Estimating the Impact of Selected Programs on Participants' Subsequent Welfare Dependence and Employment in British Columbia

William Porter Warburton

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Abstract

This thesis is concerned with estimating the impacts of employment and training programs without the use of random assignment. It reviews the literature on the American CETA programs that led researchers to conclude that random assignment was needed to produce useful estimates.

It reports an investigation of selection bias that yields four findings. It *is* possible to test for selection bias in the absence of random assignment. Pre-program tests for selection bias are not valid. Selection bias due to unmeasurables is small. Controlling for changes in major explanatory variables such as pre-program employment is crucial. It shows that the CETA data were inadequate to control for these changes.

This thesis reports the finding that 25% non-response in a survey can lead to qualitative changes in the estimates of program impact. It illustrates the way in which undetected non-linearities can bias estimates.

It reports estimates of the impacts of a range of programs. Wage subsidies with the private sector have a large (ten percentage points) sustained beneficial impact on subsequent welfare dependence and employment. On-the-job training in community projects (make-work) has no long-term impact on welfare dependence or employment. Within classroom training, upgrading (adult basic education) has no impact on subsequent welfare dependence. Vocational training has a large (15 to 20 percentage points), and sustained beneficial impact on subsequent welfare dependence. The job club studied in this thesis had a significant short term beneficial impact, but no long term impact.

Notice to Readers

The opinions expressed in this thesis are those of the author, and do not necessarily represent the official policies or positions of the the British Columbia Ministry of Social Services, the British Columbia government, Human Resources Development Canada, or any other organization.

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1. Introduction

This thesis addresses the issue of estimating the impacts of employment and training programs for disadvantaged workers without the use of random assignment. This issue is important for three reasons. First, employment and training programs are of wide policy interest. If they are effective, they can return individuals to employment thereby simultaneously reducing poverty, reducing government expenditure and increasing government revenue. Second, random assignment studies in the United States have shown that at least some programs are ineffective and that programs with identical descriptions can have very different impacts (See e.g. SRDC/MDRC 1995). Third, while these two findings point to the need for estimates of impacts of programs as they operate in other jurisdictions, it might not be possible to use random assignment to produce the estimates because random assignment is difficult to implement, raises difficult ethical issues, and may not produce results that are generalisable. (See e.g. Heckman, and Smith, 1993a and 1993b and Heckman, Clements and Smith, 1993)

This thesis contributes new facts both about the effectiveness of employment and training programs for welfare recipients in British Columbia, Canada and about the data that is needed to produce reliable estimates. The findings in brief are as follows.

- On-the-job training placements with private sector firms have large, sustained beneficial impacts on the employment and welfare dependence of participants. Even three and four years after the placement, roughly ten percentage points more participants were independent of welfare than would have been expected in the absence of the program.
- On-the-job training placements in temporary public sector projects had very little impact on welfare dependence in the medium to long run. Beyond 18 months after placement in this type of program, less than 3 percentage points more participants were independent of welfare than would have been expected in the absence of the program.
- Job clubs had a small, short-term beneficial impact on welfare dependence.
- Adult basic education had no beneficial impact on the subsequent welfare dependence of enrolees.
- Vocational training had a large, long-term beneficial impact on the welfare dependence of participants. Four to five years after enrolling, 10 to 15 percentage points fewer enrolees in short duration (less than one year) programs were dependent on welfare than would have been expected in the absence of the program. Longer programs (two years) had an even larger impact (20 percentage points after 5 years).

These estimates were produced using a variety of techniques. In each case the value of the variable of interest, (welfare dependence, Unemployment Insurance dependence, or employment) averaged across program participants, was compared with the value of the same variable, averaged across non participants. The groups of non participants used were variously:

- a control group (the remainder of a group of welfare recipients from which program participants were randomly selected);
- welfare recipients as identified by administrative data, but who had not participated in a training program; and
- welfare recipients, identified by Ministry staff as likely candidates for employment and training programs, but who did not participate.

Measured differences between program participants and the comparison groups were controlled for variously by regression analysis and by collecting the non participants into groups with similar characteristics and weighting the within-group averages by the number of participants who would have belonged to that group. (The latter technique is often referred to as cell matching.)

The central methodological issue in the production of estimates without the use of random assignment is selection bias. Are the differences in outcomes the result of program participation, or the result of unmeasured characteristics that are correlated both with program participation and the outcome variable? Five separate tests were conducted to estimate the extent to which selection bias would affect these results. The tests indicate that:

- with the data used, and with the selection mechanism used for these programs, the amount of selection bias is less than five percentage points. In most cases the bias results in an over-estimate of program impact.

This thesis provides detail on the way in which these estimates were produced in the next chapters. The rest of this chapter provides background in four sections. The first section describes the income support system and economy of British Columbia. The second section describes the programs which are the subject of analysis, and the third section provides a road map to the rest of the thesis.

1.1 Background on British Columbia

British Columbia (BC) is Canada's westernmost province, third largest in size and population. Although it covers roughly four times the area of Great Britain, its population is only 3.5 million. The vast majority of its people live in the south-western part of the province where the climate is comparable to the climate of southern England.

1.1.1 Income Support System Overview

Income support in Canada is provided through several separate systems. For those aged 18 to 64 the two principal systems are the federally administered Unemployment Insurance (UI) system and provincial welfare systems. UI provides a relatively high level of benefits to workers who have recently left their jobs. Currently, (March 1994) people qualify for UI if they have worked for a minimum of from 12 to 20 weeks within the previous 52 weeks. Minimum requirements vary with the regional[1] unemployment rate. Having qualified they receive benefits equal to 55% of their insurable earnings[2] for from 20 to 50 weeks depending on the unemployment rate and the number of weeks worked. Average UI benefits per week were $258.63 in August 1993 [Statistics Canada 73-001].

[1] Statistics Canada estimates an unemployment rate for about 60 economic regions for this purpose.

[2] UI claimants with dependants and low family income may receive benefits equal to 60% of insurable earnings. Currently the maximum insurable earnings is $780 per week. For comparison, Statistics Canada reported average weekly earnings, industrial aggregate $563.07 in August 1993 [Statistics Canada 72-002].

Those who don't qualify for and those who have exhausted their UI must rely on provincially-administered[3] welfare systems for income support. Eligibility for welfare is needs tested (assets and income must be below prescribed limits) and benefits are based on family size and structure. Average welfare benefits in British Columbia in August 1993 were $695 per case per month, roughly 60% of Unemployment Insurance benefits.

1.1.2 Trends in Welfare Since 1980

Figure 1-1 shows the dramatic increase in welfare caseload in British Columbia since 1982. However, this is only part of the story. The characteristics of the caseload have also changed markedly over the past decade.

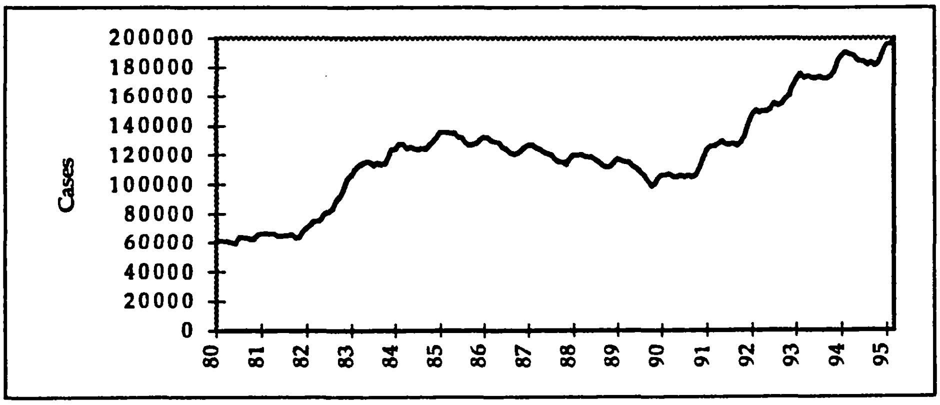

Figure 1-1 Welfare caseload, 1980 to present

In August 1980 there were 63,004 welfare cases in BC, excluding the handicapped[4] and the aged. At that time, the majority were not considered to have good prospects for employment:

- 37,029 were classified as unemployable[5]

[3] The federal government shares in the cost of the provincial welfare system and sets guidelines. For example, provinces must provide appeal mechanisms and cannot require participation in any program as a condition of receipt of benefits.

[4] to be classified as handicapped, clients must, in the opinion of a physician, be permanently unable to hold employment.

- Of those classified as employable 5,281 were single parents.

The remainder, 20,694 cases was less than a quarter of the 88,000 unemployed in 1980 as measured by Statistics Canada using the Labour Force Survey [Statistics Canada, 71-201, 1988, annual average] and less than one third of the 65,000 who received UI in 1980 [Statistics Canada 73-001]. But with the recession of the early 80's the number of employables tripled, reaching 75,859 in August 1986. The ratio of employable people on welfare to the number of unemployed had increased to more than .4.

This trend has continued with the recession of the early 90's. In August 1993 there were 130,187 cases headed by an applicant who was classified as employable compared with an average of 107,650 who received regular benefits under Unemployment Insurance program and 163,000 total unemployed as estimated by Statistics Canada based on the monthly Labour Force Survey[6]. In

[5] The definition of "unemployable" changes frequently. For single parents, it depends on the ages of their dependent children. For others it depends on their ability to hold employment. Before 1989 this determination was made by Ministry employees, financial assistance workers. After 1989 this determination was made by a physician. This is not a particularly meaningful variable. For example a derelict might not be willing to approach a physician to secure the necessary certificate, and so would be classified as employable. As a result people in the Ministry refer to an individual as "job-ready" rather than employable if they wish to indicate that an individual is suitable for employment.

[6] We cannot tell what percentage of the LFS unemployed receive welfare and what percentage receive UI directly because self declaration of welfare under-reports welfare by half (according to documentation that comes with the Survey of Consumer Finances micro data tapes.) Nonetheless, we can produce a rough estimate by a roundabout means. There are three reasons why the number of UI recipients plus the number of welfare recipients exceeds the number of unemployed.

1. About 14,000 cases received both welfare and UI simultaneously. This includes those who receive welfare while awaiting their UI (10,000) and those who receive a top-up to their UI (4,000).
2. a number of individuals on welfare are working and therefore would not be reported as unemployed in the labour force survey. About 24,000 declare earnings each month. In August 1993 17,000 employable cases closed. Very few of the closing cases will have been unemployed in August. They received benefits as a result of reporting delays.

1993 over 300,000 different cases[7] were paid welfare benefits although the average number of cases per month was 165,648. As these numbers imply, the caseload of welfare recipients is not static. In fact, in BC 75% of welfare spells were shorter than 6 months (Cragg 1994).

1.1.3 Trends in British Columbia's Economy

The province is extensively forested, and the presence of this natural resource is reflected in the economy. More than one third of manufacturing employment is related to wood products, including pulp and paper. However, employment in this field proved to be volatile and vulnerable to automation in the 80's and this has had significant implications for the provision of welfare in BC.

Housing starts in the United States fell from a high of over two million per year in 1978 to 1.1 million in 1981. (U.S. Bureau of the Census 1983, Table No. 328.) Wood products production in BC followed this trend and although employment in the wood products industry followed the trend down, it did not follow it up again. In 1981 59,000 people were employed in the wood products industry. This fell to 43,000 in 1982, and although by 1985 production, at 77 million cubic metres exceeded pre-recession levels, employment remained low at 40,000 (Central Statistics Bureau, 1991).

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3. a number of individuals would not have reported actively seeking employment to the labour force survey and so would not be counted as unemployed. About 17,000 employable welfare recipients reported participating in training programs.

Combining these, ignoring overlap, accounts for 72,000 of the 75,000 difference between the sum of employable welfare plus regular UI and total unemployed. These numbers suggest that roughly two thirds of the LFS unemployed received UI, 44% received welfare and 10% received both.

[7] Excludes Handicapped, and those over 60 years of age.

A study[8] of sawmill workers found that 56% of them read at a grade 4 level or less[9] indicating that this group of workers would be particularly vulnerable in the event of a permanent layoff.

Although employment in the primary industries and manufacturing only made up 21.4 % of total employment in 1980, the economy, as measured by the GDP, reflected this pattern. It fell dramatically between 1981 and 1983, but had regained its pre-recession level by 1985.

The state of the economy relative to the rest of Canada also shows up in the migration statistics. In-migration from the rest of Canada fell between 1980 and 1986, and has grown since.

When it had become apparent that the nature of welfare had changed, the government moved to increase access to training programs. In September 1985 federal, provincial and territorial ministers responsible for social services (including welfare) agreed in principle[10] to new means of funding programs designed to provide welfare recipients with employment training and work experience. The agreement was implemented in April 1986 in BC by a letter of understanding signed by two federal ministers (National Health and Welfare and Employment and Immigration) and two provincial ministers (Human Resources and Labour)[11]. In a separate document, federal officials offered funding for enhancements to evaluations of these initiatives.

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[8] Unpublished study by the BC Council of Forest Industries and the International Woodworkers of America

[9] While working in a logging camp, one of my co-workers told of taking an aptitude test at a Canada Employment Centre. "I must have done very badly," he said, "She just looked at me and said, 'You're going to have to be a logger.'"

[10] This agreement is generally known as *The Four Corners Agreement*.

[11] Despite reorganisations affecting all four departments, the agreement remains in place.

1.2 Description of Programs

Two new types of program were implemented as a result of the letter of understanding: on-the-job training in private sector positions and on-the-job training in public sector positions. These supplemented two other types of program that were available to welfare recipients at the time: classroom training, mainly through community colleges, and job clubs.

The range and nature of programs offered to welfare recipients varies considerably from year to year as political direction and the ministry responsible for implementing the programs change. However, the programs did remain relatively constant between 1988 and 1991. This section describes the programs as they existed in that period.

1.2.1 On-the-Job Training: Private Sector

British Columbia's on-the-job training program with the private sector offered private-sector employers a subsidy if they would hire and train welfare recipients. The subsidy was equal to half the wage, to a maximum of $3.50 per hour. In practice this led to most of the positions being filled at a wage very close to $7.00 per hour. The minimum wage in 1987 was $4.00 per hour. (This was raised to $4.50 in 1988, $4.75 in 1989, and $5.00 in 1990.) The average weekly earnings in BC, April 1989 was $483. (Statistics Canada 72-002)

Only full time positions were eligible for the subsidy, and the employer was required to certify that the creation of the subsidised position would not result in the layoff of any existing employee. The subsidy could last from two to twelve months, and there was a presumption that the employee would remain with the employer after the subsidy had ended.

The training positions were generated by Ministry employees who would approach businesses either individually or in groups (e.g. Chamber of Commerce meetings) and appeal to them to open positions for welfare recipients in exchange for the subsidy. The limiting factor seemed to be the number of employers who were willing to open positions for the program. At its peak, this program provided positions for about 6,000 welfare recipients, about 2% of the 270,000 cases open at some point in 1991/92. Similarly its $12 million budget was about 1% of the total welfare budget.

1.2.2 On-the-Job Training: Public Sector

At the same time British Columbia had three programs that offered welfare recipients employment with on-the-job training on government projects. The Ministry of Social Services supplied the wages (up to $7 an hour for labourers and up to $10 an hour for supervisors), the employers' contributions to employee benefits, and an additional amount for administrative overhead. The positions normally lasted six months but were sometimes extended to 12 months.

Table 1-1: Public Sector Programs (1991/92)

	Clients	Budget ($M)
CTETP	961	5.3
FEP	508	4.1
EYC	350	3.0
Total	1819	12.4

The three programs were as follows:

- The Community Tourism Employment Training Program (CTETP) funded work with non-profit organisations on community tourism development. Projects included heritage site restorations, parks development, and festival start-ups.
- The Forest Enhancement Program (FEP) funded work on silviculture projects throughout the province. The FEP was administered by the Ministry of Forests.
- The Environment Youth Corps (Income Assistance Component EYC) funded work for welfare recipients aged 17-24 on such outdoor projects as trail improvement and salmon enhancement. The EYC was operated in conjunction with the Ministry of Environment, Lands and Parks.

These three public-sector programs taken together had a budget comparable to the budget of the Employment Opportunity Program, but they assisted only about one-quarter the number of people. Fewer than one percent of all Income Assistance recipients participated in the public-sector programs, and the average cost of the program was about $6800 per participant.

1.2.3 Classroom Training

The Ministry refers welfare recipients to educational and vocational classroom training programs as employment preparation. Funding for tuition is provided by the Ministry, by the federal government, and by student grants and loans. Community colleges, institutes and universities receive general funding equal to about five times the tuition. Typically, the participant would be required to take out a student loan to cover tuition and the cost of books, while the Ministry would continue to provide welfare benefits. However in some circumstances, at the discretion of the worker, the Ministry might pay the tuition and or expenses directly. The training programs are provided mainly by community colleges and institutes. In 1991/92, more than 20,000 recipients registered for these courses. The Ministry's contribution to tuition and books was about $3 million.

In this thesis, analysis focuses on individuals who received welfare benefits in August or September 1986 and who enrolled in Camosun College in September 1986. An information sharing arrangement with Camosun made it possible to estimate the impact of four separate types of classroom training.

- Vocational training: 6-12 months in duration, e.g. plumbing, welding, secretarial, dental hygienist;
- Career Technical Training: 24 months in duration, e.g. criminal justice, visual arts, electronic technology;
- Adult Basic Education: from basic literacy and numeracy to high school equivalence; and
- Academic: university transfer courses.

1.2.4 Job Clubs

The Ministry of Social Services has given the name Job Action to its job search assistance initiatives. Job Action encompasses a range of short programs of the kind called "job clubs" found widely across Canada and the United States. Their purpose is to improve the intensity and effectiveness of the job search of job-ready welfare recipients. The Job Action programs, which run up to five weeks in length, combine classroom learning with actual job search. Participants learn to assess their skills, obtain job interviews, and present themselves effectively in interviews. They also receive an allowance of up to $150 for program-related expenses such as transportation, clothing, and personal grooming. Job Action is brief, inexpensive, and targeted at recipients with no apparent barriers to immediate employment.

The Job Action programs in 1991/92 had total expenditures of $3 million, a total enrolment of 6,233 recipients, and a cost per participant of about $480.

This thesis reports on only one job club, the Region A Job Action Pilot Project. Random assignment of participants was an integral part of the project.

1.2.5 Origins of this study

The work reported in this thesis had its origins in the 1985 Canadian federal-provincial Four Corners Agreement on training for welfare recipients. Implementation of the agreement in BC in 1986 resulted in the development of new training programs and called for greater emphasis on evaluation. Funding for the evaluation of these programs was offered by the federal government in December 1986, and a successful application was made by BC in 1987. Over the subsequent five years, the federal government contributed over one-half million dollars to the support of the evaluation of these training programs.

1.3 Road map

The environment in which this study was born was affected by the Americans' experience in estimating the impacts of the Comprehensive Employment Training Act (CETA) programs. There, different researchers, using the same data, and trying to estimate the impacts of the same programs, came up with qualitatively different results. Selection bias was identified as the culprit. For this reason, the second chapter of this thesis provides a review of the literature relating to the CETA studies.

There have been a number of recent attempts to assess and deal with the selection bias problem. Chapter 3 provides a summary.

Chapter 4 provides an overview of the BC study, including a description of the conceptual framework, a description of the data, and a description of the approach taken. It also includes a preview of the results.

Experts advised the US Department of Labor to address the selection bias problem by relying more heavily on random assignment in the estimation of impacts of programs, but for reasons which are beyond the scope of this thesis, random assignment was not a practical alternative in British Columbia. Nonetheless, the failure of top flight researchers such as Ashenfelter and Heckman to bring about consensus on the impacts of CETA programs, combined with a prohibition on the use of random assignment, dictated that the question of selection bias be addressed head on. This is done in Chapter 5, "Is Selection Bias the Bogeyman?" It finds that the worst problems in the CETA studies resulted from the use of annual data. It also reports the results of a number of tests for selection bias which, given the data and selection mechanism in BC, indicate that selection bias is less than five percentage points. An implicit conclusion of the chapter is that selection bias is devilishly tricky.

Estimates of impact are presented in Chapters 6 (on-the-job training in the private sector) and Chapter 7 (all other programs).

Chapter 8 provides a short summary and recaps the conclusions.

1.4 Summary

This chapter provided an introduction to the thesis. It provided background for the thesis by describing the income support system that exists in the province, and trends in welfare dependence and in the economy as a whole. It also described the four types of program that will be the subject of analysis: wage subsidy in the private sector, temporary employment in community projects, classroom training and job clubs.

2. Lessons from CETA

The literature relating to the methods used to estimate the impact of the American Comprehensive Employment Training Act (CETA) programs is not encouraging to those who wish to estimate the impact of employment and training programs, but who cannot use random assignment to do so. Many estimates of the impact of CETA programs were published but although they started with the same data and were estimating the impact of the same programs, the estimates showed an alarming lack of consistency. They seemed to be quite sensitive to model specification. For example, Ashenfelter and Card found that "different models lead to very different estimates of training effects." and concluded "that randomised clinical trials are necessary to reliably determine program effects." [Ashenfelter and Card, 1985, pages 659, 648]

The US Department of Labor (DOL), with responsibility for administering CETA, and its successor, the Job Training Partnership Act (JTPA, 1982) was at the centre of the controversy. It responded by striking a panel of experts to recommend methods of estimating the impacts of JTPA programs. The panel, with the sesquipedalian name, Job Training Longitudinal Survey Research Advisory Panel, concluded

> State-of-the-art statistical techniques simply cannot overcome certain data or design problems. [Stromsdorfer et al., 1985 page III-J-58]

and recommended that the DOL put its non-experimental data collection efforts on hold and estimate the impact of the Job Training Partnership Act programs using a limited number of classical experiments[1].

2.1.1 Road Map

The purpose of this chapter is to investigate the difficulty that researchers had in estimating the impact of the CETA programs. It reviews the literature that led to the Panel's conclusion that even very sophisticated statistical methods are unsuccessful in dealing with selection bias. Developments subsequent to the Panel's recommendations are reviewed in Chapter 3, and the reasons for the ongoing problems with selection bias are explored in Chapter 5.

A subsidiary goal of this chapter is to glean information that would be useful in an observational study. Specifically, the chapter will also

- describe a number of methods of approaching the selection bias problem, and
- identify sources of frailty of non-experimental estimates of the impact of employment and training programs.

The outline of this chapter is as follows. The next section, headed Background, gives a brief description of the data used to estimate the impact of the CETA

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[1]Classical experiment is used here to refer to a study in which estimates are produced using random assignment. The term non-experimental is used to refer to studies that estimate impacts without using random assignment.

programs and describes the challenge of selection bias. This is followed by a section that reports the Panel's recommendations and the Panel's findings on the success that researchers had had in addressing selection bias. Next come short descriptions of eight studies that influenced the panel, each headed by the author's name. The concluding section summarises the lessons learned.

2.1.2 Background

The Job Partnership Act (JTPA) was enacted in 1982 to replace the Comprehensive Employment Training Act of 1974 as the principal vehicle for employment and training programs for unemployed and disadvantaged workers in the United States. The JTPA required the U.S. Department of Labor (DOL), which administered the Act, to "evaluate the effectiveness of programs authorised under this act." [JTPA Section 454 (a).]

In order to meet its evaluation obligations under the earlier Comprehensive Employment Training Act (CETA), the DOL had assembled an impressive collection of data on participants and non-participants. The CETA data set had three components:

- the Continuous Longitudinal Manpower Survey (CLMS),
- the Current Population Survey (CPS) for March of each year and
- Social Security Administration earnings data.

The CLMS captured data on a sample of CETA enrolees shortly after they enrolled and two or three times later. The CPS collected demographic and labour market data for a sample of the general population. Annual earnings reported to the Social Security Administration, beginning in 1951, (referred to in most of the studies as SSA earnings) were added to these sets of data to produce longitudinal earnings records for all individuals on the file.

These data could be used to estimate the impact of the CETA programs on the earnings of participants in the following manner. The CLMS would identify participants in the programs and collect information on characteristics thought to affect their earnings. The CPS would collect similar information on non-participants, and SSA records would provide information on earnings on both groups, both before and after program participation. Impacts on earnings would be estimated by the difference in post-program SSA earnings between the participants and the comparison group, after statistical techniques had been used to control for differences between characteristics and pre-program earnings histories.

Although the data are impressive, they have 3 major limitations:

1) The earnings data are annual, and so do not give information on earnings fluctuations within the year.

2) The CPS file does not have information on location, so it was not possible to match participants with non-participants in the same labour market.

3) Participants were not necessarily excluded from the sample of non participants.

The U.S. DOL commissioned a number of studies to produce estimates of the impact of the CETA programs, and the Congressional Budget Office commissioned a separate study. All started with the same data, but many came up with substantially different results. Although some of the differences can be attributed to different samples[2] there remained unexplained differences. These remaining differences were felt to be due to selection bias.

2.1.3 Selection Bias

Selection bias occurs when an unmeasured variable is correlated both with program participation and with the outcome of interest. The threat of selection bias is always present unless random assignment is used in the estimation of the impact of a program.

> If randomisation is absent, it is virtually impossible in many practical circumstances to be convinced that the estimates of the effects of treatments are in fact unbiased. This follows because other variables that affect the dependent variable besides the treatment may be differently distributed across treatment groups, and thus any estimate of the treatment is confounded by these extraneous x variables. [Cochran and Rubin, 1973, page 417]

The extent to which statistical techniques, either matching or regression analysis, can ameliorate selection bias, depends on the data. If the functional form is known, and the researchers can ensure that there is no correlation between the error term (which includes unmeasured variables) and the explanatory variables (which include program participation), regression analysis can be used to produce unbiased estimates. Similarly, if the researcher can draw a comparison group that matches participants on all characteristics that affect the dependent variable but are not evenly distributed between the treatment and non-treatment groups then a comparison between the treatment and comparison groups will yield an unbiased estimate of program impact.

[2] Some researchers eliminated some youth from their samples, and others dropped participants who remained in the program for less than a week. Most researchers provided separate estimates for males and females, and for whites and non-whites. One consistent finding was that estimates of impact were higher for women than men.

At first glance, the CETA data appear to be inadequate to deal with selection bias because some variables thought to affect post-program earnings, and to be correlated with program participation, are unmeasured, and perhaps un-measurable, (e.g., motivation, intelligence). However, the extensive history of earnings included in the CETA data can, in some circumstances, be used to remove selection bias even when there are important missing variables.

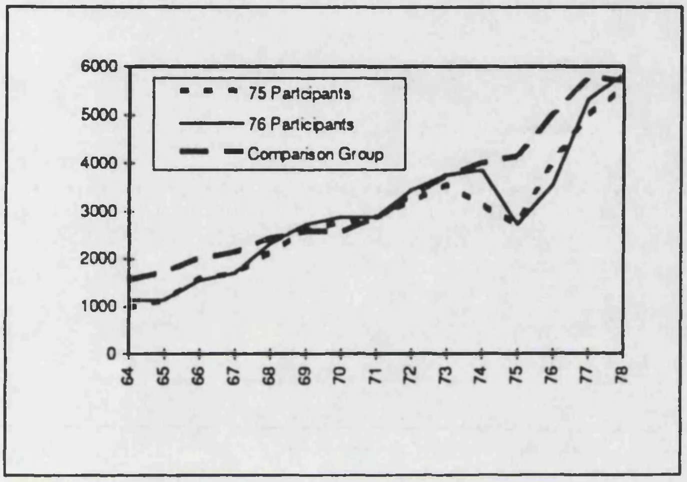

Figure 2-1 The "pre-program dip" in earnings

Ashenfelter (1978) describes a method for dealing with fixed unmeasured variables in the context of an earnings function. If motivation, intelligence or other unmeasured characteristics are constant, and the form of the earnings equation is known, then their effect can be eliminated algebraically. In the simplest case, where the unmeasured variables shift the earnings function up or down without changing its slope, a first difference will eliminate the impact of the unmeasured variables. In estimating the impact of CETA programs, the confounding effect of the unmeasured but constant variables would be eliminated by comparing the difference between pre- and post-program earnings for participants and non-participants. This technique is known as the difference-in-differences estimator.

Unfortunately, researchers estimating the impact of CETA face more difficult problems than constant unmeasured variables. Some of the additional problems are reflected in the pre-program dip. Figure 2.1 shows average annual earnings for the 15 year period 1964 to 1978 for males who enrolled in CETA in 1975 and 1976 together with the average earnings of the comparison groups. Clearly some transitory characteristics are correlated both with program participation and earnings.

In the studies of the estimates of the impact of CETA, in addition to controlling for a wide range of measured variables, researchers attempted to control for unmeasured characteristics that were variously: permanent, transitory, serially correlated and growing. With regard to Figure 2.1, the differences between participants and the comparison group between 1964 and 1968 might be due to measured differences (e.g., age), or unmeasured differences (e.g., the intercept or coefficient on the variable time in the individual's earnings function). Differences between the earnings of participants and comparison group members in the immediate pre-program years might be due to measured or unmeasured transitory differences.

Despite the number of attempts to deal with the wide variety of measured and unmeasured, permanent and transitory differences, uncertainty remained for two reasons. First, there was uncertainty regarding the functional form. Second, as is pointed out by Bloom and others, there can be no way of distinguishing between the effects of the training program itself and the effect of an undetected change in the earnings functions that coincides with participation in a training program.[3] Researchers can alleviate the first source of uncertainty somewhat by devising ways of using the extensive earnings histories in the CETA data to test

[3] Bloom (1987) cites women returning to the labour force at the same time as they enrol in a training program as an example of an undetected (by other researchers) change in the earnings function.

their earnings functions. Researchers attempt to alleviate the second source of uncertainty, by attempting to replicate the results of random assignment studies using comparison group methods. However, the Advisory Panel concluded that too much uncertainly remained.

2.2 Advisory Panel Report

The JTPA was passed in 1982, and by 1985 the DOL was already collecting data in order to produce estimates of the impact of the JTPA programs as it had for the CETA programs. In view of the controversy surrounding the CETA estimates, it decided to seek advice on the reasonableness of continuing down that path, and struck the Job Training Longitudinal Survey Research Advisory Panel. A summary of the Panel's findings and recommendations and a comment on the consequences of the recommendations follow.

2.2.1 Findings

The findings of the panel can be summed up as follows:

1) there is unacceptable variation in the estimates of the impacts of CETA programs;
2) the sources of that variation are not fully understood; and
3) it is unlikely that the controversy will be resolved in the near future.

Parts of the report are excerpted below to illustrate these findings.

> the estimates of the net impacts of CETA are not reliable and the true net impacts of CETA are still open to question. [Stromsdorfer et al, page III-J-47]

> the range of results within and across studies is disturbingly large and no particular point estimate can be said to be the correct one. [ibid. page III-J-55]

> no such study using a quasi-experimental design can be said to have controlled adequately for selection bias. [ibid. page III-J-66]

> Given the methodological problems with the CLMS and CPS data sets, there is currently little consensus on the choice of methods to estimate net program impacts using these particular data. [ibid. page III-J-55]

> it will not be possible to solve the problem of selection bias within the context of a quasi-experimental design ... in a short enough time to meet Congress' need for valid information to guide policy. [ibid. 1985, page III-J-65]

2.2.2 Recommendations

The Panel's key recommendation was that

> The JTLS/SHOW sample [which is analogous to the CLMS] should be placed on hold [ibid. page III-J-71]

and

> the DOL should perform a selected set of classical experiments over the next several years that involve random assignment of program eligible individuals to the treatment (experimental) group and to the non-treatment (control) group. [ibid. page III-J-68]

The panel did not recommend that the DOL abandon non-random assignment techniques altogether, recommending instead further research.

> ...it is intended to use these experimental results and the understanding of the selection process gained thereby to improve the effectiveness of quasi-experimental design as a strategy for program evaluation. [ibid. page III-J-68]

> The process analysis should investigate the process of selection in particular. [ibid. page III-J-77]

[the DOL] experiment with possible data sets such as the Continuous Work History Sample of the Survey of Income and Program Participation to serve as a comparison group. [ibid. page III-J-67]

further analysis of the CLMS data should be conducted with the express purpose of analysing the problem of selection and other data handling issues. [ibid. page III-J-77]

2.2.3 The National Research Council Recommendation

Random assignment was also recommended to the DOL by the Committee on Youth Employment Programs of the National Research Council. This committee was given the daunting task of reviewing 400 reports on programs funded under the Youth Employment and Demonstration Projects Act (YEDPA) of 1977 in order to draw conclusions and make recommendations. Like the Job Training Longitudinal Survey Research Advisory Panel, the committee found it easier to draw conclusions and make recommendations about the nature of evaluation research than about employment and training programs.

Their comments on random assignment were unequivocal.

> Our review of YEDPA research strongly suggests that much more could have been learned, and more confidence placed in the results, if random assignment had more frequently been used. We believe that not only has the feasibility of random assignment in program research been demonstrated, but that in situations in which program resources are scarce and program effectiveness unproven, it is ethical (see appendix C).
>
> RECOMMENDATION: *Future advances in field research on the efficacy of employment and training programs will require a more conscious commitment to research strategies using random assignment. Randomised experiments should be explicitly authorized as a device for estimating the effects of new projects, program variations and program components. Furthermore, funding authorities should back this explicit authorization with firm indications that this is the method of evaluation which is expected.* [original emphasis, Betsey et al, 1985]

2.2.4 Implications

A fundamental change in the way in which impacts of employment and training programs were estimated followed quickly after the submission of the reports of these two panels. The U.S. Department of Labor (DOL) accepted the recommendations of the Job Training Longitudinal Survey Research Advisory Panel, renouncing its 20 year practice of using quasi-experimental methods and launched an evaluation of its Job Training Partnership Act programs using random assignment.

In the Family Support Act of 1988[4] Congress required the use of random assignment to evaluate another national employment and training program, the Job Opportunities and Basic Skills (JOBS) program. The report may also have had an impact in Canada where random assignment is much less common in the evaluation of employment and training programs. The federal Ministry of Human Resources Development decided to evaluate its Self Sufficiency Project using random assignment in 1990.

2.3 Eight Influential Studies

Descriptions of eight studies that are cited by the panel, or grew out of work cited by the panel follow[5]. In each case the technique used is described and some lessons learned are extracted. The studies fall into four groups.

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[4]The *Family Support Act* is American legislation that enables welfare payments to parents of dependent children. It also enables the JOBS program which provides funding for and specifies some parameters of training programs that states provide for welfare recipients. The *Family Support Act* is administered by the Department of Health and Human Services.

[5] Later versions of reports of these studies are used since the authors will have had time to improve their papers. The fact that the panel might not have had the benefit of these revisions is not considered relevant here since the purpose of this review is to understand the problems involved in estimating the impacts of programs without the benefit of random assignment. It is not an attempt to understand the decision making process.

1) The Westat and Dickinson, Johnson and West studies use relatively straightforward matching plus regression to estimate the impacts of CETA. They provide an excellent introduction to the data and problems.

2) Papers by Bloom and Bassi illustrate methods for eliminating bias in the face of earnings functions that have error terms which are correlated both with program participation and post-program earnings. Their primary intent is to produce estimates of the impacts of CETA programs.

3) The paper by Ashenfelter and Card tests and compares various methods for obtaining unbiased estimates of program impacts using information within the CETA data set.

4) LaLonde (1986), Fraker and Maynard (1987) and Heckman and Hotz (1989) apply CETA techniques to the Supported Work data[6]. They compare the results produced by various methods with the results produced using the control group.

2.3.1 Westat, Inc. (Bryant and Rupp, 1987)

Westat, Inc. had the contract to manage the CETA database and produce estimates of the impact of CETA, and under that contract they produced a number of reports for the U.S. DOL. Westat first screened the data and then used matching, followed by regression analysis.[7]

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[6] The Supported Work project offered from 12 to 18 months of stable employment with support and close supervision to four groups of severely employment disadvantaged individuals, long term welfare recipients, ex addicts, ex-offenders and young school dropouts.. The Manpower Demonstration Research Corporation (MDRC), formed by the US federal government to run the Supported Work demonstration, used random assignment to estimate the impacts. The total sample size (both treatment and control) was 4,665. The demonstration ran between 1975 and 1979.

[7] Cochran and Rubin (1973) find

2.3.1.1 Screening

Westat began by screening the CLMS and the CPS to exclude:

- those under 14 or over 60 years of age;
- those with personal earnings over 20,000 in the prior year, or family incomes of 30,000 or more;
- those without Social Security Numbers and those whose interview records did not match the Social Security records on at least three of five identifying characteristics: year of birth, month of birth, six characters of surname, sex and race;
- participants who had been in the program for less than one week.

2.3.1.2 Matching

Next they matched program participants with comparison group members from the CPS. Five separate comparison groups were created, one for each program activity: classroom training, on-the-job training, public service employment, work experience, and combinations of activities. Eleven variables were used to create the comparison group for people who enrolled in a CETA program between July 1, 1976, and June 30, 1977:

1 sex
2 SSA earnings in 1976
3 change in SSA earnings, 1975 to 1976
4 change in SSA earnings, 1974 to 1975
5 race/ethnicity

Overall, linear regression is recommended as superior to matching alone when x is continuous and only a moderate reservoir is available... However, it appears that the approach of pair matching *plus* regression adjustment on matched pairs performs best. [Cochran and Rubin, 1973 page 445.]

6 age
7 educational attainment
8 family income
9 prior year labour force experience
10 head of household
11 poverty status

Each variable was divided into discrete categories. (e.g. Educational attainment was divided into six categories.) The divisions chosen resulted in more than 6 million possible combinations of characteristics. Westat refers to each possible combination as a cell. Ideally, Westat would have chosen its comparison group as the members of the CPS file that were in the same cell as program participants. But with millions of cells and only thousands of observations, most of the cells were empty, so they combined cells in reverse order of priority listed above. First, they combined cells with similar values of the first ten variables but with differing poverty status. If this did not produce a match, then they combined cells with similar values of the first nine variables, and so on, until a match was produced. In cases where several non-participants matched a single participant, they weighted comparison group observations by the ratio of participants to non participants in the cell. In practice, Westat was able to get exact matches on the first five variables in every case.

2.3.1.3 Regression Analysis

They augmented this matching technique with a set of 12 regressions, one for each sex and program type[8]. The dependent variable in each case was post-program earnings. Explanatory variables were: a dummy variable for program participation, three years of pre-program earnings, and variables describing

[8] The program Work Experience was subdivided in to Adult Work Experience and Youth Work Experience.

personal characteristics and employment experience in the immediate pre-program year.

Using these techniques they draw four conclusions:

2.3.1.4 Matching is important.

They calculate F-statistics to test the hypothesis that the earnings functions of comparison groups are the same as the earnings functions for program participants. This hypothesis is rejected for both the unmatched sample from the CPS and the matched, but not weighted sample[9]. It is not rejected for the matched and weighted sample.

2.3.1.5 Stratified matching is preferred to one-to-one matching.

They cite the results of Dickinson, Johnson and West, whose one-to-one matched samples failed an F-test of pre-program comparability, while the weighted comparison groups produced by Westat did not.

2.3.1.6 Program impacts vary with age.

They report overall impacts by age group that vary from $-119 (for 14 to 16 year olds) to $920 (for those age 45 and older).

2.3.1.7 Length of time in the program matters.

They report large variations in the impacts of program for people enrolled for different periods. The most striking example occurs with the impact of classroom training on females. That impact increases monotonically from $85

[9] Where the functional form is known, ordinary least squares regression will be robust to weighting. When Bryant and Rupp report different results for the weighted and unweighted samples, they are implicitly reporting undetected non linearity in the functional form of their equation. (See discussion on page 98 and following.)

for those enrolled for fewer than 11 weeks to $1,611 for those enrolled more than 40 weeks.

2.3.2 Dickinson, Johnson and West (1986)

The Dickinson, Johnson and West (DJW) paper is particularly useful because it reports the results of a number of different techniques in order to illustrate the factors to which non-experimental estimates are sensitive. From the perspective of someone interested in the weaknesses of observational studies of employment and training programs, they have four important findings.

2.3.2.1 Matching is important.

Following Cochran and Rubin (op. cit.) DJW estimate the impact of CETA using regression analysis on a matched comparison group and the participant group. When they re-estimate the impact using an unmatched comparison group from the Current Population Survey, they find the estimates change by a statistically significant amount, from $-690 to $-422 for men and from $13 to $537 for women[10].

However, in direct contrast to Westat, they conclude that the method of matching is not important. They show that estimates of the impact using their modified-Mahalanobis-distance[11] matched comparison group produces results that are not statistically significantly different from those produced using

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[10] DJW themselves draw the opposite conclusion, but the difference is one of semantics. In their article in the Evaluation Review (1987) they conclude that regression alone is sufficient to control for pre-existing measured differences, but they also conclude that regression results are sensitive to the inclusion of individuals who were out of the labour force in the CPS sample. I define the process of ensuring that both members of the comparison and treatment groups have the same labour market status as matching, but they define it as "choice of sampling frame."

[11] The Mahalanobis distance between two observations is:

$$D = (X_1 - X_2)' S^{-1} (X_1 - X_2)$$

The distance is modified by weighting the matching variables by their association with variations in earnings.

Westat's cell matched groups. They also show that the sensitivity of the estimates of the impacts to weighting disappears when they include pre-program earnings in the regression equation.

2.3.2.2 Including no-shows is not important.

When DJW change their sample to include no-shows, the overall estimate of impact changes from $-260 to $-273[12]. This result is surprising since no-shows are often felt to have lower than average motivation, and unmeasured characteristics such as motivation often are cited as sources of selection bias.[13] If no-shows do have low motivation, and if motivation is positively correlated with income, then systematically excluding no-shows from the participant group will bias estimates of program impact upward. But DJW's findings indicate that either no-shows are no different in terms of unmeasured characteristics, or the presence of unmeasured characteristics is relatively unimportant given other variables such as history of earnings that may act as proxies for them.

2.3.2.3 Timing is important.

Pre-program earnings are felt to be correlated with program participation and with post program earnings, so in order to avoid bias, researchers needed to include a measure of pre-program earnings in their regression equations. Because earnings data in the CETA studies were annual, this was not straightforward. The relevant pre-program earnings for individuals who enrolled in late 1976 might be early 1976 earnings, while the relevant earnings of enrolees in early 1976 are more likely to be 1975 earnings. To test the sensitivity of their model to the choice of year for pre-program earnings, DJW estimated the model separately for those who enrolled in the first and the last half of 1976 separately. For adult men the estimate of the impact for early enrolees was $-458

[12] This change is not statistically significant.
[13] See e.g. Bloom et al. 1993 page 8.

and for late enrolees $-971. For women, the estimate for early enrolees was $246, and for late enrolees, $-220.

2.3.2.4 Labour force status is important.

Again, DJW estimate the impact of CETA programs first with comparison group members who were not necessarily in the labour force in March 1976, and then restricting the comparison group to those who were in the labour force in March 1976. (CETA participants are by definition in the labour force.) The restriction decreased the estimate of the impact of the CETA programs from $-385 to $-529 for men and from $488 to $299 for women.

2.3.3 Bloom (1987)

Bloom, reporting the work of Bloom and McLaughlin, suggested estimating the impact of CETA by running a regression of the form

$$Y_{it} = a_i + b_i t + \sum_j B_j X_{ji} + C T_{ij} + e_t + e_{it}$$

and

$$e_{it} = r e_{it-1} + v_{it}$$

where

Y_{it}	= person i's earnings in year t;
ai and bi	= person i's pre-program earnings intercept and slope;
X_{ji}	= the jth personal characteristic for person i;
T_{it}	= one for post-program years for participants and zero otherwise;
e_t	= a year-specific error component reflecting economic conditions;
e_{it}	= an individual error component for person i in year t;
v_{it}	= the random portion of person i's error component in year t;
B_j	= the coefficient for the jth personal characteristic;
C	= average annual program-induced, post-program earnings gain;

and

r	= a first order serial correlation parameter.

Bloom acknowledges that his method, using earnings growth equations, cannot resolve the fundamental question of selection bias. This method won't distinguish between program impact and self motivated return to the work force which occurs at about the same time as program participation, a likely source of selection bias in the estimates of program impact on women. Nor will it distinguish between permanent and temporary pre-program dip, a likely source of selection bias in the estimates of program impact on men. He says, "Without a randomised field experiment, there is no definitive way to determine the magnitude of these potential biases." [Bloom 1987, page 516]

Nonetheless, he argues, separate analyses can shed light on these two issues. For women, he looks at the changes in labour force participation, employment, hours worked while employed, and wage between the pre-enrolment and post program years. He finds that only 20% to 31% of the change in earnings is attributable to changes in labour force participation, indicating that the impact of this source of selection bias is significant, but not sufficient to overturn the results.

For men, he looks at fluctuations in incomes of the participants in the pre-pre-program dip period to see whether their income streams had been characterised by similar deviations from trend in the past. He found that they had, and that the dips had decayed quickly. He also looked at the experience of comparison group members who experienced earnings dips in the program year to see if economic conditions at the time of the program might have made the pre-program dip longer in the program year than in previous years. He found that the comparison group's dips were more prolonged, but that this would only increase the estimates of the impact of CETA on men by $130 in the first post program year and $50 in the second post-program year.

Bloom's paper is helpful in three ways. First, it emphasises the importance of individual-specific growth rates in earnings. Second, it illustrates a method for investigating the sources of selection bias. And third, it reinforces the finding of Dickinson, Johnson and West, that labour force participation is an important explanatory variable.

2.3.4 Bassi (1984)

Bassi (1984) extends the work of Ashenfelter (e.g. Ashenfelter 1978) by allowing the change in earnings to be related to transitory changes in income in the pre-program period. Her earnings function takes the form

$$Y_{it} = X_{it}\gamma + P_i\beta + \varepsilon_i + \varepsilon_t + \varepsilon_{it}$$

Where

Y_{it} = earnings of individual i in year t

X_{it} = measured characteristics affecting earnings

Pi = a dummy variable measuring program participation

e_i = an error term specific to individual i, and constant over time

e_t = an error term specific to period t, and constant across individuals

e_{it} = an error term specific to individual i at time t. This is later allowed to vary according to the formula,

$e_{it} = r\, e_{it-1} + V_{it}$.

If the model is correctly specified then:

- in a simple regression of Y on X and P, the coefficient on P in pre-program periods will be zero; and
- the earnings functions in the pre-program periods will be the same for the participants and the comparison group.

Within this framework she tests four assumptions:

i) That selection into the program is random with respect to unobserved variables,
ii) That selection is based only on fixed unobservables (tested for two base years),
iii) That selection is based only on fixed unobservables, but there is serial correlation of the error term, and
iv) That selection is based on transitory characteristics in the immediate pre-program year.

She gets different findings on the reliability of the estimates for each of four groups.

- None of the assumptions is rejected for white women.
- Assumption ii with a base year two years prior to training and assumption iv are not rejected for minority women.
- All assumptions are rejected for white men.
- Only the first assumption is rejected for minority men.

She then reports remarkably stable estimates of impact on white women, $740 to $987 increase in 1977 earning and $1108 to $1452 in 1978. For minority women and models that passed the specification tests, the estimates range from a not-statistically-significant $426 to $626 and from a not-statistically-significant $531 to $947 in 1978. For minority men and models that passed the specification tests, the estimates are again remarkably stable. None are statistically significantly different from zero, and over the two years they range from $27 to $271.

This paper is useful because it illustrates a method for recovering pre-program transitory earnings as a means of controlling for selection bias. It also illustrates a number of specification tests for choosing among competing estimators.

2.3.5 Ashenfelter and Card (1985)

Ashenfelter and Card use the CETA data to test a number of different forms for the earnings equation. They begin with:

$$y_{it} = \omega_i + d_t + D_{it}\beta + \varepsilon_{it}$$

where

yit = earnings of individual i in period t,

ω_i = a permanent component for individual i,

d_t = an economy wide component,

D_{it} = is a dummy variable which takes the value 1 for participants in post-training periods,

β = the effect of training, and

ε_{it} = a serially uncorrelated transitory component of earnings.

If this is the correct specification of the earnings equation and selection into the program is uncorrelated with ω_i and ε_{it} then a simple post-training difference in earnings will estimate the training effect, β. These assumptions also imply that there will be no difference between the incomes of the participants and the comparison group in the pre-program years. The second implication is easy to test, and since comparison group earnings are higher than the participants' earnings in each year, and grow at a faster rate over the pre-program period the model is rejected.

A slightly more sophisticated model allows participation in the program to be governed by the permanent component, ω_i. If this is the correct model then a "differences in differences" estimator will provide an unbiased estimate of program impact. As well, the differences in differences estimator will provide identical estimates regardless of base year chosen. Again, this second implication provides a test of the model. Ashenfelter and Card find that the estimates for men vary dramatically depending on base year and post program year chosen (from $-1,519 to $+813) and so the model is rejected.

Next they allow the transitory income component to be serially correlated. Heckman and Robb (1986) have shown that with this type of earnings function, a symmetric (about the time period in which selection into the program occurs) differences in differences estimator will give unbiased estimates of the impact of training. It should give the same estimate of program impact whether the researcher bases the estimates on one, two or three years before and after the selection year. They report four estimates of the impact of the program based on two assumptions about the year in which selection occurred, and based on symmetric differences of one and two years. With 1976 as the selection year, the estimates were $9 and $439, and with 1975 as the selection year, the estimates were $-736 and $-873. Although, with 1975 as the selection year the estimates are very similar, there is no way within this model to choose between selection years.

Next, by modelling the selection decision as a function of selection year earnings, they are able, in theory to test assumptions about the year in which the selection decision occurs. In their model training occurs if

$$z_i = (\omega_i - \omega) + \varepsilon_{i\tau-k} + v_i < \bar{y} - \omega - d_{\tau-k} \equiv z$$

where the variables are defined as above, and v_i is an additional random component associated with the selection of training. Training occurs in period t, and selection for training occurs k periods before that.

In a variant of this model, the earnings equation is supplemented by a person specific growth rate like that specified in Bloom (above). They find that the inclusion of a trend component of earnings greatly improves the fit of the model.

Since the training decision is based on the permanent, transitory, and growth components of earnings, and since average earnings of trainees are different in 1975 and 1976, assumptions regarding the decision year have implications for the earnings streams of trainees in the pre-program years. Unfortunately, although the estimates of training impact change from $41 when 1975 is the selection year to $747 when 1976 is assumed to be the selection year, the differences between the predicted pre-program earnings based on the two selection years do not differ sufficiently to enable them to reject either of the models.

The estimates of the impact of CETA programs on women are much more robust. The four estimates produced by allowing a person specific growth rate and by choice of selection year vary from $298 to $713. Nonetheless, they conclude that they cannot draw a firm conclusion regarding the impact of the program and call for more work and for more use of random assignment in the evaluation of programs.

This paper is useful because it illustrates a method for controlling for selection into programs based on permanent, transitory and trend components of income. It also underscores the importance of including a trend component in the earnings equation.

2.3.6 Fraker and Maynard (1987)

Fraker and Maynard drew comparison groups from the Current Population Survey (CPS) in order to make comparison-group-based estimates of the impact of the National Supported Work Demonstration. They then contrasted these results with the results obtained using the control groups developed as part of the Demonstration.

The National Supported Work Demonstration was a training program for four hard-to-employ groups: long term AFDC recipients, school drop outs aged 17 to 20, ex-addicts and ex-offenders. It provided employment and job coaching to 566 youths and 800 welfare recipients between 1975 and 1979. Program participants were randomly selected from a pool of eligible candidates, with the remainder forming a control group. Data from surveys of Supported Work participants were augmented by Social Security Earnings data, that were only available in aggregated form for groups of from five to ten individuals. The source for comparison group data was the public use tape developed for Westat.

Fraker and Maynard selected a comparison group from the CPS using cell matching. For youth the cells were based on gender, pre-program earnings, change in pre-program earnings, race/ethnicity, education and age. For welfare recipients the cells were based on changes in pre-program earnings, age, pre-program employment experience, pre-program earnings, and race/ethnicity. They supplemented this matching with an earnings equation regression model. They concluded that

> "had we chosen the 'basic' comparison-group construction procedure and analytic model, we would have arrived a qualitatively similar conclusions to the experimental study findings for AFDC recipients... However, the comparison group methods would have led to quite misleading conclusions about the effects of Supported Work on youth." [Fraker and Maynard, 1987 page 201.]

They also explored a number of different matching techniques[14] and functional forms. The welfare group was fairly robust to the different matching techniques. Estimates of the impact on 1977 earnings ranged from $1,266 to $1,696 compared with $1,423 made using the control group. By contrast comparison-group

[14] In one case, matching was based on predicted earnings. Unfortunately this method could result in the matching of individuals with widely different characteristics, if their predicted earnings were similar.

estimates of the impact on youth in 1979 varied from -$687 to -$1,937 compared to the control group based estimate of -$18. The results for the welfare subgroup were also robust to changes in analytic model, and again the results for the youth subgroup were not.

This paper provides several lessons. First, attempting to estimate impacts of training on youth (in this case 17 to 20 year olds) using a comparison group developed from the current population survey is extremely risky. However, for AFDC recipients the results are much more encouraging. The 'basic' matched comparison group generates estimates of the impact of Supported Work that do not appear to be statistically significantly different from the estimates based on the control group. However, they find that estimates do vary considerably when the treatment group is not matched with the comparison group.

2.3.7 LaLonde (1986)

LaLonde also compares estimates of the impact of Supported Work on participants' earnings produced using control and comparison groups. He draws seven comparison groups each from the Panel Study of Income Dynamics (PSID) and the Current Population Survey, four for women and three for men. The sample sizes in the male PSID comparison groups range from 2,493 for the broadest, to 128 for the narrowest. The female PSID comparison groups range from 595 to 118. The CPS sample sizes range from 15,992 to 305 for males and from 11,132 to 87 for females. Each selection restricts the sample to individuals who are more similar to the participants than the previous one. For example, the narrowest selects unemployed males who were heads of households, less than 55 years old, unemployed in 1976 and had incomes below the poverty line in 1975.

He points out first that a simple pre/post design would produce inaccurate estimates of program impact. The average earnings of the welfare control group grew by $700 per quarter during the 18 months of treatment.

He then reports four different econometric methods for estimating the impact of Supported Work using these 14 comparison groups and contrasts the results with the same methods applied to the control group. The four methods are:

i differences between treatment and comparison groups' pre/post earnings growth, the difference in differences estimator;
ii the same difference in earnings growth, but controlling for pre-training earnings;
iii the same as ii above, but including additional explanatory variables; and
iv Heckman's two stage technique for controlling for selection bias.

In each case the estimates based on the Supported Work control group are remarkably stable. By contrast the estimates based on the CPS comparison group fluctuate widely.

He finds very different results for the same estimation technique, but different comparison groups. He points out that many of his estimates are straw men in that they would not have been reported since they failed simple specification tests. LaLonde suggests that a researcher might reasonably not reject the second or third methods, yet he points to large variations in the results they might obtain depending on the comparison group used and the variables used in the regression equations.

The last procedure is Heckman's two step procedure in which the likelihood of participation is estimated first, then the predicted likelihood from that equation is used as an instrument for participation in the earnings equation. This method produces estimates that range from $439 to $1,564 for women and from $-1,333 to $213 for men depending on the comparison group and which variables were used in the participation equation. The estimates based on the control group ranged from $837 to $861 for women and from $889 to $899 for men. He concludes that,

> even when the econometric estimates pass conventional specification tests, they still fail to replicate the experimentally determined results. [LaLonde 1986, page 617.]

This paper compares estimates of the impact of a program using a control group and various comparison groups. It finds that the results produced with the control group are remarkably stable across different econometric specifications, while the estimates using comparison groups are not. Unfortunately, the data used in this analysis have only one year of pre-program earnings, so it was not possible to test any of the earnings functions.

2.3.8 Heckman and Hotz (1989)

Heckman and Hotz also use data from the National Supported Work project to test non-experimental estimators of program impact[15]. They test their estimators in three ways. First, they estimate their models using pre-program earnings as the dependent variable, and look for significant coefficients on program participation. Next, they look for significant coefficients on incomes for years that would be superfluous if the earnings were specified correctly. Finally, they look for significant coefficients on a dummy variable indicating that the observation is for a member of the control group, rather than the comparison group.

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[15] They use the same data as Fraker and Maynard. This includes extensive income histories from the Social Security Administration, but is grouped. The data used by LaLonde were for individuals, but only had four years of earnings data, including only one year of pre-program earnings.

They perform their tests on two versions of each of three models. The three models are:

1. Linear control function estimator which embodies the assumption that selection into a program is based on observed characteristics, and therefore an unbiased estimate of program impact can be recovered from the coefficient on a dummy variable for program participation in an OLS estimation of an equation of the form

$$Y_{it} = C_i \delta_t + d_i \alpha_t + \overline{v_{it}}$$

2. Fixed effects estimator that embodies the assumption that although selection is based on unobserved characteristics, those characteristics do not change over time. In that case unbiased estimates could be recovered from an OLS estimation of the form

$$Y_{it} - Y_{it'} = d_i \alpha_t + (X_{it} - X_{it'})\beta + (v_{it} - v_{it'})$$

3. Random Growth estimator that allows the unobserved characteristics that are related to selection into a program to grow at a constant rate over time as well as a fixed component. In this case a consistent estimate of the impact of the program will be given by the coefficient on a dummy variable for program participation in an equation of the form:

$$(Y_{it} - Y_{it'}) - (t - t')(Y_{it}' - Y_{it'-1}) = d_i \alpha_t + [(X_{it} - X_{it}) - (t - t')(X_{i't} - X_{it'-1})]\beta + [v_{it} - v_{it'}) - (t - t')(v_{it} - v_{it'-1})]$$

In each case

Yit = income of individual i in period t;

Xit = a vector of explanatory variables for individual i at time t. and

v_{it} = an error term for individual i at time t.

The second variant for each model is obtained by replacing the dummy variable for program participation by a vector of personal characteristics so that program impacts are not constrained to be identical for all individuals.

The results of these tests are damning for studies that do not use random assignment. For youth there is only one model that has a greater than 5% chance of not being rejected in each of the tests[27]. This is the random growth estimator with the full set of control variables and with program impacts that are allowed to vary with personal characteristics. Even this model has only a 12% probability that the coefficients on program participation are zero. These data and techniques are not likely to give researchers much confidence in the estimates.

The results are even worse for AFDC recipients. In that case one model does quite well on the pre-program earnings test, with an 82% probability that the coefficients on program participation are zero. A researcher would be justified in placing some confidence in this model, but the point estimates of the impact produced by this model are twice as high as the estimates produced using the control group.

2.4 Lessons Learned

The main lesson from CETA is clear. The CLMS and CPS data, even when augmented with 20 years of earnings history and extensive statistical analysis

[27] I would like to see the probability that a model is not rejected to be 95% in each case in order to have confidence in the model.

were not sufficient to produce estimates of the impact of CETA that engendered confidence. The Advisory Panel recognised this, saying,

> State-of-the-art statistical techniques simply cannot overcome certain data or design problems. [Stromsdorfer et al., 1985 page III-J-58]

Clearly, observational studies must be viewed with a great deal of scepticism. Although the CETA results apply directly to estimates of training programs, pessimists must wonder how other observational studies would fare if random assignment studies to which they could be compared existed. The main lesson from CETA is that a greater understanding of the nature of selection bias is needed.

The many excellent analyses of CETA identified some factors to which estimates of the impact of the CETA programs were sensitive. Four of these are listed below.

1) The treatment of pre-program dip is central. Bloom examines the characteristics of the pre-program dip and finds that male program participants have had similar dips in earnings in the past, and that some females changed their labour force status in the pre-program period. Ashenfelter and Card find different growth rates in earnings between program participants and the comparison group. Dickinson, Johnson and West find that it is important to select a comparison group with characteristics (such as labour force participation) like those of the participants at the date of enrolment. In the CETA data the year defined as the pre-program year, and therefore the year expected to contain the pre-program dip in earnings, ended up to 11 months before entry into the program. For the comparison group, dips in earnings always referred to dips in earnings in the year that ended 3 months before the interview.

The lessons learned are:

i Comparison groups for males should be composed of individuals who have had similar variations in earnings in the past.
ii Comparison groups for females should be composed of individuals who have a similar attachment to the labour force at the time of enrolment.
iii The pre-program dip of the comparison group and the treatment group should be measured in the same way.

2) Matching is important. Dickinson Johnson and West find that their estimates were not sensitive to matching, except when they had not screened out individuals who were out of the labour force. However, LaLonde finds that his results are very sensitive to the choice of comparison group, even when regression is also used. Fraker and Maynard find the estimates for youth to be very sensitive to the comparison group used, and the estimates for AFDC participants to be sensitive to matching.

The lesson learned is:

Comparison groups should be matched to participants.

3) Program impacts vary with personal characteristics. Every researcher found different impacts for different groups of participants. Impacts varied with sex, age, minority status, and duration of time spent in the program. They also vary over time.

4) Specification tests are not sufficient. Bloom has pointed out that a change in the earnings function that coincides with enrolment in the program cannot be

distinguished from program impacts[17]. Heckman and Hotz report models that pass tests based on pre-program information, but that produce different estimates than those produced using random assignment. And Dickinson, Johnson and West point to the issue of timing. If the pre-program dip in earnings is undetected because it occurs in the year of program participation, then the earnings functions of participants and comparison group members can appear to be identical, and yet be very different.

[17] Heckman and Hotz include post-program tests of the functional form of the earnings equation. However, if the purpose of the training is to change the earnings function, failing a post-program test does not necessarily indicate bias.

3. Recent Developments

The decade since two separate blue ribbon panels advised the US Department of Labor to use random assignment to estimate the impacts of employment and training programs has seen some progress in our understanding of both the effectiveness of programs and the difficulties in estimating their impacts. The use of random assignment has become much more common. The leader in the field, the Manpower Demonstration Research Corporation (MDRC) has been involved in 20 projects that involved the assignment of over 100,000 individuals. These studies have given us fairly reliable estimates of the impact of a number of programs on a number of groups in a number of sites, however, the generalizability of the findings has not been established. On the other hand, although our knowledge of the selection process into the American JTPA programs has improved, no convincing observational technique for estimating impacts has emerged.

This chapter summarises the post-CETA debate on observational studies of employment and training programs in order to identify gaps in our understanding of the extent and sources of uncertainty. It has six sections. The introduction sets the stage by drawing a few lessons from a number of studies that used random assignment. This is followed by four sections, each of which summarises a recent study. The four studies are:

1) Heckman and Smith's (1993a) assessment of the case for random assignment;
2) Friedlander and Robins' (1994) assessment of a number of attempts to estimate the impacts of programs without the use of random assignment
3) Cain, Bell, Orr and Lin's (1993) attempt to estimate the impacts of programs without the use of random assignment, and
4) Park, Power, Riddell and Wong's (1994) assessment of other estimates of the impact of Canadian federal training programs together with their own estimates.

The first study is included because it provides a good summary of the reasons that the need for observational studies remains. The second and third studies are the most recent assessments of observational studies. They show that none of the sources of comparison groups, when combined with the data available, is sufficient to generate unbiased estimates of program impact. The final paper is a good overview of the state of estimates of program impact in Canada.

The chapter concludes by identifying four questions that will be addressed in Chapter Four, but that remain unanswered in the literature:

3.1.1 Random Assignment Studies

Manpower Demonstration Research Corporation (MDRC) is the leader in the implementation[1] of studies involving random assignment in employment and

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[1] Researchers external to MDRC carried out much of the analysis. Mathematica Policy Research Inc. carried out much of the analysis associated with the Supported Work Demonstration. Abt Associates Inc. is carrying out the analysis associated with the National JTPA Study. More recently MDRC has increased its in-house analytical capacity.

training programs. It has been involved in 20 studies in which, in aggregate, over 100,000 individuals have been assigned to treatment or control groups. The magnitude and breadth of these studies has changed the nature of program evaluation in the United States and has had a major impact on social policy in the United States. Greenberg and Wiseman (1992 p. 136) conclude that

> there exists a substantial consensus among persons active in welfare policy that the OBRA demonstrations, particularly MDRC's evaluations of them, had a major effect on the course of the debate and, possibly the success of the effort.

A detailed examination of their many studies is beyond the scope of this thesis. However two of their findings provide particular insight into the evaluation of employment and training programs whether random assignment is used or not.

The first finding is that training can increase earnings and employment for welfare recipients. Although this finding is considered self-evident by many, attempts to establish it in, for example, the CETA evaluations were unsuccessful. In addition, these studies illustrate the range of sizes of impacts that can be produced. For example, the largest impacts in any program, those in Riverside county, California, decreased welfare payments by an average of 15% in each of three years[2].

The second finding of interest is that the variability of estimates of impact across sites within a program is as great as the variability across programs. For example, the impact of California's Greater Avenues to Independence (GAIN) program on AFDC (welfare) payments varied from $+114 over three years in Tulare county to $-1,983 over three years in Riverside county. In contrast, seven

[2] Impact is estimated across all member of the treatment group, 60% of whom received any services through GAIN.

other welfare-to-work programs with a wide variety of approaches from supervised job search to intensive training had impacts ranging from zero to $-1,396. (figures from Gueron and Pauly 1991 and SRDC/MDRC 1995.)

Although the results from random assignment studies are considered incontrovertible by policy makers[3], social scientists have expressed concerns about their robustness and generalizability. Heckman and Smith (1993b) and Heckman, Clements and Smith (1993) find the estimates of the impact of the JTPA programs are sensitive to treatment of outliers[4] and to the choice of sites[5], suggesting that the results may not be robust. Hotz points out that in the JTPA program less than 10% of the sites that were invited to participate actually participated. He concludes that "the lack of a well defined sampling frame for the resulting sites makes it difficult to generalise from this set of sites to the population as a whole." (Hotz, 1992 page 97). This combined with the extreme variation across site, but within program identified by MDRC suggests that caution be used in generalising from the results of random assignment studies.

3.2 Heckman and Smith (1993a)

Heckman and Smith use information from the National JTPA Study, a study of the American Job Training Partnership Act programs (that used random assignment) in order to assess the case for randomisation in the evaluation of social programs. They identify six problems with random assignment studies.

[3] For example, Greenberg and Wiseman (1992, page 136) quote Dr. Erica Baum, Senator Moynihan's principal assistant for welfare policy as saying, "MDRC's findings were unambiguous ... [and] not subject to challenge on methodological grounds."

[4] Estimate of aggregate impact on the earnings of youth falls from $-1,154 to $-588 with the exclusion of the top 1% of earners from the sample. (Heckman, Clements and Smith (1993).

[5] Estimates of the aggregate impact vary from $-310 to $-1,107 as each of the 15 sites is excluded in turn. (Heckman, Clements and Smith, 1993)

- First, they point out that randomisation is likely to change the pool of participants, that will limit the generalizability of the results. In the vernacular of people estimating the impacts of programs, studies strive for internal and external validity. Random assignment, properly implemented, will guarantee internal validity. That is, a comparison between the treatment and control groups will generate an unbiased estimate of program impact[6]. External validity enables the application of the findings from the sample to the population at large. It can be guaranteed by using random selection to draw the treatment and control groups from the population at large. This did not happen in JTPA. Doolittle and Traeger (1990 p ix) report "one objective of the original study plan has not been achieved: recruitment of a statistically representative sample of sites."
- Second, while random assignment can generate an unbiased estimate of the mean of the distribution of the impacts of a program, it can only generate bounds on the distribution of the impacts[7], and in practice, they find that the bounds are rather wide. Using data from the JTPA evaluation, they find that the results are consistent with from 0 to 28% of adult male participants having had their employment prospects diminished by participating in the program.
- Third, randomisation will change the nature of the program. They quote Doolittle and Traeger (1990) in their report on the implementation of the JTPA study who say, "implementing a complex random assignment research design in an ongoing program providing a variety of services does inevitably change its

[6] This follows from the Central Limit Theorem which says that the mean of *every* random sample from a probability distribution (with finite mean and variance) will be normally distributed about the mean of the parent distribution with variance $\sigma^2/\sqrt{N}$ where σ^2 is the variance of the parent distribution and N is the sample size. Since the treatment and control groups are both random samples of the same population the mean of any variable that describes them will be the same except for sampling variation and the effect of the treatment.

[7] As noted above, the Central Limit Theorem provides the theoretical justification for the interpretation of the differences in means between the treatment and control groups as estimates of the impact of treatments. Clements, Heckman and Smith (1993) explore the extent to which the distribution of impacts can be recovered.

operation in some ways." An example of this is referred to as the Hawthorne effect, named for a study at the Western Electric Hawthorne plant in Chicago that purported to find that a subject's awareness of being under study could affect the actions being studied.

- Fourth, they point to the problem of drop-outs. Fully one third of "participants" in the JTPA program received no services. As a result, the JTPA experiment will not generate an estimate of the impact of training on those actually trained without using non-experimental methods.
- Fifth, they point to the difficulties encountered in defining the treatment received. For example, in the JTPA experiment, of females included in the on-the-job training stream, just over half received any service, and only half of those receiving service received on-the-job training. Clearly, random assignment cannot, by itself, generate an estimate of the impact of on-the-job training in this case.
- Sixth, they cites problems with JTPA controls finding substitutes for the training that they are denied through randomisation. Overall 32% of controls reported receiving training compared with 48% of the treatment group.

They conclude with a call for greater use of non-experimental estimates of program impacts that will simultaneously estimate the factors affecting program participation and outcomes.

3.3 Friedlander and Robins (1994)

These authors assess four sources of comparison groups for their suitability by comparing the estimates of program impact produced by each with estimates produced using a control group. In addition, they assess the usefulness of pre-program tests in determining which of the estimates are reliable.

Their information comes from four American employment and training programs for welfare recipients that were evaluated by MDRC in the mid 80's using random assignment. Although the four programs had the same goal, to increase employment and income, and the same target group, single parents, they took very different approaches (the average cost per participant varied from $118 to $953) and occurred in very different jurisdictions, Baltimore, Arkansas, Virginia and San Diego.

The authors construct four comparison groups for each of the four programs. The first is simply the control group from a program in another state. The second is generated by selecting for each participant, the non participant who is most similar in measured characteristics. The third comparison group is selected from control groups in different program sites, within the same state. The final comparison group was selected from control groups in the same site, but from a different time.

Estimates of program impact are generated by estimating the parameters of a linear regression model[8] across the treatment and control/comparison groups. Two models are estimated, each with bivariate dependent variables. The first takes the value one if the individual had any earnings in quarter three, quarter one being the quarter in which random assignment occurred. The second takes the value one if the individual had any earnings during quarters six through nine. The independent variables in the regression include employment in the

[8] These authors may be criticised for using a linear regression model when their dependent variable is binary. In general heteroskedasticity will render OLS inefficient and some meaningless results may be generated (predicted probabilities greater than one or less than zero, or negative variances). Nonetheless, I doubt that these problems will jeopardise their results. Greene (1983) and Chueng and Goldberger (1984) and Stoker (1986 all cited in Greene 1990 p 693 to 695) have, found that under many circumstances (e.g. if the probit model is correct, and if the regressors are multinormally distributed) then, in the probability limit, the OLS estimates are directly proportional to the probit estimates.

immediate pre-program quarter, a vector of demographic variables, and a dummy variable which takes the value one if the sample member is in a treatment group and zero otherwise. Although they have four quarters of pre-program employment and earnings, they find that including more than one quarter of pre-program data has little impact on the non experimental estimates.

Friedlander and Robins also re-estimate each model with pre-program employment as the dependent variable. A statistically significant coefficient on program participation in this model indicates mis-specification.

