen cell matrix composed of five age ranges (22-25, 26-30, 31-35, 36-40, and 41 to 54) and three schooling categories (drop out, high school graduate/equivalent, and post-high school) was constructed for each of the four gender-race groups of JTPA trainees. This distribution was then replicated by random sampling within each cell of the larger sub-set of Employment Security applicants.

13. Since a relatively large percentage of JTPA and Employment Security registrants are out of the labor force in a given year, the wage level distribution and the change in wage distribution were first separated into those working and those out of the labor force. The JTPA distribution of those out of the labor force was replicated with the Employment Security sample, and the remaining distribution of workers was categorized into wage level and wage change quartiles with the ranges defined by the observed JTPA distributions. Random samples of Employment Security registrants in each of these wage level and wage change distribution categories were taken when the number of Employment Security registrants allowed. The actual number of Employment Security registrants was used in those categories when the number of clients was too small to derive the expected distribution of the JTPA enrollee group.

14. The same methodology for replicating the distribution of the level of earnings for the first comparison group is followed here in that the JTPA individuals without wages are separated from the remaining quartiles of quarterly wage earners. To the extent the reduced sample size allows, this distribution is replicated by random sampling the larger Employment Security sample within each category.

15. As defined earlier, a fully attached worker is one with positive quarterly earnings for the four quarter period; a partly attached worker has positive quarterly earnings for one to three quarters; and one who is out of the labor force has no positive quarterly earnings for the four-quarter period. Two successive four quarter periods are defined prior to the time a member of the comparison group utilized the Utah Employment Security.

16. As before, all wage distributions are first categorized according to individuals not working or employed, with the latter being divided into quartiles according to the wage level or wage change measure of earnings. To the extent the reduced sample size allows, these distributions are replicated by random sampling the larger Employment Security sample within each category.

17. In a statistical sense, omitted variables which affect an independent variable (JTPA participation) as well as the dependent variable (say post-training earnings) violate the assumption of independence between an independent variable and the error term. This results in biased estimators, as the expected value of the error term -- given the value of the independent JTPA participation variable -- is not equal to zero. The goal of specifying non-experimental models is to find a way to break this dependence by adding omitted variables to the model and/or re-specifying the models in ways to break the dependence between independent

variables and the error term. The ability to re-specify the non-experimental models to break this dependence rests upon the underlying assumptions one must make regarding the selection process and the error structure of the models. Further discussion on these points may be found in Heckman (1979), Heckman and Robb (1985), and Heckman and Hotz (1989).

It may be noted that in experimental models, selection bias will not arise as individuals are randomly assigned to either the treatment or control group. In this case the selection process is random and not systematically related to any independent variables, so that the independence between the JTPA participation variable and the error term is maintained.

18. Since much of their analysis was to compare controls in experimental demonstrations to selected comparison groups, many tests were designed to analyze post-training, as well as, pre-training earnings. Several of these tests are not relevant to the present study since no control group from a JTPA experiment in Utah exists, and as such, only pre-enrollment validity tests were undertaken.

19. See: Ashenfelter (1978) and Ashenfelter and Card (1985) for non-experimental training impact models which utilize the longitudinal structure of earnings.

20. It may be noted that tests for selection bias in all cross-section earnings models using the earnings for the second year prior to entry failed. The same was true for the year prior to entry for males, which experience a greater "earnings dip" in the immediate pre-entry periods than do females.

21. Another variant of the fixed effects model is the random effects model which requires more pre-entry earnings observations than existed in this Utah Study.

22. For a discussion of these complex assumptions, see Heckman (1979).

23. As noted earlier, the 6-month period refers to the third and fourth pre-entry quarter that occurs just prior to the "dip" in earnings of JTPA enrollees. Since 30 regression models were used to test for selection bias, only a listing of dependent and independent variables for the five tests for each comparison group is given in Appendix C. Complete regression results are available through the Commission.

24. Placement for those enrolled in classroom occupational skills and job search assistance programs is straightforward, whereas placement in OJT programs requires the participant to be hired permanently following termination of the wage subsidized period of JTPA training.

25. It may be noted that for a period during PY 1987, eligible individuals were permitted to find their own OJT employer - which may well have resulted in a different type of person selecting OJT compared to the other activities offered by Utah's SDAs.

26. It may be noted that the National JTPA Experiment design precludes experimental estimates of activity enrollment and termination status impacts as both are subject to individual choice -- something the random assignment experiment is not designed to address. Failure to capture such individual choice seriously limits the understanding of <u>how</u> JTPA works. Such an understanding is crucial to improving the effectiveness of future JTPA program operations. It is for this reason, that non-experimental impact models need to be developed and improved.

CHAPTER FIVE: Utah JTPA II-A Adult Program Impacts

Net impacts of the Utah JTPA II-A adult programs for PY 1987 are estimated using non-experimental techniques, which compare the observed outcomes of program participants with those of the ES comparison group derived in Chapter Four. The post-intervention work history of the ES group is used to approximate the "counterfactual" -- the employment and earnings of program participants had they not enrolled in JTPA.[1] To allow for participants to complete their training. the second year following entry is used as the appropriate period for analyzing two major labor market outcomes -- the likelihood that a participant is employed and the earnings of those who are employed.[2]

This chapter presents the findings of program impact models for all adults who enrolled for the first time in the Title II-A program in Utah during PY 1987. The findings are presented in two sections. The first section begins with a discussion of the differences in the observed, or statistically "unadjusted" post-entry employment and earnings of enrollees and those of members of a similar comparison group. Then these observed differences are contrasted to the statistically "adjusted" differences, which are derived from models of post-program employment and earnings that adjust for both observed and unobserved characteristics of participants.[3]

One purpose of the discussion is to demonstrate that simple bar chart descriptive studies, which indicate the differences in unadjusted labor market outcomes between the two groups, may be misleading indicators of the program's performance. The second purpose is to demonstrate the extent to which JTPA "works," as measured by the estimates of program impacts, and the degree JTPA provides assistance to identified "hard-to-serve" clients in the state of Utah.

In the second section of this chapter, the findings of the statistical models are subjected to "sensitivity analyses" for the purpose of determining how much the estimated program impacts change if major alterations are made in the construct of the models. This latter analysis is important because often only one set of statistical findings is presented which, in fact, could be misleading or simply suspect if the models are poorly or improperly specified.

PART I: Unadjusted Versus Adjusted Program Impacts

As indicated earlier, the vast majority of JTPA training is composed of relatively short duration intervention strategies -- most lasting less than 6 months with nearly all being less than 12 months in duration. Accordingly, program impact measures emphasized in this study are calculated during the <u>second</u> year following entry into JTPA, which is clearly in the "post-training" period. First, the unadjusted difference between JTPA participants and Employment Security (ES) comparison group members (Group C) are compared with the "point" estimates of each adjusted program impact. Next, a probability level of statistical significance is attached to each "point estimate" to give a sense of a level of confidence that can put in the program impact estimates derived from the adjusted models.

POINT ESTIMATES OF PROGRAM IMPACTS

According to the descriptive statistics shown in Figure 5.1 below, the JTPA enrollment group realizes mixed post-program work experiences compared with the "similar" ES comparison group members.[4] Such descriptive findings, if accepted, could suggest that while some enrollees remain employed, the jobs they find pay less than those they would have found on their own -- based on the experiences of the comparison group.

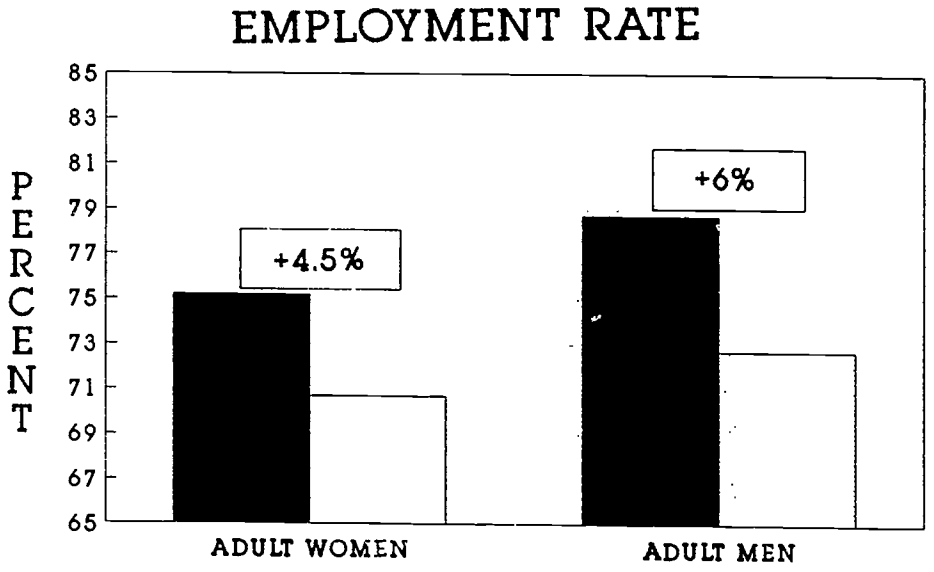
The problem with such descriptive information is that these statistically "unadjusted" differences between the two groups do not account for either observed or unobserved differences that exist between JTPA enrollees and members of a "similar" comparison group. In reality, the differences in post-program employment and earnings are affected first by observed differences in demographic characteristics (such as age, race, marital, and welfare status among others), schooling levels, and pre-entry work histories of the two groups. Secondly, differences in the outcomes are affected by differences in unobserved factors including among others, differences in attitudes toward self-improvement, motivation to work, and English language barriers between the enrollment and comparison groups. These factors, in conjunction with enrolling in JTPA, result in the unadjusted differences in post-program work experiences depicted in Figure 5.1. The objective of building statistical models of post-program employment and earnings is to separate the JTPA impact from the other observed and unobserved factors which are combined in simple bar graph descriptions.

The significant differences between employment and earnings levels of adult women and men seen in Figure 5.1 reinforce the widely understood fact that the labor market processes for the two gender groups are very different. For this reason, statistical models are developed separately for adult women and men. For each gender, a complete model is derived which first accounts for (or in econometric language "controls for") observed differences between JTPA enrollees and the ES comparison group (Group C) selected. In addition, unobserved factors which affect an individual's choice to enroll in JTPA and later affect post-program employment and earnings are included in the models through a complex statistical procedure. The full set of results is given in Appendix D; the relevant JTPA program impact results are given below in Table 5.1.[5]

Average JTPA Program Impacts. The adjusted program impact is measured as the difference between the program outcomes of participants and the outcomes estimated to exist had the participants not enrolled in JTPA. Two features shown in Table 5.1 are noteworthy with respect to the adjusted impacts compared with the unadjusted outcomes described above. First, the adjusted employment rate impact estimates are similar to the unadjusted differences although they are somewhat higher (+5.1% versus +4.5% for adult women and +7.5% versus +6.0% for adult men). Second, the adjusted earnings impacts estimates differ significantly from the unadjusted differences.

Adult women participants are now estimated to realize $510 higher annual earnings in the second post-program year because of program participation even though the mean unadjusted difference between participants and comparison group members is a negative $326. The reason the figures differ so much is that JTPA participants may have less human capital (i.e., both weaker education and work histories) and/or perhaps weaker work motivation or other non-measured factors related to later employment and earnings potential. Failure to capture these observed and unobserved factors, while only slightly biasing the employment rate program impact, seriously biases the earnings impact downward by over $800 in the second year following entry.

FIGURE 5.1
Work Experience In Second Year Following Entry Date



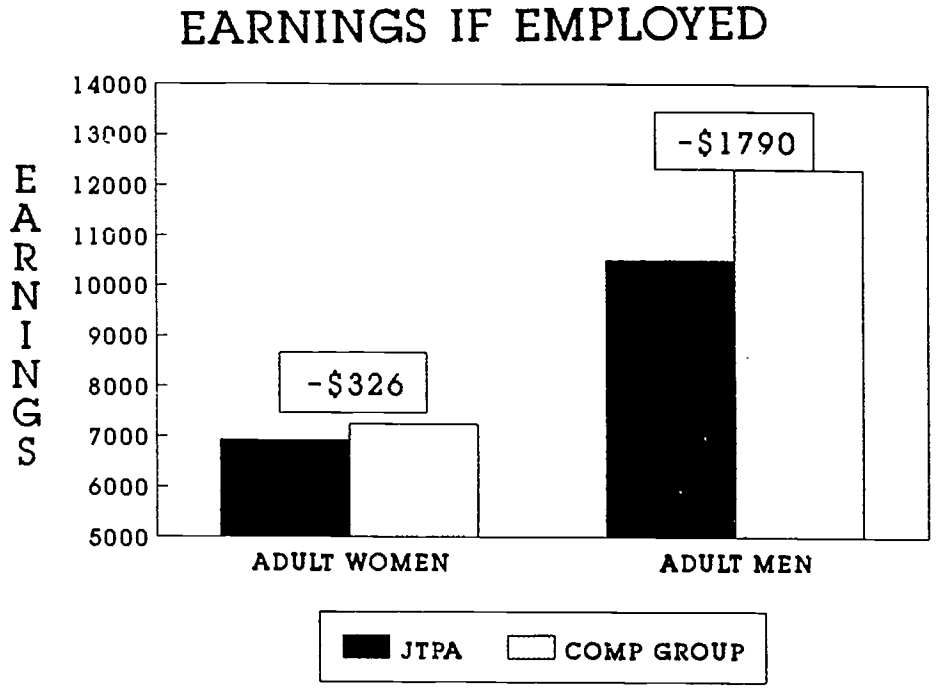


TABLE 5.1

COMPARISON OF UNADJUSTED WITH ADJUSTED PROGRAM IMPACTS DURING SECOND YEAR FOLLOWING ENTRY[a]

(A) ADULT WOMEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	UNADJUSTED	ADJUSTED	UNADJUSTED	ADJUSTED
JTPA TOTAL	+.045	+.051	-326	+510
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.050	+.052	-113	+849
ON-THE-JOB TRAINING	+.096	+.101	-367	+1026
OTHER ACTIVITY	-.035	-.040	-636	-278
TERMINATION STATUS:				
PLACED	+.081	+.087	+520	+905
NOT PLACED	-.069	-.066	-2463	-1771
NOT COMPLETE	+.052	-.046	-3342	-2040

(A) ADULT MEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	UNADJUSTED	ADJUSTED	UNADJUSTED	ADJUSTED
JTPA TOTAL	+.060	+.075	-1786	+444
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.077	+.084	-712	+1103
ON-THE-JOB TRAINING	+.059	+.074	-1674	+396
OTHER ACTIVITY	+.050	+.068	-2606	-77
TERMINATION STATUS:				
PLACED	+.089	+.104	-1319	+1303
NOT PLACED	-.006	+.032	-5811	-780
NOT COMPLETE	-.143	-.120	-4736	-2054

[a] Adjusted program impact estimates are derived from two stage Heckman-type regressions using Group "C" from Chapter Four as the representative comparison group. See Appendix D, Tables D.1 through D.4 for complete listings of these regressions.