They present a summary table of 160 comparisons between estimates made with control groups and those made using comparison groups.[9] The cross-state estimates did very badly. Seventy percent of the estimates were statistically significantly different from the estimates produced using random assignment. Matching only reduced this to 58%. Almost half (47%) of the estimates resulted in a different inference (38% for the matched comparison groups). The within-state estimates are much better, but hardly encouraging. Thirty-one percent of the cross-site and 4% of the cross-cohort estimates were statistically different from the estimates produced using random assignment. Thirteen percent of the cross-site and 29% of the cross-cohort estimates resulted in a different inference.

The specification test provided some help in discriminating between estimates that were similar to those made with control groups and those that were not, but they conclude that the test "was more effective in eliminating wildly inaccurate

[9] They estimated 96 pairs of equations for unmatched cross-state comparisons. [Four programs times four comparison groups (each of the other three states individually, plus the three states combined) times two dependent variables (short-term and long-term unemployment) times three subgroups (short-term recipients, long-term recipients and the combined group)]. Similarly, they estimated 24 pairs of equations based on matched comparison groups and 40 pairs of equations for within state comparisons.

'outlier' estimates than in pinpointing the most accurate non experimental estimates." (Friedlander and Robins 1994 page 18)

3.4 Cain, Bell, Orr and Lin (1993)

Cain, Bell, Orr and Lin also make an assessment of an observational study design by comparing the results it produces with the results generated using random assignment. They draw their comparison group members from those who applied for, but did not participate in the program in question. On the basis of the analysis of Friedlander and Robins this is a promising group with which to start because the comparison group is both within-state and contemporaneous with the participants.

The data comes from the Homemaker-Home Health Aide Demonstration, a training and employment program for American welfare recipients. The demonstration took place in seven states. Of the 11,102 applicants, 9262 showed up, were determined to be suitable, and were randomly assigned either to the treatment or control groups. Of those assigned to the participant group, 725 did not attend. Of the 1,840 applicants who did not make it as far as random assignment, 909 dropped out and 931 were screened out by program administrators. Cain et al. assess these latter two groups as potential comparison groups.

By selecting their comparison group from applicants, the authors argue that they have dealt with a number of sources of selection bias. The applicants have demonstrated self-selection, they are participating in the same labour market as participants, and they will have experienced the pre-program dip in earnings. The remaining task is to model the self-selection of the drop outs and the administrative selection for those who were screened out.

The principal technique for analysing the data is the estimation of the parameters of a model of earnings. The dependent variable is post-program earnings. The explanatory variables are personal characteristics; dummy variables indicating that the sample member is a participant, control, no-show, screen out or drop out; and the administrators' subjective assessment of the sample member's suitability for the program. They use the results to assess the suitability of each of the three groups of non participants, (no-shows, screen outs and drop outs) to serve as a control group. In addition they assess the usefulness of the subjective ranking variable.

They list a number of weaknesses in their data. First, their earnings information, which comes from income tax records, is grouped with a minimum group size of ten in order to protect the confidentiality of the income tax records. All groups are homogeneous with respect to the applicant groups, and "most are homogeneous with respect to race ... and the subjective ranking variable" (Cain et al. 1993 page 4). The values of the characteristics used in the regression are mean values. Second, the subjective ranking is not reported for 63% of the drop outs, and 36% of the screen outs. Finally, information on drop outs and screen outs is only available for one of the cohorts, comprising about half the sample.

They find first, that the conventional independent variables, age, marital status, education, etc. have very little impact on the difference between the earnings of the control group and of the other groups of non participants. In contrast they find that pre-program earnings are a very important explanatory variable (T=9) even four years later. These two findings are at least in part the result of grouping the data. They also find differences between the controls and each of the potential comparison groups that persist, even in the face of all of their explanatory variables. Although these differences are not always statistically significant, they range from 4.3% to 7.7% of control group earnings compared with a program impact of 13.9%.

Finally, they find that regression analysis reduces the control group estimates of the impacts of the program by $187 or 4.8% of control group earnings, suggesting that assignment might not have been completely random.

They conclude that the use of applicants who either are screened out, or who drop out from the program together with information on the subjective assessment of the suitability of applicants for the program shows promise for producing estimates of the impacts of programs where random assignment is not possible.

3.5 Park, Power, Riddell and Wong (1994)

This paper reports five estimates of the impact of five Canadian Unemployment-Insurance sponsored programs on subsequent earnings. The five estimates are produced as follows.

1) They compare the earnings of participants and the comparison group in the post-program period.
2) They compare the growth in earnings of the participants and the comparison group. They refer to this as a "differences in differences"[10] estimator.
3) They re-estimate the differences-in-differences estimator using different base years.

[10] cf. Ashenfelter and Card (1985) discussed in Chapter 2.

4) They use a two step estimator, in which the first step models selection into the program and the second step uses these estimates to correct for selection bias in a model of earnings.

5) They compare the rate of growth of earnings of the participants and the comparison group.

The estimates are based on information on program participation, personal characteristics and earnings for 3,377 individuals, 927 individuals who were receiving UI, but who did not participate in any of the five programs (the comparison group) and 2,450 trainees. The information on personal characteristics, percentage of time employed and welfare dependence come from a series of mail-in questionnaires. The information on annual earnings comes from T4's, information slips that are produced by all employers for all employees, as required under the Income Tax Act.

Before proceeding with the estimation, they note four characteristics of the earnings data. First they note the absence of a pre-program dip in earnings, although they note that earnings were depressed in the year in which training occurs and the following year. They also note that a similar dip in earnings is "likely to be observed for some UI claimants." [op. cit. page 12] As a result, the comparison group will also experience a dip in earnings, at least in one year, since the comparison group is drawn from those receiving UI. Second, they note substantial growth in earnings among many of the trained groups that is not evident in the comparison group. Third, they note that average pre-program earnings vary widely across the programs and comparison group. Finally they note that although changes in earnings vary across the program groups with the rates of change correlated with levels of earnings, the changes in earnings are relatively constant across the pre-program years.

The combination of these observations suggests to the authors that selection into program is correlated with the level of pre-program earnings, but not the rate of change of pre-program earnings. This implies that:

1) The difference between the earnings of the comparison and treatment groups will be a biased estimate of program impact. If post-program earnings are correlated with pre-program earnings and with program participation, then estimates of post-program earnings that do not control for pre-program earnings will be biased.

2) The differences-in-differences estimator may produce unbiased estimates of program impact. This estimator controls for differences in pre-program earnings and implicitly for the unmeasured fixed variables that caused the differences. If there are no transitory differences between the participants and the comparison group that are correlated with program participation and post-program earnings, then this estimator will yield unbiased estimates of program impact.

They find that the simple difference between the post-program earnings of the comparison group with the trainees yields estimates of positive impacts of the programs in which participants had high pre-program earnings, and negative impacts for those in which the participants had low pre-program earnings. When the second (the differences in difference) estimator is used the estimates change dramatically. For one of the programs the estimate changes from -$3,334 to +3,458 per year.

Next, they estimate the impact using the differences in differences estimator for several groups and several base years. They find that the estimates of impact do not vary with the base year for those who received training in 1988, but did vary substantially for those who began in 1989. The results for the 1989 cohort suggest that program participation is correlated both with permanent and transitory characteristics that are in turn correlated with subsequent earnings. This conclusion is tempered somewhat by the observation that sample sizes are reduced for the analysis by cohort, and although the differences are large (just under $4,000 for two of the five programs) they are not statistically significant ($t \approx 1.4$). They also find that the differences are greater the farther apart are the base years.

If these transitory characteristics have their impact through changes in the growth rate of earnings, then the fifth estimator will be appropriate. They find broad similarity between the results from this estimator and those produced using the differences in differences estimator. The most notable difference occurs for one cohort in one program in which the estimate of the impact was halved from a statistically significant $4,000 to a not-statistically-significant $2,200.

Finally, they estimate the joint determination of earnings and selection into training. They model selection into training using a multinominal logit model in which there are six possible outcomes, participation in each of the five training programs and no training. In addition to personal characteristics, they have four variables that are expected to affect the training decision:

- a dummy variable indicating that the individual received counselling related to training,
- a dummy variable indicating that the individual stated that he believed training to be important,
- a dummy variable indicating that the individual stated that those close to him believed training to be important, and

- a dummy variable indicating that the individual expected to be recalled by his employer.

They use this model to form a selection bias correction term which is then included in an equation in which post-program earnings is the dependent variable and program participation dummies included as explanatory variables.

They report that cross-sectional OLS estimates of program impact yield negative estimates of the impact of programs that attract individuals with low pre-program earnings and positive impacts of programs with high pre-program earnings. The inclusion of the selection bias correction term changes the coefficients associated with the low pre-program earnings from negative and statistically significant to not significantly different from zero and those with high pre-program earnings from positive and statistically significant to not significantly different from zero.

The conclusions that one would draw from the two step estimator and the differences in differences estimators are clearly different. The two step estimator finds no significant impact of training on earnings, while the differences in differences estimator finds positive and significant impacts for three of the five programs in at least one cohort.

They conclude that the differences in differences estimator is more credible, and that the two step estimator is "not able to completely offset the very strong tendency of a cross-sectional analysis using data on post-training earnings alone[11] to estimate a positive impact for programs whose participants have above

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[11] This is the technique used in earlier estimates of the impacts of these programs. See, e.g., Goss, Gilroy & Associates Ltd. 1989.

average earnings in the absence of training and a negative impact for programs whose participants have below average earnings in the absence of training." (Park, Power, Riddell and Wong 1994, page 29)

3.6 Conclusions

This chapter has reviewed the recent literature on North American training programs for disadvantaged workers. Two conclusions can be drawn.

1) Random assignment has not provided all the answers needed to make informed policy decisions regarding employment and training programs. MDRC has found greater variation across sites than across programs. Clearly generalising from the results found at any particular site would be risky. Another jurisdiction implementing a program with a sequence of activities like those in GAIN could not determine whether it was getting results like those in Riverside county or Tulare county without estimating the impact in its own jurisdiction.[12]

2) Researchers have yet to come up with a set of guidelines for observational estimates of the impacts of employment and training programs that are likely to result in estimates that are free from selection bias.

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[12] This finding has led Greenberg, Meyer and Wiseman (1993) to call for the government to implement multi-level studies, with different strategies implemented at different sites in order to "pry the lid from the black box" of employment and training programs.

In their assessment of the case for random assignment in the evaluation of social programs Heckman and Smith point to the desirability of the structural approach. The specification of training inputs and the estimation of the relationship between outcomes of interest and these inputs makes it possible to predict the impacts of programs not yet operating. They also point to the need for knowledge of the program inputs in order to implement the structural approach, and so confine most of their attention to the problem of estimating the impact of existing programs.

However, five pieces of information that would assist in producing such guidelines are not available in the literature. If we wish to estimate the impact of employment and training programs when random assignment is not feasible, then we must work to increase our understanding of selection bias. Testing for selection bias is clearly necessary to increasing our understanding. These tests are generally made by comparing the results of observational studies with random assignment studies of the same program[13] or by comparing the means of pre-program values of important variables for the treatment and comparison groups. The work of Friedlander and Robins (based on one year of pre-program data) concludes that pre-program tests for selection bias are not reliable when only one year of pre-program data is available. This suggests the first question.

1. Are pre-program tests for selection bias reliable when more than one year of pre-program information is available?

The work of Park, Power, Riddell and Wong, like the work of Ashenfelter and Card (1985) suggests comparing the estimates produced using different base years as a test for the reliability of differences in differences estimators. This suggests the second question.

2. Does robustness to choice of base year mean that differences in differences estimators are unbiased?

Clearly, it would be advantageous to be able to test for selection bias both in the absence of random assignment, but if pre-program tests are not valid, we need a different type of test.

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[13] See discussion on page 124.

3. Can we conduct post-program tests for selection bias in the absence of random assignment?

Even if we have reliable tests for selection bias, we may find it difficult to generate unbiased estimates of program impact, if selection is based on unmeasurable variables. Bloom et al. (1993 page 9) claim "without perfect measures of the unmeasured variables, one cannot be certain whether the selection bias has been removed," and they cite motivation as an unmeasurable that is likely to cause selection bias. (ibid. page 8)

4. Are unmeasurable variables the primary source of selection bias?

Finally, and perhaps mainly for rhetorical purposes, we have to be able to explain the reasons that different researchers come up with such different estimates of the impacts of the CETA programs. If we do not identify the sources of the discrepancies, then surely, anyone wishing to use the results of an observational study would have good cause to worry that if a different researcher were to analyse the data, the estimates would be substantially different. And yet this question remains unanswered in the literature.

5. Why did different researchers come up with such different estimates of the impacts of the CETA programs?

The next chapter provides background on the BC study, including a description of the data and approach taken. Chapter 5 uses that data and approach to address these five questions.

3A. The Character And Relative Advantages Of Random Assignment, Data Collection, One Stage Regression Techniques, Two Stage Regression Techniques And Matching In Estimating The Impact Of Employment And Training Programs.

3A.1 Introduction

Although there is a large number of interesting questions regarding the impacts of employment and Training programs (See e.g. Heckman and Smith, 1995), as indicated in Chapters 2 and 3, there has been little agreement on how to answer even the simplest of these questions, "What is the average impact of training on the trained?" Producing transparently reliable answers to that question without the use of random assignment is the fundamental goal of this thesis.

This chapter has three sections. The remainder of this section gives a definition of transparency and gives some reasons why transparency might be considered a desirable goal. This is followed by a listing of three of the obstacles that need to be overcome in order to achieve reliable estimates. The second section deals with the issue of undetected non linearity. Within that section is a discussion of ordinary least squares regression, its character and relative advantages. This is followed by a discussion of matching and its advantages and disadvantages relative to ordinary least squares. Finally, there is a discussion of the seriousness of undetected non linearities. The third section deals with selection bias. Within that section are discussions of random assignment, two stage regression techniques and collecting more data.

3A.1.1 Transparency

I define transparency as the ease with which an reader can grasp an argument. Transparency will decrease as the number of assumptions that must hold for the argument to hold increases. Transparency is not the same as reliability. A very complex argument that relies on a large number of assumptions, will be reliable if all of the assumptions have been tested. However, if an argument is complex, a policy maker will have to rely on others with expertise in the field to assure him or her that all the necessary tests have been completed. The argument is not transparent to the policy maker. Greenberg and Wiseman (1992) attribute the success of random assignment studies in influencing policy to this transparency.

Transparency is rarely a goal within economics. This would not be a problem if policy makers had demonstrated a willingness to rely on the advice of economists. But however willing policy makers may be to rely on advice of economists as economists elsewhere, they clearly are not in British Columbia. Here it is common for politicians when disputing the results of a study to say, "That was *your* economist who said that. I could hire one to say the opposite." However unjust this type of comment, it bespeaks the need for arguments that can be expressed simply, arguments that are transparent.

3A.1.2 Notation

For consistency throughout this chapter, the following notation is used

1 $$Y = g(X, U_1, T)$$

where Y is the outcome of interest;

g is an unspecified function,

X is a vector of measured characteristics that affect Y

U_1 is a vector of unmeasured characteristics that affect Y

and T is a dummy variable that takes the value 1 if the individual participates in training.

2 $T = h(Z,U_2)$

where Z is a vector of measured characteristics that affect the decision to participate in training

h is an unspecified function,

U_2 is a vector of unmeasured characteristics that affect the decision to participate in training.

3A.1.3 Sources Of Error

There are three sources of serious error that can affect estimates of the impacts of employment and training programs:

1. non response bias in the survey collecting the data.
2. undetected non-linearity.
3. selection bias.

As shown in Chapter 4, section 4.3.3, non response bias can qualitatively change the estimates of the impact of programs.[1] This is a common source of error. Even with a response rate of 90%, Card and Robins (1996) estimated that non response could have biased their estimates by 10%.

Mathematically, this source of bias is indistinguishable from selection bias, and so is not discussed further here.

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[1] Typically, economists do not take responsibility for the data that they work with, trusting that the survey research organization has produced a clean an reliable data set with which to work. However, in my view, we are more likely to re-establish the trust of the policy makers by producing reliable estimates than by blaming others for their lack of reliability.

3A.2 Undetected Nonlinearity

3A.2.1 One Stage Regression

Once we have picked a form for the function g in equation 1, we can use regression techniques such as ordinary least squares, non-linear least squares or maximum likelihood to estimates its parameters. The Gauss-Markov Theorem shows that Ordinary Least Squares regression produces the lowest variance estimate among those that are linear and unbiased. Of the standard assumptions that underlie the classical linear regression model, the assumptions of

1. no correlation between the regressors and the residual;
2. residual has zero mean.

are sufficient to show that the OLS estimates are unbiased. Additional assumptions, that the residuals are independently, identically distributed are sufficient to ensure minimum variance of the estimates. The assumption of normally distributed error terms is used in the calculation of variances. When it holds, the OLS estimates are also maximum likelihood.

If the first assumption is violated the OLS estimates will be biased as follows.
First re-specify equation 1 in linear form and include T in the X variables.

$Y = X\beta + U\delta$

The OLS estimate of β is

$(X'X)^{-1}X'Y$

Substituting for Y gives

3 $\qquad \beta + (X'X)^{-1}X'U\delta$

Unless X'U = 0 or δ = 0 the estimate of β will be biased.

X'U ≠ 0 and δ ≠ 0 can occur if there are non linearities in the true functional form, that have not been captured in the specification. In theory this is not a serious problem. The inclusion of extraneous variables will not introduce bias so, with sufficient sample sizes, a cautious researcher can include terms for all expected non linearities. Alternatively, a flexible functional form can be specified if the researcher is particularly concerned that the true specification is log linear. The value of λ indicates whether the true specification is linear, log-linear, or somewhere in between. In addition any specification can be tested against any competing specification by means of, for example, Wald, Lagrange multiplier or likelihood ratio tests. In the final analysis, however, the judgement of the researcher is involved. Has the researcher included enough terms to capture the non linearities? Has the researcher tested enough alternate specifications?

3A.2.2 Can Undetected Nonlinearity be a Problem?

It may be true that policy makers in jurisdictions other than British Columbia are willing to rely on the professional judgement of economists with respect to tests of functional form. However, some evidence exists that such trust may be misplaced. In their analysis of the impact of the CETA programs, Dickinson, Johnson and West (1986) specify a functional form with 45 explanatory variables of which 17 deal with interactions and non linearities. Nonetheless, the estimate of impact on earnings changes from a not-statistically-significant $13 to a statistically significant $537 when the equation is estimated on the matched and unmatched comparison group. As indicated below, this indicates that even this elaborate functional form is mis-specified in such a way as to introduce bias.[2]

Of the non linear terms included by Dickinson, Johnston and West, four had statistically significant coefficients: age cubed; age * education; age squared * education; and age * married. A quick perusal of functional forms reveals that the inclusion of these variables is rare. Payne (1991) could have included all of these but included none (although age was

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[2] For another view see Heckman and Robb (1986, page 289) who assert, "Recent claims about the robustness of matching methods in the case in which the functional form of a regression model unknown are not yet supported by systematic theoretical arguments or by compelling theoretical evidence."

specified as a categorical variable). Bjorkland and Moffitt (1987) included none of these variables, and Fraker and Maynard (1987) included only age cubed. It may well be that these variables were not significant in their data sets, or that there was no correlation between training and these variables, but we have no evidence to support these conjectures. Policy makers would have to take this on faith. In some jurisdictions, this faith does not exist.

3A.2.3 Matching

3A.2.3.1 What is Matching?

Matching is the simplest method for controlling for observed differences between participants and non participants. Non participants whose characteristics most closely resemble the participants are selected to form the comparison group. One to one matches can be made by simple but lengthy searches through the pool of non participants. The first participant is selected, the distance between that participant and each potential comparison group member is calculated[3]. The potential comparison group member with the minimum distance to the participant is selected as a comparison group member and is removed from the pool of potential comparison group members. The process is then repeated for the second participant and so on until a comparison group member has been selected for each participant.

Cell matching is an alternative to one to one matching. In cell matching, each variable describing the participants is divided into discrete amounts. Each possible combination of these variables constitutes a cell. For example, if we have three variables to match on and each is divided into seven categories, we would have $7^3 = 343$ cells. Non participants are allocated to the cells according to their characteristics. The outcomes for the non participants are weighted by the ratio of participants to non participants in that cell.

[3] Often the distance is defined as $(X_1 - X_2)'S^{-1}(X_1 - X_2)$ where X_1 and X_2 are the vectors of explanatory variables and S is the covariance matrix.

3A.2.3.2 Why Match?

Matching is an alternative to specifying the functional form. The process involves selecting comparison group members who are similar to participants in measured characteristics. Clearly, matching is not an option in most cases because it is necessary to have a large pool of potential comparison group members from which to select the comparison group. Of course, even when it is possible it is not free. By selecting comparison group members, other potential comparison group members are discarded and so the variance of the estimates will be higher than if the entire pool of potential comparison group members were used.

Matching ensures that $x_{cij} = x_{tij} + \eta_{ij}$ where x_{cij} and x_{tij} are values of the j^{th} explanatory variable, x_j for the i^{th} individual in the comparison and treatment groups, and η_{ij} is the matching error. If $E(\eta_{ij}) = 0$ then the matching is unbiased.

If the matching were perfect, then the values of all the explanatory variables for each participant would exactly equal the values for one comparison group member. The correlation between a variable describing program participation and all matching variables is zero by design.

3A.2.4 The Relationship Between Matching And OLS Regression

Suppose that we have a data set with N observations. N_1 observations have had a treatment (participants) and N_2 have not had the treatment (the comparison group). $(N_1 + N_2 = N)$ Y is the outcome of interest and X is a $(k\times1)$ vector of explanatory variables.

We begin by specifying a linear functional form for equation 1. Because we are examining the functional form issue, we replace U_1 with ε which is assumed to be uncorrelated with X and T. In the next section we relax this assumption.

4 $\quad Y = X\beta + T\delta + \varepsilon$

T is a dummy variable indicating training, and δ is the average training effect.

We sort the observations so that T is an $(N\times 1)$ vector with N_1 1's followed by N_2 0's.

The standard result for a partitioned regression (See e.g. Greene, 1990, page 182) gives

5 $\quad d = (T'T)^{-1}\, T'(Y - Xb)$

where d and b are the OLS estimates of δ and β.

As a result of the simple nature of T the following relationships hold.

$(T'T)^{-1} = 1/N$

$T'Y = N_1\overline{Y_1}$

$T'X = N_1\overline{X_1}$

Where $\overline{Y_1}$ and $\overline{X_1}$ are the means of Y and X across the first N_1 observations.

Substituting, these relationships into 5 gives

6 $\quad d = \overline{Y_1} - \overline{X_1}\, b$

We also know that

7 $\quad \overline{Y} = \overline{X}\,b + \overline{T}d$ since with OLS the mean of the estimated error is zero when a constant is included among the regressors. Decomposing these means into the means for participants and comparison group gives:

8 $$\frac{N_1}{N}\overline{Y_1} + \frac{N_2}{N}\overline{Y_2} = \left(\frac{N_1}{N}\overline{X_1} + \frac{N_2}{N}\overline{X_2}\right)b + \frac{N_1}{N}d$$

then using 6 gives

9 $$\overline{Y_2} = \overline{X_2}b$$

or

10 $$b = \left(\overline{X'}_2\overline{X}_2\right)^{-1}\overline{X'}_2\overline{Y_2}$$

Substituting this into 6 gives

11 $$d = \overline{Y_1} - \overline{X}_1\left(\overline{X'}_2\overline{X}_2\right)^{-1}\overline{X'}_2\overline{Y_2}$$

Pre-multiplying both sides by $\overline{X'}_2$ gives

12 $$\overline{X'}_2 d = \overline{X'}_2 \overline{Y}_1 - \overline{X'}_2 \overline{X}_1 \left(\overline{X'}_2 \overline{X}_2 \right)^{-1} \overline{X'}_2 \overline{Y}_2 .$$

If the means of the explanatory variables are the same for the treatment and comparison groups then any of the k equations can be solved to get

13 $$d = \overline{Y}_1 - \overline{Y}_2$$

That is to say, if the means of the explanatory variables are the same for the treatment and comparison groups then the difference in means of the explanatory variables is equal to the coefficient on the dummy variable for treatment in an OLS (best linear unbiased estimator) regression.

There is an interesting large sample application of this result since it does not depend on the content of X, only that the means of the columns of X be the same for the treatment and comparison groups. If we apply the Slutsky theorem to the matrix X, [that plim g(X) = g(plim X)] and if our matching is unbiased, then in the probability limit, the mean of any continuous function of the explanatory variables will be the same for the treatment and comparison group. Thus, in the probability limit, if the treatment and comparison group have been matched on all explanatory variables, then the mean impact of training on the trained will equal the difference in the outcome variable between the treatment and comparison group, regardless of the underlying relationship between the explanatory variables and the outcome variable.

So if Y is any continuous function g(X) and we have matched on all elements of X, then the matched results will be equal to the regression results. If the regression results are statistically significantly different from the matched results, then the functional form in the regression equation must have been mis-specified.

3A.2.5 Switching Regression

The switching regression model is a special case of this result. Specify g from equation 1 such that:

$Y_{1it} = X_{it}\beta_1 + U_{1it}$ if the individual takes training

$Y_{0it} = X_{it}\beta_0 + U_{0it}$ otherwise.

The parameter that we are trying to estimate, the mean impact of training on the trained can be recovered from this specification. It is equal to $E(X_{it}|training)(\beta_1 - \beta_0)$.

We have already shown that matching will give an identical result to the OLS regression when training is included as a dummy variable and the means of the explanatory variables are the same for the trainees and the comparison group. It is straightforward to extend this result for this formulation.

Following Heckman and Robb, (1986, page 254) let d be a dummy variable that takes the value 1 if the individual receives training. Then

$Y_{it} = d_i Y_{0it} + (1 - d_i) Y_{0it}$

$= d_i(X_{it}\beta_1 + U_{1it}) + (1 - d_i)(X_{it}\beta_0 + U_{0it})$

expanding and adding and subtracting $E(X_{it}|d = 1)(\beta_1 - \beta_0)$ gives

$$Y_{it} = X_{it}\beta_0 + d_i\overline{\alpha} + [X_{it} - E(X_{it}|d = 1)](\beta_1 - \beta_0)d_i + U_{0it} + d_i(U_{1it} - U_{0it})$$

where $\overline{\alpha} = E(X_{it}|d = 1)(\beta_1 - \beta_0)$. This gives the familiar result that even if the true model has different coefficients for participants and the comparison group, OLS regression with a dummy variable for program participation will give an unbiased estimate of mean impact of training on the trained if the mean of the explanatory variable is the same for participants and the comparison group ($X_{it} = E(X_{it}|d = 1)$), a condition that will be true in the probability limit if the matching is unbiased.

3A.2.6 Matching Plus Regression

We can use the same logic to show that the coefficient on program participation in regression analysis on a matched sample will not be inconsistent. Matching ensures that T is uncorrelated with each column of X. By a similar application of Slutsky's theorem, T will be uncorelated with any function of X in the probability limit.

$$\mathrm{Cov}(g(x),T) = \sum (g(x_i) - \overline{g(x_i)})(T - \overline{T})$$

Recall that matching ensures that $x_t = x_c + \eta_{ij}$. Suppose for convenience that the matching is 1:1 so that the mean of T = .5. Then $(T - \overline{T}) = .5$ for the treatment group and -.5 for the comparison group and

$$\mathrm{Cov}(g(x),T) = .5\sum (g(x_{it}) - \overline{g(x_{it})}) - .5\sum (g(x_{it} + \eta_i) - \overline{g(x_{it} + \eta_i)})$$

Taking Plim's and using Slutsky's theorem

$$= .5\sum (g(\mathrm{Plim}x_{it}) - \overline{g(\mathrm{Plim}x_{it})}) - .5\sum (g(\mathrm{Plim}\,x_{it} + \mathrm{Plim}\eta_i) - \overline{g(\mathrm{Plim}x_t + \mathrm{Plim}\eta_i)})$$

If Plim $\eta = 0$ then the expression as a whole equals zero. So, even if there are undetected non linearities in the functional form that has been specified, the coefficient on a dummy variable for training will be consistent, if the regression is estimated across a matched data set. For this reason, matching is often used in conjunction with regression analysis rather than as a substitute for it.

3A.3 Selection Bias

Selection bias occurs when there is a correlation between unmeasured characteristics that affect the outcome of interest and program participation. The bias in the OLS regression with correctly specified functional form is given in equation 3. In this case unmeasured characteristics such as job loss affect eligibility for programs as well as the outcome variable of interest. Clearly matching offers no assistance in dealing with this problem. There are three possible solutions to this problem

1. Random assignment. Structure the program so that selection into the program is random and there is no correlation between program participation and any explanatory variable;

2. Two stage regression procedures. Estimate two equations, one that models the enrolment decision and a second that models the outcome. Use either an estimate of the error term from the first equation to proxy the omitted variables or the predicted value as an instrument for program participation. Find a restriction that identifies the system.
3. Find more data. Including all elements of U that are correlated with training in the first equation will break the correlation between program participation and the residual and yield an unbiased estimate of program impact.

3A.3.1 Random Assignment

In theory, random assignment can provide an estimate of the impact of training on the trained without any assumptions. The central limit theorem tells us that if $y_1, \ldots, y_n$ are a random sample from any probability distribution with finite mean μ and finite variance σ^2, and $\overline{y_n} = (1/n)\sum_i y_i$ then $\sqrt{n}(\overline{y_n} - \mu) \xrightarrow{d} N[0, \sigma^2]$. Random assignment means that both the treatment and control groups are random samples of the parent distribution[4]. That is to say, the mean of any variable describing either population will be the same up to a sampling variation. Statistically significant differences between the means of the treatment and control groups can logically be ascribed to the treatment, so the calculation of the average impact of training on the trained is $\overline{Y_T} - \overline{Y_C}$ where $\overline{Y_T}$ is the is the mean of the outcome variable for the trainees and $\overline{Y_C}$ is the mean of the outcome variable for the

[4] If the sample that is randomly divided into treatment and control group is itself randomly selected from the population to which the results are to be extrapolated then the study can be said to have external validity. Random assignment, by itself will guarantee internal validity, an unbiased answer to the question, "What is the mean impact of training on the trained?"

controls. The appeal of random assignment is that nothing else need be known about the participants in the programs, and no assumptions need be made.[5]

Clearly one stage regression analysis and random assignment address different issues. However, random assignment ensures that assumption 1 will hold. Once random assignment has been completed, it is common for researchers to increase the efficiency of their estimates by using regression analysis. In this case neither mis-specified functional form, nor errors in variables (except the dummy variable indicating training) will bias the coefficient on T. This result occurs because random assignment ensures that T is not only independent of U_1 but also of X. (See discussion in section 3A.2.6)

Conversely, if assumption 1 holds and program participation is measured accurately, then there is no need for random assignment.

3A.3.2 Two Stage Regression

In the case of employment and training programs, there is good reason to believe a priori that there will be a correlation between program participation (a regressor) and the residual, and so the results of a one stage regression would be biased. It is still possible to produce consistent estimates of program impact in this event using a two stage regression technique.

If an excluded relevant variable exists, that is in this case, a variable that is known to affect program participation (is legitimately included in equation 2), but does not affect the outcome of interest and is uncorrelated with U_1 (is legitimately excluded from equation 1), then two stage least squares can be used to produce a consistent estimate of program impact. To show this specify equations 1 and 2 as follows:

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[5] Heckman's comments on the limitations of random assignment are discussed in Chapter 3. They deal with practical problems that arise in the implementation of random assignment and its (limited) usefulness in answering questions other than the mean impact of training on the trained. The theory remains incontrovertible.

14 $Y = X\beta + T\delta + \varepsilon_1$

15 $T = Z\gamma + \varepsilon_2$

The standard assumptions hold, except that $\mathrm{cov}(\varepsilon_1, \varepsilon_2) \neq 0$ because for example, more motivated individuals participate in training and this motivation also increases their incomes.

16 $d = (T'T - T'X(X'X)^{-1}X'T)^{-1}[T'X(X'X)^{-1}X'Y - T'Y]$

17 $d = \delta + (T'T - T'X(X'X)^{-1}X'T)^{-1}[T'X(X'X)^{-1}X'\varepsilon_1 - T'\varepsilon_1]$

Clearly if $\mathrm{cov}(\varepsilon_1, \varepsilon_2) \neq 0$ then $\mathrm{cov}(\varepsilon_1, T) \neq 0$ and $E(d) \neq \delta$.

Two stage least squares replaces T with $\hat{T}$ in equation 14. Where

18 $\hat{T} = Z\hat{\gamma}$, and $\hat{\gamma}$ is the OLS estimate of γ. This gives the 2SLS estimator of δ,

19 $d_{2SLS} = (\hat{T}'\hat{T} - \hat{T}'X(X'X)^{-1}X'\hat{T})^{-1}[\hat{T}'X(X'X)^{-1}X'Y - \hat{T}'Y]$

Slutsky's Theorem and the consistency of the OLS estimate ($\mathrm{plim}\,\hat{T}$ =T) allow us to substitute T for $\hat{T}$ and (relying on the standard assumptions that $p\lim(\frac{\hat{T}'\hat{T}}{n}) = Q_1; p\lim(\frac{X'X}{n}) = Q_2; p\lim(\frac{X\varepsilon_1}{n}) = 0$; where Q_1 and Q_2 are finite positive definite matrices) obtain the result that

20 $p\lim d_{2SLS} = \delta + p\lim(\hat{T}'\hat{T} - \hat{T}'X(X'X)^{-1}X'\hat{T})^{-1}[\hat{T}'X(X'X)^{-1}X'\varepsilon_1 - \hat{T}'\varepsilon_1]$

This time, plim $\hat{T}'\varepsilon_1 = 0$ because plim $\hat{T}'\varepsilon_2 = 0$, a property of the OLS estimator.

The asymptotic variance of the two stage estimator is given by

21 $\sigma^2(\hat{T}'\hat{T} - \hat{T}'X(X'X)^{-1}X'\hat{T})^{-1}$.

We can compare the asymptotic variance of the two stage estimate with the one stage estimate in the specific case in which $\mathrm{plim}(\varepsilon_2'X) = \mathrm{plim}(X'\varepsilon_2) = 0$. (This would occur if Z contained all the elements of X that were correlated with T.) Substituting T = $\hat{T} + \varepsilon_2$ into the formula for the asymptotic variance gives $\sigma^2(\hat{T}'\hat{T} + \sigma^2_{\varepsilon 2} - \hat{T}'X(X'X)^{-1}X'\hat{T})^{-1}$

which is clearly larger than equation 21 because the inverse contains the additional positive element $\sigma^2_{\varepsilon 2}$.

Because the asymptotic variance of the OLS estimator is smaller in many practical circumstances, the question of which estimator gives the lower mean square error is not clear-cut. On the basis of his monte carlo studies, Cragg (1967 page 109) concluded "the choice of DLS [Direct Least Squares] also may be sensible, even for very simple models conforming to the assumptions under which the simultaneous-equation estimators were derived." This conclusion has stood up over time. Greene, (1994 page 616) says, "The advantage of systems estimators in finite samples may be more modest than the asymptotic results would suggest."

3A.3.2.1 Identification

Traditionally, identification is discussed in terms of either having been achieved or not. In this case we might say, for the order condition to be met, that is a necessary condition for equation 14 to be identified, there must be at least one excluded predetermined variable. That is, Z must contain a variable that is not in X and is not a linear combination of the variables in X[6]. Although this will show that equation 14 is identified, if the excluded relevant variables are not sufficiently powerful, and sufficiently independent of X, the variance will be so large that the estimate will be useless.

For illustrative purposes, regress $\hat{T}$ on X to get $\hat{T} = X\varsigma + \omega$, substitute this into 21 and use the fact that $\omega' X = 0$ to get

22 $\sigma^2(\omega'\omega)^{-1}$.

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[6] In terms of our discussion, this will ensure that a regression of $\hat{T}$ on X will have non zero residuals.

Although the presence of an excluded relevant variable means that an equation is technically identified, if $\omega'\omega$ is not sufficiently large, it would not be identified in a practical sense.

3A.3.2.2 Non Linear Restrictions

An approach which has become very popular of late secures identification by imposing a non-linear relationship in the impact of the exogenous variables on program participation in equation 15. Re-write equation 15 as

23 $\quad T^{*} = Z\gamma + \varepsilon_2$

where T^{*} is a latent variable. The variable indicating training takes the value 1 if $T^{*} > 0$ and 0 otherwise. In this model, the probability that the individual is selected for training will be $\Phi(Z\gamma)$, the probability that they are not selected will be $1 - \Phi(Z\gamma)$. Suppose further that ε_1 and ε_2 each have a standard normal distribution with correlation ρ. Then, using Theorem 21.4 in Greene (1990 page 740)

$E(Y|Z = 1) = X\beta + \delta T + \dfrac{\phi(Z\gamma)}{\Phi(Z\gamma)}$ for the participants and

$$E(Y|Z = 1) = X\beta + \delta T + \frac{-\phi(Z\gamma)}{1 - \Phi(Z\gamma)}$$

for the non participants.

Consistent estimates of δ can be obtained by including the terms $\dfrac{\phi(Z\hat{\gamma})}{\Phi(Z\hat{\gamma})}$ for participants and $\dfrac{-\phi(Z\hat{\gamma})}{1 - \Phi(Z\hat{\gamma})}$ for non participants in the regression equation.

This particular technique has been criticised (Goldberger 1983) because it depends on the strong and untestable assumption that the errors are distributed normally.

3A.3.2.3 Monte Carlo Experiments

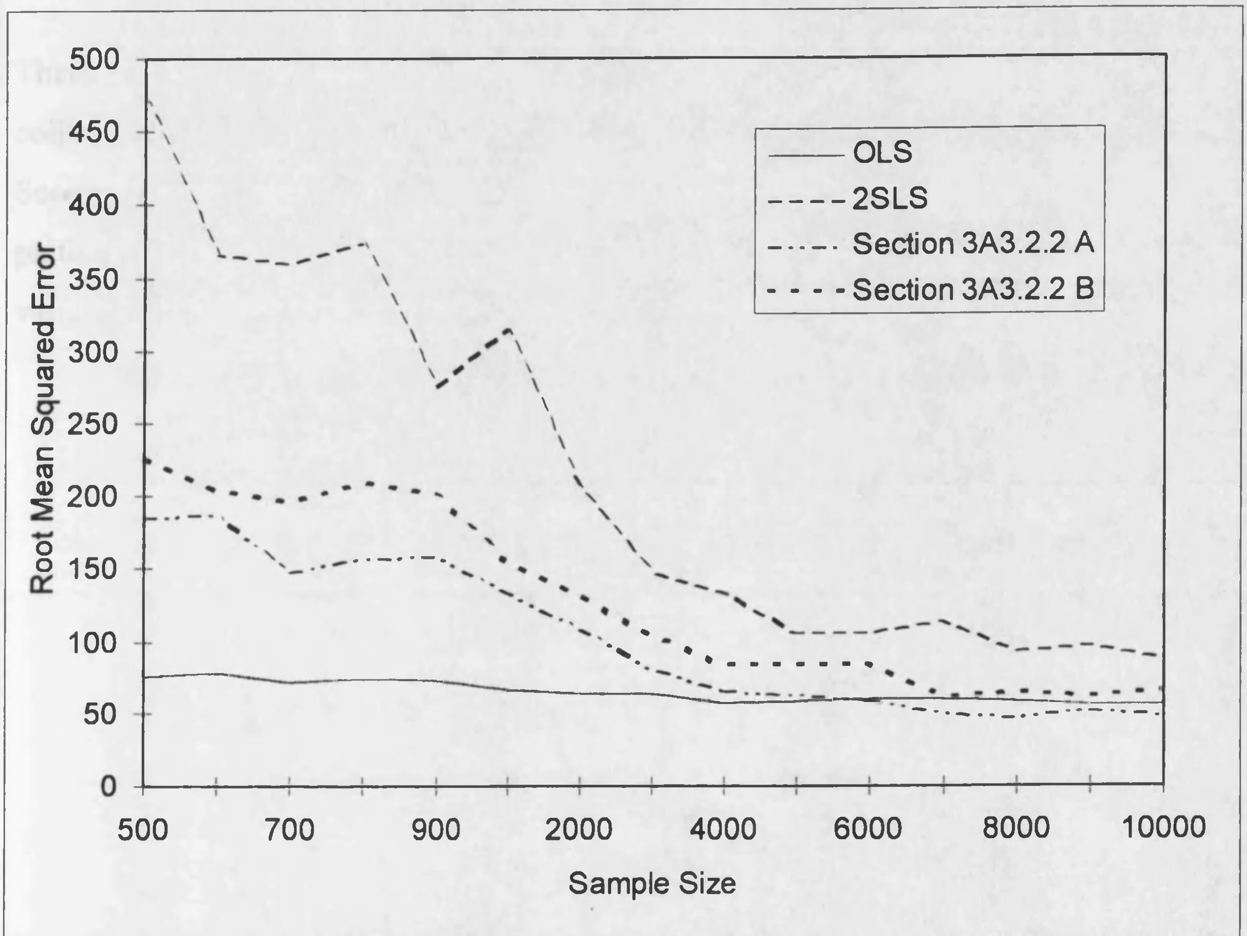
The relative advantages of various estimators when the sample is finite are not clear-cut. As Greene (1994 page 616) says on this subject, "Unfortunately there are few useable general results." For this reason, when comparing estimators it is useful to perform monte carlo experiments.

The data sets used in this analysis of the earnings equations are modelled on that used by Dickinson, Johnson and West (1986) (DJW) in their estimates of the impact of CETA on women. They have 45 explanatory variables of which 18 are dummy variables and 11 are interactions. Eight of the parameters are statistically significantly different from zero. The R^2 in their equation was .19. They have 5,438 observations.

The data sets in the specification of the participation equation were based on personal experience. It included all variables specified in the earnings equation plus one excluded relevant variable. The model predicted participation correctly about 80% of the time.

The data set is set up as follows. There are 40 explanatory variables of which 25 are dummy variables. For simplicity the explanatory variables are independent. The dummy variables all had means of .5, the continuous variables were all uniformly distributed from 0 to 60. ε_1 consisted of two parts. The first, well behaved, part had mean zero and standard deviation 500. The second, with mean zero and standard deviation 50, was included in ε_2, the error term of the selection equation. This gave an R^2 of .13 in the earnings equation. The mean of the dependent variable averages just under 1,000, the effect of training is 100 and selection bias is just over 50. There is one excluded relevant variable in the selection equation which also is uniformly distributed between 0 and 60. When the sample size is 10,000 it has a t statistic of 15.

The parameters of equation 14 are estimated using 4 methods: ordinary least squares, two stage least squares, the method described in section 3A3.2.2 on non linear restrictions above, first with the excluded relevant variable (A), and then without (B). The parameters of the models were estimated 100 times for each sample size, which increased in increments of 100 from 500 to 1,000 and in increments from 1,000 to 10,000. The results are shown in Figure 3A.1 below. For sample sizes of less than 5,000 OLS has the lowest mean squared error. For sample sizes greater than 5,000 the mean squared error for OLS is close to the mean squared error of the two stage techniques.



This result reaffirms Greene's comment, (1994 page 616), "The advantage of systems estimators in finite samples may be more modest than the asymptotic results would suggest."

3A.3.3 Getting more Data

This does not mean that it is impossible to estimate the impacts of employment and training programs without random assignment. An alternative is to get more data. Economists are taught in introductory texts

> The econometrician should always keep the inaccuracies of the data in mind. If an econometric study is not satisfactory in some sense, the temptation is typically to revise the model or try a different techniques. Only infrequently will the data be investigated more carefully and further refined or else alternative data be utilised, yet often the data, rather than the model or the econometric technique, may be the source of unsatisfactory performance. The alternative of improving the data or obtaining new data should be seriously considered in such a situation. [Intriligator 1978 page 71.]

There are three types of data that can help us make more reliable estimates. First, we can collect information on very large samples and use sophisticated two stage techniques. Second, we can collect information on additional variables that explain program participation as Park et al. did. Finally, we can try to collect more information on the variables in U.

4. Overview of the BC Study

The BC study, the results of which are reported in this thesis was heavily influenced by the American experience in estimating the impact of the CETA programs. It began in 1986, just after the US Department of Labor had concluded that the problem of selection bias was so severe that it was necessary to use random assignment to produce useful estimates of the impacts of its employment and training programs. In additions the CETA studies urged caution in the use of econometrics. The articles by LaLonde(1986) and by Fraker and Maynard (1987), discussed in Chapter 2, found that some econometric techniques not only did not remove selection bias, but also could lead to false confidence in erroneous conclusions. As a result, the BC study focusses on the issue of selection bias and works to avoid all hidden assumptions by testing all techniques used.

To a young man in 1986, with no experience in estimating the impacts of employment and training programs, the problem of selection bias seemed relatively straightforward and simple. Selection bias is simply a name for a type of omitted variable bias, and two solutions are at hand to deal with it. One solution is to collect more information so that the omitted variables can be included in a regression equation. Another solution is to identify variables that are correlated with program participation but not with the outcome of interest, and use them to create instruments for the program participation variables. The instruments would be free from correlation with the omitted variables and would therefore generate unbiased estimates of program impact. Even if there were no such excluded relevant variable, Heckman (1976) had produced a method for securing unbiased estimates.Unfortunately, not only did the issue of selection bias turn out to be far from simple[1], it was only one of many problems encountered. For example, a more fundamental problem involved finding out who was enrolled in the programs.

This chapter provides an overview of the BC Study, and in doing so, describes some of the other problems encountered. It has four sections. The first section gives the conceptual framework for the study, a discussion that arises from the difficulties in determining enrolment. The second section describes the data initially collected for the study, and the sources that were used/uncovered later. The third section reports the results of three tests that influenced the direction of the study. First, a test of Heckman's two stage method for correcting for selection bias; second, a test for non linearity, and third, a test for non response bias in the survey data. The final section gives a preview of the results. Three short appendices are attached to this chapter. The first is a brief chronology of the BC Study. The second gives some algebra showing the relationship between results produced by cell matching and those produced using regression analysis. The third reports regression results for survey respondents and the full sample.

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[1] Experience is a teacher,
but here's what makes me burn.
She's always teaching me the things
I do not care to learn.

4.1 Conceptual Framework

There is a large number of interesting questions that can be asked about a set of employment and training programs[2], but this study is restricted to the examination of the subset related to the impacts of the programs on participants' subsequent welfare dependence and employment. Within this subset, the study is further restricted to estimates of the incremental impact of the programs in question compared with the status quo. The estimates are limited in this way as a direct result of the problem of identifying participants.

4.1.1 Identifying Participants

In 1986 program participants were identified by the Individual Opportunity Plan (IOP) code on their file. Welfare recipients who wished to take training would develop a plan (the IOP) in co-operation with their worker. The worker would make a record of the anticipated training by entering a one-digit code into the client's file at the time that the plan was signed. This method of tracking enrolment was very unsatisfactory for three reasons.

1. A busy worker might not record the plan on the system,
2. the recipients might not participate in the anticipated training, and
3. recipients without plans might participate in training. These individuals might receive funding through another agency such as Canada Employment Centres or Canada Student Loans.

The extent of inaccuracy is not trivial. In a joint project with Camosun college in Victoria[3], 1,460 individuals were identified as students who were receiving welfare. Of these only 859 had an IOP code that indicated that they were

[2] See e.g. Employment and Immigration Canada, 1987
[3] See Chapter 7

receiving any training, and only 716 had an IOP code that indicated that the individual was participating in *classroom* training. Another 486 individuals on welfare in Victoria had an IOP code that indicated that they were taking classroom training, although they were not at Camosun[4].

4.1.2 Implications

Ignorance about program participation causes problems because members of the comparison group may actually be participants. This will bias the estimates of program impact toward zero. Heckman and Smith (1993 p. 51) refer to this problem as substitution bias. Although Heckman and Smith present this as a mechanical problem, a conceptual distinction is also involved. Use of a comparison group that contains individuals who receive training results in a biased estimate of the impact of training compared with no training, but (as long as the comparison group receives a 'normal' amount of training) yields an unbiased estimate of the incremental impact of the new program compared with the status quo. Sometimes it is the comparison with the status quo that is of interest. For example, when we estimate the impacts of job search assistance programs we are interested in the incremental effect. The comparison between the effects of supervised job search and no search is not useful since individuals will do some search on their own.

The seriousness of this problem clearly depends on the likelihood of members of the comparison group participating in another program and the effectiveness of the programs in which they participate.

[4] Some of these might well have been taking classroom training through another agency, for example the Read Society, the school board or the University. 486 puts an upper limit on the number coded as receiving training, but who did not actually participate.

In the conceptual stages of the BC study, substitution bias was not considered a serious problem for three reasons.

1. The initial focus of the study was the new on-the-job training programs that were funded under the Four Corners Agreement. These programs were new, unusual in BC and expensive. Our discussions with our federal counterparts indicated that few such services were available to our clients through federal programs at that time.

2. The existing programs were felt to be ineffective. Abt had published a study that concluded "both BTSD [Basic Training and Skills Development] and Skill[5] trainees show no significant benefit from participating in the training relative to a comparison group." [Abt Associates 1985, p. 7] We obtained similar results for welfare recipients in BC when we compared the rate of welfare dependence for those with IOP codes and those without. [Jamieson, 1987]

3. Finally, the purpose of the study was not to decide whether welfare recipients should receive training or not, but rather to determine which types of training they should receive. For the former purpose, the appropriate comparison would be between training and nothing, but for the latter, a comparison with the status quo is acceptable.

Although the concept of comparing the new programs with the status quo was quite acceptable in 1986, its acceptability became less clear cut as time went on for two reasons. First, the scope of the study expanded to include all types of training, and second, the alternatives to the programs offered by the Ministry of

[5] Both BTSD and Skill are federally funded classroom training programs.

Social Services became more effective[6]. The effect of this bias (provided that programs do not harm participants) will be to lower the estimates of the impacts of the programs, so the estimates reported in this study will be conservative.

4.2 Data

When the BC study was launched, two principal data sources had been identified, administrative data on welfare dependence and a survey of participants and non participants. By the end of the study, data had been collected from ten sources. This section describes each of these ten sets of data.

4.2.1 Welfare

The primary source of information is the administrative records of welfare dependence. The welfare payment records report whether a case received a benefit for each month since 1980. That is, up to six years of welfare history when the project began in 1986[7].

Benefits are paid to all people in need between the ages of 19 and 65; singles and childless couples as well as families with children. The files are linked and the applicant is identified by Social Insurance Number (SIN). About 1.5% of the cases do not have SIN's, and SIN's are not reported for dependants. (About 9% of adults receiving welfare are dependants.) The files also contain audited

[6] See Chapter 7 for an estimate of the impact of classroom training beginning in 1986.

[7] Although these data existed, the cost of processing the data limited its usefulness, especially in the early stages. Considerable effort was expended in the production of estimates at minimal cost, and many promising areas were bypassed because they were simply too expensive. For example, early estimates compared participants with non participants whose welfare dependence was similar in the 25 months preceding enrolment. Data on UI dependence and Records of Employment were only available for a one-in-ten sample, and even that was not used until years after it became available. Later, longer pre-program information on welfare dependence was produced, but only for the one-in-ten sample that matched the UI and ROE samples in order to contain processing costs. This naturally had a cost in that the small programs and groups could not be analysed using the new data. (e.g. we did not assemble the long pre-program welfare histories of the RO selected group (data item 5) even though the data exists.)

information on factors that affect benefits: age, sex, classification, handicapped designation and marital status. Welfare entitlement is determined and benefits are paid by month.

The administrative data used in this thesis is kept accurate by force of law. BC's Ministry of Social Services employs ten auditors whose sole job is to ensure that payments made by the Ministry are approved under policy, are for the purposes recorded, and are made to the people indicated in Ministry records.

These data, by themselves, provide a means for estimating the impact of the programs on subsequent welfare dependence. The initial estimates of impact were made by drawing comparison groups from those on welfare who did not participate in the new programs, and who were similar to the participants in all recorded and audited variables, that is age, sex, marital status, classification handicapped status and history of welfare dependence. This method is limited in two significant ways. First, it only provides information on one outcome, welfare dependence, and many other outcomes are of interest (e.g. employment and earnings). Second, there are clearly many important explanatory variables that are missing and which could easily be correlated with program participation (e.g. education, employment and earnings history) and therefore cause selection bias.

4.2.2 The survey

An ambitious survey was launched to provide the missing information. The survey gathered data from four groups: one group of participants in the wage subsidy program, another group of participants in the public sector programs, a third group of participants in Job Action, and a fourth, comparison group of recipients who did not participate in any program. The eventual size was 1,905 composed of: EOP 500, public sector programs 501, Job Action 389, (including control group) and non-participants 515. Of the 501 public sector program interviewees, more than three-quarters (376) came from the Community Tourism Employment Training Program, 20 percent (101) from the Environment Youth Program, and only five percent (24) from the Forest Worker Assistance Program.

From the start we were concerned about response rate. Lee Bawden of the Urban Institute, who provided advice on the project, told us that the standard in the United States required an 80% response rate, but when we let a request for proposals requiring an 80% response rate, no one would bid. We were fortunate in securing the assistance of Celia Homans, former head of NORC at the University of Chicago, in developing another RFP, selecting a contractor and providing advice in how to achieve an acceptable response rate. Information that would lead to increased contact rates was collected at the time the individual enrolled in the program (name of a contact person, normally a parent), this information was updated at each interview, response rates were monitored monthly, large numbers of attempts were made to contact by phone, and where these failed, face to face contact was attempted.

Interviews were conducted in three waves so that we would not lose respondents between waves. The first interview was conducted as soon as possible after the participant had entered the program or been identified as a comparison group member. In practice this was normally about two months after they had enrolled in the program. They were interviewed again six months later and again nine months after that. The final set of interviews was conducted about 17 months after the participants had entered their programs in order to examine the circumstances of the interviewees after their program participation had ended and after any associated UI eligibility had expired.

The survey work was performed under contract by Campbell Goodell Consultants. The cost was about $180 per Wave 3 contact, but paid off with a 75.1% response rate[8], the best response rate for this type of respondent yet achieved in Canada[9].

The main source for the questionnaire was the questionnaire used in the Urban Institute's evaluation of Massachussets' ET Choices program (Nightingale, 1991.) additional questions were asked regarding barriers to employment, attitudes to themselves (the Nowicki-Strickland locus of control measure) and attitudes to work. Analysis of the attitudinal variables was extremely frustrating, since none of them was statistically significant in an equation predicting subsequent welfare dependence. Apparently, this finding is far from new. The technology for measuring these attitudes for these purposes apparently does not exist, and these attitudinal variables (motivation, determination, etc.) are generally referred to as unmeasurable[10].

Interpretation of the results of the survey relating to earnings was also frustrating. Inconsistencies in the data were common with many individuals reporting more than 100 hours per week, overlapping employment etc. Sorting out these problems is a field of expertise in its own right[11], and this survey would have profited from the application of this expertise[12]. Nonetheless, the survey did produce measures of schooling, employment history, and employment subsequent to the program that were relatively easy to interpret.

[8] This response rate is calculated as the number of wave 3 contacts divided by the number of names initially supplied to the contractor.
[9] By contrast, the response rate on the National Institutional Training Program Evaluation, reported above, was less than 35%.
[10] D. Lee Bawden, personal communication.
[11] Paul Decker, personal communication.
[12] The strong impression that I came away with was that saying that 'econometrics, like sausage, was better not seen in the making,' could equally be applied to survey research.

4.2.3 Program Participation

The other essential ingredient needed to estimate the impacts of programs is information on enrolment. As noted above, the existing source, IOP codes, were unreliable, so a new system for tracking enrolment in the on-the-job training programs was devised. Employers were required to identify the participants and the period for which the wages were paid on the billing forms that they submitted to the Ministry. Start dates were defined as the first date for which a wage subsidy payment was made. Where these forms were improperly filled out or missing, the start date would appear to be later than the true start date.

With these three sources of information, a reasonable study was possible. However, in the implementation of the survey, three other sources of data were developed to enhance the study, one from a survey of rehab officers (RO's) in the province, and two from the *Participant Referral Form.*

4.2.4 Participant Referral Form

In order to develop a sample for the survey, a participant referral form was developed. The forms were filled out by the rehab officer and client at the time that the client was referred to a program. This was then carried by the client to the potential employer as a letter of introduction. The employer would complete the form and return it to the Ministry.

The most important element on the form was a declaration by the client that he/she agreed to participate in the evaluation. Although this was completely voluntary more than 95% agreed to participate. In addition, the form collected information on education, information on contacts, identified the employer, and identified whether the client had been hired or rejected by the employer. This latter group, that had made it almost all the way through the selection process, was used in a test for selection bias. (See Chapter 5.)

4.2.5 Survey of RO's

We wanted the comparison group for the survey to be as similar to the participants as possible in both measured and unmeasured variables. We felt that the RO's would be the best source since they would be most familiar with the type of individual that they referred to programs, and so we enlisted their aid. We asked each of the roughly 70 RO's to identify 7 clients that they judged to be similar to participants in the on-the-job training programs, but who had not participated. This was a very labour intensive process, both for the RO's who had to identify and contact the clients, and also for the support staff in the Research, Evaluation and Statistics Branch who had to nag each RO to make his/her contribution. Nonetheless, this initiative was a great success[13] with the RO's identifying about 500 members for the comparison group.

We were also able to obtain or develop data with which to shed additional light on the issues after the survey had begun. Two sources resulted from an information exchange agreement between the provincial and federal governments. A section on exchanging information for research purposes was added to the existing administrative agreement specifically for the purpose of enhancing our estimates of the impact of the employment and training programs. Although such exchanges were anticipated in the *Four Corners Agreement*, BC was the first province to sign such an agreement. It provided information from two sources on a one-in-ten sample.

4.2.6 ROE's

Information on pre-program earnings was provided by a one-in-ten sample of records of employment (ROE's). These forms must be submitted to the federal

[13] Thanks in large part to the work of Leslie Matheson, our supervisor of Admin Services.

Ministry of Human Resources Development whenever an individual leaves employment that is UI insurable. HRD reports that about 93% of the paid labour force is covered by UI. This includes all wage and salary employees who work at least 15 hours per week or who earn more than a stipulated amount, $156 per week in 1994. (Canada 1994 page 93) There are three main weaknesses in these data that apply to their use in this study. First, because these forms are only completed at the time of separation they cannot provide information on ongoing employment. Second, since Unemployment Insurance claims are based on the last year of employment, employers are only required to provide accurate information on the last 12 months of employment. For people who leave employment that they have held for more than a year, the level of earnings for prior years is not available, and the true start date may not be reported accurately. Third, the earnings information on each record of employment is only available as an average for the period reported, up to one year, and does not show growth. In addition, ROE's, especially those not used in support of a UI claim, must suffer data entry problems like those of any form that is completed manually, irregularly, and by a wide variety of individuals.

Although the ROE's are not a good source of information on ongoing employment, they are an excellent source of information on the history of employment for people in these programs. Because participants in these programs are drawn from people on welfare, and because the programs provide full time jobs, we can be confident that the participants have terminated their previous employment, and records of employment will have been issued for them. ROE's will provide a complete history. Similarly, if the comparison group is chosen from individuals on welfare who do not declare earnings[14], the ROE's will provide a complete history of earnings and employment.

[14]except when earnings are not accurately reported

The usefulness of records of employment in examining post-program histories of employment is limited because ROE's are not produced until employment is terminated. An unknown number of participants will not have terminated employment at the time the data are collected and an absence of records could indicate either ongoing employment or ongoing unemployment. Average earnings based on ROE's in the post-enrolment period will understate true average earnings for both the treatment and comparison groups by an unknown amount. Readers are cautioned not to interpret post-program earnings in the graphs in Chapter 5 as reliable estimates of program impact on earnings.

4.2.7 Unemployment Insurance (UI)

The information exchange agreement also provided information on use of Unemployment Insurance (UI). The UI file records payments to the same one-in-ten sample as the ROE's. As well as information on UI benefits paid, it contains information on the age and sex of the recipient. This file records benefits paid by week.

4.2.8 Job Action Pilot Project

More data was generated by the Region A Job Action Pilot Project. Region A, which comprises the south-western part of greater Vancouver, volunteered to test the concept of providing job search assistance to applicants for welfare at the time of application. Further, the region very kindly agreed to integrate random assignment into their program. The Job Action Pilot Project enhanced the BC study in two ways. First it enabled us to broaden the range of programs for which estimates were produced. Second, it generated one estimate of impact that was, for methodological reasons, free from selection bias against which a non experimental estimate might be compared.

4.2.9 Classroom Training

In 1990, results from the Urban Institute's Evaluation of ET Choices became available (Nightengale, 1991). That study produced dis-aggregated estimates of the impact of classroom training. They found that neither adult basic education (ABE) nor English as a second language (ESL) had a beneficial impact, but that vocational training did. This was one of the first optimistic findings regarding classroom training, and it prompted us to approach Camosun College, the local community college and propose a joint study. They agreed, and we matched tapes to identify individuals on welfare who were taking classroom training. We developed a comparison group using administrative data on welfare dependence and produced estimates of program impact for four types of classroom training, ABE, vocational, career-technical and academic. The estimates of the impact of classroom training greatly enhanced the BC study because more welfare recipients participate in classroom training than all other types of training combined.

4.2.10 Clients Classified as Job Ready in an Interview

The final source of data came from a contractor who was hired to identify job-ready clients. When the contract was finished, it became apparent that very few of the clients who were classified as job ready on the basis of an interview actually went on to participate in an on-the-job training program. Because the group was large (over 4,000) and the criteria for classification included variables that are not normally recorded[15], it provided an excellent means of testing for selection bias.

4.2.11 Summary of Sources of Data

This section described ten sources of data used in this thesis. They were

[15] e.g. Interviewers were asked to note "sloppy, careless dress"

1. administrative data on welfare benefits paid;
2. a survey of participants and non participants;
3. participant referral forms that identified participants and rejectees;
4. monthly claim forms that identified participants in on-the-job training programs;
5. a survey of RO's that identified a group of non participants who, in the opinion of the RO's were similar to participants in measured and unmeasured characteristics;
6. Records of Employment that provided histories of employment and earnings on a one-in-ten sample of the BC population;
7. administrative records of UI payments that provided histories of UI dependence both pre- and post program for a one-in-ten sample of the BC population;
8. the Job Action Pilot Project that identified a participant and control group of job club participants;
9. classroom training records from Camosun College that identified welfare recipients who participated in classroom training and the courses that they took; and
10. a group of individuals who by self identification and interview were identified as job ready.

Comparison groups were drawn in two distinct manners

A. comparison groups that matched participants in variables recorded in 1. above and
B. comparison groups that matched participants in variables recorded in 1. 6. and 7. above.

4.3 Three Tests

The BC study began in an era of scepticism regarding arcane (and even common) statistical techniques. As a result, the study includes many tests for reliability. Most of these were related to selection bias and are reported in Chapter 5. But the results of three tests had a more fundamental effect on the approach taken in the study. These were:

1. tests of Heckman's two stage method for dealing with selection bias.

2. tests for undetected non linearity.
3. tests for non response bias.

4.3.1 A common two stage method

The selection bias correction technique described in Chapter 3A, Section 3.2.2 is widely used (see e.g. HRD 1993), but unfortunately, it has been shown to be very sensitive to assumptions regarding the distribution of the error terms. Goldberger (1983) showed that if the error terms deviated even slightly from normal substantial bias would be introduced.

This is particularly relevant in the estimation of the impacts of employment and training programs for disadvantaged individuals because our dependent variables are likely to be truncated. This truncation is obvious when the dependent variable is the percentage of time employed (which is bounded by zero and one) or UI dependence[16] which is bounded by zero and maximum benefits. It is perhaps less obvious in the case of earnings which are widely assumed to be log normally distributed, an approximation that may work well at the middle of the distribution, but which is a poor approximation for disadvantaged workers where the distribution is truncated as a result of income support programs and minimum wage laws.

Table 4.1 illustrates the fragility of estimates produced using this technique when the error terms are not distributed normally.

The model is specified as in chapter 3A:

[16] These were dependent variables in equations estimated using this technique in HRD Canada 1993.

1 $Y = X\beta + T\delta + \varepsilon_1$

2 $T^* = Z\gamma + \varepsilon_2$

where T^* is a latent variable. The variable T, indicating training takes the value 1 if $T^* > 0$ and 0 otherwise. X is specified as 2 independent variables that are normally distributed with mean 0 and standard deviation 50. Z contains both X variables. All elements of β and γ are 1, except the constant which is zero. δ=40. There are three error terms each normally distributed with mean zero and standard deviation 50, referred to below as η_1, η_2, and η_3. $\varepsilon_1 = \eta_1 + \eta_2$, and $\varepsilon_2 = \eta_3 + \eta_2$. Throughout, the sample size is 10,000 and the parameters of the models are estimated 100 times.

In the first row of table 4.1, where the assumptions of the model hold, the results are encouraging. The estimate of δ is unbiased and the variance is reasonable.

The second row reports the results when 2.5 percent of the outcome variable is censored at each end of the distribution. This apparently trivial deviation from the normal distribution increases the estimate of the coefficient, δ by two thirds. Although the assumption of normal or log normal distributions of income may be appropriate in some populations, it clearly is not in this population where roughly half of program participants are dependent on welfare in any given month in the subsequent four years.

This suggests that a more realistic censoring would censor a higher proportion an only on one side of the distribution. The third row reports the result when 25% of the sample is censored on one side. As expected, the bias is larger.

The fourth row replicates the third row, except that a variable relevant to the selection equation, but that does not affect the outcome variable, is included. Even with an excluded relevant variable, the distribution's seemingly minor deviation from normal results in considerable bias.

Table 4-1 Sensitivity of two-stage method described is Chapter 3A, Section 3.2.2

Model Characteristics	Mean Estimates of δ	Var.
Error terms normal	39.0	94
2.5% of dependent variable censored at each end of distribution.	67.3	94
25% of dependent variable censored at one end of distribution.	90.1	64
2.5% censored as above, but equation 3 contains a relevant variable, that is excluded from equation 1.	61.3	65
Two stage least squares	38.4	328

The final row gives the results for two stage least squares. It is unbiased and has a lower mean squared error.

4.3.1.1 Impact on the BC study

The results of this test had both a direct and an indirect effect on the study. The direct effect was a decision to not use that method. The indirect effect was a tendency to search out new data rather than new statistical methods. In 1986 Heckman's method was still considered valid and useful[16] even though Goldberger had published his study showing that it had severe problems three years earlier. Perhaps more significantly, Goldberger's study was not published until seven years after Heckman had proposed the method and it had become widely accepted. This suggests that "recent developments in econometrics" that Heckman and Smith (1993a p. 65) allude to may well be shown, in five or six years, to have some as yet undetected sensitivities[17]. This possibility, combined with existing scepticism regarding arcane statistical techniques[18] dictated that the BC study not rely on a new statistical technique.

4.3.2 Undetected Non Linearity

Ordinary least squares regression analysis seems old fashioned to economists, but to many without a background in quantitative analysis it seems arcane, and for that reason alone should be subject to scrutiny in this study. However, there are two

[16] Human Resources Development Canada continued to use the method in 1989. See e.g. Goss, Gilroy & Associates Ltd. 1989, Appendix G.

[17] In any event, they have not released the computer programs that they used in their estimation of the impacts of the JTPA so the process of independent verification hasn't even started yet.

[18] This scepticism was particularly acute in the Ministry of Social Services as a result of the Ministry's forecasting activities. In the 1970's considerable resources were devoted to predicting caseload growth, and an econometric model was produced. Although it was a single equation model, it was considered state of the art when it was introduced. (It controlled for heteroskedasticity!!) However, it under-predicted the growth of the caseload in the recession of the early 1980's badly.

additional reasons for testing the appropriateness of using regression analysis in producing estimates of the impact of employment and training programs in British Columbia.

The first is that non linearities are common in relationships in welfare data. Rob Bruce (1994 and personal communication) in estimating the probability of leaving welfare in BC, estimated thirty separate models[19] because F-tests led to rejection of the hypothesis that pooling the data was acceptable.

The second is that there is a history of suspicion regarding functional form in employment and training programs. The main contractor for the US Department of Labor controlled specifically for undetected non linearity in their model and concluded that they had been wise to do so. (Bryant and Rupp 1987) In addition Dickinson, Johnson and West (1986)[20] found that restricting the sample frame improved the regression results although within the restricted sample matching plus regression analysis offered no advantage over regression analysis by itself.

This section outlines the problem, and shows how it applies in the data used in this thesis and how matching can reduce the problem.

4.3.2.1 The Problem

Figures 4-1 and 4-2 show the actual (non linear) and fitted (linear) relationships between history of welfare dependence and subsequent welfare dependence for two non random samples of non participants. The first group was selected to have roughly even distribution of welfare histories (from zero to 12 months in

[19] He had initially intended to estimate 48 models, but reduced it to thirty because the sample sizes were small in some of the sub groups.

[20] Mahalanobis metric matching is a popular alternative to the cell matching used here, but Dickinson, Johnson and West found that the technique offered no improvement over cell matching. Its disadvantages are that it requires more comparisons, and it generates a one to one match. Cell matching gives one to many matches if the matches are available.

the previous year). The second sample was selected to have a distribution of welfare histories like that of the individuals in data source 10. The distribution for the second sample was far from even with almost 70% having been dependent on welfare for all 12 months in the previous year, and less than 1% having been dependent for zero months in the previous year.

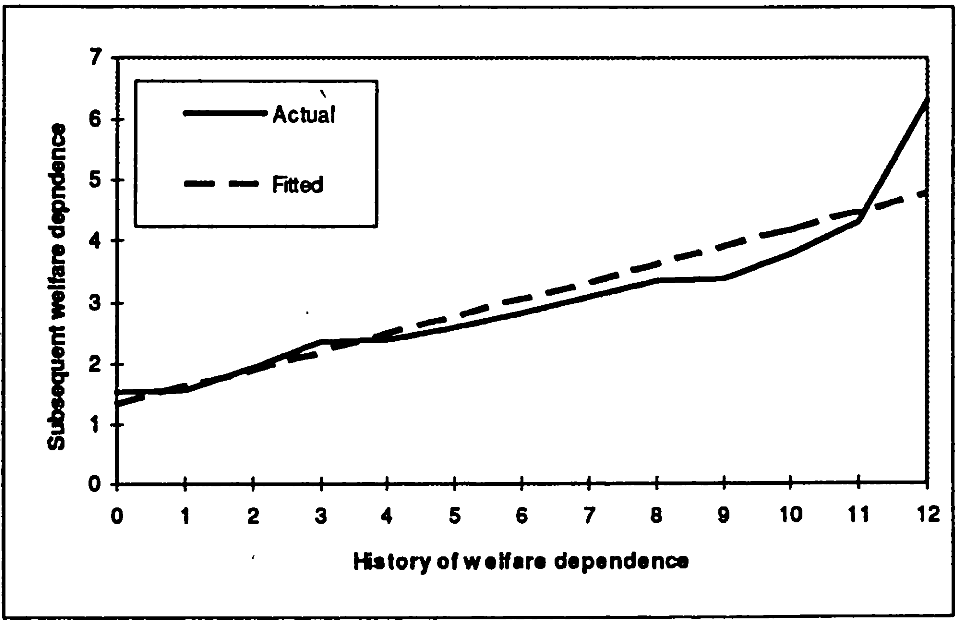

Figure 4-1 Actual and fitted (linear) welfare dependence, even distribution of welfare histories.