Likewise, the program impact for adult men is biased downward when observed and unobserved factors related to later employment and earnings are omitted. In this case, adult men are expected to realize a $444 gain in the second post-program year because of program participation even though the unadjusted difference is nearly $1,800 less.

These figures suggest while **simple comparisons may be good approximations for employment rate impacts, they seriously distort the impact JTPA appears to have on the earnings of employed participants.** In particular, once both observed and unobserved differences between program enrollees and similar ES comparison group members are accounted for, JTPA has positive effects on both the employment and earnings experiences of enrollees in the selected follow-up period.

In addition to the over-all impact JTPA participation may have on enrollees, it is also useful to identify the different impacts various types of training activities have, as well as the impact program completion and placement have on later enrollee employment and earnings.

Impacts by Types of Training Activities. As indicated in Table 5.1, three types of major training activity are examined for each gender group.[6] For women, on-the-job training (OJT) is associated with the greatest earnings impact (+$1,026); next occupational classroom training (OCT, with an estimated earnings impact of +$849); and finally other activities (OA, with an estimated earnings impact of -$278 compared with the ES comparison group). For men the order of training activity impacts is reversed.[7] The greatest earnings impact for men is realized by those enrolled in OCT (+$1,103), while positive but smaller earnings impacts are realized by men in OJT programs (+$396).[8] Similar to women, "other activities" has a negative earnings impact for adult men (-$77).

It may be noted that the program impact findings presented above are possible because of the availability of work experience data for the selected comparison group. While this information may not be easily obtained in other states, it is still possible to obtain "gross impact" estimates of alternative types of training activities using only JTPA terminees. In particular, estimates of gross impacts may be derived with statistical models of post-program outcomes of those enrolled in more intensive OCT and OJT participants compared with those enrolled in less intensive OA programs. Such models for Utah are analogous to the ones used in the National Commission for Employment Policy training choices and relative outcomes study, which analyzed employment and earnings outcomes for the population of JTPA terminees from 11 states with UI wage records.[9] A discussion of such impact models for Utah, and how they relate to the net impacts estimated in the study, is given in Appendix E.

Impacts of Termination Status. Three types of termination status are recorded for JTPA participants in Utah: individuals who complete training objectives and are placed into unsubsidized private sector employment, those who complete the program but are not placed into jobs, and those who fail to complete the program objectives.

As indicated above in Table 5.1, placement activities of JTPA appear to be extremely important. The estimates of the adjusted employment and earnings impacts for those placed into fully unsubsidized private sector jobs are significantly greater than the average impact estimates discussed earlier.[10] Adult women and men are 9 to 10% more likely to be employed later if they were initially placed as compared with the smaller estimated average JTPA employment impact (i.e., 5 to 8 percentage point premium of the average program participant regardless of termination status). In addition, among those employed during the second post-program year, both women and men who were placed later earned more than JTPA enrollees in general. Program completion and placement are associated with earnings premiums substantially above those reported earlier for all JTPA enrollees combined; adult women who were placed earned

over $900 more and adult men earned over $1,300 more, relative to members of the comparison group.

In general, individuals who complete the program but are not placed fare better than those who fail to complete JTPA, but program completion without placement does not carry the same positive post-entry employment and earnings effects as does completion with placement. Adult men who drop out of JTPA are far less likely to be employed later (-12%) and earn far less (-$2,054) than estimated had they not enrolled in JTPA. Those who complete JTPA but are not placed also fare less well, but the differences are smaller (i.e., they are slightly more likely to be employed later (+3.2%) and earn slightly less ($780)).

For adult women, the premium to job placement is even greater. Those who complete, but fail to be placed, experience roughly the same reduced employment and earnings if employed as do those who drop out of JTPA before completion. Both types of individuals are roughly 5% to 6% less likely to be employed later and earn roughly $2,000 less than those in the comparison group. These findings suggest JTPA placement activities are extremely important, especially for adult women who typically have less prior attachment to the work force than men.[11]

CONFIDENCE LEVELS OF ADJUSTED PROGRAM IMPACT ESTIMATES

Before adjusted estimates of program impacts are used for policy making, it is critical to indicate the probabilistic nature of the point estimates discussed above. This is necessary because no single statistical model can ever include all variables that affect program impacts and no single statistical model of individual's employment decisions or earnings processes can be specified in a manner that fully accords with reality. In all cases, a margin of error must be added to these single "point" estimates which indicates the degree of confidence a researcher has in concluding the program did indeed have an effect on employment or earnings. As such, the program estimates in Table 5.1 are duplicated in Table 5.2 below along with the "confidence" or probability level, one can attach to the point estimates.

Another interpretation of this probabilistic nature of program impact estimates is the likelihood that the impact point estimate has either a positive or negative effect. If the probability is low, one could equally conclude the impact estimate is zero. In essence, the probability numbers included in Table 5.1 indicate the likelihood of the point estimate being "non-zero," that is of having a significant positive or negative effect on program impacts.

Generally, estimates are said to be "statistically significant" if the probability level of the point estimate is equal to or greater than 90%. For example, while all program impacts on employment for adult women in the second post-program year are positive, only two estimates are considered to be "statistically significant": OJT enrollment (+10.1%) and completing and being placed into a job at termination (+8.7%). Using the tests of significance, one can conclude that adult women enrolled in OJT (or who are placed into a job at termination) will have a higher probability of being employed 2 years after enrollment than their counterparts not enrolled in OJT (or placed in a job at termination).[12] Enrollment of women in OCT programs may increase the probability of employment, but the confidence one places in this estimate is less. Similarly, the confidence of the positive estimates for employment among adult men is significantly higher than that of the impacts on earnings.

TABLE 5.2

ADJUSTED PROGRAM IMPACT ESTIMATES AND PROBABILITY OF ESTIMATES BEING NON-ZERO

(A) ADULT WOMEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	ADJUSTED	PROB LEVEL	ADJUSTED	PROB LEVEL
JTPA TOTAL	+.051	88%	+510.	68%
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.052	85%	+849.	80%
ON-THE-JOB TRAINING	+.101***	97%	+1026.	88%
OTHER ACTIVITY	-.040	72%	-278.	29%
TERMINATION STATUS:				
PLACED	+.087***	97%	+905.	84%
NOT PLACED	-.066	68%	-1771.	83%
NOT COMPLETE	-.046	75%	-2040.***	97%

(A) ADULT MEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	ADJUSTED	PROB LEVEL	ADJUSTED	PROB LEVEL
JTPA TOTAL	+.075***	99%	+444.	46%
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.084**	96%	+1103.	82%
ON-THE-JOB TRAINING	+.074**	98%	+396.	40%
OTHER ACTIVITY	+.068**	96%	-77.	8%
TERMINATION STATUS:				
PLACED	+.104***	99%	+1303.**	96%
NOT PLACED	+.032	38%	-780.	36%
NOT COMPLETE	-.120***	99%	-2054.**	97%

NOTE: The level of probability that the estimated program impact is non-zero (i.e., having no impact) which is statistically significant, varies from 90% (minimally acceptable probability) to 99% (highest level of probability). The three major probability levels are noted by the number of asterisks appearing after a statistically significant estimate: * = 90%; ** = 95%; and *** = 99%.

A major factor contributing to the relatively low significance of the point estimates reported in this study is related to small sample size. Unlike the National JTPA Experiment which sampled over 20,000 individuals or even the National Commission for Employment Policy's impact study based upon 10,000 adult women and 10,000 adult men from the universe of JTPA terminees in eleven states, the Utah study is based on 1,655 adult women and 1,737 adult men. While this size is sufficiently large to derive stable point estimates from individual data, they may not be stated with the same level of confidence as multi-state or nationally based studies.

Trade-offs may need to be made when undertaking program impact analyses. On the one hand, states may not want to apply "statistically significant" estimates of program impacts from large national (or multi-state) JTPA studies to their own state because they believe their state has its own distribution of JTPA clientele or particular anomalies regarding its regional and local labor markets. On the other hand, state-specific studies -- which can be adjusted for these anomalies -- are smaller in size and often result in lower levels of confidence that the estimates of JTPA program impacts are "meaningful" (i.e., are likely to have a strong positive effect).

In summary, the employment impact estimates for Utah's Title II-A adult enrollees for PY 1987 suggest **JTPA participation has a strong positive effect on employment for adult women and men who complete more intensive training programs and who are placed into fully unsubsidized private sector jobs.** JTPA resources directed towards job placement, in addition to job training, appear to be beneficial in Utah. This positive employment impact during the second post-program year in Utah for PY 1987 is somewhat greater for adult men than adult women. In addition, JTPA results in higher second year earnings, if employed, for those placed through more intensive training programs. For adult women, this earnings impact is stronger for those enrolled in OJT, while for adult men it is stronger for those enrolled in classroom training.

Finally, not all JTPA terminees from a particular activity benefit equally from program participation. Those who drop out may even be worse off if they had never enrolled. The time spent in a program and the knowledge of program failure may have a harmful effect on those individuals who, otherwise, do not benefit from JTPA participation.

THE "HARD-TO-SERVE" ADULTS IN UTAH

While the focus of the study is on estimating JTPA program impacts, the statistical models may also be used to identify which of the personal demographic factors are characteristic of "hard-to-serve" enrollees. That is, the complete statistical models contain personal demographic factors along with JTPA intervention factors which are both related to post-enrollment employment and earnings. (The complete model results are given in Appendix D.) Those personal factors that are significantly related to a smaller probability of being employed in the second year following enrollment, and/or significantly related to lower earnings if employed, are indicative of "hard-to-serve" individuals.

The first major finding in this vein is that personal demographic factors <u>are not related to post-training employment barriers in finding a job, but rather to barriers in realizing higher earnings when employed.</u> As shown in Table 5.3, two personal demographic variables -- high school drop-outs and being black -- present significant barriers to higher post-enrollment earnings for adult women and men. Female high school drop-outs earn $1,437 less than high school graduates, while male high school drop-outs earn $1,688 less; black women earn $1,463 less than white women, while black men earn $3,777 less than white men. In addition, women who were former welfare recipients earn less than non-recipients (-$1,355) as did those residing in rural relative to urban SDAs (-$1,148). Men who experienced a "dip" in earnings over the

TABLE 5.3

DEMOGRAPHIC CHARACTERISTICS OF THE "HARD-TO-SERVE"

DEMOGRAPHIC CHARACTERISTIC	POST-ENROLLMENT EARNINGS	LIKELIHOOD OF ENROLLING IN JTPA
WOMEN:		
HIGH SCHOOL DROP-OUT	-$1,437.	Same As Others
BLACK (Non-hispanic)	-$1,463.	Same As Others
WELFARE RECIPIENT	-$1,355.	More Likely
RURAL SDA	-$1,148.	Less Likely
MEN:		
HIGH SCHOOL DROP-OUT	-$1,688.	Same As Others
BLACK (Non-hispanic)	-$3,777.	Same As Others
PRIOR EARNINGS "DIP"	-$183/$1,000 Decline	More Likely

2-year pre-enrollment period also realized lower post-enrollment earnings than those not characterized by pre-entry earnings declines (-$183 per $1,000 decrease).

A complicating factor in improving the welfare of the "hard-to-serve," is the ability of SDAs to recruit and provide services to this sub-set of program eligibles. This phenomenon is shown in Table 5.3 by the relationship between the personal demographic factors and the likelihood of program enrollment. (These relationships were estimated in the first step of the two stage statistical program impact models.) SDAs in Utah did not appear to have a difficult time in recruiting and enrolling high school drop-outs and blacks. They also were very successful in enrolling women welfare recipients and men who had experienced a "dip" in pre-enrollment earnings.

Women in Utah living in rural SDAs relative to those living in urban SDAs, were less likely to enroll in Title II-A programs, however. This may have been due to a failure of these SDAs to actively recruit economically disadvantaged women, and/or the eligible women were less interested in enrolling in JTPA. Whatever the reason, women in rural SDAs appear to be the most "hard-to-serve" in Utah; they are less likely to enroll, and those that do enroll, earn substantially less if employed in the post-enrollment period.

PART II: Sensitivity Of Program Impact Estimates

The adjusted net program impacts discussed above are derived from a particular statistical model with a particular comparison group. The model and comparison group were not chosen lightly, however. Their selection was based on an analysis of prior findings and alternative econometric techniques discussed in the economics literature on program evaluations.

This point is important because often only one set of findings is presented. Policy makers who are not fully versed in the literature on employment and training evaluations or in econometrics are forced to accept results at face value; they must trust the analysis is both complete and

objective. This section discusses alternative ways that the statistical analysis in this report could have been conducted and indicates how the outcomes can differ, and in fact be misleading, depending upon the approach taken. While there are many alternative approaches that could have been included, the ones selected highlight key aspects of evaluation research: the choice of comparison groups, the availability of data on pre-program employment and earnings experiences, and adjustments for unobserved selection bias (through self-selection or selection by program operators).

THE IMPORTANCE OF A WELL CHOSEN COMPARISON GROUP

A major choice common to non-experimental impact models is that of identifying a comparison group similar to JTPA participants. To indicate the importance of choosing the best available comparison group, the same specified statistical models used earlier are re-estimated with the first of the three potential comparison groups (i.e., Group A) identified in Chapter Four. The key difference between this comparison group and the one chosen for analysis in this study (i.e., Group C) is the emphasis on matching the distribution of personal demographics rather than prior earnings patterns over the 2-year period prior to enrollment. As noted in Chapter Four, while Group C minimized selection bias, Group A resulted in the greatest selection bias for both adult women and men.

Table 5.4 shows the results associated with using the comparison group having the greatest self-selection bias (identified as "dissimilar") and repeats the results given earlier based upon the comparison group with the least selection bias (identified as "similar"). While the estimates of the employment rate impacts seem to differ little between using either comparison group, the estimates of earnings impacts differ widely. Use of the "dissimilar" comparison group seriously biases the estimates of adult women post-program earnings; indeed these estimates differ from those derived with the superior comparison group (Group C) by over $2,000. The estimates of earnings impacts of adult men differ from that using the superior comparison group by over $1,000. Such different estimates of program impacts clearly show the importance of identifying a comparison group that is similar to the JTPA participant sample.[13]

IMPROPER SPECIFICATION OF PRE-ENTRY WORK EXPERIENCE

A modeling specification that weakens non-experimental studies of program impacts occurs when data bases do not contain significant historical employment and earnings information, for example, when UI earnings record data are not available. If only that experience just prior to entry is used in program impact models, misinformation is likely to arise for those enrollees having atypical or temporary work patterns. Many adult JTPA enrollees are individuals who only decide to enter the program after going through difficult times in the labor force. Their pre-entry employment and earnings records are poor indicators of the stock of human capital they have acquired over longer periods of work experience.

To demonstrate the importance of having more than one year's worth of information on a person's work history, this alternative uses only the work experience occurring in the year just prior to enrollment for both the JTPA enrollees and the ES comparison group members. The results of this specification are listed under the column labeled "one year" in Table 5.5. All the information in the second year is purposely omitted, which prohibits the common "dip" in earnings from being included in the impact models, especially for adult men. (The columns labeled "two years" refer to the full model version.)

TABLE 5.4

SENSITIVITY OF PROGRAM IMPACT ESTIMATES TO SIMILARITY OF COMPARISON GROUPS

(A) ADULT WOMEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	DISSIMILAR	SIMILAR	DISSIMILAR	SIMILAR
JTPA TOTAL	+.036	+.051	+2937.***	+510.
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.038	+.052	+3519.***	+849.
ON-THE-JOB TRAINING	+.089**	+.101**	+3711.***	+1026.
OTHER ACTIVITY	-.056	-.040	+2511.***	-278.
TERMINATION STATUS:				
PLACED	+.067**	+.081**	+3775***	+905.
NOT PLACED	-.086	-.066	+1413.	-1771.
NOT COMPLETE	-.062	-.046	+1237.	-2040.**

(B) ADULT MEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	DISSIMILAR	SIMILAR	DISSIMILAR	SIMILAR
JTPA TOTAL	+.069***	+.075***	-2688.***	+444.
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.064**	+.084**	-2823.***	+1103.
ON-THE-JOB TRAINING	+.079**	+.074**	-1590.**	+396.
OTHER ACTIVITY	+.067**	+.068**	-2169.***	-77.
TERMINATION STATUS:				
PLACED	+.098***	+.104***	-1369.**	+1303.**
NOT PLACED	+.037	+.032	-3609.*	-780.
NOT COMPLETE	-.125***	-.120**	-4767.***	-2054.**

NOTE: See note on Table 5.2 for an explanation of the three probability levels indicated by the asterisks on relevant program impact estimates.

As shown in Table 5.5, omission of the second pre-entry year in the "truncated model" has little effect on the estimates of employment rate impacts. However, again there are sizable differences between the two models in their estimates of earnings impacts. The truncated model produces significantly larger estimates of earnings impacts for adult women and men. For example, omission of employment and earnings information in the second year prior to entry results in an estimated $2,000 earnings impact for women and a $5,000 earnings impact for men. These estimates are considerably greater than reported earlier in models which specify an additional pre-entry year (i.e., $510 earnings impact for women and $444 earnings impact for men). The higher earnings impacts occur because the low pre-entry earnings are assumed to reflect a permanently low stock of human capital in the truncated model. Once the higher levels of earlier pre-entry earnings are included in the full model construct, lower earnings impacts are estimated.

OBSERVED VERSUS UNOBSERVED ADJUSTMENTS

The final characteristic of the models used in this study is the specification of both observed and unobserved differences between the JTPA participants and ES comparison group members. To determine the importance of controlling for selection bias in non-experimental model designs, an alternative model is constructed that accounts only for the observed differences between the two groups. (In Table 5.6 the columns labeled "observed only" refer to this alternative.) Results of this model are compared to those of the adjusted model used throughout this study, which account for both observed and unobserved factors in a two stage statistical analysis. (The columns in Table 5.6 labeled "observed and unobserved" refer to the full model version.) As shown in Table 5.6, estimates of the "observed only" model version are fairly good approximations of the program impacts on employment rates. But, like before, they are poor approximations of the program impacts on earnings of those employed. For example, the program impact estimate of JTPA enrollment is nearly doubled for the single stage models. The program impact estimate for adult women is $805 and for men is $934 in the second post-program period; both are significantly greater than estimated with the two step model reported earlier.

These results suggest individuals who are more likely to self-select into JTPA (or be selected for JTPA) are also more likely to find better paying jobs in the post-training period compared to persons in the ES comparison group. Thus, with a single step model, part of the increase in post-entry earnings is due to program participation and part is due to program participants whose unobserved characteristics are positively related to higher earnings. As discussed earlier, these unobserved factors may include greater work motivation, greater self-esteem, or fewer English language problems.

TABLE 5.5

SENSITIVITY OF PROGRAM IMPACT ESTIMATES TO SPECIFICATION OF PRE-ENTRY WORK EXPERIENCE

(A) ADULT WOMEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	PRE-ENTRY WORK EXPERIENCE:		PRE-ENTRY WORK EXPERIENCE:	
	ONE YEAR	TWO YEARS	ONE YEAR	TWO YEARS
JTPA TOTAL	+.056*	+.051	+1921.***	+510.
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.054	+.052	+1892.***	+849.
ON-THE-JOB TRAINING	+.109***	+.101**	+2249.***	+1026.
OTHER ACTIVITY	-.038	-.040	+850.	-278.
TERMINATION STATUS:				
PLACED	+.085***	+.081**	+1634.***	+905.
NOT PLACED	-.053	-.066	-675.	-1771.
NOT COMPLETE	-.044	-.046	-1013.	-2040.**

(B) ADULT MEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	PRE-ENTRY WORK EXPERIENCE:		PRE-ENTRY WORK EXPERIENCE:	
	ONE YEAR	TWO YEARS	ONE YEAR	TWO YEARS
JTPA TOTAL	+.069***	+.075***	+5004.***	+444.
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.064***	+.084**	+4539.***	+1103.
ON-THE-JOB TRAINING	+.086***	+.074**	+3881.***	+396.
OTHER ACTIVITY	+.028	+.068**	+3380.***	-77.
TERMINATION STATUS:				
PLACED	+.097***	+.104***	+5178.***	+1303.**
NOT PLACED	-.014	+.032	+5186.***	-780.
NOT COMPLETE	-.069***	-.120**	+3786.***	-2054.**

NOTE: See note on Table 5.2 for an explanation of the three probability levels indicated by the asterisks on relevant program impact estimates.

TABLE 5.6

SENSITIVITY OF PROGRAM IMPACT ESTIMATES TO SELF-SELECTION BIAS CORRECTION

(A) ADULT WOMEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	ADJUST MODEL TO ACCOUNT FOR:		ADJUST MODEL TO ACCOUNT FOR:	
	OBSERVED ONLY	OBSERVED AND UNOBSERVED	OBSERVED ONLY	OBSERVED AND UNOBSERVED
JTPA TOTAL	+.052**	+.051	+805.*	+510.
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.052*	+.052	+1021.*	+849.
ON-THE-JOB TRAINING	+.102***	+.101**	+1420.***	+1026.
OTHER ACTIVITY	-.041	-.040	-747.	-278.
TERMINATION STATUS:				
PLACED	+.082***	+.081**	+1745.***	+905.
NOT PLACED	-.066	-.066	-2580.**	-1771.
NOT COMPLETE	-.045	-.046	-2607.***	-2040.**

(A) ADULT MEN

JTPA PROGRAM CHARACTERISTIC	EMPLOYMENT RATE		EARNINGS IF EMPLOYED	
	ADJUST MODEL TO ACCOUNT FOR:		ADJUST MODEL TO ACCOUNT FOR:	
	OBSERVED ONLY	OBSERVED AND UNOBSERVED	OBSERVED ONLY	OBSERVED AND UNOBSERVED
JTPA TOTAL	+.078***	+.075***	+934.*	+444.
JTPA ACTIVITY:				
CLASSROOM TRAINING	+.087**	+.084**	+1542.*	+1103.
ON-THE-JOB TRAINING	+.074***	+.074**	+981.	+396.
OTHER ACTIVITY	+.077**	+.068**	+412.	-77.
TERMINATION STATUS:				
PLACED	+.108***	+.104***	+1720.***	+1303.**
NOT PLACED	+.042	+.032	-1669.	-780.
NOT COMPLETE	-.119**	-.120**	-4330.**	-2054.**

NOTE: See note on Table 5.2 for an explanation of the three probability levels indicated by the asterisks on relevant program impact estimates.

Endnotes

1. The use of the entry date, rather than the exit date, as the departure point of analysis is in contrast with traditional JTPA follow-up reports. The former should be used in **net impact** analysis because a program eligible individual who enrolls in JTPA would have remained in the labor force either working or continuing to search for work beyond the entry date. Reports or **gross impact** studies that focus solely on JTPA enrollees should use the exit date as the relevant departure point for describing and analyzing different employment and earnings patterns of enrollees. As such, the term "post-program" as used in this net impact study refers to the entry not exit date.

2. Because longer periods of post-entry work histories were not available in this study, it was not possible to undertake a full-scale cost-benefit study, which would compare the participants' foregone earnings and program costs with the JTPA enrollees' earnings gains, and resulting higher tax revenues and reduced welfare expenditures related to their improved work histories.

3. Selection into JTPA is modeled in the first step. The results are then used in the post-program models of the second step. In essence, the unobserved factors which are related to program participation are included in an error term of the first stage model. This error term is then used as an additional explanatory factor in the second step, which explains post-program employment and earnings of program participants.

4. The employment and earnings outcome measures shown in Figure 5.1 are calculated as: (1) the percent of treatment or comparison group members who had positive earnings during the second year following entry to JTPA or ES, and (2) the average annual earnings of those who had earnings during this period.

5. The modeling of observed factors has traditionally been carried out with linear or non-linear multivariate regression models, whereby the variation in a labor market outcome, say earnings if employed, is explained by a host of causal factors. These models are common to all modern statistical software packages. The statistical models utilizing unobserved factors contained in the error structures of the two processes of participation and later work history outcomes is relatively new and supported in specialized econometric software packages only. This procedure uses information about the structure of modeling errors in the program participation model along with the post-program work history models. For a statistical explanation of this sophisticated technique, see: William Greene (1993). The same author has also compiled specific computer software to handle such complexities, and is available under the trademark "LIMDEP" (Limited Dependent Variables).

6. In reality, JTPA participants receive minimal supportive services, career counseling, and brief short job search assistance in addition to their major activity. In this sense, "activity" as used here refers to the major enrollment activity of program enrollees.

7. Job Search Assistance is the predominant activity in "other activities" included in this study.

8. Given the self-selection bias found for adult men in classroom training -- as distinct from other activities -- the conclusion that the greatest earnings impact is found for males in OCT must be qualified somewhat. This apparent earnings premium is related to the type of individual who selects into this activity and who later realizes higher earnings. As explained in Chapter Four, the statistical models used in this study do not account for selection bias across activity types, as none was found to exist for adult women or adult men enrolled in OJT or OA programs.

9. See Geraci and Romero, forthcoming.

10. It may be noted that these estimates have been corrected for selection bias as explained in the previous chapter.

11. A USDOL-OIG report (1988) took the U.S. Department of Labor to task for emphasizing placement activities at the expense of training activities for the economically disadvantaged. The findings in this study suggest JTPA placement-related expenditures appear to be justified for Utah.

12. Consider the hypothetical situation, where a researcher derives 100 samples of JTPA participants from the population. Adult women enrolled in OJT are 10.1% more likely to be employed than others in 97 out of the 100 randomly chosen samples. The higher the probability, the greater the confidence in the estimate.

13. It may be noted that another comparison group which was unavailable to the study at the time may be composed of individuals who are even more similar than those selected for the identified "similar" group. This sub-set of Employment Security participants are those who report themselves as "economically disadvantaged" on the ES intake form. It may be of interest to determine if this sub-set of individuals who is more "similar" to JTPA participants, especially in terms of unobserved factors, results in different program impact estimates.

CHAPTER SIX: Conclusions

Since the Job Training Partnership Act (JTPA) was enacted in 1982, states have been limited in their ability to evaluate the training programs in terms of improving the post-program employment and earnings of participants. States have largely relied upon JTPA's system of performance standards that provides descriptive data on program outcomes and only measures the relative effectiveness of the programs across local Service Delivery Areas (SDAs). Rigorous state-level evaluations of the effectiveness of JTPA as a whole in improving the participants' economic welfare have not been undertaken, with a few exceptions.[1]

Two types of evaluations are possible. One is experimental, where treatment and control groups are randomly selected from among individuals who seek to participate in JTPA programs and are eligible for them. The former receive JTPA services and the latter do not. The employment and earnings of these two groups are later compared to determine if the participants of the programs are more likely to be employed, and if they are employed, whether their earnings are higher. The U.S. Department of Labor (USDOL) undertook such an experiment of JTPA in the mid-1980s.

The second type of evaluation is non-experimental. Instead of having treatment and control groups, this approach uses treatment and comparison groups. The treatment group is comprised of JTPA participants, while the comparison group is selected from a group of non-participants such as Employment Service registrants. With this technique it is critical that both observed and unobserved characteristics of the comparison group are as similar as possible to those of the JTPA participants. If this is not the case, then the program impacts that are estimated could be due to differences between the two groups' characteristics rather than to the program itself (formally termed "selection bias").

States have not undertaken non-experimental evaluations for several reasons. One is that results from this approach, found in the late 1970s for the impact of training programs under the Comprehensive Employment and Training Act (CETA), were not considered reliable due to selection biases.

Second, in an attempt to overcome these problems, the USDOL undertook an evaluation of JTPA using the experimental approach. States have been awaiting the results of the National JTPA Experiment rather than undertaking either their own experiments (which are costly) or their own non-experimental evaluations (which have not been considered to produce reliable results). The problem is that the National JTPA Experiment has not proven to be a panacea for the methodological problems involved in evaluating JTPA, as this report has indicated.

Third, a study demonstrating the feasibility of using non-experimental techniques has been lacking. This report helps fill this gap. It uses statistical techniques and data (data on JTPA participants and on Employment Security registrants linked with their individual Unemployment Insurance (UI) earnings records) that were not available when the earlier non-experimental evaluations of CETA programs were undertaken. Also, advances in statistical evaluation techniques have increased the confidence that may be placed in non-experimental results. Advances in the availability of data and its maintenance over the past decade or so now make it possible for states to apply the non-experimental techniques to assess their programs.

As a cautionary note, and as this study has demonstrated, the results produced by this type of analysis are highly sensitive to the comparison group chosen, the specification of pre-entry work experience, and the technique used to control for selection bias. Clearly, those responsible for generating statistical estimates of program impacts should provide policy makers with the relevant probability levels associated with point estimates. Care should be taken when choosing "similar" comparison groups and when specifying a formulation of statistical program impact models. Finally, it may be desirable to develop a few, clearly described, alternative analyses and associated impact estimates.

In this context, it may be useful for individual states to compare the results of their evaluations with those of other states. While differences will certainly appear -- due to differences in their labor markets and particular groups served, for example -- the comparisons would reveal areas where improvements may be necessary. With these points in mind, states that wish to assess their JTPA program, and yet lack the resources to conduct experimental studies, can conduct non-experimental studies to determine the likely range of estimates of JTPA program impacts.

Given the restrictions of sample size in many states, it may prove worthwhile for states to pool their UI earnings records so that greater statistical precision can be attached to estimated program impacts. In addition, contiguous states might also pool their UI earnings records to capture out-of-state employment of residents.

Major Findings

Perhaps the most important finding from this analysis has been noted above: **It is possible to develop statistically reliable program evaluations for JTPA through the use of non-experimental techniques that rely on earnings records readily available to states.** Given the emphasis on long-term impacts for JTPA participants -- and the benefits that would be expected to accrue from a less costly methodology than random sampling, with all its attendant application difficulties -- the Utah study should be good news for evaluators of JTPA.