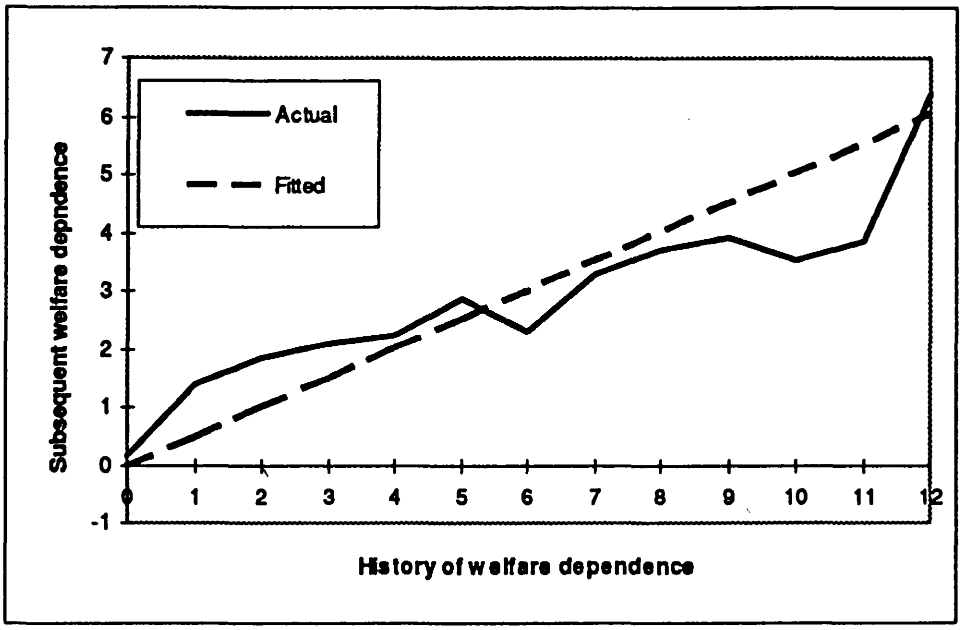

Figure 4-2. Actual and fitted (linear) welfare dependence, distribution of welfare histories like that found in group identified by interview (data source 10).

The relationship between prior welfare dependence and subsequent welfare dependence is similar for the two groups, and obviously non-linear in both cases. But the similarity in the relationship does not prevent the OLS fitted estimates from being very different. The coefficient on welfare history was .29 (t = 43.5) for the first group and .50 (t = 13.1) for the second group.

The effect of this difference will be to bias coefficients of variables that are correlated with deviations from linearity. A third regression gives us an indication of the extent of the bias that would result from mis-specifying the functional form in this way. The coefficient on a dummy variable identifying members of the selected group of is .9 or 7.5 percentage points (t=10.0). Recall that both groups were selected from non participants, were selected only on the basis of observed characteristics, and differed only in the distribution of observed characteristics.

Although the sample sizes in this example were large enough (20,000) to show the non linearities, we rarely have this luxury. Further, relationships other than that between welfare history and subsequent welfare benefits might also be mis-specified. Figure 4-3 shows the relationship between welfare history and subsequent welfare dependence for single men and single parents. Clearly the two relationships are not parallel, as would be implied by a functional form that included one of these family types as a dummy variable.

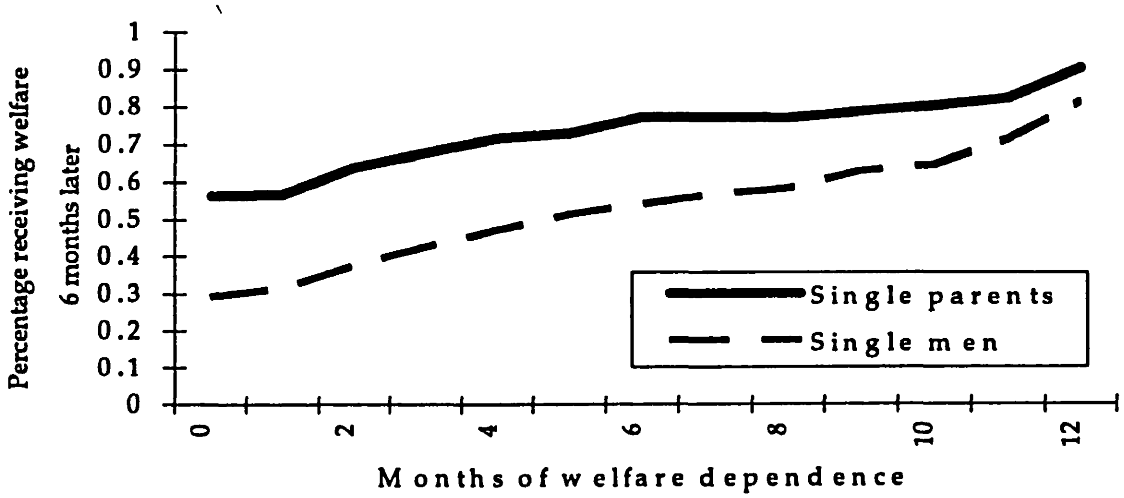

Figure 4-3 Relationship between welfare history and subsequent dependence single men and single parents.

4.3.2.2 A Solution

Fortunately, a solution to the problem of undetected non linearity has been proposed. Cochran and Rubin (1973) suggest that the comparison group be selected such that it is as similar as possible (in the explanatory variables) to the treatment group. If the match is one-to-one then no weighting is necessary. However, if the matching is one-to-many, then the comparison group observations must be weighted by the inverse of the number of matches. In this case, where we have a discrete explanatory variable, several members of the "treatment" group are matched with many members of the comparison group. In those cases the weights equal the inverse of the ratio of the number of comparison group matches to the number of treatment group matches.

4.3.2.3 Impact on the BC Study

This section has shown that reasonable specifications of functional form can lead to bias. This did not lead to a rejection of regression analysis altogether, but did lead to a greater tendency to use matched comparison groups.

4.3.3 Non response bias

Non-response bias is another form of omitted variable bias. It arises if the individuals who respond to a survey have characteristics different from the sample as a whole and if these characteristics are correlated both with program participation and with the outcome of interest. Non-response bias can arise if, for example, participants are more likely to respond than non participants (perhaps due to gratitude for the provision of the program) and if those who are less successful are more likely to respond (perhaps since those still on welfare would be easier to find or might feel an obligation to co-operate, or those who are independent of welfare are disinclined to have any further contact with welfare).

If we estimate the impact of the program using OLS to estimate the parameters of an equation of the form

$$y = X_1\beta_1 + X_2\beta_2 + \varepsilon$$

but only observe X_1, the extent of the bias in b_1 the estimate of β_1 is given by:

$$(X_1'X_1)^{-1}X_1'X_2\beta_2$$

Heroic efforts were made to increase the response rate in the survey in order to minimise problems with non response bias. Standard practice in dealing with potential non response bias is to increase the response rate, and success of the contractor in achieving a high response rate in our survey was notable. Nonetheless, the 25% of the sample that was not contacted was dramatically different from the 75% who were contacted. Administrative data showed that only 28% of the missing respondents were dependent on welfare 18 months after enrolling in a program or being selected for a comparison group, compared with 43% of the interviewed respondents.

These figures do not, by themselves, indicate that the results will be biased. If the omitted variables are not important ($\beta_2 \approx 0$) or if they are not correlated with program participation ($(X_1'X_1)^{-1} X_1'X_2 \approx 0$) then the bias will also be close to zero. Fortunately, Griliches(1986) has shown us a way to improve on the OLS estimates produced using information on the survey respondents when there is additional information on the full sample. In this case we have administrative information (program, age, sex, family type, classification and history of welfare dependence) on the full sample. He suggests that we estimate the parameters of equations in which each of the explanatory variables is on the left in turn and the rest of the explanatory variables are on the right hand side. Then use these parameters to produce predicted values for the missing variables.

Following this procedure has a dramatic impact on the estimates of impact of the program. A regression equation with welfare receipt 18 months after enrolment in the program as the dependent variable[61] was estimated using the administrative and survey data, but sample was restricted to the survey

[61] The mean of this (dummy) dependent variable is .43 for the survey sample and .39 for the full sample. Amemiya (1981) has shown that over the range 30% to 70% the coefficients of the linear

respondents. The attached tables[22] with the results of the regressions show that the all of the programs reduced dependence on welfare. The Community Tourism Employment Training Program (PGM1) reduced the probability of dependence in month 18 by 8.1 percentage points (t=2.2), the Employment Opportunity Program (EOP) by 13.9 percentage points (t=4.0) the Environment Youth Program (EYP) by 10.5 percentage points (t=1.7), job clubs by 5.0 percentage points (t=1.3) and the Forest Worker Assistance Program (FWAP) by 21.8 percentage points (t=1.8). But when we include the observation for the non-respondents, the coefficients on program participation change substantially. The coefficients associated with participation in EOP and FWAP fall from 13.9 and 21.8 percentage points to 9.1 (t=3.1) and 16.7 (t=1.8) respectively. Even more dramatic was the impact on the coefficients associated with participation in CTETP, EYP and job clubs , that fell to .2, 1.8 and 2.6 respectively, all with t's less than .6

4.3.3.1 Impact on the BC Study

The finding that, even with an unusually high response rate, non response bias could result in a qualitative change in the estimates was a major contributor to the decision to use survey data less and administrative data more.

4.4 Program impact

A full discussion of the impacts of the various types of employment and training programs for welfare recipients in British Columbia is given in Chapters 6 and 7. At this point, however, it is useful to provide a preview. The initial estimates of program impact were made using cell matching with administrative data. Three hundred and fifty cells were created for each month:

[22] In appendix C.

- Five sex and marital status classes: single men, single women, couples, two parent families, and one parent families
- Two employability classes: employable and unemployable
- Five age classes: under 23 years old, 23-30, 30-33, 33-50, over 50 years old
- Seven welfare-experience classes: out of the previous 25 months received benefits in no months, 1-3 months, 4-7, 8-12, 13-22, 23-24, all 25 months.

Each month, participants and non participants were divided into these 350 cells. This generated a comparison group for each participant, the non participants in the same cell as the participant. This group was unchanged over the observation period. The post program welfare dependence of EOP participants could then be compared with the comparison group, and a weighted[23] average dependence calculated. Results are given in Figure 4-1. It shows a dramatic reduction in the welfare dependence of the participants in the initial months together with a 10 to 15 percentage points difference in welfare dependence that lasts throughout the 48 month observation period. As with all of the graphs that follow, the X axis measures the number of months since the event in question, in this case enrolment in EOP. We were able to follow the welfare dependence of early enrolees in EOP and their comparison group for 48 months. The full sample is tracked for 24 months. The difference in welfare dependence between the participants and the comparison group is shown on the lower graph. The confidence interval is very narrow because (for this study of this program) the sample size is very large. There were 8,940 participants and more[24] (probably many more) comparison group members. The difference in the sample size is reflected in the confidence intervals.

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[23] The average dependence of the non participants in the cell weighted by the number of participants in that cell.

[24] In the early studies I created generic comparison groups in order to reduce computing costs. All non-participants (in excess of 100,000) were grouped into cells and then comparison groups were generated for each program using the cells that matched the participants for that program. I did not have the computer print out the final comparison group size, but the match is at least one-to-one in each case since no blank cells were used.

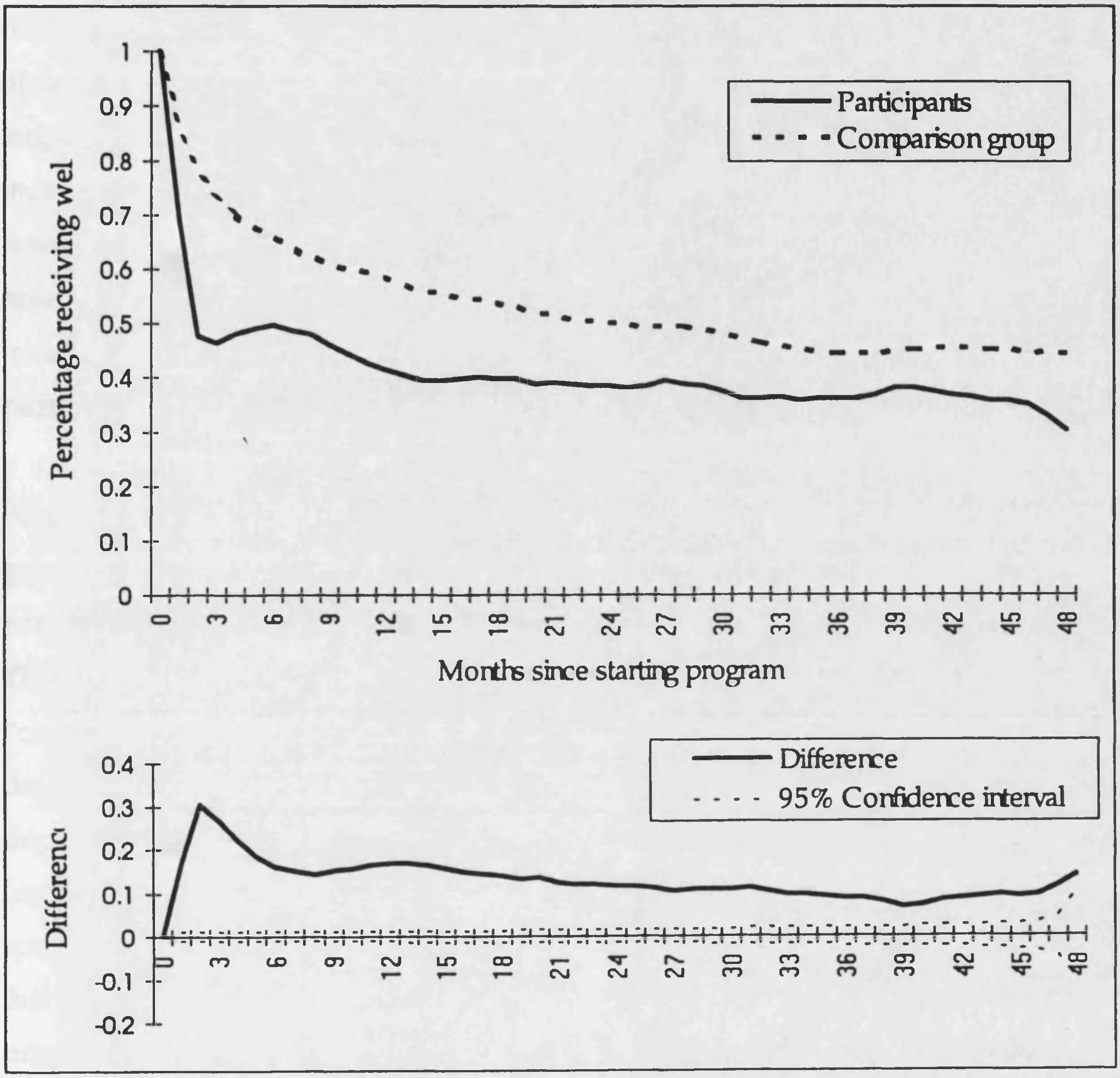

Figure 4-4 Percent dependent on welfare, EOP participants and comparison group

There are several interesting features of this result. Perhaps the first to catch the eye is the finding that two months after enrolment only half of the participants are independent of welfare. This result is due to two factors, drop outs and top ups. Recall that individuals are defined as participants if they receive any wages from an employer, even if they dropout of the program on the first day. Records of Employment indicate that almost 40% of participants in this program end employment within three months of starting. Second, individuals who receive income from employment that is less than the escape level for welfare may continue to receive (top up) benefits. Although a wage of $7 per hour [24] at 35 hours per week generates an income above the escape level for a single, this income is below the escape level for families with children. Almost 40% of participants in EOP were from cases with children.

Second, the rates of dependency for both the participants and the comparison group are strikingly high. Roughly half of participants, and people like the participants (the comparison group) were dependent on welfare four years later. This indicates that selection into this program is not characterised by extreme "creaming." In addition, this graph paints an interesting picture of welfare dependence that differs from the standard description based on spells of dependence. For example, Cragg (1994) reports that of his 18 subgroups, only female single parents over 35 years of age have more than 10% (at 10.1%) of spells with duration longer than 48 months. There are two differences between this graph and the graphs by Cragg (1994) or Cragg and Barrett (1995). First, the program participants are less likely to leave since they typically are well into a spell of dependence at the time that they enter the program, and state dependence has been clearly established for this group (See Cragg, 1994; Barrett, 1994; or Bruce, 1994). Second, this graph includes returns to dependence which is excluded from the analysis of spells.

Another interesting feature is the hint regarding the interaction between welfare and Unemployment Insurance. The increase in welfare dependence at around

[24] This is not to say that none of the cases with children became independent as a result of the program. Although many of the placements were at $7 per hour some were at higher wages.

six months coincides with the end of the subsidy and the achievement of UI eligibility in many cases. This could result in a job separation, followed by a short spell of welfare dependence while the Unemployment Insurance claim is processed. Roughly 12 months later, the UI eligibility would expire, and there is an associated return to welfare. (This relationship is more pronounced in programs with a specific duration of six months. See Chapter 7.)

Finally, the graph shows impacts that last throughout the observation period. Although this program is described as on-the-job training, it consisted almost entirely in placing an individual in a job. This, it was felt, would result in some on-the-job training, though not necessarily more than the participant would have received in a different job. If the program is interpreted in this light, then the difference in the subsequent welfare dependence can be interpreted as *scarring*, the deleterious effects of a bout of welfare dependence on the non participants.

It is appropriate at this point to wonder if the participant's success following this program should be attributed to some unmeasured variables that are correlated with program participation (selection bias) or to the program itself. That question is addressed in the next chapter.

4.5 Appendix 4-A: Chronology

Table 4-1: Chronology:
September 1985: Four Corners Agreement announced by federal and provincial ministers responsible for social services.
February 1986: Bill Warburton hired by the Research, Evaluation and Statistics Branch of the Ministry of Social Services.
April 1986: Letter of understanding implementing the Four Corners Agreement in BC comes into force, and funding for new programs begins.
April 1987: Begin identification of job-ready clients by interview by contractor.
June 1987: Estimate of the impact of the existing training programs is produced.
September 1987: Canada/BC agreement on exchange of information signed.
December 1987: Early estimates of impact of wage subsidy programs.
February 1988: Funding for enhancements to evaluations of employment and training programs approved.
January 1989: Contract with survey research firm signed and survey begins.
January 1989: Region A Job Action Pilot Project begins.
February 1989: Survey of RO's (for comparison group) begins.
April 1989: Minister reports estimates of impacts of programs to colleagues at meeting of federal/provincial ministers responsible for social services.
October 1989: Agreement for joint study with Camosun College.
April 1991: Wave 3 of the survey completed.
October 1992: Report on effectiveness of employment and training programs released by Ministry of Social Services.

4.6 Appendix 4-B: The relationship between the results from matching and the results from regression analysis

Suppose that we have a data set with N observations. N_1 observations have had a treatment (participants) and N_2 have not had the treatment (the comparison group). $(N_1 + N_2 = N)$

Y is the outcome of interest and X is a (k×1) vector of explanatory variables.

The relationship among the variables is

1. $Y = X\beta + T\delta + \varepsilon$

T is a dummy variable indicating training, and δ is the average training effect. We sort the observations so that T is an (N×1) vector with N_1 1's followed by N_2 0's.

Using the standard result for a partitioned regression (See e.g. Greene, 1990, page 182) gives

2. $d = (T'T)^{-1}\,T'(Y - Xb)$

where d and b are the OLS estimates of δ and β.

As a result of the simple nature of T the following relationships hold.

$(T'T)^{-1} = 1/N$

$T'Y = N_1\overline{Y_1}$

$T'X = N_1\overline{X_1}$

Where $\overline{Y_1}$ and $\overline{X_1}$ are the means of Y and X across the first N_1 observations.

Substituting, these relationships into 2 gives

3. $d = \overline{Y_1} - \overline{X_1}\,b$

We also know that

4. $\overline{Y} = \overline{X}$ b + $\overline{T}$ d since with OLS the mean of the estimated error is zero when a constant is included among the regressors. Decomposing these means into the means for participants and comparison group gives:

5. $\frac{N_1}{N}\overline{Y_1} + \frac{N_2}{N}\overline{Y_2} = \left(\frac{N_1}{N}\overline{X_1} + \frac{N_2}{N}\overline{X_2}\right)b + \frac{N_1}{N}d$

then using 3 gives

6. $\overline{Y_2} = \overline{X_2}b$

or

7. $b = \left(\overline{X'}_2\overline{X}_2\right)^{-1}\overline{X}_2\overline{Y_2}$

Substituting this into 3 gives

8. $d = \overline{Y_1} - \overline{X}_1\left(\overline{X'}_2\overline{X}_2\right)^{-1}\overline{X}_2\overline{Y_2}$

Premultiplying both sides by $\overline{X'}_2$ gives

9. $\overline{X'}_2 d = \overline{X'}_2 Y_1 - \overline{X'}_2\overline{X}_1\left(\overline{X'}_2\overline{X}_2\right)^{-1}\overline{X'}_2\overline{Y_2}$.

If the means of the explanatory variables are the same for the treatment and comparison groups then any of the k equations can be solved to get

10. $d = \overline{Y_1} - \overline{Y_2}$

That is to say, if the means of the explanatory variables are the same for the treatment and comparison groups then the difference in means of the explanatory variables (the matching result) is equal to the coefficient on the dummy variable for treatment in an OLS (best linear unbiased estimator) regression.

4.7 Appendix 4-C: Regression Results, survey respondents and full sample

Table 4-2 Coefficients on models of welfare dependence for survey respondents and full sample.	Survey Respondents Only	Full Sample
N	1424	1974
Variable	Coefficient (t-statistic)	
1. Constant	.476 (6.9)	.252 (5.0)
2. A dummy variable that takes the value 1 if the individual is a single female	0.032 (0.9)	0.057 (1.9)
3. A dummy variable that takes the value 1 if the individual is in a case with two adults and no children	-0.0755 (-1.1)	-.092 (-1.6)
4. A dummy variable that takes the value 1 if the individual is in a case with two adults and children	-0.031 (-0.6)	-.0088 (-0.2)
5. A dummy variable that takes the value 1 if the individual is a single parent	0.044 (1.3)	.076 (2.6)
6. A dummy variable that takes the value 1 if the individual is classified as unemployable.	-0.0458 (-1.3)	.048 (1.6)
7. The number of months in which welfare benefits were paid during the period 1 to 25 months before selection	0.016 (8.2)	.018 (11.2)
8. Years of elementary and secondary schooling	-.018 (4.0)	-.0054 (-1.9)
9. A dummy variable that takes the value 1 if the individual declared any post secondary training.	-.054 (-2.1)	-.061 (-2.8)
10. A dummy variable that takes the value 1 if the individual declared any university training.	.0014 (0.04)	.018 (.6)
11. Employment history (in months)	-.000359 (-0.7)	-.000148 (-.3)
12. Age (in months)	.579E-04 (0.5)	.134E-04 (.1)

13. A dummy variable that takes the value 1 if the individual participated in CTETP.	-0.081 (-2.2)	.0023 (.1)
14. A dummy variable that takes the value 1 if the individual participated in EOP.	-0.139 (-4.0)	-.10 (-3.1)
15. A dummy variable that takes the value 1 if the individual participated in EYC.	-0.104 (-1.7)	-.026 (-.5)
16. A dummy variable that takes the value 1 if the individual participated in Job Action.	-0.050 (1.3)	-.018 (-.6)
17. A dummy variable that takes the value 1 if the individual participated in FEP.	-0.218 (-1.8)	-.17 (-1.8)

5. Is Selection Bias the Bogeyman?

One legacy of CETA is a fear of selection bias and a hearty scepticism of estimates of the impact of employment and training programs that are not based on random assignment. For the most part, the estimates of the impact of employment and training programs reported in Chapters 6 and 7 of this thesis are not based on random assignment, and, as a result of the CETA experience, some people may view them with scepticism.

This chapter examines the issue of selection bias to determine whether this scepticism would be well founded. In addition, it provides insight into the nature of selection bias, and provides answers to the four questions identified at the end of Chapter 3:

- Can we conduct post-program tests for selection bias without random assignment?
- Are pre-program tests for selection bias reliable? and as a subsidiary question,

    - Does robustness to choice of base year mean that differences in differences estimators are unbiased?
- Are unmeasurable variables the primary source of selection bias?
- Why did different researchers come up with such different estimates of the impacts of the CETA programs?

The chapter has five sections. The remainder of the introduction provides some background on the selection process. The second section gives the results of five tests for selection bias. The analysis in it leads to the conclusion that scepticism regarding the results of this study would not be well founded. Taken together, the results of the tests lead to the conclusion that the estimates reported in Chapters 6 and 7 are reliable and that selection bias amounts to less than five percentage points. In addition, the tests illustrate the principle that it is possible to test observational studies for bias in the absence of random assignment.

In the third section evidence is presented that shows that pre-program tests are not reliable in the data, estimation techniques and selection mechanisms investigated here. This result holds even when many years of pre-program data are available. Further, robustness to choice of base year does not mean that differences in differences estimators are unbiased.

The fourth section shows that unmeasurables are not the primary source of selection bias. Rather, failure to control adequately for changes in major explanatory variables is a much more important source of variation in estimates, even in this relatively sparse data set and with this relatively vigorous selection mechanism.

The fifth section finds that the large differences in the estimates of the CETA programs produced by Westat (See, e.g., Bryant and Rupp, 1987) and those produced by Dickinson Johnson and West (1986) could have resulted from the fact that annual earnings data is inadequate to detect many pre-program dips in earnings. CETA participants experienced a dip in their earnings before enrolling in the program. Evidence presented in this chapter shows that if the dips experienced by CETA participants[1] were similar to the dips experienced by participants in programs in British Columbia, then it would not be possible to detect and control for the pre-program dip in earnings adequately with annual data.

5.1.1 Selection

Bias in estimates occurs when omitted variables are correlated both with program participation and with the outcome of interest. This bias is referred to as selection bias when it is the selection process that causes the former correlation. One approach to dealing with selection bias is to examine and model the selection process explicitly. If, for example, researchers can identify factors that affect program participation but that do not affect the outcome of interest, then it becomes possible to estimate the impact of programs using a two stage estimator.[2]

An alternative, followed in this thesis, is to investigate the circumstances under which selection bias occurs and the extent to which selection bias is a problem. Careful modelling of the selection process is not an integral part of this approach

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[1]For a detailed discussion of the 'dip' in the American context, see, e.g. Devine and Heckman, 1994 or Heckman and Smith, 1994

[2]This approach is being followed by Devine and Heckman (1994). Diagram 1 suggests that modelling the various paths into the programs is a daunting task indeed.

and so a detailed description of the selection process is not included in this thesis.

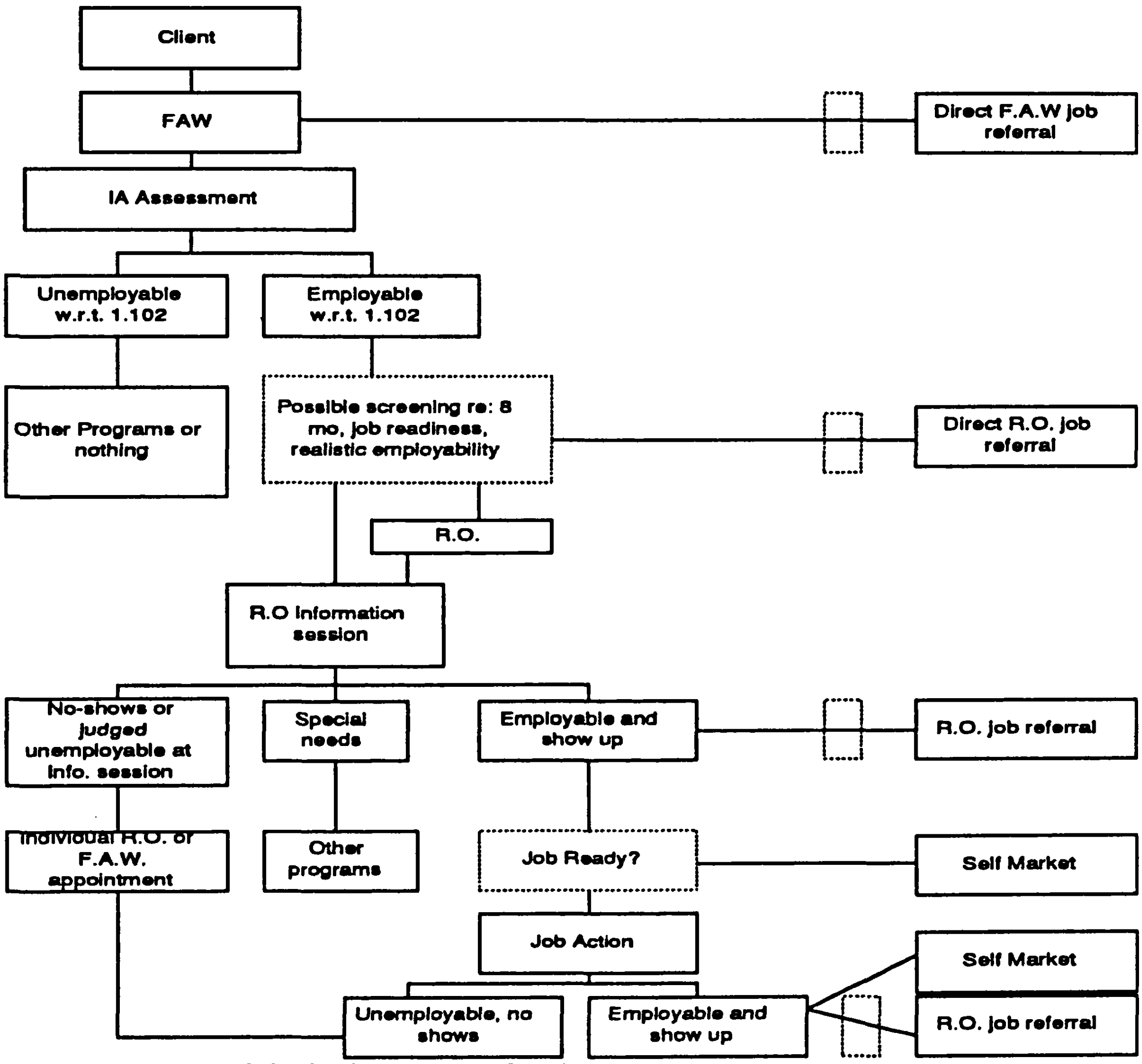

Diagram 5-1 Model of selection mechanism

Nonetheless, the extent to which selection occurs has implications for the usefulness of the findings. If the selection process into the programs investigated here were innocuous, then a finding that selection bias was not much of a problem in estimates of the impact of these programs would be of less interest than if the selection were more vigorous.

Selection into the programs analysed in this thesis occurred in many steps and involved many individuals. A schematic of the selection process prepared by the DPA Group (Diagram 5-1) shows that selection was a complex and varied process[3]. A brief summary of their conclusions follows. Selection into the program occurred in four steps. First there was an element of self-selection. Second, Financial Assistance Workers referred a subset of their clients to Rehab Officers. Third, Rehab Officers referred roughly three clients to prospective employers for each vacancy, and fourth, the employer would select the most suitable candidate. This process tended to result in program participation by welfare recipients who were neither the most employable (the most employable become independent on their own before they can complete this multi-staged process) nor the least employable (who would not be selected). Nonetheless the DPA Report concluded that it tended to select clients who were "more employable" than average [page 7-2].

5.2 Post Program Tests for Selection Bias

This section reports the results of five tests for selection bias. Four of the five tests indicate that selection bias will result in an overestimate of program impact. The fifth, a comparison between a cell matched comparison group and a randomly assigned control group suggests an underestimate in the short term and no bias in the long term. The results of the tests taken together suggest that the bias is not sufficiently large to overturn the qualitative results that are reported in the next two chapters.

Briefly, the tests were as follows:

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[3] The study of the selection process was completed by a private contractor, the DPA group. More information on the selection process is available in their report (DPA Group, 1988).

- Tests 1, 2, and 3: Persons selected to participate in a program who nevertheless did not participate were identified and compared with the comparison group.
- Test 4: A control group, generated by randomly assigning individuals referred to a job club program, is compared with a comparison group.
- Test 5: Programs in which both program impact and selection bias are believed to be positive are examined for long-term impact.

These tests are not general because the extent of selection bias depends on the selection mechanism and the data that is available. For example, Test 1 interviews the rehab officers, the programs' gatekeepers, and asks them to identify other individuals who would be suitable candidates for a program. The test consists of comparing the subsequent welfare dependence of these non participants with the subsequent welfare dependence of other non participants. Differences indicate selection bias. As with applying any empirical results from one jurisdiction in another, care must be taken in using this estimate of selection bias in other jurisdictions. A number of factors could affect the results: the attitudes of the program gatekeepers; the nature of the programs; the perception of the programs, characteristics of participants or non participants, and of course availability and quality of data. Nonetheless, the principle of testing for selection bias is quite generalizable. A person with any method of estimating the impact of programs, in almost any jurisdiction could interview program gatekeepers and ask them to identify comparable non participants. The impact of the "program" produced by the estimation method would be an indication of selection bias in that method and data.

5.2.1 Test 1: Non-participants identified by Ministry staff

Rehab Officers were asked to attempt to mimic the selection process by identifying recipients they would have referred to programs had there been space available. The weaknesses of this test are:

- We can never be sure that the Rehab Officers did identify clients which are truly comparable to program participants.
- About 3% of the group identified in this way eventually did participate in a wage subsidy program.

Nonetheless there are three reasons why this test might be valid.

- There were lots of potential candidates. About 230,000 different cases received welfare in British Columbia in 1989, and only a few thousand participated in on-the-job training.
  - The Rehab Officers as a group support evaluation.
  - By the time clients had made it into the Rehab Officers caseloads, the clients had been through two of the three selection hurdles, self selection and referral by the financial assistance worker.

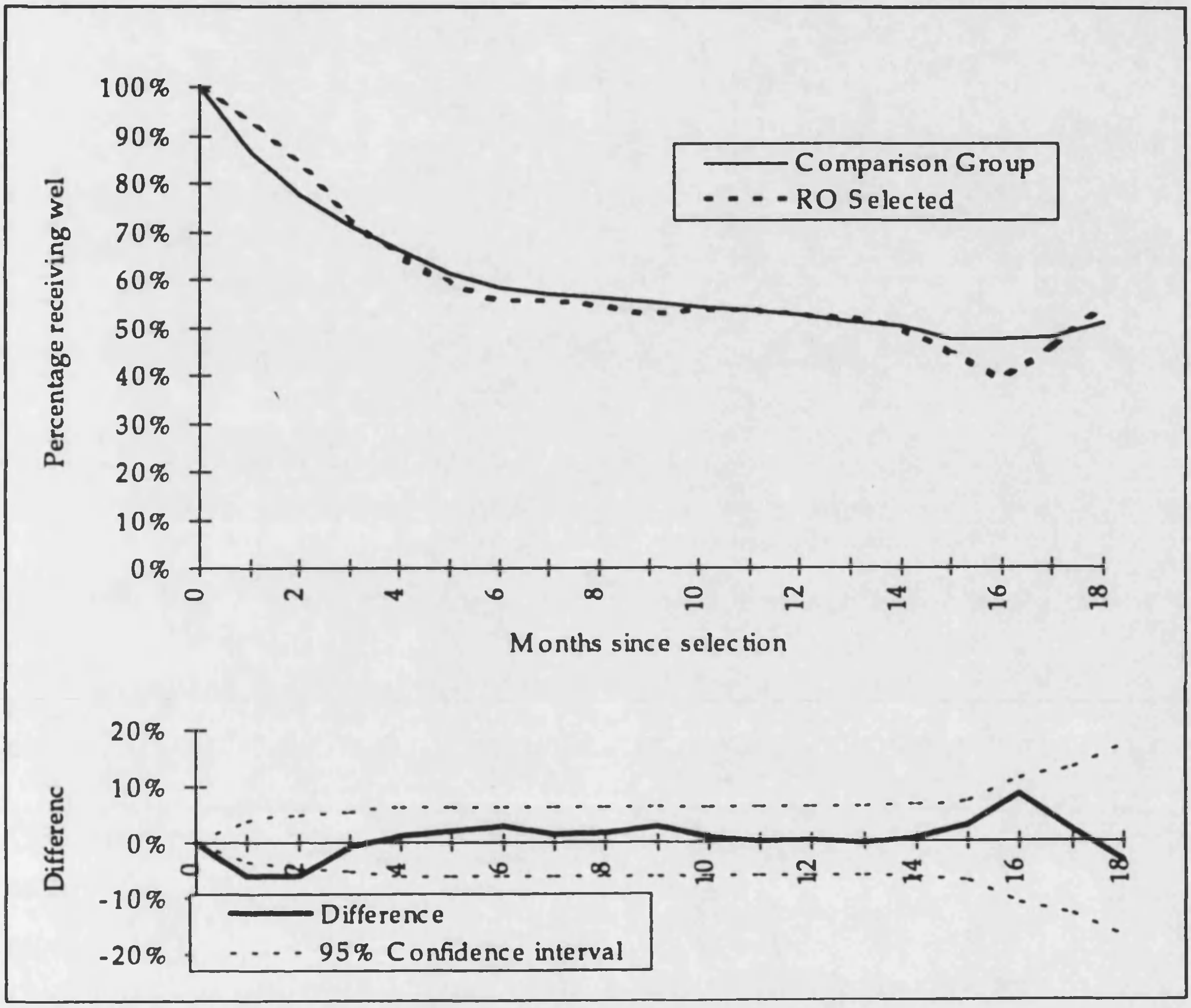

Figure 5-1 Percent dependent on welfare, Rehab Officer selected group and comparison group Data source 5; comparison group A

Four hundred and forty individuals were identified in this way. Their subsequent welfare dependence together with that of a comparison group[4] is shown in Figure 5-1. The results show an upper limit of about five percentage points in selection bias, with the selected group being less likely to be dependent on welfare.

[4] Drawn in the manner described in the last section of Chapter 4.

5.2.2 Test 2: Non-participants identified by an interview

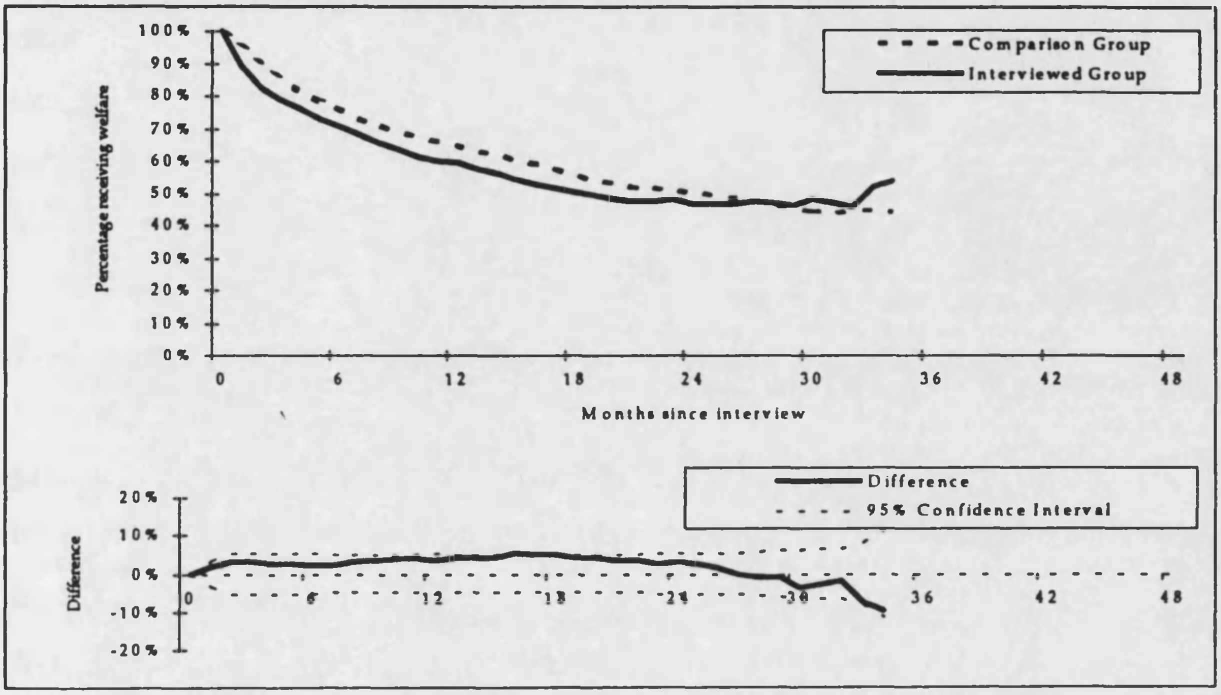

Figure 5-2. Percent dependent on welfare, clients identified in interview and comparison group Data source 10; comparison group A

A number of years ago and for purposes totally unrelated to program evaluation, a process was implemented that approximated the results of the normal selection process undergone by recipients who enter employment and training programs. A contractor was given the names and addresses of 48,000 Income Assistance recipients. Most were rejected: because their cases had closed, because they had moved from the area, by Ministry staff, and for other unspecified reasons. The remaining 12,800 recipients were sent letters inviting them to be interviewed by the contractor for possible entry into employment or training programs. Of these, 8,000 showed up to be interviewed, and, of these, 4,283 were classified as job-ready employables. At that point the amount of selection and self-selection that had occurred appeared comparable to what would be needed to get into such programs in the normal course of events.

Differences between the subsequent dependence of these 4,283 persons and the dependence of a comparison group developed to mirror them would be due to the latter's inability to account for the influences of the selection process. No difference would be strong evidence of no such unknown and uncontrolled influences, that is, no selection bias.

The results showed a difference of about five percentage points (Figure 5-2), with the selected non-participants less welfare-dependent than the comparison group. The reversal in the direction of the bias after two years is not an effect of time. It disappears when recipients interviewed in the first six months are excluded. (The reason is unknown.) With this qualification, the test indicates about five percentage points of selection bias.

5.2.3 Test 3: Non-participants identified by employer rejection

Under the Employment Opportunity Program more clients are referred to employers than are hired. A group of 325 recipients selected for on-the-job training and referred to employers, but not hired, was identified. If selection bias tends to favour greater independence, "rejection bias" by employers would tend to indicate greater dependence. The results should reflect the sum of the selection bias from the self-selection, selection by the Rehab Officers and Financial Assistance Workers, and the "rejection bias" by employers. The net effect is negative between months five and twelve and zero thereafter (Figure 5-3).

Since selection bias appeared to be positive between months four and ten in the tests reported above, this test suggests that selection by employers does increase selection bias in estimates of short term impacts, but has no effect on estimates of long term impacts.

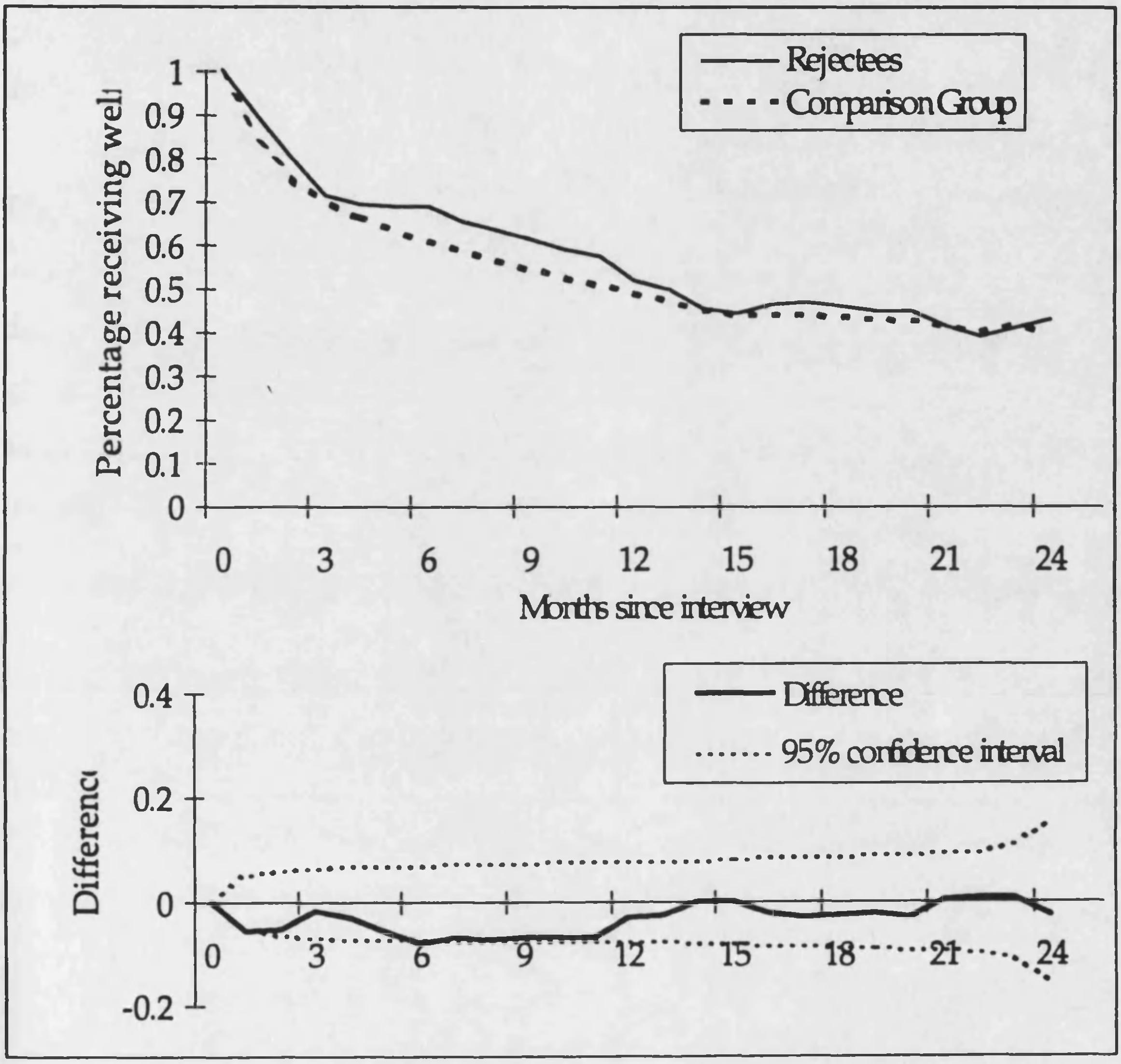

Figure 5-3. Percent dependent on welfare, clients rejected by employer and comparison group Data source 3; comparison group A

5.2.4 Test 4: Against Random Assignment

Because random assignment will produce unbiased estimates of program impacts, it is popular to assess the adequacy of comparison groups by comparing the estimates of programs produced using comparison groups with those produced using control groups. (e.g. Lalonde 1986, Fraker and Maynard 1987, Cain et al. 1993, Friedlander and Robins 1994) Although the inability of a comparison group to replicate the results of a control group is sufficient reason to reject the comparison group, its ability to replicate the results of a control group may not be a strong test, since random assignment may eliminate much of the selection that occurs in a normally operating program.

The impact of British Columbia's job search program, Job Action, was estimated using random assignment. Figure 5-4 reports the subsequent welfare dependence of the control group and a comparison group. The comparison group underestimates the program's impact, compared to the control group, by as much as 15 percentage points in the first few months. There is no underestimate in the long run.

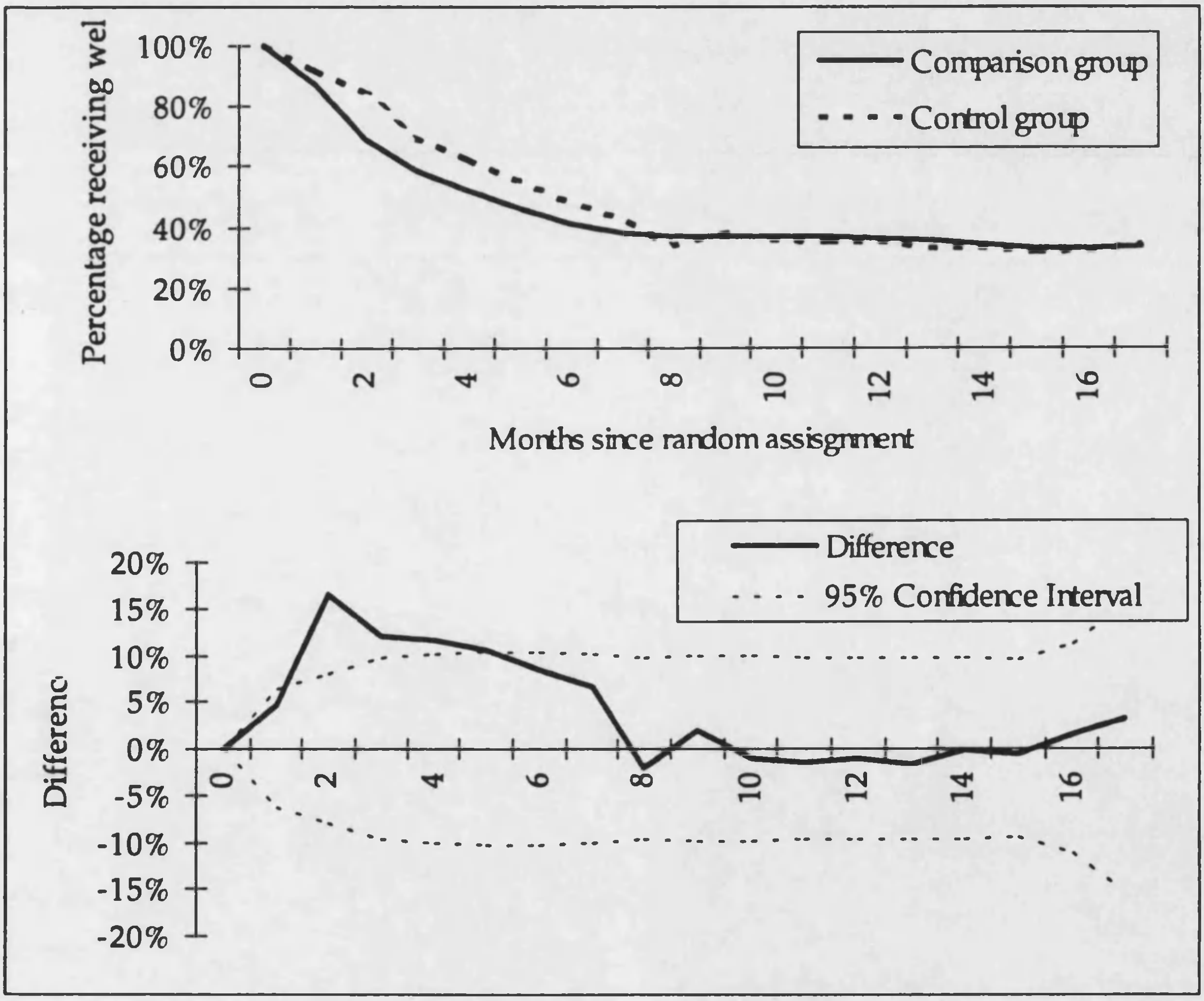

Figure 5-4. Percent dependent on welfare, pilot project control group and comparison group Data source 8; comparison group A

It is interesting to note that the comparison group underestimates program impact because selection bias in training programs is normally expected to result in an overestimate of the impact of a program. (See discussion in Test 5 following.) The reason for the direction of the bias lies with the mechanics of the payment system.

In British Columbia, welfare cheques are dated for a Wednesday, normally the last Wednesday of the month. Welfare is paid in advance. Attached to each cheque is a Request for Continued Assistance form. Recipients are required to complete this form and return it to the district office by the fifth of the following month, and the information on it is used to calculate entitlement for the subsequent month. If the Request for Continued Assistance is not returned to the office, the welfare cheque is sent to the district office instead of the home address, and if the recipient does not go to the office and submit a Request for Continued Assistance the cheque is reversed and the case is closed.

For example, cheques dated April 27, 1995 provide benefits for the month of May. Recipients must complete the Request for Continued Assistance and return it to the office by May fifth and the information on it will be used to calculate entitlement for June. As a result, a recipient may legally submit their Request for Continued Assistance at the end of April, get a job on May first, and yet receive benefits for May (through the April 27th cheque) and June by virtue of the Request for Continued Assistance submitted at the end of April.

As a result most people get at least one month of benefits after they get a job. Since from five to ten percent of the caseload becomes independent each month, five to ten percent already have a job and are receiving their last month's benefits. These individuals, according to systems information, are receiving welfare benefits and so appear to be suitable participants for training programs, but because they already have a job, would not participate. If we select a comparison group from those on welfare, a number of the individuals who already have a job, and who will become independent in the next month, will be included.

This will bias the results. There is an unmeasured difference between the participants and the comparison group (some of the comparison group members already have a job) and this unmeasured characteristic is correlated both with program participation (those who have a job are less likely to participate) and with the outcome (those who have a job are more likely to become independent). This source of bias will cause an underestimate of the impact of the program on subsequent welfare dependence.

This bias will be greatest where the caseload turnover is highest and, as a consequence, the proportion of recipients who already have a job is highest, that is among new applicants for welfare, and among young single employables.[5]

In summary, the comparison group, like the participant group, is drawn from welfare recipients. Because welfare is paid in advance, recipients who get jobs will receive benefits in that month, and may (legitimately) receive benefits in the subsequent month as well. Therefore, every comparison group will unavoidably contain recipients who have just become employed and who will leave welfare almost immediately. That would not be true of the participants, because the very fact of participating in a job search program is a sign that one does not have a job. Test 4 found evidence of substantial bias in estimates of short run impact but no evidence of bias in estimates of long-run impact from this source.

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[5]For a discussion of rates of turnover, see Bruce 1994 or Barrett and Cragg 1995.

5.2.5 Test 5: Where Analysis Shows No Long-term Impact

Notwithstanding the results of Test 4, selection bias is normally expected to result in an overestimate of program impact[6]. A process evaluation of the on-the-job training programs for welfare recipients in British Columbia found that "overall, it appears that the vast bulk of referrals by staff to job openings is of recipients with the best chance of succeeding". [DPA Group, 1989] If participants are more employable in both measured and unmeasured variables, then the correlation between the unmeasured variables and program participation and between the unmeasured variables and the outcome of interest is expected to result in an overestimate of program impact.

In addition, training programs are generally believed to either help or have no impact on participants. In this case this will be true if, on average, providing an individual with six months of paid employment does not hurt their subsequent employment prospects.

[6]As was found in Tests 1 and 2.

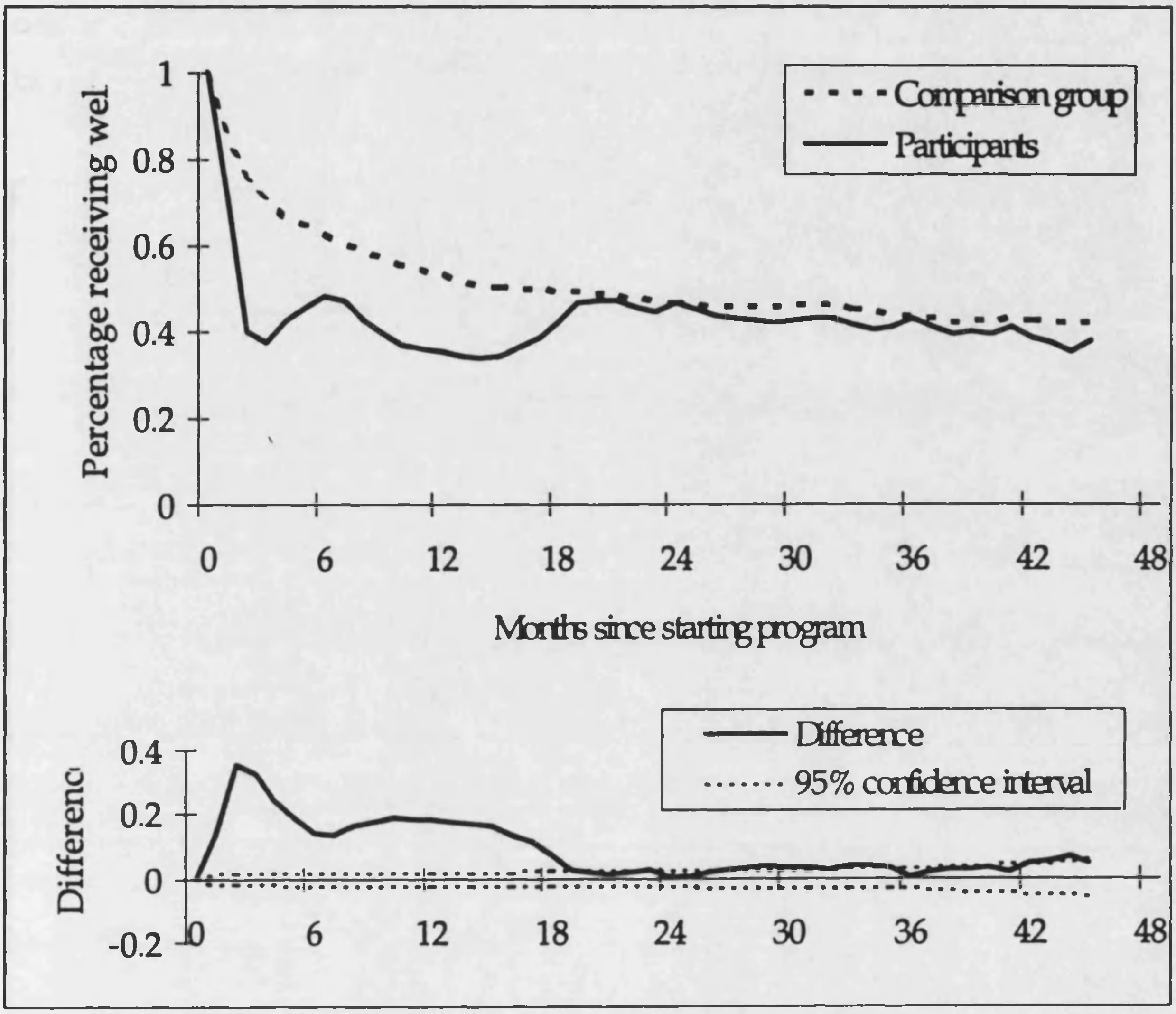

Figure 5-5. Percent dependent on welfare, CTETP participants and comparison group Data source 4; comparison group A

If both of these beliefs are true, that is, both selection bias and program impact are positive, then any observed long-term impact which is the sum of selection bias and program impact, sets an upper limit on selection bias.

Two programs present themselves as candidates for indicating this upper limit on selection bias, the Community Tourism Employment Training Program (CTETP, Figure 5-5) and the Environment Youth Corps Program (EYC, Figure 5-6). In both cases the significant short term program impacts (0-18 months) are associated first with employment under the program and then with dependence on Unemployment Insurance. The long term impact, the upper limit of selection bias in these cases is about three percentage points.

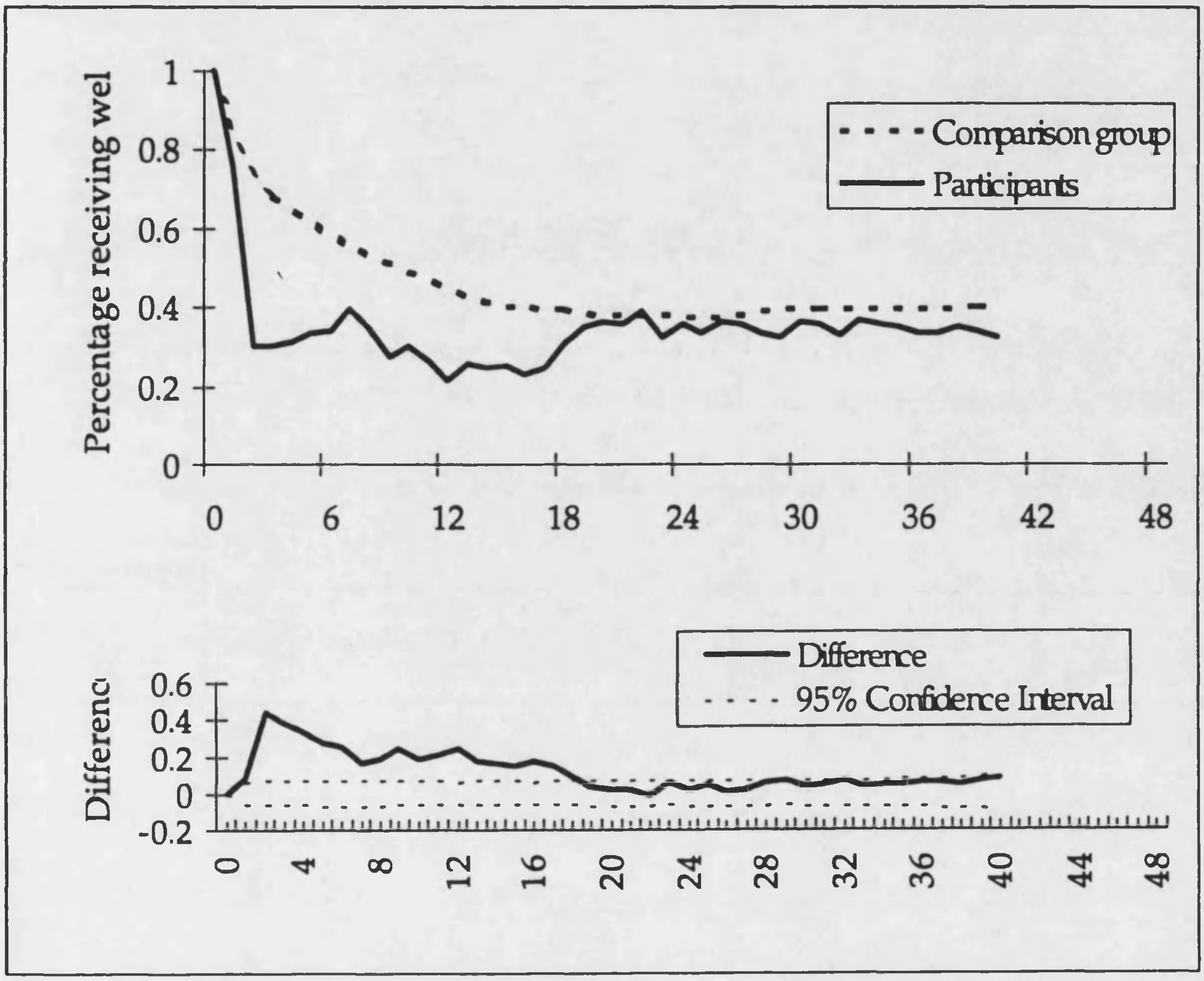

Figure 5-6. Percent dependent on welfare, EYP participants and comparison group Data source 4; comparison group A

A caveat: the results of this test are not directly applicable to the estimates of the impact of EOP. Although the selection process for the public sector employment programs (CTETP and EYC) is theoretically the same as for the private sector EOP, we might suspect that the selection by employers who are not required to bear any of the costs of employment, and who are not expected to retain the employees beyond the subsidy period (CTETP and EYC) would be less stringent than the selection by private sector employers (EOP).

Pre-program Tests for Selection

Selection bias occurs when omitted variables are correlated both with program participation and with the outcome of interest. When random assignment is used, the correlation between values of all characteristics of the participants and controls, measured prior to program participation, is close to zero by design. Indeed, comparison of pre-program characteristics is often used to ensure that assignment has in fact been random. This suggests a comparison of pre-program differences between participants and the comparison group as a test for selection bias.

Strictly speaking, similarity between participants and comparison group in pre-program variables is neither a necessary nor a sufficient condition for the comparison group to provide unbiased estimates of program impact. If an event which causes participants to differ from comparison group members is coincident with program participation then pre-program similarity will not ensure unbiased estimates (not sufficient)[71]. If the variable which differs in the pre-program period is not correlated with the outcome of interest, then the estimates will not be biased, despite the pre-program differences (not necessary).

The usefulness of these tests is an empirical question. If we find that studies in which the pre-program characteristics of participants and comparison groups are similar tend to be unbiased, we may begin to use pre-program similarity as an indication of unbiasedness in the study. Consequently this section has two objectives:

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[71]For example, if two individuals were laid off from the same plant, the one who knew that he would not be recalled would be more likely to take training.

- to use additional pre-program information to test for selection bias; and
- to test the usefulness of pre-program information in testing for selection bias.

The additional information is only available for a one-in-ten sample that differs from the full sample in two respects. First, the sample is not a random sample, it excludes those on ancillary welfare programs, it is restricted to those with valid SIN's, and it is restricted to those who received welfare in the month in which they enrolled in the program. Second, there is information on all participants for the full 36 month follow up period, so the mix of participants in the last year differs from the mix for the full sample.

5.3.1 Testing for bias

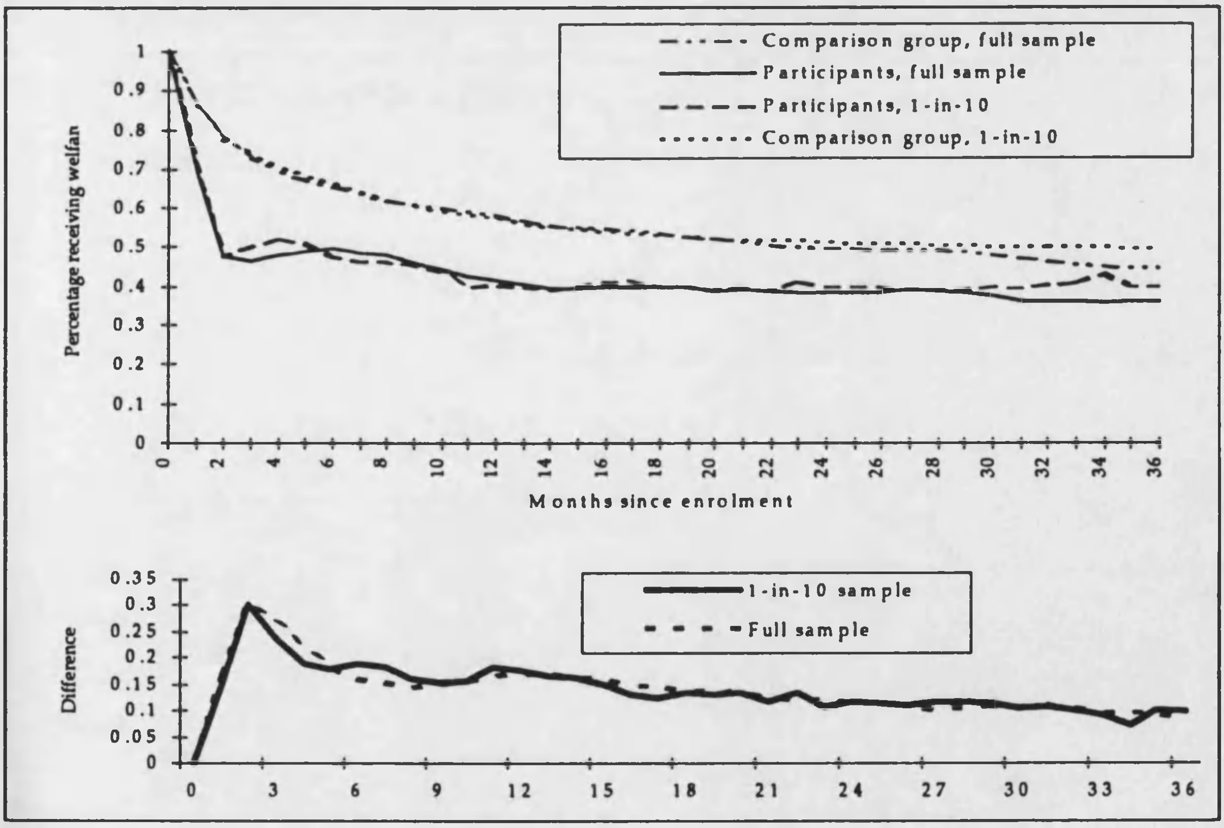

Figure 5-7 Welfare dependence of full sample and 1-in-10 sample, and comparison groups (EOP) Data source 4; comparison group A

Figure 5-7 reproduces Figure 4-4 using the one-in-ten sample. The impact of the different participant mix in the final year is evident, but the differences in the estimates of the impact are not significant in any month.

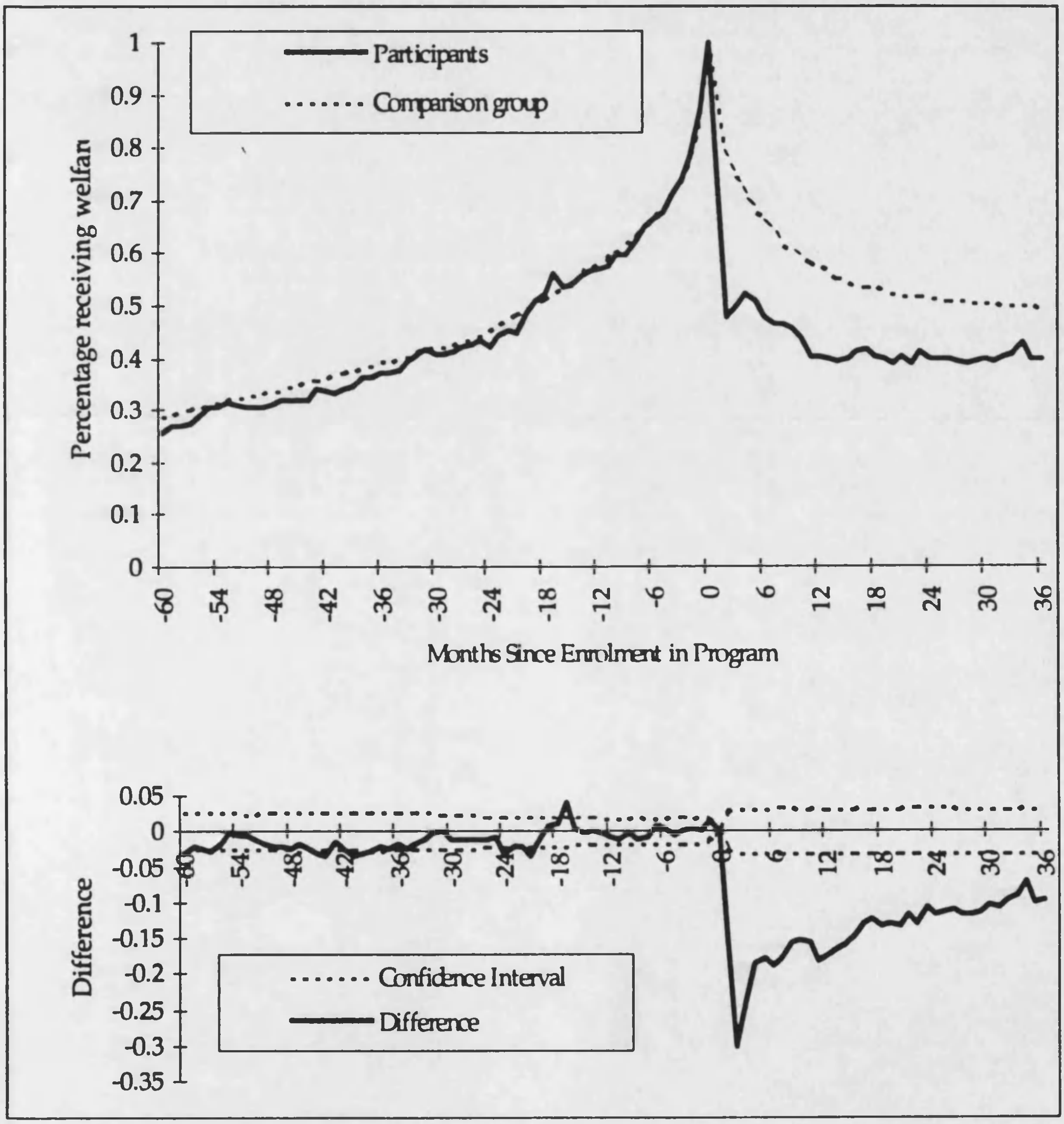

Figure 5-8. Percent dependent on welfare, EOP participants and comparison group. (Right side reproduces Figure 5-7 for 1-in-10 sample.)

Data sources 1, 4; comparison group A

Figure 5-8 gives a five year pre-program history of welfare dependence for program participants and a comparison group. Recall that the comparison group matches participants on age (five groups), gender/marital status (five groups), employability (two groups), history of welfare dependence over the past 25 months (seven groups) and welfare dependence at the time the participant enrolled in the program. The two groups track remarkably well, with the comparison group slightly more inclined to receive welfare. This pre-program test indicates that selection bias is perhaps a percentage point or two.

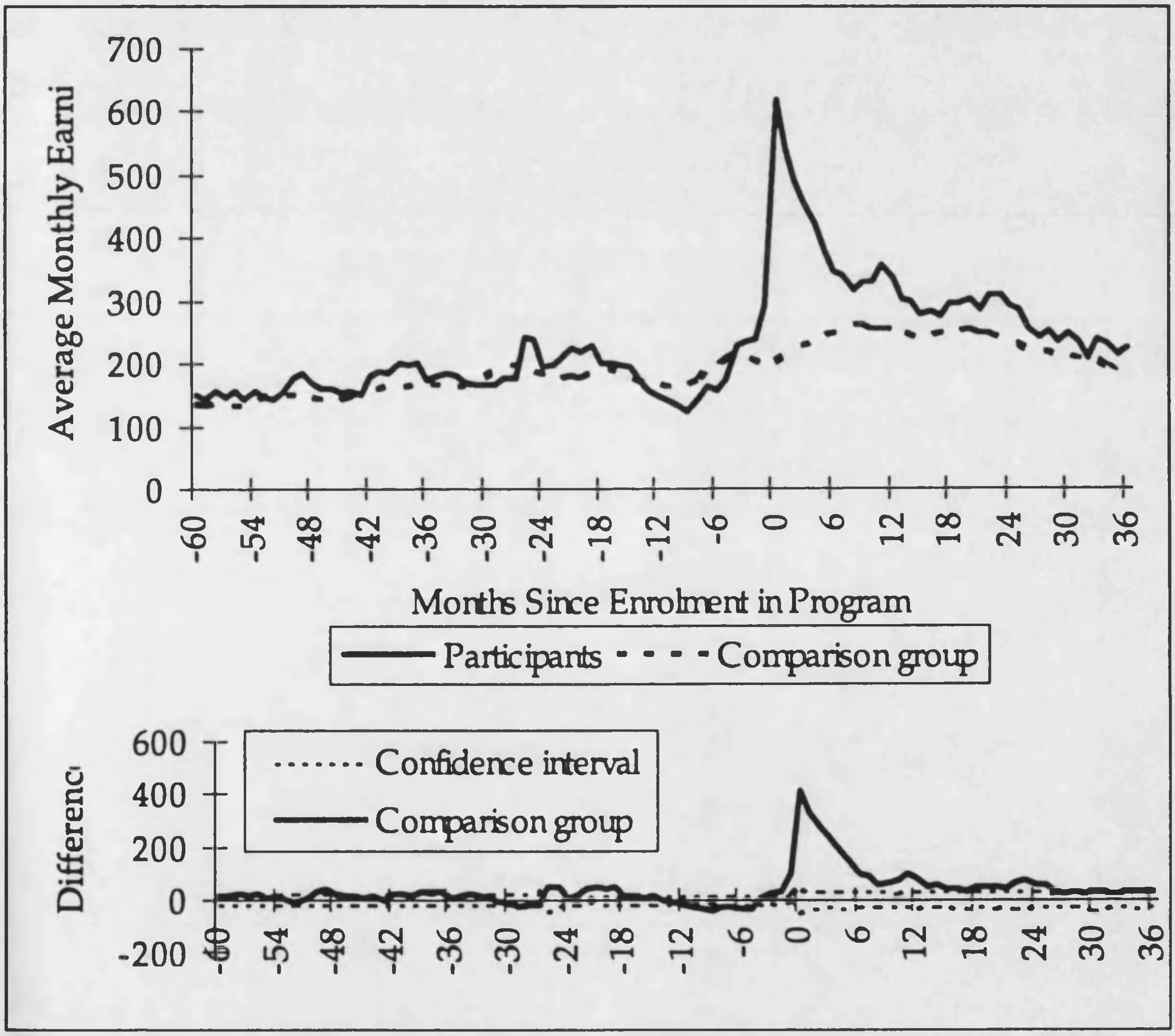

Figure 5-9 Pre-program earnings of participants and comparison group (EOP)

Data sources 4. 6: comparison group A

Figures 5-9 and 5-10 report the results of pre-program tests on earnings and UI, variables that were not used to select the comparison group. They paint a much less optimistic picture. The pre-program earnings of the comparison group do not dip in the same manner as the participants', although they do match reasonably well over the prior five years. The pre-program UI experience of participants is about four percentage points (about 25%) lower than the participants'.

The pre-program test for selection bias indicates that the estimates of the impact of the program on subsequent welfare dependence are relatively free from bias, but the estimates of program impact on UI dependence and earnings are biased.

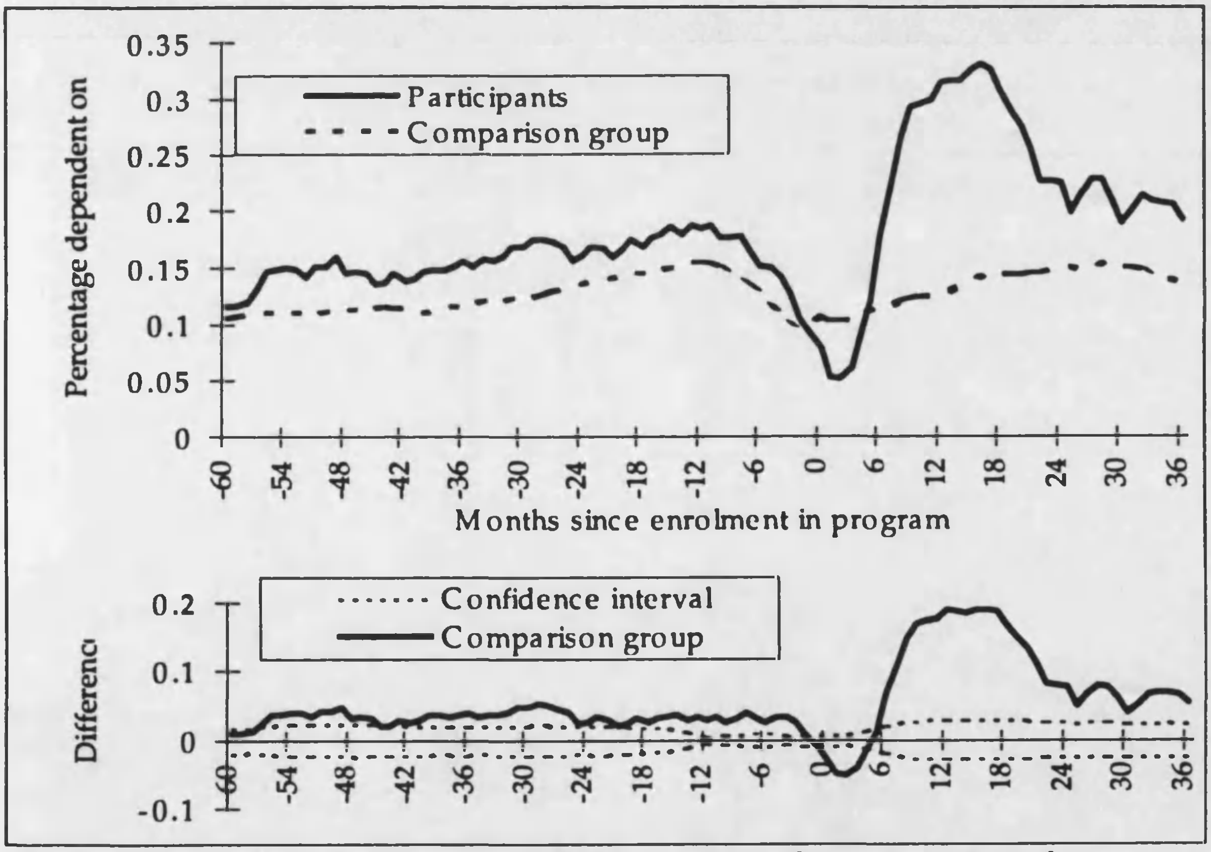

Figure 5-10 Pre- and post program UI dependence of participants and comparison group (EOP) Data sources 4, 7; comparison group A

5.3.2 Assessing the usefulness of pre-program tests

A second comparison group is selected to assess the reliability of this finding. The second group is selected to match the participants on all available pre-program characteristics including UI dependence employment and earnings, information that was not available in the selection of the original comparison group. Because the second group matches the participants in the new variables as well as in all the variables used to produce the first comparison group there are fewer unmeasured variables and (maintaining the assumption that selection will induce a correlation between program participation and factors reflecting employability) the selection bias will be no greater[8]. Difference between the estimates produced by the two comparison groups is an estimate of the selection bias in the simpler estimate.

[8] Recall that the matched results are identical to the regression results if the means of the explanatory variables are the same in the treatment and comparison groups. Matching on an additional variable on the bias has the same effect as including another variable in a regression equation. Omitted variable bias is $\sum p_i \beta_i$, where p_i is the coefficient in a regression of the included variable of interest on excluded variable i and β_i is the coefficient on the excluded variable in the true model. If selection results in more "employable" people participating in the program then p_i and β_i will have the same sign if the dependent variable is correlated with employment or opposite signs if the dependent variable is inversely correlated with employment (e.g. if the dependent variable is welfare dependence as above.). Increasing the number of variables in the equation will decrease the bias.

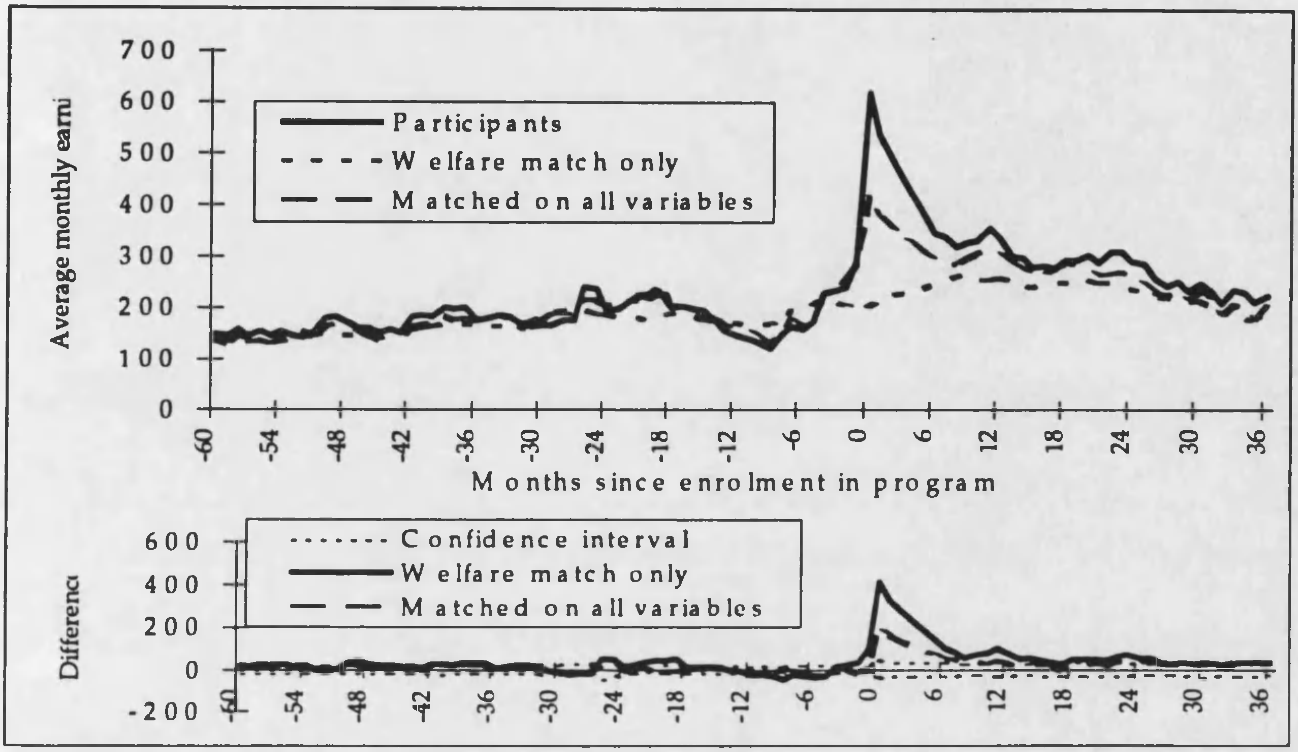

Figure 5-11. Pre- and post program earnings of participants and two comparison groups (EOP) Data sources 4, 6; comparison groups A and B

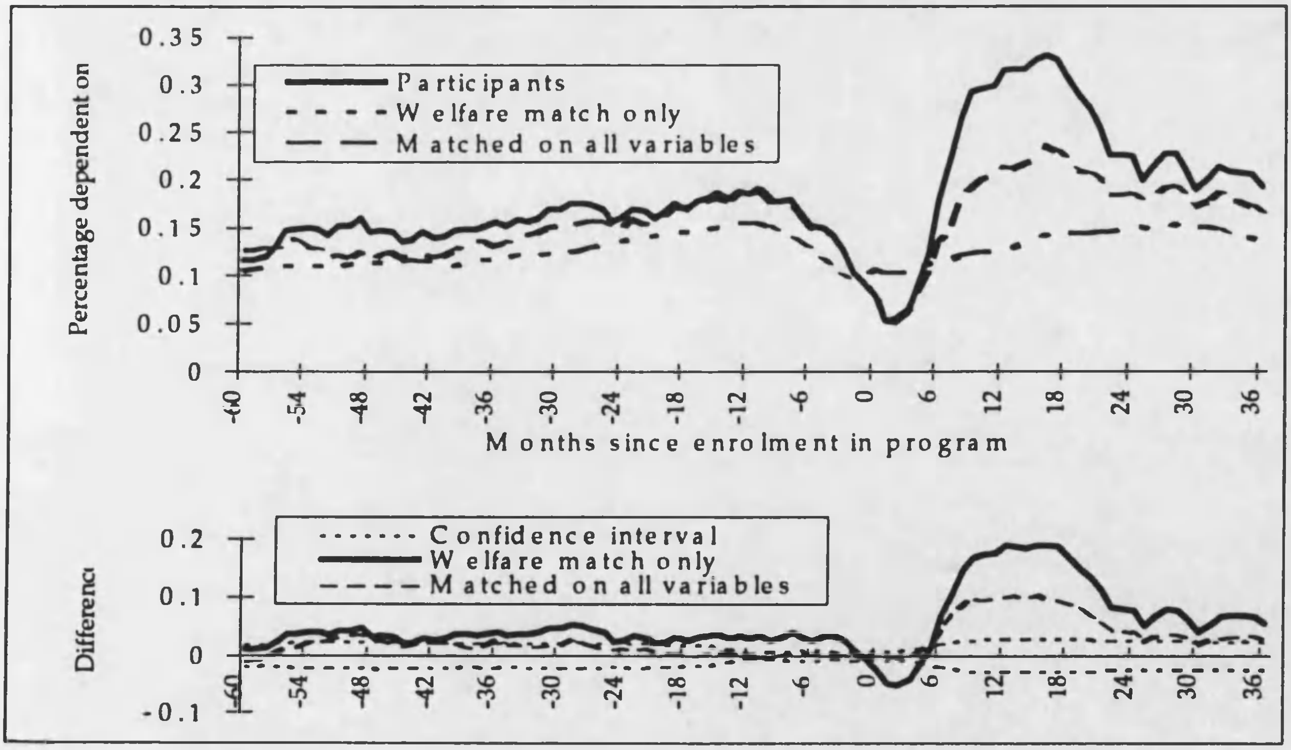

Figure 5-11a. Pre- and post program UI dependence of participants and two comparison groups (EOP) Data sources 4, 7; comparison groups A and B

Intuitively, we expect prior earnings and UI experience to be correlated with our outcome of interest, post program welfare dependence, so we expect that matching on these variables will reduce bias and affect our estimates of program impact. Figures 5-11 and 5-11a superimpose the pre-program earnings and UI experience of the new comparison group on Figures 5-9 and 5-10. The earnings of the new comparison group dip in a manner similar to the earnings of the participants, and the UI history is much more similar to the UI history of participants, although the differences are statistically significant in some months more than 24 months before the enrolment date.

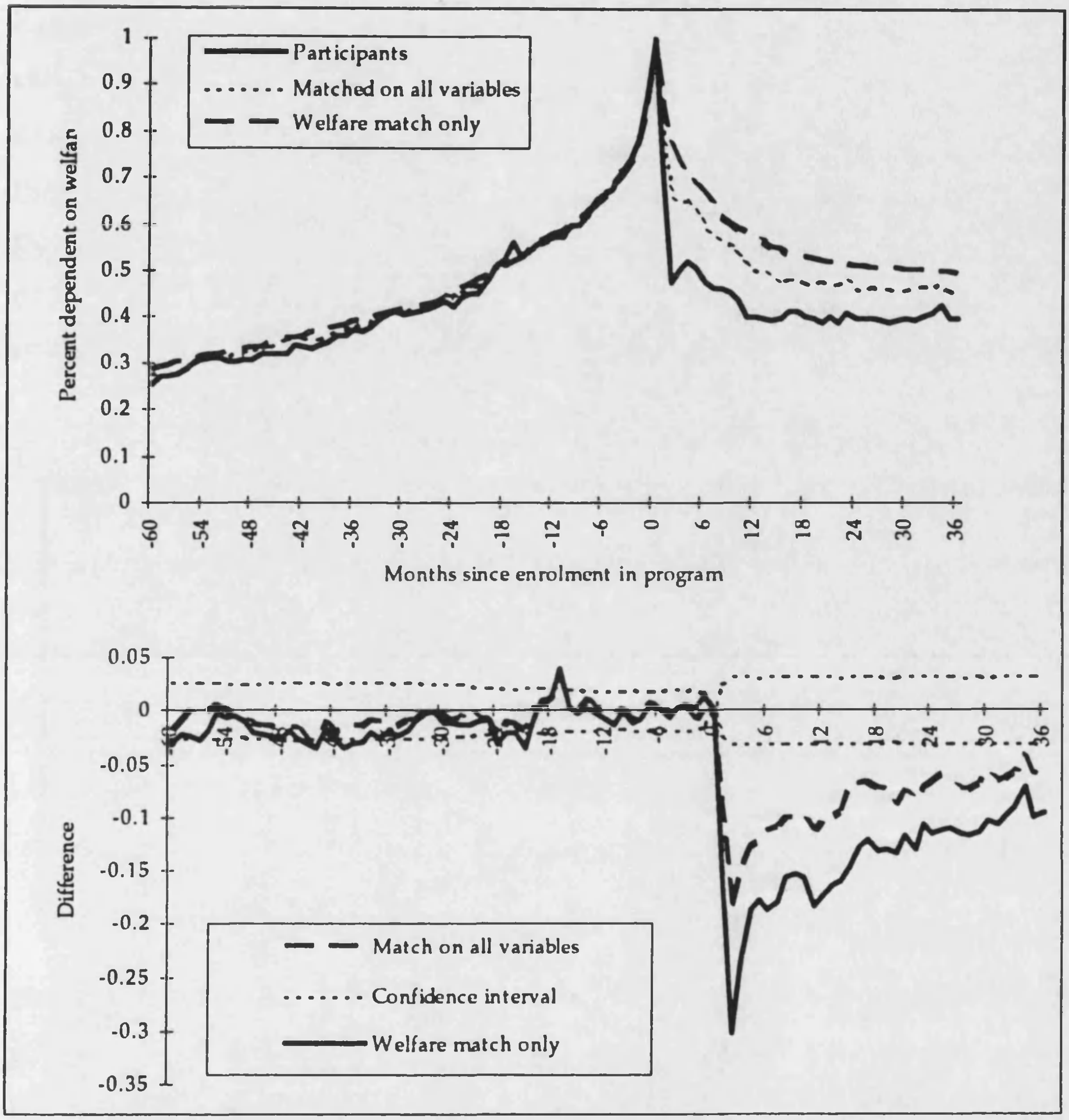

Figure 5-12. Pre- and post program welfare dependence of participants and two comparison groups (EOP) Data sources 1, 4; comparison groups A and B

Interestingly, the new comparison group, although quite different from the old comparison group in terms of earnings and UI use, is very similar in terms of welfare use before the program. A comparison of the welfare dependence of the participants and the comparison group, used as a pre-program test for selection bias, would give the estimate based on only the welfare match a clean bill of health. The post program experience of the two comparison groups paints a different picture. The estimate of the extent of selection bias, the difference in estimate of impact between the two comparison groups, spikes at twelve and nine percentage points in months two and three, before falling below six percentage points and averaging 5.2 percentage points throughout the remainder of the follow up period (Figure 5-13). Clearly, pre-program similarity is not a reliable indicator of an absence of selection bias.

Table 5-1 Estimates of impact using different base years and comparison groups

Base Year	Comparison group matched on all variables	Comparison group matched on welfare variables only
-5	0.6898	1.0446
-4	0.5544	0.9388
-3	0.6933	1.1153
-2	0.7697	1.2049
-1	0.7411	1.2457

Robustness of differences in differences estimators to choice of base year is a popular test for selection bias that is based on pre-program data[9]. The illustration of the general finding that tests based on pre-program data are not reliable using the specific test of robustness of differences in differences estimators to choice of base year is straightforward. Table 5-1 reports differences in differences estimates of the impact of EOP on welfare dependence in the third year after program enrolment. The columns correspond to the different comparison groups, the first matches participants on all variables, the second matches participants on welfare variables only. Although the two sets of

[9]See e.g. Ashenfelter and Card(1985), Bassi (1984) or Park et al. (1994).

estimates of impact of the same program differ substantially, each set is robust to choice of base year. Clearly robustness to choice of base year is not a reliable indicator of an absence of selection bias.

5.4 Is Selection Bias Due to Unmeasurable Variables?

Thus far the discussion of selection bias has focused on unmeasured but measurable variables: earnings, UI use, etc. However, the rhetoric used by proponents of random assignment tends to focus on unmeasurable variables. For example Bloom et al. (1993 page 8) cite motivation as an unmeasurable characteristic that is likely to cause selection bias. And Judith Gueron, then Executive Vice President of MDRC, in her April 11, 1986 letter to the Deputy Commissioner of the Massachusetts Department of Public Welfare advocating the use of random assignment to estimate the impact of the ET Choices program, said, "Because clients end up in activities as a result of choice (not a random process), it is clear that they would differ in very definite (if not always measurable) ways."

One reason that the proponents of random assignment may emphasise the unmeasurable variables is that if the differences between participants and non-participants that cause bias are not measurable, then our choice of techniques becomes limited. We must either model the selection process separately or use random assignment. But, if as Greenberg and Wiseman suggest (1992, p. 136), the appeal of random assignment to policy makers is that it reduces controversy, then modelling the selection process may not be a viable alternative. Uncertainty will remain, with the debate turning on whether the omitted relevant variables can legitimately be omitted, or, if identification depends on assumptions regarding the distribution of error terms, whether these assumptions are valid.

Conversely, if the impact of unmeasured variables is small, then, as long as all researchers include all measured variables then differences would not be due to selection bias. We could not expect that controversy would be eliminated[10], but only that it would not be related to selection bias, the source of controversy that random assignment eliminates. If the bias due to unmeasurables is small, there will be little benefit from random assignment.

This section produces an indication of the upper bound of the bias resulting from unmeasurable variables by redoing one of the post program tests. Recall from Test 2, the group of interviewed individuals who were classified as job-ready, but who did not participate in training. There, the interviewers attempted to identify job-ready individuals, that is they attempted to select individuals who had characteristics (whether measured or unmeasured) that were correlated with the probability of becoming independent of welfare. For this reason the bias from each omitted variable should be negative[11], and none will offset any of the others. Any residual bias that we find will be the sum of bias due to measurable but omitted variables and unmeasurable variables. That is, it will put an upper limit on selection bias due to unmeasurables.

Test 2 showed that the interviewed group did indeed differ from the (welfare-variables[12]-matched) comparison group in unmeasured variables that were

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[10] We could turn our efforts to entry effects or impacts on individuals who are outside the experimental framework. See e.g. discussions by Moffitt and by Garfinkel et. al. in Manski and Garfinkel (1992)

[11] In practice the sample had been selected from those who remained dependent on welfare for the two to three months between the mailing of the letter and the interview. Those who received the letter but became independent before the interview were less likely to show up and be classified. Omitting this variable introduces an offsetting bias. This is described in more detail below.

[12] As described in Chapter 4 Section 4.

correlated both with selection and with subsequent welfare dependence. These correlations resulted in selection bias of about five percentage points. But many of the unmeasured variables are measurable (i.e. employment, earnings, UI dependence), and are available for the one-in-ten sample[13] and so we can re-do Test 2 using the additional information.

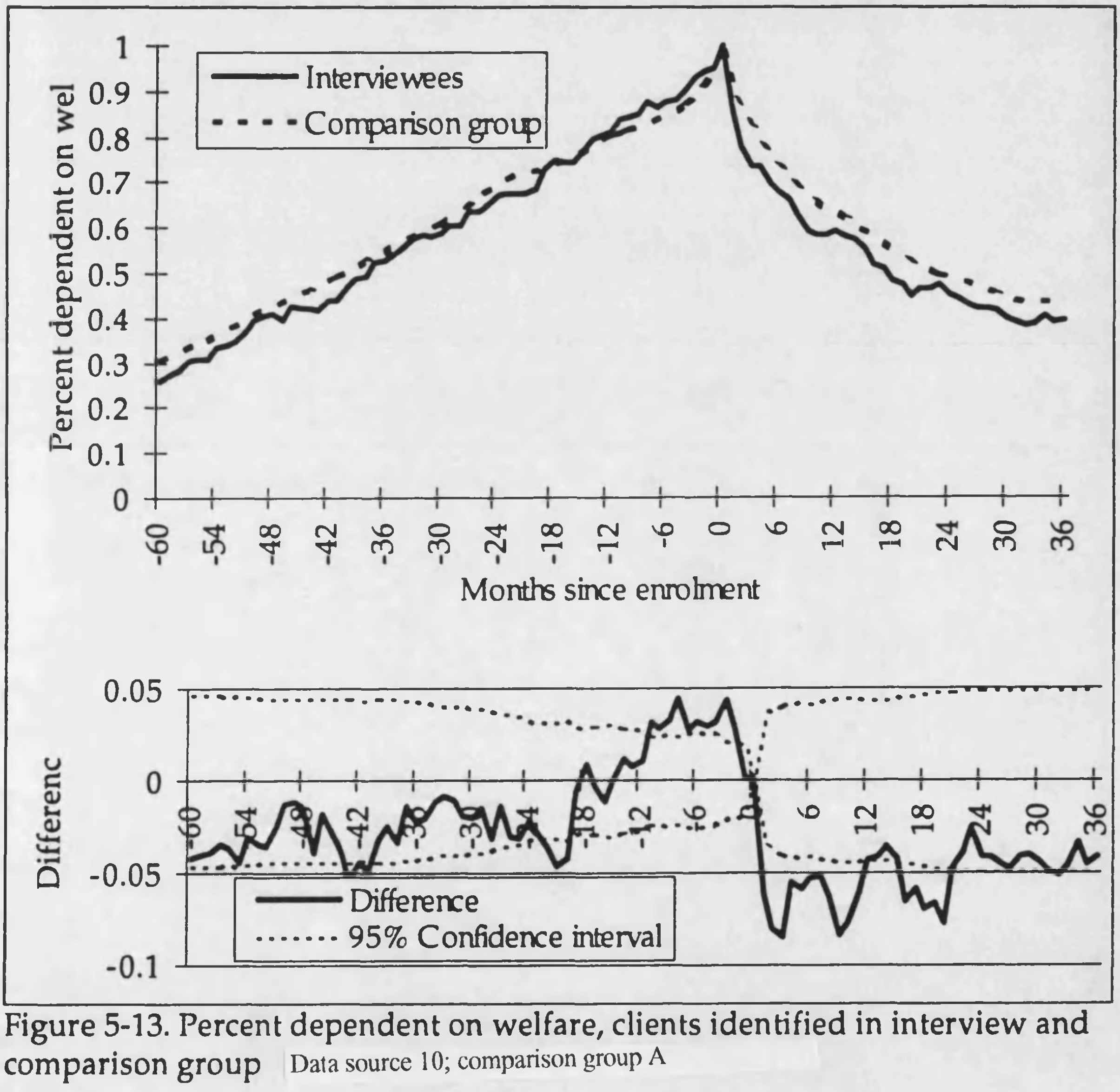

Figure 5-13. Percent dependent on welfare, clients identified in interview and comparison group Data source 10; comparison group A

[13]There are 386 individuals in this one-in-ten sample. Nine were excluded because they received benefits through an ancillary program.

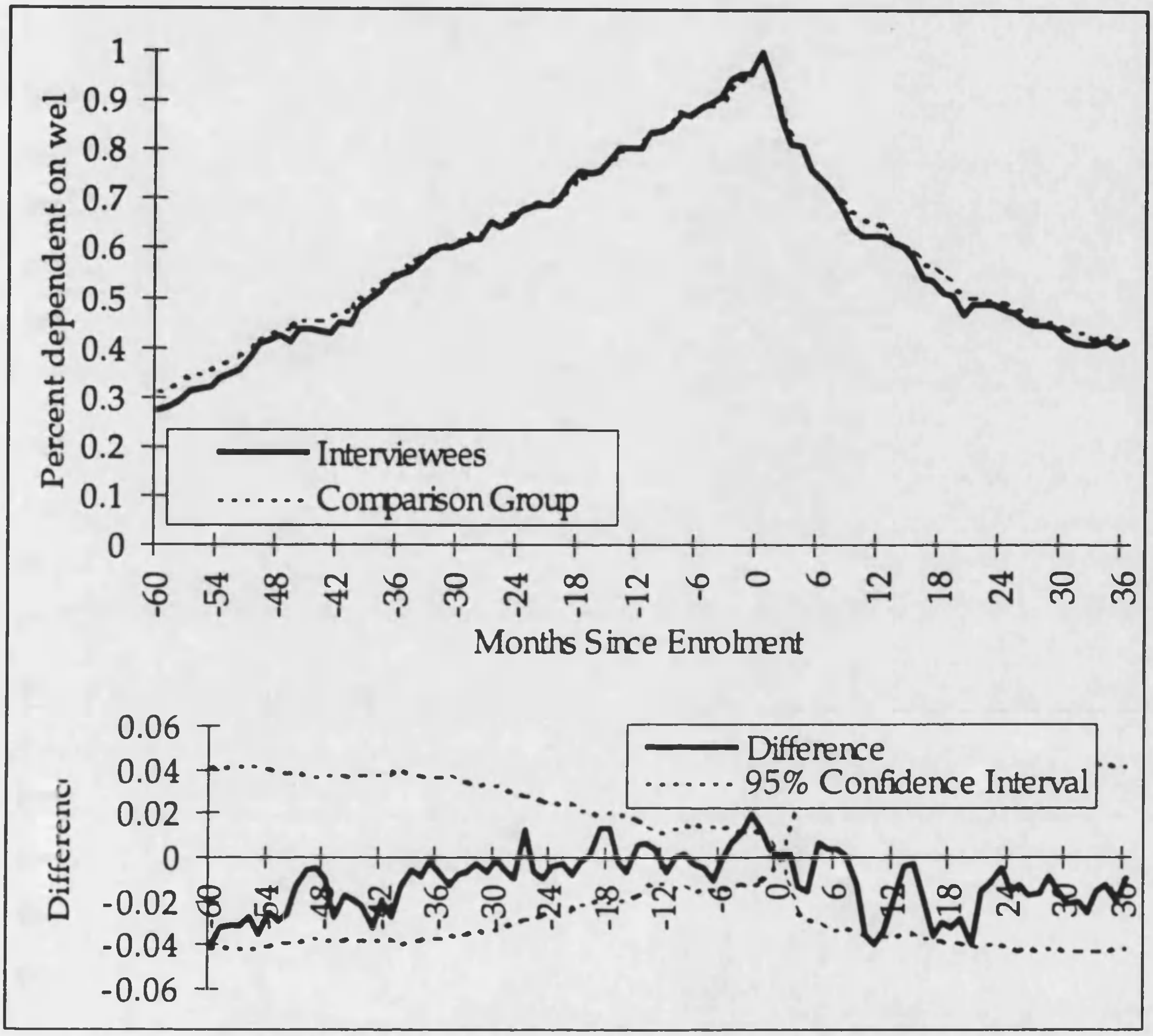

Figure 5-14 Percent dependent on welfare, clients identified in interview and comparison group matched on all variables Data source 10; comparison group B

The right-hand side of Figure 5-14 reproduces Figure 5-2. Although the smaller sample naturally exhibits greater variability, the average difference between those who had been interviewed and determined to be "job ready" and the comparison group is comparable for the two groups. In the first 19 months, the period for which observations on the full sample were available in test 2, the differences are 5.7 percentage points for the full sample and 6.1 percentage points for the one-in-ten sample.

Looking at the last 12 months, in the one-in-ten sample with the comparison group matched on welfare variables only, the difference, our estimate of the amount of selection bias, is 4.3 (t=2.1) percentage points. As we anticipated, including formerly omitted variables decreased the level of selection bias. When we match on all available variables (Figure 5-14) the estimate of residual bias falls to 2.3 (t=1.4).

Figure 5-14 suggests that further analysis would be profitable. The interviewed and comparison groups are statistically significantly different in welfare dependence in the first few pre-interview months[14], and the potential exists to control for five years of welfare dependence and earnings. Unfortunately it is expensive and time consuming to draw matched comparison groups. In addition, as the matching criteria become finer and finer, the probability of having no matches for some participants increases. Regression analysis overcomes both of these problems, so further analysis was done by estimating the parameters of an equation using the interviewed individuals and the matched (and weighted) comparison group.

5.4.1 Digression on Regression

The dependent variable in these regressions is the sum of benefits received 13 to 24 months following selection and so takes 13 discrete values from 0 to 12. Tobit analysis is appropriate since the distribution is censored at both ends, but coefficients from tobit regressions are not as easy to interpret. For this reason the coefficients from the OLS regression are reported below. In any event, the significance and sign for all the coefficients were very similar for the tobit and OLS regressions. For single parents the coefficient on welfare history between

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[14] See footnote 11.

months 4 and 12 between months 1 and 12 squared and on UI history between months 1 and 12 became significant, and the dummies on very recent work experience fell to insignificance in the tobit equation. All other signs/significance remained unchanged. For single men the changes were even smaller. The t for the coefficient on the dummy for having received no months and three months of welfare in the previous three months fell to .125 and 1.7 respectively and the t for DUI3[15] changed from .5 to -.6. All other signs/significance remained unchanged.

Table 5-2 Coefficients on models of welfare dependence for interviewed group and comparison group.	Combined	Single Men	Single Parents
Number of participants	340[16]	114	77
N	2391	861	961
Variable	Coefficient (t-statistic)		
1. A dummy variable that takes the value 1 if the observation is for a selected individual.	**-0.25742 (-1.343)**	**-.31116 (-1.047)**	**-.70134 (-2.503)**
2. Constant	4.214 (3.144)	6.846 (3.466)	5.463 (2.049)
3. A dummy variable that takes the value 1 if the individual is a single female	-0.12958 (-0.400)		
4. A dummy variable that takes the value 1 if the individual is in a case with two adults and no children	0.77876 (1.524)		
5. A dummy variable that takes the value 1 if the individual is in a case with two adults and children	-0.20529 (-0.724)		
6. A dummy variable that takes the value 1 if the individual is a single	-0.02274 (-0.086)		

[15] variable 21 defined in Table 5-2 below.
[16] 46 interviewed individuals were dropped from the sample because they did not receive welfare benefits in the month in which they were interviewed.

parent			
7. A dummy variable that takes the value 1 if the individual is classified as employable.	-0.48623 (-1.601)	-.81636 (-1.495)	-.35308 (-.976)
8. The number of months in which welfare benefits were paid during the period 13 to 25 months before selection	0.19269 (5.021)	.19978 (3.376)	.30482 (4.412)
9. The number of months in which welfare benefits were paid during the period 4 to 12 months before selection	-0.66562 (-2.841)	-.76299 (-2.208)	.3588 (2.688)
10. The number of months in which welfare benefits were paid during the period 1 to 3 months before selection	-0.99942 (-1.454)	-2.8837 (-2.9)	.01519 (.01)
11. The number of months in which welfare benefits were paid during the period 13 to 24 months before selection	4.58E-03 (0.137)	.01807 (0.332)	-.32601 (-5.989)
12. The number of months in which welfare benefits were paid during the period 38 to 49 months before selection	6.56E-03 (0.232)	.01314 (0.274)	.05242 (1.246)
13. The square of the number of months in which welfare benefits were paid during the period 1 to 12 months before selection	4.94E-02 (3.344)	.06658 (3.07)	-.0153 (1-.474)
14. A dummy variable that takes the value 1 if the individual received no benefits in the three months prior to the interview/selection date.	-0.35438 (-0.161)	-5.2606 (-1.833)	.84158 (.376)
15. A dummy variable that takes the value 1 if the individual received benefits in all three months prior to the interview/selection date.	2.6214 (2.502)	3.3245 (2.171)	
16. Age (in months)	2.80E-03 (3.201)	.00461 (3.419)	.00131 (.875)
17. The number of months in which UI benefits were paid during the period 1 to 12 months before selection	0.21546 (2.739)	.20756 (1.423)	-.09976 (-.544)
18. The number of months in which UI benefits were paid during the period 13 to 24 months before selection	-0.14438 (-2.999)	-.04392 (-0.533)	-.11689 (-1.441)

19. A dummy variable indicating employment in the three months prior to selection according to a record of employment. It takes the value 1 if reported earnings would generate UI entitlement greater than welfare entitlement, but fewer than 10 insurable weeks (the minimum number needed for UI eligibility) were reported.	-0.66844 (-1.363)	-2.0348 (-3.152)	
20. As above, except that earnings are between the level of welfare entitlement and the level of earnings that would generate UI entitlement greater than welfare entitlement.	-3.1453 (-4.829)	-2.4621 (-2.552)	-4.258 (-4.386)
21. As above, except that 10 or more insurable weeks are reported.(**DUI3**)	-3.8378 (-5.678)	-5.0846 (-4.903)	-3.4419 (-2.354)
22. A dummy variable that takes the value 1 if UI benefits greater than welfare entitlement were paid in the month before selection.	-0.67202 (-1.04)	.7323 (0.517)	-3.39 (-3.326)
23. Sum of earnings (as reported on ROE) in 12 months preceding selection.	7.57E-04 (2.86)	.0021 (4.942)	.00121 (2.56)
24. Sum of earnings (as reported on ROE) in 12 months ending 13 months preceding selection.	-1.57E-03 (-5.464)	-.0015 (-3.367)	-.00194 (-4.27)
25. Sum of earnings (as reported on ROE) in 12 months ending 25 months preceding selection.	1.16E-03 (4.415)	.00139 (3.333)	-.00046 (-1.005)
26. Sum of earnings (as reported on ROE) in 12 months ending 37 months preceding selection.	-6.20E-04 (-2.724)	-.00118 (-3.498)	-.00146 (-3.426)
27. Sum of earnings (as reported on ROE) in 12 months ending 49 months preceding selection.	-4.01E-04 (-2.335)	-.00057 (-2.321)	.00105 (2.57)

In the combined regression, the estimate of selection bias is .26 months of benefits over 12 months, or 2.2 percentage points (compared with 2.3 percentage points for the matched result). This suggests that little was gained through the use of the additional information.

The results for single men[17] lead us to a substantially different conclusion. The dis-aggregated result from the matched comparison group is +2.4 percentage points for single men, compared with -2.6 percentage points from the regression analysis. One explanation for the change is the new information available to the regression analysis. As seen above, (Figure 5-14) the selected group was significantly different from the comparison group in recent welfare dependence. The selected group was more dependent in the three months immediately preceding the interview. Three variables (Variables numbered 10, 14, 15) are included in the regression equation to capture the recent dependence and, for single men, all three are statistically significant.

The results are that the selection bias due to omitted variables, measurable and unmeasurable is 5.8 percentage points for single parents, 2.6 percentage points for single men, and 0 ("wrong" sign and t=.3) for the remainder. Many important measurable variables such as education, labour force participation and age of youngest child have not been included. Because two of these are particularly important for predicting the subsequent dependence of single parents, the results for single men and "others" are probably more relevant for assessing the importance of unmeasurables. They both indicate that the bias due to unmeasurables such as motivation or intelligence is small[18], that is, that very little is gained by the use of random assignment.

[17] The results for the "all others" group, not reported in Table 5-2, also changed, but from -.46 to +.14 (t=.3).

[18] This is not to say that motivation and intelligence are unimportant, but rather that their influence on subsequent welfare dependence is adequately reflected in their welfare dependence, employment and earnings history.

A subsidiary finding from this analysis relates to the number of variables that belong in the equation that describe the conditions immediately prior to selection. For single men the welfare dependence in the three months prior to selection[19] were important in predicting welfare dependence between one and two years later. Earnings, Unemployment Insurance benefits and employment in the three months prior to selection were also important predictors of subsequent welfare dependence. But none of these variables could be measured using annual data, and so we could expect omitted variable bias (selection bias) to be much more severe with annual data. This suggests an explanation for the problems with the CETA evaluations described in Chapter 2.

5.5 What Went Wrong in the CETA Evaluations?

The results presented in this chapter are at odds with the literature relating to the CETA evaluations. Our results suggest that the differences due to the treatment of unmeasurables should only have a moderate effect on estimates. Certainly there is nothing to suggest that the treatment of unmeasurables would change, qualitatively, estimates of impact of programs such as these, and yet that did happen in the CETA studies. The key to resolving the difference between those findings and these lies with the implications of trying to control for the "pre-program dip" in earnings using annual data.

5.5.1 Why care about Pre-Program Dip?

It has long been recognised that participants in employment and training programs experience a dip in their earnings before enrolment.[20] This has

[19] For single parents this variable was 2.96 (maximum value 3), and so had little variation.
[20] See, e.g. Ashenfelter (1978)

profound implications for estimating the impact of a training program without using random assignment. When we estimate the impact of a program on earnings using a comparison group design, we want the comparison group to be as similar to the participants in expected earnings as possible. We expect that people who suffer a dip in earnings will have lower earnings after the dip than people who do not suffer a dip in earnings. So researchers must select comparison group members who have earnings dips similar to those experienced by the program participants. For example, Westat, the main contractor the US Department of Labor for estimating the impact of the CETA programs[21] reports that three of its four highest priority variables for matching relate to pre-program earnings or changes in pre-program earnings.

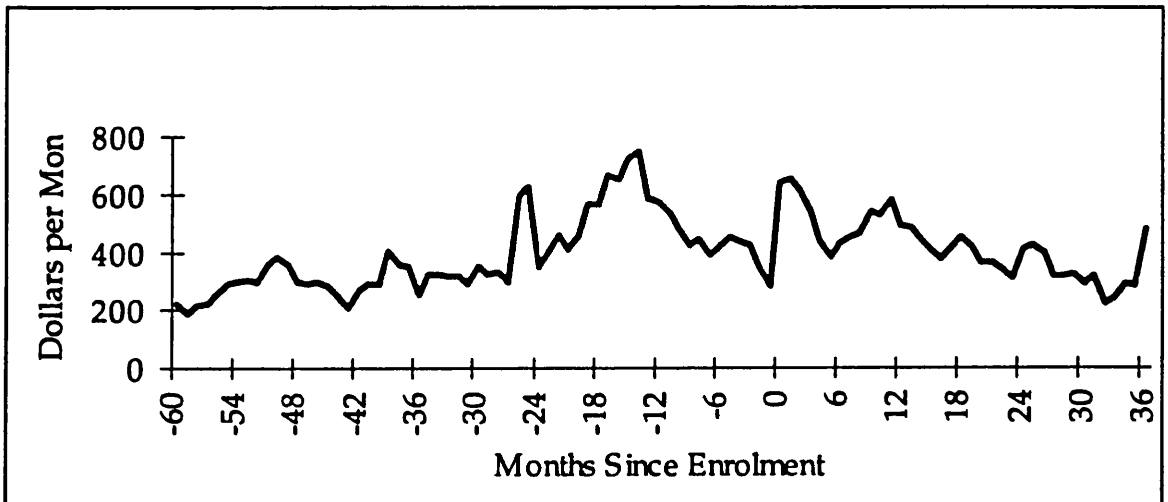

Figure 5-15. Average earnings of high-earner EOP participants
Data sources 4, 6

Participants in CETA had much higher incomes than participants in EOP.[22] To make the studies more comparable, this analysis is restricted to the 82 EOP

[21]see e.g. Bryant and Rupp, 1987.

[22]Recall that EOP participants had been welfare recipients and as such:

a) had earnings that were less than their welfare entitlement; and

b) either were not qualified for UI because they had exhausted their benefits or did not have sufficient employment experience to qualify, or did qualify for UI but had benefits lower than their welfare entitlement.

participants with the highest annual pre-program earnings. This group had average earnings of $7,500 in the immediate pre-program year (late '80's), which is roughly comparable to the pre-program earnings of CETA participants (early '70's) in real dollars.

Figure 5-15 shows the average earnings of the high-earner EOP participants 60 months leading up to participation and 36 months after. The pre-program dip in earnings is a prominent feature of these data. From peak to trough, the dip in earnings is 62%, much larger than the dip experienced by CETA participants as reported by Bloom (1987) or Bryant and Rupp (1987). Figure 5-16 reports the same information, except that earnings are expressed as annual averages. The dip now appears to be 32%, which is between the dips reported by Bloom (1987) and Bryant and Rupp (1987). Clearly, many important features of the data, including the pre-program dip are obscured simply by using annual averages.

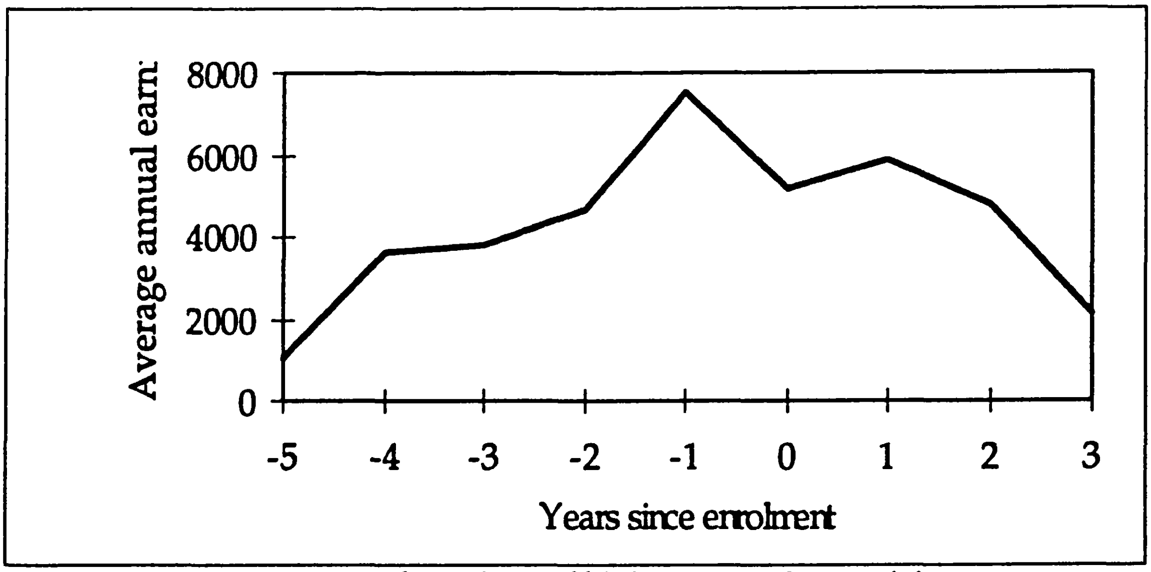

Figure 5-16. Average annual earnings of high-earner EOP participants
Data sources 4, 6

5.5.2 Which year contains the dip?

The problem of a dip obscured using annual data, is exacerbated by the problem of defining the year in which the dip is said to occur. Participants enter programs throughout the year, and so their earnings dips are also spread throughout the year. If a participant enrols late in a calendar year, then a considerable amount of the dip will occur in the year in which enrolment occurs. In that case it would be important to compare that participant's subsequent earnings with earnings of non participants who had similar earnings in the enrolment year. Conversely, if a participant enrols early in a calendar year, a considerable amount of post program earnings would be included in the enrolment year's earnings, and it would be important that enrolment year earnings not be used to select comparison group members. The seriousness of this problem depends to a large measure on the duration of the dip in earnings. If the dip spans several calendar years, then only a small amount of the dip will be missed using annual data.

5.5.3 Duration of the dip

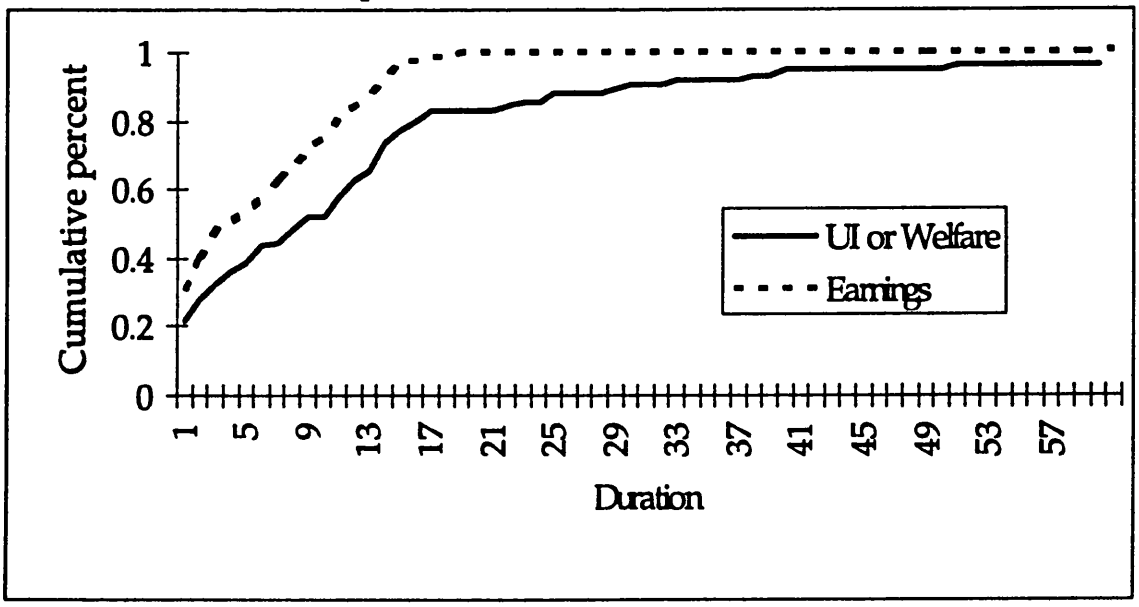

Figure 5-17. Duration of pre-program dip

Data sources 1, 4, 6, 7

We have found that the duration of the dip varies across participants but that many dips were short. Two definitions were used to describe the duration of the dip. In the first instance a participant was defined as experiencing a pre-program dip in earnings if he/she were receiving benefits through either Unemployment Insurance or welfare. In the second, a participant was defined as experiencing a pre-program dip in earnings if his/her earnings were less than 25% below the average earnings in the pre-program year. Figure 5-17 shows the cumulative distribution of the duration of pre-program dips. Using the UI/welfare definition, more than 40 percent of the dips are of six months duration or less.

5.5.4 Implications for CETA

This has profound implications for the estimation of the impact of CETA programs. If data are annual, like the CETA data are, and enrolment in the program and the distribution of the duration of pre-program dip is evenly distributed across time, then for 38% of participants the pre-program dips will occur entirely within the year in which program participation began.[23] With the dips being so short, researchers find themselves on the horns of a dilemma. If, like Westat, they define pre-program earnings as earnings in 1976 for participants who enrolled in the program between July 1, 1976 and June 30, 1977, they will pick up most of the dip, but they will be matching on some within and post program earnings. If, like Dickinson, Johnson and West, they define pre-program earnings as earnings in 1975 for participants who enrolled in the program in the calendar year 1976, they will match only on pre-program

[23]This is calculated as follows. People who enrol in the program in December (1/12 of the total) will have their pre-program dip entirely within the year if their dip is of duration 11 months or less (63% from Figure 4-19). For December the percentage is 1/12 * .63. For November, 1/12 * .58, etc.

earnings, but they will miss the pre-program dip in almost 40% of the cases. Given the shortness of the pre-program dip, we should not be surprised that the Dickinson, Johnson and West studies produced estimates that were markedly lower than the estimates based on comparison groups drawn by Westat.

The analysis of Dickinson, Johnson and West supports this interpretation of the difference. They generated separate estimates for participants who enrolled in the first and second half of the year. Because more participants in the first half of the year would have dips in earnings in the pre-program year, and as a result would be more likely to be compared with others who also experienced a dip in earnings, we would expect these estimates to be higher, and indeed they were. The Dickinson, Johnson and West estimates of the impact on men's earnings were: $-458 for the early enrolees, and $-971 for the late enrolees. For women, the estimate for early enrolees was $+246 and $-220 for the late enrolees. However, even when participation is restricted to the first half of the year, the pre-program dip will be missed in 17% of cases, so even these estimates will be lower than the true impact.

This is not to say that the Westat estimates were correct. If within-program earnings are zero, and people stay in the program more than six months, then pre-program earnings by their definition will understate true pre-program earnings, and for those who enrolled in 1975 their estimates will overstate the true estimates. On the other hand, if within program earnings are positive (as they are in wage subsidy programs with the private sector) or program participation is short, then the Westat estimates will understate true program impacts. The only conclusion that can be drawn is that purely annual data are not adequate for the purposes of estimating the impact of employment and training programs[24].

5.5.5 An Illustration

To illustrate the effect of using annual data, the impact of the program is estimated using two different methods. First, a comparison group is drawn that is comparable to the participants as described above. Figures 5-18, 5-19 and 5-20 show the earnings, UI dependence, and welfare dependence of program participants and a comparison group for five years before program participation and three years after. The comparison group tracks the participants well in the pre-program period and indicates that the program has a modest impact in the short run (four months) and no impact after that.

5.5.5.1 A new comparison group

We now convert our data from monthly to annual and draw a comparison group in a manner similar to that used by Westat and Dickinson, Johnson and West. The criteria used in this match are:

- The comparison group members were selected from those who had received welfare in at least one month in the calendar year in which the participant enrolled in the program.
- If the participant received UI or welfare in the calendar year before the year in which the participant enrolled in the program then members of the comparison group for that participant did too.

---

[24]Some data that are apparently annual, in fact contains intra-annual information that may make monthly data unnecessary. For example, a comparison group drawn from individuals who are receiving UI at the same time as and for as long as program participants, will be experiencing the same pre-program dip as participants since a) they must have been working before they began receiving UI and b) they must not be working while they are receiving UI.

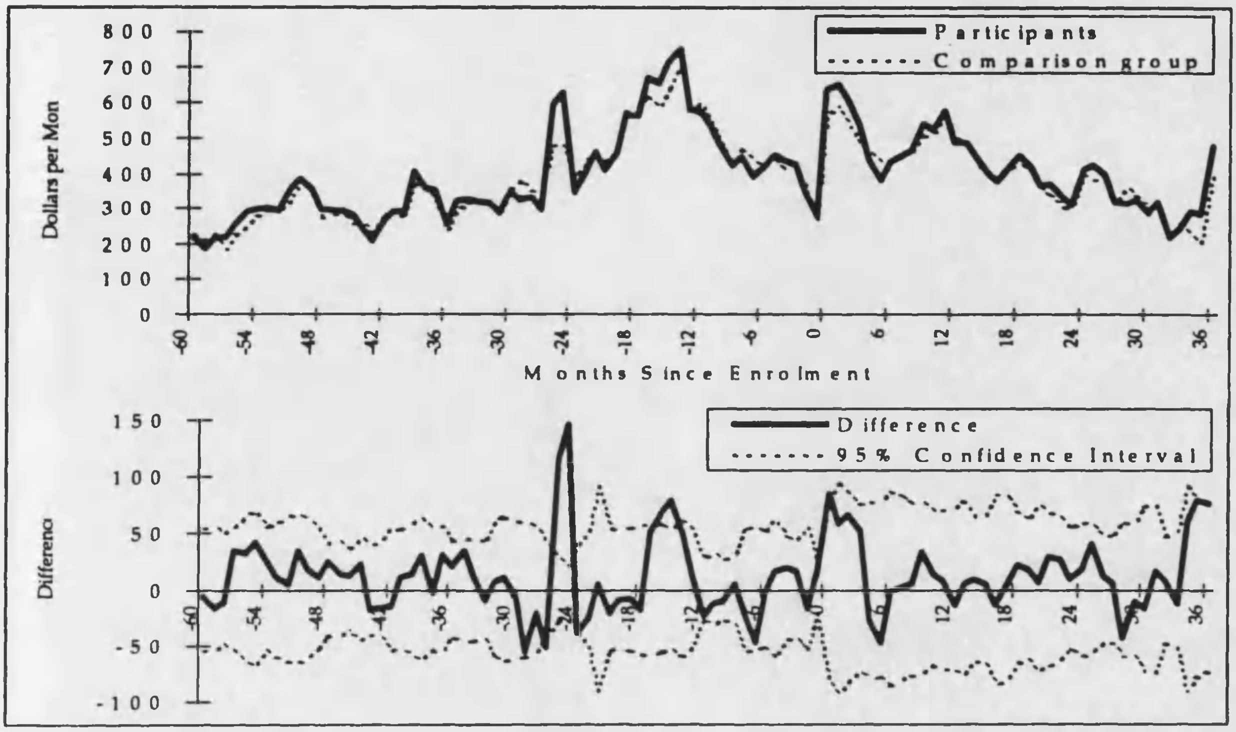

Figure 5-18 Earnings of high-income participants and comparison group
Data sources 4, 6; new comparison group

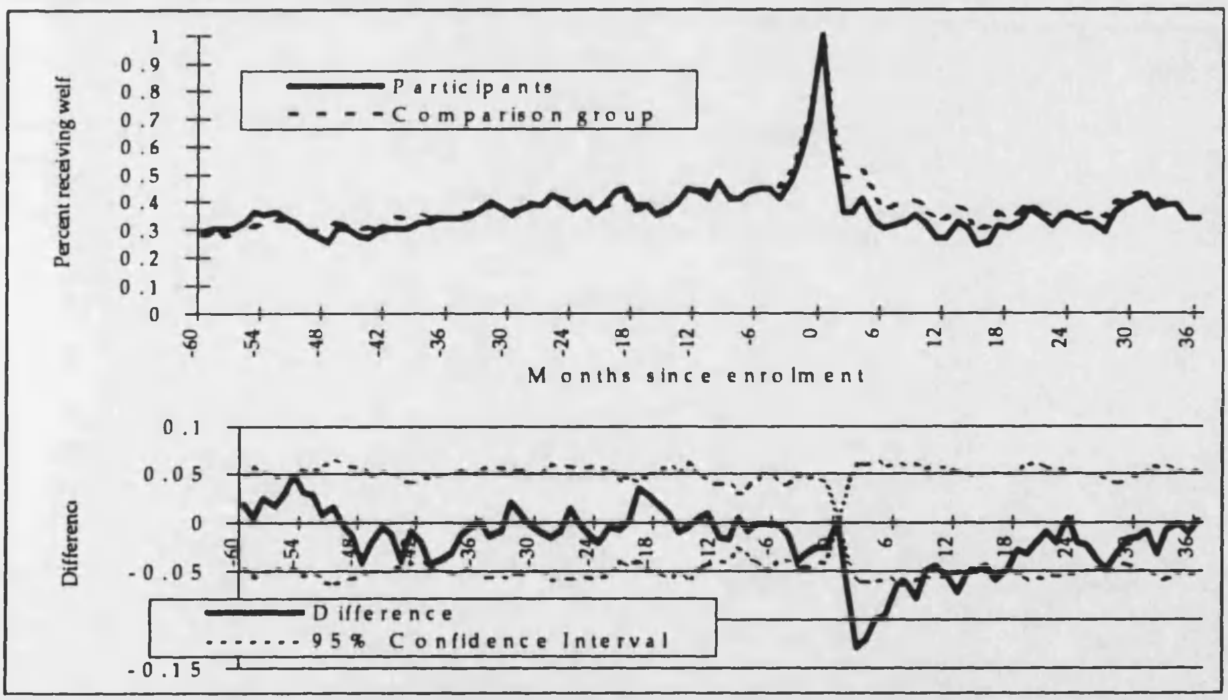

Figure 5-19 Welfare dependence of high-income participants and comparison group Data sources 1, 4; new comparison group

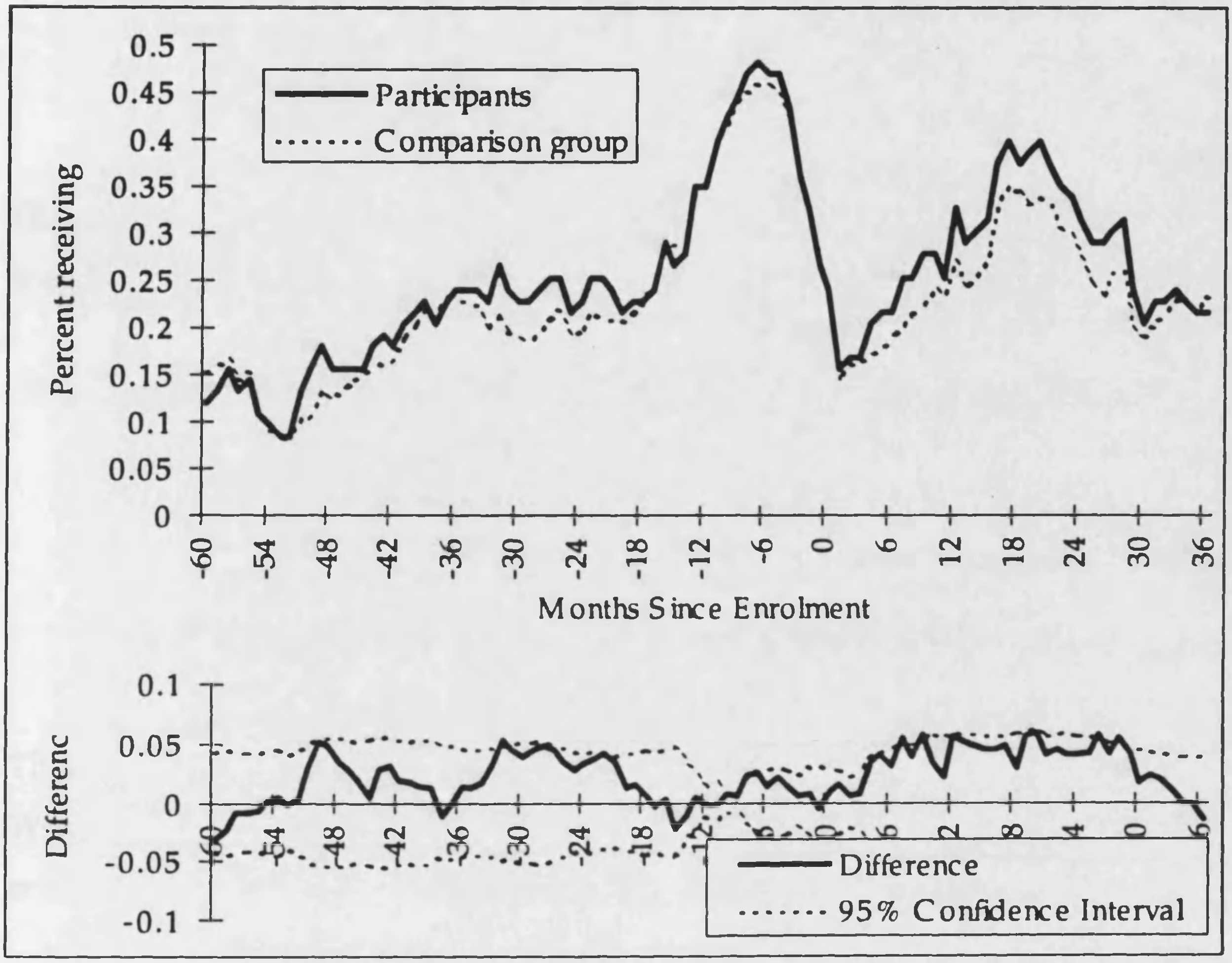

Figure 5-20. UI dependence of high-income participants and comparison group
Data sources 4, 7; new comparison group

- The comparison group members had to match participants on category (single men, single women, couples, two parent family or one parent family) and age using the five age categories described above (page 87).
- The comparison group members had to match participants' earnings as follows:
  - if participants' earnings were zero then the comparison group members had to have zero earnings as well, otherwise
  - if the participants' earnings were less than $2,000 per year, then the comparison group members had to have earnings less than $2,000 per year. Otherwise

- comparison group members had to have earnings between 80% and 125% of the earnings of the participants.

The comparison group members had to match on earnings in this way in each of the two calendar years before the enrolment date.

- The changes in earnings between one and two years before enrolment had to be negative for both participants and comparison group members, or, if positive, both be less than $2000, or if above $2000 the change for the comparison group member had to be between 80% and 125% of the change in earnings of the participant.

This match is inferior to the matches of Westat and of Dickinson, Johnson and West. For example, it does not contain education variables among the matching criteria.

5.5.6 Results

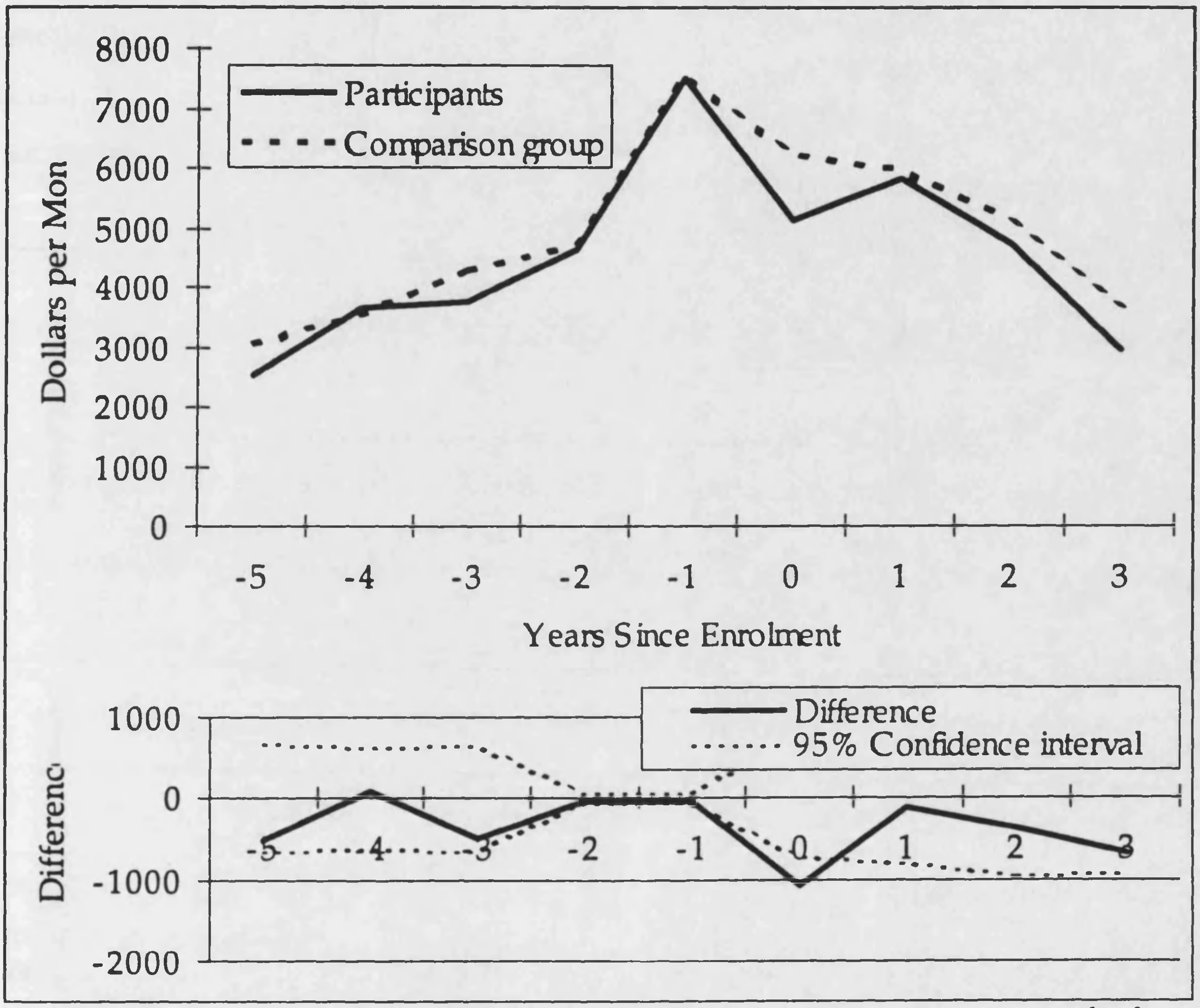

Figure 5-21. Annual earnings, EOP participants and comparison group matched using annual data (in manner of Westat or Dickinson Johnson and West).

Data sources 4, 6; new comparison group

The effect of moving to annual data and not controlling for the UI and welfare dependence is dramatic. The comparison group selected on the basis of annual data gives the result (Figures 5-21, 5-22, 5-23) that the program reduces earnings by over $1,000 in the year of enrolment (t=2.9) and that the deleterious effects persist. Even in year three, earnings are apparently reduced by almost $700 (t=1.4).

Similarly, the analysis based on annual data indicates that welfare dependence is increased by statistically significant amounts in each year. Unemployment Insurance dependence is also apparently increased by statistically significant amounts.