Equally important is the finding that in Utah, the specific state in which these analysis techniques were applied, **participants who completed and were placed in unsubsidized jobs (or were retained by their employer in the case of on-the-job training) had a significantly higher likelihood of being employed 2 years after their original enrollment. Both adult women and men were roughly 10% more likely to be employed.**

Regardless of whether the men had been in classroom occupational training, on-the-job training, or "other services" (primarily job search assistance), there was a significantly higher probability that they would be employed 2 years after enrollment than their counterparts who had registered with the Employment Security but had not participated in a JTPA program. For women, however, only on-the-job training was associated with greater employment 2 years after enrollment.

In addition, and especially for men placed into unsubsidized employment, their earnings, were significantly higher -- nearly $1,000 for women and over $1,300 for men.

Both of these findings are important for the future of JTPA, since they point to positive long-term program outcomes in the one case and to a cost-effective means of tracking these outcomes in the other.

Other programmatic findings were also observable. In Utah, at least, JTPA programs have generally been enrolling individuals whose characteristics are assumed to make them "hard-to-serve." For example, among adult women, welfare recipients are more likely than non-recipients to enroll. Similarly, among both adult men and women, school drop-outs are as likely as non-drop-outs to enroll. Given the mandate for serving more school drop-outs in the JTPA Reform Amendments, however, the ratio of drop-outs to non-drop-outs among participants will need to increase in the future.

The results from Utah on employment outcomes are generally consistent with those of other studies. For example, in the National JTPA Experiment, participation in JTPA generally was associated with higher rates of employment 18 months after leaving the program.[2]

Results from an analysis of earnings data yielded somewhat more equivocal results. Although participants showed higher post-program earnings than members of the comparison group, the results on average were not statistically significant at the conventional levels. Within these parameters, however, it can be said that on-the-job training (OJT), and to a somewhat lesser degree occupational classroom training (OCT), appear to raise the earnings of women, if they are employed.

These results on earnings are generally consistent with those of other studies. In particular, on-the-job training appears to have the greatest post-program payoff. Although the results are not strong in the case of Utah, they were quite strong in the National JTPA Experiment.[3]

For men, the Utah results suggest that classroom training may raise their earnings, but again only if they are employed. These results on earnings are not consistent with other findings. In particular, in Utah, men who enrolled in occupational classroom training appeared to have benefitted more than those who enrolled in on-the-job training. In the National JTPA Experiment, the men who were assigned to on-the-job training seemed to derive the most post-program benefits from this activity. By comparison, the men who were assigned to occupational classroom training did not experience gains in post-program earnings compared to their randomly assigned control group counterparts. These inconsistent results for Utah men could be associated with the relatively high unemployment that existed during the period under examination in industries where men are over-represented (e.g., construction and manufacturing). The fact that there is a difference between a state-specific study and a study of sites across the states indicates the importance of assessing JTPA programs within individual states.

Policy Implications

Perhaps the most important policy implication from this study can be derived from the Conference Report on the Job Training Reform Amendments of 1992, which acknowledged the work of the National Commission for Employment Policy in demonstrating "the potential cost savings of using the unemployment insurance wage records" and requiring the Secretary of Labor "to report to Congress on the feasibility of establishing such a database" (p. 140). The use of UI wage records from the State of Utah for this case study of JTPA provides evaluators with the kind of information they need to further refine this evaluation methodology. Additional studies, using UI wage records, should be welcomed to build upon this base.

A second broad policy implication stems from the finding that men and women in general appear to benefit from program participation, with both a greater likelihood of employment and higher reported earnings. Given these positive results, one obvious implication is the need

for administrators at all levels to work diligently to increase the efficiency of JTPA management and the services available to participants. The Reform Amendments of JTPA address these issues in many ways, with much greater emphasis than in the past on administrative controls to be applied at the local level. This report has demonstrated that it is feasible for states to undertake evaluations of their own programs, which can then serve as a basis for program improvements.

Third, with regard to program services -- and in keeping with the finding that on-the-job training has positive results for both men and women (although the Utah study's findings were not as strong as those from the National JTPA Experiment) -- efforts should be made to ensure that on-the-job training contracts written with private employers continue to be an important JTPA activity. Once the concern that these contracts reflect the skill level of the job is addressed (as was done in the 1992 JTPA Reform Amendments), on-the-job training could be even more effective than previously. Similarly, classroom training needs to be focused on jobs for which there is a strong demand.

Endnotes

1. See: Reed (1986), Hanna (1988), and Bowman (1988).

2. See Exhibit S.1, Bloom et al., (1991), p. XXXVI. The report did not provide findings on employment outcomes for the separate training activities. In addition, neither the Nevada nor the Indiana state impact studies derived employment outcomes for their JTPA evaluation.

3. See Exhibit S.6 in Bloom et al., (1992), p. XLV. As discussed in the text, neither the Nevada nor Indiana studies derived separate estimates of impacts by activity type.

APPENDIX A: Experimental Evaluations Of Economically Disadvantaged Adult Programs

As noted in the text, experimental studies of employment and training programs for economically disadvantaged adults have often been characterized by design, implementation, and follow-up complexities, which can introduce statistical bias into estimated program impacts. Unlike laboratory experiments in a tightly controlled environment, field experiments must be operated in the "human environment" characterized by numerous agents acting in different socio-economic and political climates. Many of these differences cannot be observed and controlled in experimentation, so rigid implementation designs become necessary to ensure the differences occur randomly, rather than systematically, across the treatment and control groups. Two large national experiments that targeted economically disadvantaged adults (in addition to out-of-school youth) are described below to indicate how even random assignment experiments may not yield a unique "true" program impact.

The National Supported Work Demonstration

The National Supported Work (NSW) Demonstration targeted adult women who were long-term AFDC recipients, had no children under age 6, and had little if any previous work experience. The demonstration was carried out in 10 sites from 1976 second quarter through 1977 second quarter. The target group of 1,600 adult women was randomly assigned in a one-to-one ratio to either a treatment or control group. The treatment group had to work in either a private or public sector job with close monitoring by the demonstration staff. The control group was precluded from receiving this additional service, leaving them in their same position as before: to find employment on their own if they so chose.

The simple design of this experiment made it easy to maintain the "internal validity" of the demonstration. Only one service was evaluated and only two target groups were identified. The control group was not denied any service for which they otherwise would be eligible. All treatment members participated and the assignment to each group (treatment and control) was purely random. Problems with this experiment came in the evaluation, rather than in the implementation-operation, phase of the demonstration.

Whereas baseline data were filed for all experimentals, follow-up interview survey data were only filed for those individuals for whom they could (1) locate, (2) contact, and (3) obtain a response. Due to resource constraints of the demonstration, the four 9-month periodic follow-up interviews for the AFDC target group were halted after the first survey. A single contact was made in the fall of 1979, occurring from 27 to 44 months after the baseline interview date. By this time 1,185 of the initial experimental group (roughly 75% of the sample) completed the survey, with others omitted for one of the three reasons listed above. As such, the potential for non-response bias came to characterize the program impact estimate. No effort was made to obtain post-baseline earnings for all experimentals from administrative sources, such as Social Security Administration or Internal Revenue Service data files, to validate the accuracy of recall

or to test for non-response bias in the 1979 follow-up. As reported in a later independent study by LaLonde (1986), the final program impact based upon this 1979 survey was estimated at $861 (in 1982 dollars).

It is also interesting to note that later research using different data cohorts resulted in differing experimental program impact estimates for the NSW study. In 1987, Fraker and Maynard derived Social Security Administration (SSA) earnings data for all experimentals and re-estimated the NSW program impact. This time the experimental program impact was reported to be only $351, or $467 in 1982 dollars. Still later, Heckman and Hotz (1989) re-analyzed the data and reported a $267 ($357 in 1982 dollars) experimental program impact. Apparently, the interview data in the original study produced results of a program net impact roughly twice the program impact later reported with SSA earnings data.[1]

The National JTPA Experiment[2]

The National JTPA Experiment (NJE), begun in 1987, is truly a heroic effort to obtain estimates of program impacts.[3] For the first time, a random assignment process was used at sites across the nation in an attempt to estimate overall program impacts, as well as those of separate activities, for economically disadvantaged adult women and men (and out-of-school youth). Whereas NSW was a demonstration involving a new program activity, NJE was a field-based experiment designed to evaluate JTPA as it naturally operates.

<u>18-Month NJE Evaluation Outcomes</u>. Currently, the first of two post-program evaluations of the NJE sites has been completed. A sub-set of the full experimental sample was examined, based on results from an 18-month follow-up survey used to estimate overall program earnings and employment impacts and separate impacts for major program activities.

The 18-month NJE evaluation of economically disadvantaged adults finds positive and strong employment impacts for JTPA as a whole. Adult men enrollees are 4.8% more likely to be employed sometime during the 18-month follow-up period than those in the control group, while adult women are 3.5% more likely to be employed. While both adult women and men realize a positive earnings impact, only the impact for women is statistically significant. Women from the treatment group who enrolled in JTPA are estimated to realize a $873 premium over the 6-quarter post-intervention period (or $582 per year) compared with otherwise similar women assigned to the control group; adult men from the treatment group who enrolled in JTPA experience a slightly higher premium (relative to those assigned to the control group), although the earnings differential is not statistically significant.[4]

In addition, based upon those members of the treatment group originally assigned to one of three major activities, only adults assigned to on-the-job (OJT) training experienced earnings impacts that were statistically significant. Specifically, adult women in the treatment group assigned to OJT were estimated to realize $742 greater earnings over the 18-month period (or $454 per year), while adult men assigned to OJT in the treatment group were estimated to realize a $781 (or $521 per year) premium over otherwise similar males in the control group.[5] Other activity impact estimates for classroom training and a "catch-all" services group are positive but not statistically significant.

As discussed below, these estimates from the first follow-up survey evaluation are derived in a complex programmatic environment, where at times individual choice, rather than random assignment, came to characterize referral, enrollment, and placement decisions on the part of experimental participants and participating SDAs.

The complexities of super-imposing a randomized social experiment onto on-going employment and training programs appear to have introduced biases into estimated program impact estimates. Further, the estimated program impacts from the first follow-up survey are conservative estimates of program performance. Differing types of problems surfaced during the early, intermediate, and final phases of the NJE due to these complexities, including: (1) site selection and design, (2) participant recruitment and activity assignment, and (3) follow-up procedures used with experimental participants.

Early Phase: Site Selection and Design. Initially the experimental design called for Service Delivery Areas (SDAs) to be selected according to probabilistic models that would ensure the national, representative nature of participating SDAs. Unfortunately, the vast majority of sites initially identified chose not to participate.[6] In the first place, since knowledge gained from the evaluation study was considered a "public good," SDAs could learn about the results without having to undergo the difficulties of participation. The marketing of an experimental program based upon altruism fell on the deaf ears of too many Private Industry Councils (PICs), locally elected officials (LEOs), program administrators, service providers, community representatives, and state officials required to reach a consensus for voluntary SDA participation. Second and more importantly, the financial compensation offered to SDAs was related exclusively to the anticipated higher administration costs and hardly matched the expected implicit costs of participating in a random assignment evaluation program, namely: (1) ethical costs, (2) recruitment costs, (3) activity-assignment costs, and (4) performance-standards costs.[7]

On the first point, many operators were opposed to the ethics of assigning eligible persons to JTPA activities on a random basis instead of a staff-determined need basis. This initial reluctance was lessened somewhat once it was understood that JTPA is not an entitlement program in which all eligibles are guaranteed government assistance. Rather, it is a "slot-limited" program where available funds are spent on a first-come-first-served basis.

In weak labor markets where a strong demand for JTPA can exist, a staff-determined rationing device may be no more or less "ethical" than rationing through random selection, since all those eligible have equal chances of participation. In fact, owing to "who one knows" and the "old boy network," one could argue quite persuasively that random assignment is at least as fair as the first-come-first-served system common to JTPA.

The real ethical problem, however, occurs if SDAs are operating in a strong labor market, where jobs are relatively plentiful and in an effort to allocate all program year funding, eligible individuals must be actively recruited by the SDA.[8] Once an interested and eligible individual is recruited, the situation becomes one of leaving actual service receipt to chance rather than being based upon need. SDA staff must now pass along judgements (often face-to-face with a perspective client) which in essence could deny services to those who would have undertaken training in absence of the experiment.[9] At first, the JTPA withholding period was set at 30 months, but was soon reduced to 1.5 years. Even this 18-month period seemed like an eternity to operators who often lose perspective clients if they are not assigned immediately to an available program.[10]

A second concern was that the requirement of a control group could exacerbate recruitment and outreach problems associated with many SDAs.[11] At the outset, the experimental design called for equal numbers of treatment and control group members, but this was soon changed to a two-to-one ratio to reduce the required size of the control group -- including many who otherwise would have enrolled in JTPA.[12] Title II-A funding dollars are at times not easily allocated to the economically disadvantaged, most of whom never contact JTPA for assistance.[13] It is not uncommon for SDAs to have to increase recruitment efforts substantially during the last 1 or 2 quarters of the program year to meet enrollment goals, especially in labor markets with low unemployment rates. It just so happened that during the period of the NJE,

unemployment rates were falling, often quite significantly.[14] The necessity of recruiting a larger intake pool to account for those assigned to the control group exacerbated this worsening situation.

Third, the random assignment process added a degree of inflexibility as to how participants are actually assigned to activities within many SDAs. In particular, the design called for the random assignment of individuals after a service activity was chosen for a participant. This design permits researchers to estimate the impact of a particular program activity in addition to estimating overall program impacts -- both of which are of interest to program operators. As is often the case, however, factors changed between the time an assignment was made and the actual activity commenced. The participant could have changed his or her mind about enrolling in a specific program, or a selected activity was not available once the training was scheduled. Individuals operating at the field level often were opposed to the inflexibility imposed by a once-and-for-all random assignment which was not perceived to be in the best interest of many JTPA participants.

Fourth, the additional burden of identifying a control group (who were excluded from JTPA training) may result in delivering services to a harder-to-serve experimental treatment group, which in turn would harm the SDA's ability to reach or exceed performance standards.[15] Such an outcome would result in SDAs' receiving fewer incentive funds at the end of the program year, funds which many of them had come to rely upon.[16]

The small benefits from participation palled in comparison to the uncertain size of implicit costs to the vast majority of SDAs asked to participate in the NJE. In spite of an altered research design plan and an intense recruiting effort aimed at nearly half of all SDAs, only 7% of the sites considered for the experiment ultimately agreed to participate.[17] The 17 sites chosen were less than the expected 20, with one dropping out almost immediately due to specific problems encountered once random assignment of participants began.

In the end, the 16 sites chosen were similar to non-participating SDAs with regard to most baseline characteristics, including economic conditions, population mix, administrative arrangements, service mix, and program performance. However, most of these 16 sites served significantly smaller numbers of clients than the 488 non-participating SDAs and also were located in small-to- medium sized cities concentrated in the Midwest. In particular, no SDAs from large cities were included in the study.[18]

SDAs chosen in a probabilistic fashion could not be induced to join the experiment and the "external validity" of the National Experiment was weakened. The 16 sites are not truly representative of all SDAs from a statistical viewpoint; and the findings from the NJE should only be generalized to the JTPA system as a whole with extreme caution.

A further problem occurred in the early phase of the experiment. Originally, the research design envisioned assigning all participants to a major program activity such as: classroom training, on-the-job training and job search assistance. Random assignment would then be made and members of the control group would be denied all available JTPA activities and services. This design appeared to be draconian to most SDAs, and a compromise was quickly reached that would structure a "minimal" amount of job search assistance (JSA) up-front in the testing and assessment process before random assignment was made. In this manner, even members of the control group would be guaranteed some JTPA assistance, which would lessen the unease of those sites considering participation in the experiment. Nevertheless, because control group members were not in fact denied all JTPA services, program impact estimates in the National Experiment are biased downward somewhat.

Intermediate Phase: Participant Recruitment, Activity Assignment, and Control Group Training. The initial recruitment design recognized the potential difficulty SDAs might encounter in recruiting sufficient numbers for overall enrollment goals and special target groups, since those assigned to the control group would be excluded from participation during the experiment. Soon after implementation, the one-to-one ratio of treatment to control group assignment was discarded for a two-to-one ratio. This reduced the number of individuals recruited and lessened the number of individuals who would be excluded from JTPA activities during the 18-month experimental period. The actual recruitment problems differed across the 16 SDAs, from some meeting their desired target treatment groups with ease, to others who finally stopped the random assignment process early or who were permitted to assign participants on a higher treatment-to-control group ratio.[19] The problem became especially severe in SDAs which experienced labor markets with low unemployment rates where interested eligible economically disadvantaged forego JTPA for direct entry to the work force, and also in a few SDAs that experienced sudden cut-backs in funding unrelated to the National Experiment.[20]

Recruitment of eligible individuals was also affected in the NJE design by purposefully omitting individuals in a participating SDA whose enrollment could not be left to the random assignment process. Certain OJT employers require a "customized" program for those they refer to JTPA for classroom training activities or other JTPA services, and these employers did not want to hire individuals they did not personally refer. Other state programs for persons with disabilities, or eligible for welfare, require JTPA eligible individuals to participate as a requirement for assistance. These individuals were omitted from the experiment, which weakens the external validity of the experiment. This problem, however, was expected to be minimal since only a small proportion of SDA participants fell into this special group.[21]

A second group of problems came to characterize the intermediate phase of the NJE once individuals were assigned to a JTPA activity. In particular, the initial objective was to develop separate impact estimates for each of the three major JTPA activities along with the overall JTPA impact. These major activities include: (1) Occupational Skills Classroom Training (OCT), which includes those who are mainly assigned to OCT, but who may also be recommended for other "services" (e.g., job search assistance, basic education, etc.), with no more than 20% of their training in on-the-job training programs; (2) On-the-Job Training (OJT), which includes those mainly assigned to an OJT program but who may also be recommended for other "services," with no more than 20% of their training in OCT programs; and (3) Other Activities (OA), which includes those not assigned mainly to a OCT or OJT program, but rather job search assistance (JSA) or other "services" and "custom-tailored" OJT/OCT programs.[22]

However, there were problems with this approach. First, it was not possible to force SDAs to offer only one major activity to each participant, because normal operating procedures often combine a series of activities or services as part of an "Employment Development Plan" (EDP) for a client.[23] In an effort to maintain normal operating procedures in SDAs, the experiment identified three categorical groups of JTPA activities. Job Search Assistance could be combined with more intensive classroom and on-the-job training activities, with the major activity defined as that which accounted for the majority of total training time. While this combination is common to most SDA service delivery, the experiment also permitted classroom and on-the-job training to be combined as long as one activity accounted for at least 80% of the total training time. In addition, the utilization of the last "catch-all" categorical group varied dramatically across SDAs; for purposes of the study, SDAs negotiated to assign from 15% to 70% of all experimental participants to this category. No longer was it possible to estimate the impact of JSA in the absence of more intense training, a frequently offered alternative by SDAs. In the end, combining major training activities/services across the three categories, while maintaining SDA program integrity, eliminated the possibility of estimating enrollee impacts accruing to a major activity -- which was part of the original random assignment design.

The second, and potentially more serious, problem developed as a result of selection into and out of an initial activity assignment. Quite often individuals assigned to a particular activity, or even a combination of activities/services, decide they prefer other activities -- or the SDA staff suggests activity changes due to changes in the financial environment or changes in the availability of specific training curriculum. In either case, the "internal validity" of uniquely assigned programs under random assignment is weakened. Unbiased estimates of program activity can only be derived for assignment to an activity, rather than participation in an activity. While participating staff was expected to hold such changes to a minimum, from 10% to nearly 20% of adults in the treatment group were enrolled in activities different from those originally assigned.[24]

Thus, the random assignment process across activities, which dominated the early design of the NJE, became subject to selectivity biases due to normal JTPA operations. In addition, impact estimates of JSA activity without intensive training -- a commonly used service alternative in most SDAs -- could not be estimated.

Another problem occurred during the activity assignment phase, which involved the amount of activities and services received by those in the treatment group who were not officially enrolled in JTPA. The actual level of services received by treatment group "no-shows" is difficult to ascertain. The 1993 18-month interim report states only that minimal service was given to one-half of a sample of 307 "non-participating" treatment group members, mostly in the form of additional JSA following activity assignment or referrals to OJT employers or classroom training providers.[25] In an earlier report, however, significant amounts of all types of JTPA activities/services were reportedly given to this treatment group sub-set.[26] In fact, an early survey of 192 non-enrollees found that nearly 60% received significant treatment.

The importance of this issue rests on whether or not program impacts should be calculated for assignees or for actual enrollees, since to measure the latter one must divide the assignee program impact by the ratio of treatment group members who enrolled in JTPA.[27] The smaller the proportion of enrollees, the greater the estimated enrollee program impact. Thus if one includes those treatment group members who unofficially participated in JTPA as "enrollees" and their actual services are minimal, the estimated enrollee program impact is biased downward. The amount of training received by enrollees is smaller (i.e. smaller assignee impact) and the proportion of enrollees greater. An upward bias results if the amount of activities and services received unofficially is significant and these individuals are excluded from the enrollee sub-set. In this case, the amount of JTPA activities and services is greater (i.e. greater assignee impact) and the proportion of enrollees is smaller. Apparently, the post-random assignment changes in actual activities discussed above, along with the uncertainty surrounding the count and significance of treatment received, altered the emphasis from enrollee activity impacts in the initial experimental design to assignee activity category impacts in the 1993 NJE interim report. **No longer would the experiment's program impact reflect the returns to training actually received by participants, but rather the returns to individuals initially assigned to grouped activities -- in which roughly 40% never enrolled in JTPA training and nearly 15% of those enrolled received different training than assigned.**

Finally, many individuals assigned to the control group were referred to other subsidized occupational training programs, OJT employers, or job search assistance programs run through employment service agencies, community colleges or the like. In spite of recall difficulties, which may have affected members of the control group surveyed 18 months after initial contact, it appears that a substantial proportion of control group members did, in fact, receive some form of non-JTPA II-A subsidized training or employment service.

The most obvious form of such non-JTPA service is job search assistance (including job placement assistance), readily available through local employment service offices and

community colleges. As such, estimated program impacts in the NJE include some treatment members who received mainly JSA -- which could amount to only small increments, if any, in this activity over and above those in the control group.

A similar situation existed in the NJE regarding classroom training for occupational skills. Whereas 29.4% of adult women and 17.4% of adult men assigned to the treatment group later enrolled in classroom training, control group members responding to the 18- month follow-up survey indicated that 20.4% of the adult women and 12.9% of the adult men enrolled in similar non-JTPA funded classroom programs. These figures indicate that enrollment rates of those assigned to the control group were from 69% (i.e. adult women) to 74% (i.e. adult men) that of the enrollment rates of those assigned to the treatment group.[28]

Accordingly, all "program impact" estimates in the NJE must be viewed as **incremental returns to the availability of JTPA training relative to non-JTPA funded training -- not relative to an absence of government subsidized training**. This fact is especially important to an experimental program that denies services to the control group. Extra referral efforts by the JTPA staff to control group members could be expected if the latter were informed they would not be permitted to receive any further JTPA assistance for the next 18 months. To the extent this occurred, all JTPA program impact estimates are likely to be biased downwards compared to the typical case where little if any recruitment and referral services are given to JTPA applicants who, after being contacted, do not enroll in training.

Follow-up Phase: Internal Validity. Even a perfectly designed random assignment experiment cannot yield valid results if those contacted during the follow-up. post-intervention period differ systematically from those included in the initial experiment. As before, this discussion focuses on adults in the experiment since JTPA's Title II adult program is the subject of this report.

According to the NJE interim report, 14,905 adults filed a four page Background Information Form (BIF), which detailed their work history and personal background characteristics prior to assignment. Complex statistical analysis of the randomly assigned treatment and control groups found the two to be identical in all observed personal and prior work history characteristics.[29] As shown in Table A.1 below, 18 months later in-person or telephone interviews were completed for a follow-up sub-set of 10,144 adult treatment and control group assignees. Thus, over 30% of the full experimental sample had either been omitted because insufficient time had elapsed since the initially scheduled assignment date or because participants failed to respond to either form of follow-up interview.

While the NJE interim report does not explicitly show how the analytical samples used to estimate program impacts for separate activities were derived, one can arrive at the final sample sizes by cross-referring tables in the earlier baseline report against the 18-month NJE interim report. The derivation of the analysis sample is shown in Table A.1.[30] Of adult men, nearly one-third were omitted: 17.8% were excluded from the 18-month follow-up because their initial assignment date occurred late during the 1987-1989 experiment period, which precluded them from having a full 18-month follow-up period. Another 17.6% did not respond to the interview. Similarly, nearly 30% of adult women were omitted from the 18-month follow-up: 18% were excluded from the survey and 10.9% failed to respond to the survey.

This is a sizeable reduction in the experimental population. The possibility of drawing misleading conclusions from an analytical sub-sample that does not reflect that of the full experiment was not addressed fully and forthrightly in the NJE. However, such a discussion would be necessary to dispel concerns as to the internal validity of the study results.

The analysis sample -- used to generate program impact estimates -- was not compared directly with the full experimental sample. The distributions of the population and the sub-set need to

TABLE A.1

DERIVATION OF ADULT ANALYSIS SAMPLE FROM FULL EXPERIMENT BY ACTIVITY AND GENDER GROUPS

GENDER-ACTIVITY SUB-SET:	FULL EXPERIMENT SAMPLE	EXCLUDED FROM 18 MONTH SURVEY		NON-RESPONDENTS TO 18 MONTH SURVEY		IMPACT ANALYSIS SAMPLE	
	[NO]	[NO]	[PCT]	[NO]	[PCT]	[NO]	[PCT]
ADULT MEN:							
OCT	1586	233	14.7	296	18.7	1057	66.6
OJT/JSA	3182	428	13.5	504	15.8	2250	70.7
OA	2080	561	27.0	407	19.6	1112	53.5
SUB-TOTAL	6848	1222	17.8	1207	17.6	4419	64.5
ADULT WOMEN:							
OCT	3409	482	14.1	NA	NA	(2847)	NA
OJT/JSA	2660	338	12.7	NA	NA	(2287)	NA
OA	1988	630	31.7	NA	NA	(1340)	NA
SUB-TOTAL	*8057	1450	18.0	882 (133)	10.9 (01.7)	5725 (6474)	71.7 (80.4)
TOTAL	14905	2672	17.9	2089	14.0	10,144 (10,893)	68.1 (73.1)

* Figures in parentheses refer to the number of cases that were omitted from the 18-month follow-up survey after 549 non-respondents whose post-assignment UI earnings were used to approximate the follow-up survey earnings, and thus reduced the number of adult women who would otherwise have been excluded from the analysis sample.

SOURCE: Figures calculated from tables in Bloom (1991) and Bloom et al. (1993).

be compared with regard to personal demographics, program participation, and pre-assignment work histories. In addition, the 18-month interim report pooled data across 16 separate sites that were not chosen randomly. Since the sites differed greatly, the distribution of individuals by sites between the population and the analysis sub-set needs also to be discussed. The only comparisons made in the 18-month interim report addressed the response bias issue for the two genders between the 18-month survey group (which excluded 18% of the population because of late assignment) and the analysis sample (which excluded another 14% due to non-response). Non-response bias was not found in the post-entry earnings analysis for adult men, but was found for adult women. The latter was corrected by using follow-up Unemployment Insurance earnings data in place of the follow-up survey earnings for those who did not respond to the survey instrument.[31]

What is of importance, however, is the extent to which the analysis sample may differ from the full experimental sample, which was never addressed in the NJE. Implicit in this oversight is the assumption that the first exclusion (due to late assignment during the experiment and accounting for 18% of the 30% omitted individuals) results in the same distribution of individuals in the follow-up as that which would occur randomly. However, Table A.1 suggests that this may not have happened, since the final (known) distributions of the analysis sample at times appear to differ from that of the full experiment. For example, adult women account for a greater proportion of the analysis sample (6,474/10,893, or 59%) than in the full

experimental population (8,057/14,905, or 54%). Second, individuals assigned to "other services" are under-represented in the analysis sample for both genders. Adult men assigned to the less intensive training activity account for only 25% (1,112/4,419) of the analysis sample, but 31% (2,080/6,848) of the full experimental population (the adult women figures are 21% and 25% respectively). These differences are not meant to imply that significant differences exist between the analysis sample and the full JTPA experimental population, but only to indicate that this issue should be addressed directly in the NJE before the results are taken to be internally valid.

Summary

In summary, the original NJE research design was altered once the experiment was superimposed upon existing SDA program operations at the 16 sites that volunteered to participate in the random assignment experiment. At all three phases of the experiment -- site selection, program implementation, and follow-up evaluation -- program complexities introduced potential for impact estimates of the total program and of specific activities to deviate from what may have been anticipated in the original design stage. While the extent of these potential biases is unknown at present, the biases must be acknowledged before the experiment's findings are interpreted as the final answer regarding the effectiveness of JTPA for economically disadvantaged adults.[32]

Even though the 16 sites displayed characteristics common to many SDAs, they were not randomly chosen and were not statistically representative of all SDAs. While these criticisms do not imply that the 18-month interim report results are meaningless, they do suggest that any generalization from this follow-up evaluation should be highly discouraged.