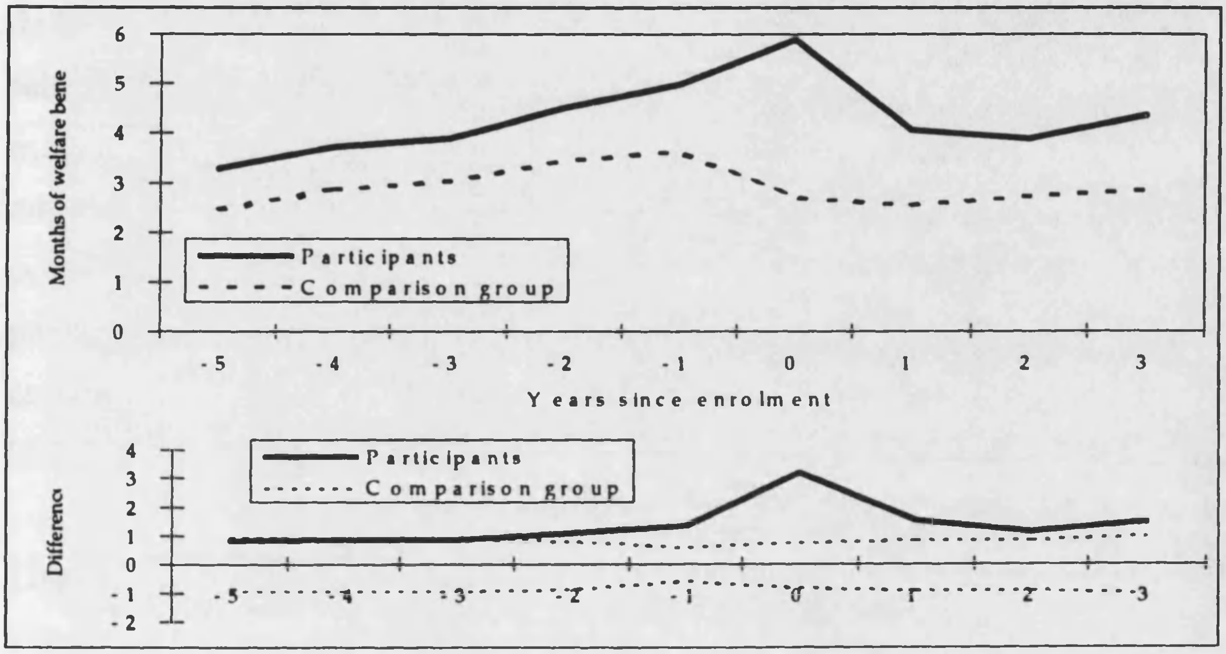

Figure 5-22. Welfare dependence, EOP participants and comparison group matched using annual data (in manner of Westat or Dickinson Johnson and West) Data sources 1, 4; new comparison group

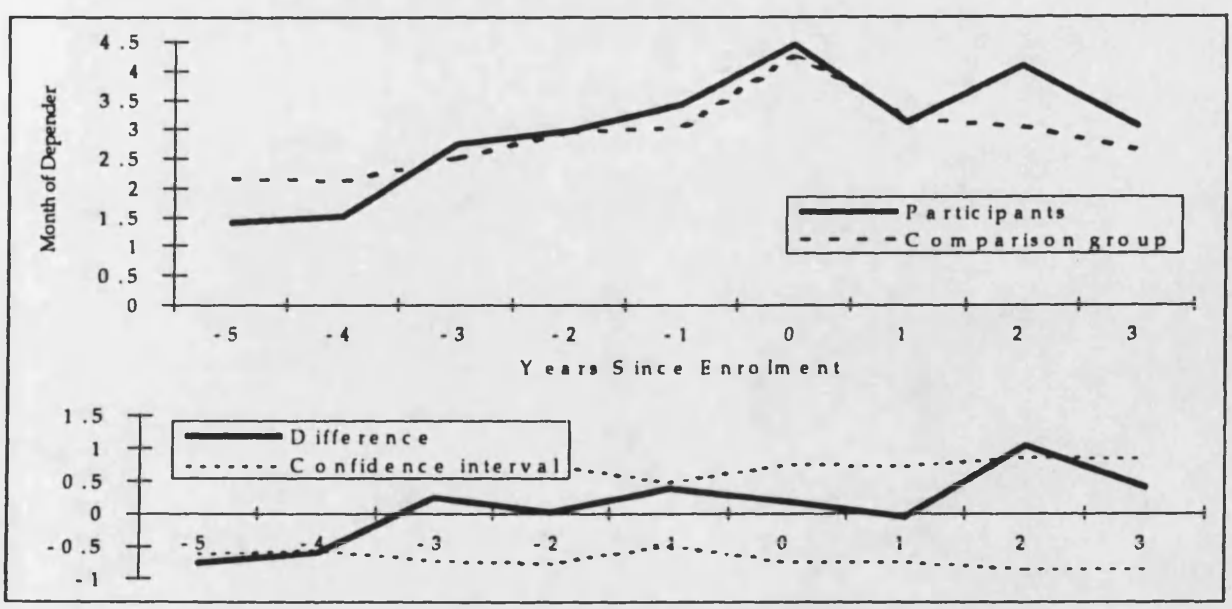

Figure 5-23. UI dependence, EOP participants and comparison group matched using annual data (in manner of Westat or Dickinson Johnson and West)
Data sources 4, 7; new comparison group

5.6 Conclusions

This chapter examined the issue of selection bias and found that with monthly data on welfare dependence together with a few demographic variables selection bias in long-term estimates of welfare dependence was of the order of magnitude of five percentage points. When data on UI dependence, employment and earnings are added selection bias fell to about two percentage points. The estimates of bias varied across programs, characteristics of participants and time since the program. Nonetheless, the analysis leads to the conclusion that selection bias is not enough to overturn the conclusions reported in the next two chapters.

This chapter also provided insight into the nature of selection bias by providing answers to four questions.

- Can we test for selection bias without random assignment?
- Are pre-program tests for selection bias reliable? and as a subsidiary question,
  - Does robustness to choice of base year mean that differences in differences estimators are unbiased?
- Are unmeasurable variables the primary source of selection bias?
- Why did different researchers come up with such different estimates of the impacts of the CETA programs?

The findings were:

1) It is possible to test observational studies for bias resulting from specific selection mechanisms. Five such tests were presented.

2) Pre-program tests for selection bias are not reliable. Further, robustness to choice of base year does not mean that differences in differences estimators are unbiased.

3) Unmeasurables are not the primary source of selection bias.

4) The large differences in the estimates of the CETA programs could have resulted from the inadequacy of annual data to detect and control for the pre-program dip in earnings.

The answers to these questions, taken as a whole suggest that the apparent wholesale condemnation of the use of observational studies to estimate the impacts of employment and training programs is unwarranted. Chapter 2 showed that the condemnation was based two factors. First, observational techniques applied to the same sources for comparison groups as were used to estimate the impacts of the CETA programs could not replicate the results of the Supported Work demonstration. Second, different techniques applied to the same data generated qualitatively different results. This chapter shows that annual data (like that used in the studies cited in Chapter 2 that discredited observational studies) are too coarse to permit reliable estimates of program impact, and that both of these results might be expected when annual data is used. Further, it shows that the residual bias due to unmeasurables is small and therefore, if we can avoid other serious problems such as non-response bias or improperly specified functional form, observational studies in general and the results in the chapters that follow in particular, can provide a useful guide to the effectiveness of programs.

6. Impact of On-the-Job Training with Wage Subsidies

This chapter presents the estimates of the impact of a wage subsidy program on the subsequent welfare dependence, UI dependence and employment of participants. The estimates have been produced using administrative data and survey data, and by the use of cell matching as well as regression analysis.

Chapter 5 found that selection bias in estimates of the impact of programs in BC on subsequent welfare dependence amounted to roughly five percentage points when estimates of impact were made by comparing subsequent welfare dependence of participants with non participants who were similar in age, sex, marital status, history of dependence classification and who were receiving welfare at the same time as the participants. The analysis was done explicitly so that the reader could assess the results presented in this and the following chapters. Where the impacts are less than five percentage points (e.g. the long term impacts of CTETP) they should be viewed with suspicion. However, where they are greater than five percentage points, (e.g. for EOP and vocational training) we can be fairly confident that there is a real long term impact.

Recall from Chapter 1 that British Columbia's Employment Opportunity Program (EOP) subsidised the wages of persons who were receiving welfare benefits and who were hired by employers on certain conditions. The conditions were that the job offered must be full-time, be of two to six months duration, and not result in the dismissal of any existing employee; moreover, the employer had to agree to provide on-the-job training. The subsidy was for half the wage up to a maximum of $3.50 an hour. Eligible welfare recipients included both welfare applicants and any dependants.

The Employment Opportunity Program had the largest budget of British Columbia's eleven programs aimed at helping recipients become independent of welfare. With a total expenditure in 1991/92 of nearly $12 million, the program accounted for 30 percent of total expenditures in the eleven programs of nearly $40 million. That year, about 6000 people participated in the program, for an average cost per participant of more than $1900.

Despite the large size of the program, its participants represented only two percent of the 270,000 applicants who received welfare in all or part of 1991/92. Similarly, its cost of $12 million was only one percent of the $1.2 billion in welfare distributed to employable persons and their dependants in 1991/92.

6.1.1 Conclusions

British Columbia's Employment Opportunity Program successfully helped recipients become independent of welfare. Additional survey results reinforce this inference, indicating that the program clearly helped recipients find employment in the long term. Program impact varied significantly with the category, welfare history, and age of the participants.

The program increased Unemployment Insurance eligibility in the short term and appears also to have encouraged greater UI dependence, especially during the 12-month eligibility period following the end of the program, and to a lesser extent in the long term as well.

Most employers clearly benefit from the program, though a few do not. The program's impact was only slightly affected by the unemployment rate.

For the provincial government, the reduction in welfare caused by the program was greater than the program expenditure. However, the program resulted in increased Unemployment Insurance payments by the federal government. Nevertheless, over 65 months the cumulative savings in welfare expenditure exceeded the combined cumulative expenditures on the program and on Unemployment Insurance by a significant amount.

6.2 Impact On Participants

6.2.1 Reducing welfare

6.2.1.1 Conclusion:
Participants moved off welfare more quickly than non-participants.

6.2.1.2 Discussion:
The program's effect on welfare dependence as shown by a cell-matched comparison group was dramatic (Figure 6-1). Three months after beginning the program, half the participants no longer received welfare, whereas only one-fifth of the comparison group had moved off it. Interestingly, at this point, though all participants still had work through the program except those who had dropped out, half the participants were still on welfare. Program employment thus did not put an end to welfare dependence for those whose income was low or whose need was high.

By month seven, when all program participation had ended and some participants had returned to welfare, the participant group had a lower dependence on welfare than the non-participant group of 10 to 15 percentage points, and this relative advantage was maintained more or less steadily up to and beyond two years as both groups gradually declined in welfare dependence.

This result is based on a sample that varied in size from 8,940 in month zero to 2,097 in month 40 and 82 in month 48. The number of individuals in the comparison group also varied, but always exceeded the number of participants. The confidence interval for the difference between the welfare dependence of the participants and the comparison group is reported at the bottom of Figure 6-1.

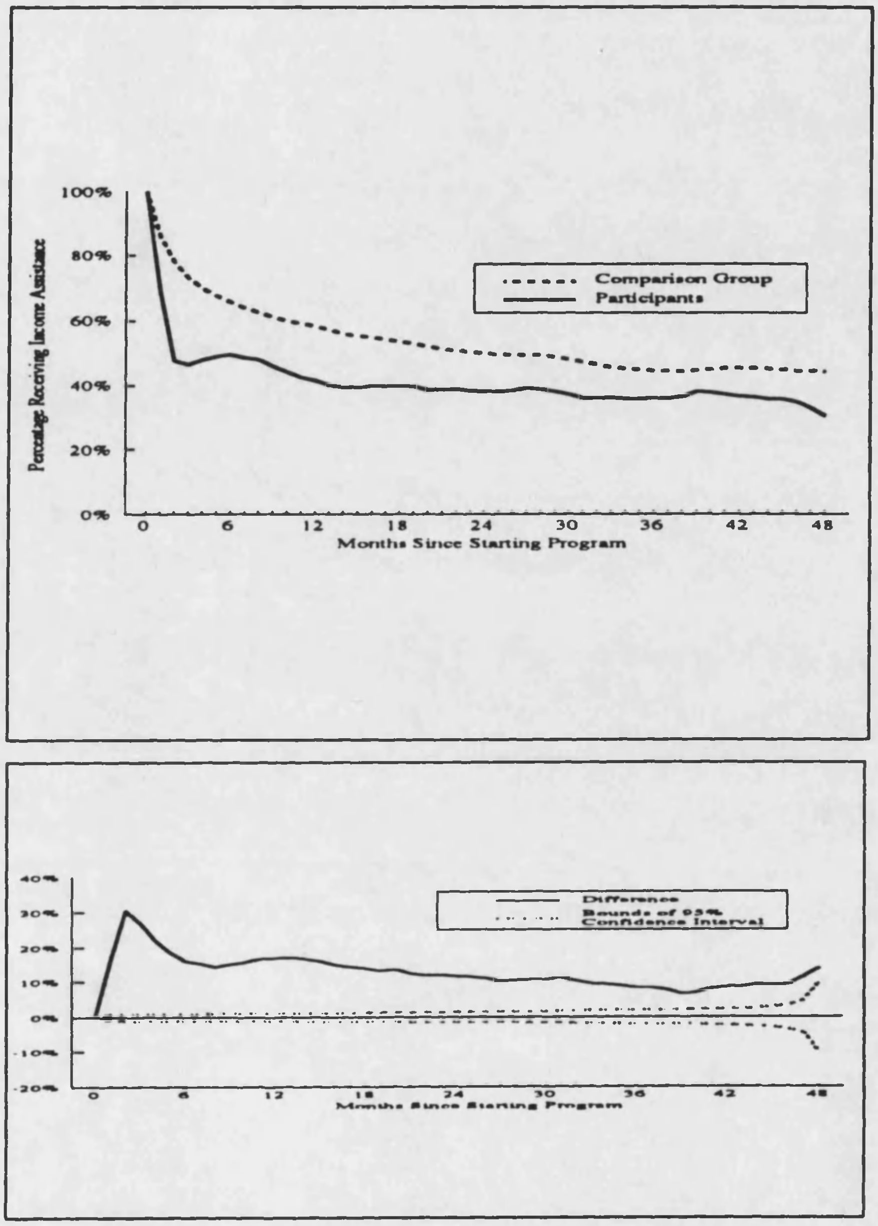

Figure 6-1 Percent dependent on welfare, EOP participants and comparison group

Data source 4; comparison group A

6.2.2 Moving to Unemployment Insurance

6.2.2.1 Conclusion:

The program resulted in increased dependence on Unemployment Insurance. Participants were more likely than non-participants to draw Unemployment Insurance, a difference that was most pronounced during the 12 months after the end of the program but which was still in evidence up to the end of observations three years after the program began.

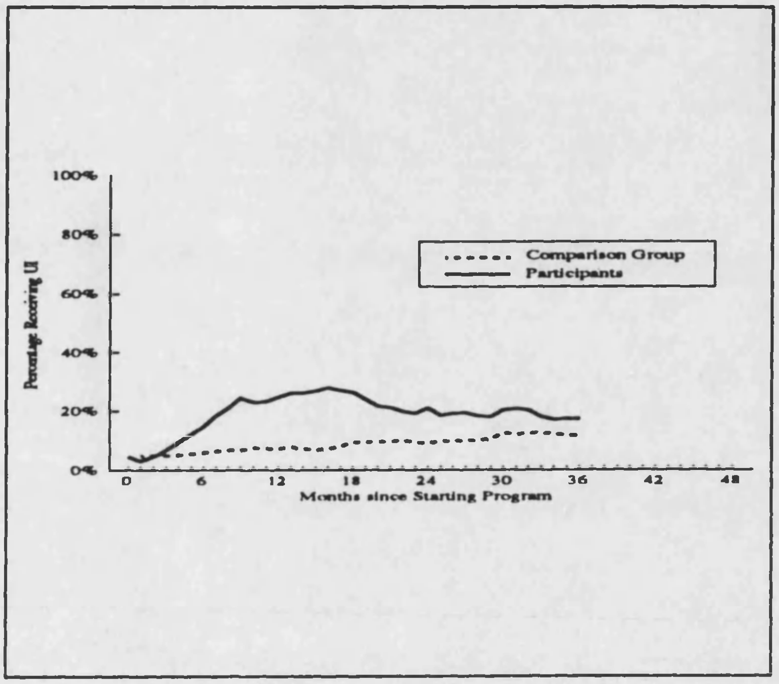

Figure 6-2 Percent dependent on UI, EOP participants and comparison group
Data sources 4, 7; regression analysis

6.2.2.2 Discussion: Estimates of the impact on UI dependence were made using regression analysis and a matched comparison group. In the first method, 36 separate regressions were used to estimate the impact of EOP on subsequent UI dependence. (Regression equations and results are reported in Appendix B.) They showed that on average participants were more dependent on Unemployment Insurance than the comparison group from month three after the beginning of the program (Figure 6-2). This greater propensity amounted to 15 to 20 percentage points during the 12-month period of their eligibility for Unemployment Insurance due to their program participation. After month 18, the difference declined to five to 10 percentage points but remained visible for as long as observations continued.

The second method used a matched comparison group described in Chapter 5 (See Figure 5-12). The matched comparison group produced estimates that were somewhat lower (about half) than the results produced using regression analysis, but the qualitative results are the same, EOP increased UI dependence in both the short and long run, but the effects were larger in the short run.

6.2.3 Finding employment

6.2.3.1 Conclusion:

Survey results showed that the program clearly helped participants find employment.

6.2.3.2 Discussion:

Data from the interview survey were used. The interviewees were asked simply: "Are you currently working?" The data were analysed using maximum likelihood (probit) analysis. The response rate was about 75%, but since the survey was the source of the dependent variable, it was not possible to estimate the impact of non-response bias on this question. With that caveat, the analysis found that the Employment Opportunity Program, increased employment by a statistically significant 11 percentage points. This finding is consistent with the results of the estimates of the impact on welfare dependence that are not subject to non-response bias.

6.2.4 Impacts on different types of recipients

6.2.4.1 Conclusion:

The program helped those with longer welfare histories more than those with shorter welfare histories. Single parents were less likely to be helped than other types of recipients, but the single parents who were helped, because of their dependants, brought a much greater reduction in benefits paid than others. Program impact also tends to rise with the age of participants.

6.2.4.2 Discussion:

Cell matching produced estimates of the impact of EOP on the amount of welfare received for each participant. These estimates were used as the dependent variable in a regression in which the characteristics of the participants were the explanatory variables.

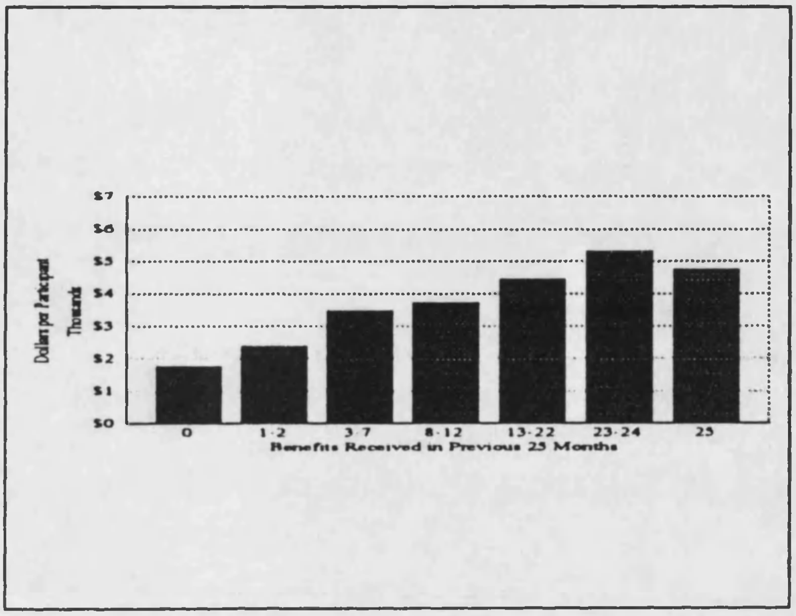

Figure 6-3 Welfare savings by welfare history

Data source 4; comparison group A

By history of benefits, program impact increased with number of months of benefits received in the previous 25 months up to a maximum of 23 to 24 months, then showed a decline for the group that had received benefits in each of the previous 25 months (Figure 6.3). This suggests, first, that program impact is least for those least dependent on welfare, increases to a maximum, and then begins to decline among those with very lengthy dependence.

Figure 6-4 Welfare savings by category

Data source 4; comparison group A

By categories of applicants, program impacts increase successively for single men, single women, couples, two-parent families, and single-parent families (Figure 6-4). Detailed results show that, **in caseload numbers,** the program did less to reduce the welfare dependence of single parents than of the others. However, because the average number of dependants was higher for single parent cases than for other cases, the effect on number of **recipients,** and thus on welfare costs, was greater for single parents than for others.

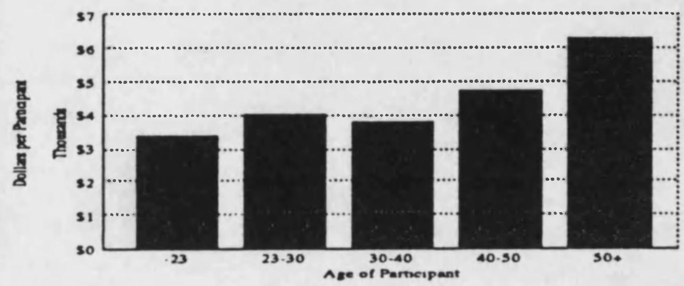

Figure 6-5 Welfare savings by age group
Data source 4; comparison group A

By age, program impacts do not vary much up to age 40, but from then on increase markedly, and are greatest for participants over 50 (Figure 6-5).

6.3 The Broader Context

6.3.1 Benefits to employers

6.3.1.1 Conclusion:
Most employers clearly benefited from the program, while a few clearly did not.

6.3.1.2 Discussion:
Employers participating in the program were asked to compare the productivity of program employees and unsubsidised employees. Nearly two-thirds stated that program employees were as productive as regular employees or more productive (see table). They were then asked how much they paid unsubsidised new employees. The answers averaged $6.04. Program data meanwhile show that employers paid an average of $3.06 for program employees.

Table 6-1 : Client Productivity (% of employers stating)	
Much Better	4
Somewhat better	22
About Same	37
Somewhat worse	19
Much Worse	18

Thus the nearly two-thirds of employers who received equal or better productivity from their program employees saved on average nearly $3.00 an hour on each of them. On the other hand, one employer in five paid $3.06 an hour for employees of poor productivity. In fact, one employer in ten rated program employees' productivity as zero, and so saw themselves as getting nothing at all in return.

6.3.2 Effect of unemployment rate on program impact

6.3.2.1 Conclusion
The unemployment rate had no clear effect on the program's impact.

6.3.2.2 Discussion
Program impact was estimated for three calendar years, 1988-90, whose employment rates differed. The average reduction in welfare benefits attributable to the program was 1.6, 1.8, and 1.9 months respectively, while the unemployment rate in the same three years was 10.3, 9.1, and 8.3 percent.

Interestingly, the largest program impact was in 1990, the year with the lowest unemployment. Moreover, the program impact remained substantial despite a large variation in unemployment.

6.3.3 Program expenditures and savings

6.3.3.1 Conclusion

For the provincial government, the expenditures on the program were lower than the savings that resulted from the reduction in welfare payments. Unemployment Insurance payments increased federal government costs.

6.3.3.2 Discussion

The dramatic effect of the program on reducing welfare dependence has already been shown (Figure 6-1). The area between the participants' curve and the comparison group curve represents[1] the difference in welfare dependence attributable to the program and thus the overall saving in welfare. In time this saving to the provincial government accumulates to a much higher level than the program expenditures, which consist mainly of the wage subsidies.

Table 6-2 :Employment Opportunity Program: Selected Costs and Savings	
Program Cost	2129
Welfare Savings	5182
UI Costs	2162
Note: Discounting at 10%	

The program causes a substantial increase in federal Unemployment Insurance payments, indicated by the area between the curves in Figure 6-2, this time with participants higher than

[1] This diagram only shows percentage receiving any benefits. Estimates of levels of savings are made by comparing benefits paid to participants and the comparison group.

the comparison group. But after 65 months, the reduction in welfare expenditures is greater than the combined increase in expenditures on the program and on Unemployment Insurance by $890 per participant in 1989 dollars (assuming a discount rate of 10 percent annually).

7. Impacts of other Programs

This chapter presents estimates of the impact of three additional employment and training programs: on-the-job training in public projects, classroom training and job clubs (job search skills training). These estimates were produced using a variety of techniques. Estimates of the impacts of the on-the-job training in public projects were produced using cell matching on administrative data and regression analysis on survey data. Estimates of the impacts of classroom training were made by combining administrative data from a community college with administrative data from the Ministry of Social Services. The comparison group was selected from welfare recipients who were not participating in Ministry on-the-job training programs. Estimates of the impact of the job club were made using random assignment. Both administrative and survey data were used in the analysis.

Recall that Chapter 5 found that selection bias in estimates of the impact of programs in BC on subsequent welfare dependence amounted to roughly five percentage points when estimates of impact were made by comparing subsequent welfare dependence of participants with non participants who were similar in age, sex, marital status, history of dependence classification and who were receiving welfare at the same time as the participants. The analysis was done explicitly so that the reader could assess the results presented in this and the following chapters. Where the impacts are less than five percentage points they should be viewed with suspicion. However, where they are greater than five percentage points, we can be fairly confident that there is a real long term impact.

7.1 On-the-Job Training in Public Projects

7.1.1 The Programs

British Columbia has three programs that offer welfare recipients employment with on-the-job training on government projects. The Ministry of Social Services supplies the wages (up to $7 an hour for labourers and up to $10 an hour for supervisors), the employers' contributions to employee benefits, and an additional amount for administrative overhead. The positions are normally for six months but are sometimes extended to 12 months.

Table 7-1 Relative size of public sector programs (1991/1992)		
	Clients	Budget
CTETP	961	5.3
FWAP	508	4.1
EYP	350	3.0
Total	1819	12.4

The three programs are as follows:

- The Community Tourism Employment Training Program (CTETP) funds work with non-profit organisations on community tourism development. Projects include heritage site restorations, parks development, and festival start-ups.
- The Forest Worker Assistance Program (FWAP) funds work on silviculture projects throughout the province. The FWAP is administered by the Ministry of Forests.
- The Environment Youth Program funds work for welfare recipients aged 17-24 on such outdoor projects as trail improvement and salmon enhancement. The EYP is operated in conjunction with the Ministry of Environment, Lands and Parks.

These three public-sector programs taken together are comparable in budget to the Employment Opportunity Program. However, they assist only about one-quarter the number of people. Fewer than one percent of all welfare recipients participated in the public-sector programs, and the average cost per participant was about $6800.

7.1.2 Conclusions

British Columbia's three public employment programs reduced welfare dependence in the short term but had little long-term impact. The survey results also indicated that the program did not help participants find jobs in the long term.

Like the private sector wage subsidy program, the public employment programs all increased UI eligibility, especially in the 12 months following the end of each program, when participants were 25 to 30 percentage points more likely than non-participants to be on UI. This effect was much greater than with the wage subsidy program, and in two of three instances lasted at a lower level throughout the observation period.

The programs created a value in public benefits estimated to range from 50 to 94 percent of the program expenditures. The combination of value created and the short term reduction in welfare benefits paid out approached program expenditures but was markedly less than program expenditures and increased Unemployment Insurance combined.

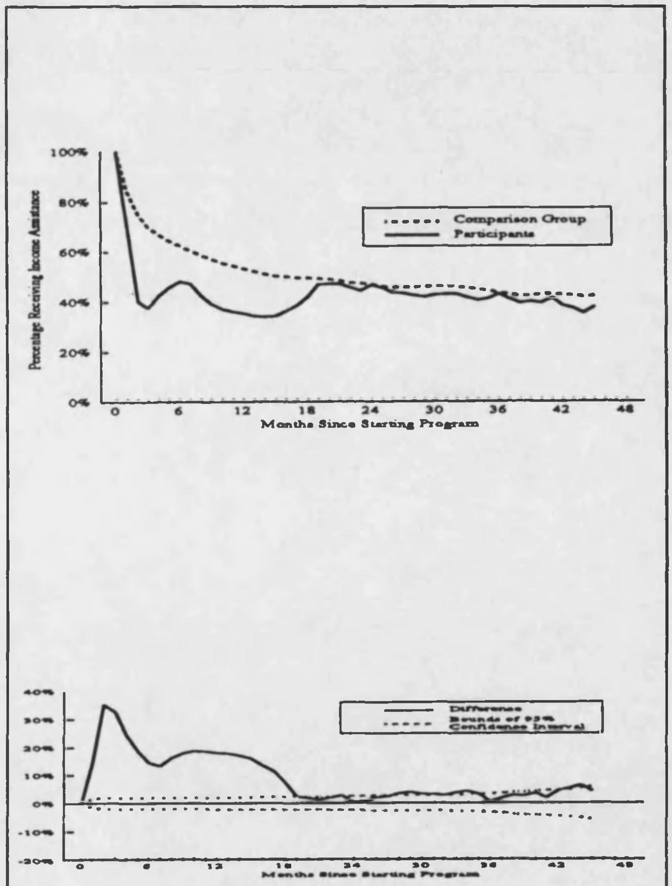

Figure 7-1 Impact of CTETP on subsequent welfare dependence

Data source 4; comparison group A

7.1.3 Impact On Participants

7.1.4 Reducing welfare

7.1.4.1 Conclusion

The public programs helped reduce welfare dependence while they were under way and during the following 12 months of Unemployment Insurance eligibility for participants. Beyond that they had little or no positive effect.

7.1.4.2 Discussion

Using a cell-matched comparison group of non-participants, it was found that the welfare dependence of program participants dropped much more quickly than for comparable non-participants, and stayed lower for about 18 months (Figures 7-1 - 7-3). In the first six months, the employment in the programs themselves accounts for the lower welfare dependence of the participants. The next 12 months corresponds to the period of participants' eligibility for Unemployment Insurance as a consequence of program employment. (Participants showed greater reliance on UI during this period; see below.)

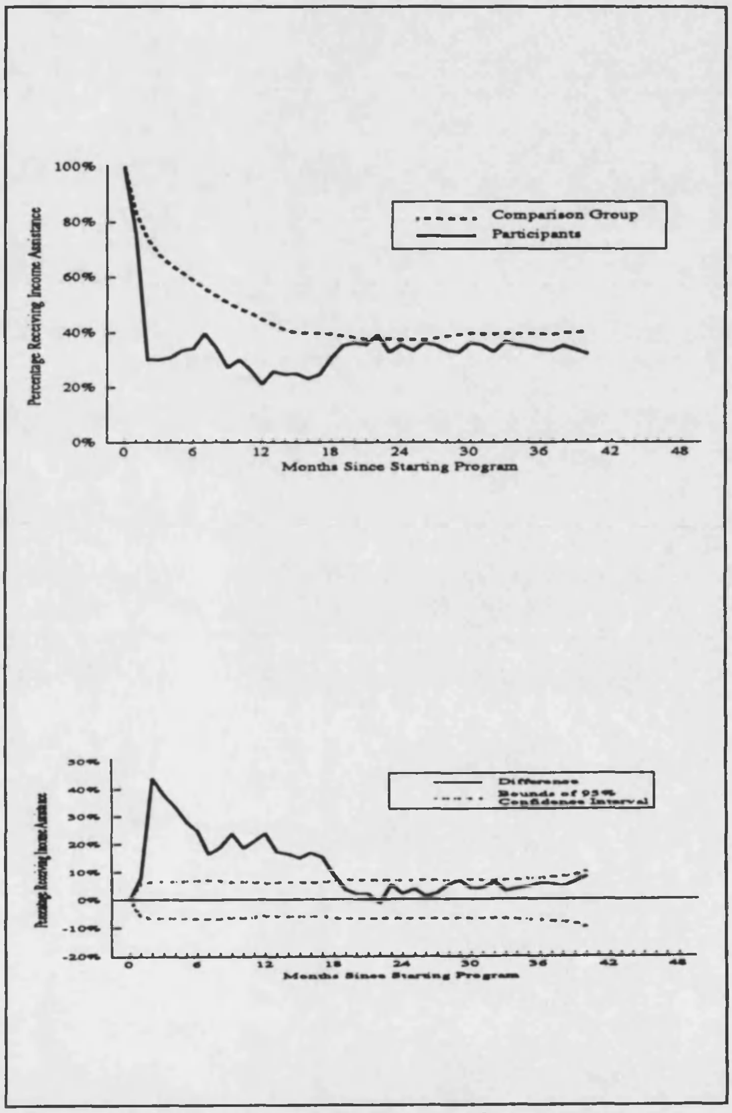

Figure 7-2 Impact of EYP on subsequent welfare dependence

Data source 4; comparison group A

After 18 months, the impact of the programs diminishes dramatically. Between months 19 and 36 the average impact of the Community Tourism Program averaged

3.5 percentage points, and the Environment Youth Program averaged 3.3 percentage points (Figures 7-1 and 7-2). Although these differences are statistically significant, they are not necessarily due to the initial program.

Of the participants in the CTETP in 1988/89, 8.3 percent participated in a subsequent on-the-job training program. Since the subsequent program had an impact of 15 to 20 percentage points in the early months, roughly half of the long-term impact can be attributed to the impact of subsequent programs. About 4.5 percent of participants in EYP participated in other programs in subsequent years so roughly one-quarter of the long term impact of EYP can be attributed to the impact of subsequent programs.

The pattern of welfare dependence of participants is strikingly different in the Forest Worker Assistance Program than in the other programs (Figure 7-3). The dependence of the participants is higher than that of the comparison group at about two years following entrance to the program, but subsequently became lower than the comparison group. This may be due to the high degree of seasonality in the forestry programs. Two-thirds of participants entered the program between July and October; virtually none entered between March and July. If the forestry programs cause people to join a seasonal

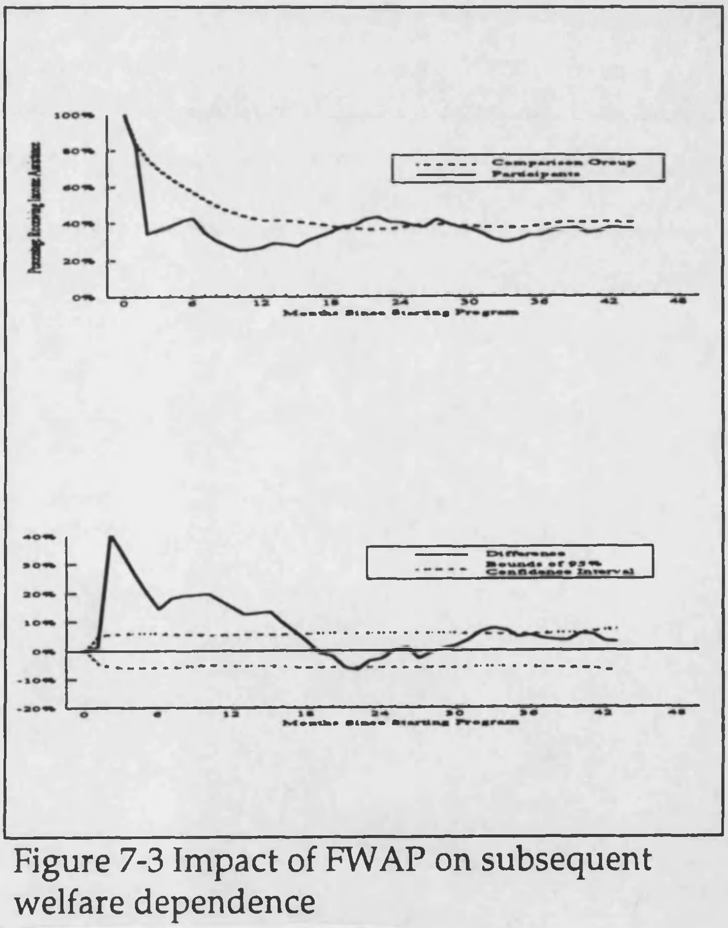

Figure 7-3 Impact of FWAP on subsequent welfare dependence
Data source 4; comparison group A

industry, then the level of their dependence might be expected to oscillate around the level of dependence of the comparison group. The impact of repeat users of the program may have exacerbated this. Roughly seven percent of participants in the 1988/89 forestry program participated in a subsequent program. If those programs started in the same months, the cyclers would leave welfare faster than the comparison group in a few specific months.

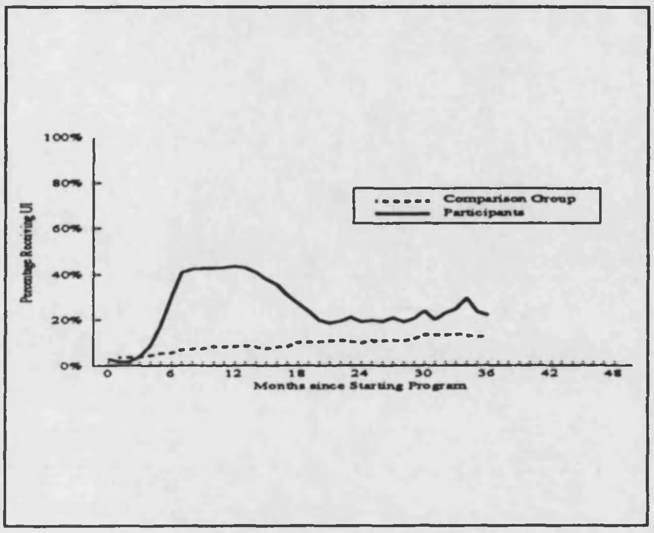

Figure 7-4 Impact of CTETP on subsequent UI dependence
Data sources 4, 7; regression analysis

7.1.5 Moving to unemployment insurance

7.1.5.1 Conclusion

All three programs enabled their participants to rely on Unemployment Insurance to a much greater extent than non-participants (25 to 30 percentage points more) for the 12-month UI eligibility period after the end of the program and then somewhat more for at least another year after that. Beginning about month 30 from program start, FWAP participants became less reliant on UI than non-participants. But greater long-term reliance on UI than non-participants persisted for CTETP participants and even more so for EYP participants.

7.1.5.2 Discussion

Regression analyses showed that participants were more dependent on average than the comparison group on Unemployment Insurance from about month four after the beginning of each program (Figures 7-4 - 7-6). Participant dependence remained 25 to 30 percentage points higher than non-participant dependence over the 12 month period of their program-derived eligibility for Unemployment Insurance. After month 18, participants remained more dependent on Unemployment Insurance than the comparison group, but only by perhaps 10 percentage points. With the Forest Worker Assistance Program this greater dependence on UI came to an end about 30 months from the beginning of the program. With the Community Tourism Employment Training Program and the Environment Youth Program, the greater dependence of participants on UI persisted throughout the observation period of 36 months.

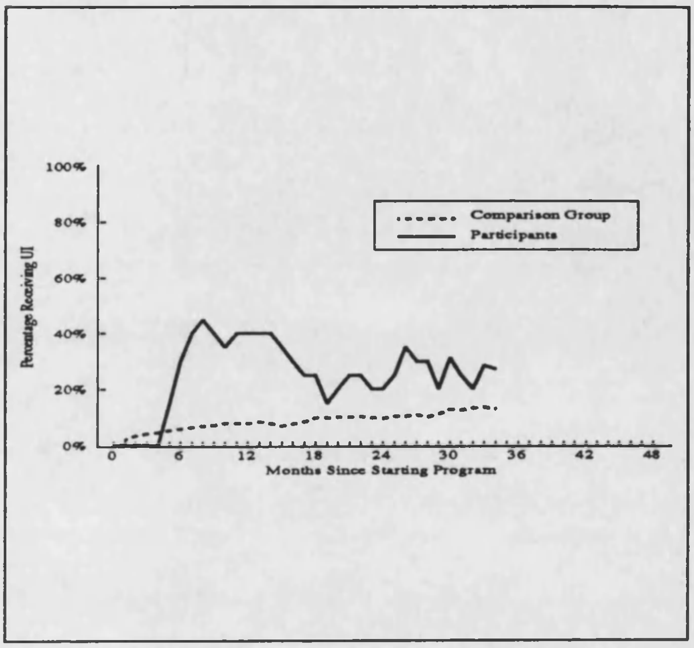

Figure 7-6 Impact of EYP on subsequent UI dependence

Data sources 4, 7; regression analysis

Details of the regression analyses are provided in Appendix B.

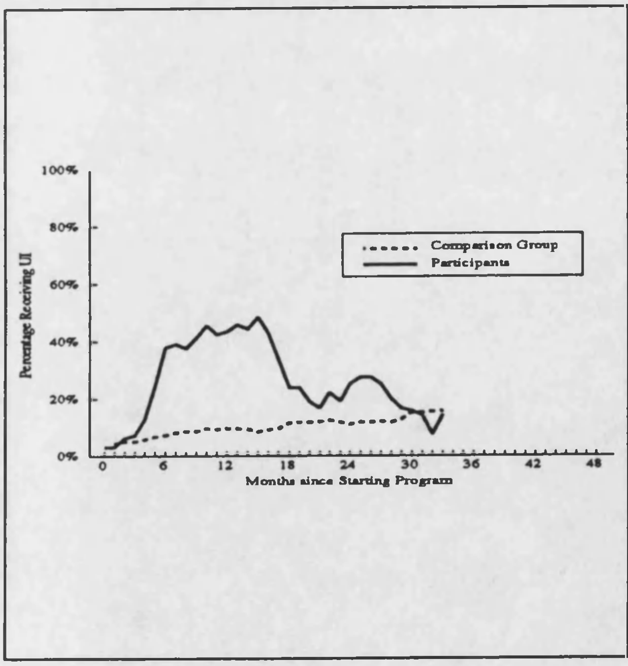

Figure 7-5 Impact of FWAP on subsequent UI dependence

Data sources 4, 7; regression analysis

7.1.6 Finding employment

7.1.6.1 Conclusion

There is no clear survey evidence that the programs helped people get work once the programs themselves were completed.

7.1.6.2 Discussion

The survey interviewees were asked simply: "Are you currently working?"

Table 7-2 Percent employed by program

	Participants	Non-Participants
CTETP	46	46
FWAP	23	43
EYP	46	41

The survey responses, when controlled for participant characteristics, showed no long-term impact for CTETP and EYP: there was no statistically significant difference in rate of current employment at the 17-month mark between the participants and the non-participants in the comparison group.

By contrast, FWAP respondents exhibited a large negative impact. However, this is probably due to the seasonal nature of forestry work and the timing of the interview. With the vast majority of FWAP projects starting in the late summer, the survey at the 17 month mark would be in the winter. Additional evidence in support of this explanation is that in response to the question, "Have you been employed since the last interview?" five percent more of the FWAP participants than the comparison group answered "yes."

7.1.7 Public value of work performed

7.1.7.1 Conclusion

The public employment programs produced value ranging from 50 percent to 94 percent of the value that would have been produced using private contractors for the same tasks.

7.1.7.2 Discussion

This question required that estimates be made by the employing agencies and supervisors. The value of the program products, mainly in the form of work completed, was taken to be the estimated cost of having the same product produced by other means, such as by contractors. The managers for whom the program participants worked were asked to estimate that alternative cost for equivalent work completed, which was then compared to the cost of the full wage subsidies for the participants. The three public programs had varying results:

- CTETP: This estimate had the lowest reliability. Because of the decentralised nature of the projects, only a small sample of managers and projects could be examined. The interview question, "What is the value of output per month?", may have been interpreted as an intrinsic value of the product rather than as the cost of an alternative supply. Comparisons could not easily be made with other projects where costs were known, and the managers interviewed were in any case not experts in the field, as they were for the other two programs. The sample surveyed found that $16,000 in work was completed for a total cost of $32,000. Thus each CTETP dollar generated 50 cents in product value.
- FWAP: The estimate here was judged the most reliable of the three, because the Ministry of Forests measured the work completed and was able to compare it directly to similar work completed by their contractors. In fiscal year 1986/87 (when the FWAP was much larger than in subsequent years), the total value of work completed was $9.7 million, for a cost of $12.5 million. Thus each FWAP dollar generated 78 cents in product value.
- EYP: This estimate was less reliable because the projects for the Ministry of Environment were not always quantifiable or comparable to other projects for which costs were available. Nevertheless, the judgement of experts in the field was that in fiscal 1987/88, $1.6 million in work was completed for a total

cost of $1.7 million. Thus each EYP dollar generated 94 cents in product value.

7.1.8 Program expenditures and savings

7.1.8.1 Conclusion

The programs reduced welfare expenditures by the provincial government, though not enough to equal program expenditures. But when the value to society of the work performed by the participants is added to the welfare savings, the combination approaches program expenditures in each case. At the same time, increased Unemployment Insurance expenditures were imposed on the federal government.

Table 7-3: Public sector employment programs, selected costs and savings ($/participant)

	CTETP	FWAP	EYP
Program Cost	5155	8971	8571
Public Value Created	2758	6295	8057
Welfare Savings	2127	1057	2335
UI Costs	3519	3194	3344

Note: Discounting at 10%

Discussion

The accompanying table shows how the welfare savings compared to program expenditures, the value of the work performed, and the increased Unemployment Insurance payments that resulted. For CTETP and FWAP, welfare savings were calculated by subtracting the welfare benefits paid to the participants from those paid to the comparison group. For EYP this difference was estimated by multiplying the difference in dependence (shown in Figure 7-2) by the average benefits per case.

7.2 Classroom Training Programs

7.2.1 The Programs

The Ministry refers welfare recipients to classroom training programs as employment preparation. Funding is provided by a number of agencies, the provincial Ministries of Social Services and of Skills Training and Labour, and the federal Human Resources Development Canada. The training programs are provided mainly by community colleges and institutes. In 1991/92, more than 20,000 recipients registered for these courses, and the cost to the Ministry (but not the total cost) of providing the courses was about $3 million, for an average cost per participant of $150.

Detail on these programs can be gained from the example of Camosun College in Victoria, where the programs are placed in four categories:

- Vocational training: 6-12 months in duration, e.g. plumbing, welding, secretarial, dental hygienist;
- Career Technical Training: 24 months in duration, e.g. criminal justice, visual arts, electronic technology;
- Adult Basic Education: from basic literacy and numeracy to high school equivalence, including Employment Opportunities for Women;
- Academic: university transfer courses.

7.2.2 Conclusions

An earlier (1987) province-wide estimate of the impact of Classroom Training programs on welfare dependence showed no positive impact and a probable small negative impact (i.e., causing additional dependence). A new (1992) province-wide analysis is only slightly more optimistic, showing a modest positive overall impact in the form of a net reduction of dependence by nearly two months of benefits per participant over a five-year period.

Another new analysis focuses on courses provided by one institution, Camosun College in Victoria, to compare the effects of different types of courses. The results show that Career Technical and Vocational courses have the most impact, Adult Basic Education has the least positive impact (if any), and Academic courses have a modest positive impact. For all courses together, the result for Camosun was consistent with the province-wide finding: a small positive impact of nearly two months in reduced welfare benefits paid.

7.2.2.1 Selection bias in the disaggregated study

Of all the estimates presented in this thesis, the disaggregated results from the joint study with Camosun College are the most likely to suffer from selection bias. In this case an important relevant variable, education, is correlated both with the program (whether upgrading or post-secondary) and with the outcome, subsequent welfare dependence. Individuals must have completed their secondary education in order to enrol in vocational, career-technical or academic courses at a community college. Otherwise they must enrol in upgrading (ABE). The results of the survey indicate that individuals who declare any post secondary training are six percentage points less likely to receive welfare[1]. To the extent that secondary schooling completion is not perfectly correlated with the variables used to select the comparison group, the comparison group will have more education than the participants in ABE and less education than participants in the other courses. In the survey sample 52% of respondents reported some post secondary training. This suggests that bias from this source would be less than three percentage points, not enough to overturn the results.

[1] See Regression Results, Full Sample, the last table in the appendix to Chapter 5, coefficient on variable APS.

7.2.3 Background

Classroom Training programs for welfare recipients have been criticised for ineffectiveness. In British Columbia, a study in 1987 took a province-wide sample of participants in all courses in 1983 and traced their experience through 1986. The results showed a probable negative impact on participants, that is, their welfare dependence was modestly increased rather than reduced.

Evaluations of Classroom Training programs conducted elsewhere show much the same results. The federal government's evaluation of the National Institutional Training Programs concluded that there was "no significant benefit from participating in the training relative to a comparison group." [Abt 1985] American studies have generated similar findings[2].

However, a Massachusetts study of state programs which was the first to break down classroom training by type produced a new and interesting result: though there was no impact from adult basic education or from English as a second language courses, there was a positive impact from vocational training courses. [Nightengale 1991] This finding encouraged us to approach Camosun College to conduct a similar disaggregated study, allowing an examination of the differences in impact between the courses, differences that turn out to be significant. The Camosun study was accompanied by a second province-wide analysis.
This report therefore discusses three estimates of the impact of Classroom Training programs for welfare recipients in British Columbia. The first is the province-wide 1987 study of participants from 1983 through 1986. The second study (1992) mirrors the first at a later period, following a province-wide sample of participants in all programs from 1986 through 1991. The third is limited to courses offered by

[2] See e.g. Lalonde 1992.

Camosun College in Victoria with a sample of participants whose experience was followed from 1986 through 1991.

The analysis of Classroom Training programs is limited to the impact on welfare dependence using provincial welfare program data. The federal Unemployment Insurance data were available for too few of the participants to be statistically useful. No interview survey was done to gather information on employment and earnings.

The method used was cell matching, with a separate matched comparison group for each participant group. Participants in all four courses totalled 1388 people distributed as follows: Adult Basic Education 760, Vocational 339, Career Technical 169, and Academic 120.

7.2.4 Impact On Participants

7.2.5 Reducing welfare

Conclusion

Classroom Training as such (all courses considered together) had a modest positive impact on welfare dependence, reducing it by nearly two months on average per participant. Career Technical had the largest impact, reducing dependence about 15 to 20 percentage points in the long term. Vocational training came next, reducing dependence about 10 to 15 percentage points.

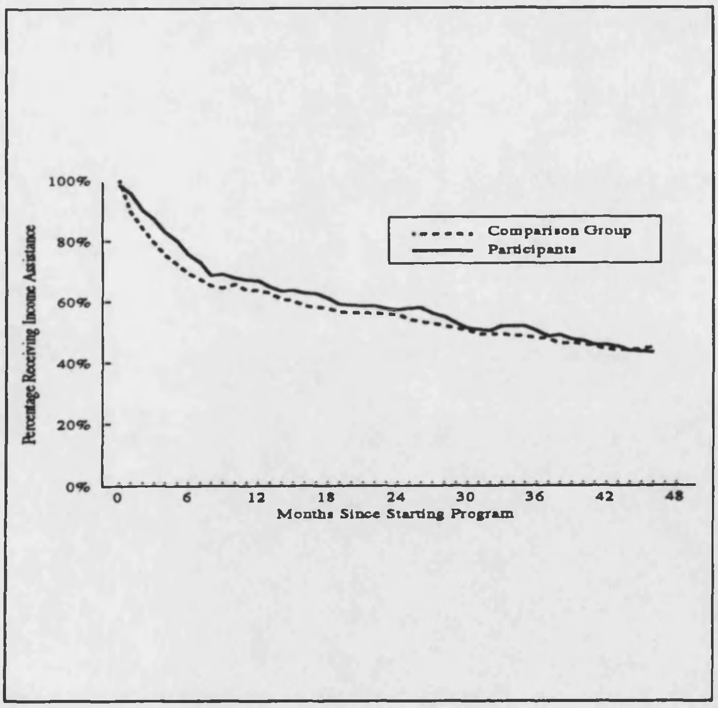

Figure 7-7 Classroom training, province-wide, aggregate impact, 1983 cohort

Data source 9; comparison group A

Academic training had a more modest positive impact. However, Adult Basic Education had no overall positive effect, and in earlier months a negative impact, actually increasing dependence on welfare.

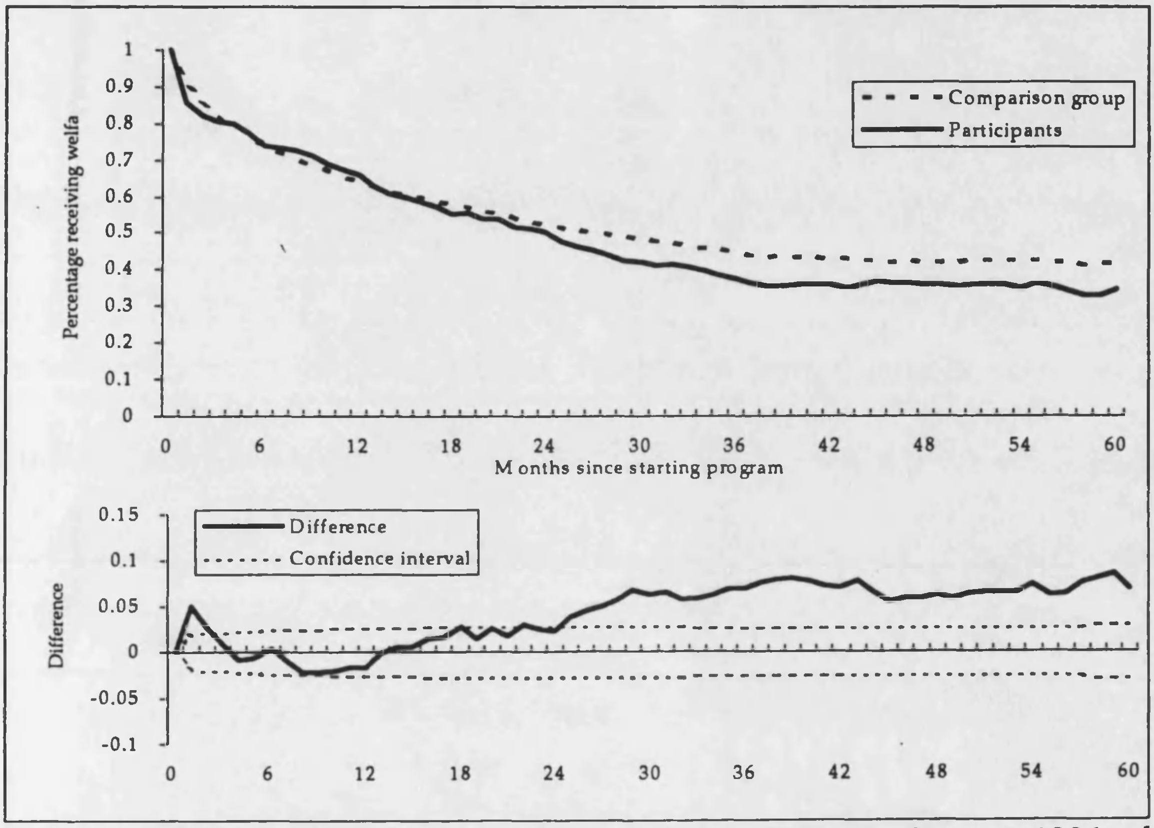

Figure 7-8 Classroom training, Camosun College, aggregate impact, 1986 cohort
Data source 9; comparison group A

7.2.5.1 Discussion

The province-wide results from the first study (1987, an analysis of recipients whose participation began in 1983) showed an aggregate impact that was not positive and possibly negative (Figure 7-7). The second study (1992, an analysis of recipients whose participation began in 1986) differed, showing a modest positive aggregate impact (Figure 7-8). However, this finding was consistent with the aggregate results of the study of Camosun students (1992) who received welfare in the month in which they enrolled (Figure 7-9).

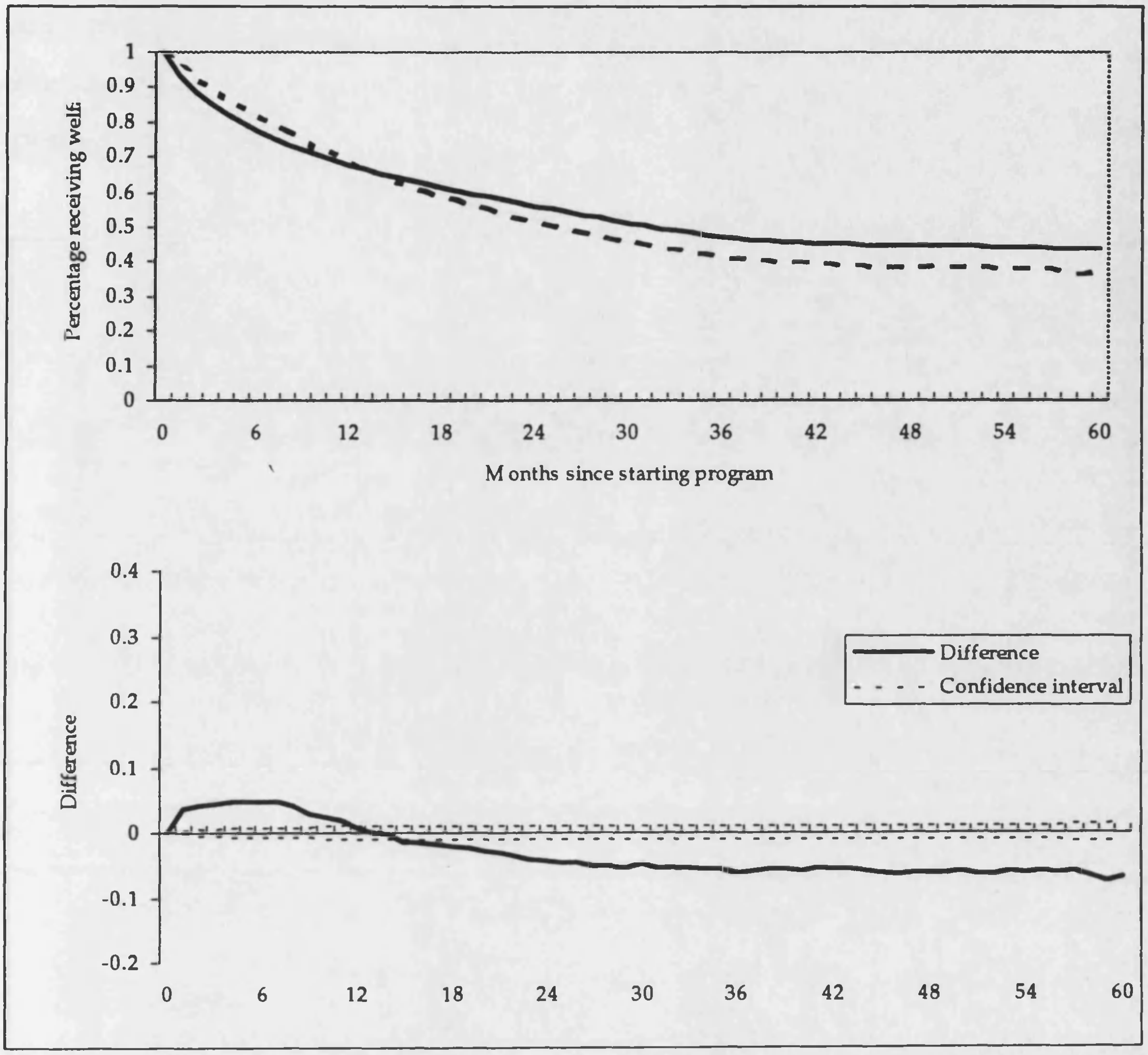

Figure 7-9 Classroom training, province-wide, aggregate impact, 1986 cohort
Data source 9; comparison group A

The difference between the negative overall result in 1987 and the positive result in 1992 is noteworthy. One reason may have been that the provincial welfare caseload increased dramatically during the years between the 1983 sample and the 1986 sample, with most of the increase consisting of recipients classed as "employable". Such recipients would be more likely to attend the more directly employment-oriented courses (Vocational and Career Technical), that also have the highest impact. If a larger proportion of recipients participated in the courses with the highest impact, the overall impact of the courses would rise even if the course impact per participant remained the same. (The overall results for the 1983 sample are strikingly similar to the results for Adult Basic Education for the 1986 sample.) Furthermore, unemployment remained high in the years following the first study, perhaps reducing the number of graduates who could find jobs.

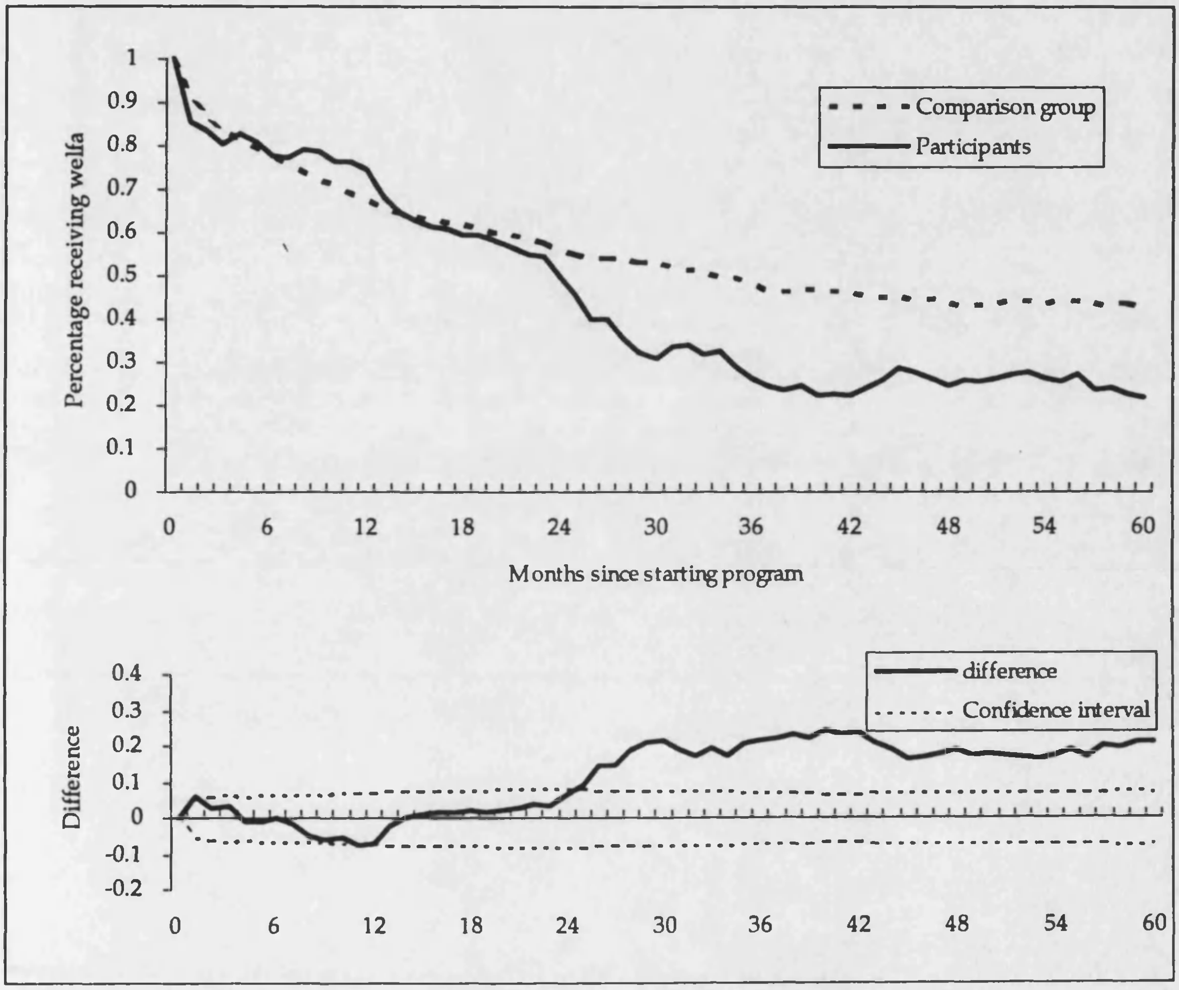

Figure 7-10 Percent dependent on welfare, career-technical training participants and comparison group Data source 9; comparison group A

The course-by-course impacts found in the Camosun study were more pronounced than the aggregate impacts, with interesting month-by-month changes in welfare dependence. Career Technical courses (Figure 7-10) usually last two years. The figure shows the onset of a training effect about month 24 and sustained from then on. Vocational training (Figure 7-11) lasts six to twelve months, averaging about nine months. This figure also shows the onset of a training effect at about nine months, which is sustained as well.

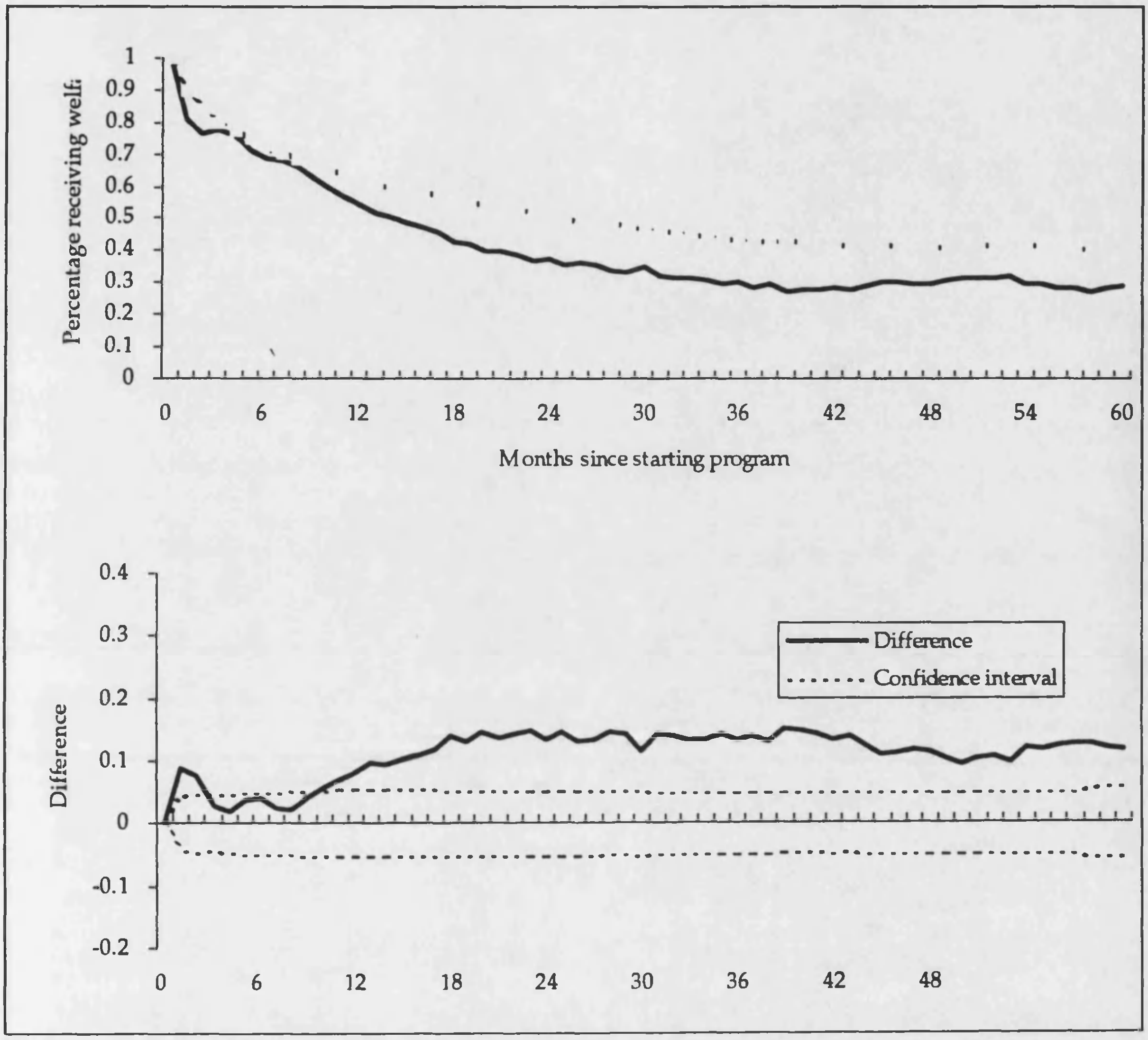

Figure 7-11 Percent dependent on welfare, vocational training participants and comparison group Data source 9; comparison group A

Academic training (Figure 7-12) usually lasts about eight months (during which the student has income from loans and grants, explaining the abrupt decline in welfare dependence in this early period and the return to dependence after the academic year is completed). The sustained but cyclical reduction in welfare dependence from then on may reflect conditions in further academic training rather than the finding of employment and the existence on other non-welfare costs for the provincial government.

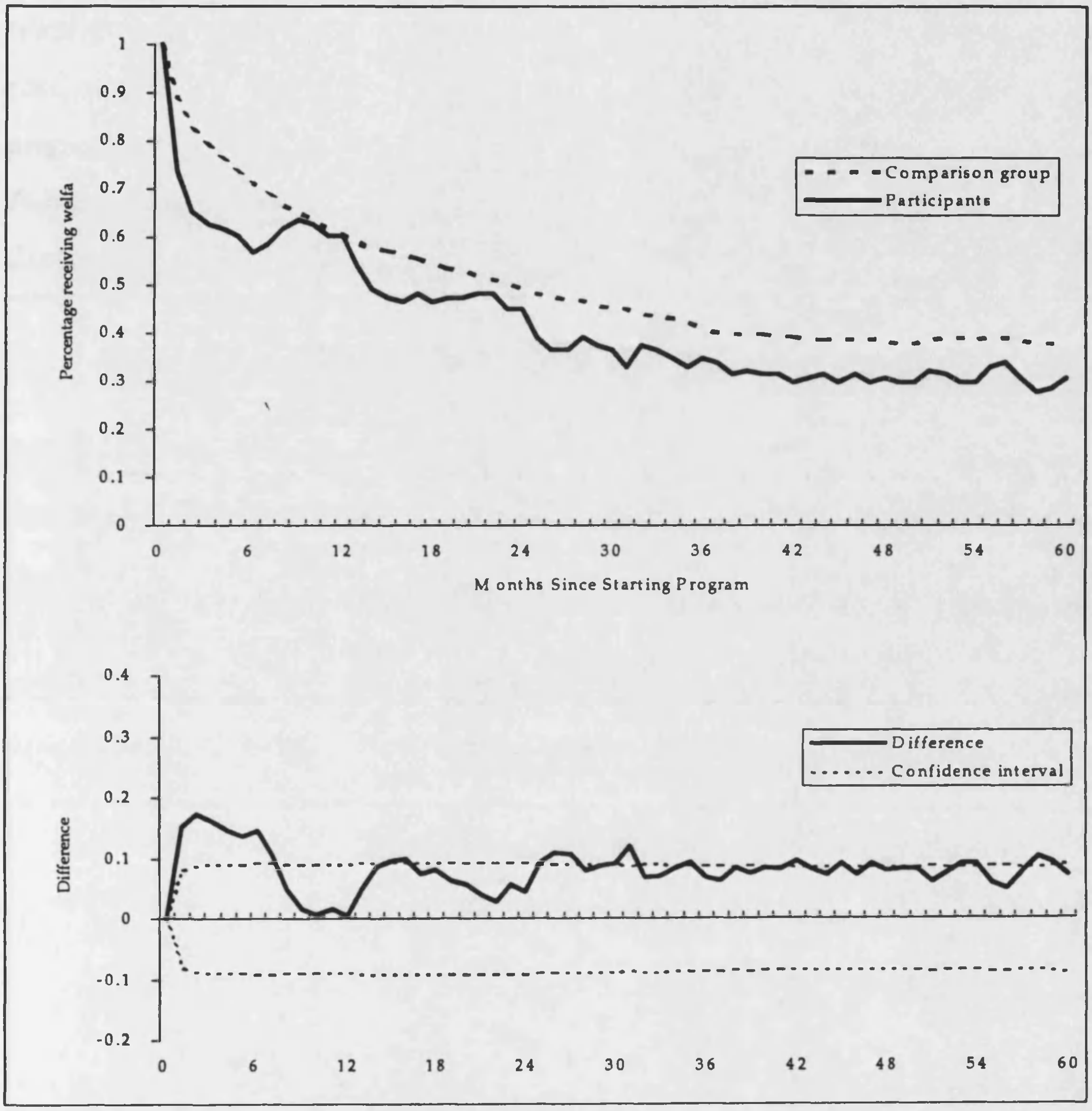

Figure 7-12 Percent dependent on welfare, academic participants and comparison group Data source 9; comparison group A

Quite a different picture is conveyed by Adult Basic Education (Figure 7-13). The absence of a positive impact and the probability of some increased welfare dependence are clear. The continued dependence of recipients on welfare while enrolled is understandable: job search is displaced by study. The similarity between participants and comparison group suggests that the participants did not receive any training that gave them an advantage in the job market. A subsidiary finding, that 46% of courses taken by ABE students in our sample resulted in failure or incomplete, suggests that an examination of the drop out rate, and the reasons for dropping out would be fruitful areas of research. In any event, because most recipients participating in classroom training take the Adult Basic Education courses, the disappointing impact of those courses dominates the overall impact of classroom training.