Further, and to varying degrees, the original research design was compromised during the implementation phase in several ways. All participants in the experiment – treatment and control group -- received minimal JTPA services directly. SDA staff's referral efforts gave assistance to control group members to help soften the blow of being denied JTPA assistance for the next one and one-half years. Furthermore, the ability to estimate program impacts for receipt of services by specific major training activities was weakened as some treatment group members changed their minds about the activities to which they had been assigned, and enrolled in others or none at all. Over one-third of individuals assigned to the treatment group never enrolled, and roughly 15% of those that did enroll participated in activities differing substantially from that initially assigned. No information is given regarding the proportion of enrollees who completed JTPA. Accordingly, the national experiment altered its initial focus from the acquisition of training to the assignment to training. It is little wonder then, that the National JTPA Experiment program impacts are relatively low, and often statistically insignificant.

The final categorization of activities combines individuals with varying amounts of training. Basic education for high school drop-outs is included with classroom training for occupational skills, and JSA is eliminated as a separate category. This basic service component is highly characteristic of JTPA service delivery; yet it cannot be analyzed within the final research design. In the end, only the total program impact estimates for assignees are put forward for determining whether or not JTPA "works."[33]

It is important to emphasize how one may misinterpret the impact estimates of the NJE. Whereas most readers may want to know whether or not federally funded training programs improve the earnings and employment of the economically disadvantaged compared to what

they would experience without such government assistance, the NJE answers a different question. In particular, the reader is given conservative estimates of the earnings and employment impacts of recommending a particular JTPA activity/service compared to recommending similar services provided by non-JTPA providers. While this difference in "recommended services" is most evident with respect to on-the-job training alternatives (few provided outside of JTPA), it is less evident with respect to classroom training, which is widely available under non-JTPA funding. In addition, less costly forms of job search assistance comprise the most readily available form of non-JTPA employment assistance.

Endnotes

1. A recent study by Couch (1992) undertook an 8-year follow-up of the experimental population and found significant earnings impacts each year, varying from $250 to $525. These earnings premiums covered program costs resulting in a small, but positive internal rate of return. Later earnings gains would increasingly improve the cost-benefit internal rate of return from this social experiment.

2. The majority of information reported in this section has been taken from four early reports on the National JTPA Experiment -- including: Doolittle and Traeger (1990); Orr, Gueron and Bloom (1990); Bloom (1991) -- and the 18-month interim report, Bloom et al., (1993).

3. Classical experiments of JTPA using random assignment were developed by USDOL for dislocated worker programs during the early 1980s. The range of program impact estimates and issues regarding follow-up procedures are summarized in Bowman (1986).

4. These figures are taken from Bloom et al. (1993), Exhibit S.1, p. XXXVI. It may be noted the "earnings impact" reported in the NJE is derived from the post-assignment earnings of all treatment and control group members, including those with zero earnings over the observed period. As such, this impact measure combines both an employment effect (i.e. probability if employed) and an earnings effect (if employed). These two effects were estimated separately in this Utah Title II-A adult study.

5. These figures are taken from Bloom, et al. (1993), Exhibit S.6, p. XLV.

6. In the first 5 months of site selection, none of the top 73 priority sites in the probabilistic sample agreed to participate given the original research design (Doolittle and Traeger (1990), p. 61).

7. Sites that agreed to participate in the national experiment were paid a negotiated fee for higher administration costs related to participation which ranged from $40,000 to $170,000 (Doolittle and Traeger (1990), p. 51).

8. Active recruitment by an SDA may also reflect a lack of information of JTPA to the eligible population or a lack of quality programming.

9. Over one-half of SDAs contacted for the study listed the ethical problem of random assignment as a major concern (Doolittle and Traeger (1990), p. 37).

10. Only 25% of those who voluntarily inquire about JTPA services are expected to enroll in an activity (Doolittle and Traeger (1990), p.50). For those who must be recruited, the expected enrollment rate would be even lower. Another related problem to the random assignment process is the potential to delay assignment from the initial point of contact. It is estimated approximately one-half of the remaining JTPA applicant pool will be lost for each additional visit required during the pre-enrollment period. These delays are often used by SDAs to "select out" less motivated applicants (Doolittle and Traeger (1990), p. 50).

11. Almost one-half of SDAs contacted for the NJE feared the study could seriously impact their ability to meet state-required enrollment goals (Doolittle and Traeger (1990), p. 37).

12. A side-effect of this policy change was to reduce somewhat the statistical confidence of program impacts resulting from a deviation from the optimal one-to-one ratio of treatment to

control group members without a corresponding increase in sample size. See Cave (1987) for more details.

13. Roughly 90% to 95% of those considered eligible for Title II-A are not serviced by JTPA (Doolittle and Traeger (1990), p. 38).

14. Of the 16 SDAs selected for the study, 11 experienced falling unemployment averaging 2 percentage points, while the remaining 5 experienced constant or slightly rising unemployment (Doolittle and Traeger (1990), p. 65).

15. One-fourth of SDAs contacted for the NJE cited this as a major concern for participation (Doolittle and Traeger (1990), p. 34).

16. Five SDAs requested that adjustments be made to their performance standards due to their participation in the experiment, of which 4 were given permission to make adjustments to be "held harmless" for program participation (Doolittle and Traeger (1990), p. 70).

17. The research design was changed to ease recruitment efforts in three phases, with the initial phase resulting in only 2% acceptance of 48 sites considered. During the next 4 months the recruiting effort raised the acceptance rate to 5% for 61 sites considered, finally reaching a 10% acceptance rate among the 89 SDAs considered. In all, 229 SDAs were considered with 16 accepting, 170 rejecting, and 43 being dropped from consideration for various reasons (Doolittle and Traeger (1990), p. 92).

18. A discussion of the similarity and differences found among baseline characteristics are found in Doolittle and Traeger (1990), Chapter Five and in Bloom et al. (1991), Chapter Three. It may be noted, the largest SDA in Utah, Wasatch-Front South, was heavily recruited for the experiment but declined to participate.

19. Five smaller sites increased the treatment/control ratio, whereby 511 adults were assigned under a 3:1 ratio and 202 adults were assigned under a 6:1 ratio. Another site ended random assignment early due to a large plant shutdown, which imposed heavy workloads on the JTPA staff that could not be accommodated under the experimental work conditions (Bloom (1991),p. 78).

20. The national unemployment rate fell from 7.0% to 6.2% between the time the experiment began and the start of random assignment (Doolittle and Traeger (1990), p. 64).

21. An additional problem of external validity characterized the research design as eligible individuals contacted in the experiment either refused to fill out the four page Basic Information Form (BIF) or who filled out the form but were never randomly assigned (Doolittle and Traeger (1990), p. 127). The extent of this problem is uncertain, however, as they may have been less motivated to participate/work or may have had better employment opportunities than could be expected under a chance of being assigned to a JTPA training activity. This problem may be small, however, as relatively few failed to complete the BIF.

22. As noted in the text, the emphasis here is on the major enrollment activity, as all participants may be exposed to minimal supportive services and even brief job search assistance.

23. On average, 75% of JTPA participants enroll in only a single activity or service (Doolittle and Traeger (1990), p. 78).

24. See: Bloom et al. (1993), Exhibit 3.19, p. 69.

25. See: Bloom et al. (1993), Exhibit F.1, p. 372.

26. See: Orr, Gueron and Bloom (1990) p. 51.

27. See Bloom (1984) for an explanation of this statistical methodology.

28. These figures are calculated from Bloom et al. (1993), Exhibit 5.7, p. 148 for men and Exhibit 4.8, p. 93 for women.

29. See: Bloom (1991), Appendix D.

30. The figures in the table were derived from internal Abt Associates reports. It may be noted the interim report did not indicate the activity assignment of the 882 adult women who failed to respond to the 18-month follow-up survey. Of this total, 749 had UI earnings records that were used as a proxy for the 18-month follow-up earnings. As such, the 18-month survey of 6,607 women was reduced by only 133 cases (882 non-respondents less the 749 who had UI earnings records), resulting in an analytical sample of 6,474 women.

31. According to Bloom et al. (1993), Exhibit D.3, p. 342 -- program impact estimates for adult women were biased upwards by 20% ($645 program impact estimated with UI wages of follow-up respondents only as compared with a $539 program impact estimated with UI wages of both follow-up respondents and non-respondents).
It may also be noted the follow-up survey quarterly earnings used in the interim report were based upon respondents having to recall their total earnings on a quarterly basis for the 6 quarters following assignment. No analysis of the comparability between survey-based data and the UI earnings data was given in the 18-month interim report.

32. Social experimentation evaluation methodologies have been criticized in the literature, including Cook and Campbell (1979) and Hausman and Wise (1985), and more recently by Heckman (1992) and Levitan (1992).

33. It has been argued that current randomized experiments like the National JTPA Experiment do not offer insight into the specific mechanisms by which social programs work, and as such only yield a "black box" answer as to whether or not social programs are effective in the short-run. (See: Heckman (1992).)

APPENDIX B: Estimating Selection Bias Of Non-Experimental Models

As discussed in the text, the general consensus regarding estimates of non-experimental impacts of training programs for the economically disadvantaged has been that they are highly variable in size and are unlikely to approximate the "true" value, which can only be known through random assignment field experiments.[1] Non-experimental net impact studies use comparison (rather than control) groups drawn from large-scale external data bases; the comparison groups are composed of individuals "similar" with respect to observed demographics and prior work histories of the targeted treatment groups. They cannot, however, ensure that unobserved factors related to program participation are unrelated to later program earnings, which biases program impact estimates due to the non-random selection process. The wide variability in estimates of program impacts for the economically disadvantaged target groups is thought to exemplify the problem of selection bias.

The only way to "prove" if impact estimates from a non-experimental (treatment versus comparison group) study differ significantly from an experimental (treatment versus control group) study is to evaluate the outcomes of a single treatment group with both comparison and control groups. This rare opportunity existed with the National Supported Work (NSW) study which served both high risk youths and long term welfare mothers without young children and who lacked prior work experience.[2] Following the national demonstration, researchers made such comparisons using different comparison groups and different modeling specifications to estimate the selection bias of non-experimental program impact models.[3]

Later, a series of statistical tests were developed to identify and eliminate from consideration non-experimental program impact estimates, which are likely to biased.[4] The findings suggest that non-experimental results can duplicate experimental results for those cases which pass a series of selection bias tests. To the extent this is true, non-experimental procedures may become valid evaluation tools our nation's employment and training programs.

The Lalonde Study

In the first study, LaLonde derived alternative comparison groups from random samples of adult female household heads taken from the Panel Study of Income Dynamics (PSID) data set (595 observations) and the March 1976 Current Population Survey (CPS) data set (11,132 individuals). Annual earnings from Social Security Administration (SSA) were appended to the CPS data set, while internal annual earnings figures were used for the PSID data set.

A weakness in choosing these samples was the relatively small number of AFDC participants. The PSID sample was composed of only 30% AFDC recipients (173 adult women), while the CPS sample contained only 2% AFDC recipients (241 adult women). Because there were so few welfare recipients, the pre- and post-enrollment earnings of these comparison groups were significantly higher than the experimental controls, reaching ten times the level in the pre-enrollment periods. In addition, while the characteristic pre-enrollment "dip" in earnings of economically disadvantaged adults was observed for the treatment group (-26%), a small

positive pre-enrollment earnings gain was observed for both comparison groups (+2% and +9% for PSID and CPS respectively).

The result of comparing a highly dissimilar set of comparison groups to the experimental treatment group was very dissimilar impact estimates. The program impact from the interview surveys of experimentals (treatment versus control groups) was $861 (in 1982 dollars) for 1979. The estimated impacts (following a non-experimental design) for even the sub-set of comparison group members who were AFDC recipients varied from $1,500 to $2,400 for the PSID sample and from not significant (but positive) to $2,000 for the CPS sample. Thus the non-experimental design estimates in the study were found to be significantly higher and they varied to a large degree from those reported in the experimental design. The reported difference in estimated impacts of the experiment and the non-experimental estimates of program impacts were taken by the author as proof that non-experimental studies should not be relied upon for policy considerations.

Before accepting these difference in program impacts as evidence that non-experimental studies, in general, can not be relied upon to yield unbiased estimates, it is important to point out two fundamental flaws in the Lalonde study. These flaws, if corrected, could result in more stable and unbiased estimates of non-experimental program impacts.

First, a response bias in reported post-program earnings was introduced when only 75% of the experimental treatment groups responded to the follow-up survey, compared with the full sample of comparison group members whose earnings were taken from SSA records.[5] In essence, post-program earnings were not observed for 1 in 4 members of the treatment group, but earnings were observed for virtually all those in the comparison group. If treatment group non-respondents were less likely to be employed or working full time than the respondents, then one would expect the program impacts to be biased upwards, as only the "more employable" members of the treatment group are used in the statistical analysis.

Secondly, it may be argued that LaLonde used an inappropriate statistical technique to remove the selection bias problem in models which were run on the sub-set of AFDC recipients. Following earlier evaluation designs of Comprehensive Employment and Training Act programs, he specified the net impact as the change in earnings between 1975 and 1979 rather than the level of earnings in 1979. The reason for this specification is that unobserved factors which may cause selection bias would be eliminated if they have a constant, or fixed, effect on earnings in any period.[6] If one specifies the program impact as the change in earnings, these "fixed effects" cancel out.[7]

To see this more clearly, assume, for example, that adult women who have greater motivation to work will earn more money in any given year regardless of program participation, and will also be more likely to take part in the supported work study program than women without such motivation. If a simple comparison is made of the level of earnings of these two groups, failure to account for this unobserved factor (motivation) will bias the program impact upwards. Not all of the earnings gain would be due to the program itself; rather some of the earnings gain would be due to the omitted variable, motivation, which is correlated with observed program participation in the model. The total impact of program participation in the model measures both the indirect impact of the omitted motivation factor (working through participation) as well as the direct impact of observed participation.

By specifying a change in earnings, however, the fixed effect of motivation on each year's level of earnings cancels out. (If more highly motivated adults earn $1,000 more each year, then the impact of motivation from one year to the next is zero.) As such, the omitted factor, motivation, should not be correlated with either program participation or the change in earnings. Selection bias would not be a problem in estimates of program impacts.

A problem with LaLonde's approach for the AFDC sub-set is that he was forced to use the pre-entry year as the base year in his calculations. As noted earlier, economically disadvantaged adults typically display a "dip" in earnings during this period, which reflects mainly a transitory component (e.g., "bad luck") of their underlying pre-entry earnings process.[8] Following enrollment, participants' earnings reflect both a return to their more permanent trend rate of earnings growth and the impact of receipt of training. Thus the impact of the unobserved factor, motivation, on the transitory level of earnings in the immediate pre-program period can not be assumed to equal its impact on the more permanent level of post-program earnings. By specifying the program outcome as the change in earnings for this sub-set of AFDC recipients, LaLonde did not remove the selection bias inherent in the model structure. In this case even "fixed effects" program impact estimates for the AFDC sub-set would be biased upward.[9]

In addition to the examination of the AFDC sub-set of recipients, LaLonde specified a more complex two-step earnings model on the full sample of PSID and CPS comparison group members. In the first step, the probability of program participation was modeled and in the second step this information was used in a model of post-program earnings. This more complex statistical technique identified a negative correlation between unobserved factors related to program participation and post-program earnings.[10] Such a strong negative correlation would cause the "fixed effects" model to bias program impacts upwards, as adults who are more likely to participate are less likely to realize higher permanent earnings. This, along with the measurement error (introduced by specifying the base year as the first year prior to entry), caused the non-experimental program impact estimates to exceed the "true" values reported in his study when using the PSID and CPS comparison groups. In addition, the more complex models found the estimates of program impacts to be far less biased than those derived from the "fixed effects" models estimated for the smaller AFDC sub-sets.

In summary, the LaLonde study is relevant to this report for two reasons. First, it is indicative of the importance of carefully choosing a comparison group for non-experimental studies. By choosing comparison groups that are highly dissimilar to the treatment groups on important observed factors, it appears nearly impossible to account for the dissimilarities among non-observed, omitted variables which affect program participation and post-program earnings. Program impacts estimated with poorly designed comparison groups are likely to be characterized by selection bias.

Secondly, selection bias can be reduced significantly through proper modeling specification. It appears that using a two-step procedure to model program participation and post-program earnings explicitly is less restrictive, and may reduce selection bias more than by altering the post-program earnings variables as is done with "fixed effects" models. This is especially true, when the length of employment and earnings records is constrained to the immediate year prior to enrollment.

The Fraker And Maynard Study

In 1987 Fraker and Maynard, who had both worked on the NSW evaluation team, undertook an independent comparative study of the non-experimental with the experimental program impact estimates for the NSW demonstration study. This study is important more for the ways in which comparison groups were selected than in the types of analytical models used for estimating program impacts in non-experimental studies.

The authors derived six separate comparison groups from the March 1976 and 1977 CPS data sets, resulting in a sample of 1,995 AFDC adult women welfare recipients, of whom 909 had

children six years of age or older. The six comparison groups were composed of various sub-sets of the full AFDC sample of women; the groups differed due to successive screens on observed characteristics to make the comparison group appear more similar to the control group of the NSW experimental demonstration. Most comparison group sub-sets were composed of from 600 to 900 individuals compared to the 800 individuals in the original NSW treatment group. SSA annual earnings were then appended to the CPS records for all experimentals and comparison group members.

Using a relatively simple one-step model to estimate program impacts, the authors found significantly different results across the six comparison groups. According to the original study, in 1979 the program impact was estimated to be $351. (This figure is based on SSA data and is stated in current year dollars as opposed to 1982 dollars used in the LaLonde study.) In contrast, the non-experimental program impacts estimated by Fraker and Maynard varied from positive, but not statistically significant, to $806 across the comparison groups. Clearly, the choice of comparison groups matters.

The authors also computed program impacts for successive "years" (i.e. 4- quarter segments based upon the baseline interview date) following the initial baseline interview. Of interest in these findings are the "fixed effects" model estimates as compared to the estimates of the experimentals. The estimates of program impacts declined from $1,243 to $349 over each of the three successive years following the entry year. The "fixed effects" estimates were roughly $100 higher each year, declining from $1,330 to $438. In the fourth post-entry year, both estimates indicated that there were no longer any meaningful, or statistically significant, program impacts.

This study shows that improved corrections for selection bias (via "fixed effects" models in this case) can yield estimates of program impacts that are relatively close approximations to the impacts estimated with experimental designs. One can only wonder what the non-experimental impact estimates would have been, had the authors used the two-step selection correction technique pioneered by Heckman (which models program participation explicitly) rather than the "fixed effects" technique (which only transforms the model's dependent earnings variable from the level to the change in pre- to post-enrollment earnings).

The Heckman And Hotz Study

Later in 1989, Heckman and Hotz returned to the NSW experiment to apply newly developed selection bias tests to determine if properly specified non-experimental models could duplicate the experimental net impact results.[11] Once again, they were forced to rely on the CPS national data base to derive a sample of 18 to 64 year-old AFDC recipients with dependents under age 16. As before, the comparison groups differed from the treatment groups significantly. Whereas the pre-entry earnings of the treatment group fell by 35%, the earnings of the comparison group rose by 22%. The comparison group was composed of 10 times the proportion of whites, were 2 years younger on average, had more dependents, and half were not enrolled during the same period as those of the treatment NSW group. In addition, the comparison group had greater prior work experience: 4 times the number of weeks worked and 6 times the number of hours worked per week.

The diverse members of the comparison group, along with numerous variations of non-experimental modeling specifications, resulted in 64 different models. Like the non-experimental models estimated for CETA in the 1980s, program impact estimates across these models varied widely -- from -$1,400 to +$1,400.

Out of all the models fitted, however, 14 were found to pass various selection bias tests developed to determine if the earnings processes prior to and following intervention were similar -- and thus to reduce significantly the chances that the non-experimental estimates were biased. As summarized below in Table B.1, the average non-experimental program net impact estimates for this sub-set of estimates were valid approximations of the experimental outcomes. For 1978, the average non-experimental estimate of $374 was 85% of the experimental estimate ($440), and for 1979 the average non-experimental estimate of $238 was 89% of the experimental estimate ($267).[12] Heckman and Hotz were able to show that the wide variation of non-experimental program impact estimates could be reduced significantly by subjecting alternative models to various selection bias tests, and that the resulting non-experimental estimates act as good approximations of the random assignment experimental results.[13]

Table B.1*

NSW PROGRAM IMPACT ESTIMATES	1978	1979
EXPERIMENTAL RESULTS	+$440 (142)	+$267 (162)
WEIGHTED AVERAGE OF LCF MODELS THAT PASSED SELECTION BIAS TESTS	+$374 (146)	+$238 (152)

* Standard errors of estimate given in parentheses.

Summary

In summary, the two studies of the NSW demonstration program, which attempted to estimate the selection bias inherent in non-experimental studies, are important more for the issues raised than the results produced. Even though the non-experimental designs lacked appropriate comparison groups and sufficient information regarding pre-enrollment employment and earnings histories, they did validate the importance of well designed non-experimental studies in order to minimize expected selection bias. These design features are important to this Utah study since alternative comparison groups and modeling techniques are examined for the importance each plays in estimates of program impacts for Utah's economically disadvantaged adults.

Finally, it is important to emphasize that the more recent work of Heckman and Hotz has shown it is possible to apply increasingly stringent selection bias tests to non-experimental models to help reduce the likelihood of selection bias. With improved pre- and post-intervention earnings measures, use of these selection bias tests appears to yield impact estimates that closely approximate experimental outcomes. Furthermore, non-experimental evaluations can be constructed within a year or two of program completion, as opposed to the field experiments that can take an additional 4 to 6 years between initial design and ultimate completion. Because of the far greater costs of designing, implementing, operating, and evaluating follow-up surveys in field experiments -- evaluating existing programs using non-experimental designs with readily available administrative data appears to be a cost-effective evaluation alternative for states.

Endnotes

1. Econometricians classify such estimators as "inconsistent" and "biased" because of modeling specification errors. That is, program impact estimates will vary from study to study without a general tendency for the estimates to converge to the "true" program impact, even with large sized samples, if the program participation and post-program outcome processes are not correctly specified. This may result, for example if the studies: (1) omit important variables, (2) incorrectly measure important variables, or (3) fail to account for the inter-relation of the two processes.

2. The original design of the National JTPA Experiment called for the identification of both non-randomly chosen comparison and randomly chosen control groups for the purpose of measuring the extent of selection bias in JTPA impact studies. Due to cost constraints, USDOL later reduced the amount of resources dedicated to the derivation of comparison groups which could be used to determine if non-experimental procedures could duplicate the experimental outcomes.

3. See: LaLonde (1986) and Fraker and Maynard (1987).

4. Heckman and Hotz (1989).

5. It may be noted that in the study, the author only discussed the possibility of response bias between the experimental treatment and control group members. This discussion led him to conclude that only a small bias should be expected as the difference in response rates between the two experimental groups were marginal.

6. It may be noted this result rests upon the assumption that unobserved personal characteristics, say motivation, have a constant effect on pre- and post-intervention earnings. This assumption may not hold true if a major intervention service in JTPA is the alteration of individuals' motivation to find work and to improve their skills to earn higher wages in jobs they find. Specific counseling services, in fact, are directed toward this particular characteristic for individuals with weak prior labor force attachment. They may be as important, if not more important, than the actual occupational skills acquired in the employment and training programs.

7. See: Bassi (1984) and Ashenfelter and Card (1985).

8. Ashenfelter (1978) and Ashenfelter and Card (1985) suggest using an earlier period (or series of years) as the base year for fixed effects models, as it will more likely represent a more permanent component of the underlying pre-entry earnings structure.

9. In a follow-on study (1989), Heckman and Hotz subjected this fixed effects model to stringent statistical tests and found it to be characterized by selection bias.

10. These models were pioneered by Heckman (1979), and since have become widely used in labor economic literature. In Barnow, Cain, and Goldberger (1980) they were specifically identified as being useful for non-experimental program impact analysis.

11. These tests are an outgrowth of earlier work. See for example: Heckman and Robb (1985).

12. These weighted averages are compiled for the group of Linear Control Function (LCF) models applicable to grouped data. Other fixed effects models were developed, but did not

perform as well as the LCF variants. Since individual earnings data was not available, non-linear Heckman-type models were not fit for the NSW sample.

13. A later study by Friedlander and Robbins (1992) applied the LCF model to individual data to estimate program impact on the binary outcome measure of employment status (1 = employed, 0 = not-employed) for a group of experimental sites for mostly single female AFDC parents. Their results were very mixed, showing a wide variation of impact estimates for even those models which passed the chosen selection bias tests. It may be noted, however, that the authors did not attempt to use either fixed effects models or more complex two-step non-linear models that could have been tested given the nature of the data. In addition, the outcome measure was binary, and not continuous as is the case for the earnings models developed by Heckman and Hotz.

APPENDIX C: Pre-Entry Earnings Models And Selection Bias Tests

REGRESSION DESIGNS USED TO TEST FOR SELECTION BIAS IN JTPA IMPACT MODELS

	SELECTION BIAS MODEL				
	CMT #1	LCF #2	2SLS #3	FEM #4	HSM #5
DEPENDENT VARIABLE:					
PRIOR EARNINGS	Y	Y	Y	N	Y
CHANGE IN PRIOR EARNINGS	N	N	N	Y	N
INDEPENDENT VARIABLE:					
JTPA ENROLLMENT	Y	Y	Y	N	Y
JTPA ENROLLMENT PROBABILITY	N	N	N	Y	N
AGE (& AGE SQUARED)	Y	Y	Y	Y	Y
SCHOOL DROP OUT	Y	Y	Y	Y	Y
BEYOND HIGH SCHOOL	Y	Y	Y	Y	Y
BLACK	Y	Y	Y	Y	Y
HISPANIC	Y	Y	Y	Y	Y
OTHER MINORITY	Y	Y	Y	Y	Y
WELFARE RECIPIENT	Y	Y	Y	Y	Y
RURAL SDA	Y	Y	Y	Y	Y
INDUSTRY IN 2ND PRIOR YR:					
AGRICULTURE, MINING, & CONSTRUCTION	Y	Y	Y	N	Y
MANUFACTURING, FINANCE, INSURANCE & REAL ESTATE	Y	Y	Y	N	Y
WHOLESALE-RETAIL TRADE, SERVICES	Y	Y	Y	N	Y
EARNINGS IN 2ND PRIOR YEAR:					
1ST 6 MONTH EARNINGS	Y	Y	Y	N	Y
CHANGE IN 1ST TO 2ND 6 MONTHS	Y	Y	Y	Y	Y

CMT = Cell Matching Technique Model #1.
LCF = Linear Control Function Model #2.
2SLS = Two Stage Least Squares Model #3.
FEM = Fixed Effects Model #4.
HSM = Heckman Selection Model #5.

Y = Yes, included in model.
N = No, not included inmodel.

APPENDIX D: Quasi-Net Impact Employment And Earnings Models

TABLE D.1

LINEAR PROBABILITY MODEL OF EMPLOYMENT IN SECOND YEAR FOLLOWING ENROLLMENT CONDITIONED UPON JTPA PARTICIPATION: ADULT WOMEN

EXPLANATORY VARIABLE	AVERAGE JTPA MODEL		ACTIVITY TYPE MODEL		TERMINATION STATUS MODEL	
	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)
JTPA AVERAGE	+.051	(1.54)	NA	-	NA	-
ACTIVITITY TYPE:						
CLASSROOM TRAINING	NA	-	+.052	(1.44)	NA	-
ON-THE-JOB TRAINING	NA	-	+.101	(2.18)	NA	-
OTHER ACTIVITIES	NA	-	-.040	(1.07)	NA	-
TERMINATION STATUS:						
COMPLETE/PLACED	NA	-	NA	-	+.087	(2.19)
COMPLETE/NOT PLACED	NA	-	NA	-	-.066	(1.00)
NOT COMPLETE	NA	-	NA	-	-.046	(1.16)
WELFARE RECIPIENT	-.009	(0.24)	-.009	(0.23)	+.006	(0.15)
URBAN SDA	+.036	(1.36)	+.057	(1.94)	+.033	(1 33)
AFRICAN-AMERICAN	-.098	(1.09)	-.092	(1.08)	-.100	(1.16)
HISPANIC NON-WHITE	+.007	(0.15)	+.007	(0.15)	+.007	(0.16)
OTHER MINORITY	-.058	(1.33)	-.050	(1.23)	-.059	(1.42)
HIGH SCHOOL DROP-OUT	-.057	(1.50)	-.051	(1.47)	-.058	(1.64)
POST-HIGH SCHOOL GRADUATE	+.015	(0.59)	+.018	(0.71)	+.014	(0.54)

TABLE D.1
(Continued)

EXPLANATORY VARIABLE	AVERAGE JTPA MODEL		ACTIVITY TYPE MODEL		TERMINATION STATUS MODEL	
	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)
SECOND YEAR PRIOR TO ENTRY:						
CHANGE IN QRLY EARNINGS (8TH-5TH)	-.017	(1.24)	-.018	(1.35)	-.017	(1.30)
NUMBER OF EMPLOYERS	+.010	(0.35)	+.010	(0.60)	+.011	(0.65)
MAJOR INDUSTRY AFFILIATION:						
RETAIL TRADE	+.044	(0.97)	+.046	(1.04)	+.042	(0.96)
BUSINESS SERVICES	+.020	(0.52)	+.026	(0.67)	+.018	(0.47)
OTHER INDUSTRIES	+.058	(1.24)	+.058	(1.31)	+.053	(1.22)
FIRST YEAR PRIOR TO ENTRY:						
CHANGE IN QRLY EARNINGS (4TH-1ST)	-.006	(0.51)	-.004	(0.33)	-.006	(0.55)
NUMBER OF EMPLOYERS	+.004	(0.35)	+.002	(0.17)	+.002	(0.21)
LAMBDA (SELECTION VARIABLE)	+.439	(5.04)	+.437	(6.44)	+.437	(6.47)
CONSTANT TERM	+.657	(8.54)	+.677	(9.48)	+.660	(9.71)
NUMBER OF OBSERVATIONS	1655		1655		1655	

NOTE: Non-linear probit model of participation run in first stage of Heckman self-selection correction model. Linear probability model run in second stage, as results of second stage not significantly different from that of selection model run with non-linear bivariate probit Heckman correction specification.