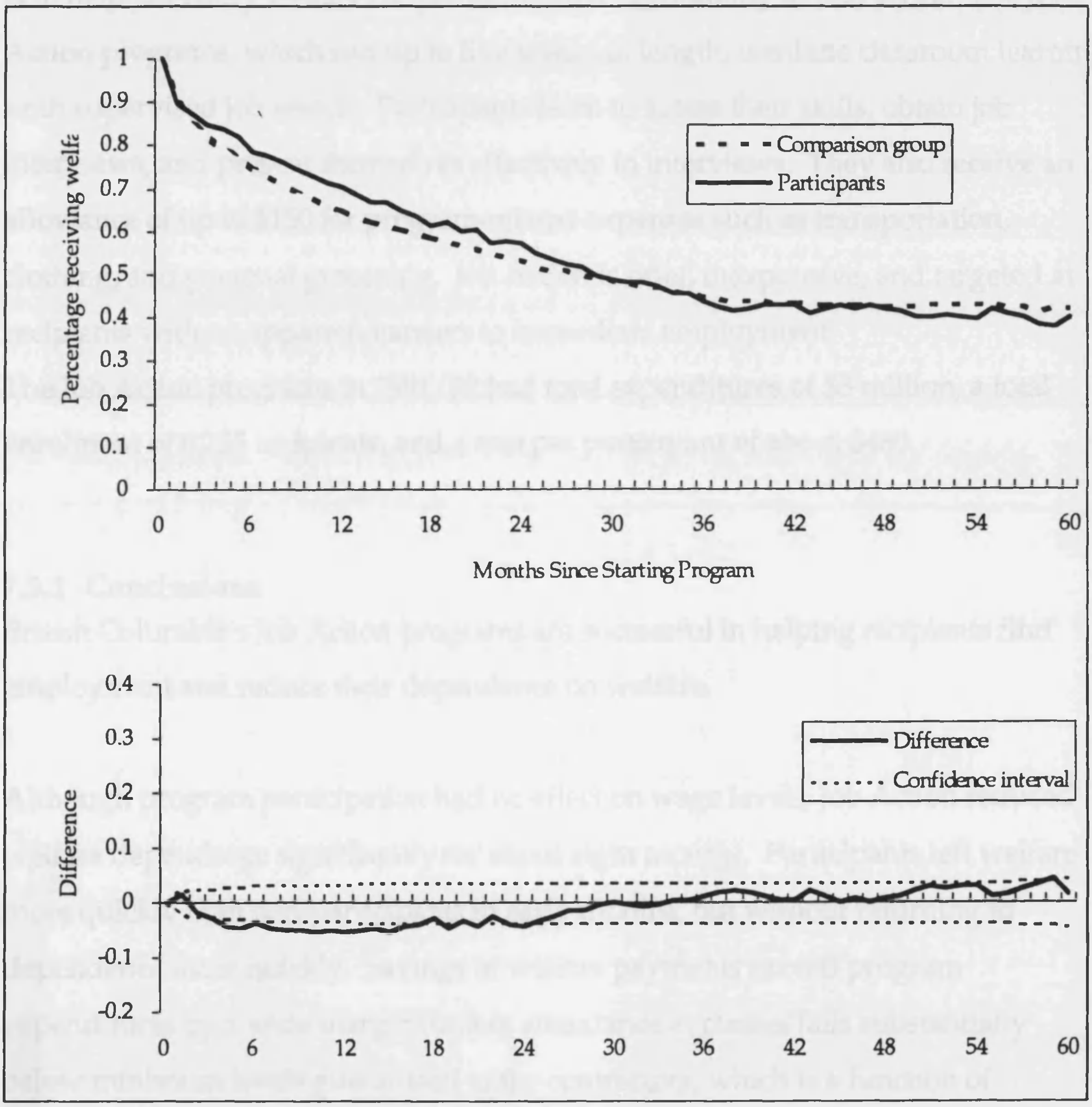

Figure 7-13 Percent dependent on welfare, ABE participants and comparison group | Data source 9; comparison group A

7.3 Job Search

7.3.1 The Program

The Ministry of Social Services has given the name Job Action to its job search assistance initiatives. Job Action encompasses a range of short programs of the kind called "job clubs" found widely across Canada and the United States. Their purpose is to help job-ready welfare recipients improve their ability to find work. The Job Action programs, which run up to five weeks in length, combine classroom learning with supervised job search. Participants learn to assess their skills, obtain job interviews, and present themselves effectively in interviews. They also receive an allowance of up to $150 for program-related expenses such as transportation, clothing, and personal grooming. Job Action is brief, inexpensive, and targeted at recipients with no apparent barriers to immediate employment.

The Job Action programs in 1991/92 had total expenditures of $3 million, a total enrolment of 6,233 recipients, and a cost per participant of about $480.

7.3.2 Conclusions

British Columbia's Job Action programs are successful in helping recipients find employment and reduce their dependence on welfare.

Although program participation had no effect on wage levels, Job Action reduced welfare dependence significantly for about eight months. Participants left welfare more quickly than non-participants in early months, but without returning to dependence more quickly. Savings in welfare payments exceed program expenditures by a wide margin (unless attendance at classes falls substantially below minimum levels guaranteed to the contractors, which is a function of program planning).

These conclusions are in agreement with the results of evaluations of similar programs in the United States.

7.3.3 Background

The estimate of the impact of Job Action was the most detailed and thorough of those described in this report. Whereas the other analyses used comparison groups with participant matching, the Job Action analysis was based on random assignment. The special project was undertaken to test a proposed change in program delivery.

7.3.3.1 The call for an evaluation

In British Columbia, Job Action traditionally has been offered only to people who have been on welfare for several months; for example, two-thirds of participants in Job Action in British Columbia have been on welfare for more than six months. This practice was based on studies that showed that employment and training programs are most helpful to "less-employable" recipients (i.e. longer-term welfare beneficiaries).[3] Because the "more-employable" recipients are more likely to leave welfare within the first few months, the policy of reserving Job Action to longer-term recipients was believed to direct it to those whom it would benefit most.

However, Ministry workers in Region A (parts of Vancouver and neighbouring Richmond) thought this policy might diminish the impact of Job Action for those recipients who, after several months on welfare, lose contact with employers and become discouraged. They urged that Job Action be made available to people from the time they applied for welfare.

Several possible contradictory effects of this policy change could be foreseen:

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[3] See e.g. Friedlander 1988.

- If the Region A workers were right, the new "early" participants would be more responsive to Job Action and thus more successful, increasing its impact.
- But the new "early" participants would not be allocated as the result of a rehab officer's assessment and recommendation, and thus might on average be less responsive than those who were, thereby reducing the program's impact.
- With the "early" participants there would be little or none of the usual preparation for the program, that tells recipients how the program works and what is expected of them, so that, again, the "early" participants might be less responsive than those who were so prepared, also reducing the program's impact.

These questions led to the Region A Job Action Pilot Project, designed to assess the impact of Job Action when offered at the time of application for welfare.

7.3.3.2 A random assignment project

A random assignment test was chosen for several reasons. Because the change in service would extend it to recipients who would not have qualified for it under existing policy, a project whose program participants were randomly selected would not deprive any recipients of services they might have received otherwise. Moreover, provided the demonstration population was large enough, the statistical uncertainties of the results would be smaller than with a comparison group method. Finally, the Region A staff, as advocates of the policy change, were willing to help implement the project.

Applicants for welfare in February, March, and April 1989 who volunteered to participate in the pilot project were referred to interviewers who collected information on their education, work experience, and attitudes. The interviewers then randomly selected participants by sending every second recipient for enrolment in a Job Action course scheduled to begin within a week. Information on subsequent welfare dependence was obtained from Ministry program data. Information on subsequent employment and income was gathered from interviews held six months and 15 months after enrolment.

In all, 236 recipient interviews were completed, from which random assignment led to 125 being referred to Job Action and 118 to the control group. Two participants and five controls refused the interview, so the analysis applied to 123 participants[4] and 113 controls. A comparison of the average of its characteristics with that of the control group revealed no statistically significant differences, thus confirming the random selection (see Appendix C).

7.3.3.3 Estimate of impact

Though only 51 of those randomly selected actually participated and generated the impacts, the data gathered on the impacts of Job Action on those 51 true participants had to be averaged across the entire randomly selected group of 123, as though the impacts on 51 had applied to the whole group. This was done to avoid "selection bias", that might have made — probably did make — the 51 not representative of the whole participant group. Then, using program data, the welfare dependence of participants and controls was followed on a monthly basis. A monthly average dependence was derived for both groups, as were monthly average benefits received. The differences between participants and controls in these basic averages are attributable to the effects of Job Action.

These basic averages were then refined using regression analyses, as detailed in Appendix B. Afterwards, the results were compared with the results of similar evaluations of job club programs in the United States.

[4] Although 123 individuals were referred to Job Action only 51 actually received any service.

7.3.4 Impact On Participants

7.3.5 Reducing welfare

7.3.5.1 Conclusion

Job Action reduced welfare dependence for about eight months. There was no evidence of a longer-term effect.

7.3.5.2 Discussion

The participants and controls were compared for welfare dependence. In Figure 7-14, the two lines show the average welfare dependence for both groups, and the lower dependence of the participant group is clear. Fewer participants than

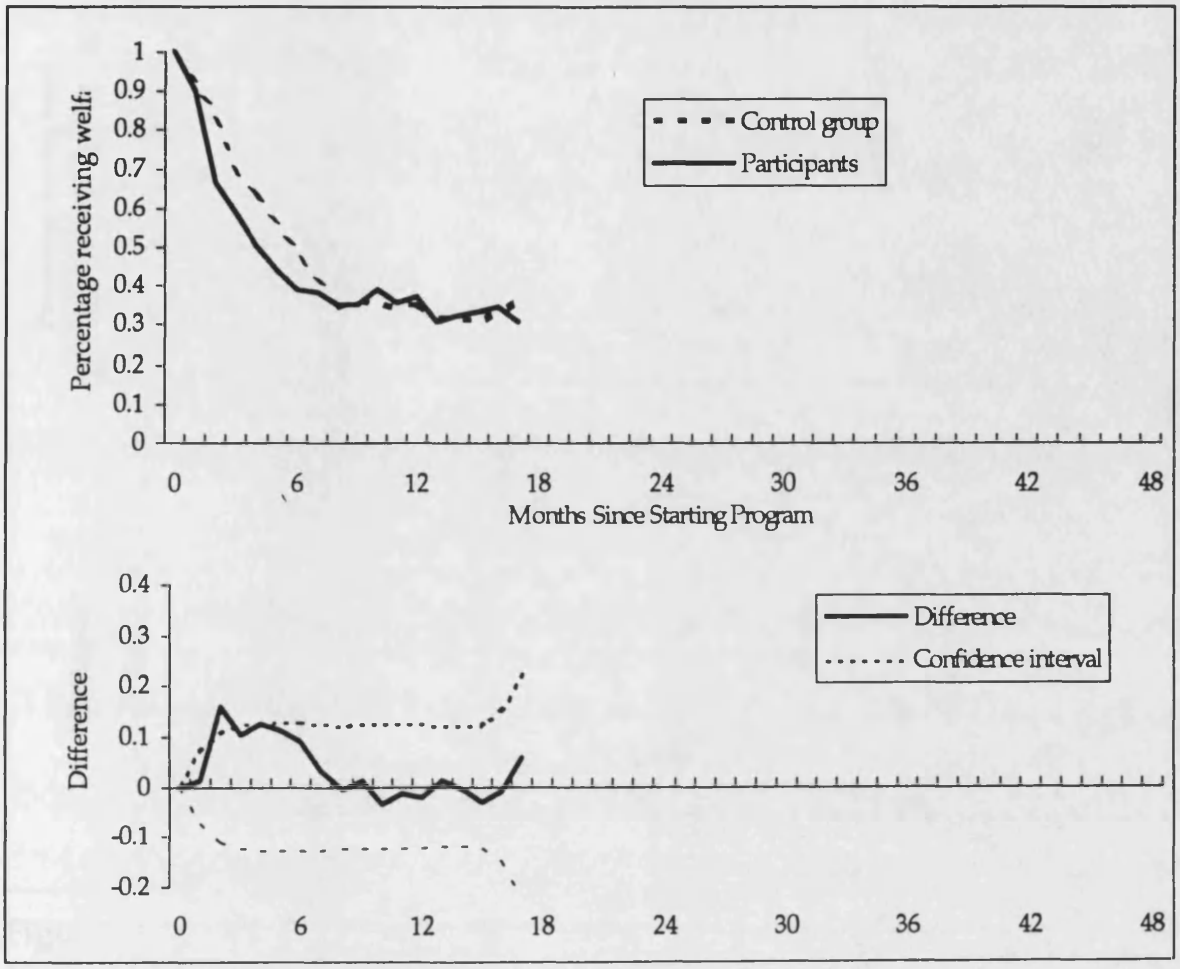

Figure 7-14 Percent dependent on welfare, Job Action participants and control group Data sources 1, 8

controls received benefits in the early months, but the difference petered out after about eight months.

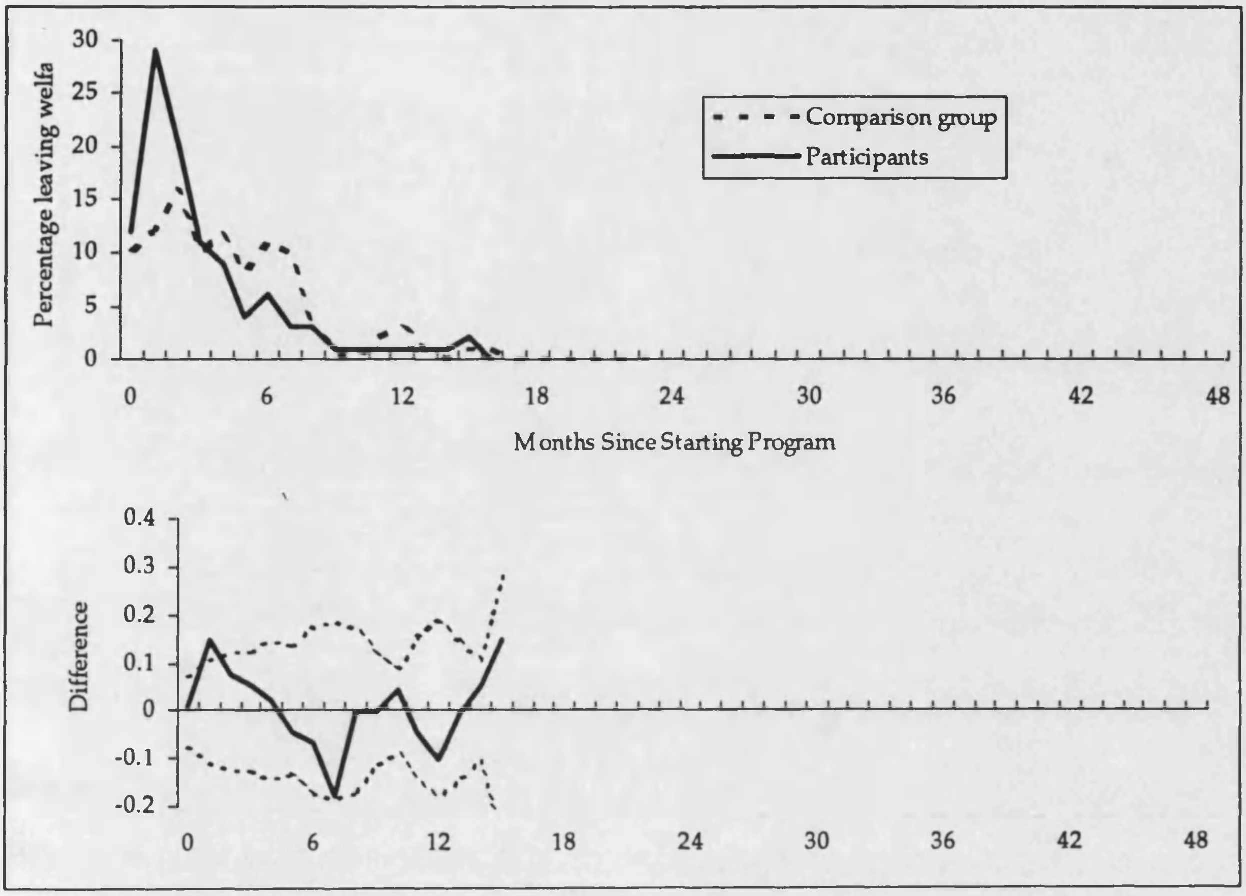

Figure 7-15 Percent leaving welfare, Job Action participants and control group
Data sources 1, 8

The same effect is shown in a different way by looking at the percentage of participants and controls who became independent in each month (Figure 7-15). The two lines show the number of months after the program began when participants and controls stopped receiving benefits. In the first three months participants become independent more quickly than controls, but in subsequent months the opposite happened.

Because Job Action participants get jobs more quickly, there is some concern that these jobs may be unsuitable, causing them to quit prematurely and return to welfare. Examination of the number of recipients returning to welfare every month did indeed show that more participants than controls returned in the early months (Figure 7-16).

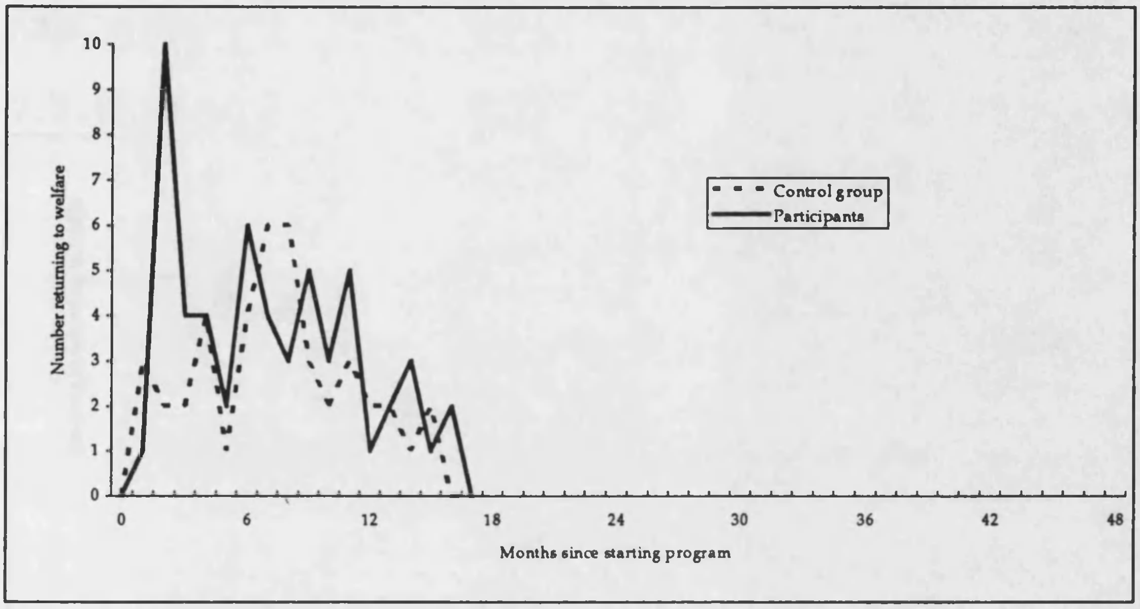

Figure 7-16 Number returning to welfare by month, Job Action participants and control group Data sources 1, 8

But in those months more participants had left and thus were available to return. If the return rate were the same for participants and controls, more participants would return in early months. To control for the different rates of leaving welfare, we looked at the proportion of recipients who returned to welfare by the number of months since they left welfare (Figure 7-17). This measure reversed the preliminary finding that more participants than controls returned to welfare; it showed that, on the contrary, members of the control group were more likely to return.

7.3.6 Finding Employment

7.3.6.1 Conclusion

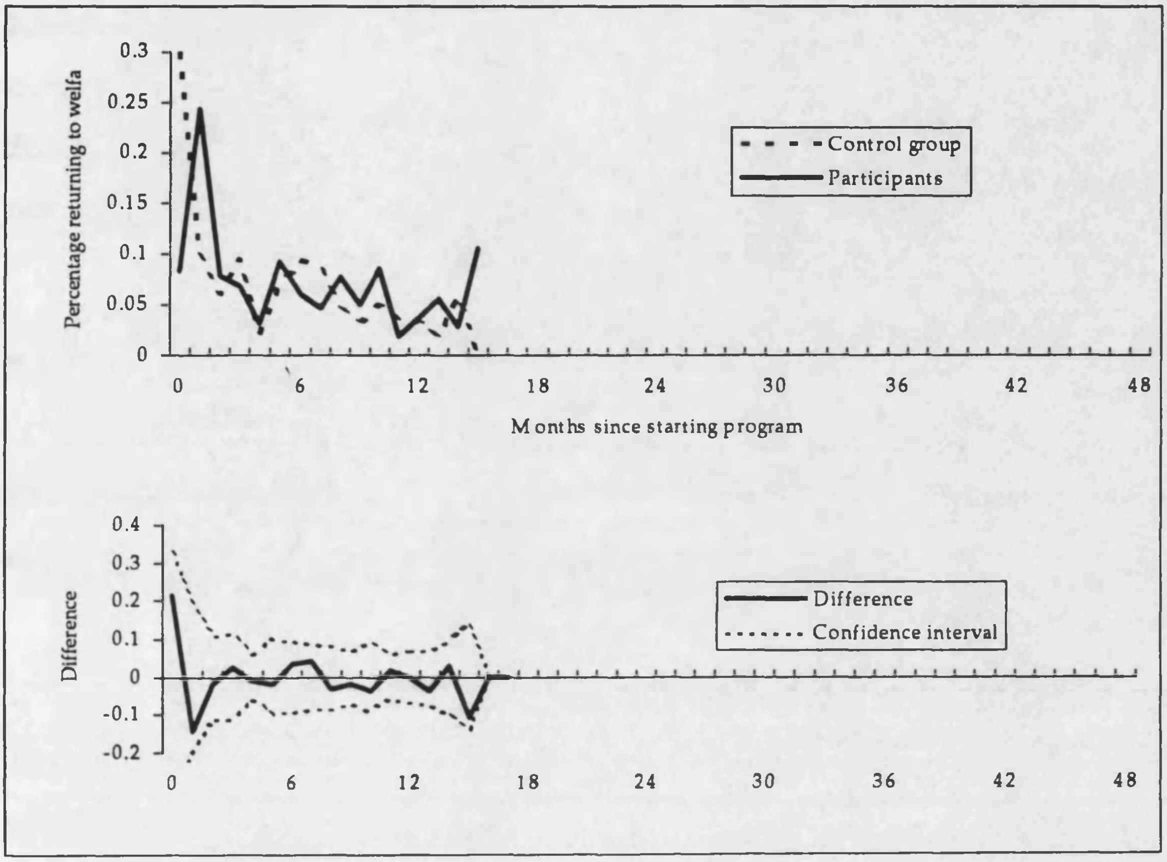

Figure 7-17 Percent returning to welfare by number of months since leaving
Data sources 1, 8

The survey gave no evidence that Job Action increased employment among participants six months after they enrolled in the program. Reduced welfare dependence, described below, suggests indirectly that there may have been a positive effect.

7.3.6.2 Discussion

The survey, which occurred six months after the program began, did not show a statistically significant difference between percentage of participants and the percentage of controls employed. At the six-month mark, 72 percent of participants and 70 percent of the control group had been employed at some point in the previous six months. After 15 months, 84 percent of participants and 83 percent of controls were employed.

There has been speculation that participants in the Job Action programs may accept lower wages than non-participants. The survey results did not support this conjecture. The average starting wage among the 38 participants responding was $8.04, while among the 28 controls responding it was $7.94. This difference too was not statistically significant.

7.3.7 Program expenditures and savings

7.3.7.1 Conclusion

Provided Job Action classes are well attended, the welfare savings exceed program expenditures by a wide margin.

7.3.7.2 Discussion

The savings and expenditures assessed were limited to those with clear, direct effects on the Ministry budget. The savings were reductions in welfare; indirect savings such as increased tax revenues were not considered. Similarly, the expenditures considered were program contract costs and recipient expenses, both borne by the Ministry; other costs, such as the other activities forgone to divert staff resources to refer recipients to Job Action, were not considered.

Welfare savings have been estimated at $367 per participant. A major appeal of random assignment studies is that estimates of the impact can be made simply by comparing the average values of the participants and the control group. It is possible to refine these estimates by the use of regression analysis, although the refinement carries a price in complexity and concomitant scepticism.

In the present case, the participants received on average $227 less than controls in the year following application. Regression analysis increases this estimate to $367. This difference demands an explanation.

Random assignment will result in similar average values of all characteristics of participants and controls, the averages being more alike the larger the sample size. In this study a few more recipients with children were assigned to the participant group than to the control group. Although the difference is not statistically significant, it has a substantial impact on the estimate. In general, recipients with children receive higher benefits. This means that if Job Action had no impact we would expect the participants to have higher benefits on average than the control group. Second, single parents leave welfare more slowly than any other group.

Turning to program costs, Job Action is delivered by contractors. Contract costs for the pilot project were set at $500 per attendee, with a guarantee of 15 attendees per class. Reimbursements for recipient expenses were set at $150 per recipient.

As noted earlier, in the pilot project only 51 of 125 referred participants actually took part in Job Action. This under-participation meant that in the pilot project classrooms the 15 seats were never full, so the true pilot project cost per attending participant was much higher than the minimum guaranteed, in fact about three times as high. In this respect the pilot project could be misleading. A more accurate picture of Job Action costs in actual practice would be given by assuming that classes were at least as large as the minimum guaranteed. Consequently the analysis assumed that the program costs would be at the contract level of $500 plus $150 in expenses, or $650, per participant.

In order to avoid selection bias, program participation was defined as being referred to the program, rather than attending classes. Costs, in contrast, will normally be determined by attendance, as the Ministry refers more people than there are spaces. Using the cost per participant of $650, total costs would have been $650 x 51 = $33,150. Thus the average cost per participant would have been $33,150 ÷ 125 = $265.20. This was well below the similarly averaged saving per participant of $367.

Obviously, if Job Action classes are full, the welfare savings exceed program expenditures by a wide margin. However, if class size falls more than about 20 percent below the guaranteed minimum level, expenditures begin to exceed welfare savings.

7.3.8 Comparison with other job club evaluations

7.3.8.1 Conclusion

Other studies indicate that job club programs at least have positive short-term effects. Some studies show persistent impacts.

7.3.8.2 Discussion

The results were compared with the results of random assignment evaluations of job club programs in three American jurisdictions: Louisville (Kentucky), San Diego (California), and Arkansas. There were differences among these programs in program content, recipient types and employability, economic climate, administrative procedures, and so forth. Even so, there was a broad consistency in the findings that such programs have a significant, if short-term, effect in increasing employment and reducing welfare dependence.

The Louisville study of 286 participants and 287 controls found in the first three months a large difference in employment (39 percent of participants and 25 percent of controls) though no difference in the percentage receiving welfare. [Wolfhagen 1983] The San Diego study of single-parent recipients (856 participants and 873 controls) and two-parent recipients (831 participants and 813 controls) found significant impacts on employment for three months and on welfare dependence for more than a year. [Goldman 1986] The Arkansas study of 554 participants and 565 controls found significant impacts on both employment and welfare over nine months. [Friedlander 1985]

7.4 Summary

This chapter reported estimates of the impact of three types of training program. On-the-job training in public projects had no measurable impact on the subsequent welfare dependence, or employment of participants. Classroom training had very different impacts by type of training. Adult basic education had no impact on the subsequent welfare dependence of participants. Vocational training had large impacts on subsequent welfare dependence. Academic training appeared to have a smaller, but still positive impact, but the sample was small, so the differences were not generally statistically significant. These findings are consistent with the disaggregated results produced in the evaluation of the Massachusetts ET Choices program, but provide more information than the existing work in Canada, which has not produced disaggregated estimates, and which generally concludes that classroom training is not effective. This chapter also reports the results of one of the first, if not *the* first, random assignment studies of an employment and training program in Canada. It found that participants left welfare faster, and contrary to expectations, did not return faster. The program had neither a wage effect, nor a long-term effect on welfare dependence.

8. Summary

This chapter provides a brief summary of the thesis, and describes the nature of the original contribution made by this thesis.

8.1 Summary of thesis

8.1.1 Chapter 1

Chapter 1 provided an introduction to the thesis and background information on the programs and the income support system in BC.

8.1.2 Chapter 2

Chapter 2 reviewed the experience in estimating the impacts of the American CETA programs. It reported two studies (LaLonde, 1986 and Fraker and Maynard, 1987) that found that methods used to estimate the CETA programs could produce the wrong answer when used to re-estimate the impacts of pilot projects that had true control groups. It reported a study (Bryant and Rupp 1987) that found generally positive and statistically significant impacts and a study (Dickinson, Johnson and West, 1986) that found generally negative and some statistically significant impacts from CETA programs, even though they made the estimates using the same data set. It reported the conclusion of the Job Training Longitudinal Survey Research Advisory Panel that labelled the source of the problem *selection bias*, and recommended that the US Department of Labor use random assignment to estimate the impacts of its employment and training programs.

8.1.3 Chapter 3

Chapter 3 reviewed recent developments in the estimation of the impacts of employment and training programs in the United States and Canada. It reported the fragility of random assignment estimates, and the problems of generalizability that have surfaced as a result of the greater use of random assignment over the past decade. It also reported the success of attempts to replicate the results of random assignment experiments using non-experimental methods. Friedlander and Robins found that non-experimental studies could not reliably replicate the results of experimental studies. In the best case, 87% of cross-site, within-state estimates produced the same statistical inference as the control group. Cain et al. drew a comparison group from screen-outs and drop-outs from a random assignment experiment and found estimates of selection bias of from 4.3% to 7.7% compared with a program impact of 13.9%. They conclude that the approach holds promise. Park et al. find that two stage methods do not remove selection bias as well as difference-in-difference estimators. They also find that a difference-in-difference estimator of the impact of training programs for UI recipients is robust to choice of base year when the comparison group is selected from individuals who are also receiving UI.

8.1.4 Chapter 4

Chapter 4 provided an overview of the BC Study. First it provided a conceptual framework. It was not possible to identify all participation in all alternatives to the programs under review, so some members of the comparison group will have received training through a different agency. As a result, the estimates of impact will be lower than if the counterfactual were "no activity".

Chapter 4 also described the ten sources of data used in the thesis:

1. administrative data on welfare benefits paid;
2. a survey of participants and non participants;
3. participant referral forms that identified participants and rejectees;
4. monthly claim forms that identified participants in on-the-job training programs;
5. a survey of RO's that identified a group of non participants who, in the opinion of the RO's were similar to participants in measured and unmeasured characteristics;
6. Records of Employment that provided histories of employment and earnings on a one-in-ten sample of the BC population;
7. administrative records of UI payments that provided histories of UI dependence both pre- and post program for a one-in-ten sample of the BC population;
8. the Job Action Pilot Project that identified a participant and control group of job club participants; and
9. classroom training records from Camosun College that identified welfare recipients who participated in classroom training and the courses that they took;
10. a group of individuals who by self identification and interview were identified as job ready.

Chapter 4 gave the results of three tests that affected the approach taken in the study. It reported that Heckman's two stage method for dealing with selection bias was very sensitive to distributional assumptions, and that this finding led the study away from arcane statistical techniques. It reported tests for undetected non linearities that led the study to rely more on matched comparison groups than on regression analysis. And third, it reported non response bias in the survey that changed the results qualitatively. This led the study to rely more on administrative data than on survey data.

Finally, Chapter 4 gave a preview of the estimates of the impact of a private sector wages subsidy program on subsequent welfare dependence.

8.1.5 Chapter 5

Chapter 5 addresses the issue of selection bias. The first section identifies and reports the results of five tests for selection bias, four of which do not require the use of random assignment. These tests illustrate the general principle that it is possible to test for selection bias in observational studies.

A number of results emerged from these tests. Pre-program tests for selection bias are not reliable. Absence of bias in one variable does not imply that estimates of the impact on different variables will be similarly unbiased. With regard to the specific programs and selection mechanism operating in BC, selection bias was less than 5 percentage points.

Chapter 5 also illustrated the danger of using annual data to estimate the impacts of employment and training programs for disadvantaged workers. It found that annual data would obscure the pre-program dip in earnings in almost 40% of the cases. A comparison group that matched the participants on all pre-program variables would not necessarily have experienced a dip in earnings in 40% of the cases, so their subsequent earnings are expected to be higher than the earnings of the participants. This is a clear source of selection bias that could easily have resulted in the variation in the estimates of the CETA programs.

Taken together, the results of the tests for selection bias in Chapter 5 indicate that selection bias in the estimates based on welfare data only amounts to less than five percentage points. Adding information on earnings and UI dependence, cuts this remaining bias in half.

8.1.6 Chapter 6

Chapter 6 reports the estimates of the impact of a wage subsidy program on subsequent welfare dependence, UI dependence, and employment. It finds that welfare dependence is decreased in both the short and long run, that UI dependence is increased, and that employment is increased 18 months following enrolment. The impacts of the program on subsequent welfare dependence are estimated for various subgroups. Chapter 6 reports higher savings for those with longer histories of welfare dependence, for categories with more dependent (one and two parent families) and for older applicants rather than younger ones. The impacts of the program did not vary dramatically with the unemployment rate.

8.1.7 Chapter 7

Chapter 7 reports the estimates of the impacts of three other types of program. The first type, on-the-job training in subsidised jobs that had a fixed term, had no impact on long term welfare dependence or employment. Chapter 7 reported the only disaggregated estimates of the impact of classroom training in Canada. It found no impact from upgrading courses (ABE) but significant impacts from vocational training. Chapter 7 also reported the results of the first random assignment study of an employment and training program in Canada. It found significant, but short-term impacts from a job club. It also found that it worked by speeding the departure of clients from welfare without speeding their return or inducing them to take lower paying jobs. Welfare savings exceeded program costs.

8.2 Nature of original contribution

The work reported in this thesis advances the study of employment and training programs in three areas: findings, data development, and methodology. Details follow under these three headings.

8.2.1 Findings

The work presented in this thesis advances our understanding of employment and training programs by generating estimates of the impact of programs for which no other estimate exists. In addition, three aspects of the findings advance our understanding of employment and training programs in general.

i) Disaggregated estimates: This study is the only one in Canada, and one of a very few in North America that presents disaggregated estimates of the impact of classroom training for disadvantaged workers. These results are extremely interesting and useful. Because different types of classroom training have very different impacts, estimates of the impact of classroom training, taken as a single treatment, can be misleading. The findings for the wage subsidy program with the private sector are disaggregated by age and marital status. This too is rare, and produces interesting findings. It is common to disaggregate by gender, but this study shows that the difference in impacts between single men and single women is small compared with the difference between single women and (93% female) single parents.

ii) *Long-term impacts:* In this thesis, impacts of programs are estimated for much longer periods than in other studies. For example, estimates of the "long- term" impact of the New Jersey on-the-job training program only went out two years (Freeman et al. 1988), and the cost benefit analysis required assumptions regarding the decay rate of the impacts of the programs. The results reported in this thesis extend beyond three years for the public sector on-the-job training programs, four years for the private sector on-the-job training programs and five years for the classroom training programs.

iii) Description of impacts: The findings also provide a much clearer exposition of the impact of programs than is found elsewhere. For example, Chapter 6 shows that only half of the participants in the wage subsidy program were independent of welfare in the first six months, suggesting that managers of this type of program should be concerned about drop out rates. Findings on all programs show that a high percentage of program participants would have become independent of welfare, even in the absence of the programs, dramatically illustrating the need for comparison groups in the study of employment and training programs for disadvantaged workers. In addition, in each case the extent of the impact varies dramatically from month to month. The results reported in this thesis extend our understanding of employment and training programs by revealing these variations.

8.2.2 Data development

i) Administrative data: This thesis reports work that is part of the exploration of the uses of administrative data on welfare receipt for research in Canada. Human Resources Development Canada (HRD) has a long history of linking administrative data on Unemployment Insurance, (UI) with tax data in order to estimate the impact of its employment and training programs, but it only gained access to welfare data in the late eighties[1]. Statistics Canada, in conjunction with the Economic Council of Canada, linked tax, welfare and UI data in order to complete a longitudinal study of low income individuals. The province of Ontario linked its welfare records in order to estimate impacts of employment and training programs in that province in the late eighties. This thesis reports the use of longitudinal data on welfare dependence to estimate the impact of employment and training programs. The work reported in this thesis was done concurrently with the work described above and information and results were shared among the groups.

ii) Random assignment: This thesis reports the results of the first use of random assignment to estimate the impact of an employment and training program in Canada, the Job Action Pilot Project of January 1989.

[1] BC was the first province to enter into an agreement to exchange welfare information for UI information for research purposes in September 1987.

iii) Other: This thesis also reports the use of other data. Ministry staff were contacted in order to get the names of individuals whom staff felt were suitable for participation in programs, but who had not participated. A sample of participants in programs was interviewed, and information on employment and earnings was collected.

8.2.3 Methodology

i) Selection bias: In my view the most important contribution of this thesis is in the development and illustration of tests for selection bias, and through them the exploration of the nature and causes of selection bias. Observational studies must grapple with the possibility of bias resulting from unknown (and possibly unknowable) characteristics that are correlated with program participation and the outcome of interest. The work reported in this thesis illustrates the types of additional data that can be used in order to shed additional light on the problem.

Some specific new findings that result from the application of these techniques are:

- tests for selection bias that are based on comparisons of pre-program characteristics are not reliable.
- estimates of the impact of programs that are based on annual data are not reliable.
- bias due to the presence of unmeasurable variables is small.

In summary, the work reported in this thesis advances the study of estimating the impacts of employment and training programs through the development of data, through the development of new methods of analysing the data, and through the production of new and more detailed information on the effectiveness of employment and training programs.

8.3 Conclusions

This thesis has shown that the number of things that can introduce bias into the estimates of employment and training programs is striking. It shows the importance of questions like, "What was the response rate in the survey used?" "What relevant variables have been omitted?" "How accurately have the variables been measured?" and "What are the small sample properties of the estimator used?" Answers to these questions are seldom published with studies, but even if studies did report the answers to these questions, there are no guidelines for acceptable answers. The work in this thesis provides a starting place for the development of guidelines. It suggests that observational studies that are based on one stage techniques (OLS or matching) may not be reliable if any of the following three conditions are not met.

1) Participants in a program should be compared to non participants who were eligible for the program. The fact that estimates of the impact of the CETA programs did not do this could account for much of the variability in those estimates.

2) Estimates should not be based on surveys that do not have an 80% response rate.[2]

3) When regression techniques are used, functional forms should be explicitly tested for non linearities.

[2] The response rate should be measured as the number responding divided by the number given to the survey research firm.

In most cases, these data, by themselves would not be adequate to address specific questions, and additional survey work would have to be done. However, these data would provide characteristics on the non respondents that would make it easier to test for non response bias. Overlap in information on some variables would give indication of the errors in measurement of variables in both data sets. Further, the administrative data could reduce the length of the survey instrument, thereby saving money.

In addition there is a need to conduct occasional random assignment studies. Where random assignment is used, the estimates of impact have a sound mathematical foundation. A valid test of observational techniques would be to produce estimates of the impact of random assignment studies in advance of those produced using the control group. This external validation of techniques is important to reassure policy makers.

As an interim and much less expensive step, estimates of the extent of selection bias should be made in the manner illustrated in chapter 5. Non participants who have been through the selection process should be identified and the impact of selection estimated for them. Where these impacts show up different from zero, researchers have an indication of omitted variables.

8.3.2 Potential directions for further research.

There is a tremendous need for research in estimating impacts of programs. I discuss a number of them below, proceeding from the specific to the general. First, extensions of the work presented in this thesis, second, research needed on the development of standards, third other research that is possible using administrative data and finally, the development of new techniques for estimating the impacts of programs.

There are many extensions to the work reported in this thesis. First, the impacts reported in this thesis are the first estimates of the impacts of most of these programs. Disappointing impacts may result from years of neglect rather than inherent weaknesses in programs. In addition, the work of MDRC shows that impacts can vary dramatically across sites. For these reasons, it is important to produce disaggregated estimates of impact, in order to identify areas of success and then work to ensure that the approaches used in the successful sites are communicated to other sites.

There is also a need to broaden the scope of these studies. This thesis has estimated the impact of training on the trained. However, training can also have impacts on others. For example, individuals placed in employment through a training program may displace others. Garfinkel et al. (1992) also suggest that programs could have impacts on community standards. Also, this study has treated prior welfare dependence as a pre-determined variable. However, the existence of training may make the welfare package as a whole more attractive, and induce individuals to apply. A complete accounting of the costs and benefits of a program must include these factors.

Second, more work is needed in the development of standards. It is important to document the effects of controlling for various variables in different types of studies.

For example, it seems likely that controlling for prior academic achievement would be important in estimating the impact of classroom training programs. I have recently matched high school leaving records with post secondary training records for welfare recipients in BC (for whom I already have records of welfare dependence). The impact of including high school leaving grades on the estimates of the impact will give an indication of the importance of this variable.

In addition, we often conduct surveys of our clients. a comparison of the accuracy of predicted rates of leaving welfare using survey data and administrative data will give an indication of the relative advantages of using both over either separately.

Third, administrative data can be used to answer many more questions than those relating to the impact of employment and training programs. The large sample sizes and the accuracy of the audited data make it suitable for application to a wide range of questions. For example, administrative data can be used to estimate the impacts of granting welfare itself. Many thousands of individuals apply for welfare and are turned down. The application of the rules involves some judgement, and the rate at which applications are granted varies from office to office. This suggests that an office's rate of granting welfare could be used to form an instrument for receipt of welfare that is independent of unmeasured personal characteristics. This instrument could then be used to measure the impact of granting welfare on subsequent welfare dependence, health, income or criminality.

Finally, more work is needed on general methods for estimating the impact of programs. The monte carlo studies in chapter 3a suggest that very large samples are needed to use these new estimators, but administrative data can provide these very large samples. Nonetheless, there is a need to understand the finite sample properties of these estimators before they are used as a basis for policy.

Despite the very large number of studies, we have very little guidance to offer those who wish to make estimates of the impact of training programs or to those who wish to make policy decisions regarding them. The work in this thesis suggests that much progress can be made through a more basic approach with an emphasis on data collection.

Appendix A: Details Of Matching

Comparison group A was produced in the following manner. Records for each individual in the Province were linked to produce longitudinal records of welfare dependence. These records were divided into those of participants and those of non participants. The characteristics of each non participant was analysed in each month to determine age, sex marital status, employability, whether they received welfare in that month and history of welfare dependence to that point. These characteristics determined which of the 350 cells they belonged to in that month. If we were looking at program participants who enrolled over a two year period, we would have a total of 350 * 24 = 8,400 cells. If the non participant had received welfare in that month, then their subsequent welfare dependence was recorded for each subsequent month. Finally, the subsequent welfare dependence was divided by the number of non participants in that cell.

Estimates of impact were made by comparing the subsequent welfare dependence of participants with the average subsequent welfare dependence of the non participants in the same cell. Where n participants were in the same cell, the average for that cell would be multiplied by n. The result of this is that the subsequent welfare dependence of each non participant was weighted by the ratio of participants to non participants in that cell.

Comparison group A generated comparison groups for EOP, CTETP, FEP and classroom training with one pass through the non participant data set.

Comparison group B was produced only for the participants in EOP and the interview (data source 10) whose SIN's ended in '5'. Non participants in this comparison group matched participants on a number of variables reflecting UI dependence and earnings (criteria specified on page 222 and following) as well as the welfare dependence and demographic characteristics in comparison group A. This made the number of potential cells much larger, and so the computer programming had to change although the concept remained the same.

In a similar fashion, the welfare records, UI records and earnings records were linked to produce longitudinal records for each individual, and participants were separated from non participants. This time the characteristics of each non participant was examined for each month to see if it belonged to the same cell as a participant. If so, the average welfare dependence, UI dependence and earnings were retained and averaged. Again, the welfare dependence, UI dependence and earnings of the comparison group were weighted by the ratio of participants to non participants in that cell.

Introduction

This appendix describes the steps which were taken in order to develop the matched comparison group used in the paper. It contains five sections. The first describes the graphing of the matching variables. The second presents the results of a regression used to refine the matching criteria. The third describes the restrictions which resulted in the sample used, and the final section describes the computer programs which matched the participants with comparison group members.

The matching criteria were determined in three steps which are described in detail below. First the probability of receiving Income Assistance 6 months later is graphed against the variables which are available for the match. Second, the graphs are inspected in order to find groups which have roughly constant probabilities. Third, an equation is estimated using regression analysis in order to estimate the importance of each variable, given the values of the other variable for that individual.

A2. Graphing the variables.

The following seven graphs show the percentage of welfare recipients who are still receiving welfare 6 months later. The graphs were produced by data from the computer program PRONIA, attached.

- The ROE, UI and welfare records of all individuals who were comparable to the participants in terms of their welfare records were stripped to get a file which was of manageable size and which was comparable to the participants. This generated a file of 23,500 records.

- Then the relationship between welfare dependence and the variables available in the ROE and UI files was examined by calculating the percentage still receiving welfare six months later by each variable. The results are shown in the seven following graphs.

The graph on page 268 shows welfare dependence by annual earnings for those with very low earnings, less than $2,000 per year. The difference between the percentage and 50% together with twice the standard deviation is reported on this graph. The difference only exceeds twice the standard deviation at two points, zero earnings and $600 per year. The difference at $600 per year is ignored since it is not part of a trend (the difference at $500 and $700 is in the other direction) and no such difference exists for earnings of $600 in the prior year.

This graph suggests that the data should be split into those with no earnings, and those with positive earnings.

The graph on page 269 shows welfare dependence by annual earnings in $1,000 increments. In this case the graphs do suggest a division based on annual earnings:

1) those with no earnings;
2) those with incomes below $4,000;
3) those with incomes below $9,000;
4) those with incomes below $13,000;
5) those with incomes above $13,000.

The observations with incomes between $13,000 and $14,000 and between $17,000 and $18,000 are not given their own groups because the sample sizes are small. A Chi-squared statistic for the groups from $12,001 to $13,000 per year to $20,000 and above is 7.99. The probability of obtaining a statistic at least that big is .33.

The graph on page 266 shows welfare dependence by the largest number of insurable weeks reported on any ROE issued within the four months before the reference month. This variable was created in order to separate those who may have had an Unemployment Insurance claim pending while receiving welfare.

The graph on page 267 reproduces the previous graph for four income groups:

1) any income level,

2) income at least $50 per week,

3) income greater than welfare entitlement, and

4) income greater than 1.6 times welfare entitlement.

Those with earnings lower than $50 per week were no more likely to leave than those without recent records of employment. Those with earnings in the higher categories were correspondingly more likely to leave. (Income more than 1.6 times their entitlement, implies that their UI benefits would be greater than their welfare entitlement.)

The divisions selected are:

1) those with no insurable weeks;
2) those with 1 to 9 insurable weeks and earnings greater than 1.6 times welfare entitlement;
3) those with 1 to 9 insurable weeks and earnings greater than their welfare entitlement, but less than 1.6 times welfare entitlement;
4) those with more than 9 insurable weeks and earnings greater than 1.6 times welfare entitlement;
5) those with more than 9 insurable weeks and earnings greater than their welfare entitlement, but less than 1.6 times welfare entitlement;
6) those with earnings less than their welfare entitlement.

In calculating insurable weeks, those with maximum earnings less than $50 per week were ignored.

Ten weeks is the smallest number which entitles an individual to Unemployment Insurance. Again sample sizes are small for most groups, more than 60% of the sample falls into the first group.

The graph of welfare dependence by number of months of UI dependence again shows a marked difference between those with no history of Unemployment Insurance dependence and those with any. In addition, those with one, two and three months of UI benefits in the previous twelve months appear to have a lower probability of being dependent on welfare six months later. The divisions selected are:

1) those with no UI dependence;
2) those with 1 month of UI dependence;
3) those with 2 or 3 months of UI dependence;
4) those with more than 3 months of UI dependence.

The graph of welfare dependence by number of months since last UI dependence (page 263) again shows a marked difference between those with no history of Unemployment Insurance dependence and those with any. It also shows that those who had been dependent on Unemployment Insurance one, two or three months prior to the reference month had a lower probability of being dependent on welfare six months later. The divisions selected are:

1) those with no UI dependence;
2) those who had been dependent on UI in the previous month and whose UI benefits were higher than their welfare entitlement;
3) those who had been dependent on UI either 2 or 3 months before the reference month and whose UI benefits were higher than their welfare entitlement;
4) those who had been dependent on UI more than 3 months before the reference month.

The graph onpage 264 shows welfare dependence by difference in annual earnings. Clearly, those whose earnings did not change in the two previous years are different from the rest. As well, those whose earnings grew are different from those whose earnings fell. The divisions selected are:

1) those whose earnings declined or did not change;
2) those whose earnings grew;

A3. Regression analysis

These graphs cannot reveal inter-relationships among the variables. For example, the number of months since last dependent on U and the number of month of UI dependence in the previous year might be highly correlated, and so it might not be appropriate to divide the sample across both variables.

Variables in the regression are defined as follows:

EARN1	Earnings in the year preceding the reference month.
DEARN	A dummy variable taking the value 1 if EARN1 = 0.
DEARN1	A dummy variable taking the value 1 if EARN1 > 0 but < 5000.
EARN2	Earnings in the year 13 to 24 months preceding the reference month.
DEARN2	A dummy variable taking the value 1 if EARN2 = 0.
UI	Number of months since the individual received UI benefits. Takes the value 0 if UI benefits never received.
UIMISS	A dummy variable taking the value 1 if UI = 0.
DUI	A dummy variable taking the value 1 if UI

DUI	A dummy variable taking the value 1 if UI = 1.
DUI2	A dummy variable taking the value 1 if UI = 2 or 3.
CUI1	Number of months in which UI benefits were received in the year preceding the reference month.
DCUI	A dummy variable taking the value 1 if CUI1 = 1.
CUI2	Number of months in which UI benefits were received in the year 13 to 24 months preceding the reference month.
DCUI2	A dummy variable taking the value 1 if CUI1 = 2 or 3.
WEEKS	The number of UI insurable weeks reported on a Record of Employment issued up to four months prior to the reference month.
WKMISS	A dummy variable taking the value 1 if WEEKS = 0.
WKLOW	A dummy variable taking the value 1 if WEEKS > 0 but < 9.
DDIFF	Takes the value EARN1 - EARN2 if EARN1 - EARN2 > 0; zero otherwise.

The additional information which we get from the regression results are:

- Earnings in the year 13 to 24 months preceding the reference month are an important predictor of subsequent welfare dependence, even when several variables reflecting prior year earnings are included.

- Growth in earnings is an important predictor.

- UI dependence in the year 13 to 24 months preceding the reference month is not an important predictor of subsequent welfare dependence when more recent UI experience is included.

As a result, the final criteria for matching are:

Earnings, both for months 1 to 12 before reference month and for months 13 to 24 before the month in which the participant entered program:

- if participant's earnings are zero then comparison group member's earnings must also be zero; otherwise
- if participant's earnings are less than $4,000 then comparison group member's earnings must also be less than $4,000; or
- comparison group members earnings must be within 40% of the participant's earnings.

Number of **weeks of insurable earnings** reported up to four months prior to the month in which the participant entered program:

- if the participant has no weeks of insurable earnings then the comparison group member must not have any weeks of insurable earnings;otherwise
- if the participant has fewer than 10 weeks of insurable earnings and earnings greater than 1.6 times welfare entitlement, then so too must the member of the comparison group;otherwise
- if the participant has fewer than 10 weeks of insurable earnings and earnings

greater than his/her welfare entitlement, then so too must the member of the comparison group;otherwise

- if the participant has more than 10 weeks of insurable earnings and earnings greater than 1.6 times welfare entitlement, then so too must the member of the comparison group;otherwise
- if the participant has more than 10 weeks of insurable earnings and earnings greater than his welfare entitlement, then so too must the member of the comparison group.
- If none of these conditions are met, but the participant has insurable earnings then so too must the member of the comparison group.

History of **UI dependence**

- If the participant had received no UI benefits in the 12 months prior to enrolment in the program, then the comparison group member must not have received any UI either; otherwise
- If the participant had received UI benefits in one of the 12 months prior to enrolment in the program, then the comparison group member must have received UI in exactly one month too; otherwise
- the number of months in which the comparison group member received UI benefits must be within 3 of the number of months in which the participant received UI benefits.

Time since receiving UI benefits

- If the participant had UI benefits in the month prior to enrolment in the program, then so too must a comparison group member; otherwise
- If the participant had UI benefits in the two or three months prior to enrolment in the program, then so too must a comparison group member.

Change in earnings

- If the participant's earnings had grown in the year over the year before that, then so too must the comparison group members. Further the extent of the growth must be within 40% of the growth experienced by the comparison group member.

A4. The Sample

This paper uses information on the Employment Opportunity Program in its analysis of selection bias. Enrolment data was taken from copy 3 of the Monthly Claim form (copy attached). The Social Insurance Number (field 8) is used for identification. The enrolment date is defined as the first *from* date (field 6) for that individual. No other fields were used in this analysis.

Participants in the program need not have been receiving Basic Income Assistance. They might have been receiving Supplementary benefits (for the handicapped and the aged), or services relating to child protection. Entry and editing errors undoubtedly occurred. This analysis is restricted to those participants who had valid Social Insurance Numbers (SIN's) and who received Basic Income Assistance benefits within one month of the enrolment date, defined as above.

Where the participant did not receive benefits in the month of enrolment, the enrolment date was adjusted by up to one month. Bad data is thought to be the main reason that an individual would not receive benefits in the month containing the enrolment date. A Monthly Claim form, on which the SIN or *from* date is missing or illegible, or which is missing altogether could make the enrolment date appear later than the true enrolment date.

This left 521 individuals in the sample, 56 (slightly more than 10%) of whom had had their enrolment date adjusted. 24 had been excluded because they were not on Basic Income Assistance, and another 111 had been excluded because they were not on any type of Income Assistance within 1 month of the enrolment date. Although the 111 were excluded from the participant group in this analysis, they were also excluded from the comparison group since they may have been participants from welfare, but some of their Claim forms didn't make it from the Accounting Department to research branch. (Another 29 would have been included if we had adjusted enrolment dates by 2 months.)

For efficiency, the welfare records, Records of Employment and Unemployment Insurance records are stripped into smaller files. Because this process is very straightforward, the programs which do this are not reproduced here.

A5. Identifying the comparison group

From a computer programming perspective, the challenge in identifying and stripping the records of comparison group members is to avoid making the same calculation twice. The straightforward approach, going through the file of participants, comparing each to each potential comparison group member, writing out the records of the comparison group members when they were sufficiently similar to the participant can result in many unnecessary calculations.

The alternative, used here, has the disadvantage of being somewhat more difficult to follow, but eliminates many unnecessary calculations. Duplicate calculations are reduced by:

a) calculating the characteristics of the participants in advance;
b) sorting the participant file by start date;
c) only checking comparability of subsequent variables if the potential comparison group member were similar on variables already checked.

The first matching criterion checked is that comparison group members must be on Basic Income Assistance in the month that contains the participant's enrolment date.

The program which calculates the characteristics of the participants, PPDPART6 is attached.

The program PPDCOMP6 (attached):

- calculates the characteristics of the comparison group members
- checks to see if they match any of the participants
- calculates and writes out average values of the variables of interest for 97 months, 60 months prior to the participants enrolment date and 36 months after. The variables of interest are UI dependence, IA dependence and earnings.
- writes out the SIN's of the comparison group members and a record of the participants they matched.

The criteria listed above generated matches for all but 7 of the 521 participants. All had exact matches on all demographic, welfare and UI variables but had inexact matches with earnings variables.

The program found 5,004 distinct matches for the 521 participants.

The output of this program is combined with information on the participants in the program PPD6. We are now in a position to look at four outcomes of interest:

- the percentage dependent on welfare;
- the percentage dependent on Unemployment Insurance;
- the percentage dependent on either welfare or Unemployment Insurance; and
- average earnings.

PPD6, attached, calculates the values of each of these variables for both the participants and the comparison group, together with the variance of the difference. In addition it calculates the variance of the sum of the difference for the first and last twelve months.

The results from PPD6 are imported into a spreadsheet and graphed.

000227

PL1 Program
PPDPART6

```
MAIN: PROC OPTIONS(MAIN);
DCL   EOF                          BIT INIT('0'B),
      SYSPRINT                     FILE OUTPUT,
      PGM(169)     FLOAT(6) INIT((169)0),
      PNULL(169)   FIXED DEC(9) INIT((169)0),
      DNULL(97)    FIXED DEC(9) INIT((97)0),
      COUNT(0:9)    FIXED DEC(9) INIT((10)0),
      COUNT1       FIXED DEC(9) INIT(0),
      COUNT2       FIXED DEC(9) INIT(0),
      COUNT3       FIXED DEC(9) INIT(0),
      COUNT4       FIXED DEC(9) INIT(0),
      COUNT5       FIXED DEC(9) INIT(0),
      COUNT6       FIXED DEC(9) INIT(0),
      SUM          FIXED DEC(9) INIT(0),
      AGE          FIXED DEC(9) INIT(0),
      I            FIXED DEC(9) INIT(0),
      TMPWKS       FIXED DEC(9) INIT(0),
      WEEKS        FIXED DEC(9) INIT(0),
      MAXEARN      FLOAT(6),
      ENT          FLOAT(6),
      DP           FLOAT(6),

      COMPIN                         FILE RECORD INPUT,
      ROEIN                          FILE RECORD INPUT,
      UIIN                           FILE RECORD INPUT,
      PARTIN                         FILE RECORD INPUT,
      IAIN                           FILE RECORD INPUT,
      OUTFILE                        FILE RECORD OUTPUT,
      OUTCNT                         FILE RECORD OUTPUT,

   1 PART,
     3 SIN        CHAR(8),
     3 SIN2       CHAR(1),
     3 BRDATE     PIC'9999',
     3 SEX        CHAR(1),
     3 CAT        PIC'99',
     3 CLASS      CHAR(2),
     3 IAHIST     PIC'99',
     3 FYY       PIC'99',
     3 FMM       PIC'99',
     3 PGM        CHAR(1),

     1 OUT,
       2 SIN      CHAR(8),
       2 CAT      PIC'9',
       2 CLASS    PIC'9',
       2 A        PIC'9',
       2 P        PIC'9',
       2 UI1      PIC'99',
       2 UI2      PIC'99',
       2 START    PIC'999',
       2 NUM      PIC'999' INIT(0),
       2 UI3      PIC'9',
       2 WKS      PIC'9',
```

```
    2 EARN1    FLOAT(6),
    2 EARN2    FLOAT(6),

  1 ROE,
      2  SIN               CHAR(8),
      2  SIN2              PIC'(1)9',
      2  MONTH(168),
         3  ROESTAT        CHAR(1),
         3  INWK1          CHAR(2),
         3  INEARN1        CHAR(5),
         3  PCODE11        CHAR(1),
         3  PCODE12        CHAR(5),
         3  EMPL1          CHAR(9),
         3  SIC1           CHAR(4),
         3  REASON1       CHAR(1),
         3  INWK2         CHAR(2),
         3  INEARN2       CHAR(5),
         3  PCODE21       CHAR(1),
         3  PCODE22       CHAR(5),
         3  EMPL2         CHAR(9),
         3  SIC2          CHAR(4),
         3  REASON2       CHAR(1),

1 UI,
    2  SIN                CHAR(8),
    2  SIN2               PIC'(1)9',
    2  AGE                 CHAR(2),
    2  SEX                 CHAR(1),
    2  MONTH(168),
       3  BEN              PIC'9',
       3  DIST             CHAR(4),
       3  PROV             CHAR(2),
       3  REG              CHAR(2),
       3  DPND             CHAR(1),
       3  BRATE            CHAR(4),
       3  STUD             CHAR(1),
       3  INWK             CHAR(2),
       3  OCC              CHAR(7),
       3  SIC              CHAR(4),
       3  WKPD             CHAR(3),

1 IA,
 2 FID1        CHAR(2),
 2 FID2        PIC'99999999',
 2 SIN         CHAR(8),
 2 SIN2               PIC'(1)9',
 2 BYR         PIC'99',
 2 BMO         PIC'99',
 2 SEX         CHAR(1),
 2 TMP(169)    PIC'9',
 2 CAT(169)    PIC'9',
```

```
2 CLASS(169)  PIC'9',
2 DPNO(169)   PIC'9';


ON ENDFILE(IAIN) IA.SIN = '99999999';
ON ENDFILE(PARTIN) EOF = '1'B;
ON ENDFILE(ROEIN) BEGIN;ROE.SIN = '99999999';
PUT SKIP EDIT('END OF ROE FILE')(A);END;
ON ENDFILE(UIIN) BEGIN;UI.SIN = '99999999';
PUT SKIP EDIT('END OF UI FILE')(A);END;
READ FILE(ROEIN) INTO(ROE);
READ FILE(IAIN) INTO(IA);
READ FILE(UIIN) INTO(UI);
DO I = 1 TO 8; /* BYPASS THOSE WITH INVALID SINS  */
READ FILE(PARTIN) INTO(PART);
END;
NUM = 1;

DO WHILE(¬EOF);
  IF ROE.SIN < PART.SIN THEN
  READ FILE(ROEIN) INTO(ROE);
  ELSE IF IA.SIN < PART.SIN THEN
  READ FILE(IAIN) INTO(IA);
  ELSE IF UI.SIN < PART.SIN THEN
  READ FILE(UIIN) INTO(UI);

  ELSE IF PART.PGM  = '2' | PART.PGM = '3'
  | PART.PGM = 'A' THEN BEGIN;
                          /* PUT IA RECORDS INTO SAME
                             ORDER AS ROE'S AND UI  */
    IF IA.SIN = PART.SIN THEN
    DO I = 1 TO 169;
      PGM(170-I) = IA.TMP(I);
    END;
    ELSE PGM = PNULL;

    START = (PART.FYY-80)*12 + PART.FMM;
                                      /* ADJUST START DATES
                                         AND COUNT THOSE ADJUSTED
*/
    IF PGM(START) = 0 THEN
    COUNT1 = COUNT1 + 1; ELSE
    IF PGM(START) = 3 THEN
    COUNT2 = COUNT2 + 1; ELSE
    COUNT3 = COUNT3 + 1;
    IF PGM(START) = 0 THEN START = START-1;
    IF PGM(START) = 0 THEN START = START+2;
    IF PGM(START) = 0 THEN
    COUNT4 = COUNT4 + 1; ELSE
    IF PGM(START) = 3 THEN
    COUNT5 = COUNT5 + 1; ELSE
    COUNT6 = COUNT6 + 1;
    IF START < 60 | START > 133 THEN BEGIN;
      PUT SKIP EDIT(START)(F(11));
```

```
    PUT SKIP EDIT(PART.FYY)(A);
    PGM(START) = 0;
  END;

  IF PGM(START) = 3 THEN BEGIN;
    OUT.SIN = PART.SIN;
                          /*  CALCULATE VALUES OF WELFARE
                              MATCHING VARIABLES FOR PARTICIPANTS
*/
    OUT.CAT = IA.CAT(170-START);
    DP = IA.DPNO(170-START);
    OUT.CLASS = IA.CLASS(170-START);
    IF OUT.CAT < 3 THEN ENT = 400; ELSE
    ENT = 500+ 100* DP;
    AGE = (PART.FYY-BYR)*12 + PART.FMM-BMO;
    IF AGE < 276 THEN A = 1;
    ELSE IF AGE <= 360 THEN A = 2;
    ELSE IF AGE <= 400 THEN A = 3;
    ELSE IF AGE <= 600 THEN A = 4;
    ELSE A = 5;
    SUM = 0;
    DO I = START - 25 TO START - 1;
      IF PGM(I) > 0 THEN SUM = SUM + 1;
    END;
    IF SUM < 1 THEN P = 1;
    ELSE IF SUM <  3 THEN P = 2;
    ELSE IF SUM <  8 THEN P = 3;
    ELSE IF SUM < 13 THEN P = 4;
    ELSE IF SUM < 23 THEN P = 5;
    ELSE IF SUM < 25 THEN P = 6;
    ELSE P = 7;
                     /*  CALCULATE VALUES OF UNEMPLOYMENT
INSURANCE
                                  MATCHING VARIABLES FOR
PARTICIPANTS */
    UI1,UI2,UI3 = 0;
    IF UI.SIN = PART.SIN THEN BEGIN;
      DO I = START-12 TO START - 1;
        IF UI.BRATE(I) > ENT THEN UI1 = UI1+1;
        IF UI.BRATE(I) > 0 THEN UI2 = 1;
      END;
      IF UI.BRATE(START-1) > ENT THEN UI3 = 1; ELSE
      IF (UI.BRATE(START-2) > ENT
      | UI.BRATE(START-3) > ENT) THEN UI3 = 2; ELSE
      IF UI.BRATE(START-1) > 0 |
       (UI.BRATE(START-2) > 0
      | UI.BRATE(START-3) > 0) THEN UI3 = 3;
    END;
                              /*  CALCULATE VALUES OF
                                MATCHING VARIABLES FROM ROE'S */
    TMPWKS,WEEKS,WKS = 0;
    EARN1,EARN2,MAXEARN = 0;
    IF ROE.SIN = PART.SIN THEN BEGIN;
      DO I = START-3 TO START - 1;
```

```
          IF INEARN1(I) ¬='*****' & INWK1(I) ¬= '**'
          & INWK1(I) ¬= '00' & INEARN1(I)/INWK1(I) > MAXEARN
          THEN MAXEARN = INEARN1(I)/INWK1(I);
          IF INWK1(I) ¬= '**' THEN TMPWKS = INWK1(I);
          IF TMPWKS > WEEKS THEN WEEKS = TMPWKS;
          IF INWK2(I) ¬= '**' THEN TMPWKS = INWK2(I);
          IF TMPWKS > WEEKS THEN WEEKS = TMPWKS;
          IF INEARN2(I) ¬='*****' & INWK2(I) ¬= '**'
          & INWK2(I) ¬= '00' & INEARN2(I)/INWK2(I) > MAXEARN
          THEN MAXEARN = INEARN2(I)/INWK2(I);
        END;
        IF MAXEARN < 50 THEN WEEKS = 0;
        IF WEEKS = 0 THEN WKS = 0; ELSE
        IF MAXEARN > ENT/2.6 & WEEKS < 10 THEN WKS = 1; ELSE
        IF MAXEARN > ENT/4.3 & WEEKS < 10 THEN WKS = 2; ELSE
        IF MAXEARN > ENT/2.6 THEN WKS = 3; ELSE
        IF MAXEARN > ENT/4.3 THEN WKS = 4; ELSE WKS = 5;
        DO I = START-12 TO START - 1;
          IF INEARN1(I) ¬='*****' & INWK1(I) ¬= '**'
          & INWK1(I) ¬= '00'
          THEN EARN1 = EARN1 + INEARN1(I)/INWK1(I);
          ELSE IF INEARN2(I) ¬='*****' &
          INWK2(I) ¬= '**' & INWK2(I) ¬= '00'
          THEN EARN1 = EARN1 + INEARN2(I)/INWK2(I);
        END;
        DO I = START-24 TO START - 13;
          IF INEARN1(I) ¬='*****' & INWK1(I) ¬= '**'
          & INWK1(I) ¬= '00'
          THEN EARN2 = EARN2 + INEARN1(I)/INWK1(I);
          ELSE IF INEARN2(I) ¬='*****' &
          INWK2(I) ¬= '**' & INWK2(I) ¬= '00'
          THEN EARN2 = EARN2 + INEARN2(I)/INWK2(I);
        END;
      END; /* ROE.SIN = PART.SIN  */
      COUNT(WKS) = COUNT(WKS) + 1;
      START = 170 - START;
     WRITE FILE(OUTFILE) FROM(OUT);
      NUM = NUM + 1;
    END;  /* PARTICIPANT ON BASIC IA IN START MONTH  */
    READ FILE(PARTIN) INTO(PART);
  END;  /*  ALL SINS >= PART.SIN & WAGE SUBSIDY PROGRAM */
  ELSE  READ FILE(PARTIN) INTO(PART);
 END; /*NOT EOF*/
 PUT SKIP EDIT('NO BENEFITS IN START MONTH: ',COUNT1)(A);
 PUT SKIP EDIT('ON BASIC IN START MONTH: ',COUNT2)(A);
 PUT SKIP EDIT('BENEFITS, BUT NOT BASIC: ',COUNT3)(A);
 PUT SKIP EDIT('AFTER ADJUSTMENT,')(A);
 PUT SKIP EDIT('NO BENEFITS IN START MONTH: ',COUNT4)(A);
 PUT SKIP EDIT('ON BASIC IN START MONTH: ',COUNT5)(A);
 PUT SKIP EDIT('BENEFITS, BUT NOT BASIC: ',COUNT6)(A);
 DO I = 0 TO 09;
   PUT SKIP EDIT(COUNT(I))(F(11));
 END;
END MAIN;
```

```
/*
//GO.ROEIN  DD DSN=HRRSD.E1.ROE5.MAY1194,DISP=SHR
//UIIN    DD DSN=HRRSD.E1.UI.PPDPART.JUN0194,DISP=SHR
//IAIN     DD  DSN=HRRSD.E1.IA.PPDPART.JUN0194,DISP=SHR
//PARTIN   DD DSN=HRRSD.E1.PART5.APR0892,DISP=SHR
//*OMPIN   DD DSN=HRRSD.E1.PPD.PERCENT.JUN0294,DISP=SHR
//OUTFILE  DD DSN=HRRSD.E1.PPDPART.SHORT6.OCT0794,UNIT=DISK,
//            DISP=OLD
//*        DISP=(NEW,CATLG,KEEP),
//*        DCB=(RECFM=FB,LRECL=32),
//*       SPACE=(TRK,(2,2),RLSE)
//*GO.INFILE DD DUMMY
//*OUTFILE DD DUMMY
```

000234

PL1 Program
PPDCOMP6

000235

```
MAIN: PROC OPTIONS(MAIN);
DCL     EOF                            BIT INIT('0'B),
        F1                             BIT INIT('0'B),
        F3                             BIT INIT('0'B),
        OK                             BIT INIT('0'B),
        FOUND                          BIT INIT('0'B),
        MATCH                          BIT INIT('0'B),
        SYSPRINT                       FILE OUTPUT,
        MINSIN   PIC'(9)9',

           SMO(560)   FIXED DEC(5) INIT((560)0),
           IAIND(521,97)        FLOAT(6) INIT((521*97)0),
           UIIND(521,97)        FLOAT(6) INIT((521*97)0),
           NOIND(521,97)        FLOAT(6) INIT((521*97)0),
           EARIND(521,97)        FLOAT(6) INIT((521*97)0),
           TOT(521)         FLOAT(6) INIT((521)0),
           NMRK(521)        FLOAT(6) INIT((521)0),
           DIND(97)         FLOAT(6)  INIT((97)0),
           COUNT(521,10)        FLOAT(6)  INIT((5210)0),
           COUNT2(0:09)     FLOAT(6)  INIT((10)0),
           DNULL(97)        FIXED DEC(6) INIT((97)0),
           TOTNO(97)        FLOAT(6) INIT((97)0),
           TOTIA(97)        FLOAT(6) INIT((97)0),
           TOTUI(97)        FLOAT(6) INIT((97)0),
           TOTEAR(97)       FLOAT(6) INIT((97)0),
           TMPWKS      FIXED DEC(9),
           WEEKS       FIXED DEC(9),
           DIFF        FLOAT(6),
           CDIFF       FLOAT(6),
           ENT         FLOAT(6),
           LL          FLOAT(6),
           KK          FLOAT(6),
           TOL         FLOAT(6),
           CWKS        PIC'9',
           CEARN1      FLOAT(6),
           CEARN2      FLOAT(6),
           MAXEARN     FLOAT(6),

           AGE          FIXED DEC(9) INIT(0),
           SUM          FIXED DEC(9) INIT(0),
           STT          FIXED DEC(9) INIT(0),
           I            FIXED DEC(9) INIT(0),
           II           FIXED DEC(9) INIT(0),
           JJ           FIXED DEC(9) INIT(0),
           A            FIXED DEC(9) INIT(0),
           P            FIXED DEC(9) INIT(0),
           STNO         FIXED DEC(9) INIT(0),
           AG           FIXED DEC(9) INIT(0),
           CUI1         FIXED DEC(9) INIT(0),
           CUI2         FIXED DEC(9) INIT(0),
           CUI3         FIXED DEC(9) INIT(0),
           J            FIXED DEC(9) INIT(0),
           N            FIXED DEC(9) INIT(0),
           K            FIXED DEC(9) INIT(0),
```

000236

```
    S             FIXED DEC(9) INIT(0),
    MIN           BUILTIN,
    SUBSTR        BUILTIN,
    TRUNC         BUILTIN,

 ROEIN                              FILE RECORD INPUT,
 UIIN                              FILE RECORD INPUT,
 PARTIN                            FILE RECORD INPUT,
 IAIN                              FILE RECORD INPUT,
 OUTFILE                           FILE RECORD OUTPUT,
 OUTSIN                            FILE RECORD OUTPUT,
 OUTCNT                            FILE RECORD OUTPUT,

1 MARK,
  2 SIN     CHAR(8),
  2 MRK(521)      PIC'9',

1 PART(521),
  2 SIN     CHAR(8),
  2 CAT     PIC'9',
  2 CLASS   PIC'9',
  2 A       PIC'9',
  2 P       PIC'9',
  2 UI1     PIC'99',
  2 UI2     PIC'99',
  2 START   PIC'999',
  2 CNT     PIC'999' INIT(0),
  2 UI3     PIC'9',
  2 WKS     PIC'9',
  2 EARN1   FLOAT(6),
  2 EARN2   FLOAT(6),

   1 ROE,
     2  SIN                  CHAR(8),
     2  SIN2                 PIC'(1)9',
     2  MONTH(168),
        3  ROESTAT           CHAR(1),
        3  INWK1             CHAR(2),
        3  INEARN1           CHAR(5),
        3  PCODE11           CHAR(1),
        3  PCODE12           CHAR(5),
        3  EMPL1             CHAR(9),
        3  SIC1              CHAR(4),
        3  REASON1          CHAR(1),
        3  INWK2            CHAR(2),
        3  INEARN2          CHAR(5),
        3  PCODE21          CHAR(1),
        3  PCODE22          CHAR(5),
        3  EMPL2            CHAR(9),
        3  SIC2             CHAR(4),
        3  REASON2          CHAR(1),

 1 UI,
```

```
          2  SIN                    CHAR(8),
          2  SIN2                   PIC'(1)9',
          2  AGE                    CHAR(2),
          2  SEX                    CHAR(1),
          2  MONTH(168),
             3  BEN                 PIC'9',
             3  DIST                CHAR(4),
             3  PROV                CHAR(2),
             3  REG                 CHAR(2),
             3  DPND                CHAR(1),
             3  BRATE               CHAR(4),
             3  STUD                CHAR(1),
             3  INWK                CHAR(2),
             3  OCC                 CHAR(7),
             3  SIC                 CHAR(4),
             3  WKPD                CHAR(3),

 1 IA,
  2 FID1          CHAR(2),
  2 FID2          PIC'99999999',
  2 SIN           CHAR(8),
  2 SIN2          PIC'(1)9',
  2 BYR           PIC'99',
  2 BMO           PIC'99',
  2 SEX           CHAR(1),
  2 PGM(169)      PIC'9',
  2 CAT(169)      PIC'9',
  2 CLASS(169)    PIC'9',
  2 DPNO(169)     PIC'9';

  ON ENDFILE(IAIN)  EOF = '1'B;
  ON ENDFILE(ROEIN) BEGIN;ROE.SIN = '99999999';
  PUT SKIP EDIT('END OF ROE FILE')(A);END;
  ON ENDFILE(UIIN) BEGIN;UI.SIN = '99999999';
  PUT SKIP EDIT('END OF UI FILE')(A);END;
  READ FILE(ROEIN) INTO(ROE);
  DO UNTIL(IA.SIN > '00000001');
    READ FILE(IAIN) INTO(IA);
  END;
  READ FILE(UIIN) INTO(UI);
  READ FILE(PARTIN) INTO(PART(1));
                         /* READ PARTICIPANT FILE INTO MEMORY
                         SMO CONTAINS POSITIONS OF PARTICIPANTS
WITH
                         NEW START DATES.  VALUES RANGING FROM
1-522
                         STNO IS THE NUMBER OF DISTINCT START
DATES */
  SMO(1) = 1;STNO = 1;
  DO I = 2 TO 521;
    READ FILE(PARTIN) INTO(PART(I));
    IF PART(I).START > PART(I-1).START THEN BEGIN;
```

```
     STNO = STNO + 1;
     SMO(STNO) = I;
   END;
 END;
 SMO(STNO+1) = 522;


 DO WHILE(¬EOF);
 IF ROE.SIN < IA.SIN THEN
 READ FILE(ROEIN) INTO(ROE);
 ELSE IF UI.SIN < IA.SIN THEN
 READ FILE(UIIN) INTO(UI);

                    /* TOO MANY NESTED LOOPS TO INDENT  */

 ELSE BEGIN;

 DO II = 1 TO STNO;  /* CHECK EACH UNIQUE START DATE  */

                        /* START(SMO(II)) IS THE START MONTH
                           FOR PARTICIPANT IN POSITION SMO(II).
                           INDIVIDUALS CAN ONLY BE CONSIDERED
                           FOR INCLUSION IN THE COMPARISON GROUP
                           IF THEY RECEIVED BENEFITS THROUGH
                           BASIC IA (PGM = 3) IN THAT MONTH.  */

 IF PGM(START(SMO(II))) = 3 THEN

                        /* IF INDIVIDUAL RECEIVED BASIC IA
                           IN START MONTH, THEN SEE IF THEY
                           MATCH ANY PARTICIPANT WITH THAT
                           START MONTH  */

 DO JJ = SMO(II) TO SMO(II+1) -1;
 DO KK = START(JJ);

                        /* DOES INDIVIDUAL MATCH ON CLASS AND
CAT?*/

 IF IA.CLASS(KK) = PART(JJ).CLASS
 &  IA.CAT(KK) = PART(JJ).CAT  THEN BEGIN;

                           /* DOES INDIVIDUAL MATCH ON AGE? */

 AGE = (79-BYR)*12 + 13 - BMO + (170-KK);
 IF AGE < 276 THEN A = 1;
 ELSE IF AGE <= 360 THEN A = 2;
 ELSE IF AGE <= 400 THEN A = 3;
 ELSE IF AGE <= 600 THEN A = 4;
 ELSE A = 5;
 IF PART(JJ).A = A THEN BEGIN;

                /* DOES INDIVIDUAL MATCH ON HISTORY OF IA? */
```

```
SUM = 0;
DO I = KK+1 TO KK + 25;
IF IA.PGM(I) > 0 THEN SUM = SUM + 1;
END;
IF SUM < 1 THEN P = 1;
ELSE IF SUM <  3 THEN P = 2;
ELSE IF SUM <  8 THEN P = 3;
ELSE IF SUM < 13 THEN P = 4;
ELSE IF SUM < 23 THEN P = 5;
ELSE IF SUM < 25 THEN P = 6;
ELSE P = 7;
IF P = PART(JJ).P THEN BEGIN;
   COUNT(CNT(JJ),1) = COUNT(CNT(JJ),1) + 1;

                  /* NOW CHECK THE UI VARIABLES. */

IF IA.CAT(KK) < 3 THEN ENT = 400;
ELSE ENT = 500 + 100 * IA.DPNO(KK);

STT = 170-KK;
CUI1,CUI2,CUI3 = 0;
IF UI.SIN = IA.SIN THEN
DO I = STT-12 TO STT - 1;
  IF UI.BRATE(I) > ENT THEN CUI1 = CUI1+1;
  IF UI.BRATE(I) > 0 THEN CUI2 = 1;
END;
IF UI2(JJ) = CUI2 THEN BEGIN;
   COUNT(CNT(JJ),8) = COUNT(CNT(JJ),8) + 1;
OK = '0'B;
IF PART.UI1(JJ) = 0 & CUI1 = 0 THEN OK = '1'B;
IF PART.UI1(JJ) = 1 & CUI1 = 1 THEN OK = '1'B;
IF PART.UI1(JJ) > 1 & CUI1 > 1 &
CUI1 > PART.UI1(JJ) - 3 & CUI1 < 3 + PART.UI1(JJ) THEN OK =
'1'B;
IF OK THEN BEGIN;
   COUNT(CNT(JJ),2) = COUNT(CNT(JJ),2) + 1;
IF UI.BRATE(STT-1) > ENT THEN CUI3 = 1; ELSE
IF (UI.BRATE(STT-2) > ENT
| UI.BRATE(STT-3) > ENT) THEN CUI3 = 2; ELSE
IF UI.BRATE(STT-1) > 0 |
 (UI.BRATE(STT-2) > 0
| UI.BRATE(STT-3) > 0) THEN CUI3 = 3;

IF CNT(JJ) = 37 THEN CUI3 = UI3(JJ);
IF UI3(JJ) = CUI3 THEN BEGIN;
   COUNT(CNT(JJ),3) = COUNT(CNT(JJ),3) + 1;


                 /* NOW THE VARIABLES FROM THE ROE. */

                 /* FIRST, THE NUMBER OF UI INSURABLE
                    WEEKS ON ROE'S ISSUED IN PAST 4 MO'S. */

TMPWKS=0;WEEKS=0;CWKS = 0;MAXEARN = 0;
```

```
IF ROE.SIN = IA.SIN THEN
DO I = STT-3 TO STT - 1;
  IF INEARN1(I) ¬='*****' & INWK1(I) ¬= '**'
  & INWK1(I) ¬= '00' & INEARN1(I)/INWK1(I) > MAXEARN
  THEN MAXEARN = INEARN1(I)/INWK1(I);
  IF INWK1(I) ¬= '**' THEN TMPWKS = INWK1(I);
  IF TMPWKS > WEEKS THEN WEEKS = TMPWKS;
  IF INWK2(I) ¬= '**' THEN TMPWKS = INWK2(I);
  IF TMPWKS > WEEKS THEN WEEKS = TMPWKS;
  IF INEARN2(I) ¬='*****' & INWK2(I) ¬= '**'
  & INWK2(I) ¬= '00' & INEARN2(I)/INWK2(I) > MAXEARN
  THEN MAXEARN = INEARN2(I)/INWK2(I);
END;
IF MAXEARN < 50 THEN WEEKS = 0;
IF WEEKS = 0 THEN CWKS = 0; ELSE
IF MAXEARN > ENT/2.6 & WEEKS < 10 THEN CWKS = 1; ELSE
IF MAXEARN > ENT/4.3 & WEEKS < 10 THEN CWKS = 2; ELSE
IF MAXEARN > ENT/2.6 THEN CWKS = 3; ELSE
IF MAXEARN > ENT/4.3 THEN CWKS = 4; ELSE CWKS = 5;
COUNT2(CWKS) = COUNT2(CWKS) + 1;
IF CNT(JJ) = 37 THEN CWKS = WKS(JJ);
IF CNT(JJ) = 153 THEN CWKS = WKS(JJ);
IF CNT(JJ) = 368 & CWKS > 2 THEN CWKS = WKS(JJ);
IF CNT(JJ) = 098 & CWKS = 2 THEN CWKS = WKS(JJ);
IF CNT(JJ) = 375 & CWKS = 2 THEN CWKS = WKS(JJ);
IF CNT(JJ) = 399 THEN CWKS = WKS(JJ);
IF CNT(JJ) = 091 & CWKS = 4 THEN CWKS = WKS(JJ);
IF CWKS = WKS(JJ) THEN BEGIN;
   COUNT(CNT(JJ),4) = COUNT(CNT(JJ),4) + 1;


                /* THEN THE EARNINGS IN EACH OF PAST 2 YEARS
*/
                /* INEARN1 IS INSURABLE EARNINGS, INWK1 IS */
                /* INSURABLE WEEKS.  AS A RESULT EARN AND */
                /* CEARN ARE ANNUAL INS EARNINGS /4.3. */

CEARN1,CEARN2 = 0;
TOL = 1.66;
IF ROE.SIN = IA.SIN THEN
DO I = STT-12 TO STT - 1;
  IF INEARN1(I) ¬='*****' & INWK1(I) ¬= '**' & INWK1(I) ¬= '00'
  THEN CEARN1 = CEARN1 + INEARN1(I)/INWK1(I);
  ELSE IF INEARN2(I) ¬='*****' & INWK2(I) ¬= '**' & INWK2(I) ¬=
'00'
  THEN CEARN1 = CEARN1 + INEARN2(I)/INWK2(I);
END;
OK = '0'B;
IF CNT(JJ) = 37 THEN OK = '1'B;
IF CNT(JJ) = 368 THEN OK = '1'B;
IF CNT(JJ) = 399 THEN OK = '1'B;
IF PART.EARN1(JJ) = 0 THEN IF CEARN1 = 0 THEN OK = '1'B;
IF UI2(JJ) = 1 & CEARN1 = 0 & PART.EARN1(JJ) < 500 THEN OK =
'1'B;
```

```
IF PART.EARN1(JJ) > 0 & PART.EARN1(JJ) < 1000
&  CEARN1 > 0 & CEARN1 < 1000 THEN OK = '1'B;
IF CEARN1 > PART.EARN1(JJ)/TOL
& CEARN1 < TOL * PART.EARN1(JJ) THEN OK = '1'B;
IF OK THEN BEGIN;
   COUNT(CNT(JJ),5) = COUNT(CNT(JJ),5) + 1;

IF ROE.SIN = IA.SIN THEN
DO I = STT-24 TO STT - 13;
  IF INEARN1(I) ¬='*****' & INWK1(I) ¬= '**' & INWK1(I) ¬= '00'
  THEN CEARN2 = CEARN2 + INEARN1(I)/INWK1(I);
  ELSE IF INEARN2(I) ¬='*****' & INWK2(I) ¬= '**' & INWK2(I) ¬=
'00'
  THEN CEARN2 = CEARN2 + INEARN2(I)/INWK2(I);
END;

OK = '0'B;
IF CNT(JJ) = 37 THEN OK = '1'B;
IF CNT(JJ) = 368 THEN OK = '1'B;
IF CNT(JJ) = 153 THEN OK = '1'B;
IF CNT(JJ) = 399 THEN OK = '1'B;
TOL = 1.66;
IF PART.EARN2(JJ) = 0 & CEARN2 = 0 THEN OK = '1'B;
IF PART.EARN2(JJ) > 0 & PART.EARN2(JJ) < 1000
&  CEARN2 > 0 & CEARN2 < 1000 THEN OK = '1'B;
IF UI2(JJ) = 1 & CEARN1 = 0 & PART.EARN2(JJ) < 500 THEN OK =
'1'B;
IF CEARN2 > PART.EARN2(JJ)/TOL
&  CEARN2 > 0 & EARN2(JJ) > 0
& CEARN2 < TOL * PART.EARN2(JJ) THEN OK = '1'B;
IF OK THEN BEGIN;
   COUNT(CNT(JJ),6) = COUNT(CNT(JJ),6) + 1;

                     /* THEN CHECK DIFFERENCE IN EARNINGS  */

OK = '0'B;
IF CNT(JJ) = 368 THEN OK = '1'B;
IF CNT(JJ) = 37 THEN OK = '1'B;
IF CNT(JJ) = 91 THEN OK = '1'B;
IF CNT(JJ) = 153 THEN OK = '1'B;
IF CNT(JJ) = 399 THEN OK = '1'B;
DIFF =  PART.EARN1(JJ) -  PART.EARN2(JJ);
CDIFF = CEARN1 - CEARN2;
IF DIFF < 1 & CDIFF < 1 THEN OK = '1'B;
IF CDIFF > DIFF/TOL & CDIFF < TOL * DIFF
THEN OK = '1'B;
IF DIFF > 0 & CDIFF > 0 & CDIFF < 500 & DIFF < 500
THEN OK = '1'B;
IF OK THEN BEGIN;
   COUNT(CNT(JJ),7) = COUNT(CNT(JJ),7) + 1;

                          /* WE HAVE A MATCH!!!  */
                          /* RECORD PRE & POST VALUES FOR
                             COMPARISON GROUP.  */
```

```
MATCH = '1'B;
MRK(PART(JJ).CNT) =  1;   /*   IDENTIFIES MATCHED PARTICIPANT
*/
TOT(PART(JJ).CNT) = TOT(PART(JJ).CNT) + 1;
DIND = DNULL;F1 = '0'B;
DO I = KK-36 TO KK + 60;
 IF PGM(I) > 0 THEN BEGIN;
   DIND(KK+61-I) = 1;
   IAIND(CNT(JJ),KK+61-I) = IAIND(CNT(JJ),KK+61-I) + 1;
 END;
END;
IF UI.SIN = IA.SIN THEN
DO I = STT-60 TO STT + 36;
  IF UI.BEN(I) > 0 THEN BEGIN;
    DIND(I-STT+61) = 1;
    UIIND(CNT(JJ),I-STT+61) = UIIND(CNT(JJ),I-STT+61) + 1;
  END;
END;
DO I = 1 TO 97;
  NOIND(CNT(JJ),I) = NOIND(CNT(JJ),I) + DIND(I);
END;
IF ROE.SIN = IA.SIN THEN
DO I = STT-60 TO STT + 36;
  IF INEARN1(I) ¬='*****' & INWK1(I) ¬= '**' & INWK1(I) ¬= '00'
  THEN EARIND(CNT(JJ),I-STT+61) = EARIND(CNT(JJ),I-STT+61)
  + INEARN1(I)/INWK1(I); ELSE
  IF INEARN2(I) ¬='*****' & INWK2(I) ¬= '**' & INWK2(I) ¬= '00'
  THEN EARIND(CNT(JJ),I-STT+61) = EARIND(CNT(JJ),I-STT+61)
  + INEARN2(I)/INWK2(I);
END;
END;   /* MATCHED ON DIFF  */
END;   /* MATCHED ON EARN2  */
END;   /* MATCHED ON EARN1  */
END;   /* MATCHED ON WKS  */
END;   /* MATCHED ON UI3   */
END;   /* MATCHED ON UI2   */
END;   /* MATCHED ON UI1  */
END;   /* MATCHED ON IA HIST (P) */
END;   /* MATCHED ON AGE  */
END;   /* MATCHED ON CLASS & CAT */
END;   /* LOOP TO CHECK ALL PARTICIPANTS WITH SAME START DATE
*/
END;   /* LOOK FOR MATCH BEFORE AND AFTER START DATE  */
END;   /* LOOP TO CHECK EACH UNIQUE START DATE  */
MARK.SIN = IA.SIN;
IF MATCH THEN
WRITE FILE(OUTSIN) FROM (MARK);
MATCH = '0';
MRK = NMRK;
READ FILE(IAIN) INTO(IA);
END;   /* IA SIN NOT LOWEST */
END; /*NOT EOF*/
DO I = 1 TO 521;
    PUT SKIP EDIT(TOT(I))(F(11));
```

```
      WRITE FILE(OUTFILE) FROM (TOT(I));
      IF TOT(I) = 0 THEN BEGIN;
        PUT SKIP EDIT('TOT = 0, I = ',I)(A(14),F(4));
        TOT(I) = .1;
      END;
    DO J = 1 TO 97;
      IAIND(I,J) = IAIND(I,J)/TOT(I);
      UIIND(I,J) = UIIND(I,J)/TOT(I);
      NOIND(I,J) = NOIND(I,J)/TOT(I);
      EARIND(I,J) = EARIND(I,J)/TOT(I);
      WRITE FILE(OUTFILE) FROM (IAIND(I,J));
      WRITE FILE(OUTFILE) FROM (UIIND(I,J));
      WRITE FILE(OUTFILE) FROM (NOIND(I,J));
      WRITE FILE(OUTFILE) FROM (EARIND(I,J));
      TOTNO(J) = TOTNO(J) + NOIND(I,J);
      TOTUI(J) = TOTUI(J) + UIIND(I,J);
      TOTIA(J) = TOTIA(J) + IAIND(I,J);
      TOTEAR(J) = TOTEAR(J) + EARIND(I,J);
    END;
  END;
  DO I = 1 TO 97;
    PUT SKIP EDIT(TOTNO(I)/521,TOTUI(I)/521,TOTIA(I)/521,
    TOTEAR(I)/521)((4)F(11,4));
  END;
  DO I = 0 TO 9;
        PUT SKIP EDIT(COUNT2(I))(F(11));
      END;
  DO I = 1 TO 521;
      IF TOT(CNT(I)) < 1 THEN BEGIN;
        PUT SKIP EDIT(PART.CNT(I),PART.CAT(I),PART.CLASS(I),
        PART.A(I),PART.P(I),UI1(I),UI2(I),
        START(I),UI3(I),WKS(I),EARN1(I),EARN2(I))
        ((10)A(4),(2)F(11,2));
      END;
      END;
  DO I = 1 TO 521;
      IF TOT(CNT(I)) < 1 THEN BEGIN;
        PUT SKIP EDIT(CNT(I),COUNT(CNT(I),1),COUNT(CNT(I),8),
        COUNT(CNT(I),2),COUNT(CNT(I),3),
        COUNT(CNT(I),4),COUNT(CNT(I),5),
        COUNT(CNT(I),6),COUNT(CNT(I),7))((9)F(7));
      END;
      END;
 END MAIN;
/*
//GO.ROEIN  DD DSN=HRRSD.E1.ROELONG.AUG2694,UNIT=TAPE,DISP=SHR
//UIIN    DD DSN=HRRSD.E1.UILONG.AUG2694,UNIT=TAPE,DISP=SHR
//IAIN     DD  DSN=HRRSD.E1.IALONG.AUG2694,DISP=SHR
//PARTIN   DD DSN=HRRSD.E1.PPDPART.SHORT6.OCT0794,DISP=SHR
//OUTFILE  DD DSN=HRRSD.E1.PPD.PERCENT.OCT0994,UNIT=DISK,
//            DISP=OLD
//*          DISP=(NEW,CATLG,KEEP),
//*          DCB=(RECFM=FB,LRECL=4),
//*          SPACE=(TRK,(40,10),RLSE)
```

```
//OUTSIN    DD DSN=HRRSD.E1.PPD.SINS.OCT0994,UNIT=DISK,
//             DISP=OLD
//*         DISP=(NEW,CATLG,KEEP),
//*         DCB=(RECFM=FB,LRECL=529),
//*         SPACE=(TRK,(50,40),RLSE)
//*GO.INFILE DD DUMMY
//*OUTFILE DD DUMMY
```

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PL1 Program
PPD6

```
MAIN: PROC OPTIONS(MAIN);
DCL    EOF                          BIT INIT('0'B),
       F1                           BIT INIT('0'B),
       F3                           BIT INIT('0'B),
       FOUND                        BIT INIT('0'B),
       SYSPRINT                     FILE OUTPUT,
       MINSIN   PIC'(9)9',

          SUMDIF(4,97)   FLOAT(6) INIT((4*97)0),
          SUMDIF2(4,97)   FLOAT(6) INIT((4*97)0),
          SUMDF(4)   FLOAT(6) INIT((4)0),
          SUMDF2(4)   FLOAT(6) INIT((4)0),
          SUMDL(4)   FLOAT(6) INIT((4)0),
          SUMDL2(4)   FLOAT(6) INIT((4)0),
          TOT(521)    FLOAT(6),
          VAR(4)   FLOAT(6) INIT((4)0),
          COMPIA(521,97)    FLOAT(6),
          COMPUI(521,97)    FLOAT(6),
          COMPNO(521,97)    FLOAT(6),
          COMPER(521,97)    FLOAT(6),
          PGM(169)   FLOAT(6) INIT((169)0),
          TOTIA(97)   FLOAT(6) INIT((97)0),
          TOTUI(97)   FLOAT(6) INIT((97)0),
          TOTIND(97)   FLOAT(6) INIT((97)0),
          TOTEARN(97)   FLOAT(14) INIT((97)0),
          CTOTIA(97)   FLOAT(6) INIT((97)0),
          CTOTUI(97)   FLOAT(6) INIT((97)0),
          CTOTIND(97)   FLOAT(6) INIT((97)0),
          CTOTEARN(97)   FLOAT(9) INIT((97)0),
          DIND(97)   FIXED DEC(9) INIT((97)0),
          DNULL(97)   FIXED DEC(9) INIT((97)0),


          DIFF         FLOAT(6) INIT(0),
          FDIFF         FLOAT(6) INIT(0),
          LDIFF         FLOAT(6) INIT(0),
          TEMPEARN      FLOAT(6) INIT(0),
          TOTAL        FLOAT(9) INIT(0),
          NUM          FIXED DEC(9) INIT(0),
          START        FIXED DEC(9) INIT(0),
          I            FIXED DEC(9) INIT(0),
          J            FIXED DEC(9) INIT(0),
          N            FIXED DEC(9) INIT(0),
          K            FIXED DEC(9) INIT(0),
          S            FIXED DEC(9) INIT(0),
          MIN          BUILTIN,
          SUBSTR       BUILTIN,
          TRUNC        BUILTIN,

      COMPIN                          FILE RECORD INPUT,
      ROEIN                          FILE RECORD INPUT,
      UIIN                          FILE RECORD INPUT,
      PARTIN                        FILE RECORD INPUT,
```

0C0247

```
IAIN                          FILE RECORD INPUT,
OUTFILE                       FILE RECORD OUTPUT,
OUTCNT                        FILE RECORD OUTPUT,

1 PART,
  2 SIN     CHAR(8),
  2 CAT     PIC'9',
  2 CLASS   PIC'9',
  2 A       PIC'9',
  2 P       PIC'9',
  2 UI1     PIC'99',
  2 UI2     PIC'99',
  2 STT     PIC'999',
  2 CNT     PIC'999' INIT(0),
  2 UI3     PIC'9',
  2 WKS     PIC'9',
  2 EARN1   FLOAT(6),
  2 EARN2   FLOAT(6),

 1 ROE,
     2  SIN                  CHAR(8),
     2  SIN2                 PIC'(1)9',
     2  MONTH(168),
        3  ROESTAT           CHAR(1),
        3  INWK1             CHAR(2),
        3  INEARN1           CHAR(5),
        3  PCODE11           CHAR(1),
        3  PCODE12           CHAR(5),
        3  EMPL1             CHAR(9),
        3  SIC1              CHAR(4),
        3  REASON1          CHAR(1),
        3  INWK2            CHAR(2),
        3  INEARN2          CHAR(5),
        3  PCODE21          CHAR(1),
        3  PCODE22          CHAR(5),
        3  EMPL2            CHAR(9),
        3  SIC2             CHAR(4),
        3  REASON2          CHAR(1),


 1 UI,
     2  SIN                  CHAR(8),
     2  SIN2                 PIC'(1)9',
     2  AGE                   CHAR(2),
     2  SEX                   CHAR(1),
     2  MONTH(168),
        3  BEN                PIC'9',
        3  DIST               CHAR(4),
        3  PROV               CHAR(2),
        3  REG                CHAR(2),
        3  DPND               CHAR(1),
        3  BRATE              CHAR(4),
        3  STUD               CHAR(1),
        3  INWK               CHAR(2),
```

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```
            3  OCC              CHAR(7),
            3  SIC              CHAR(4),
            3  WKPD             CHAR(3),


1 IA,
 2 FID1         CHAR(2),
 2 FID2         PIC'99999999',
 2 SIN          CHAR(8),
 2  SIN2                PIC'(1)9',
 2 BYR          PIC'99',
 2 BMO          PIC'99',
 2 SEX          CHAR(1),
 2 TMP(169)     PIC'9',
 2 CAT(169)     PIC'9',
 2 CLASS(169)   PIC'9',
 2 DPNO(169)    PIC'9';

                                     /* READ IN AVERAGE VALUES OF
                                        UI DEPENDENCE, IA DEPENDENCE
                                        DEPENDENCE ON EITHER, AND
                                        EARNINGS FOR COMPARISON
                                        GROUP  */
 DO I = 1 TO 521;
   READ FILE(COMPIN) INTO (TOT(I));
   DO J = 1 TO 97;
     READ FILE(COMPIN) INTO (COMPIA(I,J));
     READ FILE(COMPIN) INTO (COMPUI(I,J));
     READ FILE(COMPIN) INTO (COMPNO(I,J));
     READ FILE(COMPIN) INTO (COMPER(I,J));
   END;
 END;

 ON ENDFILE(IAIN) IA.SIN = '99999999';
 ON ENDFILE(PARTIN) EOF = '1'B;
 ON ENDFILE(ROEIN) BEGIN;ROE.SIN = '99999999';
 PUT SKIP EDIT('END OF ROE FILE')(A);END;
 ON ENDFILE(UIIN) BEGIN;UI.SIN = '99999999';
 PUT SKIP EDIT('END OF UI FILE')(A);END;
 READ FILE(ROEIN) INTO(ROE);
 READ FILE(IAIN) INTO(IA);
 READ FILE(UIIN) INTO(UI);
 READ FILE(PARTIN) INTO(PART);
 NUM = 1;

 DO WHILE(¬EOF);
 IF ROE.SIN < PART.SIN THEN
 READ FILE(ROEIN) INTO(ROE);
 ELSE IF IA.SIN < PART.SIN THEN
 READ FILE(IAIN) INTO(IA);
 ELSE IF UI.SIN < PART.SIN THEN
 READ FILE(UIIN) INTO(UI);

 ELSE BEGIN;
```

```
                           /* PUT IA INTO SAME ORDER AS
                              UI AND EARNINGS.  */

IF IA.SIN = PART.SIN THEN
DO I = 1 TO 169;
  PGM(170-I) = IA.TMP(I);
END;
ELSE PUT SKIP EDIT('*')(A);

START = (170-PART.STT);
                           /* FINAL CHECK TO MAKE SURE PARTICIPANTS
                              ARE ON BASIC IA, STARTED THE
                              TRAINING IN THE EXPECTED YEARS,
                              AND HAVE AT LEAST ONE MEMBER IN
                              THEIR COMPARISON GROUP.  */
IF TOT(NUM) = 0 THEN
PUT SKIP EDIT('TOT=0.N = ',NUM)(A(16),F(4));
ELSE IF PGM(START) = 3 THEN BEGIN;
IF START < 60 | START > 133 THEN BEGIN;
  PUT SKIP EDIT(START)(F(11));
  PUT SKIP EDIT(PART.STT)(A);
  START = 86;
END;
TOTAL = TOTAL + 1;

                       /* ADD THE AVERAGE VALUES FOR EACH
                          COMPARISON GROUP TO GET VALUES
                          FOR OVERALL COMPARISON GROUP.   */
DO I = 1 TO 97;
  CTOTIA(I) = CTOTIA(I) + COMPIA(NUM,I);
  CTOTUI(I) = CTOTUI(I) + COMPUI(NUM,I);
  CTOTIND(I) = CTOTIND(I) + COMPNO(NUM,I);
  CTOTEARN(I) = CTOTEARN(I) + COMPER(NUM,I);
END;

DIND = DNULL;F1 = '0'B;FDIFF,LDIFF= 0;
IF IA.SIN = PART.SIN THEN
DO I = START-60 TO START + 36;
  J = I-START+61;

             /*  CALCULATE SUMS OF DIFFERENCES BETWEEN PARTICIPANTS
                 AND COMPARISON GROUP, AND SUMS OF SQUARES OF
                 DIFFERENCES FOR THE PURPOSES OF CALCULATING
                 VARIANCES.           */

  IF PGM(I) > 0 THEN DIFF = 1 - COMPIA(NUM,J);
  ELSE DIFF =  0 - COMPIA(NUM,J);
  SUMDIF2(1,J) = SUMDIF2(1,J) + DIFF**2;
  SUMDIF(1,J) = SUMDIF(1,J) + DIFF;

  IF I > START + 24 THEN LDIFF = LDIFF + DIFF;
  IF I < START - 48 THEN FDIFF = FDIFF + DIFF;
  IF PGM(I) > 0 THEN DIND(I-START+61) = 1;
  IF PGM(I) > 0 THEN TOTIA(I-START+61) = TOTIA(I-START+61) + 1;
```

```
END;
SUMDF2(1) = SUMDF2(1) + FDIFF**2;
SUMDL2(1) = SUMDL2(1) + LDIFF**2;
SUMDF(1) = SUMDF(1) + FDIFF;
SUMDL(1) = SUMDL(1) + LDIFF;

LDIFF, FDIFF = 0;
IF UI.SIN = PART.SIN THEN
DO I = START-60 TO START + 36;
  J = I-START+61;
  IF UI.BEN(I) > 0 THEN DIND(I-START+61) = 1;
  IF UI.BEN(I) > 0 THEN TOTUI(I-START+61) =  TOTUI(I-START+61) +1;
  IF UI.BEN(I) > 0 THEN DIFF = 1 - COMPUI(NUM,J);
  ELSE DIFF =  0 - COMPUI(NUM,J);
  SUMDIF2(2,J) = SUMDIF2(2,J) + DIFF**2;
  SUMDIF(2,J) = SUMDIF(2,J) + DIFF;
  IF I > START + 24 THEN LDIFF = LDIFF + DIFF;
  IF I < START - 48 THEN FDIFF = FDIFF + DIFF;
END; ELSE
DO I = 1 TO 97;
  DIFF =  0 - COMPUI(NUM,I);
  SUMDIF2(2,I) = SUMDIF2(2,I) + DIFF**2;
  SUMDIF(2,I) = SUMDIF(2,I) + DIFF;
  IF I >  85 THEN LDIFF = LDIFF + DIFF;
  IF I <  13 THEN FDIFF = FDIFF + DIFF;
END;
SUMDF2(2) = SUMDF2(2) + FDIFF**2;
SUMDL2(2) = SUMDL2(2) + LDIFF**2;
SUMDF(2) = SUMDF(2) + FDIFF;
SUMDL(2) = SUMDL(2) + LDIFF;

LDIFF, FDIFF = 0;
DO I = 1 TO 97;
  TOTIND(I) = TOTIND(I) + DIND(I);
  DIFF =  DIND(I) - COMPNO(NUM,I);
  SUMDIF2(3,I) = SUMDIF2(3,I) + DIFF**2;
  SUMDIF(3,I) = SUMDIF(3,I) + DIFF;
  IF I > 85 THEN LDIFF = LDIFF + DIFF;
  IF I < 13 THEN FDIFF = FDIFF + DIFF;
END;
SUMDF2(3) = SUMDF2(3) + FDIFF**2;
SUMDL2(3) = SUMDL2(3) + LDIFF**2;
SUMDF(3) = SUMDF(3) + FDIFF;
SUMDL(3) = SUMDL(3) + LDIFF;

LDIFF, FDIFF = 0;
IF ROE.SIN = PART.SIN THEN
DO I = START-60 TO START + 36;
  J = I-START+61;
  TEMPEARN = 0;
  IF INEARN1(I) ¬='*****' & INWK1(I) ¬= '**' & INWK1(I) ¬= '00'
  THEN TEMPEARN =  INEARN1(I)/INWK1(I);
ELSE IF INEARN2(I) ¬='*****' & INWK2(I) ¬= '**' & INWK2(I) ¬= '00'
  THEN TEMPEARN = INEARN2(I)/INWK2(I);
```

```
    TOTEARN(J) = TOTEARN(J ) + TEMPEARN;
    DIFF =  TEMPEARN - COMPER(NUM,J);
    SUMDIF2(4,J) = SUMDIF2(4,J) + DIFF**2;
    SUMDIF(4,J) = SUMDIF(4,J) + DIFF;
    IF I > START + 24 THEN LDIFF = LDIFF + DIFF;
    IF I < START - 48 THEN FDIFF = FDIFF + DIFF;
  END; ELSE
  DO I = START-60 TO START + 36;
    J = I-START+61;
    DIFF =  0 - COMPER(NUM,J);
    SUMDIF2(4,J) = SUMDIF2(4,J) + DIFF**2;
    SUMDIF(4,J) = SUMDIF(4,J) + DIFF;
    IF I > START + 24 THEN LDIFF = LDIFF + DIFF;
    IF I < START - 48 THEN FDIFF = FDIFF + DIFF;
  END;
  SUMDF2(4) = SUMDF2(4) + FDIFF**2;
  SUMDL2(4) = SUMDL2(4) + LDIFF**2;
  SUMDF(4) = SUMDF(4) + FDIFF;
  SUMDL(4) = SUMDL(4) + LDIFF;
END;
READ FILE(PARTIN) INTO(PART);
NUM = NUM + 1;
END;
END; /*NOT EOF*/

                      /* WRITE OUT PERCENTAGE OF PARTICIPANTS
                         ON IA, UI NEITHER, AND AVERAGE EARNINGS. */
PUT SKIP EDIT(' ')(A);
PUT SKIP EDIT('PARTICIPANTS. ')(A);

DO I = 1 TO 97;
  PUT SKIP EDIT(TOTIND(I)/TOTAL,TOTUI(I)/TOTAL,
  TOTIA(I)/TOTAL,TOTEARN(I)/TOTAL)
  ((4)F(16,4));
END;
PUT SKIP EDIT(' ')(A);

                      /* WRITE OUT PERCENTAGE OF COMPARISON GROUP
                         ON IA, UI NEITHER, AND AVERAGE EARNINGS. */

PUT SKIP EDIT(' ')(A);
PUT SKIP EDIT('COMPARISON GROUP. ')(A);
DO I = 1 TO 97;
  PUT SKIP EDIT(CTOTIND(I)/TOTAL,CTOTUI(I)/TOTAL,
  CTOTIA(I)/TOTAL,CTOTEARN(I)/TOTAL)
  ((4)F(16,4));
END;

                         /* CALCULATE AND WRITE OUT VARIANCES
                            OF DIFFERENCES.  */

PUT SKIP EDIT(' ')(A);
PUT SKIP EDIT('VARIANCES OF DIFFERENCES. ')(A);
DO I = 1 TO 97;
```

```
   PUT SKIP EDIT(' ')(A);
   DO J = 1 TO 4;
     VAR(J) =
1/(TOTAL-1))*(SUMDIF2(J,I)-SUMDIF(J,I)**2/TOTAL)/TOTAL;
    PUT EDIT(VAR(J))(X(4),P'999V.9999999');
   END;
 END;

 PUT SKIP EDIT(' ')(A);
 PUT SKIP EDIT('VARIANCES OF DIFFERENCES IN FIRST 12 MONTHS. ')(A);
 PUT SKIP EDIT(' ')(A);
 DO J = 1 TO 4;
   VAR(J) = (1/(TOTAL-1))*(SUMDF2(J)-SUMDF(J)**2/TOTAL)/TOTAL;
   PUT EDIT(VAR(J))(X(4),P'999V.9999999');
 END;
 PUT SKIP EDIT(' ')(A);
 PUT SKIP EDIT('VARIANCES OF DIFFERENCES IN LAST 12 MONTHS. ')(A);
 PUT SKIP EDIT(' ')(A);
 DO J = 1 TO 4;
   VAR(J) = (1/(TOTAL-1))*(SUMDL2(J)-SUMDL(J)**2/TOTAL)/TOTAL;
   PUT EDIT(VAR(J))(X(4),P'999V.9999999');
 END;
 PUT SKIP EDIT(' ')(A);
 PUT SKIP EDIT(TOTAL)(F(11));
END MAIN;
/*
//GO.ROEIN  DD DSN=HRRSD.E1.ROE5.MAY1194,DISP=SHR
//UIIN    DD DSN=HRRSD.E1.UI.PPDPART.JUN0194,DISP=SHR
//IAIN     DD  DSN=HRRSD.E1.IA.PPDPART.JUN0194,DISP=SHR
//PARTIN   DD DSN=HRRSD.E1.PPDPART.SHORT6.OCT0794,DISP=SHR
//COMPIN   DD DSN=HRRSD.E1.PPD.PERCENT.OCT0994,DISP=SHR
//*GO.INFILE DD DUMMY
//*OUTFILE DD DUMMY
```

C00253

Photocopy of Copy 3 of
Monthly Claim Form

Province of British Columbia — Ministry of Social Services and Housing

MONTHLY CLAIM

EMPLOYER NOTE: DETACH COPY 5 (EMPLOYER'S COPY) AND FORWARD REMAINING COPIES TO THE MINISTRY OF SOCIAL SERVICES AND HOUSING OFFICE AT THE ADDRESS BELOW.

CLAIM NUMBER: 182408

1. LEGAL NAME OF BUSINESS/ PROJECT SPONSOR: AGENCIES LTD

3. PERSON COMPLETING: LANA

BUSINESS TELEPHONE: 599-

IS THIS YOUR FINAL CLAIM? ☐ YES ☑ NO

2. MAILING ADDRESS: ... NUE, SURR... BC

CITY/ TOWN/ VILLAG

POSTAL CODE

4. CONTRACT NUMBER: LD4511900313

NOT TO BE SIGNED UNTIL SECTIONS 1 - 17 ARE COMPLETED

COMPLETE FOR COMMUNITY TOURISM PROJECTS ONLY

5. POS. NO.	6. CLAIM PERIOD FROM YY	MM	DD	TO YY	MM	DD	7. NAME OF EMPLOYEE(S) FAMILY NAME	FIRST NAME	8. SOCIAL INSURANCE NUMBER	9. DAYS WORKED	10. HOURS WORKED	11. HOURLY WAGE PAID	12. GROSS WAGES CLAIMED (10 X 11)	13. VACATION PAY	EMPLOYER'S SHARE OF BENEFITS 14. UIC	15. CPP	16. W.C.B.	17. NET PAY	18. CERTIFICATION BY EMPLOYEE (SIGNATURE) "I HAVE RECEIVED THE INDICATED NET PAY"
1	94	02	01	94	02	15		NADINE	72273	10	61	9.50	579.50					536.58	
12	94	02	16	94	02	28		NADINE	72273	9	68 1/4	9.50	648.38					550.70	

27. COMMENTS (EXPLAIN ANY DAYS NOT WORKED)

TOTALS: 19 — 20: 129 1/4 — 21: 1227.88 — 22 — 23 — 24 — 25 — 26. TOTALS FOR SECTIONS 21-25: 1227.88

APR 12 1994

CERTIFICATION BY THE EMPLOYER: I hereby certify that the above information is true and correct and I am aware that my payroll records for this program can be examined by a Ministry of Social Services and Housing representative at any time.

28. SIGNATURE OF AUT...: X Lana

29. POSITION IN ORGANIZATION: Office Manager

30. TELEP: 5

31. DATE: Mar 12/94

MINISTRY OF SOCIAL SERVICES AND HOUSING USE ONLY

POS.NO.	HOURS APPROVED	REIMB. RATE	M.S.S.H. SHARE OF GROSS WAGES	VACATION PAY	U.I.C.	C.P.P.	W.C.B.	ADVANCE BALANCE	TOTAL REIMBURSEMENT
~~1~~	~~[illegible]~~	~~[illegible]~~	~~[illegible]~~						
1	129.25	3.50	452.37						
									452.37

REVIEWED BY: [illegible]

DATE YY/MM/DD

MIN	VOTE	STOB	ACTIVITY CODE	RESP.
39	55	8750	49700	345

OCG SUPPLIER NUMBER

PAYMENT DUE DATE

INVOICE NO.

SUPPLIER POSTAL CODE

ACCT'S USE ONLY: ALL PARTICULARS, CODING, EXTENSIONS AND TOTALS HAVE BEEN CHECKED: [illegible]

CERTIFIED THAT THE AMOUNT TO BE PAID: IS CORRECT, IS IN ACCORDANCE WITH APPROPRIATE STATUTE OR OTHER AUTHORITY FOR PAYMENT AND/ OR CONTRACT; AND, WHERE APPLICABLE, THE WORK HAS BEEN PERFORMED, THE GOODS SUPPLIED, THE SERVICES RENDERED, AND/OR OTHER CONDITIONS MET.

SIGNATURE (SPENDING AUTHORITY): [signature]

DATE YY/MM/DD: 94-03-21

RETURN TO:

FOR EMPLOYMENT OPPORTUNITY PROGRAM PLEASE SEND TO LOCAL DISTRICT OFFICE:

DISTRICT OFFICE STAMP

FOR COMMUNITY TOURISM PROGRAM PLEASE SEND TO: M.S.S.H., E.I.P., PARLIAMENT BUILDINGS VICTORIA B.C., V8V-1X4

FINAL CLAIM ☐ This contract is terminated or has expired. No further claims will be made. Finance Division is hereby authorized to decommit any unexpended funds.

MINISTRY DISTRIBUTION: EOP: COPIES 1, 2, & 3 - TO FINANCE DIV., COPY 4 - TO DISTRICT OFFICE, COPY 5 - EMPLOYER. CTETP: COPIES 1, 2, 3 & 4 TO EIP, COPY 5 - EMPLOYER

COPY 3

000255

PL1 Program
PRONIA

```
/*ROUTE PRINT R07
//STEP1       EXEC PLIXCG,CLASS=X
//PLI.SYSIN   DD *

 MAIN: PROC OPTIONS(MAIN);
 DCL    EOF                          BIT INIT('0'B),
        FOUND                        BIT INIT('0'B),
        SYSPRINT                     FILE OUTPUT,

           RAND(48)    PIC'999',
           TMPWKS      PIC'99',
           DIFF        FLOAT(6),
           CWKS        PIC'9',
           INDEARN     FLOAT(6),
           INDDIFF     FLOAT(6),
           INDEARNH    FLOAT(6),
           DEARN(0:20)        FLOAT(6) INIT((21)0),
           DEARNP(0:20)        FLOAT(6) INIT((21)0),
           DEARNH(0:20)        FLOAT(6) INIT((21)0),
           DEARNHP(0:20)        FLOAT(6) INIT((21)0),
           DEARN2(0:20)        FLOAT(6) INIT((21)0),
           DEARNP2(0:20)        FLOAT(6) INIT((21)0),
           DEARNH2(0:20)        FLOAT(6) INIT((21)0),
           DEARNHP2(0:20)       FLOAT(6) INIT((21)0),
           DISTDIF(0:20)     FLOAT(6) INIT((21)0),
           DISTDIFP(0:20)     FLOAT(6) INIT((21)0),
           DISTUI(0:12)     FLOAT(6) INIT((13)0),
           DISTUIP(0:12)     FLOAT(6) INIT((13)0),
           DISTDUI(0:24)     FLOAT(6) INIT((25)0),
           DISTDUIP(0:24)     FLOAT(6) INIT((25)0),
           DISTUI2(0:12)     FLOAT(6) INIT((13)0),
           DISTUIP2(0:12)     FLOAT(6) INIT((13)0),
           DISTINWK(0:50)     FLOAT(6) INIT((51)0),
           DISTINWKP(0:50)     FLOAT(6) INIT((51)0),

           PGM(169)    FLOAT(6) INIT((169)0),
           STT          FIXED DEC(9) INIT(0),
           I            FIXED DEC(9) INIT(0),
           J            FIXED DEC(9) INIT(0),
           K            FIXED DEC(9) INIT(0),
           MIN          BUILTIN,
           SUBSTR       BUILTIN,
           TRUNC        BUILTIN,

       ROEIN                          FILE RECORD INPUT,
       UIIN                           FILE RECORD INPUT,
       RANDIN                         FILE RECORD INPUT,
       IAIN                           FILE RECORD INPUT,
       OUTFILE                        FILE RECORD OUTPUT,
       OUTSIN                         FILE RECORD OUTPUT,
       OUTCNT                         FILE RECORD OUTPUT,

     1 OUT,
       2 DEP         PIC'9',
       2 CEARN1      PIC'99999',
       2 CEARN2      PIC'99999',
       2 DUI1      PIC'99',
       2 CUI1      PIC'99',
       2 CUI2      PIC'99',
       2 WEEKS     PIC'999',

    1 TEMP,
      2 RND   PIC'999',
      2 FILL  CHAR(77),
```

```
        1 ROE,
           2  SIN                 CHAR(8),
           2  SIN2                PIC'(1)9',
           2  MONTH(168),
              3  ROESTAT          CHAR(1),
              3  INWK1            CHAR(2),
              3  INEARN1          CHAR(5),
              3  PCODE11          CHAR(1),
              3  PCODE12          CHAR(5),
              3  EMPL1            CHAR(9),
              3  SIC1             CHAR(4),
              3  REASON1         CHAR(1),
              3  INWK2           CHAR(2),
              3  INEARN2         CHAR(5),
              3  PCODE21         CHAR(1),
              3  PCODE22         CHAR(5),
              3  EMPL2           CHAR(9),
              3  SIC2            CHAR(4),
              3  REASON2         CHAR(1),


      1 UI,
           2  SIN                 CHAR(8),
           2  SIN2                PIC'(1)9',
           2  AGE                  CHAR(2),
           2  SEX                  CHAR(1),
           2  MONTH(168),
              3  BEN               PIC'9',
              3  DIST              CHAR(4),
              3  PROV              CHAR(2),
              3  REG               CHAR(2),
              3  DPND              CHAR(1),
              3  BRATE             CHAR(4),
              3  STUD              CHAR(1),
              3  INWK              CHAR(2),
              3  OCC               CHAR(7),
              3  SIC               CHAR(4),
              3  WKPD              CHAR(3),


1 IA,
 2 FID1          CHAR(2),
 2 FID2          PIC'99999999',
 2 SIN           CHAR(8),
 2  SIN2                 PIC'(1)9',
 2 BYR           PIC'99',
 2 BMO           PIC'99',
 2 SEX           CHAR(1),
 2 TMP(169)      PIC'9',
 2 CAT(169)      PIC'9',
 2 CLASS(169)    PIC'9',
 2 DPNO(169)     PIC'9';


 ON ENDFILE(IAIN)  EOF = '1'B;
 ON ENDFILE(ROEIN) BEGIN;ROE.SIN = '99999999';
 PUT SKIP EDIT('END OF ROE FILE')(A);END;
 ON ENDFILE(UIIN) BEGIN;UI.SIN = '99999999';
 PUT SKIP EDIT('END OF UI FILE')(A);END;
 READ FILE(ROEIN) INTO(ROE);
 READ FILE(IAIN) INTO(IA);
 READ FILE(UIIN) INTO(UI);

 /* READS IN A FILE WITH THE NUMBERS WHICH CORRESPOND TO
```

```
  JANUARY 1987 TO DECEMBER 1990 (85 TO 132)  */

DO I = 1 TO 48;
  READ FILE(RANDIN) INTO (TEMP);
  RAND(I) = RND;
END;

DO WHILE(¬EOF);
IF ROE.SIN < IA.SIN THEN
READ FILE(ROEIN) INTO(ROE);
ELSE IF UI.SIN < IA.SIN THEN
READ FILE(UIIN) INTO(UI);

ELSE BEGIN;

    /* WELFARE (IA) FILE IS IN REVERSE ORDER TO UI AND
       ROE FILES.  THIS PUTS THE VARIABLE PGM INTO THE RIGHT ORDER.*/

    DO I = 1 TO 169;
      PGM(170-I) = IA.TMP(I);
    END;

    /* RANDOMLY SEARCHES PERIOD FROM JANUARY 1987 TO DECEMBER
       FOR A MONTH IN WHICH THE INDIVIDUAL RECEIVED WELFARE. */

    K = K + 1;
    IF K > 47 THEN K = 0;
    J = K;
    FOUND = '0'B;
    DO I = 1 TO 48 WHILE(¬FOUND);
      J = J + 1;
      IF J > 48 THEN J = 1;
      IF PGM(RAND(J)) = 3 THEN BEGIN;
        FOUND = '1'B;
        STT = RAND(J);
      END;
    END;
    IF I < 48 THEN BEGIN;

  /* COUNT THE NUMBER OF MONTHS SINCE LAST RECEIPT OF UI.  */

  DUI1 = 0;
  FOUND = '0'B;
  IF UI.SIN = IA.SIN THEN
  DO I = STT-1 TO STT - 24 BY -1 WHILE(¬FOUND);
    IF UI.BEN(I) > 0 THEN BEGIN;
      DUI1 = STT - I;
      FOUND = '1'B;
    END;
  END;

  /* COUNT THE NUMBER OF MONTHS OF UI BENEFITS IN EACH OF THE
     PREVIOUS TWO YEARS.  */

  CUI1,CUI2 = 0;
  IF UI.SIN = IA.SIN THEN
  DO I = STT-12 TO STT - 1;
    IF UI.BEN(I) > 0 THEN CUI1 = CUI1 + 1;
  END;
  IF UI.SIN = IA.SIN THEN
  DO I = STT-24 TO STT - 13;
    IF UI.BEN(I) > 0 THEN CUI2 = CUI2 + 1;
  END;
```

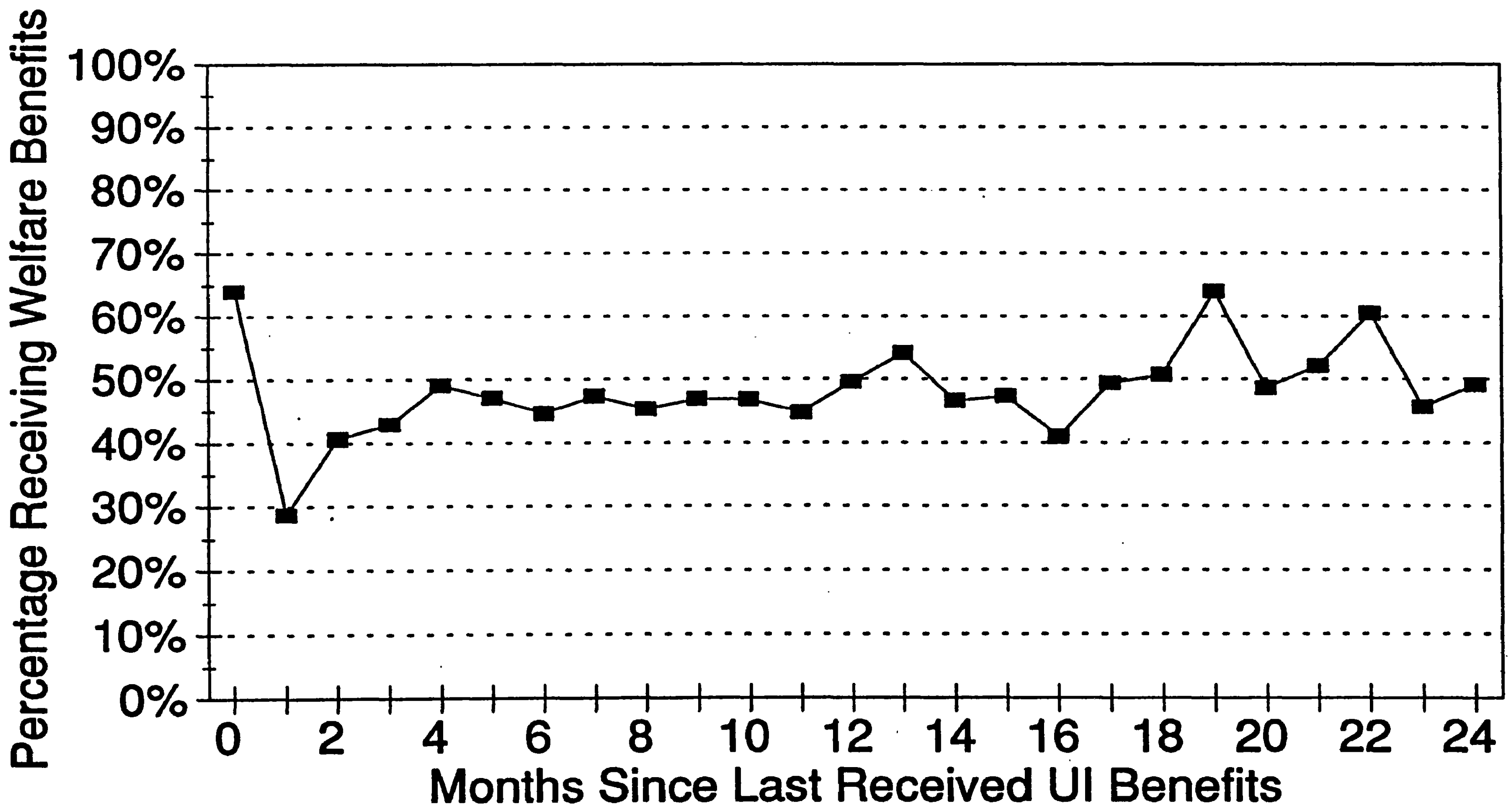
```
/* COUNT THE NUMBER OF INSURABLE WEEKS ON ROE'S ISSUED
   WITHIN THE PREVIOUS FOUR MONTHS.  */

TMPWKS,WEEKS,CWKS = 0;
IF ROE.SIN = IA.SIN THEN
DO I = STT-4 TO STT - 1;
  IF INWK1(I) ¬= '**' THEN TMPWKS = INWK1(I);
  IF TMPWKS > WEEKS THEN WEEKS = TMPWKS;
  IF INWK2(I) ¬= '**' THEN TMPWKS = INWK2(I);
  IF TMPWKS > WEEKS THEN WEEKS = TMPWKS;
END;

 /* SUM THE INSURABLE EARNINGS OVER EACH OF THE PREVIOUS 2 YEARS */

CEARN1,CEARN2 = 0;
IF ROE.SIN = IA.SIN THEN
DO I = STT-12 TO STT - 1;
  IF INEARN1(I) ¬='******' & INWK1(I) ¬= '**' & INWK1(I) ¬= '00'
  THEN CEARN1 = CEARN1 + 4.3*INEARN1(I)/INWK1(I);
  ELSE IF INEARN2(I) ¬='******' & INWK2(I) ¬= '**' & INWK2(I) ¬= '00'
  THEN CEARN1 = CEARN1 + 4.3*INEARN2(I)/INWK2(I);
END;

IF ROE.SIN = IA.SIN THEN
DO I = STT-24 TO STT - 13;
  IF INEARN1(I) ¬='******' & INWK1(I) ¬= '**' & INWK1(I) ¬= '00'
  THEN CEARN2 = CEARN2 + 4.3*INEARN1(I)/INWK1(I);
  ELSE IF INEARN2(I) ¬='******' & INWK2(I) ¬= '**' & INWK2(I) ¬= '00'
  THEN CEARN2 = CEARN2 + 4.3*INEARN2(I)/INWK2(I);
END;

 /* FIND PERCENTAGE ON WELFARE 6 MONTHS LATER BY EACH VARIABLE. */
 IF PGM(STT + 6) > 0 THEN DEP = 1; ELSE DEP = 0;

 DIFF = CEARN1 - CEARN2;
 INDDIFF = TRUNC(DIFF/1000);
 INDDIFF = INDDIFF + 10;
 IF INDDIFF < 0 THEN INDDIFF = 0;
 IF INDDIFF > 20 THEN INDDIFF = 20;
 DISTDIF(INDDIFF) = DISTDIF(INDDIFF) + 1;
 IF PGM(STT + 6) > 0 THEN DISTDIFP(INDDIFF) = DISTDIFP(INDDIFF) + 1;

 DISTUI(CUI1) = DISTUI(CUI1) + 1;
 IF PGM(STT + 6) > 0 THEN DISTUIP(CUI1) = DISTUIP(CUI1) + 1;

 DISTDUI(DUI1) = DISTDUI(DUI1) + 1;
 IF PGM(STT + 6) > 0 THEN DISTDUIP(DUI1) = DISTDUIP(DUI1) + 1;

 DISTUI2(CUI2) = DISTUI2(CUI2) + 1;
 IF PGM(STT + 6) > 0 THEN DISTUIP2(CUI2) = DISTUIP2(CUI2) + 1;

 IF WEEKS > 50 THEN WEEKS = 50;
 DISTINWK(WEEKS) = DISTINWK(WEEKS) + 1;
 IF PGM(STT + 6) > 0 THEN DISTINWKP(WEEKS) = DISTINWKP(WEEKS) + 1;

 IF CEARN1 = 0 THEN INDEARN = 0;
 ELSE INDEARN = TRUNC(CEARN1/100) + 1;
 IF INDEARN > 20 THEN INDEARN = 20;
 DEARN(INDEARN) = DEARN(INDEARN) + 1;
 IF PGM(STT+6) > 0 THEN DEARNP(INDEARN)=DEARNP(INDEARN) + 1;

 IF CEARN1 = 0 THEN INDEARNH = 0;
 ELSE INDEARNH = TRUNC(CEARN1/1000) + 1;
 IF INDEARNH > 20 THEN INDEARNH = 20;
```

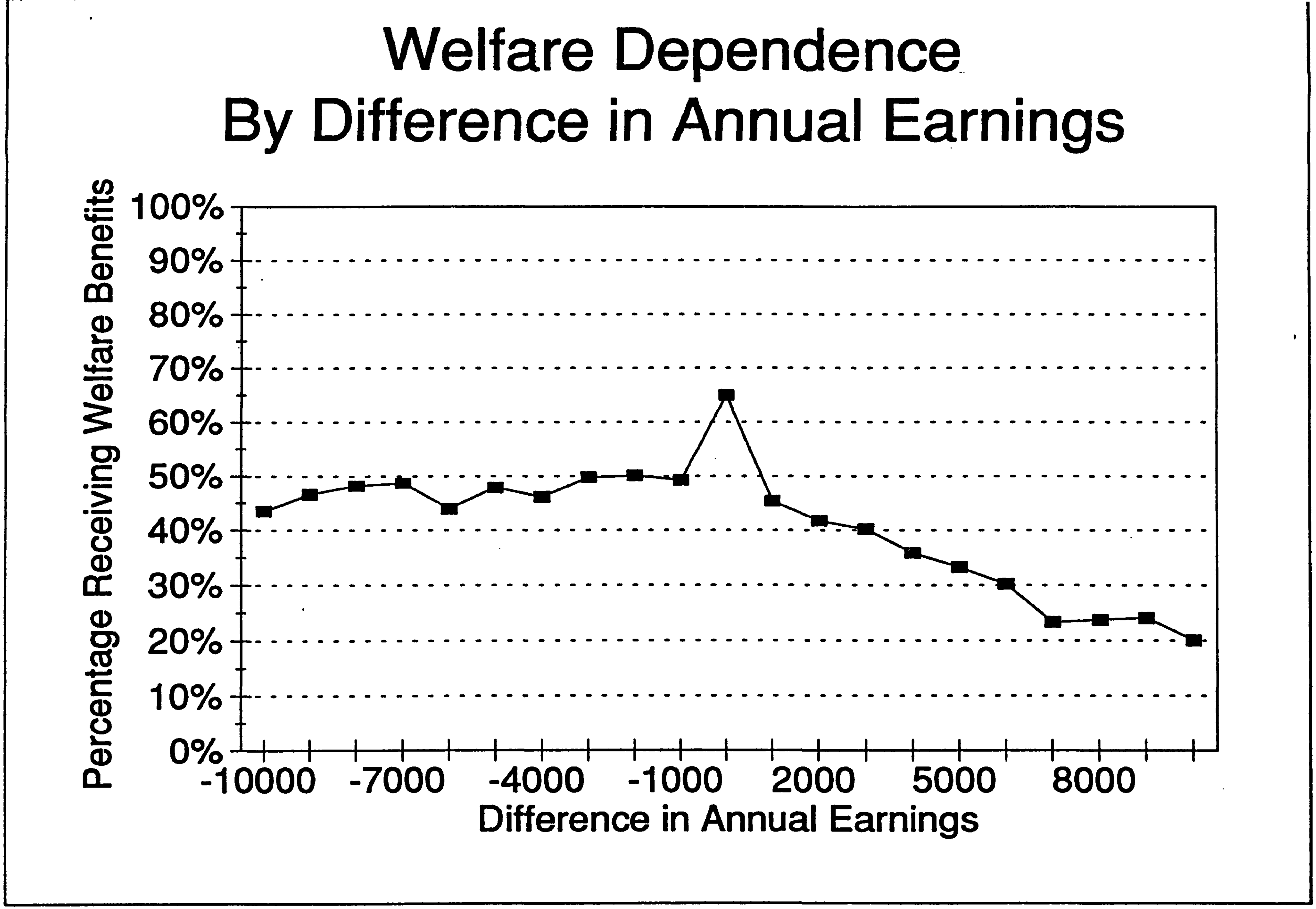
```
    DEARNH(INDEARNH) = DEARNH(INDEARNH) + 1;
    IF PGM(STT+6)>0 THEN DEARNHP(INDEARNH)=DEARNHP(INDEARNH) + 1;

    IF CEARN2 = 0 THEN INDEARN = 0;
    ELSE INDEARN = TRUNC(CEARN2/100) + 1;
    IF INDEARN > 20 THEN INDEARN = 20;
    DEARN2(INDEARN) = DEARN2(INDEARN) + 1;
    IF PGM(STT+6) > 0 THEN DEARNP2(INDEARN)=DEARNP2(INDEARN) + 1;

    IF CEARN2 = 0 THEN INDEARNH = 0;
    ELSE INDEARNH = TRUNC(CEARN2/1000) + 1;
    IF INDEARNH > 20 THEN INDEARNH = 20;
    DEARNH2(INDEARNH) = DEARNH2(INDEARNH) + 1;
    IF PGM(STT+6)>0 THEN DEARNHP2(INDEARNH)=DEARNHP2(INDEARNH) + 1;

    WRITE FILE(OUTFILE) FROM (OUT);
    END;  /* I < 48 */
    READ FILE(IAIN) INTO(IA);
    END;
  END; /*NOT EOF*/
  DO I = 0 TO 24;
    PUT SKIP EDIT(DISTDUI(I),DISTDUIP(I))
    ((2)(X(1),P'(9)9'));
  END;

  PUT SKIP EDIT(' ')(A);

  DO I = 0 TO 20;
    PUT SKIP EDIT(DISTDIF(I),DISTDIFP(I))
    ((2)(X(1),P'(9)9'));
  END;

  PUT SKIP EDIT(' ')(A);

  DO I = 0 TO 12;
    PUT SKIP EDIT(DISTUI(I),DISTUI2(I),DISTUIP(I),DISTUIP2(I))
    ((4)(X(1),P'(9)9'));
  END;

  PUT SKIP EDIT(' ')(A);

  DO I = 0 TO 50;
    PUT SKIP EDIT(DISTINWK(I),DISTINWKP(I))
    ((2)(X(1),P'(9)9'));
  END;

  PUT SKIP EDIT(' ')(A);

  DO I = 0 TO 20;
    PUT SKIP EDIT(DEARN(I),DEARNP(I),DEARNH(I),DEARNHP(I))
    ((2)(X(1),P'(9)9'));
  END;

  PUT SKIP EDIT(' ')(A);

  DO I = 0 TO 20;
    PUT SKIP EDIT(DEARN2(I),DEARNP2(I),DEARNH2(I),
    DEARNHP2(I))((2)(X(1),P'(9)9'));
  END;
 END MAIN;
/*
//GO.ROEIN  DD DSN=HRRSD.E1.ROELONG.AUG2694,UNIT=TAPE,DISP=SHR
//UIIN     DD DSN=HRRSD.E1.UILONG.AUG2694,UNIT=TAPE,DISP=SHR
//IAIN     DD  DSN=HRRSD.E1.IALONG.AUG2694,DISP=SHR
```