TABLE D.2

TOBIT MODELS OF ANNUAL EARNINGS IN SECOND YEAR
FOLLOWING ENROLLMENT CONDITIONED UPON JTPA PARTICIPATION:
ADULT WOMEN

EXPLANATORY VARIABLE	AVERAGE JTPA MODEL		ACTIVITY TYPE MODEL		TERMINATION STATUS MODEL	
	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)
JTPA AVERAGE	+510	(1.00)	NA	-	NA	-
ACTIVITITY TYPE:						
CLASSROOM TRAINING	NA	-	+849.	(1.29)	NA	-
ON-THE-JOB TRAINING	NA	-	+1026.	(1.56)	NA	-
OTHER ACTIVITIES	NA	-	-278.	(0.37)	NA	-
TERMINATION STATUS:						
COMPLETE/PLACED	NA	-	NA	-	+905.	(1.42)
COMPLETE/NOT PLACED	NA	-	NA	-	-1771.	(1.37)
NOT COMPLETE	NA	-	NA	-	-2040.	(2.24)
WELFARE RECIPIENT	-.009	(0.24)	-1290.	(2.25)	-790.	(1.36)
URBAN SDA	+1148.	(3.18)	+1489.	(3.49)	+1446.	(3.57)
AFRICAN-AMERICAN	-1463.	(1.04)	-1450.	(0.89)	-1592.	(0.89)
HISPANIC NON-WHITE	+579.	(0.82)	+567.	(0.71)	+523.	(0.70)
OTHER MINORITY	-1046.	(1.66)	-974.	(1.33)	-1137.	(1.68)
HIGH SCHOOL DROP-OUT	-1437.	(2.63)	-1413.	(2.28)	-1608.	(2.78)
POST-HIGH SCHOOL GRADUATE	+933.	(2.48)	+879.	(2.03)	+634.	(1.53)

TABLE D.2
(Continued)

EXPLANATORY VARIABLE	AVERAGE JTPA MODEL		ACTIVITY TYPE MODEL		TERMINATION STATUS MODEL	
	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)
SECOND YEAR PRIOR TO ENTRY:						
CHANGE IN QRLY EARNINGS (8TH-5TH)	-277.	(2.34)	-305.	(2.25)	-314.	(2.17)
NUMBER OF EMPLOYERS	-646.	(2.48)	-551	(1.83)	-564.	(1.84)
MAJOR INDUSTRY AFFILIATION:						
RETAIL TRADE	+524.	(0.81)	+517.	(0.69)	+417.	(0.56)
BUSINESS SERVICES	+399.	(0.67)	+416.	(0.61)	+248.	(0.38)
OTHER INDUSTRIES	+1722.	(2.79)	+1712.	(2.41)	+1578.	(2.26)
FIRST YEAR PRIOR TO ENTRY:						
CHANGE IN QRLY EARNINGS (4TH-1ST)	-151.	(0.99)	-78.	(0.44)	-55.	(0.31)
NUMBER OF EMPLOYERS	-634.	(3.40)	-543.	(2.55)	-612.	(2.87)
LAMBDA (SELECTION VARIABLE)	+7880.	(53.67)	+8361.	(48.46)	+9164.	(43.13)
CONSTANT TERM	+3670.	(6.16)	+3614.	(5.25)	+3120.	(4.52)
NUMBER OF OBSERVATIONS	1655		1655		1655	

TABLE D.3

LINEAR PROBABILITY MODELS OF EMPLOYMENT IN SECOND YEAR FOLLOWING ENROLLMENT CONDITIONED UPON JTPA PARTICIPATION: ADULT MEN

EXPLANATORY VARIABLE	AVERAGE JTPA MODEL		ACTIVITY TYPE MODEL		TERMINATION STATUS MODEL	
	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)
JTPA AVERAGE	+.075	(2.77)	NA	-	NA	-
ACTIVITITY TYPE:						
CLASSROOM TRAINING	NA	-	+ 084	(2.06)	NA	-
ON-THE-JOB TRAINING	NA	-	+.074	(2.33)	NA	-
OTHER ACTIVITIES	NA	-	+.068	(2.08)	NA	-
TERMINATION STATUS:						
COMPLETE/PLACED	NA	-	NA	-	+.104	(3.49)
COMPLETE/NOT PLACED	NA	-	NA	-	+.032	(0.49)
NOT COMPLETE	NA	-	NA	-	-.120	(2.57)
WELFARE RECIPIENT	-.044	(0.79)	-.045	(0.80)	-.042	(0.75)
URBAN SDA	-.018	(0.81)	-.017	(0.76)	-.021	(0.98)
AFRICAN-AMERICAN	+.022	(0.31)	+.022	(0.30)	+.021	(0.30)
HISPANIC NON-WHITE	-.039	(1.02)	-.040	(1.04)	-.045	(1.18)
OTHER MINORITY	-.051	(1.45)	-.051	(1.44)	-.053	(1.53)
HIGH SCHOOL DROP-OUT	-.025	(0.88)	-.025	(0.87)	-.017	(0.61)
POST-HIGH SCHOOL GRADUATE	-.009	(0.38)	-.009	(0.37)	-.008	(0.35)
CHANGE IN ANNUAL EARNINGS (/$1,000)	+.007	(2.41)	+.007	(2.40)	+.004	(1.64)
EARNINGS (2ND YEAR PRIOR/$1000)	+.004	(1.62)	+.004	(1.61)		

TABLE D.3
(Continued)

EXPLANATORY VARIABLE	AVERAGE JTPA MODEL		ACTIVITY TYPE MODEL		TERMINATION STATUS MODEL	
	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)
INDUSTRY AFFILIATION: (2ND YR PRIOR)						
RETAIL TRADE	+.096	(2.13)	+.096	(2.12)	+.093	(2.19)
BUSINESS SERVICES	+.090	(2.20)	+.090	(2.19)	+.088	(2.29)
OTHER INDUSTRIES	+.081	(2.28)	+.081	(2.27)	+.052	(2.38)
INDUSTRY AFFILIATION: (1ST YR PRIOR)						
RETAIL TRADE	+.024	(0.64)	+.024	(0.63)	+.028	(0.73)
BUSINESS SERVICES	+.041	(1.21)	+.041	(1.19)	+.033	(1.01)
OTHER INDUSTRIES	+.060	(1.73)	+.060	(1.72)	+.052	(1.56)
LAMBDA (SELECTION VARIABLE)	+.422	(8.56)	+.422	(8.55)	+.419	(9.97)
CONSTANT TERM	+.666	[illegible] 37)	+.669	(10.37)	+.658	(11.12)
NUMBER OF OBSERVATIONS	1737		737		1737	

NOTE: Non-linear probit model of participation run in first stage of Heckman self-selection correction model. Linear probability model run in second stage, as results of second stage not significantly different from that of selection model run with non-linear bivariate probit Heckman correction specification.

TABLE D.4

TOBIT MODELS OF ANNUAL EARNINGS IN SECOND YEAR
FOLLOWING ENROLLMENT CONDITIONED UPON JTPA PARTICIPATION:
ADULT MEN

EXPLANATORY VARIABLE	AVERAGE JTPA MODEL		ACTIVITY TYPE MODEL		TERMINATION STATUS MODEL	
	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)
JTPA AVERAGE	+444.	(0.61)	NA	-	NA	-
ACTIVITITY TYPE:						
CLASSROOM TRAINING	NA	-	+1103.	(1.35)	NA	-
ON-THE-JOB TRAINING	NA	-	+396.	(0.53)	NA	-
OTHER ACTIVITIES	NA	-	-77.	(0.10)	NA	-
TERMINATION STATUS:						
COMPLETE/PLACED	NA	-	NA	-	+1303.	(2.09)
COMPLETE/NOT PLACED	NA	-	NA	-	-780.	(0.47)
NOT COMPLETE	NA	-	NA	-	-2054.	(2.13)
WELFARE RECIPIENT	-1157.	(0.81)	-1006.	(0.74)	-1227.	(0.98)
URBAN SDA	+493.	(1.08)	+427.	(1.00)	+60.	(0.13)
AFRICAN-AMERICAN	-3778.	(1.74)	-3782.	(1.80)	-3671.	(1.77)
HISPANIC NON-WHITE	-621.	(0.70)	-628.	(0.76)	-631.	(0.71)
OTHER MINORITY	-1265.	(1.53)	-1189.	(1.55)	-1162.	(1.39)
HIGH SCHOOL DROP-OUT	-1688.	(2.54)	-1567.	(2.53)	-1307.	(1.98)
POST-HIGH SCHOOL GRADUATE	+297.	(0.65)	+454.	(1.04)	+698.	(1.49)
CHANGE IN ANNUAL EARNINGS (/$1000)	+123.	(3.06)	+285.	(7.23)	+223.	(5.51)
EARNINGS (2ND YEAR PRIOR/$1,000)	+291.	(8.67)	+313.	(9.77)	+284.	(8.46)

TABLE D.4
(Continued)

EXPLANATORY VARIABLE	AVERAGE JTPA MODEL		ACTIVITY TYPE MODEL		TERMINATION STATUS MODEL	
	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)	COEFFICIENT	(T-VALUE)
INDUSTRY AFFILIATION:(2ND YR PRIOR)						
RETAIL TRADE	-1315.	(1.48)	-1260.	(1.55)	-1259.	(1.33)
BUSINESS SERVICES	-1709.	(2.25)	-1618.	(2.24)	-1599.	(2.04)
OTHER INDUSTRIES	-871.	(1.35)	-768.	(1.26)	-656.	(0.99)
INDUSTRY AFFILIATION: (1ST YR PRIOR)						
RETAIL TRADE	-1522.	(1.87)	-1484.	(1.91)	-1321.	(1.52)
BUSINESS SERVICES	-1579.	(2.09)	-1521.	(2.14)	-1505.	(1.97)
OTHER INDUSTRIES	-509.	(0.80)	-431.	(0.71)	-472.	(0.73)
LAMBDA (SELECTION VARIABLE)	+12052.	(45.91)	+11327.	(49.87)	+10429.	(50.01)
CONSTANT TERM	+3585.	(4.01)	+4224.	(5.07)	+4181.	(5.05)
NUMBER OF OBSERVATIONS	1737		1737		17370	

APPENDIX E: Gross Versus Net Impacts Of JTPA Program Activities

The findings of the Utah net impact study contained in Chapter Five may be compared to those of the National Commission for Employment Policy gross impact study.[1] While both studies report positive impacts from intensive training, the Utah findings are in contrast to the others in terms of the relative size of the program impacts. In particular, the Utah findings suggest adult women realize a greater impact from on-the-job training (OJT) than from classroom training (CT) while adult men realize a greater impact from classroom programs than on-the-job training.

To examine whether these unexpected results are attributed to the statistical methodology used in this study, or to the uniqueness of the Utah JTPA system and economic environment, the results are compared with those derived from "gross impact" models estimated using only program enrollees in the Utah data base. The base of comparison in these models is enrollees who participated in the less intensive "other activity" programs, which in Utah enrolls nearly all individuals in short duration job search assistance. These estimates are derived directly in gross impact models. In contrast, they are inferred – or derived indirectly – in the net impact models above by subtracting the net impacts of the more intensive training activities (CT and OJT) from the net impact of those enrolled in the "other activities" (OA) category.

As evident in Table E.1 below, the gross impact models tend to over-estimate the relative employment and earnings impact differentials (from OA) compared with the more complex net impact models used in the study. More importantly, however, the relative impacts between classroom and on-the-job training programs are fairly consistent between gross and quasi-net impact models in Utah. For example, adult men enrolled in CT realize greater employment and

TABLE E.1

ACTIVITY PROGRAM IMPACT DIFFERENTIALS FROM NET IMPACT AND GROSS IMPACT MODELS

GENDER - TRAINING GROUP	PERCENT EMPLOYED		EARNINGS IF EMPLOYED	
	NET IMPACT	GROSS IMPACT	NET IMPACT	GROSS IMPACT
ADULT WOMEN				
CT VS OA	+.092	+.110	+$1127	+$1852
OJT VS OA	+.141	+.170	+$1304	+$1639
ADULT MEN				
CT VS OA	+.016	+.030	+$1180	+$1345
OJT VS OA	+.006	+.020	+$473	+$82220

earnings impacts than those in OJT relative to OA. In fact, the order of relative impacts is the same in all cases, except for that reported for the relative earnings impacts among adult women. That is, the gross impact results support that typically found -- classroom training has a greater relative earnings impact than OJT for adult women. (That is, the premium to CT over other activities is $200 greater than the premium of OJT over other activities.) This is not supported by the quasi-net impact models developed in this study, which indicate a greater relative earnings impact for adult women in OJT.[2]

There are two plausible reasons for this outcome. First, in Utah some OJT contracts were developed differently during the program year. In particular, individuals who were not placed in classroom training and who wanted more than short job search assistance were directed to the OJT program. On an experimental basis, some of these individuals were permitted to find employers willing to enter into the program on their own, rather than being placed into such arrangements by JTPA placement coordinators. The quasi-net impact findings would suggest that adult women who were willing to find such contracts on their own may have unobserved factors which made them more employable relative to the type of adult men who found OJT contracts on their own.[3]

Second, the economy in Utah during the post-program period in this study (PY 1987) exhibited slow growth, with the service sector leading the more sluggish manufacturing sector. Since women were over-represented in the former sector relative to men, those obtaining OJT contracts in the service sector would have experienced stronger employment and earnings growth.

Endnotes

1. Geraci and Romero (forthcoming).

2. This finding in the Utah study is supported by the National JTPA Experiment which found a larger significant earnings impact for adult women in OJT during the last 4 quarters of the 6-quarter follow-up period. (As indicated in Bloom, et al. (1993), Exhibit S.6, p. XLV, the significant estimate of the earnings impact for adult women from OJT totaled +$518 compared with +$332 for those from occupational classroom training (OCT) during this period.)

3. A greater percent of adult women formerly enrolled in OJT were employed in the second year following entry than those formerly enrolled in CT, whereas fewer adult men from OJT were later employed compared to those from CT. In addition, adult women from OJT who were later employed earned only $250 less than those from CT, compared with adult men from OJT who earned $1,000 less if later employed. These figures support the notion that a different type of adult women selects into Utah's OJT program than the type of adult men who select into OJT. Failure to capture these unobserved factors in statistical modeling would result in biased program impact estimates.

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U.S. Department of Labor
NATIONAL COMMISSION FOR EMPLOYMENT POLICY
1522 K Street, N.W., Suite 300
Washington, D.C. 20005

National Commission for Employment Policy

1522 K Street, N.W., Suite 300, Washington, D.C. 20005
(202) 724-1545