```
//RANDIN   DD DSN=HRRSD.E1.FORC94(TEMP),DISP=SHR
//OUTFILE DD DSN=HRRSD.E1.CALCGRP.SEP1894,UNIT=DISK,
//          DISP=OLD
//*      DISP=(NEW,CATLG,KEEP),
//*       DCB=(LRECL=20,RECFM=FB),
//*       SPACE=(TRK,(50,5),RLSE)
//*GO.INFILE DD DUMMY
//*OUTFILE DD DUMMY
```

Graphs of Output of PL1 Program PRONIA

Welfare Dependence
By Months since Last UI Dependence





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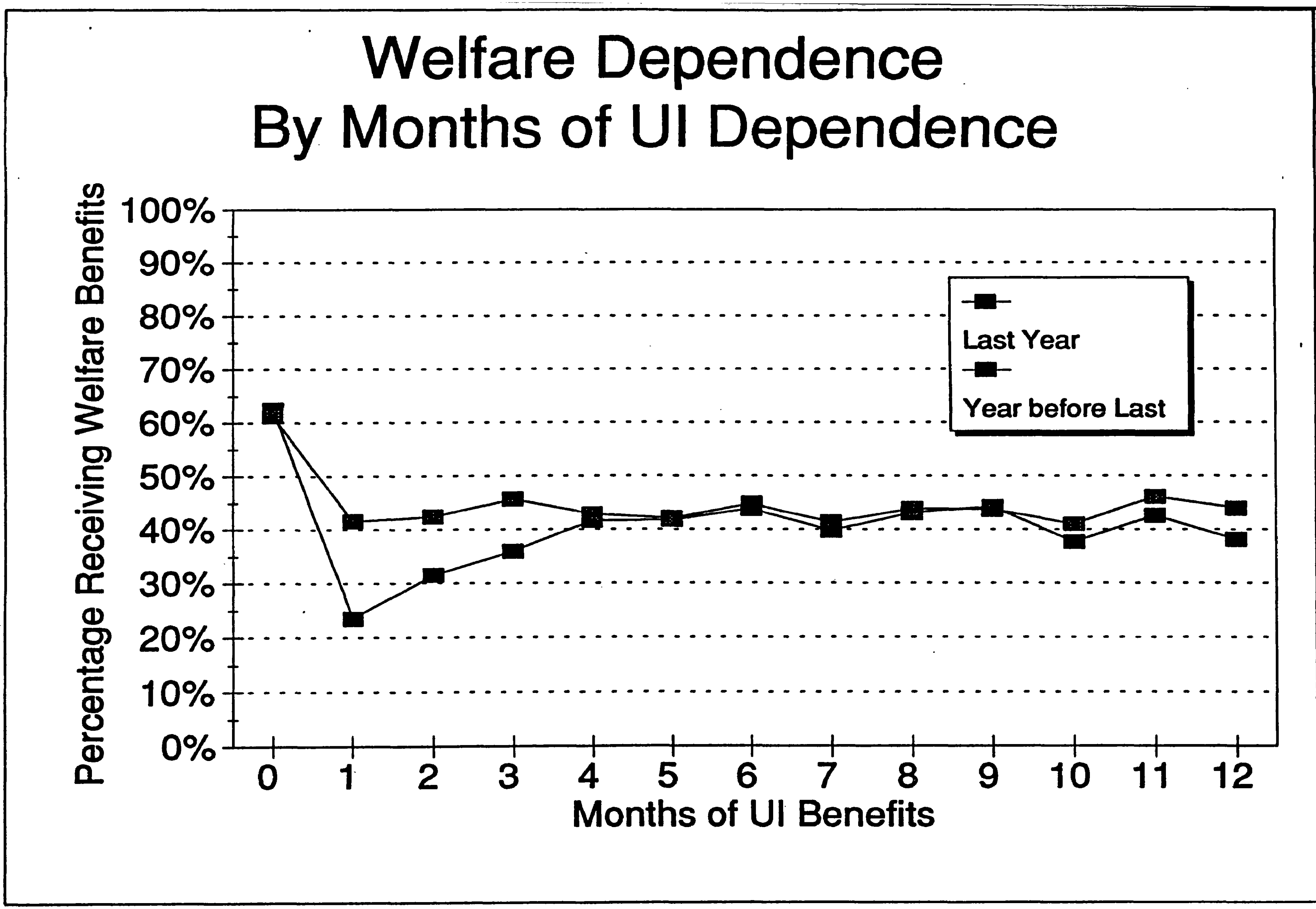
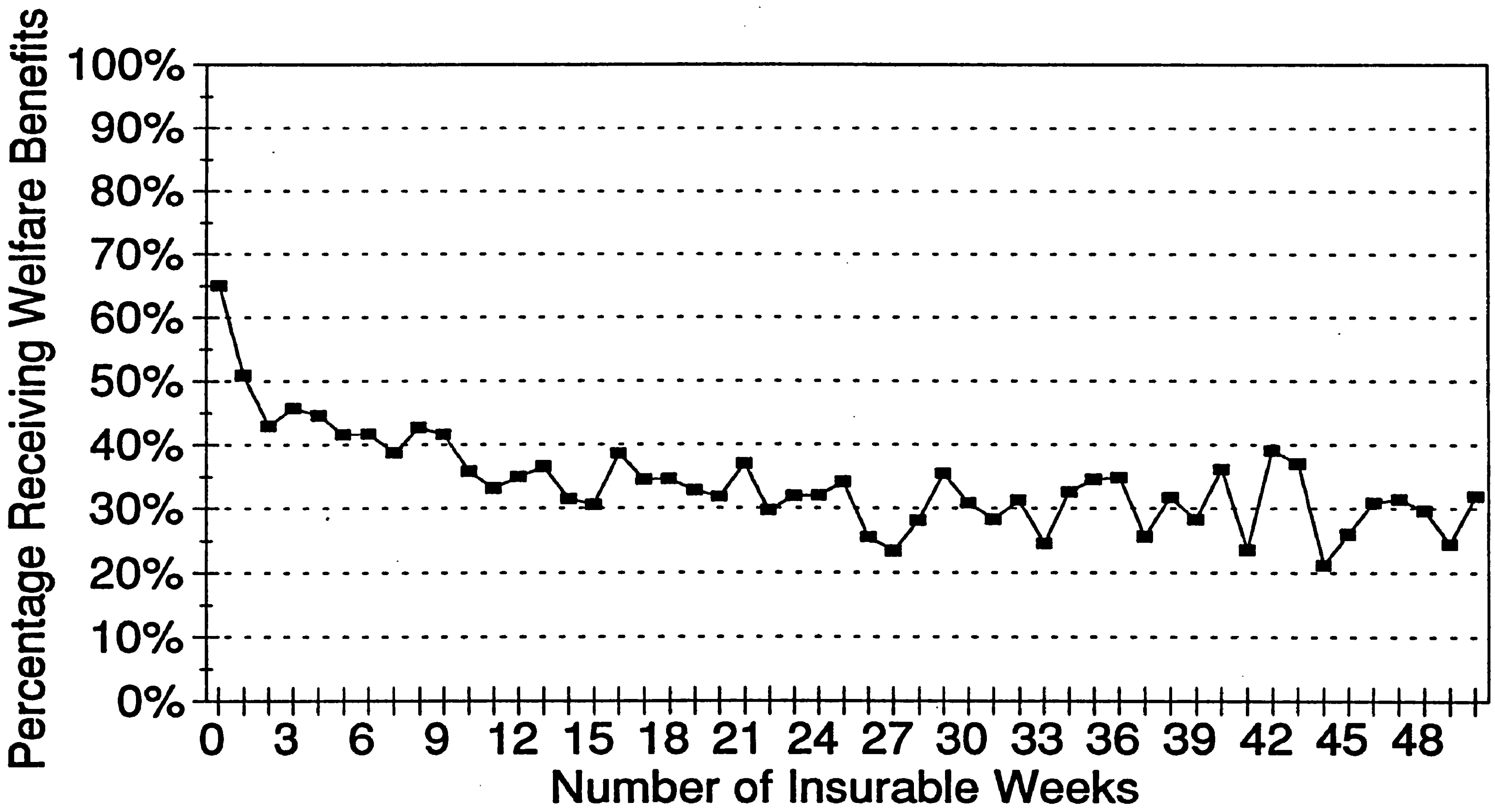
10/21/94

C:\GROUPS.

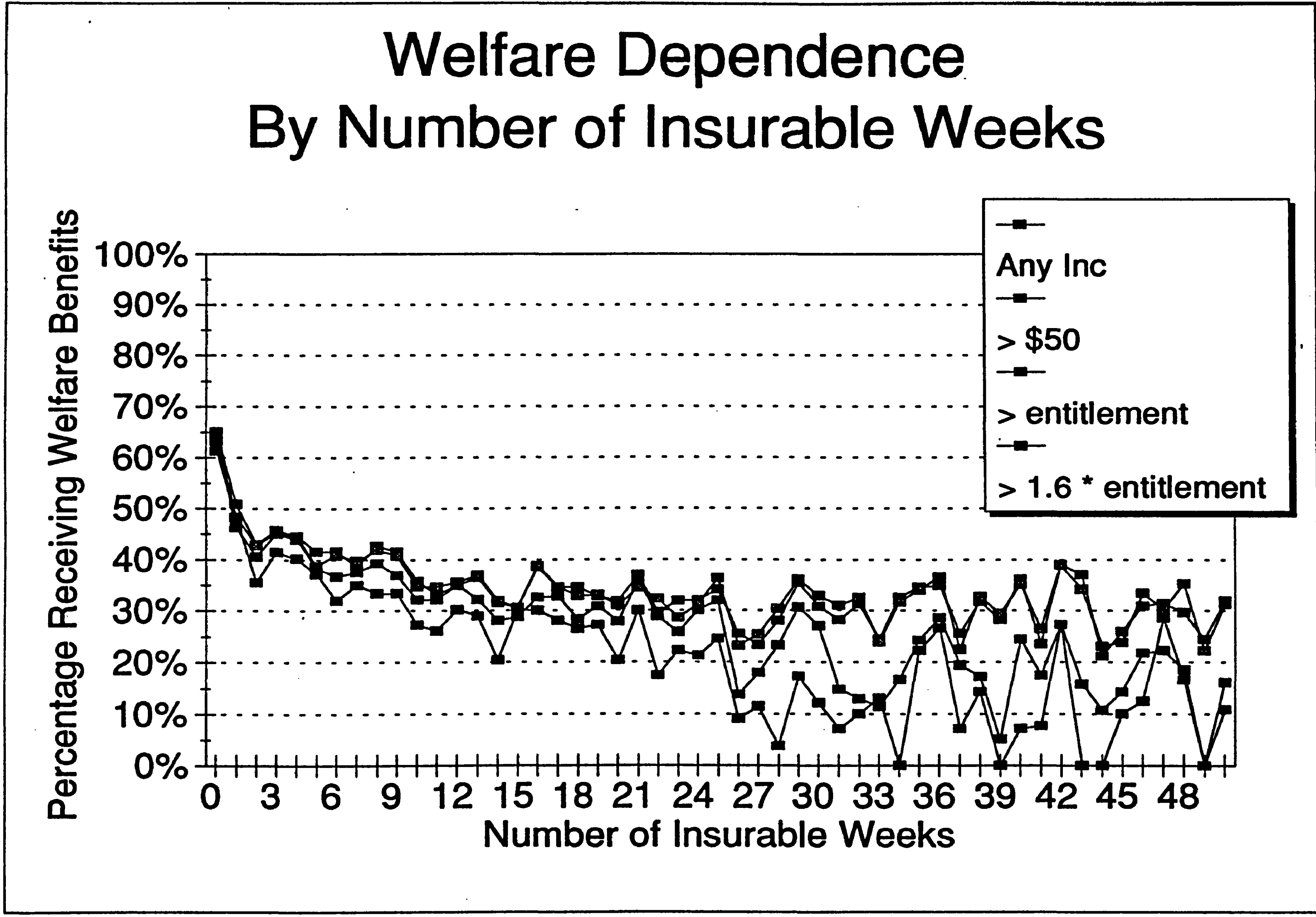


Welfare Dependence
By Months of UI Dependence
Percentage Receiving Welfare Benefits
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
0
1
2
3
4
5
6
7
8
9
10
11
12
Months of UI Benefits
Last Year
Year before Last

Welfare Dependence
By Number of Insurable Weeks

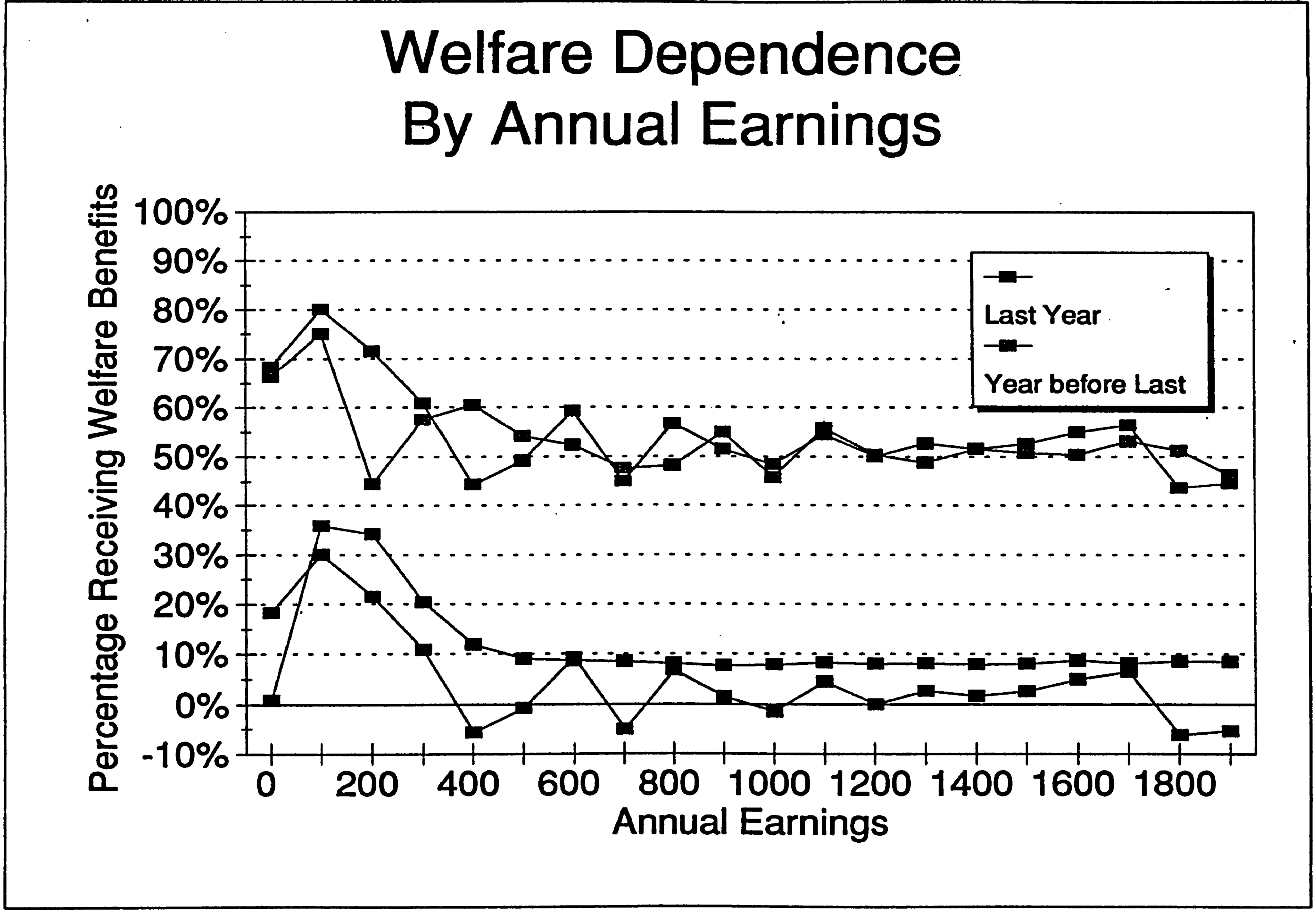


LC0266



10/21/94

C:\GROUPS.



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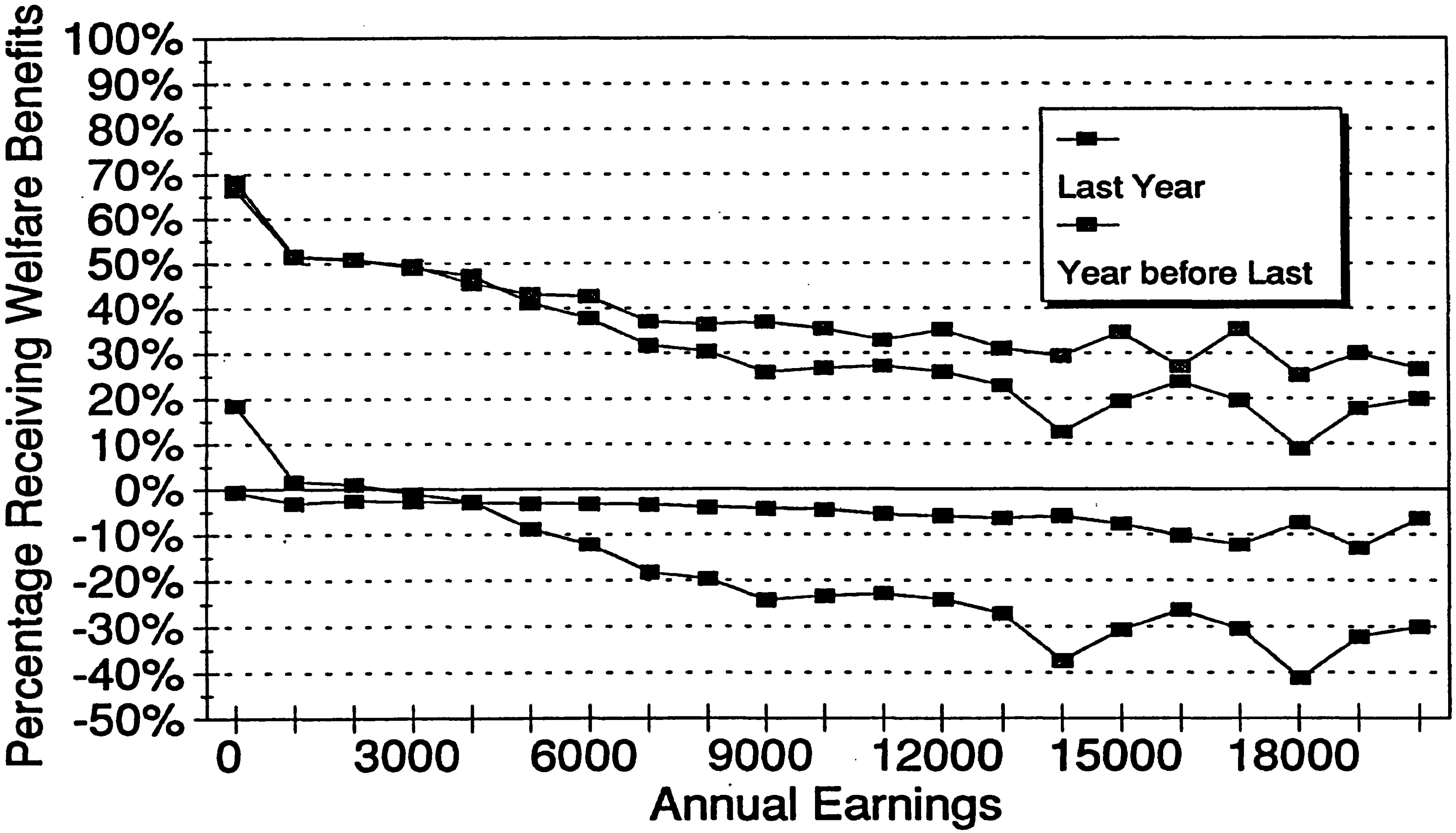
C:\GROUPS.

Welfare Dependence By Annual Earnings



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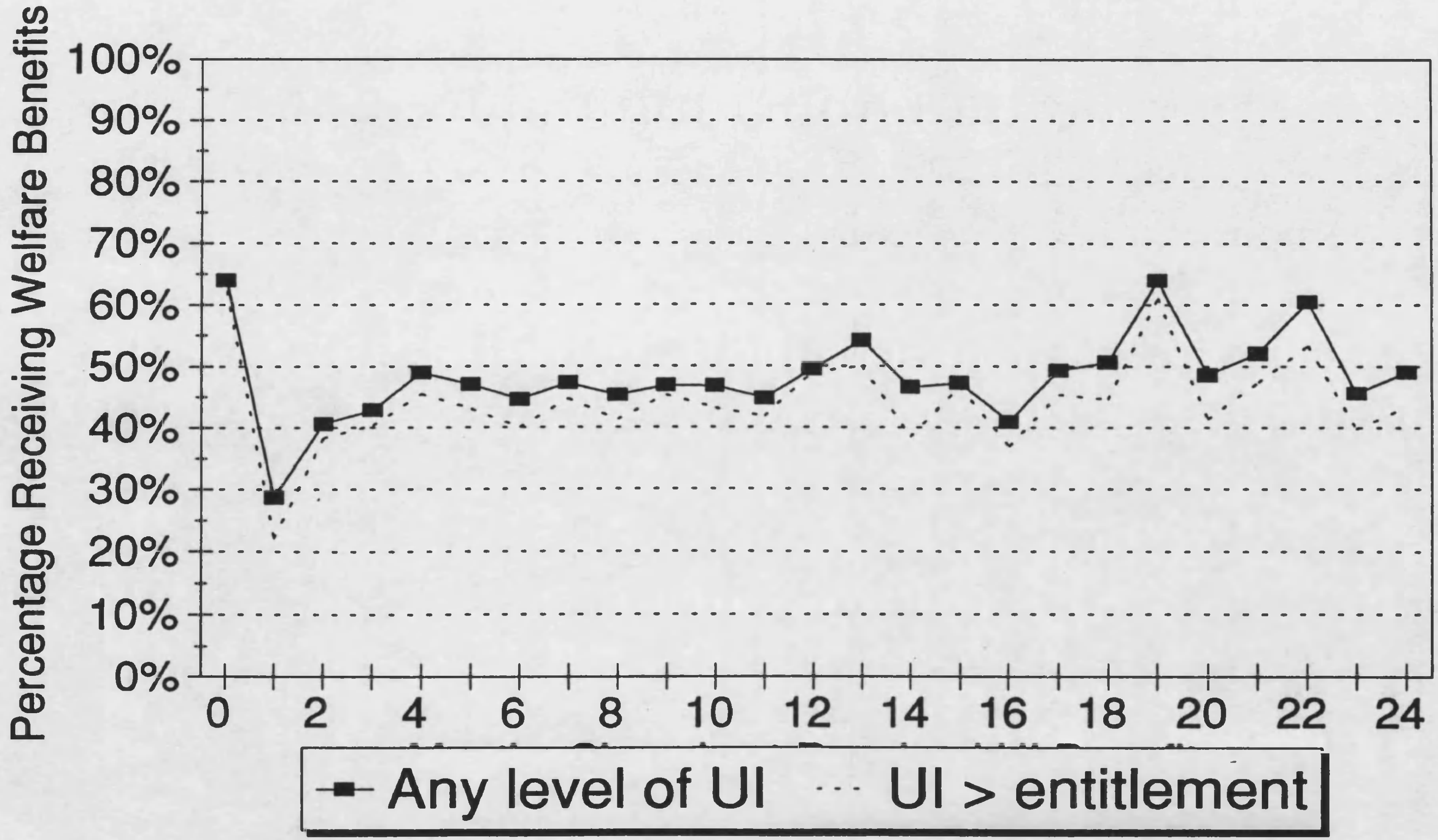
CAGROUPS.

Welfare Dependence
By Months since Last UI Dependence



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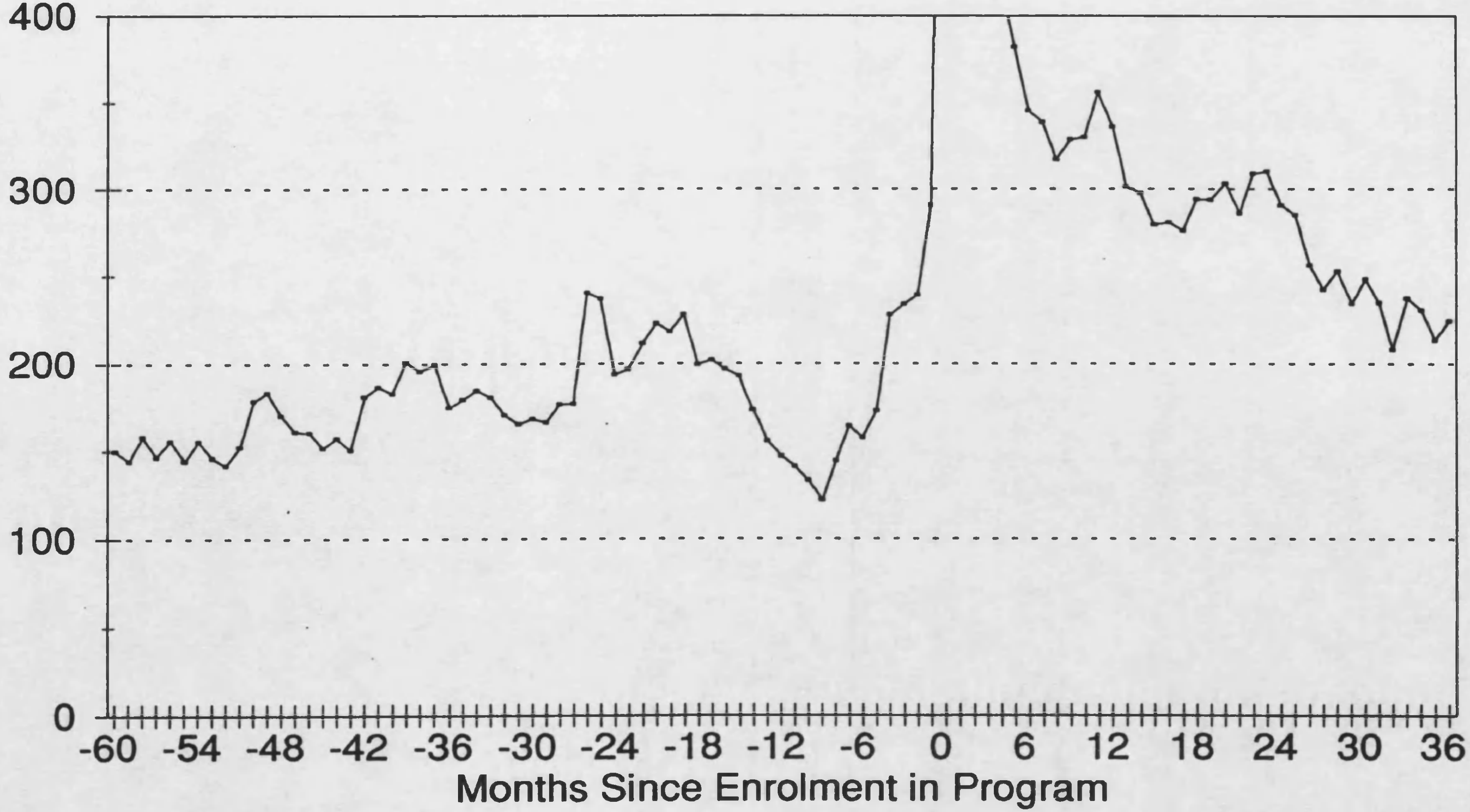
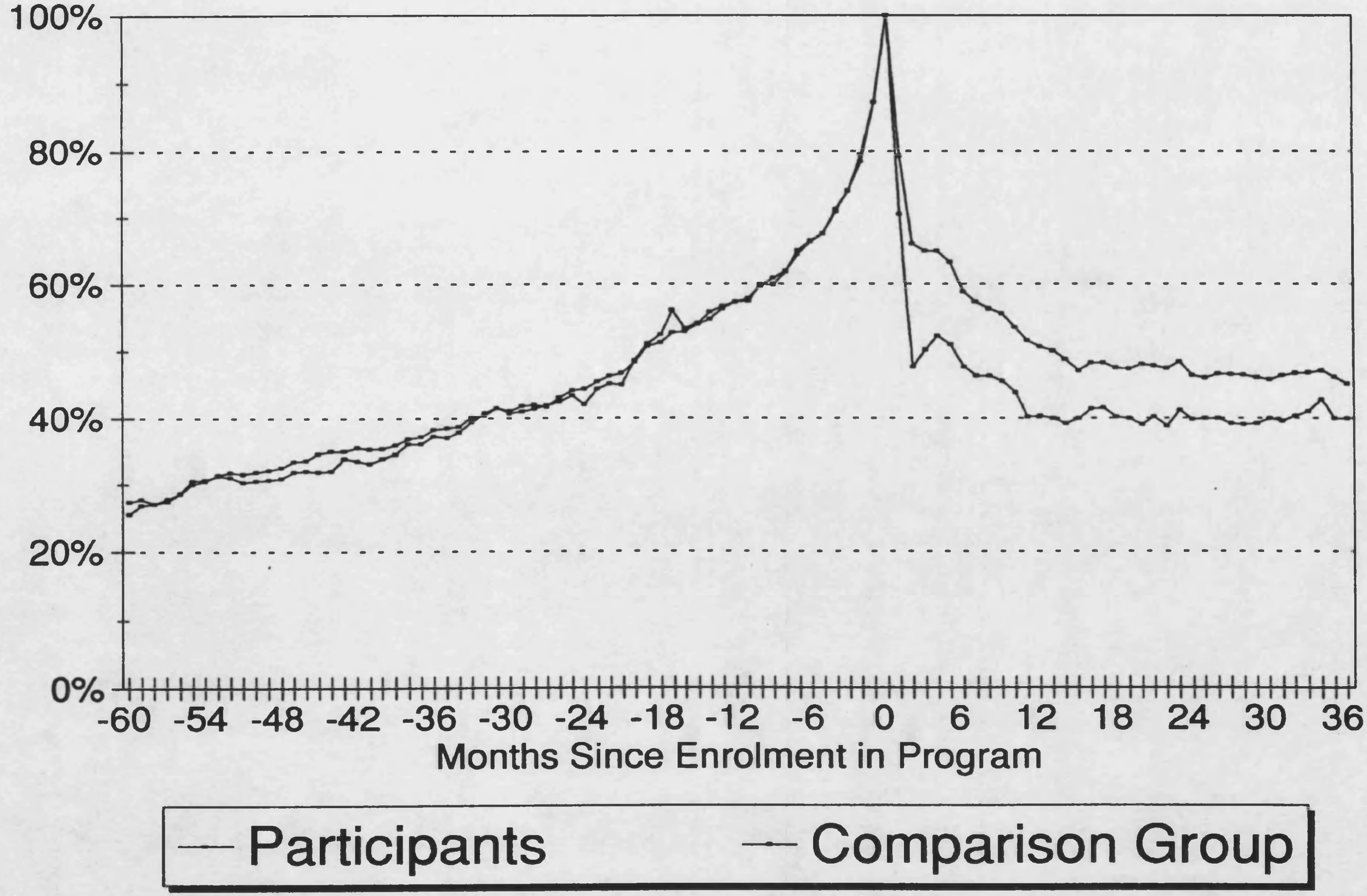
Graphs of Output of PL1 Program PPD6

Pre-Program Dip

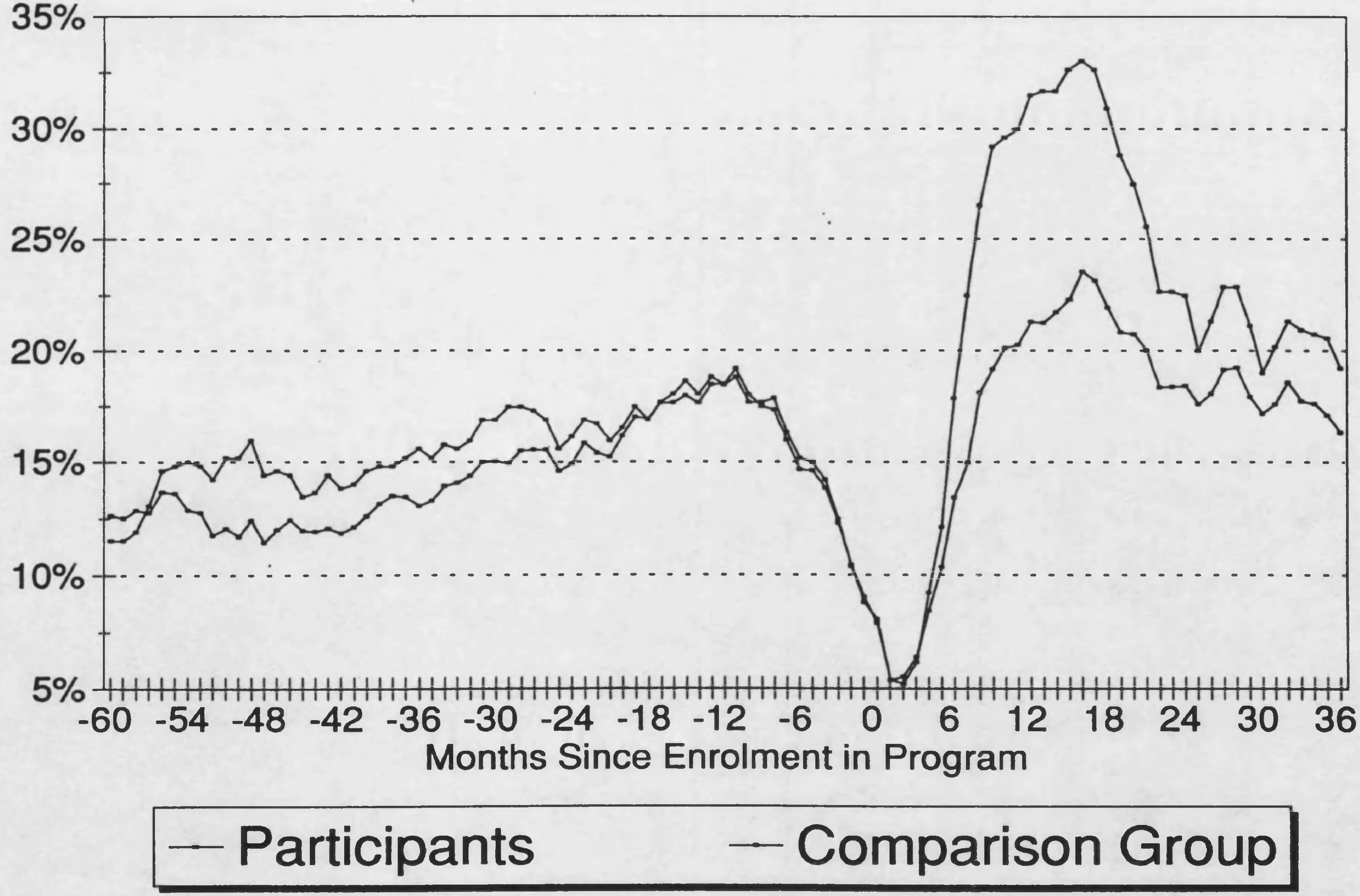
Average Monthly Earnings



Welfare Dependence



Unemployment Insurance Dependence



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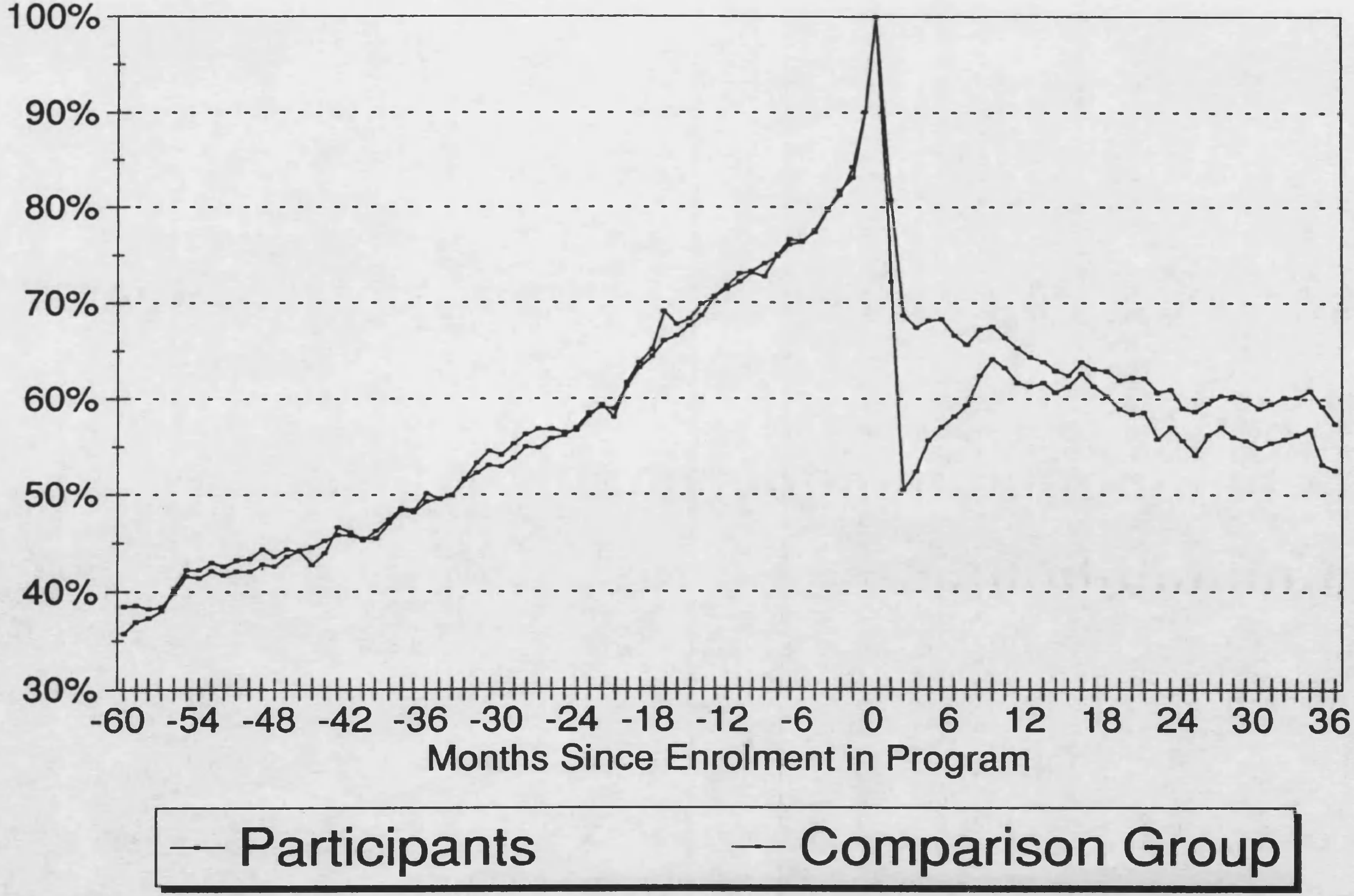
UI or Welfare Dependence



000275

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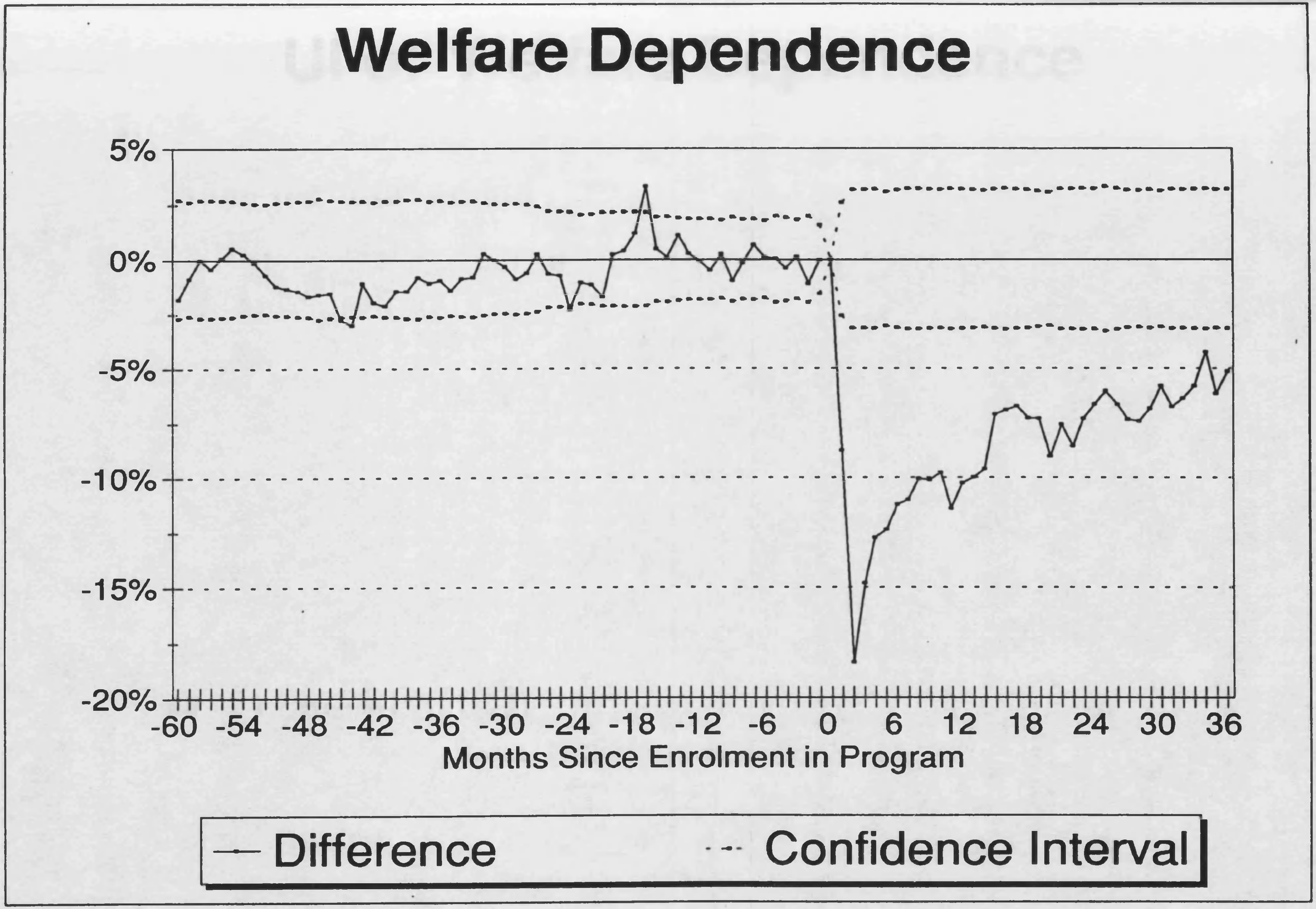
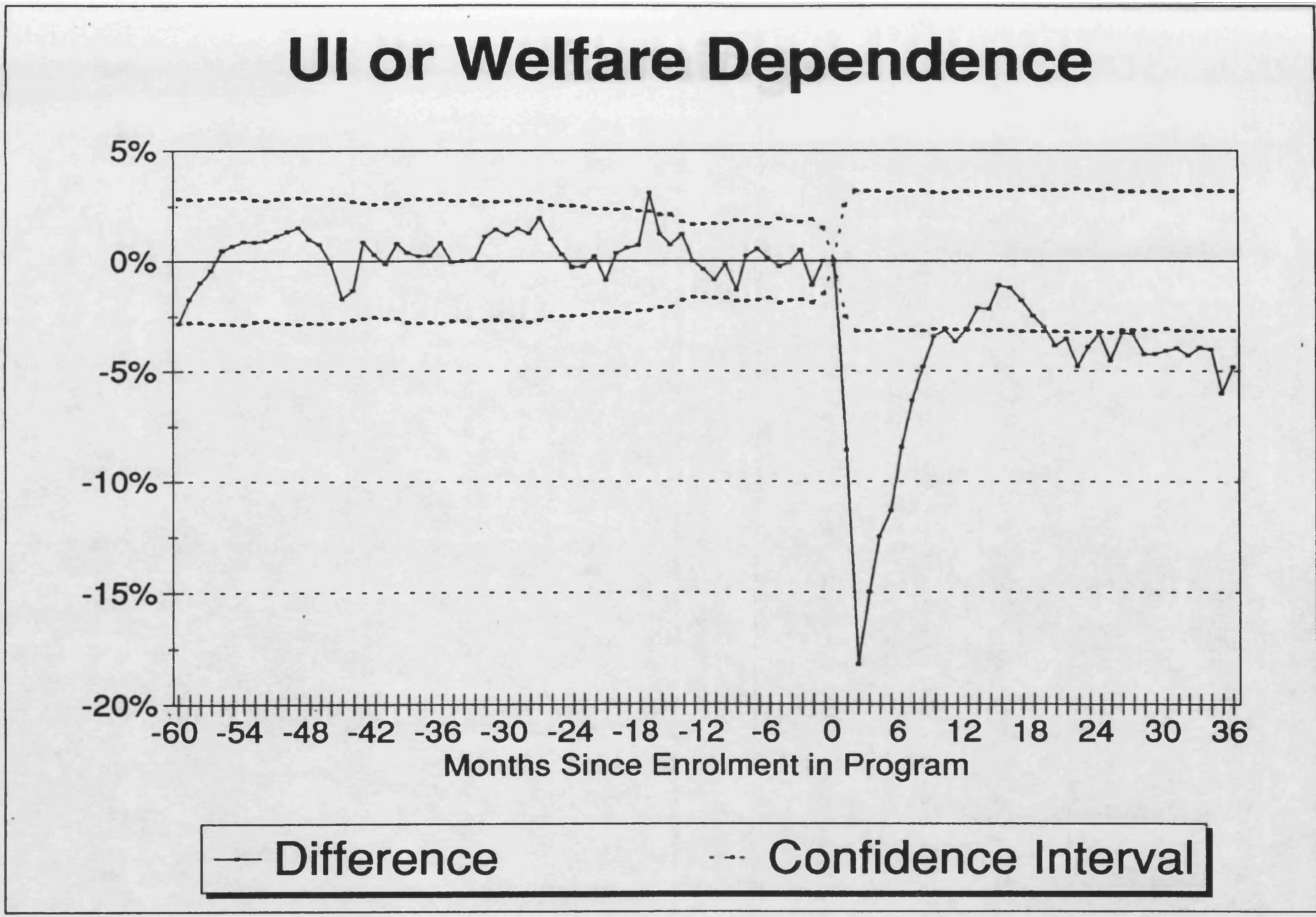

Welfare Dependence
5%
0%
-5%
-10%
-15%
-20%
-60 -54 -48 -42 -36 -30 -24 -18 -12 -6 0 6 12 18 24 30 36
Months Since Enrolment in Program
Difference
Confidence Interval



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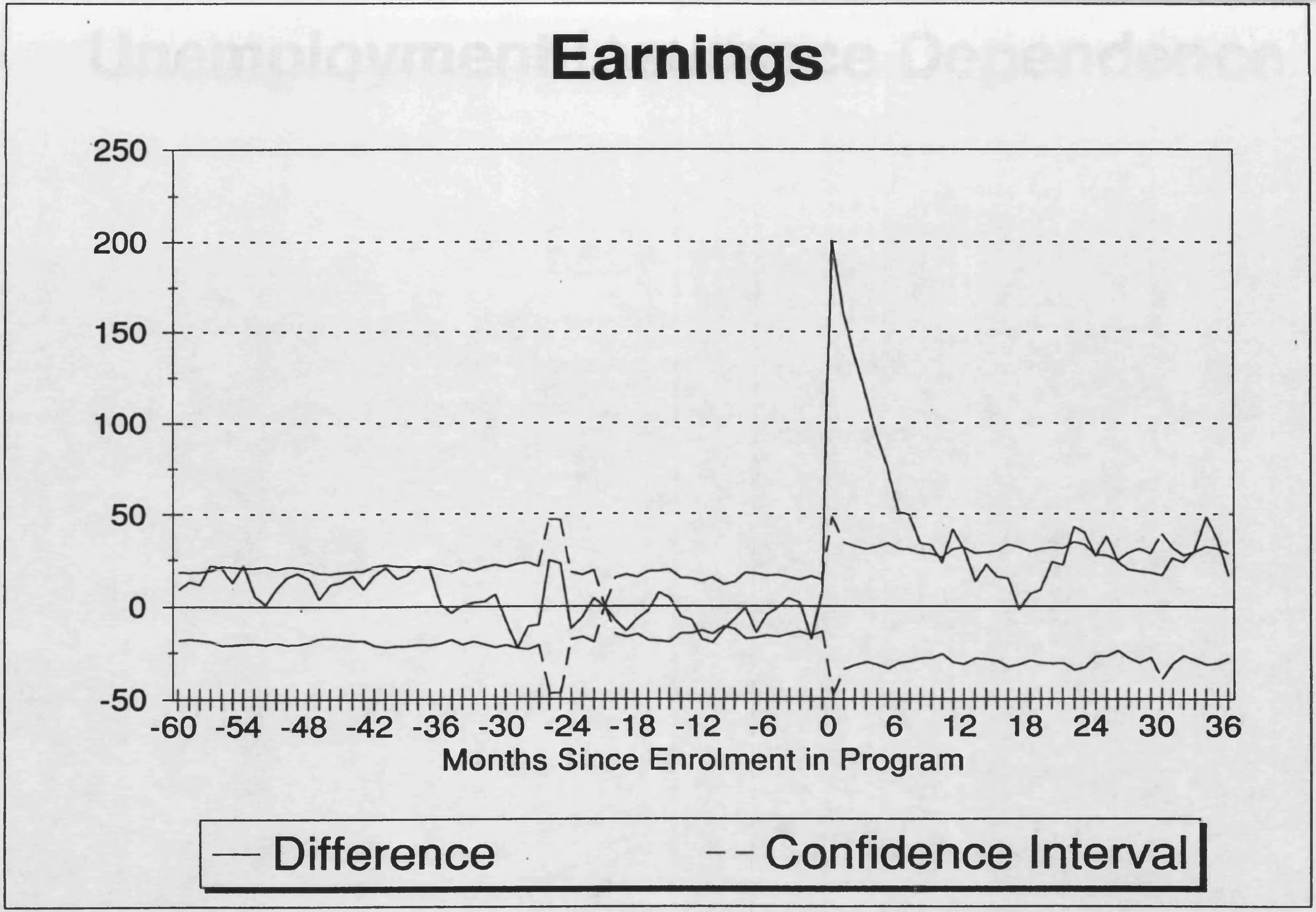

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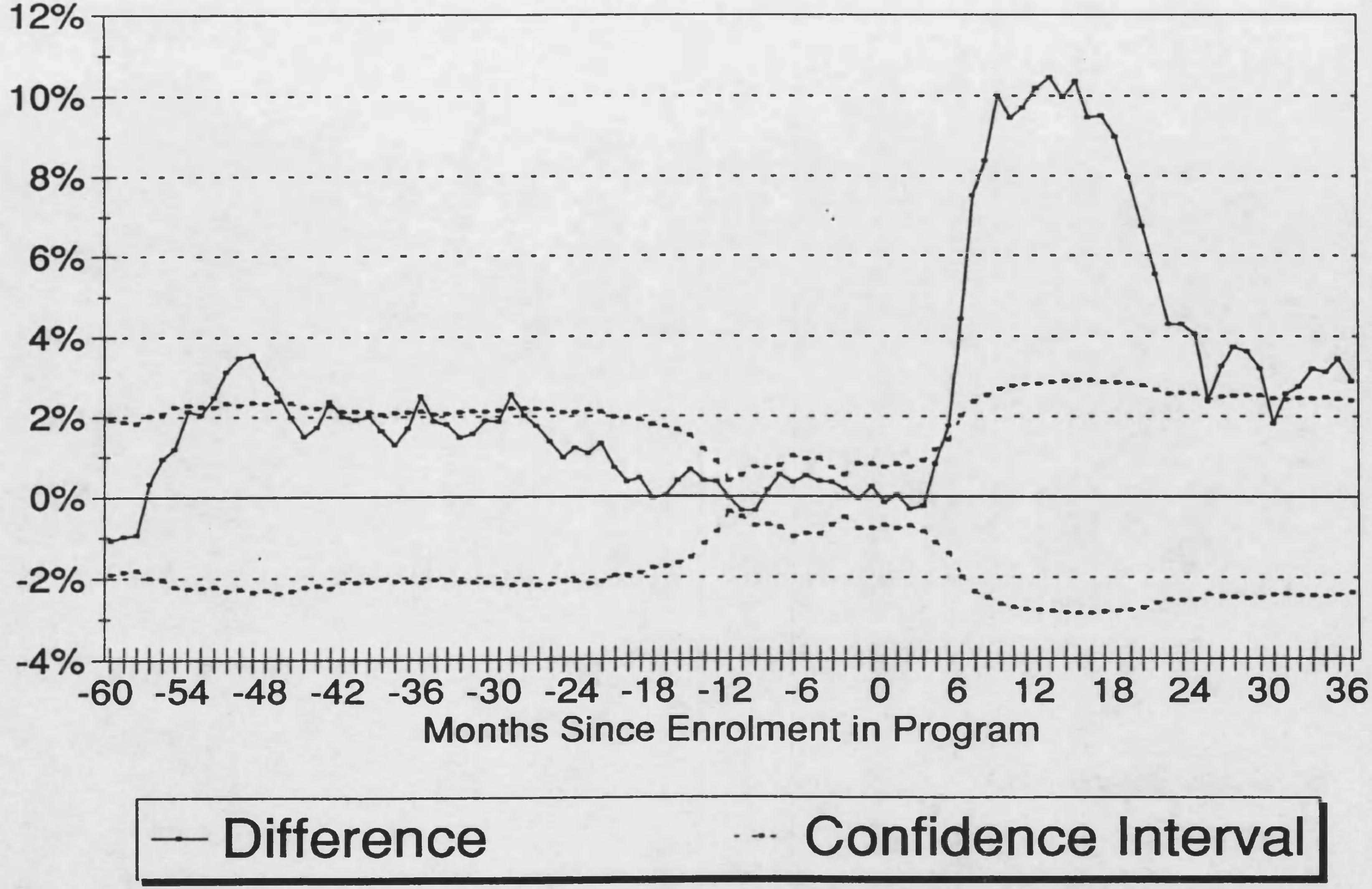
Unemployment Insurance Dependence



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Appendix B: Impacts On UI Dependence

Estimating The Impact On Unemployment Insurance Dependence

An information exchange agreement with Employment and Immigration Canada made Unemployment Insurance dependence information on a one-in-ten sample of our recipients available for the purposes of this study. Our challenge was to track the UI dependence of the program participants and to estimate the level of dependence which these recipients would have experienced had they not participated in the programs. For the latter, we drew a sample of roughly 9000 Income Assistance recipients who did not participate in a program and tracked their subsequent UI dependence. For each month following the month in which they were selected we modelled the probability of receiving UI as a function of various personal characteristics. The variables used were:

- CLASS which took the value one if the recipient was classified as unemployable;
- CAT1 which took the value one if the recipient was a single female;
- CAT2 which took the value one if the case was a couple;
- CAT3 which took the value one if the case was a two-parent family;
- CAT4 which took the value one if the case was a one-parent family;
- IAHIST the number of months of IA benefits paid to the case in the previous 25 months;
- UIHIST the number of months of UI benefits paid to the recipient in the previous 24 months;
- DUI which took the value one if the recipient had received any UI in the previous two years;
- UI2 UIHIST squared;
- ONUISTRT which took the value one if the recipient received any UI in the month in which he/she entered the program or was selected for the comparison group;

- PGM1 which took the value one if the recipient participated in CTETP;
- PGM2 which took the value one if the recipient participated in EOP;
- PGM3 which took the value one if the recipient participated in EYC;
- PGM6 which took the value one if the recipient participated in FEP.

The results of these 36 regressions were used to simulate the UI dependence of the program participants in each month by multiplying the average value for each characteristic of the participants in each program by the relevant coefficient. This value could then be compared with the actual percentage of the participants receiving UI.

The technique used was OLS, which is unbiased but inefficient. Efficiency was not considered to be a major consideration, given the sample size of the comparison group. Using OLS enabled us to do the simulations without converting the hundreds of z values to probabilities, as would have been required if we had used the fully efficient maximum-likelihood probit analysis.

The regression results for four of the months follow.

ι C0285

```
*-* LIMDEP *-* File created 06/08/92 / 15:09:16

MODEL COMMAND:
REGRESS;LHS=ONUI3M;RHS=REGS$

Ordinaryleast squares regression.         Dep. Variable            = ONUI3M
Observations                   = 10688       Weights                = ONE
Mean of LHS              = 0.4425524E-01   Std Dev of LHS             = 0.2056713E+00
StdDev of residuals      = 0.1816618E+00   Sum of squares             = 0.3522199E+03
R-squared                = 0.2208684E+00   Adjusted R-squared         = 0.2198464E+00
F[ 14, 10673]            = 0.2161132E+03
Log-likelihood           = 0.3071433E+04       Restr b=0) Log-l       = 0.1737702E+04
Amemiya Pr. Criter.      =-0.5719372E+00     Akaike Info.Crit.        =0.3304734E-01
ANOVA                    Source Variation  Degrees of Freedom          Mean Square
     Regression            0.9984738E+02     14.                        0.7131956E+01
       Residual            0.3522199E+03    10673.                      0.3300102E-01
          Total            0.4520673E+03    10687.                      0.4230067E-01
Durbin-Watson stat.      = 1.9813331         Autocorrelation          = 0.0093334
Variable      Coefficient  Std. Error    t-ratio          Prob|t| >x    Mean of X     Std.Dev.of X
Constant      0.21784      0.1366E-01   15.952           0.00000
CLASS        -0.88481E-02  0.3973E-02   -2.227           0.02594        0.36171       0.48052
CAT1          0.30270E-03  0.5140E-02    0.059           0.95303        0.17683       0.38155
CAT2          0.14784E-01  0.1036E-01    1.428           0.15339        0.31250E-01   0.17400
CAT3          0.18767E-01  0.6829E-02    2.748           0.00599        0.81306E-01   0.27332
CAT4          0.13897E-02  0.4329E-02    0.321           0.74818        0.34113       0.47411
IAHIST       -0.14246E-02  0.2231E-03   -6.386           0.00000       17.463         8.8160
UIHIST       -0.24182E-01  0.2744E-02   -8.814           0.00000        1.9161        4.2763
DUI          -0.17406      0.1310E-01  -13.291           0.00000        0.78948       0.40769
UI2           0.70161E-03  0.1385E-03    5.066           0.00000       21.957        59.814
ONUISTRT      0.37886      0.8985E-02   42.163           0.00000        0.50243E-01   0.21846
PGM1          0.62091E-03  0.1228E-01    0.051           0.95969        0.21239E-01   0.14419
PGM2          0.18568E-01  0.7375E-02    2.518           0.01181        0.61845E-01   0.24089
PGM3         -0.40089E-01  0.4072E-01   -0.984           0.32487        0.18713E-02   0.43220E-01
PGM6          0.22827E-01  0.1886E-01    1.210           0.22621        0.88885E-02   0.93863E-01
```

```
MODEL COMMAND:
REGRESS;LHS=ONUI9M;RHS=REGS$

Ordinaryleast squares regression.        Dep. Variable          = ONUI9M
Observations             = 10688                Weights             = ONE
Mean of LHS                    = 0.7438249E-01        Std.Dev of LHS = 0.2624046E+00
StdDev of residuals            = 0.2463255E+00   Sum of squares         = 0.6475978E+03
R-squared                      = 0.1199514E+00  Adjusted R-squared      = 0.1187970E+00
F[ 14, 10673]                  = 0.1039099E+03
Log-likelihood                 = -0.1831389E+03       Restr.( b=0) Log-1 = -0.8659853E+03
Amemiya Pr. Criter.            = 0.3707688E-01     Akaike Info.Crit.    = 0.6076142E-01
ANOVA     Source                 Variation          Degrees of Freedom     Mean Square
     Regression                 0.8826815E+02              14.              0.6304868E+01
       Residual                 0.6475978E+03           10673.              0.6067627E-01
          Total                 0.7358659E+03           10687.              0.6885617E-01
Durbin-Watson stat.            = 2.0136416       Autocorrelation             =            -0.0068208
```

Variable	Coefficient	Std. Error	t-ratio	Prob|t| >x	Mean of X	Std.Dev.of X
Constant	0.14480	0.1852E-01	7.820	0.00000		
CLASS	-0.31752E-01	0.5387E-02	-5.894	0.00000	0.36171	0.48052
CAT1	-0.11958E-01	0.6969E-02	-1.716	0.08618	0.17683	0.38155
CAT2	0.28421E-02	0.1404E-01	0.202	0.83960	0.31250E-01	0.17400
CAT3	-0.16171E-02	0.9259E-02	-0.175	0.86136	0.81306E-01	0.27332
CAT4	-0.50709E-02	0.5870E-02	-0.864	0.38764	0.34113	0.47411
IAHIST	-0.13603E-02	0.3025E-03	-4.497	0.00001	17.463	8.8160
UIHIST	-0.32249E-02	0.3720E-02	-0.867	0.38603	1.9161	4.2763
DUI	-0.69169E-01	0.1776E-01	-3.895	0.00010	0.78948	0.40769
UI2	0.16501E-03	0.1878E-03	0.879	0.37960	21.957	59.814
ONUISTRT	0.89039E-01	0.1218E-01	7.308	0.00000	0.50243E-01	0.21846
PGM1	0.36331	0.1666E-01	21.811	0.00000	0.21239E-01	0.14419
PGM2	0.16633T0.1000E-01		16.633	0.00000	0.61845E-01	0.24089
PGM3	0.32996	0.5522E-01	5.976	0.00000	0.18713E-02	0.43220E-01
PGM6	0.31304	0.2558E-01	12.239	0.00000	0.88885E-02	0.93863E-01

MODEL COMMAND:
REGRESS;LHS=ONUI18M;RHS=REGS$

Ordinaryleast squares regression.		Dep. Variable		= ONUI18M	
Observations	= 10393	Weights		= ONE	
Mean of LHS	= 0.8717406E-01	Std Dev of LHS		= 0.2821035E+00	
StdDev of residuals	= 0.2739533E+00	Sum of squares		= 0.7788731E+03	
R-squared	= 0.5821763E-01	Adjusted R-squared		= 0.5694716E-01	
F[14, 10378]	= 0.4582364E+02				
Log-likelihood	= -0.1282691E+04	Restr.(b=0) Log-l		= -0.1594383E+04	
Amemiya Pr. Criter.	= 0.2497241E+00	Akaike Info.Crit.		= 0.7515873E-01	
ANOVA Source	Variation	Degrees of Freedom		Mean Square	
Regression	0.4814716E+02	14.		0.3439083E+01	
Residual	0.7788731E+03	10378.		0.7505041E-01	
Total	0.8270203E+03	10392.		0.7958240E-01	
Durbin-Watson stat.	= 1.9836557	Autocorrelation			= 0.0081721

Variable	Coefficient	Std. Error	t-ratio	Prob|t|>x	Mean of X	Std.Dev.of X
Constant	0.13211	0.2082E-01	6.346	0.00000		
CLASS	-0.43720E-01	0.6040E-02	-7.239	0.00000	0.36861	0.48245
CAT1	-0.96642E-02	0.7863E-02	-1.229	0.21902	0.17743	0.38205
CAT2	-0.54831E-02	0.1593E-01	-0.344	0.73071	0.30886E-01	0.17302
CAT3	0.31830E-02	0.1044E-01	0.305	0.76056	0.81401E-01	0.27346
CAT4	-0.16366E-01	0.6615E-02	-2.474	0.01336	0.34427	0.47515
IAHIST	-0.11964E-02	0.3414E-03	-3.504	0.00046	17.577	8.7842
UIHIST	0.22480E-03	0.4199E-02	0.054	0.95731	1.8787	4.2462
DUI	-0.26048E-01	0.1997E-01	-1.304	0.19211	0.79323	0.40501
UI2	0.18943E-03	0.2127E-03	0.891	0.37320	21.558	59.416
ONUISTRT	0.89396E-01	0.1376E-01	6.499	0.00000	0.50419E-01	0.21882
PGM1	0.17215	0.2118E-01	8.128	0.00000	0.16550E-01	0.12758
PGM2	0.16598	0.1335E-01	12.429	0.00000	0.42913E-01	0.20267
PGM3	0.15351	0.6141E-01	2.500	0.01243	0.19244E-02	0.43828E-01
PGM6	0.12555	0.3306E-01	3.798	0.00015	0.67353E-02	0.81796E-01

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MODEL COMMAND:
REGRESS;LHS=ONUI30M;RHS=REGS$

```
Ordinaryleast squares regression.                      Dep. Variable          = ONUI30M
Observations                           = 10071                    Weights          = ONE
Mean of LHS               = 0.1057492E+00             Std.Dev of LHS = 0.3075316E+00
StdDev of residuals       = 0.3030612E+00   Sum of squares         = 0.9236041E+03
R-squared                 = 0.3021179E-01  Adjusted R-squared      = 0.2886165E-01
F[ 14, 10056]             = 0.2237674E+02
Log-likelihood            = -0.2259657E+04   Restr.(<b>=0) Log-l   = -0.2414134E+04
Amemiya Pr. Criter.       = 0.4517242E+00     Akaike Info.Crit.    = 0.9198287E-01
ANOVA   Source                   Variation        Degrees of Freedom          Mean Square
     Regression             0.2877302E+02              14.              0.2055215E+01
       Residual             0.9236041E+03           10056.              0.9184607E-01
          Total             0.9523771E+03           10070.              0.9457568E-01
Durbin-Watson stat.       = 2.0321442                 Autocorrelation                = -0.0160721
```

Variable	Coefficient	Std. Error	t-ratio	Prob|t|< x	Mean of X	Std.Dev.of X
CLASS	-0.43274E-01	0.6734E-02	-6.426	0.00000	0.37722	0.48472
CAT1	-0.32975E-01	0.8812E-02	-3.742	0.00018	0.17933	0.38364
CAT2	-0.22180E-01	0.1804E-01	-1.229	0.21900	0.30384E-01	0.17165
CAT3	0.38018E-02	0.1177E-01	0.323	0.74675	0.81025E-01	0.27289
CAT4	-0.17527E-01	0.7430E-02	-2.359	0.01833	0.34684	0.47599
IAHIST	-0.11170E-02	0.3840E-03	-2.909	0.00363	17.691	8.7607
UIHIST	-0.72732E-02	0.4753E-02	-1.530	0.12597	1.8341	4.2001
DUI	-0.66157E-01	0.2255E-01	-2.934	0.00334	0.79714	0.40215
UI2	0.65614E-03	0.2409E-03	2.724	0.00645	21.003	58.780
ONUISTRT	0.28068E-01	0.1546E-01	1.816	0.06941	0.50839E-01	0.21968
PGM1	0.90794E-01	0.3569E-01	2.544	0.01096	0.72485E-02	0.84833E-01
PGM2	0.66461E-01	0.1936E-01	3.433	0.00060	0.25122E-01	0.15650
PGM3	0.12293	0.6794E-01	1.809	0.07038	0.19859E-02	0.44521E-01
PGM6	-0.31107E-01	0.4819E-01	-0.646	0.51857	0.39718E-02	0.62900E-01

Appendix C: Details Of Analysis Relating To Job Club

Analyzing The Level of Benefits Received by Participants in the Region A Job Action Pilot Project

Unfortunately more people with dependents were randomly selected into the participant group of the Job Action Pilot Project, so they were entitled to a higher average level of benefits than the control group. The variables which determine eligibility, number of dependents, classification and category are known and can be used as explanatory variables. Benefits received will vary from maximum entitlement as a result of variable shelter costs, which we expect to be uncorrelated with program participation, eligibility for other allowances, which may be correlated with program participation (eg work clothing) and income, which we expect to be correlated with program participation.

Each month in which an individual in the study received benefits generated a data point. Results are given below.

```
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Reading file  REGABEN.DAT
MODEL COMMAND:
CRMODEL;LHS=BEN;RHS=ONE,D1,D2,D3,CAT3,CAT4,CAT5,XCLASS,P1,P2,P3,P4,P5$

Ordinaryleast squares regression.                    Dep. Variable       = BEN
Observations          = 1899                               Weights       = ONE
Mean of LHS                         = 0.4517841E+03     StdDev of LHS       = 0.1801924E+03
StdDev of residuals     = 0.1309431E+03                 Sum of squares      = 0.3233753E+08
R-squared                           = 0.4752677E+00     Adjusted R-squared  = 0.4719290E+00
F[ 12,  1886]                       = 0.1423511E+03
Log-likelihood          = -0.1194522E+05                       Restr(a=0) Log-l = -0.1255752E+05
Amemiya Pr. Criter.     = 0.1259422E+02                      Akaike Info.Crit.   = 0.1726347E+05
ANOVA     Source              Variation          Degrees of Freedom     Mean Square
     Regression             0.2928919E+08              12.            0.2440765E+07
       Residual             0.3233753E+08            1886.            0.1714609E+05
          Total             0.6162671E+08            1898.            0.3246929E+05
Durbin-Watson stat.     = 1.5359713          Autocorrelation      = 0.2320143
VariableCoefficient     Std. Error t-ratio     prob|t|>x  Mean of X          Std.Dev.of X
Constant      412.06        3.571     115.402  0.00000
D1            282.29       19.00       14.860  0.00000     0.56345E-01             0.23065
D2            512.41       23.29       22.004  0.00000     0.35282E-01             0.18454
D3            643.05       29.30       21.945  0.00000     0.17904E-01             0.13264
CAT3           39.430      36.62        1.077  0.28155     0.78989E-02             0.88547E-01
CAT4         -253.84       32.12       -7.904  0.00000     0.16324E-01             0.12675
CAT5          -14.520      20.66       -0.703  0.48928     0.62138E-01             0.24147
XCLASS         72.809      19.58        3.718  0.00030     0.24223E-01             0.15378
P1             -7.2357     12.89       -0.561  0.58183     0.58452E-01             0.23466
P2            -14.031      14.86       -0.944  0.34770     0.43181E-01             0.20332
P3            -23.226      15.80       -1.470  0.13743     0.37915E-01             0.19104
P4             -6.9951     16.97       -0.412  0.68263     0.32649E-01             0.17776
P5            -23.395      18.15       -1.289  0.19426     0.28436E-01             0.16626
XCLASS         72.517      19.57        3.705  0.00031     0.24223E-01             0.15378

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```

The variables used were:

- Di took the value 1 if the case had i dependents;
- CAT3 took the value 1 if the case was a couple;
- CAT4 took the value 1 if the case was a two parent family;
- CAT5 took the value 1 if the case was a one parent family;
- XCLASS took the value 1 if the case was classified as unemployable;
- Pi took the value 1 if the case was selected for Job Action, and the benefits were paid i months after selection.

Confirming Random Selection In The Job Action Control Group

It is always possible for workers to subvert the random assignment process and substitute clients of their choosing for those randomly selected. If workers are successful in substituting clients, and if the clients which they choose have characteristics which are different from the group as a whole, then once again we will face the problem of not knowing whether the difference in the outcome is due to the different characteristics or to the program itself.

In so far as the characteristics have been measured, the extent of the substitution (contamination) can be identified. The table below lists the means of a number of different characteristics for the participants and the control group. In no case is the difference between the means statistically significant - the difference is not twice the standard deviation of the difference. We can conclude that if the random assignment process was subverted, it was not subverted to such a degree that it was detectable with this sample size.

The variables used were:

- Di took the value 1 if the case had i dependents;
- CAT3 took the value 1 if the case was a couple;
- CAT4 took the value 1 if the case was a two parent family;
- CAT5 took the value 1 if the case was a one parent family;
- XCLASS took the value 1 if the case was classified as unemployable;
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Appendix D: Regression Results: Impact of programs on employment

The Impact of the Program on Employment

Information on the subsequent employment of the participants was gathered from the survey. In addition to the variables retained in the administrative records, survey records contained information on schooling and employment history. The variables APS and AUN take the value one if the recipients had received any post-secondary education or any university education. The variable SCH contained the number of years of elementary and high school completed. EMPHIST is the number of months of employment history. PGM4 takes the value one if the recipient participated in Job Action. This variable is a remnant of a failed attempt to include participants in Job Action from other than the Region A Job Action Pilot project in the study. The attempt was dropped when it became clear in conversations with contractors that they were only reporting their successes. PGM5 takes the value one if the recipient participated in the Forestry program.

Full information was available for 1,077 observations. 1,422 respondents answered the question "Are you working now?" The information on the missing explanatory variables (schooling and employment history variables) was filled using the technique outlined by Griliches. The technique used was maximum-likelihood probit.

The results follow for the question "Are you working now?"

Binomial Probit Model
Maximum Likelihood Estimates
Log-Likelihood................................ -960.2569
Restricted (Slopes=0) Log-L.......... -985.4527
Chi-Squared (16)............................ 50.39162
Significance Level.......................... 0.1985276E-04

Variable	Coefficient	Std. Error	t-ratio
Constant	-1.0051	0.2793	-3.598
AGE	0.23397E-03	0.3405E-03	0.687
SCH	0.86639E-01	0.2267E-01	3.821
APS	-0.38096E-02	0.6987E-01	-0.055
AUN	-0.51158E-01	0.9923E-01	-0.516
CLASS	-0.18278E-01	0.9241E-01	-0.198
CATI	-0.16685	0.8714E-01	-1.915
CAT2	0.41538E-01	0.1835	0.226
CAT3	0.33024	0.1312	2.518
CAT4	0.31632	0.1021	3.097
EMPHIST	0.59811E-03	0.1424E-02	0.420
IAHIST	-0.14820E-01	0.5279E-02	-2.807
PGMI	0.24283E-02	0.1004	0.024
PGM2	0.27751	0.9312E-01	2.980
PGM3	0.12248	0.1656	0.740
PGM4	0.20878	0.1033	2.021
PGM5	-0.56430	0.3473	-1.625

Appendix E: Limdep Commands for Monte Carlo Study

```
OPEN;output=monte.rot$
calc;a1=0;a2=0;a3=0;a4=0;c3=0;c4=0$
matrix;olsa=[0];tslsa=[0];hecka=[0];heckb=[0];
cnt3=[0];cnt4=[0]$

do for; iters;i = 1, 100$
sample;1-10000$
create; x1=rnn(0,50);x2=rnn(0,50);x3=rnn(0,10);
e1=rnn(0,50); bias=rnn(0,50);
e2=rnn(0,50);
in=x1 + x2 + x3 + e1 + bias;
t=in>0;
y=x1 + x2 + 40*t +e2 + bias;
if (y>246) y=246;
if (y<-226) y= -226$
2sls;lhs=y;rhs=one,t,x1,x2;inst=one,x1,x2,x3$
calc;a3=a3+b(2);a2=a2+varb(2,2)$
enddo;iters$
probit;lhs=t;rhs=one,x1,x2,x3;hold results$
select;lhs=y;rhs=one,t,x1,x2;all$
calc;a3=a3+b(2);a2=a2+varb(2,2)$

enddo;iters$
matrix;olsa=[olsa/a1];tslsa=[tslsa/a2];hecka=[hecka/a3];heckb=[heckb/a4];
cnt3=[cnt3/c3];cnt4=[cnt4/c4]$
calc;a1=0;a2=0;a3=0;a4=0;c3=0;c4=0$

enddo;samps$
```

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